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ESSAYS ON THE CRIMINAL JUSTICE SYSTEM: QUASI-EXPERIMENTAL EVIDENCE ON POLICING AND INCARCERATION

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TABLE OF CONTENTS

LIST OF FIGURES v

LIST OF TABLES vi

ACKNOWLEDGMENTS viii

ABSTRACT ix

1 DOES DISTANCE FROM HOME MATTER IN PRISON? EFFECTS ON VISITATION AND RECIDIVISM 1
- 1.1 Introduction 1
- 1.2 Background 7
  - 1.2.1 Facility Placement 7
  - 1.2.2 Visitation 11
- 1.3 Data 13
  - 1.3.1 Who Receives Visits? 20
- 1.4 Empirical Strategy 22
  - 1.4.1 The Effect of Distance on Recidivism 23
  - 1.4.2 The Effect of Visitation on Recidivism 26
- 1.5 Results 32
  - 1.5.1 The Effect of Distance on Recidivism 32
  - 1.5.2 The Effect of Distance on Visitation 33
  - 1.5.3 The Effect of Visitation on Recidivism 35
- 1.6 Mechanism: Housing Stability 48
- 1.7 Counterfactual Placement Policies 57
- 1.8 Conclusion 60

2 AN EVALUATION OF CRISIS-INTERVENTION TEAM TRAINING 62
- 2.1 Introduction 62
- 2.2 Background 64
  - 2.2.1 911 calls and Police Response 64
  - 2.2.2 Crisis-Intervention Team Training 66
- 2.3 Data 67
  - 2.3.1 Descriptive Statistics 69
- 2.4 Empirical Strategy 73
- 2.5 Results 76
  - 2.5.1 Spillovers 78
- 2.6 Conclusion 79

3 PRISON DIVERSION FOR PARENTS 81
3.1 Introduction 81
3.2 Background 82
3.2.1 Sentencing and Prison in Washington 82
3.2.2 Parental Sentencing Alternative 83
3.3 Data 85
3.4 Empirical Strategy and Results 91
3.4.1 Matching 91
3.4.2 Within-Judge Comparisons 95
3.5 Conclusion 97

REFERENCES 99

APPENDIX A DOES DISTANCE FROM HOME MATTER IN PRISON? EFFECTS ON VISITATION AND RECIDIVISM 110
A.1 Appendix Figures 110
A.2 Appendix Tables 115
A.3 Heterogeneity and Non-Linearities 142
A.3.1 Non-linearties in Visitation 142
A.3.2 Heterogeneity by observable characteristics 144
A.3.3 Subsequent Crime 145
A.3.4 Visitors 146
A.4 Extended Family Visits 154
A.5 Complier Characteristics 161
A.6 Selection Test Under Heterogeneity 164

APPENDIX B AN EVALUATION OF CRISIS-INTERVENTION TEAM TRAINING 167
B.1 Appendix Figures 167
B.2 Appendix Tables 168
B.3 Use of Force Definition 170
B.4 Alternative Specifications 171
B.4.1 Static TWFE 171
B.4.2 Dynamic Two-Way Fixed Effects 175
B.4.3 TWFE Weights 177

APPENDIX C PRISON DIVERSION FOR PARENTS 180
C.1 Appendix Tables 180
C.2 Community Parenting Alternative 181
C.3 Other Diversion Programs in Washington 181
C.4 Judge Leniency IV 182

LIST OF FIGURES

1.1 Washington State Prisons 10
1.2 Distance Distribution 11
1.3 First Stage 30
1.4 Joint Housing and Recidivism Outcomes 54

2.1 Mental Health Call Composition 76
2.2 Use of Force and Arrests 77

3.1 Common Support 93

A.1 Distance vs. Drive-Time 110
A.2 Predicted Facility Placement by Security Level 111
A.3 Heatmap of Home County of Each Prison's Population 112
A.4 Heatmap of Share of Each County in Each Prison's Population 113
A.5 Joint Housing and Recidivism Outcomes, Binary Visit Measure 114
A.6 Visitor Composition by Distance 149
A.7 Visitor Composition Over Spell 150

B.1 Top Types of Calls Resulting in Force 167
B.2 Pre-Trends 173
B.3 Always Treated vs. Eventually Treated 175
B.4 Dynamic TWFE Estimates 177

C.1 Judge Leniency 184

LIST OF TABLES

1.1 Main Sample Descriptive Statistics . . . 16
1.2 Visit Descriptive Statistics . . . 19
1.3 Selection into Visitation . . . 21
1.4 Balance on Distance from Home to Prison . . . 27
1.5 The Effect of Distance on Recidivism . . . 33
1.6 The Effect of Distance on Visitation . . . 35
1.7 The Effect of Visitation on Recidivism . . . 37
1.8 The Effect of Visitation on Time Served . . . 40
1.9 Visitation Effects, Alternate Models . . . 47
1.10 Housing and Recidivism . . . 52
1.11 Visit Timing and Housing Stability . . . 55

2.1 Incident Descriptive Statistics . . . 71
2.2 Mental Health Incident Descriptive Statistics . . . 72
2.3 Demographics by District . . . 72
2.4 Officer Demographics by CIT Status . . . 73
2.5 Pooled DiD . . . 78
2.6 Other Incident Types . . . 79

3.1 Sample Construction . . . 86
3.2 FOSA Sample Construction . . . 87
3.3 Descriptive Statistics by FOSA-Approval Status . . . 89
3.4 Descriptive Statistics by FOSA-Completion Status . . . 90
3.5 Matching Estimator and OLS . . . 94
3.6 Within Judge OLS . . . 97

A.1 Supplementary Sample Descriptive Statistics . . . 115
A.2 Balance on Distance to Initial Placement . . . 117
A.3 Descriptive Statistics by Housing Assistance Status . . . 118
A.4 Descriptive Statistics of Voucher Recipients by Visitation Status . . . 120
A.5 Supplementary Sample Descriptive Statistics by Housing Assistance Status . . . 122
A.6 Supplementary Sample, Sub-group Use of Voucher . . . 124
A.7 Distance Threshold Effects on Recidivism . . . 125
A.8 2SLS Distance Results, Seattle Metropolitan Area Excluded . . . 126
A.9 First Stage of Distance and Visits, Logit and Poisson . . . 127
A.10 The Effect of Distance Rank on Visitation . . . 127
A.11 First Stage of Distance and Visits, Alternate SE Clusters . . . 130
A.12 2SLS Visitation Results, Alternate SE Clusters . . . 131
A.13 2SLS Visitation Results, Incarcerated Individuals Only in One Facility . . . 132
A.14 2SLS Visitation Results, More Than 1 Visit . . . 133
A.15 2SLS Visitation Results, Seattle Metropolitan Area Excluded . . . 134
A.16 2SLS Visitation Results, Airway Heights Corrections Center Excluded . . . 135

A.17 The Effect of Visitation on Recidivism: Binary and Continuous Instrument . . . 136
A.18 The Effect of Visitation on Recidivism, EFV Recipients Removed 137
A.19 The Effect of Visitation on Release from Lower Security Facility 138
A.20 The Effect of Visitation on Housing Stability 139
A.21 Joint Housing and Prison Readmission Outcomes 140
A.22 Counterfactual Placement Simulations 141
A.23 Nonlinear Visitation Effects 143
A.24 Treatment Effect Heterogeneity by Observable Characteristics 145
A.25 Treatment Effect Heterogeneity by New Crime Type 147
A.26 Visits from Only One Visitor Type 152
A.27 Descriptive Statistics by Dominant-Visitor Group 153
A.28 Visitation Effects by Visitor Type 154
A.29 EFV First Stage 157
A.30 Sample Descriptives by EFV Status 158
A.31 Supplementary Sample EFV Statistics 160
A.32 Effective Population Characteristics 163
A.33 Selection Test 166

B.1 Officer Demographics by Training Class 168
B.2 Officer Demographics by Shift 169
B.3 Static Two-way Fixed Effects 174
B.4 Trimmed Panel 179

C.1 Descriptive Statistics for Within-Judge Cases 180
C.2 Judge Leniency First Stage 185
C.3 Judge Leniency Balance 186

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ABSTRACT

This dissertation consists of three papers that use quasi-experimental methods to examine key aspects of the criminal justice system, including incarceration, policing, and sentencing. Chapter 1 examines whether the distance between prison and an incarcerated individual's home affects in-prison and post-release outcomes, exploiting quasi-random facility assignment. I find that placement farther from home increases prison readmission rates, driven by reduced visitation from family and friends. Using distance as an instrument, I then estimate the causal effect of visitation on recidivism and find that additional visits reduce reoffending. I further provide evidence that improved housing stability is one mechanism underlying this relationship. Chapter 2 evaluates the causal effect of crisis intervention team (CIT) training, a mental health de-escalation method, for police officers on use of force and arrest behavior, leveraging variation in the timing of training across officers. I find no evidence that this training reduces arrests or use of force. Chapter 3 studies a prison diversion program for parents with dependent minor children. There is some suggestive evidence of a short-run increase in offending, consistent with reduced incapacitation, though these effects do not persist overtime and imprecisely estimated.

CHAPTER 1

DOES DISTANCE FROM HOME MATTER IN PRISON? EFFECTS ON VISITATION AND RECIDIVISM

1.1 Introduction

The United States incarcerates more people than any other OECD country, and the vast majority of individuals leaving prison eventually return: roughly 87 percent are rearrested and over 60 percent are re-incarcerated within ten years [Fair and Walmsley, 2024, Carson and Kluckow, 2023].[1] Despite extensive policy efforts to reduce recidivism, empirical evidence on what improves post-release outcomes is mixed.[2] Existing research spans in-prison programming, sentencing policy, and post-release services, but much less is known about how routine administrative decisions made during incarceration shape reentry success. One such administrative decision is where a person serves their sentence.

Within each state, correctional agencies operate multiple prisons and assign individuals to specific facilities. This assignment determines the distance from a person's home to the prison, which varies widely both across and within states. Distance is meaningful because it shapes whether families can sustain contact during incarceration. Visiting a prison hours away imposes significant time and financial burdens, and family support is widely viewed as important for successful desistance from crime. In recent years, Departments of Correction have begun to explicitly consider proximity to home when making placement decisions. For example, the Federal Bureau of Prisons now instructs placement within 500 miles of the residence [Congressional Research Service, 2019], Arkansas and Hawaii incorporate family contact into placement decisions, and New York requires parents to be placed as close

1. For comparison, Canada's rate is roughly 85 per 100,000, Germany's is 70 per 100,000, and the United Kingdom's is 130 per 100,000.

2. Doleac [2023] provides a comprehensive overview of empirical studies on programs aimed at reducing recidivism.

to minor children as practicable [Arkansas General Assembly, 2024, Hawaii State Legislature, 2024, New York State Department of Corrections and Community Supervision, 2023a]. However, despite this growing attention, there is limited causal evidence on whether distance affects recidivism due to both data constraints and endogeneity concerns.

In this paper, I assemble a unique dataset linking home addresses, facility assignments, and comprehensive visitation records for over 20,000 individuals and 200,000 visits in Washington State. I leverage a key institutional feature of Washington's prison assignment system: placement decisions are based on security classification and capacity constraints, and do not consider proximity to home or family ties. This policy generates plausibly exogenous variation in distance from home, which I use to estimate the causal effect of distance on post-release outcomes.

A 100-mile increase in home-to-prison distance, approximately 1.3 standard deviations, raises the probability of prison re-admission by 0.8 percentage points (a 9 percent increase) within one year of release and by 1.1 percentage points (a 4 percent increase) within three years. To contextualize this distance, a 100-mile trip in Washington typically requires 1.5 to 3 hours of driving, and public transportation options to prisons are limited.

Distance primarily affects reentry through its effect on in-person visitation. Maintaining contact with family and friends provides emotional and practical support during and after incarceration, and prior research shows that greater distance substantially reduces the likelihood of visits [Casey-Acevedo and Bakken, 2002, Duwe and Clark, 2013, Cochran et al., 2016]. Consistent with this, I find that being placed 100 miles farther from home lowers the probability of receiving any visits by 11 percentage points (a 25 percent decline from a 44 percent baseline) and reduces monthly visits by 0.6 (a 60 percent decline from a baseline of one visit per month).

Research in sociology and criminology consistently highlights the role of social support networks in shaping reentry outcomes. Theories of social and informal control emphasize

that strong ties to family, peers, and institutions can deter criminal behavior and promote desistance (see Hirschi [1969], Laub and Sampson [1993], La Vigne and Naser [2006], La Vigne et al. [2009]). One of the main ways individuals maintain contact with their non-incarcerated social network while incarcerated is through prison visitation.[3] A large body of correlational research finds that visitation is associated with lower recidivism. For example, a meta-analysis by Mitchell et al. [2016] finds that visitation is associated with a reduction in recidivism by about 26 percent, and Bales and Mears [2008] report that each additional visit is associated with lower the odds of reoffending.[4] However, this literature cannot disentangle whether these effects reflect the causal effect of visitation or the fact that individuals with stronger social ties are both more likely to receive visits and less likely to reoffend.

Other potential channels through which distance might affect recidivism in Washington are less convincing. Prisons of the same security level offer comparable programming, and I find no evidence that distance is systematically correlated with access to programming, education, or prison jobs. Distance is also not correlated with observable characteristics such as offense type or sentence length once security classification is accounted for. This leaves family and social connections as the most plausible mechanism: greater distance raises the direct costs to family and friends of maintaining contact with an incarcerated loved one. Although individuals can communicate through letters, phone calls, or video visits, these alternatives are also costly and, importantly, are not impacted by distance. Together, this points to visitation as the primary pathway through which distance affects reentry outcomes.

I next estimate the causal effect of visits on recidivism by using prison-to-home distance as an instrument. I find that each additional visit per month of incarceration reduces one-year recidivism by 1.4 percentage points (a roughly 14 percent reduction) and three-year

3. Depending on the jurisdiction incarcerated individuals may also write letters, talk on the phone, and participate in video calls.

4. Other studies include Ryan and Yang [2005], Mears et al. [2012], Duwe and Clark [2013], Cochran [2014].

recidivism by 1.9 percentage points (a roughly 7 percent reduction). This effect is comparable in magnitude to the impact of expanded welfare access documented by Palmer et al. [2019], who find a 10 percent decrease in re-incarceration within one year of release.

Beyond recidivism, I also examine the effect of visitation on the fraction of sentence served, which serves as a proxy for in-prison misconduct. During intake, individuals receive an Earned Release Date reflecting a potential reduction of 10 to 50 percent of their sentence. Earned release time can be revoked for disciplinary infractions or failure to participate in required programming, so fraction of sentence served provides a measure of disciplinary behavior in prison. I find that an additional visit per month reduces the fraction of a sentence served by one percent, which is equivalent to about 5 fewer days at the median untreated sentence length of 517 days. Combining the reduction in days served of the current sentence with the decrease in future re-incarceration, an additional visit per month lowers total time served in the five years following sentencing by 25 days. At an estimated cost of $174 per incarcerated individual per day, this translates into approximately $4,350 in savings per person.

To understand how visits improves reentry outcomes, I examine a key mechanism: access to stable housing after release. Stable housing is central to successful reentry, and many individuals leaving prison face housing insecurity. In Washington State, individuals without an approved release address cannot be released at their earned release date. To avoid continued incarceration solely due to lack of housing, the Department of Corrections issues housing vouchers to all eligible individuals. Voucher receipt therefore provides a direct measure of housing instability. I find that an additional visit per month reduces reliance on housing vouchers by 12 percent. Additionally, address verification for release begins six months prior to release, and I find that receiving visits near the end of a prison spell has the strongest effect on reducing voucher use.

Building upon these results, I simulate placement under alternative regimes that prioritize proximity to home and calculate the resulting effects on recidivism. First, I do this without constraints and then I impose security level and facility capacity constraints. When placement policies emphasize assigning individuals to facilities closer to home, subject to capacity and security level, the predicted effect is a 0.4 percentage-point reduction in recidivism within one year of release a 0.5 percentage-point reduction in recidivism within three years of release. Relative to the non-treated baseline recidivism rates, these correspond to a 4 and 2 percent reduction.

This project contributes to a large literature on recidivism and interventions aimed at reducing recidivism. Prior work has examined interventions ranging from incarceration itself, such as Bhuller et al. [2020b], who leverage a random judge design in Norway to show that incarceration can reduce future offending, to diversion programs, with Mueller-Smith and Schnepel [2021b] documenting large reductions in recidivism. Other studies emphasize interventions within prisons. Alsan et al. [2025] demonstrate that an education program in county jails reduces both misconduct and reoffending, while Lee [2019a] finds that none of the prison programming in Iowa decreased recidivism. Beyond incarceration, studies have shown that improving labor market opportunities can lower reoffending by increasing the outside option to crime [Agan and Makowsky, 2018a], and that DNA databases can significantly reduce recidivism by raising the perceived probability of detection [Doleac, 2017]. Evaluations of reentry programs providing wraparound services often report null effects [Doleac, 2019, Grommon et al., 2013, Cook et al., 2015, Wiegand and Sussell, 2016, D'Amico and Hui, 2018], although some evidence suggests that expanding access to welfare programs such as the EITC and food stamps can reduce recidivism [Agan and Makowsky, 2018a, Yang, 2017, Tuttle, 2019]. This paper examines prison placement as a policy margin that indirectly shapes reentry outcomes.

Closely related work includes Lee [2019b] and Andersen et al. [2024], both of which use

variation in distance to estimate the effects of visitation and find no statistically significant impact on re-admission to prison. Lee [2019b], using data from Iowa, reports estimates that are qualitatively similar to mine, but with a smaller sample and less precision. With a sample approximately twice as large, I obtain more precise estimates and additionally show that visitation affects post-release housing stability. Andersen et al. [2024] study visitation in Denmark using variation in distance and prison capacity and likewise find no effect on recidivism. However, the Danish institutional context differs substantially from the U.S., with higher visitation rates, a legal right to visitation, shorter sentences, and a more robust social services system.

This paper also contributes to a broader literature on prison environments. d'Este [2025] study the effects of a ban on synthetic drugs on in-prison drug use and violence, while Calamunci et al. [2025] examine the impact of single-gender prisons on recidivism for women. Chen and Shapiro [2007] analyze the effect of prison security level and harshness of the prison environment on post-release outcomes. Most closely related, Weber [2020] study the effects of distance from home but rely on a proxy measure, as prison placement and associated distance are not directly observed. In this paper, I instead measure distance directly.

This project further contributes to a growing body of economics literature examining the role of familial bonds in shaping criminal justice outcomes, such as the effect of parental criminal justice involvement on children's long-term criminal behavior and well-being (e.g., Dobbie et al. [2018a], Arteaga [2023], Finlay et al. [2023], Norris et al. [2021]) and the relationship between family formation and parents' future criminal activity [Massenkoff and Rose, 2024]. This paper contributes by examining prison visitation, which is one of the key ways in which families interact with their incarcerated loved ones.

The paper proceeds as follows: Section 2 provides institutional background information on how incarcerated individuals are placed in prisons and how visitation works, Section 3 describes the data, Section 4 details the empirical strategy, Section 5 presents results,

and Section 6 discusses the housing mechanism, Section 7 implements alternative placement policies, and Section 8 concludes.

1.2 Background

Washington State has a population of nearly 8 million, with a demographic profile that is whiter than the nation overall but with a larger share of Asian and multiracial residents. Its prison population of about 14,000 translates to an imprisonment rate of roughly 180 per 100,000 residents, which is below the national average and lower than rates in neighboring Oregon, California, and Idaho. Although Washington differs in some ways, its geographic structure of one large urban hub and many rural communities mirrors the geographic and institutional layout of much of the United States, making it a relevant setting for understanding broader patterns.

1.2.1 Facility Placement

As part of the intake process, each individual is assigned a recidivism risk score based on a range of factors, including demographics, current offense and sentence, criminal history, substance use history, and family structure.[5] This type of risk-based classification is not unique to Washington; similar systems are widely used across the country to categorize incarcerated individuals. In Washington, individuals are assigned to one of several risk categories: low risk, moderate risk, high drug risk, high property risk, high violent risk, or high diverse risk (indicating high risk across violent, drug, and property offenses). The classification system is hierarchical: high diverse supersedes high violent, which supersedes high drug , which in turn supersedes high property, moderate, and low. For example, an individual assessed as both high violent and high property will be classified as high violent;

5. This score is generated using the Washington Offender Needs Evaluation tool, which applies a weighted point system to produce an objective assessment of felony re-offense risk.

someone assessed as both high property and high drug will be classified as high drug.

The risk classification helps determine an incarcerated individual's initial security level, referred to as the Initial Custody Designation in Washington state. Individuals can be designated as "close custody", "medium custody, or "minimum custody."[6] Specific facility placement is then based on this custody classification in combination with the individual's programming needs (including work, education, and treatment, when applicable), as well as health requirements, safety concerns, and institutional capacity. According to Washington Department of Corrections policy, individuals are to be placed in the least restrictive facility that can accommodate both their programming and security needs. Importantly, incarcerated individuals have no input in their placement, and placement decisions are not subject to appeal.

As program availability is an explicit input to placement, it is useful to consider how programming varies across facilities. All facilities offer core services—basic education/GED, sobriety support (e.g., AA), and prison employment. Each facility also runs an animal-training program. Beyond these basics, offerings differ: some prisons provide vocational programs (e.g., construction trades, vehicle maintenance) or sustainability initiatives, and work opportunities vary (e.g., metal shop versus upholstery). These differences are largely stable over my study window and will be addressed in my empirical strategy.

The initial placement facility is the facility in which an incarcerated individual will serve either their entire sentence or the majority of their sentence. In my data fifty-six percent of incarcerated individuals are released from the same facility they are initially placed in. Incarcerated individuals generally re-do the risk assessment questionnaire every six months to determine if there are changes to their security classification or programming needs. If an

6. One subset of minimum custody is MI3 custody, which requires individuals to be placed in the same type of facility as medium custody individuals, and so I classify these individuals as medium custody as well. Individuals placed directly into community supervision are those serving less than 1 year and are designated as MI1 custody. WADOC also operates maximum security housing, which is a higher security designation than close custody. However, this is not one of the Initial Custody Designations but is assigned based on behavior while incarcerated.

incarcerated individuals security level has been downgraded based on program participation, lack of infractions and time served they may be transferred to a lower security facility if capacity allows. Alternatively, an incarcerated individual can be moved to a higher security facility due to disciplinary infractions. Incarcerated individuals may also request transfers, which are considered conditional on security classification and capacity constraints.[7] All transfers can be impacted by prisoner behavior or preferences, and so I will solely focus on initial placement facility in my empirical strategy.

Between 2010 and 2019 Washington state had ten facilities for men and two for women.[8] Seven of the ten male facilities in Washington state house incarcerated individuals of various custody levels, while three house only minimum security incarcerated individuals. There are five facilities that can house close custody individuals, seven for medium security and eight for minimum. Both women's facilities house minimum custody incarcerated individuals and one additionally houses incarcerated individuals with higher level custody classifications. A map of the current prison facilities is provided in Figure 1.1.

7. Of those who do not stay in their placement facility 44 percent are released from a work release facility or reentry center (both are minimum level facilities meant to help incarcerated individuals at the end of their sentences), 32 percent are released from a facility further from home (can include work release facilities), and 65 percent are released from a facility closer to home (can include work release facilities).

8. There was one facility that closed over this time period. I exclude this facility and anyone placed in it from all analysis.

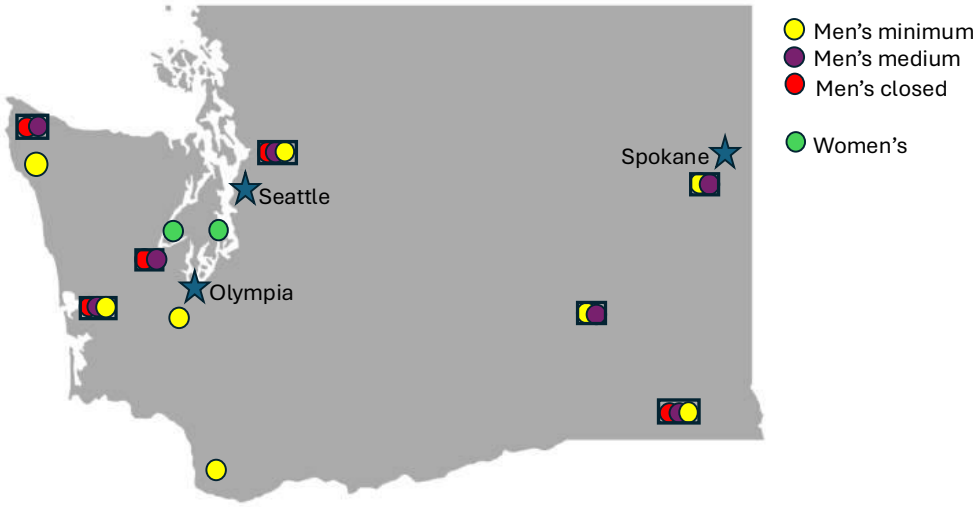


Figure 1.1: Washington State Prisons

Notes: This figure plots the location of all state prison facilities. Locations are based on Washington Department of Corrections facility addresses. Yellow dots denote men's minimum security facilities. Purple dots denote men's medium security facilities. Red dots denote men's close security facilities. Green dots denote women's facilities. Some facilities house multiple security levels. Boxes with multiple colored dots indicate a prison facility that houses multiple security levels. Blue stars denote the three largest cities: Seattle, Olympia and Spokane. These are the three largest population centers in Washington.

As indicated by Figure 1.1, facilities are spread across the state resulting in substantial variation in home-to-facility distances across incarcerated individuals. Figure 1.2 illustrates the distribution of incarcerated individual placement distances, showing that incarcerated individuals are housed between zero and three-hundred miles from home, with a median distance of ninety-nine miles.

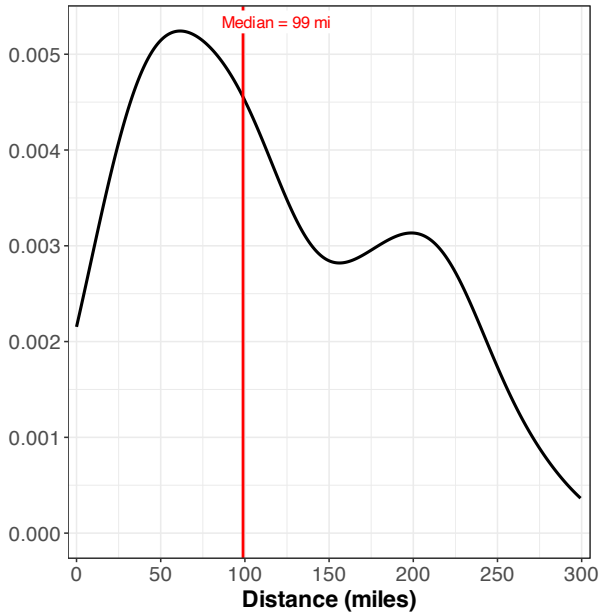


Figure 1.2: Distance Distribution

Notes: This figure presents the distribution of placement distances for individuals incarcerated in Washington state from 2010 through 2016. The left panel plots the density of placement distance measured in miles from home county to prison facility. The red line shows the median placement distance of 99 miles. The right panel plots histograms of placements by the distance rank of the placement facility, within security level. A minimum security individual has 8 possible facilities, each given a rank of 1 to 8 based on its distance from the individual's home county. A medium security individual has 7 possible facilities, each given a rank of 1 to 7 based on its distance from the individual's home county. A close security individual has 5 possible facilities, each given a rank of 1 to 5 based on its distance from the individual's home county. Data comes from Washington Department of Corrections admissions data which includes each individual's home county and placement facility.

1.2.2 Visitation

Across the United States, including in Washington, visitation by family and friends is considered a privilege rather than a guaranteed right.[9] However, all prison facilities in the state permit some form of visitation, including in-person contact visits, non-contact visits, video visits, and overnight family visits. This aligns with the practice of most other states—Washington falls within the majority of states that Boudin et al. [2013] characterize

9. Attorney visits are legally protected.

as promoting visitation in official policy. Across jurisdictions, in-person contact visits are the primary mode of visitation, and Washington follows this pattern, with approximately ninety-nine percent of visits being in-person contact visits.[10] This paper focuses specifically on in-person contact visits.[11]

There are two main categories of visitors: professionals (such as legal teams and clergy) and family and friends. This paper specifically examines visits by family and friends and abstracts from the effect of professional visitation. By law, all incarcerated individuals nationwide are guaranteed the right to legal visits and so this is not the policy relevant channel. In Washington, each facility sets its own visiting hours and guidelines, which are posted on their respective websites. Closures are also posted on the website. Generally, in-person visits are permitted on Friday, Saturday, Sunday and Monday in the afternoons and evenings. For example, visiting hours for the Minimum Security Unit at the Monroe Correctional Complex, the largest prison in Washington, are Fridays and Mondays from 1pm to 5:30pm and 6pm-8:30pm and Saturdays and Sundays from 1pm-8:30pm. Visits operate on a first-come, first-served basis, and entry may be denied if the visitation room reaches capacity.[12]

To participate in visitation, individuals must submit a visitor application, undergo a review process by the Department of Corrections, and receive approval from both the department and the incarcerated person. The review process includes identity verification and a criminal history background check. While a criminal record does not automatically disqualify someone from visiting, certain individuals, such as victims of the incarcerated person's crimes, co-conspirators, and those currently on community supervision with restrictions on

10. Contact visits allow for brief physical contact at the beginning and end of visits and are generally held in a communal visitation room. Non-contact visits forbid any form of contact and may be held in rooms where visitors are separated from incarcerated individuals by glass or another physical barrier.

11. Overnight visits are extremely rare with only 4 states allowing them. Video visitation is becoming more common but still puts Washington in the minority.

12. Visits are only recorded if they are completed. There is no record if someone showed up for visitation but was denied access or capacity was reached.

interacting with known criminals, are generally denied visitation. Visitors may only be on one incarcerated individual's approved visitor list at a time unless they have multiple immediate family members who are incarcerated. Minors are permitted to visit but must be accompanied by a non-incarcerated legal guardian or designated escort. At the time of their visit, all visitors must have a current photograph on file, present valid photo identification, and be on the incarcerated individual's approved visitor list.

The Washington DOC provides limited travel assistance for visitors who live more than 150 miles away, reimbursing up to $50 for gas or lodging no more than twice a month. To qualify, visitors must apply at least 10 days in advance, submit receipts after the visit, and then wait for reimbursement, which is only issued once a month on the 10th. At 2010 gas prices ($2.90 per gallon), driving 300 miles round trip would require about 12 gallons of fuel, or roughly $35, which does not include potential overnight lodging. Because the support comes in the form of reimbursement rather than upfront assistance, many families may lack the resources to cover these costs in advance. Moreover, with the median incarcerated individual placed 99 miles from home, most visitors are not be eligible for this program.

1.3 Data

The data for this project come from Washington Department of Corrections records and include every individual to prison admitted on or after January 1, 2010 and released as of December 31, 2016.[13] Each observation is at the prisoner-spell level as some prisoners can exit and re-enter prison more than once in this time period. Most people appear only once, but 12 percent of individuals do appear more than once in my data. Data includes date of entry, date of release, sentence length, crimes committed, race, gender, date of birth, county

13. Given that the data spans from 2010 to 2016, the maximum sentence an individual in this sample can serve is 6 years, thereby excluding anyone with a longer sentence or a life sentence. However, at most 1 percent of individuals admitted per year are sentenced to life and only between 1-7 percent of individuals are sentenced to longer than 6 years, meaning that this restriction is not particularly consequential.

of sentence, number of previous prison admits and those associated crimes, initial prison facility assignment, the prison facility released from, whether the individual is released into community supervision, the address a person moves to when released, the name of the person responsible for the incarcerated individual's post-release housing and whether the individual is re-admitted to prison within three years of release.

The outcome of interest is readmission to prison for a new criminal offense. The data on prison admissions for community supervision violations do not allow for a clear distinction between technical violations and new criminal conduct, as new crimes may be recorded as violations, and the type of readmission is inconsistently classified[14]. To address this ambiguity, I define recidivism as readmission to prison accompanied by a new sentence, which provides a cleaner and more consistent measure. The one year recidivism rate is 10 percent and the three year recidivism rate is 28 percent putting Washington state below the national average of around 37 percent [Gelb and Velazquez, 2018].

Importantly for my project, the data additionally include all visits each incarcerated individual received during their prison spell. This includes information on the date of the visit as well as the relationship of the visitor, the visitor's age, and the visitor's home county.

I make several sample restrictions. I limit the sample to men with a non-missing home ZIP code in Washington state who's placement facility was a standard facility (not a medical center or work release facility) as their initial placement.[15] Visits are limited to only those from family or friends whose home county is within Washington state. Table 1.1 presents summary statistics on the study sample. The data covers more than 24,000 unique spells in prison with an average incarcerated individual spending 11 months incarcerated. Just under half of the sample ever receives a visit. The restriction to men is due to their only

14. Violators can be sent to specific violator facilities, or to prison for up to 3 week sanctions, or can be re-confined to prison depending on context and so are not always captured accurately in the prison admission data.

15. I drop Garfield and Wahkiakum counties due to extremely small sample size.

being two women's facilities and only one of those facilities houses anyone above minimum custody. The restriction to incarcerated individuals and visitors from within Washington state because I can only observe individuals who are re-admitted to prison within Washington state. Importantly, if someone is incarcerated in another state after their release in Washington they will be classified as a non-recidivist in my data. One factor that eases this concern is that approximately 70 percent of the individuals in my sample are released under community supervision, which mandates release to an approved address within the sentencing county. This requirement reduces the likelihood that the majority of individuals will move out of state, at least during the first year after release. Additionally, Harding et al. [2013] found that only 1.2 percent of prisoners from Michigan had an out-of-state address 2 years post-release, which suggests that formerly incarcerated individuals may not move out-of-state very often.

Table 1.1: Main Sample Descriptive Statistics

	Overall	Never Visited	Visited
N Incarceration Spells	24,338	13,563	10,775
N Incarcerated Individuals	21,389	12,090	10,113
% First admit	0.52	0.49	0.55
Median time served (days)	329	270	423
Median sentence length (days)	578	517	669
% Property crime	0.34	0.34	0.33
% Drug offense	0.25	0.26	0.25
% Violent offense	0.17	0.13	0.22
% Sex offense	0.05	0.04	0.06
% White	0.62	0.59	0.67
% Black	0.15	0.17	0.13
% Hispanic	0.13	0.14	0.12
% Native American	0.05	0.06	0.04
% Asian	0.03	0.04	0.03
Mean Age at admit	33.5	35.06	31.52
% Visited	0.44	0.00	1.00
Mean No. visits per month	0.91	0.00	2.05
Median placement distance (miles)	98.65	111.94	86.37

Continued on next page

Table 1.1 continued from previous page

	Overall	Never Visited	Visited
% Readmitted within 1 year	0.09	0.10	0.07
% Readmitted within 3 years	0.26	0.28	0.22

Notes: This table presents summary statistics for all men incarcerated in Washington state prisons admitted on or after January 1, 2010 and released by December 31, 2016. Data is at the prison spell level. Incarcerated individuals are categorized as "visited" if they received one or more visits during the given spell. Offense types are not mutually exclusive and are binary indicators for if any of the crimes associated with the current spell fall in the given category. Crime category definitions are taken from the Revised Code of Washington. Distance is calculated as the Euclidean distance from the geographic centroid of a prisoner's home ZIP code to the geographic centroid of their placement facility's ZIP code. Data comes from Washington Department of Corrections records and statistics are calculated by the author.

After imposing my sample restrictions, my estimation sample of visits covers over 200,000 visits and about 23,000 unique visitors. Table 1.2 presents descriptive statistics. The plurality of visits are from parents, and this is predominantly by mothers. Friends are the next most frequent visitor. It is important to note that "friend" could encompass a partner and/or co-parent – so long as they are not married they are coded as "friend". I define a category called"partner" for visitors who are of the opposite sex as the incarcerated individual and is within a 5 year age-range of the incarcerated individual. The vast majority of friend visitors fall into this category.

Table 1.2: Visit Descriptive Statistics

N visits	207,501
N visitors	23,058
% of visited incarcerated individuals who get one visit	0.09
% of visitors who visit once	0.28
Mean days between visits for incarcerated individual	17.65
Mean days between visits for visitor	49.84
% of visits by visitor relationship	
% Parent	0.35
% Mom	*0.25*
% Friend	0.31
% Partner	*0.25*
% Child	0.08
% Spouse	0.07

Notes: This table presents summary statistics for all prison visits by family and friends for men incarcerated in Washington state prisons admitted on or after January 1, 2010 and released by December 31, 2016. Visitors can only visit Friday-Monday, so days between visits includes the fact visitors cannot visit for 5 days. "Mom" is a sub-category of "parent" - 70% of parent visits are by a mom. I define "Partner" as a sub-category of"friend" - 80% of friend visits are by a partner. Partner is defined as a visitor of the opposite sex within a 5-year age range of the incarcerated individual. Data comes from Washington Department of Corrections visitation records and statistics are calculated by the author.

In addition to my primary sample, I utilize supplementary risk assessment data available for all individuals incarcerated in 2018 or later.[16] This dataset includes each person's overall risk score, subcomponent scores for violent, drug, and property-related offenses, as well as responses to all underlying assessment items. These items cover domains such as family structure, peer associations, physical and mental health, substance use history, prior labor market attachment, program participation, and attitudinal measures. Although this dataset does not overlap with my main analytic sample, I use it to validate my empirical strategy and provide descriptive evidence on the relationship between individual characteristics and prison placement. Summary statistics for this supplementary sample can be found in Appendix Table A.1.

1.3.1 Who Receives Visits?

I begin by examining which individuals are more likely to be visited under current placement practices. To identify which characteristics are associated with visitation, I estimate a series of OLS regressions. In each specification, the dependent variable is an indicator for receiving any visit, and the right-hand side includes a single observable characteristic with fixed effects for admit year, home county and placement facility. Individuals convicted of violent or sex offenses typically receive longer sentences, so their higher visitation rates may partly reflect the mechanical relationship between sentence length and the likelihood of receiving visits. To disentangle this effect I then re-estimate these regressions adding sentence length as a control.

Results are presented in Table 1.3. Columns (1) and (2) are the results for a binary indicator for visitation and Columns (3) and (4) are the results for visit count within the visited sample. Columns (2) and (4) add sentence length as a control.

Visitation is most common among individuals convicted of violent offenses, those serving

16. WADOC switched to a new risk assessment tool, the Washington Offender Needs Evaluation Tool, in 2018 and this is the data that was readily available.

longer sentences, and younger individuals. Within the visited population, people convicted of violent or sex offenses and younger individuals also receive more visits per month, while property offenders, drug offenders, and recidivists tend to receive fewer. Interestingly, despite being more likely to receive visits, those with longer sentence lengths actually receive fewer visits per month.

Once sentence length is included as a control, the selection patterns remain largely unchanged, indicating that the associations between these characteristics and visitation are not solely driven by the mechanical effect of sentence length elongated the potential treatment period.

Table 1.3: Selection into Visitation

	Visit Received		Visit Count (per Month)	
	(1)	(2)	(3)	(4)
Non-white	-0.072***	-0.0718***	0.2109**	0.2127***
	(0.007)	(0.0069)	(0.072)	(0.0721)
Violent crime	0.1691***	0.1519***	0.9626***	0.978***
	(0.0095)	(0.0094)	(0.1053)	(0.1053)
Property crime	0.0313***	0.0233***	-0.2968***	-0.2921***
	(0.0064)	(0.0064)	(0.06)	(0.0599)
Drug crime	-0.0125*	-0.0031	-0.1118*	-0.1168*
	(0.0073)	(0.0072)	(0.0649)	(0.0648)
Sex crime	0.0658***	0.033**	0.503**	0.5277***
	(0.0145)	(0.0146)	(0.1853)	(0.187)
Recidivist	-0.1205***	-0.1189***	-0.8683***	-0.8666***
	(0.0069)	(0.0068)	(0.0635)	(0.0634)
Age at admit	-0.0079***	-0.0082***	-0.0156***	-0.0152***
	(0.0000)	(0.0000)	(0.003)	(0.003)

Continued on next page

Table 1.3 continued from previous page

	Visit Received		Visit Count (per Month)	
	(1)	(2)	(3)	(4)
Sentence length	0.0053***		-0.0039**	
	(0.0000)		(0.0018)	
Sentence Length Control	No	Yes	No	Yes
Sample	Full	Full	Visited	Visited
Observations	24,338	24,338	10,775	10,775

Notes: *p<0.1, **p<0.05, ***p<0.01. This table shows patterns of selection into visitation across observable characteristics. Each cell shows the coefficient from a separate regression with standard errors clustered at the individual level in parentheses. Columns 1 and 3 omit sentence length as a control, while Columns 2 and 4 include it. Fixed effects for admit year, placement facility, and home county are included in all models. The dependent variable in Columns 1–2 is an indicator for any visit. Columns 3–4 are restricted to those who received at least one visit and the dependent variable is the number of visits per month served. Non-white, violent crime, property crime, drug crime, sex crime and recidivist are all binary indicators for said characteristic. Age is measured in years. Sentence length is measured in months. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code. Underlying data comes from merged Washington Department of Corrections admissions and visitation records.

1.4 Empirical Strategy

This section outlines my empirical strategy for identifying the causal effects of both prison distance and visits on recidivism. I begin by outlining my research design to estimate the causal effect of prison distance on recidivism. I then examine visits as the core mechanism linking distance to recidivism, which motivates an instrumental variables strategy using distance as an instrument for visits to estimate the causal effect of visitation on recidivism.

1.4.1 The Effect of Distance on Recidivism

To estimate the effect of distance on recidivism, I estimate the following equation:

$$R_{is} = \beta_1 Dist_{is} + W'_{is}\beta_2 + \lambda_{t(is)} + \delta_{f(is)} + \theta_{h(is)} + \epsilon_{is} \tag{1.1}$$

where R_{is} is the recidivism outcome for individual i serving spell s. Recidivism is measured as both a binary indicator for re-admit within 1 year of release and within 3 years of release. $Dist_{is}$ is the placement distance from home. Distance is calculated as the Euclidean distance from the geographic centroid of an incarcerated individual i's home ZIP code to the geographic centroid of the ZIP code of their placement facility in spell s.[17] Distance is in 100-mile units. W consists of controls for race, an indicator for whether this is individual i's first spell in prison, the class of worst crime committed for spell s,[18] sentence length for spell s individual i's age at admit for spell s, and an indicator for whether the sentence comes with mandatory time under community supervision after release[19]. $\lambda_{t(is)}$ are admit year fixed effects, $\delta_{f(is)}$ are placement facility fixed effects, and $\theta_{h(is)}$ are home county fixed effects. Standard errors are clustered at the individual level since some individuals serve more than one spell in my data and I want to allow for within-individual correlation.[20]

Let $R_{is}(z)$ be the potential outcome for individual i serving spell s when placed at distance z. Let C_{is} collect the variables necessary for exogeneity: sentence length, prior admit history, and facility and county fixed effects. The remaining elements of W_{is} are

17. Another option would be to use drive-time rather than Euclidean distance. However, drive-time and distance are highly correlated as shown in Appendix Figure A.1.

18. Crimes are classified as violent, sexual, property, drug or other.

19. Sentence length is used rather than time served because time served can be impacted by incarcerated individual behavior while incarcerated which could be impacted by treatment (visitation). If an individual is released prior to their sentence end due to Earned Release Time, they will also be placed into community supervision. This indicator is for whether or not the initial sentence included mandatory time under community supervision if the full sentence is served.

20. Versions with clustering at the ZIP code level and county level to allow for within-geography and within-jurisdiction correlation are shown in Appendix Table A.11, Table A.12.

included to improve precision. The identifying assumption is:

$$\left\{ R_{is}(z) : z \in [0, z_{max}] \right\} \perp Dist_{is} \mid C_{is}.$$

In words this means that conditional on security level, home county, and placement facility, distance is independent of potential recidivism outcomes. Identifying variation comes from the fact that, even within a given security level and county, not all individuals are sent to the same facility, and within a given facility there are individuals from multiple counties.

As discussed above, facility assignment in Washington is determined primarily by security classification, not by individual preference or other individual characteristics. Therefore, conditional on security level, this process induces plausibly exogenous variation in distance from home. Although initial placement is exogenous, transfers may be influenced by incarcerated individual behavior. To avoid this source of endogeneity, I measure distance using the initial placement facility.

While I do not directly observe security level in the data, I demonstrate that combinations of observable prior incarceration history and sentence length create a reliable proxy. Based on these, I classify individuals as "minimum," "medium," or "close" security. Although many facilities house incarcerated individuals across multiple security levels, some facilities are designated for only minimum-security individuals, while others do not accommodate those above medium security, as illustrated in Figure 1.1. If the constructed security classification is valid, individuals labeled as "close" should be effectively excluded from minimum-security-only facilities, and those labeled "minimum" should be excluded from higher-security prisons. To assess this, I estimate a multinomial logit model with placement facility as the outcome and assigned security level as the predictor. I then examine the average predicted probability of assignment to each facility by security level. The results show that individuals assigned to "close" security have less than a 1 percent predicted probability of assignment to any of the three minimum-security-only facilities (Olympic Corrections Center, Larch Corrections

Center, Cedar Creek Corrections Center). Likewise, those classified as "minimum" have predicted probabilities of 5 percent or less of being assigned to the two facilities that do not house minimum-security incarcerated individuals (Washington Corrections Center, Clallam Bay Corrections Center)[21]. While exceptions exist due to individual-specific considerations (e.g., gang affiliation, safety concerns, medical needs), the classification scheme performs well overall. Figure A.2 in the Appendix presents these results.

I now discuss how I separate the effect of distance from other geographic confounders. To do this, I include home county and facility fixed effects in all specifications. Although placement is quasi-random, nearly half of Washington's population resides in the Seattle–Tacoma metropolitan area, so prison distance may reflect proximity to the population center. This matters because distance could capture systematic differences in urban versus rural home-county environments or facility characteristics rather than only distance from family and friends. With additive fixed effects for both home county and placement facility, identification comes from comparing individuals from the same home county placed in different facilities and individuals in the same facility coming from different counties.

After residualizing distance with respect to security level, home county, and placement facility, an incarcerated individual's pre-determined characteristics should not systematically predict how far they are placed from home. To assess this, I regress distance (measured in one-mile units) on the individual's race and on each of three crime-type indicators in four separate regressions, including controls for security level, admit history, home county, and placement facility. I additionally test whether the type of programming an individual participates in while in prison can be predicted by distance. Specifically, I test whether distance is correlated with participation in parenting classes, any educational class (GED-prep, college courses, college readiness, HS level classes, ESL), being in a work crew (fire

21. While Coyote Ridge Corrections Center does not house close security individuals, it does house long-term minimum security level individuals. This can be individuals with long sentences for violent offense but who are not classified as needing restrictive housing.

fighting for the Department of Natural Resources), having a prison job, participating in special reentry planning classes, or being in an undisclosed course. Undisclosed courses include prescribed mental health and substance abuse programming. Results are shown in Table C.3. Crime type, race, age and programming cannot predict how far an incarcerated individual will be placed from home.[22]

As previously mentioned, when explicit placement policies exist, they are related to the placement of families. Although WADOC policy does not formally account for family ties or proximity in placement decisions, such considerations could still influence actual placements. If individuals with children or close families are placed closer to home, the impact of distance may be conflated with the influence of an underlying social support network. I use supplementary risk score data available for individuals incarcerated between 2018 and 2020 to test whether indicators of family connections, including having children, being married or in a long-term relationship, and reporting good relationships with family, are correlated with placement distance. While the main sample covers 2010 through 2016, placement policies did not change from this period through 2020, so this supplementary data reflects the same assignment process as in my main sample. I find no evidence of such correlations between positive family relationships and placement distance. Results are presented in the "Family and Risk" section of Table C.3.[23].

1.4.2 The Effect of Visitation on Recidivism

Visiting an incarcerated individual in prison requires time, travel, and resources, meaning individuals placed farther from home may be less likely to receive visits, (see, e.g., Casey-Acevedo and Bakken [2002]; Duwe and Clark [2017]; Cochran et al. [2016]; Lee [2019b]). Ex ante, it is not obvious whether visitation improves or worsens reentry outcomes. Some visitors

22. Reentry planning is correlated with distance but in the opposite direction of concern.

23. Full balance results for the supplementary sample can be found in Appendix Table A.2.

Table 1.4: Balance on Distance from Home to Prison

Demographics (main sample)		**Family and Risk (supplementary)**	
Non-white	-0.7578 (0.8708)	Long-term partner	-2.003 (6.3508)
Violent crime	-2.4832 (1.4055)	Children	4.550 (6.295)
Property crime	-0.8260 (0.9354)	Good family relationship	4.478 (6.866)
Age at admit	0.0415 (0.0393)	Low-risk	-8.620 (7.038)
		Moderate-risk	-7.910 (9.932)
Programming Participation (main sample)		High-risk	10.037 (6.300)
Parenting program	0.0011 (0.0010)		
Education	0.0002 (0.0009)		
Work crew	0.0003 (0.0014)		
Prison job	0.0057 (0.0039)		
Reentry planning	0.0025 (0.0010)*		
Undisclosed	-0.0080 (0.0041)		
Observations	24,338		590

Notes: ***: 0.01, **: 0.05, *: 0.1. This table presents correlations between distance and observable characteristics, prison program participation, family relationships, and DOC-calculated recidivism risk. Each row is a separate regression of distance (in 1-mile units) on the given pre-determined characteristic or programming indicator, controlling for sentence length and first admit status, with fixed effects for admit year, placement facility, and home county. Standard errors clustered at the individual level are in parentheses. Demographics and programming regressions are estimated using the main sample. Family and Risk regressions are estimated using the supplementary sample of individuals incarcerated between 2018–2020 with intake records. Risk is WADOC's recidivism classification, calculated at intake. High-risk combines individuals classified by WADOC as high risk of drug crime re-offense, high risk of property crime re-offense, and high risk of violent crime re-offense. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

may offer emotional support, stability, or tangible resources that facilitate reintegration. Others, however, may reinforce harmful behaviors, relationships, or stressors. As such, the effect of visitation on recidivism is ultimately an empirical question.

To estimate the causal effect of visitation on recidivism, I employ an instrumental variables framework with distance as an instrument for visitation and estimate the following set of equations:

$$R_{is} = \gamma_1 V_{is} + W'_{is}\gamma_2 + \lambda_{t(is)} + \delta_{f(is)} + \theta_{h(is)} + e_{is} \tag{1.2}$$

$$V_{is} = \alpha_1 Dist_{is} + W'_{is}\alpha_2 + \lambda_{t(is)} + \delta_{f(is)} + \theta_{h(is)} + u_{is} \tag{1.3}$$

where R_{is} is the recidivism outcome for individual i serving prison spell s. As above, recidivism is measured as either a binary indicator for re-admit within 1 year of release and within 3 years of release. V_{is} is the count of visits incarcerated individual i received in spell s normalized by the number of months served in spell s. I use this count specification as the primary measure of visits, and as a complement, I also estimate a binary specification indicating whether an individual received any visits. $Dist_{is}$ is the distance from home to prison. W consists of race, an indicator for whether this is individual i's first spell in prison, the class of worst crime committed for spell s,[24] sentence length for spell s individual i's age at admit for spell s, and an indicator for whether the sentence comes with mandatory time under community supervision after release. $\lambda_{t(is)}$ are admit year fixed effects, $\delta_{f(is)}$ are placement facility fixed effects, and $\theta_{h(is)}$ are home county fixed effects. Standard errors are clustered at the individual level.

Let $R_{is}(v)$ be the potential outcome for individual i serving spell s who receives V_{is} visits per month. Let C_{is} collect the variables necessary for exogeneity: sentence length, prior admit history, and facility and county fixed effects. Interpretation of the estimated

24. Crimes are classified as violent, sexual, property drug or other.

effect $\hat{\gamma_1}$ as the causal effect of visitation on recidivism, requires the following assumptions:

1. Relevance (First stage):

   $\mathbb{E}\big[\, V_{is} \mid Dist_{is} = z_1,\, C_{is}\big] \;<\; \mathbb{E}\big[\, V_{is} \mid Dist_{is} = z_0,\, C_{is})\big] \quad \text{for } z_1 > z_0.$

2. Exogeneity and Exclusion:

   $\big\{\, R_{is}(v) : v \in [0, v_{max}]\big\} \;\perp\; Dist_{is} \;\big|\; C_{is}.$

Relevance requires that distance shifts visitation, and in this setting, distance weakly reduces the number of visits an incarcerated individual receives per month in prison. Exogeneity requires that, conditional on security level, placement facility, and home county, distance is independent of potential recidivism. Exclusion means that distance influences recidivism only through its effect on treatment intensity (the number of visits received).

For distance from prison to an incarcerated individual's home to be a relevant instrument for visits, this distance must influence the incarcerated individual's potential visitors. This implies that an incarcerated individual's potential visitor pool must reside near their home. As reported by the New York Times [Bui and Miller, 2015], the average American only lives about 18 miles from their mother, and those who do live further are often highly educated and wealthy, characteristics that do not often define the population of people who end up incarcerated. Individuals who are incarcerated often have limited work history and make well below the average income even prior to incarceration [Looney and Turner, 2018]. Therefore, home county is likely a valid proxy for the location of an individual's pool of potential visitors. Furthermore, Table 1.6 and Figure 1.3 show that the distance from an incarcerated individual's home to their first facility is highly correlated with whether they receive any visits and with how many visits they receive.

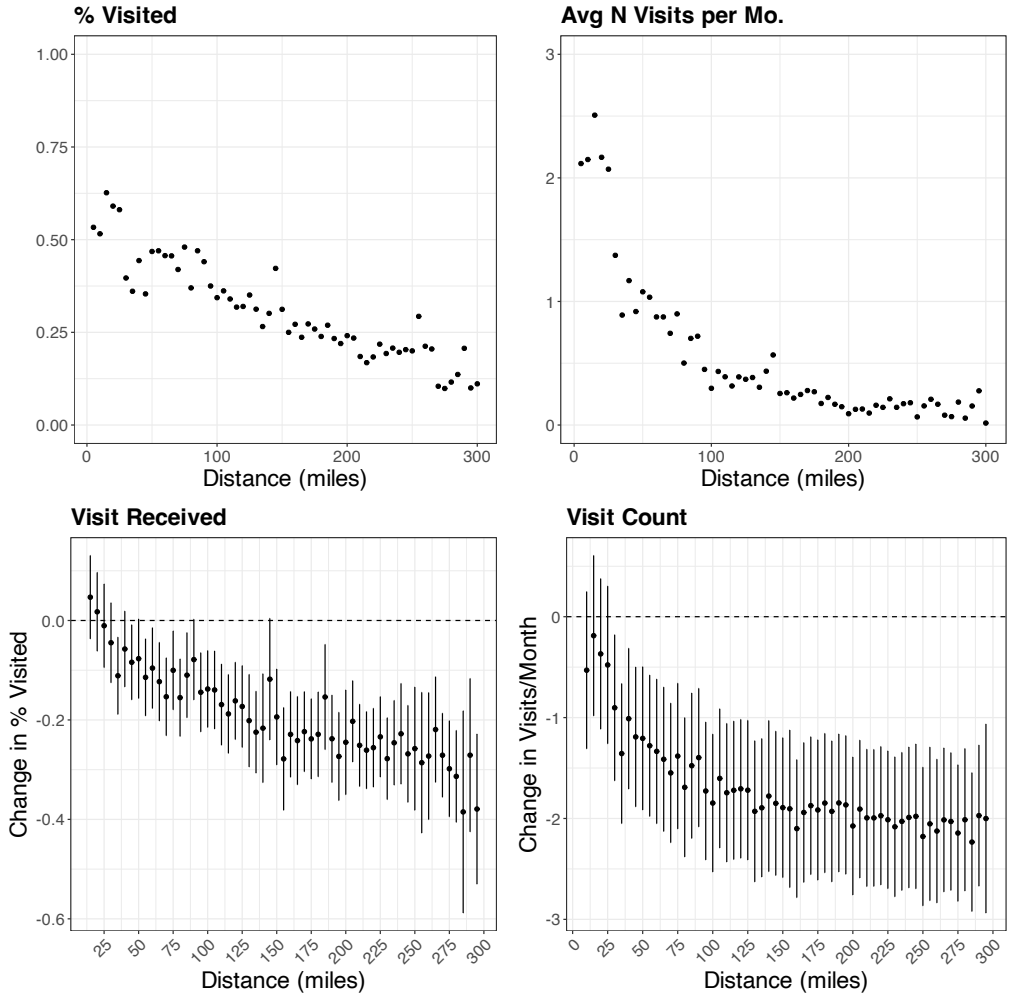


Figure 1.3: First Stage

Notes: This figure illustrates the relationship between distance and visitation rates. In the left panel, visits are normalized by number of months spent in prison. Distance is calculated from each incarcerated individual's home county to their original placement facility. The right panel plots the rate of visitation. Averages are taken over 5-mile buckets. The top two figures plot the raw data, the bottom two figures present the residualized first stage - coefficients from the regression of visitation on dummies for 5-mile distance buckets and covariates. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

Another identification assumption is that, conditional on security level, home county, and placement facility, assignment is as-good-as-random. A natural concern is that social support could confound the effect of visitation if individuals with stronger family ties both receive more visits and are less likely to reoffend. Exogeneity would also fail if distance

were correlated with predetermined characteristics or access to prison programming that independently predict recidivism. As discussed in Section 4.1, there is no evidence of such correlations: balance tests (Table C.3) show that demographics, offense type, prior incarceration, program participation, and proxies for social support are not systematically related to placement distance. This supports the exogeneity of distance in this setting.

Exclusion requires that distance influences recidivism only through its impact on the number of visits received. One potential concern is that distance may proxy for differences between rural and urban environments, since most individuals come from the Seattle metropolitan area and prisons farther from Seattle tend to be more rural. While rural placement could, in principle, matter through local labor markets or community conditions, incarcerated individuals do not leave the facility during their sentence, so the external community should not directly affect them. In addition, individuals return to their sentencing county upon release, so neither prison location nor distance determines the setting of reentry.

While the external environment is unlikely to matter directly, the facility's geographic location could still influence the quality of incarceration, particularly through staffing or prison facility conditions. If prisons draw staff from the surrounding area, geographic variation in local labor markets could shape the quality of staff or available services. To address this, I include fixed effects for both the placement facility and the incarcerated individual's home county in all specifications.[25]

A further requirement for exclusion is that distance affects only the intensity of visitation, not its composition. If individuals who live closer to a facility are visited by a broader set of family and friends, whereas those placed farther away are visited only by their closest relatives, then distance could change the type of social support received rather than just its frequency. This would violate exclusion if different visitors (e.g., parents versus partners)

25. I also estimate a version where I exclude King, Pierce and Snohomish counties, the counties that encompass the Seattle-Tacoma Metropolitan area. Effects are consistent with the main results. See Appendix Table A.16.

have distinct effects on reentry outcomes. Appendix Figure A.6 shows that the composition of visits—by relationship type—declines proportionally with distance, suggesting that distance primarily scales down the number of visits rather than altering who visits.

1.5 Results

1.5.1 The Effect of Distance on Recidivism

I begin by estimating the effect of distance from home on recidivism following Equation (1). Table 1.5 presents the results. Columns (1) and (4) present specifications without any controls or fixed effects, Columns (2) and (5) add fixed effects, and Columns (3) and (6) include the full set of controls and fixed effects. Given the previously demonstrated lack of systematic relationship between distance and pre-determined characteristics, it is both expected and reassuring that the estimates remain largely unchanged with the inclusion of controls. A 100-mile increase in the distance between an individual's home county and their assigned facility is associated with a 0.8 percentage point increase in the likelihood of being readmitted to prison within one year of release, and a 1.1 percentage point increase within three years. These results suggest that being incarcerated farther from home has a statistically significant and negative impact on reentry outcomes. Compared with recidivism rates of 9 percent within one year and 26 percent within three years for individuals placed more than 99 miles from home (the median placement distance), these estimates correspond to increases of roughly 9 percent and 4 percent, respectively.

For context, Palmer et al. [2019] finds that emergency housing assistance reduces re-arrest within one year by 18 percent, while Yang [2017] shows that expanded eligibility for welfare and food stamps lowers one-year reincarceration rates by 10 percent. Mueller-Smith and Schnepel [2021b] reports that diverting first-time offenders from prison reduces reoffending by 50 percent over ten years.

Table 1.5: The Effect of Distance on Recidivism

	Readmit in 1 year			Readmit in 3 years		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance (100 miles)	0.0048*	0.0094***	0.0084**	0.0087**	0.0126**	0.0111**
	(0.0025)	(0.0035)	(0.0034)	(0.0038)	(0.0050)	(0.0048)
Controls	No	No	Yes	No	No	Yes
Fixed-effects	No	Yes	Yes	No	Yes	Yes
Readmit (> 99 miles)	0.09	0.09	0.09	0.26	0.26	0.26
Observations	24,338	24,338	24,338	24,338	24,338	24,338

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard-errors clustered at the individual level are in parentheses. Estimates are from OLS regressions. The outcome variable for Columns (1)-(3) is a binary indicator for whether an incarcerated individual was readmitted to prison within 1 year of release. The outcome variable for Columns (4)-(6) is a binary indicator for whether an incarcerated individual was readmitted to prison within 3 years of release. Columns (1) and (4) present specifications without any controls or fixed effects. Columns (2) and (5) add fixed effects for year of admission, home county and facility. Columns (3) and (6) include controls for the individual's race, the individual's age at the time of prison admission, the length of the sentence in months, indicators the type of crime the individual was convicted for in the given spell, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility. The outcome mean is the average rate of recidivism for individuals placed greater than 99 miles from home, which is the median placement distance. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

1.5.2 The Effect of Distance on Visitation

I next turn to visitation as the mechanism through which distance may affect recidivism. As demonstrated in Section 4, distance is unrelated to pre-existing characteristics or other in-prison programming linked to reentry outcomes, leaving visitation as the most plausible pathway through which distance impacts recidivism.

Table 1.6 reports the first-stage relationship between distance and visitation. Columns (1) and (2) present the effect of distance on the number of visits per month served, which is

the primary specification. Columns (3) and (4) report the effect on an indicator for receiving any visit, which captures the extensive margin. As expected, the relationship between the distance of a prisoner's placement facility and visitation is negative and statistically significant: a 100-mile increase in home-to-prison distance reduces the number of visits received per month by approximately 0.6 visits (Column 2) and lowers the probability of receiving any visit by about 11 percentage points (Column 4). Given that the median individual in the sample is placed 99 miles from home and the average number of visits per month is 0.91, these are substantial declines.[26]

26. Facilities are not uniformly distributed across the state, and so another margin of interest may be the effect of being placed in an individual's second- or third-closest facility relative to their closest, regardless of exact distance. To test this, I restrict to individuals predicted to be medium-security and replace *Dist* in Equation (3) with a set of binary indicators for whether an individual is placed in their first- through seventh-closest facility. These results are presented in Table A.10.

Table 1.6: The Effect of Distance on Visitation

	Visit Count (per Month)		Visit Received	
	(1)	(2)	(3)	(4)
Distance (100 miles)	-0.6653***	-0.5935***	-0.1067***	-0.1069***
	(0.0211)	(0.0255)	(0.0042)	(0.0048)
Controls	No	Yes	No	Yes
Fixed-effects	No	Yes	No	Yes
Observations	24,338	24,338	24,338	24,338
F-stat	981.22	540.41	557.60	492.80

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard-errors clustered at the individual level are in parentheses. This table presents estimates from OLS regressions. The outcome variable for Columns (1) and (2) is the count of visits received in a given spell normalized by the number of in-prison months for a given prisoner-spell. The outcome variable for Columns (3) and (4) is a binary indicator for whether a prisoner received any visits. Columns (2) and (4) include controls for the individual's race, the individual's age at the time of prison admission, the length of the sentence in months, indicators the type of crime the individual was convicted for in the given spell, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

1.5.3 The Effect of Visitation on Recidivism

I now turn to IV estimates of the effect of visits on recidivism using distance as an instrument following Equations (2) and (3). The two-stage least squares estimates are presented in Table 1.7. In Table 1.7, the top panel presents results for the primary specification where treatment is the count of visits received per month of incarceration. The bottom panel presents results when treatment is a binary indicator for any visits received. Columns (1) and (2) give the causal effect of visitation on the probability of being readmitted within 1 year and Columns

(3) and (4) show the same for 3 year readmittance probability. All estimates include all controls and fixed effects discussed in Section (4). One additional visit per month reduces the probability of being readmitted to prison by 1.4 percentage points within 1 year and 1.9 percentage points within 3 years. Given a re-incarceration rate of 10 percent for non-visited incarcerated individuals within 1 year of release and 28 percent within 3 years of release for never-visited incarcerated individuals, this represents a 14 percent decline in one-year re-incarceration and an almost 7 percent decline in three-year re-incarceration. The difference between the IV and OLS estimates will be discussed further below.

These point estimates are quantitatively similar to Lee [2019b], although with twice the sample size my estimates are more precisely estimated. The point estimates in Lee [2019b] for the effect of receiving any visits on 3-year reincareration is an 8 percentage point reduction and for receiving one additional visit per month is a 1 percentage point reduction. Relative to other studies of visitation, my estimates fall within the mid-to-upper range of previous findings summarized in the meta-analysis by Mitchell et al. [2016], who reports effects ranging from a 3 percent to 62 percent reduction in recidivism.

Results showing heterogeneity by age, current crime type, and race can be found in Appendix Table A.24, heterogeneity by subsequent crime type can be found in Appendix Table A.25 and analysis of heterogeneity by visitor relationship can be found the Appendix Section A.3.4.

Table 1.7: The Effect of Visitation on Recidivism

	Readmit in 1 year		Readmit in 3 years	
	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)
Visit Count (per Mo.)	-0.0052***	-0.0141**	-0.0112***	-0.0187**
	(0.0005)	(0.0057)	(0.0009)	(0.0081)
Visit Received	-0.0296***	-0.0781**	-0.0529***	-0.1036**
	(0.0039)	(0.0318)	(0.0059)	(0.0451)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	24,338	24,338	24,338	24,338
Untreated Mean	0.10	0.10	0.28	0.28

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents both OLS and 2SLS estimates of the effect of visitation on prison readmission. Visit count (per mo.) is the count of visits received in a given spell normalized by the number of in-prison months for a given prisoner-spell. The outcome variable for Columns (1) and (2) is a binary indicator for whether a prisoner is re-admitted within 1 year and the outcome variable for Columns (3) and (4) is a binary indicator for whether a prisoner is re-admitted within 3 years. Controls include race, age at the time of admission, length of sentence in months, and indicators for crime type, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

Other Effects of Visitation: Time Served

Visitation may shape not only post-release outcomes but also experiences during incarceration. While I cannot directly observe disciplinary records, I am able to measure time served against original sentence length. Almost all incarcerated individuals are eligible to receive

early release, with time off earned through good behavior and program participation. Indeed, 94 percent of the individuals in the sample leave prison prior to the completion of their sentence. Disciplinary action as well as refusal to participate in recommended work or education programming can result in the revocation of days of earned time (Washington Administrative Code 137-28-350). Visitation is also a privilege that can be revoked in response to infractions (Washington Administrative Code 137-28-350). It is important to note that disciplinary action is often at the discretion of the corrections officer, and may not be an accurate reflection of true misconduct. Washington currently has a "some evidence" standard, which means an officer's statement is sufficient to support disciplinary action [Office of the Corrections Ombuds, 2023].

Prior correlational work in criminology shows that visitation is associated with less misconduct in prisons [Cochran, 2012]. Lee [2019b] also finds that visitation reduces misconduct as well as days served. Several theories from psychology and criminology exist as to why visitation may reduce misconduct. The first is that contact with family and friends can serve as a coping mechanism, which reduces stress and reduces misbehavior [Wooldredge, 1999]. Alternatively, maintaining contact with family may encourage individuals to avoid misconduct (which can lead a reduction in early release time) in order to complete their sentence as quickly as possible and be reunited with loved ones. Finally, because visitation is a privilege that be revoked for misconduct, it can serve as an incentive for good behavior.

At the same time, visitation is not uniformly positive: stressful or disappointing visits might worsen mood or prompt disruptive behavior, and in some cases visits can enable the introduction of contraband, leading to disciplinary sanctions. The net effect of visitation on behavior during incarceration is therefore ambiguous.

To estimate the effect of visitation on time served, I use Equations (2) and (3) and replace the outcome variable with fraction of sentence served. Table 1.8 reports the estimates. An additional visit reduces the the fraction of sentenced served by 0.07 percentage points. Non-

visited individuals serve, on average, 61 percent of their sentence, so a 0.07 percentage point reduction represents a roughly 1 percent decrease. Given that the average non-visited individual is sentenced to 654 days, this translates into about 7 fewer days in prison. Using the binary measure instead, a four percentage point reduction translates into about 43 fewer days in prison. Taking fraction of sentence served as a proxy for disciplinary action, these results indicate visitation reduces disciplinary action while incarcerated.

As an additional check, I examine whether visitation affects the likelihood of being released from a lower-security facility than the placement facility. Incarcerated individuals undergo security reviews every six months, with the expectation that those with no major infractions progressively move to lower-security facilities. Disciplinary issues can delay or prevent this transition. I find that receiving visits increases the probability of release from a lower-security facility by 11 percentage points, a 55 percent increase. This is consistent with visits reducing in-prison misconduct. Results are reported in Appendix Table A.19.

Table 1.8: The Effect of Visitation on Time Served

	Fraction of Sentence Served	
	(1)	(2)
Visit Count (per Month)	-0.0070**	
	(0.0031)	
Visit Received		-0.0372**
		(0.0167)
Controls	Yes	Yes
Fixed-effects	Yes	Yes
Observations	22,440	22,440
Non-visited fraction served	0.61	0.61

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard-errors clustered at the individual level are in parentheses. Fraction of sentence served is equal to the days served by individual i in spell s divided by the number of days of individual i's original sentence for spell s. All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted on or after January 1, 2010 to Washington state prisons who were placed in a standard facility. One percent of the sample has an indeterminate sentence length and are dropped from estimation.

Time served matters not only because it can serve as a proxy for behavior during incarceration, but also because the amount of time spent in prison can shape post-release outcomes. Mueller-Smith [2015] reports criminogenic effects, with longer incarceration actually causing higher rates of post-release criminal charges. In other work, Rose and Shem-Tov [2021] find that incarceration reduces recidivism, but that the effects are diminishing – incarceration versus no incarceration decreases crime but longer sentences do not decrease crime more than shorter sentences. Taken together, these findings imply that, for people already in custody,

prolonging imprisonment through earned-time revocations may do little to decrease post-release criminal activity and decreased time may actually reduce future crime. Visitation may therefore decrease recidivism by decreasing time spent in prison.

I next consider the cost savings associated with reduced incarceration. As shown, closer placements increase visitation, which in turn lowers both fraction of sentence served as well as re-incarceration. To estimate the reduction in incarceration due to visitation, I estimate a 2SLS model analogous to Equation 3, replacing the outcome with the total number of days incarcerated in the five years following date of sentence. For each individual, starting on the date of sentence, I count days spent incarcerated over the subsequent five-year window, including the current incarceration spell. This approach incorporates both the reduced time served in the given spell as well as the effect of reduced re-incarceration. Estimates imply that receiving an extra visit per month of incarceration reduces time served in the five years following sentencing by 25 days. The average non-visited individual spends 509 days of the five years post-sentence in prison, making this a 5 percent reduction in incarceration time.

According to budget documentation from the Washington DOC, in 2022 the cost per incarcerated individual per day was \$174.32.[27] The 25 day reduction of incarceration then results in savings of \$4,358 per person. These savings represent the expected reduction in incarceration costs due to visitation effects under the current placement system.

If instead I use the binary measure of receiving any visits versus no visits, receiving visits reduces time served in the five years following sentencing by 141 days (28 percent reduction in incarceration time) and savings of \$24,584 per person.[28]

27. This is calculated by dividing the total 2022 Fiscal Year expenditures by the average daily population, and then by 365 to get the daily average (see Washington Department of Corrections).

28. Assuming costs stay fixed, converting the per day cost of \$174.32 in 2022 to 2010 dollars using the CPI gives a per-day cost of approximately \$129.89. This implies savings of \$18,307. Using 2016 dollars, the cost is \$142.96 per day and gives savings of \$20,167.

Interpretation of Results

I interpret the results using the Imbens and Angrist (1994) LATE framework. With the added assumption that the instrument moves treatment in the same direction for everyone (monotonicity), Imbens and Angrist (1994) show that the population can be partitioned into four groups according to their response to the instrument. In this context, monotonicity means that increasing placement distance must weakly decrease the probability of receiving a visit for every individual. Always-takers receive visits regardless of how close or far they are placed from home. Never-takers never receive visits, regardless of placement distance. Compliers are visited when placed close to home, and are not visited when placed far from home. Defiers, who would be visited only when placed far from home, are assumed not to exist. In this setting, it is reasonable to assume that a shorter distance would weakly motivate people to visit more.

Under this framework, compliers are the only group whose visitation is affected by placement distance. As a result, two-stage least squares identifies the Local Average Treatment Effect (LATE), which is the causal effect of visitation on recidivism for the complier group. These individuals are the policy-relevant margin, since they are precisely the individuals for whom outcomes can be influenced by changes in placement distance.

Under these same assumptions, Imbens and Angrist (1995) show that for a treatment with variable intensity, such as the number of visits, the 2SLS estimator recovers the average causal response: a weighted average of treatment effects at each level of treatment. The weights are proportional to how strongly distance affects visitation, so the estimator places greater weight on individuals whose visitation is most responsive to distance and on ranges of visitation where distance induces the largest shifts in the distribution of visits.[29]

Under the usual positive-selection story in which incarcerated individuals with families

29. Imbens and Angrist (1995) derive this result for a binary instrument. As a direct analogue, I also estimate a binary instrument equal to one for placements beyond the median distance of 99 miles; results are similar to the continuous specification (Appendix Table A.17).

with strong ties both visit more and would reoffend less even absent visits, OLS overstates the benefit of visitation, so an IV correction should yield a smaller effect than OLS. Here however, although 2SLS is imprecise, the point estimates are more negative than OLS.

First, I test for evidence of selection using the Black et al. [2022] test, which tests for selection by checking whether the instrument predicts outcomes within the visited population and within the non-visited population. Under valid exclusion, significant coefficients on the instrument indicate selection in levels that would bias OLS relative to IV.[30] I fail to reject the null hypothesis that selection is not present. Given the large IV standard errors, I see this as inconclusive. Full test details and estimates appear in Appendix Section A.6.

Measurement error is another possible, though unlikely in this context, explanation for the 2SLS estimate being larger in magnitude than the OLS estimate. If unobserved family support both raises visitation and lowers recidivism, OLS estimates would be too negative because they confound the effect of visitation with the effect of support. Classical measurement error in the visit indicator would instead attenuate OLS estimates. These forces can offset, yielding OLS and 2SLS estimates that look similar even when selection exists. In this setting, however, substantial misclassification seems unlikely. To misclassify someone as "not visited" under a binary treatment definition, either every visit would have to be missing from the data or every visit would have to be recorded as happening for someone else. This seems most plausible for individuals who received only a single visit, and so I recode anyone with only one recorded visit as "not visited" and re-estimate the model. The resulting estimates change very little, as shown in Appendix Table A.14, suggesting that measurement error is unlikely to be driving the results.

Finally, OLS and 2SLS differ in which populations they weight. 2SLS identifies the effect for compliers and gives more weight to the observations where the instrument moves treatment more, while OLS estimates the difference in treated and untreated mean outcomes and

30. With heterogeneous effects, OLS and 2SLS target different parameters and so the traditional Wu-Hausman test for endogeneity is not informative.

with controls gives more weight to observations in cells with greater treatment variance.[31] It can be useful to look at the characteristics of the populations each estimate weights. In a similar spirit to the method in Abadie [2003] used to characterize always-takers, compliers and never-takers in the case of binary treatment and a binary instrument, Hull [2025] defines the effective population for a given estimand. For the pre-determined characteristics in the main sample, I compare the OLS and 2SLS effective populations and they appear quite similar across most characteristics, with the main difference being the 2SLS effective population is somewhat whiter (see Appendix Table A.32).

However, other important dimensions on which selection may operate, such as pre-existing family relationships and income, are only observed in the supplementary data and so I cannot apply either complier characterization method. Instead, I look at a subset of the sample who participate in Extended Family Visits (EFV), Washington's multi-day private visitation program. There are both stringent program requirements as well as significant travel and time costs associated with these multi-day visits, suggesting that EFV participants are highly motivated and extremely committed to their incarcerated loved one. Consistent with high motivation and support, distance is a very weak predictor of EFV participation (see Appendix Table A.29), so EFV recipients are conceptually similar to always-takers (those individuals who would receive visits regardless of distance). I therefore use EFV recipients as a proxy for always-takers to contrast with compliers within the visited population.

Appendix Table A.31 shows that as expected, both regular visitation recipients and EFV recipients are positively selected. Both groups are more likely to report having a long-term partner, having a good family relationship and to report having children than the non-visited individuals. However, while regular visitation recipients are more positively selected than never visited people, they are much less so than the EFV recipients. Compared to regular visitation recipients, EFV recipients are much more likely to be rated low risk of recidivism

31. A derivation of what OLS estimates in terms of the LATE-groups is in the Appendix.

and are also disproportionately in the highest income bracket. This provides suggestive evidence on why the 2SLS estimate may be larger in magnitude than the OLS estimate, because 2SLS is placing weight on this regular visitation population (the compliers), who are less advantaged and, plausibly, have larger returns to visits.

Interpretation of 2SLS with Covariates

The preceding estimates rely on parametric functional–form assumption, namely that the linear projection of the instrument on covariates approximates the instrument's conditional expectation. As Blandhol et al. [2022] show, causal interpretation of 2SLS estimates with covariates requires that the linear projection of the instrument on the covariates equals its conditional expectation: $L(Z|X) = E[Z|X]$.[32] A sufficient condition for $L(Z|X) = E[Z|X]$ is when the covariate specification is fully saturated in X, meaning it includes indicators for all possible combinations of covariate values. I estimate alternative specifications that include saturated fixed effects.

In my context, Z (distance) is as good as randomly assigned conditional on security level, making this the key variable for exogeneity to hold. An individual's home county also impacts the possible distances an individual could be placed, as well as recidivism risk through differing economic opportunities. An individual's placement facility could additionally impact recidivism risk in ways separate from its distance to the incarcerated individual's home if we think that incarceration in a certain facility is a unique experience. Fully saturating the model with respect to both home county and placement facility would require conditioning on all 1,170 possible combinations, which is unwise given the sample size, and would soak up the vast majority of variation in distance. To address this, I construct a proxy

32. Alternatively, one can assume the potential–outcome means are linear in covariates: $E[Y(t)|X] = \alpha_t + X'\beta_t, \quad t \in \{0, 1\}$. This implies for any treatment level t, the average potential outcome varies linearly with covariates. This rules out nonlinear relationships between covariates and outcomes—such as threshold effects, interactions, or diminishing returns. However, as Abadie [2003] cautions, this assumption has a problematic implication: even if the instrument Z is a deterministic or nonlinear function of X (clearly not exogenous), 2SLS may still yield an unbiased estimate of the ATE.

for security level using sentence length, crime type, and prior prison admissions and then estimate models with alternative specifications of interacted fixed effects.[33] Specifically, I present results from specifications that include: (i) fixed effects for the proxy security level alone, (ii) interactions between security level and home county, and (iii) interactions between security level, home region, and placement facility. Home regions aggregate counties into three broad groups: the Seattle–Olympia metro area, Southwestern Washington, and Eastern–Central Washington. I use home region rather than county in the final specification because including county-by-security-level fixed effects would absorb nearly all of the identifying variation. Using regions instead provides a coarser but still meaningful way to condition on home location while allowing sufficient variation in distance for estimation.

Table 1.9 presents results for both the count and binary measure of visitation with alternative covariate specifications. Columns (1) presents the main specification for 1-year and 3-year readmission, respectively, which include a full set of additive controls and fixed effects. Column (2) includes fixed effects for security level only, Columns (3) includes fixed effects for each security level-home county pair and Column (4) report estimates from a specification that includes indicators for every security level–facility–region combination. These saturated estimates are generally smaller in magnitude than the additive control models but are broadly consistent with the main results. Overall, I find consistent evidence that visitation causally reduces the likelihood of readmission.

33. Individuals who committed violent crimes and have a history of prior prison admissions are designated as “close" security. Individuals who did not commit any violent or sex offense and have sentence lengths equal to 13 months or less are designated “minimum" security. Individuals over 50 are designated as minimum. All others are designated as “medium".

Table 1.9: Visitation Effects, Alternate Models

	Main	Security	Security-County	Region-Security-Facility
	(1)	(2)	(3)	(4)
Readmit in 1 year				
Visit Count (per Mo.)	-0.0141**	-0.0080**	-0.0091**	-0.0165***
	(0.0057)	(0.0037)	(0.0039)	(0.0064)
Visit Received	-0.0781**	-0.0482**	-0.0527**	-0.1158**
	(0.0318)	(0.0225)	(0.0227)	(0.0450)
F-stat, count (1st stage)	981.2	1,003.5	986.2	288.9
F-stat, binary (1st stage)	540.4	715.9	720.5	171.9
Readmit in 3 years				
Visit Count (per Mo.)	-0.0187**	-0.0141**	-0.0185***	-0.0197**
	(0.0081)	(0.0056)	(0.0059)	(0.0098)
Visit Received	-0.1036**	-0.0854**	-0.1072***	-0.1381**
	(0.0451)	(0.0340)	(0.0341)	(0.0689)

Continued on next page

Table 1.9 continued from previous page

	Main	Security	Security-County	Region-Security-Facility
	(1)	(2)	(3)	(4)
F-stat, count (1st stage)	981.2	1,003.5	986.2	288.9
F-stat, binary (1st stage)	540.4	715.9	720.5	171.9
Saturated fixed effects	Yes	Yes	Yes	Yes
Observations	24,338	24,338	23,847	24,102

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents 2SLS results using alternate covariate specifications. The outcome variable for the top panel is a binary indicator for whether an incarcerated individual is readmitted within 1 year of release. The outcome variable for the bottom panel is a binary indicator for whether an incarcerated individual is readmitted within 3 years of release. *Visit Count (per Mo.)* and *Visit Received* are separate regressions. Column (1) presents the main results that include controls for the individual's race, the individual's age at the time of prison admission, the length of the sentence in months, indicators for the type of crime the individual was convicted for in the given spell, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility. Column (2) contains fixed effects for security level only. Column (3) is saturated by security level-home county level. Column (4) is fully saturated in that they contain indicators for every security level-facility-home region combination. The specifications in Columns (3) and (4) include only cells with 30 or more individuals. Individuals are grouped into security levels using sentence length and history of prison admissions. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

1.6 Mechanism: Housing Stability

Prior research highlights both the difficulty formerly incarcerated individuals face in securing stable housing and the critical role that stable housing plays in successful reentry. For instance, Gottlieb and Jacobs [2020] provides suggestive evidence that housing instability is prevalent among individuals released on probation, 25 percent lacked stable housing, and that this instability is associated with a 35 percent increase in the risk of recidivism. Similarly, Palmer et al. [2019] shows that emergency financial assistance for housing significantly reduced the likelihood of rearrest by 18 percent. Furthermore, qualitative research underscores the importance of social networks in securing housing after release. La Vigne et al.

[2004] documents that 42 percent of individuals released from prison moved into a family member's home immediately upon release. Likewise, Deess et al. [1999] finds that approximately 80 percent of parolees resided with family members. If visitation allows individuals to maintain relationships with family, and individuals who receive visits are more likely to secure stable housing upon release, housing is a plausible channel through which visitation operates. I therefore examine housing as a candidate mechanism linking visitation to reentry outcomes.

Most individuals released from a Washington State prison are required to submit a post-release address and sponsor for approval by the Department of Corrections.[34] A sponsor is any person willing to allow an incarcerated individual to live with them post-release, subject to several restrictions such as not having a criminal history and not being a victim of the individual's crimes.[35] While in theory an incarcerated individual could live alone when released, in practice this is exceedingly rare due to high housing costs and the financial circumstances of most released individuals. Release plans are initially developed during intake, but a staff member from the Reentry Division begins working with incarcerated individuals one-year from release to finalize a plan, and release address verification begins at 6 months from release. If an individual cannot provide an acceptable living plan, WADOC will delay release until an address is secured or until the sentence is fully served, as opposed to releasing someone a their Earned Release Time.[36] After recognizing that many individuals were remaining in prison solely due to the lack of a housing plan, WADOC launched the Reentry Housing Assistance Program in 2009.[37] Support is offered through both referrals

34. Individuals whose sentence does not require community supervision after release or are serving a misdemeanor sentence are exempt from this requirement.

35. Residences are also reviewed for weapons and drugs. Some incarcerated individuals may not live with children or animals, and this is also reviewed. All members of the household are reviewed for criminal history, not just the sponsor.

36. Incarcerated individuals are assigned an early release date at the start of their sentence and can gain time back on their sentence for misconduct, which includes non-participation in required programming and disciplinary action. Most non-violent individuals are eligible to earn up to 50 percent of their sentence off.

37. The DOC noted that it was spending thousands of dollars per month to hold individuals past their

to transitional housing programs and direct rental subsidies. The latter takes the form of a voucher that covers up to $700 in monthly rent for a maximum of six months following release.

While the goal is to understand how visitation effects post-release housing stability, the data do not directly measure this. Instead, I can proxy for instability with whether an individual uses a housing voucher at release. Requiring assistance due to the absence of a sponsor or the inability to secure housing independently strongly signals a lack of stable housing. Those who end up using a house voucher are much less likely to have been visited than those who did not – 11 percent of those who were ever visited use a housing voucher compared to 25 percent of those who were not visited.[38] Additionally, those who receive a housing voucher have higher rates of re-admittance to prison than those who do not – 29 percent of individuals who received a voucher are re-admitted within 3 years of release versus 24 percent of individuals who did not require a voucher.

To better understand how housing vouchers may impact re-admittance to prison, it is also important to examine where individuals who receive this assistance tend to live after release. Eighty-six percent of individuals who used housing assistance were released to transitional housing, compared to only 15 percent of those who did not require housing assistance.[39] These settings are commonly known as “halfway houses" and they provide short-term, non-DOC-operated housing for people leaving prison. Placements may be motivated by needs, such as sober-living support, or by religious values. However, in practice, release to transitional housing is closely tied to use of a housing voucher. Transitional housing environments vary significantly in quality and services offered, complicating causal inference of their impact

early release date due to unmet housing requirements.

38. Appendix Table A.3 presents descriptive statistics for the entire sample by housing voucher status. Appendix Table A.4 presents descriptive statistics on individuals who used housing vouchers by visitation status.

39. I define transitional housing as either a housing address where more than 5 incarcerated individuals have been released or with a sponsor name or address name such as “Sober Living Community" or “Group home".

on post-release outcomes. However, a common feature is communal living with structured supervision. Lee [2023] shows that mandated residential housing for high-risk individuals increases the risk of re-incarceration, largely due to heightened exposure to technical violations under intensive monitoring. This suggests that transitional housing may also be a potential mechanism of prison readmission.

First, I document the relationship between housing assistance and recidivism. I estimate OLS models with a binary indicator for prison readmittance as the dependent variable and housing assistance as the independent variable. I include the full set of controls and fixed effects from the main specifications. Table 1.10 presents the results. Columns (1), (3) and (5) include a binary indicator for whether an individual required a housing voucher upon release from prison. Columns (2), (4) and (6) add a binary indicator for use of transitional housing post-release. Across all specifications, the need for housing assistance is positive correlated with prison admission. Estimates at six months are small, suggesting that housing vouchers may provide short-term stability – they are valid for only six months, but even individuals who need assistance are able to maintain housing during this period. However, the effects grow at one and three years, a pattern consistent with persistent underlying housing instability among voucher recipients. If the association were driven mainly by voucher recipients living in transitional housing, where intensive supervision could potentially increase detection of illegal activity, then controlling for transitional housing should substantially attenuate the voucher coefficient. While the coefficient does not fully attenuate, it does decline somewhat, suggesting that both transitional housing and broader housing instability contribute to the relationship.

Table 1.10: Housing and Recidivism

	OLS					
	Readmit in 6 mo.		Readmit in 1 year		Readmit in 3 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Voucher	0.0065*	0.0039	0.0210***	0.0155**	0.0481***	0.0334***
	(0.0034)	(0.0040)	(0.0060)	(0.0071)	(0.0085)	(0.0100)
Transitional Housing		0.0040		0.0087		0.0232***
		(0.0034)		(0.0059)		(0.0086)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20,038	20,038	20,038	20,038	20,038	20,038
Non-voucher Readmit Rate	0.02	0.02	0.08	0.08	0.24	0.24

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents OLS estimates of the correlation between voucher use and transitional housing use with prison readmission. Transitional housing is a binary indicator for whether an individual was released to an address indicating transitional housing. Assistance is a binary indicator for whether an individual required housing assistance to obtain an approved living situation post-release. The outcome variable for Columns (1)-(2) is a binary indicator for whether an incarcerated individual was readmitted within 6 months of release. The outcome variable for Columns (3)-(4) is a binary indicator for whether an incarcerated individual was readmitted within 1 year of release. The outcome variable for Columns (5)-(6) is a binary indicator for whether an incarcerated individual was readmitted within 3 years of release. All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code. Six percent of spells are missing information on post-release housing and are excluded. Non-voucher readmit rate is the rate of readmittance for the given time frame for individuals who did not use a housing voucher.

I now estimate the causal effect of visitation on housing stability by using distance as an instrument for visitation. As shown, distance is uncorrelated with family relationships, pre-determined characteristics, and other programming. Therefore, it is plausible distance only impacts housing stability through visitation. Results are shown in the blue bar of Figure

1.4 and point estimates are given in Appendix Table A.20. Receiving an additional visit per month causally reduces housing voucher use by 3 percentage points (a 12 percent reduction).

To further investigate whether visits reduce prison readmissions through improved housing stability, I estimate IV models using distance as an instrument for the number of visits received per month and joint outcomes of recidivism and housing status as dependent variables. I define four mutually exclusive and exhaustive categories: readmitted and used a housing voucher, readmitted and did not use a housing voucher, not readmitted and used a housing voucher, and not readmitted and did not use a housing voucher. Each category represents a distinct combination of post-release housing stability and recidivism outcomes and so the estimated effects can be interpreted as reallocations across the categories. Results are presented in Figure 1.4 and point estimates are reported in Appendix Table A.21.

Results show movement from the least favorable state, *Readmit, Voucher*, into the most favorable state, *No Readmit, No Voucher*, which is consistent with housing stability being a mediating factor for recidivism. The magnitudes suggest partial mediation rather than full mediation indicating that housing stability is an important pathway but that visits also influence recidivism through other channels such as emotional or psychological support.[40]

40. Importantly, the measured housing channel likely encompasses both material and relational support, as individuals living with friends or family also likely receive other non-measured support.

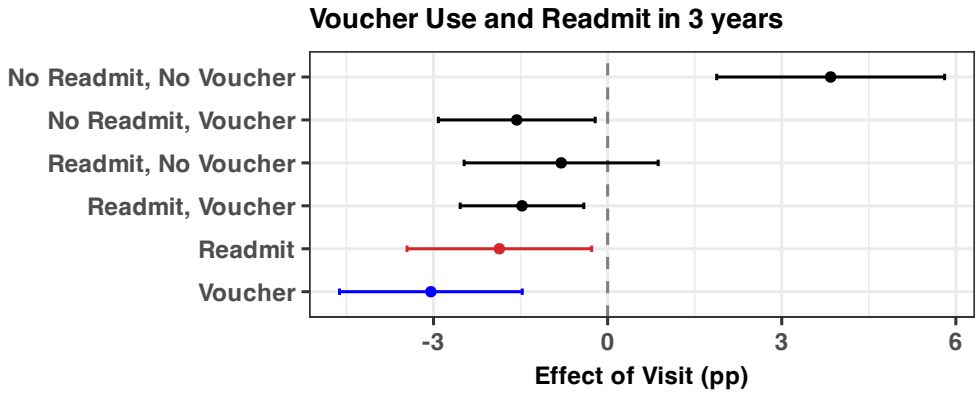


Figure 1.4: Joint Housing and Recidivism Outcomes

Notes: This figure plots the IV estimates of the effect of an additional visit on joint outcomes for housing instability and recidivism. Point estimates are indicated by dots and the lines indicate 95% confidence intervals. Each row (point estimate and line) represents a separate regression. Outcomes consists of four mutually exclusive and exhaustive categories of binary indicators for prison readmission within three years and receipt of a housing voucher. The red point and line is the marginal effect of a visit on the probability of being readmitted in three years.

As address verification for release plans begins 6 months prior to release, I investigate if visitation at the end of a spell is especially meaningful for re-offense in general and for housing stability specifically. For individuals who spent at least 14 months incarcerated, I look at the relationship between voucher receipt and visitation in the last year, last 6 months, and last 3 months.[41] To do this I regress a binary indicator for voucher use on mutually exclusive categories of visitation timing: an individual's last visit was more than 1 year prior to their release date, their last visit was between 6 and 12 months prior to release, their last visit was between 3 and 6 months prior to release, or their last visit was less than 3 months prior to release. The excluded group is individuals who received no visits. Individuals visited in the final three months of their spell are half as likely to require a housing voucher as those whose visits occurred only early in the spell. The increasing magnitude in Column (3) is

41. I restrict the sample to 14 months to allow for everyone to receive a visit at least 1 year prior to release.

consistent with housing plans being finalized close to release, and with making visitation salient for securing stable housing.

Table 1.11: Visit Timing and Housing Stability

	OLS: Housing Voucher Used		
	Any Visit	Any Visit	Visit Timing
	(1)	(2)	(3)
Visit Received	-0.1296***	-0.1545***	
	(0.0055)	(0.0090)	
Last Visit > 1 year before release			-0.0912***
			(0.0149)
Last Visit 6–12 mo. before release			-0.1533***
			(0.0114)
Last Visit 3–6 mo. before release			-0.1665***
			(0.0114)
Last Visit < 3 mo. before release			-0.1837***
			(0.0107)
Controls	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes

Continued on next page

Table 1.11 continued from previous page

	OLS: Housing Voucher Used		
	Any Visit	Any Visit	Visit Timing
	(1)	(2)	(3)
Observations	20,038	8,506	8,506
Non-visited Voucher Use Rate	0.25	0.29	0.29

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents OLS estimates of the relationship between visitation and housing voucher use. The outcome variable is a binary indicator for whether an incarcerated individual required a housing voucher in the given spell. All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. Six percent of spells are missing information on post-release housing and are excluded from Model (1). The sample for Models (2) and (3) includes individuals who spent at least 14 months in prison. The excluded category is no visits. Eight percent of spells lasting 14 months or more are missing information on post-release housing and are excluded. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

One alternative could be that visitation does not affect housing at all, and that having a place to live simply proxies for pre-existing social support. If that were true, outcomes should be similar for visited and non-visited individuals within each housing-assistance group. However, they are not. Among those with assistance, 8 percent of visited are readmitted versus 13 percent of non-visited. I also verify this pattern in my supplementary data where I have self-reported family relationship status at intake.[42] Visitation has a stronger correlation with not needing housing assistance than self-reported “good family relationship" at intake. Among those who report not having a good relationship 35.5 percent of non-visited used a housing voucher compared to only 13.4 percent of visited individuals (a 22.1 percentage point difference). For comparison, among those who do report a good family relationship, 19.9 percent of non-visited individuals used a housing voucher relative to only 12.1 percent of visited individuals (a 7.8 percentage point difference). These patterns are consistent with

42. See Appendix Table A.5 and Appendix Table A.6 for descriptive statistics on voucher use within the supplementary sample.

visits reinforcing support relevant for post-release housing, beyond just reflecting pre-existing support.

1.7 Counterfactual Placement Policies

Facility placement is an administrative decision, and as such is a potential policy mechanism. To gauge what a proximity-based rule would imply for Washington, I evaluate counterfactual placement policies that minimize home-to-facility distance. Using my estimated effects of distance on visitation and of visitation on recidivism, I simulate outcomes under three policies: (i) assign each individual to the nearest facility with no constraints; (ii) assign to the nearest eligible facility subject to security-level eligibility and facility capacity, with random tie-breaking; (iii) a targeted program that first assigns those with the largest expected benefits to the nearest eligible beds before filling remaining capacity.

For the first simulation, I set all prisons to 0 occupancy and I assign every individual admitted on or after January 1, 2010 to their nearest facility, ignoring any capacity constraints or security levels. I then compute the change in distance relative to the status quo by taking the difference between the average placement distance in the data and the average placement distance under this simulated regime. I translate this distance change into a change in the probability of being visited using the first-stage estimate of the effect of change in distance on visitation probability from Equation 2.[43] I then map the resulting change in average visitation probability into a change in recidivism using the 2SLS estimates of the binary measure of visits' effect on one and three-year prison readmission, from Equation 3.[44] This two-step procedure yields the counterfactual change in recidivism for each person, which I then aggregate to report the average change in recidivism from this counterfactual policy. Aggregating across individuals, the policy moves people 82 miles closer to home on average

43. This approach uses a local linear approximation for the effect of distance on visitation.

44. This approach assumes constant effects across individuals. This does not allow counterfactual placements to change visitation and recidivism probabilities differently.

and increases the probability of being visited by 8.7 percentage points. This results in a 0.7 percentage point reduction of predicted readmission at one year and 1.0 percentage point reduction at three years. Although unrealistic, given real capacity constraints and security level considerations, this can be thought of as the greatest possible change in recidivism from altering placements.

For the second simulation, I impose security-level eligibility and facility capacity on the counterfactual placements. I set each facility's capacity to the stated WADOC capacity and initialize occupancy to the observed headcount on January 1, 2010, and am therefore leaving anyone admitted prior to January 1, 2010 in their regularly assigned facility. The simulation then proceeds month by month from January 2010 through the end of the sample. In each month, I take the set of new admissions for the month and, for each person, rank security-level appropriate facilities by home-to-facility distance. For example, let Individual A, Individual B, and Individual C all be admitted in January 2010. Individual A is minimum security, Individual B is medium security, and Individual C is close security. I rank all minimum security facilities by distance from Individual A's home county, all medium security facilities by distance from Individual B's home county and all close security facilities by distance from Individual C's home county. If Individual A's closest security-appropriate facility has open occupancy, they are assigned there. If not, I move to the second closest and so on until there is occupancy. When there are multiple individuals of the same security level admitted in the same month with the same facility ranking, I break ties randomly. After assigning everyone admitted that month, I release all individuals with release dates that month. Occupancy at each facility is updated each month as admissions arrive and as observed releases create vacancies. I then proceed to the next month, all the way through December 2016. This produces capacity-feasible counterfactual placements. After everyone is placed I compute the counterfactual change in distances and map this into visitation using the first stage effect of distance change on visitation and then to recidivism changes using the second

stage estimated effects of visitation on recidivism. Under this assignment regime, people are moved 40 miles closer to home on average and the probability of being visited increased by 4 percentage points. This results in a 0.3 percentage point reduction of predicted readmission at one year and 0.4 percentage point reduction at three years.

Finally, I use the same dynamic monthly assignment procedure as in the second simulation, but now I prioritize older individuals. I do this by ranking every individual to be admitted in the given month by age and admitting the oldest individuals first so that they are more likely to be placed closer to home. For example, let Individual D and Individual E both be medium security custody and be admitted in the same month and have the same facility distance ranking. Individual D is 50 years old and Individual E is 27. I would assign Individual D to their closest facility before I assign Individual E. The choice to prioritize older individuals is informed by the heterogeneity analysis done in Appendix Section F that suggests older individuals have larger treatment effects than younger individuals. I use the coefficients from this heterogeneity analysis to scale both the first stage and second stage effects. This results in a 3 percentage point increase in visitation, a 0.4 percentage point reduction of predicted readmission at one year and 0.5 percentage point reduction at three years.

Next, I consider the added reduction in incarceration days and associated cost-savings from these counterfactual placement policies that prioritize distance from home. The additional reduction from the placement policy that prioritizes older individuals translates to 5 fewer days incarcerated in the 5 years post-sentence for the average person. This translates to and an additional savings of $871.60 (using the 2022 cost of incarceration) in savings per person.

While effects for the capacity constrained simulations are modest, these changes are meaningful relative to overall recidivism rates. Relative to the one-year readmittance rate of 10 percent, a 0.3 or 0.4 percentage point reduction in recidivism translates to a 3-4 percent

decrease. This is comparable to the reductions found in Agan and Makowsky [2018a], which showed a 50 cent increase in the average minimum wage reduced one-year recidivism by 2.8 percent.

1.8 Conclusion

This paper shows that prison placement distance from home is an important policy margin. Leveraging quasi-random variation in initial facility assignment, an increase of 100 miles in distance from home increases the probability of re-admission by 9 percent within one year and 4 percent within three years.

Distance impacts recidivism through its impact on contact with family and friends. Each additional 100 miles decreases the number of monthly visits by 0.6, which is a 60 percent decrease from the average of 1 visit per month. I then build on this by estimating the causal effect of in-person prison visitation on recidivism using an instrumental variables strategy. Each additional visit per month reduces re-incarceration by 1.5 percentage points at one year and 2 percentage points at three years. Using a binary model of visitation, receiving at least one in-person visit during an incarceration spell reduces re-incarceration by about 8 percentage points within one year and by about 10 percentage points within three years. These effects are comparable to some of the most effective reentry interventions. Notably, in-person visits are something that occurs during incarceration, rather than as an additional program requirement post-incarceration that can reduce re-incarceration.

In addition to recidivism, visitation reduces the amount of time individuals spend in prison for a given spell, which is consistent with visitation decreasing in-prison misconduct. One additional visit per month results in a 1 percent reduction in time served. The median sentence length for a non-visited individual is 517 days, making this a reduction of 5 days at the median. Incorporating the additional cost savings from future recidivism, this results in cost-savings of over $4,000 per person per additional visit. This is comparable to nearly

a month of custody costs for an individual.

I also provide evidence consistent with a housing stability mechanism for visitation: an additional visit per month decreases reliance on a post-release housing voucher by 12 percent. Consistent with address verification for release, where address verification occurs within 6 months before release, visits toward the end of a prison spell are most predictive of not needing a voucher.

These results imply that reducing distance, and thereby increasing visitation, can improve reentry outcomes. At a time when some county jails are eliminating in-person visitation programs in favor of video visitation, it is important to note that visitation has a meaningful positive impact on reentry outcomes. While rigorous evidence on video visitation remains limited, research on virtual communication during the COVID-19 pandemic suggests that video calls are an imperfect substitute for face-to-face interaction. Evaluating how different forms of in-prison communication support reentry is an important direction for future work.

CHAPTER 2

AN EVALUATION OF CRISIS-INTERVENTION TEAM TRAINING

2.1 Introduction

Criminal justice systems have become a primary site for managing mental illness: Los Angeles County Jail, Cook County Jail, and Rikers Island Jail Complex each house more individuals with mental illness than any remaining psychiatric hospital in the United States of Research and Affairs [2016]. Because police are often the first responders to individuals experiencing mental health crises, they play a central role in determining how these cases enter and move through the system. It is estimated that 21 to 38 percent of 911 calls nationally are related to mental health of Research and Affairs [2016]. Additionally, individuals experiencing a mental health crisis are disproportionately more likely to be killed during an interaction with police, and between 2015 and 2022, 22 percent of those shot and killed by police were reported to be in a mental health crisis [Julie Tate, 2022].

In response to the growing demand for police involvement in mental health crises, departments have adopted a range of response models. These include traditional police response with specialized training, co-responder models, and non-police response. Co-responder models pair police officers with unarmed mental health professionals, social workers, or trained community volunteers, while non-police responses rely entirely on these non-law enforcement personnel. This project examines one of the most popular police trainings for a police-response model called crisis-intervention team (CIT) training. The goal of CIT training is to teach police officers how to de-escalate mental health crises and divert civilians in mental health crisis from the criminal justice system to health care and social services. This paper does not take a normative stance on when or if use of force is necessary, justified or excessive, but rather evaluates the program on the goal set forth of de-escalation from arrest and

force. To my knowledge, this is the first paper to empirically study the causal effects of CIT training on police use of force and propensity to arrest.

To study this question, I construct a comprehensive dataset of emergency police response in New Orleans by merging multiple administrative sources, including calls for service, police reports, use-of-force reports, officer demographic records, and officer training schedules. The resulting data cover all police calls for service in New Orleans from 2017 through June 2023 and allow me to follow an incident from the 911 call to its conclusion. This allow me to examine how CIT training affects officers' use of force and arrest decisions. In my preferred specification, I implement a difference-in-differences design that compares officers before and after training, using future trainees as the control group in each month. I find no discernible effect of CIT training on either propensity to use force or make an arrest.

I also examine whether training has spillover effects on officer behavior outside of mental health incidents, including drug-related incidents and violent crime. While CIT is designed to improve responses to mental health crises, the de-escalation techniques it teaches may generalize to other types of encounters. If officers apply these skills more broadly, training could affect use of force and arrest behavior even in incidents that are not classified as mental health–related. I again find no discernible effect on either propensity to use force or make an arrest.

This paper contributes to a growing literature on emergency responses to mental health crises. Dee and Pyne [2022], Davis et al. [2025], and Ba et al. [2025] study unarmed community response programs across a range of settings and consistently find reductions in arrests and crime, as well as increased access to services and responses to calls that might otherwise go unanswered. In contrast, Dee and Pyne [2025] examine co-responder programs, in which mental health professionals respond alongside police, and find large reductions in involuntary psychiatric detentions with no effect on arrests or crime. This paper contributes to this literature by examining how a police responser model with specialized training affects

policing outcomes, rather than alternative responders.

This paper also contributes to a growing economics literature on police training. McLean et al. [2020] studies the impact of social interaction training for police and similarly find it has no impact on officer use of force. Owens et al. [2018] and Dube et al. [2022] both develop, implement and evaluate training programs and find that officers who receive training were less likely to use force and less likely to make discretionary arrests. Rather than evaluating a specific program, Adger et al. [2022] attempt to identify how the behavior of a field training officer impacts their trainees and find that recruits who had field training officers with higher use of force also demonstrate a higher use of force, shedding light on a possible avenue for effective change. This paper contributes to this literature by estimating the causal effects of mental health response training on individual policing outcomes.

The paper proceeds as follows: Section 2 provides a background on police response to 911 calls and crisis-intervention team training in New Orleans, Section 3 discusses the data and presents summary statistics. Section 4 details the empirical strategy, Section 5 presents results and discussion, and Section 6 concludes.

2.2 Background

2.2.1 911 calls and Police Response

Police officers interact with the public either in response to 911 calls or through self-initiated stops. Due to endogeneity concerns of self-reported stops, I focus solely on interactions that originate from 911 calls.

Calls for service are received by the New Orleans Police Department's (NOPD) Communications Division. Basic metadata, including the time and location of the call, are recorded in the Computer-Aided Dispatch (CAD) system. The call-taker assigns an initial incident classification and priority level, determines whether a response is required, and dispatches

the appropriate service.If a police response is deemed necessary, officers are dispatched in one-man units based on call priority and proximity to the incident.[1] Dispatch and arrival times are recorded in CAD.

After responding to the incident, officers assign a final disposition in the CAD system. Incidents may be classified as "null" or "void" if the incident was falsely reported, or as"gone on arrival" if the relevant parties are no longer present. If involved individuals are present, the disposition is typically either "necessary action taken (NAT)" or "report to follow (RTF)". An incident is classified as NAT if no arrest is made, no report is filed, and no substantial action is taken at the scene. In these cases, I do not observe additional information about the incident or responding officers. If the disposition is RTF, officers file a formal report documenting the details of the incident, including the number and characteristics of offenders and victims. I observe reports for approximately 20–30% of calls each year.[2] These reports are based on officer observations, as well as victim and witness statements.

NOPD policy requires that any use of force beyond non-resisted handcuffing be documented in a separate use-of-force report. These reports are completed by a supervising officer and include information on the officers and subjects involved, the type and level of force used, injury status, and relevant contextual factors.[3] I also observe these use-of-force reports.

1. According to correspondence with NOPD Public Information, the default is to dispatch one officer per call. However, dispatchers may assign multiple units depending on the priority and description of the call and officer availability. Officers closest to the incident are dispatched, and additional units may respond if backup is requested.

2. Approximately 60% of RTF calls have an associated report, and a small number of calls with other dispositions also result in reports.

3. One of the main findings of the Department of Justice investigation into the NOPD in 2011 was that officers use of force was "a clear pattern of unconstitutional uses of force" but that NOPD had not disciplined any officer in at least the past six years. The report also specifically notes the use of "significant force against mentally ill persons where it appeared that no use of force was justified". These findings motivated the requirements for better use of force training and reporting, and specific training for use of force in incidents of mental illness.

2.2.2 Crisis-Intervention Team Training

One of the most common mental health crisis de-escalation trainings given to police officers is called crisis-intervention team (CIT) training. The version of CIT used in most police departments today is called the Memphis Model, which was developed by the National Alliance on Mental Illness (NAMI) of Tennessee in 1988 [of Memphis CIT Center, 2022]. NAMI estimated that as of 2008 there were over 400 CIT programs across the US and by 2019 there were over 2,700 programs [on Mental Illness, 2022].

While there has been little academic study of CIT, there are plenty of anecdotal claims of success. On their website NAMI claims "...programs like CIT reduce arrests of people with mental illness while simultaneously increasing the likelihood that individuals will receive mental health services." They also make the specific claim that "in Memphis, for example, CIT resulted in an 80 percent reduction of injuries sustained by officers during mental health crisis calls. "

The New Orleans Police Department established a CIT training program in 2015 as part of a federal consent decree established in 2013. As of June 2023, there had been 17 cohorts of officers to receive CIT training.[4] In total 365 officers received training between 2015 and 2022, representing approximately 30% of all patrol officers.[5]

In New Orleans, incidents classified as "emotional disturbed person", "mental patient", "suicide threat", "suicide attempt" or "suicide" are prioritized to receive a response from a CIT-trained officers. CIT-trained officers responded to about half of the calls with these classifications over the sample period. Protocol is for the dispatcher to locate the nearest CIT-trained officer when a mental health call comes in and dispatch them if they are available. If a CIT officer is not available, the closest officer is dispatched.

4. There first class was in 2015, then there were three training cohorts in 2016, four in 2017, two in 2018, two in 2019, one in 2020, and one in 2021.

5. From communication with NOPD, I learned that the majority of officers signed up voluntarily while some were selected for the program. There is no documentation available on which officers volunteered versus those who were selected.

During the sample period, NOPD implemented several additional reforms under the federal consent decree. Most notably, the department revised its use-of-force policies, introducing stricter guidelines, expanded training, and improved data collection. These changes were motivated by findings of disproportionate and unnecessary use of force in the Department of Justice investigation.[6]

NOPD also introduced a peer-intervention training program, EPIC, which trained officers to intervene when colleagues engaged in potentially inappropriate behavior. This program included instruction on bystander intervention and scenario-based exercises focused on de-escalation. These reforms were implemented department-wide rather than targeted to CIT-trained officers. As a result, they may affect overall levels of use of force but do not differentially affect CIT and non-CIT officers.

2.3 Data

The data for this project consist of all 911 calls for service in New Orleans between January 1, 2017 and June 30, 2023. For each call, I observe the incident date and time, dispatch time, responding officer identifiers, arrival time, incident classification, and final disposition. I then restrict the sample to calls with a disposition of "report to follow" or "necessary action taken" as these are the calls with a likely police-civilian interaction (this excludes calls labeled "gone on arrival", "unfounded", and "null"). This results in a sample of 100,000 to 200,000 calls per year.

I combine these records with multiple administrative data sources, including police reports, use-of-force reports, Crisis Intervention Team (CIT) records, and officer demographic data. A key feature of the data is that I can link arrests and use-of-force incidents back to the originating call for service. Stop-and-search data, traffic stops, and police reports are all conditional on officer-initiated contact. Because officers exercise discretion in whom they

6. NOPD also issued body-worn cameras during this period, though data on their use are not available.

approach and how they respond, such data are subject to selection bias. In contrast, dispatch data allow me to observe all calls to which officers are assigned, regardless of whether an arrest is made, force is used, or a report is filed.

Call data. The call-level data include the dispatcher-assigned priority level, incident classification (e.g., assault, homicide, disturbance), date and time the call was received, dispatch time, block-level location, and the employee IDs of dispatched officers. Importantly, I observe only the officers initially dispatched to each call. I do not observe officers who arrive later as back-up or are subsequently assigned to the scene. Approximately 22% of calls between 2017 and 2023 are missing an officer identifier and I exclude these from the analysis.[7] Table 2.1 provides descriptive statistics of the sample of calls for service.

I define mental health calls as those classified by the dispatcher as "mental patient", "emotionally disturbed person", "suicide", "suicide threat", or "attempted suicide". Mental health calls account for 2–4% of calls for service each year. Table 2.2 presents descriptive statistics for the sample of mental health calls.

Police reports. Police reports contain the incident classification and priority level, date and time, and block-level location. They also include the race and gender of each offender and victim, when available, and identify the reporting officer. Reports are written by responding officers and reflect their account of the incident. All arrests are accompanied by a police report, and I use these records to measure arrest outcomes. Approximately 30-40% of calls for service result in a police report each year.

Use-of-force data. The use-of-force data include the date and shift of the incident (an eight-hour window), location by police district, and detailed information on each officer and

7. Over 90% of mental health calls include identifiers. And over 83% of calls with dispositions NAT and RTF have identifiers. Missingness skews toward unfounded and gone on arrival calls where there was likely no officer interactions.

subject involved, including demographics, type and level of force used, injury status, and whether the subject was hospitalized.[8] I link each use-of-force incident to its originating call for service using a crosswalk provided by the NOPD. Less than 1% of calls each year are recorded as resulting in force. It is important to note that force is determined by the perspective of the officers involved.

CIT data. The CIT data include (i) a list of incident numbers to which CIT-trained officers were dispatched and (ii) rosters of officers who completed CIT training, along with their training dates.[9] About one-third to one-half of all calls are responded to by a CIT-trained officer.

Officer demographics. Officer-level data include the name, race, gender, age, assigned district, hire date, and (if applicable) retirement date for all commissioned NOPD officers between 2017 and 2023.

2.3.1 Descriptive Statistics

As shown in Table 2.2, CIT-trained officers responded to around half of all mental health calls for service each year. Mental health calls are more likely to result in use of force than they are in arrest. The low rate of arrest is likely due to the department wide mandate, accompanying the Consent Decree, instructing officers to bring those in mental health distress to a medical center rather than arresting them. However, mental health calls still have a disproportionately high-share of use of force incidents given their share of overall calls - 0.007% of mental health calls result in use of force, meaning mental health calls are seven

8. A full definition of use of force is provided in the appendix.

9. Training began at the end of 2015, but dispatch data are only available beginning in 2017. As a result, I observe which officers were trained prior to 2017 but cannot identify the specific calls to which they were dispatched before that date.

times more likely to result in force than the average call. The top three call types to result in force in order are “other", “drugs" and “mental patient".[10] Furthermore, mental health calls with a CIT-trained officer dispatched are just as likely to result in use of force (about 1%) as incidents without a CIT-trained officer dispatched.

Table 2.3 presents descriptive statistics on the sample of patrol officers. There are eight police districts in New Orleans. The distribution of officer demographics is relatively steady over-time but does meaningfully differ between districts. Years of service and office age are balanced between districts but racial and gender composition are not. For instance, District 4 has a much higher share of Black and female officers compared to any other district. I include district fixed effects in all analysis to account for these differences. Average demographics of each district are presented below. I also check for demographic differences by shift. Ba et al. [2021] show that Black officers are assigned different shifts than white officers in Chicago. However, I see no systematic differences in the race or gender make-up within shifts. Table B.2 in the Appendix shows officer demographics by shift.

CIT-trained officers are relatively uniformly distributed across districts and years (ranging from 5–12% of officers). Less experienced officers and female officers were much more likely to have received CIT training than other officers. This systematic difference in observables between trained and untrained officers motivates my use of only will-be-trained officers as counterfactuals for currently-trained officers, rather than additionally relying on untrained officers.

Given that CIT training is mostly voluntary it is also essential to examine potential selection on the dependent variable – do officers who select into training have a systematically different use of force and arrst propensity than those that do not opt to do training? To address this, I compare patterns of use of force between officers who chose to participate in training versus those who did not prior to training.[11] First, I look at whether use of force is

10. See Appendix Table B.1.

11. Data collection on use of force did not begin until 2016 but the first CIT training cohort received

correlated with observable officer characteristics, ignoring CIT training status. Male officers and officers with more experience are more likely to use force than other officers, the exact opposite demographic patterns of officers who select into training. I next compare use of force and arrest propensity between officers who are never-trained in my sample and those who are always-trained in my sample. Officers who have not been trained are 8% more likely to use force, indicating that officers who choose to receive training may be already less likely to use force, even without training.

Table 2.1: Incident Descriptive Statistics

	2017	2018	2019	2020	2021	2022	2023*
N Incidents	196,442	190,865	153,143	119,704	127,867	110,340	56,500
Modal priority level	1	1	1	1	1	1	1
% police report	0.33	0.36	0.37	0.40	0.44	0.44	0.45
% property crime	0.07	0.08	0.10	0.11	0.12	0.14	0.17
% violent crime	0.02	0.02	0.02	0.03	0.03	0.03	0.04
% domestic	0.06	0.08	0.07	0.09	0.08	0.07	0.06
% welfare check	0.05	0.10	0.13	0.07	0.04	0.01	0.01
% disturbance	0.13	0.13	0.13	0.16	0.14	0.12	0.10
% mental health	0.02	0.03	0.03	0.03	0.04	0.04	0.03
% CIT dispatch	0.48	0.50	0.46	0.40	0.38	0.36	0.37
% arrest	0.09	0.09	0.11	0.11	0.14	0.15	0.16
% force used	0.002	0.002	0.002	0.002	0.002	0.003	0.003

Notes: *Only January 1, 2023 – June 30, 2023. The possible priority levels are 0–4. 0 indicates an administrative duty, 1 is lowest priority and 4 is highest priority. The two potential call dispositions are “report to follow" and “necessary action taken". Percent police report indicates the percent of calls where an official police report was written, as opposed to being resolved as “necessary action taken". The most common call classification is “complaint - other".

training in 2015, so I am not able to look at pure pre-trends between ever-treated and never-treated officers.

Table 2.2: Mental Health Incident Descriptive Statistics

	2017	2018	2019	2020	2021	2022	2023*
N Incidents	4,226	4,961	4,259	4,110	4,711	3,904	1,875*
% police report	0.11	0.12	0.12	0.14	0.17	0.15	0.12
Modal priority level	2	2	2	2	2	2	2
% emotionally disturbed person	0.94	0.91	0.90	0.88	0.86	0.71	0.17
% suicide	0.06	0.02	0.01	0.01	0.01	0.01	0.01
% suicide threat/attempt	0	0.07	0.09	0.11	0.13	0.27	0.82
% CIT response	0.53	0.57	0.51	0.43	0.43	0.4	0.36
% arrest	0.001	0.001	0.001	0.002	0.003	0.003	< 0.001
% force used	0.01	0.01	0.02	0.02	0.02	0.03	0.03

Notes: *Only January 1, 2023 – June 30, 2023. The possible priority levels are 0–4. 0 indicates an administrative duty, 1 is lowest priority and 4 is highest priority. The two potential call dispositions are "report to follow" and "necessary action taken". Percent police report indicates the percent of calls where an official police report was written, as opposed to being resolved as "necessary action taken". The classification "emotionally disturbed person" replaced "mental patient" in 2020, and refers to the same type of incident. Prior to 2018, suicide attempts and threats were classified as "suicide".

Table 2.3: Demographics by District

District:	1	2	3	4	5	6	7	8
N patrol officers	78	81	81	75	81	75	84	100
Avg Age	39	41	42	42	41	40	40	41
Avg Years of Service	12	12	12	13	13	12	12	13
% Black	0.38	0.48	0.34	0.71	0.50	0.39	0.55	0.45
% white	0.42	0.43	0.54	0.22	0.31	0.49	0.25	0.42
% female	0.17	0.24	0.17	0.31	0.25	0.22	0.26	0.20

Note: Statistics are averages per month over January 2017–June 2023.

Table 2.4: Officer Demographics by CIT Status

	CIT-trained	Untrained	Overall
% female	0.31	0.20	0.23
% Black	0.47	0.53	0.50
Avg age	43	48	41
Avg years of experience	11	16	12

Note: Column 1 is the percent of CIT-trained officers who are female and Black, their mean age and mean years of experience. Column 2 is the percent of non-CIT-trained officers who are female and Black, their mean age and mean years of experience. Column 3 presents the average demographics for all officers.

2.4 Empirical Strategy

I consider a staggered adoption design with N officers observed over T months. Let $i \in 1, \ldots, N$ index officers and $t \in 1, \ldots, T$ index months. Officers may receive CIT training at different points in time or not at all. Let G_i denote the month in which officer i completes training, with $G_i = \infty$ for officers who are never trained. Once an officer is trained, they are considered treated in all subsequent periods.

For each officer-incident, let $Y_{ict}(0)$ denote the potential outcomes without training and let $Y_{ict}(1)$ denote the potential outcome with training. Outcomes are an indicator for whether the incident results in use of force or an arrest. The object of interest is the causal effect of training on officer behavior at the incident level.

The primary challenge is that the counterfactual outcome $Y_{ict}(0)$ for treated officers is unobserved. A naive comparison of trained and untrained officers would be biased if trained officers differ systematically from untrained officers. The descriptive evidence above suggests they do differ, and so instead, I exploit variation in the timing of training across officers and compare changes in outcomes for officers before and after training to outcomes for officers who have not yet been trained. This yields a difference-in-differences design with staggered

treatment timing.

I estimate the following equation:

$$Y_{ict} = \alpha_i + \gamma_t + \beta_0 Treated_{ic} + \sum_k D_{ct}^k + \sum_{k \neq 1} \beta_k \left(Treated_{ic} \cdot D_{ct}^k \right) + \epsilon_{igt} \tag{2.1}$$

where Y_{ict} is an indicator for whether officer i uses force or makes an arrest at incident c in month t. α_i are officer fixed effects and γ_t are calendar month fixed effects. $Treated_{ic}$ is an indicator equal to 1 if officer i received CIT training in cohort c. The variables D_{ct}^k are indicators equal to 1 if month t is k periods relative to officer i's training date in cohort c. The coefficients of interest are β_k, which capture which the difference in outcomes between treated and control officers at event time k (relative to omitted period $k = -1$).

This estimation strategy uses officers who are eventually given CIT training but have not yet been trained at time t as the control group for officers trained at time t, following Deshpande and Li [2019]. For each month t, I label officers who had finished training by that month as treated and use officers who are trained in the next calendar year or later as the control group in month t. In practice, I create a stacked cohort data set. For each training cohort, I construct a dataset that includes all mental health incidents involving officers trained in that cohort (treated) and officers who are trained at least one year later (controls). For example, for the cohort trained on February 23, 2018, the sample includes incidents involving those officers as well as incidents involving officers whose training occurs at least one year after that date. I then stack these cohort-specific datasets to form the final estimation sample.

Because officers can appear in multiple cohort-specific samples, they may contribute observations as both treated and control units. To account for this dependence, I cluster

standard errors at the cohort level.[12]

The identifying assumption is that without training, the rates of use of force and arrest would have evolved in parallel for officers that experience training in a given cohort relative to officers that experience training in a later cohort. While this cannot be directly tested, evidence of parallel trends in months prior to training provide evidence for this. See Figure 2.2.

A concern is that CIT training may change the composition of calls trained officers respond to. If CIT-trained officers are assigned only to mental health incidents after training, changes in outcomes could reflect differences in exposure to these types of incidents rather than changes from training. For example, increased exposure could improve performance through experience or alter responses through fatigue. In either case, shifts in the types of calls handled could affect outcomes independently of training, which is a threat to identification. I test for changes in call composition by estimating an event-study with an indicator for mental health incidents as the outcome, including officer and shift-by-date fixed effects. Figure 2.1 presents the estimates. I find no evidence that the share of mental health calls within a shift changes after training.

12. The officer level is fully nested within the cohort level, so clustering at the higher level is appropriate [Cameron et al., 2011].

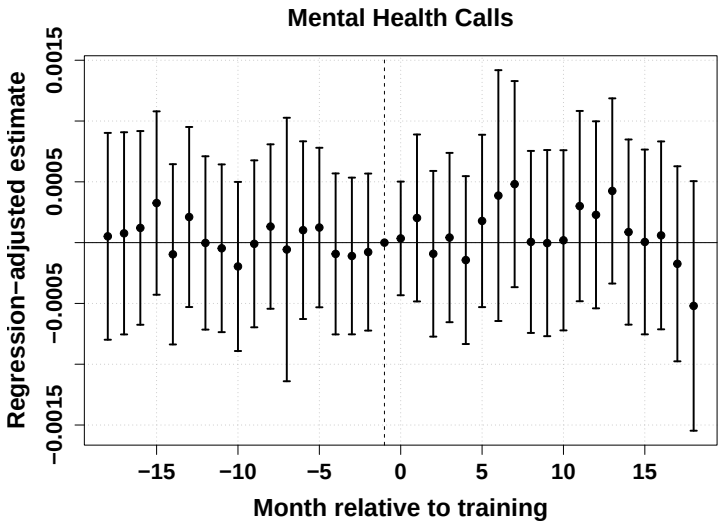


Figure 2.1: Mental Health Call Composition

Notes: This figure plots the event-study coefficients from a regression of a binary indicator for whether an incident was a mental health call on officer and shift-by-date fixed effects. This tests for a change in the composition of calls an officer responds to on a given shift in the months before and after receiving CIT training.

2.5 Results

Figure 2.2 presents estimates of the effect of CIT training on an officer's propensity to use force and to make an arrest. These are estimates of the β_k coefficients from Equation (2.1). For the estimates presented in Table 2.5 I pool all treated and u-treated periods into one pre- and one post-period.

CIT training does not appear to meaningfully affect an officer's likelihood of making an arrest or using force. While the estimates are imprecise, they do not indicate evidence of large or systematic effects. CIT training is often promoted as a key tool for improving police use of force in response to mental health incidents. The results here do not provide evidence in support of that claim. These findings are policy relevant as many departments are investing in mental health training programs and would benefit from evidence on their

effectiveness.

There are several possible explanations for these results. One possibility is that CIT training does not substantially affect arrest or use-of-force outcomes. Alternatively, the version of CIT implemented by the New Orleans Police Department may be important. Although NOPD reports following the standard Memphis Model used by many departments, I am unable to observe the content or delivery of the training directly. As a result, it is difficult to identify the mechanisms underlying the null effects. For example, the duration of training or its classroom-based format may not be sufficient to change officer behavior.

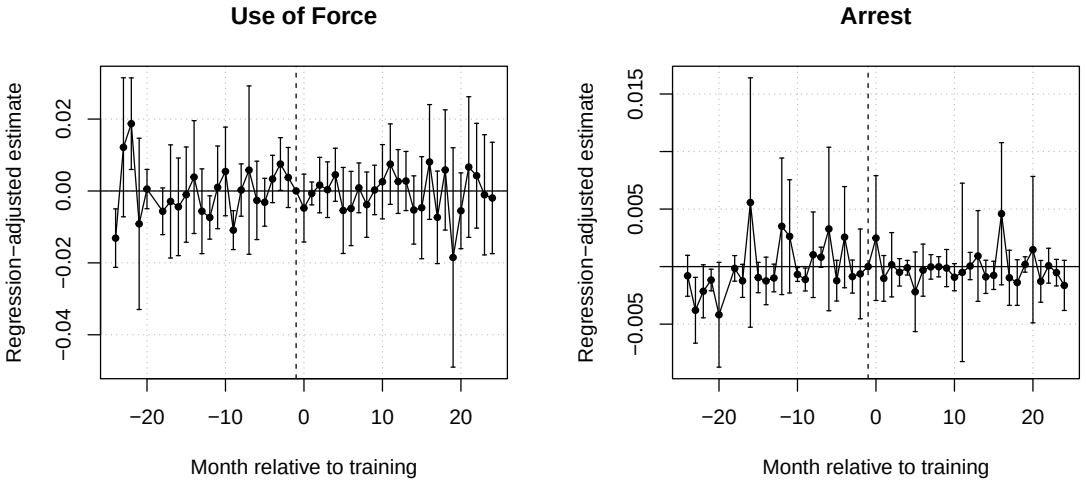


Figure 2.2: Use of Force and Arrests

Notes: This figure plots estimates of the effect of CIT training on an officer's propensity to use force (left panel) and make an arrest (right panel). These are estimates of coefficients β_k from Equation (1). The dependent variable is a binary indicator for force being used at the incident or an arrest being made. The sample is all incidents involving officers who are trained during the period of observation. Controls are yet-to-be-trained officers. The error bars plot the 95% confidence interval of the estimated coefficients.

Table 2.5: Pooled DiD

	Use of Force	Arrest
	(1)	(2)
Treated × Post	−0.0007	−0.0004
	(0.0018)	(0.0005)
Untreated mean	0.025	0.002
Observations	96,057	96,057

Notes: *p<0.1; **p<0.05; ***p<0.01. The estimates are for the coefficient δ from $Y_{igt} = \alpha_i + \gamma_t + \delta_0 Treated_{ig} + \sum_k D_{gt}^k + \beta\left(Treated_{ig} \times Post_{gt}\right) + \epsilon_{igt}$. Treated = 1 if the officer ever receives training and Post = 1 if the incident occurred after the officer received training. Fixed effects for incident priority, day of week, shift and district are included. The sample is only mental health incidents. Only officers who receive training during the window of observation (2017- June 2023) are included. This excludes always-treated and never-treated officers. Standard errors are clustered at the cohort level.

2.5.1 Spillovers

The focus of CIT training is preparing officers to respond to mental health crises, but de-escalation techniques can be easily transferable to other scenarios. While I am not able to find an effect for mental health calls, it is possible officers are using their skills on other calls. To check for possible spillovers, I perform the same analysis as above on non-mental health incidents, violent incidents, and drug-related incidents.[13] Table 2.6 presents the results. I find no evidence that CIT training affects use of force or arrest rates for any of these incident types. As expected, violent incidents have substantially higher baseline arrest rates, reflecting the nature of these offenses, which are more likely to result in arrest. Drug-related incidents

13. I define violent incidents as any incident classified in CAD as an assault, battery, homicide, weapon possession, or rape. I define drug-related offenses as any incident classified in CAD as a drug violation or drunk incident.

may be more comparable to mental health calls, as they can involve similar behaviors and may also benefit from de-escalation techniques. However, I do not detect effects for these incidents either.

Table 2.6: Other Incident Types

	Non-Mental Health		Violent		Drug	
	Use of Force	Arrest	Use of Force	Arrest	Use of Force	Arrest
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post	0.00003	0.0013	−0.0015	0.0014	−0.0024	−0.006
	(0.0002)	(0.0016)	(0.0015)	(0.0040)	(0.0043)	(0.0154)
Untreated mean	0.01	0.13	0.03	0.50	0.04	0.80
Observations	1,862,193	1,862,193	213,371	213,371	8,307	8,307

Notes: *p<0.1; **p<0.05; ***p<0.01. The coefficient presented is δ from Eq.(2): Treated = 1 if the officer ever receives training and Post = 1 if the incident occurred after the officer received training. Fixed effects for incident priority, day of week, shift and district are included. The sample is only mental health incidents. Only officers who receive training during the window of observation (2017- June 2023) are included. This excludes always-treated and never-treated officers. Standard errors are clustered at the cohort level.

2.6 Conclusion

This paper examines the impact of crisis intervention team (CIT) training on police officers' use of force and arrest behavior using data on all calls for service in New Orleans from 2017 through early 2020. I find no evidence that CIT training meaningfully affects an officer's likelihood of using force or making an arrest in response to mental health incidents. I also find no evidence of spillover effects to other types of calls.

These findings contribute to ongoing discussions about how best to respond to mental health crises. CIT training is often promoted as a key tool for improving police interactions in these contexts, but these results do not provide clear evidence that it changes key observable behaviors. Understanding why CIT training does not produce detectable effects in this setting is an important direction for future research. For example, differences in the content,

duration, or delivery of training may limit its effectiveness.

More broadly, these results highlight the importance of the implementation of reforms. CIT training was introduced in the context of the New Orleans consent decree as a mandated, top-down policy change, rather than one developed organically or accompanied by officer buy-in. These findings suggest that some reforms may be more symbolic than substantive, reflecting performative compliance rather than meaningful changes in behavior.

CHAPTER 3

PRISON DIVERSION FOR PARENTS

3.1 Introduction

Around 3.6 percent of children in the United States experience parental incarceration by age 18, and an even larger share experience parental involvement with the criminal justice system [Finlay et al., 2023]. A growing literature studies the consequences of parental criminal justice involvement for children, with mixed evidence across contexts. For example, Dobbie et al. [2018a] document increases in teen crime and pregnancy and declines in employment in Sweden, while Arteaga [2023] find increases in educational attainment in Colombia, and Norris et al. [2021] find reductions in future criminal activity in Ohio. These findings highlight that the effects of parental incarceration nay be specific to their context. An open question is whether policies that reduce parent–child separation, such as diversion from incarceration, effects parents' future criminal behavior.

In this project, I evaluate the effect of prison diversion for parents on future criminal convictions. I study Washington State's Parental Sentencing Alternative (PSA), which allows judges to waive a prison sentence for parents with dependent minor children who have been convicted of non-violent felony offenses. The alternate sentence consists of community supervision, parenting classes, and other program requirements.

This paper contributes to two strands of literature. First, it contributes to research on the effects of incarceration and diversion on future criminal behavior. Existing evidence is mixed: Dobbie et al. [2018b] find no effect of pretrial detention on subsequent crime, Mueller-Smith and Schnepel [2021a] find that diversion substantially reduces recidivism, while Bhuller et al. [2020b] and Rose and Shem-Tov [2021] find that incarceration can reduce recidivism, with diminishing marginal effects of sentence length in the latter. Agan et al. [2023] further show that avoiding misdemeanor prosecution reduces future criminal behavior. This paper adds

to this literature by studying a targeted diversion policy applied to parents, a group of particular policy interest.

Second, this paper contributes to a growing literature on the role of family connections in shaping criminal justice outcomes. Prior work examines how parental criminal justice involvement affects children's long-term outcomes [Dobbie et al., 2018a, Arteaga, 2023, Finlay et al., 2023, Norris et al., 2021] and how family formation influences parents' criminal behavior [Massenkoff and Rose, 2024]. By focusing on a policy that directly alters parent–child separation, this paper provides new evidence on how avoiding incarceration affects both criminal behavior and family dynamics.

The paper proceeds as follows: Section 2 describes the institutional setting and program, Section 3 explains the data and provides descriptive statistics, Section 4 details the empirical strategies and their accompanying results, and Section 5 concludes.

3.2 Background

3.2.1 Sentencing and Prison in Washington

In this paper, I focus on the criminal court system in Washington state, specifically the Superior Courts, which handle felony cases and major crimes. Washington has a non-unified court system, meaning that courts are administered at the county level. There are 39 counties and 30 Superior Courts, with several smaller counties combined into a single court.

To illustrate how cases enter the Superior Courts, I describe the process in King County, Washington's largest county. After an arrest, the Prosecuting Attorney's Office reviews the case and decides whether to file formal charges. If charges are filed, the defendant is scheduled for an arraignment hearing, at which the prosecuting attorney presents the charges and the defendant enters a plea ("guilty" or "not guilty"). A trial date is then set.

Leading up to trial the court holds a series of pre-trial hearings at which the parties appear

before the judge to address procedural and evidentiary issues. Arraignment is typically handled by a judge assigned on a rotating schedule, while a different judge is then assigned to the case for its duration, presiding over pre-trial proceedings, trial, and sentencing.

During the pre-trial period, parties will often negotiate a plea agreement. If a plea agreement is reached, the court schedules a hearing to accept the plea, and a sentencing hearing is typically set for approximately two weeks later. If no plea is reached, the case proceeds to trial, which is generally decided by a jury. If the defendant is found guilty at trial, a sentencing hearing is scheduled a few weeks later.

The sentencing hearing occurs after either a guilty plea is entered or a guilty verdict is reached at trial. At the sentencing hearing, the judge determines the defendant's sentence after hearing recommendations from both the prosecution and the defense. Possible sentences include incarceration, restitution, fines, community supervision (probation), and court-ordered treatment. Individuals sentenced to more than one year of incarceration serve their sentence in state prison, while those sentenced to less than one year typically serve time in a combination of county jail and under community supervision.

3.2.2 Parental Sentencing Alternative

In 2010 Washington state enacted Substitute Senate Bill 6639 which established the Parental Sentencing Alternative to allow alternatives to incarceration for nonviolent offenders who have minor children. The program is composed of two arms: a sentencing alternative and an option for early release from prison. The sentencing alternative is called the Family and Offender Sentencing Alternative (FOSA), and takes place at the time of sentencing and allows judges to divert offenders from prison into community supervision and parenting classes. The early release option is called the Community Parenting Alternative (CPA) and allows the Department of Corrections to release incarcerated parents early into partial confinement. This project focuses on the prison diversion arm and further explanation of the early-release

program can be found in the Appendix.

FOSA eligibility is as follows: the convicted individual and their children must reside in Washington state, the convicted individual must be parent (an expectant parent, a current parent with physical custody of a minor child, a legal guardian of a minor child, a biological parent, an adoptive parent, or a custodian or a step-parent with an established, ongoing and substantial relationship with a minor child that existed at the time of the current offense). The individual must then also sign an informative release waiver regarding child welfare involvement. They must also be sentenced for an offense with a standard sentence greater than one year.[1] The current conviction may not be for a violent felony offense, a felony sex offense, or a serious violent offense. The individual may also not have any prior convictions for felony sex offenses, serious violent offense, or any felony offenses where they were armed with a deadly weapon.

If a judge is considering granting FOSA, they submit a request for a Risk Assessment report to the Department of Corrections. This report is meant to determine both eligibility and suitability (whether it is in the best interest of the child). The report includes a child welfare history report from the Department of Children, Youth, and Families, a criminal history background check, a home visit, and substance use disorder screening. The risk assessment includes a recommendation from the DOC FOSA team for whether or not FOSA should be recommended, but the imposition of FOSA is at the sole discretion of the judge and they are not required to follow the recommendation.

Judges often impose mandatory programming requirements ranging from substance use treatment and vocational training to life skills and parenting classes, to quotas on time spent reading with your children per day, and distraction free dinners.[2]

If a participant does not complete programming, does not meet with their case manager

1. Sentences of one year or less are served under community supervision in Washington state. Sentences of greater than one year are served in incarceration.

2. RCW 9.94A.655.

or commits new criminal acts they will be required to reappear in court and the judge may impose additional requirements, additional time, or revoke FOSA. If FOSA is revoked, the individual must then serve their entire original sentence. Time in FOSA is not counted toward the sentence.

3.3 Data

Data for this project comes from the Administrative Office of the Courts for Washington state and the Washington Department of Corrections. The court data covers all 30 Superior Courts in Washington state and encompasses around 28,000 unique non-violent felony convictions between January 1, 2010 and December 31, 2018. [3] Court data is at the case-defendant level and includes defendant name, defendant birth date, sentence date, sentencing county, all charges, sentence, fines, and judge name.[4]

I restrict the sample to non-violent felony convictions because FOSA eligibility requires an individual to have been found guilty of a non-violent crime and sentenced to time in prison, and in Washington, most misdemeanor offenses will result in jail time or community supervision, and so the vast majority of FOSA-eligible cases will be nonviolent felonies.[5]

Information on FOSA participation comes from administrative records maintained by the Washington Department of Corrections. These records include the participant's name, date of birth, sex, race, sentencing county, offense, order date, FOSA start and end dates, and program status (active, dismissed, denied, pending, or revoked).[6] I match FOSA records to

3. Washington does not have a unified court system and instead each Superior court district operates independently. Washington has 39 counties, and while most Superior Courts cover one county, in rural areas counties are combined into multi-county districts. The multi-county districts are: Skamania-Klickitat, Columbia-Garfield-Asotin, Ferry-Stevens-Pend Oreille, Benton-Franklin, and Pacific-Wahkiakum.

4. Some courts consolidate multiple charges per single individual into one case, while others maintain separate cases per charge per individual. In both instances, all cases are heard together in one hearing and I consolidate multiple case numbers per individual within a hearing into a single observation.

5. To identify felony offenses I use the charge class identified in the court data. To identify non-violent offenses I use the defintion provided in the Revised Code of Washington RCW 9.94A.030.

6. Benton and C counties still missing. 18 individuals appear twice.

court records using name, date of birth, sentencing county, and offense type.

Court data does not include demographics, but for individuals sentenced to FOSA or admitted to prison in 2010 or later I able to merge individuals to their prison records using full name, birth date, sentence date and county.

After all merges, the sample consists 28,495 cases overall and 197 cases considered for FOSA (138 of these were approved). Table 3.1 and Table 3.2 show sample sizes as each restriction and merge is applied.

Table 3.1: Sample Construction

N Cases 2000-2020	441,410
N Non-violent Felony Cases 2010-2020	131,141
... with Prison Records 2010-2019 admit	31,121
... and sentenced before 2018	28,495

Notes: This table illustrates how the final sample used in estimation is constructed. The final sample consists of all non-violent felony cases sentenced between January 1, 2010 and December 31, 2017 with valid identifying information on the defendant (name and birth date) and a valid prison record with the Department of Corrections. Department of Corrections data is used for demographic information (race and gender) on each individual. Sentencing is restricted to before 2018, in order to allow at least 3 years for measuring recidivism.

Table 3.2: FOSA Sample Construction

	FOSA Considered	Approved	Denied
N Cases 2010-2020	483	303	180
Matched to court records	390	251	139
... without missing demographics	263	197	66
... sentenced before 2018	197	138	59

Notes: This table illustrates the final sample of FOSA-considered individuals. The final sample consists of all non-violent felony cases sentenced between January 1, 2010 and December 31, 2017 with valid identifying information on the defendant (name and birth date) and a valid prison record with the Department of Corrections. Department of Corrections data is used for demographic information (race and gender) on each individual. Sentencing is restricted to before 2018, in order to allow at least 3 years for measuring recidivism.

To measure recidivism I create a binary indicator for whether an individual appears in court sentencing records again within 6 months, 1 year and 3 years of their first sentencing appearance in the data. It is important to note when comparing recidivism rates for those sentenced to prison versus FOSA that the modal prison sentence length is 1 year and the median is closer to 2 years. Most individuals serve about 60% of their sentence and so the average individual sentenced to serve prison time is likely incarcerated for 6 months to 1 year, and therefore low recidivism rates at 6 months and 1 year for those sentenced to prison likely reflect incapacitation. FOSA participants recidivate more on average than those incarcerated within 6 months and 1 year post-sentencing. However, given the small treatment size, standard errors are quite large and confidence intervals overlap with the untreated rate. Additionally, at three years, those sentenced to FOSA have lower average rates of recidivism than non-participants. Those considered for FOSA but not granted FOSA have higher recidivism rates than those granted, but much lower recidivism rates than those not considered, likely reflecting positive selection into who is even considered for the program.

Importantly, I do not count revocations of FOSA as recidivism. I only count a new occurrence in the sentencing data. Revocation can occur if a new crime is committed, so

often a revocation also indicates recidivism, but FOSA can also be revoked for failure to follow programming requirements, which would not count as a new offense. Upon revocation, some FOSA participants incur new charges and a new sentence on top of their original sentence and some will only have their original sentence reinstated. Anytime spent under FOSA does not count toward time served.

Descriptive statistics are shown in Table 3.3. The modal crime for individuals approved for FOSA is a drug crime. They are more likely to be female then the general population appearing in court, less likely to be white, are slightly younger, and less likely to have prior court appearances, indicating selection into who is treated.

Table 3.4 shows descriptive statistics within the FOSA population by completion status. Those who have FOSA revoked are much more likely to be Black, are slightly older, and have more priors than those who complete FOSA.

Table 3.3: Descriptive Statistics by FOSA-Approval Status

	Approved	Denied	Not Considered
N Cases	138	56	27,528
% Drug crime	0.49	0.16	0.24
% Property crime	0.25	0.43	0.30
% Firearm crime	0.07	0.05	0.08
% Burglary	0.20	0.16	0.16
% Male	0.28	0.50	0.87
% White	0.28	0.64	0.65
% Black	0.09	0.18	0.12
% Asian	0.02	0.04	0.03
% Hispanic	0.02	0.09	0.14
% Native American	0.03	0.02	0.06
Mean sentencing age	40.67	44.18	42.94
Modal sentence length (days)	-	548	366
Median N priors	0.5	1	2
% Re-convicted within 6 months	0.09	0.04	0.05
% Re-convicted within 1 year	0.10	0.07	0.08
% Re-convicted within 3 years	0.15	0.11	0.24

Notes: This table presents descriptive statistics of the sample. Observations are at the case level. *Approved* cases are cases where the individual was granted a FOSA diversion. *Denied* cases are cases where the individual was considered for a FOSA diversion, as indicated by a request for a Risk Assessment Report, but it was not granted. *Not Considered* cases are cases where there was no request for a Risk Assessment Report. Sentence length is not available for FOSA participants because their sentence was waived. Reconvictions are measured from date of sentencing and are measured as a subsequent appearance by the individual in the court sentencing data.

Table 3.4: Descriptive Statistics by FOSA-Completion Status

	Completed	Revoked
N Cases	112	26
Modal county	Pierce	Kitsap
Modal crime	Residential Burglary	Controlled Substance
Mean age at sentencing	36	40.85
Median sentence length (days)	-	502
Modal sentence length (days)	-	366
Median N priors	0	1
% Male	0.29	0.23
% White	0.21	0.58
% Black	0.05	0.23
% Asian	0.02	0.04
% Hispanic	0.01	0.08
% Native American	0.02	0.08
% Re-convicted within 6 months	0.01	0.42
% Re-convicted within 1 year	0.01	0.50
% Re-convicted within 3 years	0.04	0.62

Notes: This table presents descriptive statistics of all FOSA participants in the sample. Observations are at the case level. *Completed* cases are cases where the individual was granted a FOSA diversion and completed the program without technical revocation or a new conviction. *Revoked* cases are cases where the individual was granted a FOSA diversion but the court revoked the diversion for either a new crime or technical violation and imposed the individual's original sentence. Sentence length is not available for successful FOSA participants because their sentence was waived. Reconvictions are measured from date of sentencing and are measured as a subsequent appearance by the individual in the court sentencing data.

3.4 Empirical Strategy and Results

I employ several different empirical strategies to estimate the causal effect of prison diversion on future criminal convictions. I discuss each in turn with results.

3.4.1 Matching

Empirical Strategy

I begin by using two propensity-score matching estimators to compare individuals sentenced to FOSA to comparable individuals who were never considered for FOSA. I condition on age at sentencing, gender, race, number of prior court appearances, and crime type. Importantly, I am unable to observe parental status.

Let $D_i \in \{0, 1\}$ indicate whether individual i is assigned to FOSA, let $Y_i(1)$ and $Y_i(0)$ denote potential outcomes, and let $p(X_i) = \Pr(D_i = 1 \mid X_i)$ denote the propensity score.

First, I implement one-to-one nearest-neighbor matching on the propensity score. For each treated unit i, let $m(i)$ denote the untreated unit with the closest estimated propensity score. The resulting estimator is

$$\hat{\tau}_{ATT}^{match} = \frac{1}{N_T} \sum_{i:D_i=1} \left(Y_i - Y_{m(i)} \right). \tag{3.1}$$

Second, I use inverse probability weighting.

$$\hat{\tau}_{ATT}^{IPW} = \frac{1}{N_T} \sum_{i=1}^{N} D_i Y_i - \frac{1}{N_T} \sum_{i=1}^{N} (1 - D_i) \frac{p(X_i)}{1 - p(X_i)} Y_i. \tag{3.2}$$

with weights

$$w_i = \begin{cases} 1 & \text{if } D_i = 1 \\ \dfrac{p(X_i)}{1 - p(X_i)} & \text{if } D_i = 0. \end{cases}$$

Identification Assumptions.

Under conditional independence,

$$(Y_i(1), Y_i(0)) \perp D_i \mid X_i,$$

and overlap,

$$0 < \Pr(D_i = 1 \mid X_i = x) < 1 \quad \text{for all } x,$$

both estimators can be interpreted as identifying causal effects.

In words, conditional on observed covariates, X_i (age, gender, race, priors, and crime type), treatment assignment is as good as random. This is violated if we think there is something unobserved, such as parental status, open child welfare cases, or actual parent-child relationship, that is impacting the likelihood of being assigned to FOSA.

The overlap assumption requires that, for all values of X_i, there is a positive probability of both receiving and not receiving treatment. Figure 3.1 shows limited overlap in propensity scores, with most control observations concentrated near zero. I therefore restrict the sample to the region of common support, defined as the range of propensity scores shared by treated and untreated observations:

$$\left[\max\left\{\min_{i:D_i=1} p(X_i),\ \min_{i:D_i=0} p(X_i)\right\},\ \min\left\{\max_{i:D_i=1} p(X_i),\ \max_{i:D_i=0} p(X_i)\right\}\right]$$

. Estimates should be interpreted as local to this region and may not generalize to individuals with a high probability of receiving FOSA.

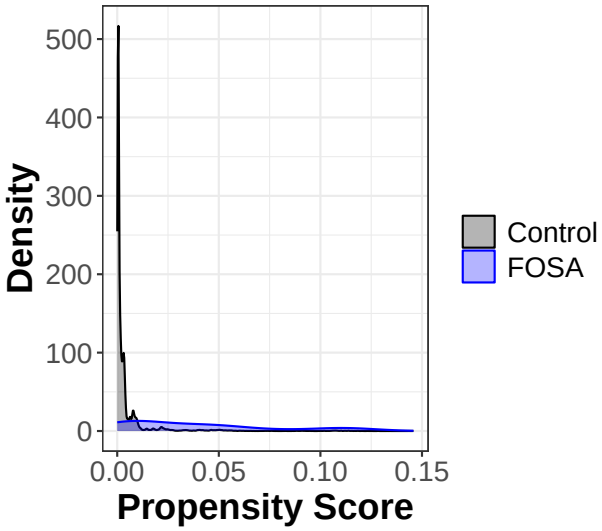


Figure 3.1: Common Support

Results

Table 3.5 displays results for both estimators as well as for OLS with controls. Just as in the raw data, individuals sentenced to FOSA are more likely to be convicted of a new crime within 6 months and within 1 year than those who are incarcerated. At three years, point estimates become negative across specifications, though they are not statistically significant for OLS or nearest-neighbor matching, while the IPW estimate is negative and statistically significant. This pattern suggests short-run incapacitation effect from prison as opposed to community supervision, with some evidence of improved longer-run outcomes. Given limited overlap and potential selection, these estimates should be interpreted with caution.

Table 3.5: Matching Estimator and OLS

	Within 6 months			Within 1 year			Within 3 years		
	Nearest	IPW	OLS	Nearest	IPW	OLS	Nearest	IPW	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	0.0507*	0.0422***	0.0428**	0.0580*	0.0374***	0.0375	-0.0580	-0.0408***	-0.0396
	(0.0289)	(0.0033)	(0.0195)	(0.0311)	(0.0036)	(0.0229)	(0.0464)	(0.0050)	(0.0363)
Mean	0.05	0.05	0.05	0.08	0.08	0.08	0.24	0.24	0.24
N	276	23,139	28,495	276	23,139	28,495	276	23,139	28,495
R^2	0.01	0.07	0.01	0.01	0.06	0.01	0.01	0.003	0.03

Notes: Standard-errors in parentheses. ***: 0.01, **: 0.05, *: 0.1. Treated refers to a case-individual observation that was assigned to FOSA. The sample for Columns 1, 4, and 7 includes only treated observations and their nearest untreated match. The sample for Columns 3, 6, and 9 includes all non-violent felony cases sentenced between January 1, 2010 and December 31, 2017. The sample for Columns 2, 5, and 8 includes all non-violent felony cases sentenced between January 1, 2010 and December 31, 2017 restricted to the region of common support.

3.4.2 Within-Judge Comparisons

Empirical Strategy

Judges may grant FOSA to individuals they believe are less likely to reoffend, which would bias comparisons between treated and untreated individuals. To partially address this concern, I exploit within-judge variation in FOSA decisions by including judge fixed effects. This removes differences across judges in overall leniency or sentencing practices, so that identification comes from comparing cases handled by the same judge in which FOSA is granted in some instances and denied in others. This also allows comparisons between parents, as those considered for FOSA must be parents.

Among the 46 judges who have considered granting FOSA, only 15 have both granted and denied FOSA. These judges collectively granted FOSA 25 times and denied it 17 times. I restrict the sample to cases heard by these judges, allowing for comparisons across defendants facing the same judge but receiving different FOSA decisions. Descriptive statistics for this sample are reported in Appendix Table C.1.

I estimate the following equation using OLS:

$$Y_i = D_i + X_i + \alpha + v_i \tag{3.3}$$

where Y_i is a binary indicator for recidivism within 6 months, 1 year or 3 years. D_i is an indicator for whether an individual was granted FOSA. X_i is a vector of observable characteristics: crime type, gender, race, and number of priors. α is the judge fixed effect. The sample is all FOSA considered cases sentenced by a judge who has granted FOSA and denied FOSA. The comparison is therefore between cases sentenced by the same judge, but where one was granted and FOSA and the other denied.

This specification compares individuals sentenced by the same judge, effectively holding

fixed time-invariant differences in judicial leniency or preferences. Identification therefore comes from variation in FOSA decisions across cases handled by the same judge.

This approach is related to the judge leniency instrumental variables design, which uses quasi-random assignment of cases to judges with different propensities to grant treatment. In this setting, however, such an approach is not feasible. FOSA is granted very infrequently, and only a small number of judges both grant and deny FOSA, resulting in a weak and poorly powered instrument (see Appendix C.4).

Instead, I exploit within-judge variation in FOSA decisions by including judge fixed effects. This approach removes differences across judges in overall leniency or sentencing practices and identifies the effect of FOSA by comparing cases handled by the same judge in which FOSA is granted in some instances and denied in others. While this strategy avoids relying on cross-judge variation, it still requires that, within a judge, the decision to grant FOSA is not driven by unobserved determinants of recidivism.

Results

Table 3.6 presents the results. None of the individuals in this sample were re-convicted within 1 year and so I only consider recidivism within 3 years. Point estimates are negative, meaning individuals granted FOSA are less likely to have been re-convicted within 3 years, but estimates are not statistically significant.

Table 3.6: Within Judge OLS

	Re-convicted within 3 years	
	(1)	(2)
Treated	-0.0375	-0.0635
	(0.0756)	(0.0936)
Controls	No	Yes
Judge FE	Yes	Yes
Mean	0.24	0.24
N	42	42
R^2	0.30657	0.53802

Notes: Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1. The sample is all non-violent felony cases heard by a judge who has considered at least 2 cases for FOSA where at least 1 was granted and 1 was denied.

3.5 Conclusion

A general concern with diversion programs is that, by reducing incarceration, they may increase criminal activity by eliminating incapacitation or by reducing the punishment and therefore reducing deterrent effects of punishment. The estimates presented here provide some suggestive evidence consistent with this concern, with higher rates of reoffending in the short term among diverted individuals. However, this pattern reverses at longer horizons, with point estimates indicating lower recidivism within three years.

Taken together, these results suggest that the effects of diversion may vary over time and across individuals. While some individuals may reoffend in the short run when not incapacitated, others may benefit from remaining in the community, leading to improved longer-term outcomes.

Future work should examine the effects of diversion on children, as well as better understand selection into diversion and heterogeneity in treatment effects, including which individuals are most likely to benefit from remaining in the community.

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APPENDIX A

DOES DISTANCE FROM HOME MATTER IN PRISON? EFFECTS ON VISITATION AND RECIDIVISM

A.1 Appendix Figures

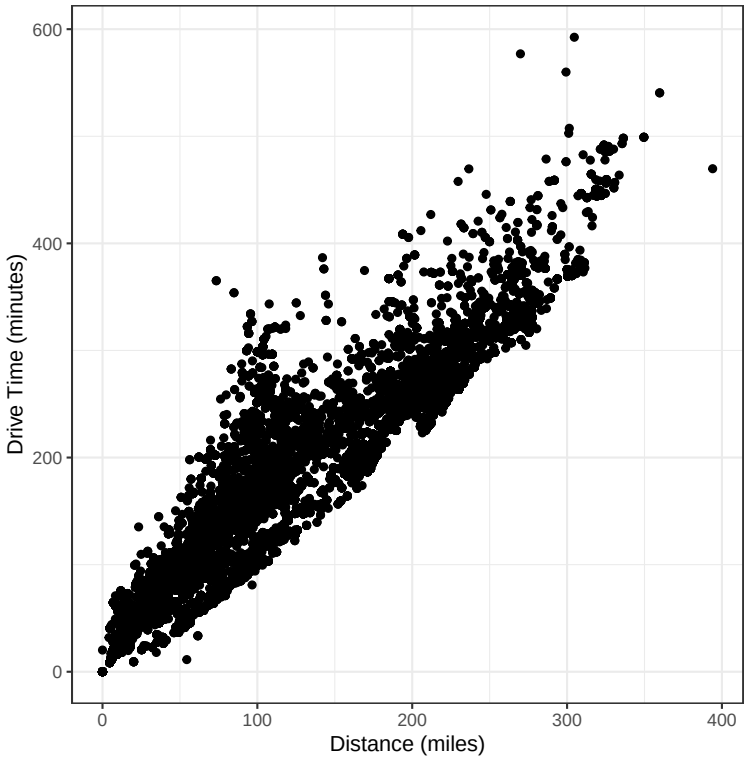


Figure A.1: Distance vs. Drive-Time

Notes: This figure plots the correlation between Euclidean distance drive-time. Drive times are calculated using Google Maps API. Distance is on the x-axis and drive-time is on the y-axis. Each dot represents how long the estimated drive-time is for the given distance.

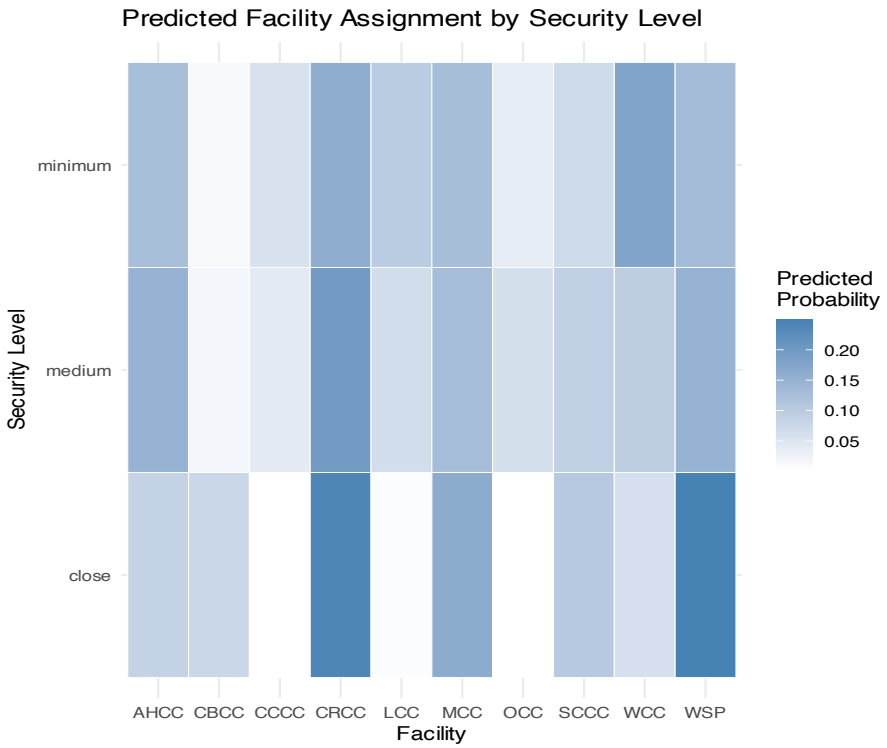


Figure A.2: Predicted Facility Placement by Security Level

Note: This figure illustrates the average predicted probability an individual is placed in each facility given the proxy security level I create. OCC, LCC, and CCCC are minimum only facilities. CBCC and WCC house only incarcerated individuals medium and above. AHCC and CRCC house minimum and medium. MCC, WSP, and SCCC house all three security levels. OCC and CCCC are the smallest facilities.

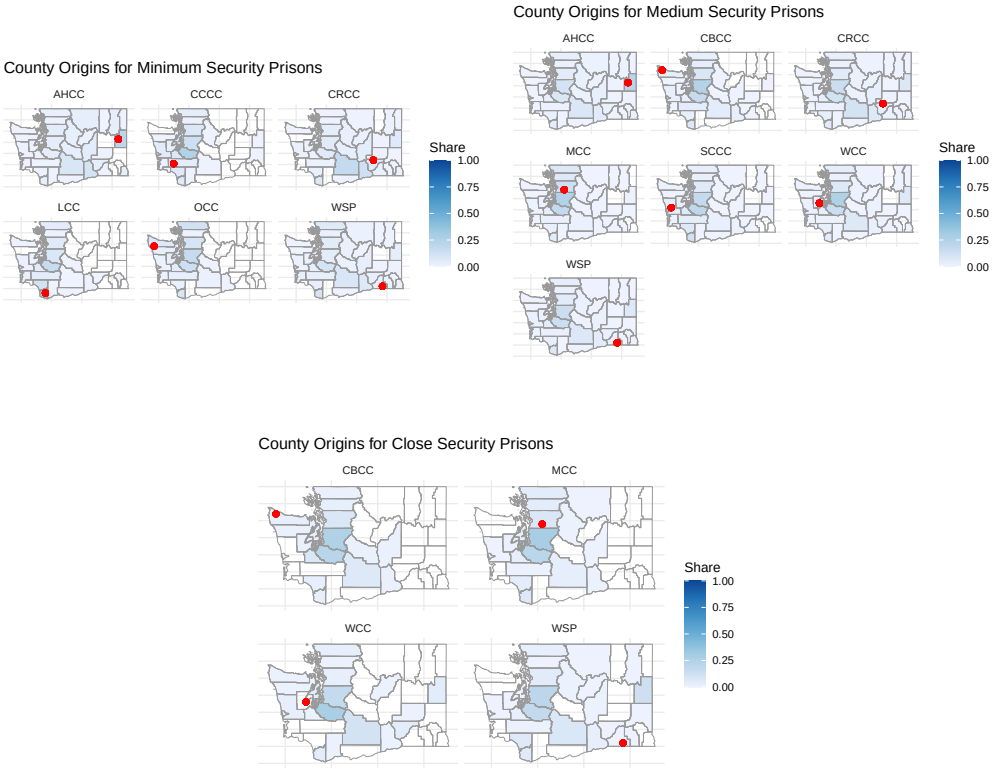


Figure A.3: Heatmap of Home County of Each Prison's Population

Note: This figure is a heatmap of the state of Washington's counties depicting the home counties of each prison population from 2010-2016. Each Washington map corresponds to one prison. Darker colors indicate a higher share of individuals placed in the given prison were from the indicated county.

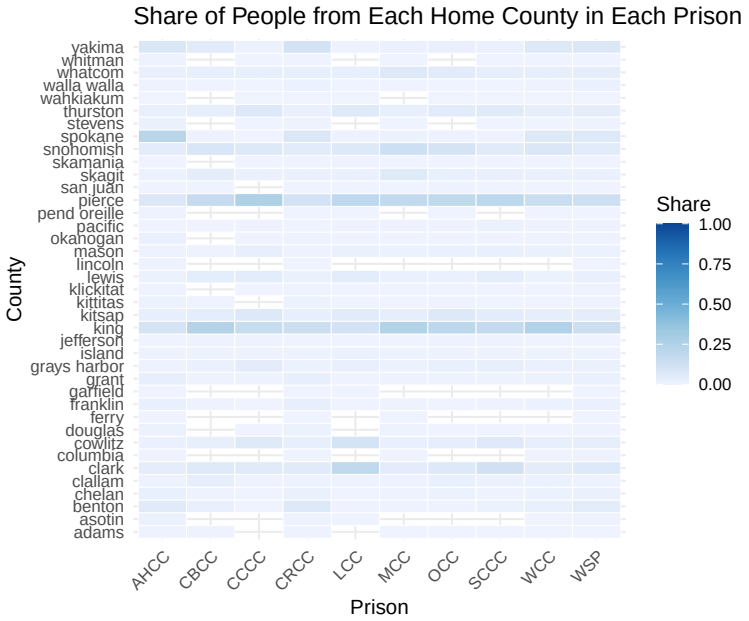


Figure A.4: Heatmap of Share of Each County in Each Prison's Population

Note: This figure is a heatmap showing the share of each county's incarcerated population that was sent to each facility. Each row is one county. Each column is one prison. The cells across a row sum to one. Darker colors indicate a higher share of the given county's incarcerated population was placed in the given prison.

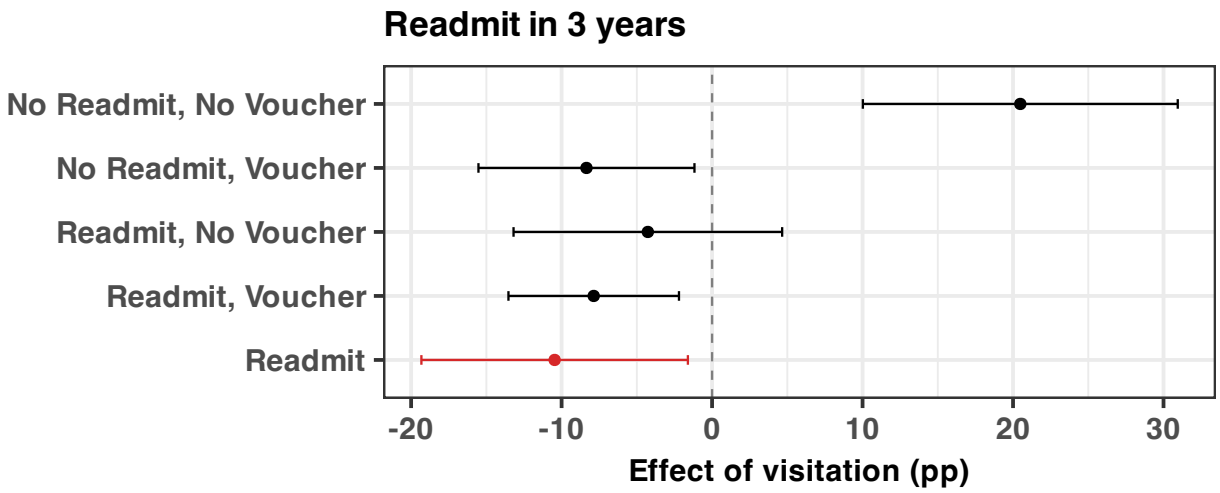


Figure A.5: Joint Housing and Recidivism Outcomes, Binary Visit Measure

Note: This figure plots the IV estimates of the effect of a binary indicator for receiving visitation on joint outcomes for housing instability and recidivism. Point estimates are indicated by dots and the lines indicate 95% confidence intervals. Each row (point estimate and line) represents a separate regression. Outcomes consists of four mutually exclusive and exhaustive categories of binary indicators for prison readmission within three years and receipt of a housing voucher. The red point and line is the marginal effect of visits on the probability of being readmitted in three years.

A.2 Appendix Tables

Table A.1: Supplementary Sample Descriptive Statistics

	Overall	Not visited	Visited
N People	585	295	293
N Spells	590	297	293
Earliest admit	2000-03-28	2000-03-28	2002-11-08
Latest admit	2019-11-01	2019-11-01	2019-07-19
Median days served	570	406	968
Median days sentenced	815	669	1247
Avg age at admit	37.23	40.05	34.36
% white	0.62	0.60	0.63
% Black	0.17	0.18	0.16
% Hispanic	0.10	0.10	0.10
% Native American	0.07	0.08	0.06
% Asian	0.04	0.04	0.04
% Drug crime	0.19	0.15	0.22
% Violent crime	0.22	0.16	0.28
% Property crime	0.42	0.42	0.41
% Sex crime	0.04	0.02	0.06
% Visited	0.50	0	1
Avg visits per month	0.26	0	0.52
Median placement distance	118.08	125.35	109.57
% Low risk	0.21	0.16	0.26
% Moderate risk	0.10	0.10	0.10
% High drug crime risk	0.07	0.07	0.07
% High property crime risk	0.16	0.19	0.14

Continued on next page

Table A.1 continued from previous page

	Overall	Not visited	Visited
% High violent crime risk	0.22	0.22	0.21
% High risk all crimes	0.24	0.26	0.22
% Mental health or substance abuse issue	0.88	0.92	0.83
% Married or with long-term partner	0.41	0.32	0.50
% with good family relationship	0.71	0.67	0.75
% with children	0.51	0.49	0.54
% with 1 child	0.21	0.19	0.24
% with 2 or more children	0.30	0.30	0.30
Avg monthly income in 6mo prior to admit			
No legal income	0.34	0.34	0.33
< $1,000	0.18	0.21	0.15
$1,000–1,999	0.22	0.25	0.20
$2,000–3,999	0.19	0.16	0.22
≥ $4,000	0.08	0.05	0.10

Notes: The sample is all men already incarcerated as of 2018 or who were incarcerated after 2018 and were placed in a standard facility without a life sentence. Data is from the first risk assessment done for each individual in a given spell. Risk score classifications come from Washington's classification system based on discrete cuts of continuous numeric scores for property crime, drug crime and violent crime re-offense risk. Scores are determined by the responses to interview questions, criminal history, and past incarceration behavior (if applicable). Relationship status, number of children, family relationships and substance abuse issues are self-reported. Mental health issues refer to a documented diagnosis. Average monthly income is self-reported for the last 6 months in the community prior to incarceration. "Under the table" income is considered illegal and is not counted.

Table A.2: Balance on Distance to Initial Placement

Panel A: Main Sample

Variable	Coefficient	Standard Error
Demographics		
Non-white	-0.7578	(0.8708)
Violent crime	-2.4832	(1.4055)
Property crime	-0.8260	(0.9354)
Age at admit	0.0415	(0.0393)
Programming Participation		
Parenting program	0.0011	(0.0010)
Education	0.0002	(0.0009)
Work crew	0.0003	(0.0014)
Prison job	0.0057	(0.0039)
Reentry planning	0.0025*	(0.0010)
Undisclosed	-0.0080	(0.0041)
Observations	24,338	

Panel B: Supplementary Sample

Variable	Coefficient	Standard Error
Demographics		
Non-white	0.4766	(0.6486)
Violent crime	0.6124	(0.6791)
Property crime	0.7953	(0.6447)
Age at admit	-0.0118	(0.0241)
Programming Participation		
Parenting program	-0.7462	(1.2018)
Education	0.6665	(0.8678)
Work crew	-1.9472	(1.0065)
Prison job	1.5469*	(0.6351)
Reentry planning	-1.9440	(1.1306)
Undisclosed	-0.4223	(0.6344)
Family and Risk		
Long-term partner	-0.2000	(0.6351)
Children	0.4550	(0.6295)
Good family relationship	0.4478	(0.6866)
Low-risk	-0.8620	(0.7038)
Moderate-risk	-0.7910	(0.9932)
High-risk	1.0037	(0.6300)
Observations	590	

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. Panel A presents statistics for the main sample. Each row is a regression of distance (in 1-mile units) on the given pre-determined characteristic or programming indicator, controlling for sentence length and first admit status, with fixed effects for admit year, placement facility, and home county. Panel B shows results for a supplementary sample incarcerated between 2018–2020 with intake records. Risk is WADOC's recidivism classification. High-risk combines high drug, property, violent, and diverse risk. No controls or fixed effects included in Panel B.

Table A.3: Descriptive Statistics by Housing Assistance Status

	Overall	No Voucher	Voucher
N	20199	16377	3822
Median Sent. Length (months)	20	20	20
Median Time Served (days)	362	364	353
Median Age at Admit	31	31	34
% with prior admit	0.49	0.50	0.54
% drug crime	0.25	0.25	0.26
% sex crime	0.05	0.04	0.11
% violent crime	0.17	0.17	0.16
% property crime	0.42	0.46	0.28
Median Placement Distance (miles)	101.77	100.55	110.14
% Visited	0.45	0.49	0.26
Mean N Visits/month	0.88	1.01	0.29
% readmitted in 1 year	0.09	0.08	0.11
% drugs	0.02	0.02	0.03
% sex crime	< 0.01	< 0.01	< 0.01
% violent crime	0.01	0.01	0.01
% property crime	0.05	0.05	0.05
% readmitted in 3 years	0.25	0.24	0.29
% drugs	0.06	0.05	0.06
% sex crime	< 0.01	< 0.01	0.01
% violent crime	0.02	0.02	0.02

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Table A.3 continued from previous page

	Overall	No Voucher	Voucher
% property crime	0.13	0.13	0.13

Notes: This table presents descriptive statistics for the sample split by housing voucher status. Voucher use data comes from Washington Department of Corrections records. Spells are categorized as receiving housing assistance if the incarcerated individual received a housing voucher in that spell. All statistics are at the incarcerated individual-spell level. Offense types are not mutually exclusive and are binary indicators for if any of the crimes associated with the current spell fall in the given category. Distance is calculated as the Euclidean distance from the geographic centroid of a prisoner's home ZIP code to the geographic centroid of their placement facility's ZIP code. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility. Six percent of spells in the main sample are missing housing voucher information and are excluded.

Table A.4: Descriptive Statistics of Voucher Recipients by Visitation Status

	Overall	Not Visited	Visited
N Spells	3822	2810	1012
% Voucher Receipt	1	0.74	0.26
Median sent. length (months)	19	18	25
Median time served (days)	353	311.5	510.5
Median age at admit	34	36	31
% with prior admit	0.54	0.57	0.48
% drug crime	0.26	0.27	0.22
% sex crime	0.11	0.08	0.18
% violent crime	0.16	0.15	0.21
% property crime	0.28	0.27	0.31
Median placement distance	110.14	115.49	96.61
% visited	0.26	0	1
Mean N Visits/month	0.29	0	1.09
% readmitted in 1 year	0.11	0.13	0.08
% drugs	0.03	0.03	0.02
% sex crime	< 0.01	< 0.01	0.01
% violent crime	0.01	0.01	0.01
% property crime	0.05	0.06	0.04
% readmitted in 3 years	0.29	0.31	0.24
% drugs	0.06	0.07	0.05
% sex crime	0.01	< 0.01	0.01
% violent crime	0.02	0.02	0.02

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Table A.4 continued from previous page

	Overall	Not Visited	Visited
% property crime	0.13	0.13	0.11

Notes: This table presents descriptive statistics for all visited individuals, split by housing voucher status. Spells are categorized as receiving housing assistance if the incarcerated individual received a housing voucher in that spell. All statistics are at the incarcerated individual-spell level. Offense types are not mutually exclusive and are binary indicators for if any of the crimes associated with the current spell fall in the given category. Distance is calculated as the Euclidean distance from the geographic centroid of a prisoner's home ZIP code to the geographic centroid of their placement facility's ZIP code. The sample includes all prison spells for visited men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

Table A.5: Supplementary Sample Descriptive Statistics by Housing Assistance Status

	Overall	No Voucher	Voucher
N	563	457	106
% Visited	0.49	0.53	0.32
% Low risk	0.17	0.17	0.18
% Moderate risk	0.09	0.09	0.11
% High drug crime risk	0.07	0.07	0.09
% High property crime risk	0.18	0.19	0.12
% High violent crime risk	0.23	0.23	0.22
% High risk all crimes	0.26	0.25	0.29
% Mental health or substance abuse issue	0.92	0.92	0.92
% Married or with long-term partner	0.41	0.44	0.31
% with good family relationship	0.72	0.74	0.60
% with children	0.52	0.53	0.47
% with 1 child	0.22	0.23	0.15
% with 2 or more children	0.30	0.29	0.32
Avg monthly income bucket in 6mo prior to admit			
No legal income	0.36	0.37	0.32
< $1,000	0.19	0.17	0.25
$1,000–1,999	0.21	0.20	0.23
$2,000–3,999	0.19	0.20	0.15

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Table A.5 continued from previous page

	Overall	No Voucher	Voucher
$\geq$ \$4,000	0.05	0.05	0.05

Notes: This table presents descriptive statistics for the supplementary sample split by housing voucher status. Voucher use data comes from Washington Department of Corrections records. Spells are categorized as receiving housing assistance if the incarcerated individual received a housing voucher in that spell. The sample is all men already incarcerated as of 2018 or who were incarcerated after 2018 and were placed in a standard facility without a life sentence. Data is from the first risk assessment done for each individual in a given spell. Risk score classifications come from Washington's classification system based on discrete cuts of continuous numeric scores for property crime, drug crime and violent crime re-offense risk. Scores are determined by the responses to interview questions, criminal history, and past incarceration behavior (if applicable). Relationship status, number of children, family relationships and substance abuse issues are self-reported. Mental health issues refer to a documented diagnosis. Average monthly income is self-reported for the last 6 months in the community prior to incarceration. "Under the table" income is considered illegal and is counted as no legal income. Ten percent of spells in the sample are missing housing voucher information and are excluded.

Table A.6: Supplementary Sample, Sub-group Use of Voucher

	% Voucher Use
Within Visited	0.12
Within Non-visited	0.25
Within Married/long-term partner	0.14
Within Non-Married/long-term partner	0.22
Within good family relationship	0.16
Within not good family relationship	0.26
Within parents	0.17
Within non-parents	0.21
Within avg monthly income bucket	
No legal income	0.17
$<$ \$1,000	0.25
\$1,000–1,999	0.21
\$2,000–3,999	0.15
$\geq$ \$4,000	0.17

Notes: This table presents what percent of each sub-group within the supplementary sample used a housing voucher. Voucher use data comes from Washington Department of Corrections records. Spells are categorized as receiving housing assistance if the incarcerated individual received a housing voucher in that spell. The sample is all men already incarcerated as of 2018 or who were incarcerated after 2018 and were placed in a standard facility without a life sentence. Data is from the first risk assessment done for each individual in a given spell. Relationship status, number of children, and family relationships are self-reported. Average monthly income is self-reported for the last 6 months in the community prior to incarceration. "Under the table" income is considered illegal and is counted as no legal income. Ten percent of spells in the sample are missing housing voucher information and are excluded.

Table A.7: Distance Threshold Effects on Recidivism

	Readmit in 1 year	Readmit in 3 years
	(1)	(2)
Distance > 30 miles	0.0078	0.0097
	(0.0063)	(0.0095)
Distance > 50 miles	0.0057	0.0086
	(0.0050)	(0.0073)
Distance > 60 miles	0.0081*	0.0093
	(0.0048)	(0.0070)
Distance > 70 miles	0.0087*	0.0102
	(0.0047)	(0.0069)
Distance > 80 miles	0.0109**	0.0164**
	(0.0046)	(0.0068)
Distance > 90 miles	0.0132***	0.0198***
	(0.0046)	(0.0068)
Controls	Yes	Yes
Fixed-effects	Yes	Yes
Observations	24,338	24,338

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors are clustered at the individual level. This table tables when distance starts to impact recidivism. Each cell is an estimate from a separate OLS regression. All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

Table A.8: 2SLS Distance Results, Seattle Metropolitan Area Excluded

	Readmit in 1 year	Readmit in 3 years
	(1)	(2)
Distance (100 miles)	0.0076**	0.0108**
	(0.0039)	(0.0055)
Controls	Yes	Yes
Fixed effects	Yes	Yes
Observations	14,718	14,718

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors are clustered at the individual level. This table tables when distance starts to impact recidivism. Each cell is an estimate from OLS regressions of a binary indicator for prison readmittance on binary indicators for whether prison distance is greater than indicated. Each cell is a separate regression. All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

Table A.9: First Stage of Distance and Visits, Logit and Poisson

	Logit: Visit Received	Poisson: N Visits per Mo.
	(1)	(2)
Distance (100 miles)	−0.5429***	−0.7831
	(0.0269)	(0.0354)
Marginal Effect	−0.1125	−0.5430
Controls	Yes	Yes
Fixed-effects	Yes	Yes
Observations	24,413	24,413

Notes: ***: 0.01, **: 0.05, *: 0.1. This table presents first stage results for the effect of distance on visitation using alternative standard error clusters. Visitation is measured as a binary indicator in Column (1) and estimation is done using logistical regression. Visitation is measured as the count of visits per month incarcerated in Column (2) and estimation is done using a Poisson regression. The marginal effect for Column (1) is the average marginal effect. For a continuous regressor x_k: $AME = \frac{1}{n}\sum_i \beta_k p_i(1-p_i)$ where $p_i = \Lambda(x_i'\beta)$. The marginal effect for Column (2) is calculated as $e^{\beta} - 1$. All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

Table A.10: The Effect of Distance Rank on Visitation

	Visit Received	Visit Count (per Month)
	(1)	(2)
Second Closest Facility	-0.0670***	-0.5913***
	(0.0116)	(0.0697)
Third Closest Facility	-0.0781***	-0.6912***
	(0.0123)	(0.0700)

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Table A.10 continued from previous page

	Visit Received	Visit Count (per Month)
	(1)	(2)
Fourth Closest Facility	-0.1104***	-1.062***
	(0.0173)	(0.0646)
Fifth Closest Facility	-0.1783***	-1.195***
	(0.0116)	(0.0609)
Sixth Closest Facility	-0.2170***	-1.300***
	(0.0132)	(0.0585)
Seventh Closest Facility	-0.2293***	-1.329***
	(0.0146)	(0.0628)
Controls	Yes	Yes
Fixed-effects	Yes	Yes

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Table A.10 continued from previous page

	Visit Received	Visit Count (per Month)
	(1)	(2)
Observations	16,371	16,371
F-stat	83.30	107.10

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard-errors clustered at the individual level are in parentheses. Estimates are from OLS regressions of visitation on indicators for an individual's placement facility's distance rank. Each individual has a set of facilities they can be placed in, which are ranked from closest to furthest. This is the facility distance rank. The omitted category is the first closest facility. The outcome variable for Column (1) is a binary indicator for whether a prisoner received any visits. The outcome variable for Column (2) is the count of visits received in a given spell normalized by the number of in-prison months for a given prisoner-spell. Controls for the individual's race, the individual's age at the time of prison admission, the length of the sentence in months, indicators the type of crime the individual was convicted for in the given spell, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility are included. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code. The sample is all men predicted to be medium security level using sentence length and history of prison admissions.

Table A.11: First Stage of Distance and Visits, Alternate SE Clusters

	Visit Received		Visit Count (per Month)	
	ZIP	County	ZIP	County
	(1)	(2)	(3)	(4)
Distance (100 miles)	-0.1071***	-0.1071***	-0.5963***	-0.5963***
	(0.0057)	(0.0068)	(0.0355)	(0.0431)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	24,413	24,413	24,413	24,413
F-test	443.07	570.58	443.76	571.48

Notes: ***: 0.01, **: 0.05, *: 0.1. This table presents first stage results for the effect of distance on visitation using alternative standard error clusters. Visitation is measured as a binary indicator in Columns (1) and (2). Visitation is measured as the count of visits per month incarcerated in Columns (3) and (4). In Columns (1) and (3) standard-errors are clustered at the ZIP-code level and are in parentheses. In Columns (2) and (4) standard-errors are clustered at the county level and are in parentheses. Counties are the relevant jurisdictions. All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

Table A.12: 2SLS Visitation Results, Alternate SE Clusters

	Readmit in 1 year		Readmit in 3 years	
	(1)	(2)	(3)	(4)
Visit Received	-0.0834**		-0.1107**	
	(0.0343)		(0.0474)	
	[0.0321]		[0.0508]	
Visit Count per Mo.		-0.0150**		-0.0199**
		(0.0060)		(0.0084)
		[0.0057]		[0.0094]
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	24,413	24,413	24,413	24,413

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors in parentheses are clustered at the ZIP-code level and are in parentheses. Standard errors in brackets are clustered at the county level and are in parentheses. Counties are the relevant sentencing jurisdiction. Visitation is measured as a binary indicator in Columns (1) and (3). Visitation is measured as the count of visits per month incarcerated in Columns (2) and (4). All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code.

Table A.13: 2SLS Visitation Results, Incarcerated Individuals Only in One Facility

	Readmit in 1 year		Readmit in 3 years	
	(1)	(2)	(3)	(4)
Visit Received	-0.0597		-0.0782	
	(0.0398)		(0.0552)	
N Visits per Mo.		-0.0107		-0.0140
		(0.0071)		(0.0098)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	13,819	13,819	13,819	13,819
F-test (1st stage)	320.07	366.13	320.07	366.13

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors are clustered at the individual level and are in parentheses. Visitation is measured as a binary indicator in Columns (1) and (3). Visitation is measured as the count of visits per month incarcerated in Columns (2) and (4). All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

Table A.14: 2SLS Visitation Results, More Than 1 Visit

	Readmit in 1 year		Readmit in 3 years	
	(1)	(2)	(3)	(4)
Visit Received	-0.0944***		-0.1389***	
	(0.0321)		(0.0444)	
N Visits per Mo.		-0.0174***		-0.0256***
		(0.0059)		(0.0082)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	24,413	24,413	24,413	24,413
F-test (1st stage)		432.30		448.33

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. Anyone who only received 1 visit is recoded as being not visited and their visit count is set to 0. Visitation is measured as a binary indicator in Columns (1) and (3). Visitation is measured as the count of visits per month incarcerated in Columns (2) and (4). All models include controls for sentence length, crime type, and race, as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

Table A.15: 2SLS Visitation Results, Seattle Metropolitan Area Excluded

	Readmit in 1 year		Readmit in 3 years	
	(1)	(2)	(3)	(4)
Visit Received	-0.0681**		-0.0970**	
	(0.0347)		(0.0492)	
Visit Count per Mo.		-0.0131**		-0.0186**
		(0.0067)		(0.0094)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	14,718	14,718	14,718	14,718
F-stat (1st stage)	448.4	591.1	448.4	591.1

Note: ***: 0.01, **: 0.05, *: 0.1. Standard-errors clustered at the individual level are in parentheses. Visitation is measured as a binary indicator in Columns (1) and (3). Visitation is measured as the count of visits per month incarcerated in Columns (2) and (4).All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted on or after January 1, 2010 to Washington state prisons excluding those with a home county of King County, Snohomish County or Pierce County.

Table A.16: 2SLS Visitation Results, Airway Heights Corrections Center Excluded

	Readmit in 1 year		Readmit in 3 years	
	(1)	(2)	(3)	(4)
Visit Received	-0.0910**		-0.1377**	
	(0.0402)		(0.0571)	
Visit Count (per Mo.)		-0.0177**		-0.0268**
		(0.0078)		(0.0111)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	20,855	20,855	20,855	120,855
F-stat (1st stage)	310.8	378.2	310.8	378.2

Note: ***: 0.01, **: 0.05, *: 0.1. Standard-errors clustered at the individual level are in parentheses. Visitation is measured as a binary indicator in Columns (1) and (3). Visitation is measured as the count of visits per month incarcerated in Columns (2) and (4). All models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted on or after January 1, 2010 to Washington state prisons excluding those placed in Airway Heights Correctional Center.

Table A.17: The Effect of Visitation on Recidivism: Binary and Continuous Instrument

	Readmit in 1 year		Readmit in 3 years	
	Main Results	$>$ 99 miles	Main Results	$>$ 99 miles
	(1)	(2)	(3)	(4)
Visit Count (per mo.)	-0.0141**	-0.0120*	-0.0187**	-0.0241**
	(0.0057)	(0.0068)	(0.0081)	(0.0100)
Visit Received	-0.0781**	-0.0519	-0.1036**	-0.0988*
	(0.0318)	(0.0358)	(0.0451)	(0.0524)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	24,338	24,338	24,338	24,338

Note: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table compares estimates from the main specification that uses a continuous measure of distance as the instrument with a specification that instead uses a binary indicator for placement above or below the median placement distance. The outcome variable for Columns (1) and (2) is a binary indicator for whether an incarcerated individual is re-admitted within 1 year of release, and the outcome variable for Columns (3) and (4) is a binary indicator for re-admit within 3 years of release for the given spell. All specifications include controls of the individual's race, spell length, offense type, and previous prison admissions as well as fixed effects for admit year, placement facility, and home county. The untreated mean is the rate of recidivism of non-visited incarcerated individuals. The median placement distance is 99 miles from home. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

Table A.18: The Effect of Visitation on Recidivism, EFV Recipients Removed

	Readmit in 1 year		Readmit in 3 years	
	(1)	(2)	(3)	(4)
Visit Received	-0.0827**		-0.1165**	
	(0.0324)		(0.0460)	
Visit Count (per Month)		-0.0155**		-0.0219**
		(0.0061)		(0.0086)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	24,046	24,046	24,046	24,046

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. The outcome variable for Columns (1) and (2) is a binary indicator for whether an incarcerated individual is re-admitted within 1 year of release, and the outcome variable for Columns (3) and (4) is a binary indicator for re-admit within 3 years of release for the given spell. All specifications include controls of the individual's race, spell length, offense type, and previous prison admissions as well as fixed effects for admit year, placement facility, and home county. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility. An incarcerated individual received at least one Extended Family Visit in 292 spells and those are dropped.

Table A.19: The Effect of Visitation on Release from Lower Security Facility

	Release from Lower Security		Release from Lower or Same Security	
	(1)	(2)	(3)	(4)
Visit Received	0.1150**		0.2142***	
	(0.0376)		(0.0317)	
Visit Count (per Month)		0.0209**		0.0389***
		(0.0069)		(0.0057)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Untreated mean	0.20	0.86	0.20	0.86
Observations	24,046	24,046	24,046	24,046

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. The outcome variable for Column (1) is a binary indicator for whether an individual's release facility was a lower security level than their placement facility. An individual is classified as being released from a lower security facility if they were either released from work-release or if an individual was admitted to a facility that houses medium and close security levels and was released from a facility that houses minimum only or minimum and medium security levels. The outcome variable for Column (2) is a binary indicator for whether an individual's release facility was a lower or same security level than their placement facility. An individual is classified as being released from a lower or same security facility if they were classified as lower in Column (1) or their release facility houses the same security levels as their placement facility. All specifications include controls of the individual's race, spell length, offense type, and previous prison admissions as well as fixed effects for admit year, placement facility, and home county. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

Table A.20: The Effect of Visitation on Housing Stability

	Housing Voucher Used	
Visit Count (per month)	-0.0305***	
	(0.0080)	
Visit Received		-0.1618***
		(0.0424)
Controls	Yes	Yes
Fixed-effects	Yes	Yes
Observations	20,038	20,038
Non-visited Voucher Use Rate	0.25	0.25

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents 2SLS estimates of the effect of visitation on housing voucher use. The outcome variable is a binary indicator for whether an incarcerated individual's required a housing voucher in the given spell. Both models include controls for sentence length, crime type, incarcerated individual race, and criminal history as well as fixed effects for admit year, placement facility, and home county. The sample consists of all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016 who were placed in a standard facility and had a non-missing home ZIP code. Six percent of spells are missing information on post-release housing and are excluded.

Table A.21: Joint Housing and Prison Readmission Outcomes

	Readmit, Voucher (1)	Readmit, No Voucher (2)	Voucher, No Readmit (3)	No Readmit, No Voucher (4)
Readmit in 1 year				
Visit Count (per Mo.)	-0.0067*	-0.0107*	-0.0237***	0.0411***
	(0.0036)	(0.0057)	(0.0076)	(0.0090)
Visit Received	-0.0356*	-0.0569*	-0.1265***	0.2190***
	(0.0194)	(0.0304)	(0.0405)	(0.0478)
Non-visited outcome mean	0.03	0.07	0.22	0.67
Readmit in 3 years				
Visit Count (per Mo.)	-0.0148***	-0.0080	-0.0157**	0.0384***
	(0.0054)	(0.0085)	(0.0069)	(0.0100)
Visit Received	-0.0787***	-0.0427	-0.0835**	0.2048***
	(0.0289)	(0.0455)	(0.0366)	(0.0534)
Non-visited outcome mean	0.08	0.17	0.20	0.55
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	19,953	19,953	19,953	19,953

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents 2SLS estimates of the effect of visits on joint housing voucher use and prison readmission within 1 and 3 years of release. Controls include race, age at admission, sentence length, crime type indicators, and an indicator for first prison spell, along with fixed effects for admission year, home county, and facility. The untreated mean is the outcome for non-visited individuals.

Table A.22: Counterfactual Placement Simulations

	Mean Δ_{Dist}	Mean Δ_{visit}	Mean $\Delta_{Readmit1}$	Mean $\Delta_{Readmit3}$	Mean Δ_{Days}
No Constraints	−82 miles	0.087	−0.007	−0.010	−12 days
Security & Capacity Constraints	−40 miles	0.040	−0.003	−0.004	−4 days
+ Age Priority	−31 miles	0.030	−0.004	−0.005	−5 days

Notes: Mean Δ_{dist} is the average change in placement distance from the placement simulation. A negative number indicates individuals were placed closer to home. Mean Δ_{visit} is the change in the probability of visitation given the change in distance, estimated as Mean $\Delta_{dist} \times \alpha_1$ from Equation 3 (first stage). Mean $\Delta_{Readmit1}$ is the change in the probability of readmission within 1 year, estimated as Mean $\Delta_{visit} \times \gamma_1$ from Equation 2 (2SLS). Mean $\Delta_{Readmit3}$ is the change in the probability of readmission within 3 years, estimated analogously. Mean Δ_{Days} is the change in total days served in prison within 5 years post-sentencing.

A.3 Heterogeneity and Non-Linearities

A.3.1 Non-linearties in Visitation

The main analysis assumes that visitation has a linear effect on recidivism—that is, the difference between receiving 0 and 1 visit is assumed to have the same impact as the difference between 30 and 31 visits. If the effect of visitation instead exhibits diminishing or increasing returns, this linearity assumption would be violated. In Table A.23, I explore several specifications that allow for nonlinear treatment effects.

Column (1) replicates the baseline linear specification, using the count of visits per month instrumented by distance, and shows a statistically significant negative effect of visitation on recidivism. Column (2) adds a squared term for visit count and instruments using both distance and distance squared to capture possible curvature. While the coefficient on the squared term is positive, suggesting possible diminishing returns, the estimate is imprecise and not statistically significant. Column (3) includes both the continuous visit count and an indicator for any visitation, attempting to separate extensive and intensive margin effects. Here too, neither estimate is significant, and standard errors are large. Finally, Column (4) splits the visit rate into discrete buckets: no visits (reference), low visitation (below the median among those visited), and high visitation (above the median). While the effect of low visitation is imprecisely estimated, the effect of higher visitation is large and statistically significant, indicating that sustained or repeated contact may be especially important.

Overall, these results suggest some evidence of nonlinearity, particularly that the extensive margin of visitation may matter more. However, the estimates across specifications are generally noisy, and wide confidence intervals reflect the limited power to detect precise nonlinear effects. This imprecision cautions against strong conclusions but does not rule out meaningful nonlinear dynamics in the effect of visitation on recidivism.

Table A.23: Nonlinear Visitation Effects

	Readmit in 3 years			
	(1)	(2)	(3)	(4)
Visit Count (per Mo.)	-0.0199**	-0.1234	0.0128	
	(0.0082)	(0.0953)	(0.0309)	
Visit Count^2		0.0096		
		(0.0090)		
Visit Received			-0.2152	
			(0.1901)	
$0 < visits <= med.$				-0.2725
				(0.3285)
$visits > med.$				-0.1486***
				(0.0515)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
Observations	24,413	24,413	24,413	24,413

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. The outcome variable is a binary indicator for whether an incarcerated individual is readmitted to prison within 3 years of release. The complete set of controls described in Section 3 are included as well as fixed effects for admit year, placement facility, and home county. Column (1) uses only distance as an instrument while Columns (2)-(4) use distance and distance squared. Column (4) splits visitation into 0 visits, 0 to median, and above median. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

A.3.2 Heterogeneity by observable characteristics

Table A.24 presents results for the sample split below and above the median age at admit of 33, by whether the individual's current conviction is for a violent crime, and by race. Effects are noisy, but suggestive that older individuals, those convicted of non-violent crime, and non-white individuals benefit the most from visitation.

Table A.24: Treatment Effect Heterogeneity by Observable Characteristics

	Readmit in 3 Years					
	Age		Crime Type		Race	
	young	old	violent	non-violent	white	non-white
	(1)	(2)	(3)	(4)	(5)	(6)
Visit Received	-0.1053*	-0.1603**	-0.1565	-0.1445***	-0.0977*	-0.2833***
	(0.0603)	(0.0699)	(0.0958)	(0.0504)	(0.0524)	(0.0906)
Visit Count (per Mo.)	-0.0178*	-0.0315**	-0.0174*	-0.0298***	-0.0181*	-0.0475***
	(0.0102)	(0.0137)	(0.0105)	(0.0104)	(0.0097)	(0.0152)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,353	11,985	4,063	20,275	15,212	9,126
Untreated mean	0.33	0.25	0.16	0.29	0.29	0.27

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents 2SLS estimates of the effect of visitation on readmission to prison within 3 years of release for splits of the sample. Visit count (per mo.) is the count of visits received in a given spell normalized by the number of in-prison months for a given prisoner-spell. The outcome variable is a binary indicator for whether an incarcerated individual is readmitted to prison within 3 years of release. The young sub-sample is men who were age 33 or young at time of admission, and the old sub-sample is men who were age 34 or older at the time of admissions. The violent sample consists of individuals convicted of a violent crime in the given spell. The non-violent sample consists of individuals who were only convicted for non-violent crimes in the given spell. Controls for the individual's race, the individual's age at the time of prison admission, the length of the sentence in months, indicators the type of crime the individual was convicted for in the given spell, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility are included. The untreated mean is the readmit rate for non-visited incarcerated men in the relevant sample.

A.3.3 Subsequent Crime

Instead of examining whether an individual is readmitted to prison at all, I now explore whether visitation affects the type of offense for which an individual is readmitted. Specifically, I estimate separate regressions using binary indicators for whether a person is read-

mitted for a violent, drug, sex, or property crime. Property crimes are the most common: about 10 percent of the full sample is readmitted for a property crime, and among those who are reincarcerated, 36 percent committed a property offense. Drug crimes follow, with a 6 percent overall readmission rate and comprising 23 percent of recidivism events. Violent and sex crime readmissions are far less common.

Results for the effect of visitation on subsequent crime are shown in Table A.25. Columns (1), (3), (5), and (7) show the unconditional effects, while columns (2), (4), (6), and (8) include controls for the individual's current offense type. The results show that visitation has no statistically significant effect on violent crime recidivism, even after controlling for offense type. For drug crime, visitation reduces the probability of recidivism by about 4.8 percentage points (column 4). For sex offenses, the effect of visitation is small but negative. The largest and most robust effect is for property crime: visitation reduces the likelihood of being readmitted for a property crime by approximately 10 percentage points, a substantial and highly statistically significant effect, even after adjusting for current offense type. Small sample sizes for violent crimes and sex crimes make it difficult to detect results for these crimes, but results are suggestive that visitation may be particularly effective at reducing recidivism related to property and drug crimes, which are often economically motivated and potentially more responsive to social and logistical support.

A.3.4 Visitors

Incarcerated individuals receive visits from parents, spouses, friends, children, and other relatives. Up to this point I have treated "visit" as a single object, but the visitor's relationship to the incarcerated person may matter in its impact on recidivism. To asses this, I next split visitation up by relationship of the visitor and investigate heterogeneous effects by visitor type.

Table A.25: Treatment Effect Heterogeneity by New Crime Type

	Readmit in 3 years							
	New Violent Crime		New Drug Crime		New Sex Crime		New Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Visit Received	0.0042	0.0013	-0.0385	-0.0476*	-0.0122*	-0.0120*	-0.1083***	-0.1019***
	(0.0122)	(0.0122)	(0.0279)	(0.0276)	(0.0063)	(0.0063)	(0.0323)	(0.0310)
Current drug offense		-0.0011		0.0985***		-0.0015**		-0.0163***
		(0.0017)		(0.0048)		(0.0007)		(0.0042)
Current violent offense		0.0539***		-0.0294***		0.0006		-0.0777***
		(0.0052)		(0.0045)		(0.0017)		(0.0071)
Current sex offense		-0.0040		-0.0244***		0.0444***		-0.0445***
		(0.0036)		(0.0040)		(0.0061)		(0.0046)
Current property crime		0.0043**		0.0227***		-0.0023***		0.1732***
		(0.0019)		(0.0037)		(0.0007)		(0.0056)
Current Crime	No	Yes	No	Yes	No	Yes	No	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,413	24,413	24,413	24,413	24,413	24,413	24,413	24,413
Untreated mean	0.02	0.02	0.07	0.07	0.004	0.004	0.10	0.10

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents 2SLS regression results for the effect of visitation on subsequent crime. The outcome variable is a binary indicator for whether an incarcerated individual is readmitted to prison within 3 years of release for the given crime type. Columns (2), (4), (6), and (8) add controls for the current conviction. Controls for the individual's race, age at admission, sentence length, and an indicator for first prison spell are included, along with fixed effects for admission year, home county, and facility. The untreated mean is the readmit rate for the given crime type among non-visited individuals.

I first examine whether the composition of visitors changes across distance. In other words, does who visits change as individuals are placed farther from home? Figure A.6 shows that visitor types remain remarkably stable across distance. While the likelihood of receiving a visit declines, it declines proportionally for all visitor types. Further, among those who are visited the mix of visitors (parents, spouses, children, friends, and other family members) changes little as distance increases.[1] However, visitor composition does change across the length of the spell. Figure **??** shows that visitor types remains stable until the last 3 months of the spell: in the last 3 months of the spell extended family members drop out and parents make up a larger share of the visitor pool.

1. There is a drop-off in spousal visits at the very tail end of distance, but as spousal visits are relatively rare this is likely noise.

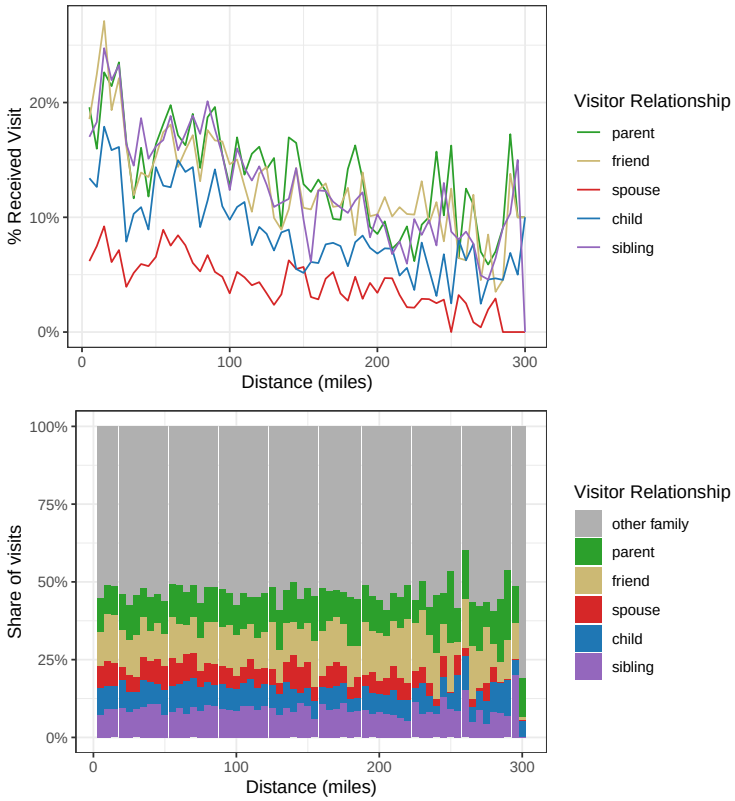


Figure A.6: Visitor Composition by Distance

Notes: This figure presents the composition of visitors by their relationship to the incarcerated individual across distance. The left panel displays the percent of individuals who receive at least one visit from the given visitor type. The right panel displays the total composition of visitor types within visited individuals. *Other family* includes grandparents, cousins, uncles, aunts, and in-laws.

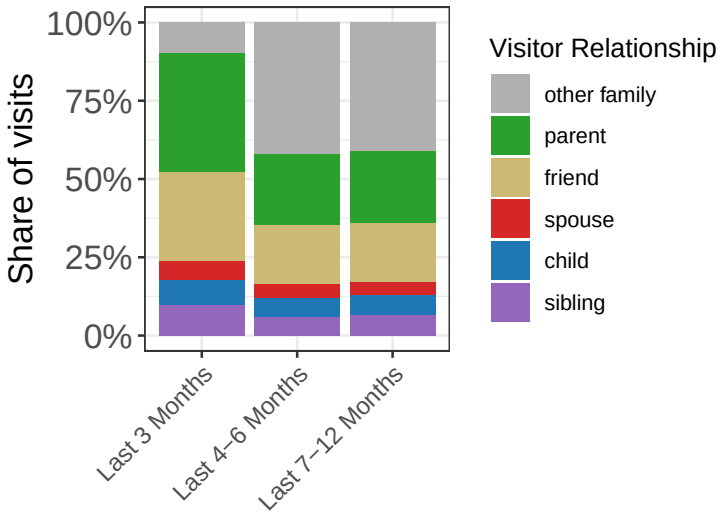


Figure A.7: Visitor Composition Over Spell

Notes: This figure presents the composition of visitors by their relationship to the incarcerated individual across the spell. The sample is limited to individuals who served at least 14 months and received at least 1 visit in the last year of the spell. Visits are grouped into mutually exclusive timing groups. *Other family* includes grandparents, cousins, uncles, aunts, and in-laws.

One avenue to investigate the differential effects of visitor-type would be to look at individuals who only ever receive visits from one type of visitor. However, nearly 70 percent of visited individuals receive visits from a mix of types of visitors, whether it be their parents and their spouse or friends, a grandparent and their children, or many other combination.[2] Therefore, I instead divide the visited population into groups based on the modal relationship of their visitors: (i) children or spouse, (ii) parents, and (iii) friends or other family.[3]

To assess the causal impact of visitor relationship, I decompose the overall effect of distance on visitation and recidivism into sub-effects defined by the dominant type of visitor. The main results estimate the LATE of distance, $LATE_{dist}$. This can be decomposed into

2. See Appendix Table A.26.

3. Appendix Table A.27 reports descriptive statistics for each visitor-group.

a weighted average of sub-LATEs measuring the effects of visitation for individuals with different visitor compositions.[4]

$$LATE_{dist} = S_{child,spouse} LATE_{child,spouse} + S_{parent} LATE_{parent} + S_{friend,other} LATE_{friend,other}$$

where $LATE_{relationship}$ is the average treatment effect for compliers who move from receiving no visits to receiving visits from mostly visitor type $relationship \in \{child/spouse, parent, friend/ot$ when they are placed closer to home. The weights $S_{relationship}$ give the fraction of compliers from each visitor-group.

It is important to clarify how these visitor groups should be interpreted. Individuals have predetermined sets of potential visitors (parents, spouses, children, friends), and distance shifts whether those visitors are able to visit, not which type of visitor they have. The estimated effects for each visitor group therefore capture the causal impact of visitation for the subset of compliers whose marginal visits come from that relationship type, rather than substitution between visitor types.

To estimate these sub-LATEs, I decompose visitation into separate endogenous variables for each visitor group. To generate instruments, I interact covariates with distance to create multiple instruments. I create quartiles of both sentence length and age and interact those with quartiles of distance. This approach requires a constant effects assumption (see klinewalters). This rules out the possibility that, for example, spouse visits reduce recidivism more for older individuals than for younger ones, or that parent visits have larger effects for those with shorter sentences than for those with longer ones. The visitor-type sub-LATEs are presented in Table A.28. The estimates indicate that compliers whose marginal visitors are spouses or children may benefit the most from visitation. However, these estimates should be interpreted with caution given the relative weakness of the instruments and the noisiness

4. This decomposition follows the approach in klinewalters.

of the effects.

Table A.26: Visits from Only One Visitor Type

Visitor Relationship	N	Percent	Visitor Relationship	N	Percent
Only Children	53	< 0.01	Most Children	670	0.06
Only Spouse	225	0.02	Most Spouse	702	0.06
Only Friends	1,777	0.16	Most Friends	3,577	0.33
Only Parents	1,497	0.14	Most Parents	4,839	0.45
Mix or Other	7,286	0.67	Most Other Family	1,059	0.10
Total	10,775	1	Total	10,775	1

Notes: This table presents counts and percentages of incarcerated individuals who only receive visits from one type of visitor as well as by dominant visitor groups. The visited sample is split into groups by their visitors relationships, which is determined by the relationship of their visitor recorded at time of visit. The left panel presents the count and percent of visited individuals who only ever received visits from one type of visitor. The right panel splits the group by their dominant visitor-type.

Table A.27: Descriptive Statistics by Dominant-Visitor Group

Most Visits from:	Child	Spouse	Parents	Friend	Other Family	No Visits
N	667	696	4,800	3,558	1,054	13,563
Age at Admit	36.93	36.65	29.25	32.30	32.42	35.06
Sentence Length (mos.)	24	25	22	22	22	17
% with prior admit	0.45	0.55	0.37	0.52	0.46	0.51
% violent offense	0.22	0.21	0.23	0.20	0.20	0.13
% drug offense	0.28	0.26	0.24	0.25	0.23	0.26
% sex offense	0.04	0.09	0.06	0.05	0.05	0.04
% property offense	0.35	0.37	0.48	0.39	0.44	0.40
% readmitted within 1 year	0.04	0.03	0.08	0.06	0.09	0.10
% readmitted within 3 years	0.16	0.15	0.24	0.22	0.25	0.28

Notes: This table presents descriptive statistics for the sample split by their dominant visitor. The sample is split into groups by their dominant visitor type, which is determined by the relationship of their visitor recorded at time of visit. "No Visits" is the group that did not receive any visits in the given spell. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

Table A.28: Visitation Effects by Visitor Type

	Readmit in 1 year		Readmit in 3 years	
	Dist × Age	Dist × Sent.	Dist × Age	Dist × Sent.
	(1)	(2)	(3)	(4)
Most Visits: spouse or child	-0.2963***	-0.4244	-0.4003***	-0.1615
	(0.1111)	(0.2602)	(0.1554)	(0.3808)
Most Visits: parents	-0.2107**	0.0136	-0.2163	0.1272
	(0.1000)	(0.1837)	(0.1399)	(0.2721)
Most Visits: other family or friend	0.2206	0.0580	0.1575	-0.1962
	(0.1368)	(0.1713)	(0.1929)	(0.2587)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
F-stat (spouse, child)	12.70	6.04	12.70	6.04
F-stat (parents)	11.00	11.4	11.00	11.4
F-stat (other)	7.05	9.77	7.05	9.77
Observations	24,413	24,413	24,413	24,413

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. The outcome variable for Columns (1) and (2) is a binary indicator for whether an incarcerated individual is readmitted to prison within 1 year of release. The outcome variable for Columns (3) and (4) is a binary indicator for whether an incarcerated individual is readmitted to prison within 3 years of release. The complete set of controls described in Section 3 are included as well as fixed effects for admit year, placement facility, and home county. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

A.4 Extended Family Visits

Washington state is one of the four states, along with California, New York and Connecticut, that offer multi-day private visitation with immediate family members. In Washington this

program is called Extended Family Visits (EFV). Visits take place in trailer units set up on prison facilities but outside on the main buildings and are unsupervised. Eligibility is limited to visitors who are immediate family (spouses, children, parents), and the relationship must be verified with legal documentation such as birth certificates and marriage licenses. Visitors are also required to maintain regular visitation schedules with the incarcerated individual in order to be eligible for EFV participation. Visitors must bring all food they wish to consume or cook during the visit and both visitors and all their belongings are screened and searched upon entry and exit.

Due to the stringent requirements, there is relatively little program take-up. Less than 1 percent of the sample (only 290 individuals) who receive an EFV in my sample. The selectivity of participation makes distance a weak instrument for EFV participation and for regular visitation within the EFV recipient pool. Table A.29 presents the first stage effects of distance on EFV participation. Effects for visitation within EFV participants cannot be estimated as all EFV recipients also received regular visitation. Although point estimates are statistically significant, F-tests reveal a weak relationship between distance and EFV participation.

Anecdotal evidence from WADOC staff suggests that EFV participants are highly motivated and extremely committed to their incarcerated loved one. Combined with their relative insensitivity to travel costs, this makes EFV recipients conceptually similar to"always-takers" (those who receive visit regardless of distance). abadie2003 shows how to directly identify and characterize always-takers, compliers and never-takers in settings with a binary treatment and binary instrument and hull shows how to describe the complier population with a continuous treatment, but I can only do this in my main sample, which does not contain the particularly salient characteristics such as family support, self-reported income and risk. I therefore use EFV recipients as a proxy for always-takers to distinguish them from compliers

within the visited population.[5]

Table A.30 displays descriptive statistics for the incarcerated individuals who receive EFVs relative to those who only receive regular visitation and relative to those who do not receive any visitation. Unsurprisingly given both the program requirements of regular visitation and the hypothesized strong family ties, EFV recipients receive almost 2.5 times more visits per month of incarceration than those who only receive receive standard visitation. EFV recipients also have much longer spells, are more likely to have committed a violent crime, are older and notably, have much lower rates of recidivism than those who receive standard visitation.

Table A.31 reports descriptive statistics for EFV recipients in the supplementary sample. Although the group is small, EFV recipients clearly differ from individuals who receive standard visitation. EFV recipients are much more likely to be rated low risk of recidivism, to report being in a long-term relationship, to describe their family relationships as good, and to report having children. They are also less likely to have a documented mental health diagnosis. Notably, EFV recipients are also disproportionately in the highest income bracket, which is consistent with the time and financial resources required for visitation and especially for multi-day visitation (taking several days off work and travel costs). It is unsurprising that EFV receipt is correlated with a low risk score because the WADOC risk score incorporates interview information on stability and social support. The positive correlation with family is also expected.

This pattern matters because positive selection into visitation among those with strong support is the standard critique of correlational studies – the estimated benefits of visitation might reflect preexisting support rather than the effect of visitation itself. While visited-no-EFV recipients are more positively selected than never visited people, they are much less so than the EFV recipients. This is important because always-takers do not receive weight in a

5. This is not to say that EFVs themselves have no effect. It is quite possible they help strengthen family support. However, I am unable to speak to their causal effect in this setting.

2SLS specification since they are not moved by the instrument. If the IV compliers are more "at-risk" than the always-takers, this is a plausible reason the 2SLS estimate could be larger in magnitude than the OLS estimate. Compliers actually benefit more from visitation and have larger effects.

Table A.29: EFV First Stage

	EFV Received		EFV Count (per Mo.)	
	Full Sample	Visited Sample	Full Sample	Visited Sample
	(1)	(2)	(3)	(4)
Distance (100 miles)	-0.0057***	-0.0086***	-0.0012***	-0.0017**
	(0.0011)	(0.0031)	(0.0003)	(0.0080)
Controls	Yes	Yes	Yes	Yes
Fixed-effects	Yes	Yes	Yes	Yes
F-stat	27.7	7.95	16.70	4.34
Observations	24,413	10,775	24,413	10,775

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents the first stage effect of distance on likelihood and intensity of Extended Family Visits. Columns 1-2 present results for a binary indicator of EFV-receipt. Columns 3-4 present results for the count of EFVs received in a spell normalized by the number of months served for that spell. Columns (1) and (3) use the full sample. Columns (2) and (4) use the visited sample and show the effect of distance on receiving an EFV and the number of EFVs given already receiving regular visitation. Controls for the individual's race, the individual's age at the time of prison admission, the length of the sentence in months, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility are included. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and who were placed in a standard facility.

Table A.30: Sample Descriptives by EFV Status

	Overall	Never Visited	Visited, No EFV	EFV
N Incarceration Spells	24,338	13,563	10,483	292
N Incarcerated Individuals	21,389	12,090	9,840	290
% First admit	0.52	0.49	0.55	0.52
Median time served (days)	329	270	412	960
Median sentence length (days)	578	517	669	1,369
% Property crime	0.34	0.34	0.43	0.46
% Drug offense	0.25	0.26	0.25	0.29
% Violent offense	0.17	0.13	0.21	0.30
% Sex offense	0.05	0.04	0.06	0.08
% White	0.62	0.59	0.67	0.72
% Black	0.15	0.17	0.13	0.12
% Hispanic	0.13	0.14	0.12	0.10
% Native American	0.05	0.06	0.04	0.02
% Asian	0.03	0.04	0.03	0.04
Mean Age at admit	33.5	35.06	31.47	33.24
% Visited	0.44	0.00	1.00	1.00
Mean No. visits per month	0.91	0.00	1.97	4.80
% EFV participant	0.01	0	0	1
Mean No. EFVs per month	0.002	0	0	0.21
Median placement distance (miles)	98.65	111.94	86.70	79.12
% Use housing voucher	0.19	0.25	0.11	0.05

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Table A.30 continued from previous page

	Overall	Never Visited	Visited, No EFV	EFV
% Readmitted within 1 year	0.09	0.10	0.07	0.02
% Readmitted within 3 years	0.26	0.28	0.23	0.12

Notes: This table presents summary statistics for all men incarcerated in Washington state prisons admitted on or after January 1, 2010 and released by December 31, 2016. Data is at the prison spell level. Incarcerated individuals are categorized as "visited, No EFV" if they received one or more visits during the given spell but did not receive an Extended Family Visit (EFV). Individuals are categorized as EFV recipients if they received one or more EFVs during the given spell. Offense types are not mutually exclusive and are binary indicators for whether any of the crimes associated with the current spell fall in the given category. Crime definitions follow the Revised Code of Washington. Distance is calculated as the Euclidean distance between the geographic centroids of the home ZIP code and placement facility ZIP code. Data come from Washington Department of Corrections records.

Table A.31: Supplementary Sample EFV Statistics

	Overall	Never Visited	Visited, No EFV	EFV
N Spells	590	297	264	29
% low risk	0.21	0.16	0.21	0.76
% moderate risk	0.10	0.10	0.10	0.14
% high drug crime risk	0.07	0.07	0.08	0.00
% high property crime risk	0.16	0.19	0.15	0.03
% high violent crime risk	0.22	0.22	0.23	0.07
% high risk all crimes	0.24	0.26	0.24	0.00
% mental health or substance abuse issue	0.88	0.92	0.87	0.52
% married or with long-term partner	0.41	0.32	0.48	0.76
% with good family relationship	0.71	0.67	0.75	0.79
% with children	0.51	0.49	0.55	0.59
% with 1 child	0.21	0.19	0.24	0.17
% with 2 or more children	0.30	0.30	0.29	0.41
Avg monthly income in 6mo prior to admit				
No legal income	0.34	0.34	0.36	0.07
<$1,000	0.18	0.21	0.17	0.07
$1,000-1,999	0.22	0.25	0.19	0.31
$2,000-3,999	0.19	0.16	0.22	0.17
≥ $4,000	0.08	0.05	0.08	0.38

Notes: The sample is all men already incarcerated as of 2018 and those incarcerated after 2018 and were placed in a standard facility without a life sentence. Data is from the first risk assessment done for each individual in a given spell. Risk score classifications come from Washington's classification system based on discrete cuts of continuous numeric scores for property crime, drug crime and violent crime re-offense risk. Scores are determined by the responses to interview questions, criminal history, and past incarceration behavior (if applicable). Relationship status, number of children, family relationships and substance abuse issues are self-reported. Mental health issues refer to a documented diagnosis.

A.5 Complier Characteristics

Consider the following instrumental variables specification where endogenous regressor x_i is a measure of visitation, y_i is the recidivism outcome, z_i is individual i's distance from home, and w_i collects fixed effects for placement facility, home county, and security level:

$$y_i = \beta x_i + w_i'\gamma + \varepsilon_i,$$

$$x_i = \pi z_i + w_i'\mu + u_i.$$

Let the true causal model have linear heterogeneous treatment effects:

$$y_i = x_i\beta_i + \varepsilon_i.$$

Then β is the IV estimand of β_i.

Following Borusyak and Hull [2024], under the following two assumptions β can be expressed as a convex combination (non-negative weights that sum to one) of heterogeneous treatment effects:

Assumption 1 (Exogeneity): $\mathbb{E}[z_i \mid y_i(x), w_i] = w_i'\lambda$, conditional on w_i, the instrument is as-good-as randomly assigned and its expectation is linear in w_i.

Assumption 2 (Monotonicity): z_i moves x_i in a single direction (with no defiers).

Under these assumptions, and assuming the IV estimator consistently estimates β, the

estimand can be written as:

$$\beta = \mathbb{E}\left[\int \phi_i(x)\beta_i(x)dx\right] / \mathbb{E}\left[\int \phi_i(x)dx\right]$$

$$\beta = \mathbb{E}\left[\int \omega_i(x) \underbrace{\frac{\partial y_i}{\partial x}(x)}_{\text{hetero effects } \beta_i} dx\right]$$

$$\text{for binary } x_i : \beta = \mathbb{E}\left[\omega_i(y_i(1) - y_i(0))\right]$$

Hull [2025] shows how to use this result to characterize the population contributing to an OLS and 2SLS estimand. Specifically, summary statistics on pre-determined characteristics can be calculated by replacing the outcome variable with the interaction of the pre-determined characteristic, c_i, and treatment x_i. This is similar to the well-known Abadie [2003] result for characterizing compliers except that here the instrument does not need to be binary.

$$x_i c_i = \tilde{\beta} x_i + w_i' \tilde{\gamma} + \epsilon_i$$

$$x_i = \pi z_i + w_i' \mu + \nu_i$$

Using the convex expression of treatment effects above, we can then write $\tilde{\beta}$:

$$\tilde{\beta} = \mathbb{E}\left[\int \omega_i(x) \frac{\partial x_i c_i}{\partial x}(x) dx\right]$$
$$= \mathbb{E}\left[\int \omega_i(x) c_i dx\right]$$

which gives you the weighted mean of characteristic c_i using the weights used in the estimand.

To illustrate this procedure I provide an example of how to estimate the mean age of

admit of compliers contributing to the 2SLS estimate in my main results. I run the following 2SLS regression (S_i are controls/FEs required for exogeneity):

$$age_i \times visit_i = \alpha_0 + S_i'\alpha_2 + \rho visit_i + \epsilon_i$$
$$visit_i = \pi distance_i + S_i'\mu + \nu_i$$

The estimated coefficient ρ gives the sample mean for the effective population.

Table A.32 reports summary statistics for pre-determined characteristics. Column (1) shows unadjusted sample means. Column (2) presents estimates obtained using the method of Hull [2025] for OLS estimation of the effect of visitation on three-year readmission rates. Column (3) applies the same method to the 2SLS estimates, instrumenting visitation with distance. The population characterized in Column (3) is the complier population. Overall, the effective sample for 2SLS is similar to that for OLS, with two notable differences: the 2SLS complier population is much whiter and includes more property crime offenders. Non-violent offenders tend to have larger treatment effects while whites have smaller treatment effects than non-white individuals.

Table A.32: Effective Population Characteristics

	Sample	OLS	2SLS
	(1)	(2)	(3)
Admit age	33.5	31.6	31.9
	(0.07)	(0.10)	(0.60)
% Above median admit age	0.49	0.41	0.42
	(0.004)	(0.005)	(0.03)
% Non-white	0.37	0.33	0.26
	(0.003)	(0.005)	(0.03)

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Table A.32 continued from previous page

	Sample	OLS	2SLS
	(1)	(2)	(3)
% Violent crime conviction	0.17	0.19	0.18
	(0.002)	(0.004)	(0.02)
% Drug crime conviction	0.25	0.25	0.26
	(0.003)	(0.004)	(0.03)
% Property crime conviction	0.41	0.43	0.46
	(0.003)	(0.005)	(0.03)

Notes: Standard errors in parentheses. "Above median admit age" is a binary indicator whether an individual was above age 33 at admit and is used for comparison to the heterogeneous treatment effect estimates that use this binary split. In Column (1) means and standard errors are calculated directly from the sample. In Column (2) means and standard errors are for the coefficient on treatment from an OLS regression with the interaction of treatment and the given characteristics as the outcome variable and treatment as the independent variable with controls for sentence length, recidivist status, home county and placement facility. In Column (3) means and standard errors are for the coefficient on treatment from a 2SLS regression with the given characteristics as the outcome variable and treatment as the independent variable, distance as the instrument and with controls for sentence length, recidivist status, home county and placement facility. Sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and who were placed in a standard placement facility.

A.6 Selection Test Under Heterogeneity

The instrumental variables approach is motivated by potential unobserved selection and by the goal of identifying a policy-relevant local effect. With heterogeneous treatment effects, however, OLS and 2SLS target different parameters, so the traditional Wu–Hausman test for endogeneity is not informative about selection. For that reason, I turn to the heterogeneity-robust test in black2022, which compares mean outcomes for treated compliers to always-takers and for untreated compliers to never-takers. Under no selection in levels, these means coincide. Essentially, I estimate the regression in Eq. (1) separately within the treated and untreated samples. The coefficient on the instrument is the parameter of interest.

To implement the test I estimate

$$R_{is} = a_0 Dist_{is} + B_0' X_{is}$$

within the sample of non-visited individuals. And I estimate

$$R_{is} = a_1 Dist_{is} + B_1' X_{is}$$

within the sample of visited individuals. R_{is} is a binary indicator for prison re-admission within 1 year and within 3 years of release from prison spell s. X_{is} includes all controls and fixed effects from the main specification. $Dist_{is}$ is the distance from home county to placement facility.

Formally, the null hypothesis is $H_0 : a_j = 0; j \in 0, 1$. Rejection of either coefficient indicates that either the exclusion restriction is violated or selection is present. Under the assumption that the exclusion restriction holds, rejection is evidence of selection. I fail to reject the null for both untreated and treated groups for both one-year and three-year recidivism (see Table A.33 below). I also conduct the test using a binary instrument indicating whether an individual is placed more than 99 miles from their home county (the median distance) and I again fail to reject the null for both untreated and treated groups for both time horizons. The 2SLS estimates have large standard errors, so the lack of statistical significance is not necessarily strong evidence of no selection, but is rather inconclusive given limited precision.

Table A.33: Selection Test

	Readmit in 3 years	
	Untreated Sample	Treated Sample
	(1)	(2)
Above median distance	0.0103	-0.0042
	(0.0079)	(0.0081)
Controls	Yes	Yes
Fixed Effects	Yes	Yes
Observations	13,563	10,775

Notes: ***: 0.01, **: 0.05, *: 0.1. Standard errors clustered at the individual level are in parentheses. This table presents results from the black2022 test for selection with heterogeneous treatment effects. The dependent variable is an indicator for whether an individual is readmitted to prison within 3 years of release. The independent variable is an indicator for whether an individual is placed above the median distance of 99 miles. The untreated sample consists of individuals who did not receive any visits in the given spell. The treated sample consists of individuals who received one or more visits in the given spell. Controls for the individual's race, the individual's age at the time of prison admission, the length of the sentence in months, indicators the type of crime the individual was convicted for in the given spell, and an indicator for if this spell is the individual's first prison spell, as well as fixed effects for year of admission, home county and facility are included. The sample includes all prison spells for men admitted to Washington state prisons on or after January 1, 2010 and released as of December 31, 2016, had a non-missing home ZIP code and whose placement facility is the same as their release facility.

APPENDIX B

AN EVALUATION OF CRISIS-INTERVENTION TEAM TRAINING

B.1 Appendix Figures

Figure B.1: Top Types of Calls Resulting in Force

2017: domestic, other complaint, mental

2018: mental, domestic, other complaint

2019: mental, battery, domestic

2020: mental, domestic, prowler

2021: other complaint, suspicious person, mental

2022: suspicious person, disturbance, mental

2023*: EDP, suspicious person, weapon

Note: Sample is all calls that resulted in force between January 1, 2017 and June 30, 2023.

B.2 Appendix Tables

Table B.1: Officer Demographics by Training Class

Cohort	N officers	% Female	% Black	Avg age	Avg years of service
1	20	0.3	0.55	40.15	14.25
2	21	0.29	0.57	40.95	16.76
3	19	0.32	0.47	40.11	15.89
4	27	0.52	0.63	40.04	11.96
5	22	0.23	0.55	38.86	14.82
6	23	0.35	0.39	35.39	12.39
7	16	0.06	0.06	31	5
8	27	0.22	0.41	38.37	12.56
9	25	0.32	0.36	37.2	10.92
10	23	0.35	0.43	34.65	9.48
11	15	0.2	0.27	35.85	8.93
12	14	0.29	0.5	37.07	12.5
13	21	0.33	0.62	31.42	7.24
14	22	0.36	0.45	33.6	8.95
15	17	0.47	0.65	34.31	7
16	17	0.18	0.53	37.5	5.06
17	16	0.25	0.5	32	4.19

Notes: The first five training classes took place in 2016, before data collection began. Training class 6 took place in February 2017, meaning there is only one month of pre-data and so I exclude them from analysis. Training class 7 was abnormally dominated by white men and so they are also excluded. Averages are taken at time of training.

Table B.2: Officer Demographics by Shift

Shift		% Male	% Black	Avg age	Avg years of service
Between 11pm-7am	Fri	0.8	0.44	38.36	10.71
Between 11pm-7am	Mon	0.8	0.44	38.38	10.67
Between 11pm-7am	Sat	0.8	0.43	38.25	10.56
Between 11pm-7am	Sun	0.8	0.44	38.28	10.65
Between 11pm-7am	Thu	0.8	0.44	38.39	10.74
Between 11pm-7am	Tue	0.8	0.44	38.21	10.53
Between 11pm-7am	Wed	0.8	0.45	38.21	10.56
Between 3pm-11pm	Fri	0.82	0.38	36.34	8.91
Between 3pm-11pm	Mon	0.82	0.39	36.42	8.97
Between 3pm-11pm	Sat	0.81	0.37	36.16	8.74
Between 3pm-11pm	Sun	0.81	0.37	36.36	8.81
Between 3pm-11pm	Thu	0.82	0.38	36.38	9.02
Between 3pm-11pm	Tue	0.82	0.41	36.47	9.05
Between 3pm-11pm	Wed	0.82	0.39	36.3	9.02
Between 7am-3pm	Fri	0.76	0.48	39.71	11
Between 7am-3pm	Mon	0.76	0.49	39.79	10.96
Between 7am-3pm	Sat	0.76	0.46	39.51	10.76
Between 7am-3pm	Sun	0.76	0.47	39.6	10.77
Between 7am-3pm	Thu	0.76	0.49	39.79	11.09
Between 7am-3pm	Tue	0.75	0.5	39.83	11.05
Between 7am-3pm	Wed	0.76	0.5	39.83	11.14

Notes: Averages taken over 2017 - June 2023. An officer's shift is only observable if they appear as a responding officer in the CAD data during said shift. However, it would be extremely unlikely for an officer to not appear in CAD data at all during a shift and so it is unlikely an officer is missed.

B.3 Use of Force Definition

Use of Force Definitions [1]:

1. Level 1: uses of force include pointing a firearm at a person and hand control or escort techniques (e.g., elbow grip, wrist grip, or shoulder grip) applied as pressure point compliance techniques that are not reasonably expected to cause injury; take-downs that do not result in actual injury or complaint of injury; and use of an impact weapon for non- striking purposes (e.g., prying limbs, moving or controlling a person) that does not result in actual injury or complaint of injury. It does not include escorting, touching, or handcuffing a person with minimal or no resistance.

2. Level 2: uses of force include use of a CEW (including where a CEW is fired at a person but misses); and force that causes or could reasonably be expected to cause an injury greater than transitory pain but does not rise to a Level 3 use of force.

3. Level 3: uses of force include any strike to the head (except for a strike with an impact weapon); use of impact weapons when contact is made (except to the head), regardless of injury; or the destruction of an animal.

4. Level 4: uses of force include all 'serious uses of force' as listed below

   (a) All uses of lethal force by an NOPD officer

   (b) All critical firearm discharges by an NOPD officer

   (c) All uses of force by an NOPD officer resulting in serious physical injury or requiring hospitalization

   (d) All neck holds

   (e) All uses of force by an NOPD officer resulting in a loss of consciousness

1. from NOPD policy manual

(f) All canine bites;

(g) More than two applications of a CEW on an individual during a single interaction, regardless of the mode or duration of the application, and whether the applications are by the same or different officers, or CEW application for 15 seconds or longer, whether continuous or consecutive

(h) Any strike, blow, kick, CEW application, or similar use of force against a handcuffed subject

(i) Any vehicle pursuit resulting in death, serious physical injury or injuries requiring hospitalization

B.4 Alternative Specifications

B.4.1 Static TWFE

I estimate a two-way fixed effects model at the officer-month level. An officer is marked as treated for any incident they are dispatched to after their training date, and all data is pooled into one pre-period and one post-period.

$$Y_{ict} = \alpha_i + \gamma_t + \phi Treated_{ict} + \beta X_c + \epsilon_{ict} \tag{B.1}$$

Y_{ict} is an indicator for whether officer i used force or made an arrest at incident c in month t. α_i are officer fixed effects and γ_t are time fixed effects. $Treated_{ict}$ is an indicator equal to one if officer i had already received CIT training by month t when responding to incident c. X_c is a vector of call characteristics including district, priority, shift, and day of week. The sample is all calls for service for mental health incidents.

The coefficient of interest is ϕ. The identifying assumptions for ϕ to provide an unbiased

causal estimate of the average treatment effect are: parallel trends and constant treatment effects between groups and over time.

In this context the parallel trends assumption would require that officers who receive CIT training and officers who never receive training would have maintained common use of force and arrest trends in the absence of treatment. To assess the validity of this assumption I plot the rates of use of force and arrest rates prior to training for officers who receiving CIT training and rates of use of force and arrest rates for officers who never do, presented in Figure B.2. If we took the average the never-treated and treated officers would have very similar rates of force use and arrests in the pre-period, but by plotting the pre-trends month-by-month we see they are not parallel. Officers who select into training have higher rates of both force and arrests prior to training than the never-treated officers and the arrest trends cross, potentially violating parallel trends.

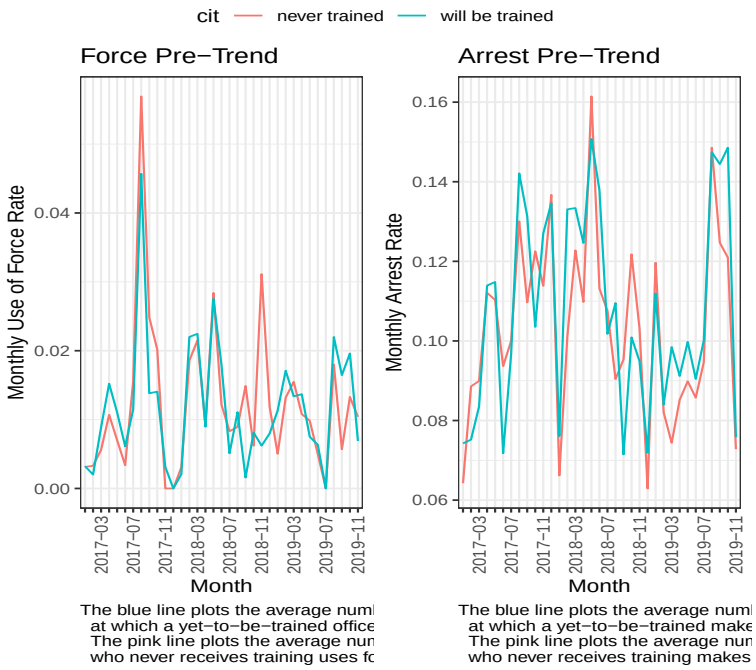


Figure B.2: Pre-Trends

Notes: This figure plots the pre-treatment use of force and arrest rates of never-trained officers and trained officers. The blue line plots the average number of calls per calendar month at which an officer who will receive training uses force or makes an arrest in the months prior to training. The pink line plots the average number of calls at which an officer who was never trained uses force or makes an arrest in the same calendar month.

Table B.3 presents the estimated coefficients on *Treated* from Equation (2). These estimates imply that CIT training is actually associated with an increase in use of force and propensity to arrest. However, to interpret these estimates causally we must assume constant treatment effects across cohorts and parallel trends and this may be violated.

Table B.3: Static Two-way Fixed Effects

	Use of Force	Arrest
	(1)	(2)
Treated	0.005*	0.016*
	(0.002)	(0.007)
Observations	35,917	35,917

Notes: *p<0.1; **p<0.05; ***p<0.01. The coefficient presented is ϕ from Eq.(2): treated = 1 if the officer had already received training at the time of the call. Fixed effects for incident priority, day of week, shift and district are included. The sample is only mental health incidents. Standard errors are clustered at the incident level.

An additional, and related, concern is that the first CIT training cohort completed training at the beginning of 2016 but data collection did not begin until 2017, therefore I have a group of officers who are “always-treated" across my panel. To test whether always-treated officers differ from the officers whose training I observe I compare use of force and rates of arrest in the post-periods for officers who experience training in my panel and officers who are “always-treated". The plots are presented below. I also test this empirically by regressing outcomes on an indicator equal to 1 if an officer will be treated and is equal to 0 if the officer is “always-treated". The coefficient of interest for both force and arrests is insignificant.

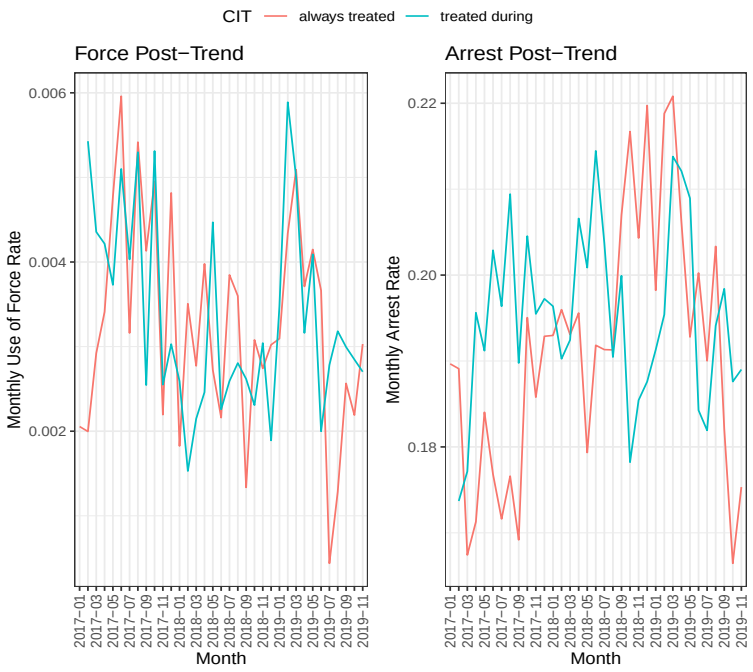


Figure B.3: Always Treated vs. Eventually Treated

Notes: This figure plots the post-treatment use of force and arrest rates of always-trained officers and eventually trained officers. The blue line plots the average number of calls per calendar month at which an officer who receives training during the observation period use force or makes an arrest after they received training. The pink line plots the average number of calls at which an officer who was trained prior to the observed period uses force or makes an arrest in the same calendar month.

B.4.2 Dynamic Two-Way Fixed Effects

Next, I allow for the possibility of heterogeneous treatment effects across cohorts. CIT training had a staggered roll-out (there had been 17 training classes as of 2021) so I estimate an event-study model with two way fixed effects to allow for variation across training cohorts. I estimate the causal impact of CIT on an officer's actions with the following equation:

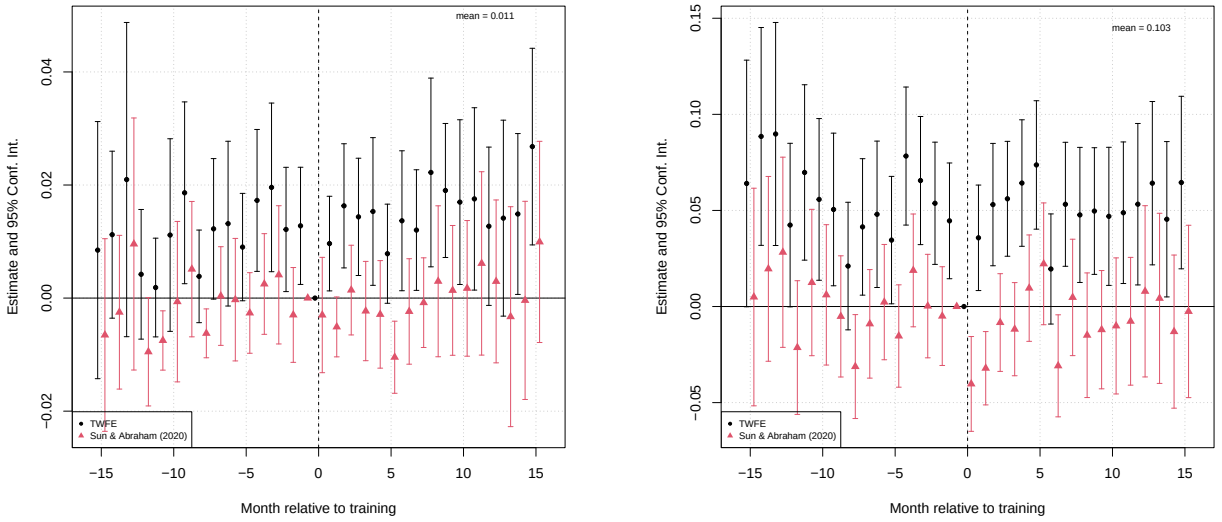
$$Y_{ict} = \alpha_i + \gamma_t + \sum_k \phi_{kt} D_{ct}^k + \beta X_c + u_{ict} \tag{B.2}$$

Y_{ict} is an indicator for whether officer i used force or made an arrest at call c in month t. α_i are officer fixed effects, and γ_t are time fixed effects. D_{ct}^k is an indicator equal to 1 if month t is k months after (or before) the month of training, k is negative for the months prior to training and positive for months after training. X_c is a vector of call characteristics including district, priority, shift, and day of week. The sample is all mental health incidents.

Sun and Abraham [2021] provide a decomposition of the population regression coefficients ϕ_{kt}. They show that the coefficients are linear combinations of differences in trends from event period k and differences in trends from other relative periods (whether they are included or excluded from the specification). Therefore, group treatment effect estimates may suffer from possible contamination from other event-periods. They propose a method to correct for contamination from other relative periods that weights the average of cohort treatment effects by the shares of cohorts that experience at least k relative treatment periods. Under the joint assumptions of parallel trends and no anticipation the Sun and Abraham interaction-weighted estimator provides an unbiased and consistent estimate of the cohort treatment effect on the treated. CIT training is meant to provide officers with hard and soft skills for managing tense situations and so it is plausible in this scenario to assume no anticipation - just knowing they will be trained will not give them the skills required to de-escalate a volatile situation. Additionally, NOPD has indicated that training is not offered regularly but at various intervals throughout the year, meaning officers cannot predict far in advance when they will be able to sign-up for training and generally are made aware of a new training class only one month (or so) in advance.

As of 2021 there had been 17 cohorts of officers to receive CIT training (1 class in 2015, 3 in 2016, 4 in 2017, 2 in 2018, 2 in 2019, 1 in 2020, and 1 in 2021). In my data I able to observe 9 of these cohorts (2017-2020). To account for the staggered roll-out I use an event-study design. I estimate both a standard two-way fixed effects model as well as the Sun & Abraham aggregated cohort method.

I restrict the sample to 15 pre- and 15 post-periods because the sample is much larger and closer to being balanced in this restricted time frame. Focusing on the Sun & Abraham estimates that account for heterogeneous effects, there are no pretends for force or arrest and the post-trends for both are noisy around zero and so I do not find any impact on use of force or arrests. The event study plots are presented below in Figure B.4.



(a) Use of Force (b) Arrest

Figure B.4: Dynamic TWFE Estimates

Notes: This figure presents the estimated coefficients from Equation B.2. The dependent variable is a binary indicator for use of force in panel (a) and arrest in panel (b). The sample is restricted to valid mental health incidents. Event time is measured relative to the month of CIT training completion (month 0). The sample includes officers trained during the observation window, never-trained officers, and always-trained officers. Standard errors are clustered at the incident level.

B.4.3 TWFE Weights

A concern from the static TWFE model is the causal interpretation of the treatment coefficient. Under the parallel trends assumption, following de Chaisemartin and D'Haultfoeuille

(2020):

$$E[\phi] = E\left[\sum W_{g,t} TE_{g,t}\right]$$

where ϕ is the "overall" treatment effect and $W_{g,t}$ is the weight assigned for treatment group g at time t and $TE_{g,t}$ is the treatment effect for this group and time period. The decomposition in Goodman-Bacon (2019) shows that this may produce negative weights, especially if there are groups that are treated most or all of the time. Negative weights themselves are not necessarily an issue if there are homogeneous treatment effects, but in the case of heterogeneous effects can bias treatment effects estimates (De Chaisemartin and d'Haultfoeuille 2020, Goodman-Bacon 2018). The existence of negative weights is directly testable, as the weights placed one each observation are proportional to the residuals from a regression of treatment on unit and time fixed effects (De Chaisemartin and d'Haultfoeuille 2020)[2]. Approximately 8% of treated observations receive negative weights and these observations tend to be later months in the observation period for officers trained early[3]. As noted by Jakiela (2021) a sufficiently large never-treated group combined with sufficient pre-treatment data will ensure non-negative weights. Only 30% of officers ever receive training so a large never-treated group exists. To address point two I estimate a version of the two-way fixed effects model where I restrict my sample to officers who were never trained or were trained 6 months or more after data collection began, which naturally excludes the always-treated group. Results do not qualitatively change and are presented in the Table B.4.

2. This can be seen by applying the Frish-Waugh-Lovell theorem to a standard DiD model of the form $Y_{it} = \lambda_i + \gamma_t + \beta D_{it} + \epsilon_{it}$.

3. Residuals are from the regression $Treated_{it} = \lambda_i + \gamma_t + \epsilon_{it}$ where i is an officer and t is a month. λ_i are officer fixed effects and γ_t are month fixed effects.

Table B.4: Trimmed Panel

	All Calls		Mental Health Calls	
	Use of Force	Arrest	Use of Force	Arrest
	(1)	(2)	(3)	(4)
Treated	−0.0004	−0.0200	0.0018	0.0190
	(0.0003)	(0.002)	(0.0033)	(0.009)
Observations	370,259	370,259	11,526	11,526

Notes: *p<0.1; **p<0.05; ***p<0.01. Treated = 1 if the officer received training and the incident occurred after training was completed. Fixed effects for incident priority, day of week, shift and district are included. The top panel uses the repeated cross-section sample of all valid incidents and the bottom panel uses the sample of only mental health incidents. Only officers who never receive training or receive training 6 months or more after the observation window begins (June 2017- Feb 2020) are included. Standard errors are clustered at the incident level.

APPENDIX C

PRISON DIVERSION FOR PARENTS

C.1 Appendix Tables

Table C.1: Descriptive Statistics for Within-Judge Cases

	Approved	Denied	Not Considered
N Cases	25	17	3,052
Modal Charge	Drug	Fraud	ID theft
% Male	0.40	0.41	0.86
% white	0.32	0.65	0.60
% Black	0.08	0.24	0.19
% Asian	0	0.06	0.04
% Hispanic	0	0.06	0.12
% Native American	0.04	0	0.05
Avg age at sentencing	42.33	45.88	43.34
% drug crime	0.56	0.18	0.25
% property crime	0.28	0.71	0.29
% firearm crime	0.08	0	0.09
Median sentence (days)	-	487	608
Modal sentence (days)	-	395	366
Median N priors	0	1	2

Continued on next page

Table C.1 continued from previous page

	Approved	Denied	Not Considered
% Re-convicted within 6 months	0	0	0.06
% Re-convicted within 1 year	0	0	0.07
% Re-convicted within 3 years	0.04	0.06	0.24

Notes: This table presents descriptive statistics of cases seen by a judge who has both approved and denied FOSA in the observation time frame. Observations are at the case level. *Approved* cases are cases where the individual was granted a FOSA diversion. *Denied* cases are cases where the judge submitted a request for risk assessment report but did not grant FOSA. Sentence length is not available for successful FOSA participants because their sentence was waived. Re-convictions are measured from date of sentencing and are measured as a subsequent appearance by the individual in the court sentencing data.

C.2 Community Parenting Alternative

The early release program is called the "Community Parenting Alternative" (CPA) and allows the Department of Corrections to release an incarcerated individual early into electronic home monitory for the last 12 months (or less) of their sentence. To be eligible for this option the individual must be a parent under the same definition provided above. Additionally, they must have at least 12 months remaining on their prison sentence at the time of the decision. They cannot be currently convicted for a felony sex offense or for a serious violent offense. They can have a current conviction for a violent offense if and only if it has been determined that they are not at a high risk of re-offending. They cannot be subject to a deportation order. See Washington State Department of Corrections [2025a] for full details.

C.3 Other Diversion Programs in Washington

Drug Offender Sentencing Alternative Washington State's Drug Offender Sentencing Alternative (DOSA) allows individuals with substance use disorders convicted of non-violent felony offenses with a documented substance use disorder and no history of violent or sexual offenses to serve either part or all of their sentence in treatment rather than in prison.

Under the prison-based DOSA, individuals serve a reduced term of incarceration followed by a period of community supervision that includes substance use treatment and supervision requirements.

Under the residential DOSA, individuals serve their sentence in the community instead of prison. This option requires placement in a licensed residential substance use treatment program, followed by a period of community supervision with continued treatment.

See Washington State Department of Corrections [2025b] and Washington State Legislature [2025a] for full details.

Special Sex Offender Sentencing Alternative Washington State's Special Sex Offender Sentencing Alternative (SSOSA) allows individuals sentenced to non-violent non-serious sex offenses without a history of such crimes to receive a suspended or shortened prison sentence and instead be supervised and treated in the community. See Washington State Legislature [2025b] for full details.

C.4 Judge Leniency IV

Assignment to FOSA is at the sole discretion of the sentencing judge. Each judge may have a different inherent propensity to be willing to divert individuals from prison. Each court sets the judges schedule and judges do not get to pick which cases they see. Therefore, assignment to one particular judge is as-good-as-random. I can then use each judge's leniency as an instrument for whether or not an individual is assigned to FOSA, and use this exogenous variation to estimate the causal effect of FOSA on criminal activity.

In most settings using judge leniency as a measure, the leniency measure is constructed using the leave-out mean allowance or diversion rate for each judge for the given program or treatment being evaluated. However, FOSA is a rarely considered treatment and the median number of cases a single judge considers FOSA for is 0. Of the 642 judge in the sample, only

119 ever submitted a request for a risk assessment for FOSA. Even among those judges who have considered a FOSA diversion, the median number of considered cases is 1. Therefore, it is infeasible to create a judge leniency measure based solely off of a judge's propensity to assign FOSA. Instead I create a leniency measure based on judge propensity to assign any diversion program.

The three sentencing alternatives that allow judges to divert a defendant from prison in Washington state are the Parenting Sentencing Alternative (PSA), the Drug Offender Sentencing Alternative (DOSA), and the Special Sex Offender Sentencing Alternative (SSOSA). More information DOSA and SSOSA can be found in the Appendix. For all drug cases, I calculate the mean rate of DOSA granting by judge. For all sex offense cases, I calculate the mean rate of SSOSA granting by judge. I then take a weighted average for a combined rate and use that as the leniency measure While not all individuals with drug cases or sex offense cases are eligible for diversion programs, I am unable to see eligibility or consideration. However, if cases are randomly assigned, the probability any judge gets an eligible case among the drug or sex offense cases they see, should be uncorrelated with potential outcomes. SSOSA is fairly rare, and so the combined rate more heavily reflects DOSA leniency.

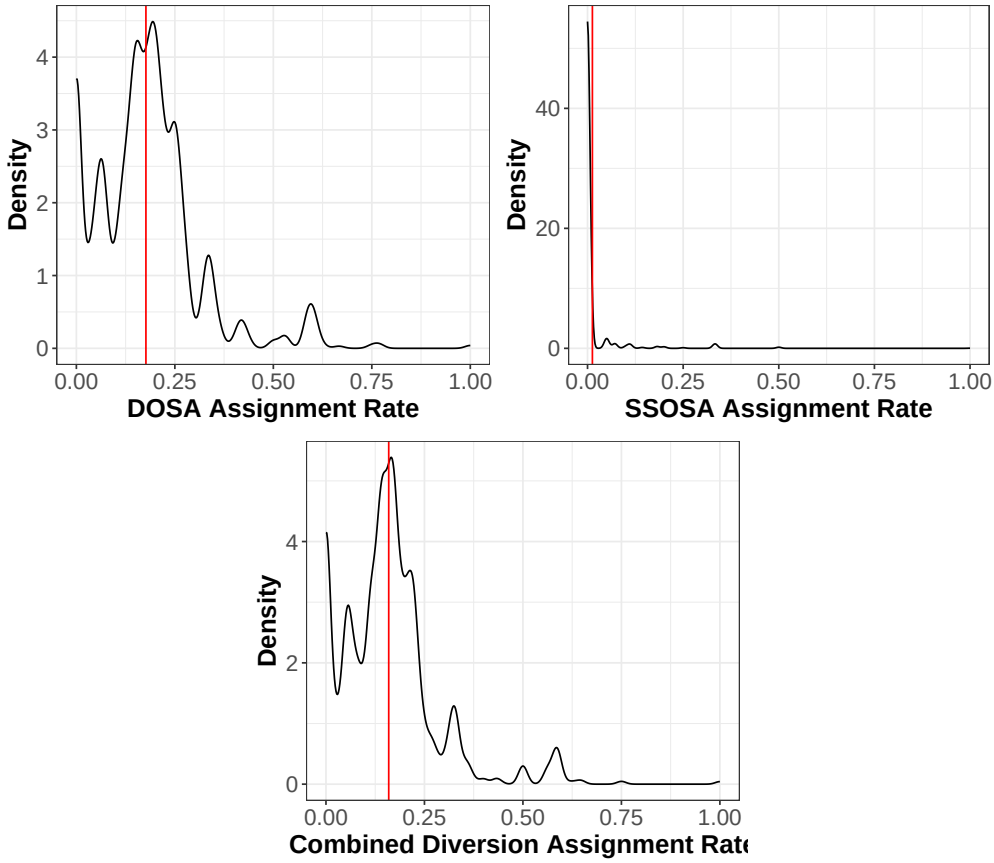


Figure C.1: Judge Leniency

Table C.2 presents the first stage estimates. The judge leniency measure is positively correlated with whether or not an individual is granted FOSA. It is also correlated with whether or not a judge even considers FOSA. This is important validation that the leniency measure constructed based on other diversion programs is picking up inherent idiosyncratic willingness by the judge to divert someone from prison, regardless of the program. However, the first stage is small, with only a 2 percentage point increase in the probability of receiving FOSA, raising concerns about weak instrument bias and the reliability of resulting estimates.

Table C.2: Judge Leniency First Stage

	FOSA approved	FOSA considered
	(1)	(2)
leniency	0.0225***	0.0286***
	(0.0055)	(0.0064)
County FE	Yes	Yes
Observations	28,495	28,495
F-test	119.17	236.12

Notes: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Standard errors are in parentheses and are clustered at the individual level. Sample consists of all sentencing events between January 1, 2010 and December 31, 2017. *FOSA approved* is a binary indicator for whether the individual was granted a FOSA prison diversion for the given sentence. *FOSA considered* is a binary indicator for whether a risk assessment report for FOSA was commissioned for the individual for the given sentence. *Leniency* is the assigned judge's weighted average rate of diversion assignment to any diversion program.

In order for judge leniency to be a valid instrument, it must satisfy exogeneity and exclusion. While these are not provable, I provide evidence in favor of it by showing that judge leniency is not correlated with predetermined case or individual characteristics. I regress the judge leniency measure on gender, race, age, a binary indicator for whether the case involves a property crime, a binary indicator for whether the case involves a firearm, and number of prior convictions controlling for a binary indicator for whether the case involves a drug crime and county fixed effects. Judge leniency is not correlated with any of these characteristics. If it were, we may be worried that judge assignment is not random and certain judges are assigned to certain cases.

Table C.3: Judge Leniency Balance

Male	-0.0021
	(0.0017)
White	-0.0002
	(0.0012)
Age at sentencing	0.00009*
	(0.000006)
Property crime	-0.0014
	(0.0013)
Firearm crime	0.0002
	(0.0020)
No. priors	0.0002
	(0.0003)
Drug Crime Control	Yes
County FE	Yes
Observations	28,495

Notes: Signif. Codes: ***: 0.01, **: 0.05, *: 0.1. Standard errors are in parentheses and are clustered at the individual level. Sample consists of all sentencing events between January 1, 2010 and December 31, 2017. *Leniency* is the assigned judge's weighted average rate of diversion assignment to any diversion program.