UNIVERSITY OF CALIFORNIA, SAN DIEGO

Policy and Behavior: Essays in Applied Microeconomics

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by

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The dissertation of Andrew David Chamberlain is approved, and it is acceptable in quality and form for publication on microfilm:

Chair

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DEDICATION

To Paul Heyne.

EPIGRAPH

"The whole of the advantages and disadvantages of the different employments of labour and stock must, in the same neighbourhood, be either perfectly equal or continually tending to equality. If in the same neighbourhood, there was any employment evidently either more or less advantageous than the rest, so many people would crowd into it in the one case, and so many would desert it in the other, that its advantages would soon return to the level of other employments. This at least would be the case in a society where things were left to follow their natural course, where there was perfect liberty, and where every man was perfectly free both to chuse what occupation he thought proper, and to change it as often as he thought proper."

—Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations* (1776)

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ABSTRACT OF THE DISSERTATION

Policy and Behavior: Essays in Applied Microeconomics

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These essays explore the microeconomic impact of federal, state and local public policies on individual behavior in three distinct settings. Chapter 1 examines the impact of municipal policies affecting retail liquor availability on the incidence of urban crime, based on a rapid 2012 expansion of liquor retailing in the City of Seattle. Chapter 2 examines whether state-level public sector employees are paid "wage rents" in excess of their outside options, based on an original survey of roughly 900 exogenously laid-off of government workers as part of a liquor privatization initiative in Washington State. Finally, Chapter 3 examines whether federal intergovernmental grants have a persistent long-term effect on state government tax policy, based on a 30-year panel of federal grants and tax revenue for the U.S. states. In all three cases I emphasize the identification of causal effects of policy characteristics on behavior, highlighting the importance of econometric program evaluation as a tool for understanding and developing well-designed public policy.

Chapter 1

Urban Crime and Spatial Proximity to Liquor

Abstract

There is a well-established correlation between retail liquor outlets and crime, but few studies identify causal effects. I exploit a unique source of identifying variation to establish causality: a 2012 privatization of liquor retailing in Washington State that rapidly expanded liquor availability into preexisting grocery and drug store chains. Based on 166,000 police reports from Seattle and a fixed-effects panel model, I find a significant positive effect of liquor availability on neighborhood crime both in OLS and IV estimates. Reducing the distance to the nearest liquor retailer by one mile leads to an average treatment effect of roughly 6 to 8 percent higher monthly crime rates. Violent crime and drug crimes are persistently affected, with more transitory effects on shoplifting and other non-violent crimes. Using an event study framework I investigate whether the results are due to new crime or spatial redistribution of existing crime, finding evidence of both effects. Overall, expanded liquor retailing appears to have had a significant causal effect on crime.

1.1 Introduction

The issue of "spillover" crime from liquor retailing dominates local debates over alcohol policy. Neighborhood activists routinely oppose new liquor stores, warning of subsequent street crime and urban decay. Retailers counter that they are themselves victims of crime, the targets of theft and burglary while attempting to serve local residents. This ongoing debate is reflected in the divided nature of U.S. state liquor laws, with 32 states exhibiting minimal restrictions on liquor retailing while 18 "control" states maintain heavy regulations or state-owned and operated liquor retailing systems.

At the heart of this debate is a simple empirical question: What is the causal effect of liquor retailing on neighborhood crime? Despite a large and diverse academic literature addressing that question, convincing answers remain elusive. Dozens of studies in the public health, epidemiology and sociology literatures have established, with varying degrees of sophistication, a clear correlation between liquor retailing and a variety of social problems including crime, traffic accidents, domestic abuse, youth violence and more. However, none of the existing research makes use of exogenous variation in liquor availability, delivering at best conditional correlations between liquor outlets and crime. Despite its limitations, this body of research has been embraced by reformers in recent years, leading in one case to a proposal to shutter hundreds of existing liquor stores in a major U.S. city.[1]

This study contributes to the literature by exploiting a unique source of identifying variation to estimate the causal effect of liquor outlets on crime: a 2012 privatization of liquor retailing in Washington State. Following privatization, the number of liquor retailers in the City of Seattle grew more than six-fold from 20 to 134. A key provision of the policy change was a requirement that all new liquor retailers occupy commercial spaces of 10,000 square feet or above. This led nearly all expansion of liquor availability to occur at the chain level as liquor permits were approved for essentially all large, preexisting grocery and drug store

[1] See Meredith Cohn, "Baltimore to Strip Some Liquor Stores of Licenses in Rezoning Effort," June 18, 2012, *Baltimore Sun* (http://bit.ly/1ipybK2).

chains in the Seattle area. This rapid chain-level expansion into a broad swath of Seattle neighborhoods breaks the endogenous link between crime and retailer location decisions—both over time and across geographic space—that has plagued past research.

Combining information on liquor retail locations and data from Seattle police reports during the 33-month period surrounding privatization, I construct a series of longitudinal panels at various levels of geographic detail to assess the impact of liquor retailing on crime. I pursue two identification strategies. First, I estimate the effect of changes in distance to the nearest liquor retailer on neighborhood crime using a standard fixed-effects panel model, which is equivalent to a difference-in-differences estimator with continuous treatments and multiple periods. Second, I estimate the longer-term effect on crime trends surrounding newly opened liquor retailers using an event study framework. By incorporating various lags into the former strategy I am able to explore intertemporal "learning" effects of variation in liquor availability over time. Similarly, by examining crime in a series of concentric rings around new retailers in the latter strategy I am able to examine interspatial effects such as whether the impact on crime is due to additional criminal activity or simply a redistribution of existing crime inward from nearby areas.

In both approaches, I find a clear causal link between liquor retailing and crime. Using a fixed-effects panel approach, reducing the distance to the nearest liquor retailer by one mile increases total crime by 6.5 to 8.2 percent in the current month, and 5.4 to 6.2 percent in the subsequent month in nearby areas. When I decompose total crime into violent crime, nonviolent crime, shoplifting and drug crime the model reveals an interesting intertemporal pattern. Shoplifting, drug crime and nonviolent crime appear to respond immediately to contemporaneous changes in liquor availability, while violent and other "spontaneous" crimes plausibly related to alcohol consumption show effects only after a one-month lag. I find a similar pattern in all six geographic levels of detail, and my results are robust to estimation both in first differences via OLS and in levels via a negative binomial model for count dependent variables. As a placebo test I show that unlike current and past changes in liquor availability, future leads of liquor distance have no effect on crime. As a robustness check I implement a 2SLS strategy using predicted liquor distance from the policy change as an instrument for observed distance, and find nearly identical results.

Using an event study approach, I find that opening a new liquor retailer leads to an average increase in total crime of 8.5 to 9.4 percent in the surrounding 0.1-mile radius area. Violent crime and drug crime are most clearly affected, increasing an average of 13.0-16.4 percent and 62.5-67.3 percent, respectively, while the effects on shoplifting and nonviolent crimes are more ambiguous. To assess whether the effects are due to redistribution of existing crime inward from nearby areas, I examine crime in two progressively more distant buffer rings around new retailers of 0.1-0.25 miles away, and 0.25-0.5 miles away. I find weak evidence that part of the effect of liquor retailing on nonviolent and shoplifting crimes is due to an inward spatial redistribution of preexisting crime. However, I find no evidence of spatial redistribution in the case of violent crime and drug crime, suggesting these effects are the result of additional criminal activity that would not have otherwise occurred in the absence of expanded liquor retailing.

I organize the remainder of the paper as follows. Section 1.2 reviews the related literature. Section 1.3 provides policy background on liquor privatization in Washington State. Section 1.4 presents a conceptual framework for my empirical strategy. Sections 1.5 and 1.6 present my data and identification strategy. Section 1.7 presents the empirical results, and Section 1.8 concludes.

1.2 Related Literature

There is a large and diverse literature examining the link between crime and liquor retailing.[2] Beginning in the early 1990s, the growth in geographic information systems (GIS) software and data led to a large number of empirical studies of the impact of liquor outlets on a variety of urban problems. The literature can be broadly divided into two groups: studies that use cross-sectional methods, and

[2]Extensive surveys of this literature are presented in White et al. (forthcoming), Roman et al. (2008) and Gruenewald et al. (1996).

studies using longitudinal or panel methods.

1.2.1 Cross-Sectional Studies

The vast majority of research has been cross sectional.[3] The typical study focuses on a single metropolitan area and uses variation in liquor density across Census blocks, Census tracts, ZIP codes, or other neighborhood areas at a single point in time to identify the effect on crime. Most authors focus on assaults, homicides, robbery and other violent crimes, although traffic accidents, domestic violence and youth violence have also been examined. Studies in this vein have been conducted for over a dozen cities including Austin, Camden, Chicago, Cincinnati, Detroit, Kansas City, Minneapolis, New Orleans, Norfolk, Los Angeles, Washington, D.C. and others. Without exception, the cross-sectional literature speaks with one voice in reporting a positive relationship between violent crime and liquor availability.

The basic weakness of this literature is the failure to identify causality. Liquor retailers are not randomly assigned throughout neighborhoods; like all firms, they endogenously choose retail locations. This process of firms optimally sorting into areas over time leads to a highly non-random assignment of retailers to neighborhoods, corrupting the basic identifying variation in cross-sectional studies. Firms select locations based partly on unobservable neighborhood characteristics that are likely correlated both with the profitability of liquor retailing and the prevalence of crime.

One cross-sectional study that attempts to isolate exogenous variation in retailer locations is Gyimah-Brempong (2001). The author employs a two-stage least squares strategy using two instruments for liquor density: (1) median area rent, and (2) count of area gas stations. Comparing OLS and 2SLS estimates, the author concludes that naive OLS estimates are downward biased, implying

[3] Cross-sectional studies include Grubesic and Pridemore (2011); Liang and Chikritzhs (2011); Franklin et al. (2010); Resko et al. (2010); Scribner et al. (2010); McKinney et al. (2009); Jones-Webb et al. (2008); Roman et al. (2008); Gruenewald et al. (2006); Britt et al. (2005); Zhu et al. (2004); Reid et al. (2003); Lipton and Gruenewald (2002); Gorman (2001); Gyimah-Brempong (2001); Scribner et al. (1999); Stevenson et al. (1999); Gorman et al. (1998); and Scribner et al. (1995).

negative selection by firms away from high-crime areas. Unfortunately there are serious concerns about instrument validity, a limitation acknowledged in the paper. Density of gas stations is likely to be correlated with the same unobservable drivers of neighborhood crime contained in the error term that also affect the location of liquor outlets; after all, both establishments are firms endogenously choosing locations. Under such a failure of instrument validity, 2SLS estimates suffer from the same bias and inconsistency as naive OLS estimates, although possibly of different sign and magnitude, and do not identify causal effects.

1.2.2 Longitudinal Studies

A smaller number of studies have been longitudinal.[4] The typical study uses a panel of N neighborhoods in a metropolitan area over T periods, using within-area variation over time to identify the effect of liquor outlets on crime. A number of cities have been examined in this way, including Los Angeles, Melbourne, Norfolk, various counties in Texas and others. The earliest longitudinal study appears to be Gruenewald and Remer (2006) who examine the effect of liquor outlets on crime in 581 ZIP-code areas in California during a 6-year period. This was followed soon after by Teh (2007) who examines crime surrounding liquor outlets in Los Angeles between 1992 and 2004 using an event study framework. As with the cross-sectional literature, longitudinal studies overwhelmingly find a positive relationship between liquor outlets and violence.

The main advantage of panel methods is well known: they allow researchers to control for unobserved area heterogeneity in a way that is impossible in cross-sectional studies. The usual fixed-effects (FE) panel estimator makes use of within-area variation in liquor outlets, a much cleaner source of identification than cross-sectional estimates. However, longitudinal data alone do not allow the identification of causal effects without strong identifying assumptions. Just as in the cross-section, changes in the presence of liquor outlets within areas over time are the result of endogenous firm location decisions, and may be correlated with un-

[4]Longitudinal studies include White et al. (forthcoming); Tang (2013); Livingston (2011); Parker et al. (2011); Cunradi et al. (2011); Yu et al. (2008); Teh (2007); and Gruenewald and Remer (2006).

observed drivers of crime.

This study contributes to the literature by making use of a unique source of identifying variation to estimate the causal effect of liquor retailing on crime: the 2012 privatization of liquor retailing in Washington State. The policy ushered in a rapid expansion of liquor availability to pre-existing grocery and drug store chains, providing plausibly exogenous identifying variation in liquor availability both across neighborhoods and over time. This quasi-experimental variation allows us to identify the causal effect of expanded liquor retailing on neighborhood crime.

1.3 Policy Background

In November 2011, Washington State voters approved ballot initiative I-1183, implementing wide-ranging reforms to the state's liquor retailing and distribution system.[5] Previously the industry had been state-owned and operated for more than seven decades under the supervision of the Washington State Liquor Control Board (WSLCB). Beginning June 1, 2012, the initiative ended the state's monopoly on liquor retailing, closing state stores and liquidating the assets at auction. Before the policy, there were 329 liquor retailers statewide with 20 located in Seattle. One year after the policy, more than 1,400 liquor retailers were in operation with 133 in Seattle. Following the privatization, 18 U.S. "control" states remain that maintain some form of state-controlled liquor retailing and distribution system.[6]

The key provision of I-1183 was a requirement that all new liquor retailers occupy commercial spaces of 10,000 square feet or larger.[7] Ostensibly, the provi-

[5]By "liquor" I refer specifically to alcoholic spirits such as vodka, whiskey and other distilled beverages. Beer and wine are privately retailed in Washington State and were largely unaffected by I-1183. The full text of I-1183 is available at http://www.sos.wa.gov/elections/initiatives/text/i1183.pdf.

[6]The remaining control states are Alabama, Iowa, Idaho, Maryland, Maine, Michigan, Mississippi, Montana, North Carolina, New Hampshire, Ohio, Oregon, Pennsylvania, Utah, Virginia, Vermont, West Virginia and Wyoming. Source: National Alcohol Beverage Control Association (http://www.nabca.org).

[7]Two exceptions are allowed for the square-footage provision: (1) a "grandfathering" clause for former state-owned liquor stores, and (2) retailers in "trade areas" where no building exists that meets the 10,000 square-foot requirement. A "trade area" is defined as having no other liquor retailer within 20 miles, and no trade-area exemptions had been granted at the time of

sion was designed to alleviate concerns about growth in smaller "nuisance" liquor stores following privatization. However, it had the effect of channeling nearly all expansion of liquor retailing into preexisting grocery and drug store chains satisfying the space requirement. The expansion occurred almost exclusively at the chain level, into every size-compliant retail location as permits were approved en masse by the WSLCB. For example, of the 20 Seattle locations of Bartell's Drugs, 18 stores satisfy the space requirement.[8] Of these, all 18 obtained liquor licenses as of September 2013, with 17 approved the day the policy went into effect. Similarly, of the 15 size-compliant Walgreens drug stores in Seattle, all 15 obtained liquor licenses within the first two months of the policy. In these and similar cases, the policy expanded liquor availability in a way that was unaffected by endogenous selection either in timing or location. Of the 108 liquor retailers granted permits during the first three months of privatization, 97 were in similar large, established grocery and drug chains including Albertson's, Cost Plus World Market, Costco, Fred Meyer, Kress IGA Supermarket, QFC, Rite Aid, Safeway, Target, Trader Joe's and Whole Foods.[9] Each of these chains had selected locations years and in some cases decades before the policy change for reasons presumably unrelated to liquor retailing. This expansion into a broad swath of Seattle neighborhoods provides time- and area-exogenous variation in liquor availability that can be used to identify the causal effect on crime.

In addition to increasing the number of retailers, the policy also expanded the number of hours per day when liquor is available for purchase. The combination of expanded retail outlets and broadened for-sale hours led to a statewide increase in liquor consumption following privatization. Despite higher retail prices due to the policy's increased liquor taxes, liquor consumption grew by roughly 7 percent in the nine months following privatization compared with a similar period in the prior year.[10] Following privatization, numerous media outlets reported a surge in

this writing.

[8]The noncompliant locations are at 4344 University Way (University District) and 1820 N. 45th Street (Wallingford).

[9]The remaining 11 stores were independent, non-chain grocery stores and wine merchants satisfying the space requirement.

[10]Liquor consumption averaged 2,521,843 liters from June 2011 to February 2012, compared with 2,699,263 liters from June 2012 to February 2013. Source: Washington State Department

liquor shoplifting in newly privatized retailers.[11] One of the few reports on impacts on overall crime comes from an NBC story from October 23, 2013 reporting that alcohol-related arrests continued their downward trend following privatization.[12] Aside from these occasional media reports, there has been no systematic study to date of the effect of Washington State's liquor privatization on ancillary crime.

1.4 Conceptual Framework

1.4.1 Basic Model

To help motivate my empirical strategy I present a simple model linking crime and retail liquor availability. The model is a straightforward extension of the classic Becker (1968) theory of criminal behavior, which models the individual decision to engage in illicit activity as a function of expected criminal penalties, expected gains from the activity and preferences. The presentation closely follows Ehrlich (1973), and similar models are presented in Gyimah-Brempong (2001) and Markowitz and Grossman (1998a, 1998b).

In Ehrlich (1973) an extension of the Becker (1968) model is developed in which individuals choose between legal and illegal behavior based on a standard utility maximization problem under uncertainty. Individuals maximize utility over a basket of goods—including earnings from both legal and illegal activities—and leisure, subject to a time constraint on hours spent in legal and illegal activities. Expected utility is maximized over two possible states of the world: (1) apprehension and punishment for illicit behavior, and (2) getting away with crime. The resulting optimal division between time spent in legal versus illegal activity is shown to depend on the probability of apprehension, the expected penalty if

of Revenue.

[11]See for example Jeremy Pawloski, "Teen Shoplifting, Liquor a Bad Mix," The Olympian, December 9, 2012 (http://bit.ly/MXXXdY); Kendall Watson, "State May Begin Requiring Stores to Report Liquor Thefts," Mercer Island Patch, February 21, 2013 (http://bit.ly/1cRza4E); and Michelle Esteban, "Stores Seeing Huge Spike in Liquor Thefts," KOMO News, November 1, 2012 (http://bit.ly/1brm25w).

[12]See Rachel Hoops, "Alcohol-Related Arrests Continue to Decrease After Liquor Privatization in Washington State," NBC News, October 23, 2013 (http://bit.ly/1c0uTYJ).

apprehended, and the relative economic returns from legal and illegal activities.

A useful feature of this class of models is that it is possible to derive a reduced-form "supply of offenses" function via the usual comparative statics, which specifies the causal determinants of crime at time t as,

$$c_{it} = \phi_{it}(p_{it}, f_{it}, w_{it}^L, w_{it}^I, \pi_{it}) \tag{1.1}$$

where c_{it} is the count of crimes committed in area i and period t, p_{it} is the probability of apprehension by local police, f_{it} is the criminal penalty if apprehended, w_{it}^L and w_{it}^I are the economic returns to legal and illegal activity respectively, and π_{it} is a collection of other socioeconomic factors that exert a causal effect on crime. It is straightforward to show that $\partial c_{it}/\partial p_{it}$, $\partial c_{it}/\partial f_{it}$, $\partial c_{it}/\partial(w_{it}^L - w_{it}^I) < 0$ under mild regularity conditions, so that crime is negatively related to the probability of apprehension, the severity of penalties, and the relative economic returns to legal and illegal activities.

Following Gyimah-Brempong (2001) I connect alcohol consumption to this model by specifying it as one of the "other" socioeconomic factors contained in the vector π_{it}, so that,

$$\pi_{it} = (l_{it}, z_{it}) \tag{1.2}$$

where l_{it} is liquor consumption in area i and period t and z_{it} is all other socioeconomic determinants of crime. There is a well-established basis for doing so: the causal link between individual alcohol consumption and physical violence and aggression has been confirmed by a large number of experimental and observational studies throughout the epidemiology and psychology literatures.[13] A variety of theories have been proposed regarding the exact physiological and psychological mechanisms by which alcohol induces violent behavior,[14] but while interesting, the underlying mechanisms are unimportant from the standpoint of modeling the observed effect on crime. For the purposes of the descriptive model, I treat the increased likelihood of crime as a negative consumption externality from alcohol. For

[13]For example, see Parker and Auerhahn (1998), Chermak and Taylor (1995), Taylor and Chermak (1993) and the various studies cited therein.

[14]See for example Parker and Auerhahn (1998).

simplicity I assume a locally monotonic relationship between alcohol consumption and these associated crime externalities so that $\partial c_{it}/\partial l_{it} > 0$.

The link between retail liquor locations and alcohol consumption is provided via a standard consumer demand model. Individuals maximize utility from alcohol and other goods subject to prices, incomes and travel distances to the nearest retail locations. For each area i in period t a typical consumer solves,

$$\underset{l_{it},x_{it}}{\text{maximize}}\ U(l_{it}, x_{it}) \text{ subject to } (p^l_{it} + d^l_{it}t)l_{it} + (p^x_{it} + d^x_{it}t)x_{it} \leq Y_{it} \tag{1.3}$$

where l_{it} is liquor consumption, x_{it} is a composite of all other goods, p^l_{it} and p^x_{it} are prices of liquor and all other goods, d^l_{it} and d^x_{it} are the distances per unit to the nearest consumption point (i.e., the nearest retailer) for goods l and x, and t is the mean cost of travel per distance.[15] Thus, in addition to the money price of liquor p^l_{it}, the term $d^l_{it}t$ represents the cost per unit consumers bear for travel to the nearest liquor retailer. Denote the optimal solutions $l^*(p^l, p^x, d^l, d^x, Y)$ and $x^*(p^l, p^x, d^l, d^x, Y)$.

The effect of changes in distance to the nearest liquor retailer on liquor demand, and thus indirectly on crime, can be seen via the usual comparative statics. Totally differentiating the first-order conditions from (1.3) we can show that,

$$\frac{\partial l_{it}}{\partial d^l_{it}} = \frac{(t/(p^x_{it} + d^x_{it}t))\,\frac{\partial U}{\partial x_{it}}}{\frac{\partial^2 U}{\partial l^2_{it}}} = \frac{(+)}{(-)} < 0 \tag{1.4}$$

As expected, the model predicts a simple negative relationship between distance to the nearest liquor retailer and liquor consumption. The resulting effect on crime is easily obtained by substituting the Marshallian liquor demand $l^*(\cdot)$ into the crime equation from (2.3) and differentiating with respect to retailer distance d^l_{it} which yields,

$$\frac{\partial c_{it}}{\partial d^l_{it}} = \frac{\partial c_{it}}{\partial l_{it}}\frac{\partial l_{it}}{\partial d^l_{it}} = (+)(-) < 0 \tag{1.5}$$

This inverse relationship between crime and distance to the nearest liquor retailer provides the conceptual basis for the empirical strategy presented in Section 6.[16]

[15] I make the usual assumptions that utility is differentiable, strictly increasing and quasiconcave. To simplify the math below I also assume without loss of generality that utility is separable in l and x so that the cross partial $\partial^2 U/\partial l_{it} x_{it} = 0$.

[16] Much of the previous literature has modeled a relationship between crime and the count of

1.5 Data

The crime data consist of 166,393 police incident reports from the Seattle Police Department between January 2011 and September 2013. They include all reported crimes for which an incident report was filed by officers during the 33-month period. Crimes are coded with 193 unique offense codes, including assault, theft, public disturbance, property damage, fraud, harassment, homicide, narcotics offenses, burglary and more. Table 1.1 presents the count and frequency of the 10 most commonly reported offenses during the sample period.

The crime reports are coded with two separate geographic identifiers: the approximate street address of the offense (known as the "hundred block location"), and the latitude and longitude. Approximately 2,200 of the roughly 166,000 reports had either blank or clearly incorrect geocoding, and these offenses were recoded using the hundred block location and the MapQuest Geocoding API web service.[17] Roughly 200 offenses had no geocoding nor hundred block location, and these were omitted from the file. All offenses falling outside city limits were also excluded, based on city boundary files provided by the Seattle Public Utilities' GIS unit.[18]

For the analysis I classified crimes into five categories:[19] (1) Total Crime, which consists of all reported offenses; (2) Violent Crime, which consists of assaults, property damage, harassment, robbery and homicides as well as other "spontaneous" types of offenses plausibly related to alcohol consumption such as drunk driving, public urination, liquor law violations, disturbances and disorderly conduct; (3) Non-Violent Crime, which is composed of total crime minus the "violent crime" category; (4) Shoplifting, which consists of retail theft offenses; and (5) Drug Crime, which consists of all narcotics-related offenses including possession, trafficking, manufacturing and smuggling.

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liquor outlets, rather than minimum distance to the nearest retailer. To the extent that outlet counts are a proxy for minimum distance, the approaches will yield similar results. However, because minimum distance is more clearly grounded in microeconomic theory I use distance as my measure of liquor availability.

[17]Information about the MapQuest Geocoding API service is available at http://developer.mapquest.com/web/products/dev-services/geocoding-ws.

[18]Official GIS boundary files for the City of Seattle are available at http://www.seattle.gov/gis/.

[19]A complete crosswalk of offense codes into crime categories is included in the Appendix.

Data on the location of liquor retailers are from public records provided by the Washington State Liquor Control Board (WSLCB). As in most municipalities, liquor retailers are classified into on- and off-premises establishments. WSLCB data provide the business name, street address and active date of the liquor license for all establishments. For each of the 33 months from January 2011 to September 2013 I constructed a historical listing of active on- and off-premise liquor establishments in Seattle. I then geocoded the locations using street addresses and the MapQuest Geocoding API web service.

Using the geocoded crime and liquor-location data, I compiled six area-month panels at varying levels of geographic detail: (1) Census blocks, (2) Census block groups, (3) Census tracts, and three uniform rectangular grids that partition the city into (4) 120 x 120 areas (442 feet wide by 731 feet long), (5) 50 x 50 areas (1,060 feet wide by 1,756 feet long), and (6) 25 x 25 areas (2,121 feet wide by 3,511 feet long). For each area and month, I coded a Python script in ArcGIS to perform a spatial join between crimes and areas, providing crime counts for area i in month t. Similarly, I calculated the minimum distance to the nearest on- and off-premise liquor establishment from the center point of each area in each month. The process resulted in six distinct longitudinal files, each with crime and liquor availability for N neighborhoods over T months. In Section 7, I present results for the largest and most detailed of the panels at the Census block level, and all other results are presented in the Appendix. Table 1.2 presents summary statistics for the Census-block-level panel.[20]

To help visualize the rapid expansion of liquor availability following privatization, Figure 1.1 plots the locations of Seattle retailers before and after the policy. The left panel shows liquor outlets two months before privatization in April 2012. The right panel shows liquor outlets 16 months after privatization in September 2013. The map lines show the city's 134 Census tracts. During the pre-policy period there were 20 state-owned liquor outlets. By September 2013 that number had expanded to 134 retailers. As is clear from the figure, the expansion was broad-based and affected virtually every neighborhood in the city. This broad pattern

[20]Summary statistics for all six panels are available upon request.

of expansion is largely the result of the 10,000-square-foot requirement for new retailers, and reflects the location of the city's preexisting large grocery and drug store chains. Figure 1.2 shows the resulting PDFs for the distribution of distance to the nearest liquor retailers among Census tracts during the pre- and post-policy periods. The pre-policy distribution is shown with wide grey bars, and the post-policy distribution is shown with narrow black bars. The pronounced leftward shift in the distribution of distances to the nearest retailer is clear from the figure, illustrating the broad-based nature of the retail expansion following privatization.

Figure 1.3 illustrates the basic identifying variation in distance to the nearest liquor retailer. For each of the 134 Census tracts in the city, it shows the distance to the nearest liquor retailer in feet from six months before privatization in December 2011 to 16 months after privatization in September 2013. The left panel shows the distance to the nearest retailer in levels, while the right panel shows changes or first-differences from the previous month. For reference, the policy change occurs in $t = 18$ along the horizontal axis. Liquor distance temporarily rose in a small number of neighborhoods as the WSLCB closed 14 retailers statewide in the months leading up to the privatization, three of which were in Seattle.[21] On June 1, 2012, roughly 80 new retailers began selling liquor, almost exclusively large grocery and drug stores. Distances to the nearest retailer fell dramatically in most neighborhoods as permits were approved by the WSLCB in the subsequent months.

Figure 1.4 shows the evolution of total crime counts in "treatment" and "control" neighborhoods before and after the I-1183 policy change. The top line corresponds to the most heavily treated Census blocks, which fall into the top decile in terms of percentage drop in distance to the nearest liquor retailer following privatization. The bottom line corresponds to the most lightly treated areas, which fall into the bottom decile which experienced little or no change in liquor distance. As above, the policy change occurs at $t = 18$ in the figure. Overall, crime trends in the two areas are similar both before and after privatization. However, two patterns

[21] See Megan Managan, "Mercer Island Liquor Store Closes Thursday, Store Sold at Auction for $200,000," *Mercer Island Reporter*, April 23, 2012. Available at http://www.mi-reporter.com/news/148562485.html.

are clear in the figure. First, treatment areas experience an upward bump in crime at the time of the policy change that, while small, is noticeably larger than in control neighborhoods. Second, total crime appears to be somewhat more volatile in treatment areas during the post-policy period than in control neighborhoods. Both patterns are broadly suggestive of a possible causal relationship between proximity to liquor retailing and neighborhood crime trends.

1.6 Identification Strategy

I pursue two identification strategies. First, I estimate a standard fixed-effects (FE) panel model to identify the effect of variation in the distance to the nearest liquor retailer on crime rates, via OLS and 2SLS. Second, I estimate an event study framework to identify the effect on crime rates in narrow areas surrounding liquor retailers before and after new store openings. The former strategy allows us to explore intertemporal "learning" effects of liquor availability on crime, while the latter strategy allows us to identify interspatial effects such as redistribution of preexisting crime between areas.

1.6.1 Fixed-Effects Panel Model

As a starting point, consider a standard fixed-effects panel model of the form,

$$y_{it} = \alpha_i + \gamma_t + \eta_i t + X_{it}^{'}\beta + d_{it}\delta + \epsilon_{it}, \qquad i = 1, ..., N, \quad t = 1, ..., T \tag{1.6}$$

where y_{it} is the number of crimes in area i at time t; α_i and γ_t are area- and time-specific fixed-effects; $n_i t$ is an area-specific fixed time trend; X_{it} is a vector of observable time-varying area determinants of crime; d_{it} is distance to the nearest liquor retailer; and ϵ_{it} is a mean-zero error term. The coefficient of interest is δ, which gives the effect of distance to the nearest liquor retailer on crime. Based on the model from Section 4 I expect to find $\delta < 0$. Due to the exogenous nature of the identifying variation in d_{it} we can interpret the resulting estimate of $\hat{\delta}$ as the causal effect of distance to the nearest liquor retailer on crime. It is straightforward to show this approach is equivalent to a difference-in-differences estimator with arbitrary continuous treatments over T periods.[22]

One advantage of the above specification is that it allows us to investigate possible learning behavior over time due to changes in liquor availability in neighborhoods. Alcohol-related crime may not adjust immediately to openings of new liquor retailers, and may instead adapt slowly over time to the changing retail landscape. To allow for this possibility I estimate a version of (1.6) that includes a series of lagged distances to the nearest retailer,

$$y_{it} = \alpha_i + \gamma_t + \eta_i t + X_{it}^{\prime}\beta + \sum_{j=0}^{3} d_{it-j}\delta_j + \epsilon_{it} \tag{1.7}$$

where the terms $d_{it}, d_{it-1}, ..., d_{it-3}$ are the contemporaneous and three lagged values of distance to the nearest liquor retailer. Equation (1.7) is the basic estimating equation for the fixed-effects model. The coefficients of interest are δ_0, δ_1, δ_2 and δ_3, which allow us to assess the intertemporal effects of liquor availability on crime for up to three subsequent months. The vector X_{it} consists of contemporaneous and three lagged values of the distance to the nearest on-premises bar or restaurant for each area and month.[23] As a placebo test, I also estimate a version of (1.7) that includes three leads of future distance to liquor retailers, illustrating that future liquor availability has no effect on contemporaneous crime as expected; I present these results in the Appendix.

To exploit my cleanest form of identifying variation I estimate equation (1.7) via OLS in first differences. Thus, the estimates are identified off month-to-month changes in crime counts and liquor distances rather than absolute levels. As a robustness check, I also estimate (1.7) in levels using a negative binomial model, a conventional approach for count dependent variables; I also present these results in the Appendix. For the purposes of presentation, I focus on the linear panel model for the simplicity of the estimation procedure and the straightforward interpretation of coefficients as the marginal causal effect of liquor availability on

[22]See Imbens and Wooldridge (2007), in particular Equation (4.5).

[23]Because on-premise locations were unaffected by the 2012 privatization, the estimates of $\hat{\beta}$ do not have a causal interpretation and are presented as an exhibit only.

crime. To show the importance of including area and time fixed effects in the specification, the first two columns of all tables show results that exclude them.

As an additional robustness check we also estimate equation 1.7 via 2SLS. While the above OLS estimates rely partly on exogenous variation in liquor availability, it is possible that they do not completely isolate the exogenous component. Following privatization, some retail chains might have endogenously selected which locations obtained liquor permits; some state stores may have endogenously closed; or some independent retailers opening months after the policy change may reflect endogenous firm location decisions. To address these concerns I implement an IV procedure designed to isolate only the exogenous variation. First, I construct a counterfactual distribution of liquor retailers in which (1) all retail chains that obtain liquor licenses do so at once for all locations; (2) all former state stores remain open; and (3) no independent retailers open later than June 2012. These counterfactual retail locations are then used to calculate a "projected liquor distance" variable. The fitted values from the first-stage regression of actual liquor distance on projected distance (along with distances to on-premise locations and area and time fixed effects) isolate the exogenous variation in liquor availability that is predictable from the policy change.

The sample period for my fixed-effects estimation is the 22 months from December 2011 to September 2013, which makes use of 110,346 crime reports. I estimate equation (1.7) using six panels for Census blocks, Census block groups, Census tracts, and three uniform grids dividing the city into 25 x 25, 50 x 50, and 120 x 120 areas. For each panel I use five dependent crime variables for y_{it}: (1) total crime, (2) violent crime, (3) nonviolent crime, (4) shoplifting and (5) drug crime. As with most spatial data, observations from nearby neighborhoods are unlikely to be statistically independent, with the degree of dependence growing more severe the closer the neighborhoods. The narrow-area panels exhibit a high degree of cross-sectional spatial autocorrelation between areas. To account for this feature, I report Driscoll and Kraay (1998) cluster- and auto-correlation-robust standard errors, which use a nonparametric covariance matrix estimator that is robust to very general forms of spatial and temporal heteroskedasticity and autocorrelation.

I implement Driscoll-Kraay standard errors using a 3-period lag structure and the *xtscc* Stata command, and report the resulting t-statistics in the tables below.[24]

1.6.2 Event Study Framework

To assess the interspatial effects of liquor retailer openings on nearby crime, I estimate an event study framework similar to Teh (2007). Detailed discussions of the event study methodology are available in Binder (1998) and Fama et al. (1969). The conceptual approach is illustrated in Figure 1.5. The policy "event" is the exogenous opening of new liquor retailers in Seattle following the 2012 privatization, which occurs at $t = 0$ in the figure. For each store opening, I examine crime trends in the surrounding neighborhood based on 14 months of observations before and after the event. I allow the intercepts and slopes to differ on either side of the policy change, labeled γ and δ in the figure. The estimate of γ identifies the local-area causal effect on crime trends from the exogenous opening new liquor retailers.

In the three months following the June 2012 privatization, 108 new retail locations opened in Seattle. Using ArcGIS software I drew circular 0.1 mile buffers around each location. These areas surrounding new retailers serve as the basic panel unit for the event study. For each area and month, I compiled counts of offenses for each of the five crime categories from January 2012 through September 2013. Additionally, I calculated the distance to the nearest on-premises bar or restaurant from each area. I used 14 months of observations on either side of these 108 store openings for the estimation, resulting in a panel of size $NT = 3,024$. To explore the interspatial effects of store openings on nearby crime, I examined two concentric rings surrounding new retailers extending outward from 0.1-0.25 miles and from 0.25-0.5 miles. Examining crime trends in these concentric buffer rings allows us to assess whether store openings induced new criminal activity or simply redistributed preexisting crime inward toward retailers from nearby areas.

The basic estimating equation for the event study is,

$$y_{it} = \alpha_i + \lambda_t + \eta_i t + \delta_i t \mathbb{1}\{t > 0\} + \gamma \mathbb{1}\{t > 0\} + X_{it}^{\prime}\beta + \epsilon_{it} \tag{1.8}$$

[24]See Driscoll and Kraay (1998) and Hoechle (2007).

where y_{it} is crime surrounding liquor retailer i in month t. The time variable is scaled so that $t = 0$ at the time of opening for each retailer and ranges from $t = -14$ to $t = 14$. Retailer-specific and month-specific fixed effects are given by α_i and λ_t, and $\eta_i t$ is a retailer-specific fixed time trend estimated for the full 28-month period. The term $\delta_i t$ is an additional retailer-specific time trend estimated only for the post-policy period when $t > 0$, allowing trend slopes to flexibly vary on either side of the event. The coefficient of interest is γ, which corresponds to the post-policy intercept-shifter depicted in Figure 1.5, and gives the causal effect of exogenous store openings on area crime trends. X_{it} contains the minimum distance to the nearest on-premise bar or restaurant from area i at time t, and ϵ_{it} is a mean-zero error term. Note that on-premise bar and restaurant locations in X_{it} may change endogenously as a result of liquor store openings, and thus the estimate of $\hat{\beta}$ does not have a causal interpretation. I estimate equation (1.8) using three panels: (1) 0.1 mile areas around new liquor retailers, (2) 0.1-0.25 mile buffer areas and (3) 0.25-0.5 mile buffer areas. As with the fixed-effects panel model above, all specifications report t-statistics based on Driscoll and Kraay (1998) cluster- and auto-correlation-robust standard errors to account for cross-sectional spatial autocorrelation in the data.

1.7 Results

1.7.1 Fixed-Effects Panel Results

Results for Census Block Panel

Census blocks are the smallest geographic unit defined by the U.S. Census Bureau, and they divide Seattle into 11,485 neighborhood areas, many of which correspond to a single city block. Figure 1.6 illustrates the Census block areas based on GIS boundary files provided by Seattle Public Utilities. When combined with 22 monthly observations of crime and liquor locations, the resulting panel contains $NT = 252,670$ month-area observations.

Tables 1.3 through 1.7 present my basic results. Each table shows the regression of a different category of crime on distance to the nearest liquor retailer, three lags of liquor distance and distances to the nearest on-premise bar or restaurant. Table 1.3 shows the effect on total crime, while Tables 1.4, 1.5, 1.6 and 1.7 show the effects on violent crime, non-violent crime, shoplifting and drug crime, respectively. Columns (1) and (2) are estimated in levels, and are presented as an illustration of the effect of excluding time and area fixed effects from the model. Columns (3) through (6) are estimated in first-differences, and Column (6) corresponds directly to my estimating equation. All coefficients have been scaled to represent the marginal effect of a one-mile change in the distance to the nearest liquor retailer. I also report the marginal effect relative to the mean for the coefficients of interest. I report t-statistics in parentheses based on Driscoll-Kraay cluster- and auto-correlation robust standard errors.

The impact of liquor availability on total crime is evident from Table 1.3. All estimated coefficients on distance to the nearest liquor retailer are negative as predicted by economic theory. Column (1) is the pooled OLS estimate that excludes all fixed effects and time trends, and results in a biased estimate of a 68.5 percent increase in crime. The effect falls significantly to a 16.4 percent increase when area fixed effects are included in Column (2). The effect shrinks further when both area and time fixed effects are included, along with area-specific time trends. In Column (6) we add three lags of distance to the nearest liquor retailer, and find that both contemporaneous liquor distance and the first lag have significant effects on total crime. The effect is large: reducing the distance to off-premises liquor retailers by one mile in a typical area increases crime by 8.2 percent in the current period, and 6.2 percent in the following period. Neither the second nor third lag of liquor distance is significant, suggesting whatever intertemporal “learning” that occurs with respect to liquor availability and crime takes place within the first two months of retailer openings.

Table 1.4 shows the effect on violent crimes, including assaults, property damage, harassment, robbery, homicide and other plausibly alcohol-related offenses such as drunk driving, liquor law violations and disorderly conduct. As with total crime, the pooled OLS estimate in Column (1) is large but shrinks considerably as fixed effects and lags are included. In Column (6), I find a lagged structure to the effect on violent crime. Contemporaneous changes in liquor availability appear to have little effect on violent crime. Instead, the first lag of liquor availability exerts a large and significant effect of a 19 percent increase in violent crime. The second and third lags of liquor distance have no additional effect, suggesting the impact of liquor availability on violent crime lags slightly behind store openings.

Table 1.5 shows the effect on nonviolent crime, which is the logical complement of violent crime above. I find a significant effect on nonviolent crime both from current and lagged distances to off-premises liquor outlets. Contemporaneous changes in liquor distance appear to increase reports of nonviolent crime by 11.5 percent in typical neighborhoods, while the second lag of liquor distance increases nonviolent crime by 3.3 percent. All other lags have small and insignificant effects. Results from the event study framework below suggest that unlike the effect on violent crime, this effect on nonviolent crime partly reflects a redistribution of existing crime inward toward liquor retailers rather than new crime. Overall, when total crime is decomposed into violent and nonviolent components we find the effect on contemporaneous total crime is largely due to nonviolent offenses, while the one-month lagged effect is due primarily to violent crimes.

Table 1.6 shows the results for shoplifting offenses. Following liquor privatization in Washington State a large number of media outlets reported a surge in shoplifting at newly privatized liquor retailers. The results appear to confirm those reports. Changes in liquor availability had a large and significant contemporaneous effect on shoplifting; reducing liquor distance by one mile increased shoplifting by 47.6 percent in a typical area in the same month. However, none of the lagged changes in liquor availability had a significant effect. When combined with the finding below from the event study that longer-term trends in shoplifting were unaffected by liquor availability, the evidence suggests whatever surge in shoplifting that occurred following privatization may have been a temporary effect.

Table 1.7 presents the results for drug offenses. Psychological and epidemiological research has suggested a link between alcohol and drug use that exhibits characteristics of both substitute and complementary goods.[25] I find a significant effect of expanded liquor availability on drug offenses. Contemporaneous liquor distance and its second lag both have a significant effect on drug crime, with one-mile effects of 36.8 percent and 23.1 percent, respectively. Neither the first nor third lags of liquor distance have a significant effect. The findings from the event study below suggest this effect is not due to a simple redistribution of narcotics offenses inward toward retailers from surrounding areas, and instead represents new crime. This pattern of effects is suggestive of a complementarity between alcohol availability and drug crime.

IV Results for Census Block Panel

Table 1.8 shows the first stage results from the 2SLS estimation. I use a "projected liquor distance" variable as an instrument for observed distance, based on a counterfactual distribution of retailers in which (1) retail grocery and drug chains stock liquor in all locations simultaneously, (2) all former state stores remain open, and (3) no independent retailers enter the market after June 2012. The part of observed liquor distance that is predictable from this policy exercise represents strongly exogenous identifying variation. In the table, the large first-stage F statistic illustrates the strength of the instrument, and an R-squared of over 96 percent suggests only a small fraction of the observed variation in liquor availability cannot be predicted by the policy change.

Table 1.9 shows the second stage of the 2SLS procedure. The columns correspond to each of the five categories of crime. In each case, I find results that are nearly identical to those obtained via OLS above. None of the estimated coefficients are significantly different from those presented in the previous section, suggesting the observed variation in liquor availability following privatization is sufficiently exogenous to allow us to identify the causal effect on crime via OLS.

[25]See for example Parker and Auerhahn (1998) and the literature discussed therein.

1.7.2 Event Study Results

I present results from the event study in three tables. Table 1.10 shows the effect on crime in the narrow 0.1-mile (528 feet) radius surrounding new liquor retailers. Tables 1.11 and 1.12 show the effect on crime in two progressively more distant buffer rings of 0.1-0.25 miles and 0.25-0.5 miles away from retailers, allowing us investigate whether store openings contribute to additional crime or simply redistribute existing crime inward from surrounding neighborhoods. In each table, the coefficient of interest is "Store Opening," corresponding to the intercept-shifter γ from my estimating equation 1.8. The columns display the effect on each of the five categories of crime. For each crime category, the first column corresponds directly to my estimating equation, while the second column includes an additional interaction term between store openings and distance to the nearest on-premises bar or restaurant to investigate whether the impact of liquor retailers is amplified or diminished by the proximity of on-premises establishments.

The impact of new liquor retailers on crime trends in surrounding areas is evident in Table 1.10. I find a positive and significant effect of liquor store openings on total crime, violent crime and drug crime. In Column (1), we see the opening of a new liquor retailer leads to a 9.4 percent average increase in total crime nearby. This effect is slightly diminished to 8.5 percent when an interaction term with distance to the nearest on-premises bar or restaurant is included in Column (2), but the effect still approaches statistical significance with a t-statistic of over 1.65. These estimates are nearly identical to the the average treatment effects of 6.5 percent to 8.2 percent found in the fixed-effect panel model from the previous section.

In Columns (3) and (4) we see that violent crimes were also affected by liquor store openings, resulting in an average increase of 13 percent, with the effect rising to 16.4 percent when the interaction term with on-premises locations is included. This provides some evidence that the presence of nearby bars and restaurants may amplify the causal effect of liquor retailing on violent crime. In Columns (5) and (6) I find no significant effect of liquor retailing on nonviolent crime. Similarly, I find no significant effect on shoplifting in Columns (7) and (8).

Both of these results are inconsistent with the findings from the fixed-effects panel model above, suggesting that the impact of liquor availability on nonviolent and shoplifting crimes may simply reflect a temporary increase, leaving longer-term crime trends unaffected.

Columns (9) and (10) show the impact on drug crimes, for which I find a positive and significant effect of liquor retailing. New liquor outlets lead to a large 67.3 percent increase in average drug crimes in the surrounding neighborhood, an effect that declines to 62.5 percent when an interaction term is included in Column (10). Taken together with the results from the fixed-effects panel model above, these findings suggest a strong link between neighborhood expansion of liquor retailing and drug offenses.

Table 1.11 presents results for the closest buffer ring surrounding liquor retailers, ranging from 0.1 mile to 0.25 miles away. If the above effects are due primarily to redistribution of existing crime inward from nearby areas, I should find negative effects on crime in nearby buffer rings, possibly decreasing in magnitude with growing distance from retailers. By contrast, if the above effects are mainly due to new criminal activity I should observe zero or positive effects in outer ring areas.

For violent crime in Columns (3) and (4) and drug crime in Columns (9) and (10), I find clear evidence that the impact of liquor retailing is not primarily due to spatial redistribution. I find positive and significant effects of store openings for both types of crime in the 0.1-0.25 mile buffer areas. As expected, the effects are smaller in magnitude than in the directly surrounding 0.1-mile area: 6.9 percent and 24.3 percent for violent and drug crime, compared to 13 percent and 67.3 percent, respectively. For these two crime categories, new liquor retailers appear to induce additional criminal activity that would not have otherwise occurred. For total crime and nonviolent crime, the evidence is less clear. In each case, I find negative point estimates in Table 1.11, suggesting some degree of spatial redistribution of crime may have occurred. However, none of the four estimates are statistically significant. The clearest evidence for spatial redistribution is for nonviolent crime, whose negative estimates have sufficiently large t-statistics to reasonably conclude that some part of the effect identified above is due to inward redistribution of crime. This result is likely the driving force behind the weakly negative estimates for total crime, which is composed of both violent and nonviolent offenses.

Table 1.12 shows results for the most distant buffer ring of 0.25-0.5 miles from liquor retailers. As above, I find positive effects of liquor retailing on violent crime and drug crime, although the former are imprecisely estimated, suggesting the above effects on these crimes are not simply due to spatial redistribution of crime. Similarly, I find negative coefficients on nonviolent crime and total crime, providing weak evidence that some of the above effects are due to an inward redistribution of existing crime from nearby areas. For shoplifting, I find a statistically zero effect in the 0.25-0.5 mile buffer ring.

Taken together, these results suggest an interesting temporal and spatial relationship between crime and liquor retailing. In the months following new retailer openings, all five categories of crime are affected either immediately or within two periods. However, the effects on shoplifting and nonviolent crime appear to be transitory, while violent and drug crimes are affected in a more persistent way that is evident in longer-term crime trends. The temporary surge in nonviolent crimes appears to be partially the result of an inward redistribution of crime from surrounding neighborhoods, while the increase in violent and drug crimes appears to be due to new criminal activity.

1.8 Conclusion

The question of whether liquor stores attract crime to nearby areas dominates debates over local liquor policy. The issue has grown more pressing in recent years as Washington State's recent privatization has led to renewed interest in similar reforms in other remaining "control" states—most notably Pennsylvania, where there is currently an active political movement to privatize the state's retail monopoly.[26] I contribute to the large literature on liquor retailing and crime by

[26]See Jeff Frantz, "Could 2014 Be the Year Pennsylvania's Liquor Privatization Movement Reaches Full Proof?," *The Patriot News*, January 7, 2014 (http://bit.ly/1kMZvmh).

exploiting a unique source of identifying variation in liquor availability to identify the causal effect of liquor outlets on crime.

Expanded liquor retailing in Seattle following privatization appears to have had a large and significant causal effect on crime, resulting in a 6.5 to 8.2 percent average increase in total crime from reducing the distance to the nearest liquor retailer by one mile. In the event study I find that opening a new liquor retailer induces an average increase in crime of 8.5 to 9.4 percent in the surrounding 0.1-mile neighborhood, with smaller effects in surrounding buffer areas of 0.1-0.25 miles and 0.25-0.5 miles away. The effects on violent and drug-related crimes appear to be persistent and the result of new criminal activity, while the impact on shoplifting and nonviolent crimes appears to be largely transitory and partially due to redistribution of preexisting crime from other areas.

These results are suggestive of the size and scope of the negative external costs imposed by expanded liquor retailing, which may be weighed by policymakers against offsetting social benefits of increased retail convenience. Lawmakers considering future expansions of liquor retailing should do so based on a full accounting of the likely effects on ancillary crime.

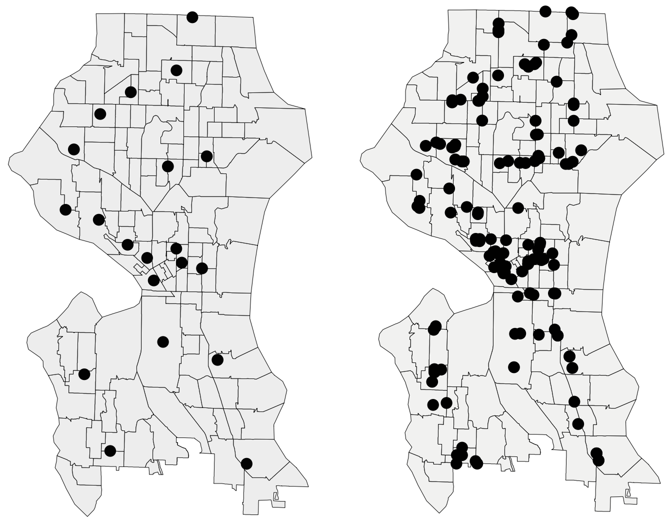

Figure 1.1: Growth in the Number of Liquor Retailers Pre- and Post-Privatization

Note: Left panel displays off-premises liquor retailers as of April 2012. Right panel displays off-premises liquor retailers as of September 2013. Borders are for 134 Census tracts.

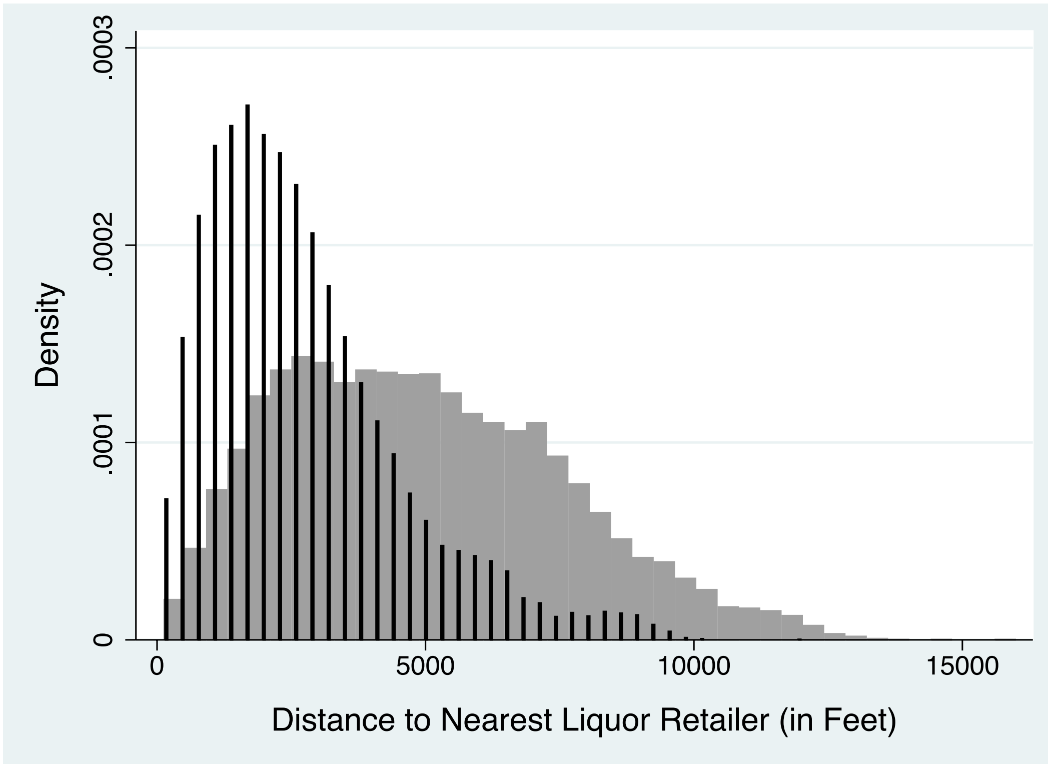

Figure 1.2: Pre- and Post-Policy PDFs of Area Distance to the Nearest Liquor Retailer

Note: The pre-policy PDF (wide gray bars) corresponds to March 2012 ($t = 15$). The post-policy PDF (narrow black bars) corresponds to January 2013 ($t = 25$).

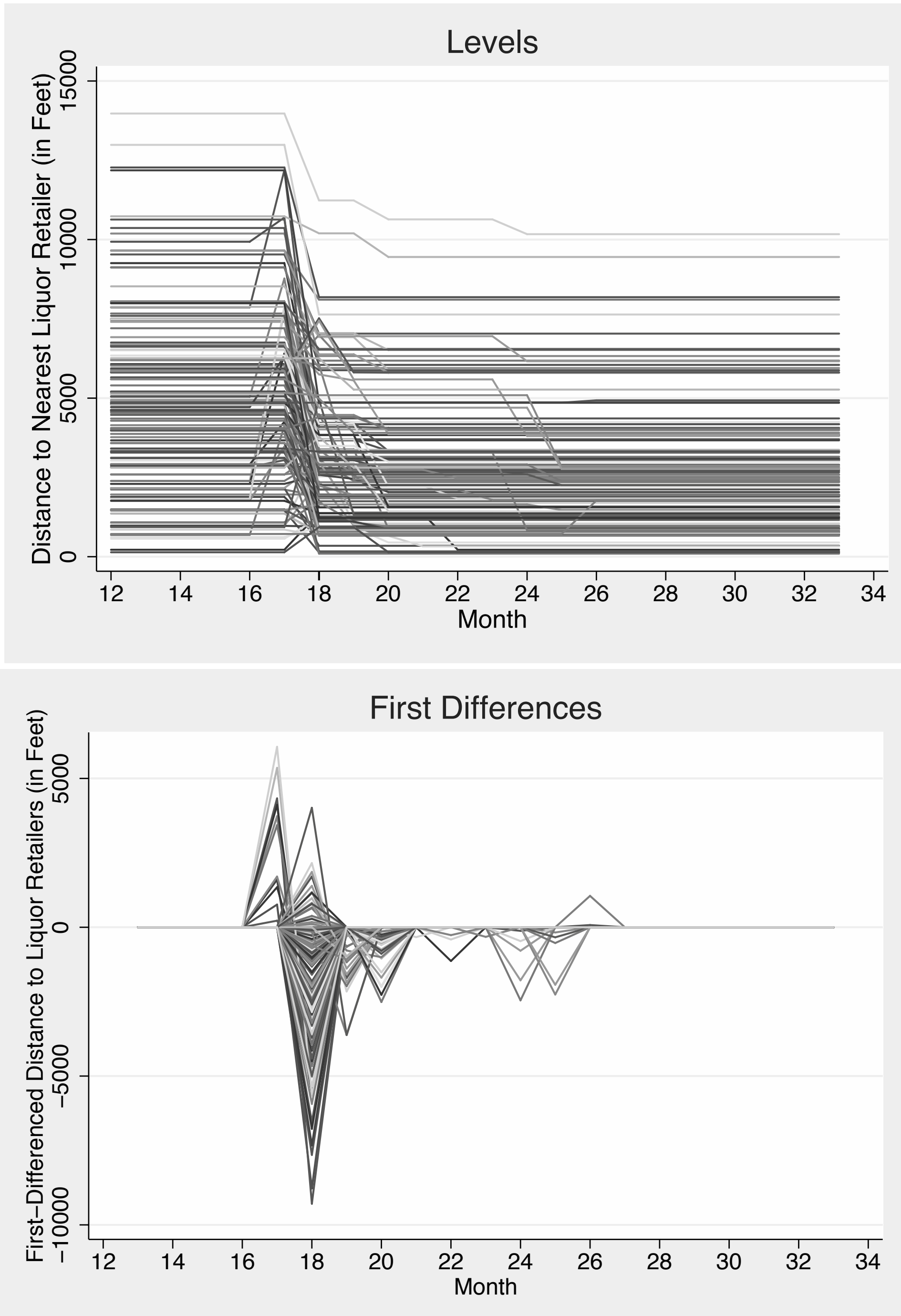

Figure 1.3: Basic Identifying Variation in Distance to the Nearest Liquor Retailer

Note: Lines correspond to 134 Seattle Census tracts. Distances are measured in feet. The policy change (Initiative 1183) occurs at $t = 18$.

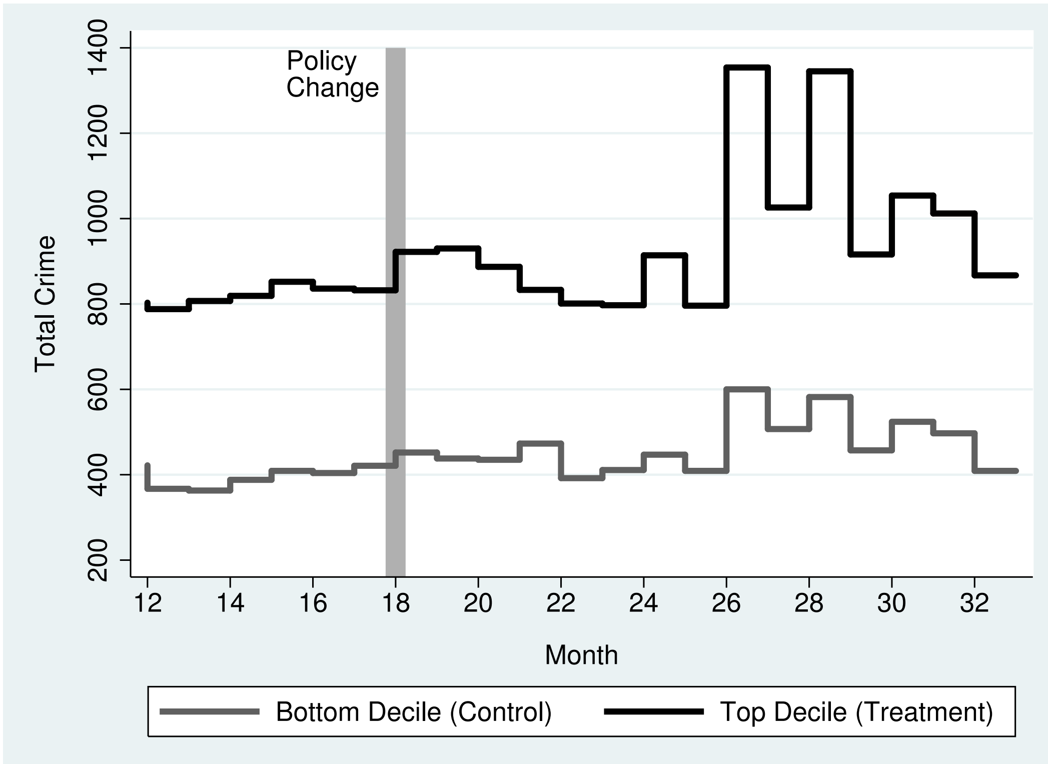

Figure 1.4: Evolution of Total Crime in “Treatment” and “Control” Areas Before and After the Policy Change (Census Block Level)

Note: Top line (black) corresponds to “treatment” areas with the largest percentage decrease in distance to the nearest liquor retailer (top decile). Bottom line (gray) corresponds to “control” areas with the smallest decrease in Liq. Dist. (bottom decile). Policy change occurs in $t = 18$.

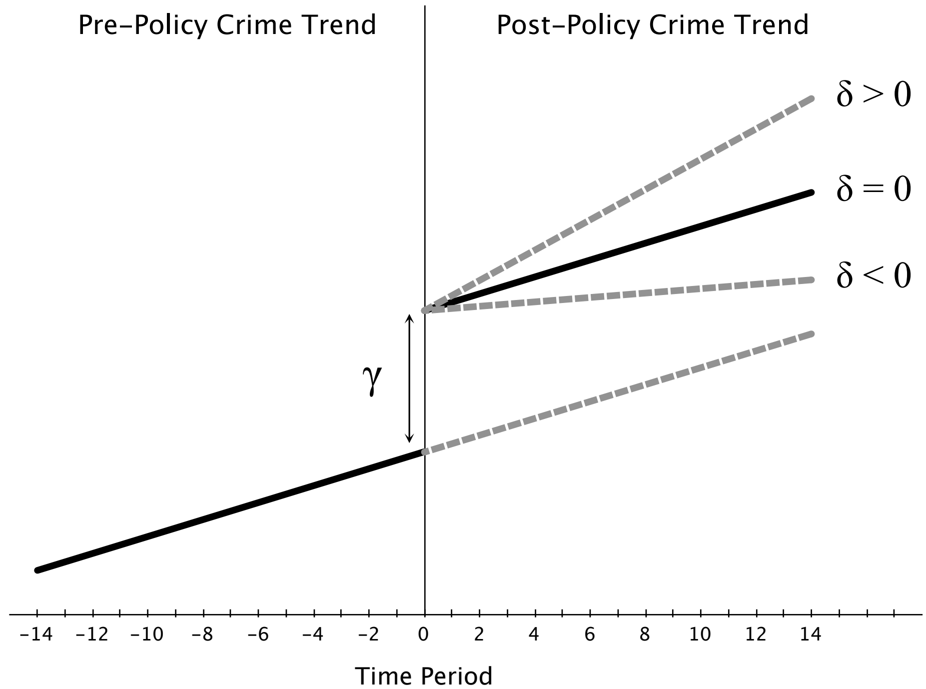

Figure 1.5: Conceptual Framework for the Event Study

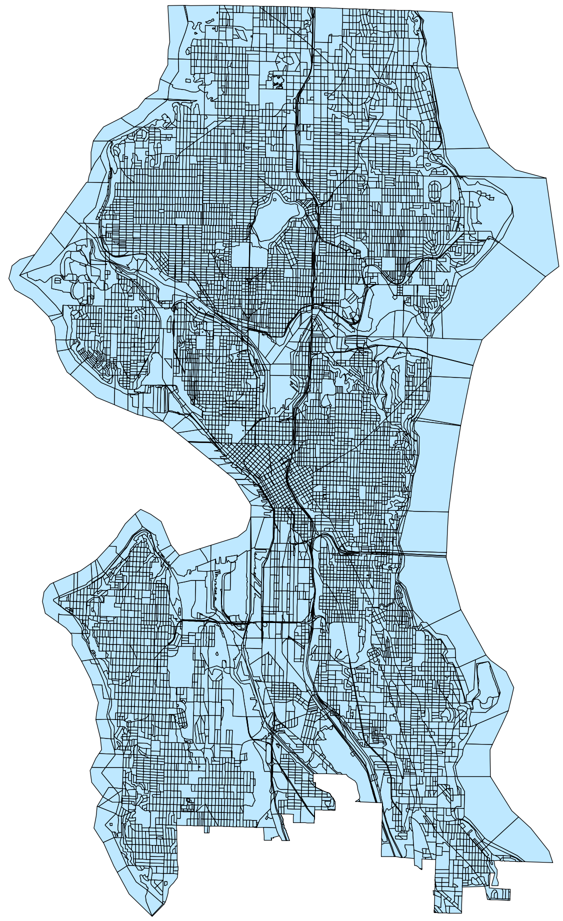

Figure 1.6: 2010 Census Block Areas for Seattle ($N = 11,485$)

Table 1.1: Count and Frequency of Various Crimes in the Data File

Offense Type	Offense Count	Frequency (%)
Theft - Car Prowl	25,978	15.6
Theft - Other	10,876	6.5
Vehicle Auto Theft	9,955	6.0
Burglary - Forced Residential	8,212	4.9
Property Damage - Non-Residential	8,098	4.9
Assault - Non-Aggravated	6,417	3.9
Disturbance - Other	5,688	3.4
Illegal Property Possession	5,494	3.3
Theft - Shoplifting	5,393	3.2
Burglary - Non-Forced Residential	4,903	2.9
All Others	75,379	45.3
Total	166,393	100.0

Source: Crime data are from the Seattle Police Department's "Police Report Incident" file available at http://data.seattle.gov, from January 1, 2011 through September 30, 2013.

Table 1.2: Summary Statistics for the Census-Block-Level Panel ($N = 11485; T = 33$)

Variable	Obs.	Mean	St. Dev.	Min.	Max.
Year	379,005	n.a.	n.a.	2011	2013
Total Crime	379,005	0.43	1.40	0	64
Violent Crime	379,005	0.14	0.67	0	43
Non-Violent Crime	379,005	0.29	0.96	0	55
Shoplifting Crime	379,005	0.01	0.22	0	32
Drug-Related Crime	379,005	0.01	0.19	0	30
Off-Premises Liquor Dist.	379,005	4,055	2,820	21	22,795
On-Premises Liquor Dist.	379,005	1,251	983	1.1	10,219

Note: Crime and liquor availability figures are for 11,485 year-2010 Census blocks in Seattle over the 33-month period from January 2011 to September 2013 ($NT = 379,005$). Distances are measured in feet. Similar panels were constructed for five other geographies: (1) Census block groups; (2) Census tracts; and three uniform rectangular grids measuring (3) 25 x 25; (4) 50 x 50; and (5) 120 x 120.

Sources: Crime data are from the Seattle Police Department's "Police Report Incident" file at http://data.seattle.gov. On- and off-premises liquor retailers locations are from historical Washington State Liquor Control Board (WSLCB) records at http://liq.wa.gov/records/frequently-requested-lists. Retailer locations were geocoded using the MapQuest Geocoding API website and ArcGIS software.

Table 1.3: Regression Results for Census Block Panel Model (1 of 5)

Variable	(1) Total Crime	(2) Total Crime	(3) Total Crime	(4) Total Crime	(5) Total Crime	(6) Total Crime
Liq. Dist. (t)	-0.29553***	-0.07088**	-0.02906***	-0.03507***	-0.03484***	-0.03550***
t-statistic	(-6.029)	(-2.376)	(-3.761)	(-4.673)	(-4.794)	(-4.995)
Effect / Mean	-68.5%	-16.4%	-6.7%	-8.1%	-8.1%	-8.2%
Liq. Dist. (t-1)				-0.02547***	-0.02565***	-0.02680***
t-statistic				(-2.978)	(-3.135)	(-3.104)
Effect / Mean				-5.9%	-5.9%	-6.2%
Liq. Dist. (t-2)					-0.00240	-0.00498
t-statistic					(-0.294)	(-0.526)
Effect / Mean					-0.6%	-1.2%
Liq. Dist. (t-3)						-0.00879
t-statistic						(-1.351)
Effect / Mean						-2.0%
On-Prem. Dist. (t)	-0.80944***	0.05772	-0.07371	-0.06046	-0.06521	0.00749
t-statistic	(-23.322)	(0.820)	(-0.462)	(-0.392)	(-0.440)	(0.054)
On-Prem. Dist. (t-1)				0.04588	0.02594	0.05094
t-statistic				(0.449)	(0.256)	(0.502)
On-Prem. Dist. (t-2)					-0.08604	-0.05830
t-statistic					(-0.898)	(-0.570)
On-Prem. Dist. (t-3)						0.09628
t-statistic						(1.208)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	252,670	252,670	241,185	229,700	218,215	206,730
Within R-Squared	0.0268	0.0005	0.0045	0.0047	0.0048	0.0050

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.4: Regression Results for Census Block Panel Model (2 of 5)

Variable	(1) Violent Crime	(2) Violent Crime	(3) Violent Crime	(4) Violent Crime	(5) Violent Crime	(6) Violent Crime
Liq. Dist. (t)	-0.11007***	-0.03082***	0.00497***	-0.00186	-0.00070	-0.00213
t-statistic	(-6.163)	(-2.646)	(3.686)	(-0.837)	(-0.303)	(-0.820)
Effect / Mean	-77.8%	-21.8%	3.5%	-1.3%	-0.5%	-1.5%
Liq. Dist. (t-1)				-0.02743***	-0.02515***	-0.02686***
t-statistic				(-6.135)	(-5.647)	(-5.427)
Effect / Mean				-19.4%	-17.8%	-19.0%
Liq. Dist. (t-2)					0.00652	0.00449
t-statistic					(1.010)	(0.629)
Effect / Mean					4.6%	3.2%
Liq. Dist. (t-3)						-0.00323
t-statistic						(-0.745)
Effect / Mean						-2.3%
On-Prem. Dist. (t)	-0.32131***	0.01728	-0.03373	-0.04749	-0.05773	-0.03571
t-statistic	(-23.188)	(1.106)	(-0.747)	(-1.071)	(-1.265)	(-0.702)
On-Prem. Dist. (t-1)				-0.05699	-0.06824	-0.04590
t-statistic				(-1.008)	(-1.306)	(-0.959)
On-Prem. Dist. (t-2)					-0.01748	0.01722
t-statistic					(-0.364)	(0.322)
On-Prem. Dist. (t-3)						0.10351**
t-statistic						(2.438)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	252,670	252,670	241,185	229,700	218,215	206,730
Within R-Squared	0.0177	0.0003	0.0019	0.0020	0.0021	0.0021

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.5: Regression Results for Census Block Panel Model (3 of 5)

Variable	(1) Nonviolent Crime	(2) Nonviolent Crime	(3) Nonviolent Crime	(4) Nonviolent Crime	(5) Nonviolent Crime	(6) Nonviolent Crime
Liq. Dist. (t)	-0.18546***	-0.04005**	-0.03402***	-0.03321***	-0.03414***	-0.03336***
t-statistic	(-5.913)	(-2.144)	(-4.242)	(-4.108)	(-4.296)	(-4.481)
Effect / Mean	-64.0%	-13.8%	-11.7%	-11.5%	-11.8%	-11.5%
Liq. Dist. (t-1)				0.00196	-0.00050	0.00005
t-statistic				(0.347)	(-0.084)	(0.010)
Effect / Mean				0.7%	-0.2%	0.0%
Liq. Dist. (t-2)					-0.00892***	-0.00947**
t-statistic					(-2.682)	(-2.312)
Effect / Mean					-3.1%	-3.3%
Liq. Dist. (t-3)						-0.00556
t-statistic						(-1.404)
Effect / Mean						-1.9%
On-Prem. Dist. (t)	-0.48813***	0.04044	-0.03998	-0.01297	-0.00748	0.04320
t-statistic	(-19.517)	(0.549)	(-0.271)	(-0.093)	(-0.058)	(0.351)
On-Prem. Dist. (t-1)				0.10286	0.09418	0.09684
t-statistic				(1.114)	(0.974)	(1.025)
On-Prem. Dist. (t-2)					-0.06856	-0.07552
t-statistic					(-0.826)	(-1.052)
On-Prem. Dist. (t-3)						-0.00723
t-statistic						(-0.121)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	252,670	252,670	241,185	229,700	218,215	206,730
Within R-Squared	0.0212	0.0002	0.0030	0.0031	0.0032	0.0033

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.6: Regression Results for Census Block Panel Model (4 of 5)

Variable	(1) Shoplift. Crime	(2) Shoplift. Crime	(3) Shoplift. Crime	(4) Shoplift. Crime	(5) Shoplift. Crime	(6) Shoplift. Crime
Liq. Dist. (t)	-0.025***	-0.006***	-0.008***	-0.007***	-0.007***	-0.007***
t-statistic	(-4.405)	(-3.393)	(-3.825)	(-3.206)	(-3.230)	(-3.171)
Effect / Mean	-160.7%	-35.9%	-49.4%	-47.1%	-47.8%	-47.6%
Liq. Dist. (t-1)				0.00138	0.00134	0.00142
t-statistic				(1.338)	(1.012)	(0.946)
Effect / Mean				8.9%	8.6%	9.1%
Liq. Dist. (t-2)					0.00043	0.00048
t-statistic					(0.318)	(0.283)
Effect / Mean					2.8%	3.1%
Liq. Dist. (t-3)						-0.00011
t-statistic						(-0.085)
Effect / Mean						-0.7%
On-Prem. Dist. (t)	-0.05049***	0.00331	-0.00447	-0.00483	-0.00474	-0.00432
t-statistic	(-13.894)	(0.954)	(-0.466)	(-0.534)	(-0.497)	(-0.480)
On-Prem. Dist. (t-1)				-0.00210	-0.00439	-0.00054
t-statistic				(-0.263)	(-0.851)	(-0.097)
On-Prem. Dist. (t-2)					-0.01279	-0.00509
t-statistic					(-0.849)	(-0.397)
On-Prem. Dist. (t-3)						0.02781**
t-statistic						(2.314)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	252,670	252,670	241,185	229,700	218,215	206,730
Within R-Squared	0.0048	0.0001	0.0005	0.0005	0.0006	0.0006

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.7: Regression Results for Census Block Panel Model (5 of 5)

Variable	(1) Drug Crime	(2) Drug Crime	(3) Drug Crime	(4) Drug Crime	(5) Drug Crime	(6) Drug Crime
Liq. Dist. (t)	-0.01274***	-0.00033	-0.00521**	-0.00450*	-0.00471**	-0.00436**
t-statistic	(-6.540)	(-0.335)	(-2.392)	(-1.918)	(-2.145)	(-1.985)
Effect / Mean	-107.6%	-2.8%	-44.0%	-38.0%	-39.8%	-36.8%
Liq. Dist. (t-1)				0.00301	0.00223	0.00265
t-statistic				(1.639)	(1.060)	(1.270)
Effect / Mean				25.4%	18.8%	22.4%
Liq. Dist. (t-2)					-0.00337**	-0.00273**
t-statistic					(-2.333)	(-2.328)
Effect / Mean					-28.5%	-23.1%
Liq. Dist. (t-3)						0.00154
t-statistic						(1.052)
Effect / Mean						13.0%
On-Prem. Dist. (t)	-0.04281***	-0.00306	-0.01036	-0.01690	-0.01296	-0.01146
t-statistic	(-19.231)	(-0.350)	(-0.746)	(-0.997)	(-1.033)	(-0.739)
On-Prem. Dist. (t-1)				-0.03094	-0.01949	-0.02079
t-statistic				(-1.166)	(-1.077)	(-1.109)
On-Prem. Dist. (t-2)					0.05105	0.04604
t-statistic					(1.056)	(1.160)
On-Prem. Dist. (t-3)						-0.02700
t-statistic						(-0.856)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	252,670	252,670	241,185	229,700	218,215	206,730
Within R-Squared	0.0029	0.0000	0.0003	0.0003	0.0003	0.0003

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.8: First-Stage IV Results for Census Block Panel Model

Variable	Observed Liq. Dist. (t)
Policy-Predicted Liq. Dist. (t)	0.98486***
t-statistic	(1784.310)
On-Premise Distance (t)	-0.01461***
t-statistic	(-3.762)
Area Fixed Effect	Yes
Time Fixed Effect	Yes
Area Time Trend	Yes
n	252,670
Within R-Squared	0.9668
F-statistic	305,473.8

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: *t*-statistics are presented in parentheses.

Table 1.9: Second-Stage IV Results for Census Block Panel Model

Variable	Total Crime	Violent Crime	Nonviolent Crime	Shoplifting Crime	Drug Crime
Liq. Dist. (t)	-0.03306***	-0.00044	-0.03263***	-0.00839***	-0.00461*
t-statistic	(-3.799)	(-0.171)	(-3.988)	(-3.474)	(-1.751)
Effect / Mean	-7.7%	-0.3%	-11.3%	-54.0%	-38.9%
Liq. Dist. (t-1)	-0.02564**	-0.02887***	0.00323	0.00231	0.00373
t-statistic	(-2.513)	(-5.692)	(0.518)	(1.34)	(1.577)
Effect / Mean	-5.9%	-20.4%	1.1%	14.9%	31.5%
Liq. Dist. (t-2)	0.00216	0.01041	-0.00825	0.00006	-0.00325***
t-statistic	(0.194)	(1.566)	(-1.469)	(0.028)	(-2.690)
Effect / Mean	0.5%	7.4%	-2.8%	0.4%	-27.5%
Liq. Dist. (t-3)	-0.01178	-0.00672	-0.00506	0.00085	0.00245
t-statistic	(-1.433)	(-1.297)	(-1.172)	(0.611)	(1.576)
Effect / Mean	-2.7%	-4.7%	-1.7%	5.5%	20.7%
Area Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes
Area Time Trend	Yes	Yes	Yes	Yes	Yes
n	206,730	206,730	206,730	206,730	206,730
Within R-Squared	0.0050	0.0021	0.0033	0.0006	0.0003

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: IV estimates instrument for observed liquor distance using a counterfactual variable for "predicted liquor distance" from the policy change. All columns are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.10: Event Study Results for 0.1-Mile Areas Surrounding Liquor Retailers (1 of 3)

	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Total Crime	Total Crime	Violent Crime	Violent Crime	Nonviolent Crime	Nonviolent Crime
Store Opening (γ)	1.10300**	1.00467	0.57780***	0.72755***	0.5252	0.27712
t-statistic	(2.563)	(1.657)	(3.084)	(3.252)	(1.406)	(0.537)
Effect / Mean	9.4%	8.5%	13.0%	16.4%	7.2%	3.8%
On-Premise Distance (β)	-0.00258**	-0.00272**	-0.00072	-0.0005	-0.00185***	-0.00222***
t-statistic	(-2.342)	(-2.270)	(-0.873)	(-0.665)	(-4.399)	(-3.346)
Effect / Mean	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
Opening x On-Premise		0.0003		-0.00046*		0.00077
t-statistic		(0.476)		(-1.803)		(1.364)
Effect / Mean		0.0%		0.0%		0.0%
n	3,024	3,024	3,024	3,024	3,024	3,024
Within R-Squared	0.265	0.265	0.223	0.223	0.189	0.189

	(7)	(8)	(9)	(10)
Variable	Shoplifting	Shoplifting	Drug Crime	Drug Crime
Store Opening (γ)	-0.04234	-0.11147	0.42999***	0.39935**
t-statistic	(-0.280)	(-0.754)	(3.295)	(2.206)
Effect / Mean	-2.8%	-7.5%	67.3%	62.5%
On-Premise Distance (β)	-0.00095**	-0.00105**	0.00001	-0.00003
t-statistic	(-2.336)	(-2.341)	(0.138)	(-0.203)
Effect / Mean	-0.1%	-0.1%	0.0%	0.0%
Opening x On-Premise		0.00021***		0.00009
t-statistic		(2.882)		(0.531)
Effect / Mean		0.0%		0.0%
n	3,024	3,024	3,024	3,024
Within R-Squared	0.219	0.220	0.316	0.316

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include area and month fixed effects and pre- and post-policy linear time trends. Robust t-statistics are reported in parentheses, based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.11: Event Study Results for 0.1-0.25 Mile Buffer Rings Surrounding Liquor Retailers (2 of 3)

	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Total Crime	Total Crime	Violent Crime	Violent Crime	Nonviolent Crime	Nonviolent Crime
Store Opening (γ)	-0.10472	-0.98650	0.83542*	0.63657	-0.94014	-1.62307
t-statistic	(-0.070)	(-0.584)	(1.828)	(1.546)	(-0.866)	(-1.217)
Effect / Mean	-0.3%	-3.0%	6.9%	5.3%	-4.6%	-8.0%
On-Premise Distance (β)	-0.00256	-0.00386**	-0.00112	-0.00142*	-0.00144	-0.00245**
t-statistic	(-1.600)	(-2.615)	(-1.331)	(-1.844)	(-1.399)	(-2.108)
Effect / Mean	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
Opening x On-Premise		0.00272		0.00061		0.00211
t-statistic		(1.455)		(0.966)		(1.449)
Effect / Mean		0.0%		0.0%		0.0%
n	3,024	3,024	3,024	3,024	3,024	3,024
Within R-Squared	0.363	0.364	0.422	0.422	0.240	0.240

	(7)	(8)	(9)	(10)
Variable	Shoplifting	Shoplifting	Drug Crime	Drug Crime
Store Opening (γ)	-0.01788	-0.22393**	0.46965**	0.59890*
t-statistic	(-0.228)	(-2.577)	(2.017)	(1.972)
Effect / Mean	-1.2%	-14.9%	24.3%	31.0%
On-Premise Distance (β)	0.00007	-0.00023	0.00000	0.00019
t-statistic	(0.502)	(-1.424)	(0.006)	(0.452)
Effect / Mean	0.0%	0.0%	0.0%	0.0%
Opening x On-Premise		0.00064***		-0.00040
t-statistic		(4.215)		(-1.210)
Effect / Mean		0.0%		0.0%
n	3,024	3,024	3,024	3,024
Within R-Squared	0.174	0.175	0.346	0.346

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include area and month fixed effects and pre- and post-policy linear time trends. Robust t-statistics are reported in parentheses, based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.12: Event Study Results for 0.25-0.5 Mile Buffer Rings Surrounding Liquor Retailers (3 of 3)

	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Total Crime	Total Crime	Violent Crime	Violent Crime	Nonviolent Crime	Nonviolent Crime
Store Opening (γ)	-0.65649	-2.37830	0.84946	0.85842	-1.50595	-3.23672
t-statistic	(-0.271)	(-0.743)	(0.891)	(0.817)	(-0.920)	(-1.360)
Effect / Mean	-0.9%	-3.1%	3.2%	3.2%	-3.0%	-6.5%
On-Premise Distance (β)	-0.00591***	-0.00846***	-0.00262	-0.00261	-0.00329**	-0.00585***
t-statistic	(-2.761)	(-3.020)	(-1.335)	(-1.303)	(-2.023)	(-2.662)
Effect / Mean	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
Opening x On-Premise		0.00531		-0.00003		0.00534*
t-statistic		(1.377)		(-0.025)		(1.723)
Effect / Mean		0.0%		0.0%		0.0%
n	3,024	3,024	3,024	3,024	3,024	3,024
Within R-Squared	0.425	0.426	0.451	0.451	0.295	0.297

	(7)	(8)	(9)	(10)
Variable	Shoplifting	Shoplifting	Drug Crime	Drug Crime
Store Opening (γ)	0.13084	-0.09470	0.78937***	0.87545***
t-statistic	(0.991)	(-0.581)	(3.109)	(2.630)
Effect / Mean	4.7%	-3.4%	26.9%	29.9%
On-Premise Distance (β)	-0.00140**	-0.00174***	0.00007	0.00020
t-statistic	(-2.529)	(-3.559)	(0.236)	(0.554)
Effect / Mean	0.0%	-0.1%	0.0%	0.0%
Opening x On-Premise		0.00070**		-0.00027
t-statistic		(2.514)		(-0.791)
Effect / Mean		0.0%		0.0%
n	3,024	3,024	3,024	3,024
Within R-Squared	0.299	0.300	0.240	0.240

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include area and month fixed effects and pre- and post-policy linear time trends. Robust t-statistics are reported in parentheses, based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

1.9 Appendix

1.9.1 Placebo Test Results

Table 1.13 shows the results of a placebo test in which we include three leads of future distance to the nearest liquor retailer in the fixed-effects model from Section 7. Unlike lagged changes in liquor availability, future changes should have no causal effect on contemporaneous crime. Because $\Delta d^l_{it+1} = d^l_{it+1} - d^l_{it}$, changes in liquor distance at time $t+1$ contain information about current liquor availability at time t. Thus, we omit the first lead of liquor distance and instead include the second through fourth leads. All models are estimated in first differences. In Table 1.13, we see that all leads of future liquor distance are statistical zeros, suggesting the effect of liquor availability on crime from Section 7 is not the result of spurious time-series correlations.

Table 1.13: Placebo Test Results; FE Panel Model with Three Leads of Liquor Distance; Census Block Level

	(1)	(2)	(3)	(4)	(5)
Variable	Total Crime	Violent Crime	Nonviolent Crime	Shoplifting	Drug Crime
Liq. Dist. (t)	-0.02538***	0.00401	-0.02939***	-0.00846**	-0.00354*
t-statistic	(-3.658)	(0.626)	(-3.648)	(-2.559)	(-1.659)
Effect / Mean	-5.9%	2.8%	-10.1%	-54.5%	-29.9%
Liq. Dist. (t-1)	-0.02296***	-0.02306***	0.00010	0.00068	0.00246
t-statistic	(-3.939)	(-3.087)	(0.014)	(0.254)	(1.017)
Effect / Mean	-5.3%	-16.3%	0.0%	4.4%	20.8%
Liq. Dist. (t-2)	-0.00079	0.00798	-0.00877	0.00007	-0.00307**
t-statistic	(-0.066)	(0.843)	(-1.287)	(0.028)	(-2.260)
Effect / Mean	-0.2%	5.6%	-3.0%	0.5%	-25.9%
Liq. Dist. (t-3)	-0.00445	-0.00076	-0.00370	-0.00058	0.00121
t-statistic	(-0.550)	(-0.125)	(-0.650)	(-0.269)	(0.790)
Effect / Mean	-1.0%	-0.5%	-1.3%	-3.7%	10.2%
Liq. Dist. (t+2)	0.01137	-0.00843	0.01980	-0.02668	-0.00264
t-statistic	(0.530)	(-0.374)	(1.132)	(-1.388)	(-0.659)
Liq. Dist. (t+3)	0.00731	0.02486	-0.01755	0.02470	-0.00342
t-statistic	(0.076)	(1.225)	(-0.199)	(0.985)	(-1.025)
Liq. Dist. (t+4)	-0.00437	0.00775	-0.01212	0.00719	-0.00059
t-statistic	(-0.071)	(0.203)	(-0.300)	(1.628)	(-0.192)
On-Premise Distance (+ 3 Lags)	Yes	Yes	Yes	Yes	Yes
Area Fixed Effect	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes
Area Time Trend	Yes	Yes	Yes	Yes	Yes
n	149,305	149,305	149,305	149,305	149,305
Within R-Squared	0.0041	0.0022	0.0024	0.0007	0.0004

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications are estimated in first differences. Liquor distance at $t+1$ is excluded as it contains information about distances at time t since $\Delta d_{it+1} = d_{it+1} - d_{it}$. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

1.9.2 Negative Binomial Model Results

Table 1.14 shows results for the the fixed-effects model from Section 7 estimated in levels via a negative binomial model. The model is specifically designed to account for the discrete and non-negative character of crime counts, as well as the over-dispersion that is common in applications. A Hausman specification test fails to reject the null hypothesis of equivalence between random and fixed effects models, and I present random effects results which are more precisely estimated. The columns correspond to the five categories of crime. For comparison to the previous results, all estimates can be compared to Column (6) of the tables from Section 7.[27]

In Table 1.14 we see the same basic pattern of results as in the linear fixed-effects model. All significant coefficients are negative. The effect on total crime is significant in Column (1), as are the effects on nonviolent crime and shoplifting in Columns (3) and (4). As above, violent crime is affected by the first lag of liquor distance, although it is imprecisely estimated and fails to reach conventional levels of significance. The effect on drug crime is more ambiguous, as only the second lag appears to have a significant effect. Overall, the results are broadly similar to those presented in Section 7.

[27] I do not report marginal effects for the estimated negative binomial coefficients; they are available upon request.

Table 1.14: Regression of Distance to the Nearest Liquor Retailer on Various Crimes, Estimated in Levels via a Negative Binomial Model (Random Effects) at the Census-Tract Level

	(1)	(2)	(3)	(4)	(5)
Variable	Total Crime	Violent Crime	Nonviolent Crime	Shoplifting	Drug Crime
Liq. Dist. (t)	-0.0000156740*	-4.2506E-06	-0.0000214121*	-0.0000859802*	1.72877E-05
t-statistic	(-1.658)	(-0.289)	(-1.872)	(-1.806)	-0.294
Liq. Dist. (t-1)	1.9063E-06	-2.20183E-05	1.00555E-05	-4.37374E-05	5.54507E-05
t-statistic	-0.186	(-1.395)	-0.804	(-0.868)	-0.858
Liq. Dist. (t-2)	2.7424E-06	4.9473E-06	-7.624E-07	-3.07962E-05	-0.0001221544*
t-statistic	-0.272	-0.328	(-0.061)	(-0.688)	(-1.775)
Liq. Dist. (t-3)	-6.6012E-06	1.10776E-05	-0.0000185290*	-7.179E-07	-2.55233E-05
t-statistic	(-0.724)	-0.825	(-1.664)	(-0.020)	(-0.449)
On-Premise Distance (t)	-0.00010174	-0.000174762	-0.000051142	3.12818E-05	0.000521203
t-statistic	(-1.210)	(-1.407)	(-0.492)	-0.064	-0.836
On-Premise Distance (t-1)	6.03599E-05	-8.32248E-05	0.000139851	0.000240944	3.00573E-05
t-statistic	-0.568	(-0.560)	-1.049	-0.372	-0.037
On-Premise Distance (t-2)	-0.000111017	-1.31039E-05	-0.000187082	-0.000466295	0.00030105
t-statistic	(-0.999)	(-0.089)	(-1.308)	(-0.676)	-0.324
On-Premise Distance (t-3)	-0.000136169	-0.000101789	-0.000153858	0.000227702	-0.001314974
t-statistic	(-1.336)	(-0.730)	(-1.206)	-0.404	(-1.494)
n	2,546	2,546	2,546	2,546	2,546

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include area and month fixed effects as well as area-specific linear time trends. I estimate via random effects. Reported coefficients are based on 10 iterations of the MLE procedure employed by the *xtnbreg* Stata command. *t*-statistics are reported in parentheses.

1.9.3 Results for Additional Geographic Areas

Tables 1.15 to 1.39 show results of the fixed-effects model from Section 7 estimated using five additional geographic panels: Census block groups, Census tracts, 120 x 120 grid areas, 50 x 50 grid areas and 25 x 25 grid areas. All tables are presented in the same format and order as in Section 7.

Results for Census Block Group Panel

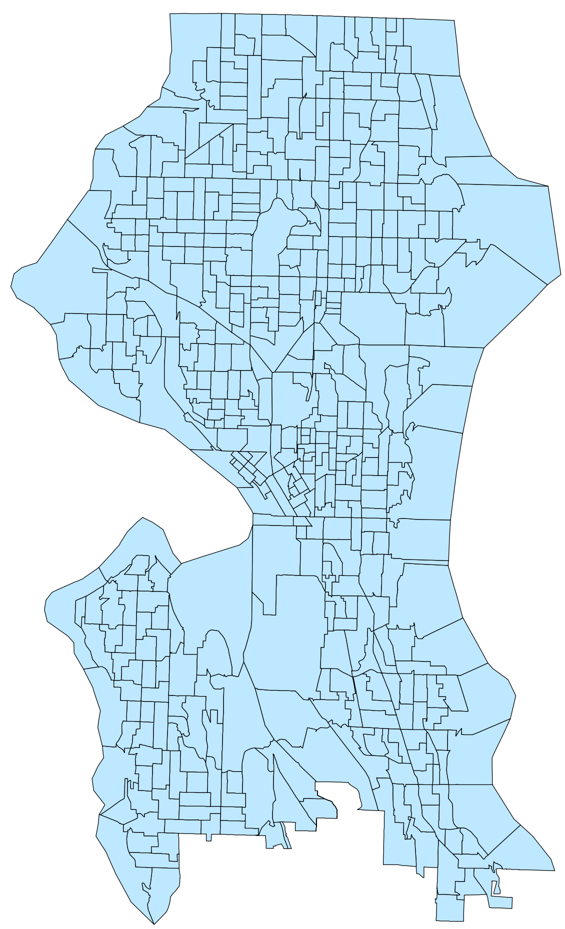

Figure 1.7: 2010 Census Block Group Areas for Seattle ($N = 481$)

Table 1.15: Regression Results for Census Block Group Panel Model (1 of 5)

Variable	(1) Total Crime	(2) Total Crime	(3) Total Crime	(4) Total Crime	(5) Total Crime	(6) Total Crime
Liq. Dist. (t)	-3.964***	-1.645**	-0.903***	-1.042***	-1.029***	-1.041***
t-statistic	(-8.327)	(-2.422)	(-5.612)	(-6.269)	(-6.265)	(-5.908)
Effect / Mean	-38.4%	-15.9%	-8.8%	-10.1%	-10.0%	-10.1%
Liq. Dist. (t-1)				-0.55962*	-0.55641*	-0.58115*
t-statistic				(-1.848)	(-1.865)	(-1.847)
Effect / Mean				-5.4%	-5.4%	-5.6%
Liq. Dist. (t-2)					-0.05524	-0.11141
t-statistic					(-0.204)	(-0.401)
Effect / Mean					-0.5%	-1.1%
Liq. Dist. (t-3)						-0.18569
t-statistic						(-0.694)
Effect / Mean						-1.8%
On-Prem. Dist. (t)	-15.87732***	0.39643	0.07696	0.78144	0.78626	1.82999
t-statistic	(-20.321)	(0.301)	(0.021)	(0.233)	(0.231)	(0.562)
On-Prem. Dist. (t-1)				3.14057	2.96050	3.15895
t-statistic				(1.179)	(1.139)	(1.187)
On-Prem. Dist. (t-2)					-0.98465	-1.01016
t-statistic					(-0.438)	(-0.541)
On-Prem. Dist. (t-3)						0.02419
t-statistic						(0.008)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,582	10,582	10,101	9,620	9,139	8,658
Within R-Squared	0.0676	0.0068	0.0801	0.0818	0.0842	0.0862

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.16: Regression Results for Census Block Group Panel Model (2 of 5)

Variable	(1) Violent Crime	(2) Violent Crime	(3) Violent Crime	(4) Violent Crime	(5) Violent Crime	(6) Violent Crime
Liq. Dist. (t)	-1.63032***	-0.68827***	0.13790	-0.01969	0.02842	0.00612
t-statistic	(-7.687)	(-2.592)	(1.550)	(-0.185)	(0.252)	(0.057)
Effect / Mean	-48.2%	-20.3%	4.1%	-0.6%	0.8%	0.2%
Liq. Dist. (t-1)				-0.58278***	-0.49369***	-0.52125***
t-statistic				(-3.817)	(-3.438)	(-3.262)
Effect / Mean				-17.2%	-14.6%	-15.4%
Liq. Dist. (t-2)					0.22703	0.19324
t-statistic					(1.061)	(0.935)
Effect / Mean					6.7%	5.7%
Liq. Dist. (t-3)						-0.05550
t-statistic						(-0.445)
Effect / Mean						-1.6%
On-Prem. Dist. (t)	-6.30390***	-0.43938	-0.66761	-0.69065	-0.44085	-0.23560
t-statistic	(-17.569)	(-0.858)	(-0.545)	(-0.538)	(-0.332)	(-0.167)
On-Prem. Dist. (t-1)				-0.17145	0.52415	0.76582
t-statistic				(-0.192)	(0.604)	(0.910)
On-Prem. Dist. (t-2)					3.32152***	3.62735***
t-statistic					(3.415)	(4.856)
On-Prem. Dist. (t-3)						0.99495
t-statistic						(0.502)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,582	10,582	10,101	9,620	9,139	8,658
Within R-Squared	0.0569	0.0046	0.0390	0.0401	0.0415	0.0426

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.17: Regression Results for Census Block Group Panel Model (3 of 5)

Variable	(1) Nonviolent Crime	(2) Nonviolent Crime	(3) Nonviolent Crime	(4) Nonviolent Crime	(5) Nonviolent Crime	(6) Nonviolent Crime
Liq. Dist. (t)	-2.33377***	-0.95637**	-1.04065***	-1.02274***	-1.05746***	-1.04698***
t-statistic	(-8.581)	(-2.242)	(-4.660)	(-4.285)	(-4.392)	(-4.488)
Effect / Mean	-33.7%	-13.8%	-15.0%	-14.8%	-15.3%	-15.1%
Liq. Dist. (t-1)				0.02316	-0.06273	-0.05990
t-statistic				(0.133)	(-0.333)	(-0.323)
Effect / Mean				0.3%	-0.9%	-0.9%
Liq. Dist. (t-2)					-0.28227**	-0.30465**
t-statistic					(-2.226)	(-2.054)
Effect / Mean					-4.1%	-4.4%
Liq. Dist. (t-3)						-0.13019
t-statistic						(-0.756)
Effect / Mean						-1.9%
On-Prem. Dist. (t)	-9.57342***	0.83580	0.74457	1.47208	1.22711	2.06559
t-statistic	(-19.620)	(0.759)	(0.200)	(0.412)	(0.342)	(0.587)
On-Prem. Dist. (t-1)				3.31202	2.43635	2.39313
t-statistic				(1.424)	(1.049)	(1.039)
On-Prem. Dist. (t-2)					-4.30617**	-4.63752***
t-statistic					(-2.354)	(-2.630)
On-Prem. Dist. (t-3)						-0.97076
t-statistic						(-0.697)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,582	10,582	10,101	9,620	9,139	8,658
Within R-Squared	0.0654	0.0040	0.0563	0.0582	0.0603	0.0622

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.18: Regression Results for Census Block Group Panel Model (4 of 5)

Variable	(1) Shoplifting Crime	(2) Shoplifting Crime	(3) Shoplifting Crime	(4) Shoplifting Crime	(5) Shoplifting Crime	(6) Shoplifting Crime
Liq. Dist. (t)	-0.44175***	-0.09577***	-0.07644	-0.05892	-0.06544	-0.06388
t-statistic	(-5.178)	(-2.712)	(-1.463)	(-0.996)	(-1.119)	(-1.059)
Effect / Mean	-119.1%	-25.8%	-20.6%	-15.9%	-17.6%	-17.2%
Liq. Dist. (t-1)				0.06632**	0.05173	0.05581
t-statistic				(2.135)	(1.601)	(1.508)
Effect / Mean				17.9%	13.9%	15.0%
Liq. Dist. (t-2)					-0.04465*	-0.03388
t-statistic					(-1.798)	(-1.001)
Effect / Mean					-12.0%	-9.1%
Liq. Dist. (t-3)						0.03852
t-statistic						(1.360)
Effect / Mean						10.4%
On-Prem. Dist. (t)	-0.74798***	-0.17977	0.06970	0.07873	0.03001	-0.06323
t-statistic	(-13.035)	(-1.398)	(0.154)	(0.178)	(0.067)	(-0.157)
On-Prem. Dist. (t-1)				-0.01008	-0.14392	-0.15861
t-statistic				(-0.038)	(-0.656)	(-0.766)
On-Prem. Dist. (t-2)					-0.61812	-0.57595
t-statistic					(-1.575)	(-1.515)
On-Prem. Dist. (t-3)						0.35278
t-statistic						(1.625)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,582	10,582	10,101	9,620	9,139	8,658
Within R-Squared	0.0416	0.0010	0.0127	0.0129	0.0132	0.0134

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.19: Regression Results for Census Block Group Panel Model (5 of 5)

Variable	(1) Drug Crime	(2) Drug Crime	(3) Drug Crime	(4) Drug Crime	(5) Drug Crime	(6) Drug Crime
Liq. Dist. (t)	-0.21341***	0.00247	-0.25036**	-0.20872*	-0.22652*	-0.21407*
t-statistic	(-8.719)	(0.097)	(-2.239)	(-1.716)	(-1.897)	(-1.823)
Effect / Mean	-75.4%	0.9%	-88.5%	-73.8%	-80.1%	-75.7%
Liq. Dist. (t-1)				0.15792**	0.11192	0.12880
t-statistic				(2.276)	(1.425)	(1.607)
Effect / Mean				55.8%	39.6%	45.5%
Liq. Dist. (t-2)					-0.15624***	-0.13231**
t-statistic					(-2.612)	(-2.375)
Effect / Mean					-55.2%	-46.8%
Liq. Dist. (t-3)						0.05156
t-statistic						(1.160)
Effect / Mean						18.2%
On-Prem. Dist. (t)	-0.93417***	0.04676	-0.27353***	-0.20849*	-0.24061**	-0.22820**
t-statistic	(-21.157)	(0.655)	(-3.376)	(-1.923)	(-2.386)	(-2.018)
On-Prem. Dist. (t-1)				0.32160**	0.25365	0.28193*
t-statistic				(2.018)	(1.559)	(1.692)
On-Prem. Dist. (t-2)					-0.25115***	-0.20062***
t-statistic					(-3.270)	(-2.879)
On-Prem. Dist. (t-3)						0.17197
t-statistic						(1.382)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,582	10,582	10,101	9,620	9,139	8,658
Within R-Squared	0.0156	0.0000	0.0055	0.0058	0.0061	0.0063

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Results for Census Tract Panel

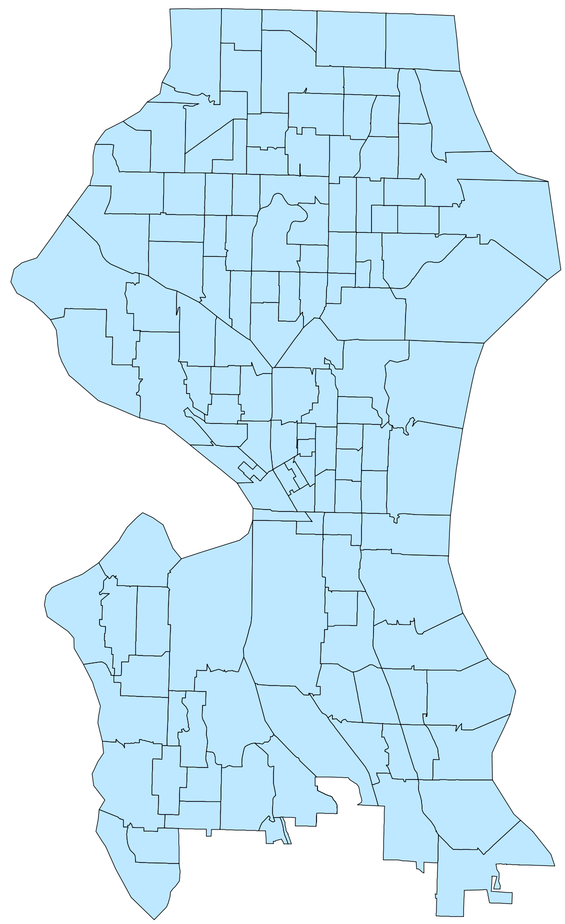

Figure 1.8: 2010 Census Tract Areas for Seattle ($N = 134$)

Table 1.20: Regression Results for Census Tract Panel Model (1 of 5)

Variable	(1) Total Crime	(2) Total Crime	(3) Total Crime	(4) Total Crime	(5) Total Crime	(6) Total Crime
Liq. Dist. (t)	-12.598***	-5.413**	-3.187***	-3.465***	-3.353***	-3.336***
t-statistic	(-10.540)	(-2.370)	(-4.232)	(-4.247)	(-4.119)	(-3.829)
Effect / Mean	-34.0%	-14.6%	-8.6%	-9.4%	-9.1%	-9.0%
Liq. Dist. (t-1)				-1.10994	-0.95478	-0.92407
t-statistic				(-1.267)	(-1.010)	(-0.864)
Effect / Mean				-3.0%	-2.6%	-2.5%
Liq. Dist. (t-2)					0.20774	0.30879
t-statistic					(0.169)	(0.246)
Effect / Mean					0.6%	0.8%
Liq. Dist. (t-3)						0.44682
t-statistic						(0.343)
Effect / Mean						1.2%
On-Prem. Dist. (t)	-52.82138***	0.33152	-0.82225	-0.60477	-1.26002	-0.43588
t-statistic	(-15.872)	(0.099)	(-0.100)	(-0.082)	(-0.167)	(-0.057)
On-Prem. Dist. (t-1)				4.82003	1.57533	2.50446
t-statistic				(0.388)	(0.133)	(0.209)
On-Prem. Dist. (t-2)					-24.67295**	-20.70122***
t-statistic					(-1.997)	(-2.894)
On-Prem. Dist. (t-3)						32.10909
t-statistic						(0.798)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	2,948	2,948	2,814	2,680	2,546	2,412
Within R-Squared	0.1444	0.0131	0.2103	0.2135	0.2198	0.2246

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.21: Regression Results for Census Tract Panel Model (2 of 5)

Variable	(1) Violent Crime	(2) Violent Crime	(3) Violent Crime	(4) Violent Crime	(5) Violent Crime	(6) Violent Crime
Liq. Dist. (t)	-4.95276***	-2.19264**	0.70409**	0.24312	0.35992	0.39398
t-statistic	(-10.241)	(-2.431)	(2.360)	(0.679)	(0.908)	(1.093)
Effect / Mean	-40.7%	-18.0%	5.8%	2.0%	3.0%	3.2%
Liq. Dist. (t-1)				-1.64024***	-1.36879***	-1.29302**
t-statistic				(-3.730)	(-3.053)	(-2.470)
Effect / Mean				-13.5%	-11.3%	-10.6%
Liq. Dist. (t-2)					0.85625	1.09611*
t-statistic					(1.211)	(1.722)
Effect / Mean					7.0%	9.0%
Liq. Dist. (t-3)						0.95080***
t-statistic						(2.826)
Effect / Mean						7.8%
On-Prem. Dist. (t)	-20.12985***	-4.55591**	-8.01739	-8.88752*	-8.87672*	-8.57838*
t-statistic	(-11.991)	(-2.428)	(-1.383)	(-1.843)	(-1.858)	(-1.680)
On-Prem. Dist. (t-1)				-2.05680	-2.67137	-2.15909
t-statistic				(-0.211)	(-0.274)	(-0.221)
On-Prem. Dist. (t-2)					-4.38148	-3.73242
t-statistic					(-0.619)	(-0.659)
On-Prem. Dist. (t-3)						3.16831
t-statistic						(0.157)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	2,948	2,948	2,814	2,680	2,546	2,412
Within R-Squared	0.1192	0.0093	0.1155	0.1183	0.1215	0.1246

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.22: Regression Results for Census Tract Panel Model (3 of 5)

Variable	(1) Nonviolent Crime	(2) Nonviolent Crime	(3) Nonviolent Crime	(4) Nonviolent Crime	(5) Nonviolent Crime	(6) Nonviolent Crime
Liq. Dist. (t)	-7.645***	-3.220**	-3.890***	-3.709***	-3.713***	-3.730***
t-statistic	(-9.813)	(-2.241)	(-4.353)	(-3.788)	(-3.781)	(-3.819)
Effect / Mean	-30.7%	-12.9%	-15.6%	-14.9%	-14.9%	-15.0%
Liq. Dist. (t-1)				0.53030	0.41401	0.36895
t-statistic				(0.851)	(0.610)	(0.500)
Effect / Mean				2.1%	1.7%	1.5%
Liq. Dist. (t-2)					-0.64851	-0.78732
t-statistic					(-0.817)	(-0.894)
Effect / Mean					-2.6%	-3.2%
Liq. Dist. (t-3)						-0.50398
t-statistic						(-0.488)
Effect / Mean						-2.0%
On-Prem. Dist. (t)	-32.69***	4.89	7.20	8.28	7.62	8.14
t-statistic	(-17.952)	(1.536)	(0.951)	(1.100)	(0.982)	(1.074)
On-Prem. Dist. (t-1)				6.87683	4.24670	4.66354
t-statistic				(1.166)	(0.738)	(0.748)
On-Prem. Dist. (t-2)					-20.29**	-16.97***
t-statistic					(-2.243)	(-2.796)
On-Prem. Dist. (t-3)						28.94078
t-statistic						(1.210)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	2,948	2,948	2,814	2,680	2,546	2,412
Within R-Squared	0.1479	0.0089	0.1610	0.1647	0.1701	0.1752

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.23: Regression Results for Census Tract Panel Model (4 of 5)

Variable	(1) Shoplifting Crime	(2) Shoplifting Crime	(3) Shoplifting Crime	(4) Shoplifting Crime	(5) Shoplifting Crime	(6) Shoplifting Crime
Liq. Dist. (t)	-1.32814***	-0.32379***	-0.02467	-0.01411	-0.02950	-0.04119
t-statistic	(-6.874)	(-3.049)	(-0.185)	(-0.106)	(-0.222)	(-0.309)
Effect / Mean	-99.8%	-24.3%	-1.9%	-1.1%	-2.2%	-3.1%
Liq. Dist. (t-1)				-0.00806	-0.03705	-0.05671
t-statistic				(-0.074)	(-0.398)	(-0.538)
Effect / Mean				-0.6%	-2.8%	-4.3%
Liq. Dist. (t-2)					-0.07924	-0.12140
t-statistic					(-0.517)	(-0.734)
Effect / Mean					-6.0%	-9.1%
Liq. Dist. (t-3)						-0.13069
t-statistic						(-0.918)
Effect / Mean						-9.8%
On-Prem. Dist. (t)	-1.78107***	0.94000**	-0.29223	-0.18973	-0.36853	-0.36002
t-statistic	(-10.465)	(2.295)	(-0.122)	(-0.082)	(-0.158)	(-0.159)
On-Prem. Dist. (t-1)				0.76474	0.39393	0.45049
t-statistic				(0.599)	(0.371)	(0.359)
On-Prem. Dist. (t-2)					-2.24596	-1.69528
t-statistic					(-1.453)	(-1.603)
On-Prem. Dist. (t-3)						4.77470*
t-statistic						(1.922)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	2,948	2,948	2,814	2,680	2,546	2,412
Within R-Squared	0.0833	0.0026	0.0409	0.0414	0.0421	0.0427

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.24: Regression Results for Census Tract Panel Model (5 of 5)

Variable	(1) Drug Crime	(2) Drug Crime	(3) Drug Crime	(4) Drug Crime	(5) Drug Crime	(6) Drug Crime
Liq. Dist. (t)	-0.65216***	0.03231	-0.97420***	-0.77100*	-0.83697**	-0.79809**
t-statistic	(-8.202)	(0.419)	(-2.733)	(-1.954)	(-2.168)	(-2.081)
Effect / Mean	-64.2%	3.2%	-95.9%	-75.9%	-82.4%	-78.6%
Liq. Dist. (t-1)				0.77970***	0.60457*	0.66171*
t-statistic				(2.655)	(1.805)	(1.947)
Effect / Mean				76.8%	59.5%	65.2%
Liq. Dist. (t-2)					-0.60845**	-0.50627**
t-statistic					(-2.505)	(-2.054)
Effect / Mean					-59.9%	-49.8%
Liq. Dist. (t-3)						0.28638**
t-statistic						(2.143)
Effect / Mean						28.2%
On-Prem. Dist. (t)	-3.12666***	0.97884***	0.04822	0.16294	0.04879	0.05349
t-statistic	(-23.536)	(3.041)	(0.089)	(0.303)	(0.103)	(0.105)
On-Prem. Dist. (t-1)				0.55429	0.45382	0.41611
t-statistic				(0.911)	(0.674)	(0.739)
On-Prem. Dist. (t-2)					-0.47161	-0.78573
t-statistic					(-0.407)	(-0.839)
On-Prem. Dist. (t-3)						-2.43349
t-statistic						(-1.374)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	2,948	2,948	2,814	2,680	2,546	2,412
Within R-Squared	0.0411	0.0002	0.0203	0.0220	0.0232	0.0243

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Results for 120 x 120 Grid Panel

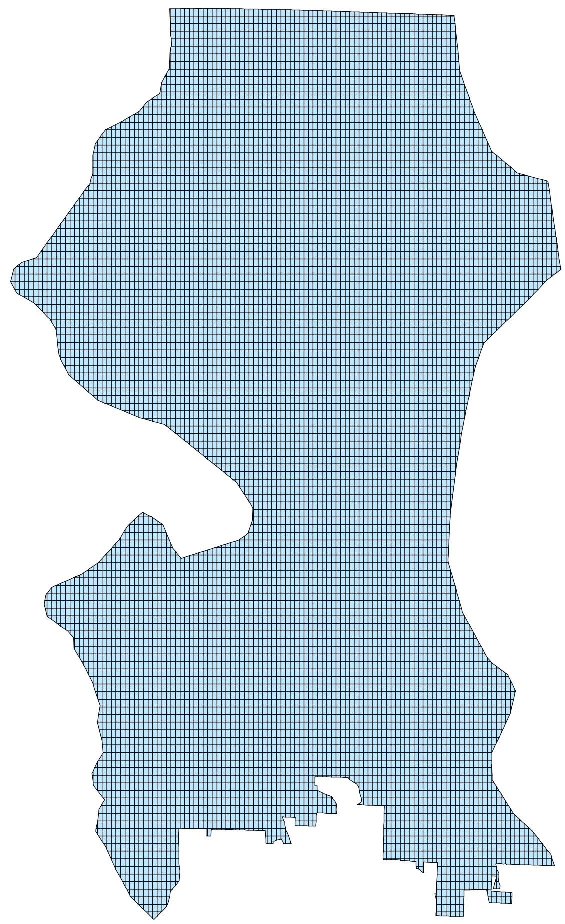

Figure 1.9: 120 x 120 Uniform Grid Areas for Seattle ($N = 9,586$)

Table 1.25: Regression Results for 120 x 120 Grid Panel Model (1 of 5)

Variable	(1) Total Crime	(2) Total Crime	(3) Total Crime	(4) Total Crime	(5) Total Crime	(6) Total Crime
Liq. Dist. (t)	-0.37688***	-0.07955**	-0.02697***	-0.03456***	-0.03328***	-0.03379***
t-statistic	(-6.409)	(-2.339)	(-4.189)	(-6.151)	(-5.916)	(-6.127)
Effect / Mean	-72.7%	-15.4%	-5.2%	-6.7%	-6.4%	-6.5%
Liq. Dist. (t-1)				-0.02892***	-0.02695**	-0.02790**
t-statistic				(-2.858)	(-2.529)	(-2.563)
Effect / Mean				-5.6%	-5.2%	-5.4%
Liq. Dist. (t-2)					0.00391	0.00149
t-statistic					(0.446)	(0.130)
Effect / Mean					0.8%	0.3%
Liq. Dist. (t-3)						-0.00847
t-statistic						(-0.681)
Effect / Mean						-1.6%
On-Prem. Dist. (t)	-0.83327***	-0.02288	-0.10727	-0.09693	-0.08763	-0.02912
t-statistic	(-21.974)	(-0.294)	(-0.684)	(-0.648)	(-0.614)	(-0.215)
On-Prem. Dist. (t-1)				0.02532	0.02643	0.04154
t-statistic				(0.250)	(0.241)	(0.376)
On-Prem. Dist. (t-2)					0.00724	0.03920
t-statistic					(0.105)	(0.482)
On-Prem. Dist. (t-3)						0.10786
t-statistic						(1.043)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	210,892	210,892	201,306	191,720	182,134	172,548
Within R-Squared	0.0542	0.0005	0.0053	0.0055	0.0056	0.0058

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.26: Regression Results for 120 x 120 Grid Panel Model (2 of 5)

Variable	(1) Violent Crime	(2) Violent Crime	(3) Violent Crime	(4) Violent Crime	(5) Violent Crime	(6) Violent Crime
Liq. Dist. (t)	-0.132***	-0.035***	0.005***	-0.002	-0.001	-0.002
t-statistic	(-6.550)	(-2.630)	(2.708)	(-0.772)	(-0.152)	(-0.646)
Effect / Mean	-77.5%	-20.6%	3.1%	-1.3%	-0.3%	-1.2%
Liq. Dist. (t-1)				-0.02795***	-0.02426***	-0.02631***
t-statistic				(-5.665)	(-4.497)	(-4.511)
Effect / Mean				-16.4%	-14.3%	-15.5%
Liq. Dist. (t-2)					0.01023	0.00745
t-statistic					(1.604)	(0.935)
Effect / Mean					6.0%	4.4%
Liq. Dist. (t-3)						-0.00556
t-statistic						(-0.876)
Effect / Mean						-3.3%
On-Prem. Dist. (t)	-0.29290***	-0.01538	-0.04859	-0.06728	-0.07098	-0.05402
t-statistic	(-25.427)	(-0.783)	(-0.816)	(-1.111)	(-1.162)	(-0.842)
On-Prem. Dist. (t-1)				-0.06770	-0.07388	-0.05826
t-statistic				(-1.260)	(-1.391)	(-1.109)
On-Prem. Dist. (t-2)					-0.00985	0.02577
t-statistic					(-0.201)	(0.496)
On-Prem. Dist. (t-3)						0.10534
t-statistic						(1.293)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	210,892	210,892	201,306	191,720	182,134	172,548
Within R-Squared	0.0298	0.0003	0.0023	0.0023	0.0024	0.0025

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.27: Regression Results for 120 x 120 Grid Panel Model (3 of 5)

Variable	(1) Nonviolent Crime	(2) Nonviolent Crime	(3) Nonviolent Crime	(4) Nonviolent Crime	(5) Nonviolent Crime	(6) Nonviolent Crime
Liq. Dist. (t)	-0.24506***	-0.04447**	-0.03221***	-0.03229***	-0.03282***	-0.03170***
t-statistic	(-6.298)	(-2.095)	(-4.438)	(-4.655)	(-4.695)	(-4.849)
Effect / Mean	-70.4%	-12.8%	-9.3%	-9.3%	-9.4%	-9.1%
Liq. Dist. (t-1)				-0.00097	-0.00268	-0.00159
t-statistic				(-0.145)	(-0.361)	(-0.228)
Effect / Mean				-0.3%	-0.8%	-0.5%
Liq. Dist. (t-2)					-0.00632	-0.00596
t-statistic					(-1.449)	(-1.137)
Effect / Mean					-1.8%	-1.7%
Liq. Dist. (t-3)						-0.00291
t-statistic						(-0.419)
Effect / Mean						-0.8%
On-Prem. Dist. (t)	-0.54037***	-0.00750	-0.05869	-0.02965	-0.01665	0.02490
t-statistic	(-18.948)	(-0.092)	(-0.420)	(-0.227)	(-0.137)	(0.208)
On-Prem. Dist. (t-1)				0.09302	0.10031	0.09980
t-statistic				(1.021)	(1.047)	(1.070)
On-Prem. Dist. (t-2)					0.01708	0.01343
t-statistic					(0.235)	(0.194)
On-Prem. Dist. (t-3)						0.00252
t-statistic						(0.044)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	210,892	210,892	201,306	191,720	182,134	172,548
Within R-Squared	0.0539	0.0003	0.0036	0.0037	0.0038	0.0039

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.28: Regression Results for 120 x 120 Grid Panel Model (4 of 5)

Variable	(1) Shoplift. Crime	(2) Shoplift. Crime	(3) Shoplift. Crime	(4) Shoplift. Crime	(5) Shoplifting Crime	(6) Shoplift. Crime
Liq. Dist. (t)	-0.0250***	-0.006***	-0.007***	-0.007***	-0.007***	-0.007***
t-statistic	(-4.975)	(-3.319)	(-4.569)	(-3.504)	(-3.440)	(-3.331)
Effect / Mean	-136.4%	-34.5%	-36.8%	-34.5%	-35.3%	-35.1%
Liq. Dist. (t-1)				0.00153	0.00147	0.00155
t-statistic				(1.225)	(0.962)	(0.908)
Effect / Mean				8.2%	7.9%	8.3%
Liq. Dist. (t-2)					0.00040	0.00048
t-statistic					(0.230)	(0.226)
Effect / Mean					2.1%	2.6%
Liq. Dist. (t-3)						0.00010
t-statistic						(0.055)
Effect / Mean						0.5%
On-Prem. Dist. (t)	-0.03057***	-0.01098*	-0.00618	-0.00841	-0.00890	-0.01191
t-statistic	(-8.529)	(-1.660)	(-0.601)	(-0.831)	(-0.837)	(-1.212)
On-Prem. Dist. (t-1)				-0.00791	-0.01026**	-0.01018**
t-statistic				(-1.448)	(-2.409)	(-2.419)
On-Prem. Dist. (t-2)					-0.01639	-0.01148
t-statistic					(-1.102)	(-1.015)
On-Prem. Dist. (t-3)						0.02205
t-statistic						(1.419)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	210,892	210,892	201,306	191,720	182,134	172,548
Within R-Squared	0.0067	0.0001	0.0006	0.0006	0.0007	0.0007

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.29: Regression Results for 120 x 120 Grid Panel Model (5 of 5)

Variable	(1) Drug Crime	(2) Drug Crime	(3) Drug Crime	(4) Drug Crime	(5) Drug Crime	(6) Drug Crime
Liq. Dist. (t)	-0.01304***	-0.00016	-0.00438**	-0.00338	-0.00355*	-0.00312
t-statistic	(-7.787)	(-0.139)	(-2.130)	(-1.547)	(-1.779)	(-1.580)
Effect / Mean	-91.7%	-1.1%	-30.8%	-23.8%	-25.0%	-21.9%
Liq. Dist. (t-1)				0.00403*	0.00323	0.00381
t-statistic				(1.800)	(1.265)	(1.476)
Effect / Mean				28.3%	22.7%	26.8%
Liq. Dist. (t-2)					-0.00336**	-0.00247**
t-statistic					(-2.331)	(-2.063)
Effect / Mean					-23.6%	-17.4%
Liq. Dist. (t-3)						0.00211
t-statistic						(1.544)
Effect / Mean						14.8%
On-Prem. Dist. (t)	-0.03005***	-0.00458	-0.01429	-0.01757	-0.01388	-0.01182
t-statistic	(-16.112)	(-0.568)	(-1.027)	(-0.993)	(-1.002)	(-0.727)
On-Prem. Dist. (t-1)				-0.01757	-0.00374	-0.00452
t-statistic				(-0.601)	(-0.181)	(-0.218)
On-Prem. Dist. (t-2)					0.05255	0.04799
t-statistic					(1.009)	(1.028)
On-Prem. Dist. (t-3)						-0.01437
t-statistic						(-0.500)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	210,892	210,892	201,306	191,720	182,134	172,548
Within R-Squared	0.0033	0.0000	0.0003	0.0003	0.0003	0.0004

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Results for 50 x 50 Grid Panel

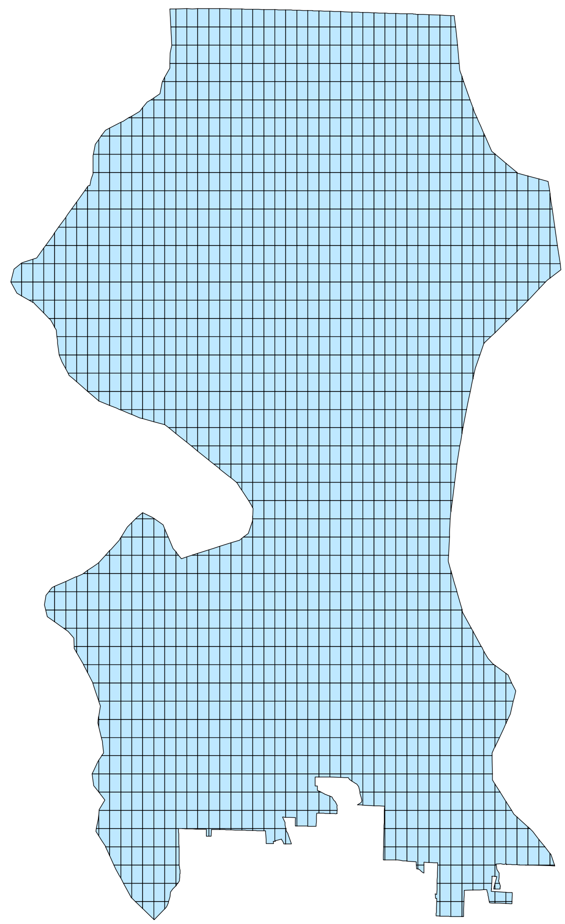

Figure 1.10: 50 x 50 Uniform Grid Areas for Seattle ($N = 1,747$)

Table 1.30: Regression Results for 50 x 50 Grid Panel Model (1 of 5)

Variable	(1) Total Crime	(2) Total Crime	(3) Total Crime	(4) Total Crime	(5) Total Crime	(6) Total Crime
Liq. Dist. (t)	-2.14710***	-0.45159**	-0.11036**	-0.16431***	-0.15880***	-0.16057***
t-statistic	(-6.502)	(-2.399)	(-2.459)	(-4.358)	(-4.135)	(-4.557)
Effect / Mean	-75.5%	-15.9%	-3.9%	-5.8%	-5.6%	-5.6%
Liq. Dist. (t-1)				-0.20060***	-0.18943***	-0.19539***
t-statistic				(-3.424)	(-2.927)	(-2.960)
Effect / Mean				-7.1%	-6.7%	-6.9%
Liq. Dist. (t-2)					0.03050	0.01214
t-statistic					(0.540)	(0.165)
Effect / Mean					1.1%	0.4%
Liq. Dist. (t-3)						-0.07015
t-statistic						(-0.961)
Effect / Mean						-2.5%
On-Prem. Dist. (t)	-4.25246***	0.02422	-0.13207	-0.06884	-0.07855	0.11659
t-statistic	(-20.271)	(0.086)	(-0.223)	(-0.122)	(-0.150)	(0.220)
On-Prem. Dist. (t-1)				0.38542	0.20113	0.20290
t-statistic				(0.866)	(0.423)	(0.437)
On-Prem. Dist. (t-2)					-0.86092***	-0.88930**
t-statistic					(-3.056)	(-2.295)
On-Prem. Dist. (t-3)						-0.25077
t-statistic						(-0.264)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	38,434	38,434	36,687	34,940	33,193	31,446
Within R-Squared	0.1039	0.0023	0.0228	0.0234	0.0241	0.0247

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.31: Regression Results for 50 x 50 Grid Panel Model (2 of 5)

Variable	(1) Violent Crime	(2) Violent Crime	(3) Violent Crime	(4) Violent Crime	(5) Violent Crime	(6) Violent Crime
Liq. Dist. (t)	-0.75378***	-0.20063***	0.00714	-0.03313**	-0.02558*	-0.03106**
t-statistic	(-6.593)	(-2.737)	(0.583)	(-2.200)	(-1.661)	(-2.036)
Effect / Mean	-80.8%	-21.5%	0.8%	-3.5%	-2.7%	-3.3%
Liq. Dist. (t-1)				-0.14842***	-0.13267***	-0.14010***
t-statistic				(-5.062)	(-4.338)	(-4.091)
Effect / Mean				-15.9%	-14.2%	-15.0%
Liq. Dist. (t-2)					0.04493	0.03676
t-statistic					(1.105)	(0.776)
Effect / Mean					4.8%	3.9%
Liq. Dist. (t-3)						-0.01014
t-statistic						(-0.305)
Effect / Mean						-1.1%
On-Prem. Dist. (t)	-1.47337***	-0.04945	-0.25284	-0.32063	-0.34787	-0.28717
t-statistic	(-24.496)	(-0.461)	(-0.844)	(-1.104)	(-1.209)	(-0.875)
On-Prem. Dist. (t-1)				-0.22365	-0.28438	-0.25888
t-statistic				(-1.035)	(-1.372)	(-1.541)
On-Prem. Dist. (t-2)					-0.09800	-0.09186
t-statistic					(-0.361)	(-0.250)
On-Prem. Dist. (t-3)						-0.29178
t-statistic						(-0.514)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	38,434	38,434	36,687	34,940	33,193	31,446
Within R-Squared	0.0654	0.0015	0.0099	0.0101	0.0104	0.0106

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.32: Regression Results for 50 x 50 Grid Panel Model (3 of 5)

Variable	(1) Nonviolent Crime	(2) Nonviolent Crime	(3) Nonviolent Crime	(4) Nonviolent Crime	(5) Nonviolent Crime	(6) Nonviolent Crime
Liq. Dist. (t)	-1.39332***	-0.25095**	-0.11750**	-0.13119***	-0.13322***	-0.12951***
t-statistic	(-6.421)	(-2.125)	(-2.516)	(-2.979)	(-3.019)	(-3.330)
Effect / Mean	-73.0%	-13.1%	-6.2%	-6.9%	-7.0%	-6.8%
Liq. Dist. (t-1)				-0.05218	-0.05676	-0.05529
t-statistic				(-1.357)	(-1.280)	(-1.307)
Effect / Mean				-2.7%	-3.0%	-2.9%
Liq. Dist. (t-2)					-0.01443	-0.02462
t-statistic					(-0.519)	(-0.724)
Effect / Mean					-0.8%	-1.3%
Liq. Dist. (t-3)						-0.06001
t-statistic						(-1.371)
Effect / Mean						-3.1%
On-Prem. Dist. (t)	-2.77909***	0.07367	0.12076	0.25179	0.26932	0.40376
t-statistic	(-17.655)	(0.229)	(0.228)	(0.504)	(0.606)	(0.951)
On-Prem. Dist. (t-1)				0.60907	0.48551	0.46177
t-statistic				(1.640)	(1.151)	(1.146)
On-Prem. Dist. (t-2)					-0.76292***	-0.79744***
t-statistic					(-3.467)	(-3.668)
On-Prem. Dist. (t-3)						0.04101
t-statistic						(0.062)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	38,434	38,434	36,687	34,940	33,193	31,446
Within R-Squared	0.1153	0.0012	0.0166	0.0171	0.0177	0.0182

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.33: Regression Results for 50 x 50 Grid Panel Model (4 of 5)

Variable	(1) Shoplifting Crime	(2) Shoplifting Crime	(3) Shoplifting Crime	(4) Shoplifting Crime	(5) Shoplifting Crime	(6) Shoplifting Crime
Liq. Dist. (t)	-0.13728***	-0.03271***	-0.02896**	-0.02729*	-0.02876**	-0.02897**
t-statistic	(-5.439)	(-3.419)	(-2.254)	(-1.910)	(-2.019)	(-1.997)
Effect / Mean	-134.4%	-32.0%	-28.4%	-26.7%	-28.2%	-28.4%
Liq. Dist. (t-1)				0.00552	0.00276	0.00280
t-statistic				(0.760)	(0.319)	(0.285)
Effect / Mean				5.4%	2.7%	2.7%
Liq. Dist. (t-2)					-0.00708	-0.00689
t-statistic					(-0.815)	(-0.635)
Effect / Mean					-6.9%	-6.7%
Liq. Dist. (t-3)						0.00065
t-statistic						(0.064)
Effect / Mean						0.6%
On-Prem. Dist. (t)	-0.14942***	-0.00663	0.08635	0.07179	0.06873	0.04753
t-statistic	(-8.501)	(-0.191)	(1.361)	(1.173)	(1.080)	(1.030)
On-Prem. Dist. (t-1)				-0.06286	-0.08365**	-0.08123*
t-statistic				(-1.358)	(-2.217)	(-1.778)
On-Prem. Dist. (t-2)					-0.08811	-0.04859
t-statistic					(-1.106)	(-0.759)
On-Prem. Dist. (t-3)						0.15473**
t-statistic						(2.111)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	38,434	38,434	36,687	34,940	33,193	31,446
Within R-Squared	0.0277	0.0004	0.0033	0.0033	0.0034	0.0035

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.34: Regression Results for 50 x 50 Grid Panel Model (5 of 5)

Variable	(1) Drug Crime	(2) Drug Crime	(3) Drug Crime	(4) Drug Crime	(5) Drug Crime	(6) Drug Crime
Liq. Dist. (t)	-0.07644***	-0.00121	-0.01286	-0.00912	-0.00883	-0.00755
t-statistic	(-7.217)	(-0.199)	(-1.249)	(-0.874)	(-0.912)	(-0.795)
Effect / Mean	-98.0%	-1.6%	-16.5%	-11.7%	-11.3%	-9.7%
Liq. Dist. (t-1)				0.01513	0.01413	0.01527
t-statistic				(1.265)	(1.080)	(1.133)
Effect / Mean				19.4%	18.1%	19.6%
Liq. Dist. (t-2)					-0.00688	-0.00608
t-statistic					(-0.808)	(-0.832)
Effect / Mean					-8.8%	-7.8%
Liq. Dist. (t-3)						-0.00081
t-statistic						(-0.120)
Effect / Mean						-1.0%
On-Prem. Dist. (t)	-0.14343***	-0.04994	-0.07487	-0.10193	-0.09246	-0.12993
t-statistic	(-12.989)	(-1.356)	(-1.013)	(-1.052)	(-1.193)	(-1.391)
On-Prem. Dist. (t-1)				-0.13252	-0.06946	-0.08608
t-statistic				(-0.822)	(-0.701)	(-0.740)
On-Prem. Dist. (t-2)					0.29932	0.23832
t-statistic					(1.001)	(1.083)
On-Prem. Dist. (t-3)						-0.28131
t-statistic						(-0.962)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	38,434	38,434	36,687	34,940	33,193	31,446
Within R-Squared	0.0085	0.0000	0.0013	0.0013	0.0014	0.0014

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Results for 25 x 25 Grid Panel

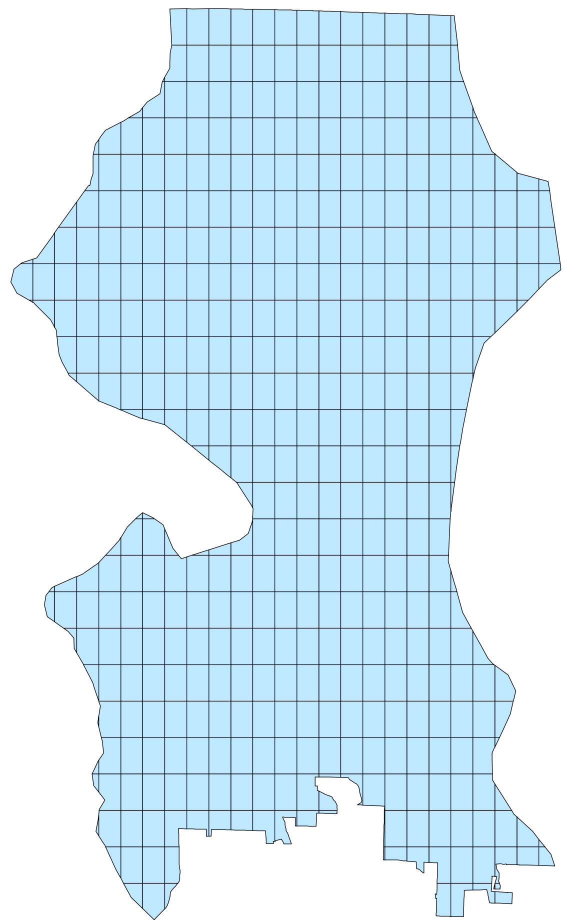

Figure 1.11: 25 x 25 Uniform Grid Areas for Seattle ($N = 465$)

Table 1.35: Regression Results for 25 x 25 Grid Panel Model (1 of 5)

Variable	(1) Total Crime	(2) Total Crime	(3) Total Crime	(4) Total Crime	(5) Total Crime	(6) Total Crime
Liq. Dist. (t)	-8.629***	-1.734**	-0.632***	-0.773***	-0.742***	-0.750***
t-statistic	(-6.642)	(-2.497)	(-3.092)	(-3.742)	(-3.722)	(-3.837)
Effect / Mean	-80.8%	-16.2%	-5.9%	-7.2%	-6.9%	-7.0%
Liq. Dist. (t-1)				-0.51095*	-0.45533*	-0.47240*
t-statistic				(-1.945)	(-1.673)	(-1.655)
Effect / Mean				-4.8%	-4.3%	-4.4%
Liq. Dist. (t-2)					0.12556	0.08430
t-statistic					(0.453)	(0.262)
Effect / Mean					1.2%	0.8%
Liq. Dist. (t-3)						-0.14301
t-statistic						(-0.427)
Effect / Mean						-1.3%
On-Prem. Dist. (t)	-14.74164***	-0.75987	-0.21847	0.31265	0.34897	1.08817
t-statistic	(-17.085)	(-0.476)	(-0.078)	(0.111)	(0.132)	(0.452)
On-Prem. Dist. (t-1)				2.74785	2.31463	2.47994
t-statistic				(1.087)	(0.876)	(0.981)
On-Prem. Dist. (t-2)					-2.46031	-2.42493
t-statistic					(-0.792)	(-0.766)
On-Prem. Dist. (t-3)						-0.07237
t-statistic						(-0.023)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,230	10,230	9,765	9,300	8,835	8,370
Within R-Squared	0.1527	0.0063	0.0648	0.0663	0.0682	0.0696

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.36: Regression Results for 25 x 25 Grid Panel Model (2 of 5)

Variable	(1) Violent Crime	(2) Violent Crime	(3) Violent Crime	(4) Violent Crime	(5) Violent Crime	(6) Violent Crime
Liq. Dist. (t)	-3.05834***	-0.72275***	0.03898	-0.08132	-0.05119	-0.07353
t-statistic	(-6.655)	(-2.666)	(0.687)	(-1.541)	(-0.846)	(-1.074)
Effect / Mean	-87.2%	-20.6%	1.1%	-2.3%	-1.5%	-2.1%
Liq. Dist. (t-1)				-0.45734***	-0.39590***	-0.42407***
t-statistic				(-3.810)	(-3.256)	(-3.099)
Effect / Mean				-13.0%	-11.3%	-12.1%
Liq. Dist. (t-2)					0.17264	0.14760
t-statistic					(1.283)	(0.931)
Effect / Mean					4.9%	4.2%
Liq. Dist. (t-3)						-0.00803
t-statistic						(-0.061)
Effect / Mean						-0.2%
On-Prem. Dist. (t)	-5.03821***	0.05251	-0.08388	-0.10772	-0.19305	0.07679
t-statistic	(-21.383)	(0.194)	(-0.078)	(-0.095)	(-0.170)	(0.073)
On-Prem. Dist. (t-1)				0.15578	0.02227	0.20447
t-statistic				(0.155)	(0.018)	(0.176)
On-Prem. Dist. (t-2)					-0.11951	-0.09895
t-statistic					(-0.068)	(-0.053)
On-Prem. Dist. (t-3)						-0.62796
t-statistic						(-0.501)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,230	10,230	9,765	9,300	8,835	8,370
Within R-Squared	0.1063	0.0040	0.0316	0.0324	0.0330	0.0338

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.37: Regression Results for 25 x 25 Grid Panel Model (3 of 5)

Variable	(1) Nonviolent Crime	(2) Nonviolent Crime	(3) Nonviolent Crime	(4) Nonviolent Crime	(5) Nonviolent Crime	(6) Nonviolent Crime
Liq. Dist. (t)	-5.57042***	-1.01141**	-0.67101***	-0.69130***	-0.69033***	-0.67628***
t-statistic	(-6.600)	(-2.321)	(-3.909)	(-3.947)	(-4.091)	(-4.263)
Effect / Mean	-77.6%	-14.1%	-9.4%	-9.6%	-9.6%	-9.4%
Liq. Dist. (t-1)				-0.05361	-0.05944	-0.04832
t-statistic				(-0.328)	(-0.334)	(-0.279)
Effect / Mean				-0.7%	-0.8%	-0.7%
Liq. Dist. (t-2)					-0.04707	-0.06331
t-statistic					(-0.281)	(-0.327)
Effect / Mean					-0.7%	-0.9%
Liq. Dist. (t-3)						-0.13498
t-statistic						(-0.581)
Effect / Mean						-1.9%
On-Prem. Dist. (t)	-9.70343***	-0.81238	-0.13458	0.42037	0.54202	1.01138
t-statistic	(-15.149)	(-0.544)	(-0.067)	(0.215)	(0.312)	(0.618)
On-Prem. Dist. (t-1)				2.59207	2.29236	2.27547
t-statistic				(1.449)	(1.329)	(1.357)
On-Prem. Dist. (t-2)					-2.34079	-2.32598
t-statistic					(-1.142)	(-1.190)
On-Prem. Dist. (t-3)						0.55559
t-statistic						(0.232)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,230	10,230	9,765	9,300	8,835	8,370
Within R-Squared	0.1752	0.0041	0.0505	0.0520	0.0538	0.0551

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.38: Regression Results for 25 x 25 Grid Panel Model (4 of 5)

Variable	(1) Shoplifting Crime	(2) Shoplifting Crime	(3) Shoplifting Crime	(4) Shoplifting Crime	(5) Shoplifting Crime	(6) Shoplifting Crime
Liq. Dist. (t)	-0.47254***	-0.10884***	-0.08687**	-0.07932*	-0.08137*	-0.08010*
t-statistic	(-6.000)	(-3.254)	(-2.047)	(-1.760)	(-1.790)	(-1.725)
Effect / Mean	-123.2%	-28.4%	-22.6%	-20.7%	-21.2%	-20.9%
Liq. Dist. (t-1)				0.02827	0.02290	0.02626
t-statistic				(1.244)	(0.819)	(0.807)
Effect / Mean				7.4%	6.0%	6.8%
Liq. Dist. (t-2)					-0.01952	-0.01165
t-statistic					(-0.530)	(-0.262)
Effect / Mean					-5.1%	-3.0%
Liq. Dist. (t-3)						0.02720
t-statistic						(0.764)
Effect / Mean						7.1%
On-Prem. Dist. (t)	-0.52461***	-0.22385*	0.00459	0.01053	-0.00463	-0.02176
t-statistic	(-10.228)	(-1.698)	(0.018)	(0.047)	(-0.020)	(-0.078)
On-Prem. Dist. (t-1)				0.06576	-0.01628	-0.00401
t-statistic				(0.207)	(-0.060)	(-0.014)
On-Prem. Dist. (t-2)					-0.32881	-0.21685
t-statistic					(-1.271)	(-0.974)
On-Prem. Dist. (t-3)						0.36795
t-statistic						(1.041)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,230	10,230	9,765	9,300	8,835	8,370
Within R-Squared	0.0792	0.0012	0.0117	0.0119	0.0122	0.0123

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

Table 1.39: Regression Results for 25 x 25 Grid Panel Model (5 of 5)

Variable	(1) Drug Crime	(2) Drug Crime	(3) Drug Crime	(4) Drug Crime	(5) Drug Crime	(6) Drug Crime
Liq. Dist. (t)	-0.31557***	-0.00405	-0.03853	-0.02661	-0.02661	-0.02266
t-statistic	(-7.472)	(-0.193)	(-0.952)	(-0.663)	(-0.710)	(-0.643)
Effect / Mean	-107.6%	-1.4%	-13.1%	-9.1%	-9.1%	-7.7%
Liq. Dist. (t-1)				0.04530	0.04184	0.04609
t-statistic				(0.992)	(0.826)	(0.863)
Effect / Mean				15.5%	14.3%	15.7%
Liq. Dist. (t-2)					-0.01930	-0.01990
t-statistic					(-0.770)	(-0.768)
Effect / Mean					-6.6%	-6.8%
Liq. Dist. (t-3)						-0.02136
t-statistic						(-0.850)
Effect / Mean						-7.3%
On-Prem. Dist. (t)	-0.49453***	-0.05633	-0.06881	-0.01699	0.00578	0.05037
t-statistic	(-12.309)	(-0.573)	(-0.220)	(-0.057)	(0.020)	(0.162)
On-Prem. Dist. (t-1)				0.22631	0.33295	0.34496
t-statistic				(0.990)	(1.438)	(1.558)
On-Prem. Dist. (t-2)					0.12718	0.14446
t-statistic					(0.556)	(0.711)
On-Prem. Dist. (t-3)						0.27349
t-statistic						(1.065)
Area Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes
Area Time Trends	No	No	Yes	Yes	Yes	Yes
n	10,230	10,230	9,765	9,300	8,835	8,370
Within R-Squared	0.0207	0.0000	0.0047	0.0047	0.0048	0.0049

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Columns (1) and (2) are estimated in levels; Columns (3)–(6) are estimated in first differences. Robust t-statistics are reported in parentheses based on Driscoll-Kraay spatial-autocorrelation and cluster-robust standard errors.

1.9.4 Crime Classifications

Table 1.40 provides a crosswalk between individual offense codes from Seattle Police Department incident reports and the crime categories used in the empirical estimation. All incident report data are available for download at https://data.seattle.gov/.

Table 1.40: Crosswalk of Seattle Police Department Offense Codes into Crime Categories

Violent Crime (Including Alcohol-Related)		
ASSLT-AGG-BODYFORCE	HOMICIDE-JUST-GUN	ROBBERY-OTHER
ASSLT-AGG-GUN	HOMICIDE-NEG-MANS-BODYFORCE	ROBBERY-RESIDENCE-BODYFORCE
ASSLT-AGG-POLICE-BODYFORCE	HOMICIDE-NEG-MANS-VEHICLE	ROBBERY-RESIDENCE-GUN
ASSLT-AGG-POLICE-GUN	HOMICIDE-PREMEDITATED-BODYFORC	ROBBERY-RESIDENCE-WEAPON
ASSLT-AGG-POLICE-WEAPON	HOMICIDE-PREMEDITATED-GUN	ROBBERY-STREET-BODYFORCE
ASSLT-AGG-WEAPON	HOMICIDE-PREMEDITATED-WEAPON	ROBBERY-STREET-GUN
ASSLT-NONAGG	INJURY - ACCIDENTAL	ROBBERY-STREET-WEAPON
ASSLT-NONAGG-POLICE	INJURY - OTHER	THREATS-DIGNITARY
ASSLT-OTHER	LIQUOR LAW VIOLATION	THREATS-KILL
DISORDERLY CONDUCT	LOITERING	THREATS-OTHER
DISPUTE-CIVIL PROPERTY (AUTO)	MALICIOUS HARASSMENT	THREATS-WEAPON
DISPUTE-CIVIL PROPERTY (NON AU	PROPERTY DAMAGE - GRAFFITI	TRAFFIC
DISPUTE-OTH	PROPERTY DAMAGE-NON RESIDENTIA	TRESPASS
DISTURBANCE-NOISE	PROPERTY DAMAGE-RESIDENTIAL	URINATING/DEFECATING-IN PUBLIC
DISTURBANCE-OTH	RECKLESS BURNING	VIOL-COURT ORDER
DRIVE-BY	ROBBERY-BANK-BODYFORCE	WARRANT-FUGITIVE
DUI-LIQUOR	ROBBERY-BANK-BODYFORCE	WARRARR-FELONY
ELUDING-FELONY FLIGHT	ROBBERY-BANK-GUN	WARRARR-MISDEMEANOR
ENDANGER	ROBBERY-BANK-GUN	WEAPON-CONCEALED
ENDANGERMENT	ROBBERY-BANK-OTHER	WEAPON-DISCHARGE
ESCAPE	ROBBERY-BANK-WEAPON	WEAPON-POSSESSION
HARASSMENT	ROBBERY-BUSINESS-BODYFORCE	WEAPON-SURRENDER-EXCLUDING FIR
HARASSMENT	ROBBERY-BUSINESS-GUN	WEAPON-UNLAWFUL USE
HARBOR - BOATING UNDER INFLUEN	ROBBERY-BUSINESS-WEAPON	
Nonviolent Crime		
ANIMAL-BITE	FRAUD-WIRE-ELECTRONIC	THEFT OF SERVICES
ANIMAL-CRUELTY	GAMBLE-BETTING	THEFT-AUTO PARTS
ANIMAL-OTH	ILLEGAL DUMPING	THEFT-AUTOACC
BIAS INCIDENT	NARC-FORGERY-PRESCRIPTION	THEFT-BICYCLE
BURGLARY-FORCE-NONRES	NARC-FRAUD-PRESCRIPTION	THEFT-BOAT
BURGLARY-FORCE-RES	OBSTRUCT	THEFT-BUILDING
BURGLARY-NOFORCE-NONRES	PORNOGRAPHY-OBSCENE MATERIAL	
BURGLARY-NOFORCE-RES	PROP RECOVERED-OTHER AGENCY	
BURGLARY-SECURE PARKING-NONRES	PROPERTY FOUND	THEFT-CARPROWL
BURGLARY-SECURE PARKING-RES	PROPERTY LOST	THEFT-COINOP
COUNTERFEIT	PROPERTY LOST - POLICE EQUIPME	THEFT-LICENSE PLATE
DUI-DRUGS	PROPERTY RECOVERED - POLICE EQ	THEFT-MAIL

Continued on next page

Table 1.40: Crosswalk of Seattle Police Department Offense Codes into Crime Categories, Continued

Nonviolent Crime		
EMBEZZLE	PROPERTY STOLEN - POLICE EQUIP	THEFT-OTH
EXTORTION	PROPERTY STOLEN-POSSESS	THEFT-PKPOCKET
FALSE REPORT	PROPERTY STOLEN-RECEIVE	THEFT-PRSNATCH
FIREWORK-POSSESS	PROPERTY STOLEN-SELL	THEFT-UNLAWFUL ISSUANCE OF BAN
FIREWORK-USE	PROPERTY STOLEN-TRAFFICKING	VEH-RCVD-FOR OTHAGY
FORGERY-CHECK	PROSTITUTION	VEH-RCVD-FOR OTHER AGENCY
FORGERY-CREDIT CARD	PROSTITUTION LOITERING	VEH-THEFT-AUTO
FORGERY-OTH	PROSTITUTION PATRONIZING	VEH-THEFT-BUS
FRAUD-CHECK	PROSTITUTION-ASSIST-PROMOTE	VEH-THEFT-HVYEQUIP
FRAUD-COMPUTER	SOAP-VIOL - ZONE 4	VEH-THEFT-MTRCYCLE
FRAUD-CREDIT CARD	SODA-VIOL-EAST	VEH-THEFT-OTHVEH
FRAUD-IDENTITY THEFT	SODA-VIOL-NORTH	VEH-THEFT-RECREATION VEH
FRAUD-OTHER	SODA-VIOL-WEST	VEH-THEFT-TRAILER
FRAUD-WELFARE		VEH-THEFT-TRUCK
		WEAPON-SELLING

Shoplifting Crime		
THEFT-SHOPLIFT		

Drug Crime		
NARC-DISTRIBUTE-HALLUCINOGEN	NARC-POSSESS-AMPHETAMINE	NARC-SELL-COCAINE
NARC-DRUG TRAFFIC LOITERING	NARC-POSSESS-BARBITUATE	NARC-SELL-HALLUCINOGEN
NARC-EQUIPMENT/PARAPHENALIA	NARC-POSSESS-COCAINE	NARC-SELL-HEROIN
NARC-FOUND-AMPHETAMINE	NARC-POSSESS-HALLUCINOGEN	NARC-SELL-MARIJU
NARC-FOUND-COCAINE	NARC-POSSESS-HEROIN	NARC-SELL-METH
NARC-FOUND-HALLUCINOGEN	NARC-POSSESS-MARIJU	NARC-SELL-OPIUM
NARC-FOUND-HEROIN	NARC-POSSESS-METH	NARC-SELL-OTHER
NARC-FOUND-MARIJU	NARC-POSSESS-OPIUM	NARC-SELL-PILL/TABLET
NARC-FOUND-METH	NARC-POSSESS-OTHER	NARC-SELL-PRESCRIPTION
NARC-FOUND-OPIUM	NARC-POSSESS-PILL/TABLET	NARC-SELL-SYNTHETIC
NARC-FOUND-OTHER	NARC-POSSESS-PRESCRIPTION	NARC-SMUGGLE-COCAINE
NARC-FOUND-PILL/TABLET	NARC-POSSESS-SYNTHETIC	NARC-SMUGGLE-MARIJU
NARC-MANUFACTURE-HALLUCINOGEN	NARC-PRODUCE-MARIJU	NARC-SMUGGLE-METH
NARC-MANUFACTURE-OTHER	NARC-SELL-AMPHETAMINE	

1.10 Bibliography

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Chapter 2

Public Sector Wage Rents and Industry Privatization

Abstract

Industry privatizations that result in exogenous job displacement of public employees can be exploited to estimate public sector wage rents. I report the findings of an original survey I administered to examine how wages of displaced government workers were affected by a 2012 privatization of liquor retailing in Washington State. Based on a panel difference-in-differences estimator I find that privatization reduced wages by \$2.51 per hour or 17 percent compared to a counterfactual group of nearly identical non-displaced workers, with larger effects for women. I decompose wage losses into three rents identified in the literature: public sector rents, union premiums and industry-specific human capital. Public sector wage premiums separately account for 85 to 90 percent of overall wage losses, while union premiums and industry-specific human capital account for just 10 to 15 percent. The results are consistent with a roughly 16 percent public sector wage premium.

2.1 Introduction

Are state government workers overpaid? The question has received renewed attention in recent years as state budget shortfalls and deteriorating public employee pension systems have prompted increased scrutiny of public sector pay. Labor economists have long recognized the potential for "rent extraction" by public employees in the form of wages and benefits in excess of workers' outside alternatives. Government wages are not set in competitive markets (Brueckner and Neumark 2014), workers are overwhelmingly unionized (Visser 2006) and public employees are politically active (O'Brien 1992). Public-sector rent extraction has taken a variety of non-wage forms as well, including increased health and pension benefits (Clemens and Cutler 2013) and deflection of budget cuts away from programs associated with strong public sector unions (Clemens 2012). For these reasons, the stylized fact of public employee wage premiums has been incorporated into a variety of formal models of public sector labor markets (Brueckner and Neumark 2014; Glaser and Ponzetto 2013; Holmlund 1993; Borjas 1980; Reder 1975; and Fogel and Lewin 1974).

While economic theory makes clear predictions about public employee wage premiums, the large empirical literature on the subject is mixed.[1] Most studies find evidence of wage premiums among federal government employees on the order of 10 to 20 percent relative to private sector counterparts. However, premiums are typically found only at the highest levels of government: the federal government in the U.S., and comparable central governments internationally. Wage premiums for state and local employees are zero or slightly negative in most estimates. This suggests the rent seeking central to most theoretical models of public sector labor markets may be a poor description of local public employee labor markets.

In recent years, a vigorous political debate surrounding state budget crises has prompted a reexamination of public compensation. At one extreme Keefe (2012) finds no evidence of public sector wage premiums, reporting a 7.6 percent earnings penalty among state workers based on cross-sectional microdata from

[1] Extensive reviews of the literature on public sector wage premiums are available in Gregory and Borland (1999); and Ehrenberg and Schwarz (1986).

IPUMS-CPS and the Employer Costs for Employee Compensation survey. At the other extreme Gittleman and Pierce (2012) find a significant 3 to 10 percent earnings premium for state employees relative to comparable private sector workers by applying a different set of controls for occupation and employer size to essentially identical data. Both studies were covered extensively in the media, fueling the longstanding political controversy over the degree to which state employees are over (or under) compensated and thus a contributing (or non-contributing) factor to state budget woes.[2]

This study presents evidence on public sector wage premiums from a different source of identification. Rather than comparing private and public sector wages in the cross section, I make use of a panel of exogenously displaced government workers laid off by a 2012 privatization of liquor retailing in Washington State. Enacted by voter ballot initiative, the privatization closed 167 state-run retail liquor stores and laid off an entire occupational category of public employees. As a comparison group, I use government workers in similar retail occupations who were unaffected by the policy. The decision by displaced workers to accept employment following layoffs serves as a revelation mechanism for the second-best wage offer facing displaced public sector workers. Due to the exogenous timing and non-selective nature of the displacements, these quasi-experimental "treatments" can be used to identify public sector wage premiums.[3]

The treatment group consists of exogenously displaced government workers for whom wages are observed before and after privatization. I collected information on post-policy wages and other demographic characteristics of the displaced workers via an original mail and online survey questionnaire. These responses

[2]See for example, Ezra Klein, "Public Employees Don't Make More than Private Employees," Washington Post, September 16, 2010 (http://voices.washingtonpost.com/ezra-klein/2010/09/public_employees_dont_make_mor.html); and Sita Slavov, "How Politicians Buy Votes By Doling Out Public Worker Benefits," U.S. News and World Report, May 2, 2013 (http://www.usnews.com/opinion/blogs/economic-intelligence/2013/05/02/public-sector-employees-receive-generous-benefits-due-to-politics).

[3]An ideal comparison group would be a collection of similar private sector workers subject to a parallel exogenous mass layoff. However, this is an infeasible identification strategy. Private sector job displacements are rarely exogenous and are typically the result of adverse demand conditions affecting both layoffs and wages. However, unlike most literature on mass layoffs, a unique feature of my setting is that demand conditions were stable throughout the period. I exploit this feature to directly estimate public wage rents.

were then matched to pre-policy wage information from administrative records, resulting in an individual-level panel of wages and other demographic details over the nine year period from 2005 to 2013. The control group consists of a panel of remarkably similar employees working in comparable retail occupations in state government: customer service specialists in the state's motor vehicle licensing offices. These individuals were employed by the same state government, worked in retail occupations with similar job skills, were similarly unionized, and worked in comparable urban and suburban retail outlets. Average wages for both groups of workers followed nearly identical trends throughout the pre-policy period. I use these data to identify the causal effect of privatization-related job displacements on public sector wages using a panel difference-in-differences estimator with individual and time fixed effects.

Based on a panel of 262 workers from 2010 to 2013, I find that privatization reduced wages of displaced government workers by $2.51 per hour, or roughly 17.2 percent compared to the control group of unaffected workers. Consistent with past literature I find somewhat larger effects among female workers, with wages falling by $2.87 per hour or 19.7 percent for females compared with $2.20 per hour or 15.1 percent for males, although the two estimates are not statistically different. Quantile regression results reveal considerable heterogeneity of treatment effects throughout the conditional wage distribution, ranging from $4.62 per hour at the 5th percentile of wages to a statistical zero effect at the 95th percentile. These basic findings are unaffected by (1) the inclusion of individual-level controls for gender and length of job tenure; by (2) excluding from the sample individuals who reported more valuable non-wage fringe benefits in their post-policy employment; and by (3) estimation via a kernel matching difference-in-differences estimator in which treated and control workers are propensity-score matched based on observables.[4]

By examining treatment effects in various subsamples, it is possible to provide a rough decomposition of overall wage losses into three types of wage rents identified by the previous literature: public sector wage premiums; union wage premiums; and wage premiums from industry-specific human capital lost following

[4] These alternative estimates are presented in the Appendix.

job displacement. I decompose overall wage losses into lost union, public-sector and industry-specific wage rents by examining linear combinations of treatment effects from various subsamples of workers who sorted into (1) private-sector retail liquor jobs, (2) government jobs, (3) union jobs and (4) non-union jobs following privatization.[5] I find that public sector wage rents separately account for roughly 86 to 90 percent of the overall decline in wages following privatization. By contrast, lost industry-specific human capital accounts for 11 to 13 percent of wage declines, while lost union wage premiums account for roughly 1 percent or less. These results suggest that displaced state government workers earned a roughly 16 percent public sector wage premium prior to displacement. This estimate lies somewhat above the 3 to 10 percent earnings premium reported by Gittleman and Pierce (2012), and is substantially larger than estimates from comparable panel studies such as Krueger (1988) and Lee (2004) which are not based on exogenous job separations. However, the effects are considerably smaller than those found in Galiani and Sturzenegger (2008), and are consistent with those found in similar international studies of the effect of privatizations on displaced workers such as Firpo and Gonzaga (2010).

This study is not the first to use the privatization of state-owned firms as an exogenous shock to identify public sector wage rents. Studies in Brazil (Firpo and Gonzaga 2010), Portugal (Monteiro 2010), Argentina (Galiani and Sturzenegger 2008) and the U.K. (Disney and Gosling 2003) have used a similar approach in recent years, yielding a wide variety of estimates from zero to roughly 40 percent wage premia. This study contributes to this growing literature by applying the approach to public employees in the United States for the first time. This extension is relevant as U.S. labor markets are generally more flexible and labor unions weaker than in previous countries examined, and thus conclusions from overseas studies may not apply to public sector workers in the U.S. While the source of identification is novel, it is not without limitations. I examine a small sample of government workers in an unusual occupational category; thus, the results may not

[5] In section 2.5.3 I examine whether these results are driven by non-random selection of displaced workers into post-policy industries. I find no evidence that selection on observables such as education, work experience and gender explain the results.

easily generalize to broader classes of public employees. However, by making use of a clean source of identifying variation the approach overcomes many of the sorting, selection and endogenous job separation problems that have plagued past research. With several states considering similar liquor privatization initiatives, this study provides the first estimates of the likely effect of those policies on displaced public employees.[6]

The remainder of the paper is organized as follows. Section 2.2 reviews the related literature. Section 2.3 gives background on the privatization of liquor retailing in Washington State. Section 2.4 describes the data and survey design. Section 2.5 explains the difference-in-differences identification strategy. Section 2.6 presents my results, and Section 2.7 concludes. The Appendix presents tables of alternative results and a copy of the original survey questionnaire.

2.2 Related Literature

2.2.1 Public Sector Wage Premiums

The empirical literature on public sector wage premiums is large, both in the U.S. and internationally. Gregory and Borland (1999) and Ehrenberg and Schwarz (1986) review the early literature. Most research has focused on federal employees, with a smaller number of studies examining state and local workers. Among those studies examining state employees, most have been cross sectional based on large, publicly available data sets. Following Smith (1976), early studies employed Oaxaca (1973)-style decompositions or simple OLS with controls to identify public sector wage premiums. Nearly all studies report a positive wage premium for federal workers on the order of 10 to 20 percent, with higher premiums for women and those in high-amenity urban locations, but zero or slightly nega-

[6]For example, Pennsylvania and Oregon are currently engaged in active political debates regarding the privatization of their state-run liquor retailing systems. See Kate Giammarise, "Pennsylvania liquor overhaul brews big spending," *Pittsburgh Post-Gazette* (May 26, 2014), available at http://www.post-gazette.com/news/politics-state/2014/05/26/Pa-liquor-overhaul-brews-big-spending/stories/201405260074; and Harry Esteve, "Liquor privatization initiative moves forward," *The Oregonian* (May 17, 2014), available at http://www.oregonlive.com/politics/index.ssf/2014/05/liquor_privatization_initiativ.html.

tive wage premiums for state and local government workers (Gregory and Borland 1999).

The basic identification problem faced by the early literature is the inability to address non-random assignment of workers into sector of employment in the cross section. Workers choose jobs partly on the basis of unobserved characteristics, including risk aversion and attitudes toward public service, which are likely correlated with productivity. Recognizing this problem, a second wave of studies beginning with Robinson and Tomes (1984) and Gyourko and Tracy (1988) employed Heckman-style correction methods to cross-sectional data, resulting in somewhat smaller estimates of federal wage premiums but still zero or slightly negative premiums for state and local workers. The modern literature has made little progress beyond these methods. For example, the two recent studies of Keefe (2012) and Gittleman and Pierce (2012) both estimate wage premiums for state government workers using OLS with controls in large, cross-sectional data sets.[7]

Just two studies have used longitudinal data to address the problem of unobserved worker heterogeneity: Krueger (1988) and Lee (2004). Both identify government wage premiums via fixed effects estimators identified off workers who voluntarily shift between private and government jobs. Krueger (1988) uses matched files from the Current Population Survey and the supplemental Displaced Worker Survey to identify "switchers" who moved between sectors in subsequent years in the two panels. He finds somewhat smaller federal wage premiums of 5 to 10 percent for federal workers, but again a statistically zero wage premium for state workers. Subsequently, Krueger (1988) has been criticized for the small number of "switchers" used for identification in the two panels—for example, the Displaced Worker Survey used in the study contains information on just 91 workers who switched between sectors—and thus the representativeness of the results (Moulton 1990; and Lee 2004).

Lee (2004) presents longitudinal estimates of public sector wage premiums using the National Longitudinal Survey of Youth (NLSY) for the first time. For

[7] One area modern literature has made progress on is the inclusion of non-wage "fringe" benefits in the estimation of public sector wage premiums, which was largely neglected in early research due to data limitations.

state government employees, the survey contains information on 214 male and 309 female "switchers," roughly five times the number used in Krueger (1988). He finds a 5 percent wage premium for federal workers that is well below most cross sectional estimates. For state government employees he reports a 4 percent wage premium for women and a 1 percent penalty for men, both of which are substantially higher (i.e., more positive) than typical cross-sectional estimates. An important criticism of both Krueger (1988) and Lee (2004) is that neither makes use of exogenous job separations to identify wage premiums. Workers' decisions to become "switchers" between sectors are likely endogenous with respect to productivity and wages. For example, if workers of low (or high) ability are disproportionately observed shifting from private sector jobs into the public sector over time, panel estimates of wage premiums will be biased and inconsistent. Without exogenous job separations the causal effect of sectoral shifts on wages is not identified, even in panel data.

In recent years, a smaller literature has emerged using quasi-experimental job displacements among government workers to identify public sector wage premiums. The transition of state-owned firms in banking, petroleum and liquor retailing into private ownership typically results in layoffs for large numbers of government employees. To the extent that the policy decision to privatize is exogenous with respect to wages of the affected employees, and if layoffs are non-selective among workers, these job displacements can be used as "treatments" to investigate the loss of wage rents among the affected workers. Recent examples of this approach include Firpo and Gonzaga (2010) in Brazil, Monteiro (2010) in Portugal, Galiani and Sturzenegger (2008) in Argentina, and Disney and Gosling (2003) in the U.K. Estimates of public sector wage premiums from this newer literature range from 40 percent among former petroleum workers in Argentina (Galiani and Sturzenegger 2008) to 11 percent among a variety of workers in Brazilian state-run firms (Firpo and Gonzaga 2010). Only Disney and Gosling (2003) find no evidence of wage premiums based on privatization from the 1990s in the U.K. This study contributes to the growing literature by applying this quasi-experimental approach to displaced public sector workers in the U.S. for the first time.

2.2.2 Displaced Worker Studies

Because this study examines public employees displaced by a mass layoff event, it is also related to the large empirical literature on “displaced workers.” Extensive reviews are provided by Kletzer (1998), Fallick (1996) and Hamermesh (1989). Among workers displaced by plant closings and other mass layoffs, studies typically find significant wage declines that persist for years after displacement. Summarizing the literature, Kletzer (1998) offers five broad explanations for these observed wage losses: (1) loss of industry-specific human capital; (2) loss of firm-specific human capital; (3) loss of high-quality matches with employers; (4) loss of industry-specific rents (including public sector wage rents); and (5) loss of union premiums. A key challenge faced in this study is separately identifying which of these effects played a role in the observed changes in wages among displaced state workers.

A common finding throughout the displaced worker literature is that wage losses are highly concentrated among workers who change industries following layoffs (Kletzer 1998). Workers who remain in the same industry typically suffer small or zero permanent wage losses. This suggests industry-specific capital plays a central role in explaining observed wage losses among displaced workers. Several high-quality studies have confirmed this pattern, finding most worker skills appear to be transferrable within the same industry following layoffs, and that workers who find post-displacement jobs in the same industry experience few wage effects (Neal 1995; Carrington 1993; Ong and Mar 1992; Addison and Portugal 1989).

Among the displaced WSLCB workers I examine, the effect of any lost industry-specific human capital should be concentrated among those who shift out of the liquor retailing industry following privatization. By contrast, those who remain in the industry following privatization should not experience this effect, as industry-specific skills have been retained. Similarly, the effect of lost union premiums and public sector rents should be concentrated among those who shift into non-union and private sector jobs, respectively, while those remaining in union and government jobs should not experience these effects.[8] It is possible that this

[8]For workers who remain in unionized jobs following privatization, I assume union wage

decomposition based on post-policy employment decisions by workers introduces biases due to endogenous sorting across occupations; however, I show below that there is no evidence of this on observable worker dimensions. For simplicity, I group the effects of job displacement on wages into three categories: (1) lost public sector wage rents; (2) lost union premiums; and (3) wage losses due to lost industry-specific capital. Using this approach, in Section 2.6 I decompose overall wage losses suffered by displaced public workers into three distinct wage rents identified in past literature.

2.3 Policy Background

In November 2011, Washington State voters approved ballot initiative I-1183, privatizing the state's liquor retailing and distribution system.[9] Previously the state was one of 19 "control" states that maintain some form of public monopoly over liquor retailing.[10] For seven decades, liquor retailing was state owned and operated under the supervision of the Washington State Liquor Control Board (WSLCB). The passage of I-1183 abruptly ended the system, closing 167 state-owned liquor retailers and liquidating the assets at auction.[11] As a consequence, approximately 900 public sector workers employed in liquor retail establishments were laid off on June 1, 2012.

A key feature of the policy is that the resulting layoffs were strongly exogenous with respect to any wage premiums enjoyed by the affected workers. The primary impetus for the ballot initiative was business lobbying by local retailers,

premiums are preserved. However, it is possible that a loss of tenure when transferring between unions could also affect wages. Because length of job tenure at the time of displacement is perfectly collinear with individual fixed effects, I am unable to fully resolve this issue in the data.

[9] By "liquor" I refer only to distilled spirits. Beer and wine have long been privately retailed in the state and were unaffected by the privatization initiative.

[10] Following Washington's privatization 18 states maintain public monopolies over liquor retailing and distribution. The remaining "control" states are: Alabama, Idaho, Iowa, Maryland (Montgomery and Worcester counties only), Maine, Michigan, Mississippi, Montana, New Hampshire, North Carolina, Ohio, Oregon, Pennsylvania, Utah, Vermont, Virginia, West Virginia and Wyoming. Source: National Alcohol Beverage Control Association (http://www.nabca.org).

[11] The state also maintained 162 privately owned "contract" liquor stores primarily located in rural areas of the state. Contract stores remained in operation following privatization, but were required to purchase all remaining inventory from the state.

and was not explicitly designed to reduce public sector employment or wages.[12] This is a unique feature of the policy relative to most existing literature on mass layoffs, as job displacements are typically accompanied by confounding shifts in industry demand conditions or endogenous state budget pressures, neither of which were present in this setting.

The displaced public employees were union represented by the United Food and Commercial Workers Local 21 (UFCW 21). Wages were set by a collective bargaining agreement with the state. Nearly all of the affected workers held the job title "Liquor Store Clerk" or "Retail Manager." Job responsibilities were typical of retail occupations: ringing up purchases at cash registers, maintaining merchandise displays, restocking shelves and answering customer questions. The formal requirement for these positions was a high school diploma, and the jobs required little specialized technical skills or knowledge. The displaced workers were employed in liquor retailers located primarily in urban and suburban areas throughout the state. Stores averaged 5,200 square feet, maintained staffs of 3 to 12 employees, and had average annual retail sales of $4 million per store. Overall, the industry was likely characterized by a similar production function to small, urban grocery and convenience stores that operate in the private sector.

Following privatization, the displaced workers were eligible for ordinary unemployment insurance benefits, but no special benefit provisions were made. Some media outlets reported local firms offering open-door job interviews to displaced workers,[13] but there was no formal process to retrain or place workers into alternative employment. Fearing wage losses and reduced employment, WSLCB employees and their union representatives were among the most vocal opponents of the privatization initiative.[14] Both before and after the policy change there was widespread speculation about the fate of the roughly 900 displaced public sector workers. This study is the first to examine how earnings and employment of the

[12]See Austin Jenkins, "Costco Breaks Records With $22M To Privatize Liquor," *NPR*, October 19, 2011 (http://www.npr.org/templates/story/story.php?storyId=141531406).

[13]See Melissa Allison, "Costco offers job interviews to displaced state liquor-store workers," *Seattle Times* (November 10, 2011), available at http://seattletimes.com/html/localnews/2016734642_costco11.html.

[14]See Melissa Allison, "Unions sue to block liquor initiative from taking effect" (December 6, 2011), available at http://seattletimes.com/html/localnews/2016947384_liquorsuit07.html.

displaced retail liquor workers were affected by the 2012 privatization.

2.4 Data

2.4.1 Survey Design

I implemented an original survey of the roughly 900 state workers whose occupations were eliminated by the 2012 liquor privatization, based on individual contact information provided by Local 21 of the United Food and Commercial Workers union (UFCW 21). The survey collected detailed information on earnings, employment and other demographic characteristics from respondents.[15] Names and home addresses were obtained for 911 displaced workers, 284 of whom listed email addresses. The survey followed a multi-mode approach consisting of (1) a recruitment letter; (2) an online survey; (3) a traditional mail survey; and (4) a follow-up reminder letter. The initial wave consisted of 284 emails to individuals for whom addresses were available inviting them to complete an online survey between July 12 and August 4, 2013, roughly one year after displacement. This was followed by a mail survey to the remaining individuals between August 5 and September 10, 2013. Mail surveys also included an option for completing an online survey using a unique 4-digit code, preventing duplicate responses. In total, the survey collected $N = 404$ responses for a response rate of 44.3 percent. 199 online questionnaires were submitted and 205 were received via mail.

For the full population of 900 affected workers, I obtained information on pre-policy wages, hours and gender from administrative data from the Washington State Office of Financial Management (WA OFM). These data are drawn directly from state accounting records and contain the name, gender, job title, hourly wage, and average weekly hours for 2010, 2011 and 2012 for all workers affected by the policy. These population characteristics enable me below to examine whether there is any evidence of systematic differences between this subset of survey respondents

[15] The survey was granted institutional review board approval by the "Human Research Protections Program" at the University of California, San Diego on March 12, 2013. Information about the review process is available at http://irb.ucsd.edu/about.shtml. For reference, a complete copy of the survey recruitment letter and questionnaire is provided in the Appendix.

and the overall population. For the $N = 404$ survey respondents, the WA OFM data were matched to individual survey responses on the basis of employee names, providing a longitudinal file of earnings and other demographic information for both pre- and post-policy periods.

Tables 2.1 and 2.2 present basic descriptive statistics for the full sample of 404 respondents to the survey. The gender balance among survey respondents closely matches the overall population, with 42.1 percent male and 57.9 female compared to 43.8 and 56.2 percent in the overall population of displaced workers, respectively. The average age is 45 to 49 years. 84 percent report their ethnicity as white or Caucasian, with 4.2 percent African American, 3.7 percent Asian or Pacific Islander and 3.5 percent Hispanic. Nearly half live in married households. The overwhelming majority of workers (84 percent) have less than a 4-year college degree.

In terms of employment, 56.2 percent of respondents reported being employed one year after displacement, with 25.2 percent full time, 29 percent part time, and 2 percent self-employed. 30 percent of respondents were unemployed by the usual definition. The remaining 13.8 percent exited the labor force for a variety of reasons, including retirement (8.4 percent), to become a student or homemaker (4 percent), or because they simply stopped looking for work (1.5 percent). The implied unemployment rate among displaced workers was 34.5 percent one year after the policy. By comparison, the state's overall unemployment rate was roughly 7 percent during the same period.

Because we do not observe post-policy wages for the subset of displaced workers who remain unemployed, our estimates of the effect of displacement on wages may suffer bias from this exclusion. To the extent that unemployed workers would have accepted wages that are above (or below) those observed among employed workers, the estimated treatment effects will over- (or under-) state the impact of the policy. To assess the importance of this concern, I examine whether employed and unemployed workers significantly differ on observable individual characteristics. Letting U_{it} be a binary indicator equal to 1 if survey respondent i was unemployed during the post-policy period and 0 otherwise, I estimate the following linear probability model,

$$\begin{aligned} \text{Prob}(U_i = 1 \mid X) = \beta_o + \beta_1 \text{Wage}_i + \beta_2 \text{Hours}_i + \beta_3 \text{Tenure}_i + \beta_4 \text{Female}_i + \\ \beta_5 \text{Age} + \beta_6 \text{Moved} + \beta_7 \text{Race} + \beta_8 \text{Education} + \beta_9 \text{HHSize} + \epsilon_i \end{aligned} \quad (2.1)$$

Pre-policy wages and hours are from administrative data from WA OFM. Mover status is determined by a comparison of individuals' pre-policy addresses with post-policy address from roughly one year after the policy based on a USPS-validated mailing list. Data for all other characteristics are from the administered survey. The estimation uses the full sample of $N = 404$ survey respondents. Table 2.3 presents the results. Pre-policy wages, hours, and all other individual characteristics have zero predictive power on post-policy employment status. Put differently, unemployed and employed workers do not differ significantly in terms of pre-policy observables. None of the coefficients on worker characteristics are significant, and together they explain less than 4 percent of the variation in employment status. It is possible that unemployed workers may differ on unobservable characteristics from employed workers, but there is no evidence of systematic selection into unemployment on observables.

Average pre-policy wages among respondents were \$14.35 per hour, with a standard deviation of \$2.57. This amounts to annual earnings of roughly \$29,850 for full-time workers, well below the average annual \$55,000 earnings for Washington state employees overall in 2012.[16] Post-policy wages were a significantly lower \$13.33 per hour, with a larger standard deviation of \$4.93.[17] Most respondents who were employed found jobs in the same or similar fields, the most common being liquor retail (26.9 percent), with 13.7 percent finding jobs in the same store they previously worked in. The second most common was general non-liquor-related retail jobs (22 percent), followed by government jobs (8.8 percent), administrative jobs (8.4 percent), education (5.7 percent), and restaurant or hotel services (4.8 percent).

[16] See the U.S. Census Bureau's "Quarterly Census of Employment and Wages," Series ID ENU5300050292

[17] I address the issue of possible misreporting of wages by survey respondents in the following section.

Among the employed, just under half reported receiving zero non-cash "fringe" benefits post-policy, with 44 percent reporting health insurance, 40.5 percent dental insurance, 44 percent paid vacation, 34.4 percent retirement benefits and less than 10 percent childcare or transportation benefits. To assess whether wage declines were partly (or completely) offset by increases in non-wage benefits, respondents were asked to compare the dollar value of their current benefits to those provided by their previous government employer. 91.2 percent reported post-policy fringe benefits were less valuable or of roughly the same value as pre-policy benefits.[18] In terms of union membership, 18.9 percent of the employed reported working in a union job. 13 percent of respondents moved to a new home address during the year after job displacement, while 87 percent remained in the same location.

2.4.2 Construction of Treated Group

For the treatment group, I restrict the sample to a balanced panel of state workers who were involuntarily laid off by the 2012 privatization and for whom wages are observed in all pre- and post-policy years. From the sample of $N = 404$ survey respondents, I omit 37 individuals who self-selected out of treatment by voluntarily quitting before the policy went into effect in June 2012. This is done to isolate only those individuals for whom job displacement was involuntary and strongly exogenous with respect to wages.[19] To avoid confounding effects of attrition from individuals moving in and out of public sector employment over time, I further restrict the sample to individuals for whom I observe public-sector wages in all years, creating a balanced panel of treated workers.[20] In choosing panel size, I face a trade-off between panel length T and total observations NT as

[18] Respondents were asked, "If you are employed, think about the dollar value of your current [fringe] benefits. Are they worth less, more, or about the same as the benefits you received at your Washington State liquor retail job?"

[19] Workers who resigned early may have done so due to ordinary job shifting that was unrelated to the policy, such as the acceptance of a superior outside offer. In the Appendix I show estimates including these 37 individuals, and doing so has no effect on the main results.

[20] Because post-policy wages are unobserved for unemployed individuals, they are excluded from the sample. If instead unemployed workers are included with their post-policy wage w^*_{post} set equal to zero, estimated treatment effects are roughly three times larger.

the number of individuals observed in all years falls sharply as the panel length is extended backward into the pre-policy period. I select the four-year panel from 2010 to 2013 that maximizes NT, consisting of three pre-policy periods and one post-policy period. This results in a balanced panel of $N = 143$ workers over $T = 4$ periods ($NT = 572$).[21] This panel serves as the "treated" group in the difference-in-differences estimates below. Table 2.4 presents summary statistics for this treated group.

Non-Response Bias in Treated Group

Because survey participation was voluntary, it is possible that the sample of treated workers differs systematically from the population due to survey non-response. To examine the representativeness of the treated group I regress an indicator of survey response on various pre-policy characteristics of workers for whom wages are observed in all years. For a random sample, pre-policy observables should have little explanatory power when regressed on the probability of response. Letting Y_{it} be a binary indicator equal to 1 for survey respondents and 0 for non-respondents, I estimate the following linear probability model,

$$\text{Prob}(Y_i = 1 \mid X) = \beta_o + \beta_1 \text{Wage}_i + \beta_2 \text{Hours}_i + \beta_3 \text{Female}_i + \beta_4 \text{Moved}_i + \epsilon_i \quad (2.2)$$

As noted above, pre-policy wages, hours and gender are drawn from administrative data from WA OFM, and mover status is determined by a comparison of individuals' pre- and post-policy addresses. The sample consists of 557 individuals for whom wages were observed on all pre-policy years, of whom 286 are survey respondents and 271 are non-respondents. Table 2.5 presents the estimation results. Taken together, worker characteristics explain roughly 1 percent of the variation in response probabilities. In Column (1), the univariate regression of 2011 wages on response probability is statistically significant but small, suggesting each \$1 in higher wages corresponds to a 1.5 percent increased likelihood of survey response. However, this effect disappears when additional controls are included. In Columns

[21] By comparison, the longest possible panel length consists of $N = 43$ over $T = 7$ periods ($NT = 301$), a significantly smaller sample size.

(2) - (5) all of the estimated coefficients on wages, hours and gender are insignificant, suggesting an absence of systematic selection into survey participation on observables. The only factor that significantly predicts survey nonresponse when all controls are included in Column (5) is having moved to a new address during the post-policy period. Movers were roughly 17 percent less likely to have responded to the survey. To the extent that movers and non-movers experienced different post-policy outcomes, this will not be fully reflected in the sample. However, for those whom I have data on post-policy outcomes, movers and non-movers report post-policy wages that are not statistically different from one another, limiting the practical importance of this potential underrepresentation of migrating households.

Figure 2.1 shows the unconditional distributions of pre-policy wages among survey respondents and non-respondents. The left panel shows the distribution of 2011 wages among the 286 survey respondents for whom wages are observed in all years, while the right panel shows the distribution of pre-policy wages for the 271 non-respondents. The two distributions are nearly identical, with a modal wage just below \$15 per hour and a range of roughly \$11 to \$19 per hour. Overall, systematic survey non-response does not appear to pose an important threat to identification.[22]

2.4.3 Construction of Control Group

As a control group, I identified a collection of similar public employees who were unaffected by the privatization, but whose path of earnings provides a reasonable counterfactual for what the treated workers would have experienced in the

[22] A second concern is possible misreporting of wages by survey respondents. Displaced workers who were politically opposed to privatization may have incentives to strategically misreport earnings to maximize apparent harm suffered from displacement. It is possible to verify reported pre-policy wages based on administrative records. Survey respondents were asked to report both wages just prior to displacement in June 2012 to allow for such a verification. The average self-reported pre-policy wage was \$14.44 per hour. From administrative records, the actual average pre-policy wage for these same individuals was \$14.18 per hour, a small difference of 26 cents. The remaining gap is likely due to timing differences between self-recall wages and official records, as administrative records are based on a snapshot of wages in early January while self-reported wages are based on self-recall from the pay period immediately preceding displacement in June. For post-policy wages, there is unfortunately no way to independently verify their accuracy and is an inherent limitation of the survey data.

absence of displacement. Ideally these individuals would be employed within the same state government, work in similar occupations with comparable skills and would work in similar geographic areas as the displaced workers. A category of public employees who closely fit this description are “Customer Service Specialists” in the Washington State Department of Licensing (WA DOL). These employees provide over-the-counter driver’s licensing application and renewal services to the public. As they have one of the few other retail occupations in state government, these employees have similar job requirements, skill profiles, and urban and suburban locations as the displaced workers. The workers are similarly unionized, and are represented by the American Federation of State, County, and Municipal Employees union (AFSCME 28) Council 28. Wages are established by collective bargaining agreement with the state in a similar way. The path of earnings for these employees would almost certainly have mirrored the path of wages among the displaced WSLCB workers had they been unaffected by the policy, making them an ideal counterfactual group.[23]

Data on wages, hours and gender for the control group of workers were provided by WA OFM for the years 2010 to 2013. Unlike treated individuals who responded to the survey, little demographic information is available for the control workers. The information provided by WA OFM was matched at the individual level to public wage data for 2005, 2007 and 2009. Doing so allowed me to construct one additional covariate for the control group: estimated job tenure at WA DOL. The resulting individual-level panel consists of $N = 281$ workers between 2005 and 2013. As with the treated group, the sample was restricted to workers for whom wages were observed in all years from 2010 to 2013. This resulted in a balanced panel of $N = 119$ individuals over $T = 4$ years, for a panel of size $NT = 476$. Table 2.6 presents summary statistics for this control group. Combining treated and control groups, the overall panel used for my estimation contains $N = 262$ individuals over $T = 4$ years, for a panel of size $NT = 1{,}048$.

[23]Human resources representatives from the WA DOL reported several cases of retail liquor clerks moving into licensing customer service jobs following privatization, further confirming the broad similarity of the two occupational categories (obtained via telephone on April 2, 2014).

2.5 Identification Strategy

2.5.1 Method for Estimating Wage Losses

I identify the causal effect of liquor privatization on wages via a standard difference-in-differences (DD) estimation strategy. Conceptually, I estimate a pooled DD estimator in which panel observations of individuals from the treated and control groups are pooled into two groups in two pre- and post-policy time periods:

$$w_i = \beta_0 + \beta_1 \text{Post}_i + \beta_2 \text{Treated}_i + \beta_3 \text{Post}_i \text{Treated}_i + X_i'\Gamma + \epsilon_i, \qquad i = 1, ..., N, \quad (2.3)$$

where w_i is the hourly wage of observation i, Post_i and Treated_i are binary indicators equal to one during the post-policy period and for members of the treated group, respectively. X_i is a matrix of individual-level controls consisting of gender and length of job tenure, and ϵ_i is a mean-zero error term. The coefficient of interest is β_3, the usual DD estimator of the treatment effect of the policy on wages. Because my estimation makes use of a balanced panel of individual-level data, estimating treatment effects via (2.3) is equivalent to a fixed-effects panel model of the form,

$$w_{it} = \alpha_i + \gamma_t + \beta_3 P_{it} + X_{it}'\Gamma + \epsilon_{it}, \qquad i = 1, ..., N \quad t = 1, ..., T \qquad (2.4)$$

where α_i and γ_t are fixed effects for individual i and time period t, and P_{it} is a dummy equal to 1 if individual i is treated in period t, and zero otherwise. In (2.3) and (2.4) the estimated $\hat{\beta}_3$ has the same expectation and standard error, and provides a consistent estimate of the causal effect of the policy on wages. Equation (2.4) is my basic estimating equation.

To examine whether treatment effects vary throughout the conditional distribution of wages, I also present quantile regression estimates of the effect of job displacement on earnings.[24] Finally, as a robustness check I present estimates

[24]Rewriting the linear model from equation (2.3) as $w_i = X_i'\beta + \epsilon_i$, the Koenker and Bassett (1978) quantile estimator $\hat{\beta}_q$ for the average treatment effect at quantile q is given as the solution to
$\hat{\beta}_q = \underset{\beta \in R^k}{\arg\min} \sum_{i=1}^{N} \rho_q(w_i - X_i'\beta)$, where $\rho_q = (q - \mathbb{1}\{w_i - X_i'\beta < 0\})(w_i - X_i'\beta)$ is the usual "check function" that penalizes positive regression residuals by q and negative residuals by $1 - q$.

of propensity-score kernel matching difference-in-difference estimates in the Appendix. For these estimates, I use individual characteristics on gender and length of job tenure to estimate likelihoods of receiving treatment conditional on observables. Individuals in the treated group are then kernel-matched to a composite group of individuals from the control group with similar propensity scores.[25]

2.5.2 Assessing Pre-Policy Wage Trends

The key identifying assumption for my difference-in-differences estimator is that pre-policy wages for treatment and control workers followed parallel trends. Figure 2.2 shows hourly wages for the balanced panel of workers for whom wages are observed in all years from 2010 to 2013. The panel consists of 143 treated individuals and 119 control individuals observed over $T = 4$ periods. During the decade before privatization, average wages followed nearly identical trends for the two groups. This is practically by construction as both groups were union represented, had wages established by similar collective bargaining agreements and were employed by the same state government in similar retail occupations. Average wages grew steadily for both groups from 2005 to 2010, dipping slightly during the post-recession state budget crisis in 2011,[26] and stabilizing near pre-recession levels in 2012. Liquor privatization went into effect in June 2012, resulting in job displacement for the treated group. In the post-policy period of 2013, average wages diverge sharply for the two groups, with wages for the control individuals continuing their upward trend while wages fell sharply for treated workers. This parallel evolution in pre-policy wages provides an ideal setting for the identification of the causal effect of privatization on earnings via a standard difference-in-differences

[25] As detailed in Heckman *et al.* (1998) and Todd (2008), the resulting kernel-matching difference-in-differences estimator $\hat{\beta}_M$ is given by $\hat{\beta_M} = \frac{1}{n_1}\sum_{i\in I_1}\left\{(w_{1ti} - w_{0t'i}) - \sum_{j\in I_0} W(i,j)(w_{0tj} - w_{0t'j})\right\}$, where I_1 is the set of treated workers, I_0 is the set of control workers, t' and t are the pre- and post-policy periods, w_1 and w_0 are earnings for the treated and control groups, and $W(i,j)$ is a weighting function based on the epanechnikov kernel with the default bandwidth of 0.06.

[26] See Andrew Garber, "Gregoire and Unions Reach Agreement on Pay, Benefit Cuts," *Seattle Times* (December 15, 2010), available at http://seattletimes.com/html/localnews/2013680687_paycuts15m.html.

estimator.[27]

2.5.3 Method for Decomposition of Wage Rents

Treatment effects for displaced WSLCB workers should reflect the loss of three distinct wage rents: union premiums; public sector wage premiums; and lost industry-specific human capital rents. If workers are choosing jobs to maximize earnings, the pre-policy wage is the maximum attainable wage w^*_{pre} for public employees. Following displacement, workers engage in job search, selecting the next highest alternative wage, w^*_{post}. The gap between pre- and post-policy wages $w^*_{pre} - w^*_{post}$ provides an estimate of the extent to which public employee wages exceeded workers' opportunity cost of employment in WSLCB jobs. The decision by workers to accept employment following job displacement serves as a mechanism for revealing second-best wage offers facing public employees.

By examining linear combinations of treatment effects from various subsamples of workers who sorted into (1) private-sector retail liquor jobs, (2) government jobs, (3) union jobs and (4) non-union jobs following privatization, overall wage losses can be decomposed into separately identifiable wage rents. Let D be a binary indicator of treatment, T a binary indicator equal to one in the post-policy period and I the set of m post-policy industries into which displaced workers select for employment. Conceptually, estimated treatment effects can be expressed as a linear combination of the three lost wage rents,

$$\mathrm{E}(\beta_3 \mid D, T, I) = \begin{bmatrix} \mathbb{1}_1(\text{union}) & \mathbb{1}_1(\text{govt.}) & \mathbb{1}_1(\text{liquor}) \\ \vdots & \ddots & \vdots \\ \mathbb{1}_m(\text{union}) & \mathbb{1}_m(\text{govt.}) & \mathbb{1}_m(\text{liquor}) \end{bmatrix} \begin{bmatrix} w_U \\ w_G \\ w_F \end{bmatrix} = \begin{bmatrix} \hat{\beta}_3^1 \\ \vdots \\ \hat{\beta}_3^m \end{bmatrix} \tag{2.5}$$

where w_U is lost union wage rents, w_G is lost public sector wage rents, w_F is lost rents from industry-specific human capital, and the lefthand matrix is an $m \times 3$ array of zeros and ones reflecting which rents are present among wages in each of the m post-policy industries. By conditioning on the post-policy industry into which

[27] In the Appendix, I include a figure illustrating parallel pre-policy wage trends for the longer (but smaller NT) panel from 2005 to 2013 as well.

workers sort, I can use the m conditional estimates of $\hat{\beta}_3$ to separately identify wage rents. For example, the subsample of workers who remained employed in retail liquor following displacement retained industry-specific human capital rents w_F, but lost union and public sector wage rents w_U and w_G. Similarly, workers who moved into union-represented government jobs elsewhere retained union and public sector rents w_U and w_G, but lost rents due to industry-specific human capital w_F. Similarly, workers in non-union government jobs and private-sector union jobs can be used to separately identify union premiums w_U and public sector premiums w_G.[28]

Assessing Bias in Wage Rent Decompositions Due to Selection into Post-Policy Occupation

The above method for decomposing wage rents assumes that displaced workers are of homogeneous ability and their distribution among post-policy occupations is as good as random. Table 2.7 examines whether systematic self-selection of displaced workers into post-policy occupations poses a threat to the above decomposition approach. For example, before privatization it is possible that wages of public employees masked heterogeneity in ability among workers, as the wage structure was determined by collective bargaining agreement rather than individual negotiations. This heterogeneity could result in non-random selection of displaced workers into post-policy occupations. If high (or low) ability workers systematically sort into high (or low) wage occupations post-policy, the above decomposition method based on subsamples of workers in various occupations may be downward (or upward) biased.

Table 2.7 shows estimates from a multinomial logistic regression of indicators for each of the four occupations used for the rent decomposition (along with a fifth excluded category for all other occupations) on observed education, experience, experience squared, gender and race for the displaced workers who were employed post-policy. Following the usual practice for Mincerian wage equations,

[28] This approach is equivalent to specifying dummy indicators for post-policy occupation, and including interaction terms in my basic estimating equation (2.3) for *Post* x *Treatment* x *Occupation*. Doing so yields identical results.

I use reported age as a proxy for labor market experience. The first four columns correspond to the occupations used in the above rent decompositions. All of the estimated coefficients are statistical zeros, and the model explains less than 8 percent of post-policy selection into occupations. The three right-hand columns show results for likelihood ratio tests of joint significance for all of the coefficients for each observable. In all cases, the tests fail to reject the null of zero coefficients with p-values ranging from 0.164 to 0.779. Although this test does not preclude the presence of occupational selection based on unobservable characteristics, there is no evidence in the data that selection on observables is an important concern.

2.6 Results

2.6.1 Effect on Earnings

Table 2.8 shows panel difference-in-differences estimates of the effect of privatization on wages for all workers from 2010 to 2013. The coefficient of interest is β_3, the standard difference-in-differences estimator. Controls for gender and length of job tenure ($X_i'\Gamma$ from equation 2.3) are omitted as they have essentially no effect on the results, and estimates that include them are reserved for the Appendix. Standard errors are reported in parentheses, which are clustered at the individual level. The difference-in-differences estimate of the treatment effect is $\hat{\beta}_3 = -\$2.508$ per hour. The estimate is highly statistically significant, and represents a 17.2 percent loss in average wages for the treated workers. As expected, liquor privatization resulted in significant earnings losses among displaced public employees and resulted in sharply lower wages during the post-policy period.

Figure 2.3 shows quantile regression results for treatment effects throughout the conditional wage distribution. The colored line plots estimates of β_3 for quantiles ranging from the 5th to the 95th percentile, in 5-percent increments. The gray band plots the 95-percent confidence interval around these estimates. For comparison, the mean OLS treatment effect and confidence interval from Table 2.8 is shown as a horizontal dashed line in the figure. The impact of privatization varied widely throughout the wage distribution, with the most severe wage losses occurring in the lowest quintiles. All quintiles below the 60th percentile suffered larger wage losses than the mean, with losses of \$4.62 per hour or 32 percent for the most heavily affected workers at the 5th percentile. By contrast, wages at the 80th, 90th and 95th percentiles were essentially unaffected by the privatization. This is suggestive that low-wage workers were disproportionately adversely affected by the privatization, and that mean effects mask considerable heterogeneity among workers.

2.6.2 Effects in Subsamples

Effects by Gender

Tables 2.9 and 2.10 show results separately for male and female employees. Previous research has found female public employees tend to exhibit somewhat larger public sector wage rents than men (Gregory and Borland 1999). Table 2.9 presents results for men only, consisting of 63 treated individuals and 35 control individuals observed over 4 periods, for panel of size $NT = 392$. The estimated treatment effect for men is $\hat{\beta}_3 = -\$2.200$ per hour, a 15.1 percent reduction in average hourly wages. The estimate is roughly 31 cents per hour smaller than for all workers, although the two figures are not statistically different.[29] Figure 2.4 presents quantile regression results for male workers. The size of treatment effects shows considerably more heterogeneity than in the full sample, with sharply different outcomes for workers at the tails of the distribution. The largest negative effects were concentrated in the lowest quantiles, with males at the 5th percentile experiencing treatment effects of -\$4.225 per hour or a 29 percent drop in wages. However, male workers above the 65th percentile had a statistically zero treatment effect on wages from displacement.

Table 2.10 shows results for female workers only. The sample consists of 80 treated individuals and 84 control workers observed over 4 years, for a panel of size $NT = 656$ observations. The mean treatment effect for female workers is

[29]The pairwise test statistic comparing treatment effects for all workers to male workers ($\hat{\beta_3^A} = \hat{\beta_3^M}$) is $z_{A,M} = -0.75$. Comparing all workers to female workers ($\hat{\beta_3^A} = \hat{\beta_3^F}$), $z_{A,F} = 1.09$. And comparing male workers to female workers ($\hat{\beta_3^M} = \hat{\beta_3^F}$), $z_{M,F} = 0.91$.

$\hat{\beta}_3 = -\$2.871$ per hour, a 19.7 percent reduction in average wages. The effect of displacement on females is 36 cents larger than for all workers, and 67 cents larger than for male workers, suggesting women's earnings were disproportionately affected by job displacement. This is consistent with the presence of somewhat larger public sector wage premiums among female employees reported in past literature, although the difference between male and female treatment effects is not statistically significant. Figure 2.5 shows quantile regression results for female workers. Unlike males, treatment effects are more homogeneous and uniformly negative throughout the conditional wage distribution. The lowest quantiles suffered larger wage losses than the upper quantiles, but treatment effects were negative and significant for all quantiles examined. Women in the 5th percentile experienced wage declines of $5.007 per hour, a 34 percent average decline, while those in the 95th percentile experienced losses of $1.586 per hour, an 11 percent decline.

Effects by Occupation, Age, Race and Education

Table 2.11 shows treatment effects for a variety of worker subsamples. Column (1) repeats the overall treatment effect for all workers as a comparison. Columns (2) to (10) show treatment effects for workers who found employment in a variety of industries during the post-policy period: those who remained in liquor retailing, those who were employed in other government agencies, those who worked in union-represented jobs and so on. These subsample treatment effects serve as the basis for the decomposition of wage rents in the following section. Treatment effects varied widely by post-policy industry, from an insignificant -$0.223 per hour for those in non-union government jobs to a highly significant -$4.433 per hour for those in non-liquor-related retail jobs.

Columns (11) to (15) show treatment effects for young, middle-age and older workers, as well as for white and non-white employees. The point estimate for younger workers aged 18 to 34 years of -$3.576 per hour is more than one dollar per hour larger than for middle aged or older workers, although it is not statistically different. White and non-white workers suffered similar wage losses from the policy, with slightly larger losses of $2.568 per hour for white employees compared with \$2.211 per hour for non-white workers. Columns (16) to (20) show treatment effects by level of education. Wage losses from the policy were monotonically decreasing in years of education, ranging from \$3.894 per hour for those with a high-school diploma or less, to \$1.098 per hour for those with some graduate school or above. As with many labor market outcomes, the severity of the effect of job displacement on wages is strongly correlated with workers' prior educational attainment.

2.6.3 Results for Wage Rent Decomposition

Table 2.12 presents the four subsamples from Table 2.11 used for the wage rent decomposition. Column (2) contains workers who remained in liquor retailing jobs (industry r_l); Column (7) contains workers who moved into union-represented government jobs (industry g_u); Column (8) contains workers who moved into non-union-represented government jobs (industry g_n); and Column (10) contains workers who moved into union-represented, non-government jobs (industry u_n). For each subsample, the table shows treatment effects and the type of wage rent(s) identified. Using these four subsamples there are ${}_4C_3 = (4!)/(3!(4-3)!) = 4$ ways to decompose wage rents as the solution to a system of three equations in three unknowns. In order of reliability from largest to smallest sample size, Method 1 uses Columns (2), (7) and (10) to decompose wage rents based on labor market information from 73 treated individuals as follows:

$$\begin{array}{rrcrl}
\text{Column (2):} & E(\beta_3 \mid D, T, I = r_l) & = & -2.054 & = E(\;w_U + w_G\;) \\
\text{Column (7):} & E(\beta_3 \mid D, T, I = g_u) & = & -0.300 & = E(\;w_F\;) \\
\text{Column (10):} & E(\beta_3 \mid D, T, I = u_n) & = & -2.319 & = E(\;w_G + w_F\;)
\end{array}$$

Method 2 uses treatment effects from Columns (2), (8) and (10) for 68 treated individuals:

$$\begin{array}{rrcrl}
\text{Column (2):} & E(\beta_3 \mid D, T, I = r_l) & = & -2.054 & = E(\;w_U + w_G\;) \\
\text{Column (8):} & E(\beta_3 \mid D, T, I = g_n) & = & -0.233 & = E(\;w_U + w_F\;) \\
\text{Column (10):} & E(\beta_3 \mid D, T, I = u_n) & = & -2.319 & = E(\;w_G + w_F\;)
\end{array}$$

Method 3 uses estimates from Columns (2), (7) and (8) for 58 treated individuals:

$$
\begin{array}{llclcl}
\text{Column (2):} & E(\beta_3 \mid D, T, I = r_l) & = & -2.054 & = E(& w_U \;+\; w_G \qquad\quad) \\
\text{Column (7):} & E(\beta_3 \mid D, T, I = g_u) & = & -0.300 & = E(& \qquad\qquad\qquad\quad w_F \;\;) \\
\text{Column (8):} & E(\beta_3 \mid D, T, I = g_n) & = & -0.223 & = E(& w_U \qquad\qquad + \;\; w_F \;\;)
\end{array}
$$

Finally, Method 4 uses treatment effects from Columns (7), (8) and (10) for 29 treated individuals:

$$
\begin{array}{llclcl}
\text{Column (7):} & E(\beta_3 \mid D, T, I = g_u) & = & -0.300 & = E(& \qquad\qquad\qquad\quad w_F \;\;) \\
\text{Column (8):} & E(\beta_3 \mid D, T, I = g_n) & = & -0.233 & = E(& w_U \qquad\qquad + \;\; w_F \;\;) \\
\text{Column (10):} & E(\beta_3 \mid D, T, I = u_n) & = & -2.319 & = E(& \qquad\quad w_G \;+\; w_F \;\;)
\end{array}
$$

Table 2.13 shows the results of the decomposition. The rows correspond to the four decomposition approaches described above, presenting separate estimates for union wage premiums w_U, public sector wage premiums w_G and a residual wage premium attributable to lost industry-specific human capital w_F. Under all four approaches, the sum of lost wage rents is consistent with the overall treatment effect above, ranging from -\$2.24 per hour to -\$2.35 per hour, compared with the overall treatment effect of -\$2.508 per hour. Wage losses attributable to public sector rents are by far the largest, accounting for -\$2.02 to -\$2.13 per hour or roughly 86 to 91 percent of total wage losses following privatization. Industry-specific human capital accounts for the second largest component, ranging from -\$0.24 to -\$0.30 per hour or roughly 11 to 13 percent of the total. Union premiums were negligible in all four decompositions, accounting for just -\$0.03 per hour of wage losses using the first approach and a slightly negative union premium of between 2 and 8 cents in the remaining three approaches. The results are consistent with the presence of a roughly 16 percent public sector wage premium among the displaced WSLCB workers.

2.7 Conclusion

The issue of public employee compensation has long been controversial. Despite a well-established literature finding public sector wage premiums among federal workers, the evidence for the roughly 5.3 million state government employees currently employed in the United States remains mixed. This study contributes to the literature by providing new estimates of public sector wage rents for state employees based on quasi-experimental evidence from a 2012 privatization of liquor retailing in Washington State.

Based on a panel difference-in-differences estimator, I find that wages of state employees displaced by privatization fell by roughly \$2.508 per hour or 17.2 percent relative to a similar group of public employees unaffected by the policy, with somewhat larger effects for female workers. By decomposing this overall effect into public sector wage rents, union premiums and losses due to industry-specific capital I find evidence of a roughly 16 percent public sector wage premium. The results are unaffected by the inclusion of controls for gender and length of job tenure; by excluding workers who reported a higher value of non-wage "fringe" benefits in post-policy jobs; and when estimated via a propensity score kernel matching estimator.

The finding of a roughly 16 percent public sector wage premium is considerably larger than estimates reported by previous longitudinal studies that do not rely on exogenous job separations, suggesting endogenous job switching may be a source of significant downward bias in these estimates. However, the findings are broadly consistent with other studies that have examined earnings of government workers following privatization-related displacements in Brazil, Argentina and elsewhere. Although the estimated wage premium for liquor retail workers may not easily generalize to broader categories of state workers, it may be informative to other U.S. states considering privatization of liquor retailing.

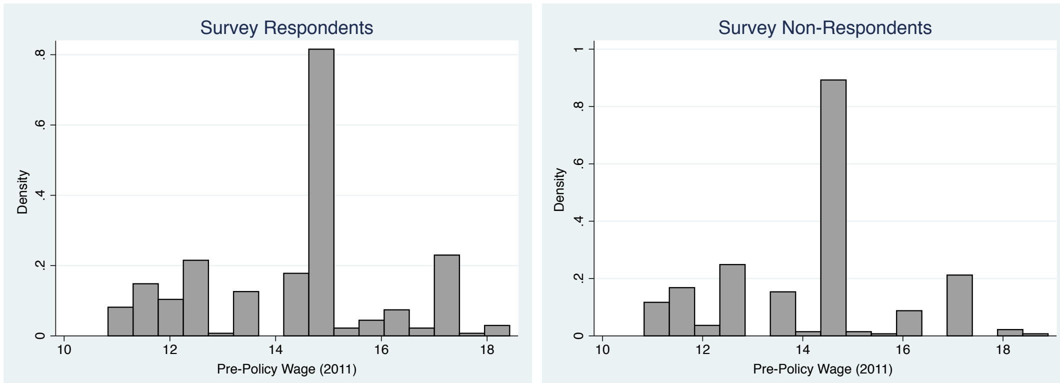

Figure 2.1: Distributions of Pre-Policy Wages for Survey Respondents (Left Panel) and Non-Respondents (Right Panel)

Note: Histograms are based on pre-policy 2011 wages for 286 survey respondents (left panel) and 271 non-respondents (right panel) for whom wages are observed in all years from 2010 to 2012. Pre-policy wages are from administrative records provided by the WA OFM.

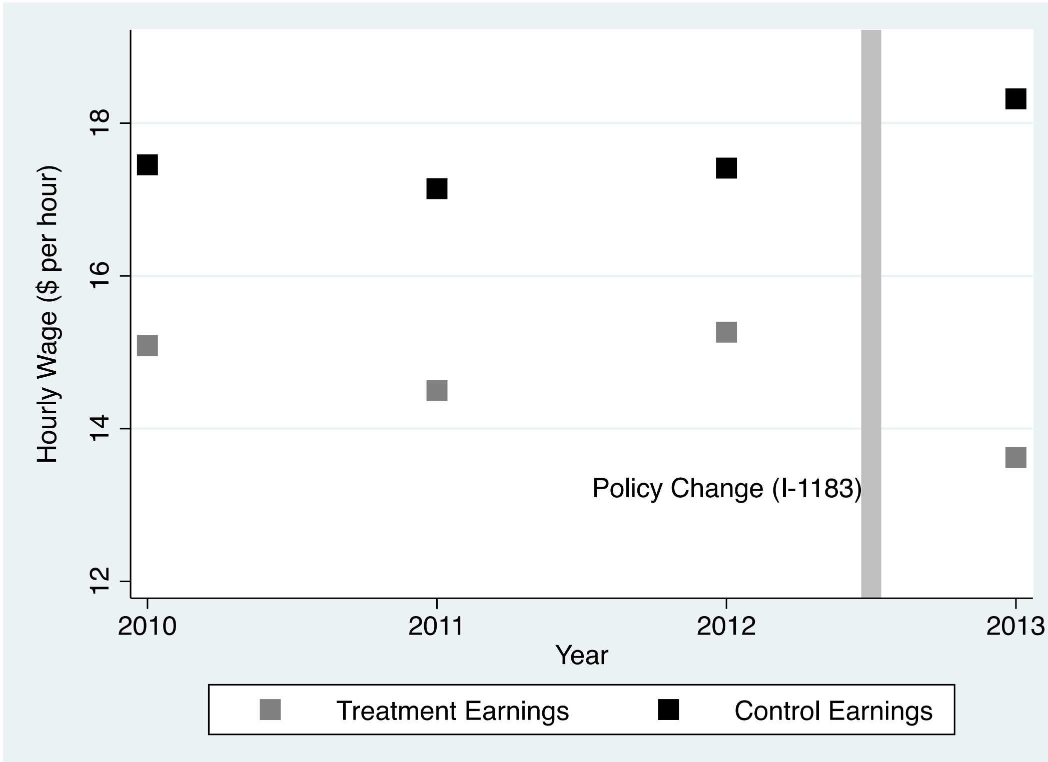

Figure 2.2: Evolution of Pre-Policy Wages for the Treatment and Control Groups, 2010-2013

Note: Figure shows mean hourly wages for the balanced panel of workers observed in all years from 2010-2013. Sample consists of 143 treated and 119 control individuals, for panel of size $N = 262$, $T = 4$. Pre-policy wages are observed in January of each year, and are based on administrative records from the WA OFM. The policy went into effect June 1, 2012. *Source*: Author's survey of displaced WSLCB workers; Washington State OFM.

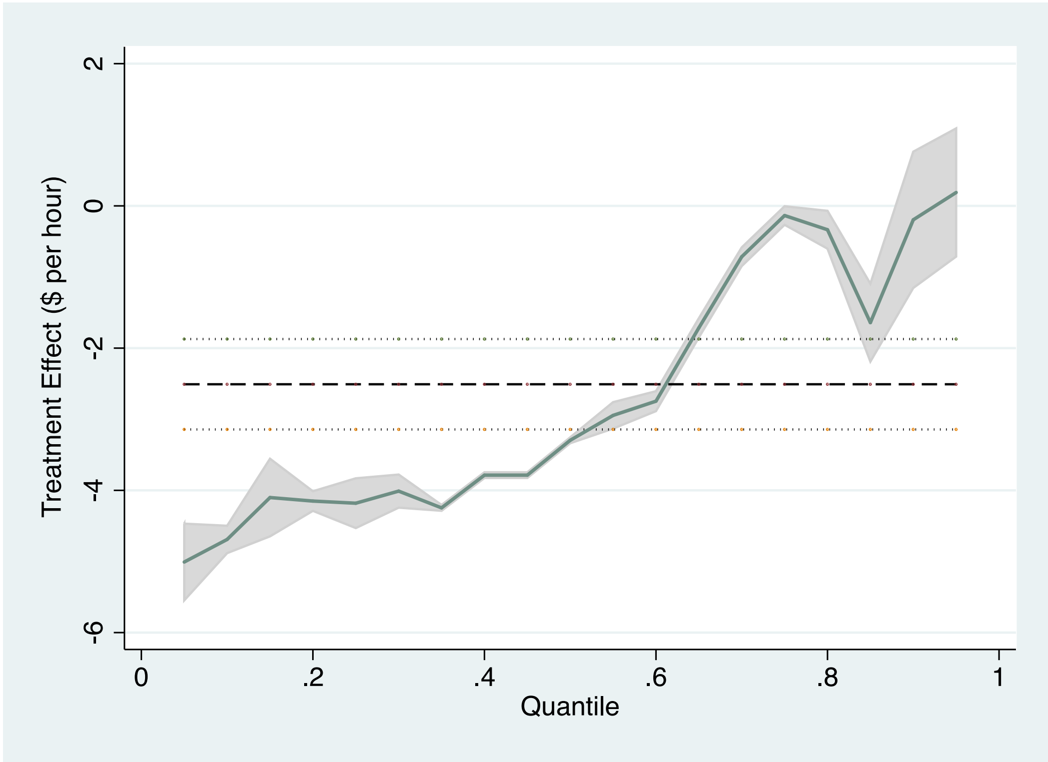

Figure 2.3: Quantile Treatment Effects for All Workers, 2010-13

Note: Quantile regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction for all workers. Effects are estimated at the 5th through 95th quantiles at 5 percent increments. Gray band illustrates 95 percent confidence intervals; dashed line is the OLS mean effect.

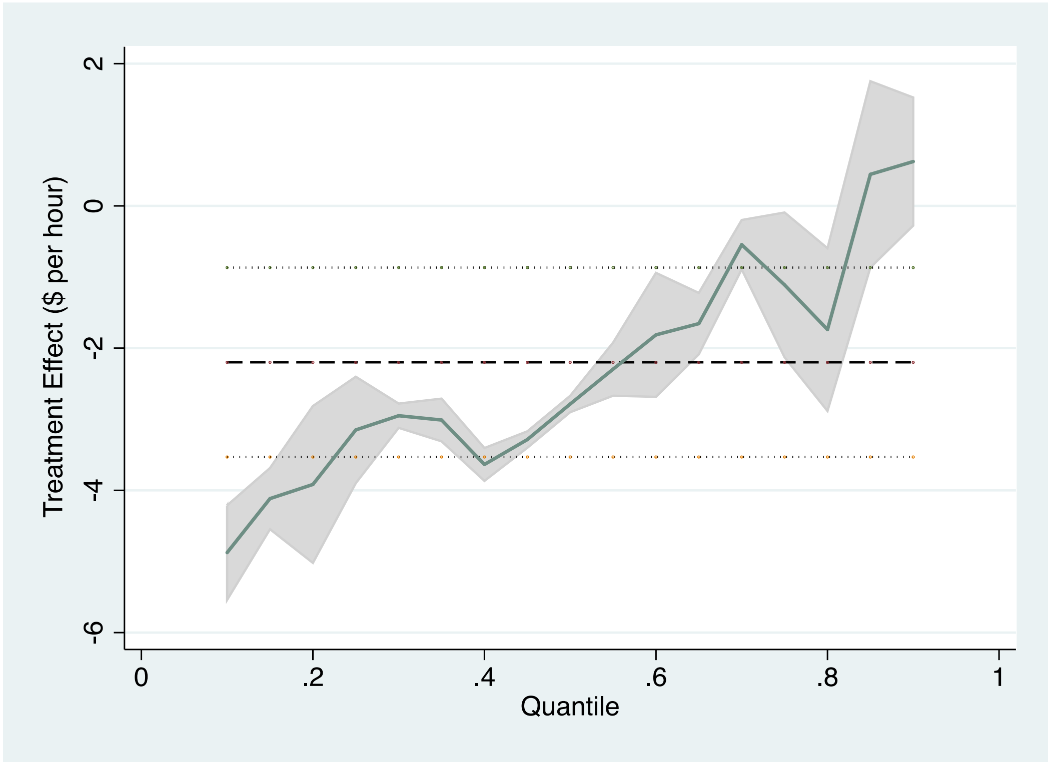

Figure 2.4: Quantile Treatment Effects for Male Workers, 2010-13

Note: Quantile regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction for male workers only. Effects are estimated at the 5th through 95th quantiles at 5 percent increments. Gray band illustrates 95 percent confidence intervals; dashed line is the OLS mean effect.

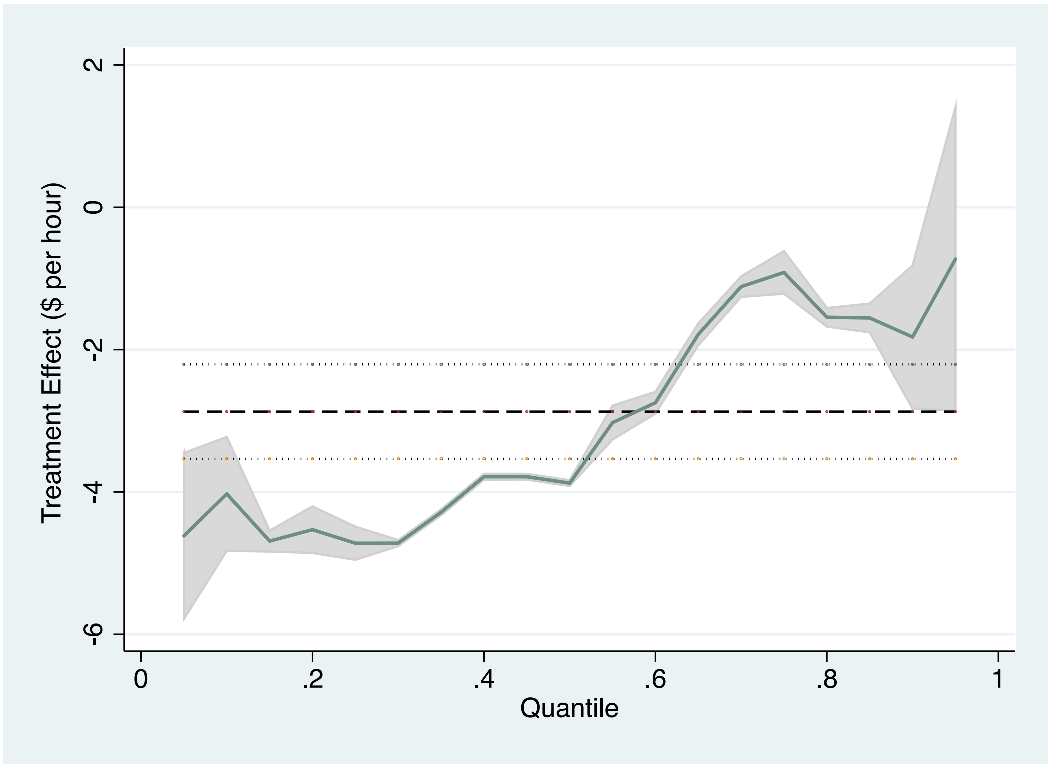

Figure 2.5: Quantile Treatment Effects for Female Workers, 2010-13

Note: Quantile regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction for all workers. Effects are estimated at the 5th through 95th quantiles at 5 percent increments. Gray band illustrates 95 percent confidence intervals; dashed line is the OLS mean effect.

Table 2.1: Descriptive Statistics for All Survey Respondents (1 of 2)

	Number	Freq.		Number	Freq.
Population	912		Separation Reason		
Responses	404		Quit Voluntarily	37	9.2
Response Rate		44.3	Fired from job	367	90.8
Response Type			Education		
Online	199	49.3	Less than high school	1	0.2
Mail	205	50.7	Some high school	3	0.7
			High school diploma	111	27.5
Gender			Some college	161	39.9
Male	170	42.1	Associate's degree (A.A.)	64	15.8
Female	234	57.9	College degree (B.A.)	47	11.6
			Some grad school	9	2.2
Age			Graduate degree	7	1.7
20-24 years	5	1.2	n.a.	1	0.2
25-29 years	15	3.7			
30-34 years	22	5.4	Adults in HH		
35-39 years	30	7.4	1	91	22.5
40-44 years	42	10.4	2	240	59.4
45-49 years	40	9.9	3	43	10.6
50-54 years	66	16.3	4	25	6.2
55-59 years	73	18.1	5+	5	1.2
60-64 years	69	17.1			
65+ years	42	10.4	Children in HH		
			0	280	69.3
Race			1	75	18.6
White	337	83.4	2	33	8.2
Black	17	4.2	3	14	3.5
Asian	15	3.7	4	1	0.2
Mixed race	15	3.7	5+	1	0.2
Hispanic	14	3.5			
Native Amer.	3	0.7	Employment Status		
Other race	3	0.7	Employed full time	102	25.2
			Employed part time	117	29.0
Marital Status			Self-employed	8	2.0
Single, never married	63	15.6	Not employed, looking	121	30.0
Living w/ partner, single	44	10.9	Not employed, not looking	6	1.5
Married	199	49.3	Homemaker	4	1.0
Separated	5	1.2	Student	12	3.0
Divorced	83	20.5	Retired	34	8.4
Widowed	10	2.5			

Notes: Survey of the displaced WSLCB workers was conducted online and via mail between July 12 and September 10, 2013. Based on $N = 404$ survey respondents. A complete copy of the survey recruitment letter and questionnaire is provided in the Appendix.

Table 2.2: Descriptive Statistics for Survey Respondents (2 of 2)

	Number	Freq.		Number	Freq.
Job Tenure			Earnings (Employed)		
Less than 1 year	60	14.9	Pre-Policy		
1-2 years	59	14.6	Mean	14.35	
3-4 years	83	20.5	St. Dev.	2.57	
5-6 years	51	12.6			
7-10 years	49	12.1	Post-Policy		
More than 10 years	102	25.2	Mean	13.33	
			St. Dev.	4.93	
Industry (Employed)					
Administrative	19	8.4	Fringe Benefits		
Agriculture and fishing	2	0.9	Health Insurance	100	44.1
Auto Repair	1	0.4	Dental Insurance	92	40.5
Construction	5	2.2	Paid Vacation	100	44.1
Custodial	1	0.4	Retirement (401(k))	78	34.4
Education	13	5.7	Childcare	3	1.3
Entertainment	1	0.4	Transportation	19	8.4
Finance or real estate	8	3.5	None	108	47.6
General labor - car wash	1	0.4			
Government	20	8.8	Value of Fringe Benefits		
Health	4	1.8	Less than Pre-Policy	168	74.0
Manufacturing	3	1.3	Same as Pre-Policy	39	17.2
Media	1	0.4	Greater than Pre-Policy	19	8.4
Personal services - daycare	1	0.4	n.a.	1	0.4
Restaurant or hotel	11	4.8			
Retail liquor, new location	30	13.2	Union Representation		
Retail liquor, same location	31	13.7	Union Member	43	18.9
Retail, non-liquor	50	22.0	Non-Union	184	81.1
Sales and marketing	5	2.2			
Security services	3	1.3	Moved to New Location		
Social services	1	0.4	Same Address	352	87.1
Technology or software	1	0.4	New Address	52	12.9
Transportation or warehouse	10	4.4			
Wholesale	4	1.8			
n.a.	1	0.4			

Notes: Survey of the displaced WSLCB workers was conducted online and via mail between July 12 and September 10, 2013. Based on $N = 404$ survey respondents. Figures reported for pre- and post-policy earnings are mean hourly wages for the subset of $n = 227$ displaced workers who reported being employed in 2013. A complete copy of the survey recruitment letter and questionnaire is provided in the Appendix.

Table 2.3: Assessing Selection into Unemployment: Regression of Probability of Post-Policy Unemployment on Observed Worker Characteristics

Variable	(1)	(2)	(3)	(4)	(5)
Pre-Policy Wage (2012)	0.009	0.007	0.002	0.001	0.005
	(0.008)	(0.008)	(0.009)	(0.010)	(0.010)
Pre-Policy Hours (2012)		0.004	0.003	0.003	0.004
		(0.003)	(0.003)	(0.003)	(0.004)
Job Tenure			0.016	0.015	0.009
			(0.015)	(0.015)	(0.016)
Female				0.061	0.068
				(0.046)	(0.050)
Age					0.012
					(0.012)
Moved to New Address					0.031
					(0.073)
Black / African American					-0.020
					(0.115)
Hispanic					0.201
					(0.136)
Education					0.061
					(0.254)
Married					-0.061
					(0.111)
Adults in Household					-0.013
					(0.020)
n					-0.110
R-Squared					(0.082)

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels. *Note*: Regression is of a binary indicator for unemployment during the post-policy period on observed pre-policy worker characteristics in the full sample of $N = 404$ survey respondents. Heteroskedasticity-robust standard errors are reported in parentheses.

Table 2.4: Summary Statistics for the Treated Group ($N = 143$; $T = 4$)

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Year	143	n.a.	n.a.	2010	2013
2010 Hourly Wage	143	15.12	2.63	11.35	40.36
2011 Hourly Wage	143	14.48	2.70	10.83	39.15
2012 Hourly Wage	143	15.30	1.57	11.50	20.00
2013 Hourly Wage	143	13.44	4.10	6.54	38.46
Job Tenure	143	4.11	1.37	2	6
Female	143	0.56	0.5	0	1

Note: Treatment group consists of the $N = 143$ involuntarily displaced WSLCB workers for whom wages are observed in all years, 2010 to 2013. Wages for 2010 to 2012 are from administrative records provided by the WA OFM. Wages for 2013, gender, and job tenure are from an original survey administered by the author. Job tenure codes correspond to the six chronological categories reported in the bottom-right section of Table 2.1.

Table 2.5: Assessing Bias from Survey Non-Response: Regression of Probability of Response on Observed Population Characteristics

	(1)	(2)	(3)	(4)	(5)
Pre-Policy Wage (2011)	0.015*	0.009	0.021	0.023	0.022
	(0.008)	(0.011)	(0.014)	(0.014)	(0.014)
Pre-Policy Wage (2010)		0.013	0.002	0.001	0.000
		(0.012)	(0.015)	(0.015)	(0.015)
Pre-Policy Hours (2011)			-0.005	-0.006	-0.006
			(0.004)	(0.005)	(0.005)
Pre-Policy Hours (2010)			0.005	0.006	0.005
			(0.005)	(0.005)	(0.005)
Female				0.037	0.041
				(0.043)	(0.043)
Mover (New Post-Policy Address)					-0.173***
					(0.064)
n	557	557	557	557	557
Adjusted R-squared	0.003	0.003	0.001	0.001	0.012
F-statistic	3.337	2.717	2.159	1.843	2.717

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Note: Regression is of a binary indicator of survey response on observed pre-policy characteristics of individuals in the population of displaced state workers. Pre-policy wage and hours information is from administrative records provided by the WA OFM. Mover status is determined by comparing pre-policy mailing addresses to a post-policy USPS-validated address list. The sample consists of 286 survey respondents and 271 non-respondents for whom wages are observed in all pre-policy years from 2010 through 2012. Heteroskedasticity-robust standard errors are reported in parentheses.

Table 2.6: Summary Statistics for the Control Group ($N = 119$; $T = 4$)

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Year	119	n.a.	n.a.	2010	2013
2010 Hourly Wage	119	17.45	1.11	14.00	21.50
2011 Hourly Wage	119	17.14	0.93	14.23	20.85
2012 Hourly Wage	119	17.41	1.03	14.54	20.85
2013 Hourly Wage	119	18.32	1.13	15.72	21.50
Job Tenure	119	4.50	0.70	3	5
Female	119	0.71	0.46	0	1

Note: Control group consists of the $N = 119$ Washington State Department of Licensing (WA DOL) "Customer Service Specialist" workers for whom wages are observed in all years, 2010 to 2013. Wages, gender, and job tenure are from administrative records provided by the WA OFM. Job tenure codes correspond to the six chronological categories reported bottom-right section of Table 2.1.

Table 2.7: Testing for Post-Policy Occupational Selection: Multinomial Logistic Regression of Post-Policy Industry on Observable Characteristics of Treated Workers

Variable	(2) Retail Liquor	(7) Government Union	(8) Government Non-Union	(10) Union Non-Government	Likelihood Ratio Test (H_0: All Coefficients = 0) Chi-Squared	d.f.	p-value
Education	-0.212	0.251	0.409	0.116	5.459	4	0.243
	(0.155)	(0.249)	(0.385)	(0.180)			
Experience	-0.446	1.215	-1.186	-0.145	4.244	4	0.374
	(0.479)	(0.964)	(1.116)	(0.639)			
Experience Squared	0.051	-0.112	0.076	0.018	6.51	4	0.164
	(0.033)	(0.075)	(0.085)	(0.044)			
Female	0.159	0.142	1.594	-0.525	3.895	4	0.420
	(0.363)	(0.612)	(1.262)	(0.476)			
Black / African American	0.089	-0.01	-13.449	-13.637	4.434	4	0.350
	(0.737)	(1.139)	(971.405)	(661.441)			
n	222						
Pseudo R-squared	0.077						
Likelihood Ratio Test (Chi-squared)	41.651						
p-value (LR Test)	0.047						

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Multinomial logistic regression of dummy indicators for post-policy occupation on observable worker characteristics. Columns (2), (7), (8) and (10) correspond to industries from Table 2.13 used for the wage rent decomposition. The excluded category is a dummy indicator for all other industries. Homoskedastic standard errors are reported in parentheses.

Table 2.8: Panel Difference-in-Differences Estimates of the Treatment Effect of Privatization on Wages, All Workers, 2010-2013

Observations	Control	Treatment	Total
Pre-Policy	357	429	786
Post-Policy	119	143	262
Total	476	572	1048
R-squared	0.334		
Mean Hourly Wages	Control	Treatment	Difference
Pre-Policy	17.34	14.96	-2.371***
	(0.088)	(0.158)	(0.181)
Post-Policy	18.32	13.44	-4.879***
	(0.104)	(0.343)	(0.358)
		Difference-in-Differences ($\hat{\beta}_3$)	-2.508***
		Standard Error	(0.396)
		Effect / Treatment Mean	-17.2%

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Note: Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level.

Table 2.9: Male Workers Only: Panel Difference-in-Differences Estimates of the Treatment Effect of Privatization on Wages, 2010-2013

Observations	Control	Treatment	Total
Pre-Policy	105	189	294
Post-Policy	35	63	98
Total	140	252	392
R-squared	0.244		
Mean Hourly Wages	Control	Treatment	Difference
Pre-Policy	17.41	14.82	-2.590***
	(0.188)	(0.309)	(0.362)
Post-Policy	18.72	13.93	-4.789***
	(0.250)	(0.520)	(0.577)
		Difference-in-Differences ($\hat{\beta}_3$)	-2.200***
		Standard Error	(0.643)
		Effect / Treatment Mean	-15.1%

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Note: Regression is of hourly wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of involuntarily displaced male workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level.

Table 2.10: Female Workers Only: Panel Difference-in-Differences Estimates of the Treatment Effect of Privatization on Wages, 2010-2013

Observations	Control	Treatment	Total
Pre-Policy	252	240	492
Post-Policy	84	80	164
Total	336	320	656
R-squared	0.428		
Mean Hourly Wages	Control	Treatment	Difference
Pre-Policy	17.30	15.07	-2.229***
	(0.098)	(0.144)	(0.174)
Post-Policy	18.15	13.05	-5.100***
	(0.099)	(0.454)	(0.465)
		Difference-in-Differences ($\hat{\beta}_3$)	-2.871***
		Standard Error	(0.496)
		Effect / Treatment Mean	-19.7%

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Note: Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of involuntarily displaced female workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level.

Table 2.11: Treatment Effect of Job Displacement on Wages in Various Subsamples, 2010-2013

	(1) Full Sample	(2) Retail Liquor (Any)	(3) Retail Liquor, Same Store	(4) Retail Liquor, Different Store	(5) Retail Non-Liquor
Treatment Effect ($\hat{\beta}_3$)	-2.508***	-2.054***	-2.019**	-2.090***	-4.433***
	(0.396)	(0.562)	(0.979)	(0.518)	(0.565)
n	1048	664	572	568	616
Adjusted R-squared	0.102	0.105	0.099	0.303	0.470

	(6) Any Government	(7) Government Union	(8) Government Non-Union	(9) Any Union	(10) Union Non-Govt.
Treatment Effect ($\hat{\beta}_3$)	-0.279	-0.300	-0.223	-1.698**	-2.319**
	(1.037)	(1.148)	(2.256)	(0.794)	(0.992)
n	520	508	488	580	548
Adjusted R-squared	0.320	0.357	0.444	0.183	0.248

	(11) Young	(12) Middle Age	(13) Older	(14) White	(15) Non-White
Treatment Effect ($\hat{\beta}_3$)	-3.576***	-2.387***	-2.524***	-2.568***	-2.211***
	(0.890)	(0.519)	(0.578)	(0.455)	(0.670)
n	520	880	600	952	572
Adjusted R-squared	0.455	0.082	0.278	0.100	0.263

	(16) High School	(17) Some College	(18) AA	(19) BA	(20) Grad School
Treatment Effect ($\hat{\beta}_3$)	-3.894***	-2.204***	-1.547*	-1.400	-1.098*
	(0.666)	(0.684)	(0.858)	(1.272)	(0.650)
n	652	680	568	548	488
Adjusted R-squared	0.226	0.125	0.195	0.153	0.474

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for various subsamples of the balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level.

Table 2.12: Subsample Treatment Effects Used to Decompose Wage Rents

	(2) Retail Liquor ($I = r_l$)	(7) Government Union ($I = g_u$)	(8) Government Non-Union ($I = g_n$)	(10) Union Non-Government ($I = u_n$)
Treatment Effect ($\hat{\beta}_3$)	-2.054***	-0.300	-0.223	-2.319**
	(0.562)	(1.148)	(2.256)	(0.992)
Lost Wage Rents:				
Union (w_U)	X		X	
Public Sector (w_G)	X			X
Industry-Specific Capital (w_F)		X	X	X
n	664	508	488	548

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Subsample treatment effects are repeated from columns of Table 2.11. Standard errors are presented in parentheses and clustered at the individual level.

Table 2.13: Estimated Decomposition of Wage Rents Earned by Former WSLCB Employees

		Lost Wage Rents			
	Treated Group Size	Union (w_U)	Public Sector (w_G)	Industry-Specific Capital (w_F)	Total
Method 1: (2), (7) and (10)	73	-0.03	-2.02	-0.30	-2.35*
		(1.577)	(1.507)	(1.142)	(1.274)
Method 2: (2), (8) and (10)	68	0.02	-2.08	-0.24	-2.30*
		(1.246)	(1.266)	(1.246)	(1.269)
Method 3: (2), (7) and (8)	58	0.08	-2.13	-0.30	-2.35*
		(2.515)	(2.577)	(1.142)	(1.274)
Method 4: (7), (8) and (10)	29	0.08	-2.02	-0.30	-2.24
		(2.515)	(1.507)	(1.142)	(2.702)

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Decomposition of wage rents based on linear combinations of estimates from the noted subsample models from Table 2.11. Standard errors reported in parentheses are clustered at the individual level, and calculated as $\text{Var}[\sum_{i=1}^{n} \beta_i] = \sum_{i=1}^{n} \sum_{j=1}^{n} \text{Cov}[\beta_i, \beta_j]$ for the linear combination of n subsample coefficients.

2.8 Appendix

2.8.1 Pre-Policy Wage Trends, 2005-2013

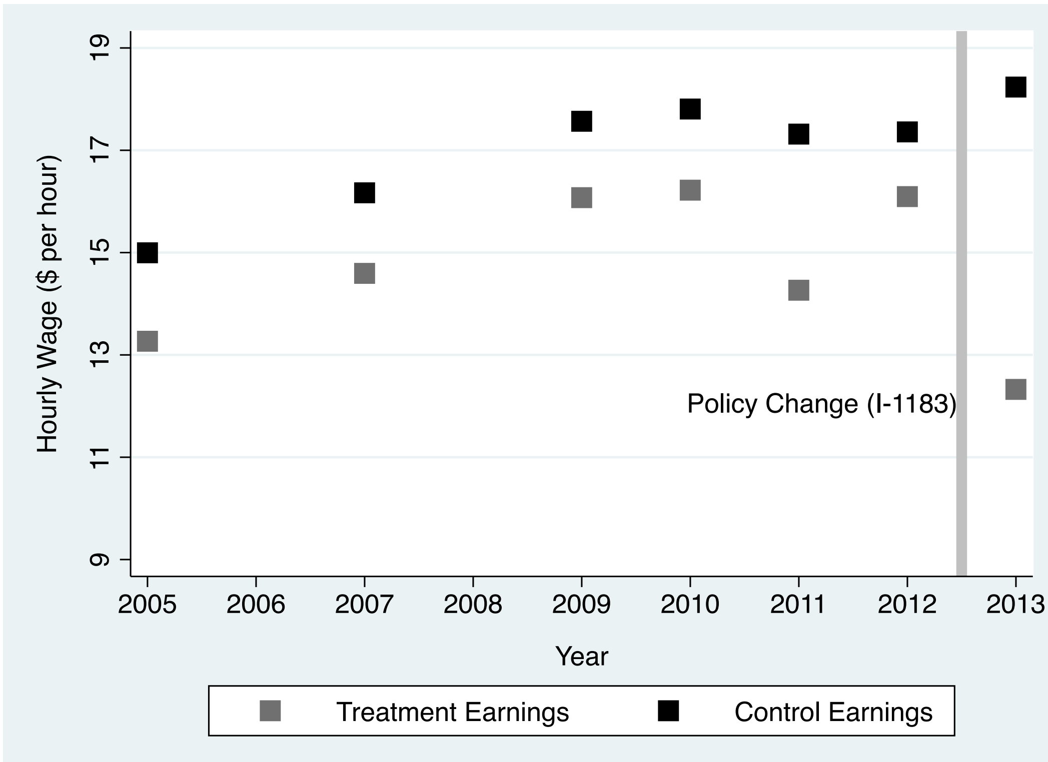

Figure 2.6: Evolution of Pre-Policy Wages for Treated and Control Workers, 2005-2013

Note: Figure shows mean hourly wages for the longer (but smaller NT) balanced panel of all involuntarily displaced workers for whom wages are observed in all years from 2005-2013. Sample consists of 43 treated and 73 control individuals, for panel of size $N = 116$, $T = 7$. Pre-policy wages are observed in January of each year, and are from administrative records provided by the WA OFM. The policy went into effect June 1, 2012. *Source*: Author's survey of displaced WSLCB workers; Washington State OFM.

2.8.2 Results Including Worker Covariates

Table 2.14 shows treatment effects of displacement on wages including individual-level covariates for gender and job tenure, which are omitted from the baseline estimates presented in the paper. The figures correspond directly to my estimating equation (2.4) and are presented separately for all workers, males, and females. The additional covariates are shown in the first two rows. In the regression for all workers in Column (1), gender and length of job tenure are statistically insignificant. Job tenure is statistically significant only in the model restricted to female workers, in which case it has a small effect of \$0.17 per hour. Overall, the point estimates for treatment effects $\hat{\beta}_3$ are identical to those that omit individual covariates presented in the paper.

Table 2.14: Panel Difference-in-Differences Estimate of Treatment Effect, Including Gender and Job Tenure Covariates, 2010-2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Female	-0.172		
	(0.184)		
Job Tenure	0.157	0.126	0.173*
	(0.105)	(0.215)	(0.100)
Post Policy	0.983***	1.307***	0.848***
	(0.062)	(0.149)	(0.055)
Treatment	-2.336***	-2.504***	-2.205***
	(0.163)	(0.308)	(0.170)
Post Policy x Treatment ($\hat{\beta}_3$)	-2.508***	-2.200***	-2.871***
	(0.396)	(0.644)	(0.496)
Constant	16.751***	16.851***	16.523***
	(0.529)	(0.991)	(0.479)
n	1048	392	656
Adjusted R-squared	0.335	0.238	0.430

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.

Note: Regression is of hourly worker wages on binary dummies for treatment, post-policy period, their interaction, and individual-level controls for gender and length of job tenure. Estimation is for the balanced panel of all workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level.

2.8.3 Results via Kernel Matching Estimator

Table 2.16 shows estimated treatment effects of displacement on wages using a propensity score kernel matching difference-in-differences estimator. Table 2.15 shows the results of the first stage of the procedure in which individual characteristics are used to estimate treatment likelihoods. In the second stage, the resulting propensity scores are used to kernel match treated individuals to a composite group of control group members based on the epanechnikov kernel with a default bandwidth of 0.06. Overall, the procedure results in somewhat larger estimated treatment effects of -$2.790 per hour for all workers, and -$2.425 and -$2.926 per hour for males and females, respectively. However, none of the results are statistically different from the OLS difference-in-differences estimates presented in the paper.

Table 2.15: First Stage Propensity Score Estimation: Estimated Probability of Treatment Conditional on Observables, 2010-2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Job Tenure	-0.173***	-0.339***	-0.074
	0.042	(0.070)	0.053
Gender	-0.335***		
	0.096		
Constant	1.410***	1.770***	0.300
	0.226	(0.304)	0.242
n	1048	392	656
Pseudo R-squared	0.03	0.06	0.01

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Regression of the probability of treatment on individual characteristics for gender and length of job tenure. Sample is a balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors in parentheses are clustered at the individual level.

Table 2.16: Second Stage: Kernel Matching Difference-in-Differences Estimates of the Treatment Effect, 2010-2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Post Policy	1.265***	1.532***	0.903***
	(0.160)	(0.267)	(0.068)
Treatment	-2.070***	-2.072***	-2.153***
	(0.252)	(0.447)	(0.187)
Post Policy x Treatment ($\hat{\beta}_3$)	-2.790***	-2.425***	-2.926***
	(0.422)	(0.680)	(0.498)
Constant	17.034***	16.897***	17.226***
	(0.196)	(0.323)	(0.119)
n	1048	392	656
Adjusted R-squared	0.299	0.207	0.409

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Regression uses a propensity score matched kernel difference-in-differences estimator to estimate the effect of treatment on hourly worker wages. Treated workers are kernel matched to members of the control group via the epanechnikov kernel using a default bandwidth of 0.06. Estimation is for the balanced panel of all involuntarily displaced workers observed between 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level.

2.8.4 Results Excluding Higher Fringe Benefits

Table 2.17 shows treatment effects of displacement on wages excluding from the sample the 8 individuals who reported receiving more valuable non-cash "fringe" benefits in their post-policy employment. The estimates address the concern that observed wage losses among displaced WSLCB workers may have been partially or completely offset by increases in post-policy non-wage benefits. On the contrary, estimated wage losses are somewhat larger when the sample is restricted to workers reporting the same or less valuable fringe benefits (-$2.809 per hour compared with -$2.508), suggesting substitution between cash and non-cash compensation does not explain the pattern of treatment effects reported in the paper.

Table 2.17: Treatment Effect of Job Displacement on Wages, Excluding Individuals Who Reported Higher Value of Fringe Benefits Post-Policy, 2010-2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Post Policy	0.983***	1.307***	0.848***
	(0.062)	(0.149)	(0.055)
Treatment	-2.331***	-2.616***	-2.128***
	(0.187)	(0.370)	(0.175)
Post Policy x Treatment ($\hat{\beta}_3$)	-2.809***	-2.271***	-3.384***
	(0.357)	(0.658)	(0.354)
Constant	17.335***	17.415***	17.302***
	(0.088)	(0.188)	(0.098)
n	1016	384	632
Adjusted R-squared	0.378	0.245	0.550

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of all involuntarily displaced workers observed between 2010 to 2013, excluding the 8 employed individuals who reported receiving more valuable non-cash "fringe" benefits in their post-policy employment. Standard errors are in parentheses, which are clustered at the individual level.

2.8.5 Results Including Voluntary Quitters

The paper's baseline estimates exclude all individuals from the sample who voluntarily separated from their WSLCB job prior to the policy enactment date of June 1, 2012. Table 2.18 shows treatment effects including in the sample the 10 individuals for whom wages are observed in all pre-policy years and who voluntarily separated. The treatment effect of job displacement is a somewhat smaller -$2.342 when these individuals are included, suggesting voluntary quitters fared better on average than involuntarily displaced workers. This is consistent with ordinary job shifting behavior that is unrelated to the policy change, as voluntary separators likely quit to accept higher wage offers elsewhere.

Table 2.18: Treatment Effect of Job Displacement on Wages, Including Individuals Who Voluntarily Quit Prior to the Policy Date of June 1, 2012, 2010-2013

Variable	(1) All Workers	(2) Men Only	(3) Women Only
Female	-0.112		
	(0.182)		
Job Tenure	0.184*	0.141	0.204**
	(0.103)	(0.211)	(0.102)
Post Policy	0.983***	1.307***	0.848***
	(0.062)	(0.149)	(0.055)
Treatment	-2.299***	-2.484***	-2.179***
	(0.159)	(0.300)	(0.165)
Post Policy x Treatment ($\hat{\beta}_3$)	-2.342***	-2.258***	-2.516***
	(0.385)	(0.618)	(0.492)
Constant	16.585***	16.788***	16.381***
	(0.527)	(0.973)	(0.487)
n	1088	404	684
Adjusted R-squared	0.316	0.241	0.382

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: Regression is of hourly worker wages on binary dummies for treatment, post-policy period, and their interaction. Estimation is for the balanced panel of all workers observed between 2010 to 2013, including the 10 individuals who voluntarily quit WSLCB jobs prior to the policy date of June 1, 2012 and for whom wages are observed in all years 2010 to 2013. Standard errors are in parentheses, which are clustered at the individual level.

2.8.6 Survey Materials

A copy of the initial recruitment letter and questionnaire for the survey of displaced WSLCB workers is provided below. Information on institutional review board approval by the University of California, San Diego's Human Research Protections Program is available at http://irb.ucsd.edu/.

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UCSD Human Research Protections Program
UCSD
Approved
Initial Approval: 03/12/2013
Current Approval: 03/12/2013
Do not use after: 03/11/2014
2 pages

Mr. First Last
Address
City, State Zip

Dear Mr. First,

My name is Andrew Chamberlain, and I am a researcher at the University of California in San Diego. I am conducting a research survey of how workers like you were affected by the recent privatization of Washington State liquor stores due to Initiative 1183.

You were selected to participate because our records show you were one of approximately 900 Washington State Liquor Control Board (WSLCB) employees affected by the privatization. We would like to collect important information from you about how you have been affected by the passage of Initiative 1183. Your participation in the survey is voluntary, and you can choose not to participate if you wish.

Please complete the enclosed questionnaire and return it using the envelope provided. I hope we can count on your participation. Your input is very important and will help state lawmakers better understand how liquor privatization has affected families in Washington State.

You also have the option of completing your survey online. To do so, visit the following website and log in with the 4-digit code printed below:

Website: www.URLHERE.com
Log-in Code: XXXX

It is a short questionnaire that will take approximately **3-6 minutes** to complete. You may choose to skip any question you would like. *Note*: Your responses will be matched to publicly available WSLCB records on wages and employment as part of the academic research study. All survey responses will be kept confidential, and will be stored securely and with no personally identifying information attached when the study is completed.

While there are no direct benefits to you from participation, you will be helping lawmakers understand the effects of privatization on workers like yourself.

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Please watch of your survey in the mail in the next few days. I hope we can count on your participation. In the meantime, please feel free to contact me if you have any questions about our study. I can be reached at (206) 366-5466 or adchamberlain@ucsd.edu.

Sincerely,

Andrew Chamberlain
Department of Economics, University of California, San Diego

Statement Regarding Risks to Participants

We have instituted procedures to help keep your survey responses secure. Your responses will only be identified by the above 4-digit code. However, participants should be aware that there is a small risk of loss of confidentiality. Additionally, you may experience fatigue, boredom, discomfort or stress when completing the survey. In addition, there may be other unforeseeable risks that we do not anticipate at this time.

If you are injured as a direct result of participation in this research, the University of California will provide any medical care needed to treat those injuries. The University will not provide any other form of compensation if you are injured. You may call UCSD Human Research Protections Program at (858) 657-5100 for more information about this or to report research-related problems.

UCSD Human Research Protections Program
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Approved
Initial Approval: 03/12/2013
Current Approval: 03/12/2013
Do not use after: 03/11/2014

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1. When was your last day at your retail liquor job for the Washington State Liquor Control Board? (Mark the month and year).

Year:	Month:			
❑ 2011	❑ Jan	❑ April	❑ July	❑ Oct
❑ 2012	❑ Feb	❑ May	❑ Aug	❑ Nov
	❑ Mar	❑ June	❑ Sept	❑ Dec

2. Were you laid off or did you quit voluntarily?
❑ Laid off
❑ Quit voluntarily

3. Before you left that job, how long did you work for the Wash. State Liquor Control Board?

❑ Less than one year	❑ 5-6 years
❑ 1-2 years	❑ 7-10 years
❑ 3-4 years	❑ 10+ years

4. What is your current employment status today? (Please check only one box.)

❑ Employed full time	❑ Not employed, not looking for work
❑ Employed part time	❑ Retired
❑ Self-employed	❑ Student
❑ Not employed, but looking for work	❑ Homemaker

5. If you are unemployed, how much are you receiving per week in unemployment benefits? (Your best estimate is fine.)
❑ My unemployment benefits are: $__________/week.
❑ I am not currently receiving any unemployment benefits.

6. If you are employed, what is the industry of your main job? (If you are not employed please skip ahead to **Question 12**.)
❑ Retail liquor store, in the same location I used to work at
❑ Retail liquor store, in a different location
❑ Retail, but not at a liquor store
❑ Education or healthcare
❑ Restaurant or hotel services
❑ Manufacturing
❑ Construction
❑ Transportation (e.g., bus driver) or warehouse job
❑ Technology or software
❑ Administrative job
❑ Finance, real estate, or insurance
❑ Agriculture, forestry, or fishing
❑ Government (city, state or federal government)
❑ Other industry (please specify) ___________________________________

7. If you are employed, how many hours per week do you work at your main job? (If you are not employed please skip this question.)
❑ Full time: 40 hours per week
❑ Part time: 20 hours per week
❑ Other: Please specify hours per week __________.

8. If you are employed, are you union represented at your current job? (If you are not employed please skip this question.)
❑ Yes
❑ No

9. If you are employed, what is your hourly wage or monthly salary? (If you are not employed please skip this question.)
Hourly wage: $[ ]/hour OR Monthly salary: $[ ]/month

10. If you are employed, what benefits does your current employer provide? (If you are not employed please skip this question.)
❑ Health insurance
❑ Dental insurance
❑ Paid vacation / sick leave time
❑ 401(k) or other retirement plan
❑ Daycare or child care
❑ Transportation or bus pass subsidy
❑ No benefits

11. If you are employed, think about the dollar value of your current benefits. Are they worth less, more, or about the same as the benefits you received at your Washington State liquor retail job? (If you are not employed please skip this question.)
❑ My benefits are less valuable than my old ones
❑ My benefits are more valuable than my old ones
❑ My benefits are about the same value as my old ones

12. Thinking back to your old Washington State liquor retail job, how many hours per week did you work at that job?
❑ Full time: 40 hours per week
❑ Part time: 20 hours per week
❑ Other: Please specify hours per week: [ ] hours/week

13. Thinking back to your old Washington State liquor retail job, what was your hourly wage or monthly salary at that job?
Hourly wage: $[ ]/hour OR Monthly salary: $[ ]/month

14. Thinking back to your old Washington State liquor retail job, do you remember the store number or address of the location where you worked?
Store Number: #[ ] Address: ____
City: __________________________ Zip: _____________

15. What is your age?

❑ 16 – 19 years	❑ 35 – 39 years	❑ 55 – 59 years
❑ 20 – 24 years	❑ 40 – 44 years	❑ 60 – 64 years
❑ 25 – 29 years	❑ 45 – 49 years	❑ 65+ years
❑ 30 – 34 years	❑ 50 – 54 years	

16. What is your race or ethnicity?

❑ White	❑ Native American or Alaskan native
❑ Black / African American	❑ Other race
❑ Hispanic	❑ Mixed racial background
❑ Asian or Pacific Islander	

17. What is the highest grade you completed in school?

❑ Less than high school	❑ Associate's degree (e.g., A.A.)
❑ Some high school	❑ College degree (e.g., B.A., B.S.)
❑ High school diploma or GED	❑ Some graduate school, but no degree
❑ Some college, but no degree	❑ Graduate degree (e.g., M.A., Ph.D.)

18. What is your marital status?

❑ Single, never married	❑ Separated
❑ Married	❑ Widowed
❑ Divorced	❑ Living with a partner, but single

19. If you are married or living with a partner, is your spouse/partner currently employed?
❑ Yes
❑ No
❑ Not Applicable (single, divorced, widowed, etc.)

20. How many <u>adults</u> currently live in your household (including you)?

❑ 1	❑ 3	❑ 5+
❑ 2	❑ 4	

21. How many <u>children</u> currently live in your household?

❑ 0	❑ 2	❑ 4
❑ 1	❑ 3	❑ 5+

22. What is your gender?
❑ Male
❑ Female

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Chapter 3

The Effect of Federal Intergovernmental Grants on State Taxes

Abstract

I examine whether federal intergovernmental grants have a persistent long-term effect on state fiscal policy. A simple theoretical framework is developed based on the median voter model and is structurally estimated based on a 30-year panel of U.S. federal grants and state tax revenues. In both OLS and IV estimates I find evidence that temporary federal aid has a persistent effect on state finances. Each \$1 of federal grants predicts eventual state tax increases of between \$0.04 and \$0.17. These effects are most evident on state personal income and corporate income taxes. There is some evidence that state tax and expenditure limitations (TELs) and supermajority voting rules mitigate these effects. To address possible endogeneity of federal grants I employ an instrumental variables strategy which yields similar results. I find temporary grant-funded expenditures persist over time in state budgets.

3.1 Introduction

In the wake of the 2009 “American Recovery and Reinvestment Act” there has been a renewed interest in the effects of federal intergovernmental grants on state budgets. A key question is whether states respond symmetrically with respect to expansions and contractions in federal aid—that is, does temporary state spending induced by federal grants disappear from state budgets when grant provisions expire? Or do federal grants have a lasting effect on state budgets, with temporary aid giving rise to permanent state expenditure programs that ultimately require increased local revenue?

There is a large literature examining whether state expenditures respond asymmetrically to federal grants, with mixed empirical results. I contribute to this literature by presenting new evidence of the long-term budget persistence of federal intergovernmental grants to states. We develop a simple theoretical extension of the median voter model that allows for the identification of asymmetric responses of state taxes to federal grants over time. We then structurally estimate the model using a large 30-year panel of U.S. federal intergovernmental grants and state tax data, both via OLS (using a first-differences panel estimator) and via instrumental variables (using a 2SLS estimator).

Our basic results suggest significant state budget persistence of federal grants. Each \$1 in federal aid temporarily stimulates U.S. state spending by roughly \$0.76 with only about \$0.65 of it ultimately disappearing from state budgets in future years. The remaining \$0.11 ultimately becomes persistent state expenditures financed by state tax revenue—an indication of positive budget asymmetry. Put differently, each \$1 of federal grants predicts eventual state own-source revenue increases of between \$0.04 and \$0.17. These effects are most evident for state personal income taxes and corporate income taxes. While I find some evidence that state tax and expenditure limitations (TELs) and supermajority voting rules on taxes help mitigate these budget asymmetries, they are present in nearly every subsample examined. My basic finding of budget persistence of federal intergovernmental grants is evident in both OLS first-differences and IV estimates. The results suggest that temporary federal grants to states may indeed have lasting, and perhaps unintentional, future budgetary consequences.

I organize the remainder of this paper as follows. Section 3.2 surveys the related literature. Section 3.3 develops a simple theoretical model allowing for the identification of budgetary persistence of federal grants. Section 3.4 presents our data and identification strategy. Section 3.5 presents the empirical results from OLS and IV estimations, and Section 3.6 briefly concludes.

3.2 Related Literature

The most closely related literature is one examining whether state budgets are asymmetric with respect to increases and decreases in federal grants. The classic median voter model predicts symmetry in the response of state taxes and spending to federal grants, and thus no long-term budget persistence from temporary intergovernmental grants. Much of the early "flypaper effect" literature examining the relationship between federal grants and state fiscal policy implicitly assumes this symmetry in its empirical specifications. This large early literature is surveyed in Gramlich and Galper (1973); Wyckoff (1991); Hines and Thaler (1995); Bailey and Connolly (1998); and Inman (2008).

By the 1970s researchers began questioning whether state and local budgets would respond symmetrically to increases and decreases in federal intergovernmental grants. One of the earliest discussions of this possibility appears in a 1973 commentary from Stephen Goldfeld who writes, "I am not sure how it would be done, but it would be desirable to incorporate the fact that, once a program is started, it is not easy to turn off." In the same commentary [Goldfeld and Brainard (1973)], William Brainard adds that "some types of spending work on a ratchet—for example, it is particularly difficult to cut educational expenditures... The asymmetry of increases and decreases in the expenditure process may be fairly unimportant for growing communities, but critical to those that are stagnant or contracting."

The first empirical evidence for state budget asymmetry did not emerge for another decade, beginning with studies of the elimination of the federal General Revenue Sharing (GRS) program. Betwen 1972 and 1986 the GRS program provided unconditional federal intergovernmental grants to state and local governments as part of a revenue sharing arrangement. Gramlich (1987) was the first to examine the impact of the GRS repeal, concluding that withdrawal of federal aid coincided with an increase in state own-source revenue—an indication that grant-financed expenditures persisted in state budgets after GRS repeal. More recently Volden (1999), Lalvani (2002) and Owens (2010) each have provided empirical evidence of state budget asymmetry. However, the literature on grant-related budget asymmetry remains mixed, with Stine (1994), Gamkhar and Oates (1996), Gamkhar (2000), Gamkhar (2001) and Gordon (2004) finding no evidence of asymmetry. These and other related works in the "grant asymmetry" literature are reviewed extensively in Alderete (2004).

The most similar recent work is Sobel and Crowley (forthcoming). In it, the authors argue that federal grants may lead to upward "ratcheting" of state taxes over time based on a variety of formal and informal arguments from the public choice and political science literatures. Using a reduced form empirical strategy and U.S. data from 1995 to 2008, they find federal intergovernmental grants have a significant positive effect on local state revenue. While carefully executed, their empirical work has the drawback of not being framed by a formal theoretical model and makes use of a fixed-effects panel estimator in levels which may yield unreliable results in the case of nonstationary federal grants and state tax revenue over time.

This paper contributes to the existing literature in three ways. First, I develop a simple theoretical model of the impact of federal grants on state taxes over time under the assumption of budget-persistent federal grants. Next, I structurally estimate the model based on a 30-year panel of federal grants and state taxes for the U.S. Finally, I use an estimation strategy that avoids a potential problem in some previous literature: the potential for spurious regressions due to nonstationary state tax and grant data. My findings provide new evidence of state budget asymmetry with respect to federal intergovernmental grants—a potentially important result for state lawmakers considering whether to increase budgetary reliance on federal intergovernmental aid.

3.3 Theory

Basic Model

We develop a simple model that allows for the identification of persistent effects of federal intergovernmental grants on state taxes. The model is a straightforward extension of the median voter model, modified to allow intergovernmental-grant-funded state spending at time t to persist beyond time $t+k$ when federal grants expire. State lawmakers choose the level of government spending G and private consumption goods C to maximize the utility of the median voting block, subject to an annual balanced-budget constraint. Suppose for simplicity that intergovernmental grants T_t are unconditional or non-matching grants.[1] At time t state lawmakers face the decision problem,

$$\max_{C_t,G_t} U(C_t, G_t) \text{ subject to } P_C C_t + P_G G_t \leq Y_t + T_t \tag{3.1}$$

where $U(C_t, G_t)$ captures the single-peaked preferences of the median voter over private consumption goods C and government services G, P_C is the average price of private consumption goods, P_G is the average price of government services, Y is state private income and T is federal intergovernmental grants. We make the usual assumptions on utility of U_C, $U_G > 0$ and U_{CC}, $U_{GG} < 0$. Further, we assume private consumption and government services are complements in the Edgeworth-Pareto sense (Samuelson, 1974) such that the marginal utility of government services increases with private consumption and vice versa, or U_{CG}, $U_{GC} \geq 0$. The state's first-order conditions are then given by,

$$U_C(C_t, G_t) + \lambda_t P_C = 0 \tag{3.2}$$

$$U_G(C_t, G_t) + \lambda_t P_G = 0 \tag{3.3}$$

$$Y_t + T_t - P_C C_t - P_G G_t = 0 \tag{3.4}$$

where U_C and U_G are the median voter's marginal utilities for C and G, respectively, and λ is the usual multiplier from the associated Lagrangian. At time t,

[1] This simplification is without loss of generality. All of the results below can be similarly derived for the case of conditional or matching intergovernmental grants.

state lawmakers follow a simple rule: continue spending on government services until the marginal utility of G relative to the price of government services P_G is just equal to the marginal utility of private consumption goods C relative to their price P_C, subject to the balanced-budget constraint from equation (4). Denote these optimal solutions $C_t^*(P_C, P_G, Y_t, T_t)$ and $G_t^*(P_C, P_G, Y_t, T_t)$.

The effects of an additional federal intergovernmental grant ΔT_t on state tax revenue R_t can be seen via the usual comparative statics. Totally differentiating equations (2) - (4) with respect to federal intergovernmental grants T_t we have the following linear system,

$$\begin{bmatrix} U_{CC} & U_{CG} & P_C \\ U_{GC} & U_{GG} & P_G \\ P_C & P_G & 0 \end{bmatrix} \begin{bmatrix} \frac{\mathrm{d}C_t}{\mathrm{d}T_t} \\ \frac{\mathrm{d}G_t}{\mathrm{d}T_t} \\ \frac{\mathrm{d}\lambda_t}{\mathrm{d}T_t} \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \\ 1 \end{bmatrix} \tag{3.5}$$

To simplify notation, let the average price of government services be the numeraire good so that $P_G = 1$, and label the 3 x 3 bordered Hessian on the left as $\mathbf{D_t}$. Solving for the marginal effect of federal grants on state government spending dG_t/dT_t via Cramer's rule we have,

$$\frac{\mathrm{d}G_t}{\mathrm{d}T_t} = \frac{\begin{vmatrix} U_{CC} & 0 & P_C \\ U_{GC} & 0 & P_G \\ P_C & 1 & 0 \end{vmatrix}}{|\mathbf{D_t}|} = \frac{U_{GC}P_C - U_{CC}}{U_{GC}P_C + U_{CG}P_C - U_{CC} - U_{GG}P_C^2} \tag{3.6}$$

Dividing the numerator and denominator by $U_{GC}P_C - U_{CC}$ we have,

$$\frac{\mathrm{d}G_t}{\mathrm{d}T_t} = \frac{1}{1 + \frac{U_{GC}P_C - U_{CC}P_C^2}{U_{GC}P_C - U_{CC}}} < 1 \tag{3.7}$$

Equation (7) yields a key testable implication of the model. In the denominator, $U_{GC} \geq 0$ by complementarity of private consumption and government services and $U_{CC} < 0$ by concavity of the utility function. Thus, we have $0 < dG_t/dT_t < 1$ so that the marginal effect of federal grants on state spending is bounded by zero and unity. Thus, we expect states receiving federal grants to use some fraction for increased spending G and refund the remaining $1 - dG_t/dT_t$ back to households in the form of tax reductions. Under the static median voter model, we thus should observe a negative same-period correlation between federal intergovernmental grants T_t and state tax revenue R_t. Previous estimates of dG_t/dT_t from the flypaper effect literature range from 0.40 to 0.90[2], implying an effect of current federal grants on state taxes dR_t/dT_t of roughly -0.10 to -0.60.

Incorporating Budget Persistence

We now extend the above framework to allow for the possibility that temporary federal grants at time t may become "persistent," having a lasting effect on state budgets at time $t+k$. Consider N states indexed by the letter $i = 1, ..., N$. Suppose state S_i initially receives no federal aid and has government spending of G_{i0} and tax revenue $R_{i0} = G_{i0}$[3]. In period $t = 1$, suppose S_i receives a federal grant T_{i1}. As noted above, state lawmakers spend a fraction of the grant $dG_t/dT_t < 1$ and refund the remaining $(1 - dG_t/dT_t)$ back to households. Define $\alpha = dG_t/dT_t$. We label α the "stimulative" parameter in the model. In period one, state spending rises to $G_{i0} + \alpha T_{i1}$ and state taxes fall to $G_{i0} + (\alpha - 1)T_{i1}$.

Now consider time $t = 2$, in which the state receives an additional federal grant T_{i2}. As before, state S_i spends part of the grant α and refunds the remaining $(1-\alpha)$ back to households. Suppose the grant-funded spending αT_{i1} from the previous period does not fully disappear from state budgets, and instead some fraction of it becomes permanent state spending financed by local tax revenue. Define β as the fraction of the previous period's grant-induced spending that persists into the current period. We label β the "persistence" parameter in the model. In period $t = 2$ state spending thus rises to $G_{i0} + \alpha\beta T_{i1} + \alpha T_{i2}$, where the term $\alpha\beta T_{i1}$ is the increase in state spending at time $t = 2$ due to previous federal aid from time $t = 1$. Due to state balanced budget requirements, local taxes in period $t = 2$ rise to $G_{i0} + \alpha\beta T_{i1} + (\alpha - 1)T_{i2}$.

We can continue tracing the "stimulative" and "persistence" effects of fed-

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[2]See Hines and Thaler (1995), Table 1 for a summary of previous estimates of dG_t/dT_t.

[3]For simplicity, we assume states maintain balanced budgets. Of the 50 states, 45 are constitutionally required to do so, and four others are required to do so by statute. Only Vermont has neither a constitutional nor statutory balanced budget requirement.

eral intergovernmental grants on state taxes and spending in future periods $t = 3$, ..., $t = T$ as well. Table 1 illustrates the evolution of state government spending G_{it} and tax revenue R_{it} over time in the model. The bottom row provides general expressions for state spending and tax levels in period t as a function of initial state spending G_{i0}, a history of federal intergovernmental grants $H_i = \{T_{i1}, T_{i2}, ..., T_{it}\}$, and the parameters α and β capturing the stimulative and persistence effects of federal aid.

Our relationship of interest is the effect of past intergovernmental grants on state tax revenue at time t. This is given in the bottom row of Table 1 as,

$$R_{it} = G_{i0} + (\alpha - 1)T_{it} + \alpha\beta \sum_{j=1}^{t-1} T_{ij} \tag{3.8}$$

Equation 8 specifies that state tax revenue in state i at time t should equal a state-specific constant G_{i0}, plus $(\alpha - 1)$ times current federal grants, plus $\alpha\beta$ times the sum of past federal grants. While state tax revenue R_{it} and federal grants T_{it} can be directly observed, the structural parameters α and β cannot. Thus, our goal is to make inferences about the symmetry of state budgets with respect to federal grants by econometrically estimating α and β from U.S. data.

3.4 Data and Identification Strategy

Our data consist of a 30-year panel of state tax revenue and federal grants for the U.S. states from 1981 through 2010. Data on federal intergovernmental grants are from the U.S. Census Bureau's annual "Federal Aid to States for Fiscal Years" and "Federal Expenditures by State for Fiscal Years" reports. Information on state tax and own-source revenues are from the Census Bureau's annual "State Government Finances" reports. All figures are inflation-adjusted into real dollars using the "Consumer Price Index—CPI-U" from the U.S. Bureau of Labor Statistics. Table 2 presents summary statistics.

Our goal is to estimate the structural parameters α and β from equation (8) above. To do so, consider the following fixed-effects panel model,

$$R_{it} = \alpha_i + \phi_1 T_{it} + \phi_2 \sum_{j=t-k}^{t-1} T_{ij} + \delta_i t + \xi_t + X_{it}\beta + \epsilon_{it} \tag{3.9}$$

where R_{it} is state tax revenue in state i at time t, α_i is a state-specific fixed effect, T_{it} is federal grants-in-aid, δ_{it} is a state-specific linear time trend to control for upward trends in federal aid over time, ξ_t is a year-specific fixed effect, X_{it} is a vector of state covariates affecting state tax revenue consisting of state population and real state personal income, and ϵ_{it} is a mean-zero error term. For the sum of previous federal aid, I choose the most recent five years ($k = 5$) in my basic model. Multi-year federal grants typically require states to spend the obligated funds within four fiscal years.[4] Thus, any associated "persistence" of federal aid on state taxes should be apparent within one to five years following the initial grant.[5],[6]

One drawback of the empirical model in equation (9) is that both federal grants T_{it} and state tax revenues R_{it} are nonstationary series. Both state taxes and federal grants are strongly trending upward in real terms throughout the sample period from 1981 to 2010. Thus, the fixed-effects estimator in levels from equation (9) runs the risk of so-called "spurious regression" and potentially misleadingly high R-squared values.[7] One solution is to transform the series via first differencing and proceed to estimate a first-differences model with the resulting stationary series. The corresponding first-differences estimator is given by,

$$\Delta R_{it} = \phi_1 \Delta T_{it} + \phi_2 \Delta \sum_{j=t-k}^{t-1} T_{ij} + \delta_i + \gamma_t + \Delta X_{it}\Gamma + \eta_{it} \tag{3.10}$$

Equation (10) is our basic estimating equation, which I estimate below via OLS and 2SLS approaches. The coefficient of interest is ϕ_2, the effect of changes in past

[4]For a discussion of the multi-year structure of federal grants-in-aid, see Gamkhar (2003).

[5]As a robustness check, I also examined models with up to 10 lags of federal aid. However, no lags beyond five years had coefficients that were statistically significant.

[6]Due to timing differences between state and federal fiscal years, I lag all federal grants in the panel by one year. This is done to assure that all information about the complete year of federal aid at time t is available to states prior to the start of their state's fiscal year.

[7]For a discussion of "spurious regression" and related issues that arise with non-stationary time-series see Hamilton (1994).

federal intergovernmental grants on current state tax levels. Note that ϕ_1 and ϕ_2 correspond directly to the structural parameters α and β from the above model. Specifically, $\phi_1 = (\alpha - 1)$, and $\phi_2 = \alpha\beta$. Thus, by econometrically estimating equation (10) we are able to recover the "stimulative" and "persistence" coefficients from our theoretical model as,

$$\alpha = \phi_1 + 1 \tag{3.11}$$

$$\beta = \phi_2/(\phi_1 + 1) \tag{3.12}$$

The estimate of $dG_t/dT_t = \alpha$ is the fraction of federal grants at time t that result in new state spending at time t. The estimate of β is the fraction of grant-funded spending in year t that results in permanent locally financed state spending at time $t+1$, ..., $t+k$. The combined effect $\alpha\beta$ is the amount by which local tax revenues must ultimately rise for each \$1 of federal intergovernmental aid received.

Based on estimates from previous literature we expect a coefficient on current grants ϕ_1 of between -0.1 and -0.6, implying a stimulative parameter α of between 0.4 and 0.9. If federal grants have no persistent effect on state taxes, $\beta = 0$ and we should expect to find a coefficient on the sum of past grants of $\alpha\beta = \phi_2 = 0$. If temporary federal grants induce permanent state spending obligations that result in higher state own-source revenue, $\beta > 0$ and we should expect a coefficient on past grants ϕ_2 of between 0 and 0.9.

3.5 Results

3.5.1 Basic Results

Table 3.3 presents our basic OLS results. It shows the regression of state own-source revenue for the 50 U.S. states on current and lagged federal grants-in-aid, state and year fixed effects, and controls for population and real personal income as specified by our estimating equation (10). Our basic model is presented in Column (6). As an exhibit, in Columns (1) though (5) we show the individual components of the overall sum of past federal grants. For example, in Column (1) we regress own-source revenue on current grants and last year's grants; in Column

(2) we regress revenue on current grants and the last two years of grants; and so on. Standard errors are reported in parentheses and are heteroskedasticity-robust and clustered at the state level to allow for arbitrary autocorrelation in state-level observations over time.

As expected, the coefficients on federal grants at time t are all negative, are between zero and one in absolute value and are statistically significant as predicted by the median voter model. A coefficient of -0.241 in Column (6) implies a "stimulative" parameter of $\alpha = 0.759$, which implies states spend an average of \$0.76 of each federal dollar on new government spending and refund the remaining \$0.24 back to households in the form of tax reductions. This estimate is within the range found in previous literature.[8]

The key finding from Table 3.3 is the positive coefficient on the sum of past federal grants in Column (6). A coefficient of $\alpha\beta = 0.106$ implies each dollar of federal aid results in higher state own-source revenue of about \$0.11 in the long run. This implies a "persistence" parameter of $\beta = (0.106/0.759) = 0.140$, suggesting that roughly 14 percent of all temporary grant-funded spending ultimately becomes a permanent tax-financed state program.[9] Put differently, of the roughly \$0.76 of each grant dollar spent by states, about \$0.11 or 14 percent ultimately becomes permanently higher state spending financed through own-source revenue. These results suggest federal intergovernmental aid may indeed have lasting effects on state government finances, and provide evidence of state budget asymmetry with respect to federal grants.

Table 3.4 presents similar results for the effect of federal intergovernmental grants on state tax revenue, which excludes all non-tax revenue sources such as fees, charges and lotteries that are included in own-source revenue above. As before,

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[8] In the "flypaper effect" literature, studies typically estimate the impact of non-matching federal grants only, whereas we estimate the combined effect of both matching and non-matching grants. Because matching grants generally have a more stimulative effect on state spending, we should expect our estimate of α to be somewhat higher than if only non-matching grants were analyzed. As expected, our estimate of $\alpha = 0.759$ falls near the high end of previous estimates, which range from roughly 0.4 to 0.9 [see Hines and Thaler (1995)].

[9] I bootstrap the standard error for our estimate of $\beta = (\phi_2/(\phi_1 + 1))$ with $n = 1,000$ replications, yielding a standard error of .0665. Thus, β is statistically different from zero at the 5 percent level ($p = 0.036$).

the estimated coefficients for our basic model in Column (6) all have the expected sign and magnitude and are highly statistically significant. I find one dollar of federal grants lowers state tax levels by an average of \$0.16 in the current year ($\alpha = 0.838$) but ultimately results in higher state tax levels by roughly \$0.09 in the future ($\alpha\beta = 0.094$, and $\beta = 0.112$).

One interesting finding is that the budget "persistence" of federal aid appears to be somewhat larger for state own-source revenue ($\beta = 0.140$) than for state tax revenue ($\beta = 0.112$). Although the difference between the two estimates does not reach statistical significance, it is suggestive that states may have incentives to rely more heavily on non-tax revenue from fees, charges and lotteries when responding to expiring federal aid than direct tax increases.[10] This may be the result of legal constraints that make direct taxes more difficult to increase than non-tax sources, or may simply reflect that non-tax revenues are generally less visible or "salient" to taxpayers and can thus be raised at lower political cost to lawmakers.

3.5.2 Effect in Subsamples

One potentially interesting question is whether the effect of past federal grants on state taxes varies within subsamples featuring different political and legal restrictions on state taxing authority. Specifically, I examine whether the above effects are influenced by the presence of a supermajority-voting rule for tax increases or a state tax and expenditure limitation (TEL).[11] Tables 3.5 and 3.6 present our results for own-source revenue and state tax revenue. For simplicity, I report estimates for our main econometric model only from Column (6) of the above tables. In Column (1) I repeat the overall national results for comparison. In Columns (2) and (3) I examine states with and without statutory TELs. In Columns (4) and (5) I show results for states with and without supermajority voting rules on tax changes.

[10]This pattern of effects is also noted in Sobel and Crowley (forthcoming).

[11]Information on U.S. states with active TELs and supermajority voting rules for tax purposes is from the National Conference of State Legislatures (see www.ncsl.org/issues-research/budget/state-tax-and-expenditure-limits-2008.aspx).

Overall, I find suggestive evidence that the presence of statutory TELs and supermajority voting rules is associated with lower degrees of budget asymmetry with respect to federal grants. In the case of state own-source revenue (Table 3.5), the estimated coefficients on past federal grants $\alpha\beta$ are statistically significantly smaller in states with both TELs and supermajority voting rules on taxes than in states with no statutory taxing limitations.[12] While this evidence is suggestive, it should be interpreted cautiously. State enactment of TELs and voting rules is clearly endogenous, and likely reflects other unobserved characteristics of states not accounted for by the simple estimation procedure used to generate the estimates in Tables 3.5 and 3.6. Thus, these estimates should not necessarily be interpreted as the causal effect of TELs or supermajority voting rules on the presence of state budget asymmetry with respect to federal grants.

3.5.3 Effect on Specific State Taxes

We next examine which state taxes appear to be most heavily affected by past federal grants. Among the major state taxes, I examine which ones lawmakers appear to rely on most heavily when filling in budgetary holes left behind by expiring or shifting federal grants. Table 3.7 presents results for five major taxes: state personal income taxes, corporate income taxes, general sales taxes, and alcohol and tobacco excise taxes.

Of the taxes examined, federal grants appear to have a significant upward effect on two: state personal income taxes and corporate income taxes. I find each dollar of federal aid predicts eventual increases in personal income tax levels of roughly 5.1 cents and corporate income tax levels of 3.6 cents. I do not find any statistically significant effect of federal grants on state general sales taxes, or on alcohol or tobacco excise taxes. As noted above, these estimates likely represent only a partial view of the fiscal response of states to expiring federal grants, as much of the response appears to occur through non-tax fees, charges, lottery and other non-tax revenue sources.

[12] The pair-wise test statistic for the two estimates is given by $t_{A,B} = \frac{E^A - E^B}{\sqrt{(s^A)^2 + (s^B)^2}}$, with t = 2.69 for TELs and t = 1.97 for supermajority voting rules on taxes.

3.5.4 Effect by Granting Federal Agency

Finally, I examine whether federal grants from some federal agencies are more likely to place upward pressure on state taxes than others. In Table 3.8 I present regressions of state tax and own-source revenue on current and lagged federal grants from a variety of federal departments including Agriculture, Education, Health and Human Services, Housing and Urban Development, Transportation and all others combined.[13] Because of limited data availability for federal grants from individual federal agencies, these estimates in Table 3.8 are based on a considerably smaller sample size of $n = 450$ (compared to $n = 1{,}150$ above). As a result, the model is much less precisely estimated for grants from individual agencies.

Of the federal agencies examined, only two had a statistically significant effect on state taxes and own-source revenue. The Department of Health and Human Services, which administers a large fraction of all federal aid through state Medicaid grants, and the Department of Transportation both had a large and significant impact on taxes and own-source revenue. Grants from Health and Human Services raised state own-source and tax revenue by \$0.386 and \$0.439 per dollar of grants, respectively. Transportation grants resulted in the largest effect, raising state tax revenue by \$0.539 per dollar of grants. I do not find a statistically significant effect from any of the other federal agencies examined.

3.5.5 Addressing Grant Endogeneity (IV Estimates)

One concern with the above estimates is that they may suffer from endogeneity bias. As with most federal policy, federal grants to states are endogenously determined by administrators within granting agencies and lawmakers in the U.S. Congress. For example, federal macroeconomic stimulus policy (such as the recent 2009 "American Recovery and Reinvestment Act") may lead to increased grants to states during economic recessions when state tax revenues typically decline. Similarly, state governments may exert additional grant-seeking effort when realizations of state own-source revenues fall below those projected for budgetary

[13]Due to limited data availability for individual federal departments prior to 1995, I restrict our sample in Table 3.8 to the 10 year period from 2001-2010 ($n = 500$).

purposes. In each of these cases, federal grants at time t are likely to be correlated with unobserved factors ϵ_{it} that determine state tax revenue at time t.

To address this possibility I use an instrumental variables (IV) approach, instrumenting for current and past federal grants to state i using two well-known predictors of federal expenditures to states: (1) the average seniority (in years) of a state's members in the U.S. House of Representatives at time t, and (2) the number of state appointees on the powerful U.S. House Appropriations Committee at time t. A variety of previous studies have found congressional political power to be a strong predictor of federal expenditures to states [see for example Gruber and Hungerman (2005), Anderson and Tollison (1991), and Couch and Shughart (1998).] Data for both instruments is from 1993 to 2010, and is drawn from an online archive compiled by MIT Political Science professor Charles Stewart.[14]

I instrument for the two potentially endogenous regressors in our basic model from equation (6): ΔT_{it} and $\Delta \sum_{j=t-1}^{t-5} T_{ij}$. To account for the presence of five lags of federal intergovernmental grants in our endogenous regressors, I instrument using current and five lagged values for each of our chosen instruments, for a total of 12 excluded instruments for two endogenous regressors. After accounting for the various lags of data, our sample period for the IV estimation is the 12 year period from 1999 through 2010 ($n = 600$).

For instrument relevance we require a significant correlation with current and past federal grants to state i. For validity, we require that these instruments be uncorrelated with unobservable determinants of state tax revenue (beyond state- and year-fixed effects, current and past federal grants, and state population and personal income), captured by ϵ_{it} in our estimating equation. While both instruments satisfy the usual criteria for relevance (as demonstrated below), there remain unresolved questions about validity. For example, we cannot rule out the possibility of reverse causality in which appointments to key congressional committees are determined by the level of federal aid received by states rather than the reverse. Similarly, it may be the case that states receiving large federal grants also happen to have more long-standing members of Congress for reasons unrelated to federal

[14]Data for both instruments is available at web.mit.edu/17.251/www/data_page.html.

grant policy. While we are able to partially establish instrument validity via the usual over-identification tests (as shown below) we cannot conclusively establish the validity of our instruments. As a result, I suggest caution in interpreting my IV estimates as representing the pure causal effect of federal intergovernmental grants on state taxes.

I follow a standard 2SLS estimation procedure. Our first-stage results are presented in Table 3.9. Both measures of state political strength appear to be reasonably strong predictors of current and past federal intergovernmental grants to states. While not all of the instruments achieve statistical significance, the joint F-statistics for the two first-stage estimations are $F = 73.06$ for predicted current grants and $F = 95.45$ for the sum of past grants—well beyond the usual first-stage rule of thumb of $F > 10$. I conclude that weak instruments are unlikely to present a serious problem.

The second stage IV estimates are presented in Table 3.10. After instrumenting for changes in current and past federal grants, I find somewhat larger effects when compared to OLS. Our coefficient of interest is $\phi_2 = \alpha\beta$ in the second row of the table, which captures the persistent effect of past grants on current state tax revenue. For own-source revenue I find $\alpha\beta = 0.804$ and for state tax revenue I find $\alpha\beta = 0.494$, both of which are statistically significant at the 0.05 and 0.10 levels, respectively. As a test of instrument validity I perform the usual Sargan procedure as a test of over-identifying restrictions. The test fails to reject in our regression for state own-source revenue ($p = 0.399$) but rejects in the case of state tax revenue ($p = 0.008$), raising some question about the validity of our instruments in the case of state tax revenue. However, in the case of state own-source revenue in Column (1) the instruments appear to be both relevant and valid.

For comparison, Table 3.11 presents OLS estimates for the comparable sample period of 1999 through 2010. At least in the case of state own-source revenue, our IV results appear to reaffirm the basic findings from our OLS estimates: federal intergovernmental grants appear to have persistent long-term effects on state tax revenues, even after accounting for the possible endogeneity of federal intergovernmental grants to states.

3.6 Conclusion

Based on a simple extension of the median voter model and a 30-year panel of U.S. federal grants and state tax revenue, I find evidence that federal intergovernmental grants have a persistent effect on state budgets. Temporary grant-financed expenditures at the state level appear to persist in local budgets long after grants have expired, placing additional demands on local revenue sources to sustain them. I find empirical evidence of this phenomenon both in OLS and IV estimates. While these findings are suggestive, an important but unresolved question is the precise theoretical mechanism behind this apparent budgetary persistence of federal intergovernmental aid—an issue I reserve for future research.

Table 3.1: Expressions for State Government Spending G_{it} and Tax Revenue R_{it} Over Time

Time	State Government Spending (G_{it})	State Tax Revenue (R_{it})	Federal Grants (T_{it})
0	G_{i0}	$R_{i0} = G_{i0}$	0
1	$G_{i0} + \alpha T_{i1}$	$G_{i0} + (\alpha - 1)T_{i1}$	T_{i1}
2	$G_{i0} + \alpha T_{i2} + \alpha\beta T_{i1}$	$G_{i0} + (\alpha - 1)T_{i2} + \alpha\beta T_{i1}$	T_{i2}
3	$G_{i0} + \alpha T_{i3} + \alpha\beta(T_{i1} + T_{i2})$	$G_{i0}+(\alpha-1)T_{i3}+\alpha\beta(T_{i1}+T_{i2)}$	T_{i3}
$\vdots$	$\vdots$	$\vdots$	$\vdots$
t	$G_{i0} + \alpha T_{it} + \alpha\beta \sum_{j=1}^{t-1} T_{ij}$	$G_{i0}+(\alpha-1)T_{it}+\alpha\beta \sum_{j=1}^{t-1} T_{ij}$	T_{it}

Table 3.2: Summary Statistics for the Panel Data Set

Variable	Observations	Mean	Standard Deviation	Minimum	Maximum
Year	1500	n.a.	n.a.	1981	2010
State Population	1500	5,345,230	5,853,118	418,000	37,300,000
Real State Personal Income ($000)	1500	85,700,000	104,000,000	5,473,533	755,000,000
Real Own-Source Revenue ($000)	1500	7,043,333	8,304,256	467,833	67,300,000
Real Total Tax Revenue ($000)	1500	5,311,147	6,659,144	295,657	55,300,000
Real Income Tax Revenue ($000)	1500	1,742,820	2,930,967	0	25,900,000
Real Corporate Tax Revenue ($000)	1500	353,660	603,192	0	5,497,981
Real General Sales Tax Revenue ($000)	1500	1,725,809	2,209,663	0	16,000,000
Real Alcohol Tax Revenue ($000)	1500	50,405	66,656	0	397,946
Real Tobacco Tax Revenue ($000)	1500	102,541	124,787	2,817	725,652
Real Total Federal Grants Received ($000)	1500	2,975,975	3,730,408	239,480	30,500,000

Note: Revenue and federal grant figures are reported in thousands of inflation-adjusted 1982-84 dollars. The data set is a panel of the 50 U.S. states from 1981-2010. Due to the use of various lags of the data, only 1200 observations from 1987-2010 are utilized in our estimation procedure.

Sources: Federal grants from 1995-2010 are from the U.S. Census Bureau's "Federal Aid to States for Fiscal Years" reports; grants from 1981-1994 are from the U.S. Census Bureau's "Federal Expenditures by State for Fiscal Years" report; all tax revenue data are from the U.S. Census Bureau's "State Government Finance" series; State Personal Income is from the U.S. Bureau of Economic Analysis. I thank George Crowley and Russell Sobel for providing observations of federal intergovernmental grants from 1995-2008.

Table 3.3: Regression of State Own-Source Revenue on Current and Past Federal Grants (OLS First-Differences Panel Estimator)

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	-0.287***	-0.270***	-0.274***	-0.289***	-0.273***	-0.241***
	(0.078)	(0.083)	(0.089)	(0.102)	(0.094)	(0.066)
Δ Federal Grants at Time $t-1$	0.087	0.074	0.071	0.058	0.080	
	(0.165)	(0.149)	(0.135)	(0.122)	(0.118)	
Δ Federal Grants at Time $t-2$		0.252***	0.253***	0.240***	0.272***	
		(0.070)	(0.068)	(0.079)	(0.071)	
Δ Federal Grants at Time $t-3$			-0.028	-0.029	-0.006	
			(0.168)	(0.165)	(0.149)	
Δ Federal Grants at Time $t-4$				-0.098	-0.113	
				(0.128)	(0.141)	
Δ Federal Grants at Time $t-5$					0.241**	
					(0.112)	
Δ Sum of Past Federal Grants ($\phi_2 = \alpha\beta$)						0.106***
						(0.035)
n	1150	1150	1150	1150	1150	1150
Adjusted R-squared	0.494	0.503	0.503	0.504	0.511	0.499

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

Table 3.4: Regression of State Tax Revenue on Current and Past Federal Grants (OLS First-Differences Panel Estimator)

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	-0.203***	-0.193***	-0.203***	-0.215***	-0.197***	-0.162***
	(0.050)	(0.052)	(0.062)	(0.068)	(0.061)	(0.043)
Δ Federal Grants at Time $t-1$	0.124	0.116	0.110	0.099	0.123	
	(0.147)	(0.137)	(0.123)	(0.115)	(0.109)	
Δ Federal Grants at Time $t-2$		0.151**	0.154**	0.143*	0.177**	
		(0.074)	(0.075)	(0.082)	(0.074)	
Δ Federal Grants at Time $t-3$			-0.059	-0.060	-0.035	
			(0.162)	(0.159)	(0.143)	
Δ Federal Grants at Time $t-4$				-0.083	-0.099	
				(0.086)	(0.093)	
Δ Federal Grants at Time $t-5$					0.263***	
					(0.093)	
Δ Sum of Past Federal Grants ($\phi_2 = \alpha\beta$)						0.094***
						(0.015)
n	1150	1150	1150	1150	1150	1150
Adjusted R-squared	0.492	0.496	0.496	0.497	0.509	0.495

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

Table 3.5: Effect of Federal Intergovernmental Grants on State Own-Source Revenue in Various Subsamples

Variable	(1) Full Sample (All U.S. States)	(2) States with Tax-Expenditure Limitations	(3) No Tax-Expenditure Limitation	(4) States with Supermajority Voting Rules on Taxes	(5) No Supermajority Voting Rule on Taxes
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	-0.241***	-0.208**	-0.291***	-0.294***	-0.177*
	(0.066)	(0.092)	(0.086)	(0.059)	(0.104)
Δ Sum of Past Federal Grants ($\phi_2 = \alpha\beta$)	0.106***	0.065*	0.193***	0.056	0.179***
	(0.035)	(0.034)	(0.036)	(0.048)	(0.040)
n	1150	690	460	368	782
Adjusted R-squared	0.499	0.511	0.507	0.578	0.443

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

Table 3.6: Effect of Federal Intergovernmental Grants on State Tax Revenue in Various Subsamples

Variable	(1) Full Sample (All U.S. States)	(2) States with Tax-Expenditure Limitations	(3) No Tax-Expenditure Limitation	(4) States with Supermajority Voting Rules on Taxes	(5) No Supermajority Voting Rule on Taxes
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	-0.162***	-0.141**	-0.201***	-0.146***	-0.161**
	(0.043)	(0.053)	(0.065)	(0.031)	(0.070)
Δ Sum of Past Federal Grants ($\phi_2 = \alpha\beta$)	0.094***	0.077***	0.135***	0.082**	0.126***
	(0.015)	(0.026)	(0.016)	(0.036)	(0.023)
n	1150	690	460	368	782
Adjusted R-squared	0.495	0.508	0.497	0.556	0.473

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

Table 3.7: Effect of Federal Grants on Various State Tax Levels

Variable	(1) Total State Tax Revenue	(2) Personal Income Tax Revenue	(3) Corporate Income Tax Revenue	(4) General Sales Tax Revenue	(5) Alcohol Tax Revenue	(6) Tobacco Tax Revenue
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	-0.162***	-0.196***	-0.021	0.016	0.002***	-0.002
	(0.043)	(0.068)	(0.028)	(0.028)	(0.001)	(0.005)
Δ Sum of Past Federal Grants ($\phi_2 = \alpha\beta$)	0.094***	0.051**	0.036***	0.005	-0.000	-0.003
	(0.015)	(0.025)	(0.008)	(0.009)	(0.001)	(0.003)
n	1150	1150	1150	1150	1150	1150
Adjusted R-squared	0.495	0.265	0.229	0.309	0.011	0.026

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

Table 3.8: Effect of Federal Intergovernmental Grants from Specific Granting Agencies

Variable	(1) State Own-Source Revenue	(2) State Total Tax Revenue
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	-0.185*	-0.110
	(0.094)	(0.089)
Δ Sum of Past Department of Agriculture Grants ($\phi_2 = \alpha\beta$)	-2.685	-3.577
	(2.872)	(3.680)
Δ Sum of Past Department of Education Grants ($\phi_2 = \alpha\beta$)	-0.192	-0.256
	(0.856)	(0.801)
Δ Sum of Past Department of Health and Human Services Grants ($\phi_2 = \alpha\beta$)	0.386**	0.439***
	(0.154)	(0.144)
Δ Sum of Past Department of Housing and Urban Development Grants ($\phi_2 = \alpha\beta$)	0.169	0.067
	(0.216)	(0.178)
Δ Sum of Past Department of Transportation Grants ($\phi_2 = \alpha\beta$)	0.532	0.539*
	(0.351)	(0.318)
Δ Sum of Past All Other Departments Grants ($\phi_2 = \alpha\beta$)	0.307	0.072
	(0.344)	(0.270)
n	450	450
Adjusted R-squared	0.558	0.563

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively.
Note: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

Table 3.9: First-Stage IV: Regression of Endogenous Variables (Current and Past Federal Grants) on Included and Excluded Instruments, 1995-2010

Variable	Δ Federal Grants at Time t	Δ Sum of Past Federal Grants
U.S. House Seniority (t)	6,782*	9,223
	(3,808)	(11,780)
U.S. House Seniority (t-1)	(2,997)	(12,293)
	(5,651)	(8,383)
U.S. House Seniority (t-2)	(7,711)	10,523***
	(7,050)	(3,851)
U.S. House Seniority (t-3)	3,854	(9,231)
	(4,893)	(8,658)
U.S. House Seniority (t-4)	(2,430)	(1,336)
	(5,209)	(5,733)
U.S. House Seniority (t-5)	7,043	(2,737)
	(7,865)	(6,145)
House Appropriations Committee (t)	38,035	58,365
	(52,504)	(123,256)
House Appropriations Committee (t-1)	(139,058)	(1,647)
	(85,555)	(45,947)
House Appropriations Committee (t-2)	150,500**	17,491
	(70,269)	(76,204)
House Appropriations Committee (t-3)	(97,825)	(78,730)
	(84,500)	(81,874)
House Appropriations Committee (t-4)	172,856	120,475
	(125,720)	(101,122)
House Appropriations Committee (t-5)	(135,621)	769
	(77,872)	(68,365)
n	550	550
Adjusted R-squared	0.542	0.802
F-Statistic (First Stage)	73.06	95.45

Table 3.10: Second-Stage IV: Regression of State Tax and Own-Source Revenue on Federal Grants, Instrumenting for Current Grants and the Sum of Past Grants, 1995-2010

Variable	(1) State Own-Source Revenue	(2) State Total Tax Revenue
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	0.002	-0.006
	(0.519)	(0.312)
Δ Sum of Past Federal Grants ($\phi_2 = \alpha\beta$)	0.804**	0.494*
	(0.401)	(0.275)
Δ State Personal Income	0.060**	0.073***
	(0.024)	(0.018)
Δ State Population	-0.349	-0.515
	(1.412)	(0.762)
n	550	550
Adjusted R-squared	0.344	0.427
Sargan's Statistic (Instrument Validity)	12.595	26.785
Critical Value	21.03	21.03
P Value	0.399	0.008

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. *Note*: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

Table 3.11: OLS Results for Comparison: Regression of State Tax and Own-Source Revenue on Federal Grants, 1995-2010

Variable	(1) State Own-Source Revenue	(2) State Total Tax Revenue
Δ Federal Grants at Time t ($\phi_1 = \alpha - 1$)	-0.339***	-0.228***
	(0.121)	(0.082)
Δ Sum of Past Federal Grants ($\phi_2 = \alpha\beta$)	0.246***	0.217*
	(0.090)	(0.121)
Δ State Personal Income	0.086***	0.085***
	(0.019)	(0.017)
Δ State Population	0.182	-0.204
	(0.759)	(0.604)
n	550	550
Adjusted R-squared	0.451	0.462

*, **, and *** denote statistical significance at the 0.10, 0.05 and 0.01 levels, respectively. *Note*: All specifications include state and year fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses, which have been clustered by state.

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