FOUR PAPERS ON PUBLIC ECONOMICS

AN ABSTRACT

SUBMITTED ON THE THIRD DAY OF MAY

TO THE DEPARTMENT OF ECONOMICS

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

OF THE SCHOOL OF LIBERAL ARTS

OF TULANE UNIVERSITY

FOR THE DEGREE

OF

DOCTOR OF PHILOSOPHY

BY

Sean Larkin

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Abstract

This dissertation contains four papers on public economics. The first chapter analyzes consumer's responses to sales tax rates and sales tax rate changes using a combination of the interview and diary portions of the Consumer Expenditure Survey (CEX) and a novel data set of the timing and type of state sales tax rate changes in a stacked regression methodology. No evidence is found to support the claim that higher sales tax rates lead to lower gross household expenditures, and suggestive evidence is found that consumers respond more strongly to sales tax changes mandated by popular vote rather than by the state legislature. The second chapter examines how hospitals have responded to the Medicaid expansion of 2014 using hospital cost reports from the Centers for Medicare & Medicaid Services in difference-in-differences and event study empirical models. The Medicaid expansion clearly resulted in higher Medicaid payments and lower costs of treating the uninsured, as well as higher capital balances. I also find suggestive evidence that the number of full-time equivalents hired by hospitals also increased. The last two are coauthored and focused on the nomination process and economic impact of Opportunity Zones respectively. In the first, my coauthors and I use a logit model to predict the likelihood of opportunity zone nomination using data from the American Community Survey data and Ballotpedia.com. We find that overall, the nomination process was technocratic, but political variables mattered on the margins. In the final chapter, my coauthors and I combine the universe of all Florida real estate transactions with the data from the previous paper and use instrumental variable and fuzzy regression discontinuity techniques to examine how OZs have affected real estate prices. We find no consistent evidence that OZs have had any impact on real estate prices.

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A DISSERTATION

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Chapter 1: The Effect of State Sales Tax Rates on Consumer Expenditures

Sean Larkin[1]

Abstract: The revenue and economic impact of a sales tax change is dependent on the consumer response to the change. To examine the long and short-run effects of state sales tax rate changes on consumer expenditure by category, I run a reduced form model using data from the Consumer Expenditure Survey and state legislatures. I find evidence that consumer responses are more pronounced for referendum-induced sales tax rate changes than sales tax rate changes passed by the state legislature. I also fail to find any evidence that larger state sales tax rates are associated with lower gross consumer expenditures. This suggests that sales taxes are efficient at generating revenue, and referendum-induced tax increases affect household behavior in the short-run.

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1 Introduction

A sales tax, or a tax in which a specified percentage of a sale is remitted to the governmental body that levies it, is a type of consumer-oriented taxation common in the United States. Sales taxes are imposed by individual state and local governments, and they can be levied either directly on retailers or buyers within a jurisdiction[2]. Most goods and services are subject to general state sales taxes in states where they are levied, although food, intermediate goods, capital goods, pharmaceuticals, and health services are often exempt.

This paper seeks to examine the effect of state sales tax rates on short and long-run household expenditures by using linear regression models to look at the effects of both the overall state sales rate and behavioral changes before and after a sales tax rate change. How households respond to consumer-oriented taxes is relevant to both the field of public finance and policy makers, as it affects the revenue changes that can be expected from a change in the sales tax rate, the calculation of tax incidence, and optimal taxation. The response to a sales tax rate also has implications for determining the macroeconomic impact of the tax (Gabaix, 2016).

Consumer taxes have been the subject of many empirical and theoretical studies. Much recent work has been done on value-added taxes around the world. Evidence from the United Kingdom and Japan suggests that there is intertemporal shifting of consumption in response to a consumption tax increase, although the effect quickly dissipates and spending returns to pre-tax levels (Buettner and Madzharova, 2017;

[2] Retailers are always required to collect and remit the taxes to the government rendering the legal distinction moot from the perspective of the consumer.

Cashin, 2017; Crossley, Low and Sleeman, 2015). However, US state sales taxes and value-added taxes used in many other countries are not identical, and American households may not respond in the same way as their international counterparts. Sales taxes are often calculated at the register rather than Recent empirical evidence indicates that there is deviation from theoretical optimizing behavior when taxes and fees are complicated or partially hidden (Chetty, Looney, and Croft, 2009). American households have also been found to respond to temporary sales tax holidays (Agarwal, Marwell, and McGranahan, 2017), although these holidays differ in scope and persistence from the sales tax rates examined in this paper. Past empirical work focusing on consumption taxes on a single good (e.g., excise taxes) such as cigarettes or gasoline have consistently found intertemporal shifting effects (Decicca, Kenkel, and Liu, 2013; Walsh and Jones, 1988).

Durable goods are of particular interest, as there is empirical evidence from Europe that a positive relationship exists between a household's inflation expectations and their willingness to purchase durable goods in the short run (D'Acunto, Hoang, and Weber, 2015). There is also evidence that consumers in the United States respond to sales tax increases by stocking up on durable goods prior to the increase (Baker, Johnson, and Kreung, 2018), and that the quantity of durable goods purchased is responsive to subsidies (House and Shapiro, 2008).

This paper adds to the existing literature in three ways: First, by making use of both the interview and diary portions of the consumer expenditure survey combined with hand-coded sales tax rates, this paper makes use of several novel data sources that contain monthly spending information at the household level for a broad array of goods and services, allowing a granular examination of household spending by month and category. Second, this paper demonstrates that households change consumption behavior in response to referendum-caused tax increases in the short run, suggesting that sales taxes and the method by which they are changed have relevance when considering the short-run impact of sales tax rate changes. Finally, this paper finds that the magnitude of sales tax rates do not significantly affect consumer expenditure, suggesting that they are an efficient form of revenue for the state and locality but ineffective as a tool for affecting long-run household behavior.

2 Background

2.1 Definition and History of State Sales Taxes

A sales tax is a tax levied on the retail value of non-exempt goods and services and paid by the consumer. It is expressed as a percentage of the transaction value. For example, if there is a sales tax rate of 5 percent, then it would cost the consumer $1.05 to purchase a candy bar labeled as $1.00, with $1.00 going to the grocery store and the additional $0.05 being sent to the government. Many goods and services are subject to sales taxes, although there are exceptions. Groceries, inputs to production, prescription and non-prescription drugs are often exempt from sales taxes. In the United States, sales taxes are commonly levied by sub-national entities such as states, counties and municipalities. These sales tax rates are cumulative, so if a state has a sales tax rate of 4 percent and a city inside that state sets a local tax rate of 2 percent, then the effective tax rate in that city would be 6 percent.

The first state-level sales tax in the United State was levied by the state of Pennsylvania in 1821 and was known as the “mercantile license tax" (*State and Local Sales Taxes*, 1970). However, this tax did not apply to most transactions involving goods and services. A few states imposed broader state sales taxes in the 1920s, starting with West Virginia in 1921 though the first modern flat state sales tax in which most goods and services in a particular state were subject to taxation at a specified percentage of the transaction value was introduced Mississippi in 1932. The sales tax rate was set to 7.0 percent, and its original motivation was to increase state revenue during the Great Depression. By 1937, thirty more states had instituted a state sales tax. Ultimately, 45 states and the District of Columbia would introduce state sales taxes, with Vermont being the latest in 1969. State general sales tax rates have varied from state to state and from year to year, but none have been fully repealed since the 1960’s. Only Oregon, Montana, New Hampshire, Alaska and Delaware do not currently levy state-level sales taxes. Many municipalities, counties, parishes and cities also levy general sales and use taxes on goods and services sold within their jurisdiction, these are cumulative with any existing state general sales tax and are often passed by local jurisdictions rather than state legislatures.

State sales taxes can be changed in one of two ways: by state legislatures, or by popular referendum. The most common method is for the state legislature to pass a sales tax rate change, in which they specify both the new tax rate and the date at which it is going to take effect. These are often passed in response to budgetary pressures since state governments are not allowed to run a budget deficit and according to the United States Census of Governments, in 2017 sales taxes were the second largest contributors to overall state revenue, surpassed only by property taxes. These changes can either be increases, often instituted if the legislature needs revenue, or a decrease often proposed should the legislature expect a budget surplus. However, budgetary considerations are not always the motivation behind legislative state sales tax rate changes, ideology and economic conditions can also play a role in the decision making process. The other way state sales tax rates can change is through a popular referendum. A referendum includes both the proposed new state sales tax rate and the date when it would take effect. It is then voted on by the populace of the state, and if more supporting votes are cast than opposing, the measure takes effect as written. Because a referendum requires popular participation and there are often campaigns for and against the proposed change prior to the vote, it is likely that citizens are more aware of state sales tax rate changes caused by a passed referendum. Voters are informed by media coverage of the campaigns to pass or repeal the sales tax change, as well as by politicians, political activists and advertisements seeking to sway public opinion for or against the referendum measure (Christin et al., 2002; Mendelsohn & Parkin, 2001).

2.2 Current Sales Tax Environment in the United States

Current general sales tax rates by state can be found in Figure 1. For most states, the highest universal combined sales tax rate is equal to the state-level sales tax rates, with some but not all county and city governments often levying their own sales taxes exclusive to their jurisdictions. However, there are some states in which all counties levy a non-zero sales and use tax, making the state sales tax rate only a part of the minimum general sales tax rate that every consumer is subject to in that state. Current universal combined sales tax rates vary between 4.0 percent and 7.5 percent for those states that levy state sales taxes. Hawaii, Wyoming, Georgia, Alabama, and New York all have the lowest universal combined sales tax rates of 4.0 percent, while California has the highest at 7.5 percent.

Local jurisdictions such as counties and cities also frequently levy sales taxes. For reference, Tuskegee, AL and Gould, AR are tied for the highest combined sales tax rate - inclusive of state, county, and city sales tax rates - of 11.5 percent (*Sales Tax Handbook*, 2021) .

2.3 Effects of Sales Taxes

Price changes due to general state sales taxes happen very quickly, and state sales taxes are usually fully shifted to consumers (Poterba 1996; Besley and Rosen, 1999), although they can be overshifted for goods with relatively inelastic demands (Woodard and Seigelman, 1967; Besley and Rosen, 1999). Sales taxes are considered “hidden,” with consumers not knowing how much they are really being taxed until they are paying for their goods (Boyer and Russell, 1995). Sales taxes are regressive in nature, that is, they fall heavier on households with less income (Derrick and Scott, 1998).

Consumer responses to sales tax rate changes depend on both the particular goods being looked at and the ease of tax avoidance (Cornia et al., 2010). Consumers exhibit stockpiling behaviors and seek to avoid sales taxes by substituting towards less-taxed goods, or seeking out retailers that are not subject to sales taxes such as online retailers and those located across state boundaries (Einav et al., 2014; Zheng et al., 2019; Baker, Johnson, and Kueng, 2021). There are even consumption responses to temporary and brief sales tax holidays on specific types of goods, as sales tax holidays lead to increased spending on the covered goods that is not offset by decreased spending before or after the tax holiday (Agarwal, Marwell, and McGranahan, 2017).

Changing the exemption status of particular goods, such as bottled water and groceries, can cause short-term and long-term changes in consumption patterns (Berck et al., 2016; Srithongrung, 2017; Zheng et al., 2019). Similarly, changing the tax-exempt status of internet retailers is a relatively new phenomenon and one that leads consumers to change where they spend their money. Taxing Amazon.com in particular is found to increase spending at taxed internet competitors, but consumers do not shift consumption to physical retailers (Hossain, 2020), with large expenditures being particularly elastic (Baugh, Ben-David, and Park, 2018).

Local sales tax adoption is not a strictly random occurrence, and is spurred by fiscal stress and the ability to tax consumers that do not reside within the county or municipal government's jurisdiction (Burge and Piper, 2012). Sales taxes can have effects beyond just the government revenue and consumer behavioral response. A general, ubiquitous sales tax can cause unemployment and business depression in some instances (Brown, 1939). Higher state sales tax rates can decrease retail employment in areas that border states with lower sales tax rates (Thompson and Rohlin, 2012), although local sales taxes can lead counties and municipal governments to favor retail businesses over manufacturing to maximize revenues (Burnes, Neumark, and White, 2014). Finally, while overall general sales taxes are efficient and broad-based, in some states the tax base for sales taxes has been eroded over time due to secular trends in spending and manufacturing (Russo, 2005).

2.4 International Consumer-Facing Taxes

Much of the work on consumer-facing taxes has focused on national value-added taxes. A value-added tax is a tax paid at each stage of the production process according to the value-added between stages in the production process. The cost of the tax is passed on to the receiver of the good, ultimately borne by the final consumer. For example, if a VAT is 10 percent, then a corn farmer would charge $1 for corn and receive $1.10 from a chef. The farmer would then give $0.10 to the government. For the next stage, the chef could charge customers $2.50 for corn on the cob before tax, the customer would pay $2.75, and the chef would give $0.15 to the government. Under a flat national sales tax, the farmer would only receive $1 for the corn since the chef does not have to pay sales tax on an input, but the customer would still have to pay $2.75 for the corn on the cob being sold at $2.50. While VATs prevent the possibility of the same good from being taxed multiple times ultimately the entirety of the tax is passed along to the final consumer making it indistinguishable from a sales tax from the consumers standpoint. Most VATs are collected via the credit method (Ebrill et al., 2001).

One area of intense empirical research is when and to what extent a consumption tax is fully passed through via price changes to consumers such that consumers pay for the entirety of the tax, overshifted such that consumers pay for more than the tax, or undershifted such that consumers pay less than the full amount of the tax. National-level value-added rate increases are found to be fully passed through to consumers in countries in the European Union (Benedek et al., 2020; Buettner and Madzharova, 2021), and fully passed-through or overshifted to consumers in Canada (Smart and Bird, 2009) and the United Kingdom (Lyssiotou and Savva, 2021; Chirakijja et al., 2009). The evidence on value-added tax cuts is more mixed, sometimes undershifted to consumers with little effect on consumer behavior (Blundell, 2009) and sometimes fully passed through and salient (Chirakijja et al., 2009). Value-added taxes are particularly salient to consumers when it comes to expenditures on durable and storable goods (Chirakijja et al., 2009; Cashin and Unayama, 2021). Generally, VATs are found to be efficient sources of revenue with low compliance costs and little incentive to change expenditure behavior (Crossley, Low, and Wakefield, 2009; Lee, Kim, and Borcherding, 2013) though they can have larger than anticipated behavioral effects (Jansky, 2013). Expected VAT rate changes can lead to changes in purchasing patterns of durable and storable goods (Chirakijja et al., 2009; Cashin and Unayama, 2021). Like state sales taxes, VAT incidence is relatively regressive since lower-income households generally spend a higher proportion of their wealth on consumption. In the short-term, this is exacerbated for announced VAT rate changes since wealthier households tend to be more informed of tax rate changes and have a greater ability to shift their spending intertemporally (Crossley, Low, and Wakefield, 2009). Long-run effects of VAT changes are generally found to be negligible (Blundell, 2009; Crossley, Low, and Wakefield, 2009; Buettner and Madzharova, 2021; Cashin and Unayama, 2021) although Buettner and Madzarova (2021) detected a permanent decrease in unit sales. Less work has been done examining the supply-side impact of VATs, though Velayudhan (2018) found that producers in India responded to increased compliance costs, not VAT rate changes.

Not only do these studies examine an alternative form of taxation in other countries, but VAT rates are often much higher than general sales tax rates levied by states in the United States. For example, value-added taxes in the European Union vary between 17 percent in Luxembourg to 27 percent in Hungary with a mean of 21.5 percent, substantially higher than the even the highest combined local sales tax rates in the US (11.5 percent).

2.5 Excise taxes

There are a number of excise taxes – or taxes levied on the consumer based on the amount of a specific good purchased - in the United States at the federal, state, and local levels; one of the most widely studied of these is the gasoline tax. The gasoline tax is a set tax levied on the consumer per gallon in a state, is displayed as part of the "sticker" price for gasoline and is a combination of state and federal gasoline taxes. Gasoline tax increases are found to be fully passed-through to the consumer and the prices shift almost immediately (Alm et al., 2009; Doyle & Samphantharak, 2008), although gasoline tax decreases are undershifted (Doyle & Samphantharak, 2008; Yilmazkuday, 2017). Consumers are more responsive to changes in the gasoline tax than to commensurate changes in the total price of gas (Li et al., 2014).

In the United State, the federal government and all state governments as well as some local governments levy excise taxes on cigarette and tobacco sales. Expected increases to the excise tax rate on tobacco induces stockpiling behavior among consumers prior to the rate change, and shifting to cheaper, lower-tier cigarette brands immediately after the change (Chiou & Muehlegger, 2014). Substantially higher tobacco tax rates can also induce consumers to purchase cigarettes out of state, or from unlicensed cigarette "smugglers" who can purchase cigarettes across state lines and illicitly sell them at lower prices than licensed retailers (Chernick & Merriman, 2013; Merriman, 2010; Wang et al., 2019). Finally, state alcohol excise taxes are found to be overshifted to the consumer, and prices quickly adjust to reflect the tax change (Kenkel, 2005; Young & Bielińska-Kwapisz, 2002).

3 Data

The primary data source I use in my empirical analysis is the interview portion of the Consumer Expenditure Survey (CEX), conducted by the United States Bureau of Labor Statistics (BLS) from the years 2005-2017. The CEX contains expenditure information by category for a nationally representative sample of the civilian non-institutionalized population, made up of approximately 7,000 households. This data set is semi-panel in nature, with each household being in the sample for five quarters. The survey is given each quarter, staggered such that one-third of the panel is surveyed every month. The CEX contains information on about 70 percent of a household's expenditures and includes the dollar amount, month, and spending category for each relevant transaction that a household completed over the preceding three months. In addition, the CEX contains demographic information for every household, including number of occupants, relation and age of each occupant, annual pre-tax income, race and sex of each occupant, location (e.g., if the household is in an urban area and in which state each household is located), and the maximum educational attainment for each occupant. Survey weights are included to make the sample nationally representative[3]. I aggregate this data set to the state-level for the analysis, as that addresses potential concerns of incomplete response observations, and is the level at which the sales tax rates are determined. As such, the final data set is made up of state-by-month observations for forty states over twelve years.

I supplement the CEX with tax rate information for each state by month, recorded manually from official state government records accessed via the internet from each state's department of revenue for the years 2005-2017[4]. I use the state general sales tax rate as the measure of interest because state sales taxes tend to be more widely known and are more difficult to avoid than local and special sales taxes as they apply to a much broader area.

In this sample, there are 17 sales tax increases averaging an 11 percent increase compared to the previous rate, 3 of which were mandated by popular referendums, and 8 state sales tax rate decreases averaging 6.7 percent of the previous rate, 2 of which were mandated by referendum (see Appendix A).

[3] Not all households have state identifiers associated with them, as state identifiers for states with smaller populations are repressed in the public data for privacy reasons. There are ten states missing state identifiers in at least one year of the CEX between the 2005 and 2017 (inclusive) CEX interview surveys: Idaho, Iowa, Maine, Mississippi, North Carolina, Oklahoma, Rhode Island, South Dakota, West Virginia, and Wyoming .

[4] In this sample, California has the highest sales tax rate of 8.25 percent from April 2009 through May of 2011. At the other end of the spectrum, Montana, Delaware, Oregon, and New Hampshire do not levy any sales taxes at all during this period so are considered to have effective sales tax rates of 0 percent

4 Empirical Methodology

To estimate the effect of state-level sales tax rates on overall consumer expenditure, I run an ordinary least-squares regression of the form:

$$Ln(Exp_{ts}) = \beta \, Taxrate_{ts} + \delta \, TaxHoliday_{ts} + X_{ts} + \sum_{i=1}^{S} \rho_{is} + \sum_{i=1}^{M} \sigma_{it} + \varepsilon_{ts} \qquad (1)$$

Ln(Exp) is the natural logarithm of the relevant expenditure category in real terms for each state *s* in month *t*. *Taxrate* is the relevant tax rate variable for state *s* in month *t*, and is the primary variable of interest. *TaxHoliday* is a dummy variable equal to one if there was a sales tax holiday of any kind for any duration in state *s* during month *t*. *X* is a vector of control variables, which contains variables for percent of households with: less than a high school education, a bachelors degree, bachelors equivalent or postdoctoral degree. *X* also includes the percent of households that are in rural areas, single male households single woman households, entirely black households, and entirely Hispanic households. It also includes the mean age of the heads of households, family size and natural logarithm of household income. ρ is a vector of state-level fixed effects to control for time invariant, state-specific effects on expenditures. σ is a vector of size M month fixed effects (i.e, August 2004) to control for month-level shocks that effect all states equally. ε_{ts} is the error term, clustered at the state level since that is the level of policy variation being examined. Because of inconsistent response rates and the semi-panel nature of the data, the data are treated as repeated cross-sections in the analysis. Both the state sales tax rate and the universal minimum sales tax rate are used, though in different regressions.

While the previous equation estimates the overall effect that state sales tax rates have on monthly consumer expenditures, another area of interest is the short-run impact of a state sales tax rate change i.e. do consumers hoard prior to a sales tax increase, and if so to what extent. To estimate the effect of sales tax changes on consumer expenditures in the month before and the month after the change takes effect, the following regression is run:

$$Ln(Exp_{ts}) = \sum_{i=1}^{n} \beta_i TaxDummies_{ts} + \sum_{i=1}^{n} \theta_i TaxDummies_{ts} * Ref_{ts}$$
$$+ \delta\, TaxHoliday_{ts} + X_{ts} + \sum_{i=1}^{S} \rho_s + \sum_{i=1}^{M} \sigma_i + \varepsilon_{ts} \qquad (2)$$

which is identical to the previous equation except that the variable of interest *Taxrate* has been replaced by the vectors *TaxDummies* and *RefDummies*. *TaxDummies* is a vector of four dummy variables. *PostInc* is equal to one if there was a tax increase in the state at beginning of the month, otherwise zero. *PostDec* is equal to one if there was a tax decrease in the state at the beginning of the month, otherwise equal to zero. *PreInc* is equal to one if there was a tax increase in the state in the next month, otherwise equal to zero. *PreDec* is equal to one if there was a tax decrease in the state in the next month, otherwise equal to zero. *Ref* is a dummy variable coded to one if the sales tax change in state *s* at time *t* was passed as a popular referendum, and zero otherwise. This referendum dummy variable multiplies each of the dummy variables in *TaxDummies* to create the *TaxDummies*Ref* vector of interaction terms.

5 Results

5.1 The Effect of State Sales Tax Rates on Overall Spending

Results for mean total household consumption expenditure are found in Table 2. Various specifications are estimated in the different models. The regression results in model (1) only include the state sales tax rate as a independent variable. Models (2) and (3) add economic, demographic, and geographic control variables, while model (3) adds month-by-year fixed-effects. Model (3) is the preferred specification as it is the most precise and controls for shocks constant across states during each month, as well as a variety of variables that are likely associated with household spending. Throughout all of the models, the coefficient for the tax rate is negative but statistically insignificant, indicative of a relatively precise null result, even though model (3) is only powered to detect changes greater than a 1.1 percent change in mean total expenditures (see Appendix B). The log of mean annual household income is positively associated with increased expenditure as would be expected; because both variables are logged, the coefficient can be interpreted as an elasticity. In the preferred model, a 10 percent increase in mean annual household income is associated with a 5.6 percent increase in consumer spending. Increased education is also positively associated with increased expenditures. Coefficients of non-logged variables can be interpreted as semi-elasticities, with an increase of 10 percentage points in households in which a bachelors degree is the highest level of educational achievement leading to a 2.1 percent increase in mean consumer expenditure on taxable goods and services. Similarly, a 10 percentage point increase in households in which a graduate degree is the highest level of educational achievement is associated with a 3.35 percent increase in consumer spending on taxable goods. An increase in the mean family size by one person is associated with a 9.5 percent increase in mean consumer expenditure on goods subject to the state sales tax. Race is found to be significant, with a 10 percent increase in the percentage of entirely Black households corresponding to a -2.2 percent change in mean total expenditure, and a 10 percent increase in the percentage of entirely Hispanic households corresponding to a 1.6 percent increase in mean total expenditures. Neither the percentage of rural households nor the presence of a sales tax holiday is found to have a statistically significant effect on total consumer expenditure in the preferred model.

The natural logarithm of mean consumer expenditure on taxable goods is used as the dependent variable in Table 3. The narrow definition of taxable goods is used, so this is spending on all goods and services to which sales taxes apply in every state in which a sales tax is levied, guaranteeing that categories of goods included are constant across states and time. Model (1) only includes the state sales tax rate as a dependent variable, models (2) and (3) add economic, demographic, and geographic controls, and model (3) adds year-by-month fixed-effects; model (3) is the preferred specification. The state sales tax rate is inconsistent in sign, being negative in the first model and positive in models (2) and (3), though it is not statistically significant in any of the models and is found to be a precise null in the preferred specification. A 10 percent increase in mean state income is found to correspond to a 6.2 percent increase in mean expenditure on taxable goods. A 10 percentage point increase in the share of Black households is found to correspond to a 5.2 percent decrease in mean expenditure on taxable goods. An increase in the mean family size by one corresponds to a 10 percent increase in mean expenditure on taxable goods. Marital status composition, mean age, educational attainment, the presence of a tax holiday, and the percent of the households that identify entirely as Hispanic are not found to have statistically significant impacts on mean consumption spending on taxable goods.

The natural logarithm of mean consumer expenditure on durable goods is used as the dependent variable in Table 4. Model (1) only includes the state sales tax rate as a dependent variable, models (2) and (3) add economic, demographic and geographic controls and model (3) adds year-by-month fixed-effects; model (3) is the preferred specification. The state tax rate is always positively correlated with consumer spending on durable goods, but is not found to be statistically significant. A 10 percent increase in mean state income would lead to an estimated 6.0 percent increase in spending on durable goods. A 10 percentage point increase in the share of households that identify strictly as Black is correlated with an 8.2 percent decrease in spending on durable goods. An increase in the mean number of family members by one leads to an 18 percent increase in consumer spending on durable goods. All other estimates are found to be statistically insignificant. Higher percentages of households with less than a high school education and at most some college are found to be correlated with higher spending on durable goods, while higher percentages of households with bachelors and/or postgraduate degrees spend less. Higher shares of single woman, single man and rural households is associated with lower spending on durable goods. The presence of a tax holiday is positively related to consumer spending on durable goods, while mean age of the household heads does not seem to have an impact.

The estimates for sales tax rate are relatively small and not statistically significant for all types of expenditures in all models. This aligns with previous empirical work that found many consumers do not take taxes that are not factored into the list price into consideration when making consumption decisions.

5.2 Temporal Consumption Responses to Changes in State Sales Tax Rates

Tables 5, 6, and 7 contain the regression results for the second empirical model, in which the dummy variables for months before or after a state sales tax decrease or increase are the variables of interest. These can be interpreted as the difference in mean total spending. Table 4 uses the natural logarithm of total expenditure as the dependent variable, so all estimates can be interpreted as semi-elasticities. Model (1) only includes the variables of interest as independent variables, model (2) adds in demographic and economic controls, model (3) adds in the referendum dummy variables, and model (4) includes state and month-by-year fixed effects; model (4) is the preferred specification. The only statistically significant effect in model (4) is that mean total household expenditure falls about 7.7 percent the month after a sales tax increase occurs due to a referendum. The signs for pre-tax rate increase, pre-tax rate decrease, post-tax rate increase, post-tax rate decrease, and referendum are all positive and statistically insignificant. The signs for post-tax rate decrease, pre-tax rate increase, referendum, pre-tax rate decrease, and referendum are all negative and statistically insignificant. Because the tax rate change dummy variables are equal to one whenever the corresponding referendum tax rate change dummy variable is equal to one, the mean change in consumer expenditures due to a sales tax change voted in by a referendum is calculated by summing the corresponding referendum and straight sales tax change dummy estimates.

The regression results with the natural logarithm of taxable expenditures, narrow as the dependent variable and the pre-and-post sales tax rate increase/decrease dummy variables as the variables of interest can be found in Table 6. Model (1) only includes the variables of interest as independent variables, model (2) adds in demographic and economic controls, model (3) adds in the referendum dummy variables, and model (4) includes state and month-by-year fixed effects. Model (4) is the preferred specification….. For the preferred specification, all of the dummy variables both referendum and are statistically insignificant, with pre-tax rate increase, post tax rate increase, pre-tax rate decrease, post-tax rate decrease, and referendum estimates all being positive. Post-tax rate decrease, pre-tax rate increase referendum, post-tax rate increase-referendum, and pre-tax rate decrease-referendum are all negative.

The regression results with the natural logarithm of expenditures durable goods as the dependent variable and the pre-and-post sales tax rate increase/decrease dummy variables as the variables of interest can be found in Table 7. Model (1) only includes the variables of interest as independent variables, model (2) adds in demographic and economic controls, model (3) adds in the referendum dummy variables, and model (4) adds state and month-by-year fixed effects. Model (4) is the preferred specification….. Like the results for other dependent variables, the only statistically significant estimate is being in a month after a sales tax increase that was mandated by a popular vote. This is found to result in a 46.5 percent decrease in consumer spending on durable goods relative to a non-referendum tax rate decrease. Pre- and post-tax rate increase and pre-tax rate decreases are found to be positively related to consumer spending on durable goods, while post-tax rate decrease is found to be negatively related to consumer spending on durable goods. Being caused by a referendum is found to be negatively associated with spending on durable goods immediately prior to a sales tax rate increase, and positively associated with spending on durable goods immediately prior and immediately after a sales tax rate decrease.

Because the straight tax rate change dummy variables can be interpreted as the percent change in expenditure due to being in a state the month before or after a tax rate change relative to not experiencing that shock, the difference-in-differences between the treated and untreated states between the pre- and post- periods can be calculated as the difference between the “pre” and “post” dummy variables. If this is positive, then the difference between the treated and untreated group is higher immediately prior to the tax change than immediately after, so an overall increase between the pre- and post-periods relative to the control group would be expected. If the difference-in-differences calculated is negative, then the difference between the treated and untreated group is higher immediately before the tax change than afterwards, so a decrease between the pre- and post-periods relative to the control group would be expected.

Table 8 contains the difference-in-differences estimates derived from Tables 5, 6, and 7. The estimates are constructed by taking the difference in relevant temporal dummy variables (i.e. Post non-referendum tax rate increase – Pre non-referendum tax rate increase), as the dummy variables are estimates of the differences between the treated and untreated states at a given distance from a tax change. The estimates can be interpreted as difference in percentage points between the month before and the month after a state sales tax rate change. None of the estimates are statistically significant but have large standard deviations, making the estimates suggestive. The difference-in-differences estimates for total expenditure are universally negative, suggesting that all sales tax rate changes regardless of method of approval result in lower overall consumer expenditure in the post period relative to the month before.

Unlike the estimates for total expenditure, both the change in consumer expenditure on taxable goods between the month before and month after the tax change is still negative for both referendum and non-referendum sales tax rate increase as well as non-referendum sales tax rate decreases. Even so, for sales tax rate decreases that occurred due to a passed referendum, the difference-in-differences estimate is positive. This is also true for expenditure on durable goods. This implies that there is an increase in consumer expenditure on taxable and durable goods relative to the states that did not change their tax rates that month from immediately before the sales tax rate change to immediately after, which is how consumers are likely to react to a known tax rate change. Also, consumers seem to act as expected towards sales tax rate increases and announced decreases, with higher spending relative to the control group prior to the sales tax rate increase than after. That state sales tax rate changes caused by referendums have a stronger effect on consumer behavior aligns with the hypothesis that referendums increase the public knowledge of both the type and date of the sales tax rate change.

6 Robustness Checks

6.1 Broad Taxable Expenditures

Because the narrow definition of taxable expenditure naturally does not include all taxable expenditure in each state but rather maintains equal categories for comparability, there is a concern that some effects are being missed. Both temporal and state tax rate regressions are run, results can be found in Appendix B. The sign and significance of most variables are the same in both definitions of taxable expenditures, except the effect of being a month after a sales tax rate decrease is found to be positive for the broader definition and negative in the narrow. Also, the effect of the general sales tax rate on taxable expenditure is negative, but statistically insignificant.

6.2 Universal Minimum State Sales Tax Rate

Because California, Utah, Nevada and Virginia all levy universal county taxes legislated at the state level and these are seen by all consumers in the state in the same manner as the state sales tax rate with the only difference being how the state allocates the revenue, the same empirical methods were run but using this universal minimum state sales tax rate rather than just the state sales tax rates in those states with this tax policy. Results can be found in Appendix C. The signs are identical to the regression done with state sales tax rates, the magnitudes are similar and the estimates are also statistically insignificant.

6.3 Omit States

In North Carolina, New Mexico, Tennessee, and Georgia all counties levy independent local sales taxes, so the state sales tax rate is lower than that facing all consumers in that state. However, the local sales tax rates vary county-by-county while the identifiers in our data only match households to the state-level, preventing more accurate geo-matching. As such, for these four states the state sales tax rate is used in the primary analysis since only changes in the state sales tax rate is certain to affect all households in that state. As a robustness check, the analysis is run excluding these states from the sample; the results can be found in Appendix D. The estimates from both regressions are similar in magnitude and identical in sign to corresponding estimates in the unrestricted model. The post-tax rate decrease, referendum coefficient is the only statistically significant estimate of all the temporal dummies, and the sales tax rate is never statistically significant.

6.4 Unweighted Analysis

analysis is reconducted over the unweighted sample, results can be found in Appendix E. The state sales tax estimates are very close in magnitude, identical in sign, and not statistically significant. The temporal dummy variable estimates are identical in sign and similar in magnitude to the weighted regression, but the post-tax rate decrease, referendum coefficient is not found to be statistically significant.

7 Day-level analysis

7.1 Data

The Diary section of the Consumer Expenditure Survey conducted by the United State Census Bureau is the companion dataset to the interview portion. Like the interview portion, the diary data also ask consumers their spending habits, and the survey is conducted over a representative sample of United States households. However, it is conducted over a smaller and independent sample of households, and it is aimed at collecting data on small, frequently bought items like food and clothing and contains information on about 30 percent of household expenditures during that period. Each household included in the sample keeps a diary of the purchases made for two weeks, and then drops out of the sample. Like the interview portion, there is demographic information on each household, including race, annual income, family size, marital status, and maximum educational attainment for both primary respondent and spouses (where applicable). Since the diary survey is designed to examine only 30 percent of household spending, especially smaller repeat purchases and larger purchases such as furniture and vehicles are not usually captured in this dataset. However, from January 1st 2005 through December 31st 2011 I have the exact day of every transaction which allows examination of consumer responses in the days and weeks immediately preceding and immediately following sales tax rate changes. The dataset also contains the first day of each survey week for every household included in the sample. While the original observations are transaction-level, they have been reshaped into household-by-day observations, including those days in the survey in which no transactions were reported.

All prices are in real 2018 USD using the consumer price index from the FRED, dates and states for tax holidays from 2007-2011 are from the Tax Policy Center, and for 2005-2006 these dates are from Cole (2008). State sales tax rates by state and date are taken from state revenue department and state legislature websites.

Total expenditure is the sum of all household expenditure in a given day. Taxable expenditure is the sum of all expenditure that a household made in categories subject to sales tax in their state of residence. Demographic variables are treated as follows: educational attainment is a series of dummy variables indicating the highest educational level attained by the primary reference person or their spouse, if applicable. The possible categories are less than high school, high school, some college, bachelors, and postgraduate. For example, if the respondent completed some college but did not receive a bachelors degree and the spouse has a postgraduate degree, then the *Postgraduate* dummy variable would be coded as a one, and the rest of the variables would be equal to zero. Race demographic variables are dummy variables equal to one if both the spouse (where applicable) and the primary respondent are the same race, and zero otherwise. The possible categories are Hispanic, non-Hispanic White, non-Hispanic Black, and other. Marital status dummy variables are constructed as follows: *MaritalStatus* is equal to one if the primary respondent reports a spouse, else zero. *SingleMan* is equal to one if the primary respondent does not report a spouse and identifies as male. *SingleWoman* is equal to one if the primary respondent does not report a spouse and identifies as female. Summary statistics are in Table 9.

In this sample, there are 15 state sales tax increases averaging 0.73 percentage points, about a 13.5 mean percent change, and 3 state sales tax rate decreases averaging 0.71 percentage points, which is a 10.7 mean percent change. There are two sales tax rate increases due to referendums, and no sales tax rate decreases caused by referendums.

7.2 Empirical Methodology

To examine the effects of sales tax rates on small routine consumer expenditures, the following regression is run for household h on day t in state s:

$$Exp_{ht} = \beta\, StateTaxRate_{ts} + \delta\, TaxHoliday_{ts} + \sum_{i=1}^{C} \gamma_i\, X_h + \sum_{t=1}^{T} \sigma_t\, Day_t + \varepsilon_{ht} \quad (3)$$

where Exp is the expenditure category of interest, either total expenditure or taxable expenditure. Unlike in the previous section, this variable is not in logarithm form since about a third of observations are equal to zero, and not including them would introduce bias into the estimation. *TaxHoliday* is a dummy variable equal to one if state s has a sales tax holiday of any kind occurring on day t, else zero. X is a vector of household-level demographic and economic controls, including maximum educational attainment, race, income, marital status and family size. *Day* is a vector of fixed effects for every date in the sample (i.e. May 4th, 2009) omitting January 1st, 2005. The error term ε is clustered at the state level since that is the level of policy variation.

To examine the effects of sales tax changes on the timing of small, routine consumer expenditures, the following regression is run for household h on day t in state s:

$$Exp_{ht} = \sum_{i=1}^{8} \beta_i WeeklyTaxDummies_{ts} + \sum_{i=1}^{8} \theta_i RefDummies_{ts} + \delta\, TaxHoliday_{ts}$$

$$+ \sum_{i=1}^{C} \gamma_i\, X_h + \sum_{s=1}^{S} \rho\, State_s + \sum_{t=1}^{T} \sigma_t\, Day_t + \varepsilon_{ht} \quad (4)$$

The variables *Exp*, *X*, *TaxHoliday*, and *Day* are treated the same as in the previous regression. *WeeklyTaxDummies* is a vector of dummy variables that is constructed depending on how far away from a state sales rate tax change a household-day observation is, and what kind of change occurs. *PreIncrease4* is a dummy variable equal to one if the household-day observation occurs between 22 and 28 days prior to a sales tax increase (inclusive), and zero otherwise. Similarly, *PostIncrease1* is a dummy variable equal to one if the household-day observation occurs up to 7 days after a sales tax decrease. These dummy variables are constructed for up to four weeks before and after the change, for both sales tax decreases and increases. *RefDummies* is a vector of referendum dummy variables is also constructed, which are equal to one if both the corresponding weekly tax dummy variable is equal to one and the sales tax change is due to a referendum, and zero otherwise. *State* is a vector of state fixed-effects, omitting Alabama. The error term ε is clustered at the state level since that is the level of policy variation.

7.3 Results

Sales tax rates

Results for the model examining state sales tax rates can be found in Table 10. The first column contains results for total daily household expenditures, and the second total household expenditure subject to general state sales taxes. For total expenditure, if the maximum educational attainment of a household is some college, expenditure increases by 13.0 percent, while having at most an undergraduate degree increases daily expenditure by 25.7 percent and postgraduate households spend 36.3 percent more on average. Married couples spend 10.4 percent more than their single counterparts, Hispanic households spend 6.9 percent less than white households, and all-black households spend 14.8 percent less. An increase in the mean age of primary householders by one year is correlated with an increase in daily spending of 0.4 percent. An increase in annual income of $1,000 corresponds to an increase in spending of 0.6 percent, while each additional member of the household increases spending by 6.8 percent. Tax holidays and the size of the state sales tax rate are found to be positive correlated with total spending, but their impact is statistically insignificant. Being in a rural area and having a maximum educational attainment level of less than high school are negatively associated with total spending, but these coefficients are not statistically significant.

The results for total expenditure on categories subject to general sales taxes are found in the second column of Table 10. For taxable expenditure, if the maximum educational attainment of a household is some college, expenditure increases by 20.6 percent, while having at most an undergraduate degree increases daily taxable expenditure by 37.4 percent and postgraduate households spend 59.6 percent more on average. Married couples spend 13.1 percent more than their single counterparts on taxable goods and services, Hispanic households spend 14.9 percent less than white households, and all-black households spend 22.8 percent less. An increase in the mean age of primary householders by one year is correlated with an increase in daily spending of 0.4 percent. An increase in annual income of $1,000 corresponds to an increase in daily spending on taxable goods and services of 9.4 percent, while an additional member of the household increases spending on taxable goods and services by 5.4 percent. Tax holidays are found to be positive correlated with taxable expenditure, but their impact is statistically insignificant. The size of the state sales tax rate, being in a rural area, and having a maximum educational attainment level of less than high school are negatively associated with taxable expenditure, but are not found to be statistically significant.

The estimates for sales tax rate are relatively small and not statistically significant for all types of expenditures in all models. This aligns with previous empirical work that found many consumers do not take taxes that are not factored into the list price into consideration when making consumption decisions.

Temporal analysis

The results for the temporal analysis regressions can be found in Table 11. The first column contains the model in which total expenditure is used as the dependent variable. Estimates for all dummy variables prior to an increase are positive and not statistically significant. Values for the dummy variables one and two weeks prior to a sales tax rate decrease are positive and statistically insignificant, values for the dummy variables three and four weeks prior to the state sales tax rate decrease are negative and statistically insignificant. The effect one and three weeks after a tax increase is found to be positive and not statistically significant. The effect two and four weeks after a tax increase are found to be negative and not statistically significant. The first week after a tax decrease is found to have a positive, not statistically significant effect on total expenditure. The second week after a sales tax rate decrease is correlated with a 44.3 percent increase in total expenditure, the third week is correlated with a 176.7 percent increase in total expenditure, and the fourth week is correlated with a 30.1 percent decrease in total expenditure. The weeks prior to a tax decrease caused by a referendum are found to be negatively associated with total consumer expenditure. Being one week prior to the referendum-mandated tax increase is found to be correlated with a 42.0 percent decrease in consumer expenditure. Being two weeks before is not found to be statistically significant. Three weeks before is associated with a decrease in expenditure of 63.5 percent, and four weeks before is associated with a decrease of 62.5 percent. The results for the weeks immediately after a tax increase caused by a referendum are mixed in sign and statistically insignificant.

The second column of Table 11 contains the results for the regression model examining temporal responses with respect to a tax increase or decrease in which taxable expenditure is used as the dependent variable. The signs are mixed and not statistically significant for all dummies prior to a sales tax rate increase, prior to a sales tax decrease, after a sales tax rate increase and before a sales tax rate increase caused by a referendum. The weekly dummies for the first three weeks after a tax rate decrease are positive, with the second and third weeks related to sizable increases of taxable expenditure of 158.4 percent and 824.0 percent, respectively. Being four weeks after a sales tax rate decrease is associated with a decrease in taxable expenditure of 32.9 percent. This suggests that there is an increase in taxable expenditure in the first three weeks after the tax rate decrease, and a reversion to the mean that starts in the fourth week. All the weekly dummies for the weeks after a sales tax rate increase caused by a referendum are negative, with the second and third weeks being statistically significant. The second week after a sales tax increase caused by a referendum is associated with a 54.0 percent decrease in taxable expenditure, while the third week is associated with a 32 percent decrease in taxable expenditure. This is indicative of households spending less on taxable goods and services after a tax rate increase, though this effect is strongest in the second and third weeks and is weaker in the first and fourth.

8 Conclusions

Sales tax rates are not statistically significant for any of the regressions, and this holds true across expenditure categories and type of data utilized, suggesting that consumer expenditure is independent of state sales tax rates for the states examined from 2005-2017 since the estimates are neither large nor statistically significant. This is in-line with previous empirical and theoretical work (Chetty, 2009; Sheshinski, 2003) that suggests that consumers are not attentive to taxes that are not included in posted prices. Given the lack of behavioral response, general sales taxes are likely an efficient source of revenue for state governments, though the tax is potentially regressive in nature (see Appendix H). Temporal results are more mixed, with consumer responses to sales tax changes passed by legislatures having almost entirely statistically insignificant estimates. While weak evidence is found of stockpiling behavior prior to a sales tax rate increase, there is suggestive evidence that sales tax rate increases cause a decrease in spending in the month immediately after the increase relative to the month before. However, there is likely a consumer response to sales tax rate increases caused by referendum, which are associated with lower total, taxable and durable goods expenditures immediately after a sales tax rate increase, consistent with neoclassical theoretical predictions though this is not offset by spending in the month prior to the increase. The temporal responses to sales tax rate decreases caused by referendums are not found to be statistically significant, though if anything suggest an increase in expenditure on taxable and durable goods in the month after the sales tax rate change compared to the month before. Much of the response is likely driven by consumer responses in the second and third weeks after the tax rate change, potentially because after one week of experiencing higher taxes, households change short-run behavior. The models employed do not include controls for contemporaneous state-level tax or welfare changes that could directly or indirectly affect household expenditures potentially biasing estimates. All the effects found are of higher magnitude among durable goods and taxable goods than overall expenditure, suggesting consumers are well aware of what is and is not subject to sales taxes.

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Tables and Figures

Figure 1: General state sales tax rates in the United States, 2021.

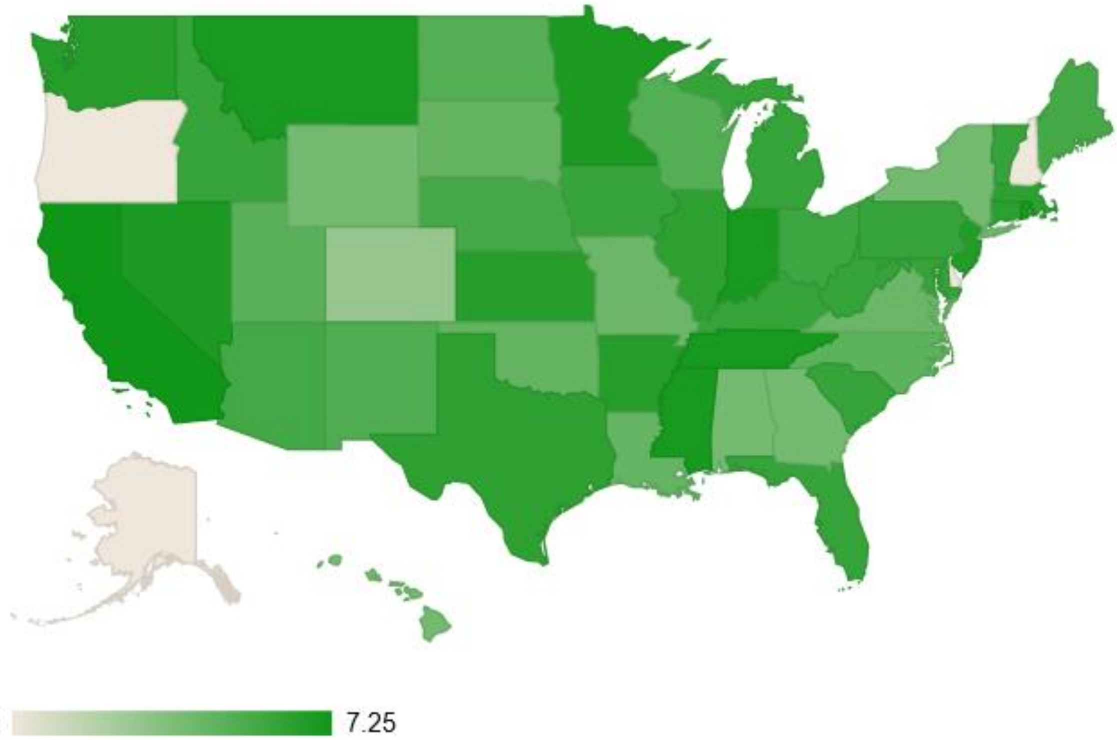

Table 1: Summary statistics for all states, weighted. Month-by-state observations from the interview portion of the CEX.

	mean	sd
Total expenditure	3,284.834	903.498
Taxable expenditure, narrow	917.516	404.442
Taxable expenditure, broad	1,025.259	438.607
Expenditure on durable goods	454.780	332.782
Mean annual household income (1,000s 2018 USD)	64.065	15.187
Percent less than high school	0.092	0.048
Percent high school	0.208	0.068
Percent some college	0.310	0.077
Percent bachelors	0.232	0.068
Percent postgrad	0.158	0.067
Percent rural households	0.017	0.073
Percent married households	0.512	0.083
Percent single woman households	0.289	0.066
Percent single man households	0.199	0.057
Percent all non-Hispanic White	0.686	0.166
Percent all Hispanic	0.081	0.085
Percent all non-Hispanic Black	0.129	0.117
Mean age	49.023	3.110
Mean family size	2.481	0.254
State sales tax rate	4.791	1.991
Percent of months with sales tax increases	0.003	0.056
Percent of months with sales tax decreases	0.001	0.038
Observations	5,495	

Note that all percent variables are coded from 0 to 1, with 1 being 100 percent and 0 being zero percent with the exception of sales tax rates which are coded directly as percentages, so a 5 percent sales tax rate would be coded as a 5 in the data set. All dollar variables are in 2018 USD adjusted dollars using the consumer price index provided by the United State Bureau of Labor Statistics

Table 2 : Regression results. Natural log of mean total expenditures is dependent variable

	(1)	(2)	(3)
State sales tax rate	-0.015	-0.001	-0.004
	(0.009)	(0.003)	(0.003)
Percent less than high school		-0.223*	-0.144
		(0.129)	(0.134)
Percent some college		0.400***	0.148*
		(0.128)	(0.085)
Percent Bachelors		0.297***	0.208**
		(0.101)	(0.093)
Percent postgraduate		0.221	0.335***
		(0.138)	(0.095)
Percent rural households		0.118	-0.073
		(0.075)	(0.071)
Percent single woman households		0.195**	-0.004
		(0.088)	(0.057)
Percent single man households		0.318***	0.007
		(0.085)	(0.053)
Percent Hispanic		0.350***	0.157**
		(0.080)	(0.071)
Percent Black		-0.147**	-0.223***
		(0.071)	(0.053)
Percent other race/ethnicity		-0.182***	-0.085*
		(0.045)	(0.047)
Mean age		0.011***	0.004*
		(0.002)	(0.002)
Ln(Income)		0.790***	0.560***
		(0.042)	(0.043)
Mean family size		0.095**	0.095***
		(0.037)	(0.024)
Tax holiday		0.044***	0.003
		(0.015)	(0.017)
Year and Month FEs	No	No	Yes
R^2	0.014	0.607	0.740
N	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3 : Regression results. Natural log of taxable expenditure, narrow definition† is dependent variable

	(1)	(2)	(3)
State sales tax rate	-0.012	0.003	0.000
	(0.011)	(0.005)	(0.006)
Percent less than high school		-0.012	0.067
		(0.276)	(0.294)
Percent some college		0.580^{***}	0.272
		(0.140)	(0.168)
Percent Bachelors		0.208	0.071
		(0.141)	(0.159)
Percent postgraduate		0.163	0.273^{*}
		(0.178)	(0.157)
Percent rural households		0.175	-0.085
		(0.151)	(0.143)
Percent single woman households		0.214	-0.027
		(0.168)	(0.142)
Percent single man households		0.290^{**}	-0.069
		(0.124)	(0.109)
Percent Hispanic		0.324^{*}	0.082
		(0.160)	(0.163)
Percent Black		-0.423^{**}	-0.518^{***}
		(0.172)	(0.155)
Percent other race/ethnicity		-0.359^{***}	-0.243^{***}
		(0.067)	(0.072)
Mean age		0.010^{***}	0.001
		(0.003)	(0.003)
Ln(Income)		0.882^{***}	0.621^{***}
		(0.069)	(0.064)
Mean family size		0.096^{*}	0.099^{**}
		(0.051)	(0.044)
Tax holiday		0.041	-0.005
		(0.031)	(0.032)
Year and Month FEs	No	No	Yes
R^2	0.004	0.294	0.397
N	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
†Includes only those goods subject to state sales taxes in all states.

Table 4: Regression results. Natural log of mean durable goods expenditure is dependent variable.

	(1)	(2)	(3)
State tax rate	0.003	0.014	0.013
	(0.017)	(0.013)	(0.013)
Percent less than high school		0.126	0.232
		(0.523)	(0.556)
Percent some college		0.533*	0.290
		(0.285)	(0.311)
Percent Bachelors		-0.226	-0.387
		(0.276)	(0.286)
Percent postgraduate		-0.251	-0.228
		(0.380)	(0.327)
Percent rural households		0.175	-0.100
		(0.278)	(0.268)
Percent single woman households		-0.207	-0.438
		(0.257)	(0.279)
Percent single man households		-0.007	-0.336
		(0.234)	(0.235)
Percent Hispanic		0.246	0.014
		(0.278)	(0.311)
Percent Black		-0.751**	-0.817**
		(0.302)	(0.300)
Percent other race/ethnicity		-0.603***	-0.512***
		(0.130)	(0.125)
Mean age		0.009	0.000
		(0.007)	(0.007)
Ln(Income)		0.774***	0.602***
		(0.106)	(0.109)
Mean family size		0.178**	0.180**
		(0.077)	(0.072)
Tax holiday		0.080*	0.035
		(0.040)	(0.050)
Year and Month FEs	No	No	Yes
R^2	0.000	0.105	0.186
N	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Temporal response regression results. Log of total expenditure is dependent variable.

	(1)	(2)	(3)	(4)
Pre-tax rate increase	0.055	0.026	0.043	0.038
	(0.042)	(0.036)	(0.038)	(0.038)
Post tax rate increase	0.014	-0.003	0.025	0.011
	(0.037)	(0.032)	(0.033)	(0.031)
Pre-tax rate decrease	0.009	0.068*	0.071	0.009
	(0.080)	(0.039)	(0.056)	(0.035)
Post tax rate decrease	-0.050	0.044	0.036	-0.007
	(0.094)	(0.027)	(0.038)	(0.035)
Pre-tax rate increase, referendum			-0.119***	-0.041
			(0.035)	(0.042)
Post tax rate increase, referendum			-0.148***	-0.077**
			(0.033)	(0.037)
Pre-tax rate decrease, referendum			-0.009	-0.044
			(0.080)	(0.046)
Post tax rate decrease, referendum			0.036	0.033
			(0.060)	(0.047)
Controls	No	Yes	Yes	Yes
Month-by-year FEs	No	No	No	Yes
State FEs	No	No	No	Yes
R^2	0.000	0.606	0.607	0.760
N	5,495	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Temporal response regression results. Log of taxable expenditure, narrowly defined† is dependent variable.

	(1)	(2)	(3)	(4)
Pre-tax rate increase	0.118	0.083	0.117	0.118
	(0.085)	(0.073)	(0.074)	(0.080)
Post tax rate increase	0.008	-0.015	0.032	0.033
	(0.087)	(0.087)	(0.097)	(0.095)
Pre-tax rate decrease	0.015	0.086	0.082	0.028
	(0.062)	(0.069)	(0.094)	(0.084)
Post tax rate decrease	-0.081	0.025	0.001	-0.007
	(0.127)	(0.054)	(0.069)	(0.117)
Pre-tax rate increase, referendum			-0.268**	-0.095
			(0.105)	(0.127)
Post tax rate increase, referendum			-0.255**	-0.149
			(0.120)	(0.093)
Pre-tax rate decrease, referendum			0.019	-0.020
			(0.105)	(0.105)
Post tax rate decrease, referendum			0.096	0.091
			(0.097)	(0.138)
Controls	No	Yes	Yes	Yes
Month-by-year FEs	No	No	No	Yes
State FEs	No	No	No	Yes
R^2	0.000	0.293	0.294	0.423
N	5,495	5,495	5,495	5,495

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

†Includes only those goods subject to state sales taxes in all states.

Table 7: Regression results. Natural log of durable goods is dependent variable.

	(1)	(2)	(3)	(4)
Pre-tax rate increase	0.244 (0.167)	0.209 (0.160)	0.279* (0.161)	0.302 (0.187)
Post tax rate increase	0.002 (0.172)	-0.025 (0.176)	0.092 (0.197)	0.140 (0.201)
Pre-tax rate decrease	0.091 (0.107)	0.172 (0.117)	0.133 (0.157)	0.060 (0.143)
Post tax rate decrease	-0.136 (0.236)	-0.031 (0.191)	-0.136 (0.243)	-0.111 (0.295)
Pre-tax rate increase, referendum			-0.563* (0.280)	-0.282 (0.314)
Post tax rate increase, referendum			-0.640*** (0.211)	-0.465** (0.208)
Pre-tax rate decrease, referendum			0.162 (0.160)	0.081 (0.190)
Post tax rate decrease, referendum			0.423 (0.271)	0.467 (0.329)
Controls	No	Yes	Yes	Yes
Month-by-year FEs	No	No	No	Yes
State FEs	No	No	No	Yes
R^2	0.001	0.103	0.105	0.233
N	5,495	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Difference-in-differences estimates of semi-elasticities of dependent variables between a month before and a month after a sales tax rate change.

	Sales Tax Rate Change	
Mean Total Expenditure	Increase	Decrease
Non-Referendum	-2.7 (6.1) p.p.	-1.5 (3.7) p.p.
Referendum	-6.3 (4.0) p.p.	6.1 (8.2) p.p.
Mean Taxable Expenditure, narrow	Increase	Decrease
Non-Referendum	-8.5 (15.5) p.p.	-3.5 (13.3) p.p.
Referendum	-13.9 (16.5) p.p	7.6 (22.5) p.p.
Mean Durable Goods Expenditure	Increase	Decrease
Non-Referendum	-16.2 (35.2) p.p.	-17.1 (31.9) p.p.
Referendum	-34.5 (22.5) p.p.	21.5* (12.3) p.p.

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Summary statistics for household-by-day observations, 2005-2011. All dollar values are in 2018 USD.

	Mean
Total expenditures	103.649
Taxable expenditures	23.399
Expenditure on gasoline	5.22
Annual income (thousands)	59.981
Less than high school	0.100
High school	0.227
Some college	0.297
Undergraduate	0.225
Postgraduate	0.151
Rural	0.008
Married	0.537
Single woman	0.281
Single man	0.182
White	0.679
Hispanic	0.115
Black	0.113
Mean age	48.866
Family size	2.546
State sales tax rate	5.441
Tax holiday	0.009
Percent of days with zero expenditure	0.301
Observations	561,491

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Table 10: Regression results, Diary portion 2005-2011. State sales tax rate is variable of interest. All dollar values are in 2018 USD.

Dependent variable	Total expenditure	Taxable expenditure
Mean of dependent variable	103.65	23.40
State sales tax rate	0.629	-0.167
	(1.128)	(0.367)
Less than high school	-3.626	-0.376
	(2.340)	(1.045)
Some college	13.441***	4.810***
	(1.941)	(0.809)
Undergraduate	26.684***	8.756***
	(2.139)	(1.082)
Postgraduate	37.597***	13.942***
	(2.749)	(1.315)
Rural	-7.233*	-1.924
	(3.923)	(1.567)
Married	10.794***	3.067***
	(1.474)	(0.888)
Hispanic	-7.112***	-3.483**
	(2.477)	(1.383)
Black	-15.332***	-5.326***
	(2.238)	(1.045)
Mean age	0.365***	0.094***
	(0.045)	(0.020)
Annual income, thousands	0.649***	0.184***
	(0.023)	(0.012)
Family size	6.999***	1.254***
	(0.637)	(0.296)
Tax holiday	3.509	3.453
	(5.275)	(4.174)
Date FEs	Yes	Yes
R^2	0.019	0.008
N	561,491	561,491

Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Regression results, Diary portion 2005-2011. Temporal dummies as variables of interest. All dollar values are in 2018 USD.

Dependent variable	Total expenditure	Taxable expenditure
Mean of dependent variable	103.65	23.40
1 weeks before increase	22.833 (18.112)	30.641 (19.482)
2 weeks before increase	31.373 (39.558)	17.457 (21.566)
3 weeks before increase	4.871 (11.645)	-4.144 (6.187)
4 weeks before increase	25.656 (18.406)	16.810 (11.033)
1 weeks before decrease	1.491 (24.037)	-6.362* (3.548)
2 weeks before decrease	1.878 (17.961)	-4.453 (4.793)
3 weeks before decrease	-17.705* (9.912)	-4.981 (4.507)
4 weeks before decrease	-4.529 (17.041)	3.513 (5.677)
1 weeks after increase	15.638* (8.237)	12.803 (7.743)
2 weeks after increase	-2.343 (12.183)	-1.189 (3.091)
3 weeks after increase	0.250 (14.004)	3.755 (5.283)
4 weeks after increase	-10.791 (22.366)	-12.831 (15.760)
1 weeks after decrease	21.212 (19.026)	26.483* (14.308)
2 weeks after decrease	45.951** (17.358)	37.086*** (7.887)
3 weeks after decrease	183.125*** (62.499)	192.822*** (51.266)
4 weeks after decrease	-31.204*** (8.102)	-7.691*** (2.699)
1 week before increase, referendum	-66.412*** (21.463)	-51.497** (19.473)
2 weeks before increase, referendum	-12.299 (41.595)	10.220 (28.924)
3 weeks before increase, referendum	-70.682*** (23.513)	-14.490 (10.117)
4 weeks before increase, referendum	-90.483*** (30.720)	-38.042*** (11.715)
1 week after increase, referendum	-13.893 (41.677)	-6.484 (7.513)
2 weeks after increase, referendum	18.477 (51.459)	-11.443** (5.109)
3 weeks after increase, referendum	-31.332 (24.062)	-11.265** (4.970)
4 weeks after increase, referendum	-34.427 (26.428)	-16.207 (11.237)
Controls	Yes	Yes
State FEs	Yes	Yes
Date FEs	Yes	Yes
R^2	0.019	0.008
N	561,491	561,491

Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix A: Sales tax rate increases and decreases

Table A1: Sales tax rate increases by state and date, 1/2004-12/2018.

State	Increase/Decrease	Day Initiated	Percent Change	New Sales Tax Rate	Referendum
Arizona	Increase	6/1/2010	17.9	6.6	Yes
Arizona	Decrease	6/1/2013	-15.2	5.6	Yes
Arkansas	Increase	3/1/2004	17.1	6	No
Arkansas	Increase	7/1/2013	8.3	6.5	Yes
California	Increase	7/1/2004	4.2	6.25	No
California	Increase	4/1/2009	16.0	7.25	No
California	Decrease	7/1/2011	-13.8	6.25	No
California	Increase	1/1/2013	4.0	6.5	Yes
California	Decrease	1/1/2017	-7.7	6	Yes
Connecticut	Increase	7/1/2011	5.8	6.35	No
District of Columbia	Increase	10/1/2009	4.3	6	No
District of Columbia	Decrease	10/1/2013	-4.2	5.75	No
Idaho	Decrease	7/1/2005	-16.7	5	No
Idaho	Increase	10/1/2006	20.0	6	No
Indiana	Increase	4/1/2008	16.7	7	No
Iowa	Increase	7/1/2008	20.0	6	No
Kansas	Increase	7/1/2010	18.9	6.3	No
Kansas	Decrease	7/1/2013	-2.4	6.15	No
Louisiana	Increase	4/1/2016	25.0	5	No
Louisiana	Decrease	7/1/2018	-11.0	4.45	No
Maine	Increase	10/1/2013	10.0	5.5	No
Maryland	Increase	1/3/2008	20.0	6	No
Massachusetts	Increase	8/1/2009	25.0	6.25	No
Minnesota	Increase	7/1/2009	5.8	6.875	Yes
Nevada	Increase	7/1/2009	8.2	4.6	No
New Jersey	Increase	7/15/2006	16.7	7	No
New Jersey	Decrease	1/1/2017	-1.8	6.875	No
New Jersey	Decrease	1/1/2018	-3.6	6.625	No
New Mexico	Increase	7/1/2010	2.5	5.125	No
New York	Increase	6/1/2005	6.3	4.25	No
North Carolina	Decrease	12/1/2006	-5.6	4.25	No
North Carolina	Increase	9/1/2009	35.3	5.75	No
North Carolina	Decrease	7/1/2011	-17.4	4.75	No
Ohio	Decrease	7/1/2005	-8.3	5.5	No
Ohio	Increase	9/1/2013	4.5	5.75	No
South Carolina	Increase	7/1/2007	20.0	6	No
South Dakota	Increase	6/1/2016	12.5	4.5	No
Utah	Decrease	1/1/2008	-2.1	4.65	No
Utah	Increase	1/1/2009	1.1	4.7	No

Virginia	Increase	9/1/2004	11.1	5	No
Virginia	Increase	7/1/2013	6.0	5.3	No

Appendix B: Alternative taxable expenditure definition

Table B1: Regression results, taxable expenditure, broad is dependent variable.

	(1)	(2)
State tax rate	-6.971	
	(7.720)	
Pre-tax rate increase		114.380
		(73.843)
Post tax rate increase		20.914
		(84.315)
Pre-tax rate decrease		46.755
		(52.861)
Post tax rate decrease		11.812
		(82.152)
Pre-tax rate increase,		-65.007
referendum		(92.545)
Post tax rate increase,		-137.388*
referendum		(80.553)
Pre-tax rate decrease,		-51.095
referendum		(101.601)
Post tax rate decrease,		78.177
referendum		(130.512)
Controls	Yes	Yes
State fixed effects	No	Yes
Month-by-year fixed effects	Yes	Yes
R^2	0.343	0.410
N	5,495	5,495

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix C: Alternative tax measure

Table C1: Regression results, universal combined sales tax rate.

	(1) Ln(mean total expenditure)	(2) Ln(mean taxable expenditure, narrow)	(3) Ln(mean expenditure on durable goods)
Universal combined tax rate	-0.002 (0.003)	0.001 (0.005)	0.015 (0.012)
Controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Month-by-year fixed effects	No	No	No
R^2	0.740	0.397	0.186
N	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C2: Regression results, tax dummies and universal combined sales tax rate changes.

	(1) Ln(mean total expenditure)	(2) Ln(mean taxable expenditure, narrow)	(3) Ln(mean expenditure on durable goods)
Pre-tax rate increase	0.038	0.118	0.302
	(0.038)	(0.080)	(0.187)
Post- tax rate inrease	0.011	0.033	0.140
	(0.031)	(0.095)	(0.201)
Pre-tax rate decrease	0.009	0.028	0.060
	(0.035)	(0.084)	(0.143)
Post tax rate decrease	-0.007	-0.007	-0.111
	(0.035)	(0.117)	(0.295)
Pre-tax rate increase, referendum	-0.041 (0.042)	-0.095 (0.127)	-0.282 (0.314)
Post tax rate increase, referendum	-0.077** (0.037)	-0.149 (0.093)	-0.465** (0.208)
Pre-tax rate decrease, referendum	-0.044 (0.046)	-0.020 (0.105)	0.081 (0.190)
Post tax rate decrease, referendum	0.033 (0.047)	0.091 (0.138)	0.467 (0.329)
Controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Month-by-year fixed effects	Yes	Yes	Yes
R^2	0.760	0.423	0.233
N	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D: Restricted sample

Table D1: State tax rate regression results, restricted sample

	(1) Ln(mean total expenditure)	(2) Ln(mean taxable expenditure, narrow)	(3) Ln(mean expenditure on durable goods)
State tax rate	-0.004	-0.001	0.013
	(0.003)	(0.006)	(0.013)
Controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Month-by-year fixed effects	No	No	No
R^2	0.736	0.393	0.184
N	5,181	5,181	5,181

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D2: Temporal dummy variable regression results, restricted sample

	(1) Ln(mean total expenditure)	(2) Ln(mean taxable expenditure, narrow)	(3) Ln(mean expenditure on durable goods)
Pre-tax rate increase	0.038	0.115	0.296
	(0.037)	(0.078)	(0.184)
Post tax rate increase	0.010	0.028	0.131
	(0.031)	(0.095)	(0.200)
Pre-tax rate decrease	0.010	0.026	0.052
	(0.035)	(0.085)	(0.142)
Post tax rate decrease	-0.001	0.005	-0.094
	(0.034)	(0.114)	(0.295)
Pre-tax rate increase, referendum	-0.040	-0.087	-0.263
	(0.041)	(0.125)	(0.306)
Post tax rate increase, referendum	-0.080**	-0.155	-0.480**
	(0.037)	(0.093)	(0.204)
Pre-tax rate decrease, referendum	-0.046	-0.017	0.093
	(0.045)	(0.104)	(0.180)
Post tax rate decrease, referendum	0.023	0.061	0.425
	(0.046)	(0.138)	(0.334)
Controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Month-by-year fixed effects	Yes	Yes	Yes
R^2	0.756	0.420	0.233
N	5,181	5,181	5,181

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix E: Unweighted interview sample

Table E1: Summary statistics for all states by month, 2004-2018. Unweighted

	mean
Mean total expenditure	3281.114
Mean taxable expenditure, narrow	912.962
Mean expenditure on durable goods	449.764
Mean annual household income	63.700
Percent less than high school	0.094
Percent high school	0.209
Percent some college	0.308
Percent Bachelors	0.231
Percent postgraduate	0.159
Percent rural households	0.016
Percent married households	0.515
Percent single woman households	0.289
Percent single man households	0.197
Percent non-Hispanic White	0.692
Percent Hispanic	0.083
Percent Black	0.122
Percent other race/ethnicity	0.104
Mean age	49.592
Mean family size	2.480
State tax rate	4.791
Pre-tax rate increase	0.003
Pre-tax rate decrease	0.001
Observations	5,495

Table E2: Regression results, state tax rate as variable of interest. Unweighted sample

	(1)	(2)	(3)
	Ln(mean total expenditure)	Ln(mean taxable expenditure, narrow)	Ln(mean expenditure on durable goods)
State tax rate	-0.004	0.001	0.014
	(0.003)	(0.006)	(0.013)
Controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Month-by-year fixed effects	No	No	No
R^2	0.744	0.403	0.189
N	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table E3: Regression results, temporal dummies as variable of interest. Unweighted sample

	(1) Ln(mean total expenditure)	(2) Ln(mean taxable expenditure, narrow)	(3) Ln(mean expenditure on durable goods)
Pre-tax rate increase	0.028	0.090	0.251
	(0.034)	(0.069)	(0.170)
Post tax rate increase	0.010	0.029	0.133
	(0.028)	(0.085)	(0.192)
Pre-tax rate decrease	0.024	0.072	0.124
	(0.040)	(0.091)	(0.150)
Post tax rate decrease	-0.019	-0.024	-0.130
	(0.033)	(0.111)	(0.290)
Pre-tax rate increase,	-0.030	-0.094	-0.268
referendum	(0.033)	(0.132)	(0.324)
Post tax rate increase,	-0.060	-0.101	-0.378
referendum	(0.040)	(0.082)	(0.239)
Pre-tax rate decrease,	-0.041	-0.024	0.074
referendum	(0.046)	(0.107)	(0.194)
Post tax rate decrease,	0.048	0.077	0.390
referendum	(0.047)	(0.139)	(0.339)
Controls	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Month-by-year fixed effects	Yes	Yes	Yes
R^2	0.764	0.429	0.235
N	5,495	5,495	5,495

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix F: Gasoline analysis

To examine whether consumers adjust their spending on gasoline in response to a sales tax change, suggestive of driving farther or less far to avoid or take advantage of a sales tax rate change, I run a regression using the Diary portion of the CEX, identical in composition to the econometric models found in section 8.2. Results can be found in Table F1. The state sales tax rate is positively associated with expenditure on gasoline, but is not statistically significant (Table F1, column 1). The temporal dummies themselves are mixed in sign, and often statistically insignificant, though one week after a sales tax rate decrease is found to be correlated with a 58.1 percent decrease in spending on gasoline, while one week before a tax increase caused by a referendum is correlated with a 62.0 percent increase in spending on gas and one week after is correlated with a 63.2 percent increase in spending on gas, suggesting that there may be some initial additional travel in the week before and after a widely known sales tax rate increase, but this is transitory.

Table F1: Regression results, expenditure on gasoline as dependent variable.

	(1)	(2)
Mean expenditure on gasoline	5.22	5.22
State sales tax rate	0.102 (0.070)	
1 week before increase		0.415 (0.627)
2 weeks before increase		-0.801 (0.527)
3 weeks before increase		-0.410 (0.585)
4 weeks before increase		0.178 (0.348)
1 weeks before decrease		-0.239 (0.783)
2 weeks before decrease		-0.478 (0.501)
3 weeks before decrease		0.552 (0.874)
4 weeks before decrease		-0.021 (0.300)
1 week after increase		-0.150 (0.638)
2 weeks after increase		1.269* (0.740)
3 weeks after increase		1.292 (1.039)
4 weeks after increase		1.864*** (0.434)
1 week after decrease		-3.028*** (0.825)
2 weeks after decrease		-0.222 (1.788)
3 weeks after decrease		0.178 (0.769)
4 weeks after decrease		0.485 (0.815)
1 week before increase, referendum		2.824*** (0.815)
2 weeks before increase, referendum		-1.074 (1.343)
3 weeks before increase, referendum		-1.275 (0.958)
4 weeks before increase, referendum		0.855 (1.127)
1 week after increase, referendum		3.448*** (0.872)
2 weeks after increase, referendum		0.432 (0.827)
3 weeks after increase, referendum		-1.045 (2.587)
4 weeks after increase, referendum		0.190 (3.403)
Controls	Yes	Yes
Date FEs	Yes	Yes
State FEs	No	Yes
R^2	0.029	0.030
N	561,491	561,491

Standard errors in parentheses

(d) for discrete change of dummy variable from 0 to 1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix G: Power Analysis

To determine the minimum detectable effect in the linear regression model, I run a power analysis using the F-test detailed in Cohen (2013). The F-statistic is defined as follows:

$$F = \delta \frac{n-(k+1)}{k}$$

Where n is equal to the number of observations, k is equal to the number of coefficients in the linear regression model, and δ is equal to the minimum detectable effect. This can be rewritten as

$$\delta = F \frac{k}{n-(k+1)}$$

The likelihood of a type II error (β) is defined as a function of the cumulative F distribution with

$$\beta = F_{n,n-(k+1),n\delta}\,(F_{n,n-(k+1),1-\alpha})$$

Using the widely accepted power (π) of 0.8, where power is one minus the probability of Type II error (β) and significance (α) of 0.05 where significance is equal to one minus the probability of type I error, setting the sample size equal to 5,495 and the number of covariates equal to 233, as that is the highest number of covariates used in regressions including fixed-effect dummies yields the following equation:

$$0.8 = 1 - F_{5495,5262\ ,\delta*5495}(F_{5495,5262,0.95})$$

Which is equal to 0.0110 when solved iteratively. Therefore, based on the sample size and number of covariates the interview empirical model has the power to detect effects if they are greater than 0.0110 percentage points in absolute value.

For the diary portion, the power is once more set at 0.8 and the statistical significance is set to 0.05, but the number of observations is equal to 558,629 and the number of covariates is equal to 2,629 at most, so the following equation is found:

$$0.8 = 1 - F_{558629,556000\ ,\delta*558629}\left(F_{558629,556000,0.95}\right)$$

Which yields 0.0003 when solved iteratively, so the empirical models in the diary portion have the power to detect changes if they are greater than 0.0003 percentage points in absolute value.

Appendix H: Tax Burden

To examine how state sales taxes in the United States have fallen on different portions of the population from 2005- 2017, I use the following equation:

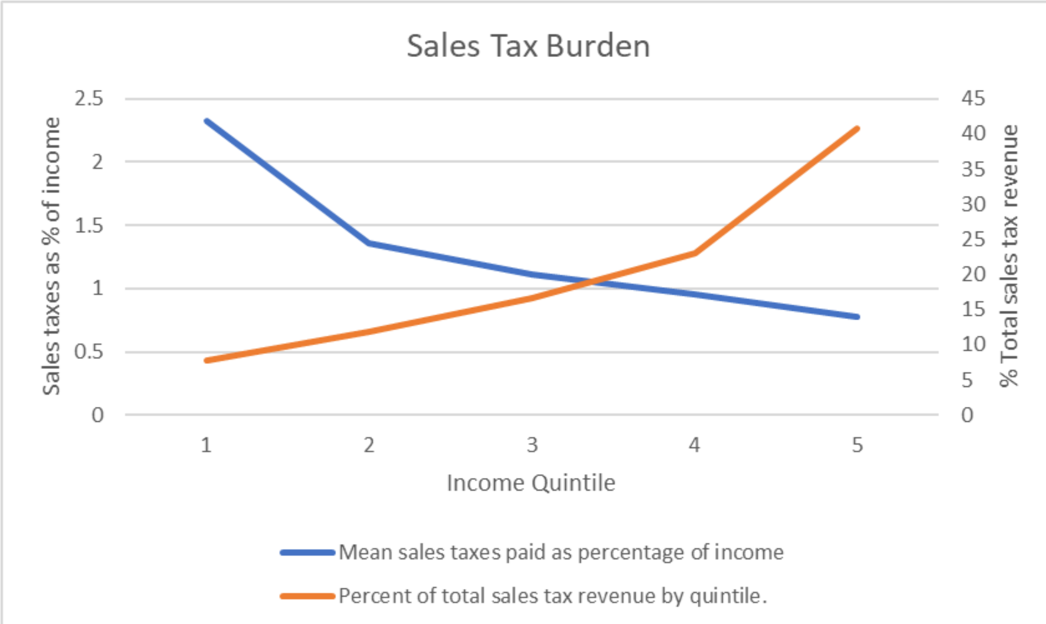
$$TaxableExpenditure_{hm} = Consumption_{hm}(1 + t_{ms})$$
$$= Consumption_{hm} + SalesTaxPaid_{hm}$$

$$SalesTaxPaid_{hm} = \frac{TaxableExpenditure_{hm}}{(1 + t_{ms})} * t_{ms}$$

Where for household *h* during month *m* in state *s*, taxable expenditure can be defined as amount spent on consumption multiplied by one plus the state sales tax rate (*t*). After some algebra, the total amount of sales taxes paid by household *h* in month *m* can be determined to be the total amount of expenditure on taxable goods divided by one plus the states sales tax rate, multiplied by the state sales tax rate.

This is found for every household by month observation in the Interview portion of the Consumer Expenditure Survey from 2005-2017. Dollars are real 2018 USD using CPI data from the Federal Reserve at St. Louis. The observations are divided into quintiles by total annual household income, then the sales tax paid for each household is divided by mean monthly household income. This is state sales taxes paid as a percentage of mean monthly household income. The means are taken over each income quintile, as seen in Figure H1. This makes up the blue, downward-sloping line, the orange upward-sloping line is the percentage of the total observed sales tax revenue (the summation of all sales taxes paid for all households in all months of the sample) provided by households in each quintile. As can be clearly seen, richer households account for a larger portion of the state sales revenues, but as a percentage of mean monthly income it falls more heavily on the poorer households. While the top 20 percent of households account for 40.8 percent of state sales tax revenues, this only accounts for about 0.8 percent of their mean monthly incomes. On the other hand, the bottom 20 percent of households account for only 7.7 percent of tax revenue but this accounts for 2.3 percent of their mean monthly income. Note that this analysis does not account for local sales taxes.

Figure H1: State sales tax rate burden in the United State and percent of total sales revenue provided by income quintile, 2005-2017.



Note: Local taxes are not factored into this analysis, so this is an approximation.

AR, ID, IW, ME, MS, MO, NJ, NM, NC, ND, OK, RI, SD, VT, WV, WY are not included in this analysis.

Chapter 2: The Supply-Side Impact of the Affordable Care Act Medicaid Expansion

Sean Larkin[1]

Abstract: In January 2014, 24 US States and the District of Columbia expanded Medicaid coverage following the Affordable Care Act of 2010, primarily to low-income adults without children. In this paper I estimate the impact of this insurance expansion on hospital expenditures, services offered, infrastructure investment as well as number of full-time equivalents hired using data from Medicare and Medicaid cost reports. Using a difference-in-differences methodology for the primary analysis supplemented with a number of event studies I find that the Medicaid expansion led to a 37 percent increase in Medicaid payments to hospitals, an 85 percent reduction in expenditures on services for uninsured patients, a 4 percent increase in hospital employment and a 10 percent increase in hospital capital balances.

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1. Introduction

Since 1970, health expenditures as a percentage of gross national product in the US have increased drastically (see Figure 1), from just over 5 percent of GDP in 1970 to 17 percent in 2018, and notably increased during the early 2000's, the Great Recession (2008-2009) and immediately after Medicaid expansion of 2014. The 17 percent of US GDP allocated to healthcare is substantially higher than the OECD average of 10 percentage points. This understates the saliency of this topic to political and policy discussions, since public health programs currently account for 26 percent of US federal spending, and on average 29 percent of US state spending. The largest recipient of increased health expenditures was hospital care, which received the plurality of the increase (38%) (Levit et al., 1991). While all developed countries have experienced similar trends, the US stands out for its unique system of health insurance and the magnitude of the trend. Past empirical work has categorized the US health system as "complex, costly and unequal" (Camillo, 2016).

The goal of this paper is to examine the magnitude and the margins of the response of medical care providers to the changes in demand for healthcare services caused by the Medicaid expansion of 2014 that occurred as part of the Affordable Care Act of 2010. While much ink has been spilled on examining the demand-side effects of the expansion, with empirical research consistently finding evidence for increased health insurance coverage (Sommers et al., 2014), better quality health insurance coverage (Courtemanche et al., 2017; Wherry & Miller, 2016) and lower opioid use (Cher et al., 2019), there have been few studies on the supply-side responses to the shock, and this is the first to examine the effects of the expansion on a broad range of supply side characteristics across the US. Using difference-in-differences and event study techniques, I find that the Medicaid expansion led to a 35 percent increase in Medicaid payments to hospitals, an 85 percent in expenditures on services for uninsured patients, a 10 percent increase in capital investments and a 4 percent increase in the number of full-time equivalents hired.

It has been widely acknowledged since the RAND health experiment that the demand for health services is responsive to price, so demand is not strictly inelastic and based solely on need (Aron-Dine et al., 2013; Manning et al., 1987). However, both the RAND experiment and following empirical research indicated that the elasticity was low enough that increased access to health insurance did not have a large impact on the increase in health expenditures, and so something else must be the primary driver (Manning et al., 1987; Newhouse, 1992; Normand, 1994). However, these results are inherently flawed. Estimates of the increased expenditures induced by increased access and utilization of health insurance based entirely on individual-level spending estimates could potentially dramatically underestimate the total market impact (Finkelstein, 2007), so the supply-side effects are important to capture. This paper seeks to add to the empirical literature examining the supply-side response to demand-side insurance shocks by looking at a shock that has not yet been analyzed in this manner- the Medicaid expansion.

The Medicaid expansion led to a decrease in the percentage of individuals under 65 that did not hold health insurance (Courtemanche et al., 2017). That increased insurance coverage leads to increased utilization of medical services (Card et al., 2008; Finkelstein et al., 2012) and lower insurance deductibles also lead to increased utilization of medical services (Aron-Dine et al., 2013) are empirically well-documented though the effects on patient health outcomes and hospital expenditures are open empirical questions. There is also empirical evidence that higher medical prices- such as those under with high-deductible insurance plans or the uninsured- are associated with a general decrease of all healthcare utilization, not just wasteful health services (Baicker et al., 2015; Brot-Goldberg et al., 2017; Haviland et al., 2012). If this holds true in general, one expects an uptick in health insurance access and the corresponding increase in healthcare expenditures to lead to an increase in both effective and ineffective medical care utilization, which in turn would likely lead to better health outcomes, but not more efficient medical care utilization.

There is also evidence that suggest that the distribution of gains in use of health services is driven by the interaction between supply-side incentives and shifts in insurance characteristics, and these effects differ by socio-economic groups (Card et al., 2008). Since the Medicaid expansion targeted low-income individuals, the effects found in this study are going to be in reaction to that income group, and not necessarily generalizable to other health insurance expansions.

2. Background

Medicaid Expansion

The Patient Protection and Affordable Care Act (ACA) was signed into law in 2010, with its major provisions taking effect in 2014. Alongside major regulatory changes, an expansion of Medicaid was announced. This expansion provided access to

Medicaid to all individuals in households making less than 133% of the federal poverty line, including those without dependents. Prior to the expansion, states were allowed to determine their own guidelines, resulting in substantial variation from state to state. Initially, all states with Medicaid programs were required to join or have their federal funding for Medicaid programs stripped, but this retaliatory provision was struck down in 2012 in the Supreme Court ruling *National Federation of Independent Business v. Sebelius,* making participation voluntary. As a result, only 24 states and the District of Columbia participated in the initial Medicaid expansion, with ideology being the stated primary motivation for abstention (see Figure 2). I exploit this exogenous variation in policy exposure to identify supply-side responses to the Medicaid expansion. Between January 1st, 2014, and December 31st, 2020, 12 states have expanded their Medicaid programs, and 14 had not yet done so. Six states passed the Medicaid expansion through ballot initiatives, though the state legislature has blocked its implementation in two of them. While popular support for the Medicaid expansion seems in-line with economic self-interest, it has also garnered considerable support from individuals with at least a bachelor's degree (Matsa & Miller, 2019). Past empirical work has found that the Medicaid expansion lowered the compensated care costs as a percentage of total costs in hospitals subject to the Medicaid expansion (Callison et al, 2021; Camilleri, 2018; Kim & Zhou, 2020).

Hospital employment and capital investment patterns

Hospital employment is affected by demographic changes, with higher proportions of individuals over 65 associated with higher employment. Health insurance coverage is a key factor, as the uninsured tend to use less health services. Competition from other hospitals has a negative influence on total employment at a given hospital (Goodman, 2006). Hospitals are also sensitive to changes in reimbursement policies (Appelbaum & Granrose, 1986).

There are meaningful differences in hospital expenditure by governance type, with private for-profit, private non-profit and public hospitals facing different incentive structures (Amin et al., 2018; Lee et al., 2013). Private for-profit hospitals tend to be more expensive ceteris paribus than non-profit hospitals due to higher tax burdens and higher cost of equity capital (Reinhardt, 2000) though both are affected by financial flows (Adelino et al., 2015). Geography also plays a role, with rural hospitals having different employee mixes than metropolitan hospitals (Wootton & Ross, 1995)

While hospital spending is independent of local public health spending (Singh & Young, 2017), hospital employment matters to the nearby community, not just to prospective employees. Not only are they often large, anchoring facilities but their presence has a positive impact on both wages and employment in the labor markets in which they operate (Mandich & Dorfman, 2017). Hospital investment in capital also matters, as higher investment in technological advancements often leads to better performance metrics (Bojja & Liu, 2020).

Many factors play a role in how hospitals make the decision to invest in new technology, including the direct costs, the healthcare benefits and how the proposed technology will impact decision making (Vassolo et al., 2021). Higher insurance rates are actually associated with greater hospital investment in technology (Newhouse, 1992), so one would expect a change in the insured population to affect hospital investment patterns. Far from being passive, patient expectations and demands often drive change, as their expectations play a role in quality of healthcare services provided (Sá & Straume, 2021). This is directly communicated via feedback from the patients to the provider, and indirectly as consumers can choose to go to other hospitals and both the patient mix and managerial factors are key components of hospital profitability (Gapenski et al., 1993).

Certificates of Need

NY enacted the first CON law in 1964, and this expanded to 26 states over the next 10 years. Early CONs regulated expenditures on beds and health service expansions above $100,000 (around $900,000 2022 USD). In 1974, Federal government passed the National Health Planning and Resources Development Act. This required states to pass CON laws similar to the federal model, leading to every state except Louisiana to have a CON law in place by 1982. These laws gave states broad regulatory oversight over the expansion and development of a variety of healthcare facilities including hospitals, nursing and intermediate care facilities. The federal mandate was repealed in 1987 along with associated federal funding. Since then, several states have repealed or modified their CON laws. New Hampshire was the most recent state to repeal their CON law in 2016. As of 2022, 35 states have a CON program enacted, 12 have repealed their CON programs and 3 states have repealed their CON programs but still maintain moratoria on certain projects (see Appendix B).

In a state with a Certificate of Need (CON) program, a state health planning agency or other entity must review and approve projects like establishing a new health care facility or expanding a facility's health service capacity. The goal of CON programs is to control health care costs by restricting duplicative services and determining whether new capital expenditures meet a community need, though the opponents' claim that of the programs claim that CON laws have the opposite effect (Ohlhausen, 2015) has empirical support (Chiu, 2021). CON laws are also designed to ensure health services are provided to historically underserved communities and indigent patients. The specific structure of CON review and approval varies state to state, but generally a health care facility must seek state approval from a state-wide regulatory body, often a state health planning agency, department of health or a CON council appointed by the governor or legislature, based on mandated criteria such as: the projected need for the proposed health care service/expansion within the area, the projected effects of the proposed project on the health care costs and specific populations and the expected cost of the project. After submission, the CON regulatory body can approve, deny, or set limitations on the proposal.

Healthcare Geographies

Healthcare markets are not identical to ZIP, county, or metropolitan areas, with hospitals serving populations across city and state boundaries. To account for this, the Dartmouth Atlas of Health Geography in its *Appendix on the Geography of Health Care in the United States* constructs both Hospital Service Areas (HSAs) and Health Referral Regions (HRRs) every year, starting in 1995. Hospital Service Areas are the collection of ZIP codes from which a particular hospital receives the majority of its Medicare patients. There is only one hospital in most HSAs, and there are 3,436 HSAs in the United States.

Healthcare Referral Regions (HRRs) are designed to measure the tertiary markets for healthcare and are composed of multiple HSAs. Each HRR contains at least one hospital that performs major cardiovascular procedures and neurosurgery. HRRs are defined by assigning HSAs in the region where the greatest proportion of major cardiovascular procedures were performed. There are 306 HRRs in the US. Each HRR can be viewed as a distinct healthcare market, with patients generally deciding between the various hospitals and facilities within an HRR to receive healthcare services, and hospitals generally refer patients to other hospitals and specialists within the HRR.

3. Data

Sources

The primary data source is Medicare/Medicaid cost reports from 2011-2020 from every hospital in the United States that sought reimbursement from Medicare and/or Medicaid programs; it is provided and maintained by the US Centers for Medicare and Medicaid Services (CMS). The data includes the address, the type of each medical service provider, governance category, number of beds, total expenses, whether the hospital is classified as rural, total Medicaid payments, and the number of hours paid for by each provider broken down by type of worker. Of course, total expenses do not correspond to output prices. This provides the necessary geographic information to allow creation of regional and market-level controls, as well as the supply-side variables needed for this analysis. All dollars are converted to 2020 USD using the consumer price index.

For the purposes of comparing summary statistics, this is supplemented by demographic information from the American Community Survey (ACS) provided by the US Census Bureau and accessed through IPUMS USA. This data is aggregated to the HRR level using geographic crosswalks from the Dartmouth Atlas of Healthcare as those are the overall markets served by the hospitals. Relevant variables include the percent of population uninsured, total population, real per-capita income, percent of population without a high school diploma, percent of population with at most a bachelor's degree, percent of population with a graduate degree, percent White, percent Black, percent Hispanic, and percent over 65 years of age. The ACS sample used is the 2010-2014 5-year estimates so can be interpreted as the demographics in the years before the first wave of the Medicaid expansion. Total full-time equivalents are calculated from hours worked by dividing the annual total number of hours of work paid for by the hospital and divided by 2,087, the number of hours a full-time employee works according to the US Office of Personnel Management (OPM, 1984).

This analysis only examines those institutions categorized in the health reports as general short-term care to ensure comparability within sample. To ensure a balanced panel, only those general short-term healthcare facilities that contain payroll information for every year in the sample, 2011-2020 are used in-sample. To ensure a clean sample, all healthcare facilities in an HRR in which over 90 percent of the population is located in a state that expanded Medicaid after the initial wave are dropped. This drops 324 healthcare facilities in 55 HRRs. The final sample contains 2,262 hospitals across 251 HRRs, of which 990 hospitals are in HRRs exposed to the Medicaid expansion and 1,263 hospitals are in HRRs that were never exposed to the Medicaid expansion.

This data set is comprised of 2,586 healthcare facilities, compared to 4,039 healthcare facilities listed in the CMS, so this sample contains about 64 percent of all short-term care facilities in the data set. The data is in hospital-by-year observations, with the demographic controls being time-invariant and at the HRR level. Certificate-of-Need law status by state and year is taken from the National Council of State Legislatures and state healthcare regulatory body websites. Each model has a different number of observations, as hospitals are dropped from the panel for all years for having at least one suspicious observation, a suspicious observation being a negative number where one is not possible, or an abnormally large or small stock change, or a massive (magnitude of over 100,000 percent) change in flow variable. For dependent variables in logarithmic form, any hospital with at least one zero valued observation is dropped as well.

Coding

All percentage variables are coded from 0 to 1, with 1 being 100 percent of the population and 0 being 0 percent of the population. All dollar amounts are in 2020 USD, and all demographic information is at the HRR-level for the period immediately prior to the Medicaid expansion. Total FTEs are defined as the total number of full-time equivalents employed by the hospital during a given fiscal year. Physician and nurse FTEs Intern and resident FTEs. Average salary is the total wages paid out by the hospital divided by the total number of hours, so can be considered the mean hourly wage. The number of beds is the total number of beds in the facility. Nonprofit is a dummy variable equal to one if the hospital governance type is private not-for-profit and zero otherwise. Total Medicaid Payments is the amount of money the hospital received from Medicaid that year. Public is a dummy variable equal to one if the hospital governance type is any type of public, and zero otherwise. Proprietary is a dummy variable equal to one if the hospital governance type is private for-profit, and zero otherwise. Rural is a dummy variable equal to one if the hospital is classified as a "rural health facility" by the CMS, and zero otherwise. Total population is the total population in thousands. Percent of population above 65 is the percentage of the population in the HRR above the age of 65. For race and ethnicity, Percent Black and Percent Hispanic are the percent of the population that identify as non-Hispanic Black or Hispanic/Latino, respectively. Educational attainment is identified by the percentage of the population that do not hold a high school diploma, percentage of the population with at most a bachelor's degree and percentage of the population with a graduate degree of any kind, such as a Masters, Doctorate, Juris Doctor, Doctor of Medicine, etc. The percentage of the population without health insurance measures access to healthcare services. To measure economic wellbeing, the percentage of the population under the poverty line, the percent unemployed, and the per capita income in the area in thousands USD are also included. Certificate of Need is a dummy variable equal to one if the hospital is located within a state that had an active Certificate of Need program during that year.

Summary statistics for this balanced panel sample can be found in Table 1. Hospitals in HRRs that are mostly serve populations affected by the Medicaid expansion on January 1st 2014 on average have higher mean capital balances, FTEs, salaries, number of beds, Medicaid payments, and more private nonprofit hospitals. Initial expansion states also have higher mean HRR populations, higher percent Hispanic and percent other race/ethnicity in HRR. Expansion hospitals exist in HRRs with higher per capita income and higher levels of educational attainment on average.

Hospitals located in HRRs in which most of the population was never eligible for the Medicaid expansion on average see a higher probability of taking out or revising a loan or lease, cost of treating uninsured, more public and private for-profit hospitals. Hospitals in never expanded HRRs are more likely to be rural, with a higher average percent Black in HRR, and more hospitals per HRR. The average percent over 65 years of age, percent without a high school diploma, percent unemployed and likelihood of being in a certificate of need state are similar between hospitals in primarily initially expanded areas and never expanded HRRs.

4. Empirical Methodology

The basic difference-in-differences model used is:

$$y_{it} = \omega_{th}(Post_t \; x \; ExpansionHRR_h) + \mu_h \, HRR_h + \delta_t Year_t + \mathbf{X}_i \, \beta + \varepsilon_{it} \qquad (1)$$

Where *i* is the hospital, *t* is the year, and *h* is the HRR. *y* is the dependent variable and outcome variable of interest. To examine the direct impact of the Medicaid expansion on hospital revenues and check that this policy could plausibly drive changes in hospital investment and employment, the outcome variables used are the Medicaid payments and the total cost of treating the uninsured. To examine changes in labor outcomes due to the Medicaid expansion, the outcome variables are the total full-time equivalents and change in average salary in thousands 2020 USD. To estimate the effect of the Medicaid expansion on hospital investment, total number of beds at the facility, total capital balance and a dummy variable equal to one if the hospital took out a loan during this fiscal year, and zero otherwise are used. All dependent variables except the probability of new or revised loan or lease is in logarithmic form, so right-hand side estimates can be interpreted as percent change in the dependent variable. *ExpansionHRR* is a dummy variable equal to one if the hospital is in an HRR in which at least 90% of the population is in a state that expanded Medicaid on January 1st, 2014. *Post* is a dummy variable equal to one if the fiscal year is 2014 or later, and zero otherwise. The variable of interest is the interaction term between *Post and ExpansionHRR* as this is the change in *y* attributable to being in an initial expansion state after the expansion took effect.

HRR is a vector of healthcare referral region fixed effects to control for time-invariant differences between healthcare referral regions. *Year* Is a vector of fiscal-year fixed effects to control for shocks that would have affected all hospitals the same way at the same time. **X** is a vector of hospital level-controls, containing controls for rural and for-profit or public governance structures as defined above. The error term is clustered at the state level to address potential serial correlation between hospitals within the same state observations. To be interpreted as causal, this specification must satisfy both the parallel-trends assumption, e.g. that the control group and treatment group had similar trends in the period before the Medicaid expansion and the assumption that there is no anticipatory behavior prior to treatment (Ashenfelter, 1978; Sun & Abraham, 2021).

This analysis is supplemented with an event study which allows examination of the pre-trends, a check on potential anticipatory behavior and an analysis of the timing response of the form:

$$y_{it} = \omega_t(Year_t \; x \; ExpansionHRR_r) + \mu_h \, HRR_s + \delta_h Year_t + \mathbf{X}_r \, \beta + \varepsilon_{it} \quad (2)$$

In equation 2, *y*, *HRR*, *Year*, and **X** are all defined as in equation 1. However, the variables of interest are now $Year_t\ x\ ExpansionHRR$ which estimate the effect of being in an HRR with high exposure to the initial Medicaid expansion in a given year, regardless of if that year is before or after the Medicaid expansion. The controls are the same as defined above. By not placing assumptions on the timing of changes and going year-by-year rather than pre and post averages, the event study specification allows for both the testing of the parallel trends assumption that the previous model requires to be interpreted causally and for closer examination of the timing of these changes, i.e. if the effects are stronger closer to or farther away from the implementation of the Medicaid expansion. The error term is again clustered at the state level to address potential serial correlation between observations at the state level.

5. Results

Estimates for the difference-in-differences regression can be found in Tables 2-4. Each column contains the results for a different dependent variable, and the interaction term between post and initial Medicaid expansion dummy variable is the variable of interest for all regressions. There are no separate dummy variables for "post" and "treated" as mentioned above, as the year and HRR fixed effects saturate the model and a "post" or "treated" dummy variable by itself would not add any explanatory power to the models. The difference-in-differences estimates for the first stage results can be found in Table 2. Being in a Medicaid expansion state is leads to an estimated increase in Medicaid payments by around 38 percent and a decrease in the costs incurred by treating uninsured patients by almost 85 percent. These results are statistically significant and quite large in magnitude, so it is likely that the differences in hospital investment or employment between the treatment and control groups are due to the Medicaid expansion.

The results for the difference-in-differences regressions with labor outcomes as the dependent variables can be found in Table 3. Being in a treated healthcare referral region in the post period accounts for a 4 percent increase in number of FTEs hired, and no change in the average salary, the estimate being a zero with small standard deviation. Being in a treated healthcare referral region in the post period is associated with increased hiring, but no change to average salary.

Capital difference-in-differences estimates can be found in Table 4. Being in a treated healthcare referral region in the post period is associated with a 3 percent increase in the number of beds and a negative effect on the likelihood of taking out or revising a loan or lease, though the effects on the new loan or lease are small in magnitude and not statistically significant. The Medicaid expansion has led to overall capital balances being about 10 percent higher on average indicating increased levels of investment, though this has a large standard deviation and is only significant at the 10 percent level.

Results for the event study regressions can be found in Figures 3 through 9. The base year for comparison is always 2013, the year before the "treated" states expanded Medicaid. Figures 3 and 4 contain the event study results for the first-stage estimates. The event studies indicate that Medicaid payments were if anything consistently lower and uninsured payments were consistently higher in the pre-period, which would bias the difference-in-difference estimates towards zero. The average Medicaid payment increased after the expansion in expansion states, plateauing at around

35 percent after 2016. The cost of treating uninsured patients steadily decreased, from 60 percent less in 2014 to 90 percent less in 2020.

Figure 5 contains the event study results for the log of full-time equivalents by year. All estimates in the pre-period are not statistically different from zero, suggesting that the difference-in-differences regression likely satisfies the parallel trends assumption. The estimates from 2014-2020 are all positive and trending upwards, starting at zero in 2014 and ending at about eight percent higher in 2020 though only the estimates from 2017 onwards are statistically different from zero This suggests that the number of full-time equivalents employed by healthcare facilities was changed by exposure to the Medicaid expansion, but with a three-year lag.

Results for the event study in which the change in average salary by year is the dependent variable can be found in Figure 6. The estimate for 2011 and 2012 are again not statistically significant, suggesting the difference-in-differences regression for the change in average salary dependent variable satisfies the parallel trends assumption. The post-period is mixed, with some estimates being positive and some negative, though all are small in magnitude and none are statistically significant suggesting that the Medicaid expansion has had no effect on the mean salary of hospital employees.

The event study in which the dependent variable is the total number of beds at the facility can be found in Figure 7. The estimates for 2011 and 2012 are of different signs but both are small in magnitude and not statistically significant, suggesting that the parallel trends assumption holds for the difference-in-differences regression with the same dependent variable. The estimates afterwards are all positive and trending upwards, from zero in 2014 to 7 percent in 2020, but are only statistically significant for the 2017 and later estimates. This supports the finding that the Medicaid expansion led to an increase in the number of beds at affected hospitals.

The event study results for the model in which the dependent variable is the probability of taking out a new loan or lease or revising an old one can be found in Figure 8. The estimates for the years 2012 and 2013 are not statistically significant, suggesting that the difference=in-differences model with the same dependent variable satisfies the parallel trends assumption. All of the results from 2014-2020 are not statistically significant and negative with a downward trend. The magnitudes are small, at most negative two percent in 2020. These results do not provide any evidence that exposure to the Medicaid expansion resulted in hospitals changing their propensity to take out new loans and leases or revise old ones.

Figure 9 contains the event study for the ending capital balance by year. All the estimates in the pre-period are positive but not statistically significant from zero, so the parallel trends assumption likely holds for the difference-in-differences estimates. Being in a treated HRR is found to be associated with higher capital balances, from 5 percent higher balances in 2014 to 21 percent higher capital balances in 2020. The estimates in the post-period are all positive, trending upward and statistically significant, suggesting that this trend could potentially continue into the future.

The findings that the Medicaid expansion lowered the total cost of treating the uninsured, increased Medicaid payments and led to increased overall hospital capital are robust to restricting the sample to those hospitals with fiscal years that start on January 1st and end on December 31st of a given year (see Appendix B), restricting the sample to only those HRRs entirely exposed or entirely never exposed to the Medicaid expansion (see Appendix C), and removing the hospital controls (Appendix D). However, the increase in full-time employment is not found when limiting the samples by fiscal year or removing hospital controls, suggesting that this finding is sensitive to specification.

6. Conclusions

By studying the implementation of the Medicaid expansion, this paper has examined the impact of a widespread demand-side shock on hospital costs, revenues, employment and investment. My central findings are that the Medicaid expansion led to a 37 percent increase in Medicaid payments to hospitals, an 85 percent reduction in expenditures on services for uninsured patients, and a 10 percent increase in overall capital stock with suggestive evidence of an increase in the number of FTEs employed and number of beds available. The estimate for expenditures on uninsured patients is of the same sign as other empirical studies, though is about twice the magnitude of that estimated by Callison et al for the state of Louisiana, though this can be explained by different approaches to spending category, as well as time frame and difference in treated groups. These estimates indicate that not only has the Medicaid expansion had the desired impact of dramatically lowering hospital expenditures on uninsured patients, but the additional demand for health services created by the Medicaid expansion has resulted in increased employment and capital stock without corresponding changes in the likelihood of taking out a loan. This paper fails to find evidence that hospitals changed their average salaries in response to the Medicaid expansion, or that hospitals changed their financing behavior. The effects on most variables are stronger the further from the date of Medicaid expansion, as would follow from the triple lag of consumers taking advantage of the recently expanded Medicaid benefits, the behavioral changes would likely not occur all at once as individuals without health insurance or with high-deductible health insurance gain access to more inexpensive health services, and the time necessary for hospital administrations to react to the increased revenues and demands placed on them by this previously underserved population.

The methodology used is not without its limitations. The pre-period is only three years in length, making it impossible to compare long-term trends between treatment and control groups prior to the Medicaid expansion and as this sample is specific to the states and years involved, the results cannot be generalized to other years or states much less other countries with vastly different healthcare systems. Given that these increases in capital and employment occurred without affecting the likelihood of taking out a loan suggests that overall profitability has increased at these hospitals as a result of the Medicaid expansion, and the increasing share of GDP dedicated to healthcare from 2013-2019 could very well be driven in part by the supply-side response to the Medicaid expansion.

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Tables & Figures

Table 1: Summary statistics, means for balanced panel of general short-term hospitals.

	Total	Treated	Untreated
Capital balance (millions USD)	290.279	358.581	235.960
Total FTEs	1,345.488	1,627.735	1,122.239
Average salary	72.216	80.908	65.340
Number of beds	234.115	260.409	213.316
New loan or lease	0.077	0.067	0.084
Medicaid payments (millions USD)	32.244	48.020	19.766
Total cost of uninsured (millions USD)	33.216	20.940	42.926
Nonprofit	0.594	0.747	0.473
Public	0.148	0.091	0.193
Proprietary	0.258	0.162	0.334
Rural	0.286	0.212	0.344
Total HRR population (1000's)	2,138.197	2,377.183	1,949.165
Percent of population above 65	0.140	0.138	0.142
Percent Black	0.127	0.089	0.157
Percent Hispanic	0.157	0.184	0.135
Percent other	0.074	0.100	0.053
Percent less than HS	0.140	0.135	0.144
Percent Bachelors	0.176	0.190	0.166
Percent graduate	0.104	0.120	0.092
Percent under poverty line	0.161	0.149	0.170
Percent unemployed	0.091	0.094	0.089
Per capita income, (Thousands USD)	32.481	35.432	30.146
Number of hospitals in HRR	17.455	15.998	18.608
Certificate of Need	0.731	0.722	0.738
Total uninsured (1000's)	284.551	291.535	279.027
Observations	22,620	9,990	12,630

Table 2: Difference-in-differences regression results, first-stage outcomes.

Dependent Variables	Ln(Medicaid Payments)	Ln(Cost of Uninsured)
Treated x Post	0.372***	-0.845***
	(0.049)	(0.161)
HRR fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Hospital level controls	Yes	Yes
r2	0.357	0.360
N	20,030	18,780

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Difference-in-differences regression results, labor outcomes.

Dependent variable	Ln(FTE)	Ln(Mean Salary)
Treated x Post	0.037**	0.000
	(0.017)	(0.007)
HRR fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Hospital level controls	Yes	Yes
R^2	0.348	0.679
N	22,590	22,550

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Difference-in-differences regression results, capital outcomes.

Dependent variable	Ln(Beds)	P(New loan or lease)	Ln(Capital Balance)
Treated x Post	0.028**	-0.009	0.097*
	(0.012)	(0.010)	(0.050)
HRR fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Hospital level controls	Yes	Yes	Yes
R^2	0.300	0.097	0.338
N	22,480	22,620	22,170

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Healthcare expenditures as a percentage of GDP, 1970-2018.

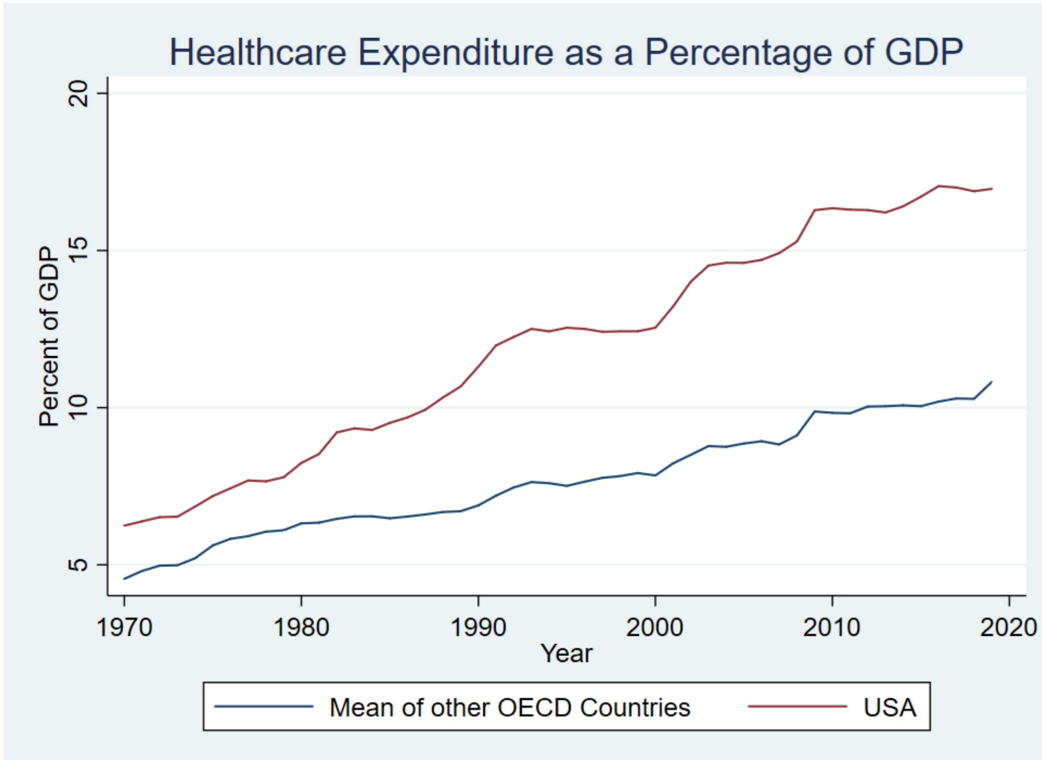


Figure 2: Map of the implementation time of the 2014 Medicaid expansion.

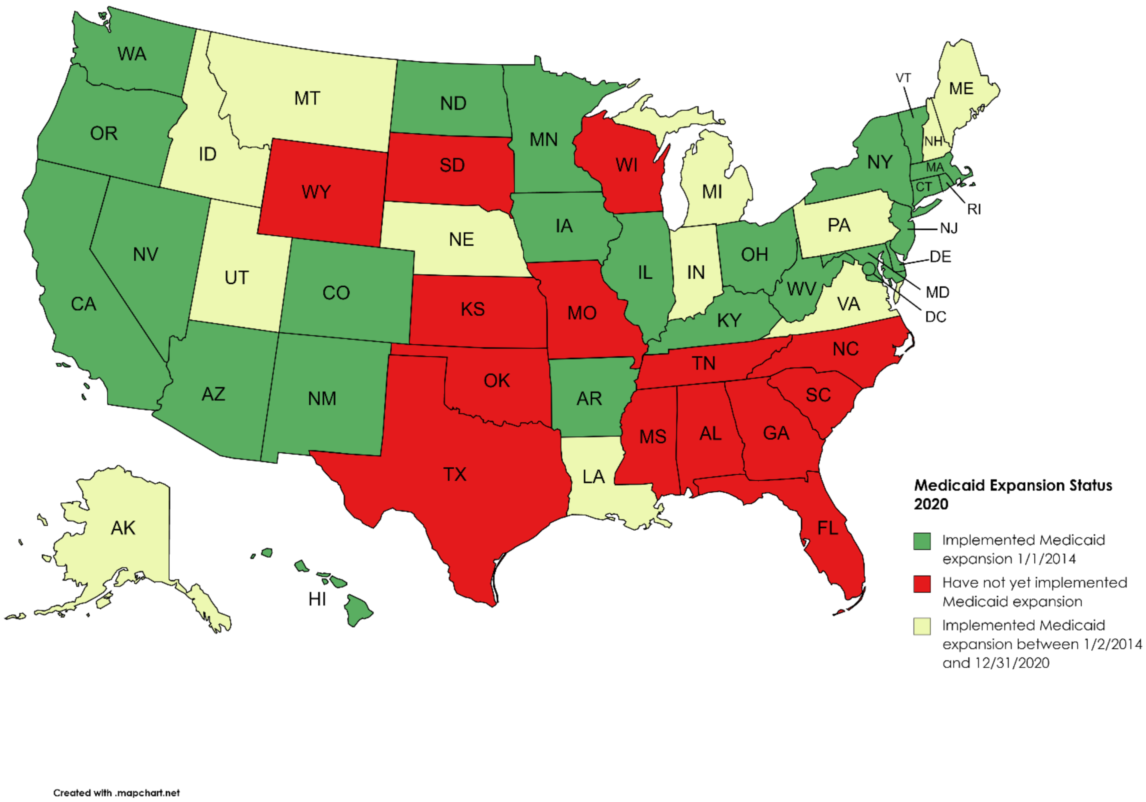


Figure 3: Event Study, logarithm of real Medicaid payments by year.

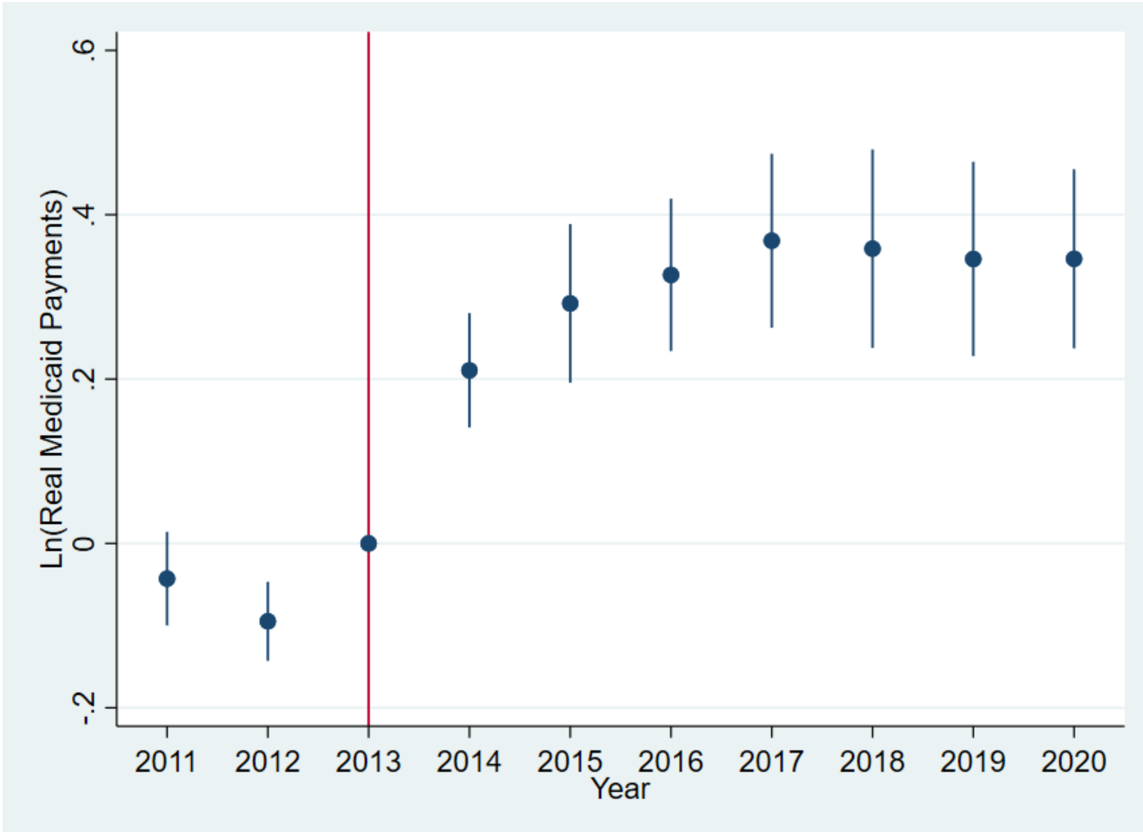


Figure 4: Event study, logarithm of real cost of treating uninsured patients by year.

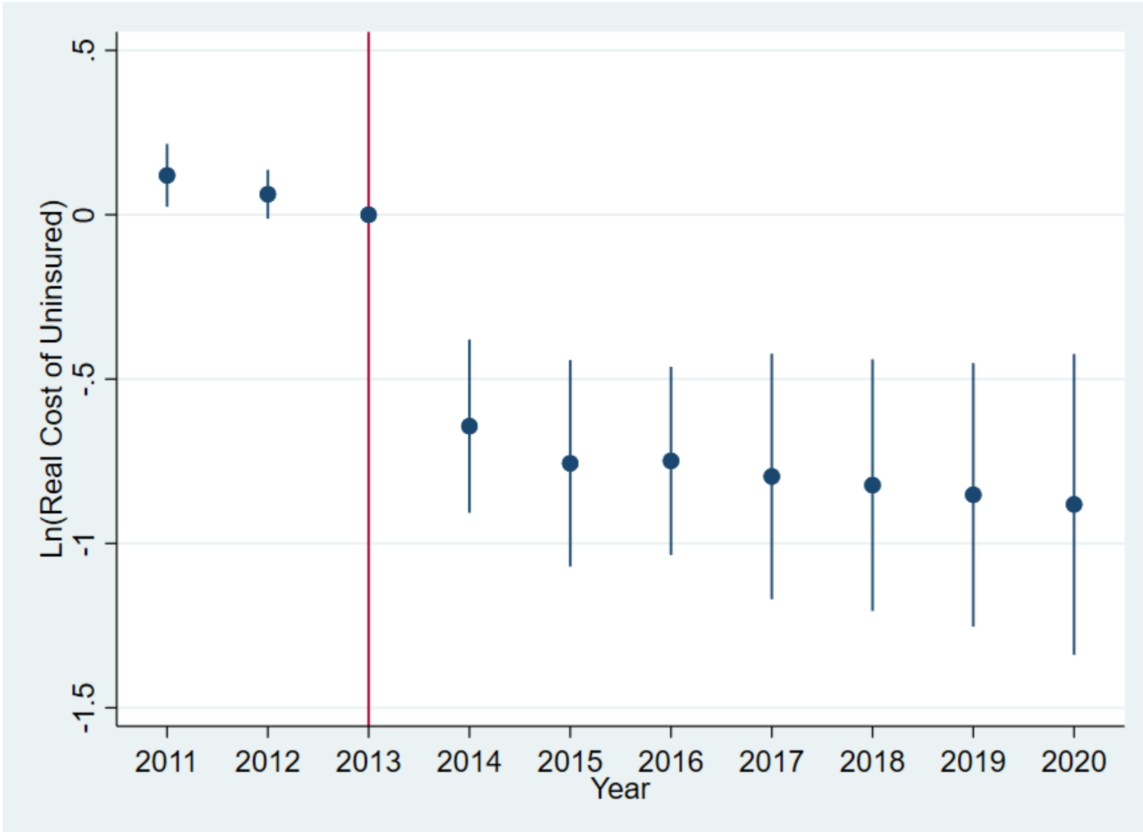


Figure 5: Event study, logarithm of full time equivalents by year.

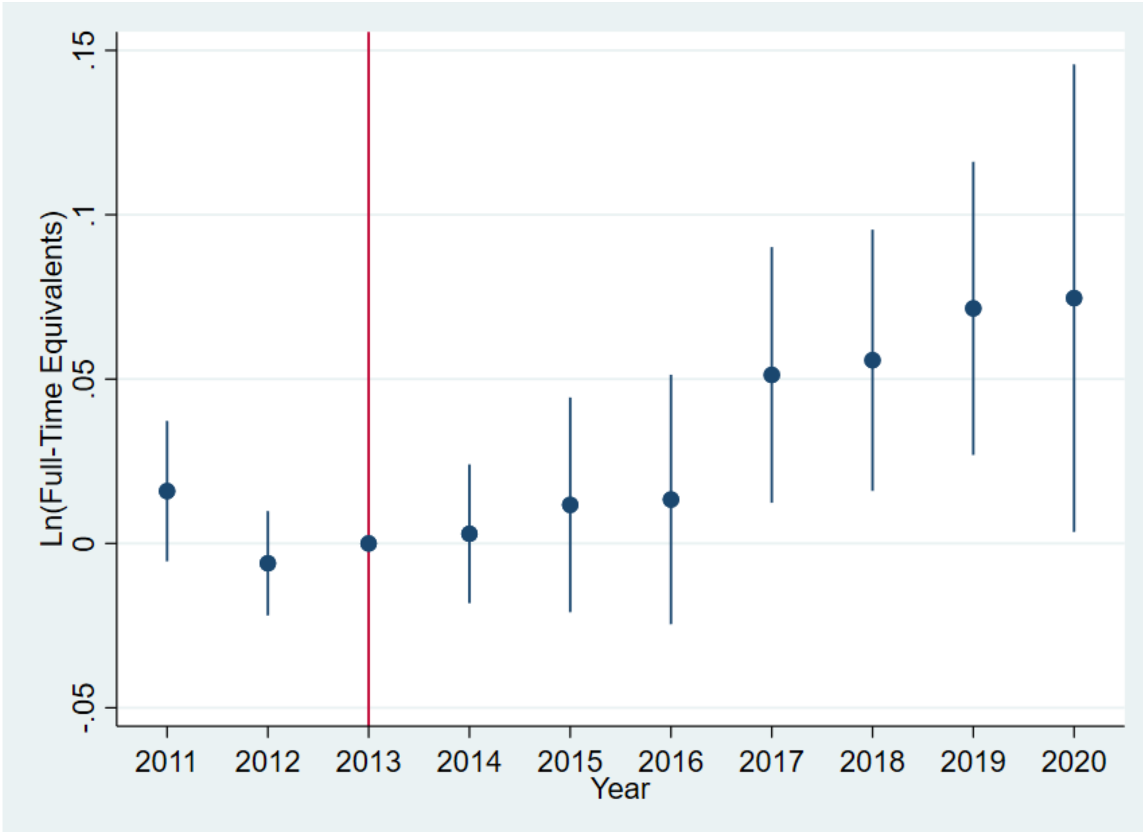


Figure 6: Event study, logarithm of real average salary by year.

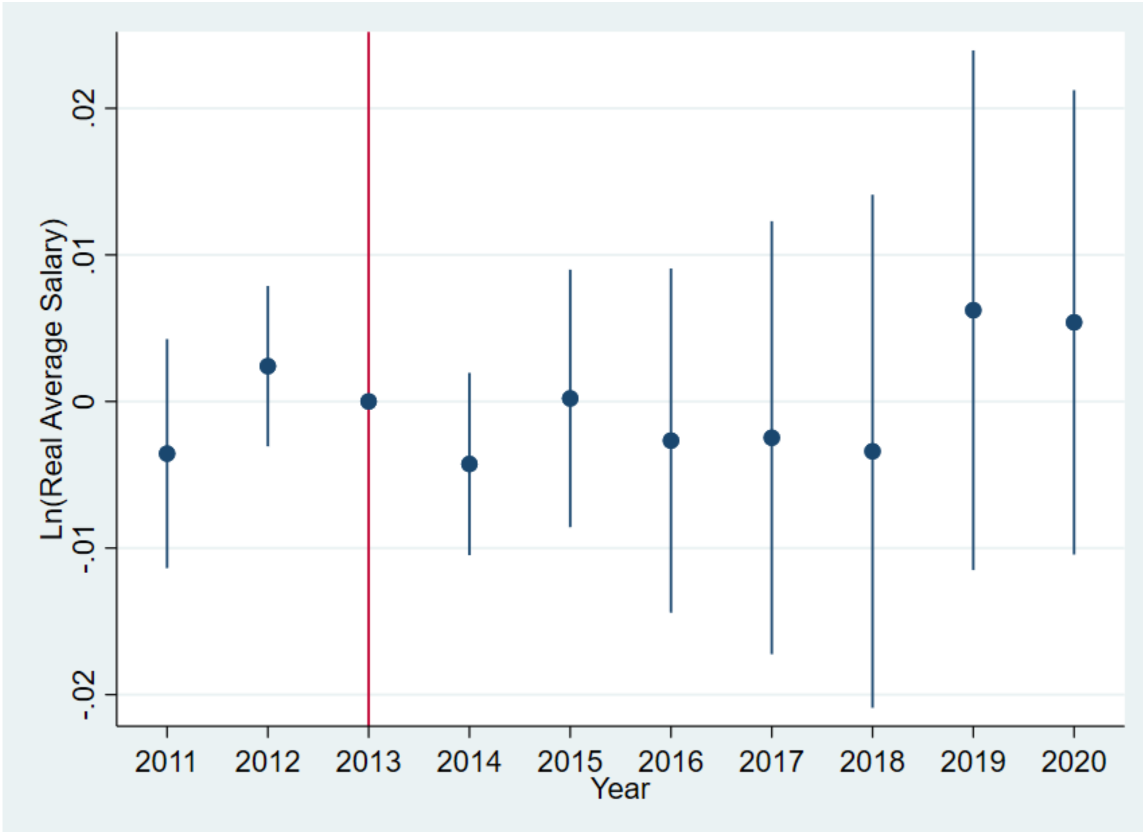


Figure 7: Event study, logarithm of number of beds by year.

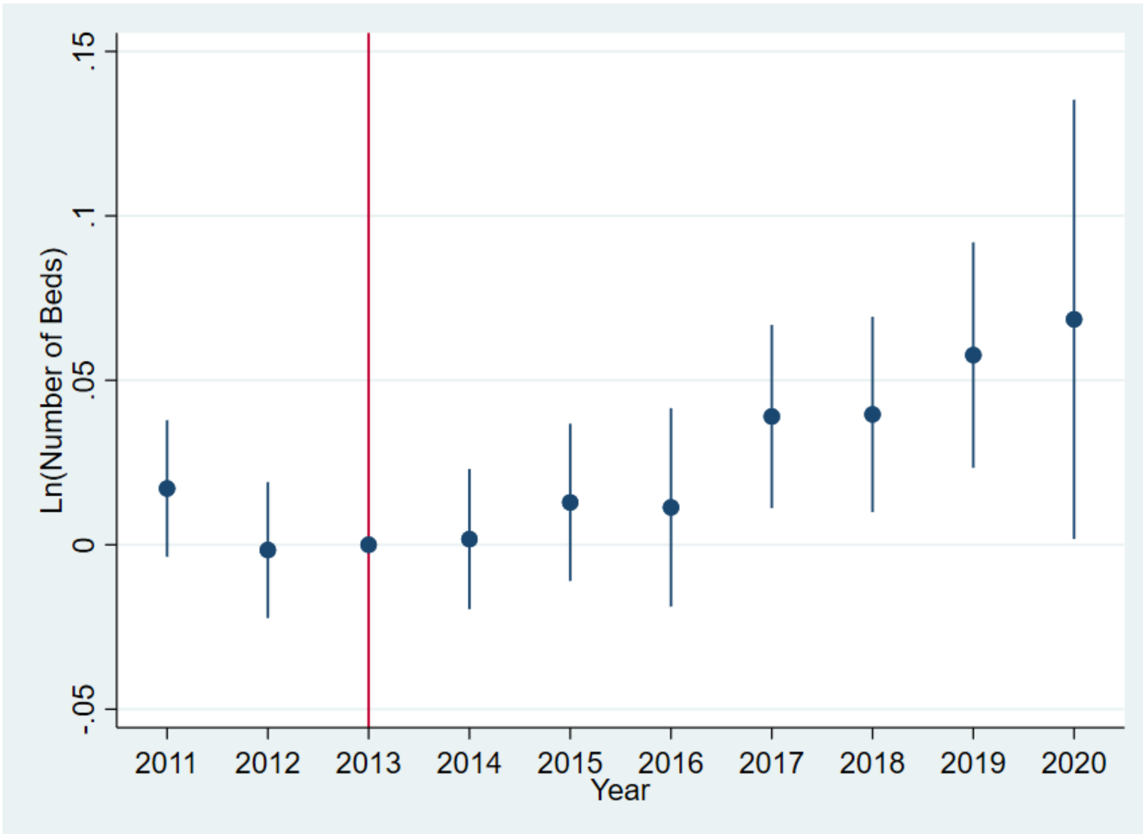


Figure 8: Event study, probability of new or revised loan or lease by year.

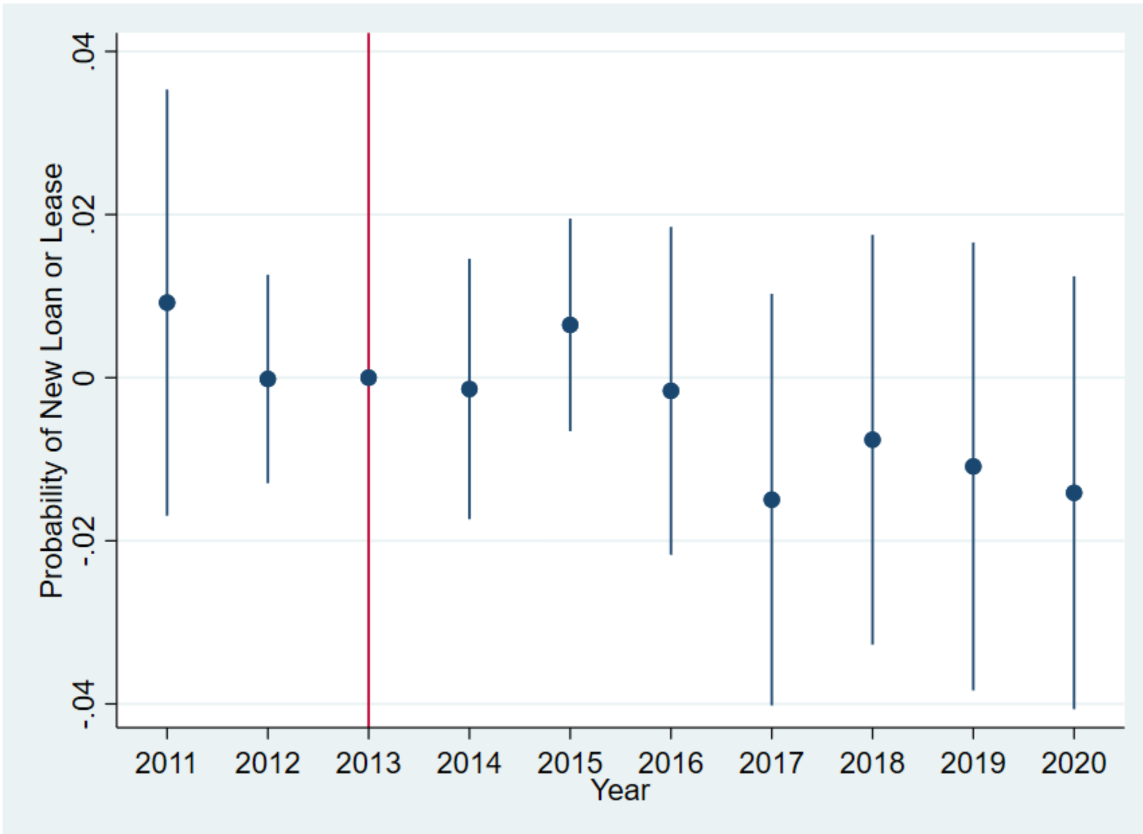
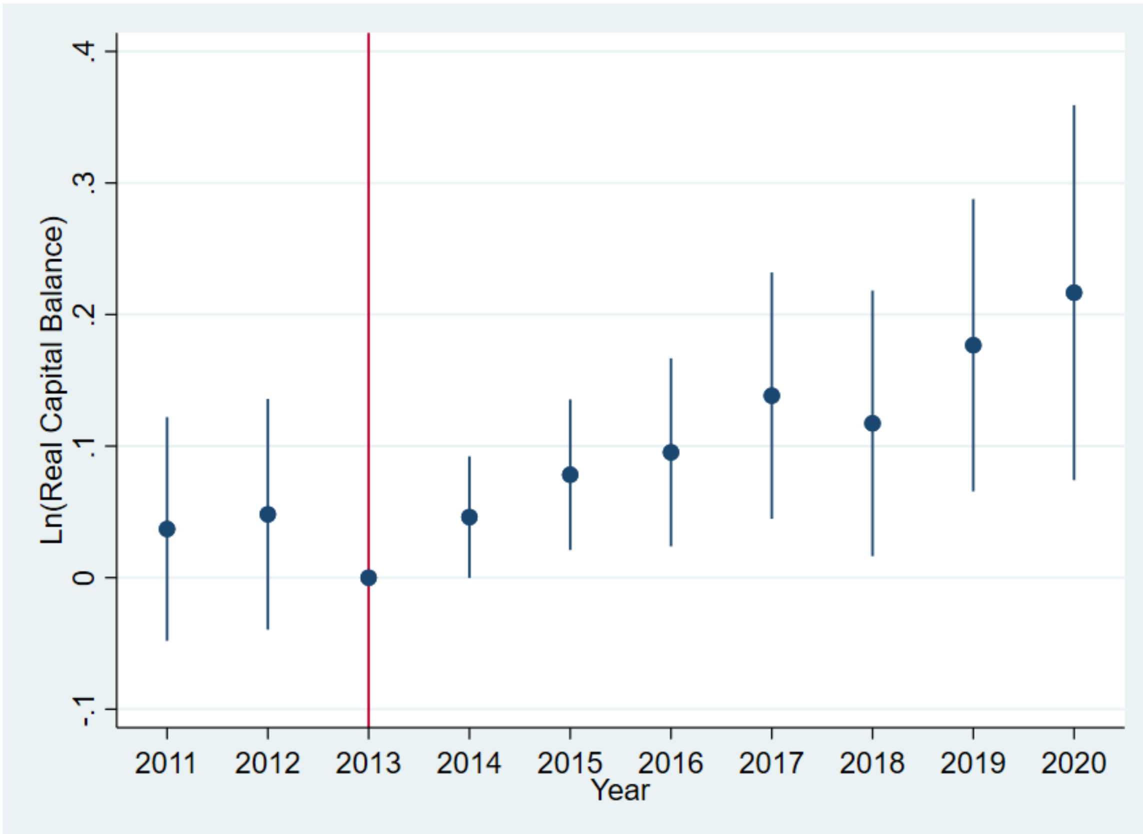
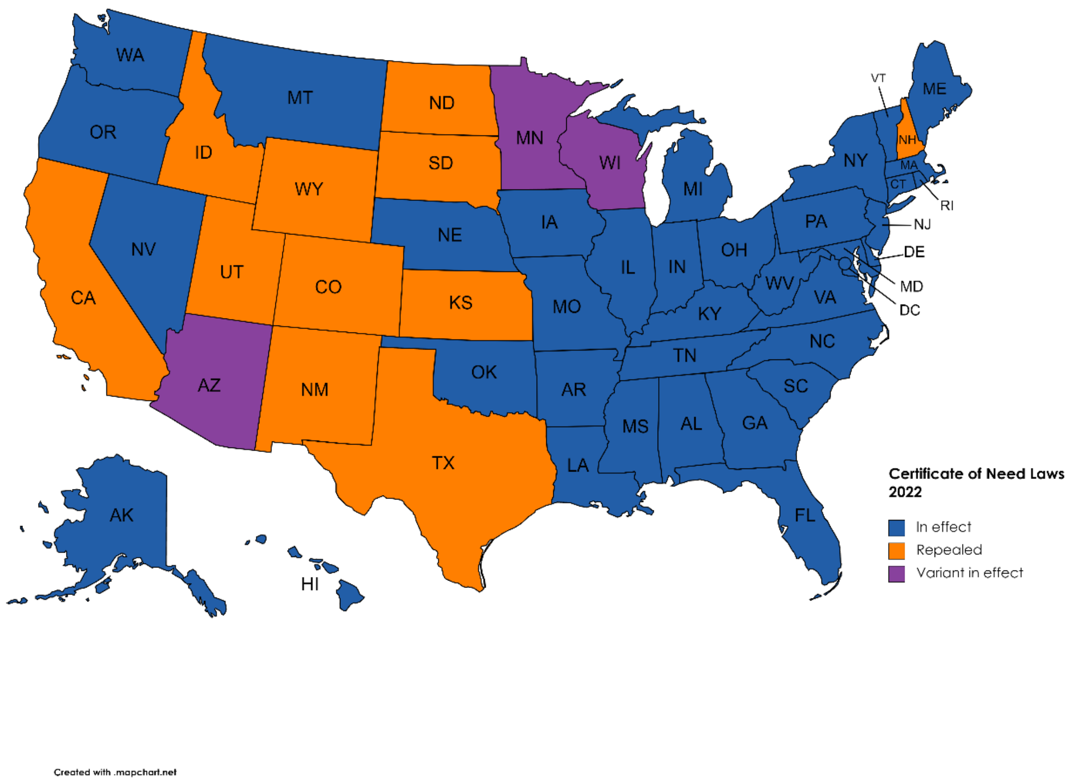


Figure 9: Event study, Logarithm of real capital balance by year.



Appendix A: Certificate of Need Laws

Figure A1: Map of certificate of need law status as of March 2022.



Appendix B: Fixed Fiscal Year.

Table B1: Difference-in-differences regression results, first-stage outcomes. January 1st fiscal year only.

Dependent variable	Ln(Medicaid Payments)	Ln(Cost of Uninsured)
Treated x Post	0.350***	-0.874***
	(0.075)	(0.194)
HRR fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Controls	Yes	Yes
R^2	0.470	0.459
N	9,060	8,400

Marginal effects; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B2: Difference-in-differences regression results, labor outcomes. January 1st fiscal year only.

Dependent variable	Ln(FTE)	Ln(Mean Salary)
Treated x Post	-0.018	0.011
	(0.026)	(0.009)
HRR fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Controls	Yes	Yes
R^2	0.467	0.728
N	9,190	9,180

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B3: Difference-in-differences regression results, capital outcomes. January 1st fiscal year only.

Dependent variable	Ln(Beds)	P(New Loan or Lease)	Ln(Capital Balance)
Treated x Post	-0.008	-0.008	0.079
	(0.020)	(0.018)	(0.078)
HRR fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
R^2	0.432	0.152	0.444
N	9,150	9,190	9,150

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure B1: Event study, Logarithm of full-time equivalents by year. January 1st-December 31st fiscal years only.

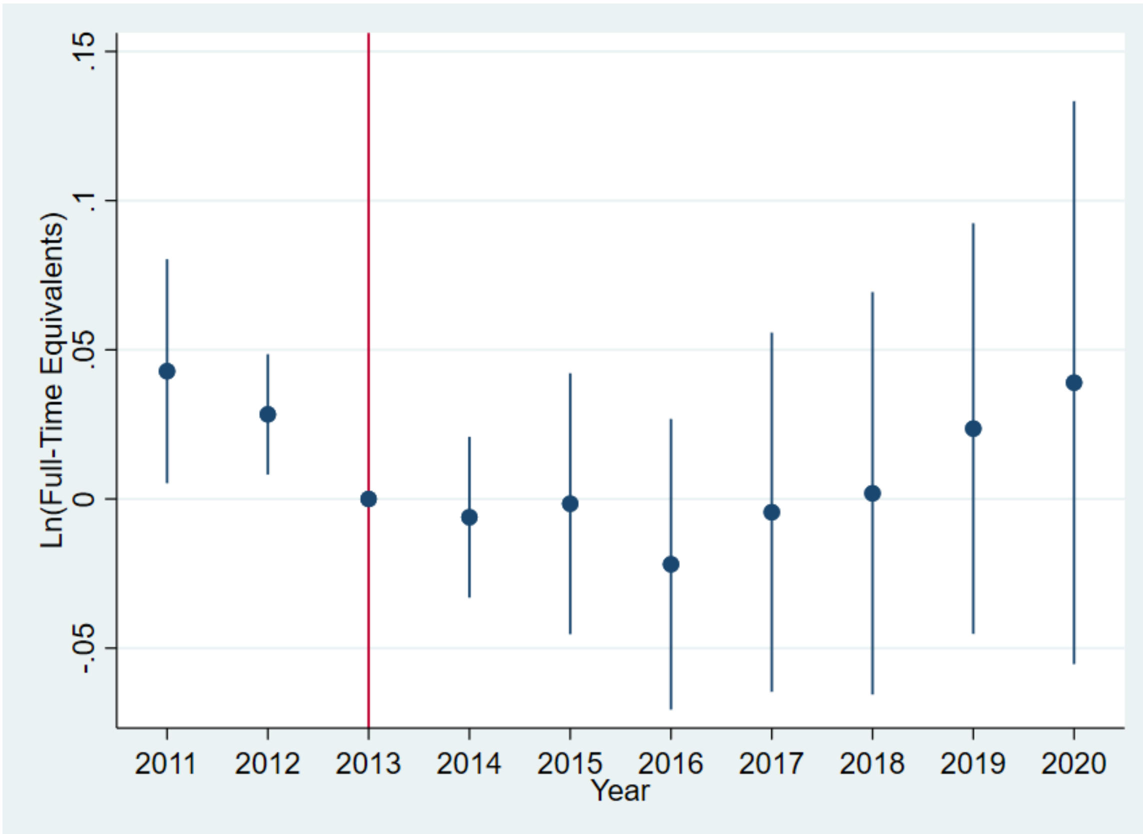


Figure B2: Event study, Logarithm of number of beds by year. January 1st- December 31st fiscal years only.

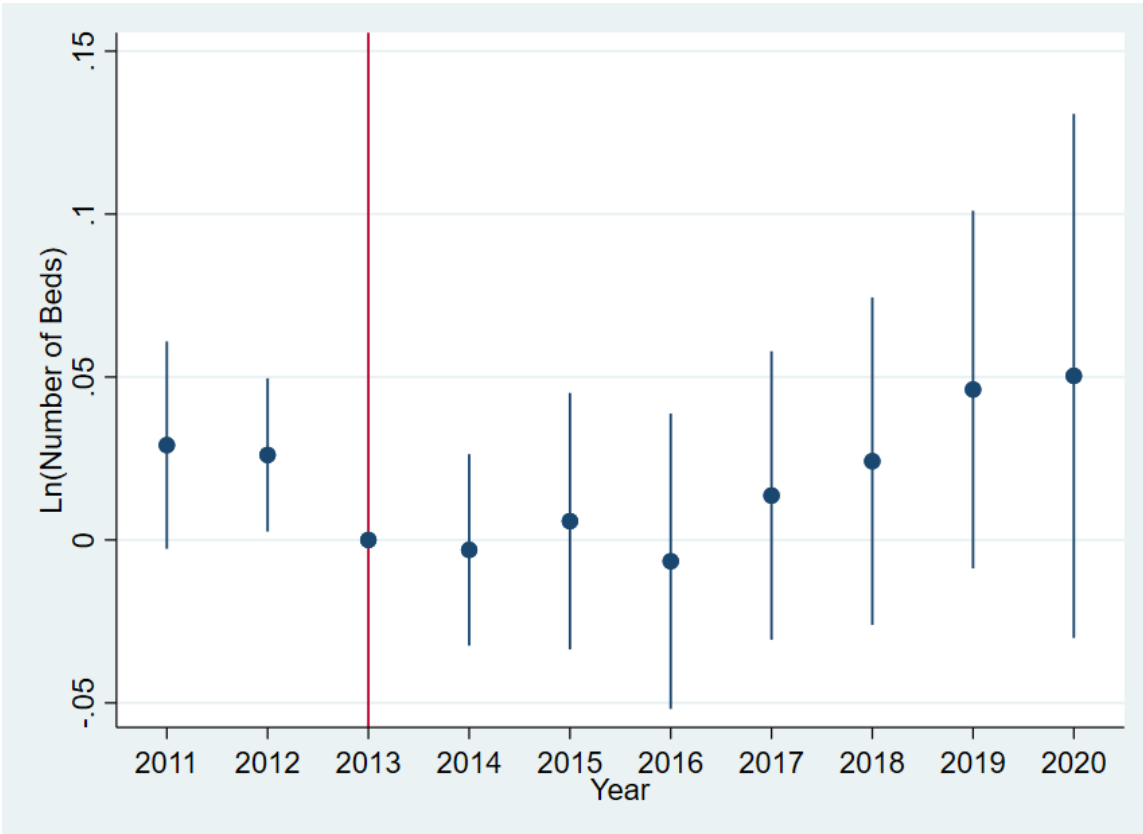
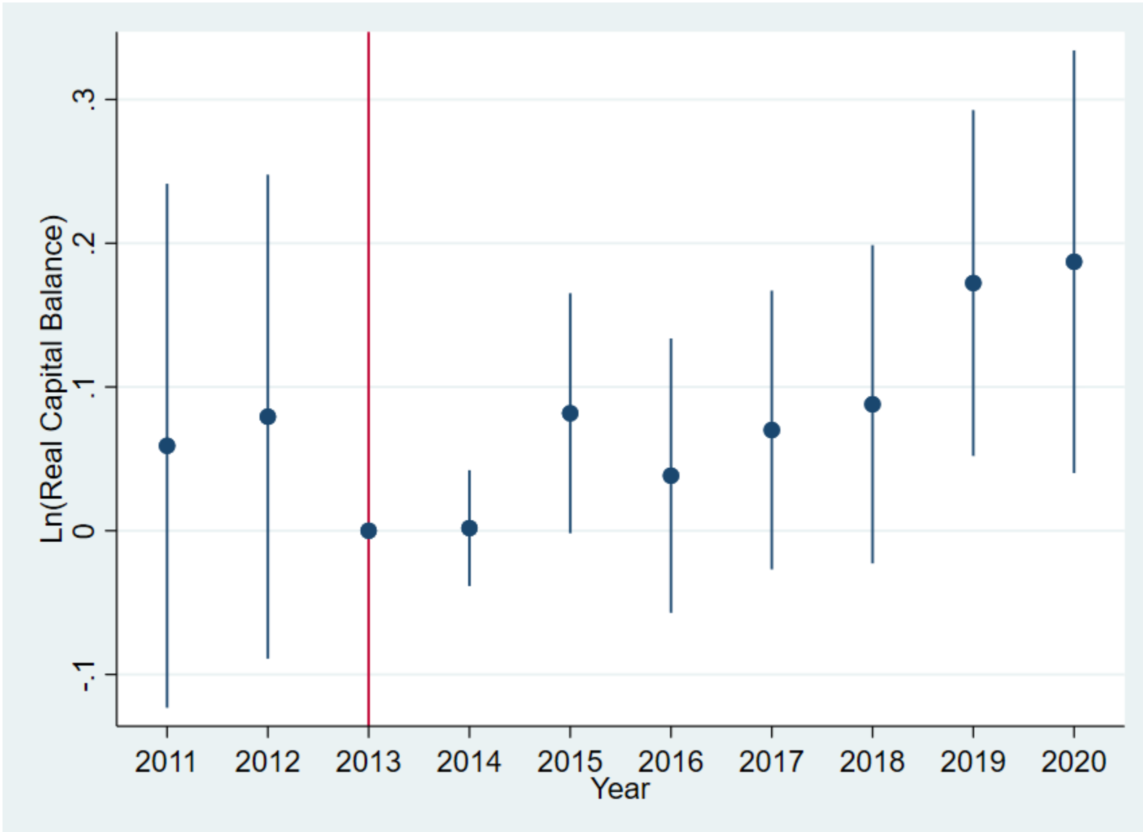


Figure B3: Event study, Logarithm of real capital balance by year. January 1st- December 31st fiscal years only.



Appendix C: Regressions with Restricted Sample

Table C1: Difference-in-differences regression results, first-stage outcomes. Restricted HRRs only.

Dependent variable	Ln(Medicaid Payments)	Ln(Cost of Uninsured)
Treated x Post	0.413***	-1.129***
	(0.048)	(0.143)
HRR fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Controls	Yes	Yes
R^2	0.392	0.361
N	13,440	12,660

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C2: Difference-in-differences regression results, labor outcomes. Restricted HRRs only.

Dependent Variable	Ln(FTEs)	Ln(Mean Salary)
Treated x Post	0.021	0.000
	(0.021)	(0.009)
HRR fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Controls	Yes	Yes
R^2	0.343	0.703
N	15,270	15,260

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C3: Difference-in-differences regression results, capital outcomes. Restricted HRRs only.

Dependent variable	Ln(Beds)	P(New or Revised Loan/Lease)	Ln(Real Capital Balance)
Treated x Post	0.015	-0.020	0.077
	(0.014)	(0.013)	(0.061)
HRR fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
R^2	0.305	0.115	0.335
N	15,160	15,290	15,000

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure C1: Event study, Logarithm of full-time equivalents by year. Restricted HRRs only.

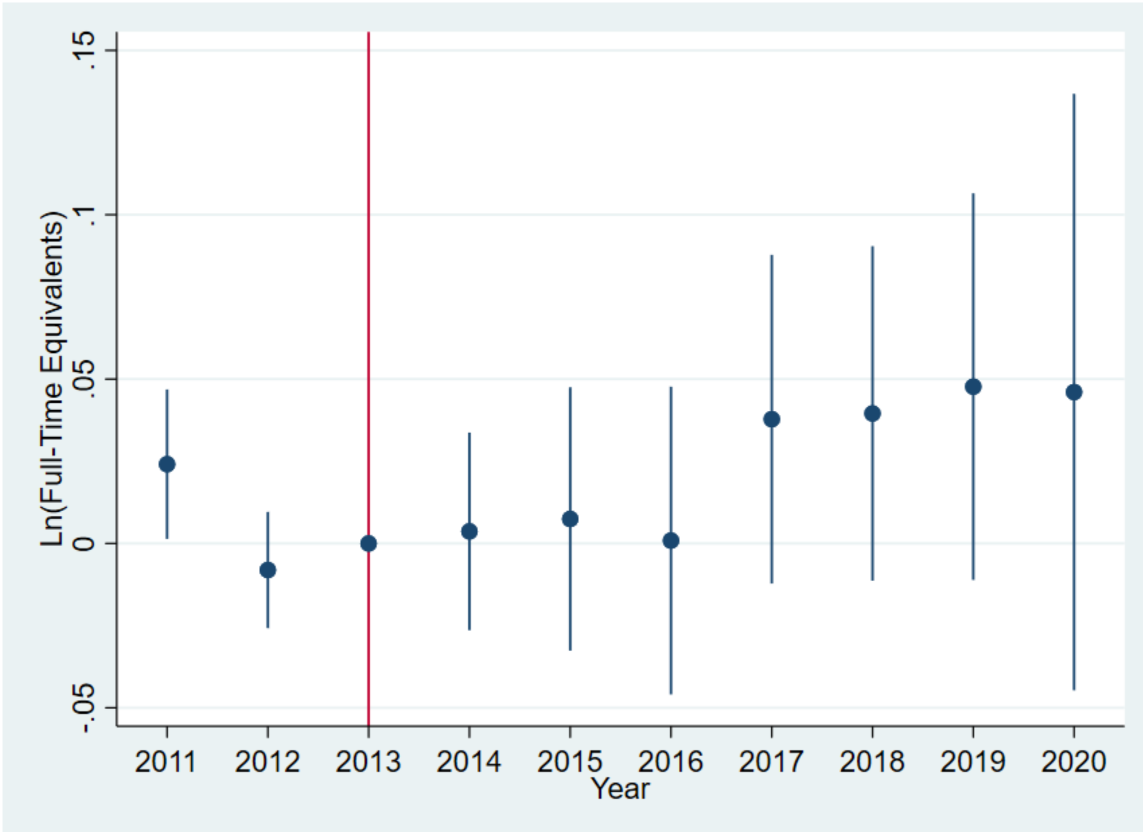


Figure C2: Event study, Logarithm of number of beds by year. Restricted HRRs only.

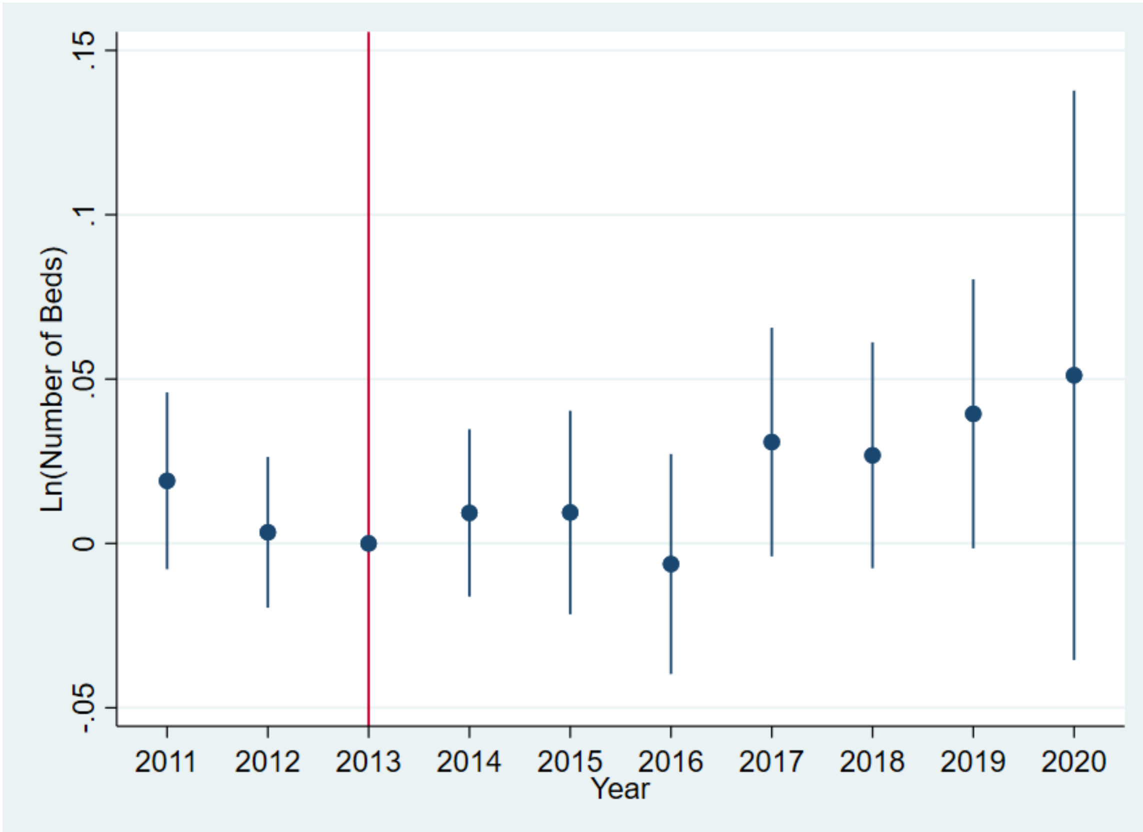
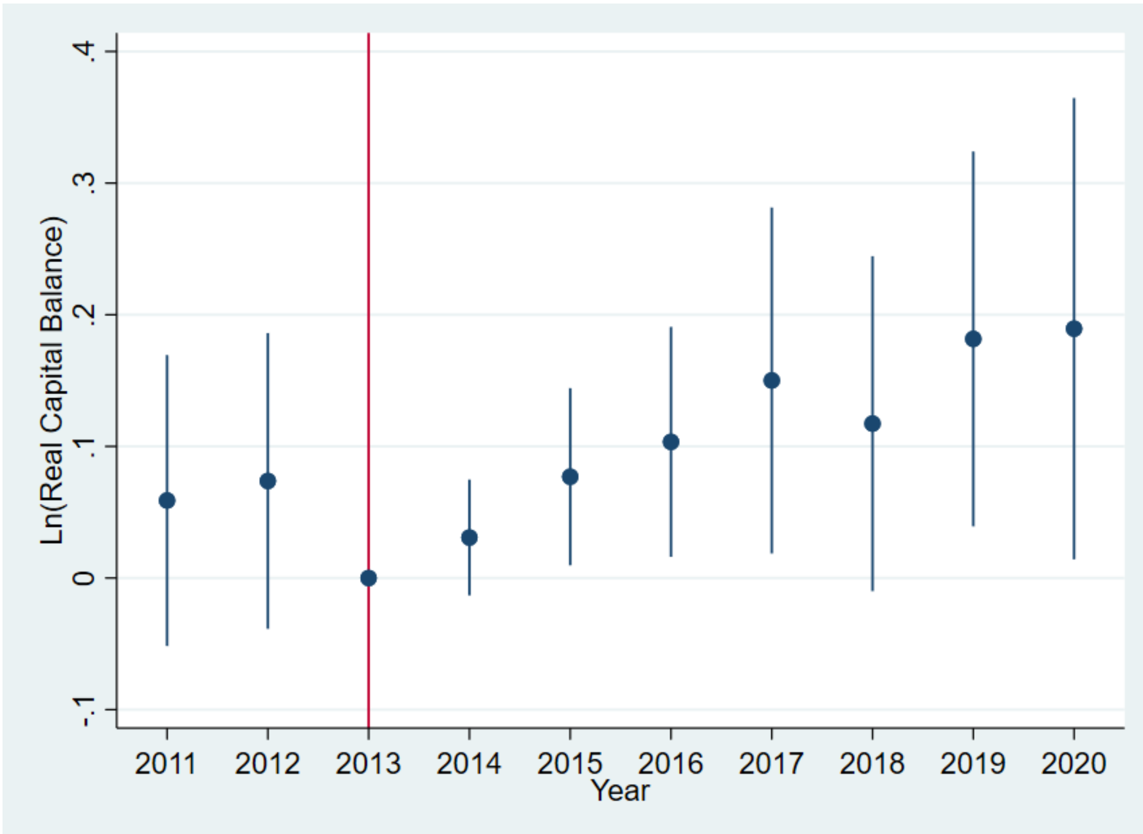


Figure C3: Event study, Logarithm of real capital balance by year. Restricted HRRs only.



Appendix D: Regressions without Hospital Controls

Table D1: Difference-in-differences regression results, first-stage outcomes, no hospital-level controls.

Dependent variable	Ln(Medicaid Payments)	Ln(Cost of Uninsured)
Treated x Post	0.336***	-0.890***
	(0.051)	(0.162)
HRR fixed-effects	Yes	Yes
Year fixed-effects	Yes	Yes
Controls	No	No
R^2	0.308	0.314
N	20,030	18,780

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D2: Difference-in-differences regression results, labor outcomes, no hospital-level controls.

Dependent variable	Ln(FTEs)	Ln(Mean Salary)
Treated x Post	0.010	-0.002
	(0.017)	(0.007)
HRR fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
Controls	No	No
R^2	0.237	0.663
N	22,590	22,550

Marginal effects; Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D3: Difference-in-differences regression results, capital outcomes, no hospital-level controls.

Dependent variable	Ln(Beds)	P(New or Revised Loan/Lease)	Ln(Real Capital Balance)
Treated x Post	0.007	-0.010	0.064
	(0.013)	(0.010)	(0.048)
HRR fixed-effects	Yes	Yes	Yes
Year fixed-effects	Yes	Yes	Yes
Hospital level controls	No	No	No
r2	0.225	0.095	0.265
N	22,480	22,620	22,170

Marginal effects; Standard errors in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure D1: Event study, Logarithm of full-time equivalents by year. No hospital controls included.

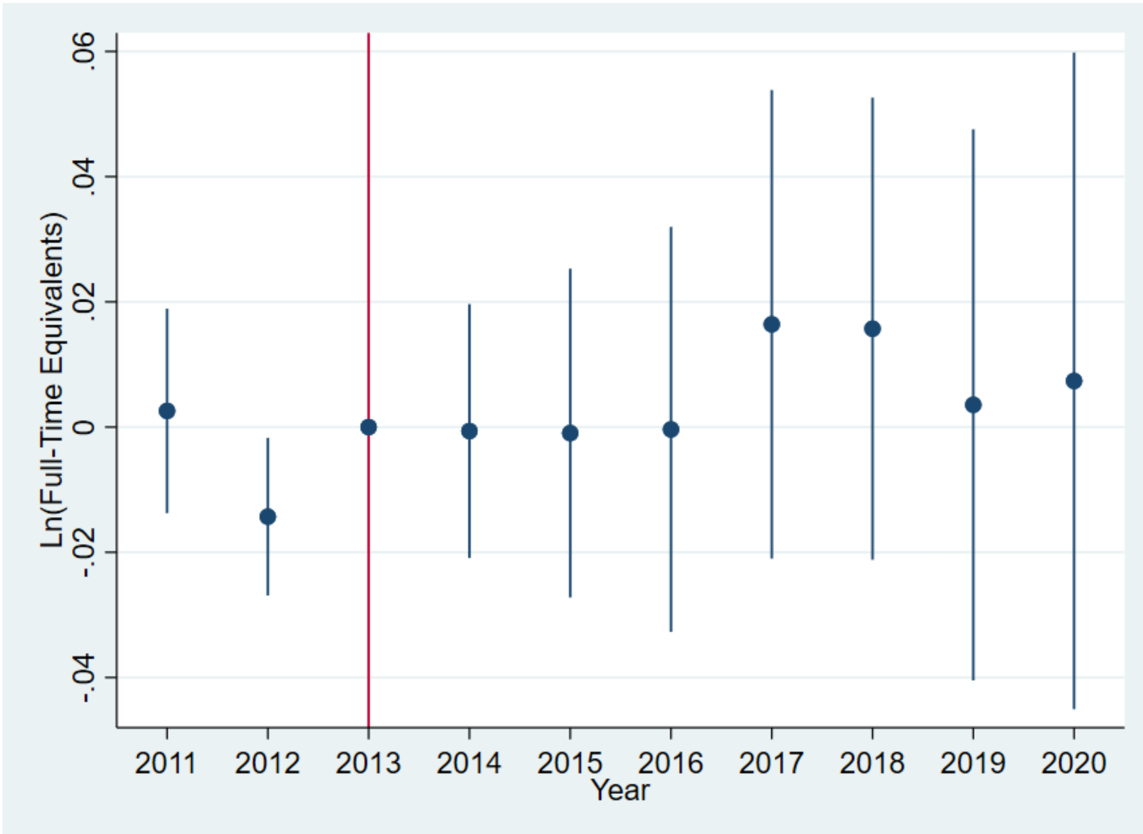


Figure D2: Event study, Logarithm of number of beds by year. No hospital controls included.

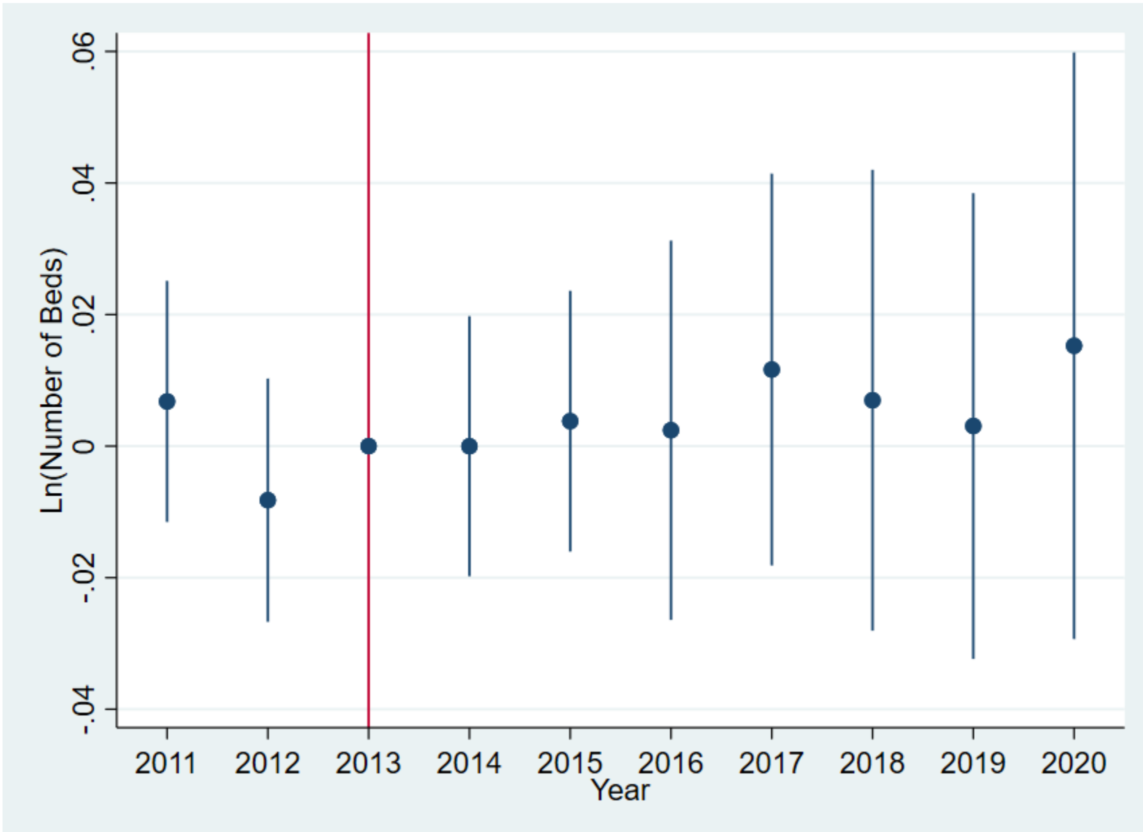
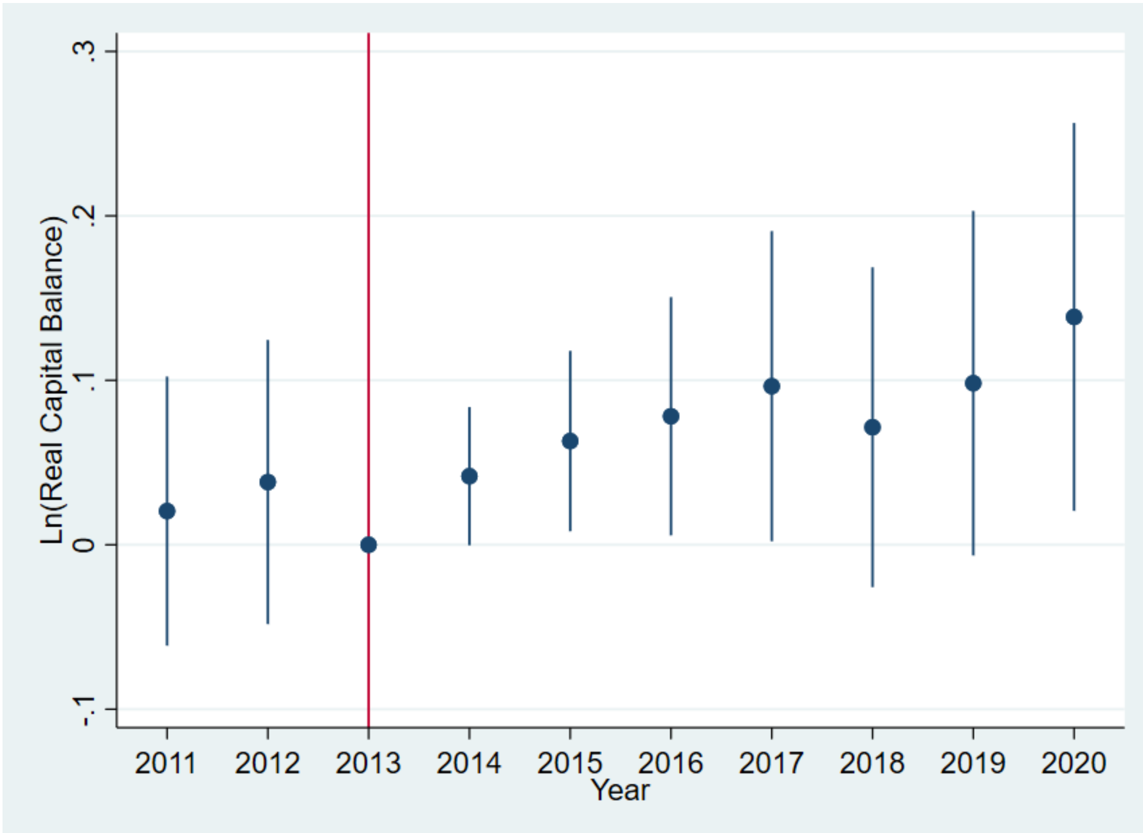


Figure D3: Event study, Logarithm of real capital balance by year. No hospital controls included.



Chapter 3: In the Land of OZ: Designating Opportunity Zones

James Alm, Trey Dronyk-Trosper, and Sean Larkin*[1]

Abstract: The Tax Cuts and Jobs Act of 2017 allowed governors of the fifty states to designate low-income areas as a "Qualified Opportunity Zone" (QOZ), which entitled the investors in these QOZs to significant tax incentives. As a result, each governor's designation of QOZs provided an opportunity for the governor to introduce investments in low-income communities that would, in principle, increase economic opportunities in these areas. At the same time, each governor's decision also provided an opportunity for the governor to reward political allies, to buy voter support, and to help business interests. Which of these many factors influenced the designation of QOZs? In this paper we estimate the impact of economic and political variables on the governors' decisions to choose which areas among all eligible areas would receive QOZ status and which would not. We find that the QOZ selection process overall seems to have been relatively technocratic, with many of the strongest factors that determine QOZ designation being indicators of economic distress such as higher rates of unemployment, welfare receipt, or lower median income, all of which are consistent with the presumed goals of QOZs. Even so, we also find that political factors are significant in QOZ designation, with Democratic representation being negatively associated with QOZ nomination and with political representation by a local politician of the same party as the governor being positively

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associated with QOZ nomination. Of some note, we also find that areas with higher college attainment are favored.

Keywords: Opportunity zones, tax incentives, place-based development policies.

JEL Codes: H24, I38, O23, R38.

1. Introduction

An important if somewhat overlooked feature of the Tax Cuts and Jobs Act (TCJA) of 2017 was the creation of the "Opportunity Zone" (OZ) program. An OZ is a designated low-income area within a state, selected by the governor of the state from census tracts in the state that meet specified eligibility requirements, with investments in an OZ then eligible for a range of generous tax incentives. At the time the TCJA was signed into law on 22 December 2017, the national unemployment rate was 4.1 percent, and the overall poverty rate was 12.3 percent. However, these national rates mask enormous heterogeneity across census tracts. The presumed intention of the OZ incentives was to encourage investment in these low-income areas in order to improve incomes, jobs, and economic development in areas that were seen as lagging behind in opportunities, especially opportunities for minority groups. In this paper we estimate the impact of economic and political variables on the governors' decisions to choose which areas among all possible areas would receive OZ status and which would not.

In the specific case of OZs, these tax incentives are of several types, of which the main ones relate to the treatment of realized capital gains on the investments. As discussed in more detail later, there is a temporary deferral of realized capital gains from a sale of an investment outside of an OZ investment, if the realized gains are reinvested in an OZ. Also, there is a step up in basis of 10 percent if the investment stays in the OZ for 5 years and a step up in basis of 15 percent if the investment is held for 7 years. Finally, all capital gains from the sale of an investment in an OZ are excluded from taxable income if the investment is held for at least 10 years. In their entirety, these tax incentives create significant tax breaks for investors, tax breaks that are of more value to higher income investors.

The TCJA specified two criteria that census tracts had to meet to qualify for these incentives, thereby receiving a special "Qualified Opportunity Zone" (QOZ) designation. To be designated, each potential QOZ must meet one of two criteria. First, the poverty rate in the census tract must be at least 20 percent. Second, the median family income in the census tract must be less than or equal to 80 percent of either the statewide median family income or the metro family median income (where applicable), whichever is higher. The governor of each state can then nominate up to 25 percent of these "low-income census" (LIC) tracts in the state as a QOZ, and up to 5 percent of all QOZs nominated can be non-LICs if these census tracts are geographically contiguous with an LIC. This process was a one-time process that was completed before the end of 2018, and in December 2018 the U.S. Treasury finalized its certification of QOZs.

In total, Treasury designated 8764 OZs in the fifty states and in Washington, D.C., Guam, Northern Mariana Islands, Puerto Rico, Samoa, and the Virgin Islands, from 42,160 potential census tracts out of a nationwide total of 74,163 census tracts.[2] All tracts that were nominated by the governor and subsequently certified by the Secretary of the U.S. Treasury become designated OZs, and investors in these OZs become eligible for

[2] The various government regulations for OZs include, among others: "Investing in Qualified Opportunity Funds", available online at https://www.federalregister.gov/documents/2018/10/29/2018-23382/investing-in-qualified-opportunity-funds; "Investing in Qualified Opportunity Funds", available online at: www.federalregister.gov/documents/2019/05/01/2019-08075/investing-in-qualified-opportunity-funds; "Treasury, IRS issue proposed regulations on new Opportunity Zone tax incentive", available online at https://www.irs.gov/newsroom/treasury-irs-issue-proposed-regulations-on-new-opportunity-zone-tax-incentive; and "Special Rules for Capital Gains Invested in Opportunity Zones", available online at https://www.irs.gov/pub/irs-drop/rr-18-29.pdf. See also Novogradic (2018), Eastman and Kaeding (2019), Nitti (2019), Tankersley (2019), and Tax Policy Center (2019) for useful information.

the tax incentives. As a result, each governor's designation of OZs provided an opportunity for the governor to introduce investments in low-income communities that will, in principle, increase economic opportunities in these areas. However, because investments held in an OZ for more than ten years can avoid virtually all taxes on new capital gains, there are strong incentives both to invest in OZs and also to exploit this tax avoidance mechanism. As a result, each governor's decision also provided an opportunity for the governor to reward political allies, to buy voter support, and to help business interests. Perhaps as a result, opportunity zones have faced increased criticism about the politicization of QOZ designation[3], including unintended consequences[4] and anticipated failures[5] of OZ designation, and these criticisms have even made their way into recent high-profit entertainment programs.[6] Indeed, as discussed later, our tabulations demonstrate that 38 of the 8764 QOZs do not appear to meet the Treasury Department's guidelines for QOZ designation, suggesting a failure in the nomination process. Some

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[3] See "A Trump Tax Break To Help The Poor Went To a Rich GOP Donor's Superyacht Marina", available online at https://www.propublica.org/article/superyacht-marina-west-palm-beach-opportunity-zone-trump-tax-break-to-help-the-poor-went-to-a-rich-gop-donor. See also "Symbol of '80s Greed Stands to Profit from Trump Tax Break for Poor Areas", available online at https://www.nytimes.com/2019/10/26/business/michael-milken-trump-opportunity-zones.html.

[4] See "Fixing America's Forgotten Places – Opportunity Zones, created by Trump's tax law, are meant to help the heartland thrive and make the country more equal, but can they pull it off?", available online at https://www.theatlantic.com/ideas/archive/2018/07/how-do-we-help-this-place/565862/.

[5] See: "The Problem with Opportunity Zones", available online at https://www.citylab.com/equity/2018/05/the-problem-with-opportunity-zones/560510/; "How a Trump Tax Break to Help Poor Communities Became a Windfall for the Rich", available online at https://www.nytimes.com/2019/08/31/business/tax-opportunity-zones.html; "Trump Tax Break That Benefited the Rich Is Being Investigated", available online at https://www.nytimes.com/2020/01/15/business/trump-opportunity-zone-investigation.html; and "Developers Rushing to Opportunity Zones for Tax Break, But Is It Helping Louisiana's Low-Income Areas?", available online at www.theadvocate.com/new_orleans/news/business/article_0ddb2d22-2576-11e9-bde9837b83173a57.html.

[6] See the episode of the HBO series *Billions* entitled "Opportunity Zone", in which the character Bobby Axelrod (or Axe) wants to invest in an OZ in the Yonkers neighborhood in which he grew up.

politicians have already begun crafting bills to address these criticisms and even to advocate the complete dissolution of the OZ program.[7]

The tax incentives included in OZs are similar to a range of "place-based development policies" that have been utilized over the years. In the United States, these place-based development policies include programs like Enterprise Zones, Renewal Communities, Enterprise Communities, the New Market Tax Credit, the Historic Tax Credit, and the Low-income Housing Tax Credit. There are also place-based policies around the world, such as Structural Funds and Enterprise Zones in the European Union and Special Economic Zones in China, among many other programs. The specific provisions of these many programs vary, but the common feature is the use of targeted incentives that are intended to encourage investment in underperforming areas. There has been much research that has examined the impact of these policies on economic development. Overall, this research has found that the success of these policies is decidedly mixed, both in the United States and abroad (Bartik, 1991, 2003, 2019; Ladd, 1994; Papke 1994; Peters and Fisher, 2002, 2004; Bondonio and Greenbaum, 2007; Billings, 2009; Hanson, 2009; Neumark and Kolko, 2010; Bowers et al., 2011; Ham et al., 2011; Hanson and Rohlin, 2011, 2013; Accetturo and de Blasio, 2012; Gobillon, Magnac, and Selod, 2012; Givord, Rathelot, and Sillard, 2013; Reynolds and Rohlin, 2014; The World Bank, 2015; Jenson 2018).[8] Indeed, preliminary work on OZs by Chen, Glaeser, and Wessel (2019) and Theodos, González, and Meixell (2020) finds that OZs

[7] On 6 November 2019 Sen. Ron Wyden (D-OR) introduced in the U.S. Senate a bill to reform the OZ program. See https://www.finance.senate.gov/imo/media/doc/Opportunity%20Zone%20Reporting%20and%20Reform%20Act%20of%202019%20Bill%20Text.pdf.

[8] See Glaeser and Gottlieb (2008), Neumark and Simpson (2014) and Duranton and Venables (2018) for recent and comprehensive surveys of this literature.

are not having their hoped-for impacts. However, this research on place-based incentives has seldom examined the factors that determine the selection of specific geographic areas for inclusion in the tax incentive program.[9] An important and recent exception is Frank, Hoopes, and Lester (2020), who examine the factors associated with QOZ selection.[10] Like Frank, Hoopes, and Lester (2020), the purpose of our paper is to estimate the impact of political and economic factors on the governors' decisions to choose which eligible census tracts would receive QOZ status and which would not.

We collect information on all eligible census tracts in the U.S., and we then estimate a variety of specifications that identify the role of economic and political variables on the QOZ designation. We find that the OZ selection process overall seems to have been relatively technocratic, with many of the strongest factors that determine OZ designation being indicators of economic distress such as higher rates of unemployment, welfare receipt, or lower median income. Even so, we also find that political factors are quite significant in QOZ designation, with Democratic representation being negatively associated with QOZ designation and with political representation by a local politician of the same party as the governor being positively associated with QOZ designation. Of some note, we also find that areas with higher college attainment are favored, which is a potential concern because higher educational attainment is positively associated with earning potential and political engagement.

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[9] An important recent exception is Frank, Hoopes, and Lester (2020), who examine the political processes associated with QOZ selection.

[10] Theodos, Meixell, and Hedman (2018) also examine QOZ selection, although their analysis of QOZ selection relies mainly on simple comparisons of the mean characteristics of OZs that are selected versus those not are designated for QOZ selection. See also Theodos and Meixell (2018), who apply similar methods to the specific case of California.

As noted, our work is similar to Frank, Hoopes, and Lester (2020), with several important distinctions. In particular, Frank, Hoopes, and Lester (2020) use a linear probability estimation strategy to focus upon the political processes associated with QOZ selection, including the engagement of external advisors and agencies in the selection process. Our paper expands their modeling to include both the linear probability estimation and logit estimation. Also, we concentrate on identifying in more detail the underlying census tract characteristics that influence QOZ selection, in addition to various political variables like lower and upper house legislative controls. Of note, we use more expansive demographic and education variables in addition to more descriptive political variables, and we also employ a more extensive array of robustness tests. Even so, our estimation results are broadly similar to those of Frank, Hoopes, and Lester (2020).

In the next section, we discuss the details of opportunity zones. We then present our data and methods, followed by our results. We conclude in the final section.

2. What is an "Opportunity Zone"? Definitions and Tax Incentives

2.1. Definitions

To facilitate our discussion, we begin with some basic definitions that define the main features of the OZ program.

A low-income census tract (LIC) is a census tract in which either the poverty rate is at least 20 percent or tracts in which the median family income is less than or equal to 80 percent of the statewide median family income or metro family median income (where applicable), whichever is higher. A related definition is a Treasury-identified census tract, which is a census tract that is contiguous with one or more LICs but which does not meet the LIC criteria.

A state governor may declare 25 percent of the LICs in the state as a Qualified Opportunity Zone (QOZ) based on 2011-2015 ACS 5-year data from the Census Bureau.[11] Note that 5 percent of all QOZs nominated can be contiguous with an LIC, rather than an LIC itself, as specified by a Treasury-identified census tract. Because of this provision, census tracts adjacent to an LIC, but not necessarily meeting the criteria for QZ nomination, may still be nominated for QOZ status. However, no more than 5 percent of the QOZs that are nominated within each state may be these contiguous tracts.

A Qualified Opportunity Fund (QOF) is a self-certified entity treated as a partnership or corporation for federal tax purposes and organized in any of the 50 states, District of Columbia, or the five U.S. territories for the purpose of investing in qualified opportunity zone property. At least 90 percent (or more) of held assets must be QOZ property.

A QOZ business is a business with substantially all of its tangible assets located in QOZs. Internal Revenue Service (IRS) regulations require that 70 percent of all tangible property held be in a QOZ, and that 50 percent of the gross income from a QOZ business be derived from active trade or conduct in a QOZ (Internal Revenue Service, 2018). Several enterprises cannot qualify as a QOZ business, including a golf course, a country club, a massage parlor, a hot tub facility, a suntan facility, a gambling facility, and stores specializing in alcoholic beverages to be consumed off the premises. A QOZ business may include houses and apartments for rent.

[11] Note that for 51 QOZs nominated late in the process, the 2012-2016 ACS data was used.

A QOZ property must be a property purchased after 31 December 2017, be qualified as a QOZ at the time of purchase, and remain qualified for substantially all of the time held. These properties include:

- QOZ Stock: Equity in a QOZ business held by a QOF.
- QOZ Partnership Interest: Partnership interest in a QOZ business held by a QOF.
- QOZ Business Property: Tangible property used in a trade or business in a QOZ if the original use of such tangible property commences with the QOF or the QOF substantially improves the tangible property, where "substantial improvement" means that during any 30-month period *additions* to the tax basis of the building (excluding land values) are made such that the value added to the tax basis is higher than the adjusted taxpayer basis at the beginning of any 30-month period.

Note that a 90 percent investment in a business with a 70 percent QOZ business property means that there must be a minimum 63 percent investment in QOZs for a QOF.

2.2. Tax incentives

There are three tax incentives from investing in a QOF. First, there is a temporary deferral of realized capital gains from a sale outside of an OZ if reinvested in a QOF, which must be realized (and taxed) when the property is sold or at the end of 2026, whichever occurs first. An investor must invest in a QOF within 180 days of realizing the capital gains to qualify for deferment.

Secondly, capital gains newly invested into a QOF will receive a step-up in basis of 10 percent if the investment is held for 5 years, and another 5 percent (for a total of 15 percent) if held for 7 years. This provision enables investors to reduce 15 percent of their capital gains invested into a QOF from taxable income if held for the full 7 years.

Third, there is permanent exclusion from taxable income of capital gains from the sale or exchange of an investment in a QOF if the investment is held for 10 years. This incentive only applies to gains accrued after an investment in a QOF. As a result, capital gains earned before investment in the QOF receive benefits from the first and second tax incentives, while capital gains earned after investing in the QOF benefit from this third incentive.

In their entirety, these tax incentives mean that, for an investment that is held for ten years, all unrealized capital gains used for investment in a QOF will not be taxed until 2026, only 85 percent of the original capital gains invested will be taxed (100 percent would have been if realized originally), and no taxes will be paid on the appreciation of the investment. These represent quite significant tax breaks for investments in a QOF. Given that the marginal tax rate on capital gains varies from 0 percent for low income earners to 20 percent for higher income earners, these tax benefits will be of more value to higher income investors.

As an example that illustrates the magnitude of these benefits, consider the case of an individual facing a 20 percent capital gains tax rate who sells stocks, earns $1 million in capital gains on these sales, and then reinvests these capital gains in a QOF that earns $50,000 every year. After 6 years, the investor will have made $1,300,000 (or the initial $1,000,000 in capital gains plus $300,000 from the [6 X $50,000] in returns each year). Selling this QOF in its entirety would result in capital gains taxes on $300,000 of earnings, plus $900,000 from the original investment due to the step up in basis (e.g.,

"…if the investment is held in the QOF for 5 years"), thereby reducing the capital gains tax base by $100,000. Selling the QOF after 8 years would result in earnings of $1,400,000 but capital gains taxes on only $850,000 of the original investment plus the $400,000 in newly earned capital gains (e.g., "…if the investment is held in the QOF for 5 years, up to a total of 15 percent if the investment is held in the QOF for 7 years"), reducing the capital gains tax base by $150,000. However, selling the investment in year 11 would result in capital gains taxes on only the initial amount less the 15 percent reductions because of the permanent exclusion of capital gains from holding the investment for 10 years (e.g., "…there is permanent exclusion from taxable income of capital gains from the sale or exchange of an investment in a QOF if the investment is held for 10 years"). All of accumulated capital gains from the QOF investment would avoid the 20 percent capital gains tax rate, and only $850,000 of the initial $1 million in capital gains would be subject to the capital gains tax rate, and any additional capital gains earned would be received tax free.

3. Data and Methods

Our data come from several sources. First, our data on designated opportunity zones and all LICs (including Treasury-identified census tracts) come from the U.S. Department of the Treasury Community Development Financial Institutions Fund. Demographic and economic data come from the American Community Survey (ACS) five year survey of 2011-2015.[12] The ACS data include information at the census tract

[12] Note that there are more recent ACS data from the 2012-2016 survey. We use the ACS data from the 2011-2015 survey because these are the data that were available at the time of QOZ designation by the governors of the states.

level on counts of sex, age, race, median house value, median household income, population, employment status (including the unemployment rate), educational attainment, and public assistance recipients. The ACS data also contain information on median income at the county and state levels. We aggregate the county-level income data to the metropolitan level by using a county-metropolitan area crosswalk provided by the National Bureau of Economic Research. We use this information to construct economic and demographic variables at the census tract level, including: *Median House Value*, *Unemployment Rate*, *Median Household Income*, *Proportion with Less Than High School Diploma*, *Proportion with 4-year Degree*, *Proportion Black*, *Proportion Hispanic*, *Proportion Native American*, *Proportion Under 18*, *Proportion Over 65*, and *Proportion on Welfare*.

We also obtain data on institutions of higher learning from the Census of Institutions of Higher Learning taken from the U.S. Department of Homeland Security Homeland Infrastructure Foundation Level Data. We use this information to construct a dummy variable *Higher Education Campus*, equal to 1 if there is an institution of higher learning located in a census tract and 0 otherwise. We include this variable on college campus locations because locations with college campuses may more easily meet the LIC requirements since students can be included in the poverty rate calculations.[13] We also create a dummy variable *In Metropolitan Area*, equal to 1 if the census tract is located in a metropolitan area and 0 otherwise.

[13] For example, see "Opportunity Zones Knock Where They're Needed Least", available online at https://www.wsj.com/articles/opportunity-zones-knock-where-theyre-needed-least-11568412633. See also Gelfond and Looney (2018). We are grateful to an anonymous referee for this suggestion.

For political variables, we use data from Ballotpedia and state legislator websites to match both upper and lower house state legislators and their party affiliations to each QOZ tract, using representatives listed at the time of OZ nomination in March 2018. From the same datasets, we also include governor party affiliation at the same date. Since governors are the final arbiters of deciding which OZs will be nominated, it is important to control for potential partisan selection. This procedure allows us to create several variables that examine the representation of each census tract in the state legislature. The first two variables measure the percent of the census tract represented by Democrats in the lower or upper chamber of the legislature (*Percent of Tract Represented by Democrat (Lower)* and *Percent of Tract Represented by Democrat (Upper)*). The other two variables are dummy variables that measure whether the majority of the geographic area of the census tract is represented by representatives in the lower or upper legislature chamber who are affiliated with the same political party as the current governor (*Legislature-Governor Partisan Match (Lower)*, *Legislature-Governor Partisan Match (Upper)*). These variables are coded as dummy variables with 1 indicating a match, and 0 otherwise. Because governors select which OZs will be nominated, their relationship with co-partisans and opposing parties may play a role in which OZs they select. Similarly, legislators may lobby the governor for certain tracts in their legislative districts to be nominated, and the governor can reward or punish legislators through the selection of nominated OZs.

Starting with 74,133 census tracts in the entire U.S., we remove tracts in Nebraska (because of its unicameral legislature), and we also drop census tracts outside the continental U.S. (Alaska, Hawaii, and U.S. territories) and in Washington, D.C. This results in a sample size of 71,847 census tracts. This sample size is further reduced to 71,250 because we remove census tracts that are considered unpopulated in the ACS survey. Finally, *Median House Value* and *Median Household Income* information are not included for all census tracts, restricting the sample of census tracts to 69,921. From these 69,921 observations, we then choose the census tracts that are eligible for designation as either a LIC or Treasury-determined census tract. These total 29,549. We call these census tracts *Potential QOZs*; that is, Potential QOZs are the tracts that could *potentially* be chosen by the governor of each state. From these Potential QOZs, ultimately 7410 were selected by state governors to receive what we term *Designated QOZs*. Our goal is to estimate the factors that determine the selection of the 7410 Designated QOZs from the 29,549 Potential QOZs.

Of the states in the sample, California has the most Designated QOZs (879), followed by Texas (628), New York (514), Florida (427), and Illinois (327); the states with the fewest QOZs are Alaska, Delaware, Hawaii, Montana, North Dakota, Rhode Island, South Dakota, Vermont, and Wyoming, all with 25. The top city locations for Designated QOZs are New York City (306), followed by Los Angeles (274), Chicago (181), Houston (105), and Detroit (94). A map of the location of these QOZs is shown in Figure 1.

Summary statistics of our variables for all census tracts, for Potential QOZs, and for Designated QOZs are given in Table 1. All proportion variables (e.g., *Proportion with Less Than High School Diploma*) are coded from 0 to 1, with 1 being 100 percent of the population and 0 being zero percent. Nominal variables like *Median House Value* and *Median Household Income* are in thousands of dollars (USD), and *Population* is measured in thousands. Not surprisingly, *Median Household Income* and *Median House Value* are lower in Potential QOZs and Designated QOZs relative to similar measures across all census tracts, and the *Unemployment Rate* is higher in Potential and Designated QOZs relative to the national average. Also, the proportion of the census tract with a college degree is lower for Potential and Designated QOZs than in the U.S. on average, while the *Proportion Black* is much higher in Potential and Designated QOS than in all census tracts. The *Proportion Over 65* years of age is not statistically different in the various census tract measures.

Our estimation strategy then estimates the factors that determine the choice of Designated QOZs from all possible Potential QOZs. We follow the public choice literature by estimating the impact of economic and political variables on the selection of Designated QOZs from all Potential QOZs, using the following model:[14]

$$QOZ_i = \beta + \alpha\ Demographic_i + \gamma\ Political_i + \delta\ Metropolitan_i + \theta\ State_i\ + \varepsilon_i, \quad (1)$$

where QOZ_i is a dummy variable equal to 1 indicating a census tract that was designated by the governor of the state and approved by the U.S. Treasury as a *Designated QOZ*, and 0 indicating a QOZ that met Treasury guidelines but was not designated as an OZ. The variables $Demographic_i$, $Political_i$, and $Metropolitan_i$ represent demographic, political, and metropolitan dummy control variables, respectively. Finally, $State_i$ includes state specific dummy variables that control for time invariant, state-specific

[14] For a comprehensive recent survey of the empirical literature on the impact of economic and political variables on a wide range of outcomes, see Potrafke (2018); for an earlier but still useful survey, see Besley and Case (2003). See especially empirical papers on the role of economic and political variables in state government policy decisions, including Alt, Lessen, and Skilling (2002), Chang, Kim, and Ying (2009), Alm and Rogers (2011), Fredriksson, Wang, and Warren (2013), Pickering and Rockey (2013), Joshi (2015), Beland and Oloomi (2017), and Hill and Jones (2017).

effects between census tracts in each state. Equation (1) is estimated via logit and linear probability (LP) models, with standard errors clustered at the state level.

It should be noted again that our approach is similar in some respects to the approach of Frank, Hoopes, and Lester (2020). However, there are some significant differences in estimation methods, variable definitions, and model specifications, even aside from minor differences like our use of the unemployment rate versus their use of the employment rate. For example, they include the poverty rate in a census tract as an explanatory variable; we do not include this variable because the poverty rate is in fact one of the two criteria for QOZ designation and its inclusion as an explanatory variable may lead to biased coefficient estimates. For demographic controls, they include only the percent of a census tract that is white, while we include a much richer array of demographic controls, including age variables. For education controls, they include only the percent with at least a high school education; we include this variable as well as the *Proportion with At Least 4-year Degree*. Geographic controls differ across the two studies, including our use of a variable that measures the presence of a higher education campus. Of special note, Frank, Hoopes, and Lester (2020) include only a measure of lower state house partisanship, along with variables that attempt to capture the process by which QOZ designation occurs; we include a similar measure of lower house partisanship along with additional measures of upper house partisanship and of legislature-governor partisanship. Even so, our estimation results are broadly similar to those of Frank, Hoopes, and Lester (2020).

4. Results

Logit regression results are presented in Table 2, and LP regressions are given in Table 3. Models 2, 4, 6, and 8 include state fixed-effects, while models 1, 3, 5, and 7 exclude state fixed-effects. Also, models 3, 4, 7, and 8 include contiguous OZs in the sample. Our preferred specifications are models 4 (logit) and 8 (LP).

Looking at economic variables first, we find that census tracts with a higher proportion of population unemployed are statistically insignificant but positively correlated with OZ selection. The proportion of welfare recipients is significant predictor of OZ designation, with a 1 percentage point increase in welfare recipients leading to a 0.37 to 0.42 percentage point increase in OZ designation likelihood. Median household income is negatively and significantly correlated with OZ designation, and an increase in the median household income of a census tract of 10 percent decreases the likelihood that it is designated as a QOZ by 1.4 to 1.5 percentage points. These results are similar to Frank, Hoopes, and Lester (2020).

We estimate that rural and micropolitan tracts are favored in QOZ designation over metropolitan tracts, as can be seen by the negative and significant coefficient on *In Metropolitan Area*. Across all specifications, being in a metropolitan area decreases the likelihood of QOZ designation by about 8 percentage points. We also find that, as the share of the population over 65 increases, a census tract is less likely to be selected as an OZ. The coefficient on *Proportion Over 65* is relatively large in magnitude compared to the other estimated coefficients; however, the small differences in this variable across census tracts implies that this variable is relatively unimportant for QOZ designation. Notably, the proportion of the population below 18 is also negative, but only statistically significant for the logit regression. As for race, the proportion of the population that identifies as Black is positively and significantly associated with QOZ designation, a result that is different than Frank, Hoopes, and Lester (2020), while *Proportion Hispanic* and *Proportion Native American* are both negative but insignificant. Census tracts with higher (logged) populations are also more likely to be designated as QOZs.

Perhaps surprisingly and importantly, census tracts with higher rates of college diploma attainment are more likely to be designated as a QOZ. An increase of 1 percentage point in the percentage of the population with at least a bachelor's degree increases the likelihood that the tract is designated as a QOZ by 0.16 to 0.18 percentage points. The relatively large impact from increasing bachelor's degrees may indicate that QOZs are being selected at least partially based on expected future gentrification. As Rosenthal (2007) notes, increasing social capital in neighborhoods is a significant predictor of gentrification, and our evidence that QOZ designations are more likely to be associated with whether a tract is an "up-and-coming" area over census tracts with less rosy future expectations is consistent with Rosenthal (2007). Indeed, we also find that designated OZs are more likely to have an institute of higher learning within their borders. A census tract with a degree granting institution has an increased designation likelihood between 7.9 and 8.8 percentage points.[15]

These results are largely consistent with the intended purpose of the OZ program; that is, our results indicate that the designation process tended to favor those communities with more unfavorable economic conditions, even though the selection process also seemed to favor OZs with higher future growth expectations.

[15] See also Papke (1994) for a similar result on gentrification.

Even so, our estimation results also demonstrate that political variables matter in important ways. A census tract that has a higher proportion of representation by Democrats in the state lower legislative chamber is negatively and significantly associated with QOZ designation, while increased representation by Democrats in the upper legislative chamber is negatively but not significantly correlated with QOZ designation. For every additional percentage point of a census tract represented by a Democrat, the OZ is 0.02 percentage points less likely to be designated a QOZ. Further, a tract mostly represented by politicians in the state lower legislative chamber that are in the same party as the state executive is positively and significantly associated with QOZ designation, increasing the likelihood of designation by 2.7 percentage points. Also, a matching of parties in the upper legislature has a positive even if insignificant impact on QOZ designation. Frank, Hoopes, and Lester (2020) also tend to find that partisan matching increases the likelihood of QOZ designation. Such partisan matching may be indicative of governors acting to reward members of their own party through nominating OZs within selected members' legislative districts. This combination of results implies that governors are more responsive to lower house representative partisanship compared to the legislative upper house. This result likely represents the fact that lower house legislators generally represent smaller populations and thus may be more sensitive to any policy impacts at the census tract level than upper house legislators.

Additionally, we test for whether there is heterogeneity in these coefficients based on governor partisanship. To do this, we estimate separate regressions based on whether the state governor identifies as Republican or Democratic. These results are in Table 4. While both Republican and Democratic governors react similarly to the presence of higher education institutions, metropolitan census tracts, and median household income, partisanship is correlated with different responses on other variables. Republican governors are more likely to designate OZs with higher proportions of non-high school graduates, while Democratic governors are more likely to designate a census tract as a QOZ if the tract contains more 4-year degree holders. Both of these effects are nearly the same size in magnitude, with a one percentage point increase in each demographic leading to a 0.21 percentage point increase in the likelihood of QOZ designation. In addition, for Republican governors, increasing proportions of either Hispanic or Native American populations result in a lower likelihood of census tract selection. A one percentage point increase in these populations leads to a reduction in selection likelihood of 0.22 and 0.21 percentage points, respectively. Note that average population proportions of Native Americans are relatively low, at about 1 percent across low-income census tracts. In contrast, there is a much higher average proportion of the Hispanic population across census tracts of between 22 and 23 percent. In combination with a high standard deviation for *Proportion Hispanic* in LIC tracts, our estimation results imply that Republican governors are relatively sensitive to Hispanic concentrations in their QOZ designation. As for *Proportion Black*, the proportion of Black residents in a census tract is no longer statistically significant for either Democratic or Republican governors.

Note that we have estimated a wide range of alternative specifications as robustness tests. For some state legislatures, districts vote for multiple representatives. Since there is no way to divide census tracts in these areas between the representatives, we run a restricted model with just those census tracts that have a single representative. Table 5 shows the results for both logit and LP estimations. Our coefficient estimates are largely unchanged in both sign and magnitude, with exception of a negative and statistically significant sign on the proportion of census tracts represented by Democrats in the state's upper house. This result implies that multiple representatives for one district reduce the impact of upper house senator partisanship.

In other unreported results, we have included a variable intended to rank census tracts on the amount of investment flows that they have recently received, a variable constructed by Theodos, Meisell, and Hedman (2018) and used by Frank, Hoopes, and Lester (2020), and we find that this variable has a small and positive impact on QOZ designation; our other results are not affected. We have also identified what we term "Suspicious" QOZs, or census tracts that do not meet the stated criteria for Designated QOZ status but are so designated anyway. Tables 6 and 7 give information on these Suspicious QOZs. These tracts differ significantly from the "typical" Designated QOZ, with higher income and lower poverty rates that do not meet the official criteria. Also, these Suspicious QOZs have half the rate of welfare recipients, they are less likely to be in a metropolitan area, and they are less likely to be represented by a Democrat. Even so, omitting these Suspicious QOZs from our various estimations does not affect our earlier results in any significant way, and we do not report these results.[16]

5. Conclusions

On the whole, the QOZ selection process seems to have been relatively technocratic, with many of the strongest factors being indicators of economic distress such as greater unemployment, more welfare recipients, and lower median household

[16] All estimation results are available upon request.

income. Median household income in particular is a significant predictor of QOZ selection, with a 10 percent increase in median household income reducing the likelihood of selection by 1.4 to 1.5 percentage points. Additionally, we find evidence that QOZ designation is correlated with census tracts that are already experiencing demographic changes visible through the increased presence of college educated individuals, which may in turn lead to higher future incomes and housing wealth through gentrification.[17]

However, we also find that political partisanship is an important consideration. For example, Democratic representation in a census tract is negatively associated with QOZ designation (a 0.02 percentage point decrease in QOZ designation for every additional percent of a tract represented by a Democrat), and political representation by a local politician of the same party as the governor is positively associated with QOZ designation (a 0.03 percentage point increase when affiliation is shared), effects that are largely restricted to lower house representatives. Further, we find partisan effects that vary by governor partisanship, and, while median household income, population, metropolitan location, and the existence of a higher education institution all have relatively similar impacts across states, the impact of demographic variables is very different across Republican- versus Democratic- governed states. Republican governors are more likely to designate QOZs with lower levels of education (a 0.2 percentage point increase for every additional percent of the population without a high school diploma), and they are less likely to designated QOZs with higher Hispanic or Native American populations. Democratic governors are 0.2 percentage points more likely to select QOZs for every additional percent of the population with a 4-year degree.

[17] Papke (1994) and Rosenthal (2007) discuss this channel in more detail. See also Layser (2019) for a recent analysis that emphasizes legal issues.

In short, it seems clear that it is difficult to separate political considerations from a program intended to help poorer communities. Whether these political considerations ultimately affect the outcomes of the OZ program remains to be determined.[18]

[18] See Alm, Dronyk-Trosper, and Larkin (2020) for estimation results on this issue. See also Chen, Glaeser, and Wessel (2019) and Theodos, González, and Meixell (2020), who find that opportunity zones have had little impact on housing prices and other desired outcomes, at least to date.

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Tables & Figures

Table 1: Summary statistics for all qualified opportunity zones from 2011-2015 ACS Survey

	All Census Tracts, Mean	Potential QOZs, Mean	Designated QOZs, Mean
Qualified Opportunity Zone	0.106	0.244	1.000
Median House Value (in $1000s)	227.744	154.979	143.639
Unemployment Rate	0.089	0.120	0.136
Median Household Income (in $1000s)	58.668	37.731	34.316
Population (in 1000s)	4.402	4.058	4.041
Proportion with Less Than High School Diploma	0.140	0.215	0.229
Proportion with At Least 4-year Degree	0.286	0.178	0.165
Proportion Black	0.133	0.224	0.267
Proportion Hispanic	0.158	0.231	0.220
Proportion Native American	0.007	0.011	0.013
Proportion Under 18 (in years)	0.227	0.239	0.242
Proportion Over 65 (in years)	0.149	0.133	0.130
Proportion of Tract Represented by Democrat (Lower)	0.467	0.594	0.596
Proportion of Tract Represented by Democrat (Upper)	0.444	0.545	0.540
Legislature-Governor Partisan Match (Lower)	0.559	0.517	0.530
Legislature-Governor Partisan Match (Upper)	0.571	0.529	0.533
Proportion on Welfare	0.150	0.245	0.281
In Metropolitan Area (Yes=1; No=0)	0.834	0.801	0.758
Higher Education Campus (Yes=1; No=0)	0.076	0.090	0.123
Observations	69,921	29,549	7410

Notes: Asterisks represent the difference-in-means test between nominated and designated OZs, with * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Table 2: Marginal effects from logit regressions

	Model			
Variable	**(1)**	**(2)**	**(3)**	**(4)**
Median House Value	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Unemployment Rate	0.440	0.396	0.446	0.408
	(0.275)	(0.289)	(0.274)	(0.288)
log (Median Household Income)	-0.148***	-0.158***	-0.131***	-0.140***
	(0.038)	(0.041)	(0.038)	(0.041)
log (Population)	0.032***	0.037***	0.033***	0.037***
	(0.009)	(0.010)	(0.009)	(0.010)
Proportion with Less Than High School	0.113	0.197**	0.116	0.200**
Diploma	(0.089)	(0.083)	(0.091)	(0.086)
Proportion with At Least 4-year Degree	0.091**	0.115**	0.115**	0.139***
	(0.044)	(0.047)	(0.049)	(0.051)
Proportion Black	0.039	0.062**	0.040	0.064**
	(0.028)	(0.030)	(0.028)	(0.030)
Proportion Hispanic	0.003	-0.096	-0.003	-0.098
	(0.045)	(0.061)	(0.047)	(0.061)
Proportion Native American	0.019	-0.069	0.018	-0.076
	(0.079)	(0.089)	(0.080)	(0.090)
Proportion Under 18	-0.171	-0.216*	-0.167	-0.213*
	(0.112)	(0.111)	(0.115)	(0.114)
Proportion Over 65	-0.147***	-0.160***	-0.111*	-0.124**
	(0.057)	(0.055)	(0.057)	(0.056)
Proportion of Tract Represented by Democrat	-0.021	-0.019	-0.023*	-0.021*
(Lower)	(0.013)	(0.012)	(0.013)	(0.012)
Proportion of Tract Represented by Democrat	-0.010	-0.016	-0.010	-0.017
(Upper)	(0.010)	(0.010)	(0.010)	(0.011)
Legislature-Governor Partisan Match (Lower)	0.033**	0.028**	0.030**	0.027**
	(0.014)	(0.014)	(0.014)	(0.014)
Legislative-Governor Partisan Match (Upper)	-0.001	0.004	-0.000	0.004
	(0.009)	(0.009)	(0.009)	(0.009)
Proportion on Welfare	0.271***	0.328***	0.281***	0.337***
	(0.071)	(0.063)	(0.071)	(0.064)
In Metropolitan Area	-0.078***	-0.087***	-0.081***	-0.089***
	(0.017)	(0.016)	(0.018)	(0.017)
Higher Education Campus	0.080***	0.079***	0.080***	0.079***
	(0.011)	(0.011)	(0.011)	(0.011)
State Fixed Effects?	No	Yes	No	Yes
Contiguous OZs?	No	No	Yes	Yes
Pseudo-R^2	0.054	0.065	0.048	0.059
N	29,549	29,549	29,753	29,753

Notes: Marginal effects are reported with standard errors in parentheses, where Δ is a discrete change of a dummy variable from 0 to 1. Note that the difference in observations for specifications (1) and (2) versus specifications (3) and (4) is due to adding to specifications (3) and (4) 170 observations from non-LIC tracts contiguous to an LIC, 32 observations that meet LIC requirements but are not listed as LICs by IRS, and 2 observations that are "suspicious". * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Estimation results from linear probability models

	Model			
Variable	**(5)**	**(6)**	**(7)**	**(8)**
Median House Value	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Unemployment Rate	0.515*	0.471	0.514*	0.476
	(0.306)	(0.322)	(0.303)	(0.319)
log (Median Household Income)	-0.157***	-0.172***	-0.138***	-0.151***
	(0.040)	(0.043)	(0.040)	(0.043)
log (Population)	0.030***	0.034***	0.031***	0.035***
	(0.009)	(0.010)	(0.009)	(0.010)
Proportion with Less Than High School Diploma	0.118	0.203**	0.119	0.202**
	(0.095)	(0.090)	(0.096)	(0.091)
Proportion with At Least 4-year Degree	0.117**	0.144***	0.134**	0.160***
	(0.046)	(0.052)	(0.051)	(0.055)
Proportion Black	0.039	0.058*	0.040	0.061*
	(0.030)	(0.031)	(0.029)	(0.031)
Proportion Hispanic	-0.002	-0.103*	-0.007	-0.103*
	(0.047)	(0.061)	(0.048)	(0.061)
Proportion Native American	0.024	-0.067	0.023	-0.075
	(0.093)	(0.098)	(0.093)	(0.099)
Proportion Under 18	-0.165	-0.202	-0.163	-0.203
	(0.123)	(0.122)	(0.125)	(0.124)
Proportion Over 65	-0.142***	-0.163***	-0.111**	-0.129**
	(0.048)	(0.048)	(0.050)	(0.050)
Proportion of Tract Represented by Democrat (Lower)	-0.022*	-0.019	-0.024*	-0.021*
	(0.013)	(0.013)	(0.013)	(0.012)
Proportion of Tract Represented by Democrat (Upper)	-0.009	-0.016	-0.009	-0.017
	(0.010)	(0.011)	(0.010)	(0.011)
Legislature-Governor Partisan Match (Lower)	0.033**	0.029**	0.031**	0.028*
	(0.014)	(0.014)	(0.014)	(0.014)
Legislature-Governor Partisan Match (Upper)	-0.002	0.003	-0.001	0.004
	(0.009)	(0.009)	(0.009)	(0.009)
Proportion on Welfare	0.298***	0.356***	0.305***	0.361***
	(0.073)	(0.062)	(0.073)	(0.062)
In Metropolitan Area	-0.081***	-0.088***	-0.084***	-0.090***
	(0.019)	(0.019)	(0.019)	(0.019)
Higher Education Campus	0.089***	0.088***	0.089***	0.087***
	(0.013)	(0.013)	(0.013)	(0.013)
State Fixed Effects?	No	Yes	No	Yes
Contiguous OZs?	No	No	Yes	Yes
R^2	0.061	0.074	0.055	0.067
N	29,549	29,549	29,753	29,753

Notes: Standard errors are in parentheses. Note that the difference in observations for specifications (5) and (6) versus specifications (7) and (8) is due to adding to specifications (7) and (8) 170 observations from non-LIC tracts contiguous to an LIC, 32 observations that meet LIC requirements but are not listed as LICs by IRS, and 2 observations that are suspicious. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Marginal effects from logit regressions on samples split by executive partisanship

	Model	
Variable	**(9) States with Republican Governors**	**(10) States with Democrat Governors**
Median House Value	0.000	-0.000*
	(0.000)	(0.000)
Unemployment Rate	0.578	0.112
	(0.434)	(0.158)
log (Median Household Income)	-0.117**	-0.168***
	(0.046)	(0.063)
log (Population)	0.044***	0.025*
	(0.015)	(0.014)
Proportion with Less Than High School Diploma	0.207**	0.130
	(0.103)	(0.084)
Proportion with At Least 4-year Degree	0.086	0.212***
	(0.063)	(0.066)
Proportion Black	0.029	0.078
	(0.041)	(0.053)
Proportion Hispanic	-0.221***	0.055
	(0.043)	(0.059)
Proportion Native American	-0.212**	0.149
	(0.097)	(0.124)
Proportion Under 18	-0.258**	-0.147
	(0.117)	(0.182)
Proportion Over 65	-0.135*	-0.091
	(0.075)	(0.113)
Proportion of Tract Represented by Democrat (Lower)	-0.034**	-0.008
	(0.016)	(0.018)
Proportion of Tract Represented by Democrat (Upper)	-0.010	-0.013
	(0.017)	(0.014)
Proportion on Welfare	0.307***	0.420***
	(0.058)	(0.084)
In Metropolitan Area	-0.087***	-0.092***
	(0.022)	(0.029)
Higher Education Campus	0.074***	0.085***
	(0.015)	(0.015)
State Fixed Effects?	Yes	Yes
Contiguous OZs?	Yes	Yes
Pseudo-R^2	0.061	0.063
N	17,033	12,720

Notes: Marginal effects are presented with standard errors in parentheses, where Δ is a discrete change of a dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Estimation results from a restricted sample including only tracts with one representative

	Model	
Variable	**(11) Logit Model**	**(12) Linear Probability Model**
Median House Value	-0.000	-0.000
	(0.000)	(0.000)
Unemployment Rate	0.447	0.527
	(0.333)	(0.365)
log (Median Household Income)	-0.169***	-0.175***
	(0.052)	(0.052)
log (Population)	0.023**	0.020*
	(0.011)	(0.010)
Proportion with Less Than High School Diploma	0.243***	0.250***
	(0.083)	(0.091)
Proportion with At Least 4-year Degree	0.123*	0.163**
	(0.073)	(0.079)
Proportion Black	0.087***	0.083***
	(0.029)	(0.031)
Proportion Hispanic	-0.060	-0.064
	(0.057)	(0.056)
Proportion Native American	-0.122	-0.126
	(0.121)	(0.131)
Proportion Under 18	-0.142	-0.131
	(0.141)	(0.151)
Proportion Over 65	-0.086	-0.090
	(0.074)	(0.066)
Proportion of Tract Represented by Democrat	-0.033**	-0.032*
(Lower)	(0.017)	(0.016)
Proportion of Tract Represented by Democrat	-0.035***	-0.036***
(Upper)	(0.011)	(0.012)
Legislature-Governor Partisan Match (Lower)	0.036**	0.037*
	(0.018)	(0.018)
Legislature-Governor Partisan Match (Upper)	-0.005	-0.007
	(0.012)	(0.012)
Proportion on Welfare	0.269***	0.304***
	(0.069)	(0.072)
In Metropolitan Area	-0.124***	-0.132***
	(0.017)	(0.020)
Higher Education Campus	0.082***	0.089***
	(0.014)	(0.017)
State Fixed Effects?	Yes	Yes
Contiguous OZs?	Yes	Yes
R^2	--	0.086
Pseudo-R^2	0.075	---
N	12,327	12,334

Notes: Marginal effects are presented with standard errors in parentheses, where Δ is a discrete change of a dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

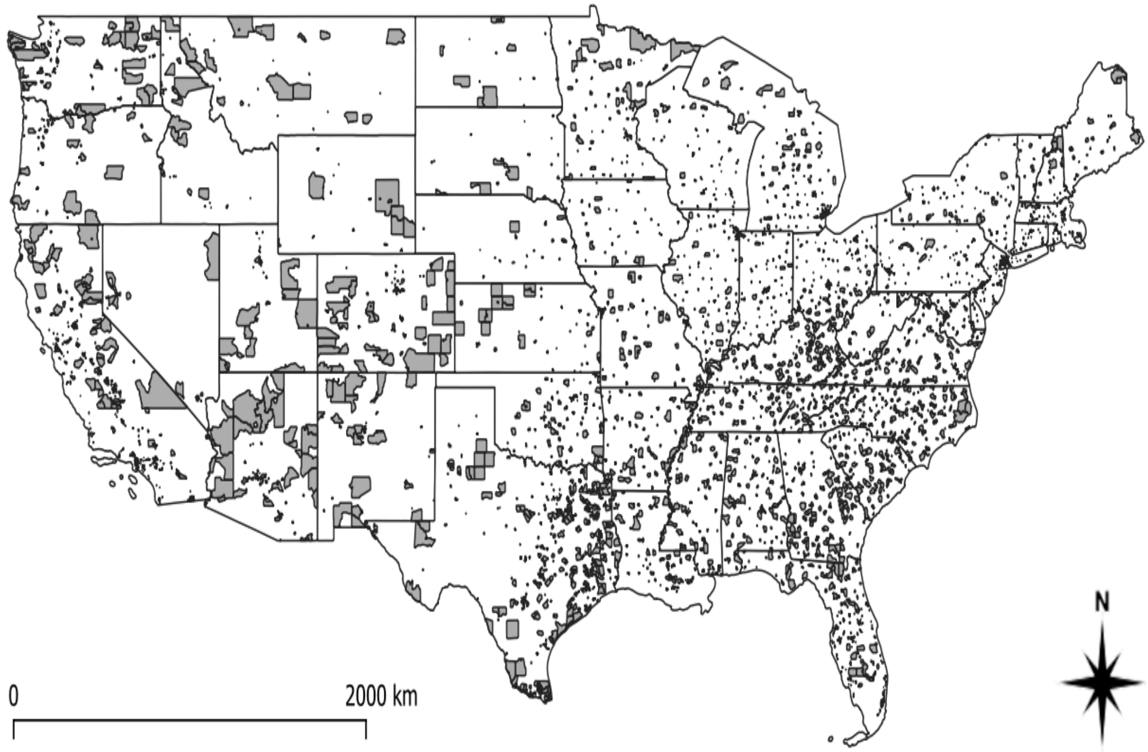
Table 6: Summary statistics for "suspicious" opportunity zones

Variable	**Mean**	**Standard Deviation**	**Min**	**Max**
Qualified Opportunity Zone	1.000	0.000	1.000	1.000
Median House Value	147.777	117.013	47.814	584.197
Unemployment Rate	0.068	0.087	0.000	0.500
Median Household Income	50.746	15.471	31.069	95.497
Population	2.635	1.964	0.000	7.579
Proportion with Less Than High School Diploma	0.126	0.106	0.000	0.500
Proportion with At Least 4-year Degree	0.295	0.202	0.000	0.760
Proportion Black	0.146	0.210	0.000	0.852
Proportion Hispanic	0.105	0.225	0.000	1.000
Proportion Native American	0.012	0.030	0.000	0.156
Proportion Under 18	0.159	0.095	0.000	0.334
Proportion Over 65	0.157	0.093	0.000	0.314
Proportion of Tract Represented by Democrat (Lower)	0.490	0.500	0.000	1.000
Proportion of Tract Represented by Democrat (Upper)	0.498	0.486	0.000	1.000
Legislature-Governor Partisan Match (Lower)	0.421	0.500	0	1
Legislature-Governor Partisan Match (Upper)	0.447	0.504	0	1
Proportion on Welfare	0.023	0.019	0.000	0.067
In Metropolitan Area	0.605	0.495	0	1
Higher Education Campus	0.079	0.273	0	1
Observations	38	38	38	38

Table 7: Cross-tabulations for "suspicious" opportunity zones

State	OZ is nominated when LIC does not meet criteria	Contiguous non-LIC tract is not adjacent to OZ	OZ is not populated in ACS	Total
Arkansas	3	0	0	3
California	1	0	0	1
Colorado	1	0	0	1
Connecticut	1	0	0	1
Florida	1	0	1	2
Illinois	1	0	0	1
Iowa	3	0	0	3
Kansas	1	0	0	1
Kentucky	1	0	0	1
Maine	1	0	0	1
Maryland	1	0	0	1
Michigan	4	0	1	5
Minnesota	2	0	0	2
Montana	1	0	0	1
Nebraska	1	0	0	1
Nevada	1	0	0	1
New York	1	0	0	1
North Carolina	1	0	0	1
Oklahoma	4	1	0	5
Pennsylvania	1	0	0	1
Puerto Rico	1	0	0	1
South Carolina	2	0	0	2
Texas	1	0	0	1
N	35	1	2	38

Figure 1: Map of designated opportunity zones



Chapter 4: Do Opportunity Zones Create Opportunities?

James Alm, Trey Dronyk-Trosper, and Sean Larkin *[1]

Abstract: The Tax Cuts and Jobs Act of 2017 allowed governors of the fifty states to designate low-income areas as "Qualified Opportunity Zones" (QOZs). This designation entitled investors in these QOZs to significant tax incentives, with the goal of encouraging increased investment in the designated low-income communities that in turn would increase economic opportunities in these areas. In this paper we estimate the impact of QOZ designation on several dimensions of economic development – residential and business real estate prices – using data from Florida for the period 2016-2020 and controlling for endogenous QOZ designation in our estimations. Our estimation results indicate little consistent and robust evidence that QOZ designation has had a positive impact on sales prices for single family homes, commercial lots, or vacant lots; there is also little evidence that QOZ designation has affected the frequency of sales. Even when using several different models and specifications, our results remain unchanged.

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1. Introduction

The Tax Cuts and Jobs Act (TCJA) of 2017 allowed governors of the fifty states to designate some low-income areas as special "Qualified Opportunity Zones" (QOZs). This designation entitled the investors in these QOZs to significant tax incentives, with the goal of encouraging investments in low-income communities that would increase economic opportunities in these areas.[2] In this paper we estimate the impact of QOZ designation on several dimensions of economic development – business and residential real estate prices – using data from Florida for the period 2016-2020 and controlling for endogenous QOZ designation in our estimations. Our overall results indicate that there is little consistent and robust evidence that QOZ designation has had a positive impact on sales prices for single family homes, commercial lots, or vacant lots; there is also little evidence that QOZ designation has affected the frequency of sales. Even when using several different models and specifications, our results remain largely unchanged.

A "Qualified Opportunity Zone" (QOZ) is a designated low-income area within a state, selected by the governor of the state from census tracts in the state that meet specified eligibility requirements, with investments in a QOZ then eligible for a range of generous tax incentives. The TCJA specified that a census tract must meet at least one of two criteria to qualify as a low-income census (LIC) tract, thereby becoming eligible for nomination as a QOZ: the poverty rate in the census tract must be at least 20 percent, and/or the median family income in the census tract must be less than or equal to 80 percent of either the statewide median family income or the metro family median income (where applicable), whichever is higher. The governor of each state can then nominate up

[2] The U.S. Government Accountability Office (GAO) has recently estimated that $29 billion were held in opportunity zone asset funds as of October 2021. See GAO (2021).

to 25 percent of these LIC tracts in the state as QOZs, and up to 5 percent of all QOZs nominated can be non-LICs if these census tracts are geographically contiguous with an LIC. This process was a one-time process that was completed before the end of 2018, and in December 2018 the U.S. Department of the Treasury finalized its certification of QOZs.

The stated intention of the QOZ incentives was to encourage investment in these low-income areas in order to improve incomes, jobs, and economic development in areas that were seen as lagging behind in opportunities, especially opportunities for minority groups. These tax incentives are of several types, of which the main ones relate to the treatment of realized capital gains on the investments. As discussed in more detail later, there is a temporary deferral of realized capital gains from a sale of an investment outside of a QOZ investment, if the realized gains are reinvested in a QOZ. Also, there is a step up in basis of 10 percent if the investment stays in the QOZ for 5 years and a step up in basis of 15 percent if the investment is held for 7 years. Finally, all capital gains from the sale of an investment in an QOZ are excluded from taxable income if the investment is held for at least 10 years. In their entirety, these tax incentives create significant tax breaks for investors, tax breaks that are of more value to higher income investors.

In total, the U.S. Department of the Treasury designated 8764 opportunity zones (OZs) in the fifty states and in Washington, D.C., Guam, Northern Mariana Islands, Puerto Rico, Samoa, and the Virgin Islands, from 42,160 potential census tracts out of a nationwide total of 74,163 census tracts.[3] All tracts that were nominated by the governor

[3] The various government regulations for OZs include, among others: "Investing in Qualified Opportunity Funds", available online at https://www.federalregister.gov/documents/2018/10/29/2018-23382/investing-in-qualified-opportunity-funds; "Investing in Qualified Opportunity Funds", available online at: www.federalregister.gov/documents/2019/05/01/2019-08075/investing-in-qualified-opportunity-funds;

and subsequently certified by the Secretary of the U.S. Department of the Treasury become designated QOZs, and investors in these QOZs become eligible for the tax incentives. As a result, each governor's designation provided an opportunity for the governor to introduce investments in low-income communities that will, in principle, increase economic opportunities in these areas.

The tax incentives included in QOZs are similar to a range of "place-based development policies" that have been utilized over the years. In the United States, these place-based development policies include programs like Enterprise Zones, Renewal Communities, Enterprise Communities, the New Market Tax Credit, the Historic Tax Credit, and the Low-income Housing Tax Credit. There are also place-based policies around the world, such as Structural Funds and Enterprise Zones in the European Union and Special Economic Zones in China, among many other programs. The specific provisions of these many programs vary, but the common feature is the use of targeted incentives that are intended to encourage investment in underperforming areas. There has been much research that has examined the impact of these policies on economic development. Overall, this research has found that the success of these policies is decidedly mixed, both in the United States and abroad (Bartik, 1991, 2003, 2019; Ladd, 1994; Papke 1994; Peters and Fisher, 2002; Bondonio and Greenbaum, 2007; Billings, 2009; Hanson, 2009; Neumark and Kolko, 2010; Bowers et al., 2011; Ham et al., 2011; Hanson and Rohlin, 2011, 2013; Accetturo and de Blasio, 2012; Freedman, 2015;

"Treasury, IRS issue proposed regulations on new Opportunity Zone tax incentive", available online at https://www.irs.gov/newsroom/treasury-irs-issue-proposed-regulations-on-new-opportunity-zone-tax-incentive; and "Special Rules for Capital Gains Invested in Opportunity Zones", available online at https://www.irs.gov/pub/irs-drop/rr-18-29.pdf. See also Novogradic (2018), Eastman and Kaeding (2019), Nitti (2019), Tankersley (2019), and Tax Policy Center (2019) for useful information.

Gobillon, Magnac, and Selod, 2012; Givord, Rathelot, and Sillard, 2013; Reynolds and Rohlin, 2014; The World Bank, 2015; Jenson 2018).[4] Indeed, initial studies on QOZs by Chen, Glaeser, and Wessel (2019), Sage, Langen, and van de Minne (2019), Theodos, González, and Meixell (2020), Atkins et al. (2020), Corinth and Feldman (2020), and Freedman, Khanna, and Neumark (2021) find that OZs are not having their hoped-for impacts, while Arefeva et al. (2020) and Bekkerman et al. (2021) find somewhat more encouraging initial impacts on jobs and real estate prices, respectively.

Using Florida data for the period 2016 to 2020, we estimate the impact of QOZ designation on residential and commercial real estate prices.[5] Our simplest estimation method uses OLS methods, with the main explanatory variable of interest a dummy variable for whether or not an area is designated as a QOZ. However, estimating these price effects is complicated by the endogenous nature of QOZ designation. Alm, Dronyk-Trosper, and Larkin (2021), Eldar and Garber (2021), and Frank, Hoopes, and Lester (2022) examine the factors associated with QOZ selection, and all find evidence that determine QOZ designation is more likely in areas that have higher rates of unemployment, higher levels of welfare receipt, and lower median income, all of which are consistent with the presumed goals of QOZs; these studies also demonstrate the importance of several political drivers.[6] These studies therefore indicate that QOZ

[4] See Glaeser and Gottlieb (2008), Neumark and Simpson (2014), and Duranton and Venables (2018) for recent and comprehensive surveys of this literature.

[5] Florida's selection process is similar to a number of other states, such as California, Illinois, and Maryland, all of which designated one OZ per county. Similarly, Florida's opportunity zones were identified in part through algorithmic modeling by Florida's Department of Economic Opportunity, but identification also incorporated requests from local governments, investors, developers, and nonprofits, among others. This method was used in other states as well. Admittedly, to the extent that heterogeneity exists in selection across the states, the applicability of our results may be affected.

[6] Theodos, Meixell, and Hedman (2018) also examine QOZ selection, although their analysis of QOZ selection relies mainly on simple comparisons of the mean characteristics of OZs that are selected versus

selection is endogenous, dependent on specific determinants of the eligible areas, and this endogenous selection must be considered in any estimations of the effects of QOZ designation on economic opportunities. Accordingly, we also use several other methods for evaluating the impact of QOZ designation on residential and commercial prices. In one method, we generate a predicted probability of QOZ designation using only *national* data on QOZ designation along with various control variables, and we then use this predicted probability of QOZ designation in our estimation of the impacts of qualified opportunity zones in our *Florida-specific* specifications. Because it is unlikely that *national* qualified opportunity zone nomination is correlated with *Florida-specific* trends, this method should control for any endogeneity in QOZ designation. We also employ a fuzzy regression discontinuity (RD) approach using the income and poverty rate cutoffs to compare similar census tracts. Although our results vary somewhat based on method, overall we find little consistent or robust evidence that QOZ designation has had a positive impact on sales prices for single family homes, commercial lots, or vacant lots, and we also find little evidence that QOZ designation has affected the frequency of sales.

Note that opportunity zones have faced increased criticism along several fronts, including the politicization of initial QOZ designation[7], their unintended consequences[8],

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those not are designated for QOZ selection. See also Theodos and Meixell (2018), who apply similar methods to the specific case of California.

[7] See "A Trump Tax Break To Help The Poor Went To a Rich GOP Donor's Superyacht Marina", available online at https://www.propublica.org/article/superyacht-marina-west-palm-beach-opportunity-zone-trump-tax-break-to-help-the-poor-went-to-a-rich-gop-donor. See also "Symbol of '80s Greed Stands to Profit from Trump Tax Break for Poor Areas", available online at https://www.nytimes.com/2019/10/26/business/michael-milken-trump-opportunity-zones.html.

[8] See "Fixing America's Forgotten Places – Opportunity Zones, created by Trump's tax law, are meant to help the heartland thrive and make the country more equal, but can they pull it off?", available online at https://www.theatlantic.com/ideas/archive/2018/07/how-do-we-help-this-place/565862/.

and the anticipated failures of QOZ designation[9], and these criticisms have even made their way into recent high-profit entertainment programs.[10] Some politicians have already begun crafting bills to address these criticisms, including the complete dissolution of the QOZ program.[11] We do not discuss these dimensions of the QOZ program.

In the next section, we discuss the details of opportunity zones. We then present our data and methods, followed by our results. We conclude in the final section.

2. What is an "Opportunity Zone"? Definitions and Tax Incentives

2.1. Definitions

To facilitate our discussion, we begin with some basic definitions that define the main features of the Opportunity Zone (OZ) program.

A low-income census tract (LIC) is a census tract in which either the poverty rate is at least 20 percent or tracts in which the median family income is less than or equal to 80 percent of the statewide median family income or metro family median income (where applicable), whichever is higher. A related definition is a Treasury-identified census tract,

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[9] See: "The Problem with Opportunity Zones", available online at https://www.citylab.com/equity/2018/05/the-problem-with-opportunity-zones/560510/; "How a Trump Tax Break to Help Poor Communities Became a Windfall for the Rich", available online at https://www.nytimes.com/2019/08/31/business/tax-opportunity-zones.html; "Trump Tax Break That Benefited the Rich Is Being Investigated", available online at https://www.nytimes.com/2020/01/15/business/trump-opportunity-zone-investigation.html; and "Developers Rushing to Opportunity Zones for Tax Break, But Is It Helping Louisiana's Low-Income Areas?", available online at www.theadvocate.com/new_orleans/news/business/article_0ddb2d22-2576-11e9-bde9837b83173a57.html.

[10] See the episode of the HBO series *Billions* entitled "Opportunity Zone", in which the character Bobby Axelrod (or Axe) wants to invest in an QOZ in the Yonkers neighborhood in which he grew up.

[11] On 6 November 2019 Sen. Ron Wyden (D-OR) introduced in the U.S. Senate a bill to reform the QOZ program. See https://www.finance.senate.gov/imo/media/doc/Opportunity%20Zone%20Reporting%20and%20Reform%20Act%20of%202019%20Bill%20Text.pdf.

which is a census tract that is contiguous with one or more LICs but which does not meet the LIC criteria.

A state governor may declare 25 percent of the LICs in the state as a Qualified Opportunity Zone (QOZ) based on 2011-2015 ACS 5-year data from the Census Bureau.[12] Note that 5 percent of all QOZs nominated can be contiguous with an LIC, rather than an LIC itself, as specified by a Treasury-identified census tract. Because of this provision, census tracts adjacent to an LIC, but not necessarily meeting the criteria for QOZ nomination, may still be nominated for QOZ status. However, no more than 5 percent of the QOZs that are nominated within each state may be these contiguous tracts.

A Qualified Opportunity Fund (QOF) is a self-certified entity treated as a partnership or corporation for federal tax purposes and organized in any of the 50 states, District of Columbia, or the five U.S. territories for the purpose of investing in qualified opportunity zone property. At least 90 percent of held assets must be QOZ property.

A QOZ business is a business with substantially all of its tangible assets located in QOZs. Internal Revenue Service (IRS) regulations require that 70 percent of all tangible property held be in a QOZ, and that 50 percent of the gross income from a QOZ business be derived from active trade or conduct in a QOZ (Internal Revenue Service, 2018). Several enterprises cannot qualify as a QOZ business, including: a golf course, a country club, a massage parlor, a hot tub facility, a suntan facility, a gambling facility, and stores specializing in alcoholic beverages to be consumed off the premises. A QOZ business may include houses and apartments for rent.

[12] Note that for 51 QOZs nominated late in the process, the 2012-2016 ACS data was used.

A QOZ property must be a property purchased after 31 December 2017, be qualified as a QOZ at the time of purchase and remain qualified for substantially all of the time held. These properties include:

- QOZ Stock: Equity in a QOZ business held by a QOF.
- QOZ Partnership Interest: Partnership interest in a QOZ business held by a QOF.
- QOZ Business Property: Tangible property used in a trade or business in a QOZ if the original use of such tangible property commences with the QOF or the QOF substantially improves the tangible property, where "substantial improvement" means that during any 30-month period *additions* to the tax basis of the building (excluding land values) are made such that the value added to the tax basis is higher than the adjusted taxpayer basis at the beginning of any 30-month period.

Note that a 90 percent investment in a business with a 70 percent QOZ business property means that there must be a minimum 63 percent investment in QOZs for a QOF.

2.2. Tax incentives

There are three tax incentives from investing in a QOF. First, there is a temporary deferral of realized capital gains from a sale outside of an QOZ if reinvested in a QOF, which must be realized (and taxed) when the property is sold or at the end of 2026, whichever occurs first. An investor must invest in a QOF within 180 days of realizing the capital gains to qualify for deferment.

Second, capital gains newly invested into a QOF will receive a step-up in basis of 10 percent if the investment is held for 5 years, and another 5 percent (for a total of 15 percent) if held for 7 years. This provision enables investors to reduce 15 percent of their capital gains invested into a QOF from taxable income if held for the full 7 years.

Third, there is permanent exclusion from taxable income of capital gains from the sale or exchange of an investment in a QOF if the investment is held for 10 years. This incentive only applies to gains accrued after an investment in a QOF. As a result, capital gains earned before investment in the QOF receive benefits from the first and second tax incentives, while capital gains earned after investing in the QOF benefit from this third incentive.

In their entirety, these tax incentives mean that for an investment that is held for ten years all realized capital gains used for investment in a QOF will not be taxed until 2026, only 85 percent of the original capital gains invested will be taxed (100 percent would have been if realized originally), and no taxes will be paid on the appreciation of the investment. These represent quite significant tax breaks for investments in a QOF. Given that the marginal tax rate on capital gains varies from 0 percent for low income earners to 20 percent for higher income earners, these tax benefits will be of more value to higher income investors.

As an example that illustrates the magnitude of these benefits, consider the case of an individual facing a 20 percent capital gains tax rate who sells stocks, earns $1 million in capital gains on these sales, and then reinvests these capital gains in a QOF that earns $50,000 every year. After 6 years, the investor will have made $1,300,000 (or the initial $1,000,000 in capital gains plus $300,000 from the [6 X $50,000] in returns each year). Selling this QOF in its entirety would result in capital gains taxes on $300,000 of earnings, plus $900,000 from the original investment due to the step up in basis (e.g.,

"…if the investment is held in the QOF for 5 years"), thereby reducing the capital gains tax base by $100,000. Selling the QOF after 8 years would result in earnings of $1,400,000 but capital gains taxes on only $850,000 of the original investment plus the $400,000 in newly earned capital gains (e.g., "…if the investment is held in the QOF for 5 years, up to a total of 15 percent if the investment is held in the QOF for 7 years"), reducing the capital gains tax base by $150,000. However, selling the investment in year 11 would result in capital gains taxes on only the initial amount less the 15 percent reductions because of the permanent exclusion of capital gains from holding the investment for 10 years (e.g., "…there is permanent exclusion from taxable income of capital gains from the sale or exchange of an investment in a QOF if the investment is held for 10 years"). All of the accumulated capital gains from the QOF investment would avoid the 20 percent capital gains tax rate, and only $850,000 of the initial $1 million in capital gains would be subject to the capital gains tax rate, and any additional capital gains earned would be received tax free.

3. Data and Methods

3.1. Data

Our main variables that capture economic development effects are residential and commercial parcel sales prices in the state of Florida. As noted earlier, Florida is an especially useful state to examine. Its QOZ selection process is similar to the process used by many other states, including the way in which information from relevant parties was incorporated in the selection process. In addition, Florida has very detailed information on properties.[13] Florida's selection process resulted in fewer OZs than the U.S. average as being designated as undergoing socioeconomic change (1.4% vs. 3.2%), however there were 17 states with lower percentages Theodos et al. 2018. This implies that Florida's selection process targeted fewer OZs that may have been likely to see future growth in a counterfactual sense. As such, our results should hold true for a significant portion of the states, though may not be representative of states with very high socioeconomic change percentages such as Washington D.C. or New York (32% and 13%, respectively).

Home price information comes from Florida state tax rolls that incorporate real estate transaction data at the individual transaction level, including census tract identifiers, month, year, and type of transaction for every real estate transaction in Florida from 2016-2020. We use only those transactions that are considered to be "arms-length" transactions, i.e. between strangers. These data include, separately, residential and commercial real estate prices, as well as designations for whether the lot is vacant (and improved) or built. Given that QOZ designation occurs at the census tract level, we aggregate these sales to the tract level.

Our explanatory factors include demographic variables, economic variables, and political variables. Demographic and economic variables are drawn from the American Communities Survey (ACS), for 2011-2015, 2012-2016, and 2014-2018 5-year estimates. ACS data include median household income, median family income, educational

[13] Generally, Florida's OZs are similar to OZs in the rest of the United States, at least prior to the enactment of the TCJA. However, there are a few areas where Florida OZs appear to be different from nationwide averages, which may impact the external validity for the following analyses. In particular, median home prices are lower in Florida, though this could be driven by minimum reporting costs and responses to the Documentary Stamp and Transfer Taxes. Additionally, there are proportionately fewer Native Americans in Florida than other states.

attainment, race and ethnicity information, total population, unemployment rate, metropolitan area population, the percent of the population on welfare, and the percent of the population in various age groups.

We also use information on the specific geographic location of campus of higher education, obtained from the U.S. Department of Homeland Security Homeland Infrastructure Foundation-Level Data. This source includes location information from a census of institutions of higher learning, including doctoral/research universities, masters colleges and universities, baccalaureate colleges, associates colleges, theological seminaries, medical and other health care-related schools, schools of engineering and technology, business and management schools, art, music, and design schools, law schools, teachers colleges, tribal colleges, and other specialized institutions.

Our political variables measure political control of state government institutions at the time of QOZ nomination. We generated some of these variables from ballotpedia.com, which we coded by hand. We also coded the legislative district and census tract crosswalk, using GIS data from the U.S. Census Bureau. These data measure the upper and lower state legislative partisanship by district and state executive partisanship for January-March 2018, the period immediately following enactment of the QOZ program in the TCJA of 2017 during which states could nominate eligible census tracts to be qualified opportunity zones.

We use the complete list of QOZs and LIC census tracts in Florida from the IRS. Also, we use consumer price index information to adjust nominal dollars to real dollars from the Federal Reserve Bank of St. Louis. All dollar amounts are in 2018 USD, and all observations are at the census tract level. We distinguish between the *Pre-period*, or January 2016 to March 2018, and the *Post-period*, or March 2018 to November 2020.

The national data include all census tracts in the lower 48 states except Nebraska. For the Florida data, there are 4245 Census tracts in ACS data, including 1706 LICs, and 427 QOZs in Florida; however, we do not include tracts that are unpopulated in any of the ACS periods, that do not have arms-length real estate transactions in both the pre- and the post-period, or that are missing any ACS variables. Our final Florida data include 4037 Census tracts, 1621 LICs, and 411 QOZs. Summary statistics are reported in Table 1. A list of all variable names and definitions is provided in the Appendix.

3.2. Methods

Our regressions only look at those census tracts in Florida classified by the IRS as LICs. We estimate the impact of QOZ designation on the percent change in real mean real estate transaction prices in Florida between the pre- and post-periods, controlling for demographic, political, and economic variables.

Our basic model is as follows:

$$\%\Delta Price_{i,t} = \beta_0 + \beta_1 QOZ_{i,t} + \beta_2 X_i + \varepsilon_{i,t}$$

where $\%\Delta Price$ denotes the percentage change in price between the pre- and post-period for census tract i at time t, as determined by the dummy variable QOZ (equal to 1 for a census tract designated as a qualified opportunity zone and 0 otherwise), X is a set of control variables, ε is the error term, and β are estimated coefficients.

We estimate several models. In the first and simplest model, we estimate OLS regressions that include many of these demographic and economic variables, with our main explanatory variable of interest a dummy variable for QOZ designation equal to one if the census tract is a qualified opportunity zone and zero otherwise. However, as noted earlier, Frank, Hoopes, and Lester (2020), Alm, Dronyk-Trosper, and Larkin (2021), and Eldar and Garber (2021) provide evidence that QOZ designation is likely to be endogenous, determined in part by many of these same demographic and economic variables, along with various political variables; that is, selection into the treatment group (e.g., QOZ designation) may be influenced by these variables, along with prior trends toward relatively accelerating real estate prices (pro-investors) or relatively decelerating real estate prices (pro-distressed community residents). This endogenous selection as a QOZ must be considered in estimating the impact of QOZ designation on economic opportunities.

We address this potential endogeneity through several additional methods. In a second model, we first estimate the likelihood of QOZ nomination using the *national* sample of qualified opportunity zones along with partisanship variables and demographic information used by policy makers at the time (2011-2015 and 2012-2016 ACS 5-year estimates). We then include this calculated probability of nomination as the right-hand side variable of interest as a replacement for the QOZ binary variable in the original specification, in an OLS equation of the percent change in *Florida-specific* real estate transaction prices. Because it is unlikely that *national* qualified opportunity zone nomination is correlated with *Florida-specific* trends, this method should control for any endogeneity in QOZ designation.

In a third model, we employ a fuzzy regression discontinuity (RD) approach that also deals with potential endogeneity concerns. Although the eligibility criteria based on median family income and poverty rates were laid out by the TCJA of 2017, meeting these criteria did not guarantee selection into treatment but simply meant that the census tract was eligible for selection into treatment. As a result, it is possible to compare the performance of qualified opportunity zones that were designated as QOZs with those census tracts that met these criteria but that were not designated as QOZs, to determine the effects of QOZ designation.[14] The first stage of this fuzzy regression discontinuity approach estimates the probability of selection into the treatment based on which side of the cutoff into which the census tract falls, and the second stage estimates the effect of the probability of QOZ designation on the percent change in real estate transaction prices between the pre- and the post-period.

We apply both parametric and non-parametric RD models with several bandwidths to ensure robustness. Our parametric model is specified as:

$$\%\Delta Price_i = \beta_0 + \beta_1 c_i + \beta_2 c_i^2 + \beta_3 c_i^3 + \beta_4 c_i^4 + \beta_5 D_i + \varepsilon_i \ ,$$

where *c* is one of three potential running variables used for QOZ designation, median household income, poverty rate, or both and *D* is a dummy variable where 1 means the census tract meets the cut-off for the particular running variable. For the non-parametric models, a triangular weight is used, and the appropriate bandwidth is calculated following the algorithm laid out by Calonico, Cattaneo, and Titiunik (2014). Our non-parametric model is specified as:

$$\%\Delta Price_i = \beta_0 + \beta_1 D_i + \beta_2 c_i + \varepsilon_{i,t} \ ,$$

where observation *i* is included only if *c* is within a given distance from the cut-off of the running variable. This approximates a local linear regression around the cut-off point.

[14] Note that, while there are other programs that use similar cut-offs, an RD should still be a valid causal mechanism so long as those programs were either implemented at a different time, or as long as they were ongoing programs for which their expenditures did not differ over this period.

Because any results may be driven by a few very large or very small transactions, we run all of these models on the entire sample of low-income census tracts, a trimmed subsample in which the ten tracts with the highest percent change in real estate prices and the ten tracts with the lowest percent change in real estate prices are dropped from the sample prior to analysis, and a winsorized sample in which all observations below (say) the 10th percentile in real estate prices are considered to be equal to the 10th percentile and all observations above (say) the 90th percentile are considered equal to the 90th percentile. We also use different percentile cutoffs (e.g., 95th and 5th, 99th and 1st) in alternative winsorized estimations.[15]

The next section discusses these estimation results.[16] As discussed there, all of these approaches give results that are largely the same.

4. Results

4.1. Some initial results: Pre-trends

[15] To winsorize a variable, we take all observations below the 10th percentile and set them equal to that percentile, and we also take all observations above the 90th percentile and set them equal to the 90th percentile. We repeat this process for the 5th and 95th percentiles and the 1st and 99th percentiles, respectively

[16] Note that we have also utilized another estimation method, an instrumental variables approach using two-stage least squares (2SLS), with the percent of a census tract zoned as residential in 2017 used as an instrument for the probability that a tract is nominated as a QOZ. We believe that this variable is a plausible instrument: it is likely relevant to QOZ nomination, as qualified opportunity zones were originally designed to increase employment prospects and support businesses; and it is likely to satisfy the exclusion restriction because it changes very little over time among the census tracts analyzed here, and so it should be uncorrelated with potentially omitted variables that explain home prices. These 2SLS results confirm our results from the other methods; that is, we find no evidence that QOZ designation has had a positive impact on sales prices or the frequency of sales. Even so, we believe that there are reasons for concern with this variable as an instrument. For example, it is possible that the percent of properties that are residential versus commercial may be mechanically correlated with the outcome variable of interest, which would violate the exclusion restriction. It is also possible that a number of unobservable factors could drive both zoning patterns and future price growth. Finally, even if the residential share is perfectly fixed over time, there is no reason that it could not be spuriously correlated with house price trends during this time period. For these reasons, we do not report these 2SLS results. All results are available upon request.

There may be concerns in each of our methods regarding whether QOZs and non-QOZs are comparable in real estate sales price changes before the TCJA was enacted. It is therefore necessary to check the pre-treatment trends in home prices between these two groups. Figure 1 shows the comparison between QOZs and non-QOZs overall. Notably, both QOZs and non-QOZs move in similar fashion though non-QOZs have a higher mean real estate sales price, although there may be some differences in the trend starting in 2017. When restricting the data to just residential structures (Figure 2), we see very similar trends over the entire period. While the usage of an RD should resolve any concerns over underlying differences between the QOZs and non-QOZs, Figure 3 presents the pre-trends for residential structures using only the census tracts used in the RD. Once again, we see similar trends in home price changes up to the treatment date.

4.2. OLS estimation results

We present the OLS estimation results in Table 2, which estimate the impact of QOZ designation on the percent change in real mean real estate transaction prices between the pre- and post-periods after controlling for demographic and economic characteristics.[17] The results provide weak evidence that overall real estate prices have in fact grown at a slightly slower rate (10 percent slower) in QOZs compared to the rest of the state. These results seem to be driven by the slower growth in vacant real estate prices relative to other LICs. There is also suggestive evidence that non-vacant residential properties have increased in value faster in QOZs than in non-QOZ low-income census tracts. However, because QOZ designation is likely endogenous these findings cannot be

[17] Note that we are unable to use political variables in these OLS estimations because the only political variables that are available are time-invariant political variables, which of course do not provide an accurate picture of partisanship over time.

interpreted as causal. The following sections present models that deal with this selection issue.

4.3. Predicted QOZ designation estimation results

As one method to control for endogenous QOZ designation, we first estimate the likelihood of QOZ nomination using the national sample of qualified opportunity zones, along with demographic, economic, and partisanship variables used by policy makers at the time (2011-2015 and 2012-2016 ACS 5-year estimates). We then include this calculated probability of nomination as the right-hand side variable of interest in an OLS equation of percent change in Florida real estate transaction prices as a replacement for the QOZ binary variable in the original specification.[18] These results are reported in Table 3.

The extraordinarily large estimates for both categories of vacant property appear to be driven by a few extremely large percent changes in means, likely driven by the relatively low transaction count of vacant properties and the wide variance in their value. In any event, only non-vacant commercial prices have a statistically significant effect, but only barely. These results provide little evidence that predicted QOZ designation has any consistent impact on real estate prices.

4.4. Fuzzy regression discontinuity estimation results

Results for the first stage of the regression discontinuity models can be seen in Figures 4 and 5, using the poverty rate cutoff in Figure 4 and the income level cutoff in Figure 5. Recall that the first stage of the fuzzy RD approach estimates the probability of

[18] Note that we tried to use both partisanship variables and distance to nearest metropolitan area as instruments for selection for Florida alone, as an alternative approach for generating the predicted probability of QOZ designation. However, both variables had F-statistics of around 2.0, and so were too weak to be of use.

selection into the treatment based on which side of the cutoff the census tract falls, and the second stage estimates the effect of the probability of QOZ nomination on percent change in real estate transaction prices between the pre- and the post-periods.

In both Figures 4 and 5, there is no compelling evidence of a discrete jump in probability of selection at the cutoff of either criteria. This explains the apparent lack of a result in the second stage results for the impact on real estate prices between the pre- and the post-periods (Figure 6 for the poverty rate cutoff and Figure 7 for the income level cutoff).

Further examination of QOZ selection compared to the eligibility criteria (Figures 8 and 9) suggest that, although there is no discrete jump at the cut-offs because of the dual nature of the criteria, there is certainly a marked increase in the likelihood of nomination when at least one of the criteria is met. Indeed, Figures 8 and 9 suggest that there is a dosage effect, as the higher the poverty rate and the lower the income the more likely a census tract is to be nominated in the first place. Figures 10 and 11 also examine the overall percent change in real estate prices compared to the two criteria. These figures do not provide causal evidence of the impact of QOZ designation. Even so, the results in Figures 10 and 11 fail to find convincing evidence of an increase in value of properties in qualified opportunity zones.

Table 4 shows the results of non-parametric fuzzy RD estimation with percent change in real estate prices as the dependent variable in the second stage. The first two columns (Models 11 and 12) display results only using median family income as the running variable, while the other two columns (Models 13 and 14) display estimation results in which only the poverty rate was used as the running variable. Controls for economic and demographic variables are included in Models 12 and 14. The sign of the first stage estimates are expected; that is, being the above the income threshold is negatively associated with the likelihood of being nominated as a QOZ, while being above the poverty threshold is positively associated with the likelihood of being nominated as an QOZ. These signs align with previous estimates and expectations, although the first stage estimates are not statistically significant for any of the models. The second stage estimates are also not statistically significant, though of opposite sign. Because the variables in the first stage lack significance, it is possible that these cut-offs function as weak instruments because only one of the relevant running variables is examined at a time so the cut-offs are not strict. Also, when examining the figures it is apparent that the likelihood of QOZ nomination increases as distance from the cut-offs increases, so there may not be an immediate “jump” along either dimension when examined in isolation.

Tables 5 contains the results for the parametric fuzzy RD regressions. This approach allowed multiple bandwidths to be examined. Like the non-parametric estimation methods, the results are generally mixed and statistically insignificant, though this could be due to the relatively small number of observations available in each bandwidth causing reduced precision of the estimates. The results when using income as a running variable are all negative; the results when using the poverty rate as a running variable are all positive; and the results when using both running variables are all negative. While these are in-line with the results from the non-parametric methods, once more none of these results are statistically significant.

By combining the results from both Table 4 and Table 5, we find that using an RD methodology is associated with little to no evidence of a statistically significant impact of QOZ selection on parcel sales prices.

4.5. Additional results

Because about 85 percent of all transactions in our data involve non-vacant residential properties and these are the properties that tend to be more standardized, we apply the same OLS methods to examine the percent change in mean non-vacant real estate prices by census tract. The OLS results can be found in Table 6, with model 29 estimated using the trimmed dataset and models 28 and 29 including additional controls. The estimates are consistently positive and all except for model 29 are statistically significant. Model 28 is our preferred model, as trimming is less appropriate when dealing with residential property: the property itself is more standardized and comparable, and almost all of the outlying transactions in the pooled sample are commercial property. When we include winsorizing, the effects turn negative and statistically insignificant.

In addition, transaction frequency may also be affected by QOZ designation. To examine this possibility, we estimate the impact of QOZ designation on the percent change in number of real estate transactions between the pre- and post-period using the OLS methods. These OLS results are in Table 7. Models 33 and 34 examine all types of real estate transactions, while models 35 and 36 only include non-vacant residential transactions. Models 34 and 36 include controls. We find that the effect of QOZ nomination is negative but statistically insignificant when no control variables are included for all real estate transactions. In addition, when controls are not included, non-vacant residential transactions are positive and statistically significant. However, upon inclusion of control variables, we find no statistically significant correlations between QOZ designation and transaction counts.

These additional results should be viewed mainly as suggestive, given endogeneity concerns. Even so, we believe that these results are useful, providing additional evidence that QOZ designation has had little impact on prices or transactions.

5. Conclusions

Overall, our results suggest that QOZ designation has not had a substantial impact on residential and commercial parcel sales prices or on the volume of real estate transactions. In some of our simpler specifications (mainly those without many control variables), our estimation results suggest that qualified opportunity zones may have had a small positive effect on non-vacant residential property values. However, in nearly all models that include economic and demographic controls and that also control for potential endogeneity of QOZ designation, we find statistically insignificant results for the impacts of qualified opportunity zones.

What might explain these results? There are several likely explanations. An obvious one is that the program is simply ineffective in achieving its stated aims, a conclusion that characterizes many if not all place-based initiatives. Another, more positive explanation is that the QOZ program is still in its infancy, and so it may not have had sufficient time to achieve its intended effects. Still other possibilities relate to the data that we used. For example, the use of real estate price changes as the indicator of economic opportunity may not able to capture the relevant impacts on such other indicators as poverty rates, unemployment rates, and income levels.[19] Also, although Florida appears to be a typical state in its administration of opportunity zones, there may be specific features of Florida that affected the estimation results. These other explanations suggest that more time may be needed before examining the effects of QOZs, that other measures of economic opportunity should be used in future empirical work, and that effects in other states should be considered. We anticipate over the next several years that more comprehensive data covering a longer time period and additional states will bring clarity to the impacts of the opportunity zone program.

[19] Relatedly, the use of real estate price changes is likely influenced by outliers in price changes. Using a windsorized data set and/or using the percent change in median prices (rather than the percent change in mean prices) as the dependent variable are approaches that may deal with the issue of outliers.

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Tables & Figures

Table 1: Summary statistics (means) for Florida low-income census tracts, 2016-2020

	All census tracts	Low-income census tracts	Opportunity Zones
%Δ in Mean Price, Total	0.157	0.223	0.186
%Δ in Mean Price, Commercial Non-vacant	2.111	1.188	0.708
%Δ in Mean Price, Commercial Vacant	7.125	11.566	3.156
%Δ in Mean Price, Residential Vacant	3.198	6.157	1.374
%Δ in Mean Price, Residential Non-vacant	0.082	0.123	0.152
Low-income Census Tract	0.402	1.000	1.000
Percent Tract Zoned as Residential, 2017	0.887	0.849	0.815
Opportunity Zone	0.102	0.253	1.000
$\widehat{QOZ}$	0.159	0.247	0.337
Percent Under 18	0.190	0.209	0.222
Percent Over 65	0.216	0.178	0.167
Total Population	4.999	4.765	4.675
Percent Black	0.150	0.251	0.390
Percent Hispanic	0.222	0.274	0.219
Percent Native American	0.002	0.002	0.002
Percent Family Households	0.641	0.611	0.607
Percent Less Than High School	0.122	0.188	0.222
Percent College	0.290	0.180	0.143
Median Household Income	57.207	39.345	34.866
Percent on Welfare	0.154	0.254	0.315
Unemployment Rate	0.065	0.084	0.118
Percent Non-citizen	0.084	0.115	0.098
Campus of Higher Education	0.073	0.096	0.085
In Metropolitan Area	0.960	0.935	0.922
N	4037	1621	411

Table 2: OLS regressions for percent change in price.

	(1)	(2)	(3)	(4)	(5)
Type of Property	All Real Estate	Vacant Commercial	Non-vacant Commercial	Vacant Residential	Non-vacant Residential
Qualified Opportunity Zone	-0.101*	-9.407	-0.388	-6.335	0.019*
	(0.057)	(22.230)	(0.394)	(11.817)	(0.011)
Percent Under 18	-0.514	108.853	2.689	43.731	-0.042
	(0.504)	(226.948)	(3.649)	(110.637)	(0.095)
Percent Over 65	0.246	-0.470	2.284	-71.777	-0.181***
	(0.288)	(141.301)	(2.183)	(68.476)	(0.057)
Total Population	-0.006	3.079	-0.134*	-2.281	-0.005***
	(0.011)	(4.438)	(0.079)	(2.261)	(0.002)
Percent Black	0.012	10.229	0.463	-39.262	0.050*
	(0.141)	(66.961)	(1.052)	(31.738)	(0.026)
Percent Hispanic	-0.052	-26.094	1.161	-109.471**	0.043
	(0.194)	(90.643)	(1.427)	(46.598)	(0.037)
Percent Native American	1.258	-221.633	64.464**	564.530	-0.475
	(3.047)	(1816.369)	(31.621)	(654.064)	(0.624)
Percent Family Households	-0.419	-11.620	0.254	168.539***	-0.002
	(0.287)	(131.319)	(2.090)	(64.541)	(0.055)
Percent Less Than High School	0.039	-165.535	6.744**	-56.355	0.054
	(0.395)	(181.628)	(2.807)	(88.915)	(0.075)
Percent College	-0.233	-150.332	-0.493	195.261**	-0.233***
	(0.364)	(169.884)	(2.636)	(88.138)	(0.071)
Median Household Income	-0.001	-0.931	0.010	-1.889**	-0.001**
	(0.003)	(1.681)	(0.025)	(0.794)	(0.001)
Percent on Welfare	0.568*	-106.648	-5.624**	-79.553	-0.028
	(0.335)	(144.211)	(2.390)	(76.849)	(0.064)
Unemployment Rate	0.082	-2.587	-2.891	188.890	-0.214*
	(0.611)	(253.370)	(4.350)	(127.189)	(0.114)
Percent Non-citizen	0.561	138.105	-2.134	295.210***	-0.047
	(0.381)	(179.989)	(2.766)	(97.196)	(0.073)
Campus of Higher Education	-0.071	79.753***	-0.418	-12.237	0.002
	(0.076)	(30.061)	(0.512)	(17.537)	(0.014)
In Metropolitan Area	0.107	15.929	1.306**	6.070	0.008
	(0.094)	(43.098)	(0.612)	(17.604)	(0.017)
Constant	0.359	70.219	-0.574	-23.423	0.271***
	(0.245)	(116.601)	(1.770)	(56.140)	(0.048)
R^2	0.021	0.025	0.027	0.022	0.064
N	1621	455	1178	1161	1576

Notes: Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: OLS estimates for percent change in prices from pre- and post-period as dependent variable using estimated probability of QOZ designation from national sample in first stage probit

	(6)	(7)	(8)	(9)	(10)
Type of Property	All Real Estate	Vacant Commercial	Non-Vacant Commercial	Vacant Residential	Non-Vacant Residential
$\widehat{QOZ}$	-0.013	84.530	-4.725*	-97.240	0.097
	(0.397)	(157.001)	(2.820)	(86.900)	(0.076)
Controls?	Yes	Yes	Yes	Yes	Yes
R^2	0.019	0.025	0.028	0.023	0.063
N	1621	455	1178	1161	1576

Notes: Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Fuzzy regression discontinuity results with non-parametric methods

	(11)	(12)	(13)	(14)
Running Variable	Income	Income	Poverty Rate	Poverty Rate
First Stage Estimates				
Meets LIC Criteria	-0.028	-0.016	0.047	1.034
	(0.038)	(0.037)	(0.032)	(0.301)
Second Stage Estimates				
$\widehat{QOZ}$	-2.189	-2.858	0.744	0.337
	(3.437)	(7.474)	(1.159)	(1.405)
Bandwidth	+/- 8.435	+/- 7.933	+/- 0.055	+/- 0.067
Controls?	No	Yes	No	Yes
N	1313	1256	1210	1473

Notes: Standard errors are in parentheses. Outcomes measured in percent change in sales price. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Parametric regression discontinuity results

Running Variable	Distance from Income Threshold			Poverty Rate			Distance from Income Threshold and Poverty Rate		
	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
$\widehat{QOZ}$ (% Change Sales Price)	-1.356 (1.625)	-0.368 (2.349)	-2.006 (1.799)	1.214 (0.984)	1.156 (6.861)	0.813 (0.529)	-0.885 (0.432)	-0.449 (0.239)	-0.066 (0.247)
Bandwidth	+/- 0.5	+/- 1	+/- 2	+/- 0.5	+/- 1	+/- 2	+/- 0.5	+/- 1	+/- 2
Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	89	184	321	99	103	400	184	269	652

Notes: Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: OLS estimates, with percent change in mean total real estate prices as dependent variable and with winsorized tails

	Total Real Estate Prices			Non-Vacant Real Estate Prices			Non-Vacant Real Estate Prices		
	(24)	(25)	(26)	(27)	(28)	(29)	(30)	(31)	(32)
Qualified Opportunity Zone	-0.003 (0.010)	-0.000 (0.012)	-0.003 (0.017)	0.039*** (0.010)	0.019* (0.011)	0.006 (0.009)	-0.003 (0.010)	-0.004 (0.012)	-0.004 (0.012)
Controls?	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
Trimmed Data?	No	No	No	No	No	Yes	No	No	No
Winsorized Tails	10%	5%	1%	No	No	No	10%	5%	1%
R^2	0.115	0.101	0.071	0.010	0.064	0.083	0.100	0.098	0.098
N	1621	1621	1621	1576	1576	1557	1576	1576	1576

Notes: Standard errors are in parentheses. Controls include economic and demographic variables.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: OLS estimates, with percent change in number of transactions as dependent variable

	(33)	(34)	(35)	(36)
Type of Transaction	All Real Estate	All Real Estate	Non-vacant Residential	Non-vacant Residential
Qualified Opportunity Zone	-0.053	0.085	0.042^{**}	0.002
	(0.197)	(0.224)	(0.021)	(0.023)
Controls?	No	Yes	No	Yes
R^2	0.000	0.020	0.003	0.047
N	1621	1621	1576	1576

Notes: Standard errors are in parentheses. Controls include economic and demographic variables.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Average yearly real estate price of QOZs versus non-QOZs

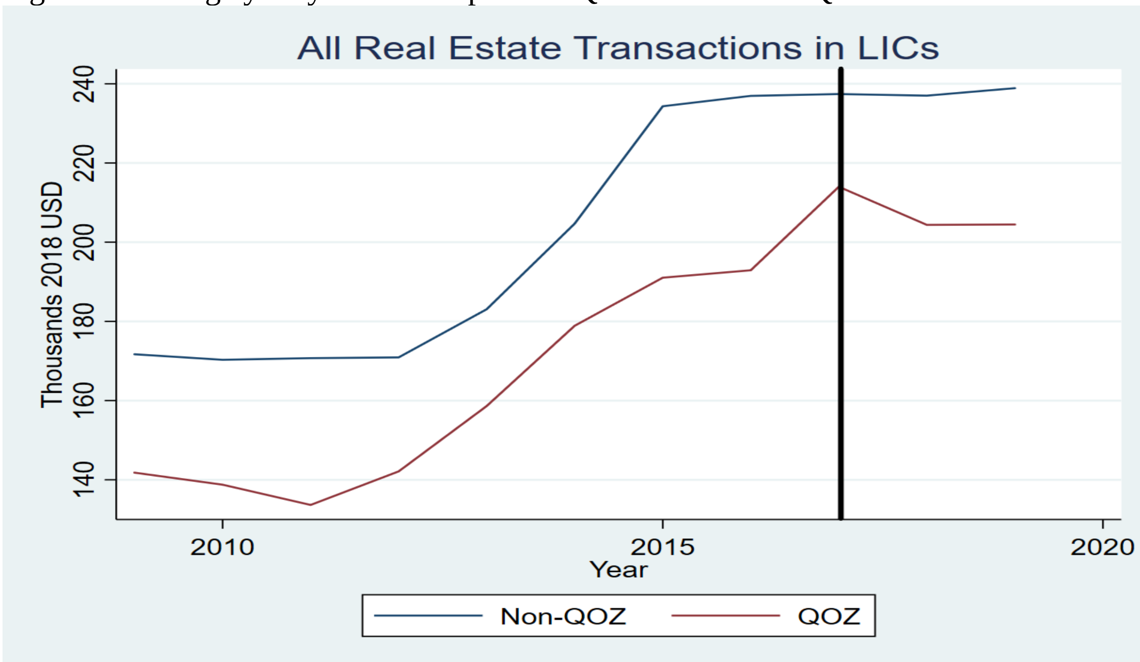


Figure 2: Average yearly home price of QOZs versus Non-QOZs

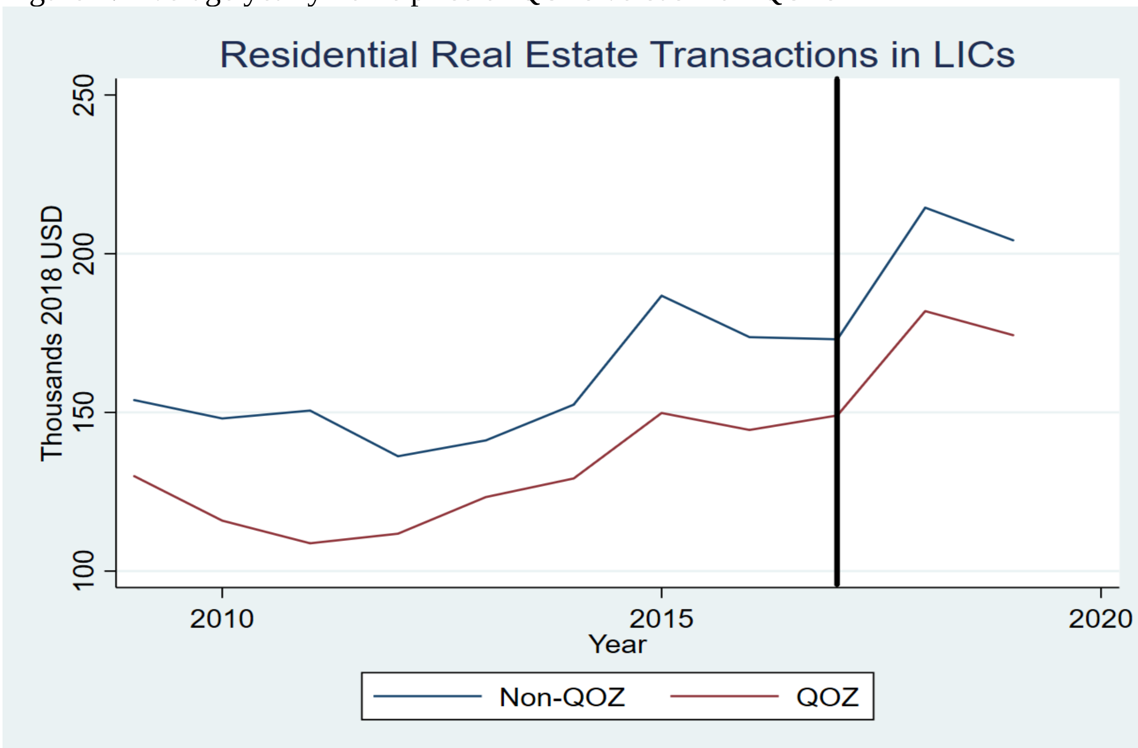


Figure 3: Average yearly home price of QOZs versus Non-QOZs used in regression discontinuity estimates

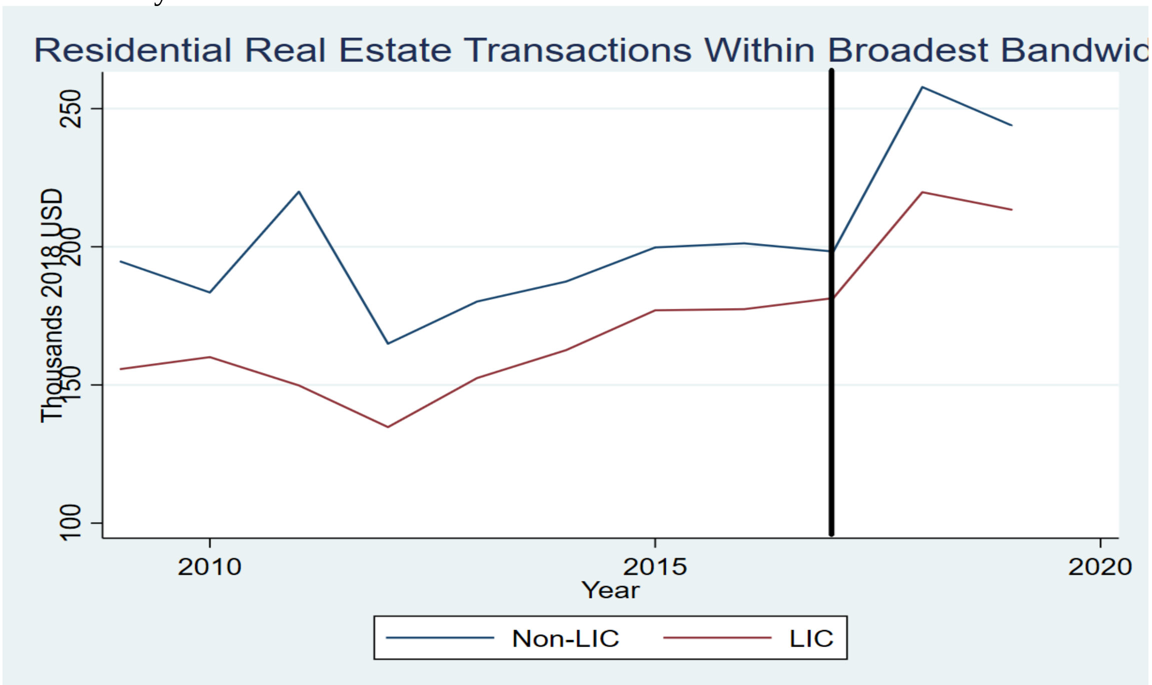


Figure 4: Percent of census tracts nominated as QOZs by poverty rate (Florida only) – Subsample included in the broad bandwidth shown

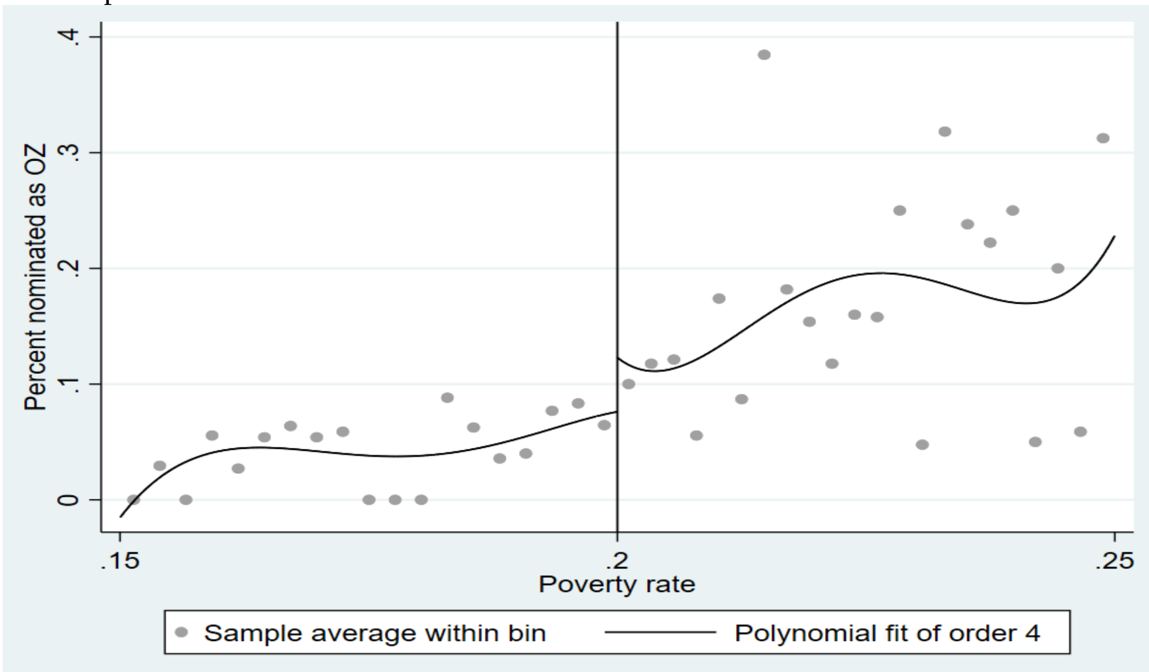


Figure 5: Percent of census tracts nominated as QOZs by distance from income eligibility cutoff (Florida only) – Subsample included in the broad bandwidth shown

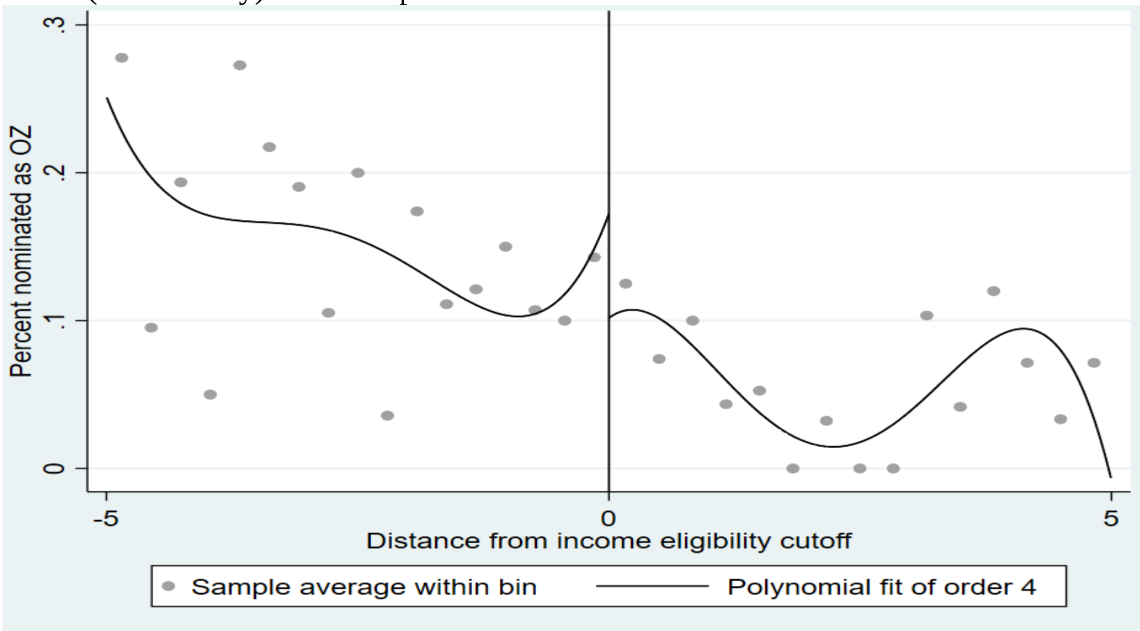


Figure 6: Percent change in mean total real estate prices by distance from the poverty cutoff (Florida only) – Subsample included in the broad bandwidth shown

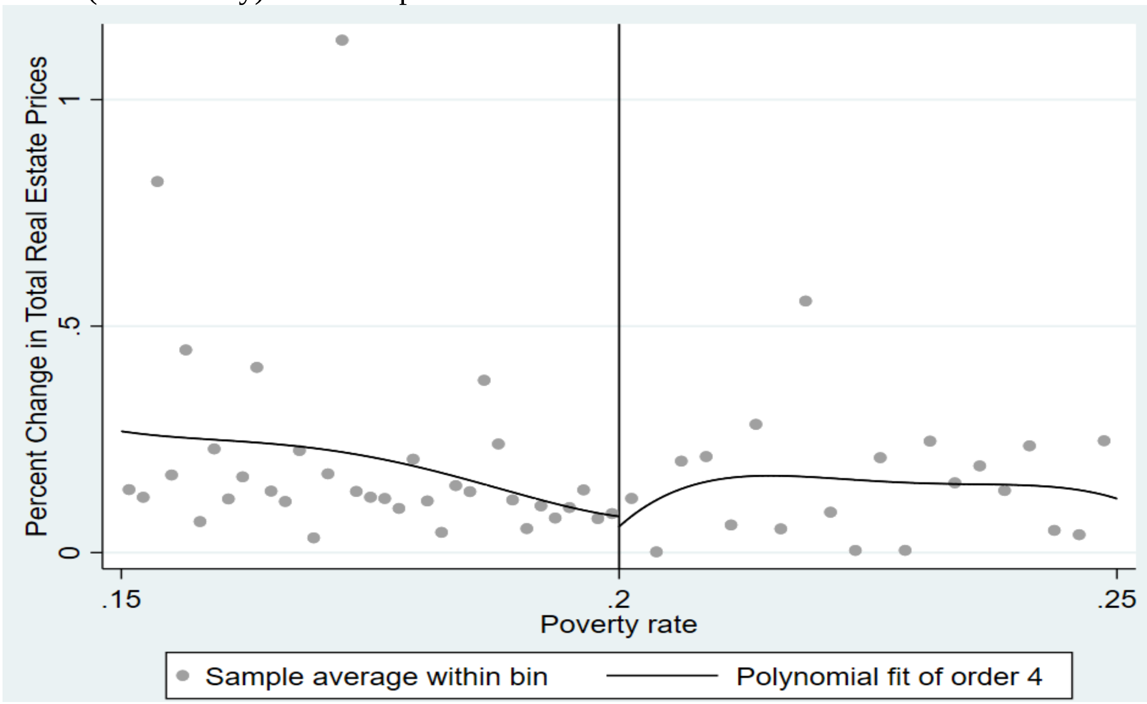


Figure 7: Percent of census tracts nominated as QOZs by distance from income eligibility cutoff (Florida only) – Subsample included in the broad bandwidth shown

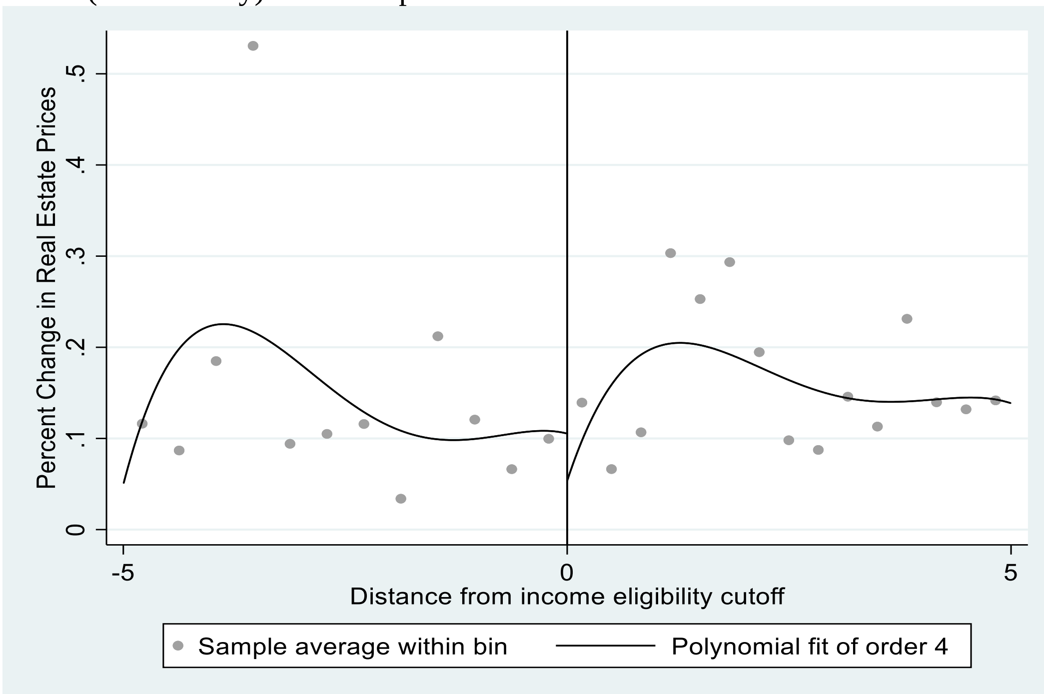


Figure 8: Percent of census tracts nominated as QOZs by poverty rate (Florida only) – Entire sample

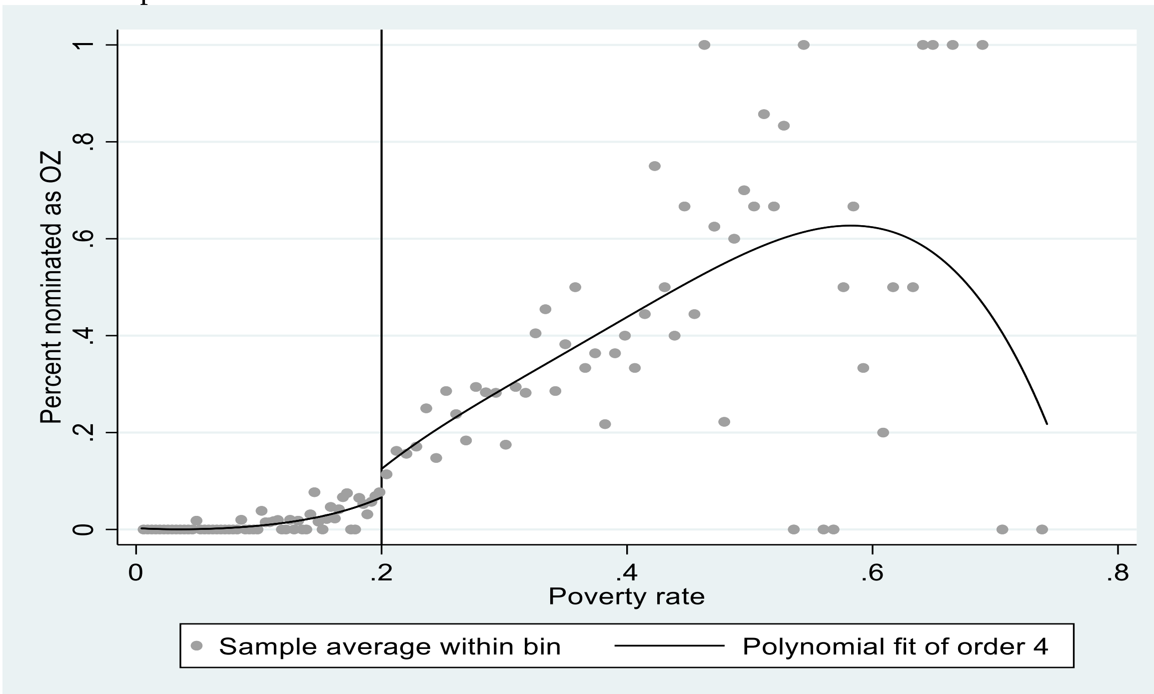


Figure 9: Percent of census tracts nominated as QOZs by distance from income eligibility cutoff (Florida only) – Entire sample

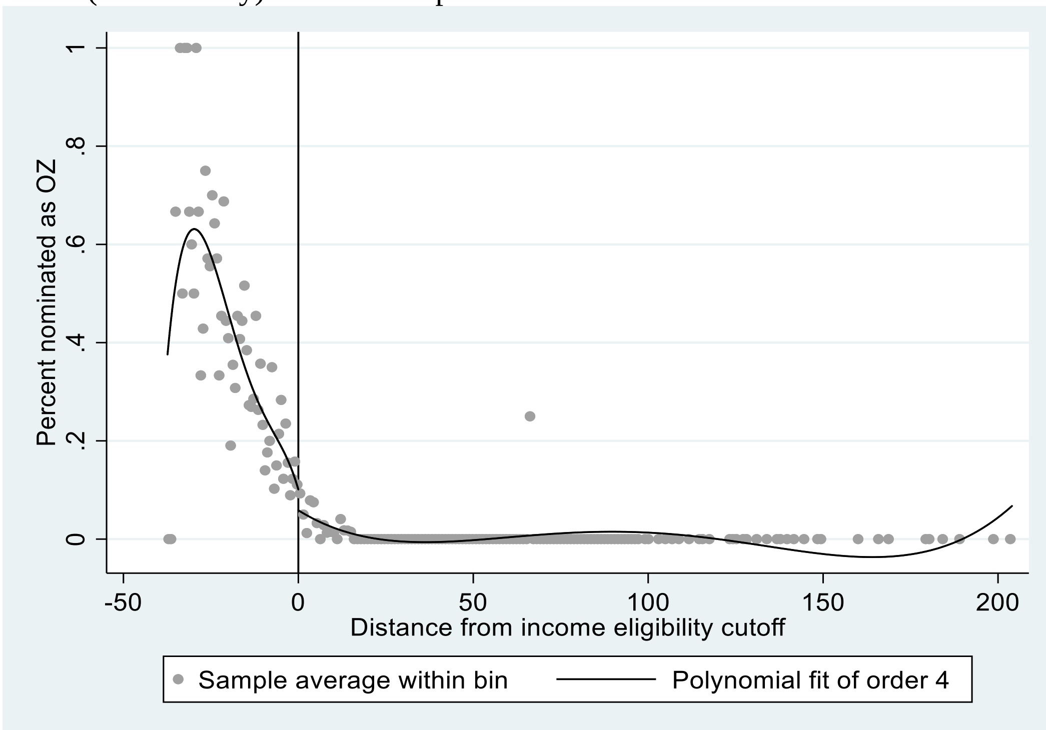


Figure 10: Percent change in mean real estate value by poverty rate (Florida only) – Entire sample

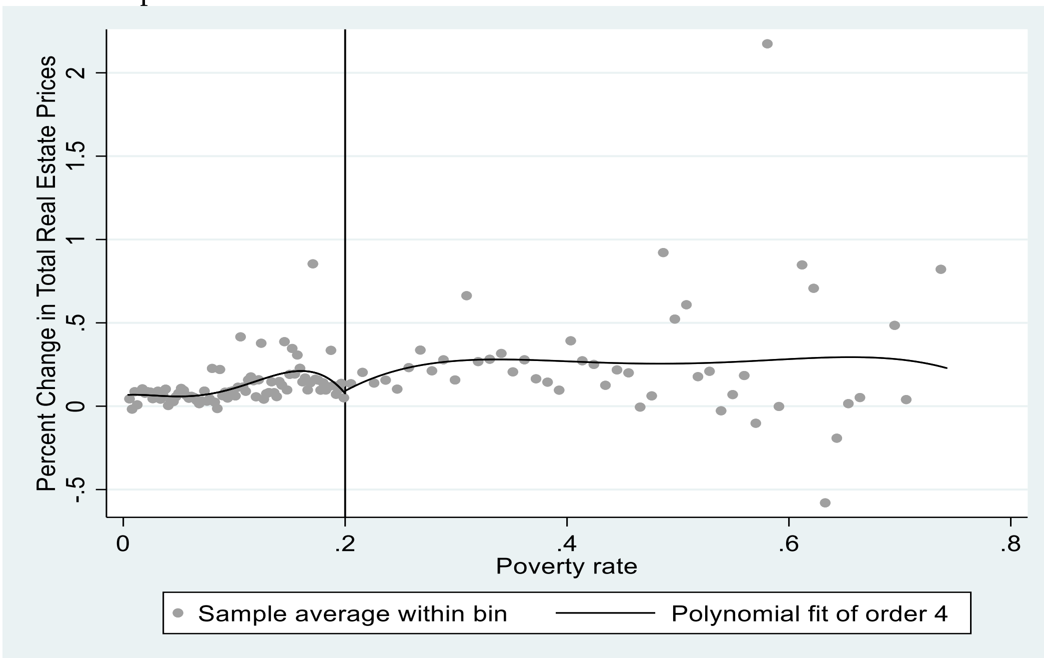
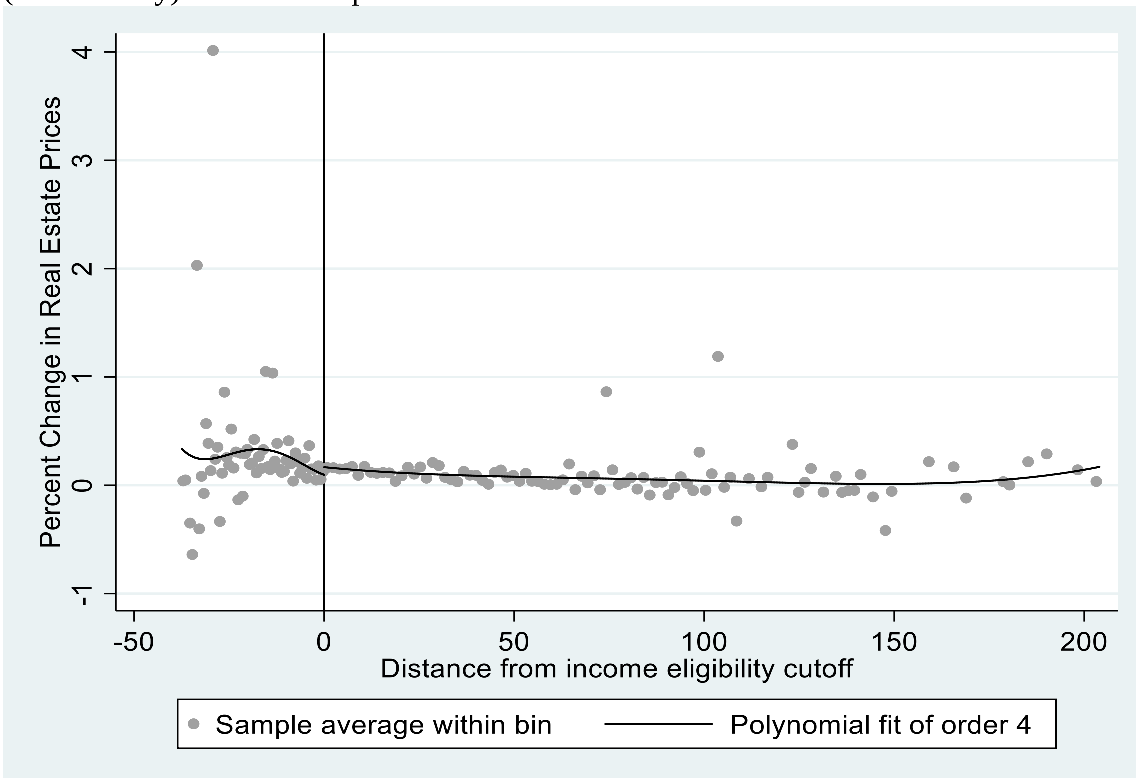


Figure 11: Percent change in mean real estate by distance from income eligibility cutoff (Florida only) – Entire sample



Biography

Sean Larkin was raised in Bellevue, WA along with a younger sister by loving parents and grandparents before going off to Pacific Lutheran University, primarily because almost no one from his high school was going there. It was at PLU that he found his primary hobby- board games- met his future wife and was able to explore a wide variety of subjects, ultimately earning bachelor's degrees in both economics and applied physics. Wanting to see more of the country and continue exploring economics- the subject that most held his attention- he and his then fiancée moved to Orono, ME where he earned an MA in Economics at the University of Maine. Having experienced both the depths of a couple New England winters and the joys of graduate research, after earning his masters he moved down to New Orleans to pursue a PhD in Economics at Tulane University. It was in New Orleans that he finally married his very patient significant other. More recently, he is about to move to the Maryland suburbs of Washington, DC to join the US Census Bureau as an economist. His economic research has been empirical in nature, looking at issues of public finance, health economics and applied microeconomics.