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# Essays in Public Policy Evaluation

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by

Andranik Andy Zanazanian

Dissertation Committee:
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# DEDICATION

To my wife, Nvart, and our son, Noah–the inspiration for my determination and dedication.

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I am indebted to my family for their resolute support during my academic career. This is *our* achievement!

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# ABSTRACT OF THE DISSERTATION

Essays in Public Policy Evaluation

By

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Doctor of Philosophy in Economics

University of California, Irvine, 2020

Professor Matthew Freedman, Chair

This dissertation examines the extent to which commercial land values capitalize the subsidies of place-based policies, whether the opening of a recreational marijuana dispensary generates spillovers onto other nearby retail establishments, and if grandparents' pension eligibility engenders an intergenerational redistribution of labor supply among co-residing mothers and grandparents, and by extension, the amount of time that grandparents can transfer to mothers in the form of child care. The data used for this dissertation include restricted-access commercial land sale and retail rent data from CoStar and publicly available data from government entities, such as the United States Census Bureau, the United States Department of the Treasury's Community Development Financial Institutions Fund, and the Washington State Liquor and Cannabis Board. The empirical methods used in this dissertation include regression discontinuity and difference-in-differences models. In the first chapter, I show that a United States federal policy that is designed to provide tax incentives to promote business investment in low-income communities throughout the country increases commercial land values by 15 percent, which implies that roughly 75 percent of the incidence of the policy's subsidies fall on landowners in the form of higher land values. In the second chapter, I estimate that the spillover effects of recreational marijuana dispensaries onto nearby businesses are small or offsetting. In the third chapter, I find a negative but minimal impact of the United States Social Security early-retirement age on grandparents'

labor supply and that this plausibly exogenous source of variation in grandparents' time does not impact mothers' labor market outcomes.

# Chapter 1

# Evaluating the Incidence of Place-Based Policies on Land Values: Evidence from the New Markets Tax Credit Program

## 1.1 Introduction

Place-based policies are government interventions that subsidize economically disadvantaged areas to stimulate economic growth and development. In contrast to *person*-based policies, like welfare, which directly transfers income to specific groups of needy individuals, the rationale behind targeting by geography is to create jobs, and thus help the disadvantaged residents living in and near the targeted region. Due to their appeal as a vehicle for improving job opportunities, place-based policies remain a popular tool for policymakers (Kline and Moretti, 2014). However, economic theory predicts that in the absence of market frictions, combined with an inelastic supply of land, in-migration and the subsequent increased demand for land in targeted areas can result in the incidence of the location-specific subsidy falling primarily onto landowners, and thus undermining the primary objectives of the policy (Lynch and Zax, 2011, Busso et al., 2013).[1]

In this chapter, I test this theoretical prediction by evaluating the case of the U.S. federal government's New Markets Tax Credit (NMTC) program, which incentivizes private investment in low-income areas by awarding federal income tax credits to participating private investors. The NMTC is an ideal candidate for studying this question for three reasons. First, a prevailing characteristic of the program is that it favors real estate investment, which suggests that the incidence of the subsidy on land ought to be more discernible within its context, relative to in the context other place-based policies that more directly incentivize job creation. Second, unlike other place-based policies like enterprise zones, which are often defined by streets and addresses, the NMTC targets commonly identifiable geographic locations–census tracts–thereby minimizing attenuation bias by improving the likelihood of correct assignment of targeted and un-targeted areas on behalf of researchers. Third, a discontinuity in the rule determining census tracts' eligibility for the NMTC enables the construction of credible counterfactuals, and accordingly, reliable estimation of the causal effects of the policy.

I identify the impact of the NMTC on land values by implementing a regression discontinuity design that compares areas around a local income cutoff–the ratio of the tract's median family income (MFI) to the state's or metropolitan area's MFI–which largely determines eligibility for the NMTC. Using restricted-access commercial land sale data from CoStar, I find positive and large estimated effects across a variety of specifications, the most conservative of which corresponds to a roughly 15 percent increase in land values. To lend credence to the causal interpretation of the regression discontinuity estimates, I perform a battery of robustness

[1] See Glaeser and Gottlieb (2008) for a detailed discussion on a spatial equilibrium model and its implications on place-based policies.

tests, such as varying the size of the window around the MFI ratio cutoff, exploiting the timing of the announcement of the NMTC, and considering placebo cutoffs different from the true cutoff. I also explore heterogeneous effects across various commercial land types as well as across more restrictive and less restrictive metropolitan areas. The results substantiate the overarching conclusion of this research, which is that the NMTC has a significant and long-lasting positive effect on commercial land values.

This is the first study to credibly estimate the causal impact of a large-scale and long-lasting place-based policy directly on commercial land values.[2] Most closely related are the few papers that have studied the capitalization of place-based policies into non-residential structures. Erickson and Syms (1986) analyze trends in industrial building rental rates in and around enterprise zones within Manchester, England, during the late 1970s and early 1980s. They find that prices inside the zone and on its periphery declined before zone designation, but increased inside the zone and flatlined on the periphery, post-designation. Bond et al. (2013) investigate rental payments for commercial structures inside enterprise zones throughout England, during the early 1990s and the mid-2000s. They employ a standard hedonic model and estimate a roughly full pass-through of the subsidy to property owners. Landers (2006) also uses a hedonic model to estimate the effects of a state enterprise zone program on commercial and industrial buildings in the greater Cleveland, Ohio area, during the mid-1980s and the early 1990s. The author compares properties within enterprise zones to those outside of them and generally estimates large, but statistically insignificant positive effects. Recent working papers by Burnes (2016) and Sage et al. (2019) estimate the impact of California's enterprise zones and the federal government's newly instituted Opportunity Zones, respectively, and attain results in line with the aforementioned studies.[3]

[2]Sage et al. (2019) study the impact of Opportunity Zones–a recently sanctioned national place-based policy–on land values using a difference-in-differences design and construct control groups via propensity score matching.

[3]On the other hand, evidence on the capitalization of place-based policies into residential home values is mixed. Boarnet and Bogart (1996), Freedman (2012), Busso et al. (2013) and Hodge and Komarek (2016) estimate coefficients near zero or statistically indistinguishable from zero, yet estimates by Hanson (2009), Krupka and Noonan (2009), and Freedman (2013) are positive and statistically significant. While these

This article's main takeaway, which is that a substantial portion of the benefits of the NMTC accrue to landowners, is consistent with previous research on other place-based policies that find only limited improvements to employment opportunities and wages for residents in targeted areas (Bondonio and Greenbaum, 2007, Hanson, 2009, Neumark and Kolko, 2010, Lynch and Zax, 2011). In contrast, multiple other studies find positive effects on these specific outcomes, which are the primary aims of programs that explicitly target by geography (Billings, 2009, Hanson and Rohlin, 2011, Busso et al., 2013, Freedman, 2013). Naturally, this research contributes to the voluminous empirical literature on place-based policies, but by inquiring into an understudied outcome like commercial land values, it imparts additional evidence on the costs and benefits of spatial targeting, thereby shedding light on the policy debate (Neumark and Simpson, 2015). More broadly, this research is also linked to a long literature on the economic incidence of redistributive government policies– such as food assistance programs (Hastings and Shapiro, 2018) or housing vouchers (Eriksen and Ross, 2015), covered thoroughly by Moffitt (2016).

I organize the rest of the chapter as follows. Section 1.2 describes the structure of the NMTC, profiles the prevalence of real estate investments within the program, and briefly reviews its relevant literature. Section 1.3 outlines the empirical strategy, which leverages a discontinuity in the rule that determines eligibility for NMTC-subsidized investments. Section 1.4 describes the CoStar data and provides descriptive statistics for tracts stratified by their eligibility status. Section 1.5 presents the results, Section 1.6 discusses them, and Section 1.7 concludes.

studies are related to this research, they are also distinct from it for at least two reasons. One, the supply of residential homes can more easily respond to the increased demand for land in targeted areas, which can lead to an attenuation of the price effect. Second, because place-based policies are supply side incentives that subsidize businesses, then their benefits are more discernable in the context of commercial rather than residential land values.

## 1.2 New Markets Tax Credit Program

### 1.2.1 Program Structure

The NMTC was signed into law in December 2000 as part of the Community Renewal Tax Relief Act. It is intended to attract private investment into economically distressed areas by leveraging public-sector resources–federal tax credits–to subsidize activities that impact the development of distressed communities (Rubin and Stankiewicz, 2005). The original legislation authorized the program from 2001 to 2007 and allowed for $15 billion of equity investment, but Congress has subsequently extended it through 2019 and the total amount awarded to date is more than $57 billion.

Figure A.1 provides an overview of the key components of the NMTC. Community Development Entities (CDEs), which are for-profit domestic corporations or partnerships that are certified by the CDFI Fund, are at the center of New Markets Tax Credit transactions.[4] CDEs make competitive bids to the CDFI Fund for an NMTC allocation award, which is the authority to solicit private investment capital, known as Qualified Equity Investments (QEIs).[5] In exchange for QEIs, and in addition to any return on investment, investors receive federal tax credits that reduce the investor's federal tax liability by an amount equal to 39 percent of the QEI, distributed over a seven-year compliance period; five percent in each of the first three years, and six percent annually in the final four years. In turn, CDEs make debt or equity investments, known as Qualified Low-Income Community Investments (QLICIs),

[4]To receive certification, a CDE must demonstrate a primary mission of serving or providing investment capital for low-income communities or people and maintain accountability to them through representation on the CDE's advisory or governing board (Abravanel et al., 2013).

[5]The application process involves CDEs disclosing detailed information on their intended use for the requested tax credits. The applications are then reviewed and rated by three independent readers on four primary criteria (0-25 points each): business strategy, community outcomes, management capacity, and capitalization strategy; as well as two additional criteria (0-5 points each): history of serving disadvantaged businesses or communities and a commitment to making investments in unrelated entities. The scores of the three readers, which have a maximum of 110 points each, are then summed and ranked. The CDFI staff then review the highest-ranking applications, and the NMTC manager makes the final allocation determination (Abravanel et al., 2013).

into qualifying businesses, known as Qualified Active Low-Income Businesses (QALICBs), which are located in Low-Income Communities (LICs).[6]

Between 2001 and 2011 (henceforth referred to as the RD period), the NMTC allocated $33 billion and distributed it across nine rounds, with allocation amounts per round ranging from $2 billion to $5 billion. Years 2001-2002 and 2003-2004 were bundled into a single round each, while each year thereafter was assigned its own round. Demand for allocations dwarfed supply during this period, as the total amount requested by all CDEs summed to more than $230 billion for all nine rounds, and varied between $21 billion and $30 billion per round. Over this time, the NMTC financed roughly 3,400 projects, summing to $54 billion in total project costs, of which $27 billion were backed by the program (New Markets Tax Credit Coalition, 2017).[7]

The rules of the NMTC stipulate that "substantially all" (no less than 85 percent) of QEIs are used as QLICIs within QALICBs located in LICs. Over the sample period, the NMTC subsidized projects in more than 2,300 census tracts, 93 percent of which were LICs.[8] With few exceptions, LICs are census tracts with a poverty rate of at least 20 percent or an MFI that does not exceed 80 percent of the greater of statewide MFI or the metropolitan area MFI (whenever applicable).[9] From its inception until 2011, the NMTC used 2000 Decennial Census data and geographies to define LICs.[10] Since then, it has used poverty and income

[6]For example, consider the hypothetical case of the CDFI Fund awarding an NMTC allocation of $1 million to a CDE. The CDE has five years to transfer the tax credit to a private investor in exchange for $1 million worth of QEIs. Once equipped with the QEIs, the CDE has one year to deploy the QEIs as QLICIs within QALICBs located in LICs. For the investor, the $1 million QEI generates an annual tax credit equal to $50,000 for the first three years and $60,000 for the final four years, summing to $390,000 over seven years.

[7]That is, the mean QLICI amount and project cost were roughly $8 million and $16 million, respectively. The median QLICI amount and project cost were $3.8 million and $5.8 million, respectively. On a per-project basis, the proportion of project cost backed by the NMTC to total project cost had a mean of 0.72 and a median of 0.8.

[8]The mean and median dollar amount of NMTC-subsidized investments per census tract equaled roughly $11.4 million and $5 million, respectively.

[9]The two exceptions are (1) census tracts in high migration rural counties with an MFI ratio that does not exceed 85 percent, and (2) census tracts with a population of less than 2,000 that are contained within a Federally-designated Empowerment Zone and are contiguous to at least one other LIC.

[10]Under certain circumstances, CDEs applying for allocations during the early years of the NMTC were

data from the American Community Survey (ACS) and applied them to 2010 census tracts. Accordingly, I use 2000 Decennial Census data and geographies throughout the analysis, and the sample period does not continue past 2011.[11]

Figure A.2 provides a visual representation of census tracts' LIC designation as a function of its poverty rate (vertical axis) and MFI ratio (horizontal axis). Of the 65,443 U.S. census tracts established by the 2000 Decennial Census, nearly 39 percent qualified as LICs. Among the 25,149 tracts designated as LICs (non-green points), 12,430 qualified on both criteria (purple points), and 11,215 qualified on the MFI ratio criterion alone (red points), while only 1,305 qualified on the poverty rate criterion alone (blue points). The remaining 199 census tracts that qualified as LICs did so by being high-migration and rural census tracts or low-population census tracts that share a boundary with at least one other LIC.[12] Thus, more than 94 percent of all LICs qualified on the MFI ratio criterion, while less than 55 percent of all LICs qualified on the poverty rate criterion.

### 1.2.2 Prevalence of Commercial Real Estate Investment

Historically, QLICIs fall under two distinct categories: (1) real estate acquisition, construction, or rehabilitation and (2) non-real estate operations, such as working capital or fixed asset loans, roughly comprising two-thirds and one-third of NMTC-subsidized investments, respectively. Among the first category, a large portion, approximately three-fourths, goes

allowed to use 1990 Decennial Census data and geographies because 2000 Decennial Census data were not available in sufficient detail (CDFI Fund, 2002).

[11] Ideally, I would have leveraged the reshuffling of LICs in 2012 to potentially exploit changes to tracts' LIC status over time, but due to download restrictions I was unable to obtain commercial land sale data after 2011. About 17 percent of all U.S. census tracts' changed eligibility status during the second wave of designations; 35 percent within the narrow window of the 0.80 MFI ratio that are considered in the forthcoming analysis, which is not surprising since eligibility status is most likely to flip within this range of MFI ratio. However, among all U.S. census tracts as well as the subset, nearly two-thirds of reshuffled tracts transitioned from not qualifying to qualifying for the NMTC. This does necessarily imply that census tracts became poorer, as it could also be the case that poor areas with lower MFIs became more geographically distinct from surrounding areas with higher MFIs.

[12] The 2000 Decennial Census did not report MFI information on 561 U.S. census tracts, including these 199 LICs. Naturally, all tracts with missing income information are excluded from the forthcoming analysis.

to QALICBs that are real estate companies, which develop spaces for sale or lease to tenant businesses, with the remainder going to operating firms that use the infusion of funds for real estate expansion (Hula and Jordan, 2018). Notably, commercial real estate that involves primarily residential rental properties, which are defined as structures that derive 80 percent or more of their gross rental income from renting dwelling units, are ineligible for NMTC-subsidized financing.[13] Other forbidden uses of QLICIs include the financing of gambling-related businesses (e.g., racetracks, casinos, etc.), as well as tanning salons, massage parlors, liquor stores, and golf courses.

As outlined by Lambie-Hanson (2008), compliance and profitability bases give rise to the "tilt" toward real estate investment within the NMTC. Concerning compliance, one of the principal requirements for a business to be considered a QALICB is for it to be located in an LIC. Thus, a business that is located in a qualifying census tract that receives funding from the NMTC may be at risk of credit recapture if it relocates to a non-qualifying census tract. Since real estate properties are fixed to a location, real estate investments are not subject to this source of credit recapture. Another potential violation can arise if investors redeem the investment principal (i.e., QEI) during the seven-year credit allowance period. Since the return on investment on real estate projects often requires a longer time horizon than non-real estate investments, real estate investments are less prone to credit recapture during the seven-year credit allowance period.

Real estate deals are also generally more profitable because they involve fewer third-party expenses, carry less financial risk, and can be packaged with other subsidies that target by geography. Due to the aforementioned disparity in compliance hazard between non-real estate and real estate investments, the former may require more expertise to formulate financing structures to ensure compliance, ergo higher transaction costs. Moreover, since the property serves as collateral, real estate investments are intrinsically less risky. Lastly, real

[13]The construction and rehabilitation of low-income rental housing is subsidized by another federal place-based policy–the Low-Income Housing Tax Credit (LIHTC).

estate projects often qualify for additional federal as well as state and local subsidies, such as Historic Rehabilitation Tax Credits, Brownfields Tax Incentive, federal Empowerment Zones, state enterprise zones, and state NMTC programs.[14]

### 1.2.3 Related Research

For a nationally implemented place-based policy that has been in existence for nearly two decades, there is a dearth of academic research on the NMTC (Neumark and Simpson, 2015). Gurley-Calvez et al. (2009) explored whether NMTC-subsidized investments represent an increase in overall investments or a shift from investments in higher-income communities to NMTC-eligible communities. They found that at least a portion of investments are new, but also that many investments would have occurred in the absence of the NMTC, albeit not necessarily within NMTC-eligible communities. Harger and Ross (2016) studied whether the NMTC impacts businesses' location decisions. The authors estimated positive effects on the number of new firms in the retail industry, as well as an increase in employment in existing firms in the retail and manufacturing industries in tracts that are eligible for the tax credits. They attributed these results to the nature of the program, arguing that it is more favorable to firms in the retail and manufacturing industries.

Freedman (2012), who was the first to exploit the nonlinear relationship between MFI ratio and LIC designation, demonstrated that the 0.80 MFI ratio cutoff discontinuously increased NMTC-subsidized investments and slightly reduced unemployment and poverty rates. The author also showed an absence of a discontinuity in NMTC-subsidized investments at the cutoff among a subset of census tracts with poverty rates of 20 percent or more, for which the MFI ratio cutoff is non-binding. Freedman (2015) found evidence that new jobs created

[14]Enactment of the NMTC at the federal level spurred a number of states to offer similar state tax incentives. State NMTC programs often align with the federal program on several specific provisions, such as financial products offered, and eligibility of geographies, investors, and businesses. Currently, 14 states–AL, AK, AR, FL, GA, IL, KY, LA, ME, MS, NE, NV, OH, and UT–have their version of an NMTC.

in areas that receive NMTC financing do not necessarily go to residents of NMTC-eligible tracts. Additionally, there were relatively large increases in the amount of higher-paying jobs, which suggests that the NMTC may exacerbate the spatial mismatch of jobs and housing. Lastly, Freedman and Kuhns (2018) estimated a modest increase in the number of supermarkets in NMTC-eligible tracts, which have traditionally been underserved by food retailers. Using household-level scanner data, they found that the increase in food supply had no discernible effect on food purchasing patterns.

As discussed in the next section, I follow the same empirical approach employed by Freedman (2012)–regression discontinuity–to identify the effects LIC designation on average commercial land values for tracts within a narrow window of the 0.80 MFI ratio cutoff.

## 1.3 Empirical Strategy

Before describing the empirical model, it is perhaps useful to conceptualize the framework by which commercial land values capitalize the benefits of a place-based policy, particularly in the case of the NMTC. As already mentioned, nearly all NMTC-subsidized investments can be broadly divided into two major categories: real estate investments and non-real estate investments, the former of which may be further split into investments to real estate businesses (e.g., developers) and to operating businesses (e.g., retailers). The connection is straightforward for the case of real estate QLICIs. Subsidizing real estate projects, whether they are undertaken by real estate businesses or operating businesses, increases the profitability of development, and in turn the demand for land intended for business use and thus its value (Brueckner et al., 2017).

The mechanism by which non-real estate investments can impact commercial land values is slightly more complex. NMTC-subsidized investments in operating businesses typically involve loans with more attractive features than creditors would otherwise offer, such as below-market interest rates or lower than standard origination fees (Abravanel et al., 2013). QALICBs use these loans as working capital to cover wages, utilities, or rents, or to finance the purchase of non-real estate fixed assets, such as equipment or machinery. To the extent that this infusion of funds raises the marginal product of labor or capital within the impacted firm, the firm's profit will rise, thereby increasing demand for the applicable factor(s) of production. Assuming factors are supplied competitively at a marginal cost that is equal across targeted and un-targeted areas, mobile factor(s) will migrate to targeted areas until respective returns are equalized across areas (Lynch and Zax, 2011, Busso et al., 2013). In turn, land prices will adjust to meet the increased demand in targeted areas, with the rate of adjustment depending on the degree of non-land factor mobility and the supply elasticity of land that is suitable for businesses within the targeted area (Moretti, 2011).

Although the latter channel applies to place-based policies in general, the prevalence of real estate investment is a distinctive corollary of the NMTC. Moreover, unlike federal Empowerment Zones or state enterprise zones programs, the tax benefits from the NMTC do not go directly to businesses. The credit merely provides an incentive for CDEs to stimulate economic development in economically depressed areas with no requirement for passing through its real value to investors or businesses. Accordingly, the NMTC serves as a compelling case for evaluating the incidence of place-based policies on commercial land values because it tests a theoretical postulation, which is that a subsidy's economic incidence need not correspond to its statutory incidence. Instead, theory predicts that the more inelastic party bears the economic incidence. This implies that even though a subsidy's statutory incidence may fall onto mobile factors like firms or labor, for place-based policies in general, or investors, for the NMTC in particular, a substantial portion of the economic incidence will fall onto land, and more specifically land intended for commercial use, because it is the least mobile factor of production, and thus the most inelastically supplied.

The structure of the NMTC also mitigates some of the econometric challenges that are inherent in reliably estimating the effects of place-based policies. These include the potential endogeneity of where policies are adopted, which by definition are a highly selected set of geographic areas, and the threat of measurement error brought about by the incongruence between the boundaries of targeted regions and the boundaries of other commonly identifiable regions, like counties or census tracts, to which targeted locations are often matched (Neumark and Simpson, 2015).[15] As explained in Section 1.2.1, the NMTC targets census tracts largely based on a discontinuous eligibility rule around the 0.80 MFI ratio cutoff. Therefore, the natural empirical strategy is a regression discontinuity design that uses census tracts just above the cutoff as counterfactuals to tracts that are just below the cutoff, to identify the NMTC program's causal effect on commercial land values.[16]

Regression discontinuity (RD) is a quasi-experimental research design often used to evaluate a program's effect when the probability of receiving the program's *treatment* changes discontinuously due to an underlying running variable exceeding a known cutoff (Imbens and Lemieux, 2008, Lee and Lemieux, 2010, DiNardo and Lee, 2011). Within the context of this research, a census tract's LIC designation and MFI ratio are the treatment and underlying running variable, respectively, and the 0.80 MFI ratio is the known cutoff. "Sharp-RD" is a special case when the assignment rule is deterministic and the discontinuous shift in

[15]Baum-Snow and Ferreira (2015) provide a very useful review of the empirical strategies used in urban and regional economics for estimating causal effects.

[16]The structure of the Community Reinvestment Act (CRA), which is a federal law that encourages federally insured depository institutions (e.g., banks) to serve the credit needs of low- and moderate-income neighborhoods, also uses a 0.80 MFI ratio cutoff to designate "moderate-income" census tracts, but defines the ratio differently than the NMTC. More specifically, it uses the metropolitan area's (MSA's) MFI–as opposed to the greater of the state's or MSA's MFI–for all census tracts in MSAs and the state's nonmetropolitan areas' MFI–as opposed to the entire state's MFI–for all census tracts not in MSAs. Thus, the MFI ratio is different for all census tracts not in MSAs and also different for census tracts in MSAs where the MSA's MFI is less than the state's MFI; amounting to roughly half of all U.S. census tracts. Moreover, the difference-in-discontinuities analysis (see Section 1.5.2), which exploits the 0.80 MFI ratio cutoff in conjunction with the timing of the announcement of the NMTC, yields statistically significant estimated effects on the parameter of interest that are similar in magnitude to the main results' (see Section 1.5.1) and also null effects of the 0.80 MFI ratio cutoff on commercial land values independent of the NMTC. Taken together, this suggests that tracts' CRA eligibility status does not confound the impact of NMTC eligibility for commercial land values.

the probability at the cutoff is binary; otherwise, the design is "fuzzy." Due to the NMTC program's dual criteria for how a tract may qualify, I apply the fuzzy-RD design, wherein the probability of LIC designation above the 0.80 MFI ratio cutoff to below it changes from 0.032 to one.[17] More formally, I estimate the following equation:

$$Y_{ict} = \alpha_0 + \beta_1 LIC_i + f(MFIR_i - 0.80) + \mathbf{X}_i \psi + \theta_c + \delta_t + \mu_{ict} \tag{1.1}$$

Here, $Y_{ict}$ is the natural log of the average annual commercial land value (measured as log price per square foot) of census tract $i$, in county $c$, at time $t$. The dummy variable $LIC_i$ takes the value of one if the census tract's MFI ratio (MFIR) is at or below the 0.80 cutoff, and zero otherwise. The function $f(\cdot)$ controls for the relationship between the MFI ratio and the outcome of interest. Following Gelman and Imbens (2019), I use local linear or quadratic polynomials as specifications for the control function $f(\cdot)$ and allow the running variable's coefficients to vary on either side of the cutoff. The vector $\mathbf{X}_i$ includes time-invariant census tract-level characteristics; $\theta_c$ are county fixed effects; $\delta_t$ are year fixed effects, and $\mu_{ict}$ is an error term.

The fundamental identifying assumption underlying this approach is that within a narrow window of the 0.80 MFI ratio cutoff, census tracts just below the cutoff are comparable to census tracts just above the cutoff. Thus, any observed discontinuity in outcomes is attributable to the census tract's treatment status. Although I cannot test this assumption directly, I can test for some of its implications, such as the absence of sorting around the 0.80 MFI ratio cutoff and smoothness of predetermined covariates' means through the cutoff,

[17] Excluding the 561 U.S. census tracts with missing income information, the probability of LIC designation conditional on not qualifying on the income ratio criterion is $1{,}305/41{,}237 = 0.032$ (i.e., the number of tracts that qualified on the poverty rate criterion alone, divided by the number of tracts that did not qualify on the income ratio criterion). As described in Section 1.4.1, I also perform a supplementary analysis that is equivalent to a sharp-RD by excluding census tracts with poverty rates of 20 percent or more, for which the 0.80 MFI ratio cutoff is non-binding. Additionally, using a single index that considers the minimum of census tracts' signed distance from the 20 percent poverty rate and 80 percent median family income ratio cutoffs as the underlying running variable, with negative values corresponding to NMTC-eligible tracts, yields results that are similar to the ones presented in this chapter, and are available upon request.

which I show in Section 1.4.2. Moreover, I appeal to two key aspects of the institutional setting to buttress the validity of the identification strategy. First, LIC designation is based on 2000 Decennial Census data, which were collected before the NMTC was signed into law but released only after it was signed into law. Second, even if census tracts correctly anticipated the assignment rules of the NMTC, it is unlikely that they could manipulate MFI data to the extent of ensuring LIC designation, as doing so would not only require the coordination of roughly 2,000 families, per tract but also good knowledge of the state's or metropolitan area's MFI.

By virtue of the NMTC program's institutional background and the satisfaction of the assumptions of the RD design, I interpret the estimate of $\beta_1$ from equation (1.1) as the average treatment effect (ATE) of LIC designation on commercial land values for census tracts near the 0.80 MFI ratio cutoff. It is important to note that while a hallmark of RD is its internal validity, it often comes at the expense of external validity, which suggests that there may be differential effects of eligibility for NMTC-subsidized investments at other parts of the MFI ratio distribution. For example, since the estimated effects in the forthcoming analysis are for tracts right at the 0.80 MFI ratio cutoff, then by definition these are LICs with the highest levels of relative income. To the extent that these are also the LICs that are relatively better developed and with a limited supply of commercially zoned parcels of land, then the increased demand induced by the NMTC may lead to estimated effects that are different than in less developed tracts with MFI ratios that are far from the cutoff.

Figure A.3 plots the density of the MFI ratio for all U.S. census tracts, with the solid black vertical line denoting the 0.80 MFI ratio cutoff. Given its placement near the center of the distribution, a strong case can be made that the results of this research may still generalize to a large portion of moderate-to-low income tracts. Also contributing to the external validity of this research is that the rules determining tracts' LIC designation are based on economic characteristics of tracts' *residents*, which may not necessarily correlate with its *commercial* land values. To illustrate, consider a bustling downtown area with relatively poor residents and a lagging community far away from the central business district. While both may be eligible to receive NMTC-subsidized investments, due to varying market conditions across locations, treatment effects may also vary. Thus, pooling tracts with a high degree of variability in the outcome of interest, but that are still within a narrow window of the 0.80 MFI ratio, can be conducive to the overall interpretability of the results.

## 1.4 Data

### 1.4.1 Data Sources

I use three data sources for the analysis. Commercial land value data come from CoStar– one of the leading providers of commercial real estate information, analytics, and online marketing for industry professionals. Through "CoStar University," academics are granted a free subscription to CoStar's data set, which contains millions of researched and verified sale records. These data feature variables on the characteristics of the sold parcel of land, such as address, its corresponding latitude and longitude coordinates, land area square feet, its secondary type (i.e., commercial, industrial, or residential), a CoStar defined star rating of the land parcel; as well as variables on the terms of the sale, such as the sale amount, sale date, and other transaction notes.[18] There are more than 205,000 property-level commercial land sale records in the CoStar data set from January 1999 to December 2011, covering roughly 50 percent census tracts and 56 percent of the population within the U.S.[19]

[18]CoStar's star rating is an evaluation of properties on a five-point scale based on the characteristics of each property type, such as architectural attributes, amenities, detailed property type specifics, etc. The rating is primarily applied to structures but can also be used to evaluate undeveloped land on specific features such as topography, existing connections to utilities, boundary geometry, etc.

[19]While commercial land sale data before 1999 and after 2011 were available, it was necessary to select 1999 as the start year because CoStar's coverage is inconsistent in previous years, and 2011 as the end year because the NMTC transitioned into applying poverty and income data from the American Community Survey (ACS) onto 2010 census tracts in 2011.

The unit of observation in the forthcoming analysis is a census tract-by-year pairing. I arrive at this level of aggregation by first matching the microdata observations in the CoStar data set, for which information on sale price and land size are available, to census tracts using Stata shapefiles and Stata's geoinpoly command. I then collapse to the census tract-by-year level, which results in about 100,000 tract-by-year pairings spread out across roughly 33,000 unique census tracts. As seen in Figure A.4, census tract-by-year pairings are distributed roughly uniformly over this time window with a slight uptick in later years, which is most likely attributable to improvements in CoStar's coverage.

The data on NMTC-subsidized investments and census tracts' LIC designation come from the CDFI Fund, which I match with tract-level information from the 2000 Decennial Census.[20] As a consequence of commercial real estate transactions occurring primarily in large urban areas, about 90 percent of tracts in the CoStar data set are in metropolitan areas. By comparison, only about 78 percent of all United States census tracts are in metropolitan areas. Importantly, however, for LICs with observed commercial land sales in the CoStar data set, 95 percent qualified on the MFI ratio criterion, and 54 percent on the poverty rate criterion, roughly the same percentages, respectively, as all United States LICs. This enables me to use a tract's positioning relative to the 0.80 MFI ratio cutoff to evaluate the effect of LIC designation on average commercial land values. That is, I compare red- and purple-colored census tracts just to the left of the 0.80 MFI ratio cutoff in Figure A.2 to green- and blue-colored census tracts that are just to the right of the cutoff. As a robustness check, I also perform the analysis on a subset of census tracts with poverty rates less than 20 percent, for which the 80 percent income ratio criterion alone determines LIC eligibility. This "cleaner" discontinuity allows for comparison of red-colored census tracts to green-colored census tracts ans is identical to a sharp-RD.

For the main analysis, I restrict the sample to census tracts within five percentage points of

[20] Tracts with missing information in the 2000 Decennial Census are excluded.

the 0.80 MFI ratio cutoff and with observed commercial land sales in the CoStar data set from January 2001 to December 2011 (henceforth referred to as the CoStar sample). Later, I extend the beginning of the sample period to January 1999 and employ a difference-in-discontinuities analysis that exploits the timing of the announcement of the NMTC in the manner of Grembi et al. (2016).

Because tracts' LIC status does not change during the RD period (i.e., 2001-2011), ideally, one would use the tract as the unit of observation rather than the tract-by-year. However, in the CoStar sample, commercial land sales are not observed for each tract during each year of the RD period. Figure A.5 shows the histogram of the number of years that a sale is observed during the RD period for each tract. If at least one commercial land sale was observed in each tract for each year of the RD period then the shape of the distribution would have been unimodal with a mass at eleven–the total number of years in the RD period. Instead, the actual distribution is roughly a reflexive version of the hypothetical one, as sales for most tracts are only observed for a few years; more than three-quarters of tracts have at most three years of sales data.

While this temporal imbalance alone does not warrant aggregation to the tract-by-year level, the volatility of commercial real estate prices during the RD period, spawned by the real estate housing bubble of the 2000s and the ensuing Great Recession, suggests that secular trends affecting commercial real estate prices that are common to all census tracts may emerge disproportionately if the data are aggregated to the tract level. Figure A.6 illustrates the sharp downturn in commercial real estate prices during the later years of the RD period preceded by rapid increases (or stable prices) during earlier years–the dashed black line represents a quarterly time series of the percent change from a year ago of commercial land prices in the United States of the CoStar microdata and the solid gray line represents a quarterly time series of the percent change from a year ago of commercial real estate prices in the United States reported by the IMF (data before 2005 were unavailable). Thus, aggregating to the tract-by-year level and controlling for year fixed effects in all regression specifications can help mitigate the potential impact of this confounding factor.[21]

### 1.4.2 Descriptive Statistics

Figure 1.1 plots the number of tract-by-year pairings (black points) in half percentage point bins of the MFI ratio between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 cutoff.[22] The graph demonstrates that there is no evidence of sorting on either side of the cutoff. Both McCrary (2008) and Cattaneo et al. (2018) tests for a discontinuity in the density of tract-by-year counts also conclude that there is no statistically significant discontinuity at the cutoff. Figure A.7 presents a similar graph where the number of unique tracts in the CoStar sample are grouped into half percentage point bins of MFI ratio, between 0.70 and 0.90. Once more, there are no signs of sorting around the cutoff, and the similarity between the two illustrations suggests that tract-by-year pairings within the sample are roughly evenly distributed across census tracts.

To check for balance on observable characteristics, Table 1.1 reports descriptive statistics on demographic, housing, and income data from the 2000 Decennial Census for census tracts that are within five percentage points of the 0.80 MFI ratio cutoff (i.e., 0.75-0.85, inclusive),

[21] In Table 1.1 I provide evidence to support the validity of the RD design irrespective of the unit of observation under consideration. An analysis of LIC designation on average commercial land values aggregated to the census tract level yields results that are similar in sign and relatively similar in magnitude to the ones presented in this chapter, but are estimated with less precision, seemingly due to measurement error in the dependent variable. Results are available upon request.

[22] While the estimated effects from the main analysis use linear or quadratic control functions around a narrow window of the 0.80 MFI ratio cutoff, when depicting paths as a function of the running variable (e.g., figures 1.1 - 1.3), I widen the window around the cutoff and also use a cubic polynomial for lucidity. In Figure 1.4 I present results from a robustness test that directly analyzes tracts around a wider window of the 0.80 median family cutoff. Results from regressions that use a fully flexible cubic polynomial control function yield similar estimated coefficients, compared to regressions that use lower-order polynomial control functions, but much larger standard errors due to the collinearity among the terms of the control function.

stratified by tracts that are at or below the cutoff (odd-numbered columns) and tracts that are above the cutoff (even-numbered columns). The unit of observation is a tract-by-year pairing in columns (1) and (2), and a tract in columns (3) and (4), for census tracts with observed commercial land sales in the CoStar data set from January 2001 to December 2011. For reference, I present a comparison between the two strata for all U.S. census tracts within five percentage points of the 0.80 MFI ratio cutoff (henceforth referred to as the RD subset), in columns (5) and (6).[23] Parentheses are p-values from two-sided tests of differences in means. Due to the possibility of repeat occurrences of the same census tract for columns (1) and (2), standard errors are clustered at the census tract level for tract-by-year differences in means.

As seen by comparing within all three column pairs, the means of most demographic and housing variables do not differ across *treated* (at or below the cutoff) and *control* (above the cutoff) groups in a statistically and economically significant way. This suggests that within a narrow window of the 0.80 MFI ratio cutoff, census tracts' LIC designation and demographic or housing characteristics are uncorrelated. The uniformity within *treated* and *control* groups, respectively, across the first two pairs of columns also verifies that tract-by-year pairings within the CoStar sample are not disproportionately distributed across census tracts along demographic or housing characteristic lines. Lastly, the consistency across census tracts in the CoStar sample (columns (1)-(4)) to the RD subset (columns (5) and (6)) suggests that the subset of tracts with observed commercial land sales are not substantially different on observable characteristics from all census tracts as a whole. The congruence between *treated* and *control* groups within all three pairs of columns, as well as

[23] The number of census tracts from the CoStar sample (i.e., census tracts within five percentage points of the 0.80 MFI ratio cutoff and with observed commercial land sales in the CoStar data set from January 2001 to December 2011) comprise roughly 40 percent of census tracts and 46 percent of the population within the RD subset (i.e., all U.S. census tracts within five percentage points of the 0.80 MFI ratio cutoff). The smaller proportion of census tracts in the CoStar sample relative to the RD subset, in comparison to the proportion of census tracts in the CoStar data set relative to the entire U.S. (see Section 1.4.1), is most likely attributable to relatively temperate commercial real estate activity within lower-income census tracts, which are those under consideration in the RD analysis.

across the first two pairs of columns substantiates the internal validity of the research design, while the compatibility between the first two pairs of columns to the third pair of columns lends credence to its external validity.

Expectedly, MFI is lower and the poverty rate is higher in tracts below the 0.80 MFI ratio cutoff, on the order of six to seven percent, and 13-16 percent, respectively. There is also some evidence that the share of Hispanic population is higher and the share of housing that is owner-occupied is lower in the treated group of census tracts across all three pairs of columns. However, these detectable differences in means do not necessarily pose a threat to the identification strategy, because the identifying assumption of the RD design is smoothness of these covariates' respective distributions through the cutoff. I test this assumption and present the results in Table 1.2 and Figure 1.2.

Table 1.2 shows the estimated coefficients on the discontinuity variable from regressions where the outcome is a demographic, housing, or income characteristic. The unit of observation for each column pair corresponds to the respective column pair from Table 1.1. The first column in each column pair represents a regression specification that controls for linear polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff, while the second column represents a regression specification that controls for a fully flexible quadratic polynomial function. All regression specifications also control for county fixed effects. Due to the possibility of repeat occurrences of the same census tract, standard errors are clustered at the census tract level for tract-by-year regressions (columns (1) and (2)). All estimated discontinuities are small and for the most part, statistically indistinguishable from zero. These estimates suggest that among the subset of tracts with observed commercial land sales in the CoStar data set, LIC designation for census tracts close to the 0.80 MFI ratio cutoff is as good as randomly assigned.

Visual inspection of the graphs in Figure 1.2 also corroborates this claim. Points are tract-by-year mean values of various demographic, housing, or income characteristics, in half percentage point bins of MFI ratio, for tracts with observed commercial land sales from January 2001 to December 2011. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 MFI ratio cutoff. Figure A.8 presents similar graphs where the CoStar sample's unique tracts' mean values are grouped into half percentage point bins of MFI ratio, between 0.70 and 0.90. Both illustrations show that the distributions of observable characteristics are smooth through the cutoff, which suggests that *treatment* is as good as randomly assigned near the 0.80 MFI ratio cutoff.

## 1.5 Results

### 1.5.1 Main Analysis

Table 1.3 reports the estimates on the discontinuity variable from equation (1.1). The unit of observation is a tract-by-year pairing for tracts with observed commercial land sales from January 2001 to December 2011 that are within five percentage points of the 0.80 MFI ratio cutoff. The first two columns represent regression specifications that control for a linear polynomial function in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff, while the last two columns represent regression specifications that control for a fully flexible quadratic polynomial function. Year and county fixed effects are included in all specifications, and control covariates are also included in specifications represented by columns (2) and (4). Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district.[24]

The county fixed effects, of which there are 1,135, help control for underlying local commercial real estate market conditions that affect average commercial land values and may be common to both targeted and un-targeted areas alike. In doing so, the effect of LIC designation on average commercial land values is identified by comparing tracts that are eligible to receive NMTC-subsidized investments to similar tracts within the same county that are ineligible for the subsidy. Due to the sparsity of tracts with commercial land sales, coupled with the RD identification strategy, which restricts attention to tracts within a narrow window of the 0.80 MFI ratio cutoff, many of the counties in the CoStar sample feature only a single tract. Thus, much of the identifying variation from equation (1.1) comes from comparing tracts on either side of the cutoff within counties with multiple tracts.[25]

Since the outcome in equation (1.1) is a logged variable, the regression discontinuity estimates in Table 1.3 can be interpreted approximately as percentage changes when multiplied by a hundred. On account of the sizable variation across specifications, I caution against interpreting their magnitudes and simply regard them as positive and fairly large. Standard errors, which are clustered at the census tract, county, or metropolitan (MSA) levels, are reported in parentheses, brackets, or curly brackets, respectively. The estimated effects are statistically significant between the five to ten percent significance levels, regardless of the geographical level wherein correlation of the error term is allowed.[26] Thus, standard errors are henceforth clustered at the assignment level–the census tract–which is a common prac-

[24]Distance from a census tract's centroid to its corresponding metropolitan area's centroid is used as a proxy for distance to central business district. For census tracts outside of metropolitan areas, the maximum value for all metropolitan census tracts within the same state is used.

[25]Put differently, initially, commercial land sales in the CoStar data set are not observed for every tract in a county, and because tracts with an MFI ratio far away from the 0.80 cutoff are excluded from the CoStar sample, regardless of whether commercial sales are observed, there is a disproportionate number of one-to-one pairings between census tracts and counties in the CoStar sample. Seven percent of all U.S. counties have only a single census tract; 27 percent in the initial CoStar data set and 51 percent in the CoStar sample. Unsurprisingly, regressions parallel to those presented in Table 1.3 that are on a subset of the CoStar sample that excludes counties with singleton tracts generate nearly identical results.

[26]Clustering on various-sized bins of the running variable (e.g., half percentage point, full percentage point) also does not substantively change the interpretation of the estimated effects.

tice in the RD literature (Cameron and Miller, 2015). The insertion of control covariates reduces the estimated standard errors and Wald tests indicate that their inclusion statistically significantly improves the fit of the model. Wald tests also show that the inclusion of the additional terms from the higher-order control functions do not improve the fit of the model, and therefore the regression represented by the second column, which includes a linear control function and the standard set of control covariates, as well as county and year fixed effects is the preferred specification. In Section 1.6.2 I discuss the plausibility of these estimated effects and their implications on the percentage of the subsidy that is passed through to landowners.

Figure 1.3 offers a graphical representation of the effect of the underlying running variable crossing the 0.80 cutoff on commercial land values, while also controlling for temporal and spatial dimensions that are correlated with the outcome of interest. Points are tract-by-year mean values for *residualized* natural log of average sale price per square foot of land, in half percentage point bins of MFI ratio between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. As before, red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 MFI ratio cutoff. Residuals are obtained from regressions that control for county and year fixed effects only. These plots are analogous to the estimated effects from columns (1) and (3) in Table 1.3, and for that reason, there is a large and positive discontinuity depicted at the 0.80 MFI ratio cutoff. For comparison, Figure A.9 presents a graph that corresponds to the most parsimonious regression specifications (not shown in Table 1.3), which exclude county and year fixed effects and the standard set of control covariates. As expected, these illustrations of the *raw* data, which do not properly account for potentially confounding factors, reveal no discontinuity at the 0.80 MFI ratio cutoff.

As described in Section 1.4.1, I also perform a supplementary analysis that is equivalent to a sharp-RD by excluding census tracts with poverty rates of 20 percent or more, for which the 0.80 MFI ratio cutoff is non-binding. That is, I ignore the purple- and blue-colored census tracts in Figure A.2 altogether, and compare red-colored census tracts just to the left of the 0.80 MFI ratio cutoff to green-colored census tracts that are just to the right of the cutoff. Table 1.4 presents the estimates on the discontinuity variable from regressions on this restricted sample. Predictably, when the identifying variation is not confounded by the dual-threshold, the amplitudes of the estimated coefficients are larger and standard errors are relatively similar to those from the full sample, despite the ten percent decrease in sample size. These findings suggest that there are positive effects of LIC designation from the NMTC on commercial land values within a narrow window of the 0.80 MFI ratio cutoff.

As a robustness check on the sensitivity of the main results to the size of the window around the 0.80 MFI ratio cutoff, I implement local linear or quadratic regression discontinuity estimators using various bandwidths, like Imbens and Kalyanaraman (2012). Figure 1.4 shows point estimates and corresponding 95 percent upper and lower bounds on the discontinuity variable from regressions that control for linear (red) or quadratic (blue) polynomial functions, county and year fixed effects, and the standard set of control covariates. Each tick mark along the horizontal axis refers to the sample of tracts with observed commercial land sales from January 2001 to December 2011 that are within 2, 3, etc. percentage points of the 0.80 MFI ratio cutoff. The dashed black vertical line denotes five percentage points of the 0.80 MFI ratio cutoff, which is the bandwidth used in the main analysis. The point estimates from both linear and quadratic models are positive, relatively stable, and statistically significant between the five to ten percent significance levels, which indicates that the results from the main analysis are not sensitive to bandwidth choice around the 0.80 MFI ratio cutoff.

I conclude the main analysis by checking for an effect of LIC designation on the number of commercial land sales. To the extent that the NMTC had a detectable impact on real estate activity at the 0.80 MFI ratio cutoff, a change in the number of sales at the cutoff suggests a supply response to the outward shift in demand for commercial land in targeted areas. On the other hand, no effect on the volume of sales, despite the increase in land values suggests a relatively inelastic supply of commercial land.

Estimated coefficients on the discontinuity variable, from regressions where the outcome is the average number of yearly commercial land sales are reported in Table 1.5. Panel A uses the full CoStar sample and is analogous to Table 1.3, while Panel B uses a restricted sample, which is a subset of the CoStar sample that excludes census tracts with poverty rates of 20 percent or more and is analogous to Table 1.4. All estimated coefficients are small and statistically indistinguishable from zero, and differential effects across the two samples are nonexistent. These findings demonstrate that there are positive effects of the NMTC on commercial land values, but not on the number of commercial land sales, which is consistent with an inelastic supply of land.

### 1.5.2 Difference-in-Discontinuities Analysis

Next, I exploit the timing of the announcement of the NMTC and report the results in Table 1.6. To identify the effects off a *change* in a census tracts' LIC status, I extend the beginning of the sample period to January 1999 and include a dummy variable that interacts the discontinuity variable ($LIC$) with the period after the announcement of the NMTC (i.e., 2001-2011), denoted as $LIC * POST$. More formally, I estimate the following equation:

$$Y_{ict} = \alpha_0 + \beta_1 LIC_i + \beta_2 LIC_i * POST_t + f(MFIR_i - 0.80) + \mathbf{X}_i \psi + \theta_c + \delta_t + \mu_{ict} \tag{1.2}$$

If the 0.80 MFI ratio cutoff has an impact on commercial land values independent of the NMTC, the coefficient on $LIC$ will capture any of these potential confounding effects, while the estimated effect on $LIC * POST$ will measure the impact of LIC status engendered by the 0.80 MFI ratio cutoff.

In line with the reasoning that the collection of 2000 Decennial Census data, which LIC designation is based on, preceded the enactment of the NMTC and therefore precluded sorting around the 0.80 MFI ratio cutoff, the magnitudes of the estimated coefficients on the $LIC$ variable are small and imprecisely estimated. This bolsters the validity of the identification strategy because it indicates that there are no unobserved time-invariant census tract-level characteristics that may be potentially correlated with treatment and the outcome at the 0.80 MFI ratio cutoff. Conversely, the estimated coefficients on the $LIC * POST$ variable are positive, large, statistically significant at the five percent significance level, and consistently estimated regardless of control function used. These estimates imply an increase on average of 15 percent to census tracts' commercial land values from the NMTC program's LIC designation.

Figure 1.5 depicts the results of a more flexible analysis of the timing of the announcement of the NMTC, by interacting the discontinuity variable ($LIC$) with year indicator variables. Point estimates and corresponding 95 percent confidence intervals are on the discontinuity variable from regressions that control for linear (red) or quadratic (blue) polynomial functions, county and year fixed effects, and the standard set of control covariates. More formally, I estimate the following equation:

$$Y_{ict} = \alpha_0 + \beta_1 LIC_i + \gamma_{1999} LIC_i * YEAR_{1999} + \sum_{t=2001}^{2011} \gamma_t LIC_i * YEAR_t + f(MFIR_i - 0.80) + \mathbf{X}_i \psi + \theta_c + \delta_t + \mu_{ict} \tag{1.3}$$

The estimates denoted by 2001 (i.e., $\hat{\gamma}_{2001}$) capture the immediate impact of LIC designation on commercial land values. Following the terminology in Autor (2003), the other estimates to the left and right of the solid black vertical line, leads and lags respectively, capture the year-by-year evolution of the estimated effect on commercial land values relative to the year before the announcement of the NMTC. Leads are used to test for any pre-existing trends before LIC designation, while lags test for the dynamic effects of LIC designation.

Three features of the graph are worth noting. First, regardless of the control function used, the estimated effects follow a similar (countercyclical) path across time, with more positive effects experienced in trough years and less positive effects in peak years. Second, there is little evidence of an anticipatory response for tracts about to be designated as LICs. Third, there are immediate and persistent positive effects on commercial land values after the announcement of the NMTC, and the null hypothesis that the sum of the lead coefficient is equal to zero is rejected at less than the ten percent significance level under both specifications. This illustration, coupled with the results from the previous table, lucidly demonstrate that the positive impact on commercial land values from the NMTC program's LIC designation of census tracts appears right when the program is announced; a finding that is consistent with the main analysis, wherein identification comes from census tract's eligibility rather than actual investment.[27]

### 1.5.3 Further Analyses

To confirm that the main findings in this research are not merely artifacts of the sampled data, I explore parallel analyses that consider placebo cutoffs to the true cutoff and report the results in Figure 1.6. Like before, point estimates and corresponding 95 percent upper and lower bounds on the discontinuity variable from regressions that control for linear (red) or quadratic (blue) polynomial functions, county and year fixed effects, and the standard set of control covariates. Each tick mark along the horizontal axis refers to a different sample– tracts with observed commercial land sales from January 2001 to December 2011 that are within five percentage points of each percentage point between 0.75 and 0.85, respectively. As already established in Table 1.3, the estimated effects from both linear and quadratic

[27] While I have information about which census tracts received funding from the NMTC, understanding the impact of census tracts' eligibility for a place-based policy's subsidies is arguably of greater importance from the perspective of policymakers. It is precisely the designation of eligibility that officials have greater authority over, as census tracts' receipt of a subsidy is also largely based on various observable and unobservable economic conditions.

models at the true cutoff (i.e., 0.80) are positive and statistically significant between the five to ten percent significance levels. Contrarily, coefficient estimates at the placebo cutoffs hover near zero or are statistically indistinguishable from zero. These differing results at the true cutoff compared to placebo cutoffs provide credibility to the regression discontinuity estimates from the main analysis.

Next, I test for differential effects across more restrictive and less restrictive metropolitan areas. Based on more than two-thirds of tracts in the CoStar sample being in metropolitan areas, I categorize tracts according to their metropolitan area's Wharton Residential Land Use Regulatory Index (WRLURI) without much loss of statistical power. Constructed by Gyourko et al. (2008), the WRLURI is an aggregate measure that is comprised of various sub-indexes, which are used to summarize information on U.S. localities' regulations of the local housing market.[28] The rationale for exploring heterogeneous effects across this dimension is based on the assumption that the regulatory stringency of residential housing is a proxy for the supply elasticity of commercial land within an urban area. Hence, to the extent that LIC designation increases the demand for commercial land, the magnitude of the effect ought to be larger in more supply-inelastic urban areas. Table 1.7 shows that this proposition is moderately borne out in the data, as estimated coefficients on the discontinuity variable from both linear and quadratic models are larger and more significant for the sample of tracts in more restrictive metropolitan areas compared to less restrictive metropolitan areas.

As a further test on heterogeneous effects, I leverage information in the CoStar data set on the secondary type (i.e., commercial, industrial, or residential) of the sold parcel of land, which provides details on the current or intended use of the land parcel. Common uses for commercial land parcels with a *commercial* secondary type are retail or office space, while commercial land parcels with an *industrial* secondary type are typically used for the

[28] I categorize more restrictive (above median) and less restrictive (below median) metropolitan areas by aggregating the WRLURI of each locality to its respective metropolitan area using metropolitan-area-specific weights.

manufacturing of goods, but not their sale. Furthermore, commercial land parcels with a *residential* secondary type are used to contain large buildings or a complex of buildings with residential rental properties (i.e., apartments) or owner-occupied homes (i.e., condominiums). This differs from purely residential real estate which includes properties that are often used as residences (i.e., single-family dwellings). As previously mentioned in Section 1.2.2, the NMTC does not permit the financing of primarily residential rental property. Thus, most commercial real estate investment from the NMTC goes into supporting non-residential projects. For this reason, I perform a supplemental analysis by first categorizing the microdata observations in the CoStar data set into non-residential (i.e., commercial or industrial) and residential secondary types and then collapsing on this indicator variable as well as the census tract-by-year-by level.[29]

I present the estimates on the discontinuity variable from equation (1.1) on these alternatively defined tract-by-year pairings of the CoStar sample in Table 1.8. Similar to the robustness exercise along the lines of metropolitan areas' land stringency index, this analysis also offers suggestive evidence in support of the motivating hypothesis. That is, the estimated coefficients on the discontinuity variable within linear or quadratic models, respectively, show that LIC designation from the NMTC had a more positive and distinguishable impact on non-residential land types, which are more likely to receive funding from this particular place-based subsidy than their residential counterparts.

[29] The distribution of commercial:industrial:residential secondary land type in the CoStar data set is roughly $48 : 12 : 40$.

## 1.6 Discussion

### 1.6.1 Spillovers

It is important to note that the NMTC, similar to other place-based programs, allocates subsidies to qualifying businesses within targeted areas. Thus, estimated effects on census tracts' average commercial land values measure both the internal and external impacts of NMTC-subsidized investments on recipient land parcels. For example, improvements to properties linked to NMTC-subsidized projects will assuredly add to the own property's value but can also increase traffic within the area, thereby adding real value to nearby properties. Due to the NMTC program's selection criteria for funding projects, it is unlikely that any potential intra-tract spillovers on commercial land would be negative, at least in aggregate. Therefore, interpretation of the estimated effects of the NMTC on commercial land values from the preceding analysis should be regarded as a fusion of direct and positive spillover effects.

In addition to *within* tract spillovers, there may also exist *between* tract spillovers, for which, *a priori*, the expected sign is ambiguous. To the extent that there are positive inter-tract spillovers, they are likely to increase with geographic proximity, since the process by which they materialize resembles the mechanisms of intra-tract spillovers. On the other hand, negative inter-tract spillovers are not confined to narrow geographic areas, as they typically emerge when both nearby or faraway resources migrate to targeted areas to take advantage of the program's benefits (Neumark and Simpson, 2015).

To address this issue, I vary the geographic area in which comparisons of targeted and untargeted locations are made and examine whether the magnitude of the estimated effects change. More specifically, I modify the specification from the main analysis by substituting state, metropolitan area, or sub-county fixed effects for county fixed effects. To the extent that there are positive inter-tract spillovers, narrowing the geographic area will attenuate the magnitude of the estimated effects, as the policy favorably impacts both targeted and un-targeted areas. Similarly, narrowing the geographic area and observing a decrease in the magnitude of the estimated effects, suggests that potential displacement comes from farther distances. On the other hand, more positive estimated effects indicate that displacement, if present, comes from closer areas. Naturally, if spatial spillovers are not present or too small to be detected, varying the proximity of the control group should not affect the magnitudes of the main estimates.

Table 1.9 presents the estimated effects on the discontinuity variable from these alternatively defined regression specifications. All regressions include the standard set of control covariates and year fixed effects. Regressions that additionally control for state fixed effects are represented by columns (1) and (2), state and metropolitan area fixed effects by columns (3) and (4), and sub-county fixed effects, for only tracts that do not extend across multiple sub-counties, by columns (7) and (8). For comparison, columns (5) and (6) represent regressions from the main analysis, which control for county fixed effects. All estimated coefficients are positive and their magnitudes are generally increasing as the control group's geographic area narrows (left to right). This suggests that spatial displacement or crowd-out is sourced from nearby areas and that positive spillovers between tracts are minimal. Moreover, greater precision on the estimated effects from regressions with county fixed effects, compared to sub-county fixed effects, but similarity in the magnitudes, suggests that the county fixed effects adequately control for any omitted variables without saturating the empirical model.

### 1.6.2 Quantitative Analysis

As mentioned above, regression discontinuity estimates from the main analysis subsume both the direct and spillover effects of the NMTC within LICs, and also the potential reallocation of resources from un-targeted areas to targeted areas. In light of these arguments, the fairly large and positive estimated effects obtained from this research are within reason.

To more formally establish the plausibility of these estimated effects as well as determine the extent to which the subsidy is passed through to landowners, I conduct a quantitative analysis, which assumes that the capitalization of the NMTC into commercial land values is a function of various parameters, and use sample statistics as reasonable guesses for the parameter values. Under no more than full pass-through of the subsidy to landowners, the following inequality must hold:

$$\beta_1 * \lambda \leq \pi * \delta * \nu \tag{1.4}$$

Here, the left-hand side is the average appreciation of commercial land values and the right-hand side is the expected present value of the NMTC subsidy.

Table A.1 lists the respective values of each parameter and the data sources used to estimate them. NMTC-subsidized investment as a percentage of the project's total cost ($\nu$), is the mean of the ratio of a subsidized project's QLICI amount to its total cost. The cost of land as a proportion of a project's total cost ($\lambda$) is a ratio of two values; the numerator is the mean sale price of commercial land parcels from tracts that are in the CoStar sample, and the denominator is the mean amount of NMTC-subsidized projects' total cost. The conditional probability of an LIC receiving NMTC-subsidized investment ($\pi$), is the proportion of LICs with at least a single commercial land sale from the CoStar data that received NMTC funding during the RD period. Lastly, the discount factor of the 39 percent income tax credit distributed over seven years ($\delta$), derives from a 6.7 percent discount rate.

Inserting the proposed parameter values into equation (1.4) results in an upper bound on the effect on the NMTC on commercial land values of roughly 20 percent. Thus, the most conservative estimates from the preceding analysis, which are on the order of 15 percent, seem fairly plausible. Moreover, these calculations suggest that at least 75 percent of the economic incidence of the NMTC may fall on landowners in the form of higher land values.

## 1.7 Conclusion

In recent decades, federal, state, and local governments throughout the United States have shifted their approach towards addressing poverty and blight within their respective jurisdictions. Rather than directly allocating government resources, policymakers increasingly favor the use of public subsidies to encourage private investment in distressed areas (Hula and Jordan, 2018). These market-based incentives, known as place-based policies, generally resonate with the public because they suppose that improvements to residents' living conditions will come via wage growth or capital investment within targeted areas. Contrarily, economic theory predicts that irrespective of the statutory incidence of a supply-side subsidy, the economic incidence will fall onto land–the most inelastically supplied factor of production–and thus reduce the redistributive impact of the policy.

I test this prediction by evaluating the federal government's New Markets Tax Credit (NMTC) program, a nationally implemented place-based policy that has been in existence since the early 2000s. While the theoretical underpinnings outlined above are common to all place-based policies, in many respects, the NMTC serves as the quintessential case for studying this question. A natural consequence of the NMTC is that its rules tend towards the subsidization of commercial real estate development. *A priori*, this suggests that capitalization of place-based policies into commercial land values are more detectable within its context, relative to in the context of other place-based policies that more directly subsidize jobs.

A rule in the program's structure around a local income cutoff, which largely determines eligibility, allows me to circumvent many of the identification challenges that are intrinsic to estimating the causal impact of place-based policies. By employing a regression discontinuity design, I find causal evidence that eligibility for the NMTC increased commercial land values. I carry out various robustness checks, all of which support the main findings. I also find some evidence that the estimated coefficients combine the direct effect of the program on eligible areas with a displacement effect on nearby non-eligible areas. I conduct a quantitative analysis that explores the credibility of the estimated effects' magnitudes and show that under full pass-through of the subsidy to landowners, the increased effect on land values can be as large as 20 percent, which means that the 15 percent estimates from the most conservative empirical models account for roughly 75 percent of the economic incidence of the program.

Previous work on the NMTC has found that it has only modestly improved the welfare of residents in targeted areas, and is associated with greater residential home values and household turnover rates (Freedman, 2012, 2015). Given that less than two-thirds of the housing share in areas near the 0.80 MFI ratio cutoff, which largely determines eligibility for the NMTC, are occupied by its owners, this suggests that residents in targeted areas may be impelled to move due to higher rents. Ultimately, these findings provide strong evidence in support of the claim that a substantial portion of the program's benefits accrue to landowners, who are not typically the program's target population.

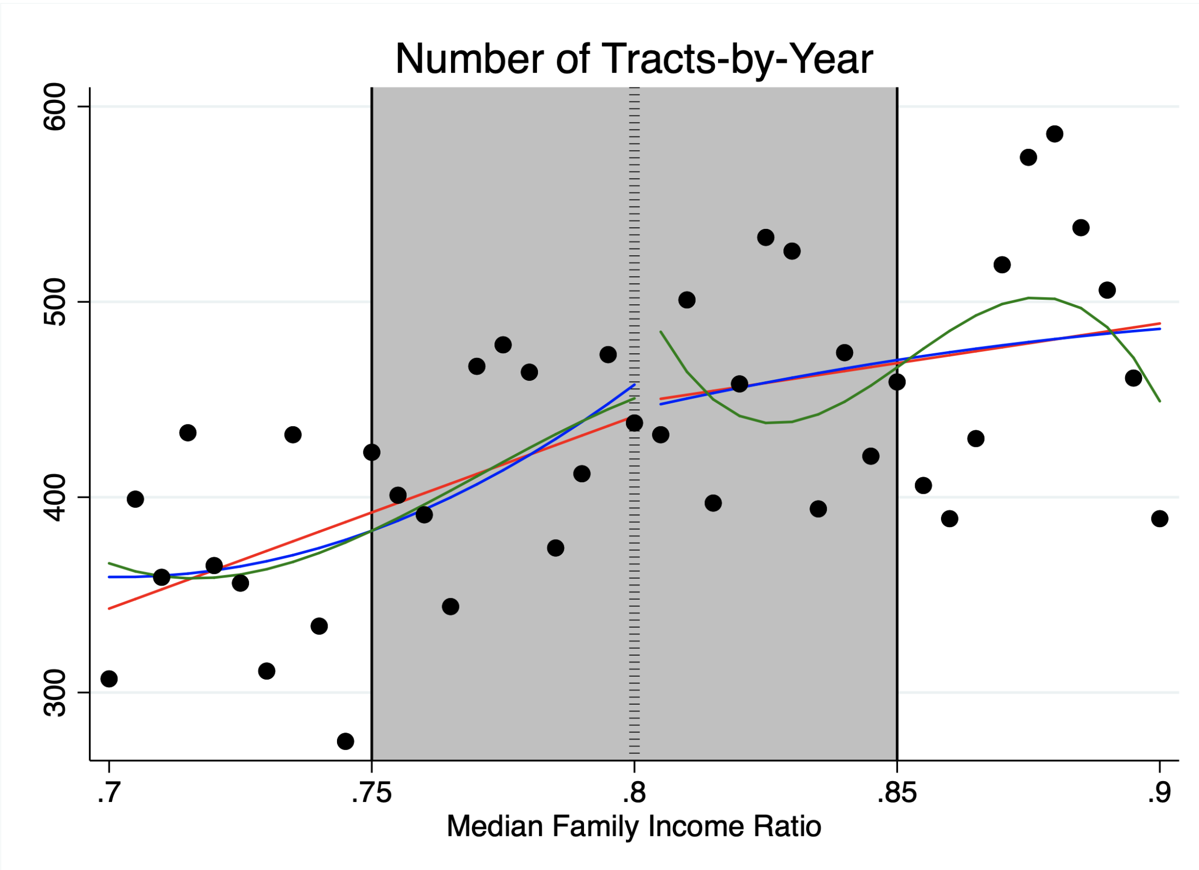
The large effects of the NMTC on commercial land values, which amount to $145,000 on average, do not necessarily disqualify it from being a successful place-based policy. A compelling argument can be made that the positive effect on commercial land values is an offshoot of the policy's incentives, which encourage businesses to locate or expand in targeted areas. Moreover, the prevalence of commercial real estate investment within the program can beget additional renovation projects that are not backed by the NMTC, and thus promote long-term growth within the region by revitalizing neighborhoods and facilitating the development of other industries. The fruits of these investments may take years, even decades, to fully emerge.

Nevertheless, by raising firms' costs and thereby curbing the incentive to establish or expand operations in targeted areas, land appreciation can curtail the policy's re-distributive impact. Hence, policymakers must consider these behavioral responses when adopting policies that intend to help economically disadvantaged people by subsidizing the areas in which they reside. While they may be more palatable to voters than means-tested welfare programs, place-based programs frequently induce greater market distortions due to factor mobility and accordingly exacerbate the efficiency costs that are inherent in government tax and spending programs (Kline and Moretti, 2014).

Admittedly, this research has at least two limitations that are worth noting. First, I am not able to disentangle the absolute effect of the NMTC for eligible tracts from its effect relative to ineligible tracts. That is, the positive impact on targeted areas' commercial land values may be due to real value-added in aggregate or displacement of economic activity from un-targeted to targeted areas. Obtaining administrative data on subsidized and un-subsidized commercial real estate investment for all tracts can improve the interpretability of estimates. Second, because of their sensitivity to regression specification, I am not able to pin down a specific magnitude on the estimated effects. Stabler results are certainly more desirable, but given the consistently positive and large estimated effects across various plausible models, the results from the preceding analysis are adequately reliable. Future work that evaluates the capitalization of place-based policies into commercial land values can benefit from more or better outcome data, perhaps even an alternative empirical strategy–one that does not necessitate the dropping of many data points in the interest of identification of causal effects.

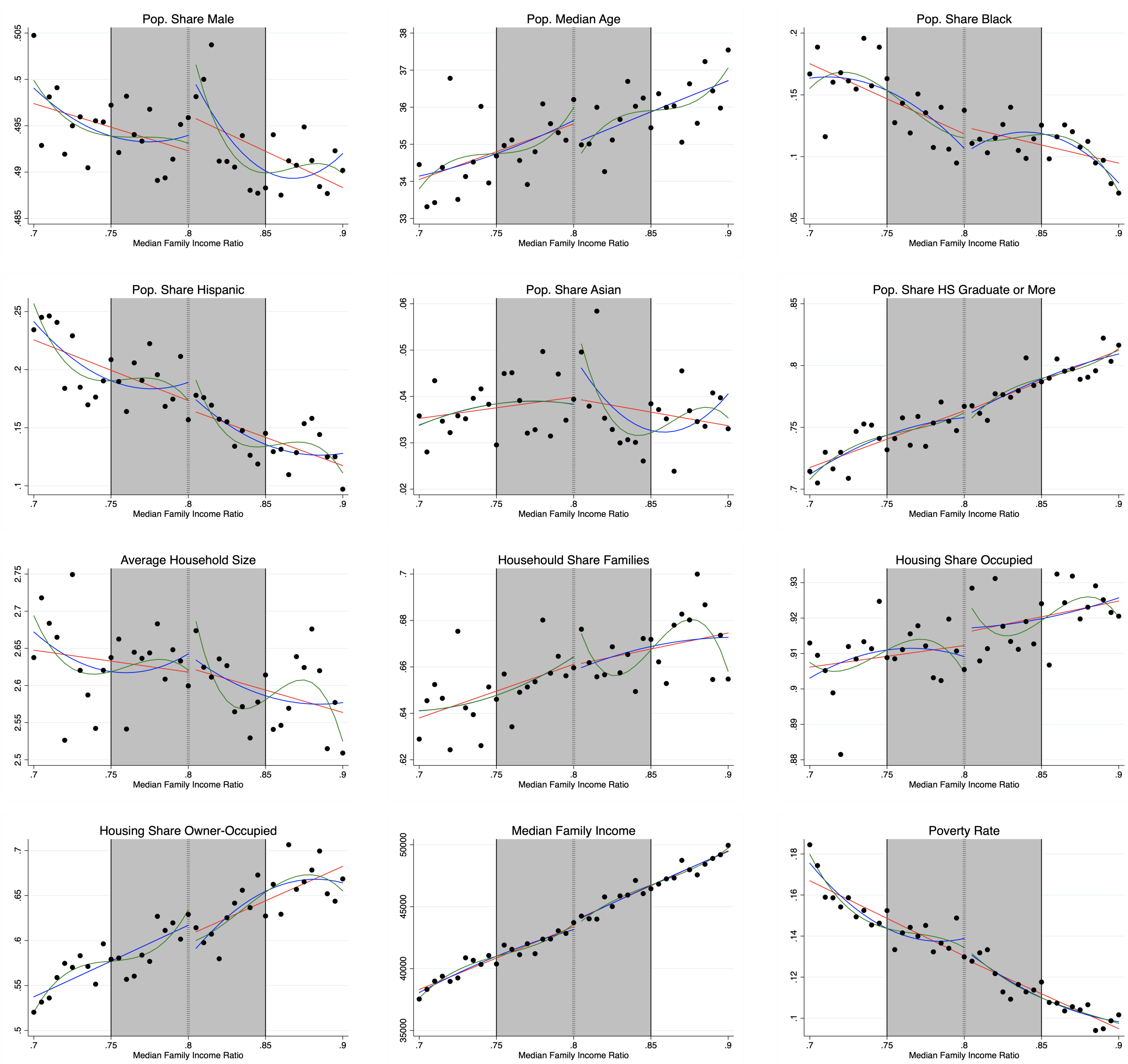
## 1.8 Figures

Figure 1.1: Density



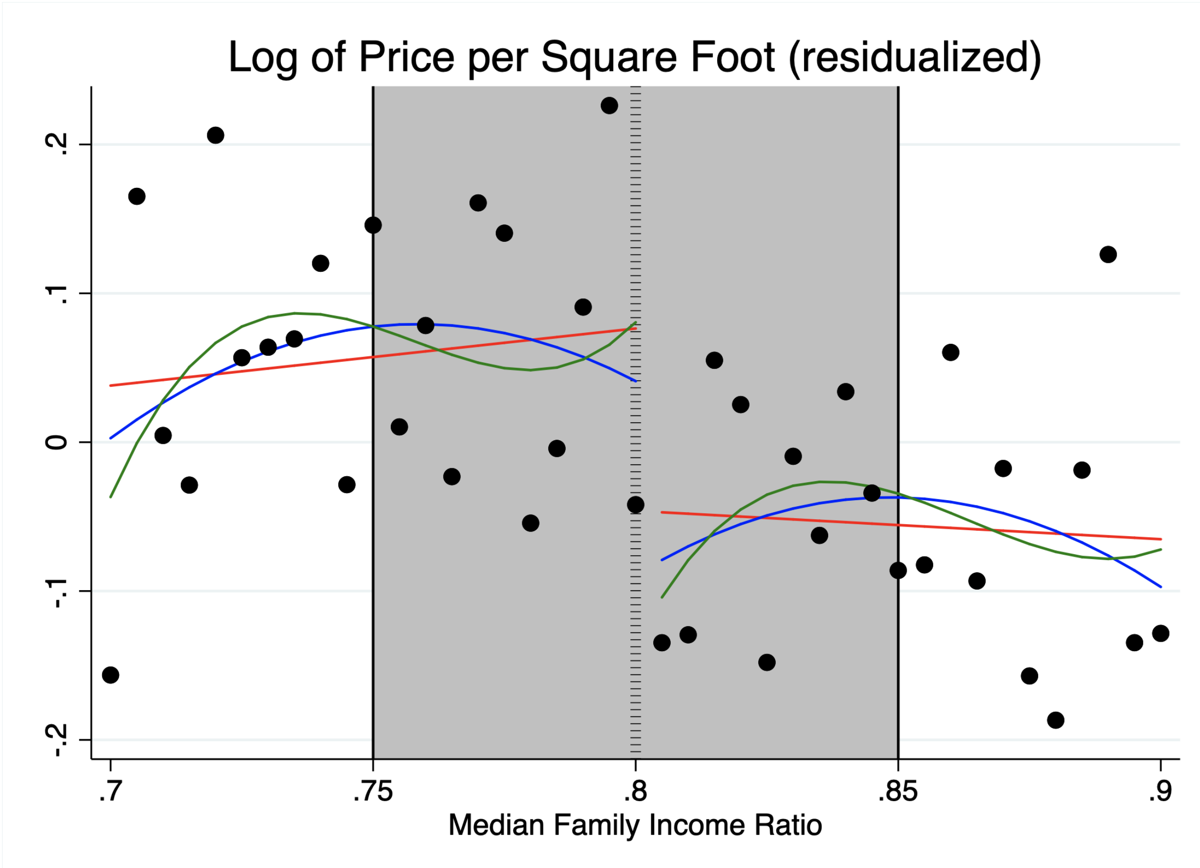

NOTE - Points are tract-by-year counts, in half percentage point bins of median family income (MFI) ratio between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. Sample includes tract-by-year pairings for tracts with observed commercial land sales from January 2001 to December 2011. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 MFI ratio cutoff.

Figure 1.2: Covariate Smoothness



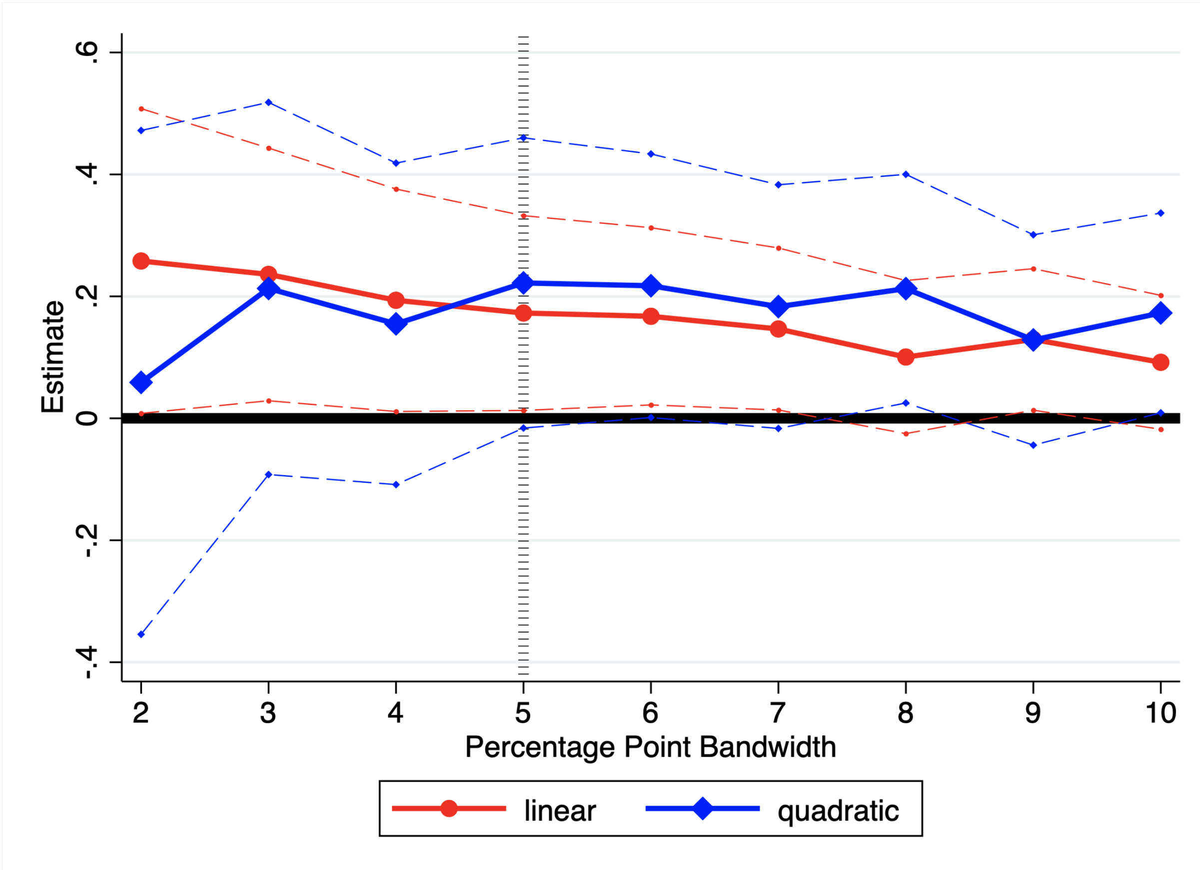

NOTE - Points are tract-by-year mean values for demographic, housing, or income characteristics, in half percentage point bins of median family income (MFI) ratio between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. Sample includes tract-by-year pairings for tracts with observed commercial land sales from January 2001 to December 2011. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 cutoff.

Figure 1.3: Residualized Land Values



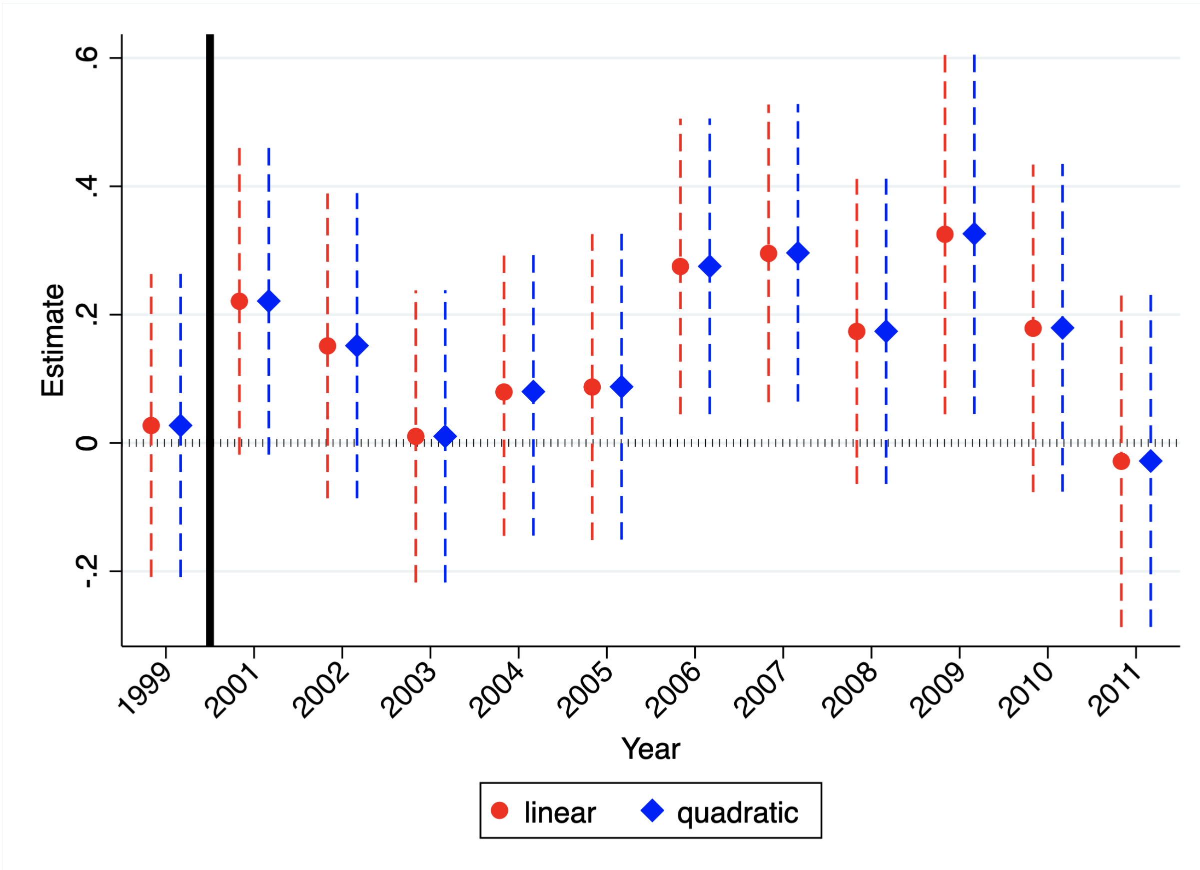

NOTE - Points are tract-by-year mean values for residualized natural log of average sale price per square foot of land, in half percentage point bins of median family income (MFI) ratio between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. Sample includes tract-by-year pairings for tracts with observed commercial land sales from January 2001 to December 2011. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 MFI ratio cutoff. Residuals are obtained from regressions that control for county and year fixed effects only.

Figure 1.4: Bandwidth Choice



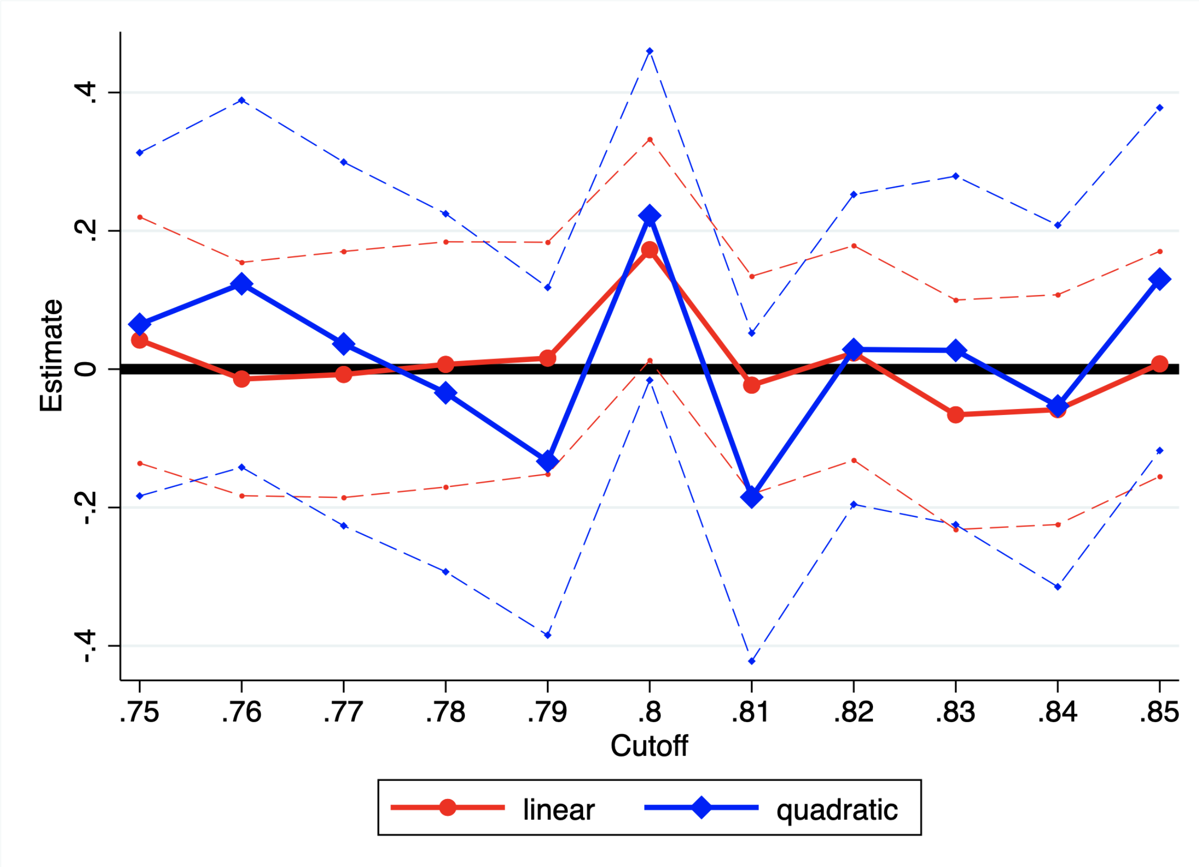

NOTE - Point estimates and corresponding 95 percent upper and lower bounds (dashed lines) on the discontinuity variable from regressions where the outcome is the natural log of the average sale price per square foot of land. Sample includes tract-by-year pairings for tracts with observed commercial land sales from January 2001 to December 2011. The unit of observation is a tract-by-year pairing, within 2, 3, etc. percentage points of the 0.80 median family income (MFI) ratio cutoff. The dashed black vertical line denotes five percentage points of the 0.80 MFI ratio cutoff, which is the bandwidth used in the main analysis. Regressions control for linear (red) or quadratic (blue) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff. Standard errors are clustered at the census tract level. Year and county fixed effects, as well as control covariates, are included in all specifications. Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district.

Figure 1.5: Estimated Discontinuities on Land Values - Dynamics




NOTE - Point estimates and corresponding 95 percent confidence intervals are on the interaction of the discontinuity variable (i.e., $LIC$) and year indicator variables, from regressions where the outcome is the natural log of the average sale price per square foot. The omitted year is the year before the announcement of the NMTC (i.e., 2000), which is denoted by the solid black vertical line. Only tracts with observed commercial land sales from January 1999 to December 2011 are included. The unit of observation is a tract-by-year pairing, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Regressions control for linear (red) or quadratic (blue) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff. Standard errors are clustered at the census tract level. Year and county fixed effects, as well as control covariates, are included in all specifications. Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district.

Figure 1.6: True and Placebo Cutoffs




NOTE - Point estimates and corresponding 95 percent upper and lower bounds (dashed lines) on the discontinuity variable from regressions where the outcome is the natural log of the average sale price per square foot of land. Only tracts with observed commercial land sales from January 2001 to December 2011 are included. The unit of observation is a tract-by-year pairing, within five percentage points of each percentage point between 0.75 and 0.85. Regressions control for linear (red) or quadratic (blue) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff. Standard errors are clustered at the census tract level. Year and county fixed effects, as well as control covariates, are included in all specifications. Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district.

## 1.9 Tables

Table 1.1: Descriptive Statistics

| | CoStar Sample | | | | RD Subset | |
|---|---|---|---|---|---|---|
| | Tract-by-Year | | Tract | | Tract | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pop. Share Male | 0.494 | 0.493 | 0.491 | 0.490 | 0.491 | 0.491 |
| | (0.627) | | (0.504) | | (0.695) | |
| Pop. Median Age | 35.2 | 35.5 | 35.5 | 36.0 | 36.3 | 36.8 |
| | (0.246) | | (0.029)** | | (0.000)*** | |
| Pop. Share Black | 0.126 | 0.116 | 0.126 | 0.112 | 0.116 | 0.099 |
| | (0.176) | | (0.014)** | | (0.000)*** | |
| Pop. Share Hispanic | 0.189 | 0.151 | 0.162 | 0.126 | 0.115 | 0.094 |
| | (0.000)*** | | (0.000)*** | | (0.000)*** | |
| Pop. Share Asian | 0.039 | 0.037 | 0.038 | 0.034 | 0.027 | 0.025 |
| | (0.323) | | (0.088)* | | (0.076)* | |
| Pop. Share HS Graduate or More | 0.752 | 0.777 | 0.755 | 0.778 | 0.760 | 0.778 |
| | (0.000)*** | | (0.000)*** | | (0.000)*** | |
| Average Household Size | 2.63 | 2.60 | 2.60 | 2.58 | 2.56 | 2.55 |
| | (0.241) | | (0.161) | | (0.312) | |
| Household Share Families | 0.656 | 0.663 | 0.657 | 0.666 | 0.667 | 0.677 |
| | (0.163) | | (0.017)** | | (0.000)*** | |
| Housing Share Occupied | 0.910 | 0.918 | 0.909 | 0.913 | 0.892 | 0.897 |
| | (0.025)** | | (0.115) | | (0.022)** | |
| Housing Share Owner-Occupied | 0.594 | 0.625 | 0.608 | 0.646 | 0.653 | 0.682 |
| | (0.000)*** | | (0.000)*** | | (0.000)*** | |
| Median Family Income ($) | 42,188 | 45,454 | 41,995 | 44,806 | 40,699 | 43,011 |
| | (0.000)*** | | (0.000)*** | | (0.000)*** | |
| Poverty Rate | 0.139 | 0.119 | 0.139 | 0.120 | 0.141 | 0.124 |
| | (0.000)*** | | (0.000)*** | | (0.000)*** | |
| Median Family Income Ratio | [0.75,0.80] | (0.80,0.85] | [0.75,0.80] | (0.80,0.85] | [0.75,0.80] | (0.80,0.85] |
| Sample Size | 4,309 | 4,595 | 1,826 | 1,891 | 4,535 | 4,691 |
| Tracts | 1,826 | 1,891 | | | | |

NOTE - Means of demographic, housing, and income characteristics. The unit of observation is a tract-by-year pairing for columns (1) and (2), or a tract for columns (3)-(6), within five percentage points of the 0.80 median family income ratio cutoff. Columns (1)-(4) include only tracts with observed commercial land sales from January 2001 to December 2011, and columns (5) and (6) include all U.S. census tracts with an MFI ratio within five percentage points of the 0.80 MFI ratio cutoff. Parentheses are p-values from two-sided tests of differences in means. Standard errors are clustered at the census tract level for tract-by-year means. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.2: Estimated Discontinuities on Covariates

| | CoStar Sample | | | | RD Subset | |
|---|---|---|---|---|---|---|
| | Tract-by-Year | | Tract | | Tract | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pop. Share Male | -0.010* | -0.006 | -0.004* | -0.002 | -0.001 | -0.001 |
| | (0.005) | (0.008) | (0.002) | (0.003) | (0.001) | (0.002) |
| Pop. Median Age | 0.062 | 0.447 | -0.025 | -0.185 | 0.172 | -0.124 |
| | (0.617) | (0.914) | (0.433) | (0.646) | (0.223) | (0.331) |
| Pop. Share Black | 0.018 | 0.015 | 0.013 | 0.010 | 0.006 | 0.008 |
| | (0.014) | (0.021) | (0.012) | (0.018) | (0.007) | (0.010) |
| Pop. Share Hispanic | 0.001 | -0.003 | -0.008 | -0.003 | -0.005 | -0.005 |
| | (0.013) | (0.019) | (0.009) | (0.014) | (0.005) | (0.007) |
| Pop. Share Asian | -0.001 | 0.003 | -0.001 | 0.001 | -0.002 | -0.004 |
| | (0.006) | (0.008) | (0.005) | (0.007) | (0.002) | (0.003) |
| Pop. Share HS | 0.011 | 0.009 | 0.008 | 0.006 | 0.003 | 0.005 |
| Graduate or More | (0.008) | (0.011) | (0.006) | (0.009) | (0.003) | (0.005) |
| Average Household | -0.012 | -0.053 | -0.017 | -0.036 | -0.008 | -0.015 |
| Size | (0.048) | (0.069) | (0.035) | (0.053) | (0.018) | (0.026) |
| Household Share | -0.002 | -0.025 | -0.001 | -0.019 | 0.002 | -0.007 |
| Families | (0.012) | (0.017) | (0.009) | (0.014) | (0.005) | (0.007) |
| Housing Share | -0.001 | -0.011 | 0.001 | -0.007 | 0.001 | -0.001 |
| Occupied | (0.006) | (0.008) | (0.005) | (0.007) | (0.003) | (0.005) |
| Housing Share | 0.007 | -0.006 | 0.003 | -0.017 | 0.010 | -0.006 |
| Owner-Occupied | (0.017) | (0.026) | (0.013) | (0.019) | (0.007) | (0.011) |
| Median Family | 16 | 3 | 16 | -14 | 8 | -10 |
| Income ($) | (14) | (18) | (18) | (26) | (10) | (15) |
| Poverty Rate | 0.006 | 0.007 | 0.004 | 0.009 | 0.002 | 0.009*** |
| | (0.005) | (0.007) | (0.004) | (0.005) | (0.002) | (0.003) |
| Polynomial Degree | 1 | 2 | 1 | 2 | 1 | 2 |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is a demographic, housing, or income characteristic. The unit of observation is a tract-by-year pairing for columns (1) and (2), or a tract for columns (3)-(6), within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Columns (1)-(4) include only tracts with observed commercial land sales from January 2001 to December 2011, and columns (5) and (6) include all U.S. census tracts with an MFI ratio within five percentage points of the 0.80 MFI ratio cutoff. Regressions control for linear (odd-numbered columns) or quadratic (even-numbered columns) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff, as well as county fixed effects. Parentheses are standard errors, which are clustered at the census tract level for tract-by-year regressions. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.3: Estimated Discontinuities on Land Values

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *LIC* | 0.177 | 0.173 | 0.306 | 0.222 |
| | (0.104)* | (0.081)** | (0.157)* | (0.121)* |
| | [0.104]* | [0.084]* | [0.158]* | [0.129]* |
| | {0.096}* | {0.084}* | {0.144}** | {0.123}* |
| Sample Size | | 8,904 | | |
| Tracts | | 3,717 | | |
| Counties | | 1,135 | | |
| Polynomial Degree | 1 | 1 | 2 | 2 |
| Year Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| County Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | | ✓ | | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is the natural log of the average sale price per square foot of land. The unit of observation is a tract-by-year pairing, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Only tracts with observed commercial land sales from January 2001 to December 2011 are included. Regressions control for linear (columns (1) and (2)) or quadratic (columns (3) and (4)) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff, as well as county and year fixed effects. Control covariates are also included in specifications represented by columns (2) and (4). Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district. Clustered standard errors at the census tract, county, and metropolitan (MSA) levels are reported in parentheses, square brackets, and curly brackets, respectively. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.4: Estimated Discontinuities on Land Values - Restricted Sample

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *LIC* | 0.299*** | 0.274*** | 0.423*** | 0.356*** |
| | (0.098) | (0.081) | (0.152) | (0.121) |
| Sample Size | | 8,023 | | |
| Tracts | | 3,355 | | |
| Counties | | 1,040 | | |
| Polynomial Degree | 1 | 1 | 2 | 2 |
| Year Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| County Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | | ✓ | | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is the natural log of the average sale price per square foot of land. The unit of observation is a tract-by-year pairing, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Only tracts with observed commercial land sales from January 2001 to December 2011 are included. Tracts with poverty rates of 20 percent or more are excluded. Regressions control for linear (columns (1) and (2)) or quadratic (columns (3) and (4)) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff, as well as county and year fixed effects. Control covariates are also included in specifications represented by columns (2) and (4). Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district. Parentheses are standard errors, which are clustered at the census tract level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.5: Estimated Discontinuities on Land Sales

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Panel A: Full Sample | | | | |
| *LIC* | -0.345 | -0.218 | -0.221 | -0.098 |
| | (0.258) | (0.246) | (0.259) | (0.267) |
| Sample Size | | 8,904 | | |
| Tracts | | 3,717 | | |
| Counties | | 1,135 | | |
| Panel B: Restricted Sample | | | | |
| *LIC* | -0.332 | -0.232 | -0.120 | -0.016 |
| | (0.286) | (0.263) | (0.278) | (0.304) |
| Sample Size | | 8,023 | | |
| Tracts | | 3,355 | | |
| Counties | | 1,040 | | |
| Polynomial Degree | 1 | 1 | 2 | 2 |
| Year Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| County Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | | ✓ | | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is the average number of land sales. The unit of observation is a tract-by-year pairing, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Only tracts with observed commercial land sales from January 2001 to December 2011 are included. Panel A uses the full CoStar sample, and Panel B uses a restricted sample, which is a subset of the CoStar sample that excludes census tracts with poverty rates of 20 percent or more. Regressions control for linear (odd-numbered columns) or quadratic (even-numbered columns) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff, as well as county and year fixed effects. Control covariates are also included in specifications represented by columns (2) and (4). Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district. Parentheses are standard errors, which are clustered at the census tract level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.6: Estimated Discontinuities on Land Values - Exploiting Timing

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *LIC* | 0.020 | 0.030 | 0.100 | 0.040 |
| | (0.124) | (0.101) | (0.169) | (0.134) |
| $LIC * POST$ | 0.159** | 0.147** | 0.160** | 0.147** |
| | (0.077) | (0.071) | (0.077) | (0.070) |
| Sample Size | 10,290 | | | |
| Tracts | 3,873 | | | |
| Counties | 1,140 | | | |
| Polynomial Degree | 1 | 1 | 2 | 2 |
| Year Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| County Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | | ✓ | | ✓ |

NOTE - Estimated coefficients on the discontinuity variable, denoted as $LIC$, and its interaction with an indicator variable for the period after the announcement of the NMTC (i.e., 2001-2011), denoted as $LIC * POST$, from regressions where the outcome is the natural log of the average sale price per square foot of land. Only tracts with observed commercial land sales from January 1999 to December 2011 are included. The unit of observation is a tract-by-year pairing, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Regressions control for linear (columns (1) and (2)) or quadratic (columns (3) and (4)) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff, as well as county and year fixed effects. Control covariates are also included in specifications represented by columns (2) and (4). Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district. Parentheses are standard errors, which are clustered at the census tract level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.7: Estimated Discontinuities on Land Values - Land Stringency

| | More Restrictive | | Less Restrictive | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *LIC* | 0.182** | 0.195 | 0.118 | 0.147 |
| | (0.090) | (0.134) | (0.193) | (0.299) |
| Sample Size | 5,914 | | 1,832 | |
| Tracts | 2,019 | | 886 | |
| Counties | 288 | | 281 | |
| Polynomial Degree | 1 | 2 | 1 | 2 |
| Year Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| County Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | ✓ | ✓ | ✓ | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is the natural log of the average sale price per square foot of land. The unit of observation is a tract-by-year pairing, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Only tracts in a metropolitan area with observed commercial land sales from January 2001 to December 2011 are included. Sample is stratified by more restrictive (above median) and less restrictive (below median) metropolitan areas, according to the Wharton Residential Land Use Regulatory Index (WRLURI). Regressions control for linear (odd-numbered columns) or quadratic (even-numbered columns) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff. County and year fixed effects, as well as the standard set of control covariates, are included in all specifications. Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district. Parentheses are standard errors, which are clustered at the census tract level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.8: Estimated Discontinuities on Land Values - Land Types

| | Non-Residential | | Residential | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *LIC* | 0.165** | 0.171 | 0.051 | 0.123 |
| | (0.081) | (0.120) | (0.145) | (0.209) |
| Sample Size | 7,105 | | 2,932 | |
| Tracts | 3,298 | | 1,426 | |
| Counties | 1,059 | | 446 | |
| Polynomial Degree | 1 | 2 | 1 | 2 |
| Year Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| County Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | ✓ | ✓ | ✓ | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is the natural log of the average sale price per square foot of land. The unit of observation is a tract-by-year pairing, stratified by non-residential (i.e., commercial or industrial) and residential land types, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Only tracts with observed commercial land sales from January 2001 to December 2011 are included. Regressions control for linear (odd-numbered columns) or quadratic (even-numbered columns) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff. County and year fixed effects, as well as the standard set of control covariates, are included in all specifications. Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district. Parentheses are standard errors, which are clustered at the census tract level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 1.9: Estimated Discontinuities on Land Values - Alternative Specifications

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| *LIC* | 0.137* | 0.175 | 0.138* | 0.117 | 0.173** | 0.222* | 0.158* | 0.162 |
| | (0.082) | (0.122) | (0.077) | (0.115) | (0.081) | (0.121) | (0.088) | (0.128) |
| Sample Size | | | | 8,904 | | | | 8,562 |
| Polynomial Degree | 1 | 2 | 1 | 2 | 1 | 2 | 1 | 2 |
| Year Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| State Fixed Effects | ✓ | ✓ | ✓ | ✓ | | | | |
| MSA Fixed Effects | | | ✓ | ✓ | | | | |
| County Fixed Effects | | | | | ✓ | ✓ | | |
| Sub-County Fixed Effects | | | | | | | ✓ | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is the natural log of the average sale price per square foot of land. The unit of observation is a tract-by-year pairing, within five percentage points of the 0.80 median family income (MFI) ratio cutoff. Only tracts with observed commercial land sales from January 2001 to December 2011 are included. Regressions control for linear (odd-numbered columns) or quadratic (even-numbered columns) polynomial functions in the MFI ratio, with coefficients on the running variable allowed to vary on either side of the cutoff. Regressions that control for: state fixed effects are represented by columns (1) and (2), state and metropolitan area fixed effects by columns (3) and (4), county fixed effects by columns (5) and (6), and sub-county fixed effects, for only tracts that do not extend across multiple sub-counties, by columns (7) and (8). Year fixed effects, as well as the standard set of control covariates, are included in all specifications. Control covariates are share of population that is male, population's median age, share of population that is Black, share of population that is Hispanic, share of population that is Asian, share of population that is a high school graduate or more, average household size, share of households that are families, share of housing that is occupied, share of housing that is owner-occupied, and distance to central business district. Parentheses are standard errors, which are clustered at the census tract level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

# Chapter 2

# Effects of Recreational Marijuana Dispensaries on Nearby Retail Rents

## 2.1 Introduction

Despite the federal ban, ten states and the District of Columbia have legalized the possession and use of marijuana for recreational purposes, and an additional 23 states have passed laws legalizing marijuana possession for medicinal use. Consequently, a growing body of research has recently emerged that exploits the variation in these laws to study their effects on various outcomes, such as traffic fatalities (Anderson et al., 2013), suicides (Anderson et al., 2014), crime rates (Chang and Jacobson, 2017, Gavrilova et al., 2019), drug treatment admissions (Chu, 2014, Pacula et al., 2015), youth marijuana use (Pacula et al., 2015), migration (Baggio et al., 2017), hourly earnings (Sabia and Nguyen, 2018), and residential property values (Conklin et al., 2020, Burkhardt and Flyr, 2019, Cheng et al., 2018). This research builds on the burgeoning literature by being the first to estimate the spillover effects of recreational marijuana dispensaries onto nearby retail properties.

The opening of a recreational marijuana dispensary can impact nearby retail properties by generating both positive and negative externalities. For example, to the extent that dispensaries open in areas where previously black-market demand was high, legalization can reduce the burden of criminal drug enforcement, which means that police can reallocate their resources towards preventing other forms of criminal activity (Adda et al., 2014). Another positive spillover is that dispensaries typically implement security measures, such as surveillance cameras, security guards, and patrols, to a greater extent than rival businesses that may be vying for tenancy (Freisthler et al., 2013). This, in concert with the increased guardianship from dispensary patrons, can help deter criminal activity, which is consistent with the eyes upon the street hypothesis (Jacobs, 1961). Also, local zoning laws often restrict dispensaries and other marijuana businesses to commercial areas with lower quality buildings, which means that the increased demand can help revitalize these neighborhoods. On the other hand, dispensaries may face an increased risk of burglaries, because they are a repository for a banned but attractive substance and because, due to the federal ban, which precludes access to traditional banking services, they often operate as "cash only" businesses (Association et al., 2009). Some citizens also feel that dispensaries are a disamenity because they have nuisance-attracting characteristics, similar to other businesses that cater to vices like bars or strip clubs (Németh and Ross, 2014). Finally, there is a general stigma attached to marijuana from the federal ban, and specifically, marijuana dispensaries, which is driven by their association with low socioeconomic status neighborhoods (Shi et al., 2016).

Using proprietary commercial real estate data from CoStar, this research employs retail rents as a suitable outcome that encapsulates both the positive and negative spillovers emanating from a recreational marijuana dispensary onto nearby businesses. It focuses on a single state–Washington–which serves as an ideal setting for studying this question for two reasons. One, Washington, along with Colorado, was the first state to pass a recreational marijuana law (RML), which means enough time has passed since legalization to be able to accurately assess the local impacts of recreational marijuana dispensaries, at least in the short run.

Second, unlike Colorado, where recreational marijuana dispensaries could only form from conversions out of existent medical marijuana dispensaries, Washington initially banned medical marijuana dispensaries, which had been clandestinely operating for years, after legalization, and established a brand new regulatory framework that distributed recreational marijuana dispensary licenses via a lottery. The results of the lottery are crucial to the methodological approach employed in this research, which is hedonic difference-in-differences because information on the proposed address of lottery losers enables the construction of credible counterfactual sites where recreational marijuana dispensaries would have opened. Since underlying market conditions, like rents, are endogenous to firms' location decisions, leveraging this plausibly exogenous source of variation can help identify the causal effect of recreational marijuana dispensaries on nearby retail rents.

The main finding of this research is that the opening of a recreational marijuana dispensary has little to no impact on nearby retail rents. The estimated effects across various buffer sizes, for which actual and counterfactual dispensaries serve as a centroid, are small and statistically indistinguishable from zero. Moreover, a supplementary event study analysis reveals no divergence in the trend of average retail rents for retail properties that are close to a recreational marijuana dispensary after the dispensary's opening. Finally, a test for heterogeneous treatment effects shows the possibility of positive spillovers in low-income urban areas. Altogether, these results suggest that the externalities associated with recreational marijuana dispensaries for nearby retail establishments are small or offsetting.

While previous work by Conklin et al. (2020) and Burkhardt and Flyr (2019) explored the spillover effects of recreational marijuana dispensaries onto nearby residential properties, and found increases to housing values that are on the order of eight percent, this research estimates the impact of recreational marijuana dispensaries on nearby retail properties, which is a related but distinct question. In contrast to residential properties, commercial retail properties are more similar in terms of property characteristics and closer to recreational marijuana dispensaries in proximity, which means that the localized spillovers originating from a recreational marijuana dispensary are more discernible to nearby retail establishments than residents. Moreover, the same spillover, such as increased security due to the installation of surveillance cameras or greater foot traffic, may be viewed as positive externalities by establishments but as disamenities by residents. Lastly, due to the low importance that the general public ascribes to RMLs and the market-clearing conditions that are fundamental to hedonic modeling, commercial retail rents, which are more dynamic than residential property values, are more suitable outcomes for the short-run analysis presented in this strand of research.[1]

The rest of the chapter is organized as follows. Section 2.2 details the relevant features of Washington's RML. Section 2.3 describes the various datasets used in the analysis and illustrates how leased properties are assigned to treatment and control groups. Section 2.4 outlines the primary empirical strategy, which is hedonic difference-in-differences, as well as a supplementary event study analysis, which allows for studying the effects of a dispensary opening by months relative to the month in which it opens. Section 2.5 presents the results and Section 2.6 concludes.

## 2.2 Background on Washington's Recreational Marijuana Law

Residents in Washington voted in favor of Initiative 502 (I-502) in November 2012, which legalized the possession, consumption, and sale of recreational marijuana for all adults 21 years or older. Although Washingtonian adults (18 years or older) had been protected

[1] A 2014 poll by the Associated Press asked survey respondents what they felt the importance of marijuana laws were, and only about a quarter believed them to be "extremely" or "very important" whereas half thought they were "slightly important" or "not at all important." (Source: The AP-GfK September 2014 Poll)

from prosecution for possessing or consuming marijuana for medicinal purposes since 1998, the state's medical marijuana laws did not explicitly sanction the establishment of medical marijuana dispensaries. Nevertheless, many came into existence over the next decade and a half but remained unlicensed and unregulated by the state government (WaS, 2011). The passing of I-502 harmonized the medical and recreational marijuana sectors via the creation of the Washington State Liquor and Cannabis Board (WSLCB), which was "authorized to regulate and tax marijuana...under a tightly regulated, state-licensed system similar to that for controlling hard alcohol." (WaS, 2012a)

The WSLCB instituted three separate tiers of licenses: marijuana producers, which grow and harvest the plant; marijuana processors, which transform the plant into its smokable form and other alternative forms (e.g., edible, vape); and marijuana retailers, which dispense the final product to consumers. In essence, the newly established state-licensed system officially criminalized the unlicensed production, processing, and sale of marijuana that had previously taken place under the guise of medical marijuana. Licensing restrictions or quotas were not placed on producers and processors, but retailers could not vertically integrate and were not allowed to exceed a statewide cap of 334 licenses. The WSLCB implemented this retail license quota rule by partitioning the state into 125 jurisdictions–incorporated cities and rural county areas–and distributing the 334 licenses across them using a formula that was primarily a function of population and estimated marijuana demand. For example, Seattle was allotted 21 licenses, the most in King County, while Bellevue, the second most populated city in the county was allotted four licenses. Other cities in King County were allotted one, two or three licenses, and unincorporated parts of the county were allotted a combined 11 licenses. Altogether, King County was allotted 61 licenses.

A 30-day application window for applying for a retailer's license opened from November 2013 to December 2013. The application fee was $250, and the license fee was $1,000 per year. Per the laws of the state, the WSLCB would not issue a retailer's license for any premises within

1,000 feet of an elementary or secondary school, playground, recreation center or facility, child care center, public park, public transit center, or library (WaS, 2012b).[2] Hence, applications had to include a potential store address with proof of the right to occupy the property (e.g., deed, lease, note from landlord) so that state regulators could pre-screen applicants based on this criterion. Applicants that successfully passed the pre-screening process, which also included other criteria such as criminal history statements, proof of residency, etc., were considered eligible for a license. No individual/entity (e.g., sole proprietor, LLC) was allowed to possess more than three licensees per jurisdiction or more than one-third of a respective jurisdiction's licenses. Application addresses were not binding, but there were limits on the circumstances for why an applicant may move locations, such as a landlord withdrawing from a commitment or local zoning restrictions disqualifying the proposed location as a site for a dispensary. A potential store address could appear on more than one application for one of two reasons: (1) more than one individual/entity could submit an application using the same address, (2) an individual/entity could apply more than once, but no more than thrice, within a jurisdiction, with as many as three of those applications using the same address.

In 75 of the 125 jurisdictions where there were more eligible applicants than licenses, which accounted for 257 of the 334 retailer licenses statewide, a lottery was used to determine which applicants could continue onto the next stage of the licensing process. The WSLCB contracted the Social and Economic Sciences Research Center of Washington State University and the accounting firm Kraght-Snell to independently produce rank-ordered lists of applicants in each jurisdiction where a lottery was necessary, which took place from April 21-25, 2014. Lottery winners were applicants that received a number lower than or equal to the number of allotted licenses within the respective jurisdiction. Winning a lottery did not

[2]Local governments were permitted to reduce the 1,000 feet buffer to 100 feet around all premises except elementary schools, secondary schools, and playgrounds, "provided that such distance reduction will not negatively impact the jurisdiction's civil regulatory enforcement, criminal law enforcement interests, public safety, or public health." (Source: Revised Code of Washington)

guarantee that an applicant was awarded a retailer's license, as the WSLCB stipulated that winning applicants submit additional documentation, such as criminal background checks, financial statements, etc., as part of the requirements for licensure. The WSLCB posted the results of marijuana retail store lotteries on its website on May 2, 2014, and it issued the first marijuana retailer license on July 7, 2014.

In December 2015, following an analysis of the recreational, medical and illicit sectors of the marijuana industry, the WSLCB announced an increase in the number of marijuana retailer licensees from 334 to 556. As before, the number of licenses allotted to each jurisdiction was determined by population and estimated demand. However rather than a lottery, this time, a priority-based system was used to distribute the additional 222 licensees within jurisdictions.[3] More specifically, medical marijuana dispensary operators who applied for a marijuana retailer license in 2013 and were in good standing in regard to maintaining the requisite business licenses and reporting revenues to tax authorities, were given priority. In addition to adding new stores, the WSLCB also modified many existing stores' retailer licenses with an endorsement to sell medical marijuana. These actions by the WSLCB ensured that medical patients were offered adequate access to marijuana and further absorbed the unregulated medical marijuana sector into the tightly regulated state-licensed system.

## 2.3 Data

The data on retail rents come from CoStar–one of the leading providers of commercial real estate information, analytics, and online marketing for real estate industry professionals. Through "CoStar University," academics are granted a free subscription to CoStar's dataset, which contains millions of researched and verified lease records. The dataset features variables on the characteristics of the leased property, such as property address, total square

[3] Accordingly, the sample period used in this analysis does not continue past 2015.

feet leased, the floor the property is on, and a CoStar defined star rating of the property; as well as variables on the terms of the lease transaction, such as the annual rent amount per square foot, the month and year in which the lease was signed, and the number of months that the property was on the market before the lease's execution.[4] The unit of observation in the analysis is an executed, new, and direct retail commercial property lease transaction. As many as three different rent types, (1) effective, (2) starting, and (3) asking, may be reported per lease transaction. If more than one rent type per lease transaction is reported, then the rent type is assigned according to the order listed above.[5] The sample period is from January 2010-December 2015, inclusive.[6]

The dispensary and lottery data come from the public records section of the Washington State Liquor and Cannabis Board (WSLCB). I define a dispensary's establishing of operations as the month in which it begins paying taxes to the state. Dispensaries that could not be linked by their monthly tax obligation to the state, and therefore could not be assigned start dates in such a manner, are excluded from the analysis. Lottery losers that are not associated with an address that later houses a dispensary, henceforth known as *losers*, are obtained from the retail license lottery results list. The following algorithm was employed to derive the list of losers. First, applications with addresses that appear on more than one application were dropped. Next, applications that received a number lower than or equal to the license quota of the respective jurisdiction (i.e., lottery winners) were dropped. Lastly, applications that were associated with an address that later housed a dispensary were dropped. The addresses on the remaining applications were coded as losers. Dispensaries

[4]CoStar's star rating is an evaluation of commercial properties on a five-point scale based on the characteristics of each property type, such as architectural attributes, amenities, detailed property type specifics, etc.

[5]Effective rent is the most informative rent type as it is the average rent paid over the term by a tenant adjusted for concessions paid for by the landlord and costs that are the responsibility of the tenant. For lease transactions where both the effective and asking rent are known, the correlation coefficient between them is 0.908.

[6]While commercial rents data before 2010 and after 2015 were available, it was necessary to select January 2010 as the beginning month because of download restrictions, and December 2015 as the end month because the WSLCB expanded the number of marijuana retailer licenses after 2015 via a priority-based system, as described in Section 2.2.

in jurisdictions that did not require a lottery were also excluded from the analysis, because losers, which serve as counterfactual locations to dispensaries, could only be obtained in jurisdictions where a lottery occurred. Figure 2.1 shows the geographic distribution of the 167 dispensaries (blue diamonds) and the 348 losers (red circles) throughout the state that are considered in the analysis.

Addresses for leased properties, dispensaries, and losers were geocoded using Google's API, and geodesic distances were calculated from the latitude-longitude coordinates of leased properties to the latitude-longitude coordinates of dispensaries and losers. Leased properties that are within an $r$-mile radius of a dispensary were assigned to the *treatment* group, while leased properties that are within an $r$-mile radius of a loser but not within an $r$-mile radius of a dispensary were assigned to the *control* group.[7] The parameter $r$ is allowed to take on the values of $1/8$, $1/4$, $3/8$, or $1/2$, which denote the size of the buffer for which dispensaries and losers serve as a centroid.[8] Depending on the prevailing value of $r$, a specific dispensary or loser is assigned to each leased property in the following manner. If only one dispensary or loser is within an $r$-mile radius of a leased property, then that particular dispensary or loser is assigned. If more than one dispensary is within an $r$-mile radius of a leased property, then the nearest dispensary is assigned, irrespective of the number of losers that are also within the radius; except if there is a dispensary that is in operation at the time that the lease was signed, then the nearest operational dispensary is assigned. If no dispensary, but more than one loser is within an $r$-mile radius of a leased property, then the nearest loser is assigned.

To help conceptualize the relationship between radius choice and a particular leased property's assignment into treatment and control groups, Figure 2.2 shows a map of Downtown Seattle under the four values of $r$. As in Figure 2.1, solid blue diamonds denote dispensaries

[7] In cases where the calculated distance from a leased property to a dispensary/loser was zero, or arbitrarily close to zero (i.e., $1/100^{th}$ of a mile), the leased properties in the CoStar data were assumed to be the dispensary or loser, and were excluded from the analysis as a means of measuring the true spillover effect of recreational marijuana dispensaries on nearby retail rents.

[8] Intervenient values of $r$ were also considered but the results are not substantively different from those presented in this analysis. Results are available upon request.

and solid red circles denote losers, while striped blue and red circles indicate the $r$-mile buffers around dispensaries and losers, respectively. Green squares indicate lease transactions that occur near a dispensary, while dark green squares further indicate lease transactions that occur at a time when the said dispensary is in operation. Orange squares indicate lease transactions that occur near a loser. Varying $r$ changes the number of leased properties within treatment and control groups, respectively, and also changes a particular leased property's assignment into those groups. For example, under $r = 3/8$, a leased property may only be within the buffer of a loser, and hence is assigned into the control group, while under $r = 1/2$, that same leased property may also be within the buffer of a dispensary, which means it is assigned into the treatment group. As a consequence of this assignment rule, the relative difference between leased properties near dispensaries (i.e., treatment group) and leased properties near losers (i.e., control group) is increasing in $r$.[9]

Additional property-level and census tract-level characteristics, obtained from three supplementary data sources, are also used to enrich the analysis. Walk scores for each leased property were scraped from Walkscore.com. An address's walk score is based on its distance to nearby amenities, such as businesses, parks, schools, etc. Using a 100-point scale, with higher values indicating areas with greater foot traffic, addresses are classified into four categories: car-dependent (0–49), somewhat walkable (50–69), very walkable (70–89) and walker's paradise (90–100). Demographics and income data at the census tract-level come from the 2010 Decennial Census and the 2013 American Community Survey five-year sample, respectively, which are matched to the census tract of each leased property. These data include information on the population, number of households, age, sex, race and ethnicity, housing occupancy rate, and household income.

[9] A practical way to visualize the assignment rule is to first consider a layer of all lease transactions within an $r$-mile radius of a dispensary or loser (excluding the dispensary or loser) as yellow squares, and then superimpose a layer of dispensaries and losers along with their respective $r$-mile buffers over them. The blue of the dispensaries transforms the yellow leased properties into green (treated) squares, with the varying shades reflecting the signing date of the lease relative to the dispensary's start date, while the red of the losers transforms the remaining yellow leased properties into orange (control) squares.

Table 2.1 reports mean values from the pre-period (January 2010-June 2014), stratified by properties near dispensaries and properties near losers, under the four values of $r$. Corresponding p-values from two-sided tests of differences in means are reported in parentheses. Tests on the equality of means for census tract-level characteristics are performed in a regression framework, and standard errors are clustered at the census tract-level to account for the within-group correlation of the error term.[10]

As seen by comparing across the set of four column pairs, average yearly rent per square foot for the near-dispensary group of leased properties was about \$1-2 less at $r = 1/8$ and $r = 1/4$, but nearly the same at $r = 3/8$ and $r = 1/2$. There are at least two possible explanations for the discrepancy between the two groups at smaller radii. First, commercial areas that are zoned for recreational marijuana dispensaries may disproportionately feature lower quality or untenanted buildings. Second, landlords of higher quality properties or properties in bustling neighborhoods may have been more likely to withdraw from a commitment to lottery winners, ex-post, relative to landlords of lower quality properties or properties in areas with fewer commercial activity. While either scenario poses a threat to the notion that leased properties near dispensaries are virtually identical to leased properties near losers, it does not undermine the identification strategy used in the forthcoming analysis, which leverages within dispensary group changes before and after the establishment of a dispensary relative to the loser group, and relies on the differences between the two groups to be equal in trends rather than levels. The means for the other covariates for all radii are virtually indistinguishable from each other; except for household income, even the statistically significant differences are less than roughly one-tenth of either group's mean (in magnitude). These results suggest that before the opening of dispensaries, leased properties near dispensaries were comparable to leased properties near losers, particularly concerning the population residing in these areas.

[10] That is, each census tract-level characteristic is regressed on a dummy variable that takes the value one if the leased property is in the near-dispensary group and zero otherwise. Standard errors are clustered at the census tract-level and the reported p-values are those on the coefficient estimate of the dummy variable.

## 2.4 Empirical Strategy

This study uses information on operating dispensaries' location and opening date, as well as the proposed address on losers' applications, to compare areas where a recreational marijuana dispensary opens, before and after its opening, to counterfactual areas where losers would have potentially opened. A hedonic model within a difference-in-differences setting is employed to capture the benefits and costs of a recreational marijuana dispensary opening on nearby retail rents. As reviewed by Malpezzi (2002), hedonic modeling is a commonly used tool in real estate and urban economics for measuring the implicit price of a property feature or neighborhood characteristic that does not have an explicit market value. Formally theorized by Rosen (1974), hedonic pricing relies on the assumption that, in equilibrium, the "utility-bearing" characteristics of a differentiated product are capitalized in its observed market price. In the context of this research, the yearly rent per square foot represents the value that lessees of other nearby retail properties place on the physical characteristics of their own leased property, as well as the characteristics of its location, in this case, proximity to a recreational marijuana dispensary.

The identifying assumption underlying the analysis is that, conditional on any time-invariant characteristics of dispensary and loser locations, as well as any time-varying market conditions affecting retail rents that are common to all areas, the establishment of a dispensary is exogenous to the rents paid by nearby retail establishments. More formally, the following hedonic difference-in-differences model is estimated:

$$Y_{pit} = \alpha_0 + \beta_1 * DISPENSARY_{it} + \mathbf{X}_{pt}\psi + \theta_i + \delta_t + \mu_{pit} \tag{2.1}$$

Here, $Y_{pit}$ is yearly rent per square foot or the natural log of yearly rent per square foot for property $p$, in location $i$, at time $t$. The dummy variable $DISPENSARY_{it}$ takes the value of one if the lease transaction occurs at a location $i$ that is near a dispensary and at a time $t$ when the said dispensary is in operation, and zero otherwise. $\mathbf{X}_{pt}$ are retail property characteristics, such as building size, building quality, etc.; $\theta_i$ are dispensary/loser fixed effects; $\delta_t$ are month-by-year fixed effects, and $\mu_{pit}$ is an error term. Retail rents may also depend on other control covariates, such as census tract-level characteristics which are suppressed for now but included as robustness checks to the validity of the research design and to improve the efficiency of the estimated effects.

Given the randomization of the distribution of recreational marijuana dispensary licenses, perhaps a more natural empirical strategy would have been instrumental variables, with the following equation corresponding to the most parsimonious form of the first stage of the two-stage least squares model:

$$DISPENSARY_i = \pi_0 + \pi_1 * WINNER_i + \varepsilon_i \tag{2.2}$$

Here, the dummy variable $DISPENSARY_i$ takes the value of one if an address is associated with an application that later houses a dispensary, and zero otherwise, and $WINNER_i$ is an indicator variable for whether an address is associated with a lottery-winning applicant.

Panel A of Table 2.2 displays a two-way table for lottery applicants by $WINNER$ and $DISPENSARY$ status. The 167 dispensaries that are considered in the analysis are comprised of the 32 dispensaries with an address that is associated with a lottery-winning applicant, the 24 dispensaries with an address that is associated with a lottery-losing applicant, and the 111 dispensaries with an address that could not be linked to a lottery applicant.[11] The 348 losers are addresses that are associated with a lottery-losing applicant and where a recreational marijuana dispensary is not established within the sample period. After 2015, when the WSLCB increased the number of marijuana retailer licensees from 334 to 556,

[11] Recall that the "lottery" was actually a set of rank-ordered lists of applicants in each jurisdiction where there were more eligible applicants than licenses, and if lottery "winning" applicants failed to procure a retail license, then the next license application on the list (i.e., a lottery "losing" applicant) could be eligible for licensure.

nearly 40 additional recreational marijuana dispensaries were established at lottery losers' addresses while only three additional recreational marijuana dispensaries were established at lottery winners' addresses. This suggests that even though application addresses were not binding, lottery losers were initially constrained but lottery winners had ample time throughout the sample period to open a dispensary at their respective application addresses.[12]

The probability that a lottery-winning applicant is associated with an address that later houses a recreational marijuana dispensary is 0.234, while the probability that a recreational marijuana dispensary opens at an address that is associated with a lottery-losing applicant is 0.065. The difference between these two probabilities is 0.169. This is equivalent to the estimated slope coefficient from equation (2.2), which is displayed in Panel B of Table 2.2 along with its corresponding standard error. Being associated with a lottery-winning applicant positively and significantly impacts whether a recreational marijuana dispensary opens at that address. Notwithstanding the satisfaction of both the relevance and independence assumptions for a good instrument, an instrumental variables approach was ultimately infeasible because a majority of established dispensaries could not be linked by address to lottery applicants. Nevertheless, this finding is substantial evidence in support of the validity of the quasi-experimental design employed in this study.

To further substantiate the causal interpretation of the estimated effects presented in this research, the base difference-in-differences model is supplemented by an event study analysis. More specifically, a panel is formed by binning the retail lease microdata into monthly intervals at the dispensary/loser level, and the following event study model, which allows for studying the effects of a dispensary opening by months relative to the month in which it opens, is estimated:

[12]While it may seem unusual that a recreational marijuana dispensary was not established at so many (i.e., 105) addresses that were associated with lottery-winning applicants, 55 of the 105 opened dispensaries at different locations within the sample period, and hence were part of the 111 dispensaries with an address that could not be linked to a lottery applicant. Instead of a street address, these applicants were linked to dispensaries by matching application numbers to dispensary license numbers, and the mean and median distances between application address and dispensary address were 2.9 and 1.9 miles, respectively.

$$Y_{it} = \alpha_1 + \left( \gamma_{-6} * \mathbb{1}[t - T_i \leq -6] + \sum_{j \in J} \gamma_j * \mathbb{1}[t - T_i = j] + \gamma_6 * \mathbb{1}[t - T_i \geq 6] \right) * DISPENSARY_i + \theta_i + \delta_t + \nu_{it} \quad (2.3)$$

Here, $J = \{-5, -4, -3, -2, 0, 1, 2, 3, 4, 5\}$ and $Y_{it}$ is the mean yearly rent per square foot for location $i$ at time $t$. The subscript $j$ denotes event time, with $j = 0$ representing a dispensary's opening month, $j = -6$ representing the sixth month and prior from a dispensary's opening month and $j = 6$ representing the sixth month and later from a dispensary's opening, the earliest and latest event times, respectively. The dummy variable $DISPENSARY_i$ takes the value of one if the location is a dispensary, and zero if it is a loser. $T_i$ is the month in which a dispensary opens at location $i$, $\mathbb{1}[t - j = T_i]$ is an indicator function that takes the value of one when its argument is true and zero otherwise, $\theta_i$ and $\delta_t$ are location and time fixed effects, which are defined in the same manner as before, and $\nu_{it}$ is an error term. The $\gamma_0$ coefficient captures the immediate impact on nearby retail rents from the opening of a recreational marijuana dispensary, while the coefficients with negative and positive subscripts, leads and lags, respectively, capture the month-by-month evolution of retail rents in relation to the month before the first month of the establishment of a dispensary–the omitted event month. Lead coefficients are used to test for any pre-existing trends before the opening of a dispensary, while lag coefficients test for the dynamic effects of dispensary openings.

## 2.5 Results

Paramount to the validity of a difference-in-differences design is the parallel trends assumption, which implies that treated and control groups experience similar trends in outcomes before treatment. A commonly used approach for establishing this condition is to plot the means of the outcome variable for treatment and control groups over time and visually inspect their trajectories over the pre-treatment period. However, since dispensaries open at different dates, the timing of treatment varies across locations, and therefore no single date divides pre and post periods for all units. A comparable approach is to normalize the time dimension, such that it is defined relative to the periods before and after the opening of a dispensary, and then plot means and inspect trajectories.

Figure 2.3 plots the mean yearly rent per square foot by this normalized measure of time (i.e., months before and after the opening of a dispensary) for each of the four values of $r$. The first month of the post-period for the near-loser group is July 2014, which coincides with the opening of the first recreational marijuana dispensary in the state.[13] Three features of the graphs are worth noting. First, before the opening of a dispensary, the dashed lines that represent the means for the near-dispensary group follow a similar path as the solid lines that represent the means for the near-loser group. Second, there is no clear indication of a change in patterns caused by the opening of a dispensary. Third, the variance of yearly rent per square in the months after the opening of a dispensary is higher for the near-dispensary group than the near-loser group, particularly at the three smaller radii. Figure 2.4 presents a less noisy version of Figure 2.3, in which time is normalized according to the quarters before and after the opening of a dispensary, with the first quarter of the post-period, for the near-loser group represented by July-September 2014. This auxiliary illustration accentuates the three main takeaways of the underlying data that are listed above; especially that the mean differences of yearly rent per square foot between the near-dispensary group and the near-loser group are not changing before the opening of a dispensary.

Table 2.3 reports the estimated coefficients on the difference-in-differences variable from equation (2.1), which by virtue of the institutional setting and the satisfaction of the parallel trends assumption can be interpreted as the causal effects of a recreational marijuana dispensary opening on nearby retail rents. The first two columns represent regressions where

[13] Alternative definitions of the pre and post period for the near-loser group were considered, but the results are not substantively different from those presented in this analysis. Results are available upon request.

the outcome is the yearly rent per square foot while the last two columns represent regressions where the outcome is the natural log of yearly rent per square foot. All regressions cluster standard errors at the dispensary/loser-address level and control for the natural log of distance to dispensary/loser-address, the number of additional operating marijuana dispensaries within the corresponding radius, natural log of building square feet, a dummy variable for whether the leased property is on the ground floor and categorical variables for walk score, CoStar star rating, and rent type. Regressions represented by columns (2) and (4) add census tract-level characteristics, which are the number of households, median age, percent male, percent White, percent Hispanic, housing occupancy percent, and median household income, as covariates to the regressions represented by columns (1) and (3), respectively.

The estimated difference-in-differences for yearly rent per square foot models at all radii except $r = 1/4$, are small, statistically indistinguishable from zero, and robust to the inclusion of census tract-level characteristics as controls. The numbers in brackets are percentage changes relative to the outcome mean of the dispensary group of leased properties in the period immediately before the opening of the dispensary, and they too are small for all radii except $r = 1/4$. While the estimated effects for only $r = 1/4$ are statistically significant and should naturally be interpreted with caution, doing so is even more appropriate in this context for two reasons. One, the effects on leased properties that are within a smaller buffer (i.e., $r = 1/8$), which constitutes a subset of the leased properties that are within a $1/4$-mile radius of a dispensary/loser, are not similar in magnitude. Second, the graph for $r = 1/4$ in Figure 2.3 clearly reveals the existence of positive outliers in the post-period for the near-dispensary group of leased properties. Log-transforming yearly rent per square foot remedies this skewness towards large values, and as a result, the estimated effects reported in columns (3) and (4) for all radii, including $r = 1/4$, are statistically indistinguishable from zero at the five percent significance level. These estimated effects, which can be approximately interpreted as percentage changes when multiplied by a hundred, are also small and robust to the inclusion of census tract-level characteristics as controls.

Table 2.4 presents robustness tests to the hedonic difference-in-differences equation that controls for census tract-level characteristics. The first two columns represent regressions on a sample that restricts building square feet to 5,000 square feet or less while the last two columns represent regressions on a sample that restricts the CoStar star rating to three or less; which are thresholds that, on average, exclude about ten percent of the respective samples. Odd-numbered columns represent regressions where the outcome is the yearly rent per square foot, and even-numbered columns represent regressions where the outcome is the natural log of yearly rent per square foot. The interpretation of these estimates is not qualitatively different from the ones presented before; that is, they are small and imprecisely estimated.

To establish the tenability of these null results, Table 2.5 presents the estimated coefficients on property-level covariates from the preferred specification of equation (2.1), which is the regression for the natural log of yearly rent per square foot that controls for census tract-level characteristics (i.e., the regression represented by column (4) in Table 2.3), for all radii. While these estimates merely provide a descriptive overview of each covariate's relationship with retail rents, the sensibility of their signs and magnitudes offers support for the plausibility of the model. The interpretation of three particularly noteworthy sets of estimates is expounded below. First, consistent with the findings reported in Table 2.1, which shows that average retail rents increase as the size of the buffer for which dispensaries and losers serve as a centroid increase, distance to the nearest dispensary or loser is positively correlated with the outcome at all radii.[14] Second, increasing the number of additional operating marijuana dispensaries within the corresponding radius is negatively correlated with retail

[14]A parallel analysis, which restricts the sample to the post-period (i.e., July 2014-December 2015, inclusive) and regresses retail rents on the natural log of distance to dispensary/loser-address and its interaction with the $DISPENSARY$ variable, produces a similar result. Namely, that retail rents are increasing in distance to the nearest dispensary or loser, for all radii, and that the interaction with the $DISPENSARY$ variable dampens the gradient, albeit not in a statistically significant manner. For brevity, the results are not reported but are available upon request.

rents, with stronger associations at smaller radii. This fact may arise from near-dispensary group properties being different than near-loser group properties, as discussed in Section 2.3, but may also suggest that conditional on the presence of an existing dispensary within the corresponding radius, an additional dispensary can hurt nearby retail rents. Third, retail rents are increasing in CoStar star rating categories, and the estimated relationships are highly significant, which demonstrates that the CoStar star rating subsumes many of the structural and locational attributes of the retail property that are not directly included in the regression.

Figure 2.5 reports the results of the event study analysis, defined by equation (2.3), at various radii, where the outcome is the mean of the natural log of yearly rent per square foot aggregated to the dispensary/loser-address level in monthly intervals. As before, there is little evidence of dispensary group properties trending differently from loser group properties before the opening of a dispensary, and hypothesis tests for whether each of the lead coefficients, individually or in sum, are equal to zero are never rejected at the five percent significance level. Similarly, there are no immediate or lagged effects on nearby retail rents from the opening of a recreational marijuana dispensary. While it is not surprising that the event study analysis also produces null findings, this illustration lucidly demonstrates the validity of the identification strategy, lends credence to the causal interpretation of the estimated effects presented in this research, and reaffirms the overarching conclusion of this research.

Finally, Table 2.6 reports estimates of heterogeneous treatment effects, from the preferred specification, across high- and low-walkability, as well as high- and low-income locations. These different locations are designated as follows. For each prevailing value of $r$, dispensary/loser-addresses' walk scores and their census tracts' median household income are ranked, and dispensaries/losers with walk scores or census tract median household incomes above (at or below) the $50^{th}$ percentile are designated as high- (low-) walkability or high- (low-) income, respectively.[15] Leased properties that are stratified by their assigned dispensary/loser's walkability designation are represented in columns (1) and (2) and leased properties that are stratified by their assigned dispensary/loser's income designation are represented in columns (3) and (4). For comparison, column (5) replicates the output from the regression of the preferred specification on the full sample (i.e., the regression represented by column (4) in Table 2.3).

Two aspects of the estimates in Table 2.6 stand out. First, all estimates, except for the ones in high-walkability areas at 1/4-mile and 3/8-mile radii, and low-income areas at the 1/4-mile radius, are small and statistically indistinguishable from zero. Second, the precisely estimated coefficients are all positive and greater than their respective full-sample counterparts. Even though most of the heterogeneous effects are imprecisely estimated, and therefore, should be interpreted cautiously, they suggest that the impact of a recreational dispensary opening on nearby retail rents depends on the area in which the dispensary is established. More specifically, the analysis shows that the positive spillovers onto nearby businesses are most identifiable in low-income urban areas. There are at least two possible explanations for this. One, competition for retail space may be higher in densely populated areas, which can prompt dispensary owners to invest in more attractive and safe-looking storefronts (Burkhardt and Flyr, 2019). Second, to the extent that increased access to retail marijuana outlets lowers the sum of both the pecuniary and non-pecuniary costs of purchasing marijuana, then low-income consumers, who are presumably more price-sensitive, will be more responsive to this price change. Thus, dispensaries in low-income areas are more likely to be well patronized, which can have a positive effect on nearby retail stores.

[15]Exploring heterogeneous treatment effects across urban and rural areas were infeasible because nearly all of the leased retail properties in the CoStar dataset were in Urbanized Areas.

## 2.6 Conclusion

Over the last two decades, a majority of U.S. states have established medical or recreational marijuana laws that regulate the possession, consumption, and sale of marijuana to varying degrees. As a result, numerous studies have evaluated the impacts of these laws on various outcomes, including residential real estate markets, but no research has previously explored their effect on commercial real estate. This is despite the fact that industry reports show that due to rising demand and zoning restrictions, locations that are used for marijuana operations are rented at two to three times the amount of comparable properties (Vance and Murtaugh, 2017).

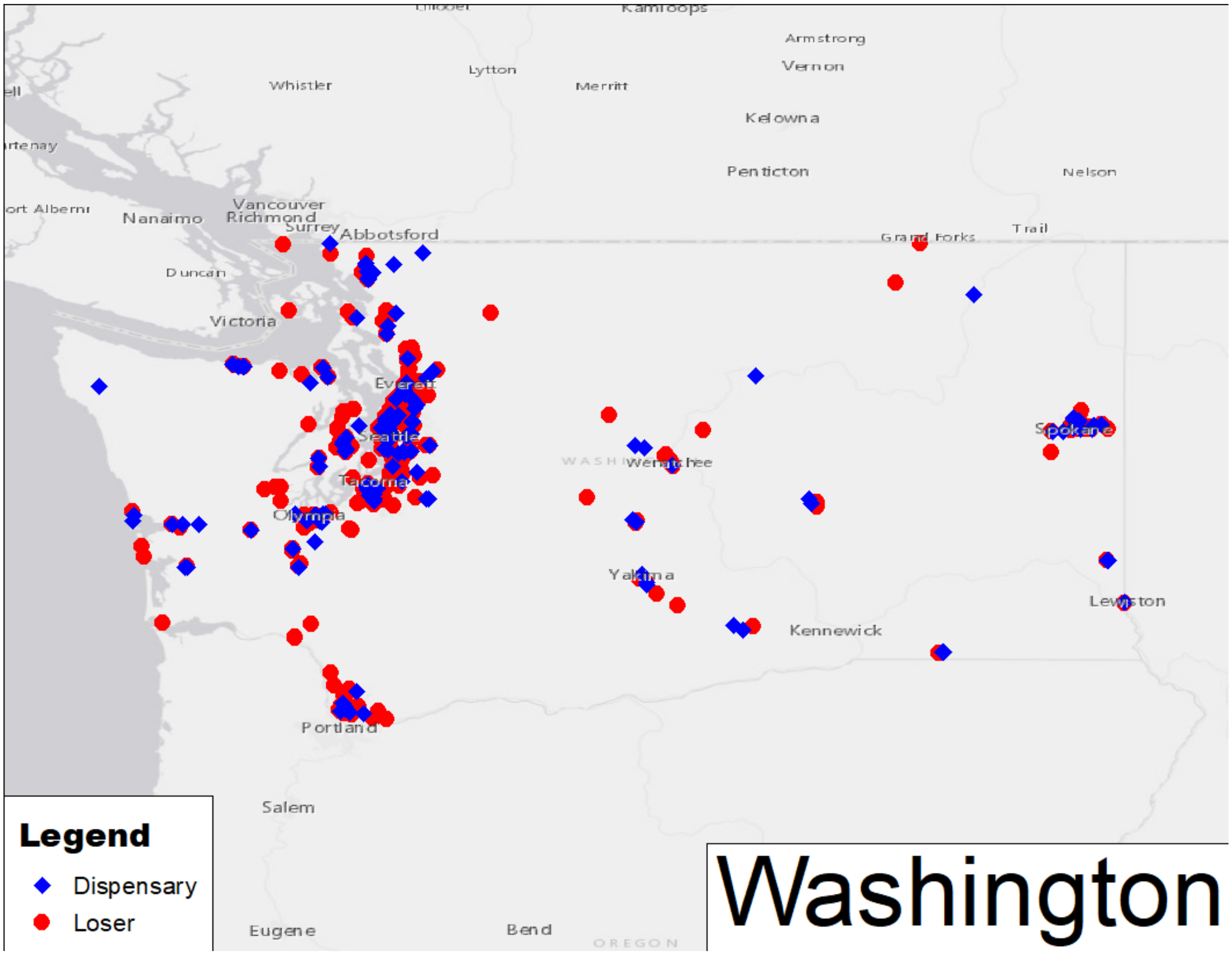
This study adds to the literature by estimating the effects of recreational marijuana dispensary openings on nearby retail rents. It leverages plausibly exogenous variation in where recreational marijuana dispensaries locate by constructing a control group out of areas where dispensary licenses were sought in a lottery process but were not granted. This is arguably a superior methodology than that used by earlier studies of dispensaries' effects on real estate prices, which do not recognize that areas where dispensaries locate may be contemporaneously different than a typical area in a city.

This chapter's key takeaway is that the opening of a recreational marijuana dispensary does not generate a large or significant spillover onto nearby commercial properties, although there is some evidence of positive externalities in low-income urban areas. This conclusion is different from those of recent research, which study the effects of marijuana dispensaries on nearby residential properties in Colorado and find large positive effects. Conklin et al. (2020) and Burkhardt and Flyr (2019) both study the Denver, Colorado area and estimate spillover effects of approximately eight percent. Since these studies are interested in a different outcome, use data from a different state, and employ different empirical approaches, they do not necessarily contradict the null effects reported in this article. They, along with the present article, all contribute to the nascent literature on the impacts that looser restrictions on the sale of marijuana can have on property values.

Research in this realm is pertinent to local governments in states with RMLs that have been granted the autonomy to ban or allow marijuana sales (i.e., dispensaries) (Mikos, 2017). Policymakers within these jurisdictions must fully evaluate all of the potential benefits and costs that may simultaneously emerge from such policies. One immediate benefit is the additional revenue generated by local sales and excise taxes, as well as the sharing of marijuana-related taxes collected by state governments, which in many states, including Washington, is conditional on local permission of recreational marijuana businesses (Lar, 2018). On the other hand, enacting policies that explicitly conflict with federal law can expose local officials, as well as landlords and dispensary operators to federal prosecution (Jarrett, 2018). This chapter suggests that the opening of a dispensary is neither a net cost nor a net benefit from the perspective of nearby retail establishments.

## 2.7 Figures

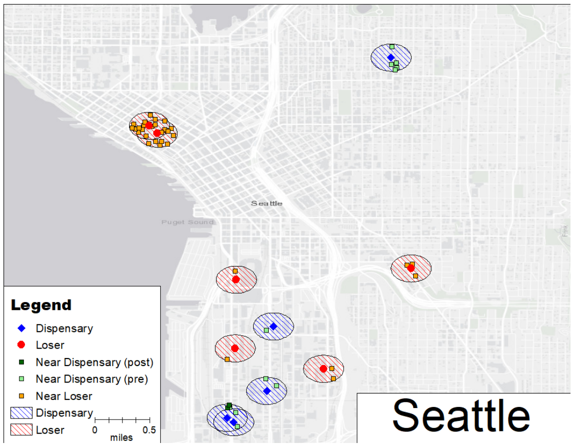
Figure 2.1: Dispensary and Loser Locations



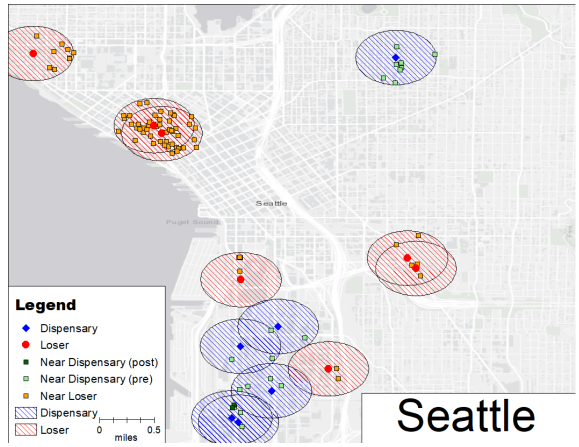

NOTE - Solid blue diamonds denote dispensaries and solid red circles denote losers.

Figure 2.2: Varying Control and Treatment Groups by $r$ – Downtown Seattle

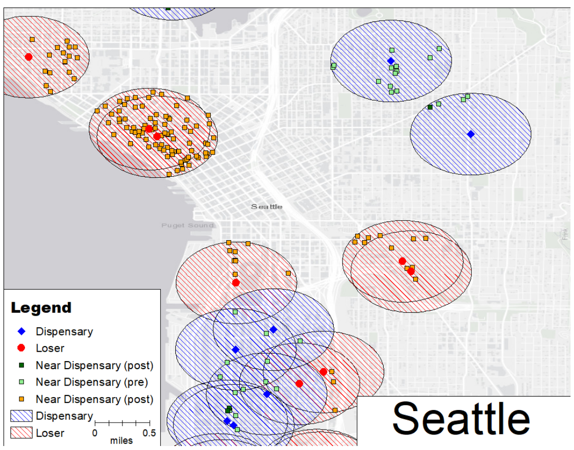
$r = 1/8$



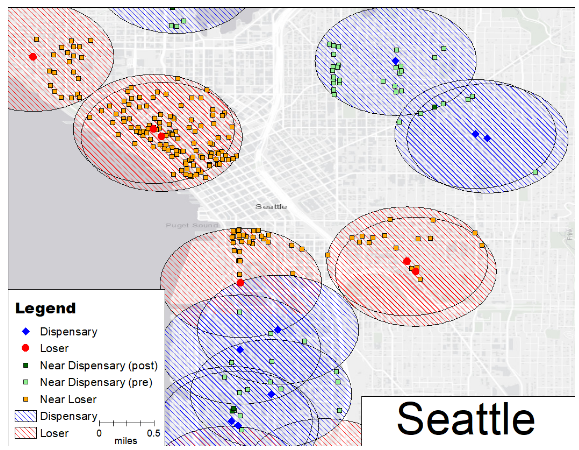

$r = 1/4$




$r = 3/8$

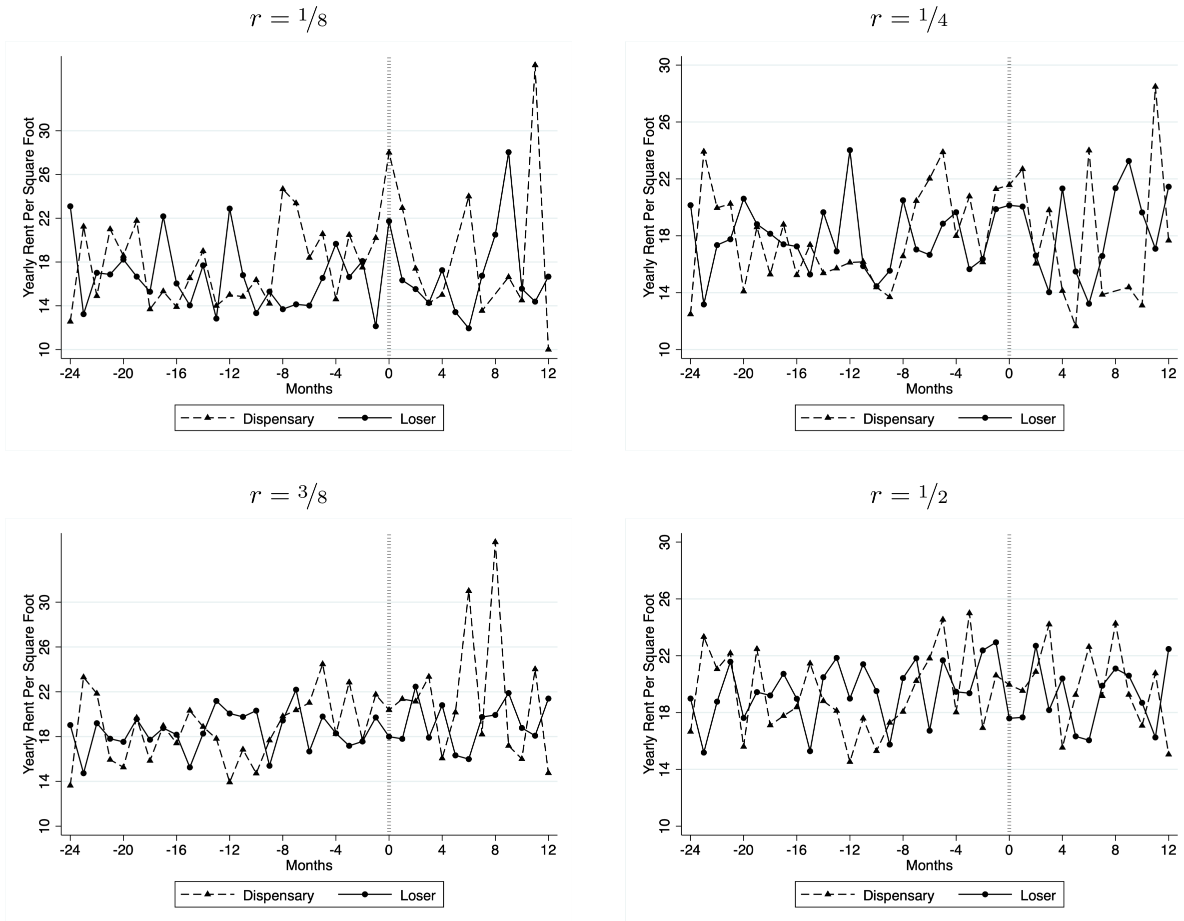



$r = 1/2$



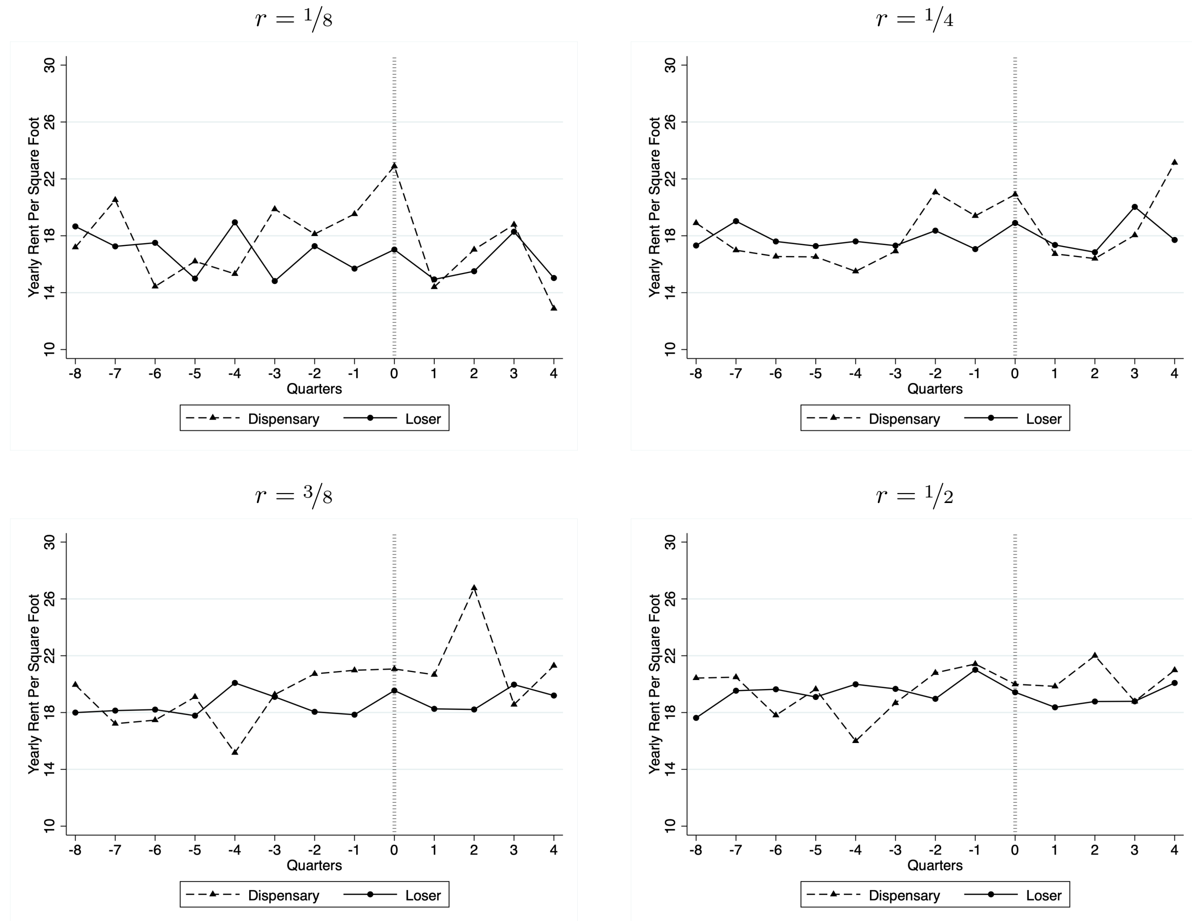

NOTE - Solid blue diamonds denote dispensaries and solid red circles denote losers. Striped blue and red circles indicate $r$-mile buffers around dispensaries and losers, respectively. Green squares indicate lease transactions that occur near a dispensary, while dark green squares further indicate lease transactions that occur at a time when said dispensary is in operation. Orange squares indicate lease transactions that occur near a loser.

Figure 2.3: Mean Yearly Rent per Square Foot by Time (Monthly)



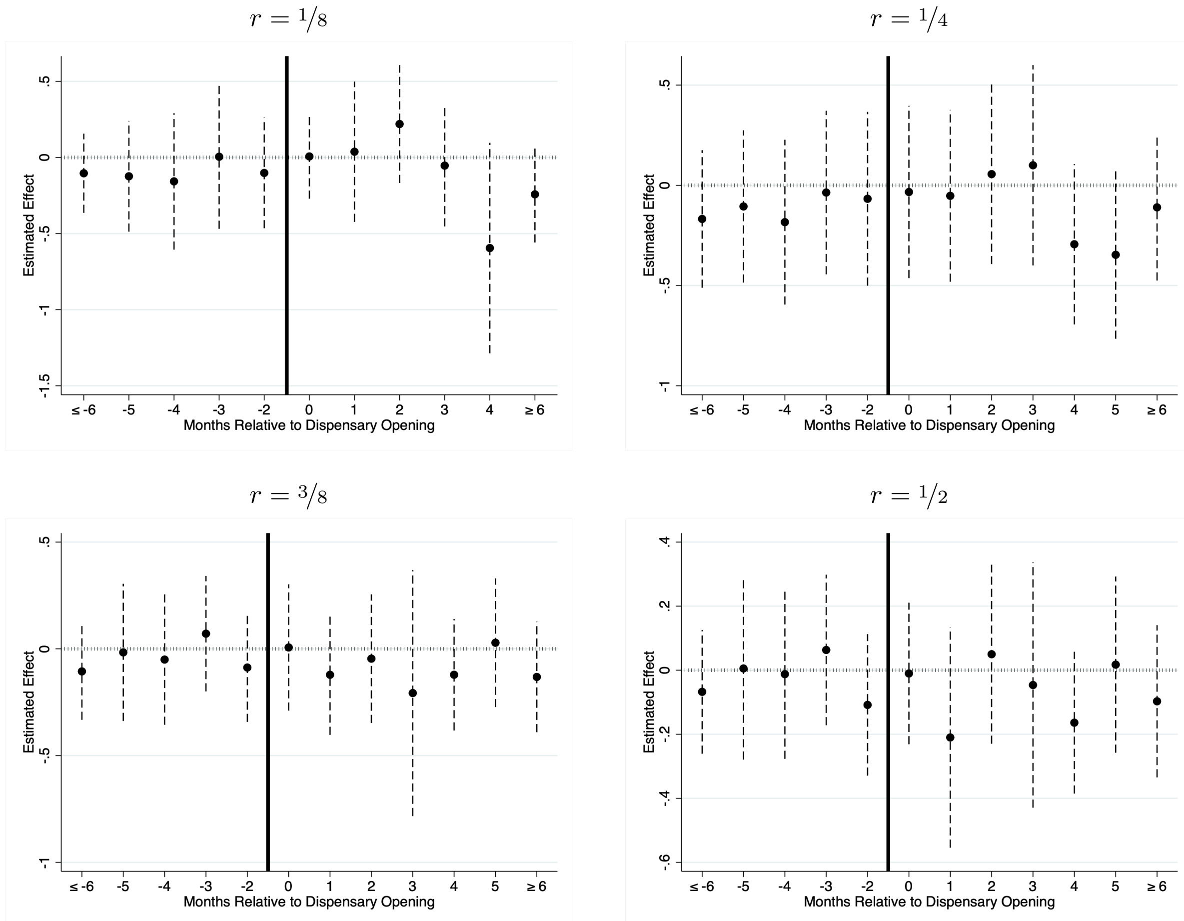

NOTE - Mean yearly rent per square foot by time (monthly) around dispensaries/losers at various radii. Time = 0 is July 2014 for the loser group.

Figure 2.4: Mean Yearly Rent per Square Foot by Time (Quarterly)




NOTE - Mean yearly rent per square foot by time (quarterly), around dispensaries/losers at various radii. Time = 0 is July-September 2014 for the loser group.

Figure 2.5: Event Study




NOTE - Point estimates and corresponding 95 percent confidence intervals are from models that control for dispensary/loser-address and month-by-year fixed effects, with standard errors clustered at the dispensary/loser-address level. Outcome is the mean of the natural log of yearly rent per square foot aggregated to the dispensary/loser-address level in monthly intervals. The omitted event month is the month prior to the first month of the establishing of the dispensary, which is denoted by the solid black vertical line.

## 2.8 Tables

Table 2.1: Descriptive Statistics

| | $r = 1/8$ | | $r = 1/4$ | | $r = 3/8$ | | $r = 1/2$ | |
|---|---|---|---|---|---|---|---|---|
| | Near Dispensary (1) | Near Loser (2) | Near Dispensary (3) | Near Loser (4) | Near Dispensary (5) | Near Loser (6) | Near Dispensary (7) | Near Loser (8) |
| Yearly Rent per Square Foot ($) | 15.28 | 16.91 | 16.23 | 17.37 | 17.80 | 18.00 | 18.34 | 18.42 |
| | (0.006)*** | | (0.011)** | | (0.603) | | (0.829) | |
| Distance in Miles to Dispensary/Loser | 0.072 | 0.074 | 0.134 | 0.138 | 0.203 | 0.211 | 0.276 | 0.270 |
| | (0.505) | | (0.280) | | (0.117) | | (0.272) | |
| Square Feet Leased | 2,681 | 2,669 | 2,748 | 2,777 | 2,990 | 2,748 | 2,991 | 2,704 |
| | (0.965) | | (0.896) | | (0.240) | | (0.103) | |
| CoStar Star Rating | 2.4 | 2.3 | 2.4 | 2.4 | 2.5 | 2.5 | 2.5 | 2.6 |
| | (0.007)*** | | (0.943) | | (0.961) | | (0.011)** | |
| Months on Market | 20.0 | 19.2 | 19.5 | 19.9 | 18.9 | 19.8 | 18.7 | 20.2 |
| | (0.638) | | (0.709) | | (0.261) | | (0.036)** | |
| $P$(Ground Floor) | 0.928 | 0.949 | 0.936 | 0.940 | 0.923 | 0.921 | 0.930 | 0.905 |
| | (0.293) | | (0.771) | | (0.871) | | (0.020)** | |
| Walk Score | 67.3 | 65.6 | 66.6 | 67.9 | 69.5 | 69.8 | 70.1 | 71.3 |
| | (0.267) | | (0.220) | | (0.730) | | (0.116) | |
| Population | 4,592 | 4,604 | 4,529 | 4,573 | 4,685 | 4,396 | 4,774 | 4,318 |
| | (0.970) | | (0.861) | | (0.310) | | (0.100)* | |
| Number of Households | 1,909 | 1,998 | 1,932 | 2,027 | 2,086 | 1,996 | 2,147 | 1,986 |
| | (0.524) | | (0.420) | | (0.547) | | (0.279) | |
| Median Age | 34.7 | 36.1 | 35.0 | 36.2 | 35.0 | 36.7 | 35.1 | 37.3 |
| | (0.238) | | (0.241) | | (0.087)* | | (0.039)** | |
| Male (%) | 50.0 | 50.0 | 50.1 | 50.4 | 50.4 | 51.1 | 50.2 | 51.9 |
| | (0.953) | | (0.669) | | (0.345) | | (0.071)* | |
| White (%) | 73.7 | 73.5 | 73.5 | 73.3 | 74.3 | 73.5 | 74.8 | 73.3 |
| | (0.925) | | (0.942) | | (0.678) | | (0.451) | |
| Hispanic or Latino (%) | 10.1 | 10.0 | 10.0 | 10.3 | 9.3 | 10.3 | 9.3 | 9.9 |
| | (0.921) | | (0.763) | | (0.318) | | (0.510) | |
| Housing Occupancy (%) | 90.6 | 91.0 | 89.4 | 90.9 | 89.3 | 90.6 | 90.0 | 90.1 |
| | (0.820) | | (0.543) | | (0.542) | | (0.981) | |
| Median Household Income ($) | 54,360 | 49,211 | 54,524 | 48,386 | 56,296 | 48,971 | 56,460 | 47,440 |
| | (0.119) | | (0.087)* | | (0.050)* | | (0.018)** | |
| Sample Size | 237 | 372 | 517 | 752 | 830 | 1,214 | 1,212 | 1,483 |

NOTE - Parentheses are p-values from two-sided tests of differences in means. Tests on the equality of means for census tract-level characteristics are performed in a regressions framework with standard errors clustered at the census tract-level. Comparisons are made for leased properties near dispensaries vs. leased properties near losers at various radii around dispensaries/losers, during the pre-period (January 2010-June 2014). *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 2.2: First Stage Results

Panel A

| | | $DISPENSARY$ | |
|---|---|---|---|
| | | 1 | 0 |
| $WINNER$ | 1 | 32 | 105 |
| | 0 | 24 | 348 |
| | . | 111 | . |

$P(DISPENSARY = 1|WINNER = 1) = 0.234$
$P(DISPENSARY = 1|WINNER = 0) = 0.065$

Panel B

| | | |
|---|---|---|
| $\hat{\pi}_1$ | 0.169*** | (0.030) |

NOTE - Estimates on the coefficient of the $WINNER$ variable ($\pi_1$) from the first stage equation and the corresponding standard error. *Significant at 10% level ; **Significant at 5% level ; ***Significant at 1% level

Table 2.3: Difference-in-Differences Estimates

| | Yearly Rent per Square Foot | | ln(Yearly Rent per Square Foot) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $r = 1/8$ | 0.259 | 0.478 | 0.010 | 0.018 |
| | (2.135) | (2.131) | (0.099) | (0.099) |
| | [1.28%] | [2.37%] | | |
| Sample Size | | 762 | | |
| Dispensaries/Losers | | 176 | | |
| $r = 1/4$ | 2.374** | 2.424** | 0.096 | 0.095 |
| | (1.128) | (1.090) | (0.061) | (0.060) |
| | [11.16%] | [11.39%] | | |
| Sample Size | | 1,586 | | |
| Dispensaries/Losers | | 239 | | |
| $r = 3/8$ | 0.757 | 0.608 | 0.028 | 0.020 |
| | (1.095) | (1.124) | (0.059) | (0.060) |
| | [3.50%] | [2.80%] | | |
| Sample Size | | 2,566 | | |
| Dispensaries/Losers | | 270 | | |
| $r = 1/2$ | 0.476 | 0.330 | 0.005 | -0.003 |
| | (0.935) | (0.938) | (0.051) | (0.052) |
| | [2.31%] | [1.60%] | | |
| Sample Size | | 3,340 | | |
| Dispensaries/Losers | | 278 | | |
| Month Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Dispensary/Loser Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | | ✓ | | ✓ |

NOTE - Estimates on the coefficient of the $DISPENSARY$ variable ($\beta_1$) from the hedonic difference-in-differences equation, by various radii. Parentheses are clustered standard errors, clustered at the dispensary/loser-address level. Brackets are percentage changes in reference to means for dispensary group in the period immediately before the establishing of the dispensary, reported only for yearly rent per square foot regressions. All regressions control for the natural log of distance to dispensary/loser-address, number of additional operating marijuana dispensaries within the corresponding radius, natural log of building square feet, a dummy variable for whether the leased property is on the ground floor and categorical variables for walk score, CoStar star rating, and rent type. Regressions represented by columns (2) and (4) add census tract-level characteristics as controls, which are number of households, median age, percent male, percent White, percent Hispanic, housing occupancy percent, and median household income. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 2.4: Robustness Checks

| | Square Feet $\leq$ 5,000 | | CoStar Star Rating $\leq$ 3 | |
|---|---|---|---|---|
| | Yearly Rent per Square Foot | $ln$(Yearly Rent per Square Foot | Yearly Rent per Square Foot | $ln$(Yearly Rent) per Square Foot |
| | (1) | (2) | (3) | (4) |
| $r = 1/8$ | 0.358 | 0.021 | 1.106 | 0.045 |
| | (2.100) | (0.088) | (2.123) | (0.098) |
| Sample Size | 685 | | 735 | |
| $r = 1/4$ | 2.433* | 0.080 | 1.779 | 0.089 |
| | (1.306) | (0.061) | (1.338) | (0.068) |
| Sample Size | 1,409 | | 1,488 | |
| $r = 3/8$ | 0.854 | 0.018 | -0.082 | 0.021 |
| | (1.219) | (0.061) | (1.291) | (0.070) |
| Sample Size | 2,270 | | 2,339 | |
| $r = 1/2$ | 0.690 | 0.015 | -0.454 | -0.023 |
| | (1.036) | (0.054) | (1.019) | (0.059) |
| Sample Size | 2,961 | | 3,007 | |
| Month Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Dispensary/Loser Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | ✓ | ✓ | ✓ | ✓ |

NOTE - Estimates on the coefficient of the $DISPENSARY$ variable ($\beta_1$) from the hedonic difference-in-differences equation, by various radii. Parentheses are clustered standard errors, clustered at the dispensary/loser-address level. All regressions control for the natural log of distance to dispensary/loser-address, number of additional operating marijuana dispensaries within the corresponding radius, natural log of building square feet, a dummy variable for whether the leased property is on the ground floor and categorical variables for walk score, CoStar star rating, and rent type, as well as census tract-level characteristics, which are number of households, median age, percent male, percent White, percent Hispanic, housing occupancy percent, and median household income. Columns (1) and (2) represent samples that restrict leased properties to 5,000 square feet or less. Columns (3) and (4) represent samples that restrict leased properties with a CoStar star rating of 3 or less. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 2.5: Coefficients on Property-Level Covariates

| | $r = 1/8$ | $r = 1/4$ | $r = 3/8$ | $r = 1/2$ |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $ln$(Distance to Dispensary/Loser-Address) | 0.031 | 0.040* | 0.030 | 0.031* |
| | (0.038) | (0.022) | (0.020) | (0.017) |
| Number of Additional Dispensaries | -0.125 | -0.212** | -0.056 | -0.044 |
| | (0.109) | (0.083) | (0.079) | (0.063) |
| $ln$(Building Square Feet) | -0.087*** | -0.108*** | -0.104*** | -0.091*** |
| | (0.021) | (0.015) | (0.012) | (0.010) |
| Ground Floor | 0.190*** | 0.226*** | 0.170*** | 0.142*** |
| | (0.064) | (0.050) | (0.031) | (0.032) |
| Walk Score = Somewhat Walkable (50–69) | 0.280** | 0.127 | 0.057 | 0.067 |
| | (0.139) | (0.087) | (0.064) | (0.065) |
| Walk Score = Very Walkable (70–89) | 0.153 | 0.039 | 0.024 | 0.033 |
| | (0.185) | (0.105) | (0.082) | (0.070) |
| Walk Score = Walker's Paradise (90–100) | | 0.138 | 0.124 | 0.118 |
| | . | (0.126) | (0.089) | (0.080) |
| CoStar Star Rating = 2 | 0.050 | 0.091* | 0.103*** | 0.102*** |
| | (0.068) | (0.053) | (0.039) | (0.036) |
| CoStar Star Rating = 3 | 0.148* | 0.190*** | 0.231*** | 0.248*** |
| | (0.076) | (0.063) | (0.046) | (0.042) |
| CoStar Star Rating = 4 | 0.182* | 0.255*** | 0.312*** | 0.334*** |
| | (0.107) | (0.080) | (0.059) | (0.056) |
| CoStar Star Rating = 5 | 0.753*** | 0.373** | 0.315*** | 0.412*** |
| | (0.198) | (0.169) | (0.058) | (0.082) |
| Rent Type = Starting | -0.035 | -0.012 | 0.017 | 0.019 |
| | (0.051) | (0.037) | (0.026) | (0.025) |
| Rent Type = Asking | 0.071* | 0.090*** | 0.090*** | 0.102*** |
| | (0.036) | (0.025) | (0.022) | (0.022) |
| Month Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Dispensary/Loser Fixed Effects | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | ✓ | ✓ | ✓ | ✓ |

NOTE - Estimates on the coefficient of the $DISPENSARY$ variable ($\beta_1$) from the hedonic difference-in-differences equation, by various radii. Parentheses are clustered standard errors, clustered at the dispensary/loser-address level. All regressions control for the natural log of distance to dispensary/loser-address, number of additional operating marijuana dispensaries within the corresponding radius, natural log of building square feet, a dummy variable for whether the leased property is on the ground floor and categorical variables for walk score, CoStar star rating and rent type, as well as census tract-level characteristics, which are number of households, median age, percent male, percent White, percent Hispanic, housing occupancy percent, and median household income. Columns (1) and (2) represent samples that restrict leased properties to 5,000 square feet or less. Note: Due to collinearity, the dummy variable for Walk Score = Walker's Paradise (90–100) is omitted from the regression for $r = 1/8$. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 2.6: Heterogeneous Treatment Effects

| | Walkability | | Income | | Full Sample |
|---|---|---|---|---|---|
| | High | Low | High | Low | |
| | (1) | (2) | (3) | (4) | (5) |
| $r = 1/8$ | -0.005 | -0.062 | -0.067 | 0.177 | 0.018 |
| | (0.199) | (0.092) | (0.153) | (0.114) | (0.099) |
| Sample Size | 334 | 428 | 373 | 389 | 762 |
| $r = 1/4$ | 0.278** | 0.060 | 0.047 | 0.169** | 0.095 |
| | (0.130) | (0.071) | (0.076) | (0.081) | (0.060) |
| Sample Size | 591 | 995 | 821 | 765 | 1,586 |
| $r = 3/8$ | 0.223* | -0.020 | 0.046 | 0.016 | 0.020 |
| | (0.118) | (0.065) | (0.088) | (0.078) | (0.060) |
| Sample Size | 846 | 1,720 | 1,318 | 1,248 | 2,566 |
| $r = 1/2$ | 0.004 | -0.007 | 0.028 | 0.015 | -0.003 |
| | (0.133) | (0.060) | (0.095) | (0.057) | (0.052) |
| Sample Size | 1,019 | 2,321 | 1,640 | 1,700 | 3,340 |
| Month Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Dispensary/Loser Fixed Effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| Control Covariates | ✓ | ✓ | ✓ | ✓ | ✓ |

NOTE - Estimates on the coefficient of the $DISPENSARY$ variable ($\beta_1$) from the hedonic difference-in-differences equation where the outcome is the natural log of yearly rent per square foot, by various radii. Parentheses are clustered standard errors, clustered at the dispensary/loser-address level. All regressions control for the natural log of distance to dispensary/loser-address, number of additional operating marijuana dispensaries within the corresponding radius, natural log of building square feet, a dummy variable for whether the leased property is on the ground floor and categorical variables for walk score, CoStar star rating and rent type, as well as census tract-level characteristics, which are number of households, median age, percent male, percent White, percent Hispanic, housing occupancy percent, and median household income. High- (low-) walkability and high- (low-) income denote lease transactions within the $r$-mile buffer of a dispensary/loser that has a census tract median household income above (at or below) the $50^{th}$ percentile and a walk score above (at or below) the $50^{th}$ percentile, respectively. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

# Chapter 3

# Availability of Grandparent-Provided Child Care and Mothers' Labor Market Outcomes

## 3.1 Introduction

How much does grandparent-provided child care affect the labor market outcomes of mothers? Given the well-established negative impact of childbearing on mothers' labor supply, one would expect that time transfers from grandparents to mothers with young children, in the form of low-cost and reliable child care, would lower mothers' reservation wages, and thus increase the probability of partaking in paid work. Indeed, a large body of empirical literature shows that there is a strong and positive correlation between grandparent-provided child care and mothers' labor supply (Casper et al., 1994, Guzman, 1999). Despite this positive correlation, identifying the causal link between grandparent-provided child care and mothers' labor market outcomes is difficult because child care, along with labor and leisure, is often included as an argument of the household's utility function (Heckman, 1974, Connelly, 1992).

Much of the early research attempted to address this endogeneity problem by exploring variations in grandparents' proximity to mothers (Leibowitz et al., 1992, Ogawa and Ermisch, 1996, Del Boca, 2002, Compton and Pollak, 2014).[1] However, the residency choice of mothers and grandparents may also be endogenous to mothers' labor supply. For example, if more career-oriented women are more likely to have larger job search radii, then this can lead to them living further away from their parents. On the other hand, more career-oriented women may opt to live near their child's grandparents and take advantage of this source of low-cost child care. In the first case, estimates of the coefficient on grandparental proximity will be biased downward, while the latter case will overstate the effects. Furthermore, mothers and grandparents may make labor supply, proximity, and child care decisions simultaneously, such as grandparents dropping out of the labor force and locating near mothers to provide child care.

This research circumvents this potential source of endogeneity by focusing on a sample of three-generation households. That is, households with a grandparent, a mother, and a child. In doing so, identifying variation does not hinge on parents' proximity to grandparents, but rather grandparents' time availability. Before describing the research in full and in an attempt to attain a convincing answer to the question posed above, it is perhaps useful to conceptualize an ideal environment for studying such a question. Consider a sample of young mothers who wish to participate in the labor market and live near or with at least one of the child's grandparents. Assuming that these mothers could effectively be randomly assigned to receive child care from a grandparent, then the labor market outcomes of *treated group* and *control group* mothers could be compared to obtain unbiased

[1] In addition to their main analysis, Compton and Pollak (2014) also estimate similar, albeit statistically insignificant, effects for a subsample of military wives, whose husbands' residency location is determined by the military.

estimated effects of grandparent-provided child care. While this methodology resembles a randomized control trial, often considered the gold standard for clinical trials, it seems overly complex to effectively randomize grandparent-provided child care within a real-world setting and simultaneously keep all other variables constant. A more feasible solution may be to randomly endow grandparents with varying levels of time and instrument the provision of any grandparent-provided child care with this time endowment and subsequently compare outcomes of treated and control groups of mothers.

Following the framework outlined by this thought experiment, this research uses a fuzzy regression discontinuity (RD) research design that leverages the United States Social Security early-retirement age as a plausibly exogenous source of variation in grandparents' time endowment and, by extension, the amount of time that the grandparent can transfer to the mother in the form of child care. The identifying assumption underlying this approach is that within a narrow window of the early-retirement age threshold, mothers that are associated with grandparents that are just above the early-retirement age threshold are comparable to mothers that are associated with grandparents that are just below the threshold, but that the former group of mothers are more likely to receive time transfers from grandparents than the latter group. That is, mothers associated with grandparents whose age is at or above the early-retirement age threshold are the *treated group*, and the remaining group of mothers is the *control group*. Thus, any observed discontinuity in mothers' labor market outcomes can be attributed to treatment status.

Estimating the effects on the availability of grandparent-provided child care on mothers' labor supply at the Social Security early-retirement age has policy implications for the United States pension system. For example, if grandparents are forced to work longer due to increases in the retirement age, which in turn causes mothers to work less, then pension reforms aimed at making the system more financially viable must account for the intergenerational redistribution of labor and its effect on tax revenues (Rupert and Zanella, 2018). More generally, child care induced labor supply effects are also linked to other policy-relevant economic phenomena, such as the widening of the mother wage gap (Budig and England, 2001), lower fertility rates (Blau and Robins, 1989), and the rising motherhood entry age (Rindfuss et al., 2007). Accordingly, the results stemming from this research are pertinent to the fields of labor and public economics, as well as the sociology and demography disciplines.

In the forthcoming analysis, I show that there is a decrease in grandparents' labor supply at the early-retirement age threshold, although the magnitude and precision of the estimated effects are sensitive to empirical specification. In turn, for the sample under consideration, the early-retirement age threshold has little impact on mothers' labor market outcomes. These findings differ from those of recent literature that use pension-reform induced changes in Italy to find large labor supply effects for mothers whose own mother (maternal grandmother) is pension eligible (Aparicio-Fenoll and Vidal-Fernandez, 2014, Bratti et al., 2016). I discuss the underlying mechanisms driving these null effects in Section 3.6, and organize the rest of the chapter as follows. The relevant features of the United States Social Security system are detailed in Section 3.2. Section 3.3 and Section 3.4 describe the data and outline the identification strategy, respectively, and Section 3.5 presents the results.

## 3.2 Background on Social Security

In the United States, Social Security is a commonly used term for the federal Old-Age, Survivors, and Disability Insurance (OASDI) program, which was established by the original Social Security Act in 1935 to provide retirement income to eligible individuals. Aside from a few minor exceptions, individuals that are eligible to receive retirement benefits can begin receiving them as early as age 62, known as the early-retirement age.[2] Pensioners earning

[2]The minimum eligibility requirement is 40-quarters of employment, which need not be consecutive, in which the gains from work for that quarter meet an annually adjusted minimum (e.g., $1,260 in 2016). Survivor benefits are available to children and spouses, so long as the deceased met the 40-quarter employment

early-retirement income receive smaller payments for the rest of their lives than those who wait to start receiving benefits at their full-retirement age; 65 for those born before 1938, between 65 and 67, depending on an individual's year of birth, for those born between 1938 and 1960, and 67 for those born after 1960. Conversely, individuals can also increase the size of their pension by opting to delay receipt of Social Security benefits up until age 70, after which the size of the monthly benefit payment does not increase. Adjustments to the standard benefit amount in the form of reductions for beginning collection of benefits before the full-retirement age or credits for beginning collection of benefits after the full-retirement age are actuarially fair, such that the expected discounted present value of lifetime benefit payouts do not vary by the age at which the average person begins receiving benefits.

The varying full-retirement ages are a result of federal legislation enacted in 1983, aimed at strengthening Social Security's financing by gradually increasing the full retirement age from 65 to 67. The transition is scheduled to be fully complete by January 1, 2027. While the law change increased the age at which some individuals could receive "full" benefits, it did not affect the early-retirement age. However, since benefits claimed before the full-retirement age are subject to monthly reductions, the rise in the full-retirement age did lower the monthly amount that an early-retirement claimant is eligible to receive. For example, when the full-retirement age was 65, individuals retiring at age 62 received a monthly payment that was 20 percent less than their full-benefit amount, while individuals retiring at age 62 will receive a 30 percent reduction to their full-benefit amount when the full-retirement age reaches 67.

All else equal, monthly benefit amount can best be described as an increasing function of the retirement age between the ages of 62 and 70, and a constant at all other ages. Panel A of Figure 3.1 depicts the relationship between monthly benefit amount and the age at which an individual starts receiving benefits (i.e., retirement age). This hypothetical example considers a standard benefit amount of $1,500 per month and a full-retirement age of 65,

minimum.

which is the full-retirement age for almost all individuals eligible to receive Social Security benefits at the time of the 2000 Decennial Census–the data source used in the analysis for this research.[3] Panel B of Figure 3.1 shows the relationship between the cumulative benefit amount and age, by various retirement ages. No strategy is strictly dominated, and thus the age at which most people retire is an empirical question.

Panels A and B of Figure 3.2 plot the means of the labor force participation rate and Social Security receipt rate, respectively, by age for the non-group quartered five percent sample of the 2000 Decennial Census. Since an individual can stop working before she is eligible to receive retirement benefits or can continue working while receiving benefits, labor force participation rates are greater than zero at all ages within the presented age range. As expected, the greatest changes in both the probability of retirement and the probability of receiving Social Security benefits coincide with the early and full retirement ages, 62 and 65, respectively.

The spike in the retirement hazard rate at age 65, as witnessed in Panel A of Figure 3.2, only emerged around the 1940s, which is when Social Security was first introduced, and has persisted ever since (Costa, 1998). Similarly, the spike at age 62 did not exist prior to the 1960s, when the option for early-retirement was not yet available. Institutional forces other than Social Security, such as the correspondence with Medicare and private pension plan eligibility ages, or its perceived notion as the "normal" age for retirement, also help explain the large drop in labor force participation rates at age 65 (Diamond and Gruber, 1999). On the other hand, liquidity constraints, particularly among low-wealth workers, play a significant role in the retirement behavior at the early-retirement age (Kahn, 1988). Hence, on both theoretical and empirical grounds, it makes the most sense to consider the early-retirement age (i.e., 62) rather than the full-retirement age (i.e., 65) as the threshold

[3] Sixty-two year-olds in the 2000 Decennial Census with an actual birth year of 1938, have a full-retirement age of 65 and two months. This is because the birth year variable in the public-use census file is a crude calculation; it is one year late for all individuals who have a birthday after the date of survey (April 1, 2000).

above which the probability of retirement changes discontinuously primarily due to pension *eligibility* rules.

## 3.3 Data

### 3.3.1 Data Source and Descriptive Statistics

The data set used for the analysis is the public-use five percent weighted sample of the 2000 Decennial Census.[4] The size and scope of the data are their clear strengths. They contain information on each respondent's relation to the householder, sex, age in years, demographic characteristics, educational attainment, labor force and employment status, weeks and hours worked, income, and whether the respondent has any disabilities (e.g., self-care difficulty, independent living difficulty). The rich employment information allows for analysis along both the extensive margin (e.g., labor force participation and employment) and the intensive margin (e.g., weeks and hours worked).[5]

Mothers are the unit of observation in the analysis and all samples are restricted to non-group quartered households that have the mother as the householder or as the spouse or unmarried partner of the householder.[6] Importantly, census data do not link grandparents, mothers, and children across households, and therefore, the full sample of mothers does not condition on the presence of a grandparent within the household. On the other hand, the three-generation sample, which is a subset of the full sample and the sample upon which I perform

[4] All analyses in this research use census sampling weights.

[5] The 2000 Decennial Census collected information on extensive labor supply (i.e., labor force participation and employment) in reference to the four weeks prior to questioning, and information on intensive labor supply (i.e., weeks worked and hours worked) and wage income in reference to the previous calendar year (1999). This means that a respondent may currently be out of the labor force at the time of questioning but also have positive weeks worked, hours worked and wage income from the previous calendar year. Such cases are rare and the main results in the research are robust to this discrepancy.

[6] Including only parent-maintained households ensures that sisters of mothers are not erroneously coded as mothers themselves.

the main analysis, features households with a mother, a child, and a singleton grandparent (i.e., maternal grandmother only, maternal grandfather only, paternal grandmother only, or paternal grandfather only).[7] This sample also excludes households where the grandparent is less than 15 years older than the mother. All samples restrict the youngest child's age to less than five years old and the mother's age from 20 to 40 years old, which is a common restriction within the labor literature (Angrist and Evans, 1998).

Columns (1) and (2) of Table 3.1 report means of mothers' demographic and education characteristics, mothers' labor market outcomes, and households' characteristics, stratified by sample. As seen by comparing the two columns, mothers are near the same age across samples, have the same number of children and also have similar labor market outcomes. Consistent with previous findings, mothers in the three-generation sample are less educated and more likely to be from an ethnic minority (Kamo, 2000, Choi, 2003). Mothers from the three-generation sample are also six percentage points more likely to be the head of the household and five percentage points less likely to reside with a spouse or partner. While family non-mother total income, which is the difference between family income and the mother's wage income, is about ten percent higher within the three-generation sample, it is lower in per-capita terms, since households in the three-generation sample, on average, have nearly 1.5 more members. Columns (3) and (4) of Table 3.1 report means of mothers' spouses' or partners' demographic and education characteristics, and labor market outcomes across the two samples. Like mothers, spouses or partners in the three-generation sample are less educated and more likely to be from an ethnic minority. These statistics suggest that the three-generation sample is not representative of the broader population, the implications of which are discussed further in Section 3.6.

[7]Multi-grandparent households are excluded from the analysis to not bias the estimated effects by assigning more than one grandparent to a mother. See Figure B.3 and tables B.2 - B.3 for results from an alternative sample trimming procedure that relaxes this restriction by considering all grandparents and clusters standard errors at the household level.

### 3.3.2 Data Issues

The disadvantages of using census data are twofold. First, census data do not contain information on the provision of child care, which precludes the testing of whether the mechanism driving any potential labor market effects for mothers can be directly linked to greater child care from grandparents. Second, the public-use sample of the 2000 Decennial Census only contains age information in years, hence it is prone to rounding error. These limitations are directly addressed below.

**No direct measure of child care**–Although it would be preferable to have information on the amount of child care that a grandparent *does* provide, it is also important to consider the amount of child care that a grandparent *is able* to provide. Following Compton and Pollak (2014), availability of grandparental child care is defined to include not only regularly scheduled child care, which is substitutable with child care from an organized child care facility, but also the insurance provided by the grandparent for irregular or unanticipated child care needs, such as caring for a child whose mother is traveling for work or picking up a sick child from school. In the analysis that follows, grandparents' labor supply is used as a proxy for the availability of child care.

**Age in years**–Since age data are recorded in years, and hence discretized, a grandparent that is reported to be 62 in the census can have a true age anywhere between 62 and 63 minus one day. Thus, the age reported in the census, $A$, can be considered the actual age, $A^*$ rounded down to the nearest integer. That is, $A = A^* - \mu$, where $\mu \sim U[0, 1)$, and $\mathbb{E}[A^*] = A + 0.5$. Dong (2015) shows that under the assumption of a correctly specified first-order polynomial in the running variable, re-centering the integer age to the midpoint of the age cell by adding 0.5 to each integer age, corrects the rounding bias caused by using a rounded discrete running variable. Moreover, for any $J^{th}$-order specification, where $J \geq 1$, bias-corrected treatment effects can be attained via a linear combination of the $J$ regression coefficients on the interacted terms. In the forthcoming analysis, and wherever noted in the tables, this bias-corrected technique is applied.

## 3.4 Methodology

### 3.4.1 Model

As a quasi-experimental research design, RD identifies the effect of a treatment when the probability of receiving treatment changes discontinuously due to an underlying running variable exceeding a known threshold. Sharp RD necessitates that the discontinuity in the probability of *treatment receipt* is zero to one at the threshold, whereas fuzzy RD merely requires that the discontinuity in the probability of *treatment assignment* is zero to one at the threshold. Thus, fuzzy RD is equivalent to Instrumental Variables (IV), and accordingly, the local average treatment effect (LATE) may be inferred for compliers–the subpopulation induced into treatment at the threshold (Hahn et al., 2001).

Here, the treatment is grandparents' labor supply, the underlying running variable is grandparents' age, the known threshold is the early-retirement age, and the outcome is mothers' labor market outcomes, which are labor force participation, weeks worked, hours worked, and wage income. Compliers are mothers who are associated with grandparents whose labor supply is affected by the early-retirement age threshold (i.e., grandparents supply less labor if above the threshold or maintain the status quo if below the threshold).

The models in this research use the positioning of grandparents' age relative to the early-retirement age threshold as an instrumental variable for grandparents' labor supply. More formally, the following equations are estimated:

$$Y_i = \beta_0 + \beta_1 X_i + f(AGE_i - 62) + \varepsilon_i \tag{3.1}$$

$$X_i = \alpha_0 + \pi_1 ABOVE_i + g(AGE_i - 62) + \nu_i \tag{3.2}$$

Here, $Y_i$ is the labor market outcome for mother $i$, $X_i$ is the labor supply for the grandparent associated with mother $i$, and $ABOVE_i$ is an indicator for the grandparent being at or above the early-retirement age threshold, which is used as an instrument for the endogenous variable–$X_i$. The functions $f(\cdot)$ and $g(\cdot)$ control for the effects of grandparents' age on mothers' labor market outcomes and grandparents' labor supply, respectively, and $\varepsilon_i$ and $\nu_i$ are both error terms. Outcomes may also depend on other control covariates, which are suppressed for now, but included as robustness checks to the validity of the RD design and to improve the efficiency of the estimated effects.

The key identifying assumption underlying this approach is that $f(\cdot)$ and $g(\cdot)$ are continuous through the early-retirement age threshold, which implies that households associated with grandparents just below the early-retirement age threshold are comparable to households that are associated with grandparents just above the early-retirement age threshold. Thus, any observed discontinuity in $Y$ or $X$ can be attributed to the positioning of grandparents' age relative to the early-retirement age threshold. This assumption would be violated if there is systematic sorting around the early-retirement age threshold or if the means of predetermined covariates changed discontinuously at the early-retirement age threshold. In the absence of these violations and contingent upon the correct specification of $f(\cdot)$ and $g(\cdot)$, the estimate of $\beta_1$ can be interpreted as the LATE of grandparents' labor supply on mothers' labor market outcomes at the early-retirement age threshold.

### 3.4.2 Validity of Design

Figures 3.3 - 3.5 present evidence which indicates that the above strategy will yield valid estimates of the impact of grandparents' labor supply on mothers' labor market outcomes. Figure 3.3 shows the density of grandparents' age for the three-generation sample. Points are the densities of observations within the three-generation sample and lines are fitted values of second-order polynomials with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold. Visual inspection of the graph suggests that a statistically significant but small negative discontinuity exists at the early-retirement age threshold. Applying the methods proposed in McCrary (2008) yields an estimated log discontinuity in the density at the early-retirement age threshold of -0.001, or roughly -0.1 percent, and is statistically significant at the one percent significance level, which implies that there is no manipulation of the running variable within the three-generation sample.

Figure 3.4 plots grandparents' age cell means of mothers' demographic and education characteristics, which are indicator variables for White, Black, Hispanic, other race, less than high school graduate and more than high school graduate, and superimposes a second-order polynomial in the fitted outcomes along with 95 percent confidence intervals estimated separately on either side of the early-retirement threshold. Figure 3.5 plots the same information for households' characteristics, which are indicator variables for grandparents' self-care disability status and mobility disability status, as well as the number of people in the household. The relative smoothness of these figures suggests that within the three-generation sample, grandparents' age near the early-retirement threshold is approximately randomly assigned.

Column (2) of Table 3.2 reports the estimated discontinuities seen within the graphs, which for many of the variables are small and statistically indistinguishable from zero. Column (1) of Table 3.2 shows estimated discontinuities from regressions that control for a fully flexible first-order polynomial in grandparents' age, centered around the early-retirement age threshold, and reveals that except for one variable–grandparents' mobility disability status–statistical significance is not robust to the order of the polynomial degree. However, it is not particularly surprising that there is one statistically significant discontinuity across the two columns. The smoothness of these characteristics is demonstrated most clearly in Table 3.3 which reports estimated discontinuities from regressions where the outcome is the fitted value of mothers' labor market outcome as predicted by indicator variables for mothers' race, mothers' education, and grandparents' disability status, as well as the number of people in the household. All estimated discontinuities are small and statistically different from zero at the five percent significance level. This suggests that these single-index measures which subsume mothers' demographic and education characteristics, and households' characteristics are smooth through the early-retirement age threshold.

**Income effects**–An important feature of this empirical strategy is that it requires the variation in grandparents' labor supply to impact mothers' labor market outcomes via only grandparent-provided child care, and no additional causal pathway, such as income. For example, it may be the case that grandparents' exit from the labor force increases intergenerational time transfers (e.g., provision of child care) at the expense of intergenerational income transfers, thus creating a negative wealth effect that prompts mothers to substitute towards more work. Figure 3.6 shows that households associated with grandparents that are past the early-retirement age have higher levels of family non-mother total income, even though grandparents' average total income on either side of the early-retirement age threshold is relatively constant at about $10,000, which is roughly 90 percent of the full sample mean. The strong positive correlation between family non-mother total income and grandparents' age is largely driven by spouses' or partners' income. However, the positive effects on family non-mother total income at the early-retirement age threshold cannot be attributed to spouses' or partners' labor market outcomes.[8] For completeness, Figure 3.6 also plots the wage income of mothers' sibling(s), when the grandparent is maternal, or sibling(s)-in-law, when the grandparent is paternal.[9] Although, the estimated discontinuity in siblings' (in-laws') wage income matches that of family non-mother total income, Table 3.4 shows that the precision of the estimated effects are not robust to the order of polynomial degree and that

[8]See Figure B.4 for spouse's or partner's labor supply response.

[9]Only working-age siblings or siblings-in-law are considered and the average is used when there is more than one per household.

less than 15 percent of households in the three-generation sample had siblings or siblings-in-law also living in the household. While the absence of a labor supply response from siblings (in-law) (see Figure B.5) further shrouds the source of the positively estimated change in family non-mother total income at the early-retirement age threshold, it also suggests that confounding income effects do not seem to exist within the three-generation sample.

**Polynomial Specification**–Consistent estimation of effects via a regression discontinuity designs requires the correct specification of the functions that control for the running variable, here $f(\cdot)$ and $g(\cdot)$. Following Gelman and Imbens (2019), $f(\cdot)$ and $g(\cdot)$ are modeled as low-order polynomials (linear or quadratic), and the slopes on grandparents' age terms are free to be different on either side of the early-retirement age threshold. To check for the robustness and validity of the RD design, models are estimated both with and without control covariates, which are indicator variables for mothers' age, race, and education, as well as state dummies. The main analysis uses the full range of grandparents' ages but, local-linear methods are also considered.

## 3.5 Results

### 3.5.1 Estimated Effects on Grandparents' Labor Supply

Table 3.5 presents a detailed analysis of grandparents' labor supply effects for being past the early-retirement age threshold, for both the entire three-generation sample (Panel A) and a subset that excludes disabled grandparents (Panel B). This distinction is necessitated by the fact that before reaching the early-retirement age, the Social Security receipt rate for grandparents in the three-generation sample was about two to three percentage points greater than the Social Security receipt rate for the general sample. Presumably, these younger than early-retirement age grandparents were collecting Social Security Disability

Insurance (SSDI)–which is equivalent to an individual's full-retirement amount (i.e., more than the early-retirement amount), and therefore unconstrained by the early-retirement age threshold.[10] As expected, the means of grandparents' labor supply, particularly along the extensive margin, are higher in Panel B, but the economic and statistical significance of the estimated effects are similar across the two panels, and therefore they are discussed simultaneously below.

Column (1) shows estimated discontinuities from regressions that control for fully flexible first-order polynomials in grandparents' age, centered around the early-retirement age threshold, while column (3) shows estimated discontinuities from regressions that control for fully flexible second-order polynomials. Columns (2) and (4) add indicator variables for mothers' age, race and education, as well as state dummies as control covariates to the regressions represented by columns (1) and (3), respectively. The first row shows the effect of being past the early-retirement age threshold on labor force participation. Linear models, both with and without control covariates, suggest discontinuities of around negative six to seven percentage points, while quadratic specifications yield smaller and much less precise estimates. A similar assessment holds for the estimated discontinuities from unconditional employment models, which define the dependent variable as employment that is not conditional on labor force participation. The dependent variable in these regressions takes the value of one if the grandparent is employed and zero otherwise. The final two rows of each panel show that being past the early-retirement age threshold has small and statistically insignificant effects on weeks and hours worked, respectively.

The sensitivity to polynomial degree within labor force participation and unconditional employment models motivates an analysis of these estimated discontinuities using local-linear methods. Figure B.1 presents estimated discontinuities with corresponding 95 percent con-

[10] A more direct approach would have been to exclude grandparents that collect SSDI. However, this is not possible because the census reports information about Social Security income received from retirement and disability pensions collectively. Disability benefits automatically convert to retirement benefits at the full-retirement age.

fidence intervals from linear regressions without control covariates that are estimated separately on either side of the early-retirement age threshold using various bandwidths and a uniform kernel. The coefficients for both labor supply variables and across both panels are negative and statistically distinguishable from zero at bandwidths of 13 or greater. This indicates that restricting grandparents' age from 50 to 74, which is not an overly strict restriction, would yield imprecisely estimated discontinuities at the five percent significance level. Hence, the estimated effects are sensitive to bandwidth choice when local-linear methods are employed

Figure 3.7 depicts the relationship between grandparents' labor supply and grandparents' age normalized around the early-retirement age threshold for the entire three-generation sample. Points are age cell means and lines of best fit along with corresponding 95 percent confidence intervals are from regressions that control for a second-order polynomial in grandparents' age, centered around the early-retirement age threshold that is estimated separately on either side of the threshold. The upper two graphs plot the extensive margin–labor force participation and unconditional employment–while the bottom two graphs plot the intensive margin–weeks and hours worked. Visual inspection of the graphs does not reveal an obvious discontinuity at the early-retirement age threshold, although grandparents that are past the early-retirement age threshold do supply less labor. Moreover, it is clear that the second-order polynomial fits the data fairly well and goodness of fit statistics also suggest that they are preferable to linear specifications.[11]

Although these graphs show that there are no mean changes in weeks and hours worked at the early-retirement age, it is also interesting to see whether there are heterogeneous effects at various parts of the distribution of these continuous variables. One hypothesis might be that grandparents that work fewer weeks or hours do not change their behavior as much

[11] Adjusted R-square and the Akaike information criterion (AIC) statistic both suggest that the regression that includes a quadratic control function and the standard set of control covariates is the preferred specification.

as those that are employed on a full-time basis. Figure 3.8 plots the cumulative distributions of grandparents' labor supply, from the entire three-generation sample, separated by grandparents' age. Panel A depicts the distribution of weeks worked for 62-year-old grandparents, relative to 61 and 63-year-old grandparents, receptively. Similarly, Panel B depicts the distribution of hours worked for 62-year-old grandparents, relative to 61 and 63-year-old grandparents, respectively. The graphs show that the distributions of intensive margin labor supply for grandparents just below (i.e., age 61) and just above the early-retirement age (i.e., age 63), closely outline the distribution of grandparents' labor supply at the early-retirement age (i.e., age 62), with working grandparents continuing to work primarily at full-time levels regardless of age.[12]

### 3.5.2 Estimated Effects on Mothers' Labor Market Outcomes

Table 3.6 reports the estimated discontinuities on mothers' labor market outcomes. As before, column (1) shows estimated discontinuities from regressions that control for fully flexible first-order polynomials in grandparents' age, centered around the early-retirement age threshold, while column (3) shows estimated discontinuities from regressions that control for fully flexible second-order polynomials. Columns (2) and (4) add the standard set of control covariates to the regressions represented by columns (1) and (3), respectively. All estimates presented in the table are small, statistically insignificant, and robust across specifications. These findings suggest that the *intent-to-treat* effects of grandparents' retirement eligibility on mothers' labor market outcomes are largely insufficient.

Figure 3.9 shows the relationship between mothers' labor market outcomes and grandparents' age normalized around the early-retirement age threshold. As before, points are age cell means and lines of best fit along with corresponding 95 percent confidence intervals are from regressions that control for a second-order polynomial in grandparents' age, centered

[12] A similar assessment also holds for other ages near the early-retirement age.

around the early-retirement age threshold that is estimated separately on either side of the threshold. The dependent variables under consideration are labor force participation, weeks worked, hours worked, and wage income. Two features of the graphs are worth noting. First, there is no indication of a clear jump in any labor market outcome for mothers who are associated with grandparents near the early-retirement age threshold. Second, an inspection of the y-axes reveals that mothers' labor supply is of a reasonable magnitude, which is not surprising since the results in Table 3.1 showed that mothers within the three-generation sample have similar labor market outcomes as mothers within the full sample.

Table 3.7 reports the instrumental variables estimates of grandparents' labor supply on mothers' labor market outcomes. These estimates are based on two-stage least squares models that include the standard set of control covariates and instrument grandparents' labor force participation with a dummy for the grandparent being past the early-retirement age threshold.[13] Columns (1) and (3) show the estimated effects from outcome equations that control for first-order polynomials in grandparents' age, centered around the early-retirement age threshold, while columns (2) and (4) show estimated effects from outcome equations that control for second-order polynomials. Column (1) and (2) fully interact grandparents' age polynomials with the instrument, and column (1) can be interpreted as the ratio of the estimates in column (2) of Table 3.6, to the labor force participation estimates in column (2) of Table 3.5. Similarly, column (2) can be interpreted as the ratio of the estimates in column (4) of Table 3.6, to the labor force participation estimates in column (4) of Table 3.5. For comparison, columns (3) and (4) report estimates from restricted polynomial specifications, which constrain the slopes on grandparents' age polynomials to be equal on either side of the early-retirement age threshold. Clearly, the estimated effects in column (2) are too large to be taken credibly, and while the remaining estimates are also sizable and imprecisely estimated, they are robust across specifications.

[13] Instrumenting for grandparents' unconditional employment produces similar results.

To produce more tenable results, two alternative strategies are considered. Following Clark and Del Bono (2016), the first approach uses non-linear fit instrumental variables, which accounts for the dual drops in grandparents' labor force participation at age 62 and 65, respectively. More specifically, grandparents' labor force participation is instrumented with a dummy variable that takes the value of one if grandparents' age is in the interval [62,65) and zero otherwise, a dummy variable that takes the value of one if grandparents' age is at least 65 and zero otherwise, and interactions of those dummy variables and grandparents' age. Table B.1 formally defines the first-stage equation of the non-linear fit instrumental variables procedure and reports the two-stage least-squares estimates of grandparents' labor force participation on mothers' labor market outcomes from models that include the standard set of control covariates. Graphs that superimpose the fitted values from regressions that predict grandparents' labor force participation by controlling for the excluded instruments, as well as linear and quadratic polynomials in grandparents' age, respectively, are also presented in Figure B.2. The magnitude and precision of this set of two-stage least square estimates appear to be comparable to earlier ones. Moreover, the relatively small f-statistics for tests of the hypothesis that the excluded instruments have no explanatory power on grandparents' labor force participation indicate that the non-linear fit instrumental variables procedure does not produce stronger first-stage results than the general approach. Taken together, these findings suggest that no additional information can be gleaned from this alternative strategy.

The second approach uses an alternative sample trimming procedure to construct the three-generation sample by relaxing the singleton grandparent restriction. Grandparents replace mothers as the unit of observation in the analysis, which makes it possible that the same mother may appear more than once in the sample, increasing the sample size to 17,566. This helps exploit all of the information within three-generation households, and standard errors are clustered at the household level to account for the possibility of intra-household correlation. Tables B.2 and B.3 present grandparents' labor supply effects for being past the early-retirement age threshold and the instrumental variable estimates of grandparents' labor force participation on mothers' labor market outcomes, respectively. The only precisely estimated coefficients are the discontinuities on grandparents' labor force participation and unconditional employment from linear models. Moreover, as seen in Figure B.3, the estimated discontinuities from linear regressions for the extensive margin labor supply variables are imprecisely estimated when local-linear methods are used and the assessments of the estimated effects are similar to before. Thus, this alternative approach is not put into practice because it does not offer an advantage over the standard sample trimming procedure.

## 3.6 Conclusion

Grandparents are a commonly used source of child care for working mothers with young children (Francese, 2011). Estimates from the Survey of Income and Program Participation (SIPP) show that in 2011, about a quarter of all children under the age of five that lived with employed mothers received primary child care from a grandparent (Laughlin, 2013). This arrangement is even more typical for single-mothers, lower-income parents, and families with strong inter-generational ties (Bengtson and Roberts, 1991, Casper et al., 1994). The preceding analysis showed that three-generation households have fewer economic resources (e.g., income and education) and are disproportionately represented by minorities–conceivably due to greater family cohesion driven by cultural norms (Hawkins and Eggebeen, 1991). Hence, *a priori*, it seems justifiable that the interplay of labor supply between the two older generations is greatest for the sample of mothers considered in this research.

The results across a variety of specifications do not bear out this prediction. While the unresponsiveness to the Social Security early-retirement age threshold for these lower-income grandparents, who are presumably in poorer health and more liquidity constrained, may seem puzzling at first, it is important to remember that the estimated effects in this research do not speak to a potential intergenerational redistribution of labor at any age other than the early-retirement age. Thus, it may still be the case that, in general, co-residing grandparents' presence has a positive causal effect through the child care pathway, and that mothers' labor market outcomes would suffer in grandparents' absence. Compared to the general population, grandparents within the three-generation sample work substantially less at all ages before the early-retirement age and are less likely to receive Social Security benefits at all ages after the threshold. This suggests that within the three-generation sample, the early-retirement age threshold is a relatively inconsequential constraint on grandparents' labor supply and that the three-generation sample is largely comprised of *always-takers*–mothers associated with grandparents who supply less labor regardless of grandparents' positioning relative to the early-retirement age threshold.[14]

Two other forces may be contributing to the imprecisely estimated effects of grandparents being past the early-retirement age on own labor supply and mothers' labor market outcomes. First, the United States may not serve as the best setting for studying this question. As noted in Section 3.2, although there are increases in the hazard rate out of the labor force at age 62 and age 65, the adjustments to Social Security benefits between ages 62 and 70 dampens the magnitude of these spikes. This is in stark contrast to other Western countries that offer more generous early-retirement options, or Asian countries that enforce *de facto* (Japan) or *de jure* (China) mandatory retirement ages. Second, measurement (rounding) error in the running variable may result in attenuation bias. Replicating these analyses on restricted use census data, which include the month of birth data, may potentially allay the incapacitating effects of a mismeasured running variable, but will not address the issues of non-compliance among the three-generation sample. And while results from a similar research design on other data sets like the Panel Study of Income Dynamics (PSID) or the National Longitudinal Survey of Youth 1997 (NLSY97), which link families across households, might be more generalizable,

[14] Assuming monotonicity and grandparents' labor force participation as the treatment, then the share of always-takers to compliers to never-takers is roughly 54 to 31 to 15 (Imbens and Wooldridge, 2007).

their interpretation will be limited by the institutional features of the United States Social Security system.

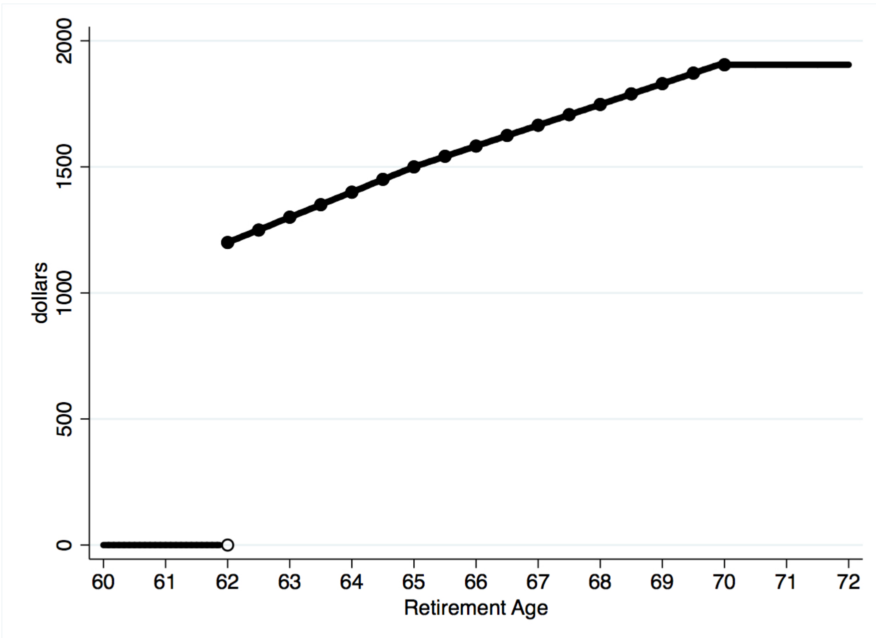
The findings from this research are not necessarily generalizable for the entire population of the United States, as less than three percent of all households are three-generation households. However, studying the effects of grandparent-provided child care on mothers' labor market outcomes is only pertinent for a group of mothers that can take advantage of the provision of child care from a grandparent. To illustrate this point, consider the case of a mother that lives on one side of the country while her mother lives on the other. Aside from allowing for more frequent temporary visits, it is hard to imagine how the grandmother's labor supply could impact the amount of child care that she provides the mother. Although the sample used in this research (i.e., co-residing mothers and grandparents) is the other extreme case of this example, it is still important to remain mindful of the distinction.[15] Accordingly, future reforms aimed at making pension systems more financially viable by modifying pension eligibility rules might still account for the intergenerational redistribution of labor and its effect on tax revenues for the general population.

[15] Compton and Pollak (2015) report that the median distance between married women and their mothers is 20 miles.

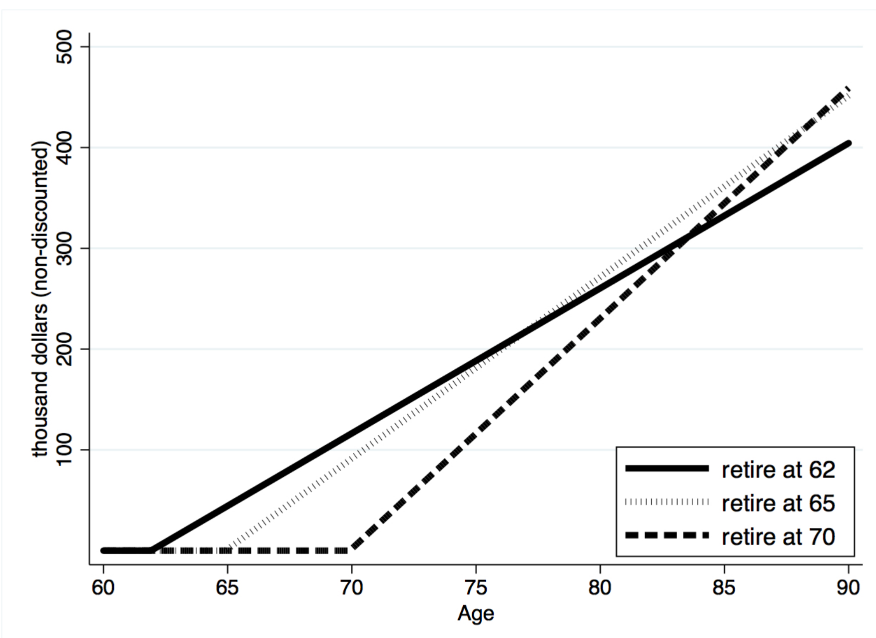
## 3.7 Figures

Figure 3.1: Social Security

Panel A: Monthly Benefit Amount



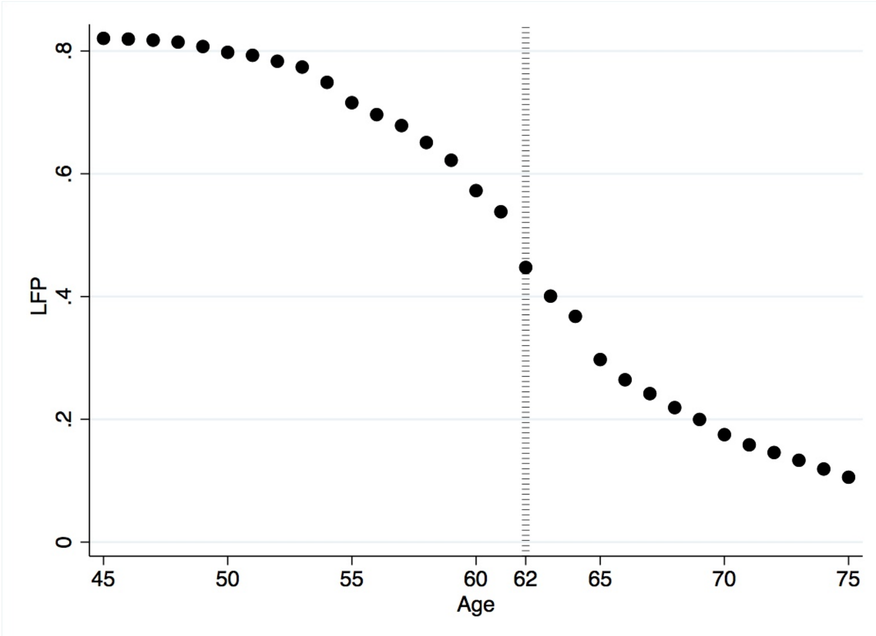

Panel B: Cumulative Benefit Amount



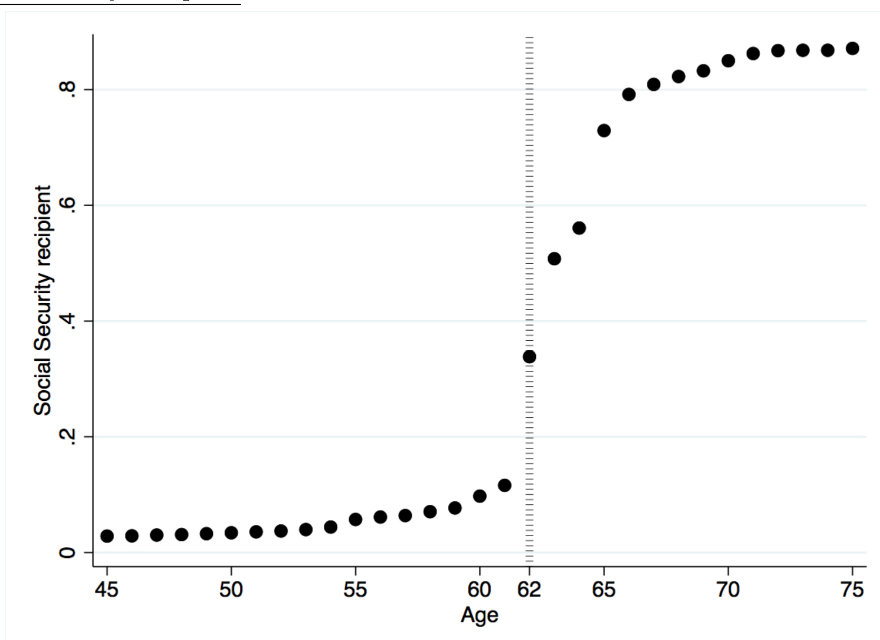

NOTE - Panel A depicts the relationship between monthly benefit amount and the age at which a recipient starts receiving benefits (i.e., retirement age). Example is from a standard benefit amount of $1,500 per month and a full-retirement age of 65. Panel B depicts the relationship between cumulative benefit amount and age, by various retirement ages.

Figure 3.2: Population Means

Panel A: Labor Force Participation

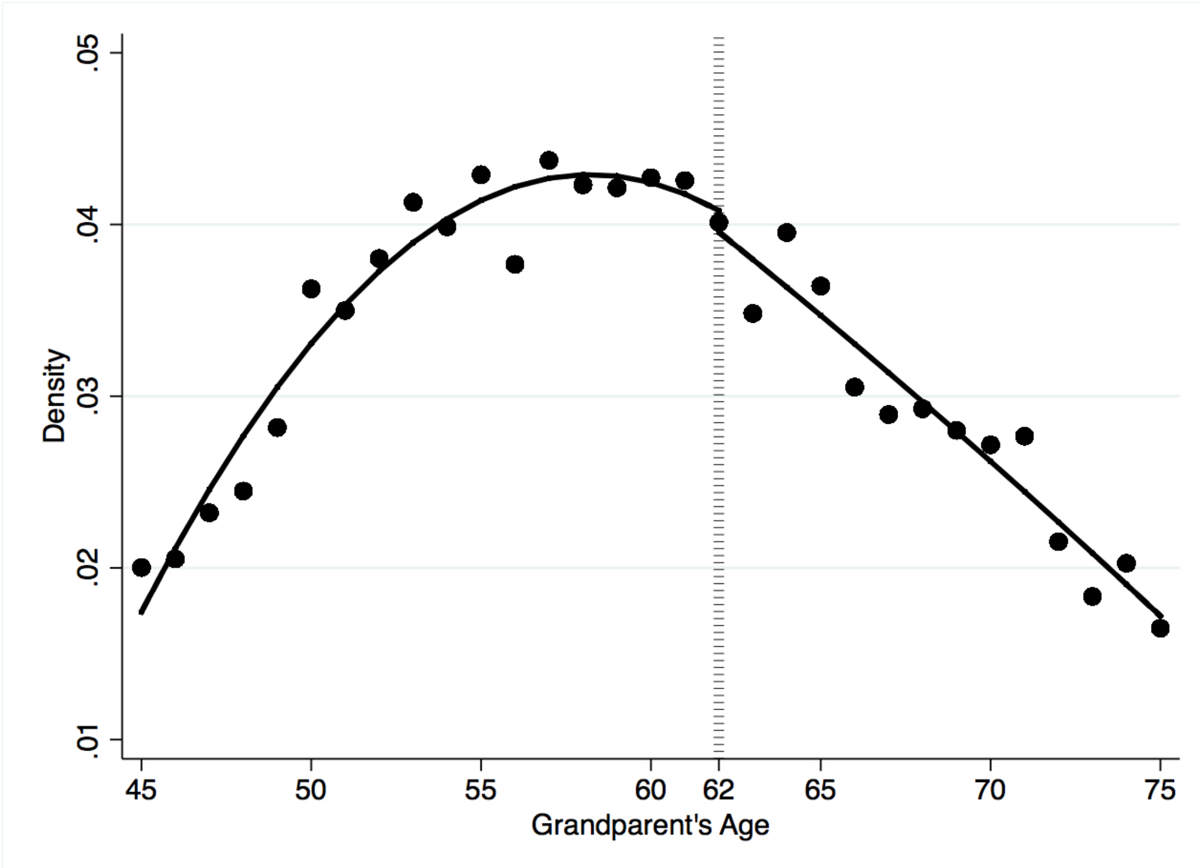



Panel B: Social Security Recipient



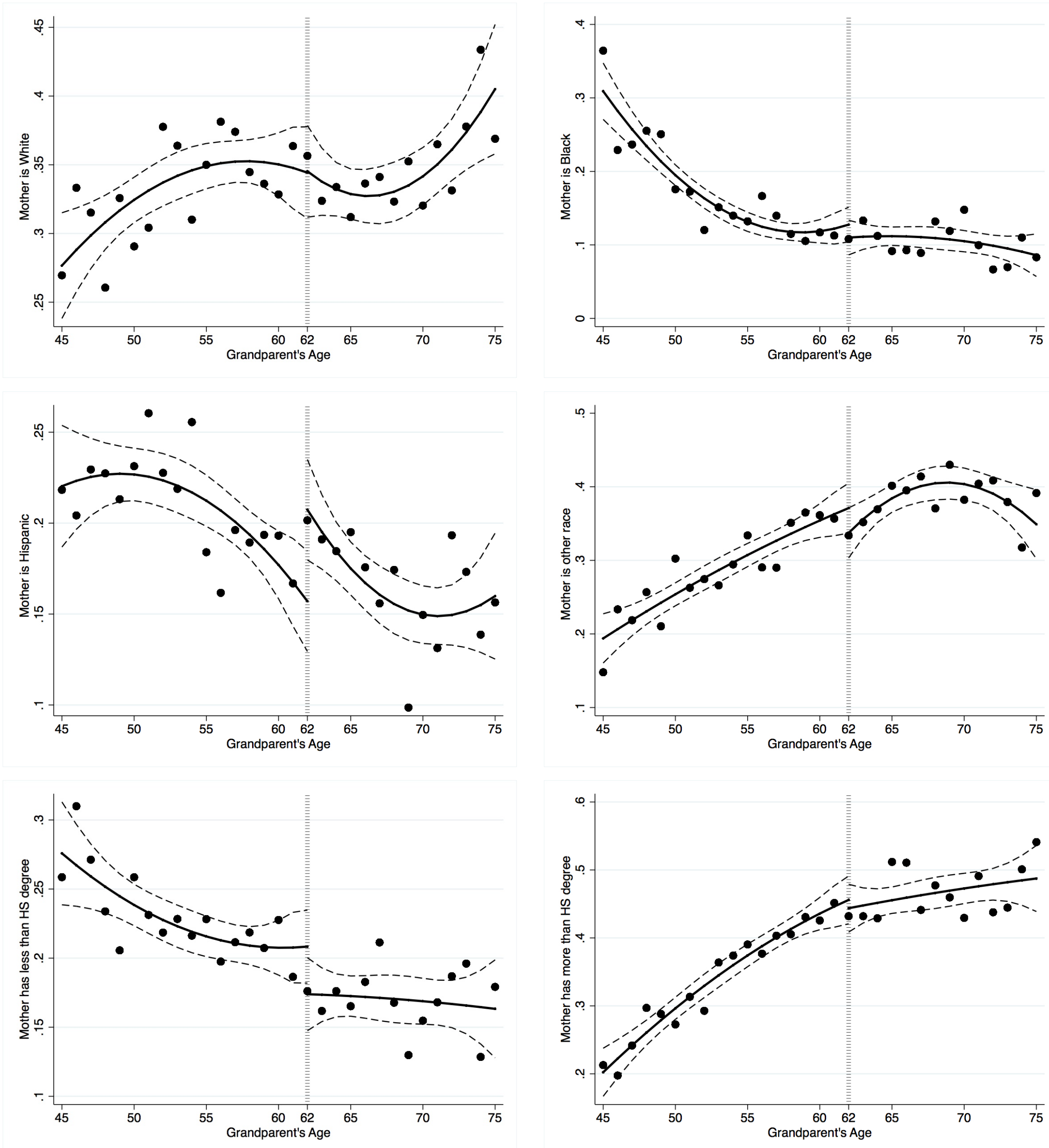

NOTE - Points are mean values for the labor force participation rate in Panel A and Social Security receipt rate in Panel B, in yearly age bins of grandparents' age between 45 and 75 years old. Sample includes non-group quartered respondents of the 2000 Decennial Census.

Figure 3.3: Density



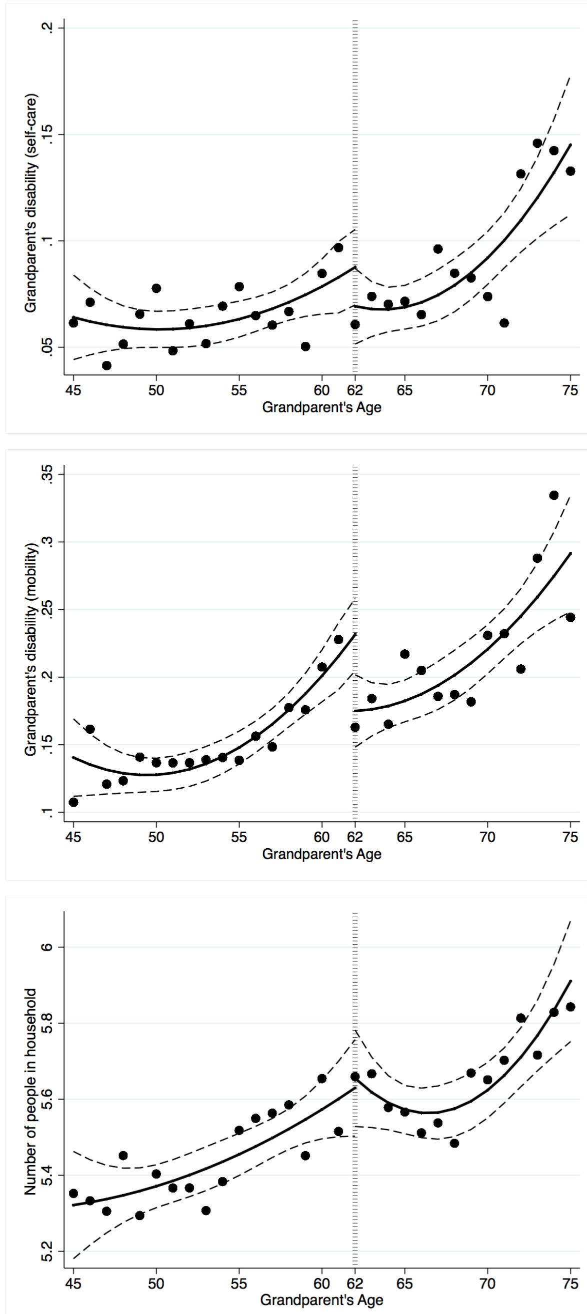

NOTE - Points are the densities of observations within the three-generation sample in yearly age bins of grandparents' age between 45 and 75 years old. Lines are fitted values of second-order polynomials with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

Figure 3.4: Covariate Smoothness



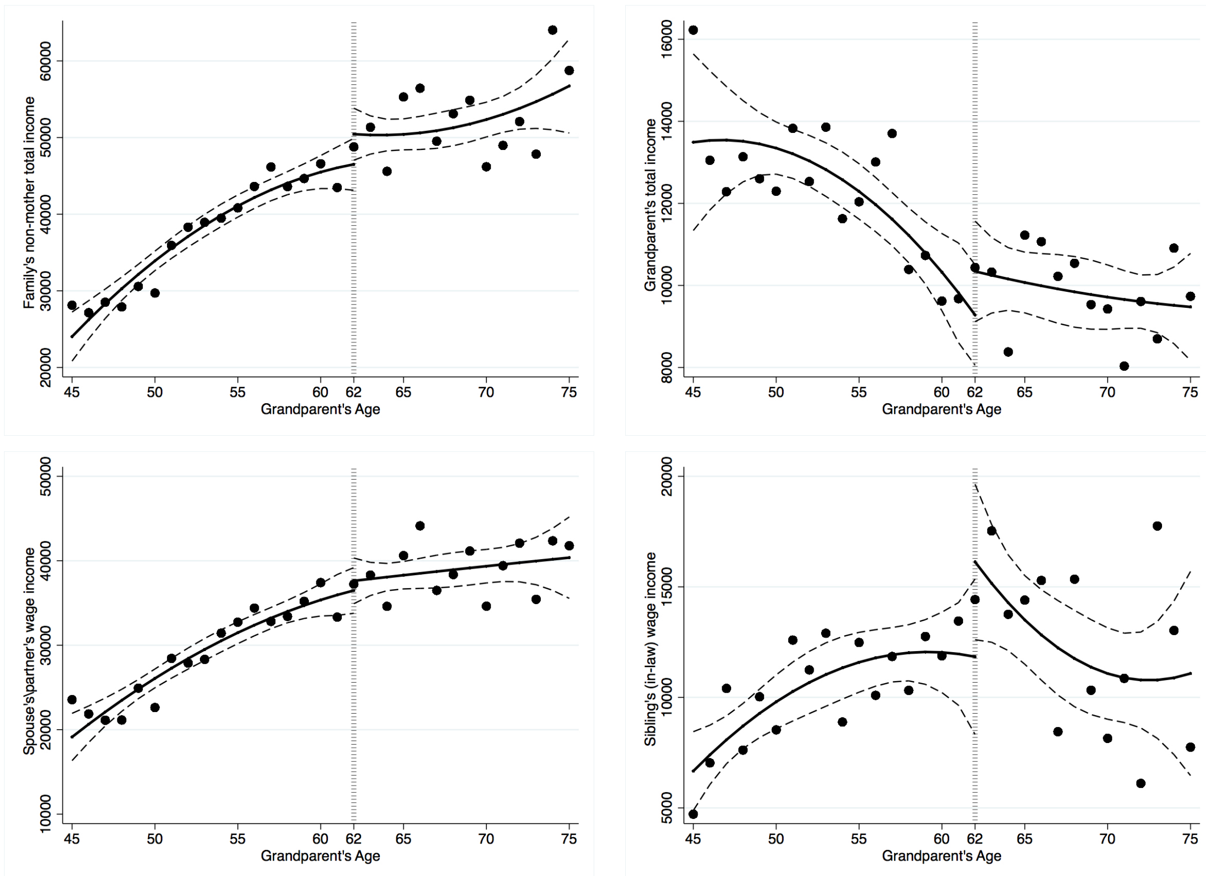

NOTE - Points are mean values for mothers' demographic and education characteristics in yearly age bins of grandparents' age between 45 and 75 years old. Lines are fitted values of second-order polynomials with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

Figure 3.5: Grandparents' Disability and Household Size



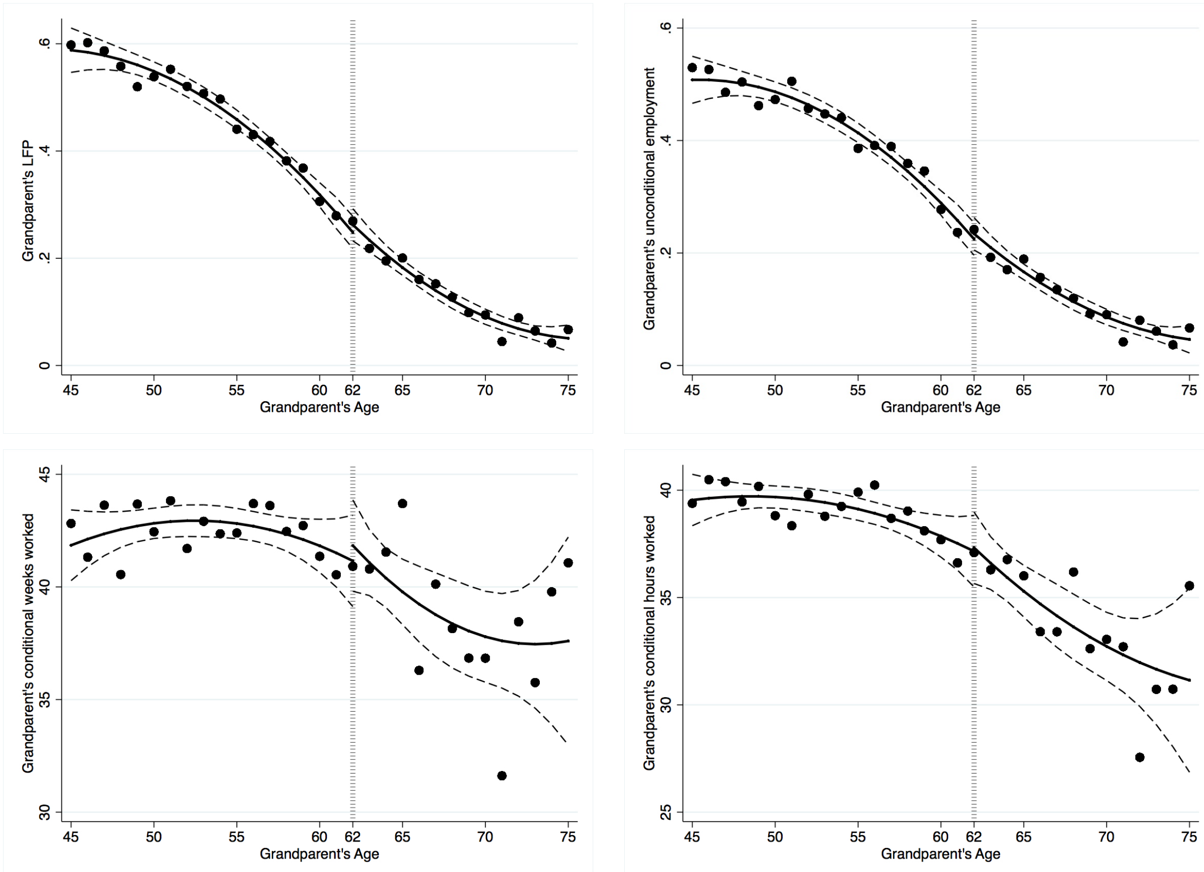

NOTE - Points are mean values for households' characteristics in yearly age bins of grandparents' age between 45 and 75 years old. Lines are fitted values of second-order polynomials with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

Figure 3.6: Income Effects




NOTE - Points are mean values for households' characteristics in yearly age bins of grandparents' age between 45 and 75 years old. Lines are fitted values of second-order polynomials with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

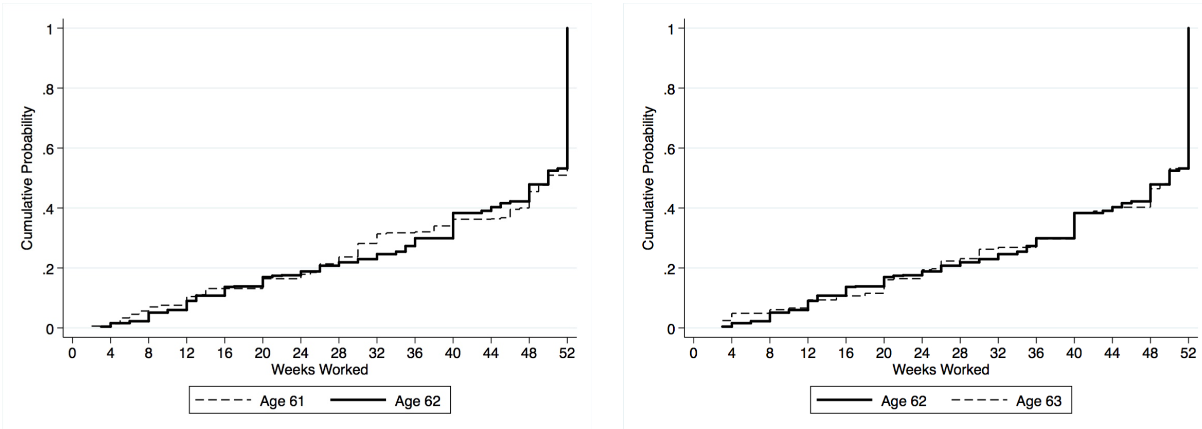
Figure 3.7: Grandparents' Labor Supply



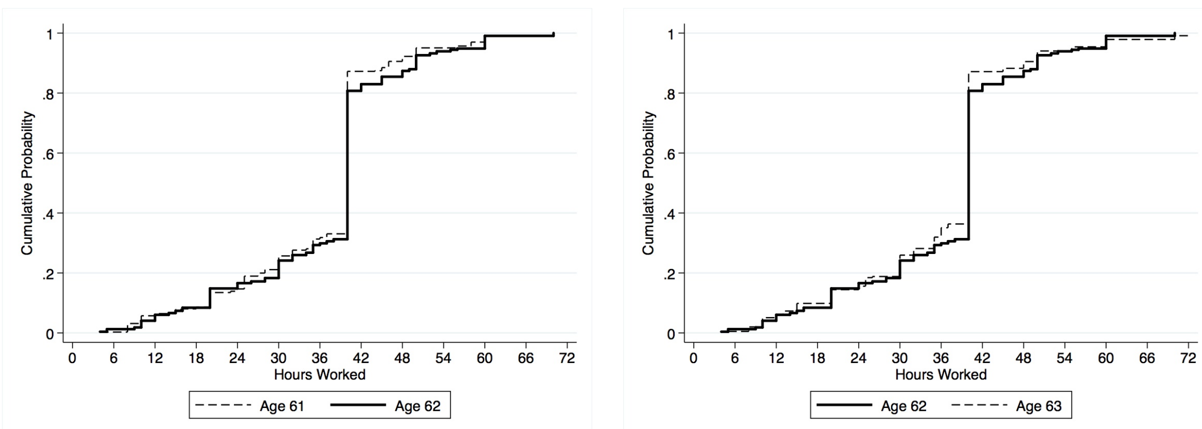

NOTE - Points are mean values for grandparents' labor supply in yearly age bins of grandparents' age between 45 and 75 years old. Lines are fitted values of second-order polynomials with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

Figure 3.8: Cumulative Distributions of Grandparents' Labor Supply

Panel A: Weeks Worked

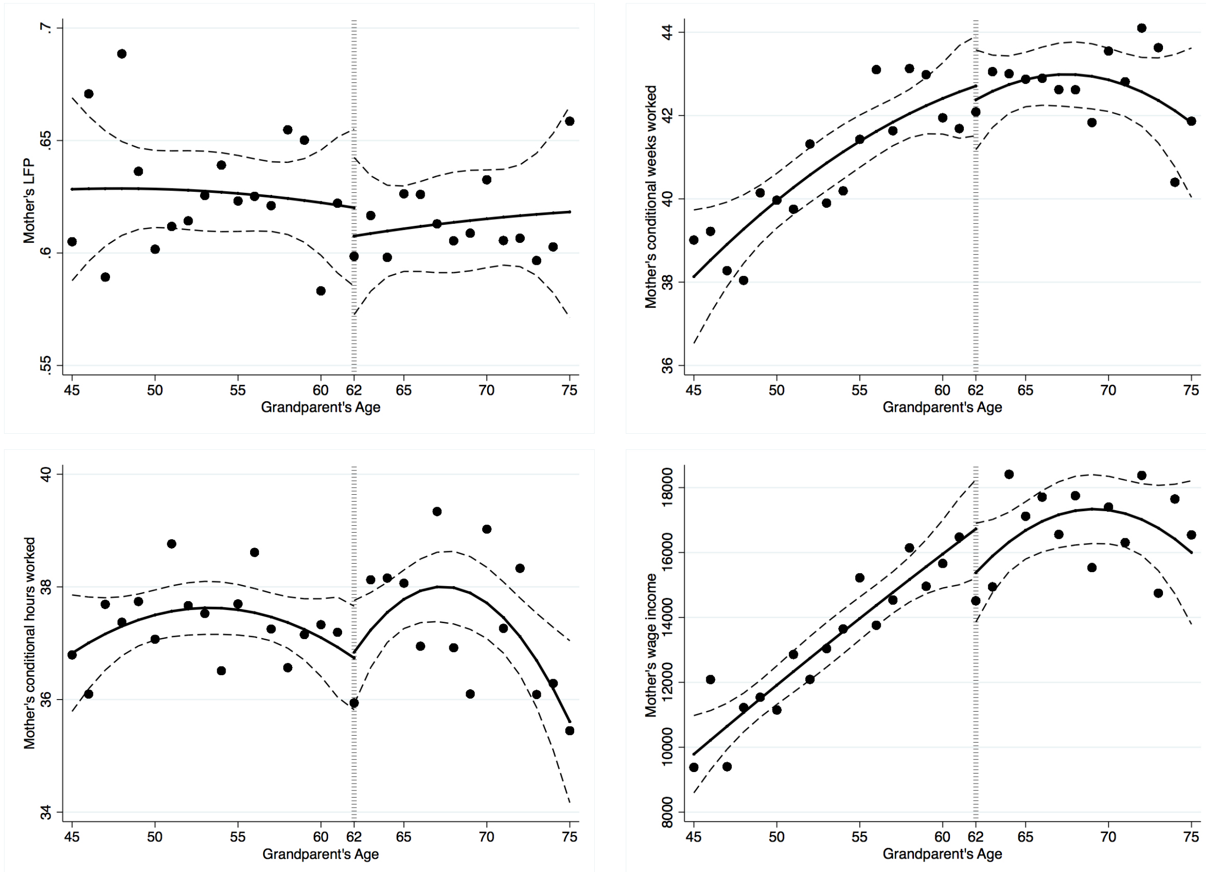



Panel B: Hours Worked




NOTE - Cumulative distributions of grandparents' labor supply, separated by grandparents' age. Panel A depicts the distribution of weeks worked for 62-year-old grandparents, relative to 61 and 63-year-old grandparents, receptively. Panel B depicts the distribution of hours worked for 62-year-old grandparents, relative to 61 and 63-year-old grandparents, respectively.

Figure 3.9: Mothers' Labor Market Outcomes




NOTE - Points are mean values for mothers' labor market outcomes in yearly age bins of grandparents' age between 45 and 75 years old. Lines are fitted values of second-order polynomials with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

## 3.8 Tables

Table 3.1: Descriptive Statistics

| | Mother | | Spouse/Partner | |
|---|---|---|---|---|
| | Full Sample | Three-Generation Sample | Full Sample | Three-Generation Sample |
| | (1) | (2) | (3) | (4) |
| Age | 30.4 | 30.4 | 33.2 | 33.5 |
| | (5.2) | (5.0) | (6.2) | (5.9) |
| Householder | 0.269 | 0.331 | 0.731 | 0.669 |
| White | 0.648 | 0.339 | 0.683 | 0.365 |
| Black | 0.117 | 0.139 | 0.083 | 0.093 |
| Hispanic | 0.09 | 0.192 | 0.091 | 0.197 |
| Other Race | 0.146 | 0.329 | 0.143 | 0.346 |
| Less than HS Degree | 0.123 | 0.203 | 0.125 | 0.218 |
| More than HS Degree | 0.501 | 0.398 | 0.505 | 0.406 |
| LFP | 0.611 | 0.621 | 0.912 | 0.85 |
| Weeks Worked | 41.5 | 41.7 | 48.7 | 47.6 |
| | (14.9) | (18.9) | (8.6) | (9.8) |
| Hours Worked | 35.6 | 37.4 | 45.5 | 44.6 |
| | (11.6) | (11.1) | (10.5) | (10.9) |
| Wage Income ($) | 14,723 | 14,791 | 41,074 | 34,135 |
| | (22,507) | (21,150) | (46,127) | (37,052) |
| Number of Children | 2.1 | 2.2 | - | - |
| in Household | (1.1) | (1.2) | - | - |
| Number of People | 4.1 | 5.5 | - | - |
| in Household | (1.3) | (1.6) | - | - |
| Family Non-Mother | 39,392 | 43,979 | - | - |
| Total Income ($) | (50,937) | (47,356) | - | - |
| Grandparent | - | 11,274 | - | - |
| Total Income ($) | - | (18,747) | - | - |
| Sample Size | 576,405 | 11,889 | 496,208 | 9,634 |

NOTE - Means and standard deviations (reported in parentheses) of mothers' demographic and education characteristics, mothers' labor market outcomes, and households' characteristics stratified by sample. Full sample includes non-group quartered households that feature a mother aged 20-40, with a child under the age of 5, and where the mother is the spouse or unmarried partner of the householder, or the householder herself. Three-generation sample restricts the full sample to households with a single grandparent aged 45-75 that is at least 15 years older than the mother. Computed using census sampling weights.

Table 3.2: Estimated Discontinuities on Mothers' and Households' Characteristics

| | mean | (1) | (2) |
|---|---|---|---|
| Mother is White | 0.364 | -0.044** | 0.004 |
| | | (0.018) | (0.026) |
| Mother is Black | 0.113 | 0.035*** | -0.015 |
| | | (0.013) | (0.019) |
| Mother is Hispanic | 0.167 | 0.014 | 0.052** |
| | | (0.015) | (0.021) |
| Mother is Other Race | 0.357 | -0.005 | -0.040 |
| | | (0.018) | (0.026) |
| Mother has Less than HS Degree | 0.186 | -0.020 | -0.034 |
| | | (0.015) | (0.022) |
| Mother has More than HS Degree | 0.451 | -0.022 | -0.009 |
| | | (0.019) | (0.027) |
| Grandparent Disability (Self-Care) | 0.097 | -0.022** | -0.015 |
| | | (0.010) | (0.014) |
| Grandparent Disability (Mobility) | 0.228 | -0.038*** | -0.048** |
| | | (0.015) | (0.021) |
| Number of People in Household | 5.5 | -0.038 | 0.062 |
| | | (0.063) | (0.095) |
| Polynomial Degree | | 1 | 2 |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is mothers' demographic or education characteristics, or households' characteristics. Regressions control for linear (column (1)) or quadratic (column (2)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61). Means are for observations just to the left of the early-retirement age threshold. Estimates are bias-corrected, following Dong (2015), and standard errors, which are reported in parentheses, are computed using the delta method. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 3.3: Estimated Discontinuities on Mothers' "Predicted" Labor Market Outcomes

| | mean | (1) | (2) |
|---|---|---|---|
| LFP | 0.622 | 0.007 | 0.003 |
| | | (0.005) | (0.007) |
| Weeks Worked | 41.7 | -0.071 | -0.047 |
| | | (0.112) | (0.164) |
| Hours Worked | 37.2 | 0.111** | -0.076 |
| | | (0.052) | (0.076) |
| Wage Income ($) | 16,475 | 2 | -53 |
| | | (242) | (354) |
| Polynomial Degree | | 1 | 2 |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is the fitted value of each labor market outcome as predicted by indicator variables for mothers' race, mothers' education and grandparents' disability status, as well as the number of people in the household. Regressions control for linear (column (1)) or quadratic (column (2)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61). Means are for observations just to the left of the early-retirement age threshold. Estimates are bias-corrected, following Dong (2015), and standard errors, which are reported in parentheses, are computed using the delta method. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 3.4: Income Effects

| | mean | (1) | (2) |
|---|---|---|---|
| Family Non-Mother | 43,483 | 571 | 4,222* |
| Total Income ($) | | (1,834) | (2,557) |
| Spouse/Partner | 33,331 | 120 | 1,278 |
| Wage Income ($) | | (1,508) | (2,139) |
| Grandparent | 9,675 | -3 | 835 |
| Total Income ($) | | (720) | (1,011) |
| Sibling (In-Law) | 13,454 | 2,250 | 4,734* |
| Wage Income ($) | | (1,733) | (2,462) |
| Polynomial Degree | | 1 | 2 |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is non-mothers' income. Regressions control for linear (column (1)) or quadratic (column (2)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61). Means are for observations just to the left of the early-retirement age threshold. Estimates are bias-corrected, following Dong (2015), and standard errors, which are reported in parentheses, are computed using the delta method. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 3.5: Estimated Discontinuities on Grandparents' Labor Supply

| | mean | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| Panel A: Entire Three-Generation Sample | | | | | |
| LFP | 0.279 | -0.060*** | -0.060*** | 0.011 | 0.009 |
| | | (0.016) | (0.016) | (0.024) | (0.024) |
| Unconditional Employment | 0.237 | -0.061*** | -0.060*** | 0.005 | 0.002 |
| | | (0.016) | 0.016) | (0.023) | (0.023) |
| Weeks Worked | 40.5 | -0.744 | -1.083 | 0.900 | 0.628 |
| | | (1.045) | (1.049) | (1.515) | (1.514) |
| Hours Worked | 36.6 | -0.721 | -0.831 | 0.345 | 0.420 |
| | | (0.882) | (0.875) | (1.237) | (1.214) |
| Panel B: Excluding Disabled Grandparents | | | | | |
| LFP | 0.323 | -0.066*** | -0.068*** | 0.004 | -0.001 |
| | | (0.019) | (0.019) | (0.028) | (0.028) |
| Unconditional Employment | 0.279 | -0.070*** | -0.071*** | -0.007 | -0.013 |
| | | (0.019) | (0.018) | (0.028) | (0.027) |
| Weeks Worked | 41.6 | -1.283 | -1.753 | 0.602 | 0.055 |
| | | (1.072) | (1.073) | (1.537) | (1.536) |
| Hours Worked | 36.2 | -0.208 | -0.340 | 0.608 | 0.703 |
| | | (0.921) | (0.912) | (1.277) | (1.259) |
| Polynomial Degree | | 1 | 1 | 2 | 2 |
| Control Covariates | | | ✓ | | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is grandparents' labor supply. Panel A uses the full three-generation sample, and Panel B uses a restricted sample, which is a subset of the three-generation sample that excludes disabled grandparents. Regressions control for linear (columns (1) and (2)) or quadratic (column (3) and (4)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61). Control covariates are also included in specifications represented by columns (2) and (4). Control covariates are indicator variables for mothers' age, race and education, as well as state dummies. Means are for observations just to the left of the early-retirement age threshold. Estimates are bias-corrected, following Dong (2015), and standard errors, which are reported in parentheses, are computed using the delta method. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 3.6: Estimated Discontinuities on Mothers' Labor Market Outcomes

| | mean | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| LFP | 0.622 | -0.015 | -0.017 | -0.014 | -0.016 |
| | | (0.018) | (0.018) | (0.027) | (0.026) |
| Weeks Worked | 41.7 | -0.149 | 0.321 | -0.378 | -0.529 |
| | | (0.649) | (0.643) | (0.965) | (0.959) |
| Hours Worked | 37.2 | 0.412 | 0.329 | -0.224 | -0.278 |
| | | (0.506) | (0.502) | (0.753) | (0.742) |
| Wage Income ($) | 16,475 | -441 | -337 | -1,437 | -1,616 |
| | | (814) | (774) | (1,186) | (1,124) |
| Polynomial Degree | | 1 | 1 | 2 | 2 |
| Control Covariates | | | ✓ | | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is mothers' labor market outcomes. Regressions control for linear (columns (1) and (2)) or quadratic (columns (3) and (4)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61). Control covariates are also included in specifications represented by columns (2) and (4). Control covariates are indicator variables for mothers' age, race and education, as well as state dummies. Means are for observations just to the left of the early-retirement age threshold. Estimates are bias-corrected, following Dong (2015), and standard errors, which are reported in parentheses, are computed using the delta method. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table 3.7: Effect of Grandparents' Labor Supply on Mothers' Labor Market Outcomes

| | mean | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| LFP | 0.622 | 0.253 | -1.161 | 0.202 | 0.305 |
| | | (0.305) | (3.185) | (0.308) | (0.323) |
| Weeks Worked | 41.7 | -5.015 | -47.568 | -3.626 | -6.597 |
| | | (11.619) | (175.901) | (12.279) | (12.057) |
| Hours Worked | 37.2 | -5.607 | 4.899 | -5.233 | -7.987 |
| | | (9.111) | (78.633) | (9.614) | (9.652) |
| Wage Income ($) | 16,475 | 6,092 | -115,831 | 6,985 | 4,938 |
| | | (12,966) | (249,678) | (12,994) | (13,871) |
| Polynomial Degree | | 1 | 2 | 1 | 2 |
| Control Covariates | | ✓ | ✓ | ✓ | ✓ |
| Control Specification | | Unrestricted | Unrestricted | Restricted | Restricted |

NOTE - Estimates are from two-stage least squares models that instrument grandparents' labor force participation with a dummy for the grandparent being past the early-retirement age threshold, and where the outcome is mothers' labor market outcomes. Regressions control for linear (columns (1) and (3)) or quadratic (columns (2) and (4)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61) for specifications represented by columns (1) and (2). Control covariates, which are included in all specifications, are indicator variables for mothers' age, race and education, as well as state dummies. Means are for observations just to the left of the early-retirement age threshold. Heteroscedastic robust standard errors are reported in parentheses. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

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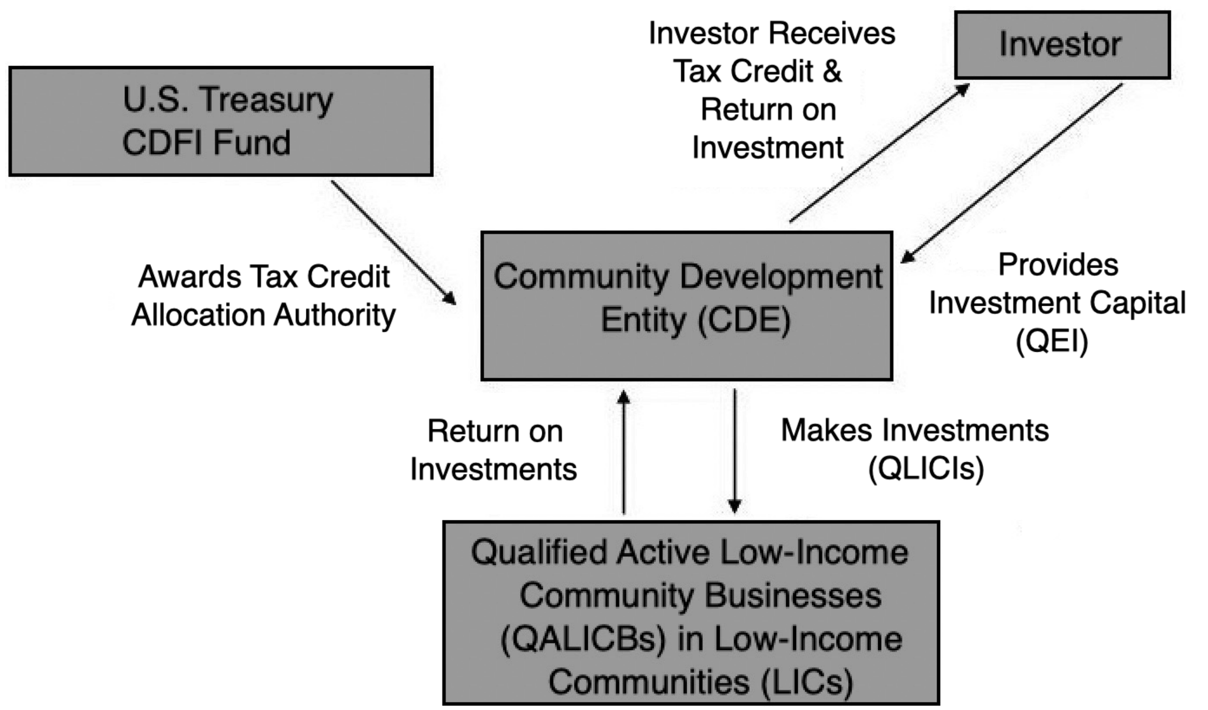
# Appendix A

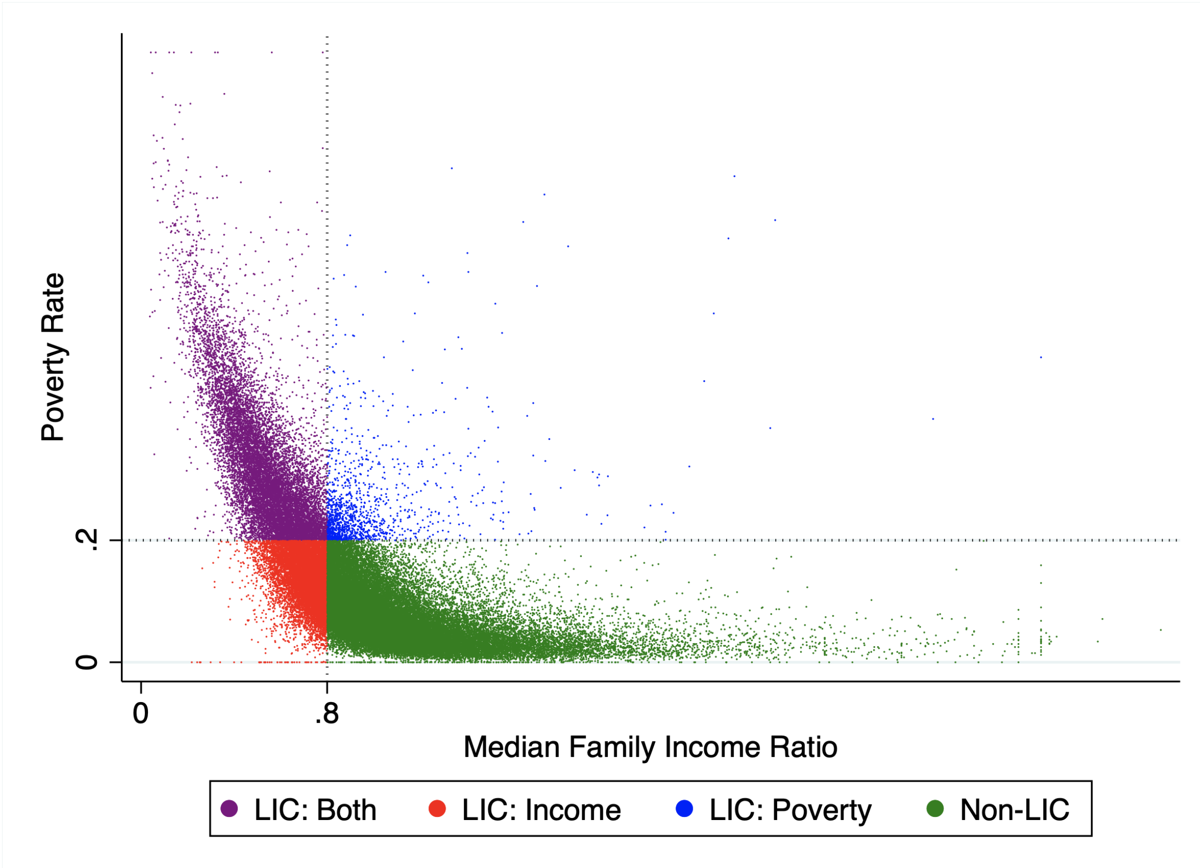
# Supplementary Figures and Table for Evaluating the Incidence of Place-Based Policies on Land Values: Evidence from the New Markets Tax Credit Program

Figure A.1: Overview of the New Markets Tax Credit Program



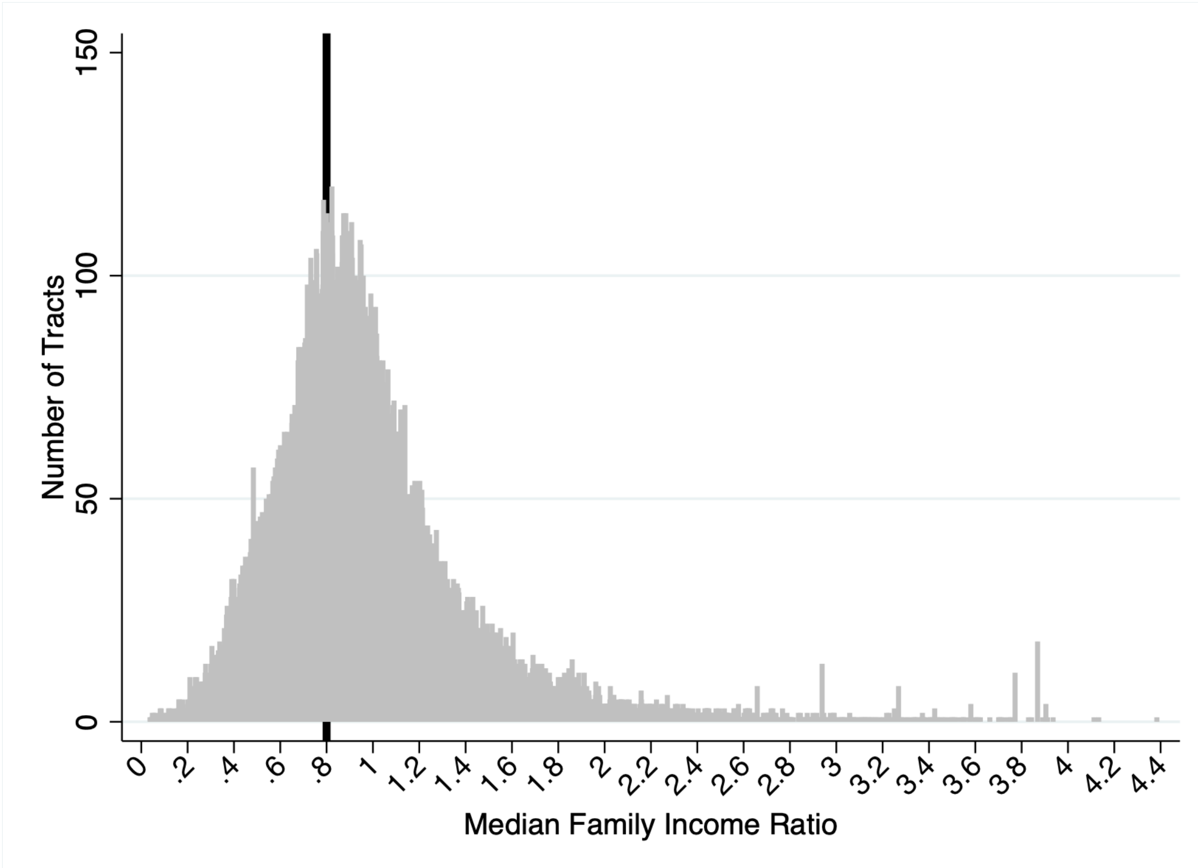

NOTE - Illustration of the key components of the New Markets Tax Credit (NMTC) Program. Source: Lambie-Hanson (2008)

Figure A.2: Low-Income Community Designation



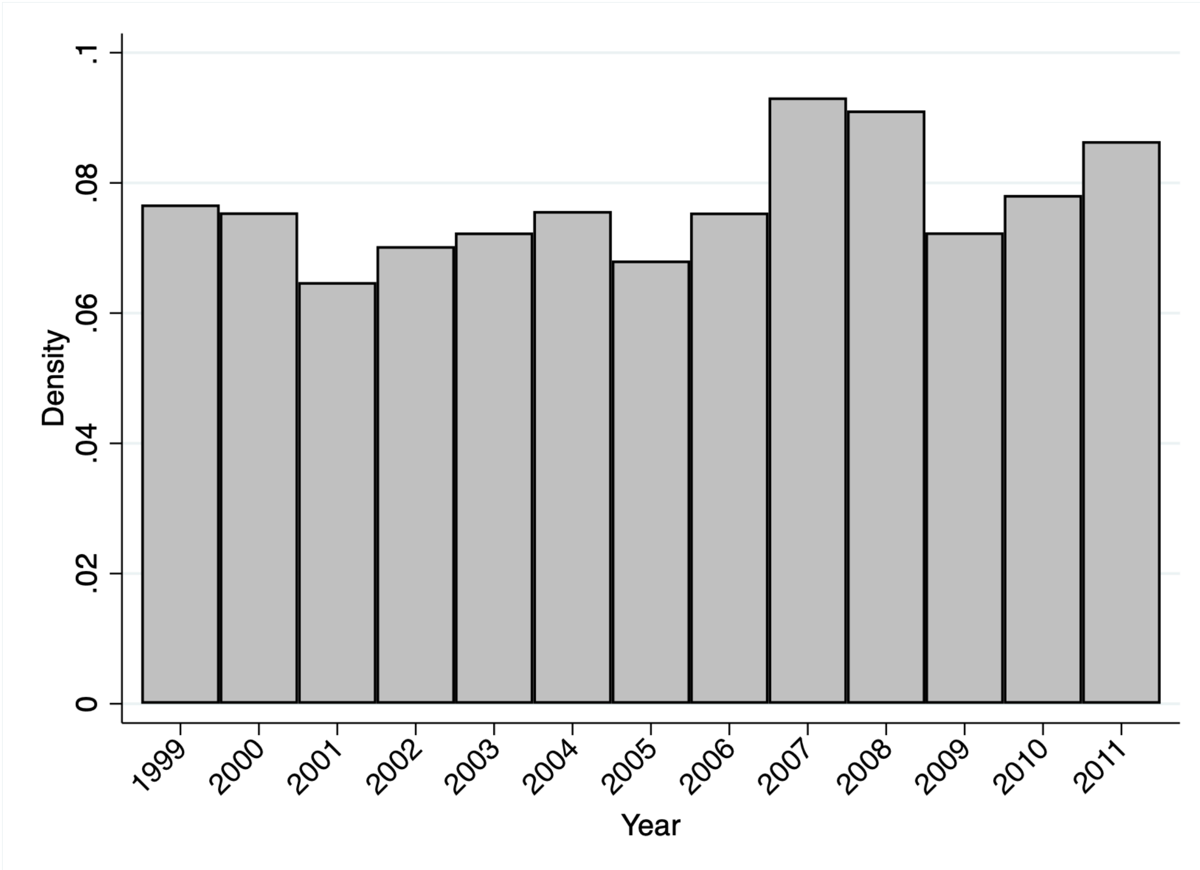

NOTE - Census tracts' Low-Income Community (LIC) designation as a function of its poverty rate and MFI ratio.

Figure A.3: Distribution of MFI Ratio



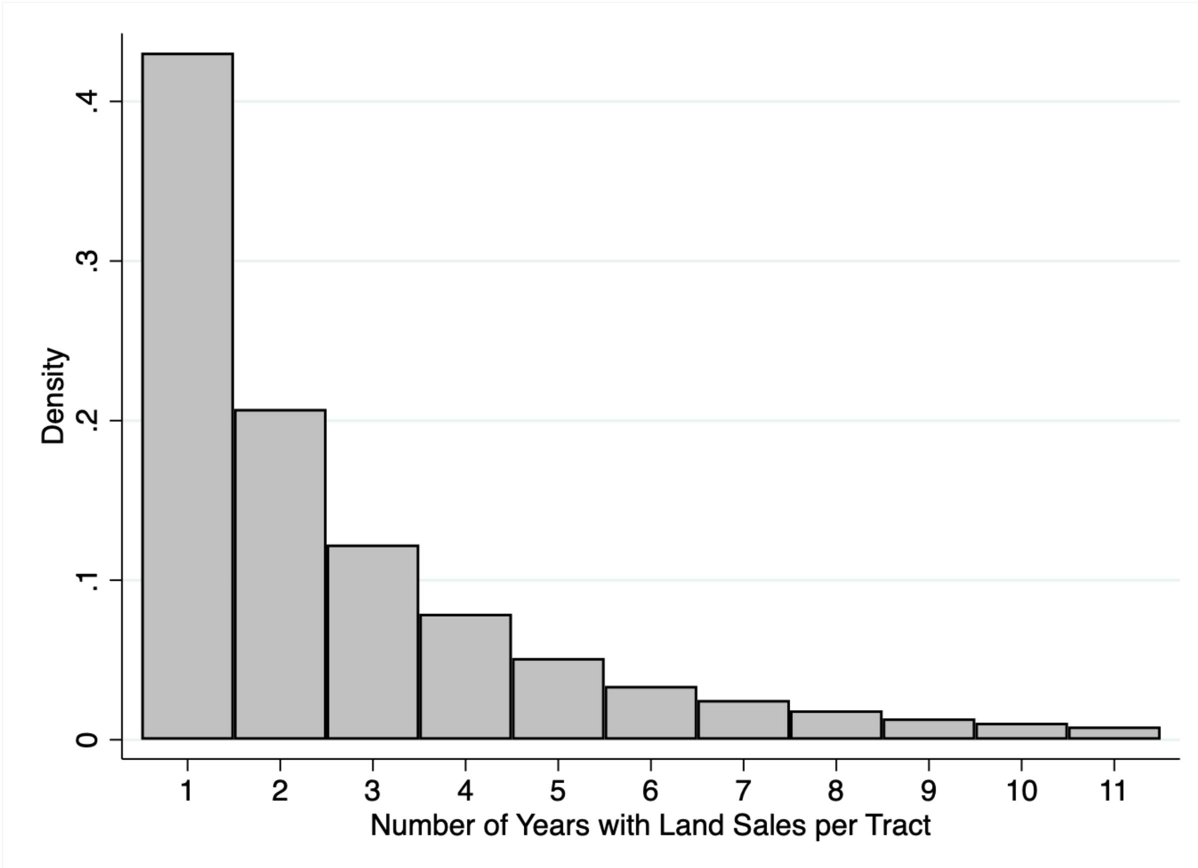

NOTE - Histogram of median family income (MFI) ratio scaled in frequency units. The solid black vertical line denotes the 0.80 MFI ratio cutoff.

Figure A.4: Distribution of Years



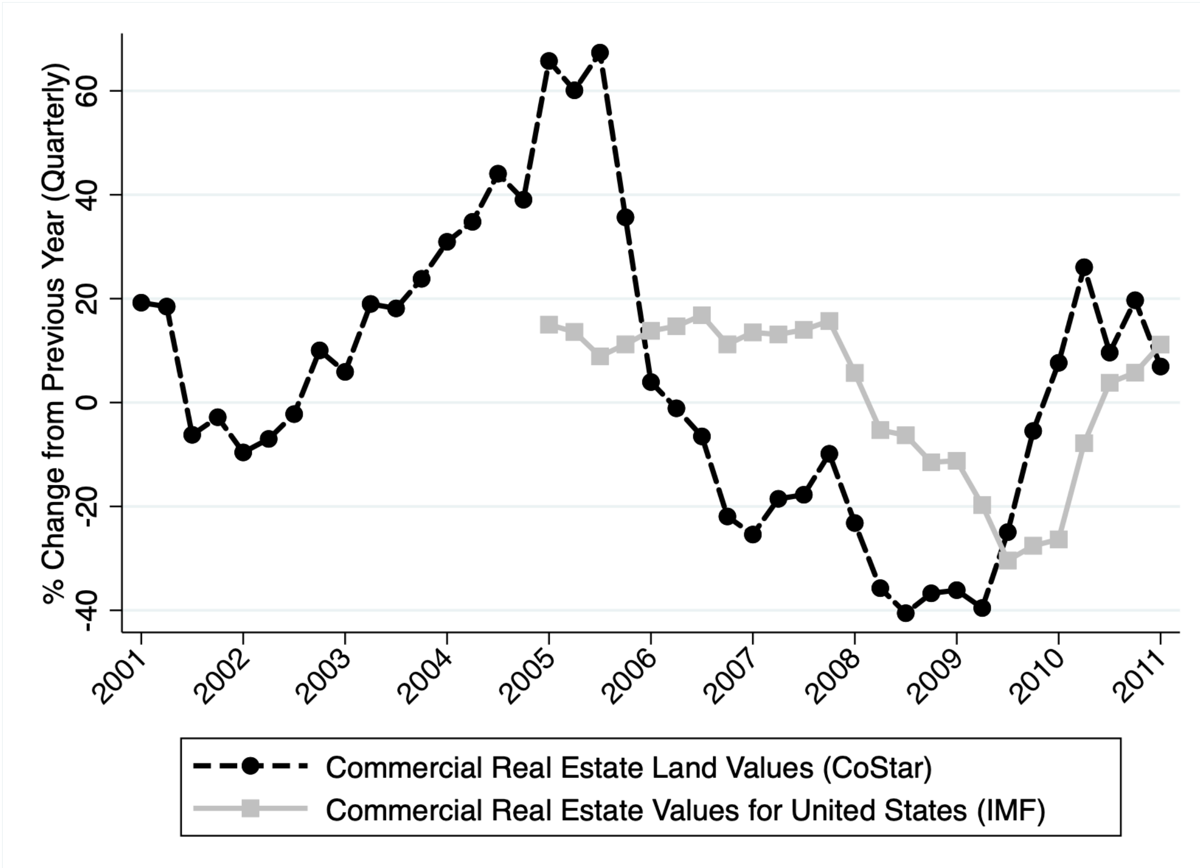

NOTE - Histogram of years for tracts with observed commercial land sales from January 1999 to December 2011.

Figure A.5: Distribution of Number of Years per Tract



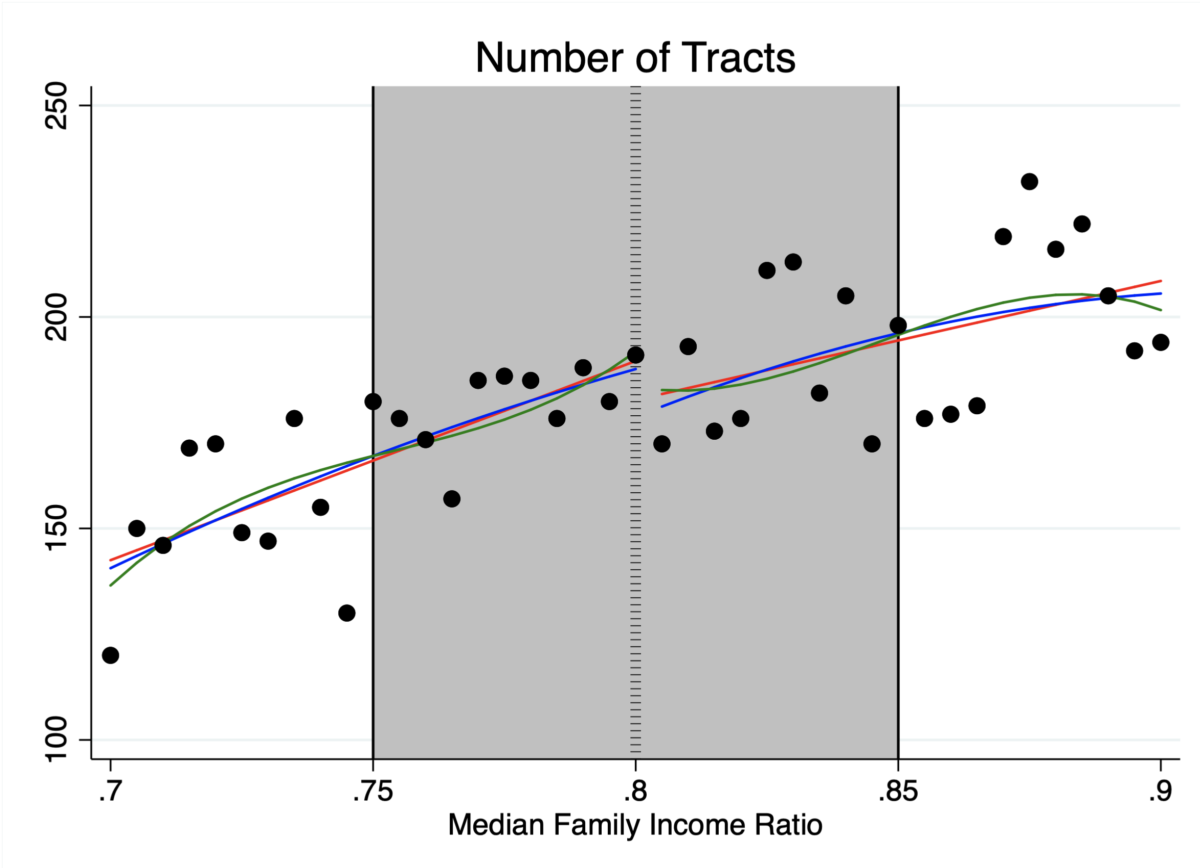

NOTE - Histogram of number of years that a land sale is observed in the CoStar data for each tract with observed commercial land sales from January 2001 to December 2011.

Figure A.6: Commercial Real Estate Prices for United States



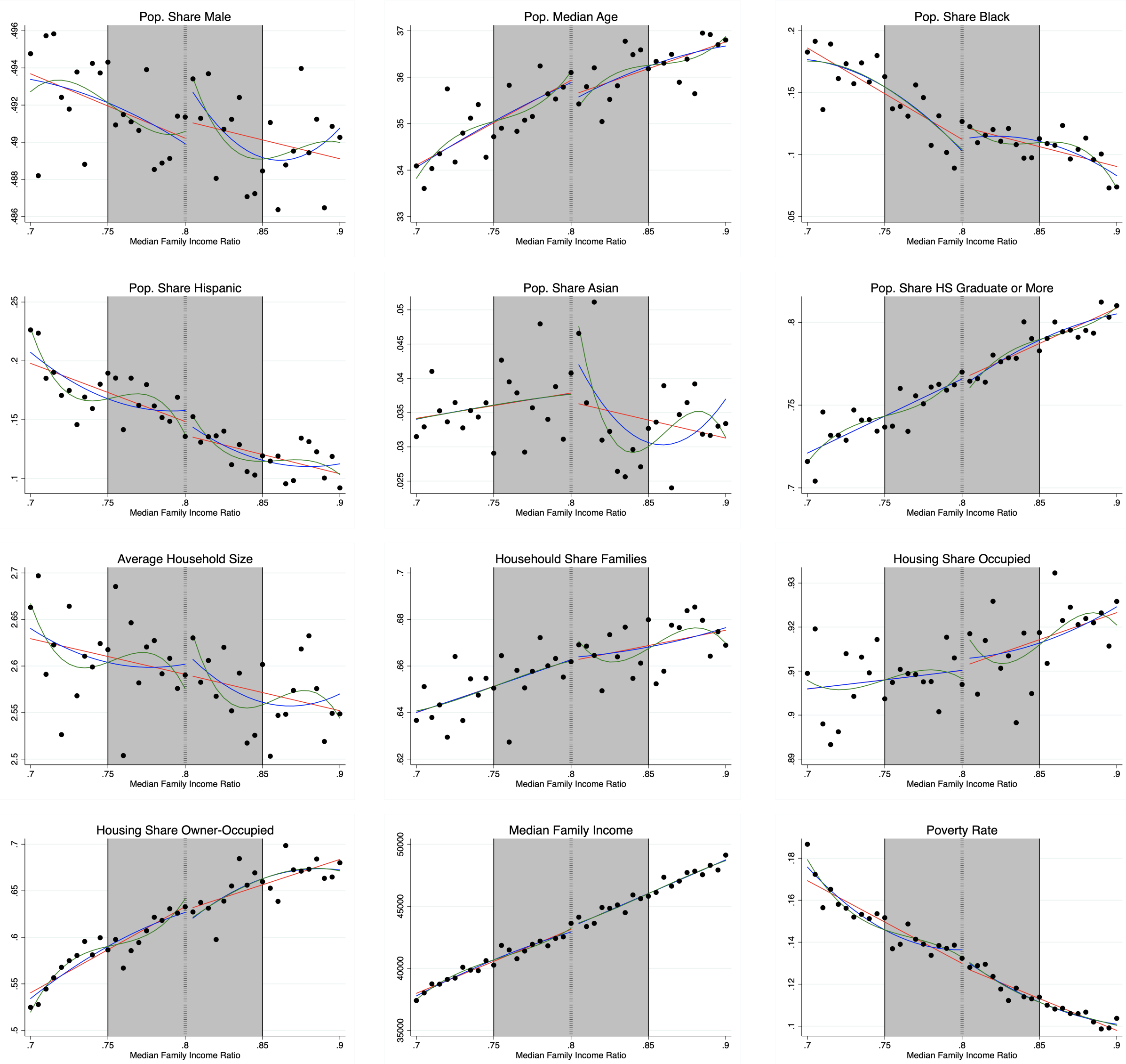

NOTE - The dashed black line with circle points is a time series of the percent change from a year ago of commercial land prices in the United States (Source: CoStar). The solid gray line with square points is a time series of the percent change from a year ago of commercial real estate prices in the United States (Source: IMF). Both series are in quarterly intervals and are not seasonally adjusted.

Figure A.7: Density (tract)



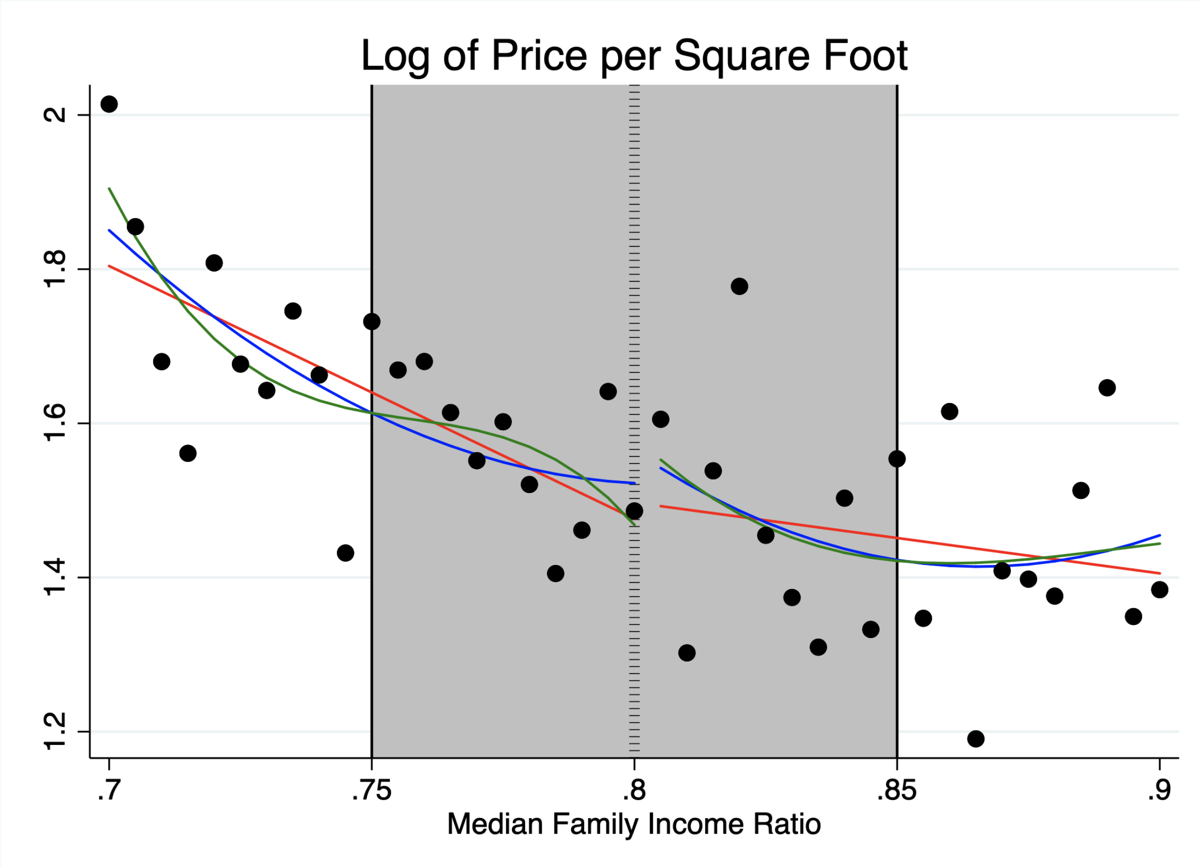

NOTE - Points are tract counts in half percentage point bins of median family income (MFI) ratio, between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. Sample includes tracts with observed commercial land sales from January 2001 to December 2011. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 MFI ratio cutoff.

Figure A.8: Covariate Smoothness (tract)




NOTE - Points are tract mean values for demographic, housing, or income characteristics, in half percentage point bins of median family income (MFI) ratio, between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. Sample includes tracts with observed commercial land sales from January 2001 to December 2011. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 MFI ratio cutoff.

Figure A.9: Land Values




NOTE - Points are tract-by-year mean values for natural log of average sale price per square foot of land, in half percentage point bins of median family income (MFI) ratio, between 0.70 and 0.90, which encompasses the RD window (i.e., 0.75 to 0.85) shaded in gray. Sample includes tracts with observed commercial land sales from January 2001 to December 2011. Red, blue, and green lines are linear, quadratic, and cubic fits, respectively, through the points, estimated separately on either side of the 0.80 MFI ratio cutoff.

Table A.1: Parameters for Quantitative Analysis

| Parameter | Value | Description | Source(s) |
| --- | --- | --- | --- |
| $\nu$ | 0.72 | Proportion of a project's total cost subsidized by the NMTC | CDFI Fund |
| $\lambda$ | 0.12 | Proportion of a project's total land cost | CoStar, CDFI Fund |
| $\pi$ | 0.11 | Probability of a census tract receiving investment, conditional on it being an LIC with commercial land | CoStar, CDFI Fund |
| $\delta$ | 0.30 | Discount factor of the 39 percent income tax credit distributed over seven years | Marples and Lowry (2019) |

NOTE - Parameter values and the data sources used to estimate them.

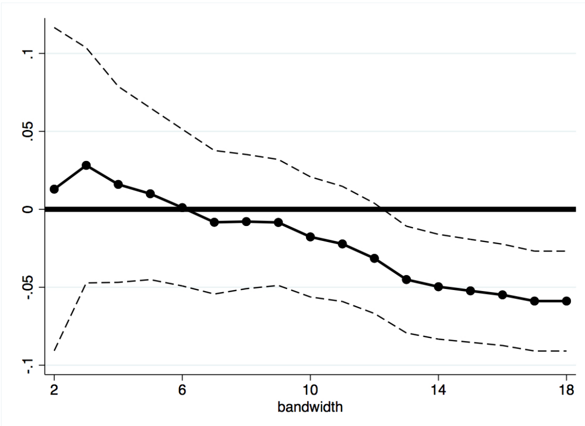
# Appendix B

# Supplementary Figures and Tables for Availability of Grandparent-Provided Childcare and Mothers' Labor Market Outcomes

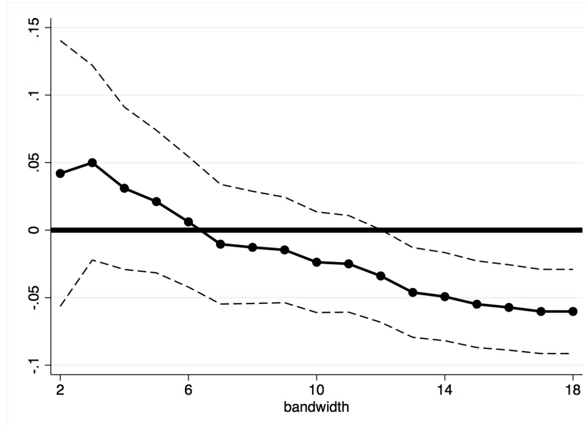
Figure B.1: Bandwidth Choice - Grandparents' Labor Supply

Panel A: Entire Three-Generation Sample

Labor Force Participation

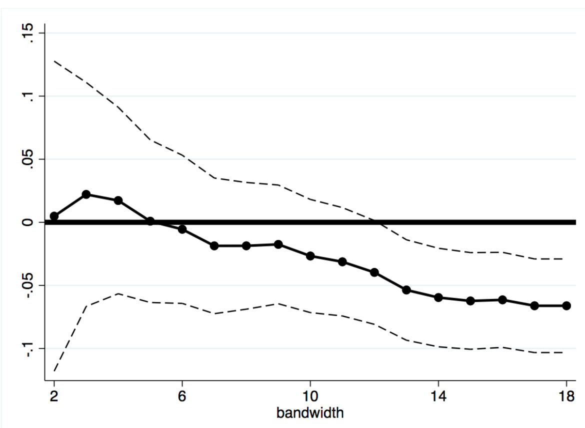



Unconditional Employment

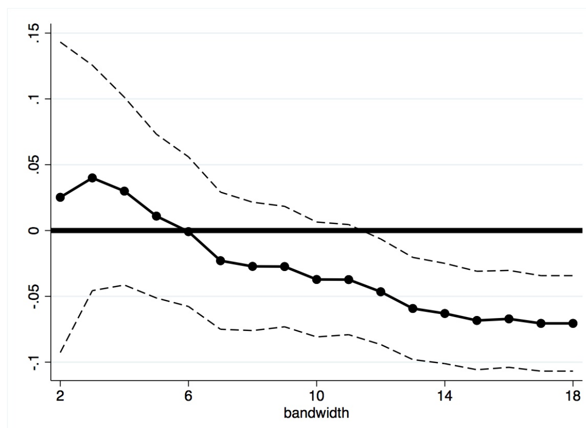



Panel B: Excluding Disabled Grandparents

Labor Force Participation



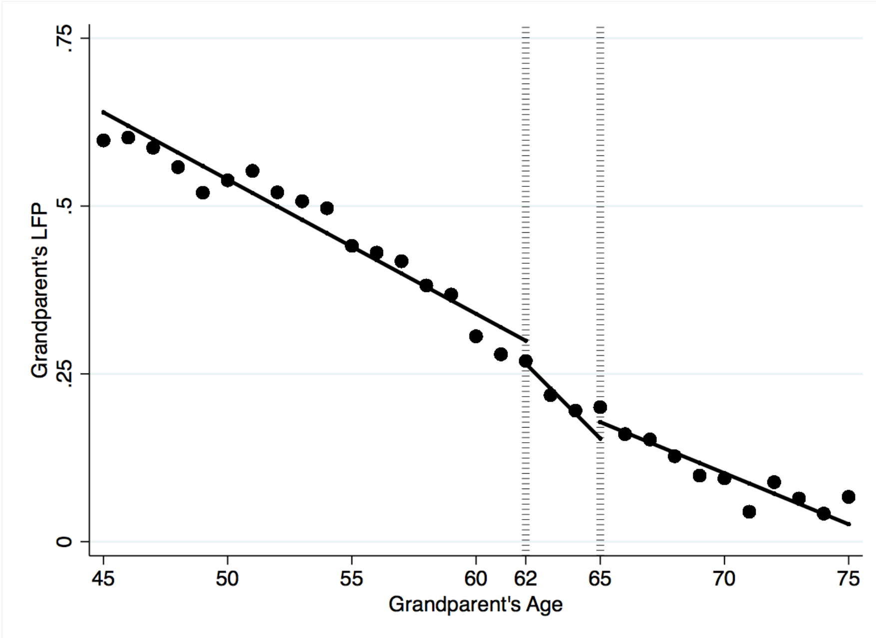

Unconditional Employment



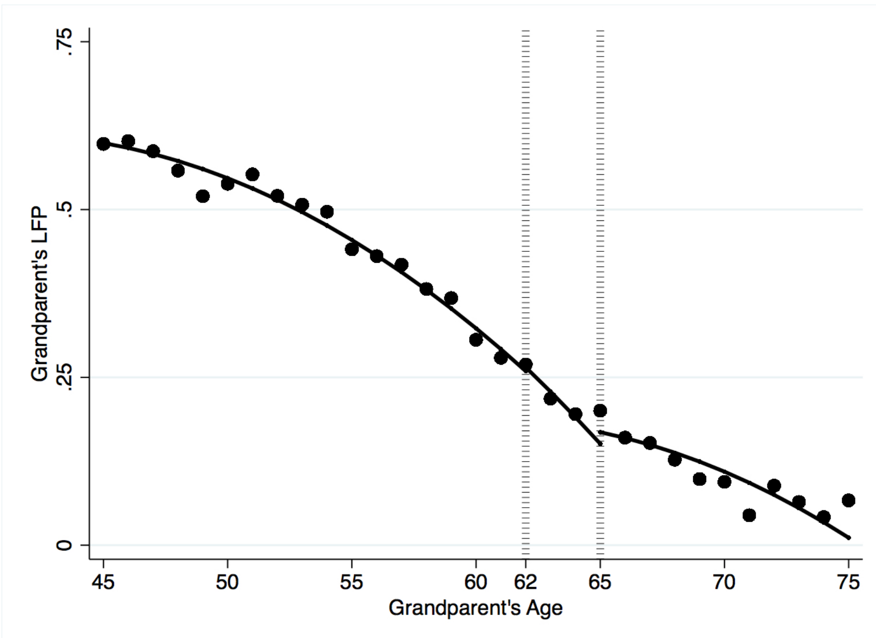

NOTE - Point estimates and corresponding 95 percent upper and lower bounds (dashed lines) on the discontinuity variable from regressions where the outcome is grandparents' labor force participation or unconditional employment (i.e., employment not conditional on labor force participation). Regressions control for linear polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold using 2, 3, etc. year bandwidths. Panel A uses the full three-generation sample, and Panel B uses a restricted sample, which is a subset of the three-generation sample that excludes disabled grandparents.

Figure B.2: Non-Linear Fit IV

Linear Polynomial Degree



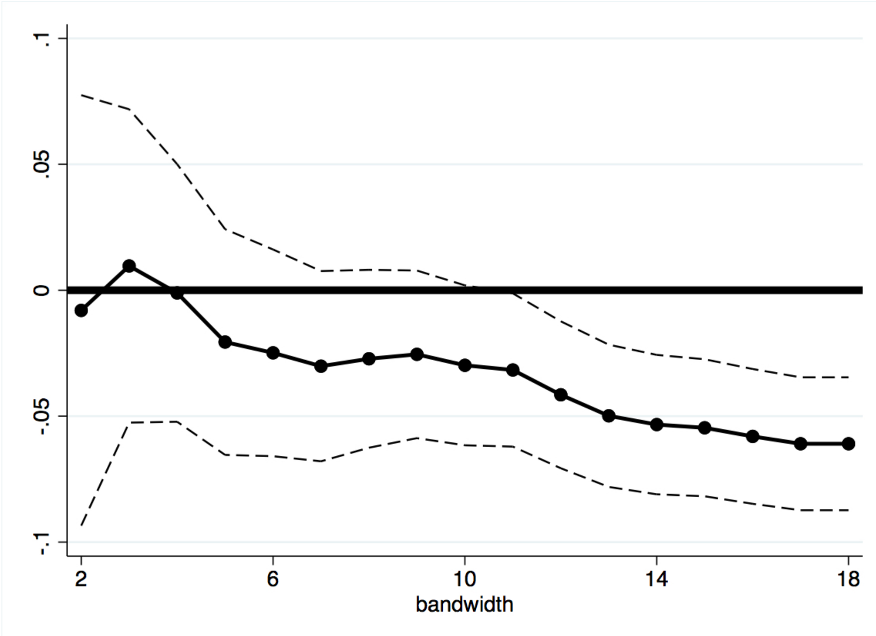

Quadratic Polynomial Degree



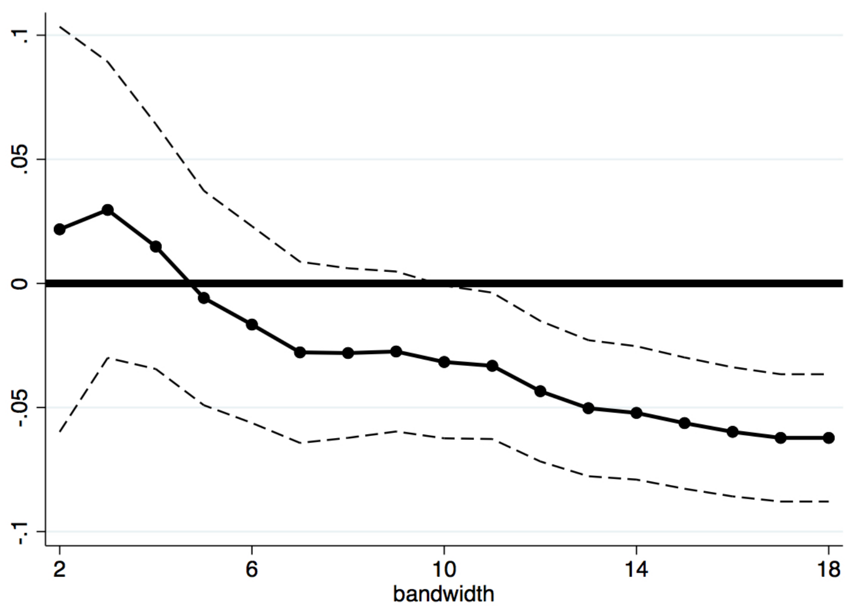

NOTE - Points are mean values for grandparents' labor force participation in yearly age bins of grandparents' age between 45 and 75 years old. Lines are fitted values from regressions that predict grandparents' labor force participation by controlling for a dummy variable that takes the value of one if the grandparent's age is in the interval [62,65) and zero otherwise, a dummy variable that takes the value of one if the grandparent's age is at least 65 and zero otherwise, interactions of those dummy variables and grandparents' age as well as linear or quadratic polynomials in the grandparents' age, respectively.

Figure B.3: Bandwidth Choice - All Grandparent Sample

Labor Force Participation

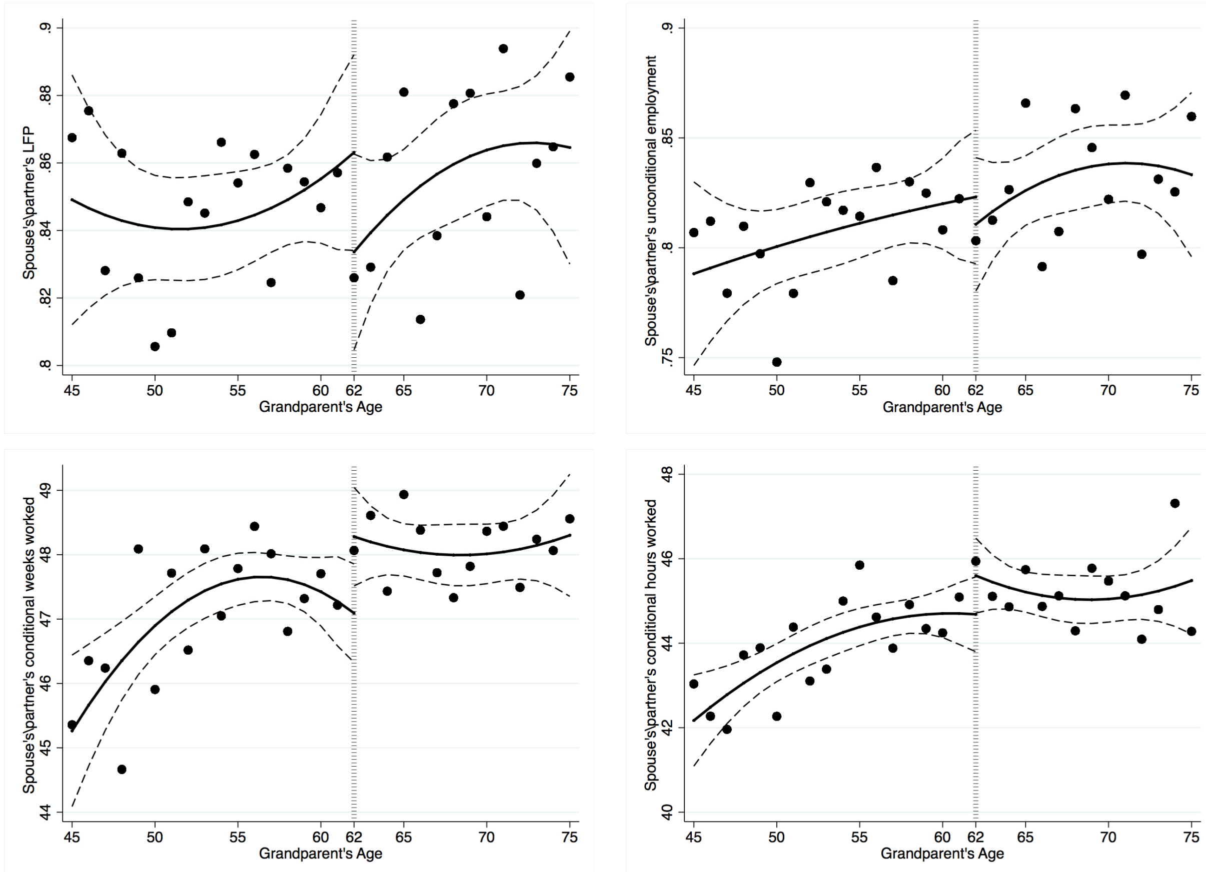



Unconditional Employment



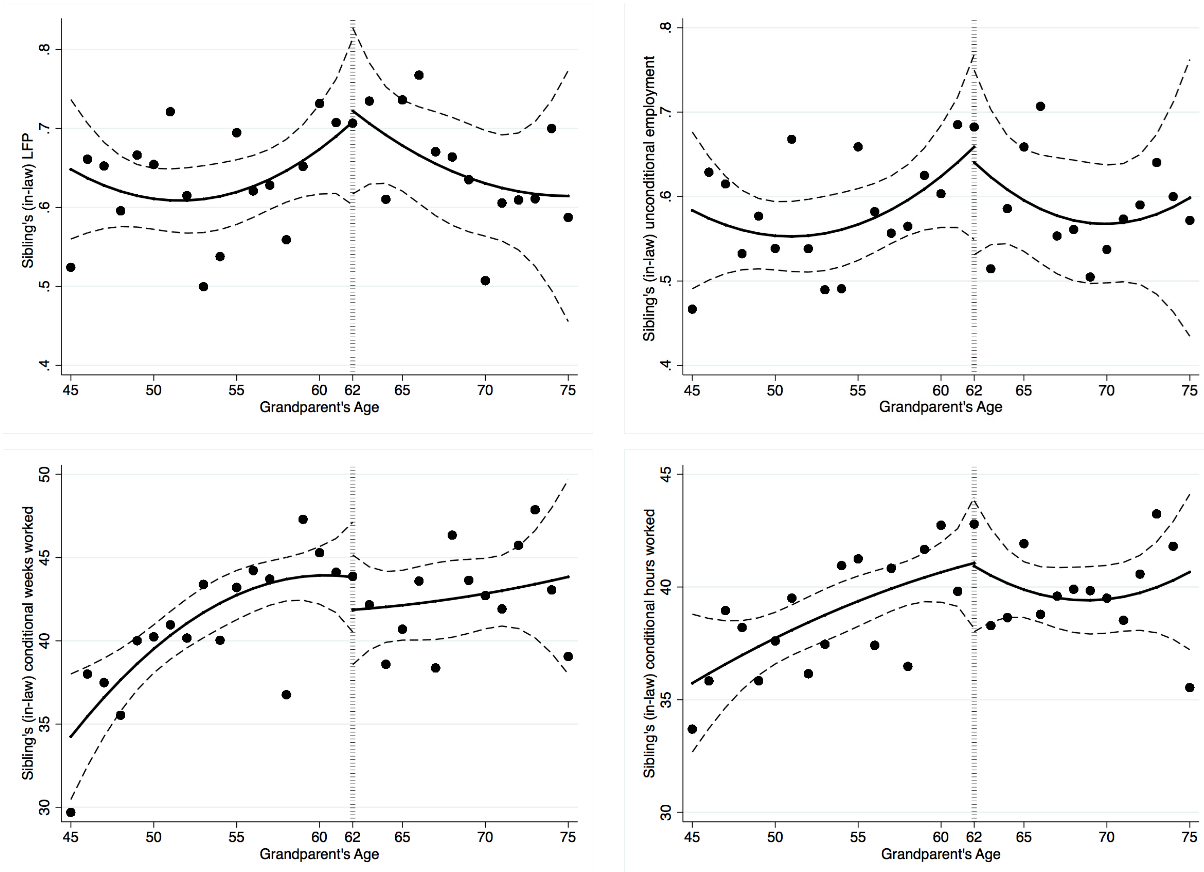

NOTE - Point estimates and corresponding 95 percent upper and lower bounds (dashed lines) on the discontinuity variable from regressions where the outcome is grandparents' labor force participation or unconditional employment (i.e., employment not conditional on labor force participation). The unit of observation are grandparents and the sample includes all households with a mother, a child, and a grandparent, such that the the grandparent is less than 15 years older than the mother, the mother is 20 to 40 years old, and the youngest child's is less than five years old. Regressions control for linear polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold using 2, 3, etc. year bandwidths. Standard errors are clustered at the household level.

Figure B.4: Spouses'/Partners' Labor Supply




NOTE - Points are mean values for mothers' spouses' or partners' labor supply in yearly age bins of grandparents' age between 45 and 75 years old. Lines are quadratic fits through the points with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

Figure B.5: Siblings' (in-law) Labor Supply




NOTE - Points are mean values for mothers' sibling(s)' labor supply, when the grandparent is maternal, or sibling-in-laws' labor supply, when the grandparent is paternal, in yearly age bins of grandparents' age between 45 and 75 years old. Lines are quadratic fits through the points with corresponding 95 percent confidence intervals estimated separately on either side of the early-retirement age threshold.

Table B.1: Non-Linear Fit IV: Effect of Grandparents' Labor Supply on Mothers' Labor Market Outcomes

$$Y_i = \alpha_0 + \pi_1 ABOVE_{62,i} + \pi_2 ABOVE_{62,i} * AGE_i + \pi_3 ABOVE_{65,i} + \pi_4 ABOVE_{65,i} * AGE_i + g(AGE_i) + \nu_i \quad \text{(B.1)}$$

| | mean | (1) | (2) |
|---|---|---|---|
| LFP | 0.622 | 0.225 | 0.175 |
| | | (0.280) | (0.221) |
| Weeks Worked | 41.7 | -7.195 | -4.961 |
| | | (10.994) | (8.649) |
| Hours Worked | 37.2 | -5.470 | 3.352 |
| | | (8.600) | (6.492) |
| Wage Income ($) | 16,475 | -7,547 | -6,494 |
| | | (12,802) | (9,467) |
| F-Statistic | | 4.77 | 6.34 |
| Polynomial Degree | | 1 | 2 |
| Control Covariates | | ✓ | ✓ |

NOTE - Estimates are from two-stage least-squares models where grandparents' labor force participation is instrumented with a dummy variable that takes the value of one if the grandparent's age is in the interval [62,65) and zero otherwise, a dummy variable that takes the value of one if the grandparent's age is at least 65 and zero otherwise, and interactions of those dummy variables and grandparents' age. Regressions control for linear (column (1)) or quadratic (column (2)) polynomial functions in grandparents' age. Control covariates, which are included in all specifications, are indicator variables for mothers' age, race and education, as well as state dummies. Means are for observations just to the left of the early-retirement age threshold. F-statistics for tests of the hypothesis that the excluded instruments have no explanatory power on grandparents' labor force participation. Means are for observations just to the left of the early-retirement age threshold. Means are for observations just to the left of the early-retirement age threshold. Heteroscedastic robust standard errors are reported in parentheses. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table B.2: All Grandparent Sample: Estimated Discontinuities on Grandparents' Labor Supply

| | mean | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| LFP | 0.293 | -0.062*** | -0.058*** | -0.008 | -0.007 |
| | | (0.013) | (0.013) | (0.020) | (0.020) |
| Unconditional Employment | 0.252 | -0.063*** | -0.059*** | -0.008 | -0.008 |
| | | (0.013) | (0.013) | (0.019) | (0.019) |
| Weeks Worked | 40.3 | -0.196 | -0.497 | 1.686 | 1.417 |
| | | (0.853) | (0.854) | (1.242) | (1.235) |
| Hours Worked | 37.0 | -0.166 | -0.148 | 0.739 | 0.861 |
| | | (0.696) | (0.687) | (0.973) | (0.959) |
| Polynomial Degree | | 1 | 1 | 2 | 2 |
| Control Covariates | | | ✓ | | ✓ |

NOTE - Estimated coefficients on the discontinuity variable from regressions where the outcome is grandparents' labor supply. The unit of observation are grandparents and the sample includes all households with a mother, a child, and a grandparent, such that the the grandparent is less than 15 years older than the mother, the mother is 20 to 40 years old, and the youngest child's is less than five years old. Regressions control for linear (columns (1) and (2)) or quadratic (columns (3) and (4)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61). Control covariates are also included in specifications represented by columns (2) and (4). Control covariates are indicator variables for mothers' age, race and education, as well as state dummies. Means are for observations just to the left of the early-retirement age threshold. Parentheses are standard errors, which are clustered at the household level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level

Table B.3: All Grandparent Sample: Effect of Grandparents' Labor Supply on Mothers' Labor Market Outcomes

| | mean | (1) | (2) | (3) | (4) |
|---|---|---|---|---|---|
| LFP | 0.624 | 0.065 | 3.429 | 0.008 | 0.131 |
| | | (0.250) | (12.078) | (0.250) | (0.263) |
| Weeks Worked | 41.8 | -1.479 | 19.825 | -1.014 | -2.093 |
| | | (9.975) | (106.277) | (10.196) | (10.166) |
| Hours Worked | 37.5 | -4.259 | -32.948 | -3.472 | -6.456 |
| | | (7.878) | (114) | (7.921) | (8.291) |
| Wage Income ($) | 16,586 | -483 | 144,167 | -197 | -1,225 |
| | | (10,901) | (511,956) | (10,555) | (11,812) |
| Polynomial Degree | | 1 | 2 | 1 | 2 |
| Control Covariates | | ✓ | ✓ | ✓ | ✓ |
| Control Specification | | Unrestricted | Unrestricted | Restricted | Restricted |

NOTE - Estimates are from two-stage least squares models that instrument grandparents' labor force participation with a dummy for the grandparent being past the early-retirement age threshold, and where the outcome is mothers' labor market outcomes. The unit of observation are grandparents and the sample includes all households with a mother, a child, and a grandparent, such that the the grandparent is less than 15 years older than the mother, the mother is 20 to 40 years old, and the youngest child's is less than five years old. Regressions control for linear (columns (1) and (3)) or quadratic (columns (2) and (4)) polynomial functions in grandparents' age, with coefficients on the running variable allowed to vary on either side of the early-retirement age threshold (i.e., age 61) for specifications represented by columns (1) and (2). Control covariates, which are included in all specifications, are indicator variables for mothers' age, race and education, as well as state dummies. Means are for observations just to the left of the early-retirement age threshold. Parentheses are standard errors, which are clustered at the household level. *Significant at 10% level; **Significant at 5% level; ***Significant at 1% level