Beyond the Local Impacts of Place-Based Policies: Spillovers through Latent Housing Markets

Anna Ziff*

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Abstract

Many analyses of place-based policies, which target geographic areas often to foster economic development, focus on their direct effects. Responses of households and firms that propagate within similar markets (e.g., housing markets) are also important for studying overall effectiveness. I propose an approach to estimate non-spatial spillover effects on non-targeted areas in the same markets as the targeted areas. My approach can be adapted to other settings with possible non-spatial spillovers. I illustrate the approach and discuss the economic framework using a widespread, place-based policy, Tax Increment Financing (TIF). To characterize housing markets, I construct a network of connected neighborhoods using data on household moves and define markets based on a model of community detection from network theory. With this data-driven characterization, I estimate the "market" spillover effects to non-targeted areas within the same housing market. TIF is locally effective at increasing property values within the targeted area. However, the market spillover effects indicate a negative effect on non-targeted areas within the same housing markets. This result implies that the policy relocates investment from non-targeted to targeted areas. I analyze outcomes related to household and firm characteristics and find support for the relocation mechanism. I combine the direct and spillover effects to calculate a back-of-the-envelope estimate of an overall effect close to zero, with the policy redistributing investment towards relatively disadvantaged targeted areas within housing markets.

*Department of Economics, Duke University. E-mail: anna.ziff@duke.edu. I am thankful for the guidance of the members of my dissertation committee, Patrick Bayer, V. Joseph Hotz, Matthew Masten, Arnaud Maurel, Michael Pollmann, and Chris Timmins, as well as feedback from participants of the public and labor workshops at Duke University.

1 Introduction

The presence of inequality due to spatially distributed market failures often justifies place-based economic development (Gaubert et al., 2021; Kline and Moretti, 2014b). However, the presence of market failures and the responses of households and firms can challenge the effectiveness of such investments (Coates and Humphreys, 2008; Fajgelbaum et al., 2019; Glaeser and Gottlieb, 2008; Moretti, 2010). Even if appropriately targeted and locally effective, effects of these investments outside the targeted area may magnify or diminish their effectiveness, as well as introduce equity concerns. These spillover effects are challenging to identify making it difficult to assess the overall effectiveness of providing place-based investment.

I propose an approach to study spillover effects of spatially targeted investments alongside direct treatment effects within the targeted areas. I illustrate the approach and corresponding economic framework with a particular place-based policy that targets investment to well-defined geographic areas. I use the staggered implementation of the policy to estimate the direct treatment effect on property values and find that the policy is locally effective. Ideally, it would be possible to study the spillover effect of the investment on the whole region. However, it is challenging to characterize the relevant non-targeted areas that the investment is likely to affect from those that it is not. One natural solution is to consider areas geographically close to the investment as potentially affected. Due to responses of households and firms, however, there may be spillover effects to untreated areas that are geographically far from the investments. Because I primarily study the effects of the policy on residential property values, I focus on spillover effects propagated through housing markets and refer to them as market spillover effects.

For example, suppose a policy subsidizes a retail development of a large vacant lot. The property values of residential properties immediately surrounding the retail development may increase in response to the development. Because these properties are targeted, I consider this to be a direct treatment effect. In addition, there may be a (smaller) positive effect on non-targeted properties that are close to the development. This is the spatial spillover effect. However, the retail development may affect areas within the same housing markets as the targeted areas. Housing markets may not be contiguous due to housing supply, location of amenities, segregation patterns, or other context-specific factors that determine housing markets. If the subsidy relocates investment away from areas within the same housing markets, the values of those non-targeted properties may decrease. This is an example of a negative market spillover effect.

To characterize housing markets, I construct a network of connected neighborhoods using data on household moves, and define markets based on a model of community detection from network theory. Under a revealed preference framework, I interpret moves as informative about the households' underlying choice sets, which I use to characterize latent housing markets and estimate market spillover effects. My approach can be applied to other empirical frameworks that already identify direct treatment effects, and thus bolster economic analysis by assessing overall effectiveness. In my particular policy context, I find evidence of relocation of investment to the targeted area away from non-targeted areas in the same housing market.

I present the empirical framework and approach in Section 2. The exposition matches my particular policy context in which I rely on panel data for identification of parameters of interest. I describe my methodological approach with both a simple difference-in-differences framework with one treatment group and a framework that allows for multiple, staggered, treatment groups. Areas may be targeted for place-based investment due to their potential to grow. This selection can be difficult to account for only using observed characteristics and challenges the identifying assumption of parallel trends when comparing targeted areas to non-targeted areas. Instead, I compare targeted areas to areas targeted in the future using stacked difference-in-differences (Cengiz et al., 2019; Deshpande and Li, 2019; Fadlon and Nielsen, 2015; Guryan, 2004). I use a similar approach to estimate spatial spillover effects, comparing areas that are close to those that are far. For market spillover effects, I characterize markets to define areas that are within the same market as the targeted areas. I apply Stochastic Blockmodeling (SBM) to household-level data on moves to characterize markets (see Peixoto, 2019, 2023, for overviews of SBM). Intuitively, areas between which households move are more likely to be in their choice sets when making moving decisions than areas between which households do not move. This revealed preference framework indicates which neighborhoods are within the same housing market. SBM allows me to define housing markets based on observable and unobservable characteristics rather than only relying on spatial proximity or observed characteristics (Goodman and Thibodeau, 2003).

After establishing the methodological approach, I explore a policy example of place-based economic development. I study Tax Increment Financing (TIF) because of its staggered implementation and large concentration of public investments into well-defined geographic areas. I use variation in the timing of TIF districts to identify and estimate the effects of economic development. Municipalities implement TIF by creating TIF districts in which a portion of property taxes are used for a wide variety of economic development activities. TIF districts are selected based on a combination of eligibility factors, potential for success in repayment of costs, and public-private mutual interest (Johnson and Kriz, 2001; Klacik and Nunn, 2001; Kriz and Johnson, 2019). The property taxes can be used directly for investment, such as for demolitions of vacant structures, or as leverage for debt mechanisms and private investment (Johnson, 1999, 2001; Luby et al., 2019; Weber, 2010). TIF is a common policy that constitutes a large investment for municipalities in local economic development. I use Chicago's implementation of TIF, which is particularly widespread lending to its recognition as emblematic of TIF (Craft and Weber, 2019; Healey and McCormick, 1999). My analysis includes TIF districts established between 2000 and 2022, corresponding to the height of its implementation. I describe more details about the implementation of TIF in Chicago in Section 3.

I construct a dataset that combines the policy information of TIF with property, household, and firm characteristics. The City of Chicago publishes data on TIF districts, including geographic boundaries, initiation dates, funding amounts, and details of the economic development activities (Office of Budget and Management, 2022). I combine these data with assessed values, sale prices, and property characteristics on single-family residential properties (Cook County Government, 2023). I use data from InfoUSA to observe household-level moves across years (Data Axle, 2020). Observing flows between neighborhoods is a necessary input for my strategy of determining housing markets to calculate market spillover effects. In addition to InfoUSA, I use data from the Census to characterize neighborhood composition and estimate how TIF may alter it (U.S. Census Bureau, 2021). To measure firm responses, I use data on business licenses (Business Affairs and Consumer Protection, 2023) and commercial and industrial assessed property values (Cook County Government, 2023). In Section 4, I further describe each of these data sources and the data construction.

In Section 5, I present estimates of the direct and spillover effects of place-based economic development on property values. For local governments, property values dictate property taxes that provide a revenue stream to fund public goods and services (Brülhart et al., 2015) so many economic development policies aim to increase property values (Foell and Pitzer, 2020). In the case of TIF and other policies of land-value capture, the increasing property taxes are designed to pay for the investment (Youngman, 2016). I apply the framework described in Section 2 to compare Census blocks treated by TIF districts to those that will eventually be treated by TIF districts. I estimate an average treatment effect across time periods on property values of 3.06% (from a baseline average of $\$171,630$). Dynamic treatment effects demonstrate that the impact of TIF begins soon after the initiation of a TIF district and increases over time. I split the sample by characteristics of the TIF districts. The heterogeneous estimates indicate that mixed-use development and districts with more structural and infrastructural issues drive the main effects. I estimate weakly positive spatial spillover effects (0.8%). While informative, the spatial spillover effects do not include responses of households and firms outside of the immediate vicinity of the TIF districts. I estimate a market spillover effect of -1.57%. For the spatial and market spillover effects, the baseline average for non-targeted properties is $\$195,059$. Negative market spillover effects suggest that investment is relocated to TIF districts from similar areas.

I apply the approach of estimating direct treatment effects and spatial and market spillovers to other outcomes related to households and firms. These outcomes include demographic composition (income, race, renter or owner status), commercial and industrial property values, and business licenses. The direct estimates on demographic composition are not precisely estimated except for a decrease in the proportion below the poverty line and the proportion of Hispanic residents. While I do not find evidence of spatial spillover effects, the market spillover effects indicate an increase in the proportion of Black and Hispanic residents and a decrease in the median household income in the non-targeted, market-close areas. These results are suggestive of TIF leading to a small amount sorting of low-income households outside of targeted areas. I estimate an increase in the flow of business licenses within TIF districts and a decrease outside of TIF districts, supporting that TIF redirects investment away from non-targeted areas in the same housing markets.

Estimating market spillover effects enriches analysis on the overall effectiveness and possible equity concerns as a result of place-based investment. I combine the direct effects on the targeted areas with the spillover effects outside of TIF districts to calculate a back-of-the-envelope, overall treatment effect. I calculate a population-weighted sum of the direct treatment effect on targeted areas, the spatial spillover effect on non-targeted areas, and the market spillover effect on non-targeted areas. This results in an estimated overall effect on property values that is close to zero, illustrating that omitting market spillover effects may substantially alter the understanding of the effectiveness of place-based economic development. I discuss the overall effect in the context of observed differences between TIF and non-TIF areas. Because TIF areas tend to be negatively selected on property values, the policy may result in redistribution even if the overall effectiveness is small. In Section 6, I provide a calculation of the overall effectiveness of TIF and discuss equity considerations.

Related Literature. Methodologically, this paper builds on the literature studying spillover effects, which focuses on spatial spillover effects (Hudgens and Halloran, 2008),[1] with recent developments expanding design-based methods (Borusyak and Hull, 2021; Pollmann, 2023; Wang et al., 2023). I build on this work by adding the consideration of market spillover effects. While a growing literature uses SBM in the context of labor markets to classify types of jobs and workers (Costa Dias et al., 2021; Fogel and Modenesi, 2022; Jarosch et al., 2023; Nimczik, 2023), SBM has not been applied to the problem of defining housing markets to my knowledge.[2] My approach introduces a method to define latent markets for estimating effects on non-targeted areas. Given data on household or firm relocations, this approach may be applicable to other literatures that have the potential for spatial and market spillovers (Huber and Steinmayr, 2021), such as research on gentrification (e.g., Asquith et al., 2023; Ding et al., 2016; Ferreira et al., 2023).

Empirically, this paper contributes to the literatures studying TIF and economic development more broadly. Greenbaum and Landers (2014) provide a recent review of the literature evaluating TIF, which generally finds positive impacts of TIF on property values, including residential (Smith, 2006), commercial (Carroll, 2008; Smith, 2009), and industrial properties (Weber et al., 2003). Instead of relying on observed characteristics for matching or control function approaches, my empirical framework assumes parallel trends between treated and not-yet-treated areas and allows for dynamic treatment effects. I use fine-grained time-varying fixed effects to account for unobserved heterogeneity that may otherwise threaten identification.

[1]Bollinger and Ihlanfeldt (1997); Bowes and Ihlanfeldt (2001); Diamond and McQuade (2019); Lu et al. (2019); Schwartz et al. (2006) are some examples of empirical papers that study spatial spillover effects of place-based policies.

[2]Holland et al. (1983); Nowicki and Snijders (2001); Snijders and Nowicki (1997); Wang and Wong (1987) are foundational works introducing SBM. Jackson (2011) provides an introduction to community detection methods.

I further extend this work by considering spillovers, especially by including the concept of market spillover effects. Prior work studying the spatial spillover effects of TIF finds heterogeneous effects on nearby property values depending on the timing and purpose of the TIF-funded development (Weber et al., 2007; Yadavalli and Delgado, 2019). My findings on market spillover effects align with these estimates of spatial spillover effects. A positive effect within the district and a negative effect farther away from it may indicate that the two areas are within the same housing market. My approach separates these two spillover mechanisms allowing for separate analyses. My approach to study the overall effectiveness of TIF relates to papers studying the municipal-level effect of adopting TIF in the the 1980s and 1990s when the policy was gaining traction (Anderson, 1990; Dye and Merriman, 2000; Man, 1999; Man and Rosentraub, 1998; Merriman et al., 2011). Because my approach uses smaller units of analysis and separates targeted and non-targeted areas within the municipality, it allows for more nuanced empirical results and interpretation.

My consideration of market spillover effects relates to previous studies that consider overall implications of spatially targeted economic development. Many of these studies also consider place-based policies, which target geographic areas rather than populations of individuals, households, or firms (see Bartik, 1990, 1991; Neumark and Simpson, 2015, for reviews of place-based policies). My paper relates more closely to studies that consider the overall impacts. Notably, Kline and Moretti (2014a) impose structure to consider how the Tennessee Valley Authority (TVA) affected the nation at large. Through the specification of the agglomeration economies, they allow for the direct investment in the TVA region to propagate as a function of manufacturing density. Another example is Wheeler (2022), who estimates spatial spillovers of Opportunity Zones and allows for the equivalent of "market spillovers" through a structural model of developers' investment decisions. More generally, a structural approach allows for an investigation of the mechanisms of the general equilibrium effects (e.g., Auerbach and Hassett, 1991; Gaubert, 2018). The approach in my paper to characterize markets allows for consideration of some general equilibrium responses within an empirical framework. It relies on assumptions on household behavior that allow for a revealed preference framework instead of fully specifying structure on the underlying market failures or firm decisions. I rely on estimating direct and spillover effects on other outcomes related to residential sorting and firm behavior to consider those policy responses.

2 Spillover Effects through Latent Markets

In response to locally targeted economic development, targeted units may experience a benefit, resulting in positive direct treatment effects. Units in nearby, non-targeted areas may be affected alongside those in targeted areas, resulting in positive spatial spillover effects. However, it is possible that spillovers propagate through other mechanisms. For example, firms may concentrate investments near the new economic development, perhaps substituting away from other similar markets that may not be tied to geographic proximity. I call these spillovers "market spillover effects" to highlight that they propagate through similarities in the space of observable or unobservable characteristics that relate to markets.

This section describes my methodological approach to identify market spillover effects within a framework that also allows for identification of direct treatment effects and spatial spillover effects. Spatial spillover effects are important to evaluate the local effectiveness of a policy as well as provide benchmarks for the market spillover effects. First, in Section 2.1, I introduce the notation and setup for a difference-in-differences framework with one treatment group. To focus the discussion on identification, I assume that a market structure is observed. In Section 2.2, I extend the framework to allow for staggered treatment. Although I use general notation and language, the staggered-treatment setup mirrors the setting of the implementation of TIF in Chicago. In reality, the market structure is unobserved. I describe my approach to characterize it in Section 2.3 and present the estimated markets in Chicago that I use for my empirical analysis.

2.1 Market Spillover Effects with One Treatment Group and Observed Markets

Suppose there is an intervention that targets economic development in one geographic area starting in period 0. I will call this geographic area a district to align with my empirical application in which the districts are at least two acres. In other contexts, the geographic areas of interest may be neighborhoods, counties, or other geographic designations. I illustrate this example in Figure 1a in which I represent the district using a solid black line. Let i index units of analysis and t index time periods. The treatment indicator D_i equals one if the unit is targeted by the district. For now, assume that markets are observed. An analogous indicator D_i^m equals one if the unit is in the same market as the district. In Figure 1a, the district overlaps with part of market B. If unit i is in market B, $D_i^m = 1$.[3] I define the variable D_{it}^m to indicate if unit i is in the same market as the district and $t \geq 0$.[4] I define the counterfactual outcome as $Y_{it}(d, d^m)$ where $d = 1$ fixes the unit to be targeted and $d = 0$ fixes it to be non-targeted. Similarly, $d^m = 1$ fixes the unit to be in the same market and $d^m = 0$ fixes it to be in a different market. Under this framework with observed markets, it is straightforward to define a parameter of interest to capture market spillover effects as an average treatment effect on units that are in the same market as the district:

$$\mathbb{E}\left[Y_{it}(0,1) - Y_{it}(0,0) \mid D_i = 0, D_i^m = 1\right]. \tag{1}$$

While informative, this parameter does not distinguish between the spillover effect due to the market from a spillover effect due to geographic proximity. Observable and unobservable characteristics determine markets. One of those characteristics may be a function of the geographic proximity of units. Even if not entirely determined by geographic proximity, it may be an important factor. Its potential importance motivates the study of spatial spillover

[3] In practice, the district may overlap with multiple markets. In this case, D_i^m equals one if unit i is in any market that also overlaps with the district. This is common in my empirical setting.

[4] I consider a binary definition of market-proximity; either a unit is in the same market as the district or not. For other empirical frameworks and identifying assumptions, the definition could be continuous corresponding to an underlying continuous notion of market-proximity.

effects, in which nearby non-targeted areas are affected. Spatial spillover effects propagate over geographic space, with the assumption that closer areas are more affected (Tobler, 1970). In contrast, market spillover effects arise due to responses of households and firms within similar markets. I expand the definition of the counterfactual outcome and the parameter of interest to isolate the market spillover effect from the spatial one.

The indicator D_i^s equals one if unit i is spatially close to the district and zero if it is spatially far.[5] In Figure 1a, I indicate a buffer around the example district with a dashed line. Some of the units that are inside this dashed line are also market-close to the district (market B). I redefine the counterfactual outcome to allow for this using $Y_{it}(d, d^m, d^s)$. Then, the unit can be fixed to being market-close but not spatially close, $Y_{it}(0,1,0)$, spatially-close but not market close, $Y_{it}(0,0,1)$, both market-close and spatially close, $Y_{it}(0,1,1)$, or neither market-close nor spatially close, $Y_{it}(0,0,0)$. Based on the definition of counterfactual outcomes, the observed outcome is

$$\begin{aligned} Y_{it} = &(1-D_{it})\Big\{(1-D_{it}^s)(1-D_{it}^m)Y_{it}(0,0,0) + (1-D_{it}^s)D_{it}^m Y_{it}(0,1,0) \\ &+ (1-D_{it}^m)D_{it}^s Y_{it}(0,0,1) + D_{it}^m D_{it}^s Y_{it}(0,1,1)\Big\} \\ &+ D_{it} Y_{it}(1,1,1). \end{aligned} \tag{2}$$

I define parameters of interest that isolate the two types of spillover effects on non-targeted units:

[5] The exact definitions of "close" and "far" depend on the particular context. In my main specification in Section 5, I define units within 400 meters of the district to be spatially close and units at least 5,000 meters away from the district to be spatially far. I rely on a parallel trends assumption to interpret the estimates resulting from this "inner-ring vs. outer-ring" approach (Pollmann, 2023). I use an analogous assumption for the market spillover effects.

$$\mathbb{E}\left[Y_{it}(0,1,0) - Y_{it}(0,0,0) \mid D_i = 0, D_i^m = 1, D_i^s = 0\right] \quad (3)$$

$$\mathbb{E}\left[Y_{it}(0,0,1) - Y_{it}(0,0,0) \mid D_i = 0, D_i^m = 0, D_i^s = 1\right]. \quad (4)$$

Each parameter compares the close counterfactual outcomes, either in a market or spatial definition, to the far counterfactual outcome.[6]

In a difference-in-differences framework, identification depends on assuming parallel trends between the non-targeted units that are market- or spatially close and the non-targeted units that are market- and spatially far; in the absence of treatment, the counterfactual outcomes of the close and far units would follow parallel paths. For example, for market spillover effects,

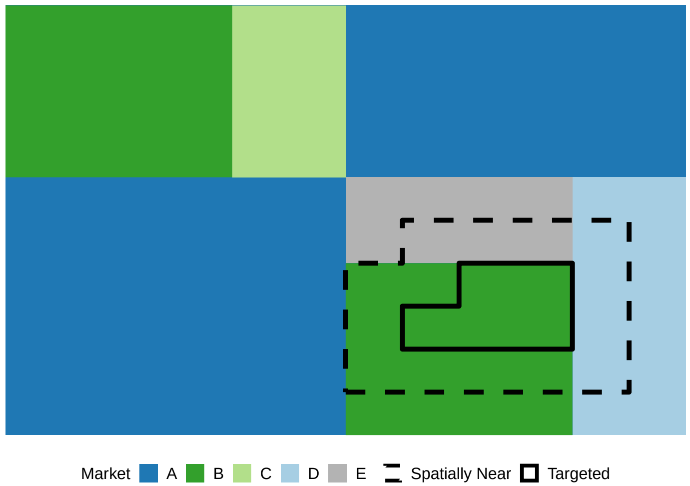
$$\begin{aligned} &\mathbb{E}[Y_{it}(0,0,0) - Y_{i,t-1}(0,0,0) \mid D_i = 0, D_i^m = 1, D_i^s = 0] \\ &\quad = \mathbb{E}[Y_{it}(0,0,0) - Y_{i,t-1}(0,0,0) \mid D_i = 0, D_i^m = 0, D_i^s = 0]. \end{aligned} \quad (5)$$

Figure 1b illustrates the comparison for the market spillover effect. I do not consider units that are either targeted or spatially close (white). The remaining areas that are in the same market as the district (green) are compared to the areas that are in different markets as the district (grey). For the comparison to be valid, I assume that parallel trends hold between the market-close and market-far areas.

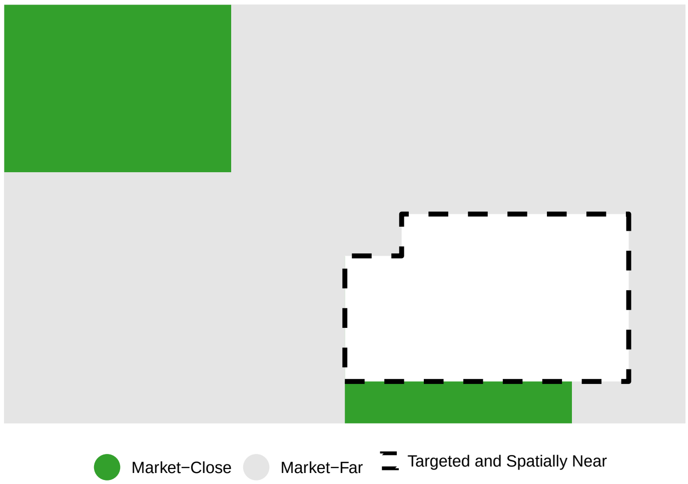
[6] It is also possible to consider the comparison as the counterfactual outcome fixing the unit to being both market and spatially close, $Y_{it}(0,1,1)$, as well as other configurations. Under analogous parallel trends assumptions, researchers can select the parameters that best fit their contexts. For example, this alternative comparison may be best when markets are primarily determined by spatial proximity.

Figure 1: Setup for Treatment-Effect Identification, Examples with Observed Markets

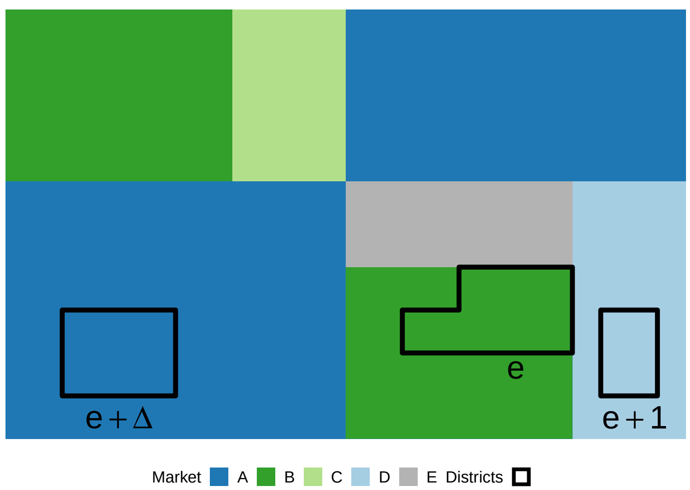
(a) One Treatment Group, Setup



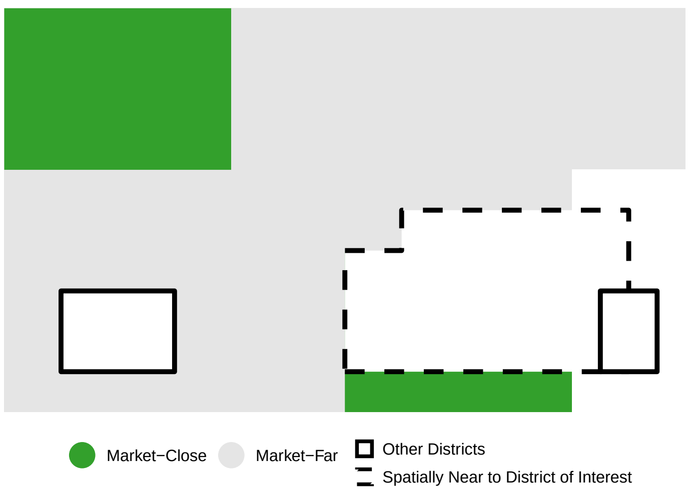
(b) One Treatment Group, Market-Close vs. Market-Far



(c) Multiple Treatment Groups, Setup



(d) Multiple Treatment Groups, Market-Close vs. Market-Far



Note: This figure displays an example to illustrate the setup with observed markets. Panel (a) shows the markets, one district, and a spatial buffer around it. The market labels, A, B, C, D, and E, are arbitrary. Panel (b) shows the non-targeted areas that are market-close and the non-targeted areas that are market-far. I omit areas that are targeted or spatially close. Panels (c) and (d) are analogous except for multiple treatment groups.

2.2 Adaptations for Staggered Treatment Groups

The above exposition allows for treatment to operate through one district. In reality, policies may operate through several districts whose staggered treatment need to be accounted for. I expand the framework to allow for staggered treatment, which is how TIF is implemented in Chicago. I exploit the variation from the staggered treatment in a stacked difference-in-differences approach (Cengiz et al., 2019; Deshpande and Li, 2019; Fadlon and Nielsen, 2015; Guryan, 2004) to estimate the direct treatment effects.

Let d index the districts that are implemented in a staggered fashion. I use E_d to denote the period in which district d is established. I denote the set of units that district d targets as $\mathcal{N}_d$. Then, the definition of the indicator for district d targeting unit i is

$$D_{id} = \begin{cases} 1 & \text{if } d = \arg\min_{d'} \{E_{d'} \mid i \in \mathcal{N}_{d'}\} \\ 0 & \text{otherwise.} \end{cases} \tag{6}$$

That is, if unit i is targeted by many districts, it is assigned to the earliest one. I denote the time period in which unit i begins treatment as E_i. If unit i is never treated, then $E_i = \infty$.

In difference-in-difference settings, the main identifying assumption is that the counterfactual outcomes of the treated group would be parallel to those of the comparison group in the absence of intervention. For some contexts, the comparison group may be the never-treated units (such as the approach in Sun and Abraham, 2021). Many policies of locally targeted economic development aim to direct resources to areas facing chronic disinvestment but also to those that are well-situated to grow further in the future. Then, in the absence of intervention, the targeted areas may have grown differently than the non-targeted areas. This selection calls into question the parallel trends assumption, challenging the comparison of the treated units to the never-treated units. While methods exist to address this issue (e.g., synthetic difference-in-differences as in Arkhangelsky et al., 2021), they rely on observable characteristics that are common between the treated and never-treated units. These observables may not capture the potential for the targeted area to develop. The parallel trends assumption may still lack credibility.

An alternative is to compare the treated units to the units that will be treated in the future, that is the not-yet-treated units. The unobservable characteristics correlated with the potential for growth may be present in all targeted areas, regardless of particular treatment timing. Then, the identifying assumption is that in the absence of treatment, the potential outcomes of the treated areas would be parallel to those of the areas treated in the future. For a unit treated in period e, selecting comparison units that are treated too close to e challenges identification. Instead, I specify that the comparison units must be treated at least Δ periods after e. Restricting the comparison group this way avoids bias from negative weights (Borusyak et al., 2021; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). The appropriate Δ depends on the particular context. While too large of a Δ may result in a comparison group that is too dissimilar (challenging parallel trends) or too thin of support in the data, too small of a Δ limits the capacity to estimate long-run effects (Fadlon and Nielsen, 2015). In this case, the identifying assumption is that in the absence of treatment, the potential outcomes of treated areas and areas treated at least Δ periods later would be parallel.

Figure 1c illustrates the comparison. Suppose the district of interest is the one established in period e. The other two districts are established in $e + 1$ and $e + \Delta$. The direct effect is estimated by comparing the district of interest to the district established in period $e + \Delta$. I extend the idea of this comparison to consider market spillover effects. Figure 1d illustrates the analogous comparison for the market spillover effect. For the district of interest (established in period e), I compare areas that are market-close (green) to those that are market-far (grey). I further refine the market-far comparison group in two ways. First, I exclude areas that are targeted by other districts (solid black lines). Second, I exclude areas that are market-close to districts established less than Δ periods from the district of interest. In the illustration, I exclude market D which overlaps with the district established in $e+1$. All of the areas in white are excluded from the comparison.

For some district d, let D^m_{id} and D^s_{id} indicate if unit i is market- or spatially close to the district. I define another set of indicators to capture if unit i is market- or spatially close to a district established in period e. For $k \in \{s, m\}$,

$$D^k_{ie} = \begin{cases} 1 & \text{there exists } d \text{ such that } D^k_{id} = 1 \text{ and } E_d = e \\ 0 & \text{otherwise.} \end{cases} \tag{7}$$

Unlike in the case of the direct effect, the notation allows for units to be spatially or market-close to multiple districts. By definition, spillover effects are not the result of targeting but rather the result of proximity (either spatial or market). This proximity is a fact of the relevant space and cannot be fully characterized based on the first exposure, as is the case for the direct treatment effects. I define the history of exposure, $\mathcal{D}_{it} := \{(D^s_{ie}, D^m_{ie})\}_{e<t}$ (Abbring and Heckman, 2007). In practice, I rely on fixed effects to account for the history of exposure.

Regression Models. I create three separate datasets to calculate the direct treatment effect, the market spillover effect, and the spatial spillover effect. For each, I create a stacked dataset from the datasets corresponding to each district. For the direct treatment effects, for each district d, I combine units treated by the district ($D_{id} = 1$) and units treated at least Δ periods after district d. I append the districts' datasets into one stacked dataset. For the market spillover effects, for each district d, I include non-targeted units that are market-close to the district ($D^m_{id} = 1$) and not spatially close to the district ($D^s_{id} = 0$). The comparison units are those that are non-targeted and not market-close to any district established before at least Δ periods after E_d. I assign the comparison units the event time of the treatment units. I stack the datasets of each TIF district to create the analysis dataset. For the spatial spillover effects, I follow the same logic as for the market spillover effects.

For the direct treatment effect, I use the model

$$Y_{it} = f(i,t) + \beta D_{id} + \sum_{\ell \in \mathcal{L}} \beta_\ell \mathbf{1}\{t - \ell = E_d\} + \sum_{\ell \in \mathcal{L}} \gamma_\ell D_{id} \mathbf{1}\{t - \ell = E_d\} + \varepsilon_{it}. \tag{8}$$

I specify general fixed effects $f(i,t)$ to represent unit and time fixed effects. The error term is ε_{it}. The number ℓ represents the number of periods until or since treatment. The set of considered event times is $\mathcal{L}$ (in practice, I omit $\ell = -2$ to avoid collinearity resulting from linear dependence with the fixed effects). Because units may appear both as treated (affected in the case of the spillover effects) and comparison units, it is possible to include the indicator D_{id} (with corresponding parameter β) and indicators for time until or since the beginning of treatment (with corresponding parameters $\{\beta_\ell\}_{\ell \in \mathcal{L}}$). The parameters $\{\gamma_\ell\}_{\ell \in \mathcal{L}}$ correspond to the parameters of interest. The model is analogous for the spillover effects except I use D^m_{id} or D^s_{id} instead of D_{id} to correspond to being market- or spatially close to district d. To calculate static treatment effects, I use the analogous model averaging over post-treatment periods:

$$Y_{it} = f(i,t) + \beta D_{id} + \sum_{\ell \in \mathcal{L}} \beta_\ell \mathbf{1}\{t - \ell = E_d\} + \gamma D_{id} \mathbf{1}\{t \geq E_d\} + \varepsilon_{it}. \tag{9}$$

2.3 Characterizing Markets

The above exposition assumes that markets are observed so that it is possible to define the areas that are within the same market as the targeted area. In reality, the market structure is latent and needs to be characterized. One strategy is to estimate markets based on observed characteristics, including spatial proximity. I propose an approach to characterize markets that relies on a revealed preference framework rather than only on observed characteristics. While applicable to many types of markets, I restrict the exposition to housing markets to align with my empirical context. The input data I use to estimate these markets are household moves. I interpret these moves as informative on housing markets. The intuition of the approach is that households move to neighborhoods that are in their choice sets and do not move to neighborhoods that are not.[7] I group the neighborhoods into housing markets so that households are more likely to move within those markets rather than between them. To perform the clustering, I apply Stochastic Block Modeling (SBM). I use a revealed preference framework as sufficient to interpret the household moves as revealing the housing markets.

Figure 2a maps the example housing markets that I previously assumed to observed. Now, consider that they are unobserved. What I observe is household movement between neighborhoods. I represent neighborhoods visually with the black grid. The input to SBM is a matrix (the terminology for this type of matrix is an "adjacency" matrix) that represents whether there is a household that moves between each neighborhood during the data span. SBM estimates the number of groupings and the transition matrix between them, which corresponds to a mapping between neighborhoods and groupings. Figure 2b displays the output of the algorithm using simulated data. Each point along the diameter of the circle represents one neighborhood. Households may stay within neighborhoods. If households move between neighborhoods, one line on the interior of the circle indicates a connection between those neighborhoods. A connection means that at least one household moves between the neighborhoods. The estimated markets are such that there are more movements within them than between them.

[7] I use the term "neighborhoods" in a general way here to denote some geographic definition. Below in my empirical estimates, I use Census blocks.

Underlying Revealed Preference Framework. My approach interprets household moves as informative on the housing markets. Several models of household decisions may be consistent with this interpretation. I introduce a revealed preference framework in which I do not explicitly model the individual contributions of the observed and unobserved inputs to the households' decision problem.

Let h index households and j index neighborhoods. In each time period t, household h chooses the neighborhood that maximizes its utility. The utility is a function of observed and unobserved amenities of the neighborhood, $\boldsymbol{X}_{jt}$, a household-specific shock, ξ_{ht}, and a household-specific satiation point, $\bar{\boldsymbol{X}}_h$. This satiation point results in households moving to destination neighborhoods in which the bundles of amenities are arbitrarily close to the those in the origin neighborhoods. Under this setup, the observed neighborhood choice for household h in period t is

$$J_{ht} = \arg\max_{j \in \mathcal{J}} U(\boldsymbol{X}_{jt}, \xi_{ht}, \bar{\boldsymbol{X}}_h). \tag{10}$$

This model imposes several implicit restrictions on household decisions. First, households make static decisions in each period. They cannot look forward to anticipate policy changes or equity accumulation. This restriction aligns with the data I use to implement the characterization of housing markets, which are most practical to match across consecutive years rather than across a longer time frame. Second, households face constant choice sets over time. For a given household, even if the neighborhood choice would be different across states of the world (the distribution of ξ_{ht}), the set of choices that the household would move to remains constant across time. This setup allows me to interpret observed moves as informative on the latent housing market; even with a large shock, the household will not move too far from its satiation point. This framework aligns with empirical and theoretical evidence of residential sorting in which the same type of households tend to move to the same type of areas (Ferreira et al., 2023). It provides sufficient structure to apply SBM and estimate housing markets.

Stochastic Blockmodeling for Defining Markets. Given the assumptions of the revealed preference framework, households tend to move between areas that are in the same housing markets and do not move between areas that are not in the same housing markets. To bring this idea to data, I use Stochastic Block Modeling (SBM). Based on foundational work in clustering network data (Breiger et al., 1975; Holland et al., 1983; Nowicki and Snijders, 2001; Snijders and Nowicki, 1997), I apply *a posteriori* SBM in which the latent structure is to be estimated through modelling rather than assumed to be known. Appendix D provides more details on the method and its estimation.

I define a $J \times J$ adjacency matrix, $\boldsymbol{A}^t$, where J is the number of neighborhoods. Each element, $\boldsymbol{A}^t_{jk}$, represents the number of households that move from neighborhood j to neighborhood k in the sequential time periods t and $t+1$. Note that $\boldsymbol{A}^t$ need not be symmetric, meaning that just because households move from neighborhood j to k does not mean that households need to move from neighborhood k to j. I aggregate the matrices across all time periods into one adjacency matrix $\boldsymbol{A}$ where

$$\boldsymbol{A}_{jk} = \begin{cases} 1 & \text{there is at least one } t \text{ such that } \boldsymbol{A}^t_{jk} > 0 \\ 0 & \text{otherwise.} \end{cases} \tag{11}$$

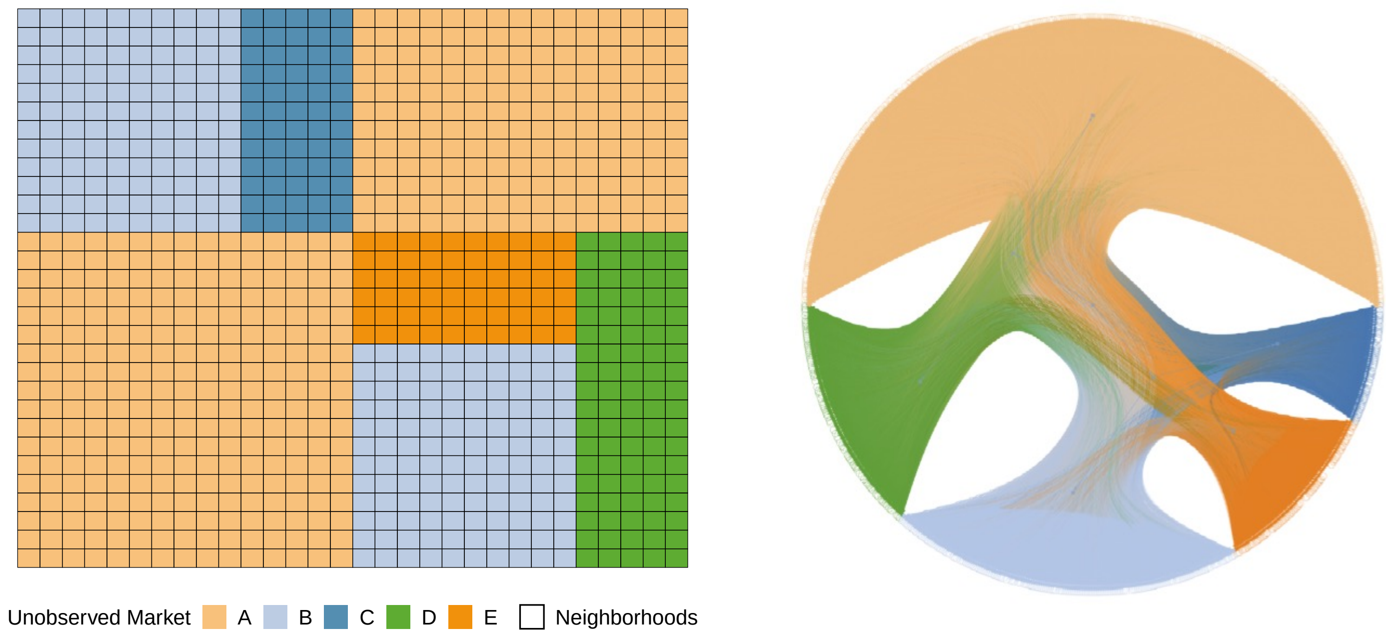
The binary SBM estimates three objects. It estimates the number of groupings, G, the mapping between neighborhoods and groupings, and the transition matrix with the probability of moving to a neighborhood in grouping m conditional on living in a neighborhood in grouping n. Each neighborhood belongs to one and only one grouping.[8]

[8] Aggregating household movement across time to characterize housing markets may introduce endogeneity concerns once I use the housing markets to estimate spillover effects. In Appendix Figure A.7, I demonstrate that the adjacency matrices remain stable over the time period and discuss concerns of alternative approaches.

Figure 2: Example of Mapping between Neighborhoods and Estimated Markets

(a) Neighborhoods and Unobserved Markets

(b) Estimated Markets from Simulated Household Moves



Note: This figure illustrates the approach to characterize markets. I now assume that the markets from Section 2.1 are unobserved. Panel (a) overlays a grid to represent the neighborhoods (e.g., Census blocks). Panel (b) is the output of a simulation of the example markets on the left. I simulate the adjacency matrix to represent household moves between neighborhoods. Each point of the circle represents one neighborhood and each line represents a connection between the two neighborhoods. A connection means that at least one household moves between the neighborhoods. SBM takes the adjacency matrix and classifies the neighborhoods into five groupings, indicated with different colors. See Appendix D for more details on the simulation.

Intuitively, given a proposed number of groupings, the estimation procedure categorizes the neighborhoods so that the probabilities of transitioning between groupings maximizes the likelihood function. In practice, the likelihood function is specified to contain a penalty term related to the number of groupings. This specification allows for the number of groups to be estimated in addition to the mapping from neighborhoods to groupings. This approach results in a resolution limit in which the number of groupings possible to estimate has an upper bound dictated by the number of neighborhoods. I use a nested model structure to increase this limit (Peixoto, 2014b). The nested model implements the estimation to categorize the neighborhoods into groupings. Then, it repeats the estimation to categorize those groupings into larger sub-groupings. The algorithm continues until there is one grouping. The output of this approach confers benefits for the application as well. I interpret theses sub-groupings as "broader" housing markets, which are useful to control for broader market trends in the regression model.

2.3.1 Estimated Housing Markets for the City of Chicago

Data on Household Moves. I longitudinally match a cross-sectional dataset to observe household moves. InfoUSA is a commercial dataset that aims to include every household in the U.S. observed annually between 2006 and 2020 (Data Axle, 2020). I clean the cross-sections of the InfoUSA data, restricting the data to households in Cook County (approximately 2.3 million households). I match the addresses' geographic coordinates with the 1990 definitions of Census blocks. I consider each property's Census block to be its "neighborhood" for the purposes of SBM. I assign all Census blocks outside of the City of Chicago as one suburban neighborhood. I match households between consecutive years (2006-2007, 2007-2008, ..., 2019-2020) using a household ID. The households that do not match may have some inconsistency in household ID, move outside of Cook County, or be absorbed in another household. This matching procedure results in matching approximately 75% of the households.

Estimated Housing Markets. I estimate 146 housing markets in Chicago as shown in Figure 3a. See Appendix Figure A.6 for maps of the broader markets estimated in the nested algorithm. I evaluate the extent to which observed and unobserved factors contribute to the groupings of the housing markets in Figure 3b. I regress the market-level average residential property value on average characteristics from the 2000 Decennial Census. As the first bar indicates, only 58.9% of the variation in the markets is due to the observed characteristics. I decompose this variation by each of the observed characteristics. Even after accounting for correlation between the characteristics (Israeli, 2007), income is the largest determinant of the housing markets.

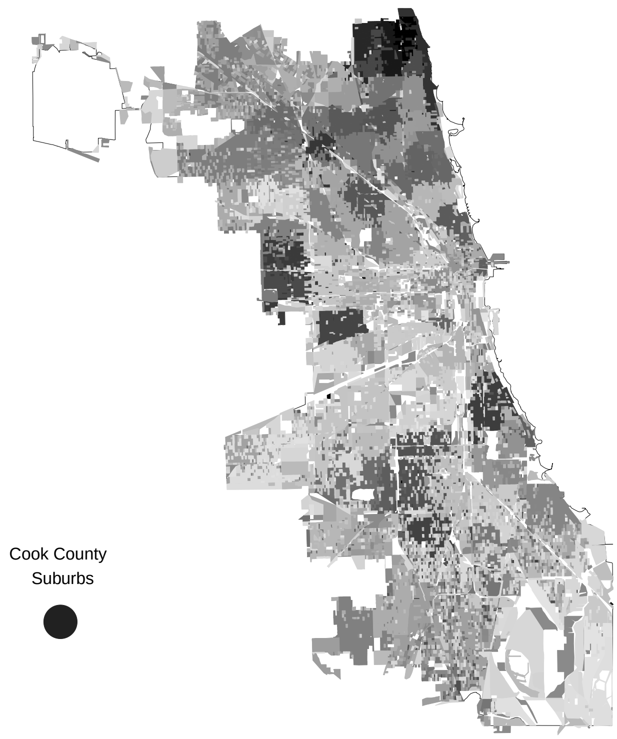
Table 1 displays the proportion of moves that are across rather than within estimated housing markets. Each year an average of 36.3% of moves are across markets and 64.8% of moves are within markets. The larger percentage of moves being within market aligns with the intuition that households tend to move within markets. This pattern holds across the income distribution and regardless of whether the household rents or owns.

3 Tax Increment Financing

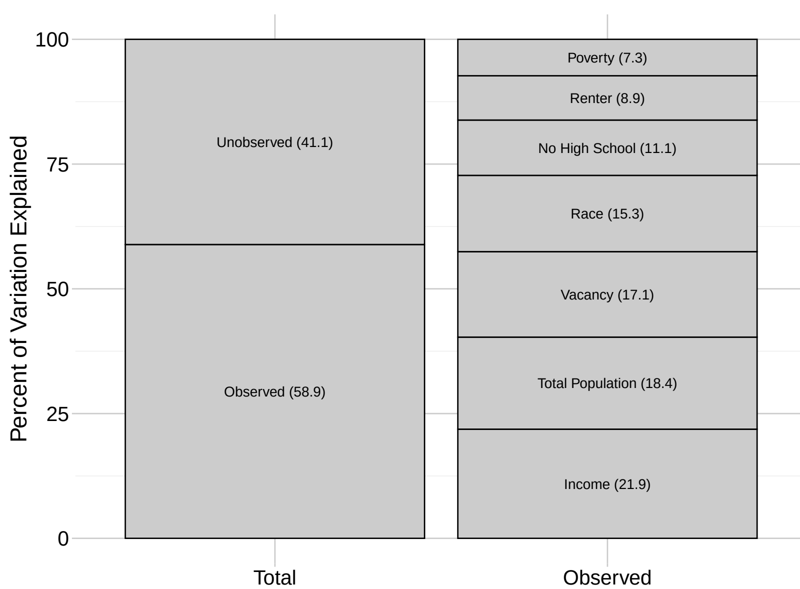
In this section, I describe the policy to which I apply my approach of estimating market spillover effects. Tax Increment Financing (TIF) is a policy that facilitates a wide range of economic development activities within defined geographic areas called TIF districts. Once a municipality establishes a TIF district, any increase in the property taxes therein above the base-year amount go towards economic development activities within the district. Meanwhile, the other taxing bodies continue to collect property taxes on the base-year amount. After the TIF district ends (usually 23 years in Chicago), the taxing bodies collect property taxes on the full amount, which in successful TIF districts is larger than it would have been in the absence of TIF (Department of Planning and Development, 2020). TIF is widespread; it is used in almost all U.S. states (Kriz and Johnson, 2019) and increasingly outside the U.S. (Baker et al., 2016) because of its potential to subsidize a broad array of

Figure 3: Estimated Markets in the City of Chicago

(a) Estimated Markets (146 Markets)



(b) Decomposition of Market Average Property Values



Note: Panel (a) displays all of the housing markets that the algorithm estimates. Panels (b) displays a decomposition of the average housing value of the estimated markets. The first bar of the figure displays the percent of the R^2 that is due to observed and unobserved factors using the observed factors in the right bar. The second bar decomposes the amount of observed variation due to each factor (Israeli, 2007).

Table 1: Household Moves Across and Within Estimated Markets

	Proportion of Moves		Total	
	Across Markets	Within Markets	Households	Moves
All	0.352	0.648	871,627	108,061
Household Income				
1st Quartile	0.363	0.637	319,453	42,996
2nd Quartile	0.359	0.641	220,272	25,936
3rd Quartile	0.335	0.665	181,438	21,534
4th Quartile	0.291	0.709	150,465	17,594
Tenure				
Renter	0.317	0.683	446,523	42,329
Owner	0.363	0.637	188,191	27,751

Note: All numbers are yearly averages using InfoUSA. I determine cutoffs for the quartiles using the distribution of household income in the base year (2006). For the calculation by tenure, I omit households with undetermined renter or owner status. I use household income and renter or owner status in the base year to classify households.

economic development activities through land value capture (Klacik and Nunn, 2001).

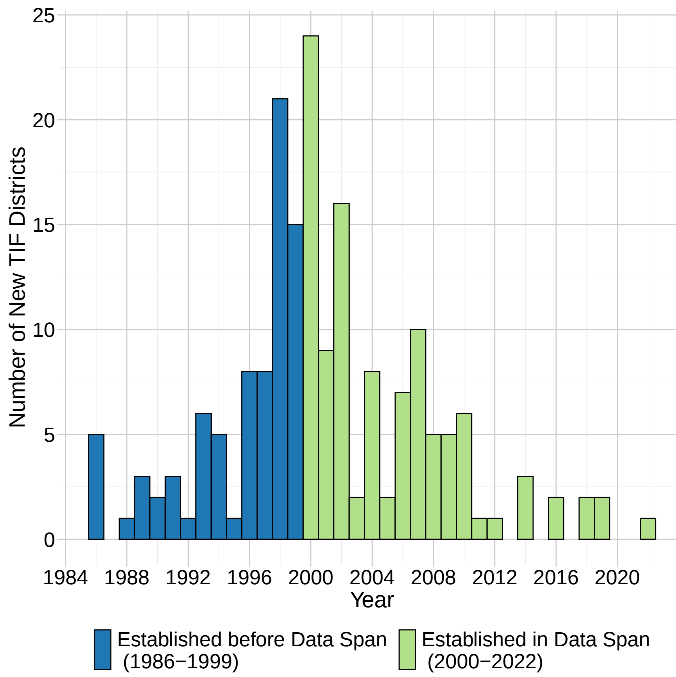
Chicago's implementation of TIF is emblematic (Craft and Weber, 2019; Healey and McCormick, 1999). TIF began in Chicago in 1984 and the first district was established in 1986. Its use started expanding in the 1990s when the number of TIF districts and property values therein increased substantially. Figure 4 demonstrates the geographic spread of TIF districts throughout the city. I highlight the 106 TIF districts established between 2000 and 2022 that overlap with the data span for my analysis. In 2021, TIF revenues were over \$1.2 billion (2020 USD), accounting for 14.5% of the total property taxes in Chicago (Cook County Clerk's Office, 2020).

The Chicago Department of Planning and Development (DPD), housed in the Office of the Mayor, coordinates with ward aldermen, special tax districts, private developers, and community stakeholders to implement TIF.[9] TIF districts must fulfill eligibility criteria: the

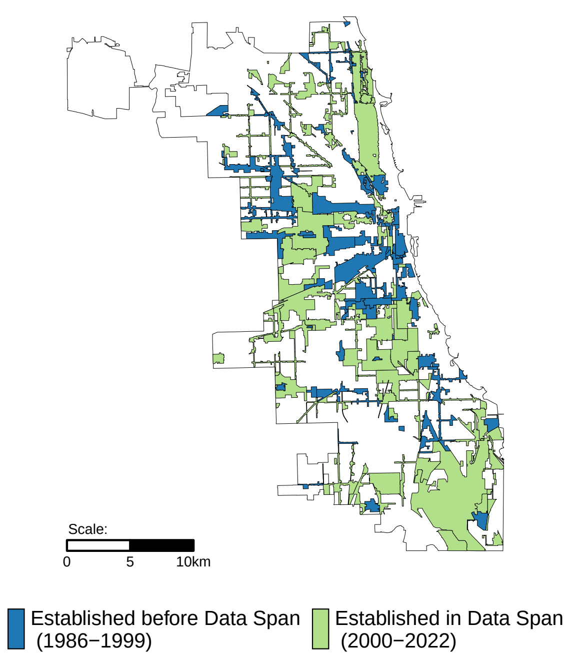
[9] See Appendix A for more details on TIF implementation.

Figure 4: Tax Increment Financing Districts in Chicago

(a) Districts by Implementation Date



(b) Geographic Distribution



Note: Panel (a) plots the number of new TIF districts established by year. Panel (b) maps the TIF districts that have been designated within Chicago. The data for the main analysis span 1999-2022, so I use districts established after 1999 in my analysis. In both panels, light green corresponds to districts with those in my period of analysis ($N = 106$). Dark blue corresponds to districts established before my period of analysis ($N = 79$). Data from City of Chicago (2022).

properties, infrastructure, and built environment of the TIF district must exhibit "blight,"[10] such as inadequate utilities or dilapidation.[11] Districts are designated as blight districts if at least five blight factors are present, conservation districts if at least three blight factors are present and the majority of the structures are at least 35 years old, both blight and conservation districts if both sets of criteria are satisfied, or transit districts which do not have any blight requirements but must be used for transportation projects. In Section 5, I perform heterogeneity analysis by these designations.

In addition to fulfilling the criteria for one of these designations, there must be justification of the "but for" clause, i.e., the area would not develop "but for" the implementation of TIF. This requirement is arguably more ambiguous and flexible than the first, and is often completed in an ad hoc manner (Briffault, 2010). A redevelopment plan outlines the planned work, the actors who will perform the work, land use plans, an estimated budget, and the expected increase in revenues. The DPD alters the redevelopment plan based on feedback from public hearings and committees comprised of stakeholders, and it goes to vote in the City Council. Once the City Council approves the district, the Cook County Clerk incorporates the district as a taxing body (Department of Planning and Development, 2020). The process to initiate, design, and approve a TIF district lasts approximately one year.

Once the city establishes a TIF district, the natural appreciation of property values may gradually accrue and fund the activities outlined in the redevelopment plan. Alternatively, the city can rely on private investments early in the TIF district's existence or borrow against the expected increase in property values as a result of the TIF-funded interventions (Luby et al., 2019).[12] TIF funds may be used within the district for a broad array of economic

[10] I acknowledge the racial implications of the term "blight" (e.g., Mock, 2017; Pritchett, 2003; Weber, 2015). In this writing, I use the term referring to its historic and current use in TIF legislation and implementation. Weber and O'Neill-Kohl (2013) argue that the close relationship between federal urban renewal policies and TIF policies led to the same reliance on the concept of "blight."

[11] See Appendix Table A.1 for a full list of the factors.

[12] For the 148 districts with information on project costs, the median total cost per project is $\$9,818,150$ with $\$2,843,244$ being the median subsidy amount from the TIF district. Cumulatively across projects with recorded costs, the median district involves $\$59,900,450$ of total investment, $\$13,945,775$ of which

development activities such as property acquisition and demolition, environmental remediation, or construction of public works.[13] These activities may contribute to different types of development including residential, commercial, industrial, institutional, transportation, or mixed-use which combine any of the above. Most districts during my period of analysis are intended for mixed-use development, with a focus on residential and commercial development. In Section 5, I perform heterogeneity analysis by these development purposes.

To assess the direct effectiveness of TIF and study the market spillover effects, I use property values as my main outcome of interest. Because TIF operates through capturing increases in property taxes to be reinvested into the TIF district, increasing property values are critical to TIF's success (Youngman, 2016). Furthermore, property values capitalize changes in amenities and can be an appropriate outcome to consider changes as a result of TIF (Rosen, 1974). Additionally, I estimate treatment effects for household and firm outcomes to study impacts of TIF on household and firm responses.

4 Data

4.1 Description of Data Sources

I combine data on the implementation of TIF in Chicago with data on properties, households, and Census Blocks. Appendix B.1 provides additional detail on the data sources and my data construction.

TIF Implementation. The City of Chicago and Cook County publish data related to the implementation of TIF. I use the shapefiles of the TIF district boundaries to define treated

is subsidized with TIF funds. All values are in 2020 USD. The costs only include projects completed in partnership with private firms or other taxing bodies (e.g., the school district). They do not include costs from infrastructure projects or other types of TIF-funded investment. In Section 5, I perform heterogeneity analysis by the amount of costs and subsidies associated with districts for which they are observed.

[13] In practice, there are two instances in which the funds raised within the TIF district are used outside of it. First, TIF funds can be used for an adjacent public space (Chen, 2013). Second, TIF funds can be "ported" between adjacent districts. I consider both of these instances as still valid in my empirical framework. The treatment that occurs inside the targeted area may include these implementation realities.

areas. For each TIF district, I observe the date on which City Council approved its establishment, the expiration date, the eligibility criteria (blight, conservation, both, or transit), and the purpose of the district (residential, commercial, industrial, institutional, transportation, or mixed-use). I combine information on projects and costs within the TIF district from two sources. I observe information on projects that are considered to be Redevelopment Agreements (RDA), which are completed in partnership with private developers, or Intergovernmental Agreements, which are completed in partnership with other taxing bodies (City of Chicago, 2022). These data do not include other types of TIF-funded investment, most notably infrastructure projects. I use them to construct proxies of the amount of costs involved with TIF-funded projects.

Property Values and Characteristics. The Cook County Assessor's Office (CCA) provides information on the universe of properties in the county between 1999 and 2022, including assessed values, sale transactions, and property characteristics (Cook County Government, 2023). I restrict the sample to single-family residential properties with observed geographic coordinates. Most TIF districts during my period of analysis are for mixed-use purposes (see Appendix Table A.2). I focus my analysis on residential property values to consider the capitalization of these varied purposes for households.

I focus on assessed values as my measure of choice for property values. Although sale prices reveal the observed market price, they provide a selected sample of properties that sell (Bishop et al., 2020). For empirical frameworks that rely on panel data, the issue of selection is even more severe (e.g., Gatzlaff and Haurin, 1994). Unlike sale prices, assessed values are observed annually for every property.[14] However, assessed values are predicted based on the recent sales of similarly situated properties (Ross et al., 2019) and there may be some non-classical prediction error (Avenancio-León and Howard, 2020).[15] For my main

[14]The CCA officially recalculates assessed values for properties in the City of Chicago every three years. In the intervening years, there is additional variation in property values from appeals and inflation adjustments.

[15]In Appendix B.2, I expand on the problems with each of these measures of property values and introduce additional predictions of property values to evaluate the degree of bias using a measurement model.

analysis, I use assessed values as my main outcome of interest. I perform the analysis at the Census block level ($18,993$ Census blocks) to aggregate the individual properties and reduce the impact of possible prediction error. I use the 1990 definitions of Census Blocks for analysis. This avoids concerns that the TIF districts established starting in 2000 would affect the boundaries of the blocks (Manson et al., 2022).

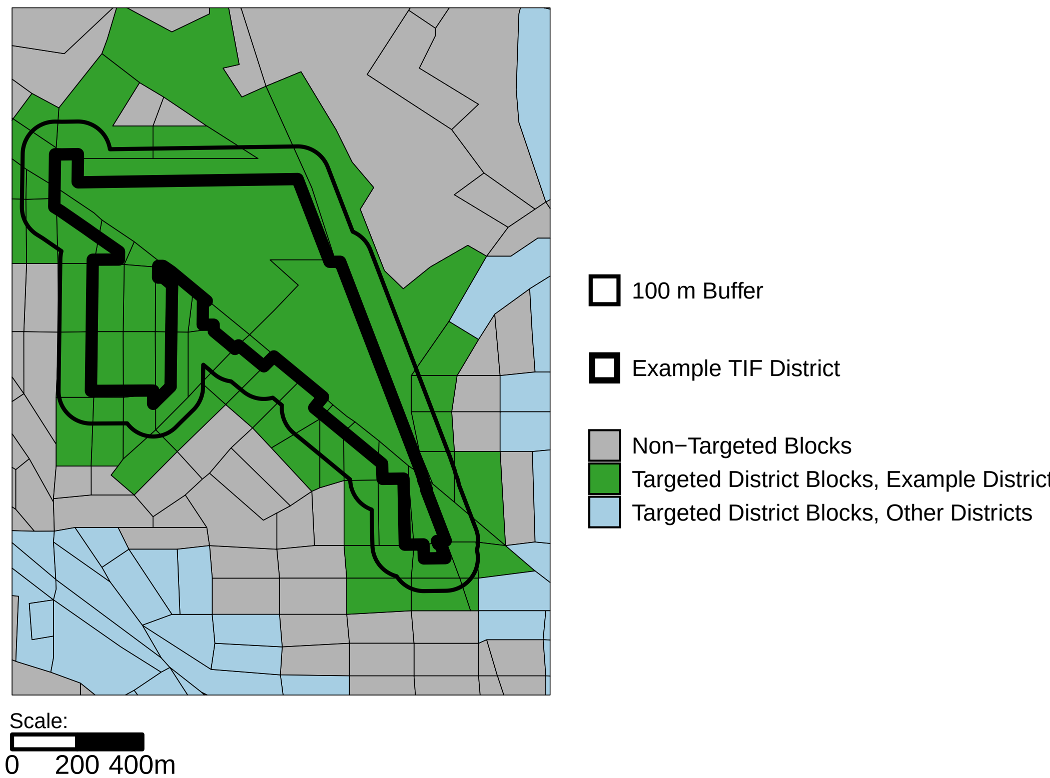
Household and Firm Characteristics. To measure household characteristics, I use the block-level Decennial Census (1990, 2000, and 2020) and the block group-level 5-year American Community Survey (ACS) estimates between 2005-2009 and 2017-2021 (U.S. Census Bureau, 2020, 2021). For each, I approximate the values to Census blocks. I measure median household income, proportion of the block below the poverty line, the proportion of the block's households that are renters, and the racial and ethnic distribution (I consolidate the categories into non-Hispanic Black, non-Hispanic white, and Hispanic). I assign the 5-year ACS estimates to the first year for the purposes of creating a time series for analysis.

I measure firm characteristics from two sources. The Chicago Data Portal publishes information on new business licenses and their approval dates starting in 2000 (Business Affairs and Consumer Protection, 2023). I observe assessed values of commercial and industrial properties in the dataset of the Cook County Assessor (Cook County Government, 2023). I use land assessed value instead of total assessed value given the large variance in building property values. For both household and firm outcomes, I restrict my analysis to TIF districts established after 2000 due to sparse data prior to that year.

4.2 Definition of Targeted Areas

I define treated Census blocks as those within 100 meters of TIF districts. This definition allows for TIF districts primarily covering commercial or industrial properties to directly treat residential properties within neighboring Census blocks. Figure 5 shows some example districts. While some of the example districts include residential properties, those that are narrow and irregularly shaped tend to follow commercial corridors. Defining treatment to be within 100 meters includes sufficient sample size while excluding blocks too far from TIF districts.

Figure 5: Treatment Definition in an Example TIF District and Its Surrounding Area



Note: This map demonstrates the definition of treatment I use in the analysis below. I map Census blocks and an example TIF district (thick black line). The thin line surrounding the TIF district represents a 100 meter buffer. I define the green Census blocks to be targeted by the TIF district because they overlap with the buffer. The light blue Census blocks are targeted by other TIF districts and the grey Census blocks are non-targeted. Data from City of Chicago (2020); Cook County Government (2023).

I define treatment timing as the establishment of the TIF district. If a block overlaps with more than one TIF district, I assign it to the district that was established earliest. Once a TIF district is established, the incremental tax revenues begin to accrue and the city may engage financial instruments to facilitate private development and TIF-funded infrastructure. Even if there is delay between the establishment of the TIF district and the actual development, there is a mutual agreement between stakeholders and there may be signaling effects.

My definition of treatment results in $7,416$ never-treated blocks and $11,577$ treated blocks, $6,907$ of which are treated starting in 2000. Although there are 106 TIF districts established in 2000 or later, I only use 94 in my analysis that have sufficient overlap with Census blocks with residential properties. I use a framework that requires observing at least one period prior to the start of treatment. This framework does not allow me study the blocks treated by districts established prior to 2000. Thus, I exclude these "always-treated" blocks from my analysis.

4.3 Summary Statistics

Table 2 describes targeted and non-targeted Census blocks. Panel (a) displays the outcomes from the CCA in the base year of 1999. Compared to the never-treated blocks, ever-treated blocks have lower assessed values. This negative selection corresponds to the blight factors that comprise eligibility criteria of TIF districts. Relative to non-targeted areas, households in targeted areas have lower income, are more likely to be renters, and are more likely to be non-Hispanic Black. The flow of new business licenses are similar across targeted and non-targeted areas. While the commercial land value is lower in targeted areas, the industrial land value is higher. This may be a result of TIF districts locating in historically industrial areas that are not so well-situated for commercial development. However, the large standard deviations on both variables highlight a large amount of heterogeneity.

In addition to observed differences between targeted and non-targeted blocks, there may be unobserved differences in not only the levels but the trends in potential outcomes. Many papers evaluating TIF and other place-based policies rely on matching strategies to compare treated to never-treated areas (e.g., Czurylo, 2023). Instead, my main empirical strategy relies on comparing treated blocks to blocks that are eventually treated in a staggered fashion.

Table 2: Baseline Property, Household, and Firm Census-Block Characteristics

	Targeted	Non-Targeted
Number of TIF Districts	94	0
Number of Census Blocks	6,907	7,416
Number of Residential Properties (1999)	150,886	207,334
Residential Property Characteristics (1999)		
Assessed Value	171,629.87	195,059.42
	(149,572.13)	(251,718.96)
Sale Price	233,157.15	264,210.93
	(305,920.37)	(310,214.43)
Transactions	0.03	0.02
	(0.33)	(0.16)
Household Characteristics (1990)		
Median Income	53,060.17	61,548.48
	(24,726.36)	(20,476.21)
Poverty (proportion)	0.21	0.15
	(0.15)	(0.11)
Renter (proportion)	0.58	0.44
	(0.26)	(0.29)
Non-Hispanic Black (proportion)	0.38	0.28
	(0.43)	(0.40)
Non-Hispanic White (proportion)	0.29	0.39
	(0.31)	(0.35)
Hispanic (proportion)	0.27	0.28
	(0.31)	(0.31)
Firm Characteristics (2000)		
New Business Licenses	0.03	0.02
	(0.24)	(0.25)
Commercial Land Assessed Value	562,039.7	825,580.8
	(3,400,677.1)	(4,024,911.6)
Industrial Land Assessed Value	406,197.6	353,330.2
	(747,124.4)	(801,767.6)

Note: Targeted Census blocks are within 100 meters of TIF districts established 2000-2022. For all variables except transactions and business licenses, I report means (standard deviations) weighing by the number of properties or households. All monetary values are in 2020 USD.

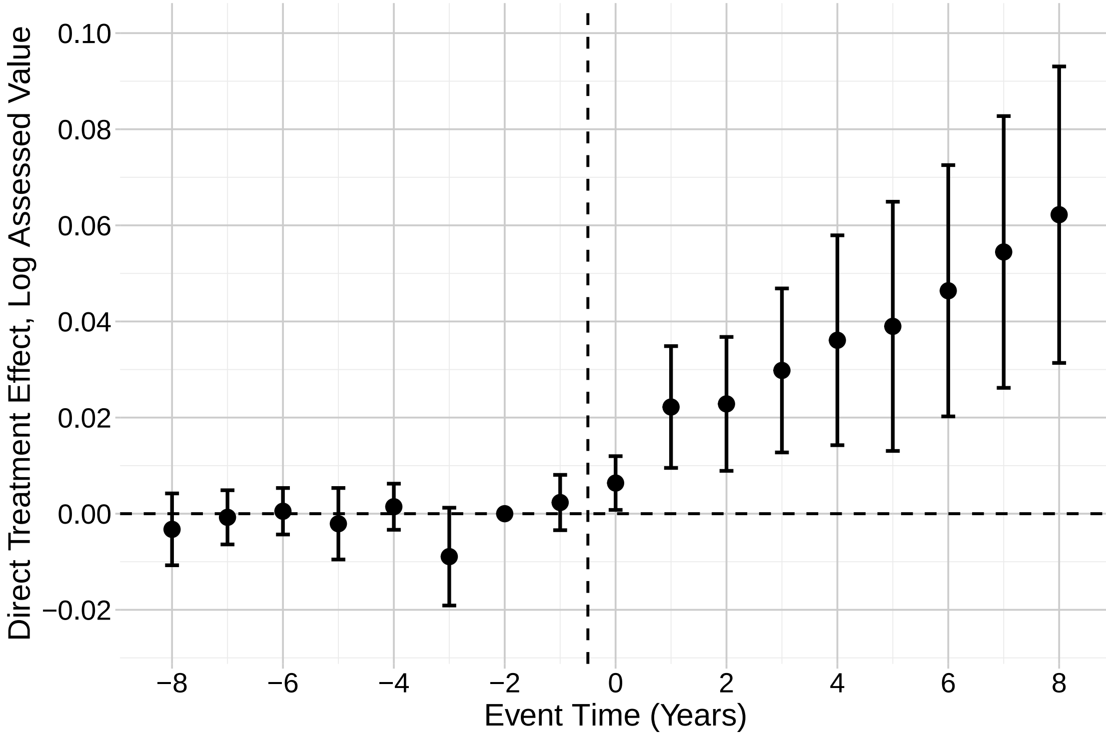
5 Empirical Estimates of Direct and Spillover Effects

The outcome of interest that I consider is property values. At the Census block-level, I use the log of the average assessed values (inflated to 2020 USD) of residential properties. For the direct treatment effects, I implement the model of Equation (8) with the stacked dataset of targeted Census blocks. I allow for unobserved heterogeneity that varies across time within Census tracts by including Census tract $\times$ year fixed effects. The inclusion of these fixed effects requires parallel trends assumption to hold within Census tract. It also requires sufficient variation of treatment status within tract. In Appendix Figure A.3, I illustrate an example of the geographic overlap of Census tracts, TIF districts, and Census blocks. Due to the spatial clustering of the TIF districts, Census tracts contain blocks that are targeted by different districts as well as blocks that are non-targeted. I cluster standard errors at the level of the TIF district (Abadie et al., 2022) to account for the fact that Census blocks appear as both treated and control units within the regression (Deshpande and Li, 2019). I allow for the comparison group to be targeted by districts established eight years after the district of interest.

Figure 6 plots the estimates of the dynamic treatment effects of TIF on property values using the specification in Equation (8). There is no evidence of anticipation before two years prior to the establishment of the TIF district. Once the TIF district is established, the treatment effects are significant, positive, and increasing over the ensuing eight years. The corresponding static estimate from Equation (9) is 0.0306 (standard error 0.0085). Because I take the log of the outcome variable, the interpretation is that property values increase by an average of 3.06% after the establishment of the TIF district.

Appendix Figure A.2 displays the sensitivity to different inference. Even with standard errors that allow for spatial correlation (Conley, 1999), the main estimates remain significant. This suggests that the district-clustered standard errors adequately account for spatial correlation while also allowing for correlation within TIF districts due to the provision of

Figure 6: Dynamic Direct Treatment Effect, Log Assessed Property Value



Note: This plot displays estimates for $\{\gamma_\ell\}_{\ell\in\mathcal{L}}$ from Equation (8). The outcome is the log of the average assessed value in 2020 USD within 1990 Census blocks. The specification compares blocks that are treated to those that are not yet treated. All estimates are relative to event time −2. The error bars indicate 95% confidence intervals with TIF district-clustered standard errors.

the policy. I also calculate the estimate with Census block and year fixed effects rather than Census block and Census tract × year fixed effects. The estimates are no longer significant and the pre-trends deviate from zero without allowing for within-tract heterogeneity. However, the point estimate is similar to that from my main specification. This supports that the tract × year fixed effects account for relevant unobserved heterogeneity to allow for identification without subsuming excessive variation.

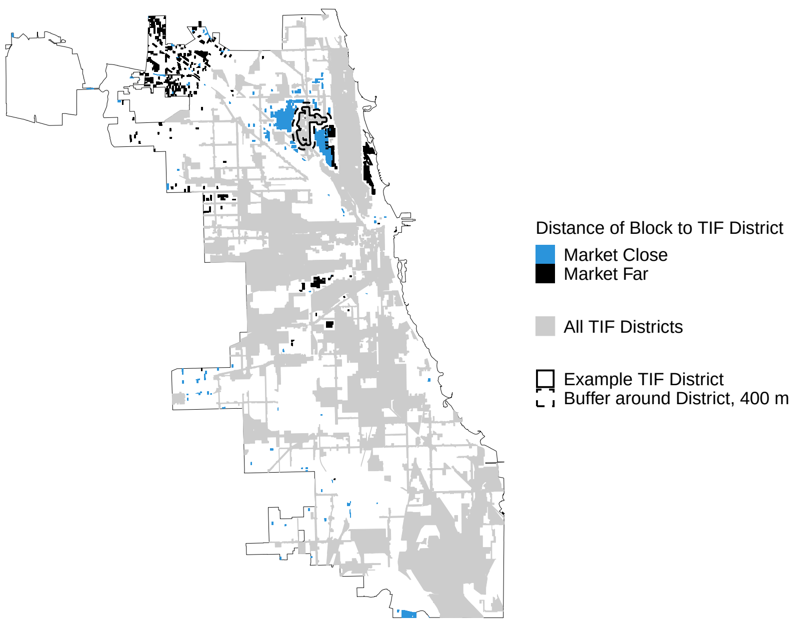
Following the approach in Section 2, I estimate spillover effects for non-targeted units. Figure 7a shows an example of the comparison of market-close vs. market-far for one district. I omit all Census blocks that are within 400 meters of the TIF district. This corresponds to the threshold I use for the spatial spillover effects as exemplified in Figure 7b. I consider blocks within 400 meters of the TIF district to be spatially close and blocks more than 5,000 kilometers to be spatially far.[16] Census tract $\times$ year fixed effects are too fine to account for unobserved heterogeneity for these comparisons. Instead, I include several fixed effects with year interactions: the purpose of the TIF district (e.g., residential) and the broad geographic side of the city (North, West, or South). These fixed effects account for unobserved trends due the development and land-use around the TIF district and trends by regions of the city. For the market spillover effects, I add fixed effects for the broader markets that I estimate from the nested algorithm. This controls for trends due to the broader market structure. Analogously, for the spatial spillover effects, I add fixed effects for Public Use Microdata Areas (PUMAs) to control for trends due to broader spatial areas. Due to thin support, I allow for the comparison group to be market- or spatially close to districts established only five years after the district of interest.

Figure 8 displays the dynamic spillover effects. In both cases, estimates before two years prior to TIF do no vary significantly from zero, supporting the empirical design. The spatial spillover effects exhibit large standard errors due to a small number of units that are spatially close. The estimates are not significant and the static effect is 0.006 (standard error 0.0129), which implies a 0.6% increase in property values within 400 meters of the TIF districts. The market spillover effects are negative and decreasing beginning two years after the start of TIF, with a static estimate of -0.0157 (standard error 0.0022). This negative effect suggests that there is relocation of investments to the targeted areas away from non-targeted areas in the same housing markets. In Appendix Table A.6, I present estimates of the spillover effects for different distances. The spatial spillover effects remain small in magnitude and statistically insignificant. The corresponding market spillover effects increase in magnitude, reaching -1.84% when removing blocks up to 1,000 meters from the TIF district. This pattern illustrates robustness of the opposite direction of the two types of spillover effects. Henceforth, I focus on the direct treatment effects and market spillover effects given the small magnitude of the spatial spillover effect estimates.

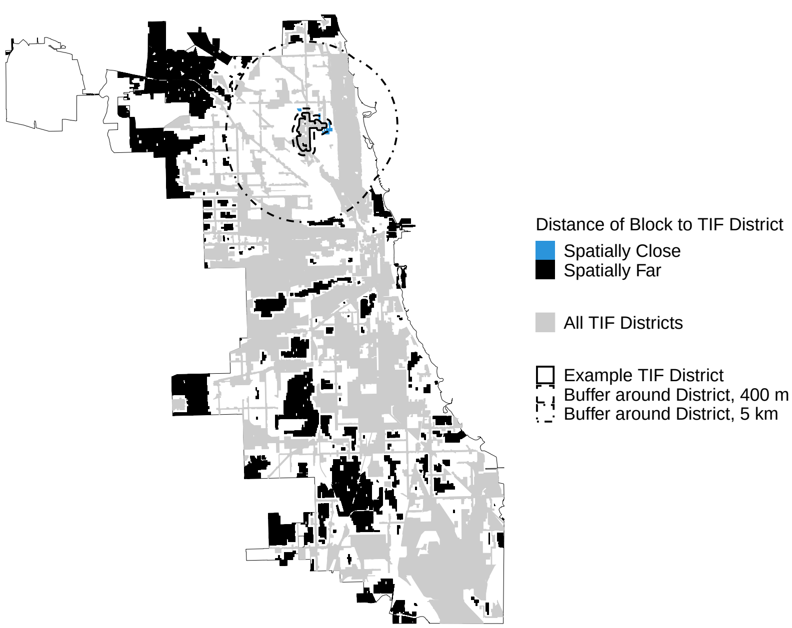
[16] In Appendix Table A.7, I present estimates varying the size of the inner ring.

Figure 7: Examples of Actual Comparisons for Computing Market (Non-Spatial) and Spatial Spillover Effects

(a) Market (Non-Spatial)



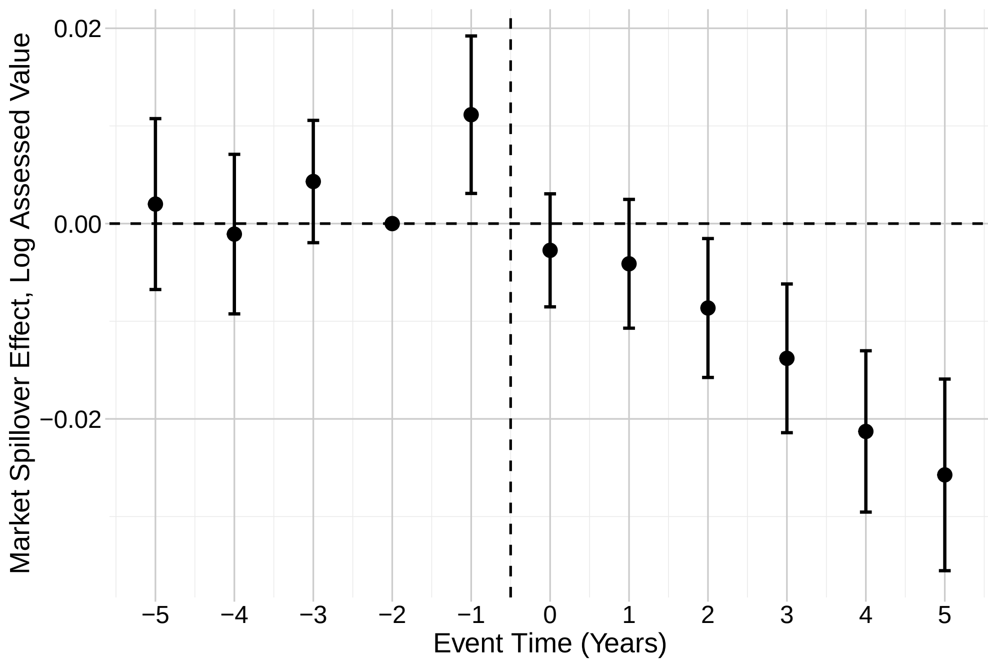
(b) Spatial



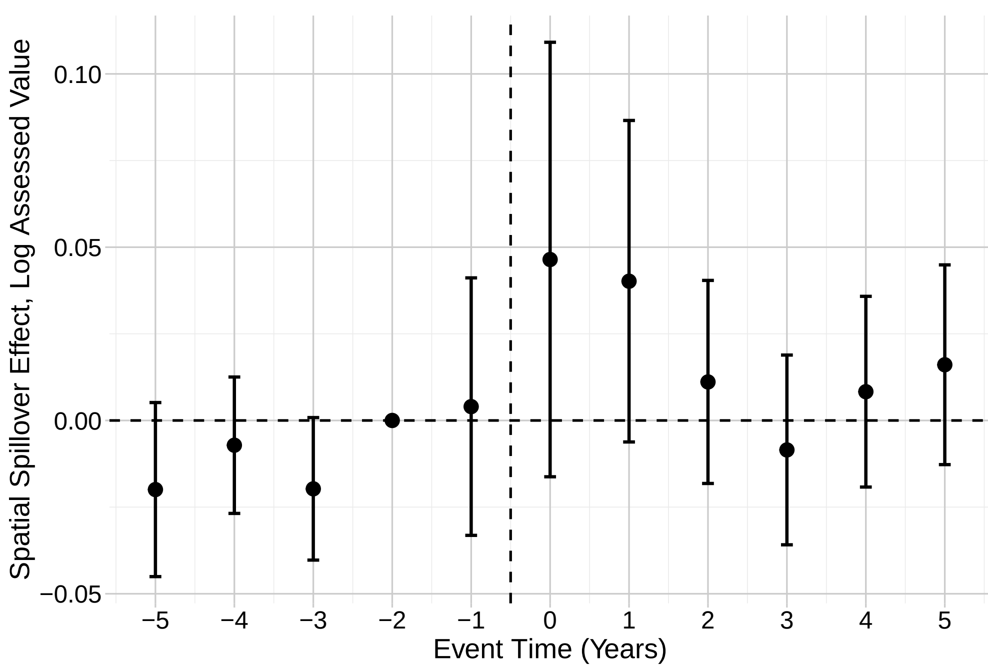
Note: These maps show one TIF district to exemplify the comparison groups for the market and spatial spillover effects. In Panel (a), the blue Census blocks are market-close to the TIF district without being spatially close, i.e., within 400 meters. The black Census blocks are market-far. In Panel (b), the blue Census blocks are spatially close to the TIF district without being market-close. The black Census blocks are spatially far.

Figure 8: Dynamic Spillover Effects, Log Assessed Property Value

(a) Market (Non-Spatial)



(b) Spatial



Note: This plot displays estimated $\{\gamma_\ell\}_{\ell\in\mathcal{L}}$ from Equation (8) using D^m_{id} or D^s_{id} for market and spatial spillover effects. The outcome is the log of the average assessed value in 2020 USD within 1990 Census blocks. The specification compares blocks that are treated to those that are not yet treated. All estimates are relative to event time -2. The error bars indicate 95% confidence intervals with TIF district-clustered standard errors.

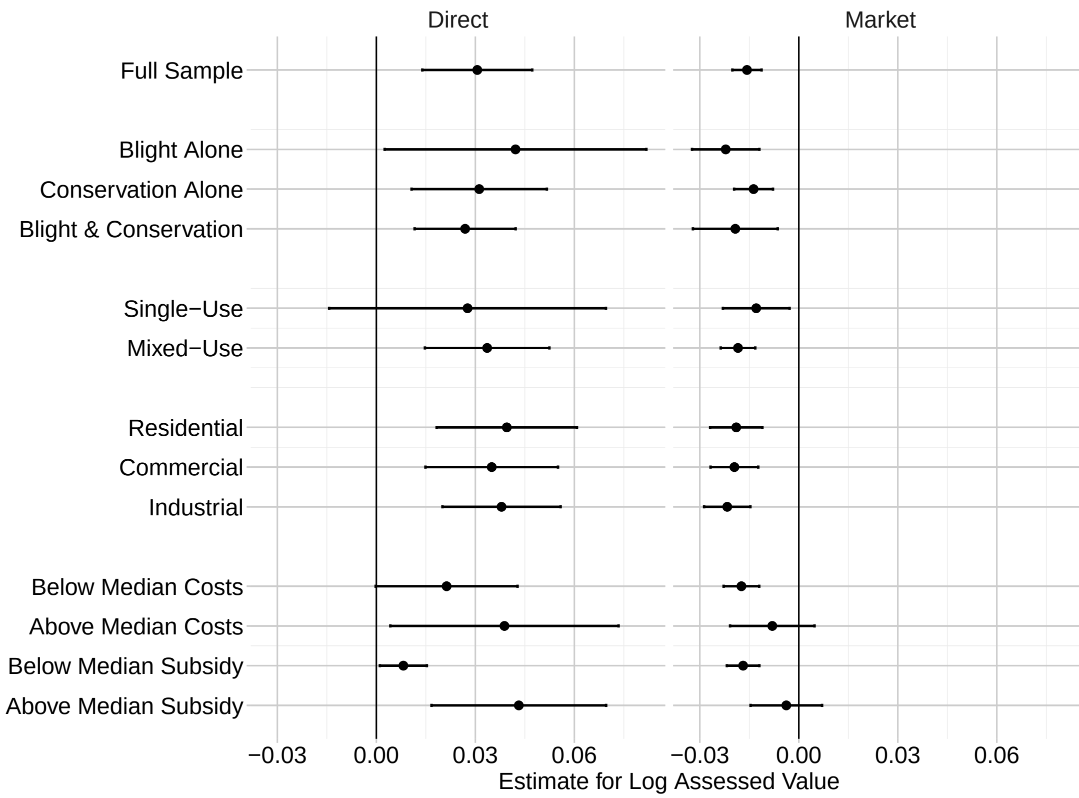
Heterogeneous Effects. TIF encompasses a variety of economic development activities. I display heterogeneous effects by characteristics of the TIF districts in Figure 9, focusing on direct treatment effects and market spillover effects. To calculate the heterogeneous effects, I limit the stacked dataset to TIF districts that belong to each subsample. Overall, the estimates are quite similar across categories of TIF districts. Across the subsamples, the point estimates for the market spillover effects remain negative even if some of the subsamples produce imprecise estimates.

Eligibility factors, either blight alone, conservation alone, or both, speak to the built environment of the area. Conservation districts require fewer blight factors (see Appendix Table A.1) and so may be more flexibly used while blight districts require more blight factors or larger amounts of vacant land. The districts that only fulfill the eligibility criteria to be blight districts exhibit a larger treatment effect than the baseline estimate. The estimate for districts that only fulfill the eligibility criteria to be conservation districts is statistically indistinguishable from the baseline effect. The districts that fulfill both eligibility criteria have the smallest treatment effect. These results suggest that TIF is most effective in areas with more degradation of the built environment but with newer structures.

The majority of the TIF districts I study are used for mixed-use purposes. These developments may include any combination of types of development, with many including both residential and commercial investments. While single-use TIF districts have an insignificant treatment effect, the mixed-use TIF districts have a large and significant direct effect. Finally, I consider how the effects may differ by the purpose of the TIF district, including residential, commercial, industrial (not mutually exclusive). All of them have statistically significant treatment effects that are larger than the baseline estimate, suggesting that the combination of development purposes in mixed-use development is more impactful on property values.

Finally, I calculate heterogeneous treatment effects by the amount of TIF-funded investment that occurs within the district. The data on project costs are limited to only projects completed in partnership with private developers or other public entities. I use these data as a proxy for the full amount of investment. I calculate the median total amount of costs of the TIF-funded projects and the median amount of the subsidy. I categorize the districts as being larger than the median ("Above Median") or smaller than or equal to the median ("Below Median"). The direct effect is much smaller for the districts with lower costs and subsidy amounts. This indicates that larger investment results in larger direct treatment effects.

Figure 9: Heterogeneous Direct and Market Spillover Effects, Log Assessed Property Value



Note: This plot displays the static estimates for the direct treatment effects and market spillover effects, dividing the sample by characteristics of the TIF districts. The error bars indicate 95% confidence intervals with TIF district-clustered standard errors.

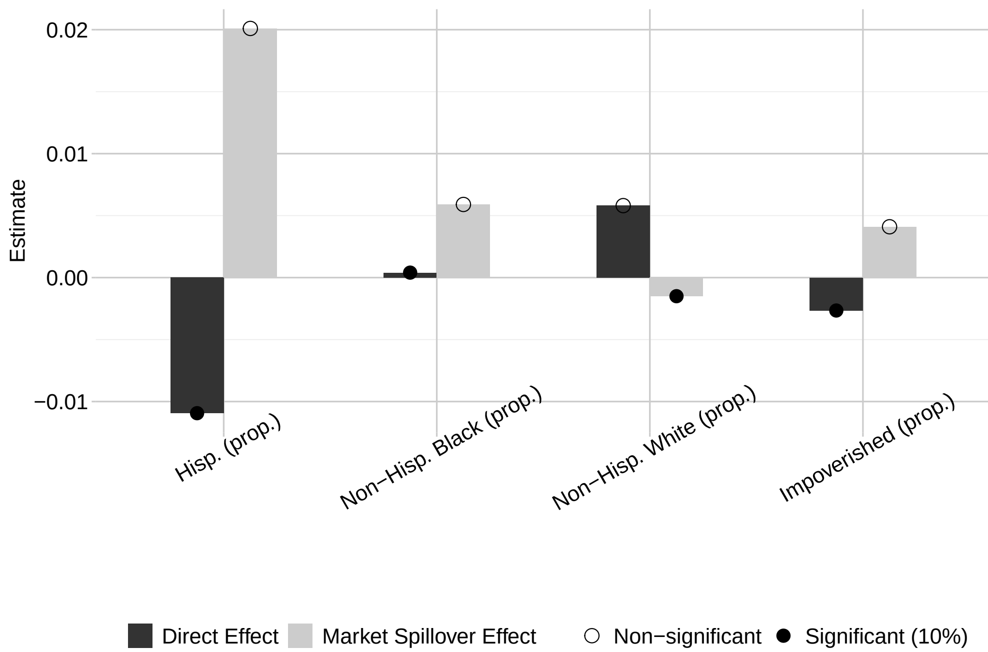
Household and Firm Outcomes. Across specifications, the estimates on property values indicate that TIF has a positive local effect but a negative effect to non-targeted areas that are in the same housing markets. This empirical finding suggests that there is some relocation of resources towards TIF districts from similarly situated areas. I consider outcomes related to household and firm movement to contextualize the estimates on property values. I follow the same strategy as in Section 5 to estimate direct and spillover effects.

Figure 10a displays the static estimates of the direct treatment effects and market spillover effects for outcomes related to households and neighborhood composition. See Appendix Figures A.4 for the corresponding dynamic estimates. I indicate estimates that are significant at least at the 10% level with solid circles. All of the outcome variables are in proportions. There is an overall pattern in which the direct effect is opposite to the market spillover effect. While the proportions of Hispanic residents and those living below the poverty line decrease locally, the proportions suggestively increase in market-close areas. A similar pattern appears for non-Hispanic white households in which they suggestively increase in targeted areas while decreasing in market-close areas. Non-Hispanic Black households increase very slightly locally and suggestively increase in market-close areas. The magnitudes of these effects are small and given the low prevalence of statistical significance, there is limited evidence supporting household sorting within five years after the establishment of TIF (Czurylo, 2023).

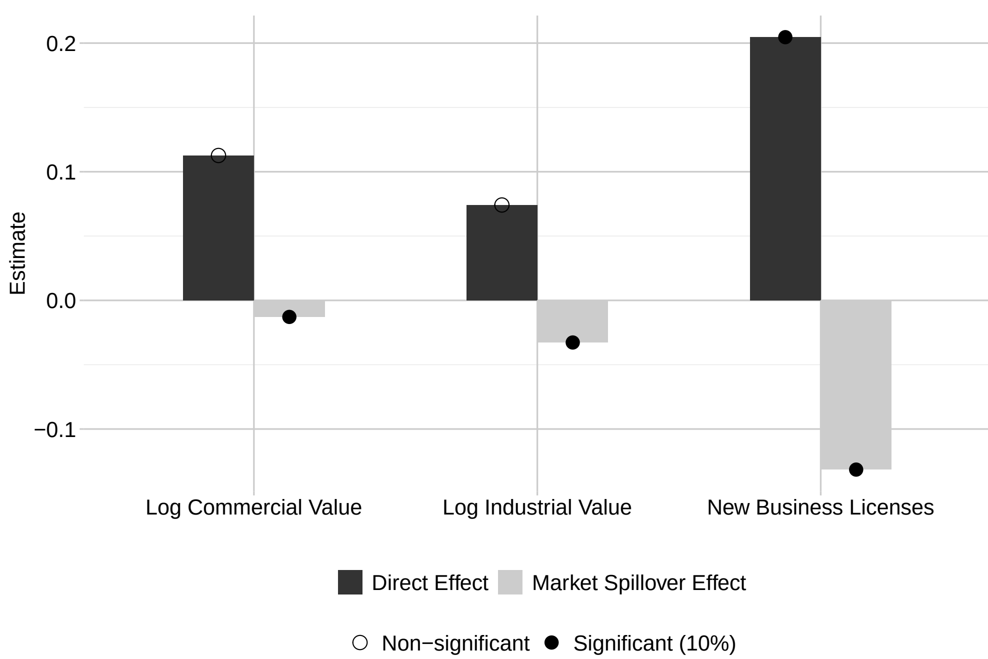
I also present static estimates on outcomes related to firms and report dynamic estimates in Appendix Figure A.5. Because the data on new business licenses begin in 2000, I restrict the sample to TIF districts established in 2001 or later. The assessed values of land for both commercial and industrial properties suggestively increase locally and significantly decrease in market-close areas. The estimates on new business licenses are more decisive. TIF increases new business licenses locally by approximately 0.2 licenses per year and decreases new business licenses by approximately -0.12 business licenses per year in market-close areas. These results align with the estimates on property values and speak to a relocation mechanism.

Figure 10: Direct and Market Spillover Effects, Household and Firm Outcomes

(a) Household Outcomes



(b) Firm Outcomes



Note: This figure plots the static estimates of the direct effects and market spillover effects of TIF on outcomes related to households and firms. For both panels, the dark grey bars indicate the direct effect and the light grey bars indicate the market spillover effect. A dark circle indicates that the p-value for the estimate is less than 0.1. A hollow circle indicates that the p-value is at least 0.1.

6 Overall Effects on Property Values and Redistribution

The market spillover effects provide insight into some of the unintended consequences that arise from TIF. While targeted areas experience benefits as a result of the policy, non-targeted areas incur costs. Without additional context, it is unclear how a centralized planner can interpret these findings. It is possible that the policy, instead of addressing observed inequality in the built environment, further consolidates resources to relatively advantaged areas. However, if the targeted areas are relatively disadvantaged compared to the market-close areas, then the policy redistributes resources throughout the city. To consider these two possibilities, I compare the observed characteristics of the targeted and market-close areas.

I display these in Table 3. The assessed value and median household income are larger in the market-close areas than in the targeted areas. The households are more likely to be renters in the targeted areas. Still, the difference is not as large as it is when comparing targeted areas to all non-targeted areas (Table 2). These differences highlight that the market-close areas are relatively less disadvantaged than the targeted areas, despite being in the same housing markets, suggesting that the policy results in a redistribution of resources.

I perform a back-of-the-envelope calculation to consider the long-term overall effect of this redistribution. I combine the direct and spillover effects into one coarse estimate of an "overall" effect. I consider a weighted sum of the long-term direct treatment effect (γ_5), the market spillover effect (γ_5^s), the spatial spillover effect (γ_5^m), and a spillover effect that compares non-targeted areas that are both market and spatially close to those that are far ($\gamma_5^{s\cap m}$). For all of these estimates, I consider the long-run effect at five years after the beginning of the policy. I calculate the weighted sum,

$$\frac{W\gamma_5 + W^s\gamma_5^s + W^m\gamma_5^m + W^{s\cap m}\gamma_5^{s\cap m}}{W + W^s + W^m + W^{s\cap m}}, \tag{12}$$

Table 3: Targeted and Market-Close Areas, Comparison

	Targeted	Market-Close
Number of Census Blocks	$6,303$	$6,741$
Assessed Value	$145,820.74$	$164,246.0^{**}$
	$(95,777.64)$	$(174,965.3)$
Median Income	$49,108.73$	$65,081.39^{***}$
	$(19,005.54)$	$(19,653.98)$
Renter (proportion)	0.55	0.34^{***}
	(0.01)	(0.01)
Non-Hispanic Black (proportion)	0.42	0.42
	(0.26)	(0.26)
Non-Hispanic White (proportion)	0.34	0.44^{*}
	(0.44)	(0.46)
Hispanic (proportion)	0.20	0.12^{***}
	(0.35)	(0.41)

Note: This table presents descriptive statistics of the targeted areas and the areas that are in the same market ("Market-Close"). For each characteristic, I report means and standard errors in parentheses. I report the significance of the comparison between the market-close and targeted means. Significance levels are indicated as *: $\alpha = 0.1$, **: $\alpha = 0.05$, ***: $\alpha = 0.01$.

with different population-based weights that correspond to the areas that are directly affect (W), spatially close (W^s), market-close (W^m), or both spatially and market-close ($W^{s\cap m}$).

Although informative, there are caveats to this back-of-the-envelope analysis. First, it does not include the Census blocks that are in TIF districts established before 2000. These blocks may receive spatial or market spillover effects as a result of the TIF districts in my sample. However, I cannot distinguish these effects from long-term complementarities between the TIF districts. My empirical framework and data span limit me to only consider non-targeted areas and areas targeted by TIF districts established in 2000 or later. Second, the calculation does not include non-targeted areas that are neither market-close nor spatially close and I do not extrapolate the spillover effects to these areas. My approach considers these areas to be unaffected. Controls outside the City of Chicago are needed to estimate spillover effects on these areas, but this design may introduce concerns of internal validity.

This exercise does not include a level change as a result of the policy. The empirical approach only identifies the parameters to a constant. In the presence of a level change, the overall estimate is biased (Heckman et al., 2006). This is plausible under the scenario in which the city-wide TIF program results in an overall increase in the property values of the city separate from the direct and spillover effects.

I weigh by the number of affected blocks and estimate an overall effect of 0.0017. I bootstrap to calculate a TIF district-clustered standard error of 0.008. I perform the same exercise weighing by the number of properties to calculate an overall effect of 0.0111 (standard error 0.010). Finally, I weigh by the average property value in each of the areas and calculate an overall effect of 0.0018 (standard error 0.008). All of these estimates are statistically indistinguishable from zero. Suggestively, this indicates that TIF is costly from the perspective of the central planner despite achieving moderate redistribution within housing markets that already share many observed and unobserved characteristics.

7 Conclusion

Analysis of the overall effects and redistribution of policies that target geographic regions, such as place-based policies, requires identification and estimation of spillover effects that propagate to areas that are not necessarily geographically close to the targeted area. The methodological approach in this paper allows for the consideration of spillover effects due to non-spatial mechanisms. I exemplify the approach with a particular place-based, economic development policy, Tax Increment Financing (TIF) in Chicago. In that context, spillovers may operate through housing markets in response to the decisions of households and firms. I apply Stochastic Blockmodeling (SBM) to data on household moves to characterize the latent housing markets and categorize which areas are in the same housing markets as TIF districts. Despite being a locally effective policy, the market spillover effects are negative, implying that TIF relocates development away from similarly situated areas. Although this may result in further consolidation of resources within an inequitable environment, it could also redistribute investment to areas that otherwise would not receive it. These results inform a policy analysis that considers overall effects without a fully specified structural model of general equilibrium, providing a bridge between structural modelling and "reduced-form" empirical frameworks.

The approach of defining underlying network structure to characterize non-targeted units that are market-close to targeted ones and consider non-spatial spillovers can be applied to other empirical frameworks. Most immediately, my setup lends itself to other panel data settings and estimators apart from stacked difference-in-differences. For example, the same approach can be applied to matched difference-in-differences (El-Khattabi and Lester, 2019) or weighted event study estimators (Callaway and Sant'Anna, 2021). The approach can also be applied to other empirical frameworks. For example, some place-based policies are implemented based on eligibility criteria. Papers studying those policies use this to compare areas that received the intervention to areas that were eligible to receive the intervention but did not (Busso et al., 2013; Chen et al., 2019). Defining market-close and market-far areas that were eligible to receive the intervention but did not can allow for an estimation of possible market spillover effects to the comparison areas.

While housing markets may be relevant to study spillovers in place-based, economic development policies, the same approach may be applicable to consider spillover effects in other contexts. Although my findings point to negative market spillover effects for TIF in Chicago, other contexts of place-based economic development may result in positive market spillover effects if overall investment and population is increasing or in the presence of other complementarities. Within urban and public economics, the characterization of the underlying housing market may also be useful for studies on gentrification or transportation (Asquith et al., 2023; Ding et al., 2016; Gechter and Tsivanidis, 2023; Tsivanidis, 2023).

Examples of possible applications in other fields include pollution and disaster spillovers in environmental contexts (Muehlenbachs et al., 2015), social networks for remittances in development contexts (Banerjee et al., 2023; Comola and Prina, 2021), foreign direct investment in trade contexts (Paul and Feliciano-Cestero, 2021; Wagner and Timmins, 2009), fiscal multipliers in macro settings (Chodorow-Reich, 2019a,b), and firm relocation in IO contexts (Holmes, 2011). Across these applications, SBM or a related algorithm can characterize the underlying market structure from data on micro-level movements or connections. In the context of housing markets, the revealed preference framework allows for interpretation of household moves as informative on the underlying housing markets. In these other contexts, other models and assumptions are necessary. For example, work that uses SBM to characterize workers and job types imposes assumptions on human capital accumulation (Fogel and Modenesi, 2022).

My results suggest that TIF redistributes resources within Chicago, however they do not speak to a sorting equilibrium. Complementary modelling frameworks can extend the work here to consider possible equity concerns. In particular, the frameworks of Epple et al. (1984, 2001) may be particularly useful to characterize how the distribution of neighborhoods changes as a result of place-based economic development. If it is indeed the case that TIF results in a redistribution of resources, the distribution of income and amenities across neighborhoods should flatten. The structure of these frameworks allow for the characterization of income thresholds that determine sorting and can speak to welfare for different types of households. These sorting models include considerations for voting and speak to political economy considerations of place-based policies (Brueckner, 2001).

Appendix

Beyond the Local Impacts of Place-Based Policies: Spillovers through Latent Housing Markets

A Additional Institutional Background

In this Appendix, I provide additional details on Chicago's implementation of Tax Increment Financing (TIF). In the U.S., states pass legislation that allow municipalities to implement TIF and define the parameters of the specific TIF policy (see Kriz and Johnson, 2019, for a recent review of each state's TIF policy). Even though Illinois passed the TIF-enabling legislation in 1977, TIF began in Chicago in 1984 and the first district was established in 1986.

The Chicago Department of Planning and Development (DPD), housed in the Office of the Mayor, coordinates with ward aldermen, special tax districts, private developers, and community stakeholders to implement TIF. The idea to implement a particular TIF district may come from any stakeholder or coalition. The DPD hires consultants to complete an eligibility study and a redevelopment plan. The eligibility study describes the built environment of the proposed district. Table A.1 lists the factors and the number required for different designations of districts. There must also be justification of the "but for" clause, i.e., the area would not develop "but for" the implementation of TIF. After the DPD incorporates feedback from public hearings into the redevelopment plan, there are two committees that vote on whether the City Council should consider the district for approval: a committee comprised of representatives of the other taxing bodies and a committee comprised of mayor-appointed members. The City Council's Committee on Finance must vote to approve the district before it goes to vote in the full City Council (Department of Planning and Development, 2020).

Once the city establishes a TIF district, the natural appreciation of property values may gradually accrue and fund the activities outlined in the redevelopment plan. However, this accrual may take many years or the district may be in an area with declining property values. The city can rely on private investments early in the TIF district's existence or borrow against the expected increase in property values as a result of the TIF-funded interventions. TIF-

Table A.1: Eligibility Criteria for TIF Districts

Panel (a): Types of TIF Districts

Blight

Improved: At least 2 acres, at least 5 factors from Panel (b)

Vacant: Vacant land, at least 2 factors from Panel (c) or previous structures had at least 5 factors from Panel (b)

Conservation

At least 2 acres, 50% or more of the structures are at least 35 years old, at least 3 factors from Panel (b)

Transit

No size or blight requirements

Panel (b): Factors for Improved Blight Areas

- Age
- Dilapidation
- Obsolescence
- Deterioration
- Illegal use of individual structures
- Presence of structures below minimum code standards
- Excessive vacancies
- Overcrowding of structures and community facilities
- Lack of ventilation, light, or sanitary facilities
- Inadequate utilities
- Excessive land coverage
- Deleterious land use or layout
- Depreciation or lack of physical maintenance
- Lack of community planning

Panel (c): Factors for Vacant Blight Areas

- Obsolete platting of the vacant land
- Diversity of ownership of vacant land
- Tax and special assessment delinquencies on vacant land
- Deterioration of structures or site improvements in neighboring areas to the vacant land

Note: 65 ILCS 5 (1977, §11-74) lists and explains these criteria.

backed debt instruments are not general obligation bonds and so must be paid back using the revenues from the TIF districts (Luby et al., 2019). Private and public actors apply for TIF funding for projects to be completed within the boundaries of the district. While the process to initiate TIF districts and implement projects requires several rounds of voting, there is little evidence that TIF districts and projects are frequently denied (Craft and Weber, 2019). For example, starting in 2019, Chicago established a committee to formalize the review of these proposed projects. Since then, almost 90% of them have been accepted without further review (City of Chicago, 2022).

The TIF legislation lists the specific economic development activities for which TIF funds can be used. These include administrative costs; property acquisition and demolition, site preparation, environmental remediation; rehabilitation, repair, and remodeling of existing structures; construction and improvements of public works; job training; financing; capital costs of the other taxing bodies; relocation; payment in lieu of taxes; reimbursing school districts to cover increases in enrollment due to TIF-funded housing; construction of affordable housing (65 ILCS 5, 1977). These activities may contribute to different types of development including residential, commercial, industrial, institutional, transportation, or mixed-use which may combine any of the above. Table A.2 presents the percentage of TIF districts falling into these categories. Most districts during my period of analysis are intended for mixed-use development and most districts fulfill the conservation eligibility criteria.

B Data

B.1 Data Construction

B.1.1 Property Values and Characteristics

The Cook County Assessor's Office (CCA) publishes historic assessed values, sale transactions, and property characteristics for the universe of parcels in the county for the years 1999-2022 (Cook County Government, 2023). I inflate all monetary values to 2020 USD using the CPI for all urban consumers in Chicago-Naperville-Elgin (Federal Reserve Bank of St. Louis, 2022). I combine four datasets into one panel with each row corresponding to

Table A.2: Characteristics of TIF Districts

	Established before Data Span (1986-1999)	Established in Data Span (2000-2022)
Number of Districts	79	106
Eligibility Factors (%)		
Blight Alone	50.63	22.64
Conservation Alone	34.18	60.38
Blight & Conservation	15.19	16.04
Transit	0	0.94
Purpose (%)		
Residential Alone	5.06	12.26
Commercial Alone	10.13	3.77
Industrial Alone	32.91	12.26
Transportation Alone	0	0.94
Mixed-Use	51.9	70.75

Note: This table describes the 185 TIF districts. I use districts established between 2000 and 2022 in my analysis in the main paper. Percentages add to 100% without rounding error. Data from City of Chicago (2022).

a property $\times$ year observation. I average within 2000 Census blocks to create an analogous block $\times$ year panel.

Assessed Values. I restrict the properties to those that are residential, commercial, or industrial. I drop very few observations that are missing assessments or those with other missing information. These missing values arise due to inconsistencies in the sampling frame of the Cook County Assessor's Office. I use assessed values (land, building, and total) defined as the initial assessed value mailed to property owners. I calculate the values by multiplying assessed values by 10. I inflate assessed values to 2020 USD.

Sale Transactions. I follow the recommendations in Nolte et al. (2021) to identify non-arms-length sales. First, I remove transactions with sale prices of $0 or $1. Second, I remove transactions with a buyer or seller with an institutional name (e.g., containing the word "veteran" or "city"). Third, I remove transactions at risk of being done within family. To do so, I calculate a string distance between the buyer and seller names and remove transactions for which the string distance is 0. The second and third steps rely on observing the buyer and seller names. At least one name is missing for %16 of transactions. I assume that these are arms-length transactions. Together, I remove $26,458$ transactions that are not considered arms-length according to this process. This leaves $2,123,297$ transactions that are credibly arms-length and thus assumed to reflect the true market price.

Residential Property Characteristics. I construct a dataset with the following objective property characteristics: number of rooms (beds, bathrooms, total), indicators for having a porch, an architect-designed plan, central air, a recent renovation, and a fireplace. There are some more subjective measures including if the site location benefits or detracts from the value, if the construction quality is below or above average, and if the condition is below or above average. If a property has multiple improvements, I only keep the first one to ensure that there is at most one observation for each property in each year.

Universe of Parcels. The above datasets do not have geographic information. The universe of parcels contains property-level coordinates. I drop properties with missing geographic coordinates ($N = 81,050$, approximately 8.2% of properties) and with coordinates outside the boundaries of the City of Chicago ($N = 2$). I merge the remaining properties with the 1990 definitions of Census tracts ($N = 929$), block groups ($N = 2,614$), and blocks ($N = 21,578$).

I merge these four datasets together using a property-level identifier (PIN) that is unique within years and stable across years. I only create this dataset for properties in the City of Chicago.

B.1.2 Household Characteristics.

InfoUSA (Data Axle, 2020) is a consumer database that compiles address lists and other public sources to predict characteristics of all households in the U.S. between 2006 and 2020. These data contain (predicted) information on the household. Relevant for my analysis, it includes household income, incumbency, owner or renter status, and race and ethnicity and name of the household head. There are three steps that I follow to prepare these data for analysis. First, I construct and clean the cross-sectional data. Second, I match households between pairwise years. Third, I calculate the in- and out-migration between Census blocks.

Cross-Sectional Data Construction. I restrict the data to addresses in Cook County and variables of interest for my analysis. The household information includes household income, home value, renter or owner status, incumbency, and age, name, and race and ethnicity of the household head. Except for the head's age, most observations of these variables contain predicted values.

I inflate household income and home value to 2020 USD. InfoUSA provides a 9-level scale for renter or owner status to reflect the certainty of the prediction. I assign households with 1-3 to be renters, those with 7-9 to be owners, and those with 4-6 to be of unknown tenure. The models that predict these variables are proprietary, but Data Axle benchmarks the estimates to align with public data sources, including the Census. Due to the large number of missing values in race and ethnicity variables, I calculate race and ethnicity based on name and geography (Census tract for the years 2006-2019 and county for the years 2020). I use the `wru` package in `R`, which implements the method in Imai and Khanna (2016).

Longitudinal Match. In addition to variables describing household characteristics, Data Axle assigns a household ID intended to be stable over time and uniquely identify families in the InfoUSA sample.

Block-to-Block Flows. For each year 2006-2020, I clean the cross section of the InfoUSA data, restricting the data to households in the City of Chicago. I match the addresses' geographic coordinates with the 1990 definitions of Census tracts and blocks. I match households between consecutive years (2006-2007, 2007-2008, ..., 2019-2020) using a household ID. The households that do not match may have some inconsistency in household ID, move outside the City of Chicago, or be absorbed in another household. This matching procedure results in matching approximately 75% of the households.

B.2 Measurement of Property Values

Researchers use four common sources of property values, sale prices, assessments, rental prices, and self-reported property values, each with their advantages and disadvantages. In this section, I study the selection bias of sale prices and the measurement error of assessments using a factor model. To do so, I combine several measures of property values (Banzhaf and Farooque, 2013; Calabrese et al., 2006; Clapp and Giaccotto, 1992). The objective of this exercise is to justify my use of assessed values in my main analysis.

Because sale prices capture the actual market value of properties, researchers consider them to be the gold standard (Bishop et al., 2020). However, the timing of placing a property on the market is non-random, implying that the distribution of observed sales prices can differ from the full distribution of true property values. This divergence in the distributions is called transaction bias, as a transaction only occurs if the expected sale price is above the seller's reservation price. Although there is sparse evidence of transaction bias for commercial properties (Munneke and Slade, 2000), there is evidence of transaction bias for residential properties (Gatzlaff and Haurin, 1997, 1998; Ihlanfeldt and Martinez-Vazquez, 1986; Jud and Seaks, 1994; Mason and Pryce, 2011). The general approach taken in these studies is to correct for the selection using standard procedures (i.e., Heckman, 1974). Comparing the standard repeat-sales price index to a selection-corrected one shows that there is significant upwards bias without accounting for selection. This upwards bias is consistent with homeowners having a reservation price, and only selling their homes if the value is above that threshold.

Predicted property values do not suffer from transaction bias. Both public and private organizations predict property values for tax, homeowner, or marketing purposes. However, they may suffer from prediction error which may be non-classical. Although the individual algorithms differ, the general approach involves predicting property values based on recent sales of similar properties (e.g., Ross et al., 2019). Appeals processes further alter assessed values. For example, Avenancio-León and Howard (2020) demonstrate that the over-weighting of hyper-local amenities and racial differences in the appeals process leads to racial differences in assessments.

Measures of Property Values. To study the measurement of property values, I use three predicted measures of property values (CCA, Redfin, and InfoUSA) and the observed sale prices (CCA). I merge the three data sources using the steps listed in Table A.3. I define the analysis sample of assessed values for residential properties and match this with the Redfin and InfoUSA data. I combine deterministic and probabilistic matching to account for inconsistencies in addresses between sources. Probabilistic matching involves computing a probability that all possible matches are correct based on some measure of string distance (see, e.g., Fellegi and Sunter, 1969; Newcombe and Kennedy, 1962). For this analysis, I use the `fastLink` package in `R` (Enamorado et al., 2020) based on the work in Imai and Khanna (2016).

Table A.4 describes the correlation between the measures. I obscure the sources of the commercial predictions to avoid unintentional statements on the quality of those predictions. The measures are correlated but at most have a correlation of approximately 0.72. This indicates that the three measures contain different information.

Table A.3: Summary of Data Construction

Matching CCA and InfoUSA
1. Standardize the addresses, combine multi-unit properties, remove incomplete addresses
2. Deterministic match on full address string
3. For unmatched properties, remove ZIP codes and deterministic match on remaining string
4. For unmatched properties, deterministic match on street number and probabilistic match on the rest of the address string if the street is not numbered
Matching CCA-InfoUSA and Redfin
1. Standardize the addresses and remove incomplete addresses
2. Deterministic match on full address string from CCA
3. For unmatched properties, deterministic match on address string from InfoUSA
4. For unmatched properties, deterministic match on street number and probabilistic match on the rest of the address string of CCA if the street is not numbered

Table A.4: Correlation Between Measures

	Assessments	Commercial A
Assessments	–	–
Commercial A	0.5721	–
Commercial B	0.7214	0.6234

Note: This table displays the correlation between the predicted measures of property values matched in Table A.3.

Measurement Model. Let i index properties and t index time periods. In each time period, each property has a true market price for the contemporaneous market conditions, P_{it}. If a property sells in t, then $S_{it} = 1$. Otherwise, $S_{it} = 0$. External parties predict prices denoted P_{it}^j where $j \in \{1, 2, 3\}$ indexes the prediction source. Suppose that for all i and t, the researcher observes:

$$\left(S_{it}, S_{it}P_{it}; P_{it}^1, P_{it}^2, P_{it}^3\right). \tag{A.1}$$

I implement a measurement model to estimate Bartlett scores using $(P_{it}^1, P_{it}^2, P_{it}^3)$ (Schennach, 2004). I define the measurement model using benchmark parameterizations and assumptions. For all t and $j \in \{1, 2, 3\}$,

$$P_i^{jt} = \gamma_0^{jt} + \gamma_1^{jt} P_{it} + u_i^{jt}. \tag{A.2}$$

Assumption 1 *Assumptions of the measurement model.*

1. *Functional form. The relationship between the measures and the true property values is linear, i.e., Equation* (A.2)*.*

2. *Exogeneity in the measurement system. For all j and t,*

$$Cov(P_{it}, u_i^{jt}) = 0.$$

3. *Measurement errors are uncorrelated.*

4. *Normalization.*

I follow Carneiro et al. (2003) and Kotlarski (1967) to identify the parameters in Equation (A.2) and generalized least squares (GLS) to estimate the Bartlett Scores.

Estimates from the Measurement Model. Table A.5 displays the estimated parameters from the measurement model. I use them to construct the Bartlett scores, whose distribution I display in Figure A.1. I report the distribution of Bartlett scores, the distribution of sale prices, and the distribution of Bartlett scores only for those properties with observed sale prices. I list the values of the 25th and 75th percentiles in the legend. The full distribution of the Bartlett scores has more mass for properties with lower value compared to those properties that sell. This highlights the issue of transaction bias in the sale prices. However, the Bartlett scores for properties with observed sale price match the sale prices. This suggests that the Bartlett scores approximate the true value of the properties across samples.

Table A.5: Measurement Model Parameter Estimates

Year	Parameter	$j=1$ Assessments	$j=2$ Commercial A	$j=3$ Commercial B
2017	γ_0^j	0	62,343	-21,160
		–	[978]	[1,830]
	γ_1^j	1	0.8646	1.1386
		–	[0.004]	[0.0083]
2018	γ_0^j	0	142,343	81,497
		–	[24,369]	[4,297]
	γ_1^j	1	1.1561	0.7819
		–	[0.0813]	[0.014]
2019	γ_0^j	0	202,388	167,827
		–	[31,714]	[4,879]
	γ_1^j	1	1.1028	0.642
		–	[0.0743]	[0.0114]

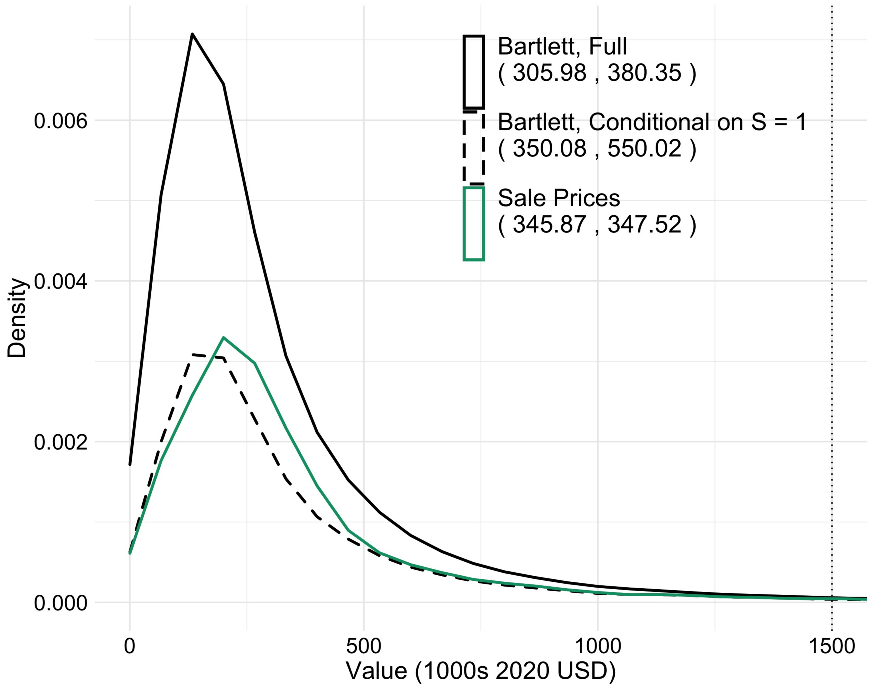
Note: This table reports the estimated parameters from a simple measurement model. The brackets contain the bootstrapped standard errors ($B = 100$) clustered by year.

In the main paper, I use assessed values as the main outcome of interest for several reasons. First, although it would be ideal to use Bartlett scores, these are only available for one set of years given the span of the data sources. My empirical framework requires a panel dataset, which the assessed values provide back to 1999. Second, while the assessed values contain prediction error, the fact that I use them as the dependent variable ameliorates concerns of biased estimates. Furthermore, I average within Census block to perform the analysis at the block-level. Third, as I demonstrate in this section, there is support in the data for transaction bias, which would challenge the interpretation of treatment effect estimates.

C Additional Results and Sensitivity

Figure A.2 explores the sensitivity of the estimate of the direct effect on log assessed property values to changes in the inference and specification. The first set of points are the baseline

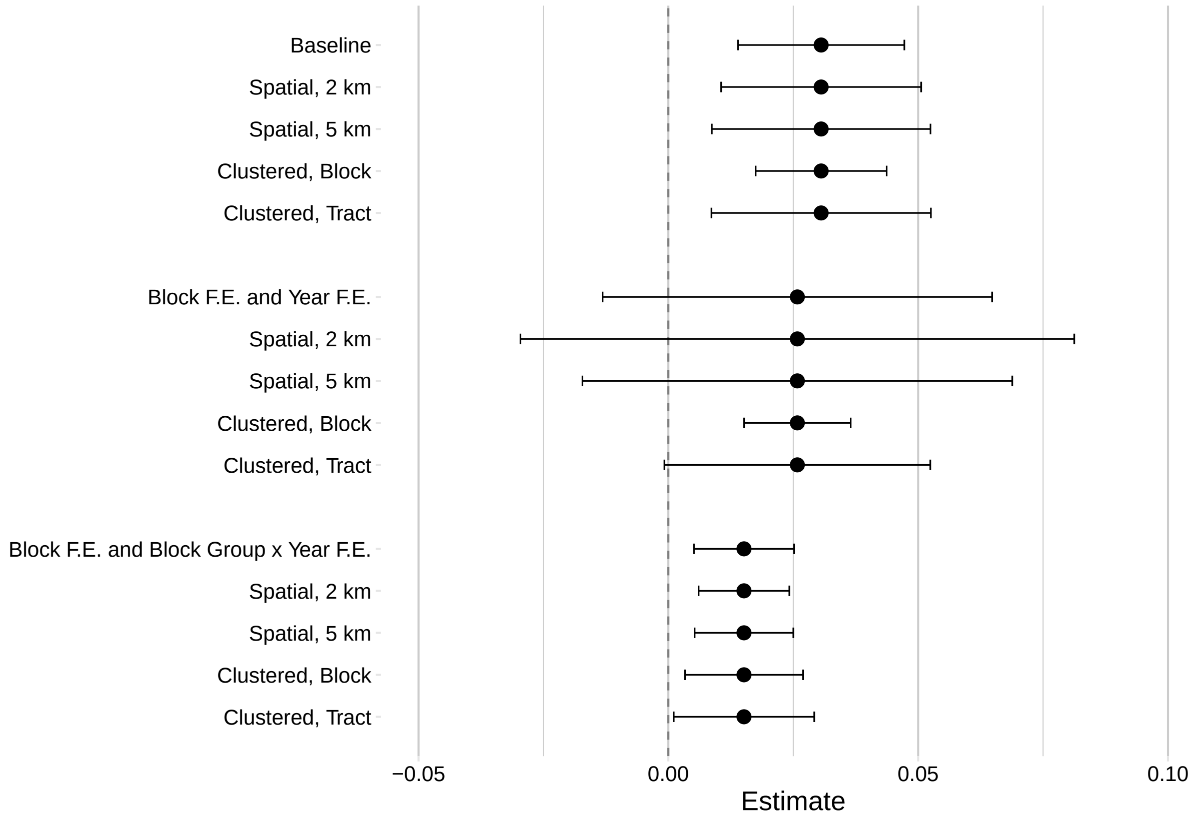
Figure A.1: Bartlett Scores and Sale Prices, Distributions



Note: This figure plots the distributions of the Bartlett scores and sale prices. I separate out the Bartlett scores for properties with observed sale price. I drop 3,600 observations with negative Bartlett scores.

estimates with confidence intervals for different types of standard errors. In my baseline specification, I cluster at the level of the TIF district. The spatial standard errors are from Conley (1999). I allow for correlation within 2 kilometers and 5 kilometers. I also show confidence intervals with standard errors clustered at the Census block and Census tract levels. The second set of points are analogous but with a specification with only Census block and year fixed effects. In my baseline specification, I include Census block and Census tract × year fixed effects. The estimates without the Census tract × year fixed effects are similar in magnitude to my baseline estimates. However, they are not significant. Also, the pre-trends for this specification are significantly different than zero suggesting that the parallel trends assumption does not hold for this specification. Finally, I present estimates with finer fixed effects, block group × year fixed effects. These estimates are closer to zero, although still statistically significant.

Figure A.2: Direct Treatment Effect on Log Assessed Property Values, Sensitivity Analysis



Note: This figure plots the static estimate of the treatment effect on log assessed property value for the different specifications labeled. The error bars indicate 95% confidence intervals.

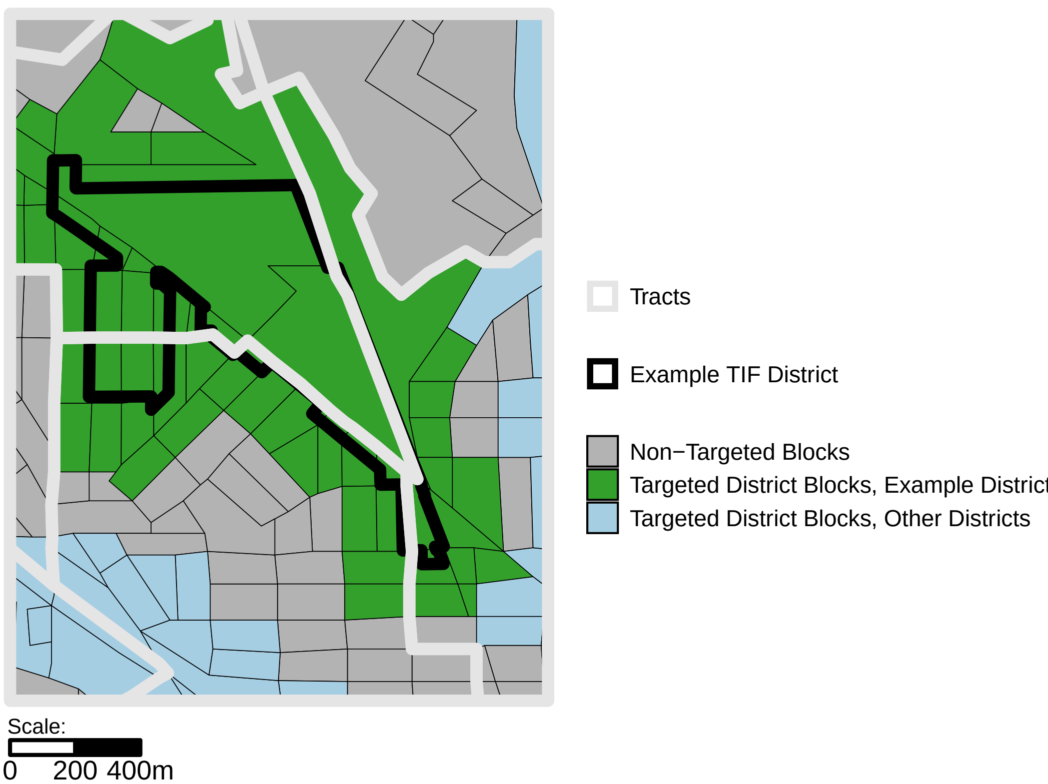
Figure A.3 maps the overlap between an example TIF district, Census blocks, and Census tracts. The map illustrates that within tract, there can be blocks targeted by multiple TIF districts as well as non-targeted blocks. Generally, TIF districts overlap with Census tracts in this way as the districts do not follow Census geographies and instead follow streets, commercial or industrial corridors, landmarks, or residential areas.

I present two exercises to contextualize the decision of the distance for the spatial spillover effects. Table A.6 shows the estimates of the spatial and market spillover effects for different distances. In the main paper, I use 400 meters to designate areas that are spatially close. I present the static estimates of the spatial spillover effects without removing the blocks that are also market-close ("All") and after removing these blocks ("Net of Market"). The latter is the estimate I use for my baseline estimate. I present analogous estimates for the market spillover effects. Across distances, the spatial spillover effects are not statistically significant. The market spillover effects increase in magnitude as the number of blocks that are considered spatially near increase. This speaks to an opposition between the spatial and market spillover effects.

The second exercise considers increasing rings from 400 meters. I report these estimates in Table A.7. Except for 700-800 meters, the spatial spillover effects are not statistically significant. Apart from that ring, the magnitudes of the effects are decreasing suggesting that the spatial spillover effect reduces outside of the 400 meters. However, the lack of statistical significance makes it challenging to compare the estimates meaningfully. The market spillover effects remain quite stable.

Figures A.4 and A.5 display the dynamic treatment effects for the static estimates in Figure 10. I display the direct treatment effects, the spatial spillover effects, and the market spillover effects.

Figure A.3: Overlap between Census Geographies and an Example TIF District



Note: This figure maps an example TIF district and the corresponding Census tracts and Census blocks. All Census geographies are from the 1990 Decennial Census.

Table A.6: Spatial Spillover Estimates: Spatially Near Definition, Sensitivity Analysis

	Spatial		Market	
Distance (m)	All (s.e.)	Net of Market (s.e.)	All (s.e.)	Net of Spatial (s.e.)
100-200	0.011327	0.006039	-0.013498^{***}	-0.014334^{***}
	(0.00923)	(0.01627)	(0.00254)	(0.00242)
100-400	0.007961	0.006185	-0.013498^{***}	-0.015712^{***}
	(0.00846)	(0.01291)	(0.00254)	(0.00226)
100-600	0.004209	0.013703	-0.013498^{***}	-0.016564^{***}
	(0.00773)	(0.01213)	(0.00254)	(0.00214)
100-800	-0.000171	0.011936	-0.013498^{***}	-0.017289^{***}
	(0.00691)	(0.01148)	(0.00254)	(0.00205)
100-1000	-0.000632	-0.001747	-0.013498^{***}	-0.018435^{***}
	(0.00605)	(0.01427)	(0.00254)	(0.00195)

Note: This table displays the corresponding static estimates for different definitions of spatially near. The baseline specification I present in the main paper is 100-400 meters. In all cases, the spatially far blocks must be at least 5 kilometers. The columns labelled All contain estimates using all blocks that are spatially or market close compared to those that are spatially or market far. The column labelled Net of Market contains estimates using blocks that are spatially close but not market close compared to those that are spatially far. The column labelled Net of Spatial contains estimates using blocks that are market close but not spatially close compared to those that are market far. In all cases, far blocks cannot be close to other districts established less than 5 years from the TIF district of interest. I report standard errors clustered by TIF district of interest in parentheses. Significance levels are indicated as *: $\alpha = 0.1$, **: $\alpha = 0.05$, ***: $\alpha = 0.01$.

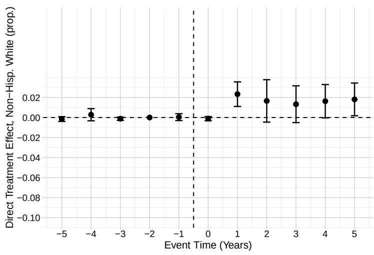
Table A.7: Spatial Spillover Estimates: Increasing Rings Outwards, Sensitivity Analysis

	Spatial		Market	
Distance (m)	All (s.e.)	Net of Market (s.e.)	All (s.e.)	Net of Spatial (s.e.)
400-500	0.008622	0.01556	-0.013498^{***}	-0.014669^{***}
	(0.00677)	(0.01564)	(0.00254)	(0.00236)
500-600	0.003652	0.014899	-0.013498^{***}	-0.014485^{***}
	(0.00692)	(0.01456)	(0.00254)	(0.00239)
600-700	-0.000503	0.014735	-0.013498^{***}	-0.014342^{***}
	(0.00709)	(0.01482)	(0.00254)	(0.00244)
700-800	0.003523	0.025564^{**}	-0.013498^{***}	-0.014349^{***}
	(0.00702)	(0.01211)	(0.00254)	(0.00241)
800-900	-0.000914	0.011852	-0.013498^{***}	-0.014448^{***}
	(0.00588)	(0.00929)	(0.00254)	(0.00241)

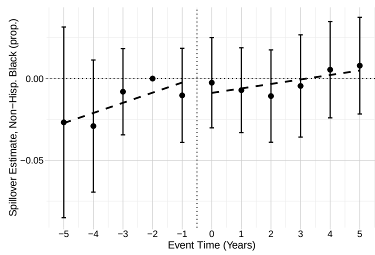
Note: This table displays the corresponding static estimates for different rings of spatially near going outwards from 400 meters from the TIF districts of interest. In all cases, the spatially far blocks must be at least 5 kilometers. The columns labelled All contain estimates using all blocks that are spatially or market close compared to those that are spatially or market far. The column labelled Net of Market contains estimates using blocks that are spatially close but not market close compared to those that are spatially far. The column labelled Net of Spatial contains estimates using blocks that are market close but not spatially close compared to those that are market far. In all cases, far blocks cannot be close to other districts established less than 5 years from the TIF district of interest. I report standard errors clustered by TIF district of interest in parentheses. Significance levels are indicated as *: $\alpha = 0.1$, **: $\alpha = 0.05$, ***: $\alpha = 0.01$.

Figure A.4: Dynamic Effects on Household Outcomes

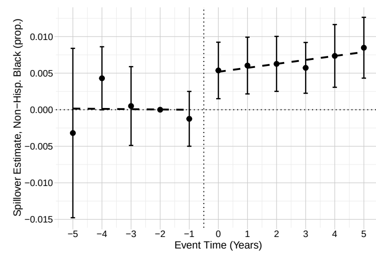
(a) Direct Effect, Non-Hisp. Black (prop.)



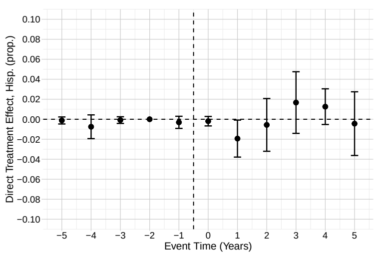
(b) Spatial Spillover Effect, Non-Hisp. Black (prop.)



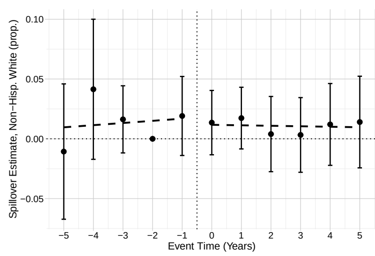
(c) Market Spillover Effect, Non-Hisp. Black (prop.)



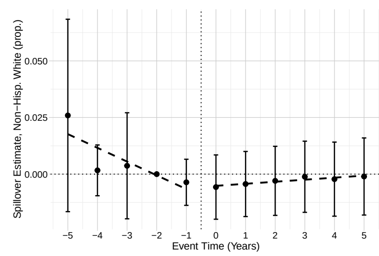
(d) Direct Effect, Non-Hisp. White (prop.)



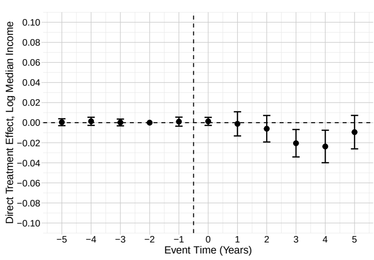
(e) Spatial Spillover Effect, Non-Hisp. White (prop.)



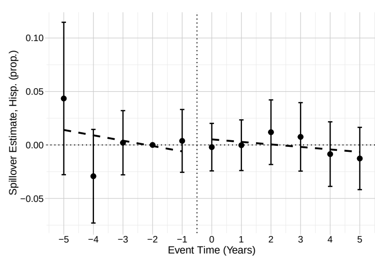
(f) Market Spillover Effect, Non-Hisp. White(prop.)



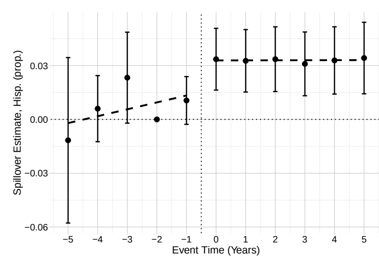
(g) Direct Effect, Hispanic (prop.)



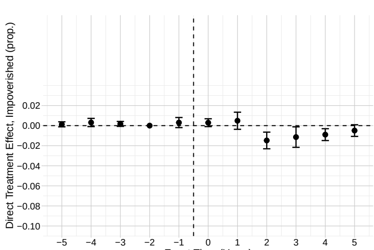
(h) Spatial Spillover Effect, Hispanic (prop.)



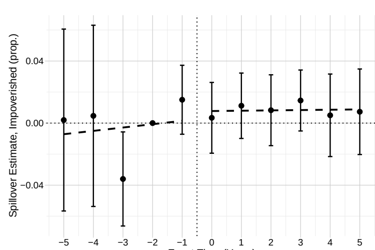
(i) Market Spillover Effect, Hispanic



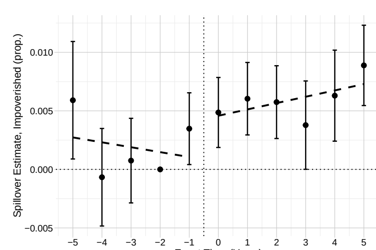
(j) Direct Effect, Poverty Rate



(k) Spatial Spillover Effect, Poverty Rate



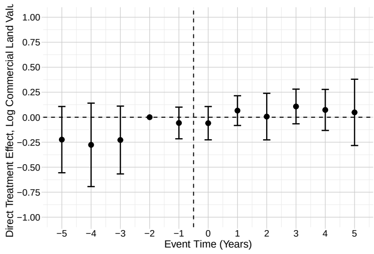
(l) Market Spillover Effect, Poverty Rate



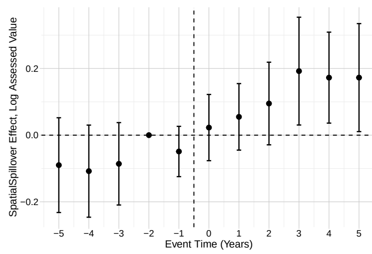
Note: This figure shows the dynamic direct treatment, spatial spillover, and market (non-spatial) spillover effects based on households observed in the ACS and Census. The error bars indicate the 95% confidence intervals using TIF district-clustered standard errors.

Figure A.5: Dynamic Effects on Firm Outcomes

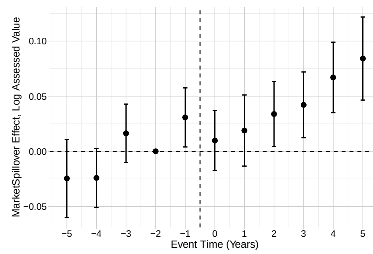
(a) Direct Effect, Commercial Land Value



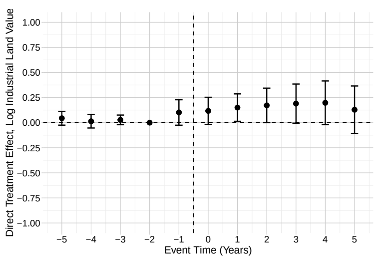
(b) Spatial Spillover Effect, Commercial Land Value



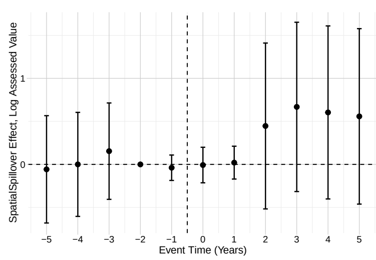
(c) Market Spillover Effect, Commercial Land Value



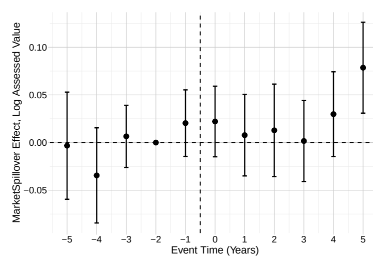
(d) Direct Effect, Industrial Land Value



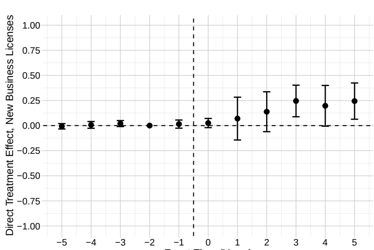
(e) Spatial Spillover Effect, Industrial Land Value



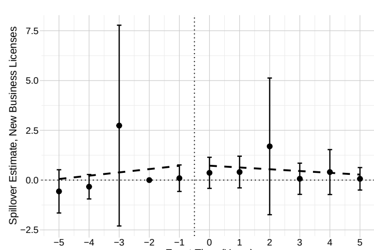
(f) Market Spillover Effect, Industrial Land Value



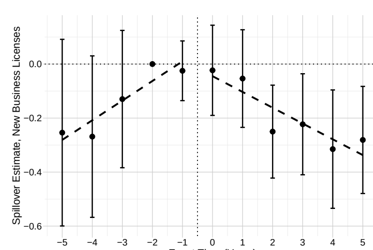
(g) Direct Effect, New Business Licenses



(h) Spatial Spillover Effect, New Business Licenses



(i) Market Spillover Effect, New Business Licenses



Note: This figure shows the dynamic direct treatment, spatial spillover, and market (non-spatial) spillover effects on firms. I observe commercial and industrial assessed land values in data from the Cook County Assessor's Office. I observe the flow of business licenses from the City of Chicago's Data Portal. The error bars indicate the 95% confidence intervals using TIF district-clustered standard errors.

D Details on SBM for Classifying Markets

Stochastic Block Modeling (SBM) is a method of community detection (Breiger et al., 1975; Holland et al., 1983; Nowicki and Snijders, 2001; Snijders and Nowicki, 1997). I draw from Peixoto (2019) to explain the intuition and details behind SBM and its estimation.

To review the notation from the main paper, I define a $J \times J$ adjacency matrix, $\boldsymbol{A}^t$, where J is the number of neighborhoods. Each element, $\boldsymbol{A}^t_{jk}$, represents the number of households that move from neighborhood j to neighborhood k in the sequential time periods t and $t+1$. Note that $\boldsymbol{A}^t$ is not symmetric, meaning that just because households move from neighborhood j to k does not mean that households need to move from neighborhood k to j. I aggregate the matrices across all time periods into one adjacency matrix $\boldsymbol{A}$ where

$$\boldsymbol{A}_{jk} = \begin{cases} 1 & \text{there is at least one } t \text{ such that } \boldsymbol{A}^t_{jk} > 0 \\ 0 & \text{otherwise.} \end{cases} \tag{A.3}$$

Estimation Details. Let G be the number of groupings and $\boldsymbol{g}_{J\times 1}$ denote the groupings for the J neighborhoods with possible values $\{1, \ldots, G\}$. I denote the probabilities of households moving between groupings with $\boldsymbol{p}_{G\times G}$. For binary SBM, the likelihood is

$$P(\boldsymbol{A} \mid \boldsymbol{p}, \boldsymbol{g}) = \prod_{i=1}^{J} \prod_{j=1}^{J} \boldsymbol{p}_{\boldsymbol{g}(i),\boldsymbol{g}(j)}^{\boldsymbol{A}_{ij}} \quad (1 - \boldsymbol{p}_{\boldsymbol{g}(i),\boldsymbol{g}(j)})^{1-\boldsymbol{A}_{ij}}. \tag{A.4}$$

In practice, I use the Minimum Description Length (MDL), which clarifies the penalization of additional groupings,

$$-\ln P(\boldsymbol{A} \mid \boldsymbol{p}, \boldsymbol{g}) - \ln P(\boldsymbol{p}, \boldsymbol{g}). \tag{A.5}$$

The first term contains the information needed to characterize the data and the second term contains the information needed to characterize the parameters of the model. The second term prevents the model from producing too many groupings, in the most extreme case categorizing each neighborhood into its own grouping.

I use the `graph-tool` library in Python to implement the estimation. The function `minimize_nested_blockmodel_dl` relies on a Markov Chain Monte Carlo (MCMC) approach for efficiency (Peixoto, 2014a). The idea is to first randomly perturb the grouping of each neighborhood and then accept the change based on a function of the change in the MDL. This algorithm increases efficiency over alternatives by reducing the number of possible groupings over which to compute the MDL.

There are two complications that I incorporate into the estimation procedure. First, I use a nested model structure (Peixoto, 2014b). While the MDL ensures that the algorithm does not estimate groupings that are too fine, there is a "resolution limit" that is a function of the number of neighborhoods. The nested model structure increases this limit, resulting in a larger number of groupings possible to characterize. Second, I correct for the number of degrees (Karrer and Newman, 2011). This avoids grouping neighborhoods together just because they are connected to the same number of neighborhoods.

Description of Simulation in Figure 2b. I present a simple simulation for illustration purposes. I impose that there are 900 neighborhoods and five latent groupings. I set the transition matrix for the groupings as

$$\begin{pmatrix} 0.8 & 0.1 & 0.01 & 0.05 & 0.04 \\ 0.2 & 0.7 & 0.02 & 0.03 & 0.05 \\ 0.01 & 0.03 & 0.9 & 0.03 & 0.03 \\ 0.04 & 0.09 & 0.05 & 0.75 & 0.07 \\ 0.04 & 0.05 & 0.03 & 0.07 & 0.81 \end{pmatrix}.$$

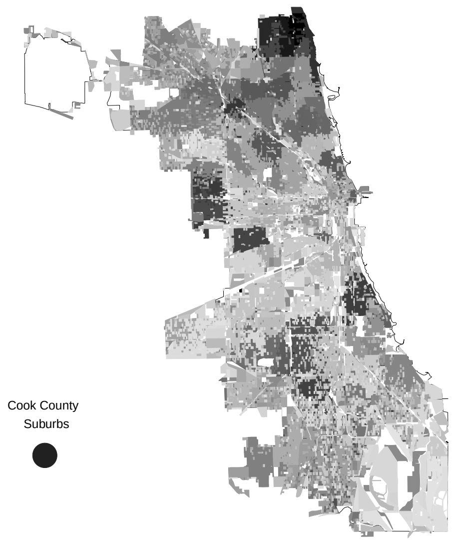
This transition matrix contains the conditional probability of moving from a neighborhood in grouping m to a neighborhood in grouping n. In reality, this transition matrix is unobservable. I set a seed and simulate a realized adjacency matrix based on these probabilities. I input the adjacency matrix into the estimation algorithm of SBM.

Estimated Housing Markets. Figure A.6 plots the groupings estimated at the different levels of the estimation. I use the 146 estimated groupings estimated in level 0 in my baseline analysis. I consider these to be the latent housing markets. Following the same approach as in level 0, the algorithm groups these 146 groupings into 28 broader groupings. I consider these to be the broader latent housing markets and use them in fixed effects to control for broader market trends. The algorithm continues to estimate broader groupings until it reaches one grouping in level 5.

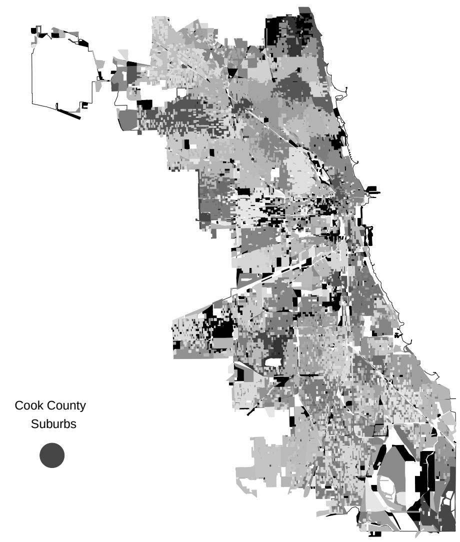
Evaluating the Change in Markets Over Time. I estimate the housing markets using observations on household movement between 2006 and 2020. This time frame overlaps with my period of analysis during which TIF districts are established. An alternative approach is to only use years prior to the establishment of a TIF district to determine the housing markets for the spillovers of that district. However, the panel of household moves starts in 2006 limiting the number of districts possible to analyze with this approach. More fundamentally, market spillover effects estimated with these housing markets would not include effects in the new housing markets. These are precisely the areas that I aim to characterize and study.

Figure A.6: Estimated Markets, City of Chicago

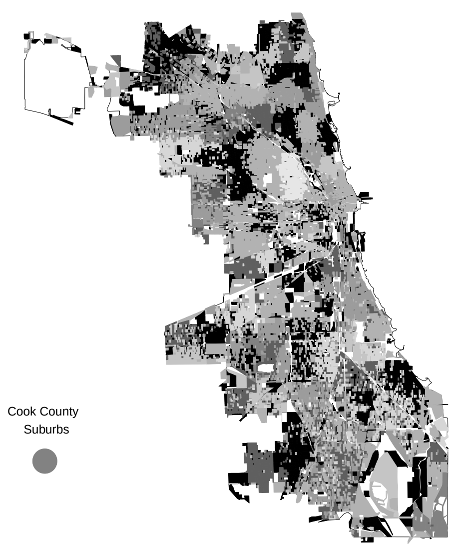
(a) Level 0: 146 Estimated Groupings



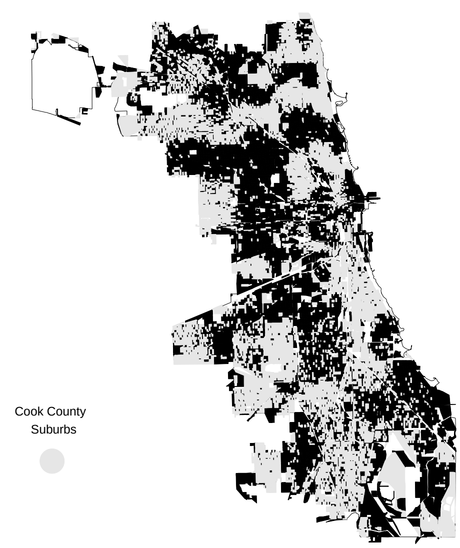
(b) Level 1: 28 Estimated Groupings



(c) Level 2: 8 Estimated Groupings



(d) Level 4: 2 Estimated Groupings



Note: This figure maps the estimated groupings for the different levels of the nested approach.

Additionally, there may be a concern that TIF generates changes in amenities and property values, which in turn change the housing markets. Then, the estimated market spillover effect contains the endogenous change in the housing markets and the spillover effect. One way I address this concern is to use binary SBM. That is, the elements of the adjacency matrix are one if there is any movement between the corresponding Census blocks and zero otherwise. The elements do not depend on the number of households that move between Census blocks. This classification allows for TIF to affect the number of households that move between Census blocks without affecting the underlying market structure. In this context, Chicago's persistent neighborhood definitions and patterns of residential segregation help bolster this approach (Aaronson et al., 2021).

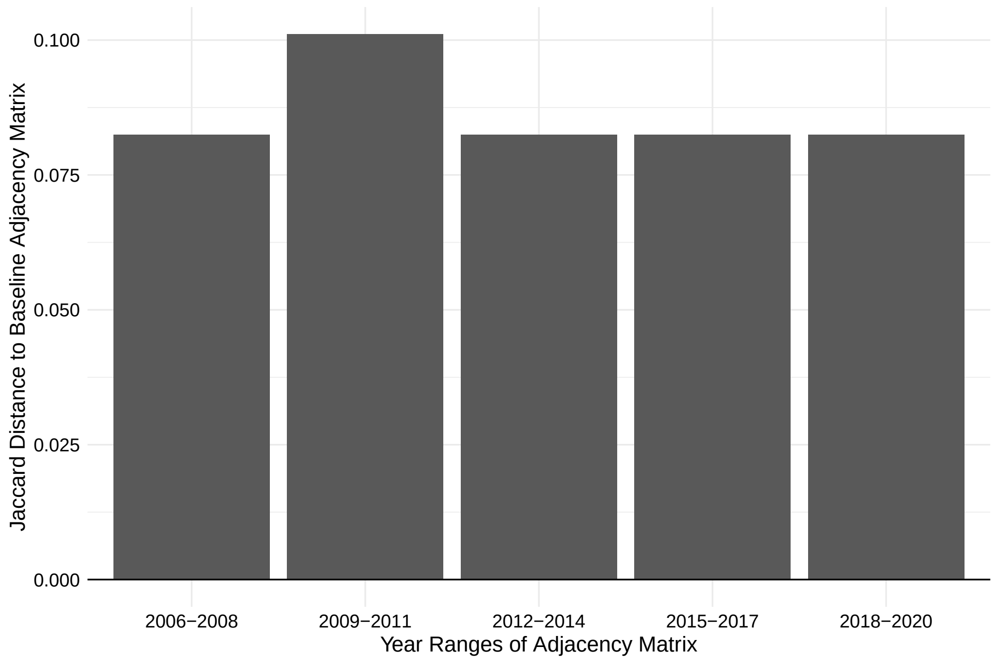
I further interrogate the decision to use all observations between 2006 and 2020 to classify housing markets. I quantify the degree to which the pattern of movements between Census blocks changes over time. If the pattern of movement in certain years varies from the overall pattern of movement observed between 2006 and 2020, then my baseline estimates may be capturing endogenous changes to the housing market structure. I calculate the binary adjacency matrices for ranges of years: 2006-2008, 2009-2011, 2012-2014, 2015-2017, 2018-2020. That is, for each year range $\tilde{t}$, I define the adjacency matrix $\boldsymbol{A}^{\tilde{t}}$ using the moves that occur during $\tilde{t}$. Grouping years into these ranges reduces noise that is natural from year-to-year while still disaggregating the full panel. I calculate the Jaccard distance between these matrices and the adjacency matrix I calculate using the full panel, $\boldsymbol{A}$ (Jaccard, 1901; Kosub, 2019). The Jaccard distance represents the proportion of elements between the two matrices that are equal:

$$\begin{aligned}\delta(\boldsymbol{A}^{\tilde{t}}, \boldsymbol{A}) &= 1 - \frac{\mid \boldsymbol{A}^{\tilde{t}} \cap \boldsymbol{A} \mid}{\mid \boldsymbol{A}^{\tilde{t}} \cup \boldsymbol{A} \mid} \\ &= 1 - \frac{\mid \boldsymbol{A}^{\tilde{t}} \cap \boldsymbol{A} \mid}{\mid \boldsymbol{A}^{\tilde{t}} \mid + \mid \boldsymbol{A} \mid - \mid \boldsymbol{A}^{\tilde{t}} \cap \boldsymbol{A} \mid}.\end{aligned} \tag{A.6}$$

A Jaccard distance of zero indicates that the matrices are identical while a distance of one indicates that no elements of the matrices are the same.

Figure A.7 plots the distances between each of the year ranges and $\boldsymbol{A}$. They are all

Figure A.7: Jaccard Distance of Adjacency Matrices



Note: This figure plots the Jaccard distances between adjacency matrices based on movement during different year ranges and the adjacency matrix based on movement across 2006-2020. The Jaccard distance is a measure of similarity between matrices that ranges from 0 (identical elements) to 1 (no elements in common) (Jaccard, 1901).

small indicating a large amount of similarity. Across the year ranges, they are quite similar with the exception being 2009-2011, coinciding with the Great Recession. Even in this year range, however, only approximately 10% of the elements of the adjacency matrix differ from baseline one.

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