Does urban development influence crime? Evidence from Philadelphia's new zoning regulations*

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Abstract

This paper estimates the effect of enacting a new zoning code in Philadelphia on urban development and crime. The new zoning code was intended to ease regulatory burdens for property development and land uses changes, but the law allowed city council members to keep prerogative over urban development in their districts. The council district prerogative created arbitrary geographic discontinuities in the ability of the zoning code to promote urban development. Using a difference-in-discontinuities design, we find that the new zoning regulation caused a 35 percent reduction in land use zoning changes and building permits in council districts less friendly to urban development relative to neighboring districts. The decline in urban development had no short-term effect on crime. Construction projects and changes in land use appear to occur in the most densely populated areas, suggesting that council districts less inclined to urban development prevent residential construction in areas that would otherwise be a source for new residential housing development.

Keywords: zoning policy, land use changes, crime, difference-in-discontinuities, machine learning

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1 Introduction

The notion that urban landscape changes influence crime has a long history in social science research (MacDonald, 2015). Since the early nineteen-hundreds, when municipal governments enacted the first comprehensive zoning ordinances, zoning has been the primary mechanism by which local governments regulate land use (Anderson et al., 2013). The economics literature on land use zoning has largely focused on how zoning restricting specific uses of land causes higher prices for housing and the location of firms and residents (Glaeser and Ward, 2009). Zoning then impacts the allocation of resources spent on housing, location decisions, and the overall density of urban land uses (Duranton and Puga, 2015, 2020). Zoning may also impact public safety.

The connection between crime and zoning gained some prominence in public discussions after Jacobs (1961) criticized the city planning and rebuilding principles that emphasized superblocks, or the zoning of high rise single-use developments that separated commercial and residential properties. Jacobs suggested that mixed-use zoning that integrated commercial and residential uses on the same block would reduce crime by creating more natural surveillance, or *eyes upon the street*. In criminology, the idea that urban land uses impact crime can be seen in the criminality of place, crime prevention through environmental design, and informal social control concepts (Jeffery, 1969; Greenberg et al., 1982; Brantingham and Brantingham, 1995; Taylor et al., 1995; Sampson and Raudenbush, 1999).

In the past couple of decades, the availability of highly granular geographic data on parcels of land and crime has permitted an examination of the association between zoning and crime (Browning et al., 2010; Sampson and Raudenbush, 1999; Stucky and Ottensmann, 2009). Research generally shows a positive association between commercial land uses and crime. However, most of this evidence is correlational, and only a few studies estimate causal impacts of zoning changes on crime (Anderson et al., 2013; Twinam, 2017).

There is a related literature that examines how alterations in the physical environment influence crime. Specifically, research suggests that building regulations that mandate the installation of operable doors and windows reduces serious crime nearby Kondo et al. (2015). Requiring the installation of burglary resistant doors and windows on new home construction also appears to reduce burglary offenses (Kondo et al., 2015; Vollaard and van Ours, 2011). Experimental evidence on crime prevention through environmental design has shown reductions in criminal offenses after vacant lots are remediated with urban greening programs (Branas et al., 2018) or when enhanced street lighting is installed in close proximity to public housing developments(Chalfin et al., 2021).

The overall effect of zoning on crime, however, remains ambiguous. Zoning changes that promote more residential development, for example, could reduce crime by promoting more neighborliness among homeowners and increasing the willingness of residents to act as guardians of their neighborhood streets (Jacobs, 1961; Taylor et al., 1995; Sampson et al., 1997). However, zoning that restricts urban development may cause crime to increase by making housing and business construction too costly, reducing the revitalization of neighborhoods, and increasing the spatial concentration of poverty.

In this paper, we exploit the variation caused by the enactment of a new zoning code in Philadelphia, Pennsylvania, that permitted an easier ability to change land uses but allowed council members the ability to decide on whether they ultimately approved economic development projects in their districts. Specifically, we compare areas closer to the council districts' borders before and after the policy change. We identify the treatment areas using the relative presence of developers and real-estate donors in council members' political contributions, while the control group is their contiguous districts. Our identification strategy is similar to Billings et al. (2013) who estimate the impact on ending race-based busing by comparing students who live in the same neighborhoods, but because of the change in busing policy are now placed in redrawn school boundaries.

We combine the difference-in-difference and regression discontinuity methods through the difference-in-discontinuities estimator (Grembi et al., 2016), finding that the zoning and construction permits per 100 parcels decreased by 35 percent in areas less friendly to property development relative to the pre-treatment years and the neighboring districts. Despite this significant difference in urban development projects, there are no discernible impacts on property (theft, burglary, and motor vehicle theft) or violent crimes (homicide, rape, robbery, and assault). These findings suggest that the net effect of economic development measured through land use zoning changes and building permits on crime is null. Our results are robust to alternative functional forms and falsification tests.

The heterogeneity analysis reveals that the least densely populated areas may have experienced a null effect on urban development. In contrast, low-pedestrian activity places may have experienced an increase in property crime. The potential causal mechanisms relied on an unsupervised machine learning method to classify the zoning permits, finding that the reduction in urban development projects came from residential rather than commercial properties. The results suggest that alterations in the housing stock exhibit little influence on crime, at least in the short-term. Pedestrian activity appears to attenuate the relationship between property crime and urban development, suggesting that *more eyes upon the street* are relevant to deter crime.

We organized the article as follows. Section 2 reviews the literature between crime, zoning, and land use changes and describes local officials' role in urban planning, including Philadelphia's zoning ordinance. Sections 3 and 4 explain the data and empirical strategy. Section 5 exhibits the results, and Section 6 concludes.

2 Background

2.1 Research on crime, zoning, and land use change

Zoning, land use, and built environment are interrelated concepts. Zoning usually refers to the prohibition or permission given to a property for a specific use. Land use refers to how owners adapt it for a particular current use, and the built environment includes the surrounding physical area around a parcel. These differences are not only conceptual but also practical. Sometimes, it is easier to change a parcel's zoning than modifying its use (i.e., applying for a new permit is faster than tearing down a house to build an apartment complex). A parcel or lot may also modify its use without transforming its zoning. For instance, when a residential property becomes a vacant lot, it has not changed its zoning, but it has changed in use (Anderson et al., 2013).

Local regulations on construction, occupancy, street design, and transportation patterns influence the urban landscape and criminal offenses (MacDonald, 2015). Different criminological theories explain the role of zoning and land use on crime. Six decades ago, Jacobs (1961) argued that mixed-land use promotes more *eyes upon the street*, as commercial uses encourage more foot traffic to neighborhoods and shop keepers, along with residents, could act as proprietors of public spaces. This idea received some criticism as Taylor et al. (1995) noted that commercial property is more likely to displace residents, bring outsiders into neighborhoods for commerce, and lower neighborhood informal social controls. Sampson et al. (1997) introduced the theory of collective efficacy, stating that the combination of informal social control and cohesion advance collective goals around neighborhood well-being and safety that are more likely in residential areas. These perspectives suggest that neighborhood changes in informal social controls will impact crime, but they differ in expectations based on land use changes.

Other theories concentrate on how land uses influence crime opportunities by shaping the ambient population in places. Routine activities theory, for example, suggests that crime is a product of motivated offenders, suitable targets, and the lack of capable guardians (Cohen and Felson, 1979). Crime pattern theory builds on this idea, suggesting that "offenders look to cues about crime opportunity in the physical and social environments in which they carry out daily routines" (Wilcox and Cullen, 2018, p.129). Zoning laws that promote economic development leading to the creation of more commercial spaces and residential turnover could increase anonymity among residents and reduce the willingness of neighbors to act as capable guardians (MacDonald, 2015). On the other hand, new urban developments could also bring wealthier residents to a neighborhood who are more likely to engage in self-protection efforts (e.g., burglar alarms, surveillance cameras, police-community meetings) to reduce the risk of crime (Cook, 1986).

While criminologists suggest there is a relationship between crime and land use changes brought on by urban development, it remains ambiguous whether land uses change will lead to net changes in public safety. Urban development may displace residents to other places (i.e., adjacent neighborhoods), targets (i.e., young versus older potential victims), or criminals' modus operandi (i.e., residential burglary rather than motor vehicle theft), leaving unchanged the overall crime levels (Weisburd et al., 2006; MacDonald, 2015).

Observational studies have found that increases in commercial and mixed-land uses associate with more criminal offenses, even after controlling for sociodemographic factors (Browning et al., 2010; Greenberg et al., 1982; Sampson and Raudenbush, 1999; Stucky and Ottensmann, 2009; Taylor et al., 1995). Cross-sectional research suggests that land use density has a curvilinear relationship with crime (Browning et al., 2010), influenced by residential tenure (Greenberg et al., 1982), and neighborhood disadvantage (Stucky and Ottensmann, 2009). Other research has found that land use zoning changes associated with residential and mixed-use development relate to crime reductions (Anderson et al., 2013).

One concern of these studies is that they rely on correlational designs.[1] The results could be an artifact of omitted variable and simultaneity biases. For instance, individuals sort themselves into neighborhoods based on their preferences and the area's characteristics (Duranton and Puga, 2020). While some factors like education and income of the residents are observable, other traits, such as an aversion for strangers or preference towards maintaining neighborhood networks, are unobservable and could be the driving factors influencing the connection between land use zoning and crime.

In the urban economics literature, few studies provide causal estimates of land use changes that closely connect to Jane Jacob's idea that *more eyes upon the street* reduce crime. Twinam (2017) relied on historical zoning laws in Chicago to estimate the impact of current land uses on crime, finding that commercially zoned areas cause significantly higher crime rates in more walkable places. Nonetheless, this effect decays and reverses in high-density population areas, implying that mixed-land use areas exhibit fewer criminal behaviors than entirely residential communities, partially vindicating Jacob's argument. Likewise, Chang and Jacobson (2017) found that the effect of business closures on crime varied with

[1] For a detailed literature review of criminology and urban planning studies analyzing the role of the built environment, zoning, and land use changes on crime, we refer readers to MacDonald (2015) and MacDonald and Stokes (2020). The authors distinguish between correlational and causal studies.

walkability indexes in Los Angeles. Areas less favorable to pedestrians have more than double the impact on property crime than walk-friendly places. Twinam (2017) provides the only study that has a plausible causal estimate of the impact of zoning on crime. But this study relies on historic zoning laws as an instrument for current zoning, and does not get to observe changes in crime before and after zoning changes occur. In general, there are no studies that provide causal estimates of the impact of changing zoning on urban development and crime. This paper contributes to this knowledge gap.

2.2 Zoning regulation and local governments

In 1914, New York City enacted the first modern zoning legislation (MacDonald and Stokes, 2020). The initial rationale for zoning policies was to physically separate residential and commercial retail from the visual eyesore of industrial manufacturing land uses. After World War II, the scope of land use regulation expanded substantially in the United States, aiming to fulfill the cities' comprehensive plan and the collective interest (Schleicher, 2012). Other reasons for restricting zoning policy include reducing exposure of residential neighborhoods to environmental toxins caused by industrial use, fostering positive agglomeration spillover effects, and homeowners' monopoly power in restricting entry to increase property values and residents' welfare at the expense of potential newcomers (Duranton and Puga, 2020).

Land use procedures in the United States have four major components (Schleicher, 2012): 1) a master plan that defines a city's land use statement of goals and classifies the areas on public and private land use; 2) a zoning map that designates the allowable land use and construction restrictions for each property parcel in the city; 3) amendments that change the zoning map on a case-by-case basis with limited scope; and 4) zoning variances that make exceptions and waive requirements to the local legislation. Local governments could regularly revisit their master plan and zoning map entirely, but in practice, they seldom do. For instance, Philadelphia published a new comprehensive master plan in 2011 after fifty years without a new one (City of Philadelphia, 2011).

In many US cities, elected officials have a substantial influence on zoning and land use changes. Formally, by setting up the procedures for developers and businesses. Informally, by enforcing the *councilmanic prerogative*,[2] a non-written tradition where elected officials defer the decision to the member in whose district resides the development project (Schleicher, 2012). Colloquially, it is summarized as "I won't mess with your turf if you won't mess with mine" (The Pew Charitable Trusts, 2015, p.3). The councilmanic courtesy works primarily in two forms. In the selling and leasing of city-owned land that requires the city council's approval, and when property developers look for exceptions to the land use regulations,

[2] Also known as *councilmanic courtesy, aldermanic or supervisorial prerogative*, and *member deference* (Louthen, 2020).

they benefit from having the support from their district's council member (The Pew Charitable Trusts, 2015). Critics of the traditional councilmanic prerogative argue that it begets a pay-to-play culture and encourages corruption. Anecdotal evidence supports such a claim.[3] Supporters, including elected officials, justify it as a way to protect neighborhoods (Briggs, 2019b).[4] Developers also have incentives to support the councilmanic courtesy as it concentrates the lobbying activity to fewer political actors and, sometimes, only to one city council member (Louthen, 2020).

Finally, council members in cities with single political party control have substantial influence over their districts' issues because they seldom face election challenges, and there is little knowledge about candidates' political platforms (Schleicher, 2012). Hence, this situation strengthens their local power over urban development projects.

2.3 Philadelphia's zoning code and local politics

Philadelphia Home Rule Charter establishes the City Council as the legislative branch of the local government and confers the power of enacting laws and resolutions, approving the city operation budget, and overseeing the appointment of boards and commissions members, among other powers (The Committee of Seventy, 2015). The City Council consists of ten members elected by district and seven members representing Philadelphia citywide. The ten council districts represent an approximately equal population, and their geographical boundaries change after each decennial US Census to achieve such a balance (The Committee of Seventy, 1980). **Figure 1** shows the edges of the ten council districts in Philadelphia based on the 2000 Census redistricting, which its the geographical division we used for this research.

Council members are elected for four years with the option of unlimited re-election terms (The Committee of Seventy, 1980). Every election, Philadelphia residents cast a vote for a council district candidate and five votes for at-large candidates' members. Commonly, the ten elected members by district focus on their neighborhoods and constituency, while citywide council members represent Philadelphia as a whole. Council members have an average tenure of 15.5 years, longer than many other large US cities. City council members representing districts cannot announce a candidacy for different local elected positions while in office, which may deter them from seeking other political offices (The Pew Charitable Trusts, 2011). In 2010, only two out of the ten council district members were in their first term in office. Four districts saw changes in their elected council member between 2010 and 2015 (our study period) as the incumbent

[3]For example, council members of Philadelphia and Chicago have faced or are facing criminal charges related to granting permits in exchange for money (Roebuck and Brennan, 2020; Freund, 2019)

[4]For instance, a spokesperson for a Philadelphia's council member declared that "councilmanic prerogative is a tool we can use to protect neighborhoods from gentrification and from out-of-town developers who don't know anything about the neighborhood" (Briggs, 2019a).

decided not to run for office.[5]

After four years of debate, in December 2011, Mayor Michael Nutter approved a new zoning regulation, changing The Philadelphia Code *Title 14 Zoning and Planning.* The new legislation became effective in August 2012 and replaced the 1962 code, written when manufacturing industries were dominant in the city. The 1962 code had become outdated, was very complicated, and was seen as being restrictive to property developers. Zoning variances for approving exceptional construction projects had become practically the only method for new property developments, and the 1962 zoning code did not recognize newer businesses. The new zoning law changed building restrictions (i.e., height and front garage limitations in many areas), made more transparent the construction process (i.e., listing the constructions you can build rather than the ones you cannot), legalized some property parcels (i.e., "non-conforming" rowhouses), and updated zoning classifications (i.e., include broader categories that have most commercial and industrial uses), among other changes (Saffron, 2012; Franklin and Gaston, 2012).

The simplification of approving building projects encouraged property development and made it easy to file a building permit. However, rather than eliminating the long-standing, informal councilmanic prerogative tradition, it may have strengthened it. For instance, the new zoning code introduced the Registered Community Organizations (RCO) and the applicant's requirement to meet to discuss any development proposal with them. If there are no RCO within the proposal's boundaries, the district's councilperson serves as the local RCO. The Zoning Board Commission, a panel that hears appeals of rejected zoning permit applications, does not need the district's council member permission for making any decision. Still, the Commission rarely votes against the councilperson's wishes. In short, developers do not require the council member's approval, but they prefer it to keep good relationships with them (The Pew Charitable Trusts, 2015).

Other practices highlight the council members' influence on urban planning. For example, developers can bypass the Zoning Board Commission by approaching a councilperson directly, as the City Council has the authority to make amendments to the zoning map. An analysis conducted by The Pew Charitable Trusts (2015) revealed that the Philadelphia City Council unanimously approved 99.5 percent of local bills concerning land use regulations within a single council district between 2008-2014. Similarly, council members may encourage community benefits agreements between developers and local organizations that state the developers' commitments in exchange for support of the construction project (Schleicher, 2012).

Interest groups shape the government's local agenda and the policy issues that affect urban development

[5] In 2012, councilpersons Anna C. Verna, Joan L. Krajewski, Donna Reed Miller, and Frank DiCicco retired or decided not to run for office after being in office since 1976, 1980, 1996, and 1996, respectively. No evidence suggests that these decisions were motivated by land use policy or local crime rates.

(Krebs, 2005). One mechanism to reflect such influence is through political donations. Candidates running for office in the US receive most of their campaign funds through private rather than public financing (NCSL, 2019). Although around one of every ten US citizens contributes to a candidate's campaign, most of them give less than 250 dollars (Hughes, 2017). There are reasons to believe that some groups are more influential than others. For example, large, well-organized, and affluent groups that overcome collective action problems are more likely to be politically active. Also, groups that are financially motivated to receive privately directed benefits from political action have strong incentives to make their voice heard. Krebs (2005) showed that individuals and organizations linked to land use and development have extensive participation in local political donations. These actors, commonly referred to as the growth machine group (real-estate developers, construction companies, home-builders, engineers, and architects), can account for one of every six dollars of local political contributions.

Given that land use and property development groups substantially influence local politicians, we decided to use the campaign contributions to identify council members' affinity towards urban development in Philadelphia. We retrieved the political contributions from elected council district members between 2010 and 2015 from the Philadelphia Campaign Finance Site. The 14,108 donations amounted to $11.82 million dollars, with an average (median) donation of $838 ($500) dollars. In comparison to Krebs (2005) that used the reported donor's occupation and employer to categorized the political contributions, our political data poorly describes such fields or are left blank. To circumvent the challenge of identifying each donor's interest group, we selected only the top 10 percent of donors of each elected council member: the 1,884 donors accounted for 53.1 percent of the total political donations, with a mean (median) of $3,336 ($2,500) dollars donation. These contributions are considerably larger than those made by the remaining 90 percent of the donors, which had a mean (median) campaign contribution of $453 ($300) dollars.

We identified construction and property development donors by manually searching online their names and addresses. Most individuals and organizations have websites and appear on industry network sites, making it feasible linking them to their employer or business. **Figure 2** exhibits the relative contribution made by the top 10 percent donors and those identified as land use and development donors. The top donors accounted for between 41.7 to 66.7 percent of the total campaign donations among council members. Meanwhile, construction and real-estate top donors' contributions have a wider interval ranging from 8.3 to 48.9 percent of the local elected officials' campaign resources.

These two groups emerge once we estimate the relative contribution of the urban growth machine. Six council districts (1, 2, 3, 5, 6, and 10) received $73 out of $100 collected by the highest decile donor from land use and development interest groups. In comparison, four remaining council districts (4, 7, 8, 9) collected $19 out of $100 from top donors identified as land use and development donors. Consequently, property developers should have less influence in local politics in the four districts where they contribute substantially less in political donations. We use these two groups to classify council districts as more (n=6) and less (n=4) friendly to urban development.

3 Data

3.1 Data sources

This research uses data from different public sources.[6] Crime incidents come from the Philadelphia Police Department and provides time of reported crimes and its geographic coordinates (latitude-longitude). The crimes are categorized into part I and II Uniform Crime Report (UCR) definitions. Building and zoning permits issued in the City of Philadelphia come from the Department of Licenses and Inspections and include type of permit, date of issuance, address, and latitude and longitude coordinates. The Philadelphia Department of Records provides property parcels information in spatial vector data format (*shapefile*). The map layer of the council district divisions based on the 2000 Census redistricting comes from the Department of Planning and Development.

We also include social-demographic variables collected from the American Community Survey. We retrieved the five-year estimates at the census tract level on the percentage of black, white, and Hispanic population, and age groups (below 14, 15-24, 25-39, 40-54, and over 55 years old). We also include the schooling attainment (percentage of residents with less than high school, high school, some college, and college education) and the unemployment rate. Finally, we extracted the political contributions from the Philadelphia Campaign Finance Site, which contains the name of the contributor, address, amount, date, and supported candidate or committee name.

3.2 Analytical database

Our analytical database uses information from 2010 to 2015 aggregated to the quarterly-year level. This period limits any effects of the Great Recession on the housing market that may reduce the external validity of our research. This restriction in time also avoids confounding zoning changes with redistricting of Philadelphia's council districts that went into effect in January 2016.

We recognize that aggregating data at uneven units may impose challenges in the spatial analysis of

[6] Data retrieved from www.opendataphilly.org/dataset, https://data.census.gov/cedsci, and http://phila-records.com/campaign-finance/web

crime and planning permits –the modifiable areal unit problem (Bernasco and Elffers, 2010; Ratcliffe, 2010). To ensure homogeneous geographical units that other administrative units like census tracts do not provide, we created hexagonal grids of 70 meters per side (12,731 m^2) covering Philadelphia. Previous research has also employed grid cells to analyze the relationship between crime and land use (Stucky and Ottensmann, 2009; Twinam, 2017).

We classified the permits type into five categories.[7] Construction includes demolitions, new constructions, operations, site work, and utility permits. Zoning refers to changes in the land and property uses, including altering the height and size of buildings, parking requirements, and population density limits, demolishing a structure, and installing certain billboards and signs. Renovation relates to additions and alterations in the inner or outer appearance of the building or structure. Electrical, mechanical, plumbing, and fire protection permits compose the fourth category. The remaining classification refers to occupancy certifications and other administrative requirements. This research focuses on the first two categories (construction and zoning permits) as they measure urban development.

The property parcels are in spatial vector form. We converted the polygons to data points by estimating their geometric center (centroid). We cannot link the permits and property parcels information on a one-to-one basis using the public data because they do not share a reliable, unique identifier. For instance, the latitude, longitude coordinates do not match for all observations even after truncating the values to a four-decimal point precision. Similarly, the address field is incomplete (i.e., without address numbers) or has typographical errors (i.e., misspelled street names). Although a machine learning-based probabilistic matching algorithm would yield better linking results than an exact matching method (Tahamont et al., 2020), it is common to file one permit for two-three adjacent properties (i.e., property address numbers follow the pattern 7313-15 XYZ Street). Consequently, none of these methods work for databases that are not meant to have a one-to-one relationship.

To overcome these limitations, we sum all property parcels and permits at the hexagonal level by overlaying the hexagonal grid cell and the data point map layers. We estimated the permit rate as the number of permits per 100 parcels in the hexagonal unit. Under the classical measurement error model in the dependent variable, this approach yields larger standard errors but consistent estimators (Wooldridge, 2010). We excluded hexagons without parcel property centroids, which usually are large public parks, the Schuylkill River, and sizable properties such as the airport or warehouses. A closer inspection to **Figure 3** reveals such excluded units within 500 meters of our main analysis area.

The crime incidents dataset includes the latitude, longitude, and address rounded to the hundred block.

[7] The dataset has 25 broad classifications, which are self-explanatory, making it easy to group them.

We removed 83 out of the 768,870 crimes reported between 2010 and 2015 with geographic coordinates outside of Philadelphia. We manually compared the coordinates and the address on a random sample of crimes, finding an accurate geo-referencing process. We aggregated all criminal offenses at the hexagonal level by spatially joining the grid cell and crime data points.

Finally, we overlap the centroids of each hexagon with the council district map layer to identify the location of each hexagon. We followed a similar process for linking the census tracts and block groups map layers to the grid cells. We also used the hexagon centroids to estimate the euclidean distance to the closest council district border.

Table 1 exhibits descriptive statistics for the areas within 500 meters of the treatment and control council districts between 2010Q1 and 2011Q2 (the pre-zoning law change). The average hexagonal unit had 1.4 crimes in any given quarter. Theft and burglary are the most common crimes, followed by assault and robbery. The distribution is consistent with national crime data. There are slightly more criminal offenses in the less development friendly (treated) council districts. Both the treatment and control grid cells have similar levels of parcels and permits: 59 parcels and 23 permits per 10,000 parcels per quarter. Most of the planning permits refer to zoning changes as there are around six zoning permits per one construction permit, reflecting the need for several zoning permits before the city agency can issue a building permit. Likewise, the commercial and residential land use extensions are similar among both groups.[8]

The treatment and control groups resemble Philadelphia's ethnic composition. Five out of ten individuals self-identified as Black, being the most prominent race group. Meanwhile, three out of ten people are White. Hispanics represent 15 and 35 percent in the treatment and control, respectively. The population is evenly distributed across age groups. Younger individuals (less than 24 years old) represent 38 and 41 percent of the average treatment and control census tract population, respectively. Around one of every five persons have more than 55 years old. Nearly 30 percent of the people above 25 years old are high school dropouts; this figure is above Philadelphia's average of 20 percent. Similarly, the fraction of individuals with a college degree or higher is slightly below the City's average of 23 percent. Finally, the unemployment rate in the average census tract is 17 percent, which is reasonably similar to the overall city rate, but above the national unemployment rate.

[8]Other land use categories not included in the descriptive statistics table are institutional development, industrial, recreational, and sports stadiums.

4 Empirical strategy

The main objective of this research is to estimate the effect of land use changes on crime. A simple comparison of neighborhoods with and without land use changes would yield a naive estimation. Areas experiencing changes in land use from development are likely different than those not having zoning changes. Even an econometric model that regress land use changes on criminal offenses while controlling for observable characteristics (Anderson et al., 2013), would not provide causal evidence. Property developers could, for example, push through zoning changes in areas that have a great demand for affluent residents to rent or buy housing.

A first candidate method to estimate the causal impact of land use changes on crime could be comparing the entire council districts before and after introducing the new zoning code in Philadelphia (a difference-in-difference model). This identification strategy relies on the parallel trends assumption among treated and control units during the pre-policy period. Although this assumption is testable, there are a priori reasons to believe that the treatment and control groups have differential trends due to geographic features that would bias the estimates. For instance, council districts four and eight contain the two largest parks in the city (Fairmount and Wissahickon Valley), hindering widespread land use changes. Likewise, Philadelphia's domestic and international airports, with particular land purposes, occupy large land extensions in council districts two and ten. A similar argument can be made for other areas of the city.

A second candidate approach could be a cross-sectional regression discontinuity comparing the areas contiguous to the council districts that should be very similar in observable and unobservable characteristics. An inspection of Philadelphia's map shows that most of the streets and avenues used in the council district boundaries do not impose geographical barriers among neighborhoods that could create differences.[9] However, some areas use rivers and large public parks to divide the council districts creating natural barriers causing differential changes across contiguous neighborhoods that limit the validity of the cross-sectional regression discontinuity model.

To overcome the previous identification limitations, we use a difference-in-discontinuities model. The estimator, formally explained in Grembi et al. (2016), combines the difference-in-difference and regression discontinuity designs. The identification comes from exploiting the before and after and discontinuous spatial variation of the policy change (Philadelphia's new zoning code). Specifically, we estimate the

[9] The census tracts and council districts boundaries do not match entirely. For instance, several council district borders divide the census tracts in irregular shapes, making it difficult to compare them using census data without making assumptions about their spatial distribution. As an alternative method, we used Google Maps and Google Street View to identify possible barriers on residents to freely move across neighborhoods. We did not find such barriers.

following model restricting the sample within distance h:

$$y_{it} = \gamma_0 + \gamma_1 dist_i + treat_i(\gamma_2 + \gamma_3 dist_i) + post_t[\beta_0 + \beta_1 dist_i + treat_i(\beta_2 + \beta_3 dist_i)] + e_{it} \quad (1)$$

where y_{it} is the outcome variable (i.e., permits per 100 parcels or crime count) in hexagon i in quarter-year t, $dist_i$ is the distance in meters to the border, being positive if hexagon i is in the treated council districts and negative otherwise, $post_t$ equals to one for observations in the post-treatment period ($t \geq$ 2011Q4). We estimate the effect relative to the fourth quarter of 2014, when the new zoning code was approved, rather than the third quarter of 2012, when it was implemented. This approach let us assess for possible anticipatory effects. As explained in detailed in **Section 2.3**, we use the relative ratio of construction and property developers among council members' top 10 percentile of political contributions to identify the treatment (more friendly) and control (less friendly) districts to zoning changes. Hence, $treat_i$ is a treatment indicator variable of whether hexagon i is a council district where construction and property developers donate a relative small share to political campaigns (Council Districts 4, 7, 8, 9).

The main coefficient of interest is β_2 that identifies the effect on crime or permit rate of the differential propensity of council districts to approve planning permits relative to its neighbors and to the pre-zoning change period. Given the non-negative count nature of crimes and permit rates, and its abundance of zeros at the quarter-year-hexagonal level, we estimate equation (1) using a Poisson regression model. We clustered the standard errors at the census tract level, but we also show that the statistical significance is qualitatively similar under different clustering selections. We select the optimal bandwidths following Imbens and Kalyanaraman (2012) and Calonico et al. (2015) methods. In the robustness sections, we test for higher-order polynomials in the functional form.

The number of studies using regression discontinuity and difference-in-differences is extensive.[10] Research studies combining spatial discontinuities and before-after variations to provide causal evidence are more recent. Grembi et al. (2016) exploited this variation to study the effects of fiscal policies, Hansen et al. (2020) used it to examine cross-border cannabis shopping. This approach has also been used in crime research to assess the effects of marijuana legalization (Dragone et al., 2019) and changes on guns policy (Chicoine, 2017). In the urban economics literature, Valentin (2020) and Koster et al. (2018) have implemented similar empirical methods to assess the effect of short-term rental regulations on housing.

[10]For instance, Imbens and Lemieux (2008) and Athey and Imbens (2017) provide a review of these techniques on policy evaluation.

5 Results

We present the estimates on zoning and construction permits rate to verify that four council districts less friendly to urban development experienced fewer land use changes than their contiguous neighbors after the approval of Philadelphia's new zoning code. Then, we analyze the impact of land use alterations on crime.

5.1 Effects on planning permits

We begin with descriptive evidence of the differential impact of the new zoning code on permit rates across council districts. **Figure 4** shows the scatter plot and second-order polynomials fit of the difference of each quarter-year post-treatment (i.e., 2011Q4, 2012Q1, ..., 2015Q4) and pre-treatment (i.e., 2010Q1, 2010Q2, ..., 2011Q3) outcome using bins of 25 meters in a half kilometer bandwidth. These plots exhibit a discontinuity at the border before and after the new zoning was approved. There are fewer zoning and construction permits in council districts that are less friendly (treatment) to urban development than their neighboring areas. The confidence intervals suggest that the difference is not due to chance alone.

Figure 5 presents the difference-in-discontinuity estimate on the permit rate following equation (1) and using Poisson regressions, alternative bandwidths ranging from 200 to 1,200 meters, and clustering the standard errors at different levels (census block group, census tract, and hexagon). As expected, estimations using smaller bandwidths have wider confidence intervals, narrowing as the bandwidth increases. The estimates are not significant at the 95 percent level for small bandwidths, but once surpassing the half-kilometer bandwidth, the effects become statistically significant, reflecting the variance-bias trade-off. The magnitude is always negative and relatively insensitive to the bandwidth selection.

Our preferred difference-in-discontinuity estimates using the optimal bandwidths and robust-standard errors clustered at the census tract level are in **Table 2**. The optimal bandwidths following Calonico et al. (2015) are around 600-700 meters to the border (Panel A), equivalent to four to six street blocks away from the closest council district. The optimal bandwidths using Imbens and Kalyanaraman (2012) approach are in the 930-990 meters range (Panel B). The estimates imply a 29 to 32 percent reduction in the quarterly zoning permit rate in council districts with fewer ties to urban development donors after approving the new legislation (percentage change estimated as $e^{-0.344}$ and $e^{-0.381}$, respectively). The construction permit rate impact lies between a 38 and 46 percent decrease, while the effect on the joint rate of zoning and building permits ranges from a 32 to 35 percent reduction. All the estimates are statistically significant.

5.2 Effects on crime

Next, we consider the effects of the new zoning code on reported crime in neighboring council districts across time. **Figure 6** exhibits the descriptive graphical evidence of the difference-in-discontinuities estimates on burglary, theft, and motor vehicle (which comprise property crime), and murder, rape, robbery, and assault (classified as violent crime). No single criminal offense reveals a sharp jump at the threshold. For instance, theft, motor vehicle theft, and assault practically have the same functional form on either side of the border. For other crimes, such as burglary, murder, and robbery, the overlapping confidence intervals between the treatment and control groups do not support the claim that there are differential changes in crimes across contiguous council districts with different propensities towards urban development.

Figure 7 shows our main coefficient of interest, β_2 from equation (1), restricting the Poisson regression to observations within 200 to 1,200 meters to the border in increments of 50 meters. None of the 12 criminal offenses for any bandwidth selection show a statistically significant effect different from zero at the 95 percent level. Also, the sign and magnitude are susceptible to the bandwidth. For instance, burglary, theft, and overall property crime exhibit positive point estimates for areas very close to the border but become negative as it moves further away. The opposite situation applies to non-residential burglary, rape, and violent crime.

Table 3 presents the optimal bandwidths for each crime outcome. The Imbens and Kalyanaraman (2012) bandwidths go from 389 to 614 meters, while the Calonico et al. (2015) optimal bandwidth ranges from 647 to 1,058 meters to the border. Robbery is the only crime with a significant negative effect at the 90 percent level. It is not surprising that at least one out of the 20 estimates shows a significant result, likely a false discovery (see section 5.3.2). In short, there is no evidence of differential changes in public safety at the border before and after the approval of the new zoning law in Philadelphia.

5.3 Robustness

The previous section exhibited significant impacts of the new legislation and the council members' prerogative over urban development projects on zoning and building permits, but no supportive evidence on differential changes in crime. In this section, we assess the robustness of our results to relevant analytical decisions taken in the research process.

5.3.1 Sensitivity analysis on zoning changes

We begin testing the sensitivity of our estimates on planning permits. To summarize our robustness checks, we use a specification curve as Simonsohn et al. (2020) to display the distribution of estimates under different assumptions in a convenient way. It also identifies (if any) the analytical decisions that are the most consequential for changing our key findings and provides a more precise statistical inference of our econometric modeling. We conducted 192 different specifications for each of our three dependent variables: zoning, construction, and its combined permit rate. The specifications come from the combination of six different bandwidths under four different polynomial functions, including or excluding covariates, using as the post-treatment variable the approval or implementation date, and removing outliers. **Figure 8** shows the point estimates and confidence intervals at the 90 and 95 percent in the vertical axis and the characteristics of the regression model on the horizontal axis. Our preferred estimation, which are the same coefficients shown in **Table 2**, are highlighted in the specification chart.

A first concern is the use of a linear relationship between the running variable (distance to the border) and the dependent variable (permit rate) on both sides of the threshold. We relaxed this assumption by estimating equation (2), where $f()$ and $g()$ are n-degree polynomial functions up to the fourth degree.

$$y_{it} = \gamma_0 + \gamma_1 f(dist_i) + treat_i(\gamma_2 + \gamma_3 g(dist_i)) + post_t[\beta_0 + \beta_1 f(dist_i) + treat_i(\beta_2 + \beta_3 g(dist_i))] + e_{it} \quad (2)$$

The range of the estimates increases for higher-order polynomials. For instance, Panel A shows that the zoning permit rate coefficients range from -0.497 to -0.255 using a second-degree polynomial, while they lie between -0.601 and -0.243 for a fourth-degree polynomial function. The confidence intervals are also larger for higher-order polynomial degrees, and they are usually not statistically significant. Still, the estimates are all negative irrespective of the n-degree polynomial function used in the estimation. The median coefficient on the zoning permit rate is similar across different polynomial functions: between -0.388 and -0.354, which are comparable to our preferred estimates.

The difference-in-discontinuities estimates on construction permit rates are measured imprecisely under higher-order polynomial degrees (Panel B). Similarly, due to the variance-bias trade-off, small bandwidths remove the selection bias but increase the estimates' imprecision. The construction estimates over the 500 meters bandwidth are between -0.975 and -0.344 (median = -0.573), but under the 500 meters bandwidth, they increased to -1.36. The fourth-degree polynomial drives these large estimates; all other functional forms lead to smaller estimates, more in line with our main estimate.

A second concern is that observable differences could be causing our results rather than the differential propensity towards urban development at contiguous council districts and the new zoning law. If this hypothesis were true, including sociodemographic variables in the regression would make our main coefficient statistically insignificant and substantially change its magnitude. To address this concern, we include a vector of sociodemographic variables that reflect the race, age, employment, and schooling attainment composition of the population at the year-census tract level (the smaller unit of analysis with available information for such data). The demographic control variables are one level above the unit of analysis; there are around 30 hexagons per census tract level. The median coefficient for zoning is -0.386 and -0.353 with and without sociodemographic variables, respectively. The median construction estimate increases to -0.617 by including covariates compared to -0.560, including and excluding covariates. The joint permit rate is reasonably similar under both specifications (median estimate of -0.440 and -0.492). Consequently, the estimates do not change by adding control variables to the model. To the extent that observations further away from the border include some bias, adding covariates will remove such bias and improve the precision of the coefficient (Imbens and Lemieux, 2008). Controlling for observables previously captured by the error term in our main specification increases the coefficients' precision. If anything, including covariates in our main specification, supports our main conclusion.

A fourth concern is that few major urban development projects needing a constant, large influx of planning permits are driving the effects (i.e., building a new shopping mall or a skyscraper). If this situation were true, only selected places would be facing differential changes in urban development rather than a widespread economic development impact across contiguous council districts. To test this alternative explanation, we remove the hexagons concentrating the top two percent of filled permits, which represent between 9 to 15 percent of the total zoning and construction permits during our study period. Reassuringly, the estimates are fairly under this alternative sample selection: the median coefficient on zoning permits is -0.396 rather than -0.363; in contrast, the construction permit estimate slightly decreases to -0.480 from -0.573 under the restricted sample.

While the new zoning regulation was approved in December 2011, the legislation took effect in August 2012. We compute the difference-in-discontinuities regression using the post-indicator variable of the second quarter of 2012 (2012Q2) rather than fourth quarter of 2011 (2011Q4). The evidence is consistent with our preferred specification that considers the possibility of anticipatory effects on planning permits.

Likewise, the specification curve also shows the confidence intervals clustering the standard errors at different levels: census block group, census tract, and hexagon level. The standard error clustering level leads to similar confidence intervals across all point estimates and does not change our main conclusions. To summarize, the specification curve visualizes that the range of the difference-in-discontinuities increases for higher-order polynomials, and some estimations lose statistical significance. We take these findings as having imprecise estimates. Still, the magnitude remains negative irrespective of the functional form, and our main conclusion holds: Philadelphia's new zoning law reduced the land use zoning changes in council districts less inclined toward urban development than their contiguous council districts.

A fifth concern is the local parallel trends assumption. It is equivalent to the parallel trends assumption of the difference-in-difference method but restricting the observations around a small bandwidth (Grembi et al., 2016). To test this assumption, we estimate an event study design under both optimal bandwidths.[11] **Appendix Figure A.1** shows that before the legislation's approval, the point estimates are around zero, without evidence of permit rates rising or decreasing before the approval of the new zoning regulation. An *F-test* on the joint significance of the pre-policy coefficients confirms this finding. After the new code, the estimates slightly become larger over time, consistent with the notion that local urban development changes take time to reflect their impact.

Another concern about our identification strategy is the selection of the treatment and control groups. Although we followed an evidence-based decision process to identify such groups (the presence of construction and real-estate donors in the council members' political contributions), it could be that there are differential effects across any contiguous council districts before and after the new legislation, irrespective of their propensity towards urban development. To address such concern, we conducted a geographical falsification test by randomly assigning a treatment indicator variable to four council districts among all possible *10 chooses 4* combinations, removing cases where at least two of the actually treated council districts were selected (jurisdictions four, seven, eight, and nine).[12] To be clear, **Appendix Figure A.2** exhibits four out of the 95 treatment-control placebo pairs that resulted from this combination process. We estimated the difference-in-discontinuities for all the placebo pairs using first, second, and third-degree polynomials at the two optimal bandwidths and clustering the standard errors at the census block group, census tract, and hexagon level.

Appendix Figure A.3 exhibits the histogram of placebo estimates ($\hat{\beta}_2$) and z-scores ($\hat{\beta}_2/SE_{\hat{\beta}_2}$) for each outcome variable. We note that the density function is not symmetric around zero. This result can be explained somewhat by chance, and most likely, because some placebo pairs partially mirror our actual treatment-control groups. Placebo iterations with at least one of the actual treated council districts could skew the density function. To the extent that at least two of the actual treated council districts are part of the control group in the placebo pairs, it could also partially skew the distribution. For transparency,

[11] The results are qualitatively similar estimating a regression discontinuity for each quarter-year at the optimal bandwidths.

[12] Our research strategy identifies four council districts with a low presence of property development donors on local political contributions. We select the same number of council districts for the falsification test to keep the research design consistent.

we do not remove such cases, but doing so centers the distribution around zero.

The median placebo estimates (z-scores) are 0.168 (0.503), 0.121 (0.707), and 0.129 (0.627) for zoning, construction, and its join permit rate; overall, they are close to zero and statistical insignificant. These results contrast our main results shown in the vertical lines, as our preferred coefficients and z-scores are among the lower left end of the distribution for the three dependent variables. Computing the absolute value of all the placebo coefficients, our preferred difference-in-discontinuities are between the top 0.03 and 0.14 percentile (the coefficients for the combined zoning and construction permit rate are in the top 0.04 percentile). Likewise, the z-scores (in absolute terms) of our main estimates are in the upper <0.001 and 0.05 percentile of the placebo distribution. In short, it is improbable that our results are due to random chance alone, supporting our causal argument of the effect of the zoning regulation on urban development.

The seventh concern on our identification strategy is the existence of discontinuities at other values besides the border. As recommended in the regression discontinuity literature (Imbens and Lemieux, 2008), we test for jumps at non-discontinuity values. We shift the threshold of the running variable from a negative to a positive 1,000 meters relative to the border in 250 meters increments, and then we estimate the regression at the optimal bandwidth at this new cutoff value. We note that for thresholds beyond 500 meters away from the border, we are solely using observations on the left or right of the cutoff value, avoiding estimating the regression at a value where we know there is a discontinuity.

Appendix Table A.1 exhibits the difference-in-discontinuities estimator on the zoning and construction permit rate under different polynomial functions for each placebo test. Only two coefficients out of the 32 are significant, which is not surprising considering a five percent false discovery rate. More importantly, the coefficients are highly sensitive to the specification form, both in magnitude and sign. In short, we would need a very selective choice of placebo estimators (i.e., a quartic polynomial function and between 500 to 750 meters away from the border) to find significant effects at other thresholds where we do not expect discontinuities. Consequently, our main effects do appear to be driven by changes at the border.

5.3.2 Sensitivity analysis on crime outcomes

As one may selectively report results to avoid the *file drawer problem* (Rosenthal, 1979), we are presenting the full set of estimates to support our main finding: there is no evidence of crime changes, at least in the short-term, after approving the new zoning law between adjacent council districts with different urban development propensities in Philadelphia.

Our main specification exhibit a negative effect on robbery. However, **Appendix Figure A.4**, Panel I in the specification chart exhibits that this result no longer holds after estimating the effect under different bandwidths and functional forms (higher-order polynomial degrees, adding covariates, and sample selection). Overall, the sign change of the difference-in-discontinuities coefficient is evident for the seven UCR part I crimes (burglary, theft, grand theft auto, assault, robbery, murder, and rape).

One may argue that as motor vehicle theft (Panel H) presents a sign change for few estimates (4.1 percent of them) provides suggestive evidence of a crime increase. We find this argument unlikely as only 14.6 (0.6) percent of the coefficients are significant at the 90 (95) percent level. Similarly, given that the probability of finding a statistically significant result relates positively to using a higher-order polynomial functional form, it raises doubts about such an argument's validity. In summary, this section exhibits that the null effects on crime are consistent across multiple model specifications, sample, and bandwidth selection.

5.4 Heterogeneity

Philadelphia exhibited significant economic development changes after approving the new zoning ordinance between adjacent council districts. The reduction in zoning and construction permits had no short-term impact on crime. Changes in land use regulations could have differential effects based on the characteristics of the neighborhood, masking out the aggregate results.

We use pre-treatment information on residential and commercial land use, population density, and a foot traffic proxy (walkability index) to test for heterogeneous effects. We measure the residential and commercial land use extension at the hexagonal level using Philadelphia's Zoning Base Districts valid before August 2012 (pre-policy change period). The population estimates come from the 2010 American Community Survey, dividing the population uniformly among the different hexagonal units in a census tract. Finally, we build a walkability index to proxy the pedestrian activity by estimating a weighted sum of the standardized number of bus stops, commercial land use, population density, restaurants, parks, schools, and supermarkets/grocery stores at the hexagon and neighboring hexagons (to avoid any sharp discontinuities due to measurement errors). Then, we normalize the values between zero and one-hundred.[13] Previous studies have used walkability indexes (Anderson et al., 2013; Chang and Jacobson, 2017; Twinam, 2017), and they positively correlate to objective and subjective measures of the physical neighborhood activity (Carr et al., 2010).

The heterogeneous analysis implements the difference-in-discontinuity estimator interacting the treatment indicator over the relevant heterogeneity dimension. The results suggest that more densely populated

[13] We do not use data from www.walkscore.com, as it provides post-treatment scores. We estimate our walkability index using similar relative weights that Johnson (2019) computed by reverse-engineering the Walk Score algorithm using machine learning methods.

areas are the places across contiguous council districts where development occurs (**Appendix Table A.2**). We note that the estimates are negative but not significant for the construction permit rate. There is no evidence of differential changes based on the prevalent commercial or residential land use in the neighborhood.

Appendix Table A.3 exhibits the heterogeneous effects on criminal offenses. The interaction with commercial land use leads to negative coefficients for property crimes. In contrast, areas with extensive residential uses exhibit a positive effect. However, none of the estimators reach statistical significance. Therefore, we observe little evidence that the prevalence of commercial and residential areas and population density attenuate the null effects of economic development on public safety.

Figure 9 visualizes the difference-in-discontinuities on crime by walkability index percentile, where we interact the difference-in-discontinuity estimator with the percentile of the walkability index to measure non-linear effects. Panels B and E exhibit that the null effects on property crime remain for areas above the median (third and fourth quartile), but there is suggestive evidence of a property crime increase for the least walkable places (first and second quartile). These results are consistent with Jacobs (1961) ideas that fewer *eyes upon the street* lead to more criminal behaviors, along with Twinam (2017) showing differential changes based on foot traffic proxies. Panels C and F reveal that there are no effects on violent crimes.

5.5 Potential mechanisms

The previous section assessed whether there are differential effects on planning permits and crimes based on the characteristics of the neighborhood (densely populated, zoning district, and friendliness to pedestrians). The council members' preferences towards urban development may not depend on the neighborhood attributes, as it is more about the property requesting zoning changes. To evaluate this hypothesis, we use the zoning permits open-ended description to classify them based on whether a property most likely use is for residential or commercial purposes.[14]

We recognize that inferring the property use of every parcel in Philadelphia is a challenging task that the base zoning district map or other data sources are unable to solve. For instance, we can expect that a property has a residential use if it is located in a residential district. Still, it is unclear the most likely use of a property situated in a mixed land use neighborhood where commercial and residential parcels are prevalent. Likewise, as there were more than 37,000 zoning permits in Philadelphia between 2010 and 2015 and around 13,000 in the neighboring council districts used in our econometric specifications, relying on a manual classification is beyond the scope of this research. Consequently, we relied on a *k-means* clustering

[14] We highlight that the analysis of this subsection only includes zoning permits, not the construction authorizations.

method to classify the zoning permits.

The *k-means* method is an unsupervised machine learning algorithm that groups similar observations into k clusters without the need for a ground truth label.[15] **Appendix B** provides a detailed explanation of the classification process, but in summary, we first perform standard text processing methods in the data mining literature (Zhao and Cen, 2013) to the open-ended descriptions of all zoning permits in the city between 2010 and 2015. Then, we tokenize the text and build a document-term-matrix. Next, we apply the *k-means* algorithm selecting an optimal k based on the Caliński and Harabasz (1974) index. Finally, we aggregate the clusters into three broad categories: residential, commercial, and other.[16] Within a 500-meter distance from the closest council district border during the pre-policy period, there are 1.9 zoning permits per 1,000 parcels in any given quarter. From this number, 0.6 and 1.0 permits are most likely for a commercial and residential property, respectively. The remaining 0.3 permits per 1,000 parcels refers to the other category.

Table 4 exhibits the difference-in-discontinuities estimates following equation (1) for the property and residential zoning permits based on the previous classification method. Column (1) replicates our main results from **Table 2** showing a decrease in the zoning permits after the new policy regulation. Columns (2) exhibits that the new legislation relates to a 41 to 45 percent decrease in the zoning permits filled by residential properties. Commercial properties do not experience a reduction in their zoning permit rate. These results hold to alternative bandwidths and functional forms as shown in **Appendix Figure B.4**. These results suggest that council members' differential propensity towards urban development is only over residential projects, not over commercial ones.

6 Discussion and conclusion

In December 2011, Philadelphia approved a new zoning ordinance, replacing a fifty-year-old regulation that had become outdated, complex, and restrictive to urban development. The new zoning legislation aimed to simplify the process for construction and property development. Simultaneously, the new law permitted councilmanic prerogative, allowing council members with fewer ties to real estate and construction donors to maintain control over approving building and property developments in their districts. We exploit the geographic discontinuity at the council district borders and the zoning ordinance change to estimate the effect of a new zoning regulation on economic development and crime. The difference-in-discontinuities

[15] The classification method does not need a priori label for the clusters and does not provide one after the clustering. Instead, the algorithm aids the researcher in identifying a common topic among the observations in a cluster.

[16] The *other* category includes zoning permits which we were not able to classify as residential or property as they mainly refer to changes in the property lot lines, demolitions, and vacant lots, among other authorizations.

estimator shows a 35 percent reduction in zoning and construction permits in council districts less inclined to urban development relative to pre-enactment years and neighboring districts. The heterogeneous treatment effects exhibit that the impact may have been more prominent in high-dense populated areas. The reductions in urban development near council district borders had no short-term impact on crime. The estimates do not change by the prevalence of residential or commercial parcels, but a foot traffic proxy attenuates them. Finally, our results are robust to alternative functional forms, sample size, bandwidth selection, geographic falsification tests where council districts were randomized to being less urban development-oriented, and we find no discontinuities at other values besides the border.

Our findings suggest that developers and construction companies respond to policy changes that simplify and make more transparent property development projects. In contrast to previous observational studies finding that land use changes correlate to more criminal behaviors (Browning et al., 2010; Greenberg et al., 1982; Stucky and Ottensmann, 2009; Taylor et al., 1995), our research suggests that economic development measured by land use zoning changes and construction permits do not have short-term impacts on crime.

Relying on an unsupervised machine learning method to classify the most likely use of a property filling for zoning permits, we found that the reduction in urban development projects came from residential rather than commercial properties. Although we lack detailed property values, Zillow's data points that Philadelphia's new residential projects are more likely to be high-end than low-end market constructions (Rao, 2016). To the extent that the bulk of affected properties were upmarket residential buildings, the reduction in urban development is consistent with null effects on crime. Had we observed decreases in affordable housing developments or impacts on zoning permits for commercial properties, then we should have expected negative effects on crime (Chang and Jacobson, 2017; Diamond and McQuade, 2019; Freedman and Owens, 2011; Tillyer and Walter, 2019). Overall, our results suggest that changes in the residential stock exhibit little influence on crime, at least in the short-term.

Finally, our study is not without limitations. Urban development spurred by zoning regulation changes may lead to changes in crime over the longer-term if there is sufficient development to bring gentrification or other forms of population change (Autor et al., 2019). However, our study suggests that in the short-term in Philadelphia, local officials can encourage zoning changes without jeopardizing public safety in neighborhoods. Future research is needed to understand the full impacts of local economic development on crime over a longer-term period.

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Table 1: Descriptive statistics

	Treatment		Control	
	Mean	Std. Dev	Mean	Std. Dev
Crimes	1.44	1.80	1.34	1.70
Violent	0.49	0.93	0.44	0.84
Murder	0.01	0.09	0.01	0.10
Robbery	0.19	0.53	0.16	0.48
Assault	0.26	0.64	0.24	0.60
Property	0.95	1.36	0.90	1.32
Burglary	0.25	0.57	0.27	0.60
Residential	0.22	0.52	0.23	0.56
Nonresidential	0.03	0.20	0.04	0.21
Theft	0.52	1.09	0.50	1.06
Motor vehicle theft	0.18	0.45	0.14	0.38
Parcels	59.98	28.64	58.82	28.61
Permits per 100 parcels	0.23	2.25	0.23	1.75
Construction	0.03	0.70	0.05	0.82
Zoning	0.19	2.01	0.19	1.34
Commercial area (%)	0.12	0.21	0.13	0.21
Residential area (%)	0.78	0.29	0.79	0.28
Population	100.73	41.73	94.34	36.63
Walkability index	57.26	11.45	55.69	11.20
% white	0.27	0.21	0.29	0.29
% black	0.44	0.30	0.54	0.37
% hispanic	0.35	0.27	0.15	0.17
% age 0-14	0.24	0.06	0.21	0.05
% age 15-24	0.17	0.05	0.17	0.07
% age 25-39	0.20	0.04	0.20	0.05
% age 40-54	0.19	0.03	0.20	0.04
% age 55+	0.19	0.07	0.22	0.07
% less than high school	0.32	0.12	0.27	0.10
% high school	0.39	0.06	0.41	0.07
% some college	0.16	0.06	0.15	0.05
% college+	0.14	0.08	0.17	0.09

Notes: Mean and standard deviation weighted by parcels from 2010Q1 to 2011Q3 (pre-policy change period) and within 500 meters to the closest council district border. Crime and urban development-related variables come from hexagon-quarterly data. We assume the census tract population is uniformly distributed across the hexagonal area. Sociodemographic and employment-related outcomes come from census tract-yearly data. Regular hexagons have a side length of 70 meters and an area of around 12,731 squared meters.

Table 2: Difference-in-discontinuities estimates of the new zoning code on permits

	zoning	construction	zon. & const.
	(1)	(2)	(3)
A. Imbens and Kalyanaraman (2012) bandwidth			
Treatment*Post	-0.344^{**}	-0.617^{**}	-0.389^{**}
	(0.166)	(0.300)	(0.160)
Bandwidth (m.)	701.0	598.0	657.0
Observations	119,616	110,352	115,416
Mean dep. var.	0.189	0.045	0.233
B. Calonico et al (2014) bandwidth			
Treatment*Post	-0.381^{**}	-0.486^{*}	-0.427^{***}
	(0.157)	(0.255)	(0.143)
Bandwidth (m.)	961.0	991.0	936.0
Observations	138,120	140,112	136,632
Mean dep. var.	0.186	0.045	0.231

Notes: Difference-in-discontinuities estimates of the impact of the differential propensity to approve construction and zoning permits relative to its neighboring council districts before and after the new zoning code. The model follows equation (1), using a Poisson regression weighted by the number of parcels. The optimal bandwidths, in meters, follow Imbens and Kalyanaraman (2012) and Calonico et al. (2015) methods. Robust-standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 3: Difference-in-discontinuities estimates of the new zoning code on crimes

	crime	property	violent	theft	burglary	burglary resd.	burglary comm.	motor vehicle theft	assault	robbery
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Imbens and Kalyanaraman (2012) bandwidth										
Treatment*Post	−0.0002	0.022	−0.029	0.028	−0.015	−0.019	0.236	0.020	0.045	−0.125
	(0.041)	(0.050)	(0.065)	(0.069)	(0.085)	(0.089)	(0.261)	(0.097)	(0.077)	(0.084)
Bandwidth (m.)	422.0	389.0	426.0	396.0	568.0	494.0	405.0	487.0	614.0	400.0
Observations	90,936	87,264	91,536	88,008	107,280	99,960	89,064	99,120	111,600	88,416
Mean dep. var.	1.275	0.821	0.453	0.487	0.247	0.215	0.028	0.091	0.246	0.173
B. Calonico et al (2014) bandwidth										
Treatment*Post	−0.023	−0.021	−0.022	−0.003	−0.008	−0.023	0.188	0.004	0.040	−0.118*
	(0.036)	(0.047)	(0.050)	(0.063)	(0.070)	(0.079)	(0.205)	(0.074)	(0.065)	(0.065)
Bandwidth (m.)	723.0	722.0	744.0	706.0	982.0	889.0	739.0	898.0	1,058.0	647.0
Observations	121,128	120,936	122,952	119,904	139,536	133,464	122,424	134,064	144,288	114,600
Mean dep. var.	1.287	0.825	0.461	0.491	0.242	0.216	0.028	0.088	0.241	0.174

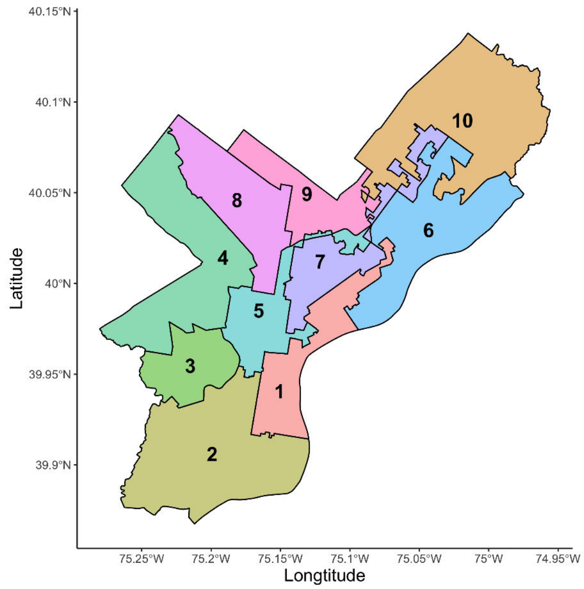
Notes: Difference-in-discontinuities estimates of the impact of the differential propensity to approve construction and zoning permits relative to its neighboring council districts before and after the new zoning code. The model follows equation (1), using a Poisson regression weighted by the number of parcels. The optimal bandwidths, in meters, follow Imbens and Kalyanaraman (2012) and Calonico et al. (2015) methods. Robust-standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 4: Difference-in-discontinuities estimates of the new zoning code on zoning permits

	zoning	residential	commercial
	(1)	(2)	(3)
A. Imbens and Kalyanaraman (2012) bandwidth			
Treatment*Post	-0.344^{**}	-0.608^{***}	0.068
	(0.166)	(0.221)	(0.204)
Bandwidth (m.)	701	615	761
Observations	119,616	111,648	124,056
Mean dep. var.	0.189	0.109	0.045
B. Calonico et al (2014) bandwidth			
Treatment*Post	-0.381^{**}	-0.543^{***}	-0.060
	(0.157)	(0.191)	(0.181)
Bandwidth (m.)	961	943	979
Observations	138,120	136,968	139,248
Mean dep. var.	0.186	0.108	0.045

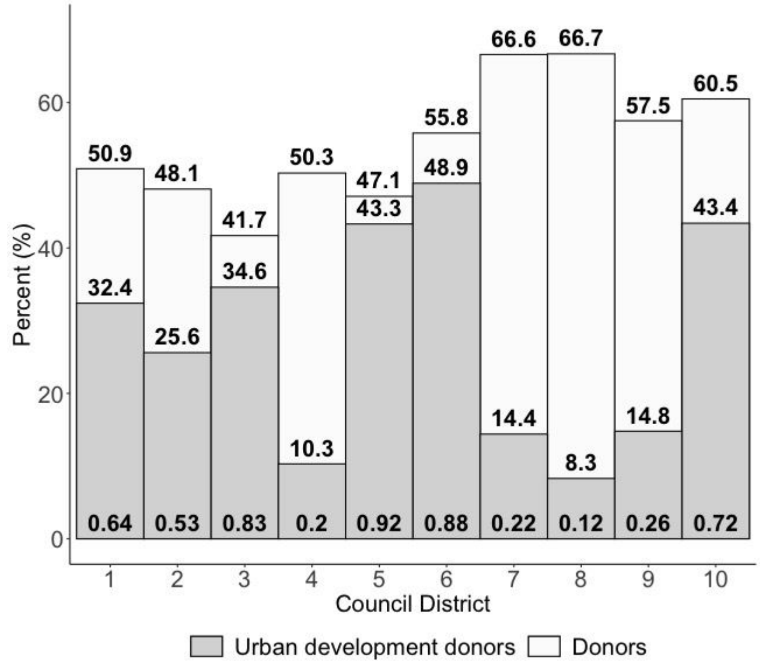
Notes: Difference-in-discontinuities estimates of the impact of the differential propensity to approve construction and zoning permits relative to its neighboring council districts before and after the new zoning regulation. The model follows equation (1), using a Poisson regression weighted by the number of parcels. The optimal bandwidths, in meters, follow Imbens and Kalyanaraman (2012) and Calonico et al. (2015) methods. Robust-standard errors clustered at the census tract level are in parentheses. Column (1) replicates the results from Table 2. The outcome variable of columns (2) and (3) refers to the most likely use of the property requesting the permit based on the unsupervised machine learning classification algorithm conducted over the zoning permits. *p<0.1; **p<0.05; ***p<0.01.

Figure 1: Philadelphia's Council Districts



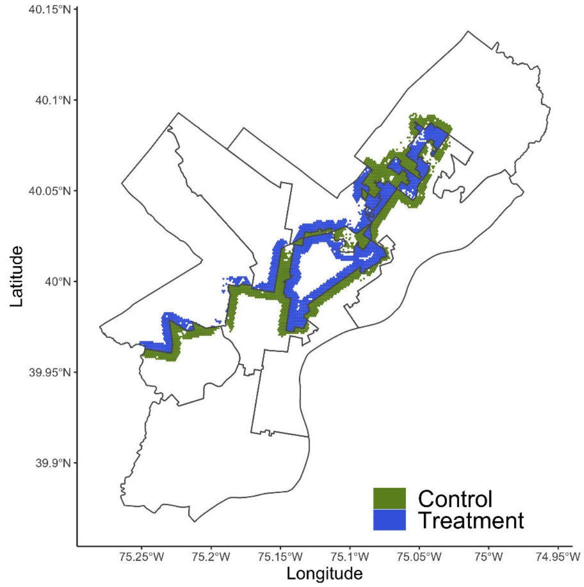
Notes: The map shows the boundaries of the ten council districts in Philadelphia using the 2000 Census redistricting.

Figure 2: Political contributions of the top 10 percent of the donors



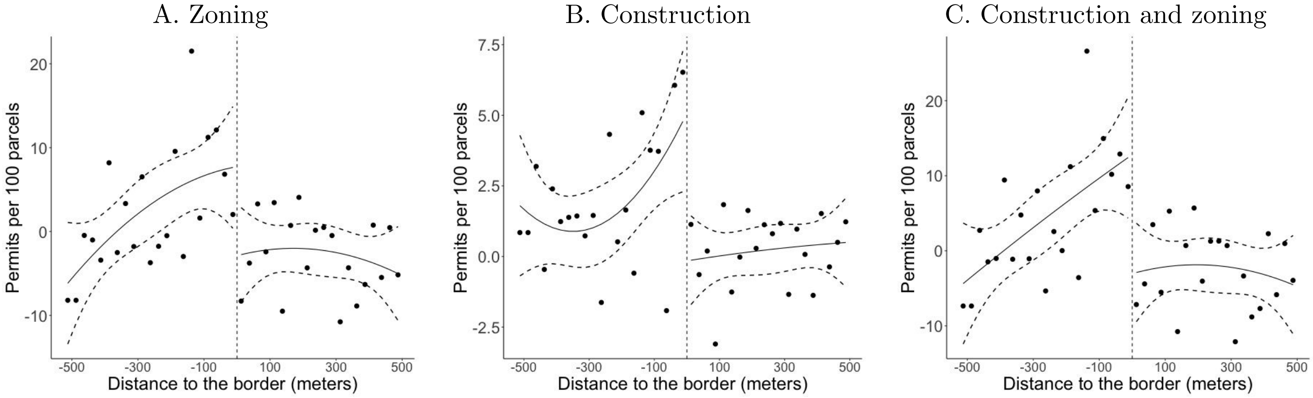
Notes: The top numbers refer to the political contributions made to the council members by the top ten percent of the donors between 2010 and 2015. The intermediate values show the relative contribution of the top ten percent of the donors working in the construction or real-estate industries. The bottom numbers are the ratio between both previous values. Source: The Philadelphia Campaign Finance Site provided the amount, names, and addresses of donors. The construction and real-estate donors' identification was made manually by searching online each donor's names and addresses and linking them to their employer or business.

Figure 3: Location of treatment and control group



Notes: The map visualizes hexagons within 500 meters from the closest council district border. Hexagons without parcels include mainly public parks and the Schuylkill River.

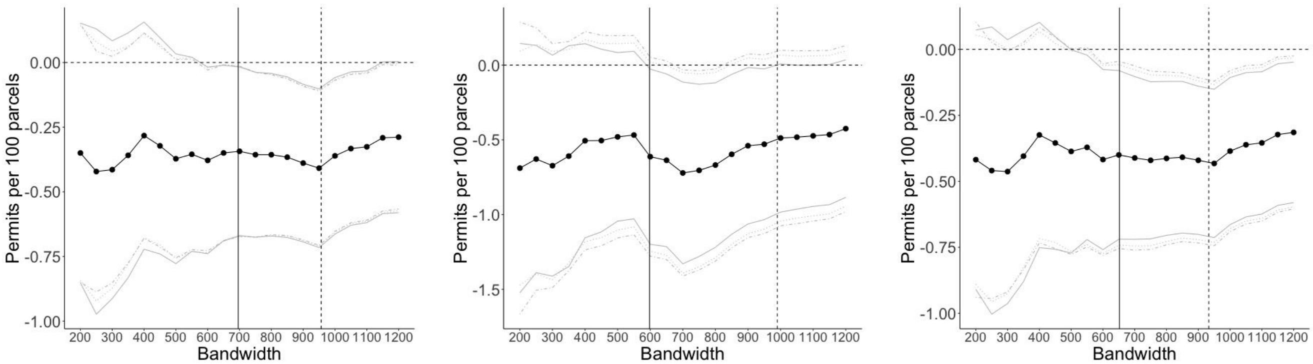
Figure 4: Difference-in-discontinuities on the permit rate



Notes: Each panel shows the difference of each post-treatment (i.e., 2011Q4, 2012Q1, ..., 2015Q4) and pre-treatment (i.e., 2010Q1,2010Q2, .. 2011Q3) outcome following Grembi et al. (2016), using bins of 25 meters, second-order polynomials (solid line), and 95 percent confidence intervals (dash lines).

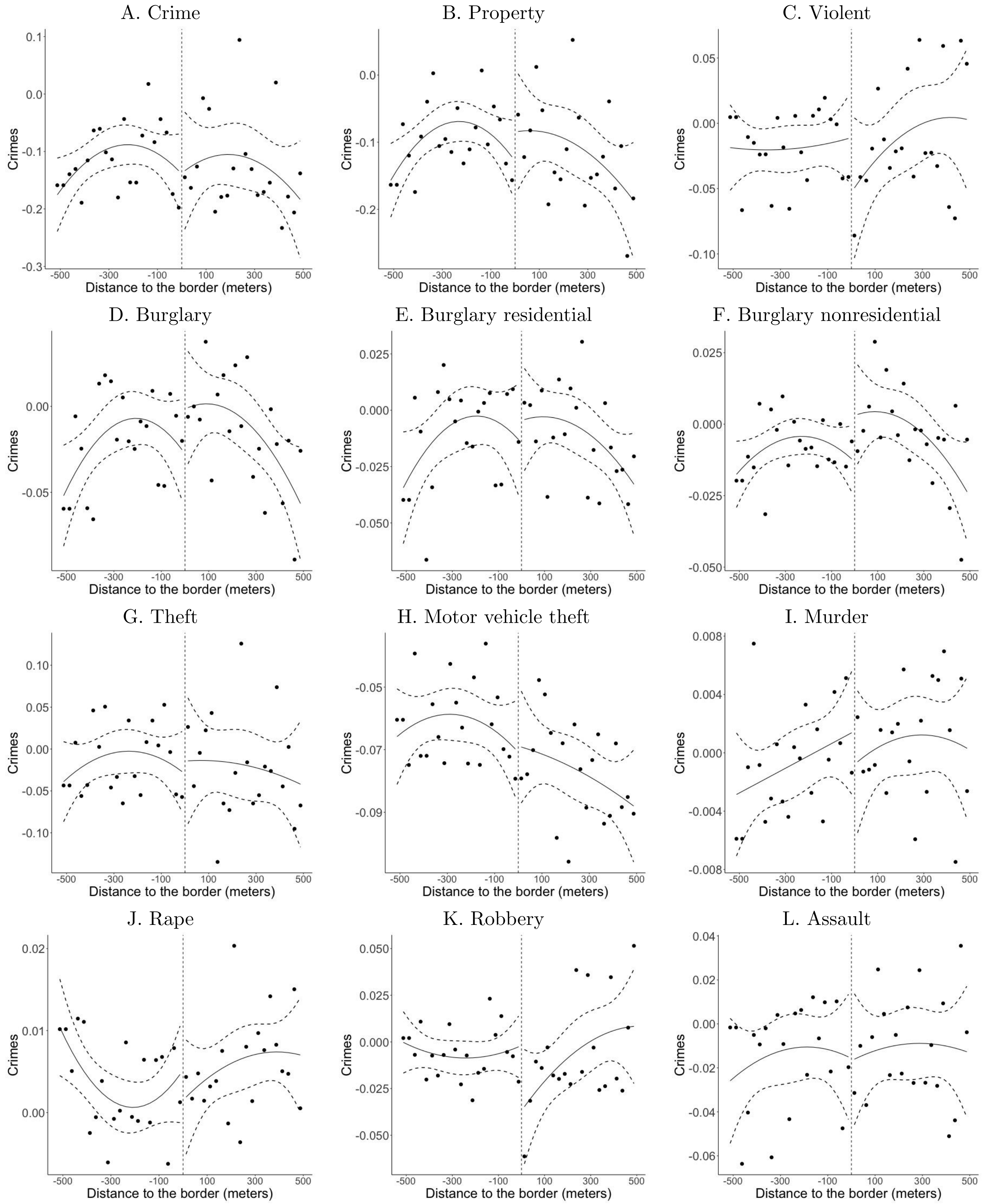
Figure 5: Difference-in-discontinuities estimator on the permit rate by bandwidth

D. Zoning E. Construction F. Construction and zoning



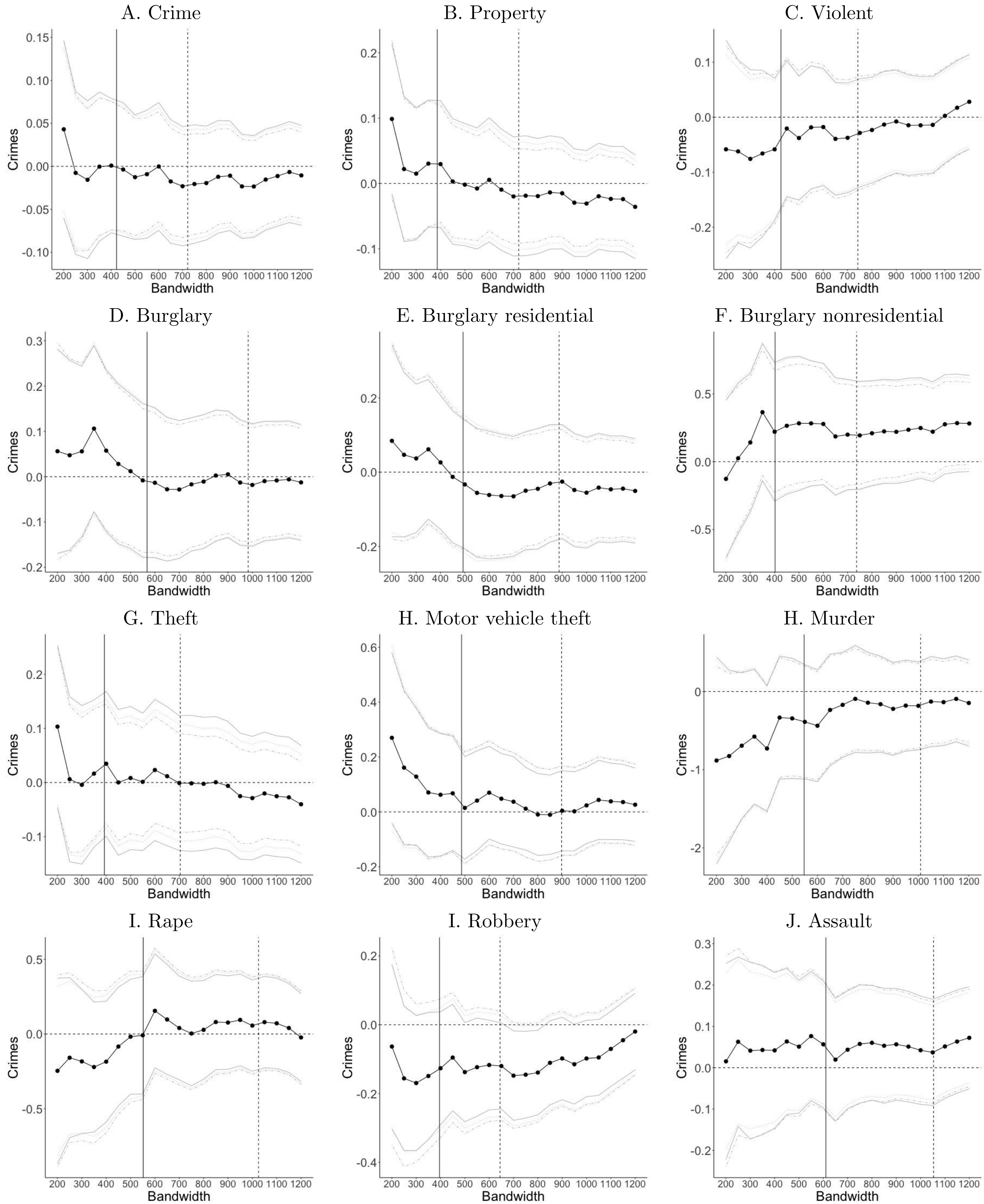
Notes: Each panel visualizes the difference-in-discontinuity estimate using a Poisson regression for different bandwidths and standard error specifications: robust-clustered at the census block group (dotted line), tract (solid line), and hexagon level (dot-dash line). All estimates are weighted by the number of parcels per hexagon. The vertical solid line shows Imbens and Kalyanaraman (2012) optimal bandwidth and the vertical dashed line exhibits Calonico et al. (2015) bias-corrected bandwidth.

Figure 6: Difference-in-discontinuities on crime



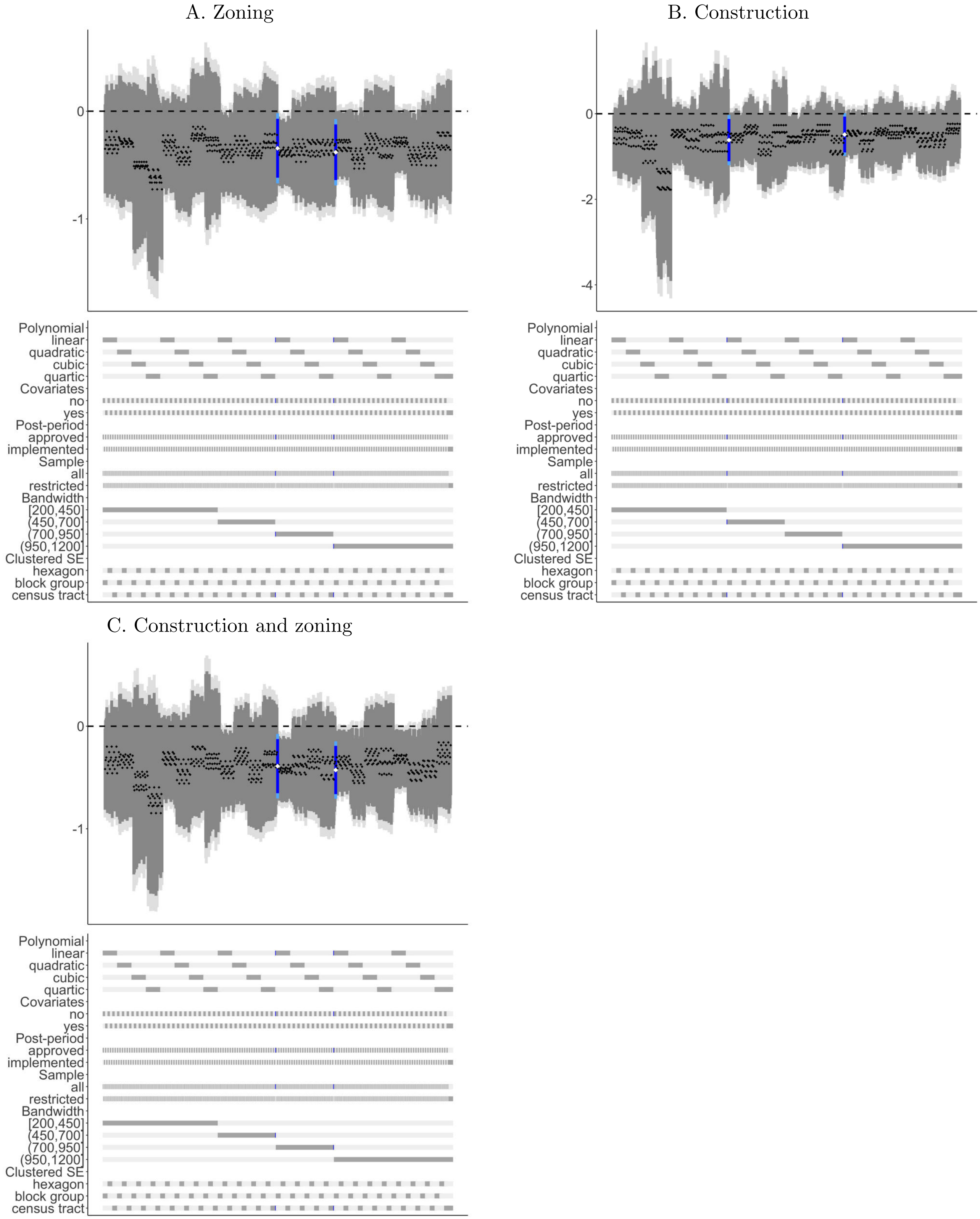
Notes: Each panel shows the difference of each post-treatment (i.e., 2011Q4, 2012Q1, ..., 2015Q4) and pre-treatment (i.e., 2010Q1, 2010Q2, .. 2011Q3) outcome following Grembi et al. (2016), using bins of 25 meters, second-order polynomials (solid line), and 95 percent confidence intervals (dashed lines).

Figure 7: Difference-in-discontinuities estimator on crime by bandwidth



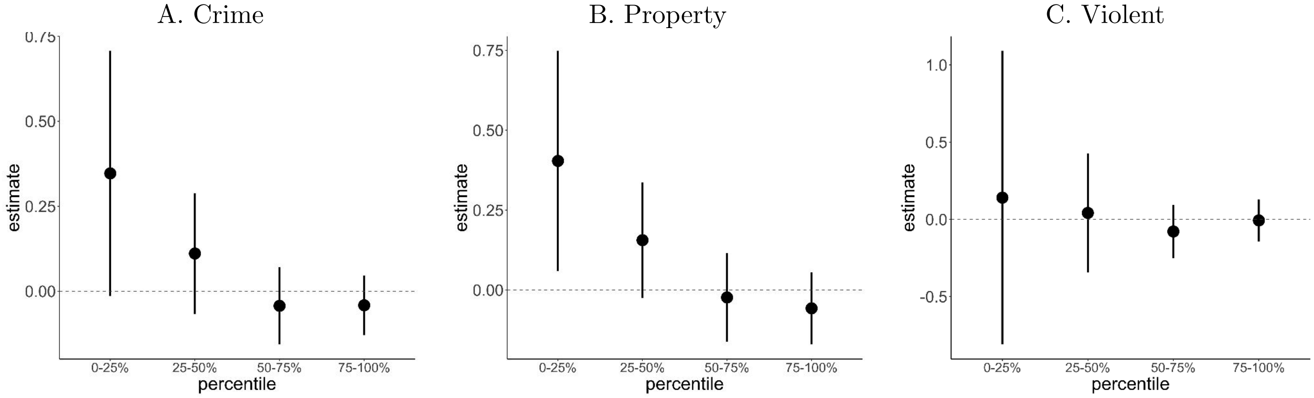
Notes: Each panel visualizes the difference-in-discontinuity estimate using a Poisson regression for different bandwidths and standard error specifications: robust-clustered at the census block group (dotted line), tract (solid line), and hexagon level (dot-dash line). All estimates are weighted by the number of parcels per hexagon. The vertical solid line shows Imbens and Kalyanaraman (2012) optimal bandwidth and the vertical dashed line exhibits Calonico et al. (2015) bias-corrected bandwidth.

Figure 8: Difference-in-discontinuities estimator on the permit rate, Alternative specifications



Notes: Each panel visualizes the difference-in-discontinuity estimate using a Poisson regression for different bandwidths (from 200 to 1,200 meters), standard errors clustering (at the census block group, census tract, and hexagon level), specification function, and sample selection. All estimates are weighted by the number of parcels per hexagon. The two main specifications are estimated using Imbens and Kalyanaraman (2012) optimal bandwidth and Calonico et al. (2015) bias-corrected bandwidth.

Figure 9: Heterogeneous effects on crime by walkability index



Notes: Difference-in-discontinuities estimates of the impact of the differential propensity to approve construction and zoning permits relative to its neighboring council districts before and after the new zoning code, interacting the parameters with pre-treatment percentile walkability index indicator variables. The model uses a Poisson regression weighted by the number of parcels. The vertical lines mark the 95 percent confidence intervals clustering the standard errors at the census tract level. The specification uses Calonico et al. (2015) bias-corrected bandwidth but the results are equivalent using Imbens and Kalyanaraman (2012) optimal bandwidth.

ONLINE APPENDIX

A Appendix: Tables and Figures

Table A.1: Placebo test. Difference-in-discontinuities estimates on zoning and construction permits at different cut-off values

	linear	quadratic	cubic	quartic
	(1)	(2)	(3)	(4)
Border -1,000 m.	−0.288	−0.220	0.311	0.620
	(0.410)	(0.607)	(0.728)	(0.658)
Bandwidth (m.)	464	464	464	464
Border -750 m.	0.100	−0.341	−0.636	−1.148*
	(0.246)	(0.421)	(0.405)	(0.591)
Bandwidth (m.)	316	316	316	316
Border -500 m.	0.092	−0.356	−0.713	−1.099
	(0.317)	(0.461)	(0.636)	(0.698)
Bandwidth (m.)	316	316	316	316
Border -250 m.	−0.158	−0.021	0.057	−0.073
	(0.183)	(0.202)	(0.295)	(0.429)
Bandwidth (m.)	613	613	613	613
Border +250 m.	0.225	0.081	0.086	0.275
	(0.225)	(0.350)	(0.436)	(0.494)
Bandwidth (m.)	264	264	264	264
Border +500 m.	−0.011	−0.344	−0.849	−0.964*
	(0.255)	(0.441)	(0.616)	(0.544)
Bandwidth (m.)	305	305	305	305
Border +750 m.	0.035	0.015	0.129	−0.146
	(0.259)	(0.399)	(0.441)	(0.460)
Bandwidth (m.)	425	425	425	425
Border +1,000 m.	−0.369	−0.028	−0.306	−0.675
	(0.245)	(0.347)	(0.319)	(0.505)
Bandwidth (m.)	403	403	403	403

Notes: Difference-in-discontinuities estimates of the impact of the differential propensity to approve construction and zoning permits relative to its neighboring council districts before and after the new zoning code. The placebo test shifts the threshold of the running variable (distance to the border), ranging from a negative to a positive one kilometer in 250 meters increments, and estimates the optimal bandwidth following Imbens and Kalyanaraman (2012) method at the placebo threshold. Each column uses a different polynomial function in the specification form of the difference-in-discontinuities estimator. The model uses a Poisson regression weighted by the number of parcels. Robust-standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A.2: Heterogeneity effects on economic development

	zoning (1)	construction (2)	zon. & const. (3)
A. Imbens and Kalyanaraman (2012) bandwidth			
Treat*Post	-0.472^{**}	-0.656	-0.489^{**}
	(0.239)	(0.480)	(0.249)
Treat*Post*Comm	0.484	0.350	0.399
	(0.475)	(1.161)	(0.499)
Treat*Post	-0.045	-0.686	-0.209
	(0.288)	(0.513)	(0.278)
Treat*Post*Resd	-0.578	0.158	-0.370
	(0.505)	(1.034)	(0.534)
Treat*Post	0.586	-0.475	0.207
	(0.475)	(0.693)	(0.377)
Treat*Post*Pop	-0.010^{*}	-0.002	-0.007
	(0.006)	(0.008)	(0.004)
Treat*Post	-0.519	-1.403	-0.700
	(0.753)	(1.517)	(0.803)
Treat*Post*WlkInd	0.003	0.014	0.005
	(0.012)	(0.024)	(0.013)
Bandwidth (m.)	701.0	598.0	657.0
B. Calonico et al (2014) bandwidth			
Treat*Post	-0.446^{**}	-0.580	-0.517^{**}
	(0.213)	(0.388)	(0.205)
Treat*Post*Comm	0.291	0.554	0.402
	(0.420)	(1.009)	(0.443)
Treat*Post	-0.116	-0.559	-0.212
	(0.261)	(0.487)	(0.262)
Treat*Post*Resd	-0.511	0.140	-0.416
	(0.428)	(0.896)	(0.453)
Treat*Post	0.352	-0.547	0.137
	(0.418)	(0.716)	(0.319)
Treat*Post*Pop	-0.008^{*}	0.001	-0.006^{*}
	(0.005)	(0.009)	(0.003)
Treat*Post	-0.395	-1.962	-0.637
	(0.702)	(1.344)	(0.703)
Treat*Post*WlkInd	0.0003	0.026	0.004
	(0.011)	(0.023)	(0.011)
Bandwidth (m.)	961.0	991.0	936.0

Notes: Difference-in-discontinuities estimates of the impact of the differential propensity to approve construction and zoning permits relative to its neighboring council district before and after the new zoning code, interacting the parameters with the relevant pre-treatment heterogeneity dimension (Comm: ratio of commercial land-use; Resd: ratio of residential land-use; Pop: population; WlkInd: walkability index). The model uses a Poisson regression weighted by the number of parcels. The optimal bandwidths, in meters, follow Calonico et al. (2015) and Imbens and Kalyanaraman (2012). Robust-standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A.3: Heterogeneity effects on crime

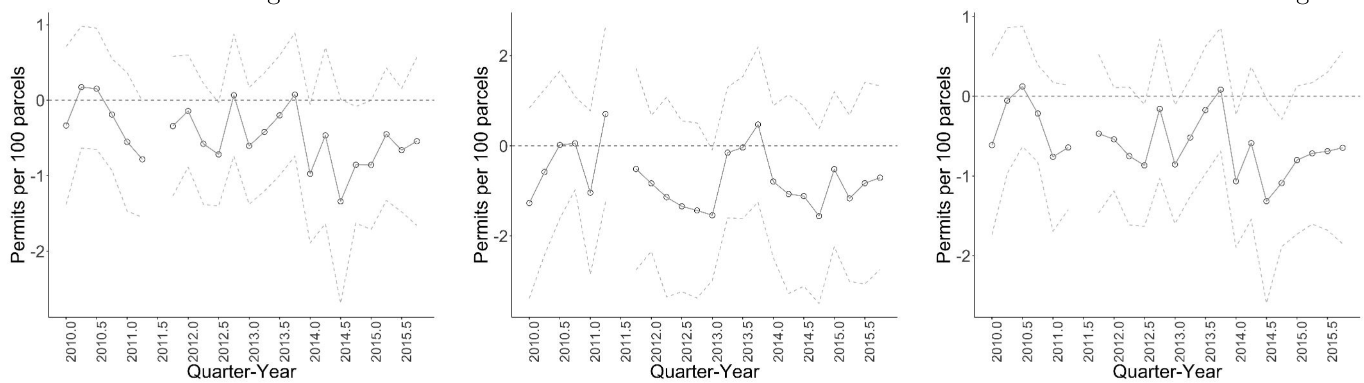
	crime	property	violent	theft	burglary	burglary resd.	burglary comm.	motor vehicle theft	assault	robbery
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Imbens and Kalyanaraman (2012) bandwidth										
Treat*Post	0.025	0.063	−0.024	0.037	0.038	0.053	0.460	−0.012	0.045	−0.134
	(0.047)	(0.062)	(0.075)	(0.095)	(0.094)	(0.090)	(0.373)	(0.115)	(0.094)	(0.115)
Treat*Post*Comm	−0.110	−0.179	−0.020	−0.044	−0.358	−0.635	−0.572	0.155	−0.019	0.056
	(0.138)	(0.157)	(0.243)	(0.193)	(0.351)	(0.420)	(0.655)	(0.363)	(0.242)	(0.341)
Treat*Post	−0.094	−0.169*	−0.024	−0.095	−0.093	−0.326	−0.113	−0.040	0.027	−0.067
	(0.096)	(0.100)	(0.176)	(0.115)	(0.232)	(0.279)	(0.344)	(0.217)	(0.163)	(0.219)
Treat*Post*Resd	0.135	0.277**	−0.008	0.192	0.096	0.371	0.688	0.082	0.019	−0.081
	(0.117)	(0.128)	(0.218)	(0.162)	(0.275)	(0.317)	(0.586)	(0.283)	(0.215)	(0.301)
Treat*Post	0.031	−0.021	0.025	−0.042	0.168	0.135	0.077	−0.082	−0.156	0.136
	(0.111)	(0.133)	(0.170)	(0.180)	(0.253)	(0.239)	(0.673)	(0.255)	(0.193)	(0.232)
Treat*Post*Pop	−0.0003	0.0004	−0.0004	0.0005	−0.002	−0.001	0.001	0.001	0.002	−0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.006)	(0.002)	(0.002)	(0.002)
Treat*Post	0.328*	0.350	0.160	0.001	1.099***	1.253***	1.086	0.115	−0.385	0.528
	(0.190)	(0.221)	(0.271)	(0.299)	(0.322)	(0.354)	(1.362)	(0.413)	(0.253)	(0.393)
Treat*Post*WlkSc	−0.005*	−0.005	−0.003	0.0005	−0.018***	−0.021***	−0.012	−0.002	0.007*	−0.010
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)	(0.019)	(0.007)	(0.004)	(0.006)
Bandwidth (m.)	422.0	389.0	426.0	396.0	568.0	494.0	405.0	487.0	614.0	400.0
B. Calonico et al (2014) bandwidth										
Treat*Post	−0.008	0.006	−0.018	0.015	0.017	0.021	0.401	−0.037	0.045	−0.135
	(0.040)	(0.052)	(0.058)	(0.079)	(0.079)	(0.085)	(0.307)	(0.088)	(0.076)	(0.096)
Treat*Post*Comm	−0.082	−0.137	−0.004	−0.091	−0.180	−0.365	−0.615	0.235	−0.044	0.099
	(0.123)	(0.138)	(0.195)	(0.169)	(0.283)	(0.332)	(0.571)	(0.319)	(0.187)	(0.298)
Treat*Post	−0.070	−0.096	−0.015	−0.083	−0.012	−0.123	−0.056	0.031	0.015	−0.061
	(0.085)	(0.094)	(0.138)	(0.110)	(0.181)	(0.215)	(0.302)	(0.204)	(0.134)	(0.188)
Treat*Post*Resd	0.061	0.103	−0.008	0.119	0.0003	0.116	0.457	−0.034	0.032	−0.079
	(0.101)	(0.109)	(0.169)	(0.140)	(0.217)	(0.247)	(0.505)	(0.254)	(0.171)	(0.260)
Treat*Post	0.008	0.055	−0.048	0.041	0.014	0.023	0.171	−0.168	−0.120	0.024
	(0.090)	(0.119)	(0.123)	(0.162)	(0.198)	(0.207)	(0.528)	(0.185)	(0.149)	(0.183)
Treat*Post*Pop	−0.0003	−0.001	0.0003	−0.001	−0.0002	−0.0003	0.00001	0.002	0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.005)	(0.002)	(0.001)	(0.002)
Treat*Post	0.191	0.354**	−0.031	−0.003	0.848***	1.021***	1.225	0.092	−0.274	0.289
	(0.140)	(0.171)	(0.192)	(0.216)	(0.290)	(0.334)	(1.062)	(0.343)	(0.217)	(0.309)
Treat*Post*WlkSc	−0.004	−0.006**	0.0001	0.00001	−0.014***	−0.018***	−0.016	−0.001	0.005	−0.006
	(0.002)	(0.003)	(0.003)	(0.003)	(0.005)	(0.006)	(0.015)	(0.006)	(0.003)	(0.005)
Bandwidth (m.)	723.0	722.0	744.0	706.0	982.0	889.0	739.0	898.0	1,058.0	647.0

Notes: Difference-in-discontinuities estimates of the impact of the differential propensity to approve construction and zoning permits relative to its neighboring council district before and after the new zoning code, interacting the parameters with the relevant pre-treatment heterogeneity dimension (Comm: ratio of commercial land-use; Resd: ratio of residential land-use; Pop: population; WlkSc: walkability score). The model uses a Poisson regression weighted by the number of parcels. The optimal bandwidths, in meters, follow Calonico et al. (2015) and Imbens and Kalyanaraman (2012). Robust-standard errors clustered at the census tract level are in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Figure A.1: Event study design on the permit rate

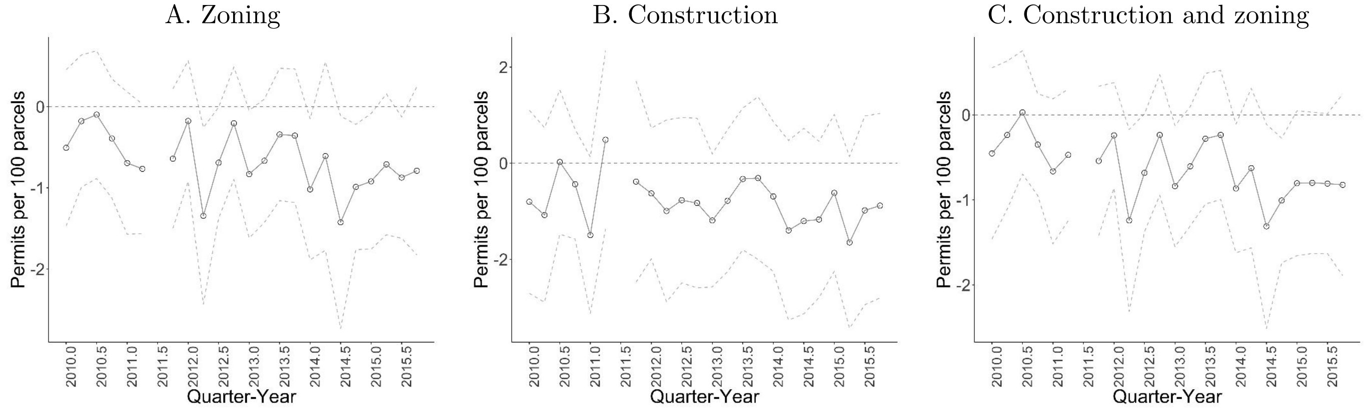
Imbens and Kalyanaraman (2012) bandwidth

A. Zoning B. Construction C. Construction and zoning



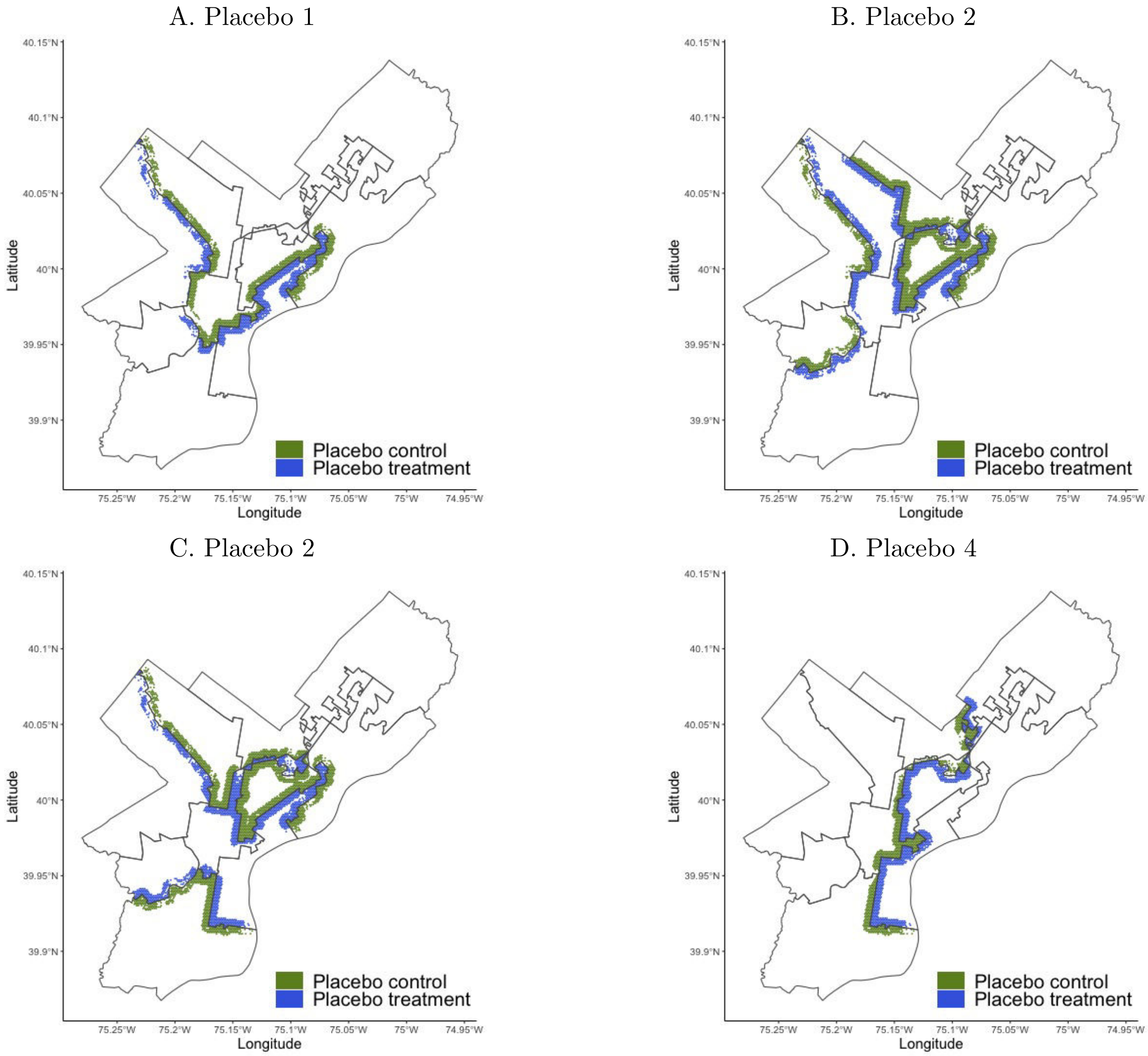
Calonico et al. (2015) bandwidth

A. Zoning B. Construction C. Construction and zoning



Notes: The estimates exhibit an event study design following: $y_{it} = \gamma_0 + \gamma_1 dist_i + treat_i(\gamma_2 + \gamma_3 dist_i) + \sum_{\tau=-q}^{m}[\beta_{1\tau} + \beta_{2\tau} dist_i + treat_i(\beta_{3\tau} + \beta_{3\tau} dist_i)] + e_{it}$, restricting the sample within distance h. The model uses a Poisson regression weighted by the number of parcels, clustering the standard errors at the census tract level. The results are qualitatively similar estimating a regression discontinuity for each quarter-year at the optimal bandwidths.

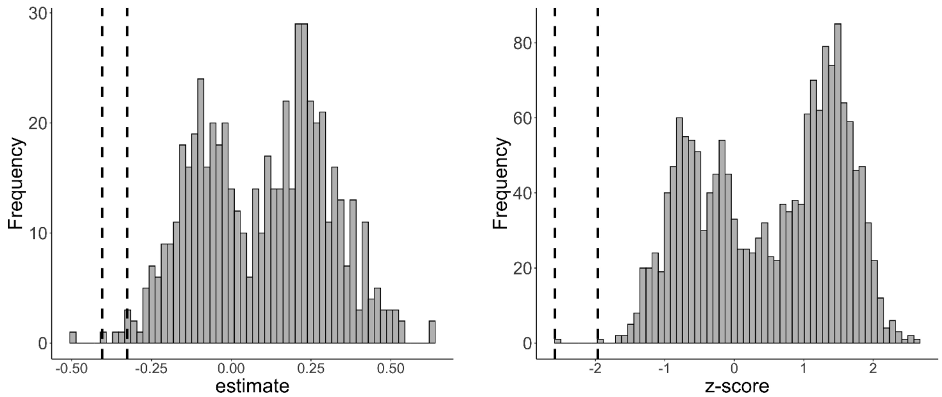
Figure A.2: Sample of the geographical placebo treatment-control pairs



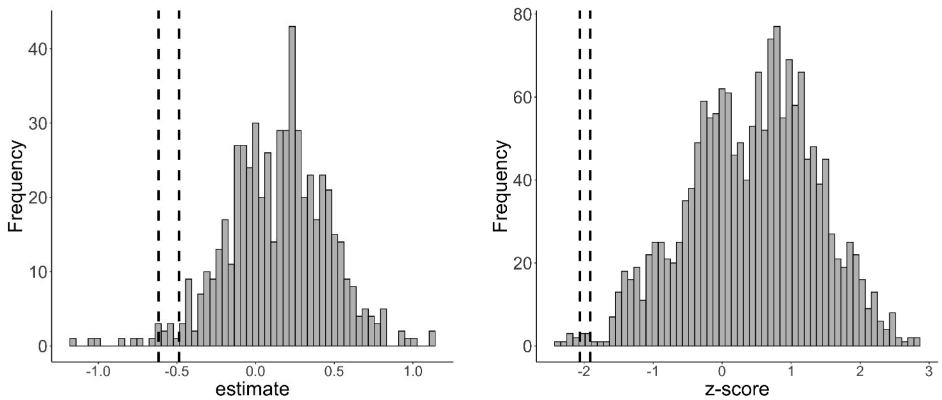
Notes: The panels visualize four out of the 95 alternative treatment-control pairs used in the geographical falsification test.

Figure A.3: Effects and statistically significance of the geographical falsification test

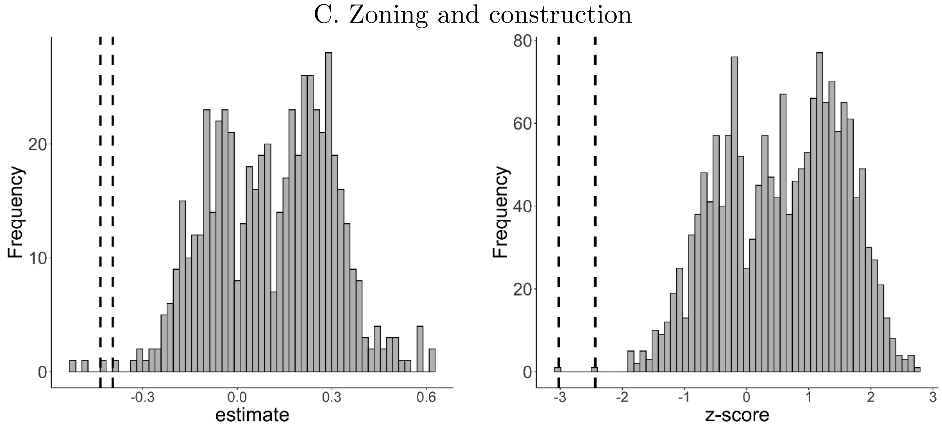
A. Zoning



B. Construction

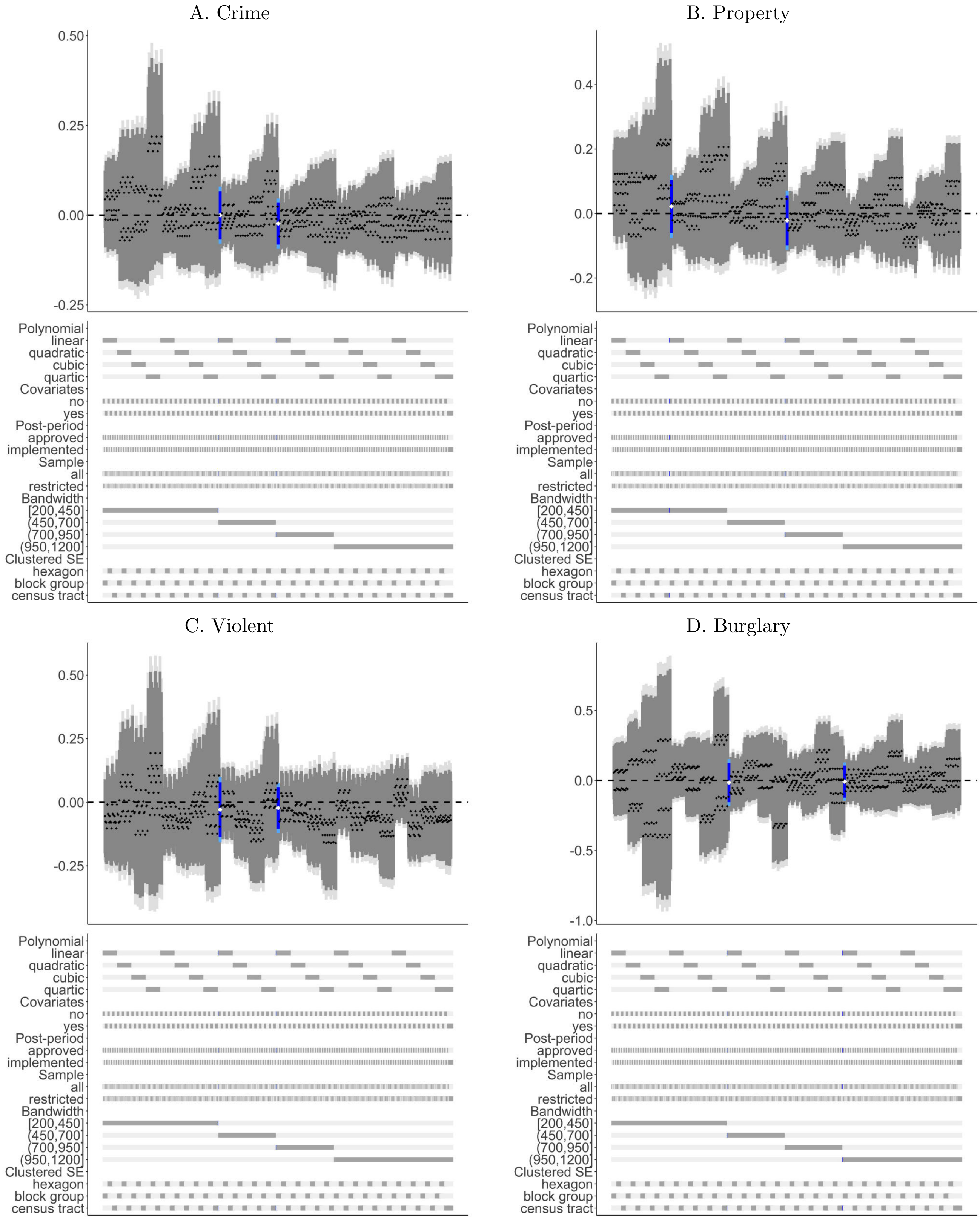


C. Zoning and construction

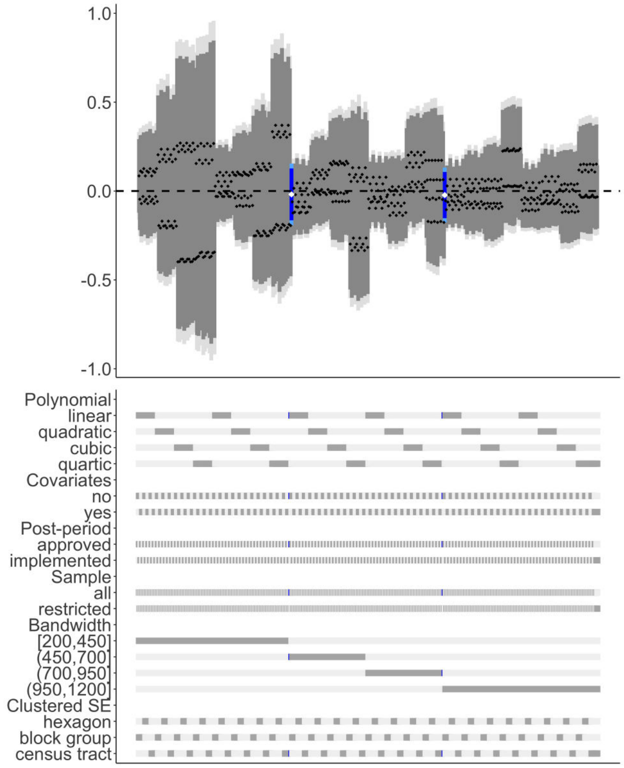


Notes: The falsification test randomly assigns a treatment indicator variable to four council districts. The treatment-control pair units come from enumerating all possible *10 chooses 4* combinations, removing those cases where at least three of the actual treatment council districts are selected (district four, seven, eight, and nine). This process results in 95 treatment-control combinations. We estimate the difference-in-discontinuities for all placebo tests using a first, second, and third degree polynomials at the two optimal bandwidths, and clustering the standard errors at the census block group, census tract, and hexagon level. The vertical lines show our main estimates using Imbens and Kalyanaraman (2012) optimal bandwidth and Calonico et al. (2015) bias-corrected bandwidth.

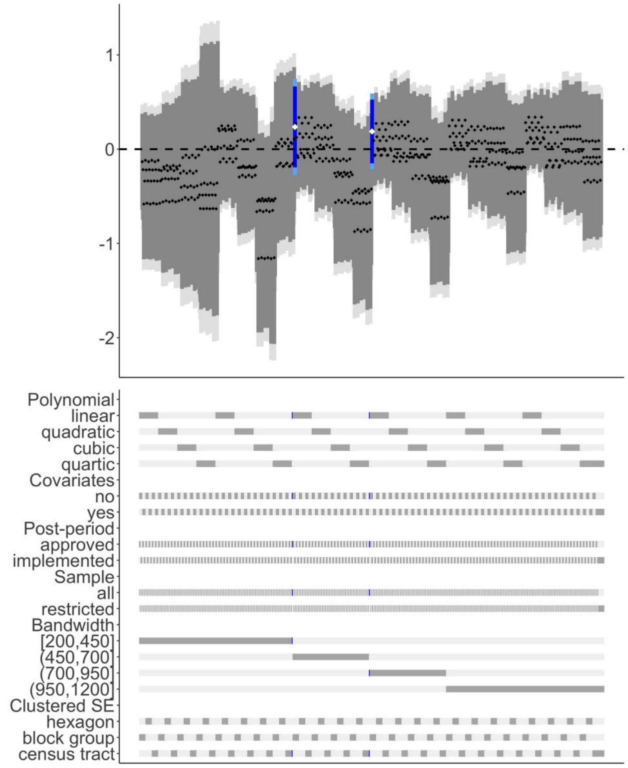
Figure A.4: Difference-in-discontinuities estimator on crime, Alternative specifications



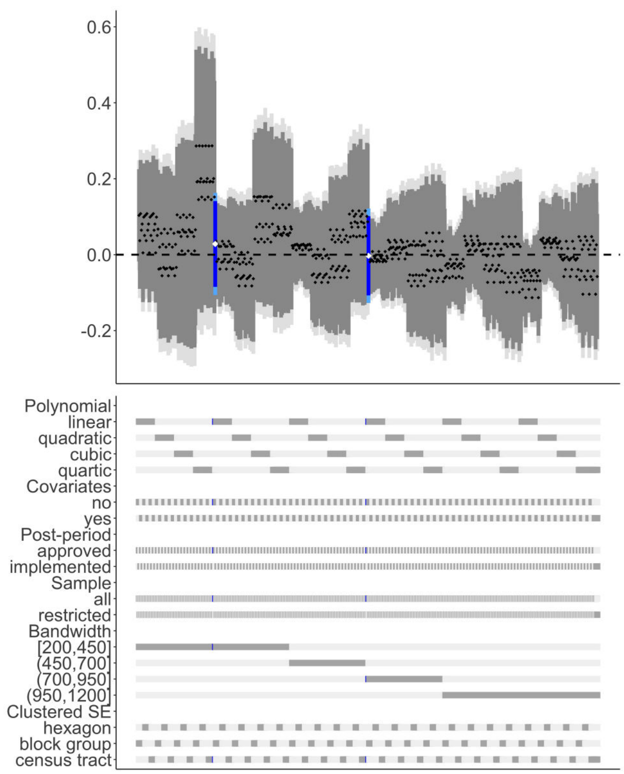
E. Burglary residential



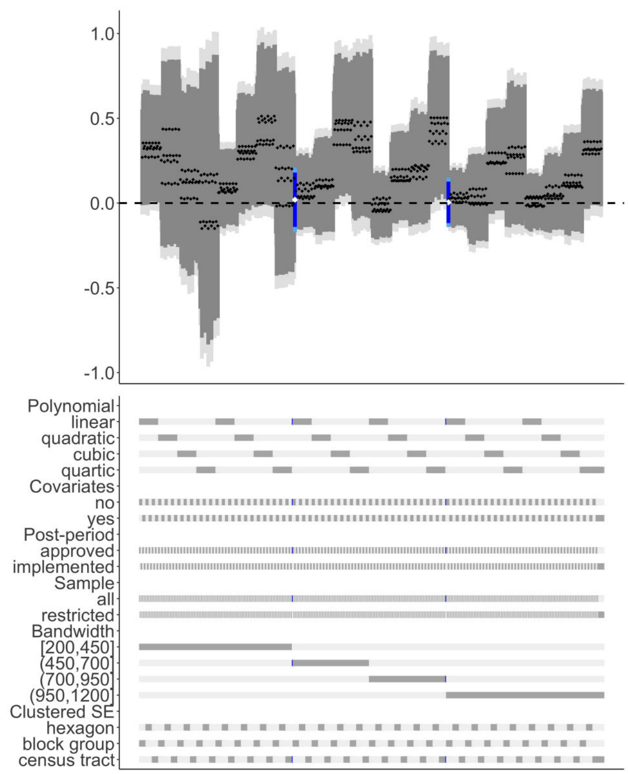
F. Burglary nonresidential

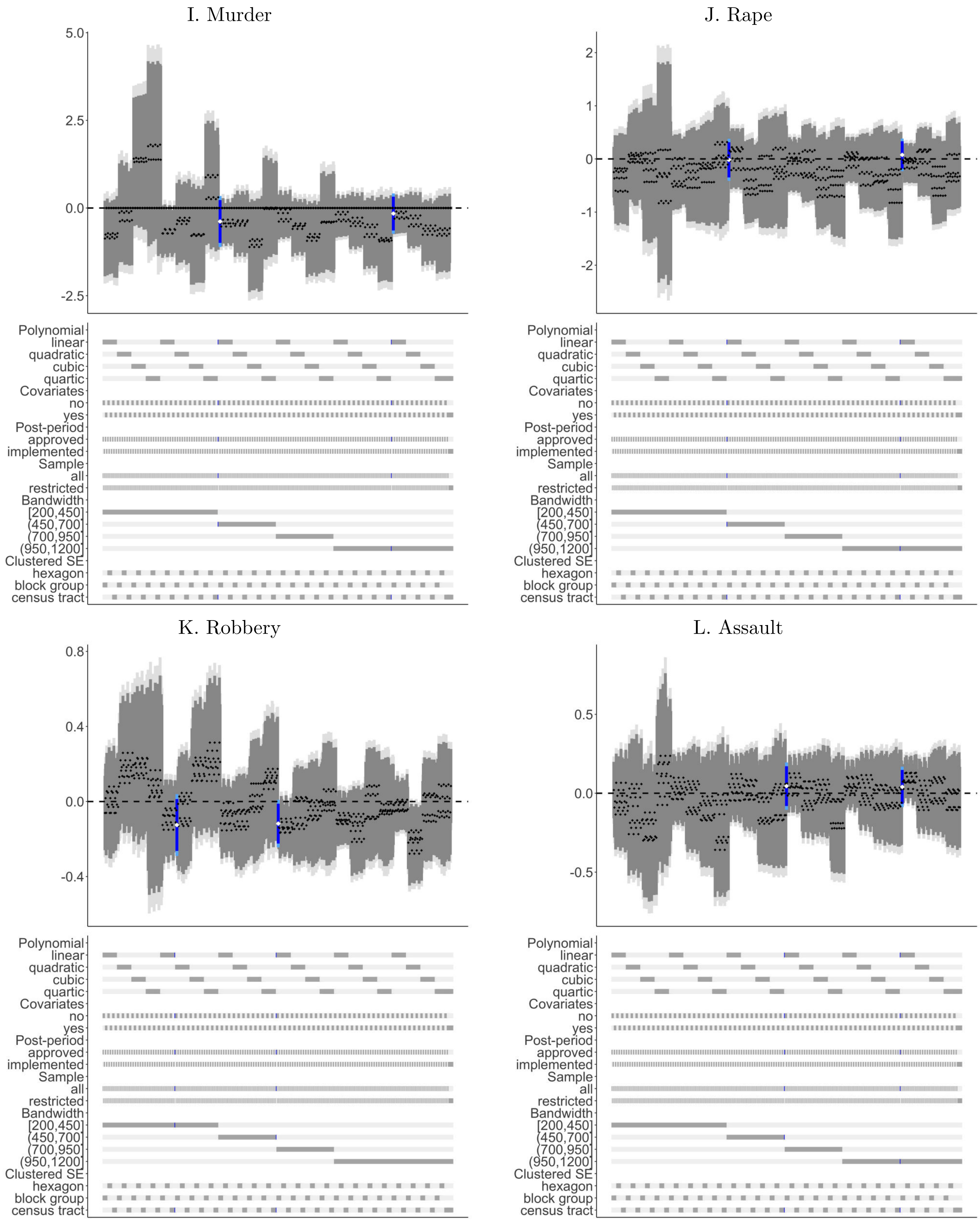


G. Theft



H. Motor vehicle theft





Notes: Each panel visualizes the difference-in-discontinuity estimator using a Poisson regression for different bandwidths (from 200 to 1,200 meters), standard errors clustering (at the census block group, census tract, and hexagon level), specification functions, and sample selection. All estimates are weighted by the number of parcels per hexagon. The two main specifications are estimated using Imbens and Kalyanaraman (2012) optimal bandwidth and Calonico et al. (2015) bias-corrected bandwidth.

B Appendix: Unsupervised classification algorithm

This appendix explains the text processing and classification method conducted on the zoning permits descriptions to identify the most likely use of a property (commercial or residential). Each permit has an open-ended description summarizing the objectives and intent of the authorized work. We converted the raw text into a machine-interpretable text to aid the classification method to recognize relevant patterns. Specifically, we applied the following standard text processing techniques in the data mining literature (Zhao and Cen, 2013). First, we removed all digits and punctuation marks. Second, we also excluded stopwords, which are commonly occurring words that provide little valuable information to the model (i.e., "a", "the", "to"). Third, among the 150 most frequent words, comprising nearly 70 percent of all words in the data, we manually corrected typographical errors (i.e., "additon" to "addition") and abbreviations (i.e., "bldgs" to "building"), and joined meaningful compound words (i.e., "day care" to "daycare"). Fourth, we reduced words to their base or root form, a process known as stemming (i.e., "relocate" to "reloc"). **Appendix Table B.1** exhibits the raw zoning permit description and the processed text of 10 random observations.

We used the processed text to build a document-term-matrix defined as a frequency of words occurring in a collection of texts (the corpus), where the rows (documents) are each zoning description, the columns (terms) are the words appearing in all texts, and each entry cell counts how many times the term appears in the document. We weighted the document-term-matrix by its term frequency-inverse document frequency, which provides larger weights to uncommon terms in the corpus, and lower weights to words appearing in most documents (i.e., a word appearing in every document of the corpus receives zero weight). Finally, we also removed sparse terms appearing in fewer than 0.01 percent of the documents. The resulting weighted document-term-matrix is the input of the unsupervised machine learning algorithm.

The *k-means* algorithm groups similar observations among k clusters by minimizing the distance of all elements to the centroid of the group (Zhao and Cen, 2013). The algorithm first defines k random centroids and assigns each data point to the closest cluster. Then, it estimates the centroid of each cluster, and assigns the observations to the closest one. This process is repeated until convergence. The researcher has to define the value of k and the results are sensitive to its choice. Two meaningless values of k, which are also its possible maximum-minimum values, are the following: selecting $k = 1$ means that all observations belong to one cluster; meanwhile, setting k equal to the number of documents means that each observation is a cluster. To determine the relevant number of clusters, one can use the Caliński and Harabasz (1974) index, a procedure shown to outperform other data-driven approaches (Milligan and Cooper, 1985). We use this method to select the optimal k.

We conducted the *k-means* method in a two-step process. First, we ran a first iteration of the unsupervised machine learning algorithm using an optimal $k = 9$ over all the observations. **Figure B.1** exhibits the most common bigrams (two consecutive words) in each cluster and its sample size.[17] Two results emerge. First, common topics emerge from each cluster. For example, clusters 7 and 8 refer to residential properties as their permits relate to changes in duplex and single-family dwellings (sfd). Clusters 3, 4, and 6 are about commercial activity referring to retail, grocery stores, restaurants, consumer goods, and daycare centers. Second, cluster 1 contains most of the permits, so we ran another *k-means* algorithm over these observations using an optimal $k = 7$. **Appendix Figure B.2** presents the bigrams frequency by cluster of these second model. The results reveal that cluster 6 is mainly about residential properties (family and single dwellings or units), while clusters 2, 3, 4, and 7 contain commercial activity descriptions (barbershops, business offices, signs, and restaurants). The clusters that we cannot identify as commercial or residential refer to vacant lots, broad zoning and land use descriptions, and alterations in the property lot lines. In short, the unsupervised classification algorithms aids in identifying the most likely use of a property requesting a zoning permit: residential, commercial, and other. **Appendix Figure B.3** shows the most common bigrams of these broad categories.

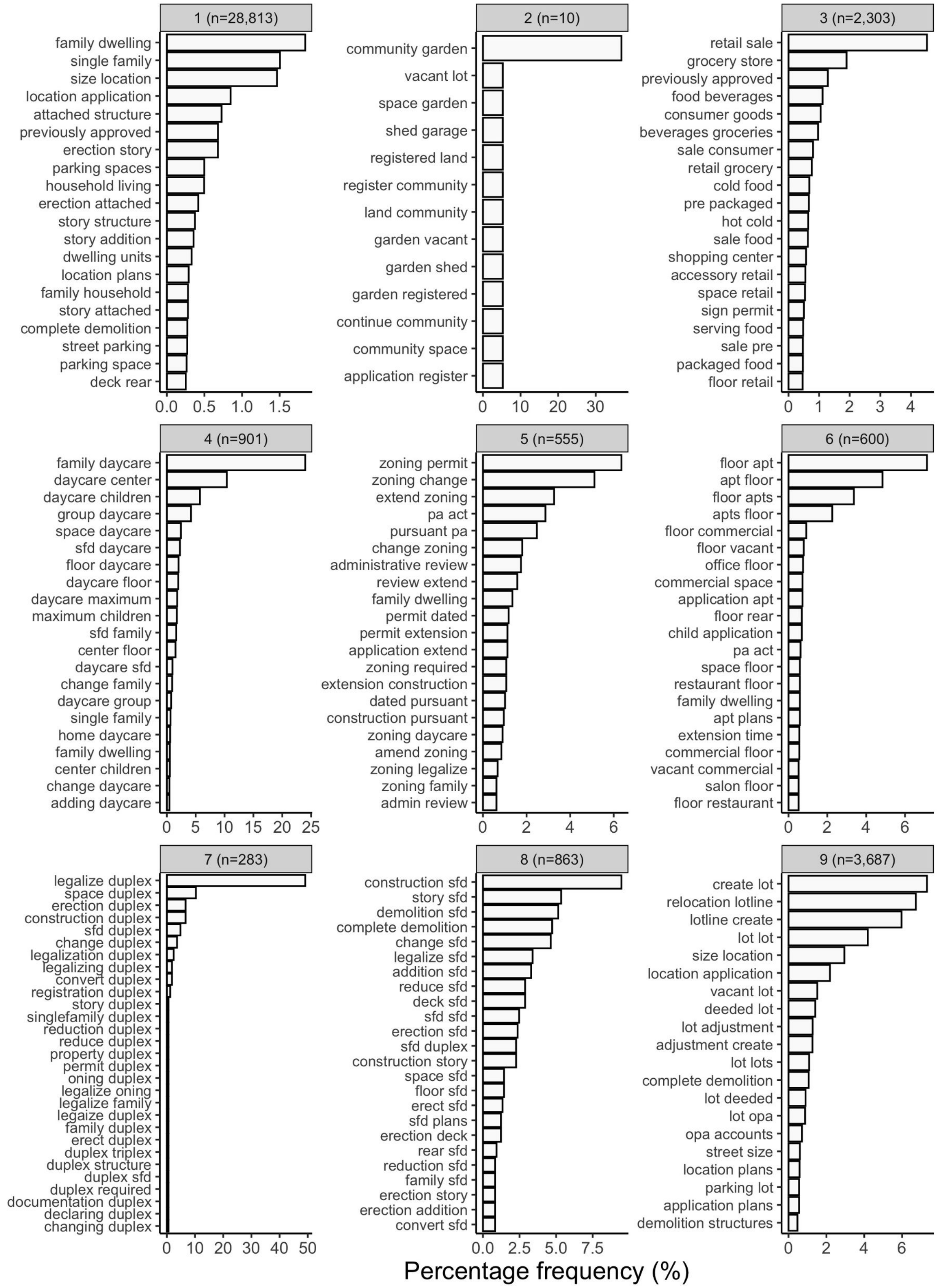
[17] To construct the bigrams, we used the text before stemming as the words are easier to read (i.e., "family" instead of "famili" or "change" instead of "chang"). The order of the words matter for building the bigrams, but it is inconsequential for the *k-means*.

Table B.1: Sample of zoning permits description

	Raw text	Processed text
1	use to hair salon in space# ? (hair cuttery)	hair salon space hair cutteri
2	1st floor use to retail store	floor retail store
3	use to grocery store with the sell of cold foods	groceri store sell cold food
4	family daycare	famili daycar
5	change use from sfd to sfd with family daycare	chang sfd sfd famili daycar
6	zoning use for 3 residential units	zone residenti unit
7	convert back to single family dwelling	convert back singl famili dwell
8	complete demo of a 2 story bldg	complet demolit stori build
9	use space as a two family dwelling.	space famili dwell
10	zoning to install (3) f/w signs	zone instal sign

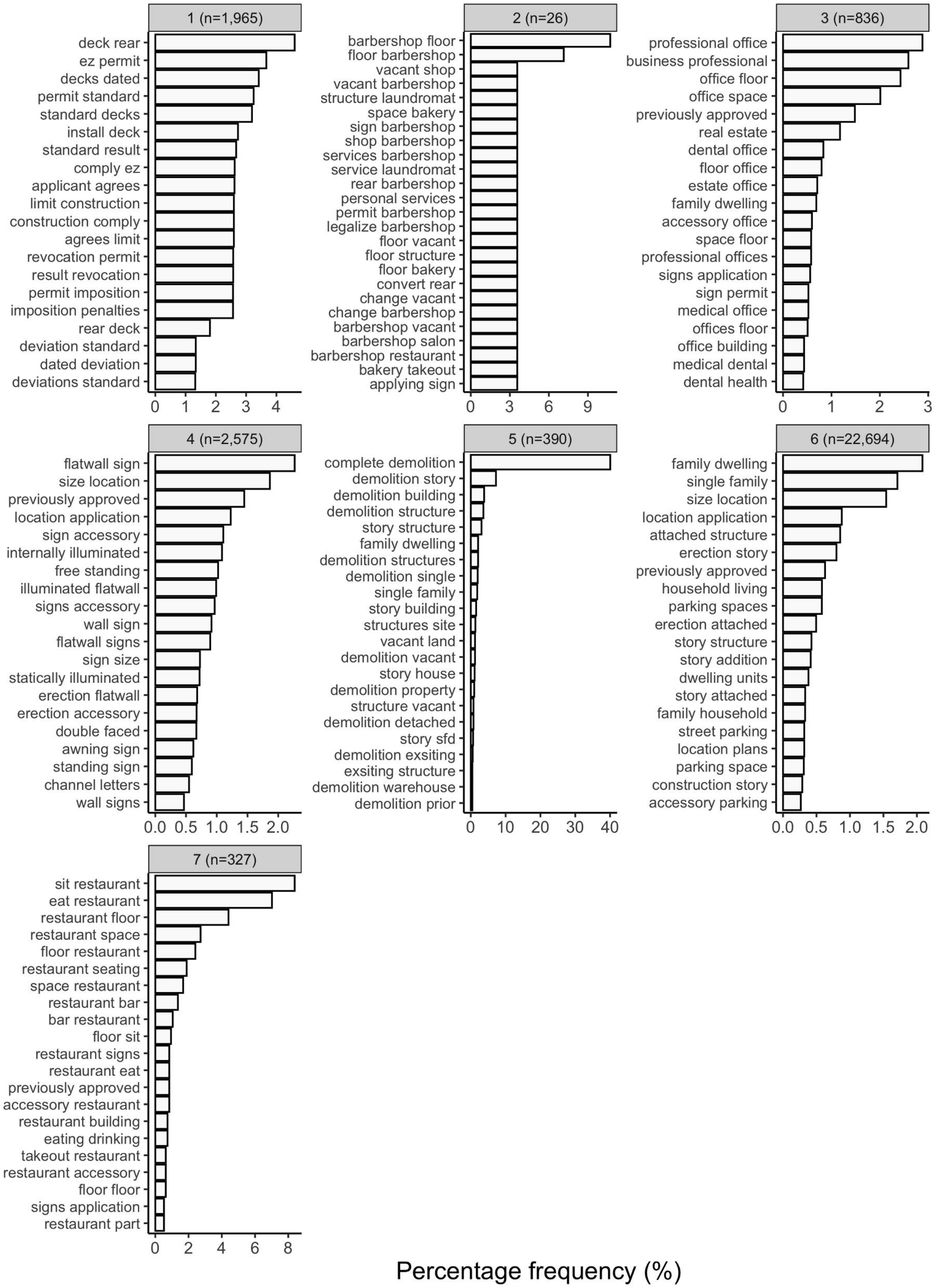
Notes: Random sample of raw open-ended descriptions of the zoning permits, and its version after performing standard data cleaning steps as explained in the text.

Figure B.1: Frequency of bigrams by cluster classification of model 1



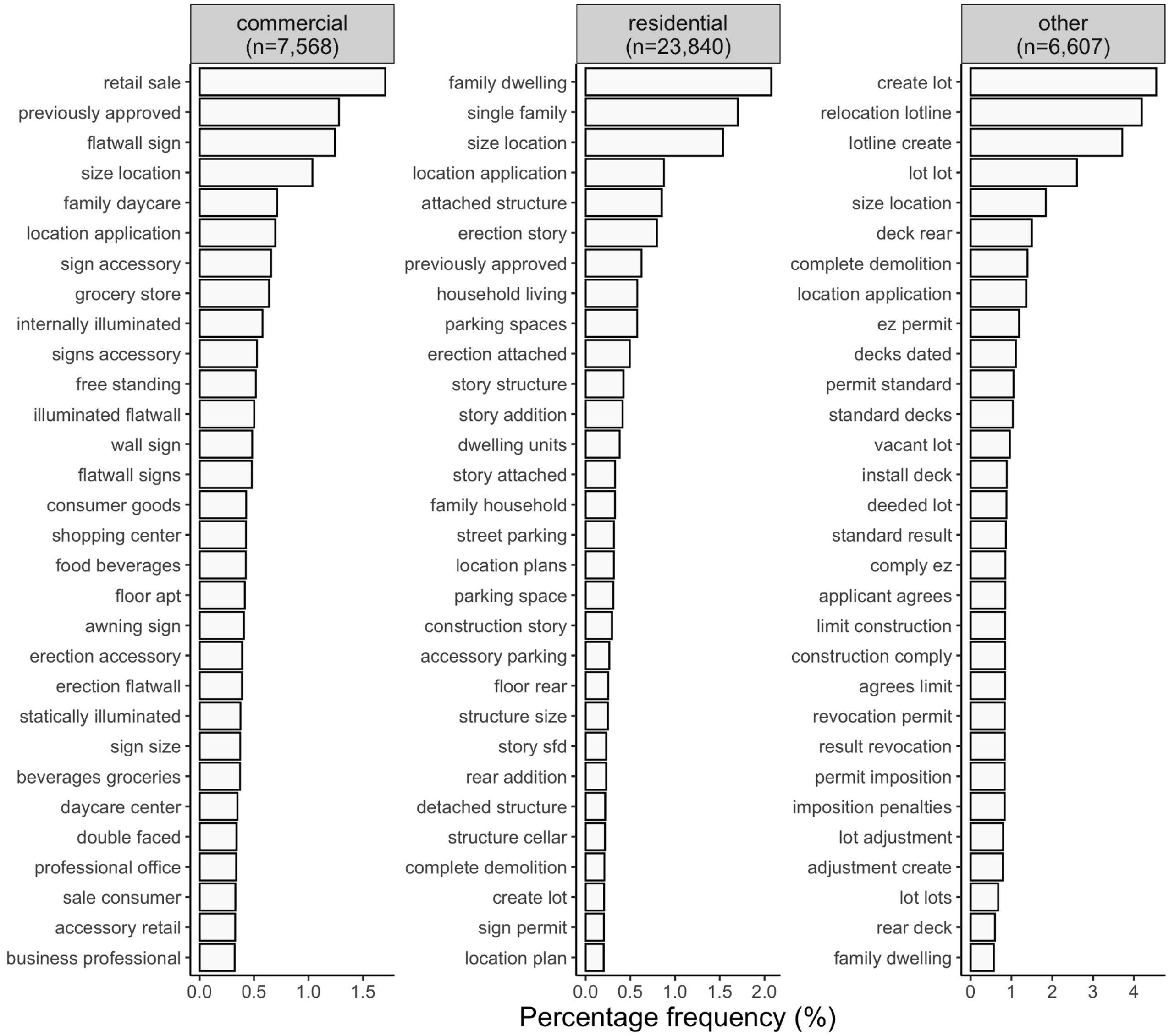
Notes: The panels exhibit the most common bigrams (two consecutive words) in each cluster and its sample size. The bigrams used the text before stemming as the words are easier to read. The order of the words matter for building the bigrams, but it is inconsequential for the *k-means*.

Figure B.2: Frequency of bigrams by cluster classification of model 2



Notes: The panels exhibit the most common bigrams (two consecutive words) in each cluster and its sample size. The bigrams used the text before stemming as the words are easier to read. The order of the words matter for building the bigrams, but it is inconsequential for the *k-means*.

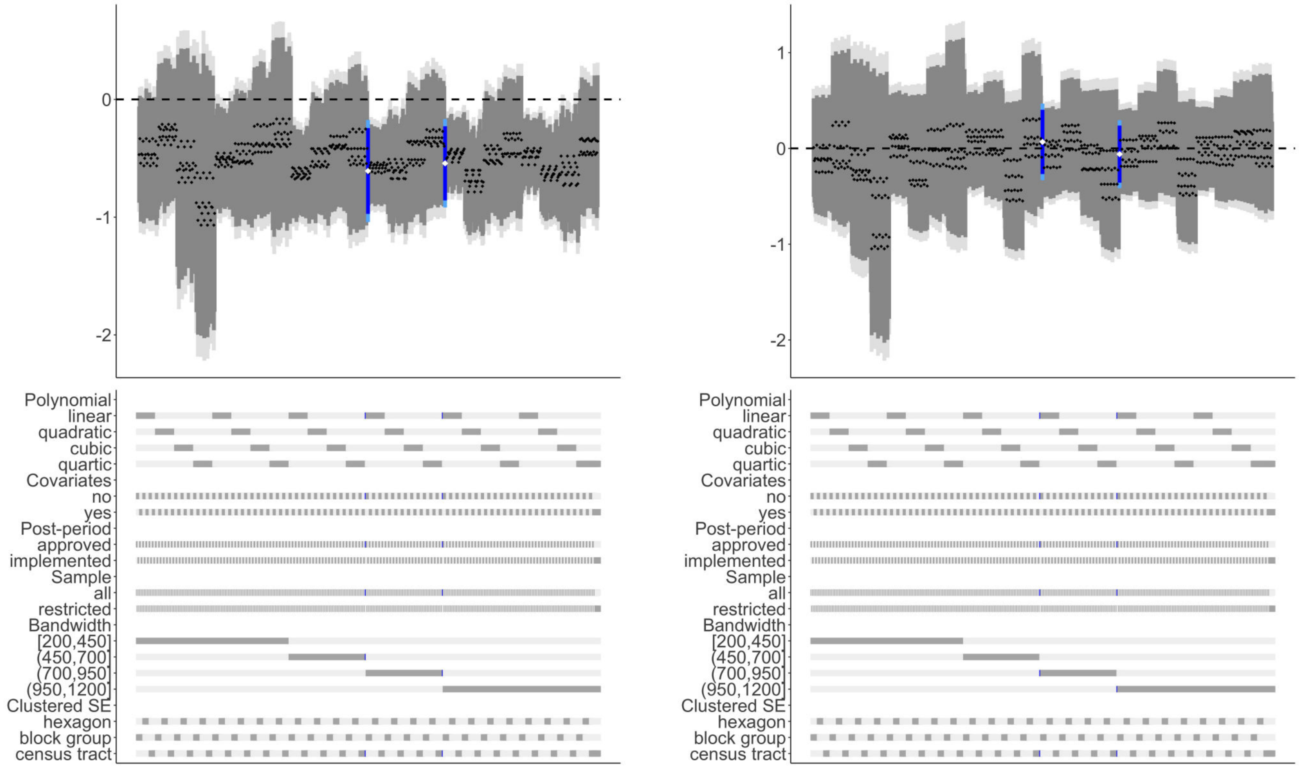
Figure B.3: Frequency of bigrams by final cluster classification



Notes: The panels exhibit the most common bigrams (two consecutive words) in each cluster and its sample size. The bigrams used the text before stemming as the words are easier to read. The order of the words matter for building the bigrams, but it is inconsequential for the *k-means*.

Figure B.4: Difference-in-discontinuities on zoning permit rate subcategories, Alternative specifications

A. Residential B. Commercial



Notes: Each panel visualizes the difference-in-discontinuity estimate using a Poisson regression for different bandwidths (from 200 to 1,200 meters), standard errors clustering (at the census block group, census tract, and hexagon level), specification functions, and sample size. All estimates are weighted by the number of parcels per hexagon. The two main specifications are estimated using Imbens and Kalyanaraman (2012) optimal bandwidth and Calonico et al. (2015) bias-corrected bandwidth.