Do Place-based Capital Investment Policies Influence Public Safety? Evidence from the Opportunity Zones Program*

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Abstract

The Opportunity Zones program, created as part of the 2017 Tax Cuts and Jobs Act, is a prominent place-based policy providing substantial tax benefits to capital investments to foster economic growth and job creation in distressed census tracts. This research evaluates its impact on economic conditions and public safety in 31 major US cities after seven years of its implementation using regression discontinuity and difference-in-differences methods. The estimates suggest that the Opportunity Zones may have caused an increase in localized equity investments and urban construction projects, but it has had limited socioeconomic neighborhood changes measured by small business loans, property prices, poverty, employment, and income levels. It has not impacted public safety, comprised of calls for service, police stops, crimes, and arrests. The overall null effects do not mask city-specific impacts on crime and arrest subcategories. The evidence suggests that place-based capital investment policies face challenges influencing long-term socioeconomic and public safety improvements. These results do not imply the abandonment of these initiatives, as there is value in targeting resources to the most disadvantaged areas. Still, they should consider the physical design of places, be well-targeted to the neighborhoods' needs, and complement other community investments.

Keywords: place-based interventions, urban crime, Opportunity Zones, community investments, regression discontinuity, difference-in-differences.

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1 Introduction

Joblessness, material deprivation, and concentrated disadvantage have been consistently found to influence crime (Kelly, 2000; Machin and Meghir, 2004; Raphael and Winter-Ebmer, 2001). More broadly, uneven economic growth and structural changes have restricted the economic mobility of the most disadvantaged. This phenomenon, compounded with historical racism and neighborhood disinvestment, has increased inner-city poverty (Aaronson et al., 2021; Wilson, 2003). While moving people out of poor communities is an effective strategy to change people's trajectories (Chyn, 2018; Kling et al., 2005; Sciandra et al., 2013), it is not scalable. Instead, policies could aim to improve the areas where people live (Sampson, 2016; Sharkey, 2013).

Place-based policies deal with pockets of distress by focusing on the vitality of a place (Ladd, 1994). These strategies usually provide fiscal incentives (e.g., tax benefits, subsidies, cash grants) for new jobs, businesses, and capital investments to promote local economic growth in delimited geographical areas. Economic and equity reasons support place-based policies that foster economic growth in distressed areas (Bartik, 2020c; Neumark and Simpson, 2015). They can promote positive externalities by increasing the sharing, matching, and learning among firms and workers, raising their productivity (agglomeration economies and network effects). These initiatives can also address market failures that partially explain differential employment rates across ethnic and racial groups (the spatial mismatch and racial mismatch hypotheses).[1]

Spatially targeted interventions encouraging urban development changes, particularly on vacant properties, will likely reduce crime (Branas et al., 2018; Cui and Walsh, 2015; Spader et al., 2016). Investments impacting construction jobs would also reduce criminal offenses, especially for those with a criminal background (Schnepel, 2018). Changes on local business activity, particularly those attracting foot traffic, can also reduce crime (Chang and Jacobson, 2017). Investments leading to more mixed-land-use areas can reduce crime by creating natural surveillance mechanisms (Jacobs, 1961; Twinam, 2017). Local officials could complement private and public investments in the built environment, such as street lighting or greening the urban space, to also reduce criminal behaviors (Chalfin et al., 2021; Branas et al., 2018).

Expectations about forthcoming development projects and new economic conditions can be another mechanism for place-based interventions to influence criminal activity before capital investments occur. The announcement of construction projects signals the intention and commitment of investors to improv-

[1] The spatial mismatch hypothesis (Kain, 1968) argues that the problem of differential employment rates among comparable individuals of different races is the lack of jobs where minorities live. In contrast, the spatial mismatch hypothesis (Hellerstein et al., 2008) sustains that the problem is the lack of jobs held by members of one's race. These terms come from economics, but they relate to the concepts of the underclass and inner-city ghetto in sociology.

ing an area so that residents, developers, and business owners adjust their beliefs and behaviors about future neighborhood conditions before the investment takes place. Policy announcements are a common mechanism explaining behavioral changes in monetary policy and financial markets (Bomfim, 2003). It can also translate into new housing units, property renovations, and higher property prices (Billings, 2011; Cao and Porter-Nelson, 2016; Yen et al., 2018). These changes capitalize even if the project is eventually canceled (Dehring et al., 2007). There is also evidence that announcing a new transit project can decrease crime before its construction (Billings et al., 2011), despite having null effects on the local labor market (Canales et al., 2019).

Place-based strategies have gained interest in policy-making since the 1980s and 1990s when elected officials enacted federal and state-level place-based programs to revitalize neighborhoods. The most well-known strategies in the US have been the New Markets Tax Credit, the Enterprise Zones, and the Empowerment Zones.[2] Their policy evaluations have found mixed evidence on employment, earnings, and business formation (Billings, 2009; Bondonio and Greenbaum, 2007; Busso et al., 2013; Freedman, 2012, 2015; Hanson and Rohlin, 2013; Harger and Ross, 2016; Neumark and Kolko, 2010; Neumark and Young, 2019; O'Keefe, 2004). While there is disagreement about their design, scholars consider place-based initiatives a promising strategy (Bartik, 2020a,b; Neumark, 2020a,b). Policymakers think alike as recent administrations have continued embracing them.[3]

The Opportunity Zones are the most recent national place-based policy in the US. It was created as part of the 2017 Tax Cuts and Jobs Act and aims to spur economic growth and job creation by providing substantial tax benefits to capital investments in low-income census tracts. It encourages sustained neighborhood investments, particularly on high-intensity capital investment properties. Research suggests that its early impacts (two or at most three post-intervention years) have been limited (Chen et al., 2023; Corinth and Feldman, 2021; Freedman et al., 2023), but these results seem to mask heterogeneous effects (Arefeva et al., 2021; Atkins et al., 2023; Sage et al., 2023; Xu, 2021).

Despite the theoretical and policy relevancy of place-based initiatives encouraging economic growth in distressed areas, the lack of geo-referenced, time-stamped crime data to identify changes in small areas has limited the research on its public safety effects. This situation is understandable as few jurisdictions have released detailed sub-city criminal offense information. The ones available cover since the mid-2000s at best. To address this knowledge gap, this study assesses the early impacts of the Opportunity Zones program on economic and public safety conditions by collecting administrative and survey data from 31 of

[2] Carmon (1999), Ladd (1994), and Van Gent et al. (2009) provide a review of place-based policies in the US and Europe.

[3] The Choice Neighborhoods, Promise Neighborhoods, and Promise Zones are other recent national place-based initiatives in the US.

the largest US cities.

This research makes three contributions. First, it uses administrative records at the census tract level to assess impacts on construction and zoning permits and small business loans. This approach aims to measure local urban development changes without relying on survey data usually aggregated at higher geographical levels (e.g., city or zip code), reducing the statistical uncertainty. Second, this research evaluates the impact of the Opportunity Zones on public safety (calls for service, police stops, crimes, and arrests). Calls for service are an alternative to measuring public safety with fewer concerns about selective reporting from law enforcement agencies (Bursik Jr and Grasmick, 1993; Klinger and Bridges, 1997; Maxfield, 1982). Police stops and arrests signal residents' increased demand for public safety and police behavioral responses to new urban developments (Beck, 2020; Laniyonu, 2018). Third, by using data from 31 US cities to measure the Opportunity Zones' impacts on economic changes and crime effects,[4] this research increases the external validity and statistical power relative to single-city case studies.

Using regression discontinuity and difference-in-differences estimators, the results suggest that subsidizing capital tax investments may have marginally increased investments from private equity, and also have led to an increase in urban development measured through construction and building permits. However, at least seven years after its implementation, it has had limited socioeconomic neighborhood changes measured by property prices, poverty, employment, and income levels. The Opportunity Zones have not improved public safety, comprised of calls for service, police stops, crimes, and arrests. The heterogeneity analysis reveals that the null impacts do not mask city-specific changes on detailed crime and arrest categories. The evidence suggests that national place-based capital investment policies face challenges in influencing long-term community changes and public safety improvements.

The remainder of the article is organized as follows. Section 2 reviews the literature on place-based interventions and the Opportunity Zones regulations. Sections 3 and 4 explain the data and empirical strategy. Sections 5 and 6 present and discuss the results, and Section 7 concludes.

2 Background

2.1 Opportunity Zones legislation

Private investments can complement public spending to spur economic growth. However, the private sector has few incentives to invest in distressed neighborhoods unless the return on the investment increases by removing existing frictions. Prior place-based programs have not leveraged the influence of financial

[4] Calls for service is available for 13 cities. Police stops and arrests use data from 18 cities due to data limitations.

intermediaries (e.g., equity firms, banks, hedge funds, venture capital) to coordinate large investments. These intermediaries can pool and deploy resources in multiple projects in targeted areas by raising capital from individual and institutional investors. In addition, by focusing on capital investments without complex regulations, there are incentives and flexibility for investing in new and small businesses as well as in large infrastructure projects and capital-intensive industries, all of which are needed to revitalize distressed neighborhoods (Bernstein and Hassett, 2015). These ideas sketched what eventually would become a bipartisan bill co-sponsored by almost 100 congressional members in the House and Senate in 2017, leading to the creation of the Opportunity Zones.

As part of the 2017 Tax Cuts and Jobs Act, the Opportunity Zones program amended the Internal Revenue Code to provide tax incentives by deferring capital gains invested in low-income communities. These communities were defined as census tracts with a poverty rate above 20 percent or below 80 percent of the greater statewide or metropolitan area median family income. In addition, tracts with less than 2,000 people within an Empowerment Zone or contiguous to one or more low-income census tracts were also considered low-income communities. Governors nominated 25 percent of their state's eligible tracts. The Internal Revenue Service released the list of designated places between April and June of 2018 (IRS, 2018c,b).

Qualified opportunity funds are the investment vehicles organized as corporations or partnerships to invest in the program as long as they hold 90 percent of their assets in Opportunity Zones. Excepting the "sin businesses" (e.g., golf courses, country clubs, massage parlors, gambling businesses, and bars), the program allows investments in many assets. After acquiring a property, investors must substantially improve it within 30 months to receive the tax benefits. Therefore, these requirements encourage sustained neighborhood investments, particularly on high-intensity capital investment properties, such as vacant lots, older properties, and large-scale commercial and residential projects, so measuring urban development is essential. The tax benefits increase as the investment is held for an extended period in the designated neighborhoods.

The legislation provides three tax benefits. First, capital gains (investment appreciation) from the sale or exchange of any property (e.g., real property or equity) invested in a qualified opportunity fund within 180 days of the transaction can be deferred until the property is sold or 2026, whichever is earlier. To relieve investors facing hardships meeting the 180-day deadline amidst the COVID pandemic, the IRS (2020, 2021) extended the deadlines to 544 days (March 2021). Second, capital gains invested in Opportunity Zones properties receive a 10 percent reduction on the taxpayer's investment basis when held for five years before 2026, increasing to 15 percent after seven years. To be clear, to accrue the 15 (10) percent tax reduction for holding the investment seven (five) years before 2026, investments should have been made by 2019 (2021) at the latest. Third, investments held for at least 10 years in an Opportunity Zone have no taxable income on capital gains from selling or exchanging such property. There are no limits on the amount taxpayers can claim under this program, which is a relevant difference from previous programs.

To better understand the tax benefits, assume a hypothetical investment of $100,000 in 2018. The financial resources come from selling another property but were reinvested in a qualified opportunity fund within 180 days of the transaction. Considering a seven percent annual compound rate without periodic dividends, the final value after 10 years is $196,715. After five years, the tax benefits mean a 10 percent reduction ($10,000) of the taxpayer's investment basis and 15 percent ($15,000) after seven years. If the property is sold after 10 years, the investor will not pay taxes on the $96,715 in capital gains, nor on $15,000 of the original investment. The investor would only pay taxes on $85,000 in 2026, while the remainder is tax-free (CRS, 2020). At the national level, the Joint Committee on Taxation (JCX, 2019, 2020) expects that the foregone tax revenue due to this program will range between 1.6 to 3.5 billion dollars annually (including pre and post-COVID estimates). This amount represents between 1.3 and 2.9 percent of the state and local expenditures on policing in the US.[5]

2.2 Prior literature

Place-based interventions providing tax benefits to business and development projects report mixed results. For instance, the New Market Tax Credit decreased poverty and unemployment (Freedman, 2012, 2015), but showed differential employment effects across industries (Harger and Ross, 2016). The Empowerment Zones increased jobs and earnings without changing housing rents (Busso et al., 2013). Still, it may have come at the expense of negative employment spillovers in neighboring areas (Hanson and Rohlin, 2013). The Enterprise Zones, enacted at the state level, have reported positive employment impacts (Billings, 2009), but these effects seem temporary (O'Keefe, 2004). Other studies report no impact on employment or poverty (Neumark and Simpson, 2015), which could be related to new firms experiencing positive effects while older ones have negative impacts (Bondonio and Greenbaum, 2007).

Possible explanations for these mixed findings could be that the tax incentives change across locations as the programs have different priorities (e.g., real estate vs. community development, business climate vs. residents' welfare). Also, the program's expansion could dilute their impacts by including less distressed areas (Greenbaum and Bondonio, 2004; Greenbaum and Landers, 2009). No research has studied the

[5] See https://www.urban.org/policy-centers/cross-center-initiatives/state-and-local-finance-initiative/state-and-local-backgrounders/criminal-justice-police-corrections-courts-expenditures

effects of these place-based initiatives on public safety, which is a relevant knowledge gap that this paper contributes to closing.

Another place-based policy promoting economic growth in distressed areas is deploying targeted public investments. These interventions lean towards positive economic impacts and crime reductions. For example, providing and repairing existing business floor space and other social interventions increase jobs but may not impact residents' employment rates (Gibbons et al., 2021). Neighborhood renewal projects focusing on a myriad of local projects (Alonso et al., 2019), low-income housing development (Freedman and Owens, 2011), and contra-cyclical programs targeting rehabilitation projects and improving public spaces (Montolio, 2018) have shown crime reductions. However, urban development changes can also lead to more crime or null effects. For instance, localized economic development can create criminal opportunities among those not benefiting from the intervention (Freedman and Owens, 2016). Short-term reductions in urban development may have limited impacts on crime, particularly if only residential projects are affected (Mitre-Becerril and MacDonald, 2024). Public-private investments in new mixed-income developments may impact property prices, but may not influence serious criminal activity (Baird et al., 2020). Furthermore, neighborhood revitalization projects can lead to unintended consequences like gentrification. While there is a negative correlation between gentrification and crime (MacDonald and Stokes, 2020; Papachristos et al., 2011), these changes are unlikely to improve the well-being of the most disadvantaged. Displacement effects are a concern for place-based interventions (Neumark and Simpson, 2015).

Policy evaluations on the early impacts of the Opportunity Zones program have reported mixed results. Evidence suggests that the program created employment and establishment growth in metropolitan areas across different industries and subpopulation groups (Arefeva et al., 2021). Vacant lots and older properties had price increases compared to similar properties at eligible, not designated tracts (Sage et al., 2023). The program may not have increased job postings on average, but seems to have had positive impacts in urban and high Black populated areas (Atkins et al., 2023). It may have had an overall increase in private investments at the expense of a decrease in entrepreneurship in the non-tradable sector, like the retail and restaurants (Xu, 2021). Others have found limited short-term impacts on overall housing prices, commercial investment, property transactions, and residents' employment, earnings, and poverty levels, indicating that investors anticipate little future economic growth or that it may be highly localized (Chen et al., 2023; Corinth and Feldman, 2021; Freedman et al., 2023). Accordingly, analyzing the Opportunity Zones program on a subset of highly populated cities after seven years of its enactment is relevant to understand better its impacts on economic and public safety changes.

3 Data

3.1 Data sources

Studies focusing on the impacts of the Opportunity Zones program designation on economic outcomes (employment, earnings, poverty, residential property prices and transactions, and commercial establishment data) use census tract data with national representation. All the studies have used the American Community Survey. Some complement it with data from the Federal Housing Finance Agency (Chen et al., 2023), Real Capital Analytics Commercial Real Estate Database (Corinth and Feldman, 2021; Sage et al., 2023), Your-economy Time Series information (Arefeva et al., 2021), Burning Glass Technologies (Atkins et al., 2023), and OpenCorporates (Xu, 2021).

Ideally, estimating the impacts of the Opportunity Zones program on public safety would rely on data for each of the nearly 73,000 census tracts in the US. Unfortunately, there is no national repository at the census tract level on public safety data (calls for service, police stops, crimes, and arrests). The nation's two crime measures –the Uniform Crime Reporting (UCR) and the National Crime Victimization Survey– do not provide subcity-level data to evaluate this intervention. This research overcomes the lack of a public safety national repository at the census tract level by gathering and geocoding time-stamped incident information from 31 of the largest cities in the US.[6] Data on calls for service, police stops, crime, and arrests come from each city's police department. The crimes and arrests are categorized into major and non-major. Major crimes include the UCR part I categories: murder, robbery, and aggravated assault, which comprise the violent crimes, and burglary, theft, and motor vehicle theft, defined as property crimes. Non-major crimes are all the other incidents reported to the police departments.

This research uses construction, zoning, and land-use change permits to measure urban development. It relies on administrative records from each city's authority regulating the permits (e.g., Department of Buildings, Department of Licenses and Inspections) aggregated at the census tract level.[7] To keep track of whether there is a change in the price of single-family housing, this research uses the House Price Index constructed by the Federal Housing Finance Agency based on repeated sales or refinancing involving

[6] Out of the 76 most populated cities, 36 do not publish detailed crime data that can be aggregated at the census tract level, while nine cover data partially (i.e., 2018-2020, missing years). The cities meeting the data requirements for this study are Aurora, CO, Austin, TX, Baltimore, MD, Boston, MA, Buffalo, NY, Chicago, IL, Cincinnati, OH, Columbus, OH, Greensboro, NC, Kansas City, MO, Los Angeles, CA, Louisville, KY, Mesa, AZ, Milwaukee, WI, Minneapolis, MN, Nashville, TN, New Orleans, LA, New York, NY, Norfolk, VA, Orlando, FL, Philadelphia, PA, Pittsburgh, PA, Portland, OR, Raleigh, NC, Sacramento, CA, Saint Paul, MN, San Francisco, CA, Seattle, WA, St. Louis, MO, Tucson, AZ, and Washington, DC.

[7] Chen et al. (2023) used the Census Building Permits Survey at the place level (e.g., town or city level usually), which prevents using detailed geographical information and introduces undesired measurement errors due to the nature of survey data.

mortgages purchased or securitized by Fannie Mae or Freddie Mac.[8]

While investors must contribute with equity to receive the tax benefits, property owners and business owners could obtain a loan as an additional source to finance their property or business. For example, the US Small Business Administration has relaxed its requirements to make it easier to acquire debt in Opportunity Zones.[9] Consequently, measuring small business loans is relevant to examining the impacts of the intervention. The Federal Financial Institutions Examination Council provides annual information on small business loans (less than one million dollars) at the census tract level.

One shortcoming in the Opportunity Zone legislation was the lack of robust tracking and reporting investment mechanisms (CRS, 2020; GAO, 2020). While corporations or partnerships must self-certify their qualified opportunity fund and disclose their capital gains using the Internal Revenue Service Forms 8996 and 8997 in their annual income tax filings, privacy protections limit disclosing taxpayer data. To overcome the lack of detailed qualified opportunity funds data, this research follows the Council of Economic Advisers (CEA, 2020) approach by relying on the Securities and Exchange Commission (SEC) Form D dataset to measure raised private equity in operating businesses.[10] Form D allows companies to submit an exemption from the SEC to offer stock to finance their operations without needing an initial public offering and selling stock to the public.[11] The equity investments are restricted to non-banking, non-financial services companies.[12] To avoid capturing atypical variations of large firms' transactions, the investments capture the filings raising less than $50 million in any quarter (results are qualitatively the same when changing this restriction). The investments were aggregated at the census tract year level by geocoding the address of the operating business.[13]

The analysis also includes socioeconomic and demographic variables collected from the American Community Survey (ACS), the common information source to measure changes at small geographical levels annually. It considers the five-year census tract-level estimates on the percentage of Black, White, and Hispanic population, age groups (below 14, 15-24, 25-39, 40-54, and over 55 years old), schooling attainment (percentage of residents with less than high school, high school, some college, and college education), the unemployment and poverty rates, employment to population ratio, gross rent, and median family in-

[8] See https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx

[9] See https://opportunityzones.hud.gov/entrepreneurs/smallbusiness

[10] See https://www.sec.gov/dera/data/form-d

[11] While some opportunity funds can be identified using keywords (e.g., "OZ fund", "QOZF", "QOFB") and matching their names to crowd-sourced opportunity funds directories, it does not capture the re-labeling of those aiming to use the tax benefits but were already happening in the census tract, and it drastically undercounts them.

[12] It excludes companies in banking and financial services (commercial banking, insurance, investing, investment banking, and pooled investment funds).

[13] Businesses can make investments outside of their address, but there are no reasons to suspect that this behavior affects different businesses in and outside of Opportunity Zones as Form D is not used for tracking the tax incentives.

come. It also uses estimates of the statistical metropolitan area and statewide median family income levels to build the cut-off ratio of the family income level. In 2020, the Census Bureau updated its geographical boundaries as it does every ten years, which usually means splitting high-populated tracts in half. The data released under the new boundaries was apportioned to the old ones using the relationship files published by the Census Bureau.[14] Finally, this research relies on the list of designated and eligible census tracts compiled by the Urban Institute.[15]

3.2 Analytical database

This research uses information from the most populated US cities with public crime data that could be aggregated to the census tract-year level. **Figure 1** presents the 31 cities included in the analysis. While these cities are not a representative sample of the US population, they have a diverse geographical variation following the patterns of the major population centers in the country, and they include around 10 percent of the total US population.[16] **Table 1** presents pre-intervention (2014 to 2017) descriptive statistics for the 5,631 eligible census tracts in the 31 cities included in the study by designation criteria, out of which 1,274 were designated Opportunity Zones. The average designated tract had 306 and 176 non-major and major crimes in any given year, 44 and 30 percent more than the typical eligible but not designated tract. Thefts and aggravated assaults are the most common crimes, followed by burglary. The distribution is consistent with national crime data. There are 214 and 33 non-major and major crime arrests in the designated tracts, but the incidents decrease to 111 and 21 among the eligible tracts. Similarly, there are 48 and 71 percent more calls for service and police stops in the designated than in the eligible census tracts in the mean pre-intervention year.

Around 98% of the designated tracts are low-income based on the Opportunity Zones definition, while this figure goes down to 82% in the eligible group. Eligible tracts raised nearly twice the private equity investments as their designated counterparts. Still, such tracts have similar planning permits (31.6 vs. 27.7). Both groups have similar age composition and population levels. While they have similar Hispanic representation, the Black (White) population is considerably higher (lower) in the designated tracts. In addition, the treated tracts have lower education attainment as they have more high school dropouts (25.9% vs. 20.4%) and fewer college graduates (25.6% vs. 34.0%). The unemployment rate in the average

[14]See https://www.census.gov/programs-surveys/geography/technical-documentation/records-layout/2020-comp-record-layout.html

[15]See https://www.urban.org/policy-centers/metropolitan-housing-and-communities-policy-center/projects/opportunity-zones

[16]**Appendix Figure A.1** shows the eligible and designated census tracts by city. In some cities, most tracts are eligible for the program; others reflect the spatial clustering of economic resources and inequality.

designated census tract is four percentage points higher (15.6% vs. 10.9%), but both groups are above the national unemployment rate. The average poverty rate is 10 percentage points higher among the designated tracts than the eligible ones (34.3% vs. 24.8%). The family income is 14.9 thousand dollars lower (28 percent difference) in the designated than in the eligible tracts. The median gross rent (contract rent plus utilities and fuel) also presents a 200 hundred dollar difference among both groups. Finally, the single-family houses had an 8.0 percent lower appreciation in the designated than the eligible tracts, and received more small business loans.[17]

In summary, the designated communities are more disadvantaged, low-income, and crime-prevalent than the eligible but not selected census tracts. This situation is consistent with previous studies (Alm et al., 2020), finding that the Opportunity Zone selection process followed the spirit of the law as the most distressed communities, even among the low-income, were chosen to receive tax subsidies to encourage capital investments. Whether the tax incentives caused neighborhood changes is the central point of this research.

4 Empirical strategy

This research estimates the early effects of the Opportunity Zone designation on economic conditions and public safety. All the public safety outcomes are estimated in levels rather than in rates as people move around the city, making the tracts' residents not an accurate number of the people at risk (the conclusions do not change by estimating the outcomes in rates).[18] A naive estimation would regress the economic and crime outcomes on an Opportunity Zone designation indicator variable. This comparison would suggest that the program reduced the family income and increased unemployment, poverty, and crime as the designated areas are negatively selected into the treatment. A fuzzy regression discontinuity and a difference-in-differences estimation address this endogeneity bias. Moreover, employing two econometric specifications provides reliable evidence by ensuring that a methodological choice does not drive the results and allows measuring different margins of the policy. For example, the difference-in-differences method can estimate a heterogeneity analysis at the city level, and provides the average treatment effect. The regression discontinuity provides a stronger identification strategy but computes the effect for those tracts near the cut-off threshold. The empirical estimations are explained as follows.

[17] The sociodemographic and crime differences hold across cities (**Appendix Table A.1**).

[18] For example, Times Square in New York City is an example of a place with few residents but an enormous number of daily visitors, so the rates mask this relationship. There are plenty of examples like this one across the 31 cities included in this research.

4.1 Difference-in-differences

This research uses the difference-in-differences estimator to provide the causal effect of the policy by comparing the Opportunity Zones tracts to those eligible but not selected before and after the policy intervention. This model relies on the parallel trends assumption, which considers that confounders across groups are time-invariant and time-varying confounders are group-invariant. The econometric specification is as follows:

$$y_{it} = \gamma_0 + \omega_i + \sigma_t + \beta_1 D_{it} + X_{it}\alpha_X + e_{it} \tag{1}$$

where y_{it} is the outcome variable (e.g., unemployment rate, crime counts) in census tract i and year t, ω_i and σ_t are census tract and year fixed effects. X_{it} is a vector of sociodemographic controls (population and race, age, and schooling attainment composition), and α_X is the coefficient vector of such controls. The controls increase the precision of the estimates by capturing any residual error not accounted for in the model.[19] D_{it} is an indicator variable equal to one if census tract i had the Opportunity Zone designation in year t, which happened only during the post-intervention period (after 2018), zero otherwise. The standard errors are clustered at the census tract level. The main coefficient of interest, β_1, captures the effect of the Opportunity Zone designation on the selected outcome.

The difference-in-differences method can use several comparison groups to account for potential biases but face sample size trade-offs. One alternative includes all the eligible but not selected low-income tracts. While this group uses all the data, there is no guarantee that these places experienced the same trends before the intervention, facing concerns about its comparability. Another approach contrasts the designated tracts with their bordering, eligible, but not selected low-income counterparts. As the First Law of Geography asserts, this group should be more similar in unobservable characteristics as near places are more related than distant ones. A third comparison group consists of designated and eligible low-income tracts with a similar poverty rate and income ratio (poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income).[20] This comparison reduces the sample size, but the treated and control groups are more similar while reducing spillover concerns.

Using propensity-score weights balances the treated and control units so that those following different pre-trends are down-weighted. This research relies on a logit model to compute the estimated propensity score of being designated an Opportunity Zone using pre-intervention sociodemographic controls (popu-

[19] As controls are available up to 2020, the 2021 values were imputed using the 2020 figures. Excluding the controls from the regression leads to the same conclusions.

[20] The regression discontinuity design compares tracts on both sides of the eligibility threshold. The difference-in-differences comparison group only uses tracts that are above the eligibility threshold.

lation and race, age, schooling attainment, labor force, unemployment rate, crime counts, police stops, calls for service, planning permits, and small business loans). Then, these scores are used to build inverse propensity-score weights. The approach of combining propensity scores in a difference-in-differences model is common among the Opportunity Zones (Arefeva et al., 2021; Chen et al., 2023; Corinth and Feldman, 2021; Freedman et al., 2023; Sage et al., 2023) and place-based literature (Billings, 2009; Busso et al., 2013; Neumark and Young, 2019; O'Keefe, 2004) as it minimizes differences in levels and changes in pre-intervention outcomes and supports finding a representative control among observations that were eligible for the program.

4.2 Regression discontinuity

The fuzzy regression discontinuity leverages the discontinuous nature of the cut-off thresholds defining a low-income census tract. The Internal Revenue Service employed the 2011-2015 American Community Survey five-year estimates to determine the eligibility thresholds. Tracts with a poverty rate above 20 percent or below 80 percent of the greater statewide or the metropolitan area median family income were eligible for the Opportunity Zones program. The probability of designation is not zero below the cut-off thresholds because tracts were also eligible based on having less than 2,000 people or being adjacent to a low-income tract.[21] The relevant consideration for this specification is that the probability of designation changes drastically at the eligibility thresholds. Consequently, comparing tracts very close to the 20 percent poverty rate or the 80 percent family income ratio allows for estimating the causal effect of the Opportunity Zone designation. The identification assumption is that besides the change in the eligibility criteria, census tracts just above and below the poverty and income requirements are similar in all characteristics that determine economic and public safety outcomes, so only the Opportunity Zones designation explains the differences between both groups. The econometric specification is as follows, restricting the sample within a small bandwidth:

$$y_i = \alpha_0 + \beta_1 D_i + \alpha_1 f(r_i) + \alpha_2 D_i g(r_i) + X_i \alpha_X + u_i \tag{2}$$

where y_i is the mean difference between the post-intervention (2018-2024) and pre-intervention (2014-2017) outcome variable (e.g., unemployment rate, crime counts) for tract i, D_i is an indicator variable for being an Opportunity Zone tract, r_i is the running variable centered around zero. X_i is a vector of pre-intervention sociodemographic controls (population and race, age, and schooling attainment composition),

[21] Six Opportunity Zones census tracts were excluded from the analysis as they were defined as low-income based on the 2012–2016 American Community Survey estimates. The Internal Revenue Service (IRS, 2018a) allowed them as the census data was released four months before the deadline for nominating them for the program. Including these tracts does not change the results of this research.

and α_X is its coefficient vector. While the results are qualitatively similar without the controls, they increase the precision of the estimates. u_i is the error term, and the standard errors are clustered at the census tract level. The specification estimates an intent-to-treat by comparing tracts just above and below the eligibility threshold. Furthermore, as it is a fuzzy regression discontinuity, D_i is instrumented using an indicator variable of whether the tract is above the threshold, meaning that it scales the effect to account that only some census tracts were designated as Opportunity Zones so that β_1, the main coefficient of interest, provides the treatment effect on the treated. This coefficient is the average effect for areas that would not have been eligible had they been on the other side of the threshold.

There are three alternative methods for building the running variable. One method would be focusing on those tracts above the 80 percent income threshold, which are ineligible for the program unless they have a poverty level of at least 20 percent. Second, the sample was restricted to those tracts below the 20 percent poverty level that become eligible if they are below the 80 percent income. Both of these methods reduce the sample size and its statistical power, so a third approach consists in combining the poverty and income ratio threshold into a single standardized running variable following Corinth and Feldman (2021): $r_i = \max\{\frac{P_i - 20}{20}, -\frac{I_i - 0.8 * I_{m,s}}{0.8 * I_{m,s}}\}$, where P_i is the poverty rate and I_i is the median family income of tract i, while $I_{m,s}$ is the greater statewide or metropolitan median family income. This running variable measures the distance to the eligibility threshold, becoming positive whenever the two thresholds become binding.

5 Results

This section presents the estimates on economic neighborhood changes –measured by poverty, employment, gross rent, planning permits, family income levels–, and public safety –comprised of calls for service, police stops, crimes, and arrests incidents. The public safety outcomes are estimated in levels. The difference-in-differences results are presented first, followed by the regression discontinuity design.

5.1 Difference-in-differences results

The difference-in-differences estimates allow studying the Opportunity Zones' impacts over time. A crucial assumption to obtain causal effects is that the control and treatment groups would have followed the same trend absent the Opportunity Zones program. An event study design allows rejecting this assumption by examining any pre-intervention trends. **Figure 2** presents the yearly point estimates and confidence intervals for the economic outcomes using all the eligible tracts as a comparison group. The gross rent, family income, and unemployment rate decreased before the law changed, while the planning permits and house price index had an upward pre-policy trajectory. Even after controlling for time-invariant individual effects, time-specific events affecting all tracts, and sociodemographic variables, there is self-selection in the treatment, so one cannot rule out that the impacts were not due to factors unrelated to the Opportunity Zone designation. To address these concerns, **Figures 3** and **4** present the propensity score weighted event study estimates on the economic and public safety outcomes. Under these specifications, no evidence suggests that the parallel trends do not hold for any of the 14 variables.[22]

Table 2 presents the difference-in-differences point estimates on economic outcomes. Columns (1) and (2) present the baseline estimators using two alternative comparison groups (the eligible and bordering samples). These results suggest that the program significantly raised planning permits by 10 percent (2.7 additional permits). In comparison, it reduced the unemployment rate by around 10 percent (1.4 percentage points) and the family income by 2.7 to 6.3 percent (1.1 to 2.8 thousand dollars). Column (3) uses the baseline difference-in-differences on the similar tracts sample (± 15 percentage points from the threshold), revealing that only the equity investments and housing prices remained statistically significant. However, as the parallel trends assumption does not hold for these specifications, these estimates do not solely reflect the effect of the Opportunity Zones program but also the self-selection into treatment.

The propensity score weighting in a difference-in-differences setting in Columns (4), (5), and (6) addresses the selection bias as the parallel trends assumption holds for this specification. The results suggest that private equity from individual and institutional investors may have increased by half a million dollars annually or a 60 to 80 percent change. There is also some evidence that the number of planning permits issued to make urban developments changes increased by 10 to 17 percent (2.6 to 4.1 more building permits annually per census tract), though it is imprecisely estimated.

Investment and urban development changes may translate into better quality of life outcomes. Moreover, to the extent that property owners and investors believe that the Opportunity Zones will foster economic growth, we may expect an increase in rents and higher property prices. However, the matching difference-in-differences estimates show that most socioeconomic outcomes are not statistically significant. Only the family income has a significant effect, but the coefficient flips its sign under alternative samples. Other outcomes also flip sign. Overall, the evidence suggests that the effects of the Opportunity Zone may have increased the capital raised by large investors and the number of building permits, though it has not translated into more loans for small businesses, nor a change in the property prices, rents, family income, poverty, and unemployment.

[22] **Appendix B** shows the event study design estimates for the two other alternative samples, showing that the propensity scores contribute to having a better comparison group.

Despite the limited socioeconomic changes, the Opportunity Zones may impact public safety as urban development and private equity seem to have grown. Likewise, a change in the expectations of the residents, developers, and business owners about future neighborhood conditions may impact local safety even if the investments have not realized socioeconomic gains for residents. **Table 3** provides the difference-in-differences point estimates on public safety.[23] Using the baseline model, Columns (1), (2), and (3) suggest a significant decrease in police stops and non-major crime arrests. These results have a self-selection bias as the parallel trends assumption does not hold for these specifications. Once addressing this concern using the propensity score weighted difference-in-differences design in columns (4), (5), and (6), almost none of the public safety estimates show a significant change. The significant result of non-major crimes is likely a false discovery rate. Furthermore, the sign change across specifications reinforces the idea that there are no effects on public safety due to the Opportunity Zones program.

5.2 Robustness

One concern with using difference-in-differences is that the eligible but not selected tracts may differ in unobservable characteristics even after down-weighting units violating the parallel trend assumption. The design of the intervention provides an alternative method to measure the causal impact by comparing tracts near the eligibility threshold in a regression discontinuity framework.

The regression discontinuity leverages the poverty and income thresholds eligibility for the program. **Figure 5** presents how these eligibility thresholds discontinuously change the probability of being an Opportunity Zone tract. Panel A shows that 42.6 percent of the tracts are eligible based on the two thresholds (poverty rate above 20 percent and below 80 percent of the greater statewide or the metropolitan area median family income), and around one of every three tracts with these characteristics were selected as Opportunity Zones. In contrast, only eligible tracts based on a single criterion represent 17.2 percent of all tracts, and only one of every eight was designated as Opportunity Zones. Very few tracts (seven of every thousand) were selected for the program while not satisfying the poverty or income thresholds as their population or adjacency to other low-income tracts made them eligible. Panel B confirms that the probability of being an Opportunity Zone changes drastically near the cut-off thresholds by around seven percentage points.

Appendix C shows the regression discontinuity estimates on economic and public safety conditions. Despite the drastic change in the probability of being designated an Opportunity Zone, there are no

[23]Not every city reported all public safety outcomes, so the number of cities included in the sample is reported on the regression tables of public safety.

significant socioeconomic and public safety changes at the neighborhood level. Equity investments and planning permits are no longer statistically significant, suggesting that the average treatment effect for eligible tracts may not be the same as the local average treatment effect for those at the margin of being designated as Opportunity Zones. The null impact on sociodemographic changes is not due to a methodological choice. Instead, the capital investment place-based intervention program has had limited effects on localized socioeconomic conditions even after seven years of its implementation.

5.3 Heterogeneity

One concern is whether the suggested impacts on equity investments and building permits are detectable across all cities. **Appendix D** presents the difference-in-differences estimates by city using the eligible sample. The changes on equity investments and urban development are not constant across cities, as some seem to have more limited effects. This situation could be consistent with investments being attracted only to certain areas, although city-specific point estimates have to be taken cautiously due to sample size limitation. Next, there are no consistent impacts on the socioeconomic and public safety outcomes across cities.

Another concern is that the lack of significant results on aggregated crime and arrests could hide public safety impacts on specific crime categories more prone to neighborhood conditions or changes in the residents' expectations about future conditions. **Appendix E** exhibits the regression discontinuity, and difference-in-differences estimates on murder, robbery, and aggravated assault (which comprise the violent crimes), and burglary, theft, and motor vehicle theft (defined as property crimes). While some crime changes may have occurred on specific outcomes, the lack of consistent, significant results suggests limited impacts on the crime subcategories. If anything, there may have been an increase in burglary (8 percent), which could relate to more urban development, though the estimate is not significant in the regression discontinuity method.

Finally, previous evidence suggests spatial concentration of the Opportunity Zone investments. **Table 4** explores this idea using a triple differences framework. It examines whether there are any differential impacts among tracts that, previous to the intervention, were receiving most of the private equity investments (top five percent). The analysis reveals mostly null impacts and lack of a consistent sign of the estimates across the three different subsamples and economic outcomes using the propensity score weighted specifications.

6 Discussion

Despite a strong rationale for implementing place-based interventions, the Opportunity Zones program shows limited impacts on socioeconomic and public safety conditions after seven years of implementation among the 31 cities included in this study. However, there is some suggestive evidence that institutional investments and urban development may have increased. This section discusses possible explanations for these results.

There are disagreements on the appropriate geographic scale of place-based interventions. Neighborhood-level policies may only reallocate jobs within the local labor market (usually, the city or metropolitan area). They could cause gentrification and residents' displacement (Bartik, 2020a,b). Counterarguments say these concerns may be overestimated. Policies should strive to develop disadvantaged neighborhoods due to potential positive externalities and multipliers from local hiring and better infrastructure (Neumark, 2020a,b). Policymakers have chosen the census tract as the standard intervention unit for previous national place-based programs (Empowerment Zones, Renewal Communities, and New Markets Tax Credit). Still, state-level interventions (Enterprise Zones) have been more flexible regarding geographical boundaries. In both cases, the evidence on economic impacts is mixed, suggesting that the unit of analysis is relevant. Still, it is unlikely to be the sole reason explaining the lack of significant results.

Another consideration is the features of the intervention. The program may not have provided the necessary tax incentives to foster a widespread, meaningful change in investing patterns. Qualitative evidence from qualified opportunity funds' representatives reports that the program's tax incentives are not generous enough to make an unprofitable project a financially sound investment. However, it increases the returns of profitable investments, making them more competitive compared to alternative opportunities (GAO, 2021). For comparison, the tax benefits, among other policies, of the New Markets Tax Credit provide up to a 39 percent tax credit on an investment in a low-income community. In contrast, the Opportunity Zones program reduces the taxpayer's basis at most 15 percent of the original investment, while most benefits come from capital gains. Hence, non-profitable, high-uncertainty investments may find it more challenging to accrue the value of the tax incentive.

Moreover, the Opportunity Zones do not have any agency regulating the investments besides the Internal Revenue Service compliance plan. While this feature was intended to remove regulatory barriers and complex structures, it misses components that could have been relevant to fostering community investments. For example, the New Tax Credit Market must have resident representation on governing or advisory boards to keep community accountability. This last program, along with the Empowerment

Zones and Enterprise Zones programs work under a competitive application process reviewed by regulatory agencies,[24] rather than providing the tax benefits to all investments as in the Opportunity Zone initiative. This feature may dilute the benefits and increase the cost of the program; a concern raised to previous programs in the US and Europe (Greenbaum and Bondonio, 2004). Similarly, scholars argue that effective place-based interventions should include subsidies for job creation and residents' skills improvements (Bartik, 2020a,c; Neumark, 2020a,b) and consider the physical design of places and neighborhood engagement (MacDonald et al., 2019). Such components were not present in the Opportunity Zones program and could have made it more challenging to encourage new jobs and neighborhood change.

Another explanation could be that seven years is still early to assess the economic and public safety conditions of a policy encouraging long-term investments (around eight years as it is the period to defer paying a considerable portion of the taxes). This situation is particularly relevant if companies created a two-tiered investment process (GAO, 2021), where first they invested in a qualified opportunity business to take advantage of the 180-day restriction to defer their capital gains taxes. Then, they use the 30-month grace period to deploy the resources in a physical property. This process means companies have up to three years to improve an urban development project. However, the event study design shows no drastic changes in the fourth post-intervention year. If the treated areas have not shown any differential effects up to this year, particularly in forward-looking variables (e.g., small business loans and property prices), it is likely the investors and developers are not expecting a large impact on the local activity in the near future due to the program.

Next consideration is the role of the unprecedented COVID pandemic that could have affected the program's influence. The pandemic could have biased the estimates,[25] but most likely restricted the amount of investment a community would receive. During a contraction period, investors are more hesitant to invest in risky projects or could demand a higher investment return. Consequently, the Opportunity Zones and the pandemic are reminders that programs with medium- to long-term goals face challenges outside policymakers' control that could limit their impacts. Hence, combining short- and long-term initiatives is critical to improving community public safety.

A final consideration is measuring the relevant estimand in the policy evaluation. This research estimates the impact of the Opportunity Zones designation (an intent-to-treat effect). If one were to have taxpayer data on the investments made in all qualified opportunity funds, then it would be possible to

[24] See https://www.hud.gov/hudprograms/empowerment_zones and https://www.cdfifund.gov/sites/cdfi/files/documents/2020-introduction-to-the-nmtc-program_-final.pdf

[25] To bias the estimates, the pandemic needed to affect the census tracts differently beyond their Opportunity Zones designation. Moreover, to the extent that local officials provided incentives to keep the investments flowing towards the Opportunity Zones tracts during the pandemic, it would be part of the treatment rather than a source of bias.

compute the program's impact on those designated tracts that received an investment compared to what those tracts would have experienced otherwise (treatment-on-the-treated). The difference between the intent-to-treat and treatment-on-the-treated increases as fewer census tracts receive investments (a low take-up rate). Investment concentration is common among programs, and the Opportunity Zone is no exception to this trend as investors may aim to target their resources in a few capital-intensive projects as early evidence on tax records seems to suggest (Kennedy and Wheeler, 2021). Hence, future research should continue evaluating this policy as data becomes available.

7 Concluding remarks

Recent US protests against police use of force and racism have prompted the exploration of alternatives to law enforcement and sentencing for crime prevention. In addition, the pervasiveness of unfading concentrated urban poverty, characterized by lack of jobs and social isolation, continues to affect disproportionately racial minorities. Place-based interventions aiming to spur economic growth and job creation in distressed communities are usually designed as widespread, scalable, and adaptable initiatives. Still, there is limited research assessing their impact on public safety. Enacting the Opportunity Zones program at a moment when georeferenced, time-stamped public safety data has become more accessible to researchers allows for providing some evidence on the topic.

This research employs a regression discontinuity design and a difference-in-differences estimator to assess the impacts of the Opportunity Zones program in 31 major US cities seven years after its implementation. There is some suggestive evidence that subsidizing capital tax investments increase equity investments and urban development, but it did not lead to sociodemographic changes –measured by property prices, poverty, employment, and income– nor improve public safety –comprised of calls for service, police stops, crimes, and arrests. There are a few heterogeneous impacts among individual US cities, but there are no consistent impacts on property or violent crimes and arrests.

More research is needed to assess whether place-based capital investments in distressed communities improve public safety. As more data becomes available, future research should continue evaluating the Opportunity Zone program. Furthermore, as neighborhood investments usually happen due to a public program, this research is relevant for assessing whether these policies work. Beyond considering the appropriate geographical unit, the components and magnitude of the incentives, and the time horizon of the intervention, the fiscal cost is also an essential factor in the policy design. To be clear, from a policy evaluation perspective, only the new investments that the program encouraged are relevant. However, from a budgetary approach, the new investments and those that would have occurred even in the absence of the program but now are taxed at a lower rate are part of the program's fiscal cost.

Finally, this study is not without limitations. Place-based capital investment interventions may lead to economic growth, job creation, and public safety improvements in the long term. However, the infusion of financial resources has to be enough to leverage the agglomeration effects and unlock potential multipliers from local hiring. It should also create better physical infrastructure or at least change the expectations of residents, developers, and business owners about future improvements in the neighborhood and their willingness to intervene in solving common problems. However, this study suggests that the most recent national place-based program fostering private capital investments in low-income areas is a limited alternative to influence long-term community changes and improve public safety. These results do not imply that policymakers should abandon the idea of place-based interventions aiming to encourage private investments in low-income neighborhoods, particularly in the inner-city pockets of distress. On the contrary, these interventions are an opportunity to improve communities, public safety included. Still, their features, incentives, policy design, and fiscal cost are crucial to creating effective mechanisms to realize its advantages and create safer neighborhoods. Investing resources in communities is a necessary but insufficient to improve public safety unless it targets components with a clear nexus to crime.

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Table 1: Descriptive statistics, pre-intervention census tract year data

	Designated		Eligible	
	Mean	Std. Dev	Mean	Std. Dev
Non-Major crimes	305.9	342.8	211.8	252.4
Major crimes	175.5	165.0	134.4	170.6
Violent	47.4	47.4	28.5	32.6
Murder	0.9	1.7	0.5	1.2
Robbery	18.6	19.9	11.7	14.5
Aggravated assault	28.1	30.7	16.5	20.5
Property	128.2	133.4	105.8	149.9
Burglary	24.6	23.8	21.1	23.2
Theft	84.1	104.9	70.2	127.5
Motor vehicle theft	19.4	23.5	14.5	17.5
Non-major crime arrests	213.8	357.5	111.7	168.5
Major crime arrests	32.8	51.4	21.1	42.0
Violent	18.3	31.4	10.6	21.3
Murder	0.7	2.9	0.3	1.8
Robbery	5.3	13.0	3.1	9.5
Aggravated assault	12.4	18.7	7.1	11.6
Property	14.5	27.8	10.5	27.7
Burglary	3.1	6.6	2.2	5.3
Theft	9.4	23.5	7.2	24.5
Motor vehicle theft	1.9	3.4	1.1	2.1
Calls for service	3,519.8	6,611.4	2,475.9	3,085.2
Police stops	507.9	1,036.8	282.1	932.8
Low-income tract (%)	98.3	13.0	82.3	38.1
Contiguous tract (%)	1.7	13.0	17.7	38.1
Equity investments (millions)	0.8	5.3	1.6	21.8
Planning permits	31.6	53.4	27.7	52.0
Population (thousands)	3.6	2.0	3.8	1.8
White (%)	31.2	24.8	45.2	28.4
Black (%)	44.7	34.5	30.3	32.0
Hispanic (%)	28.4	28.2	26.2	26.5
Age 0-14 (%)	20.4	7.5	18.3	7.3
Age 15-24 (%)	16.4	9.2	15.5	10.0
Age 25-39 (%)	23.7	7.4	25.0	8.4
Age 40-54 (%)	18.6	4.5	18.8	4.8
Age 55+ (%)	20.9	8.0	22.4	8.6
Less than high school (%)	25.9	12.4	20.4	12.9
High school (%)	28.9	9.0	26.8	10.1
Some college (%)	19.6	6.6	18.8	6.9
College+ (%)	25.6	14.6	34.0	18.8
Unemployment rate (%)	15.6	8.7	10.9	6.6
Family income (thousands)	39.0	18.1	53.9	25.6
Poverty rate (%)	34.4	13.7	24.8	12.9
Gross rent (thousands)	0.9	0.3	1.1	0.3
House price index (Y2000=100)	246.3	162.5	268.0	164.6
Small business loans (thousands)	847.5	1,924.5	752.2	1,522.2

Notes: Pre-intervention (2014-2017) census tract level mean and standard deviation from the 31 US included in the study. Some cities do not report arrest, calls for service, police stops, or planning permits data. The designated group is the Opportunity Zones census tracts (N = 1,274). The eligible group comprises the low-income eligible but not designated tracts (N = 4,357). Major crimes include murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft. Non-major crimes refer to all the other crimes reported to the police departments.

Table 2: Difference-in-differences estimates of the Opportunity Zones designation on economic outcomes

	DiD (1)	DiD (2)	DiD (3)	PSM-DiD (4)	PSM-DiD (5)	PSM-DiD (6)
A. Equity investments (millions)						
Treatment*Post	0.250**	0.230*	0.677***	0.519**	0.201	0.481*
	(0.121)	(0.120)	(0.254)	(0.248)	(0.333)	(0.271)
Mean dep. var.	0.9	0.6	0.6	0.9	0.6	0.6
Observations	52,739	33,791	131,648	52,739	33,791	131,648
B. Small business loans (thousands)						
Treatment*Post	0.928	11.234	58.276	31.197	33.486	63.688
	(19.472)	(16.895)	(48.507)	(19.835)	(20.611)	(39.482)
Mean dep. var.	685.5	650.2	720.9	685.5	650.2	720.9
Observations	47,939	30,716	119,680	47,939	30,716	119,680
C. Planning permits						
Treatment*Post	2.672***	1.544	3.325	2.681**	1.187	4.143
	(0.992)	(1.052)	(2.991)	(1.069)	(2.449)	(2.610)
Mean dep. var.	26.5	29.1	23.9	26.5	29.1	23.9
Observations	49,546	31,755	123,288	49,546	31,755	123,288
D. House price index (Y2000=100)						
Treatment*Post	10.373	5.519	18.681*	0.118	0.149	−4.371
	(6.523)	(4.784)	(10.730)	(6.355)	(5.019)	(12.252)
Mean dep. var.	206.7	244.6	285.2	206.7	244.6	285.2
Observations	2,330	9,045	4,421	2,330	9,045	4,421
E. Gross rent (thousands)						
Treatment*Post	−0.027***	−0.009	−0.020	−0.002	0.003	−0.021
	(0.005)	(0.006)	(0.015)	(0.007)	(0.007)	(0.021)
Mean dep. var.	1.0	0.9	1.1	1.0	0.9	1.1
Observations	52,339	33,536	131,131	52,339	33,536	131,131
F. Family income (thousands)						
Treatment*Post	−2.867***	−1.141**	−1.151	−1.307*	0.045	−0.676
	(0.430)	(0.477)	(1.169)	(0.689)	(0.686)	(1.302)
Mean dep. var.	44.9	41.5	57.8	44.9	41.5	57.8
Observations	51,508	32,910	130,834	51,508	32,910	130,834
G. Poverty rate (0.01=1%)						
Treatment*Post	−0.005**	−0.003	0.005	0.005*	0.004	0.005
	(0.002)	(0.003)	(0.005)	(0.003)	(0.003)	(0.005)
Mean dep. var.	0.3	0.3	0.2	0.3	0.3	0.2
Observations	52,723	33,775	131,648	52,723	33,775	131,648
H. Unemployment rate (0.01=1%)						
Treatment*Post	−0.014***	−0.011***	−0.0005	0.002	0.002	0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
Mean dep. var.	0.1	0.1	0.1	0.1	0.1	0.1
Observations	52,717	33,770	131,637	52,717	33,770	131,637
Eligible sample	X	-	-	X	-	-
Border sample	-	X	-	-	X	-
Similar sample	-	-	X	-	-	X

Notes: Estimates of the Opportunity Zones designation on selected outcomes. Robust standard errors clustered at the census tract level in parentheses. Columns (1), (2), and (3) use the difference-in-differences (DiD) estimation. Columns (4), (5) and (6) employ a propensity score weighting in a difference-in-differences (PSM-DiD) model. Columns (1) and (4) include the low-income, eligible and designated census tracts, Columns (2) and (5) consider the low-income designated and their bordering, eligible census tracts. Columns (3) and (6) use the low-income, eligible and similar tracts (with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income). *p<0.1; **p<0.05; ***p<0.01.

Table 3: Difference-in-differences estimates of the Opportunity Zones designation on public safety

	DiD (1)	DiD (2)	DiD (3)	PSM-DiD (4)	PSM-DiD (5)	PSM-DiD (6)
A. Calls for service						
Treatment*Post	193.81	165.87	430.97	11.16	49.09	90.62
	(255.38)	(258.26)	(337.49)	(130.15)	(163.36)	(119.19)
Mean dep. var.	2,810	3,036	2,132	2,810	3,036	2,132
Cities	13	13	13	13	13	13
Observations	16,302	10,923	3,706	16,302	10,923	3,706
B. Police stops						
Treatment*Post	-52.21^{**}	-26.40	-19.60	-0.32	8.57	-15.80
	(23.27)	(26.17)	(26.88)	(15.46)	(21.37)	(16.99)
Mean dep. var.	359	439	178	359	439	178
Cities	18	18	18	18	18	18
Observations	37,516	23,531	8,886	37,516	23,531	8,886
C. Non-major crimes						
Treatment*Post	-7.84	-6.97	22.57	2.38	1.10	11.94^{*}
	(5.02)	(5.39)	(18.57)	(3.96)	(4.81)	(6.92)
Mean dep. var.	244	270	183	244	270	183
Cities	29	29	29	29	29	29
Observations	49,764	31,522	11,665	49,764	31,522	11,665
D. Major crimes						
Treatment*Post	3.73	2.30	5.71	4.46^{*}	0.44	3.18
	(2.45)	(2.73)	(4.71)	(2.71)	(3.30)	(3.62)
Mean dep. var.	146	160	116	146	160	116
Cities	31	31	31	31	31	31
Observations	51,819	33,082	12,015	51,819	33,082	12,015
E. Non-major crime arrests						
Treatment*Post	-34.55^{***}	-28.01^{***}	-14.60^{*}	-5.21	-4.25	4.07
	(5.28)	(5.54)	(8.05)	(4.29)	(5.33)	(5.38)
Mean dep. var.	142	167	97	142	167	97
Cities	18	18	18	18	18	18
Observations	40,826	25,476	9,636	40,826	25,476	9,636
F. Major crime arrests						
Treatment*Post	-0.76	-0.65	-2.00	0.26	-0.33	0.86
	(0.82)	(0.86)	(2.12)	(0.88)	(0.85)	(1.34)
Mean dep. var.	25	27	17	25	27	17
Cities	18	18	18	18	18	18
Observations	40,952	25,548	9,666	40,952	25,548	9,666
Eligible sample	X	-	-	X	-	-
Border sample	-	X	-	-	X	-
Similar sample	-	-	X	-	-	X

Notes: Estimates of the Opportunity Zones designation on selected outcomes. Robust standard errors clustered at the census tract level in parentheses. The number of cities reporting the outcome is included. Columns (1), (2), and (3) use the difference-in-differences (DiD) estimation. Columns (4), (5) and (6) employ a propensity score weighting in a difference-in-differences (PSM-DiD) model. Columns (1) and (4) include the low-income, eligible and designated census tracts. Columns (2) and (5) consider the low-income designated and their bordering, eligible census tracts. Columns (3) and (6) use the low-income, eligible and similar tracts (with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income). Major crimes include the six-part I Uniform Crime Reporting categories: murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft (rape is excluded). Non-major crimes refer to all the other crimes reported to the police departments. *p<0.1; **p<0.05; ***p<0.01.

Table 4: Triple difference estimates of the Opportunity Zones designation on economic outcomes

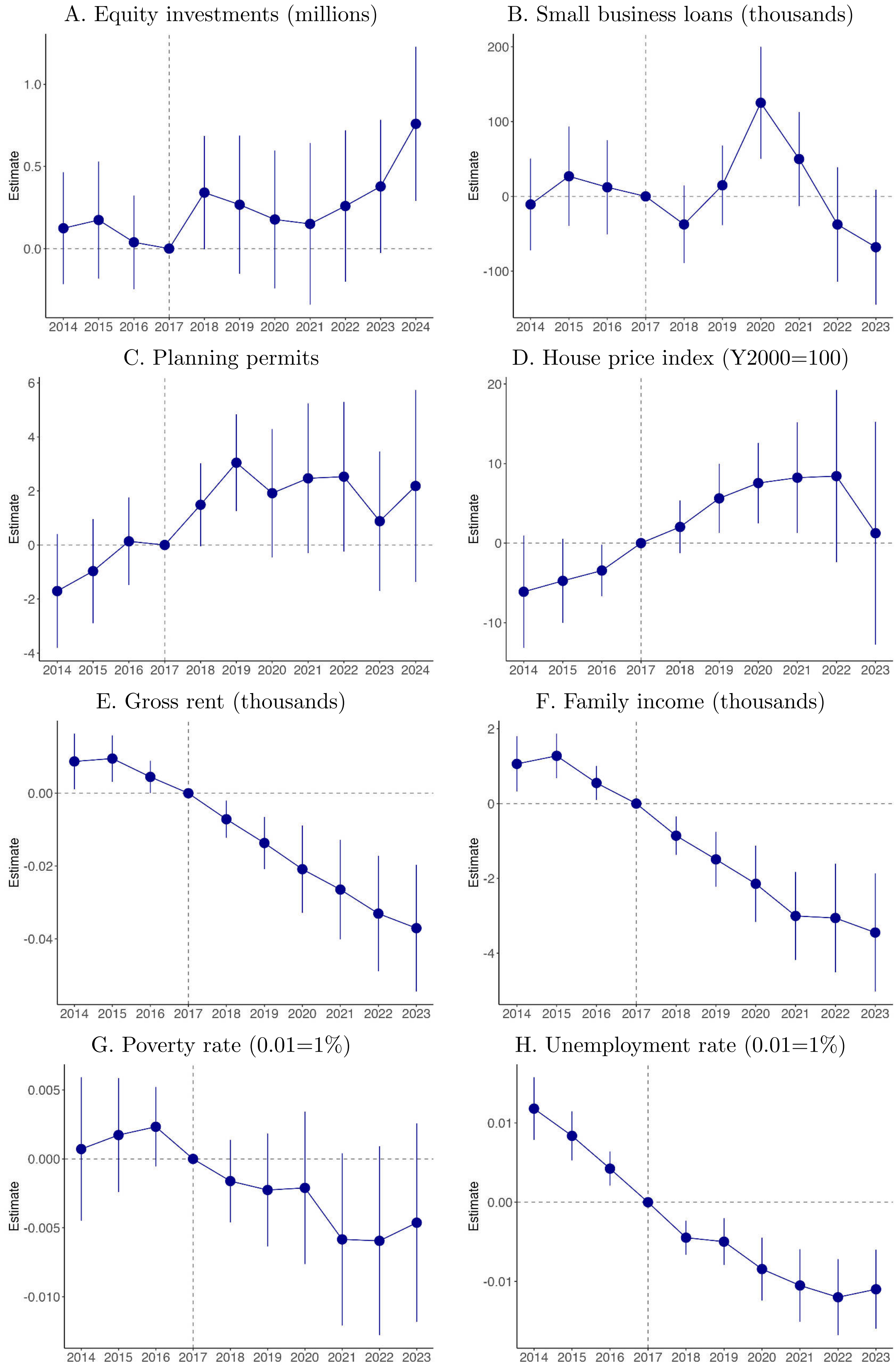
	PSM-TD	PSM-TD	PSM-TD
	(1)	(2)	(3)
A. Equity investments (millions)			
Treatment*Post*Top5	3.954**	0.166	3.246
	(1.852)	(3.210)	(2.525)
Mean dep. var.	0.9	0.6	0.6
Observations	52,739	33,791	11,968
B. Small business loans (thousands)			
Treatment*Post*Top5	121.648	121.120	317.035*
	(131.287)	(132.671)	(180.874)
Mean dep. var.	685.5	650.2	720.9
Observations	47,939	30,716	10,880
C. Planning permits			
Treatment*Post*Top5	1.636	−9.637	0.763
	(5.342)	(22.909)	(12.435)
Mean dep. var.	26.5	29.1	23.9
Observations	49,546	31,755	11,208
D. House price index (Y2000=100)			
Treatment*Post*Top5	2.898	21.498	17.423
	(14.162)	(15.500)	(25.578)
Mean dep. var.	249.1	244.6	285.2
Observations	15,104	9,045	4,421
E. Gross rent (thousands)			
Treatment*Post*Top5	−0.005	0.050	−0.019
	(0.027)	(0.046)	(0.050)
Mean dep. var.	1.0	0.9	1.1
Observations	52,339	33,536	11,921
F. Family income (thousands)			
Treatment*Post*Top5	−4.081	5.636	3.947
	(3.725)	(4.181)	(4.253)
Mean dep. var.	44.9	41.5	57.8
Observations	51,508	32,910	11,894
G. Poverty rate (0.01=1%)			
Treatment*Post*Top5	0.005	−0.005	0.010
	(0.009)	(0.012)	(0.013)
Mean dep. var.	0.3	0.3	0.2
Observations	52,723	33,775	11,968
H. Unemployment rate (0.01=1%)			
Treatment*Post*Top5	−0.0002	0.011	0.007
	(0.009)	(0.014)	(0.009)
Mean dep. var.	0.1	0.1	0.1
Observations	52,717	33,770	11,967
Eligible sample	X	-	-
Border sample	-	X	-
Similar sample	-	-	X

Notes: Triple difference estimates of the Opportunity Zones designation in tracts with highest pre-intervention investment levels following $y_{it} = \gamma_0 + \omega_i + \sigma_t + \beta_1 D_{it} + \beta_2 Post_t Top5_i + \beta_3 D_{it} Top5_i + X_{it}\alpha_X + e_{it}$, where $Post_t$ is a post-intervention period (after 2018) indicator variable and $Top5$ is also an indicator variable for being in the top five percent of the pre-intervention investment level in the city. The remaining coefficients are explained in the main text. The table shows β_3. Robust standard errors clustered at the census tract level in parentheses. Columns (1), (2), and (3) employ a propensity score weighting in a triple difference (PSM-TD) model. Column (1) includes the low-income, eligible and designated census tracts, Columns (2) considers the low-income designated and their bordering, eligible census tracts. Columns (3) uses the low-income, eligible and similar tracts (with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income). *p<0.1; **p<0.05; ***p<0.01.

Figure 1: Major cities included in the research

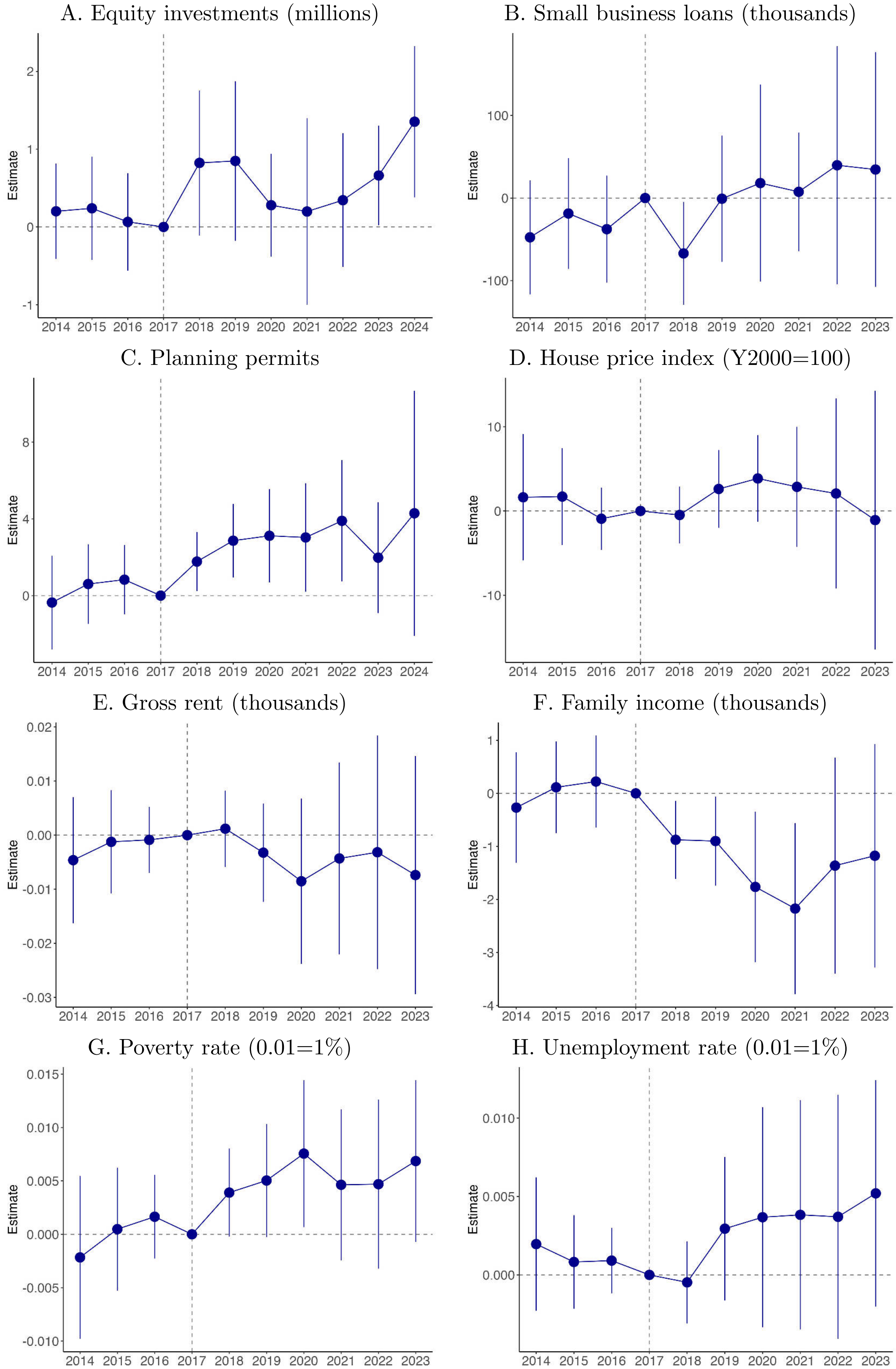
Notes: The map shows the location of the 31 US major cities included in this research. To include a city, they must have public crime data that could be aggregated to the census tract year level and at least one dataset on arrests, calls for service, police stops, or planning/construction permits that could also be computed at the census tract year level. See **Appendix ??** for a detailed description.

Figure 2: Event study estimates on economic outcomes, eligible sample



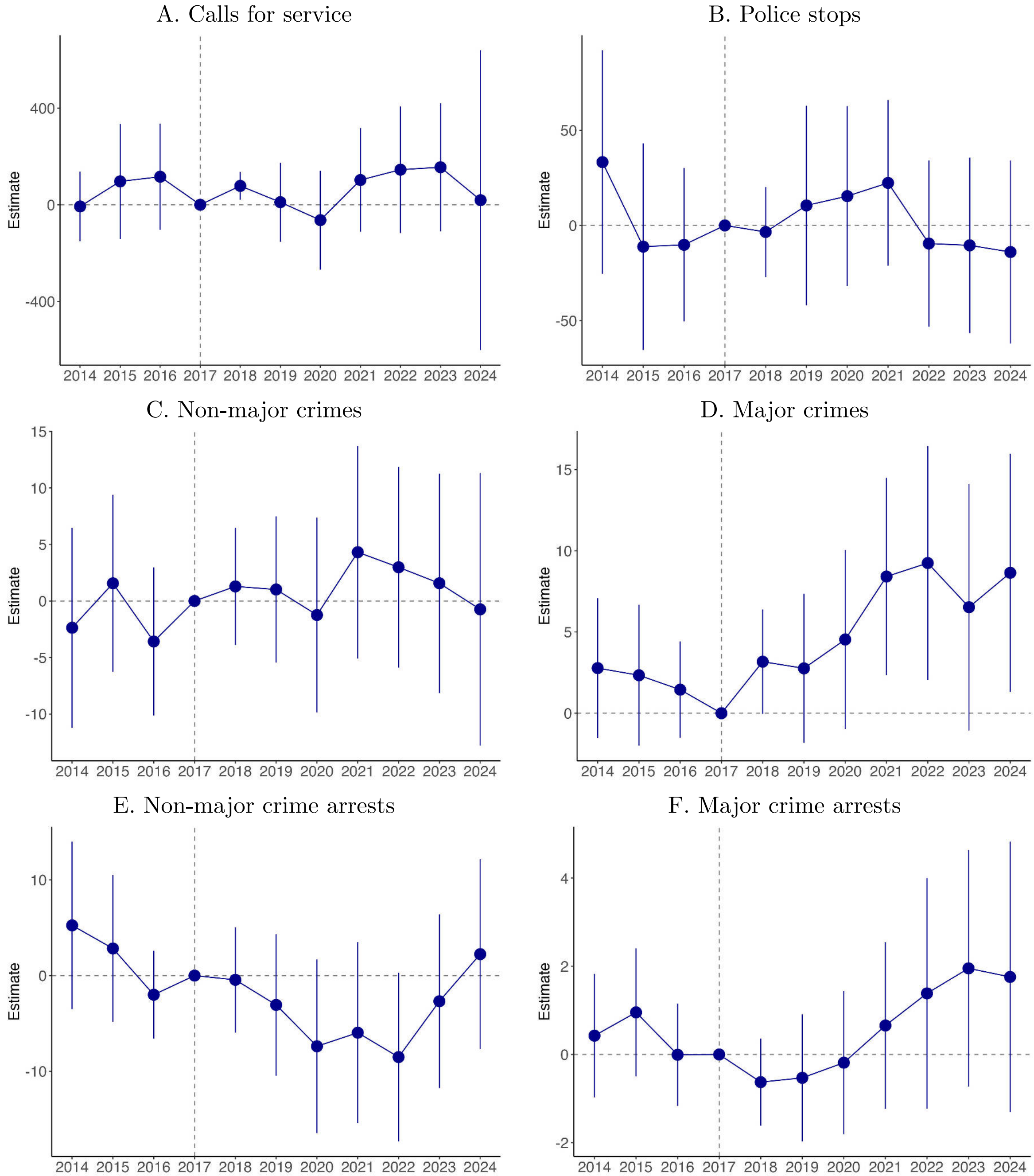
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income, eligible census tracts sample. This specification does not control for self-selection, so the parallel trends do not hold for several outcomes.

Figure 3: Propensity score weighted event study estimates on economic outcomes, eligible sample



Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity-score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income, eligible census tracts sample.

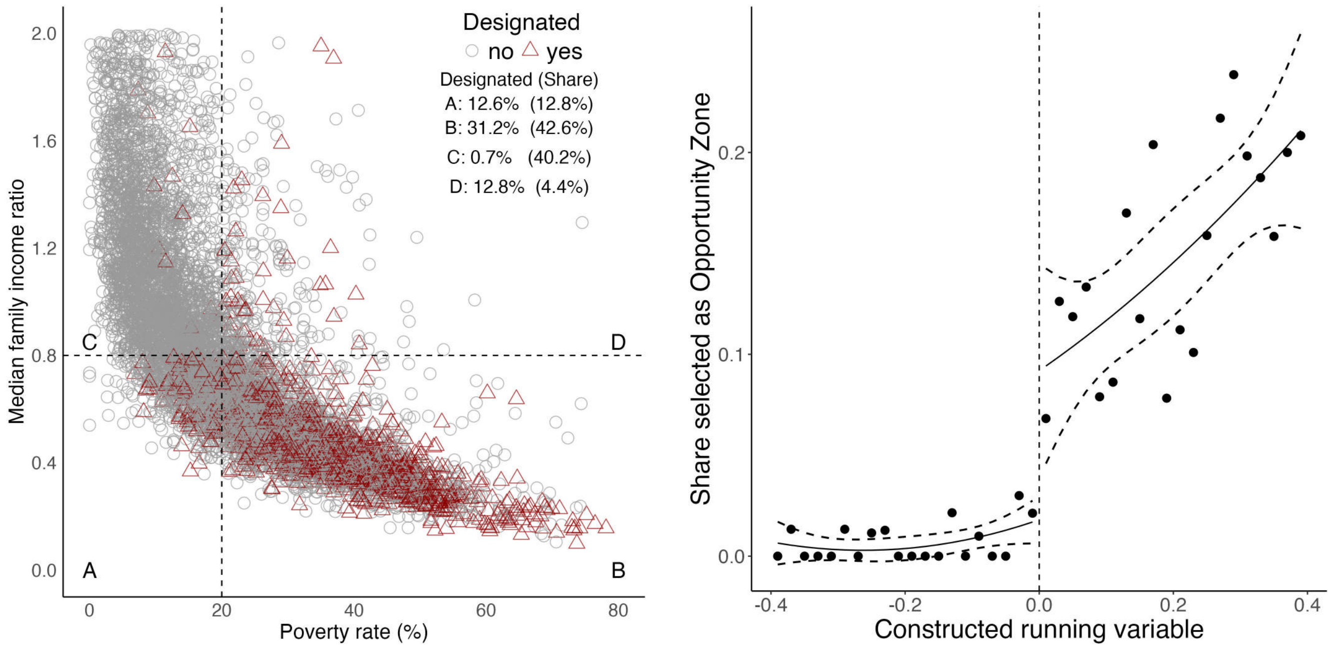
Figure 4: Propensity score weighted event study estimates on public safety, eligible sample



Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity-score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income, eligible census tracts sample.

Figure 5: Poverty and Family Income ratio on a regression discontinuity design

A. Poverty and Family Income ratio B. Regression discontinuity



Notes: Panel A presents the poverty rate and tract to the greater statewide or metropolitan family income ratio and its respective cut-off thresholds; the plot excludes tracts above the 80 percent poverty rate or with a median family income ratio above 2 but are included in the statistics on the upper right referring to the percent of tracts designated as Opportunity Zones and the share of tracts on each quadrant. Panel B presents the share of tracts selected as Opportunity Zones using the constructed running variable bins, second-order polynomials (solid line), and 95 percent confidence intervals (dash lines) around a 0.4 bandwidth.

ONLINE APPENDIX

A Appendix: City-specific descriptive statistics

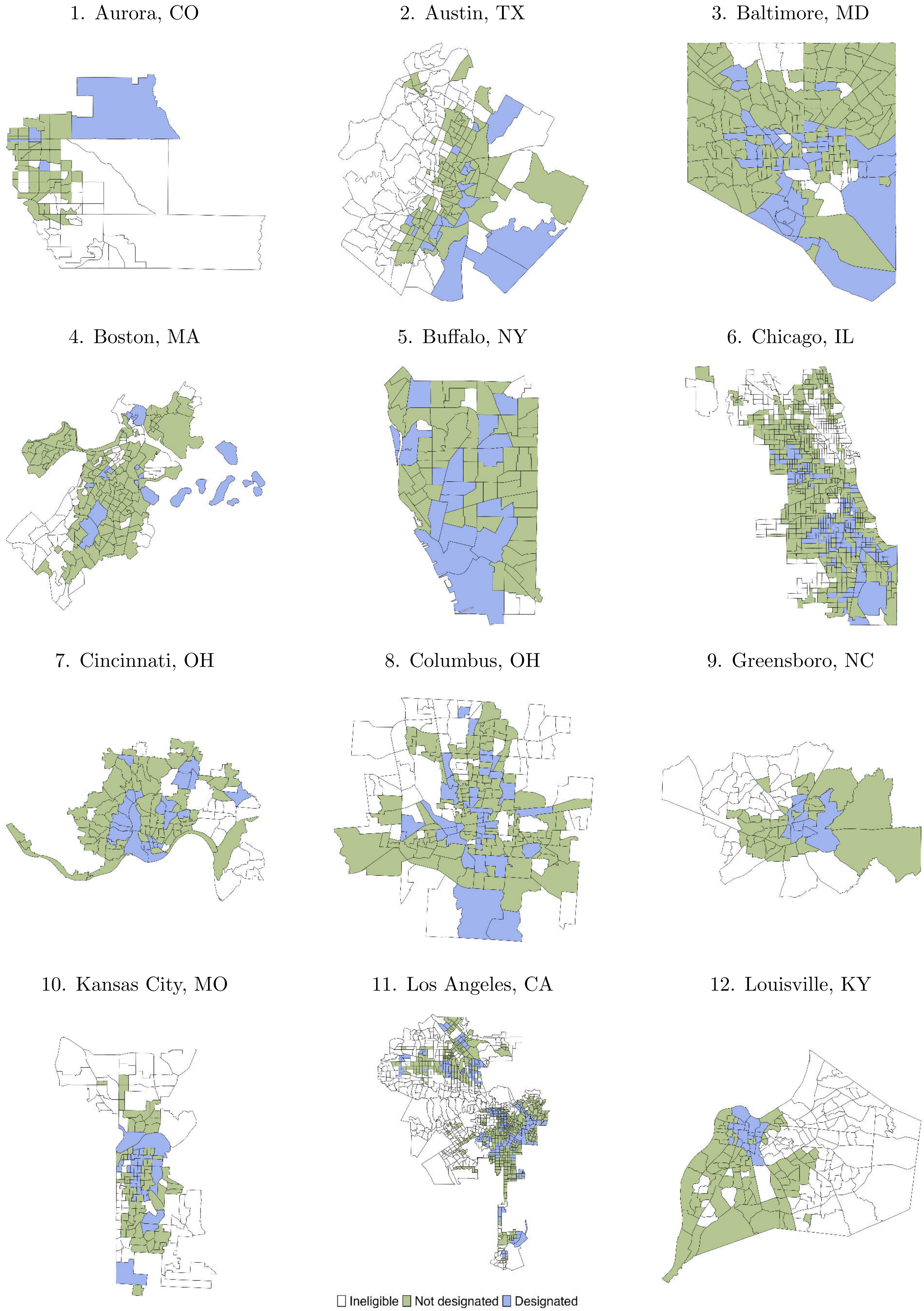
Table A.1: Descriptive statistics by city and treatment group

	Major crimes	Major crimes arrests	Calls for service	Police stops	Unempl rate (%)	Family income ($K)	Planning permits
Aurora, CO (E)	119.8	15.0		481.3	9.2	52.5	40.4
Aurora, CO (D)	218.1	43.2		618.7	8.0	48.6	70.3
Austin, TX (E)	241.4	7.5	3,286.7	190.0	6.2	55.8	85.7
Austin, TX (D)	188.2	3.3	2,840.4	149.8	10.3	45.6	82.1
Baltimore, MD (E)	179.2	12.5	4,551.8		13.2	56.4	182.8
Baltimore, MD (D)	266.3	25.0	8,376.8		18.0	38.6	191.6
Boston, MA (E)	110.6				9.7	62.4	30.4
Boston, MA (D)	56.2				19.3	39.0	14.4
Buffalo, NY (E)	194.6			635.9	11.9	45.1	13.5
Buffalo, NY (D)	283.2			661.8	13.3	36.4	25.9
Chicago, IL (E)	126.8	14.8		125.5	13.4	54.1	12.8
Chicago, IL (D)	181.0	20.2		249.4	27.0	31.4	11.3
Cincinnati, OH (E)	149.2	28.3	4,164.5	156.0	13.7	47.5	22.1
Cincinnati, OH (D)	168.4	25.9	10,451.5	300.7	19.4	34.3	40.7
Columbus, OH (E)	89.0				9.5	50.8	15.8
Columbus, OH (D)	96.9				14.4	33.8	18.6
Greensboro, NC (E)	165.9				9.1	47.5	13.7
Greensboro, NC (D)	208.8				15.5	36.6	35.0
Kansas City, MO (E)	468.8	11.6			9.7	49.3	17.8
Kansas City, MO (D)	512.7	13.0			14.2	35.7	19.5
Los Angeles, CA (E)	108.6	23.0	1,091.6	759.3	10.7	47.4	30.1
Los Angeles, CA (D)	148.7	36.6	1,325.3	1,045.8	12.1	38.7	29.5
Louisville, KY (E)	175.1	37.1		153.7	11.3	44.3	7.9
Louisville, KY (D)	279.6	76.4		746.6	18.3	26.9	16.7
Mesa, AZ (E)	113.2		1,190.7	252.9	9.5	47.8	14.3
Mesa, AZ (D)	156.0		2,132.7	511.7	9.3	46.5	59.4
Milwaukee, WI (E)	141.5				12.3	45.9	8.0
Milwaukee, WI (D)	185.8				16.6	31.4	9.5
Minneapolis, MN (E)	187.3			276.9	9.1	61.7	119.8
Minneapolis, MN (D)	259.8			421.1	12.7	37.2	67.1
Nashville, TN (E)	241.9	47.5	2,188.2	505.0	8.0	50.2	
Nashville, TN (D)	330.6	68.4	2,106.9	50.6	14.1	29.8	
New Orleans, LA (E)	103.3	22.6	2,242.9		11.5	49.3	22.9
New Orleans, LA (D)	202.9	46.9	4,476.1		16.4	35.7	27.5
New York, NY (E)	45.0	19.4		10.6	10.3	54.8	5.6
New York, NY (D)	69.4	34.7		18.2	12.0	45.0	8.7
Norfolk, VA (E)	108.6				9.5	57.4	32.1
Norfolk, VA (D)	148.1				15.3	40.1	30.8
Orlando, FL (E)	258.3	35.1	5,880.6		9.2	43.3	102.7
Orlando, FL (D)	232.1	30.3	6,800.7		18.5	33.7	84.6
Philadelphia, PA (E)	161.4	27.5		1,012.2	14.5	51.8	22.3
Philadelphia, PA (D)	194.7	32.9		1,922.0	17.3	38.0	40.2

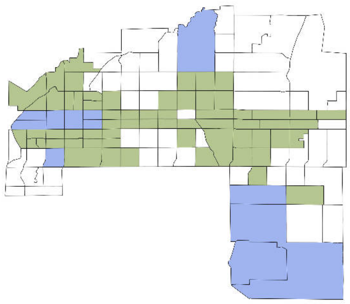
Pittsburgh, PA (E)	98.5	11.5		20.2	8.8	62.8	47.0
Pittsburgh, PA (D)	97.2	14.8		10.3	17.4	36.8	26.6
Portland, OR (E)	182.8	14.2	1,731.0	292.1	8.4	63.7	13.3
Portland, OR (D)	582.7	86.9	4,423.2	717.1	12.6	68.5	3.9
Raleigh, NC (E)	199.7				8.0	61.3	0.2
Raleigh, NC (D)	239.9				10.8	43.1	1.6
Sacramento, CA (E)	159.6		2,516.9	99.4	11.7	56.7	137.9
Sacramento, CA (D)	231.3		4,071.4	168.7	18.2	34.4	132.5
Saint Paul, MN (E)	156.0			337.8	9.2	59.0	74.0
Saint Paul, MN (D)	228.6			703.2	13.1	45.5	67.3
San Francisco, CA (E)	457.5	43.3	5,875.1	491.3	8.8	65.3	22.6
San Francisco, CA (D)	142.8	15.2	2,959.7	392.1	13.0	53.0	27.1
Seattle, WA (E)	374.6		3,321.4		7.2	91.4	59.8
Seattle, WA (D)	486.9		5,934.6		8.5	57.7	58.9
St. Louis, MO (E)	227.6				14.4	48.7	50.7
St. Louis, MO (D)	300.9				20.5	30.7	62.1
Tucson, AZ (E)	272.0	22.2	2,255.3		11.1	45.9	15.2
Tucson, AZ (D)	311.8	36.7	3,285.8		13.3	36.4	16.2
Washington, DC (E)	191.6	25.0		18.2	13.3	75.6	94.6
Washington, DC (D)	219.7	43.6		27.0	20.1	44.9	69.1

Notes: Pre-intervention (2014-2017) census tract level mean by city and program status, where E and D stand for eligible and designated tracts. The designated group is the Opportunity Zones census tracts. The eligible comprises the low-income eligible but not designated tracts. Major crimes include the six-part I Uniform Crime Reporting categories: murder, robbery, aggravated assault, burglary, theft, and motor vehicle theft (rape is excluded). Non-major crimes refer to all the other crimes reported to the police departments. Cities without public, georeferenced, time-stamped data on arrests, calls for service, police stops, and planning permits have blank cells.

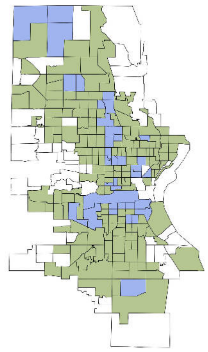
Figure A.1: Eligible and designated Opportunity Zones census tracts by city



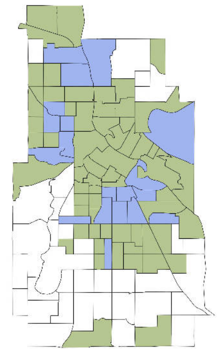
13. Mesa, AZ



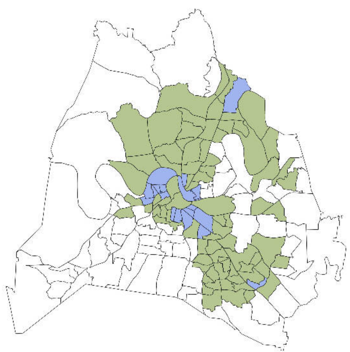
14. Milwaukee, WI



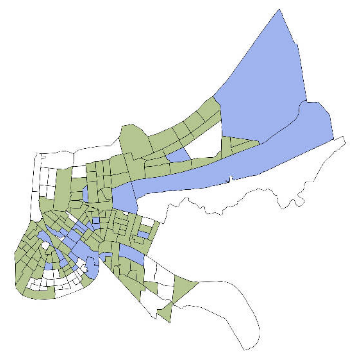
15. Minneapolis, MN



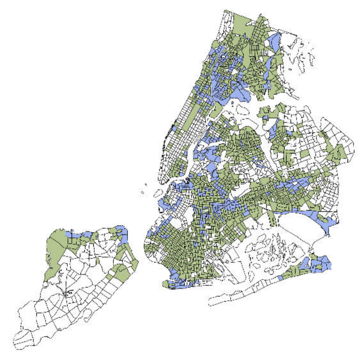
16. Nashville, TN



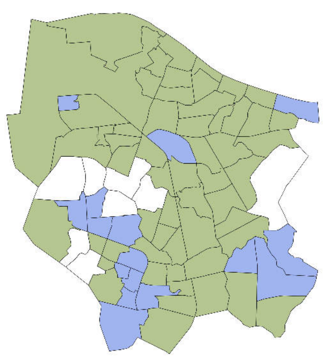
17. New Orleans, LA



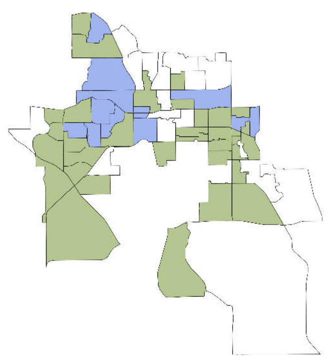
18. New York, NY



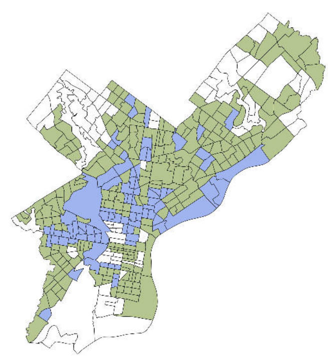
19. Norfolk, VA



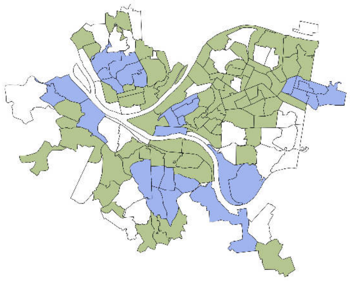
20. Orlando, FL



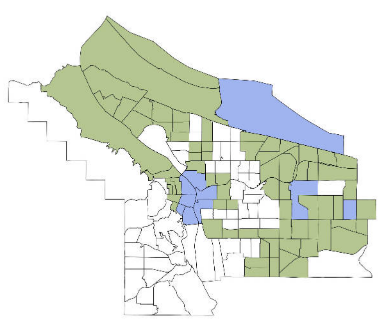
21. Philadelphia, PA



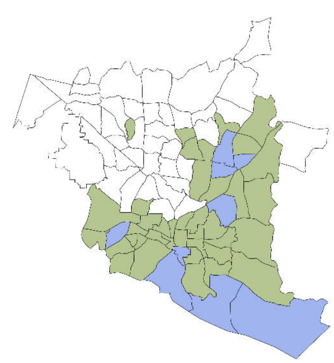
22. Pittsburgh, PA



23. Portland, OR

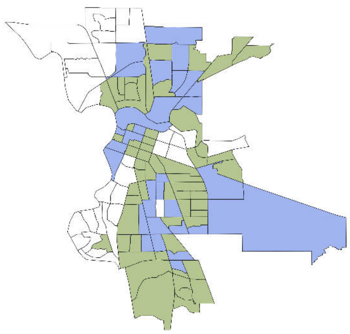


24. Raleigh, NC

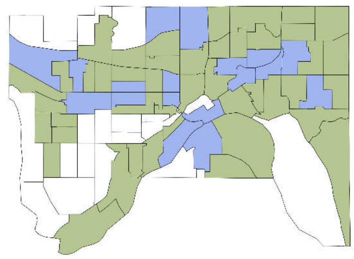


Ineligible Not designated Designated

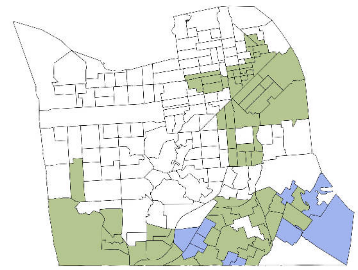
25. Sacramento, CA



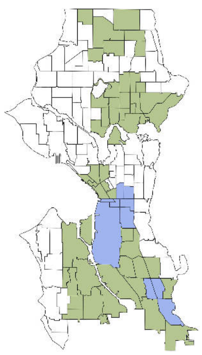
26. Saint Paul, MN



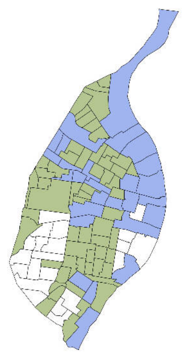
27. San Francisco, CA



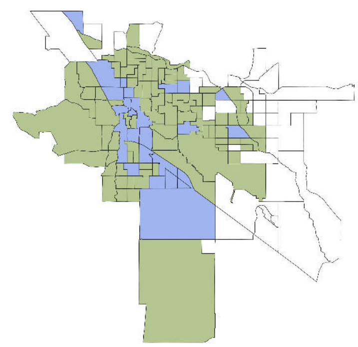
28. Seattle, WA



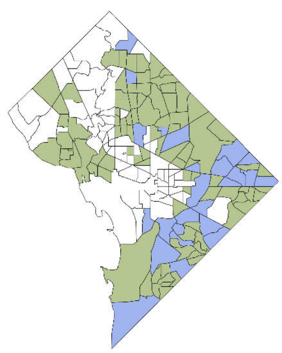
29. St. Louis, MO



30. Tucson, AZ



31. Washington, DC



Ineligible Not designated Designated

Notes: The eligible census tracts have a poverty rate of at least 20 percent, below the 80 percent median statewide family income, or with fewer than 2,000 people within an Empowerment Zone or contiguous to one or more low-income census tract. Governors proposed up to 25 percent of the eligible tracts in their state and the Internal Revenue Service approved the final list of designated tracts.

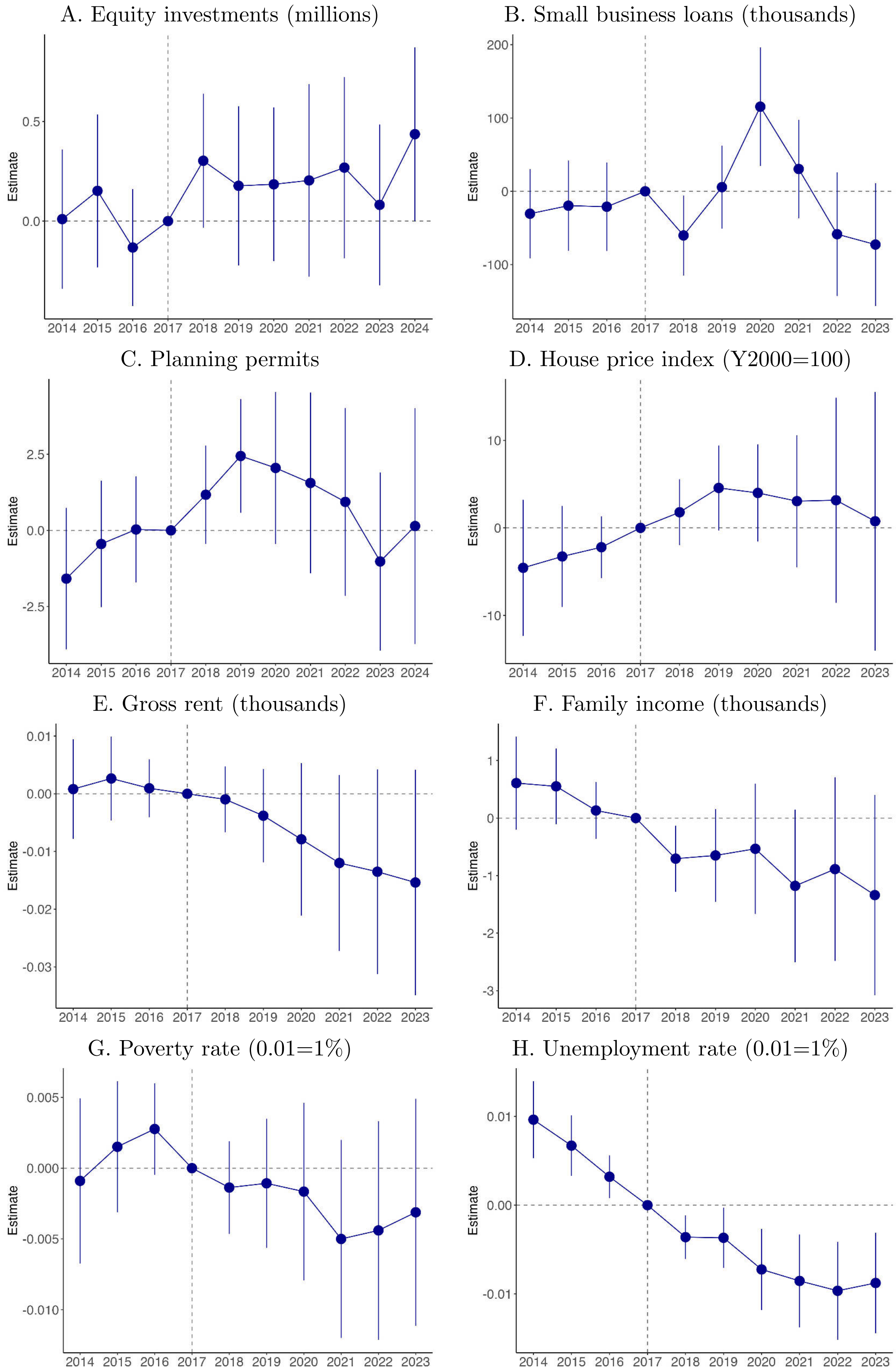
B Appendix: Event study design on alternative sample groups

The difference-in-differences method uses three alternative comparison groups to account for potential biases and sample size trade-offs. The main text presents the results using all the eligible but not selected low-income tracts as the comparison group. This appendix shows the baseline and propensity score weighted event study designs on the economic and public safety outcomes for the other two samples: 1) using only the bordering, eligible but not selected low-income census tracts, and 2) contrasting designated and eligible tracts with similar poverty rate and income ratio (± 15 percentage points from the threshold).

Concerning the economic outcomes, **Appendix Figure B.1** shows the yearly point estimates using the baseline differences-in-differences model and the eligible, bordering tracts. Under this specification, gross rent is no longer showing such trajectories. However, there are still some pre-trends on the other variables. **Appendix Figure B.2** presents the propensity score weighted event study design for the same sample. The parallel trends assumption holds for all the economic outcomes. **Appendix Figures B.3** and **B.4** present the baseline and balanced score event study estimates on the similar tracts sample, where the units have poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income (± 15 percentage points from the thresholds). Under both methods, this group shows that all the pre-intervention periods for the six economic outcomes are not statistically significant, and there are no visible pre-trends.

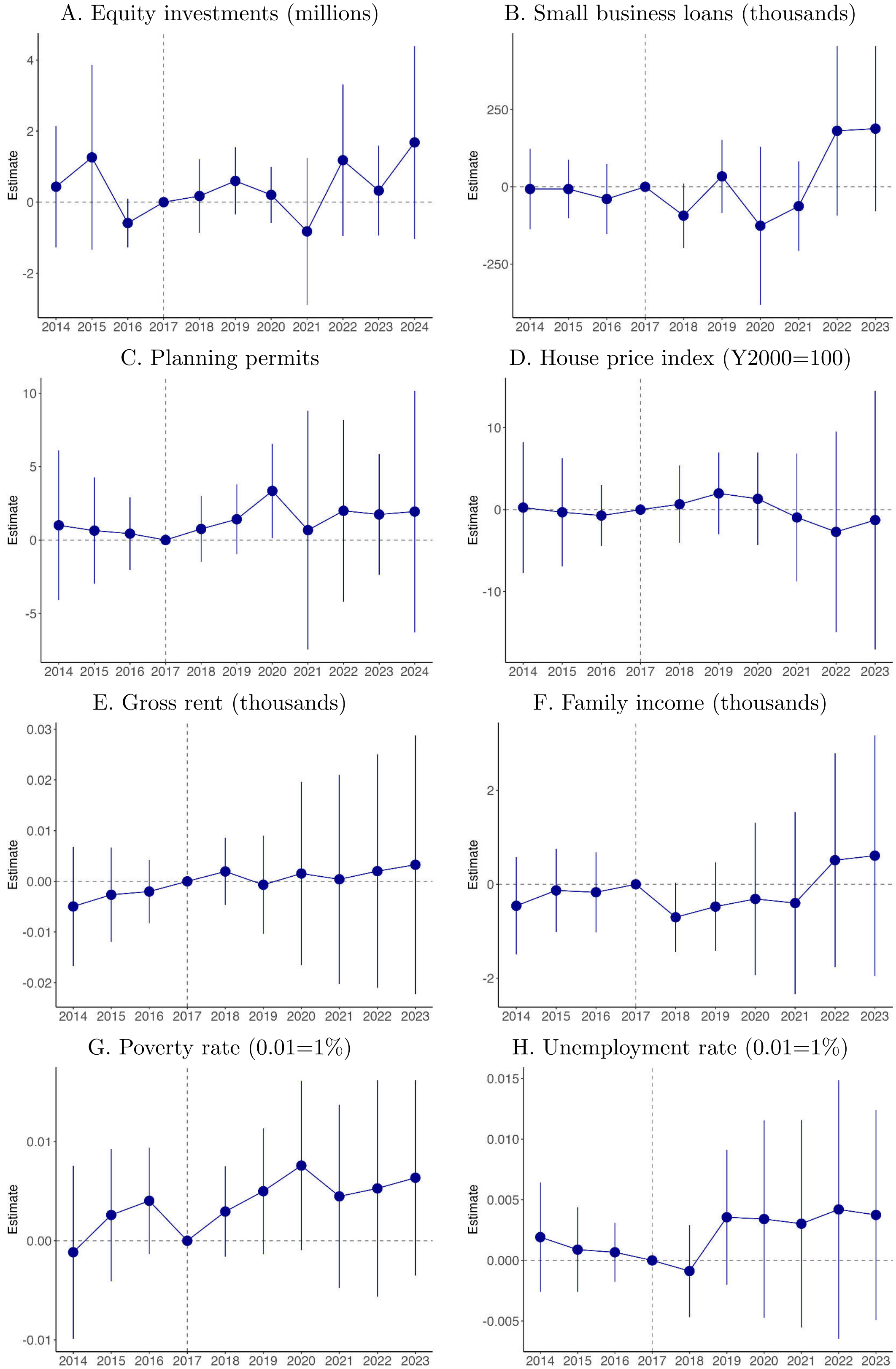
Appendix Figure B.5 presents the baseline event study design estimates for the six public safety outcomes. There are pre-trends using all the eligible tracts as a comparison group on calls for service, police stops, major crimes, and non-major and major crime arrests. **Appendix Figures B.6 and B.7** visualize the event study estimates on the bordering and similar samples. Only calls for service and non-major crime arrests have visible pre-trends on these samples. **Appendix Figures B.8 and B.9** visualize the propensity score weights in an event study design on the bordering and similar samples. The parallel trends hold for calls for service, police stops, and non-major and major crimes and arrests. However, calls for service show a slight upward trend in the eligible and similar tracts samples.

Figure B.1: Event study estimates on economic outcomes, bordering sample



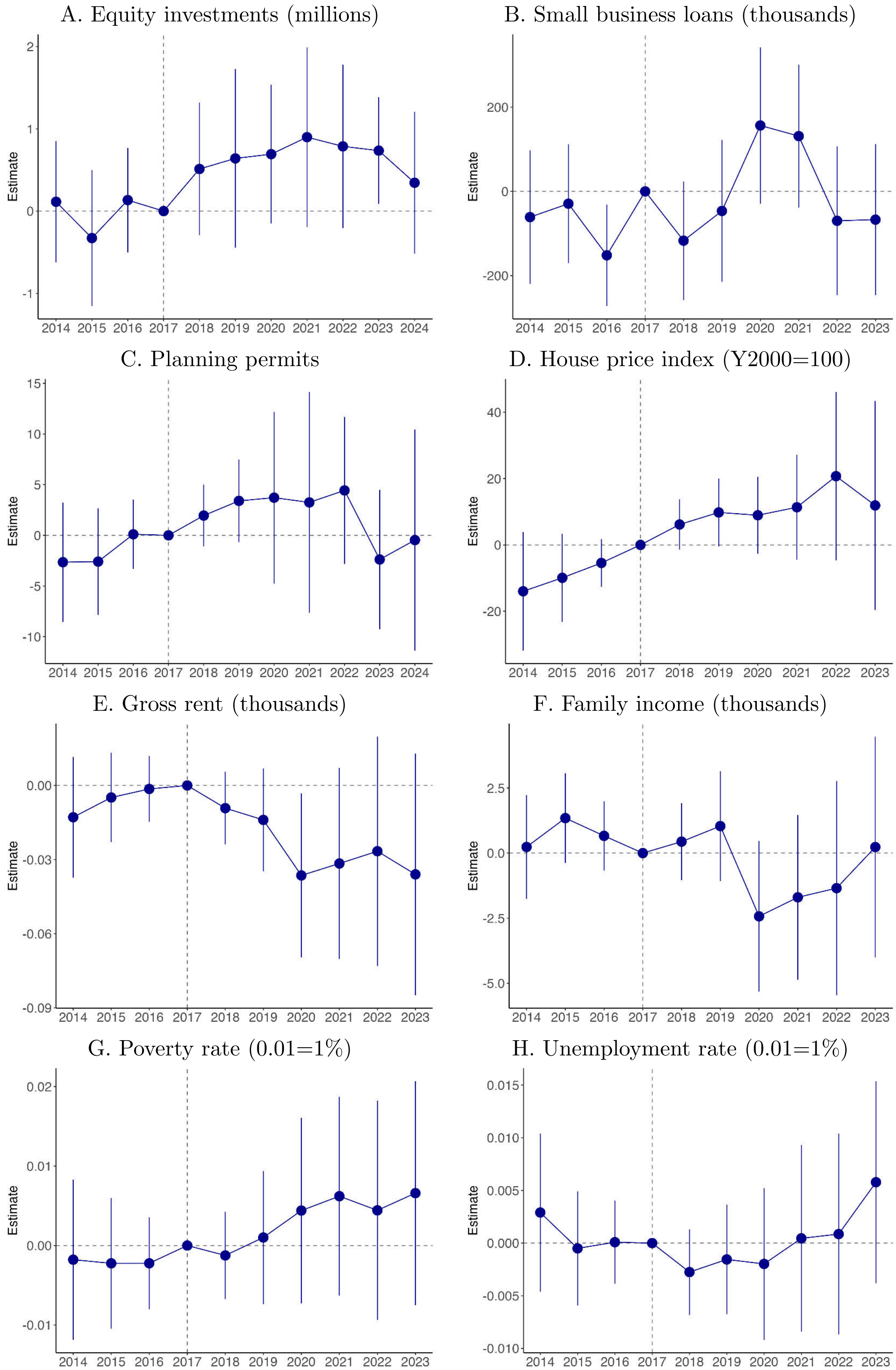
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income designated and their bordering, eligible census tracts sample. This specification does not controls for self-selection, so the parallel trends does not hold for several outcomes.

Figure B.2: Propensity score weighted event study estimates on economic outcomes, bordering sample



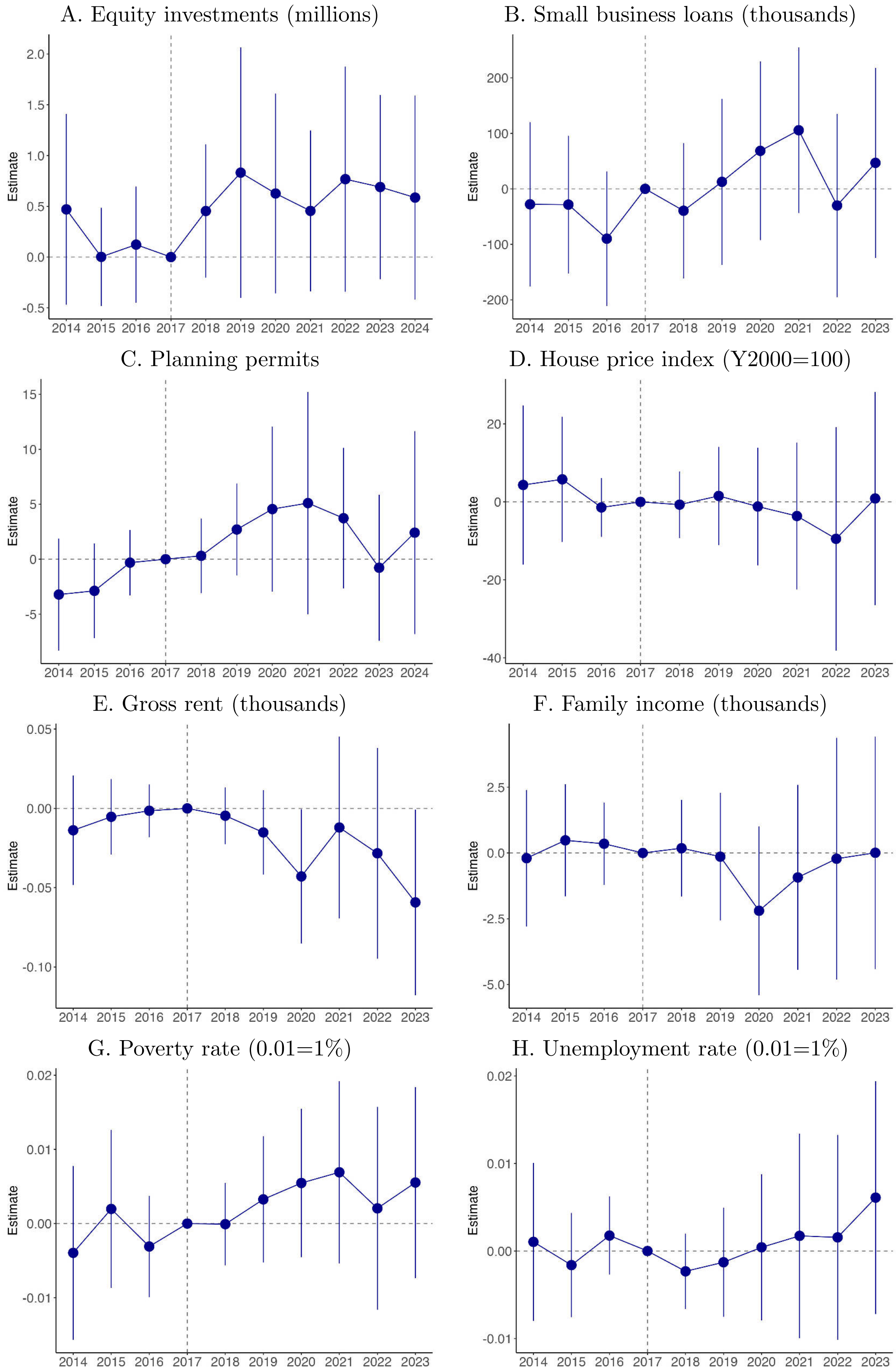
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity-score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income designated and their bordering, eligible census tracts sample.

Figure B.3: Event study estimates on economic outcomes, similar sample



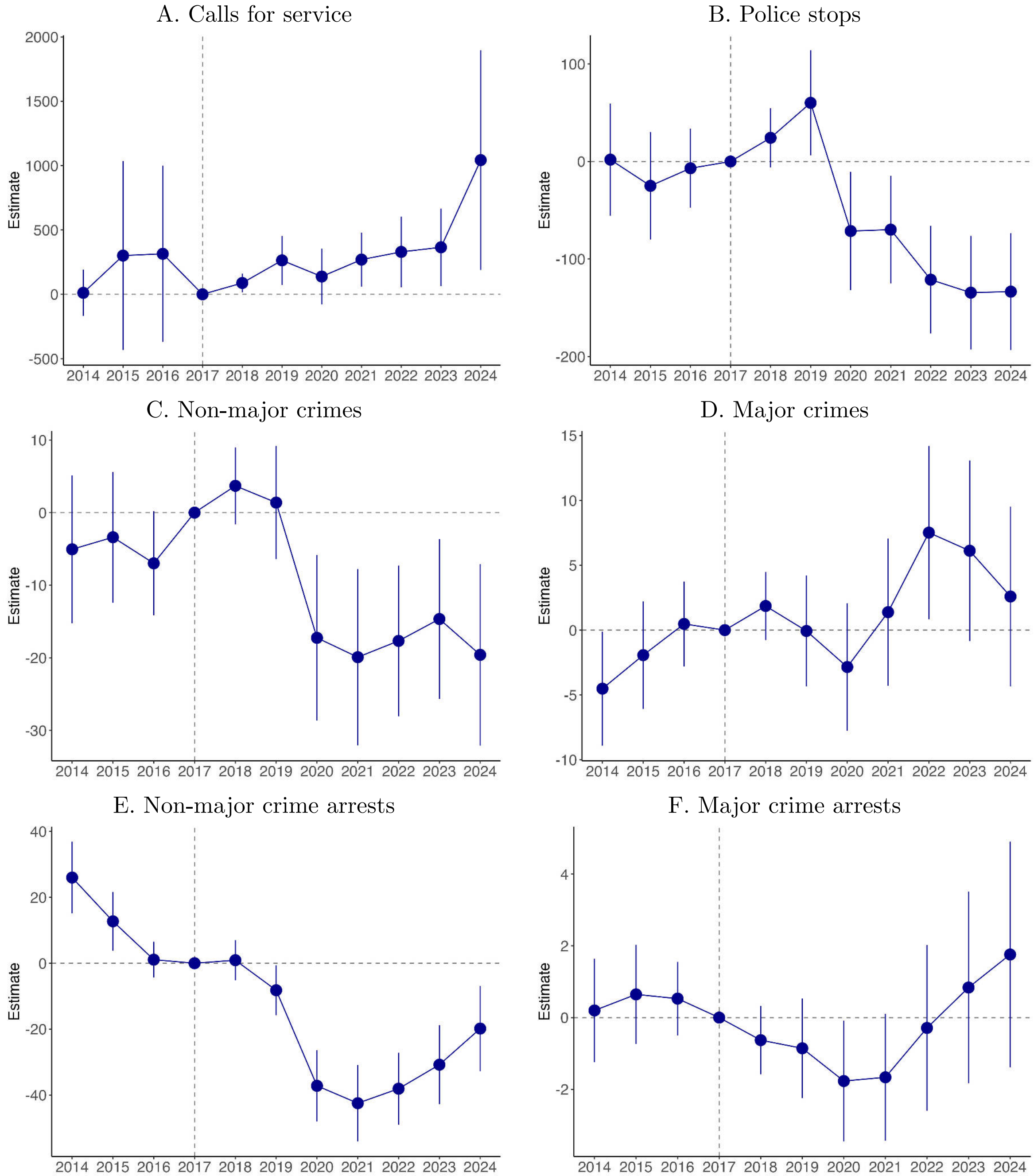
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model uses the eligible, low-income, similar but not designated tracts sample (tracts with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income). This specification does not controls for self-selection, so the parallel trends does not hold for several outcomes.

Figure B.4: Propensity score weighted event study estimates on economic outcomes, similar sample



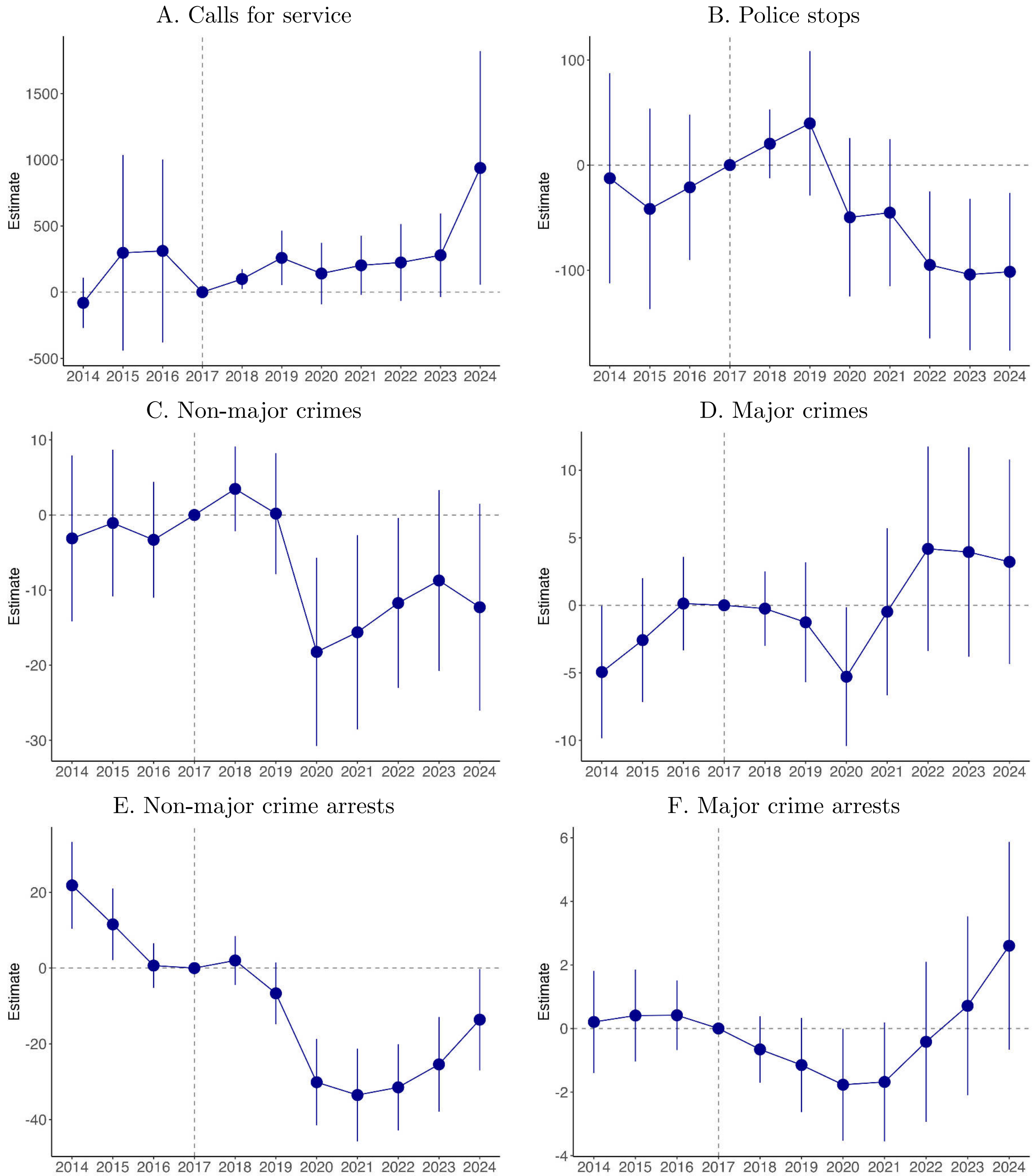
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity-score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The econometric model uses the eligible, low-income, similar but not designated tracts sample (tracts with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income).

Figure B.5: Event study estimates on public safety, eligible sample



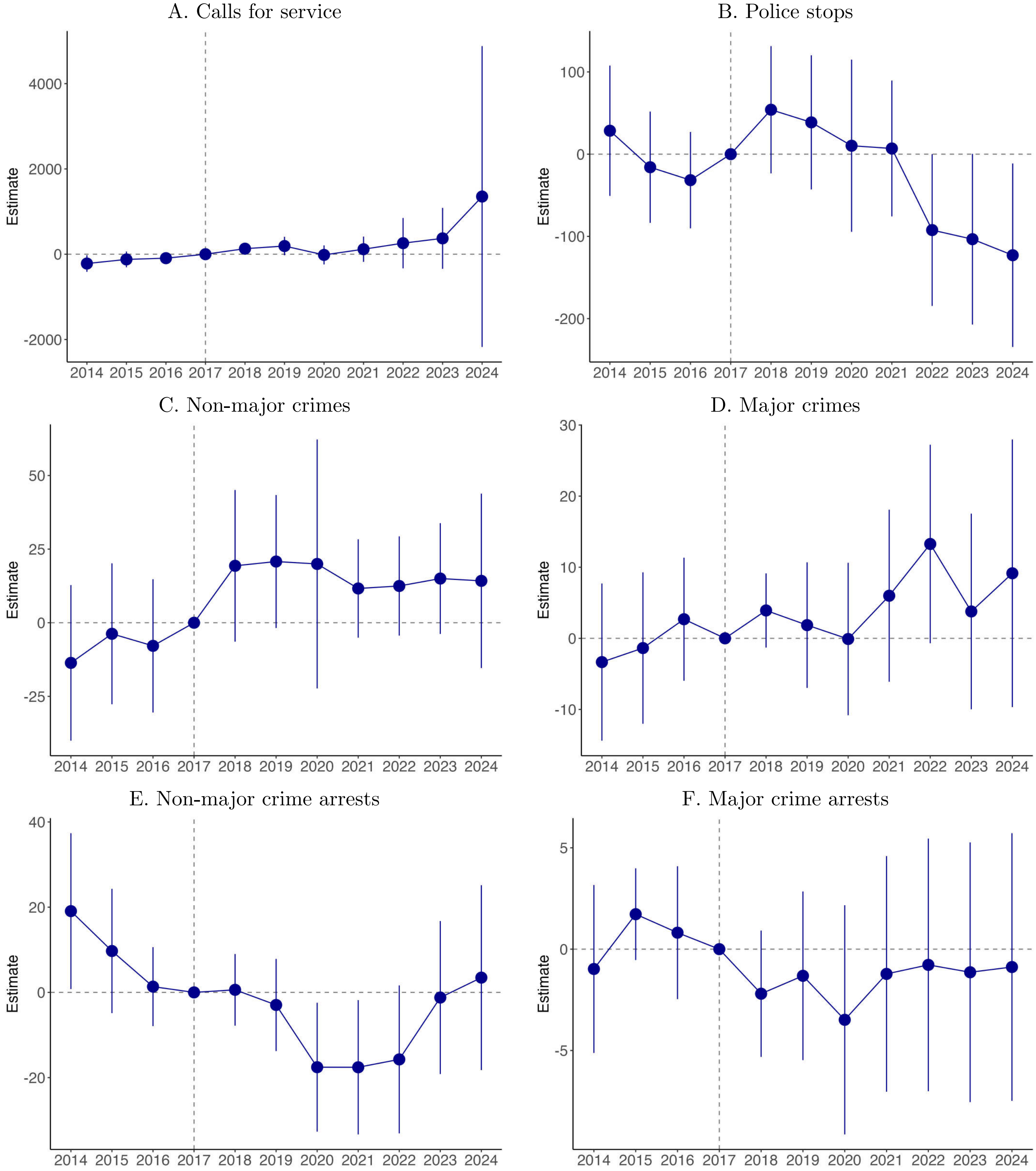
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income, eligible census tracts sample. This specification does not control for self-selection, so the parallel trends does not hold for several outcomes.

Figure B.6: Event study estimates on public safety, bordering sample



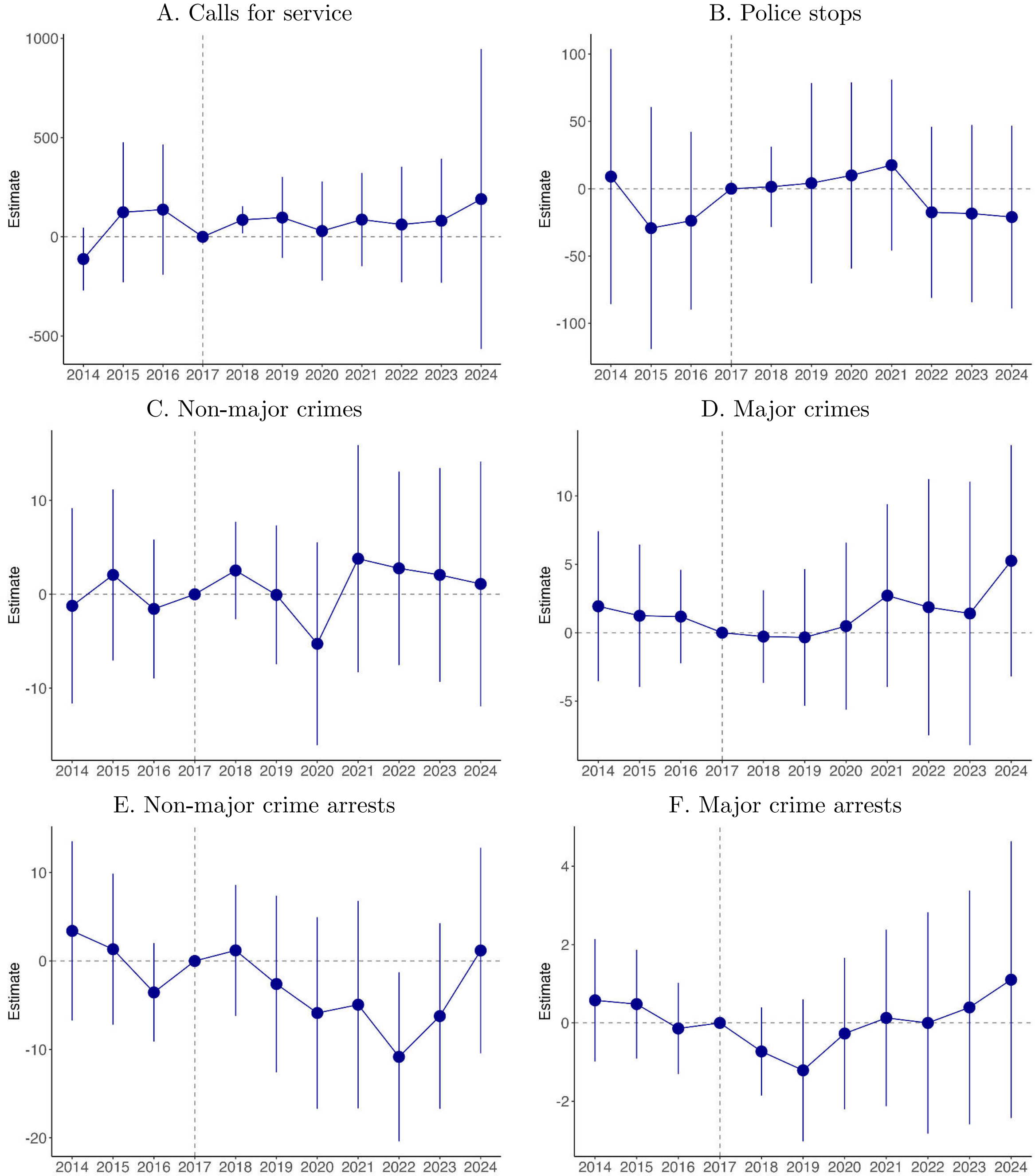
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income designated and their bordering, eligible census tracts sample. This specification does not control for self-selection, so the parallel trends does not hold for several outcomes.

Figure B.7: Event study estimates on public safety, similar sample



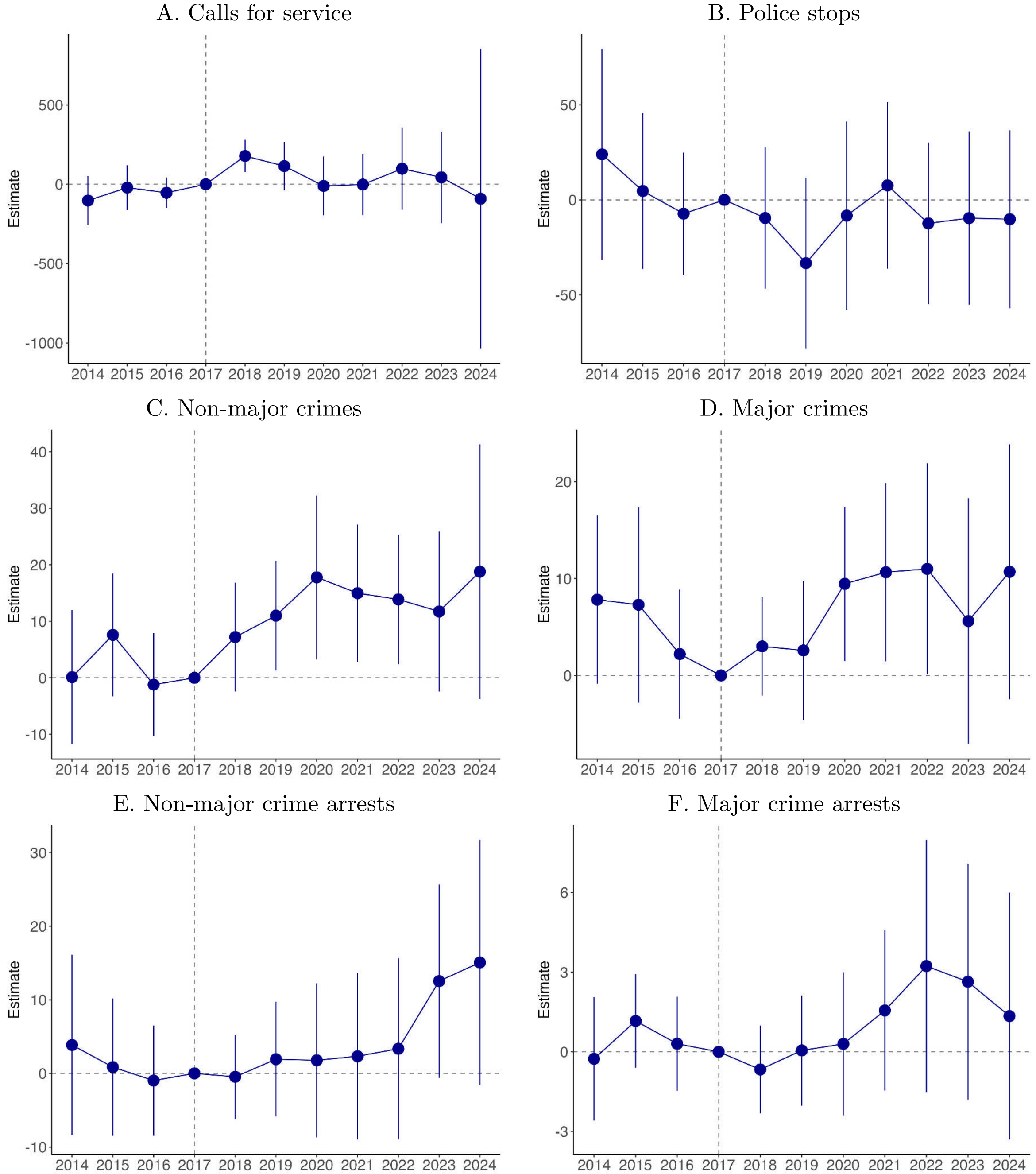
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model uses the eligible, low-income, similar but not designated tracts sample (tracts with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income). This specification does not control for self-selection, so the parallel trends does not hold for several outcomes.

Figure B.8: Propensity score weighted event study estimates on public safety, bordering sample



Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The econometric model uses the low-income, eligible census tracts sample.

Figure B.9: Propensity score weighted event study estimates on public safety, similar sample



Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The econometric model use the eligible, low-income, similar but not designated tracts sample (tracts with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income).

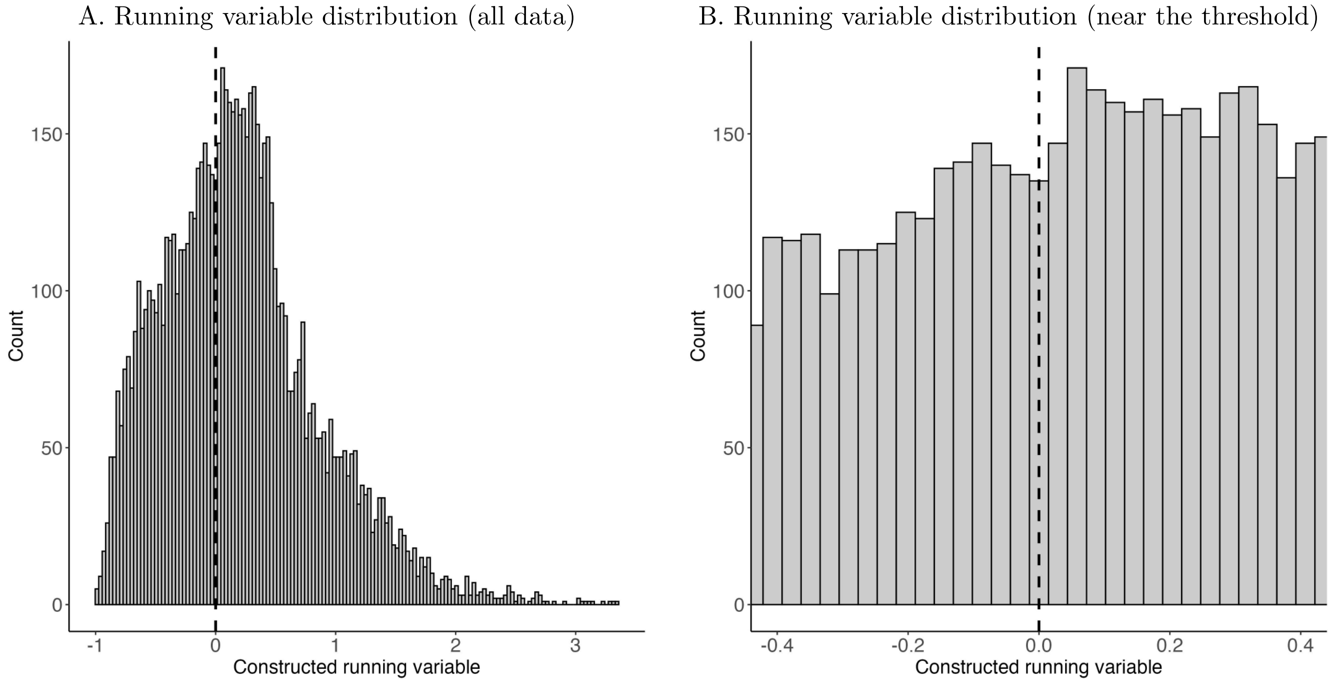
C Appendix: Regression discontinuity results

This appendix shows the results on economic and public safety conditions using the regression discontinuity design. This model requires no manipulation of the running variable around the threshold. This situation seems unlikely as the poverty and income thresholds used information collected by the Census Bureau before the Opportunity Zones program was implemented. To corroborate this finding, **Appendix Figure C.1** visualizes no evidence of manipulation as the density of tracts near the threshold is similar on either side. The formal density tests (Cattaneo et al., 2018; McCrary, 2008) corroborate this result.

Appendix Figure C.2 shows the descriptive graphical evidence of the regression discontinuity on the economic outcomes. Panel A suggests that census tracts that become eligible for the program attracted more private equity investments than similar tracts just below the eligibility threshold, but the difference is not statistically significant. There are no discontinuous changes in business loans, planning permits, property prices and rents, family income, and unemployment. While the poverty rate may have decreased among the designated tracts relative to the non-selected and similar areas, this change is not statistically significant. **Table C.1** confirms these findings by estimating the point estimate of the regression discontinuity design using the optimal bandwidths (Calonico et al., 2015; Imbens and Kalyanaraman, 2012) under different polynomial functions (linear and quadratic). There is a significant increase of 6 to 8.8 percent (around twice as high as in the control group) in the probability of Opportunity Zone designation crossing the eligibility threshold. While the sign of private equity investments is usually positive, none are statistically significant. Likewise, none of the other socioeconomic outcomes reach statistical significance, and some flip their sign under alternative specifications suggesting no meaningful early economic impacts of the Opportunity Zone program.

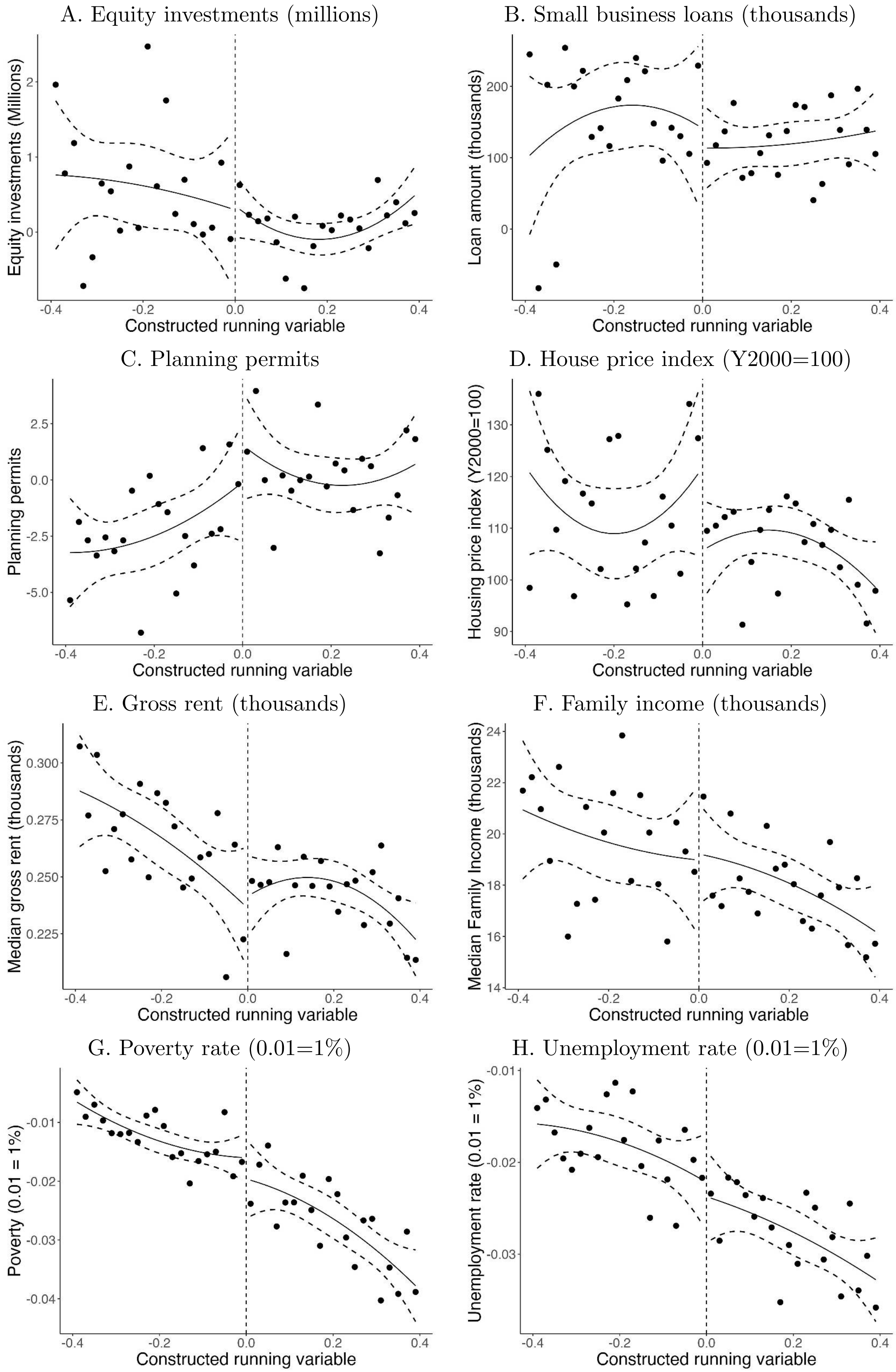
Consistent with no evidence of socioeconomic changes in the Opportunity Zone tracts, **Figure C.3** shows no effects on public safety. Police stops and major crime arrests seem to exhibit a differential change around the threshold, but there are no significant changes. Worth mentioning that in these two outcomes, only nine and eleven cities reported the outcomes, so it may be possible that under a larger sample, the results may change. The other outcomes (calls for service, non-major crimes, major crimes, and non-major crime arrests) have similar functional forms on either side of the constructed running variable, implying that there are no changes before and after the intervention on similar tracts despite having different probabilities of being designated as Opportunity Zones. **Table C.2** presents that no single outcome reaches statistical significance and the sign of the estimate flips under alternative specifications suggesting that neighborhoods have not had any changes due to the Opportunity Zone designation.

Figure C.1: Running variable distribution



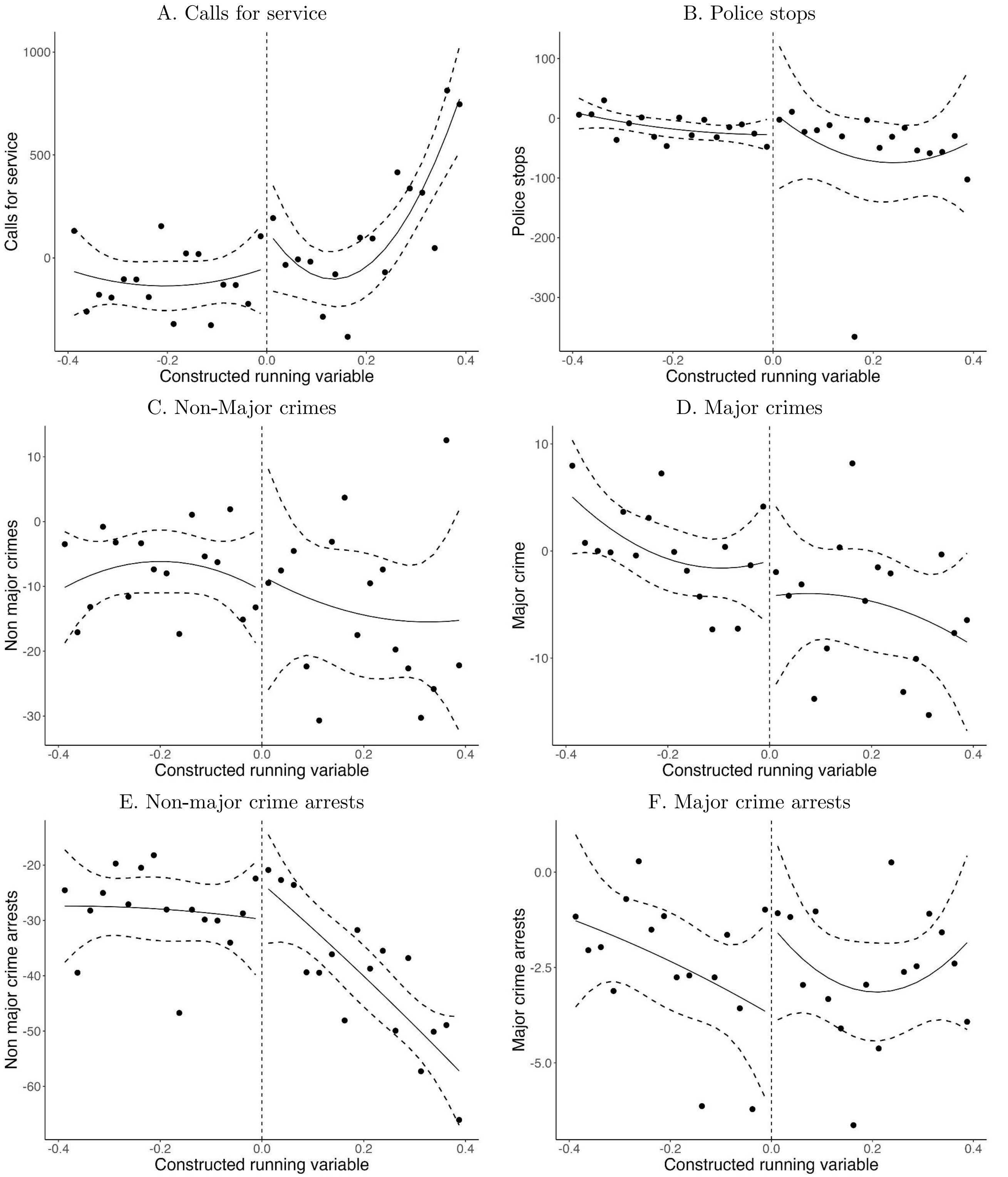
Notes: The panels show the distribution of tracts by the constructed running variable. Panel B presents the regression discontinuity of the parcels. The pvalue of the manipulation tests are 0.28 and 0.82 using the McCrary (2008) and Cattaneo et al. (2018) approaches.

Figure C.2: Regression discontinuity on economic outcomes



Notes: All panels present the mean difference of the outcome variable between the post (2018-2024) and pre-intervention (2014-2017) periods, second-order polynomials (solid line), and 95 percent confidence intervals (dash lines) around a 0.4 bandwidth of the constructed running variable.

Figure C.3: Regression discontinuity on public safety outcomes



Notes: All panels present the mean difference of the outcome variable between the post (2018-2024) and pre-intervention (2014-2017) periods, second-order polynomials (solid line), and 95 percent confidence intervals (dash lines) around a 0.4 bandwidth of the constructed running variable.

Table C.1: Regression discontinuity estimates of the Opportunity Zones designation on economic outcomes

	(1)	(2)	(3)	(4)
A. Designated Opportunity Zone				
Treatment effect	0.087***	0.062**	0.072***	0.073***
	(0.019)	(0.029)	(0.015)	(0.023)
Mean dep. var.	0.07	0.07	0.08	0.08
Observations	2,519	2,519	3,833	3,833
B. Equity investments (millions)				
Designated OZ	−0.511	14.477	−4.334	0.190
	(3.389)	(16.731)	(4.366)	(5.530)
Mean dep. var.	1.56	1.56	1.53	1.53
Observations	2,519	2,519	3,833	3,833
C. Small business loans (thousands)				
Designated OZ	−569.479	−472.545	−635.716	−532.217
	(451.922)	(905.857)	(442.069)	(646.033)
Mean dep. var.	909.58	909.58	923.37	923.37
Observations	2,519	2,519	3,833	3,833
D. Planning permits				
Designated OZ	8.282	14.700	11.755	12.850
	(21.374)	(45.102)	(18.376)	(30.381)
Mean dep. var.	32.30	32.30	33.75	33.75
Observations	2,458	2,458	3,750	3,750
E. House price index (Y2000=100)				
Designated OZ	−96.467	−386.213	−42.354	−185.964
	(92.370)	(581.770)	(81.043)	(156.982)
Mean dep. var.	294.26	294.26	295.10	295.10
Observations	1,565	1,565	2,315	2,315
F. Gross rent (thousands)				
Designated OZ	0.124	0.194	0.181	0.057
	(0.184)	(0.390)	(0.158)	(0.255)
Mean dep. var.	1.18	1.18	1.18	1.18
Observations	2,518	2,518	3,832	3,832
G. Family income (thousands)				
Designated OZ	9.183	15.244	5.482	4.446
	(15.549)	(33.931)	(13.721)	(21.572)
Mean dep. var.	68.89	68.89	68.70	68.70
Observations	2,519	2,519	3,833	3,833
H. Poverty rate (0.01=1%)				
Designated OZ	−0.015	−0.076	−0.018	−0.033
	(0.040)	(0.110)	(0.034)	(0.058)
Mean dep. var.	0.15	0.15	0.16	0.16
Observations	2,519	2,519	3,833	3,833
I. Unemployment rate (0.01=1%)				
Designated OZ	−0.009	−0.016	−0.026	−0.018
	(0.032)	(0.075)	(0.026)	(0.045)
Mean dep. var.	0.08	0.08	0.09	0.09
Observations	2,519	2,519	3,833	3,833
Bandwidth	IK: 0.25	IK: 0.25	CC: 0.40	CC: 0.40
Polynomial function	Linear	Quad	Linear	Quad

Notes: Fuzzy regression discontinuity estimates of the Opportunity Zones designation on selected outcomes. Robust standard errors clustered at the census tract level in parentheses. Panel A is the first stage where the treatment effects is an indicator variable of whether the tract is above the eligiblity threshold. Panels B-I use a designated Opportunity Zone (OZ) indicator variable instrumented with an indicator of being above the eligiblity threshold. The optimal bandwidths (IK and CC) follow Imbens and Kalyanaraman (2012) and Calonico et al. (2015). *p<0.1; **p<0.05; ***p<0.01.

Table C.2: Regression discontinuity estimates of the Opportunity Zones designation on public safety

	(1)	(2)	(3)	(4)
A. Calls for service				
Designated OZ	811.42	−355.36	1,764.32	1,363.62
	(1,714.66)	(1,189.61)	(4,165.25)	(2,545.37)
Mean dep. var.	2,014.1	2,014.1	2,121.4	2,121.4
Cities	13	13	13	13
Observations	792	792	1,243	1,243
B. Police stops				
Designated OZ	84.55	21.27	2,728.46	298.91
	(324.95)	(503.94)	(3,626.80)	(411.89)
Mean dep. var.	189.2	189.2	205.2	205.2
Cities	18	18	18	18
Observations	1,989	1,989	3,010	3,010
C. Non-Major crimes				
Designated OZ	−38.31	−35.20	122.37	11.04
	(71.84)	(69.49)	(222.19)	(88.25)
Mean dep. var.	172.4	172.4	179.4	179.4
Cities	29	29	29	29
Observations	2,444	2,444	3,707	3,707
D. Major crimes				
Designated OZ	−19.22	8.85	−99.09	−39.75
	(53.33)	(49.19)	(158.80)	(67.28)
Mean dep. var.	114.6	114.6	119.8	119.8
Cities	31	31	31	31
Observations	2,519	2,519	3,833	3,833
E. Non-major crime arrests				
Designated OZ	74.73	26.55	221.51	130.43
	(72.94)	(70.62)	(261.74)	(164.92)
Mean dep. var.	85.1	85.1	91.1	91.1
Cities	18	18	18	18
Observations	2,017	2,017	3,079	3,079
F. Major crime arrests				
Designated OZ	29.06	20.09	99.40	46.59
	(21.82)	(15.96)	(123.64)	(44.37)
Mean dep. var.	16.8	16.8	17.1	17.1
Cities	18	18	18	18
Observations	2,017	2,017	3,079	3,079
Bandwidth	IK: 0.25	IK: 0.25	CC: 0.40	CC: 0.40
Polynomial function	Linear	Quad	Linear	Quad

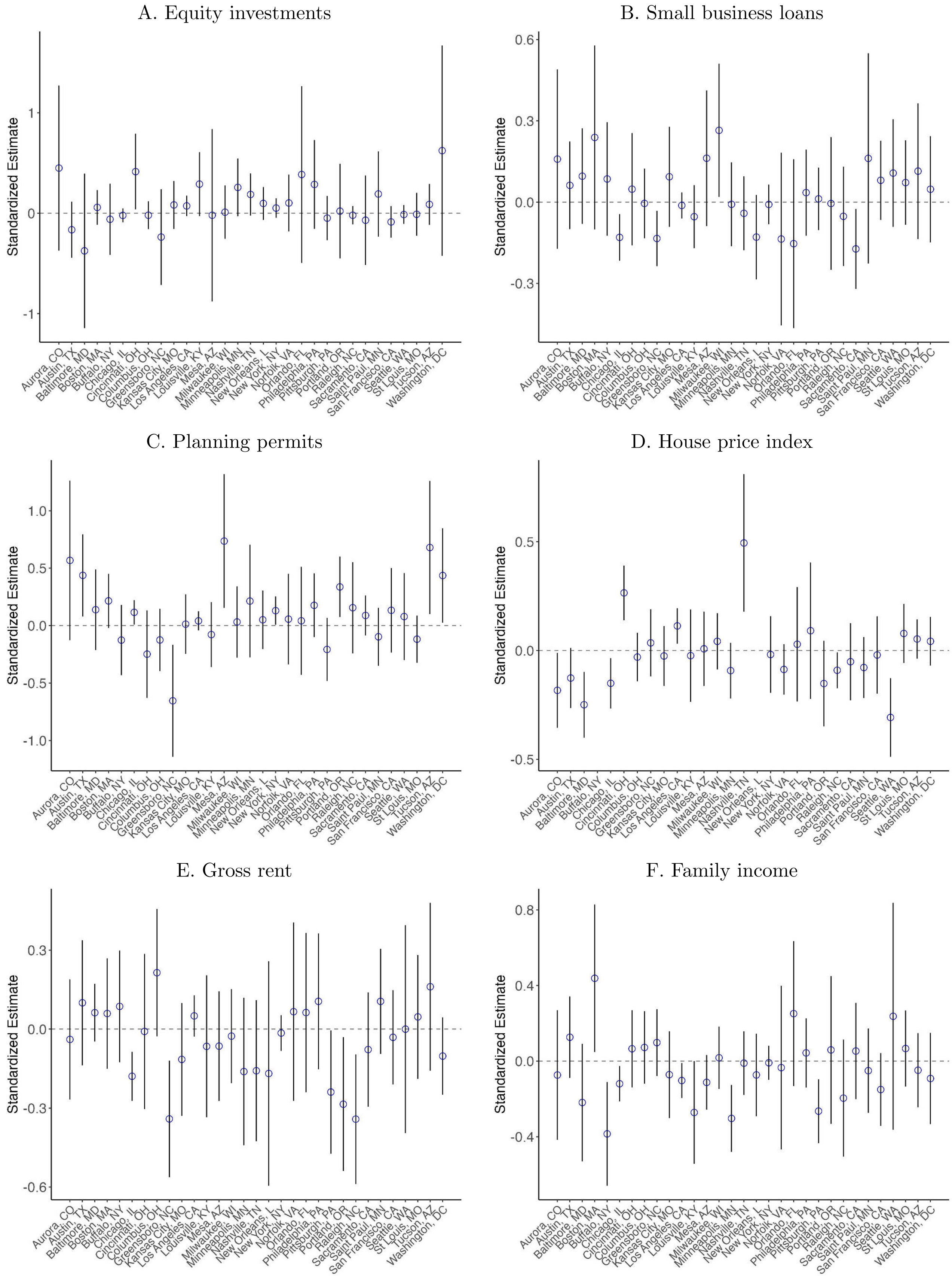
Notes: Fuzzy regression discontinuity estimates of the Opportunity Zones designation on public safety. Robust standard errors clustered at the census tract level in parentheses. The number of cities reporting the outcome is included. All panels use a designated Opportunity Zone (OZ) indicator variable instrumented with an indicator of being above the eligiblity threshold. The optimal bandwidths (IK and CC) follow Imbens and Kalyanaraman (2012) and Calonico et al. (2015). *p<0.1; **p<0.05; ***p<0.01.

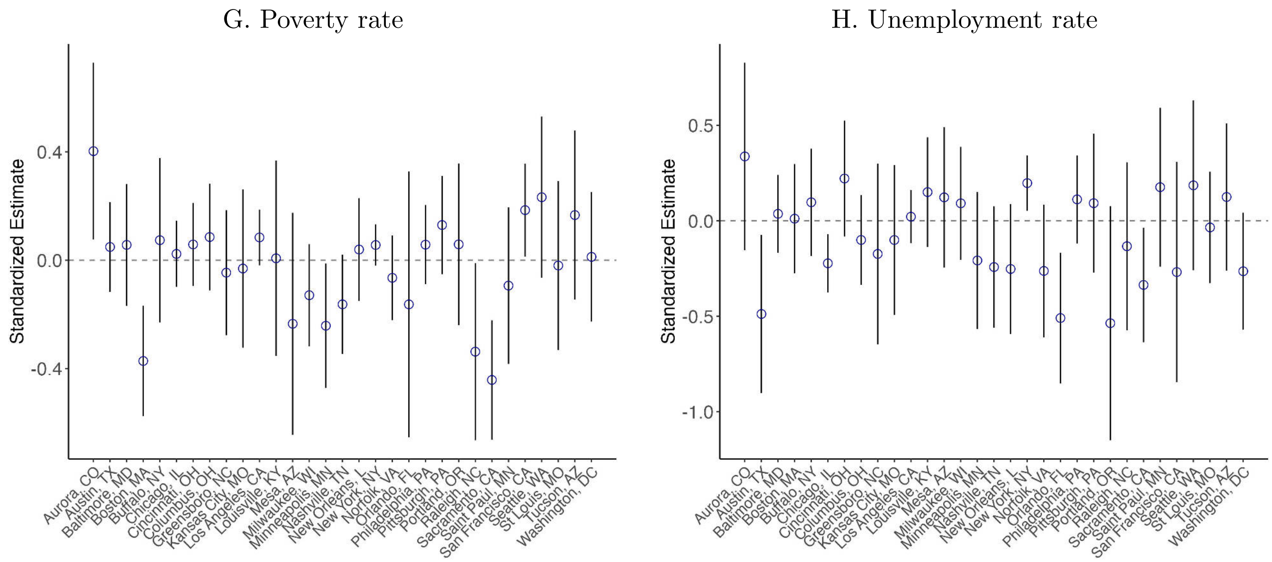
D Appendix: City-specific difference-in-differences estimates

This appendix shows the propensity score weighted difference-in-differences estimates by city level on the eligible sample. The other samples were not presented because it drastically reduced the sample size, increasing the standard errors. Similarly, the regression discontinuity design cannot be computed at the city level as no jurisdiction has the sample needed to conduct such a data-intensive estimator.

The dependent variable was standardized (mean zero and standard deviation of one) to compare cities with different outcome levels. **Appendix Figures D.1** and **D.2** show the economic and public safety propensity score weighted difference-in-differences estimates on the eligible and bordering samples. The estimates measure the number of standard deviations the dependent variable changes on being designated an Opportunity Zone tract. While some cities show significant results on some outcomes, they are more likely due to a false discovery rate as there are no consistent changes across the outcomes.

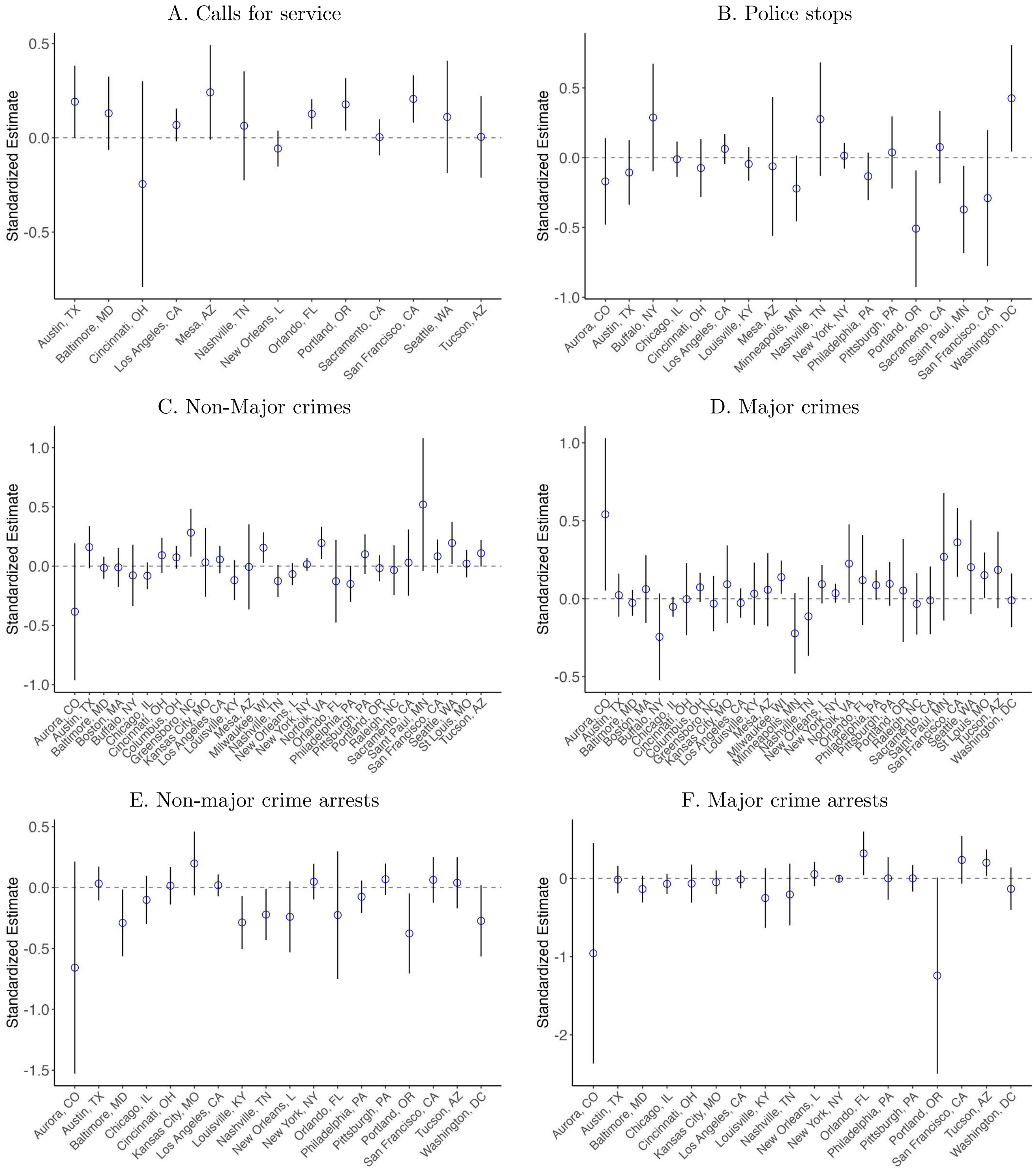
Figure D.1: Propensity score weighted difference-in-differences estimates on economic outcomes by city





Notes: The dependent variables are standardized (mean zero, and standard deviation of one). Difference-in-differences estimates of the Opportunity Zones designation on economic outcomes where the regression uses inverse propensity score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The panels include estimates on the low-income, eligible tracts.

Figure D.2: Propensity score weighted difference-in-differences estimates on public safety by city



Notes: The dependent variables are standardized (mean zero, and standard deviation of one). Difference-in-differences estimates of the Opportunity Zones designation on public safety where the regression uses inverse propensity score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The panels include estimates on the low-income, eligible tracts.

E Appendix: Estimated effects of the Opportunity Zones on UCR Part I crime and arrests categories

This appendix presents the regression discontinuity and propensity score weighted difference-in-differences estimators on the Uniform Crime Reporting Part I crime and arrest categories.

Appendix Table E.1 reveals that the individual crime offenses of murder, robbery, aggravated assault, burglary, and motor vehicle theft show significant decreases under the baseline difference-in-differences, but these results capture the self-selection bias. Once the propensity score weights are used to ensure that the parallel trends hold, the crime outcomes lose their statistical significance. Only burglary suggests a significant increase of about 8.5 percent (an additional 1.8 burglaries per year).

The regression discontinuity estimates (**Appendix Table E.2**) do not show any significant positive effects on burglary and there is a flip sign in two specifications. Consequently, overall there are no impacts on crimes. **Appendix Tables E.3** and **E.4** present no changes on the disaggregated arrests categories using the difference-in-differences or regression discontinuity designs.

Table E.1: Difference-in-Differences estimates of the Opportunity Zones designation on crime

	DiD	DiD	DiD	PSM-DiD	PSM-DiD	PSM-DiD
	(1)	(2)	(3)	(4)	(5)	(6)
A. Violent crime						
Treatment*Post	−0.157	−0.370	1.049	−0.433	−1.039	−0.296
	(0.653)	(0.725)	(0.935)	(0.548)	(0.705)	(0.720)
Mean dep. var.	35.5	41.4	21.3	35.5	41.4	21.3
Observations	51,819	33,082	12,015	51,819	33,082	12,015
B. Murder						
Treatment*Post	0.108**	0.065	0.135	0.018	0.021	0.114
	(0.045)	(0.049)	(0.097)	(0.038)	(0.050)	(0.092)
Mean dep. var.	0.6	0.8	0.3	0.6	0.8	0.3
Observations	50,106	32,281	11,395	50,106	32,281	11,395
C. Robbery						
Treatment*Post	−1.280***	−0.834**	−0.080	−0.421	−0.557	0.186
	(0.328)	(0.375)	(0.464)	(0.293)	(0.370)	(0.394)
Mean dep. var.	14.2	16.2	9.1	14.2	16.2	9.1
Observations	51,819	33,082	12,015	51,819	33,082	12,015
D. Aggravated assault						
Treatment*Post	0.938**	0.386	1.126	−0.047	−0.477	−0.482
	(0.449)	(0.485)	(0.713)	(0.371)	(0.461)	(0.562)
Mean dep. var.	20.8	24.5	12.1	20.8	24.5	12.1
Observations	51,819	33,082	12,015	51,819	33,082	12,015
E. Property crime						
Treatment*Post	3.889*	2.670	4.657	4.890**	1.480	3.474
	(2.121)	(2.349)	(4.401)	(2.460)	(2.958)	(3.317)
Mean dep. var.	110.7	118.9	94.4	110.7	118.9	94.4
Observations	51,819	33,082	12,015	51,819	33,082	12,015
F. Burglary						
Treatment*Post	1.614***	2.352***	1.954	2.043***	1.979***	1.636*
	(0.553)	(0.604)	(1.208)	(0.510)	(0.588)	(0.929)
Mean dep. var.	22.4	24.4	18.7	22.4	24.4	18.7
Observations	51,819	33,082	12,015	51,819	33,082	12,015
G. Theft						
Treatment*Post	0.443	−0.274	0.241	2.365	0.025	1.664
	(1.727)	(1.877)	(3.714)	(2.169)	(2.491)	(2.910)
Mean dep. var.	72.1	76.6	62.1	72.1	76.6	62.1
Observations	51,819	33,082	12,015	51,819	33,082	12,015
H. Motor vehicle theft						
Treatment*Post	1.835***	0.594	2.464**	0.484	−0.522	0.176
	(0.458)	(0.527)	(1.127)	(0.472)	(0.602)	(0.892)
Mean dep. var.	16.2	17.9	13.7	16.2	17.9	13.7
Observations	51,819	33,082	12,015	51,819	33,082	12,015
Eligible sample	X	-	-	X	-	-
Border sample	-	X	-	-	X	-
Similar sample	-	-	X	-	-	X

Notes: Estimates of the Opportunity Zones designation on crime. Robust standard errors clustered at the census tract level in parentheses. Columns (1), (2), and (3) use the difference-in-differences (DiD) estimation. Columns (4), (5) and (6) employ a propensity score weighting in a difference-in-difference (PSM-DiD) model. Columns (1) and (4) include the low-income, eligible and designated census tracts. Columns (2) and (5) consider the low-income designated and their bordering, eligible census tracts, Columns (3) and (6) use the low-income, eligible and similar tracts (with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income). *p<0.1; **p<0.05; ***p<0.01.

Table E.2: Regression discontinuity estimates of the Opportunity Zones designation on crime

	(1)	(2)	(3)	(4)
A. Violent crime				
Designated OZ	8.00	5.99	−13.48	10.19
	(6.80)	(6.91)	(21.69)	(9.42)
Mean dep. var.	17.9	17.9	19.6	19.6
Observations	2,519	2,519	3,833	3,833
B. Murder				
Designated OZ	−0.45	0.07	−1.16	−1.64
	(0.63)	(0.68)	(1.16)	(1.21)
Mean dep. var.	0.2	0.2	0.3	0.3
Observations	2,362	2,362	3,599	3,599
C. Robbery				
Designated OZ	2.91	2.56	−7.91	3.30
	(4.00)	(4.37)	(12.93)	(5.62)
Mean dep. var.	7.8	7.8	8.6	8.6
Observations	2,519	2,519	3,833	3,833
D. Aggravated assault				
Designated OZ	5.68	3.63	−2.47	9.34
	(5.03)	(4.31)	(11.02)	(7.39)
Mean dep. var.	10.0	10.0	10.8	10.8
Observations	2,519	2,519	3,833	3,833
E. Property crime				
Designated OZ	−27.19	2.89	−85.59	−49.90
	(50.71)	(46.23)	(143.30)	(65.22)
Mean dep. var.	96.7	96.7	100.2	100.2
Observations	2,519	2,519	3,833	3,833
F. Burglary				
Designated OZ	−6.38	0.78	−18.77	−8.48
	(11.76)	(9.74)	(27.86)	(16.84)
Mean dep. var.	17.8	17.8	18.5	18.5
Observations	2,519	2,519	3,833	3,833
G. Theft				
Designated OZ	−18.62	7.53	−47.72	−36.18
	(45.87)	(42.00)	(115.93)	(56.63)
Mean dep. var.	66.5	66.5	68.8	68.8
Observations	2,519	2,519	3,833	3,833
H. Motor vehicle theft				
Designated OZ	−2.20	−5.46	−19.10	−5.24
	(10.55)	(9.37)	(23.06)	(14.76)
Mean dep. var.	12.5	12.5	12.9	12.9
Observations	2,519	2,519	3,833	3,833
Bandwidth	IK: 0.25	IK: 0.25	CC: 0.40	CC: 0.40
Polynomial function	Linear	Quad	Linear	Quad

Notes: Fuzzy regression discontinuity estimates of the Opportunity Zones designation on crime. Robust standard errors clustered at the census tract level in parentheses. All panels use a designated Opportunity Zone (OZ) indicator variable instrumented with an indicator of being above the eligiblity threshold. The optimal bandwidths (IK and CC) follow Imbens and Kalyanaraman (2012) and Calonico et al. (2015). *p<0.1; **p<0.05; ***p<0.01.

Table E.3: Difference-in-differences estimates of the Opportunity Zones designation on arrests

	DiD (1)	DiD (2)	DiD (3)	PSM-DiD (4)	PSM-DiD (5)	PSM-DiD (6)
A. Violent crime						
Treatment*Post	−0.238	−0.210	0.190	0.080	−0.284	0.584
	(0.405)	(0.435)	(0.603)	(0.414)	(0.458)	(0.545)
Mean dep. var.	13.2	15.3	8.5	13.2	15.3	8.5
Observations	40,952	25,548	9,666	40,952	25,548	9,666
B. Murder						
Treatment*Post	0.077	0.023	0.090	0.035	−0.065	0.106
	(0.052)	(0.062)	(0.096)	(0.053)	(0.078)	(0.131)
Mean dep. var.	0.4	0.5	0.3	0.4	0.5	0.3
Observations	39,715	24,991	9,218	39,715	24,991	9,218
C. Robbery						
Treatment*Post	−0.073	−0.003	−0.159	0.009	−0.008	0.241
	(0.165)	(0.177)	(0.231)	(0.183)	(0.192)	(0.220)
Mean dep. var.	3.9	4.4	2.6	3.9	4.4	2.6
Observations	40,952	25,548	9,666	40,952	25,548	9,666
D. Aggravated assault						
Treatment*Post	−0.242	−0.231	0.263	0.036	−0.212	0.249
	(0.283)	(0.300)	(0.429)	(0.255)	(0.277)	(0.347)
Mean dep. var.	8.9	10.4	5.6	8.9	10.4	5.6
Observations	40,952	25,548	9,666	40,952	25,548	9,666
E. Property crime						
Treatment*Post	−0.521	−0.441	−2.189	0.177	−0.049	0.273
	(0.589)	(0.614)	(1.895)	(0.659)	(0.587)	(1.048)
Mean dep. var.	11.3	12.1	8.9	11.3	12.1	8.9
Observations	40,952	25,548	9,666	40,952	25,548	9,666
F. Burglary						
Treatment*Post	0.352***	0.328**	0.227	0.219*	0.151	0.276*
	(0.128)	(0.135)	(0.182)	(0.127)	(0.132)	(0.164)
Mean dep. var.	2.5	2.7	1.9	2.5	2.7	1.9
Observations	40,952	25,548	9,666	40,952	25,548	9,666
G. Theft						
Treatment*Post	−0.975*	−0.886	−2.701	−0.121	−0.290	−0.124
	(0.539)	(0.559)	(1.934)	(0.618)	(0.539)	(1.021)
Mean dep. var.	7.5	7.8	5.9	7.5	7.8	5.9
Observations	40,952	25,548	9,666	40,952	25,548	9,666
H. Motor vehicle theft						
Treatment*Post	0.102	0.117	0.285	0.079	0.090	0.121
	(0.093)	(0.098)	(0.182)	(0.070)	(0.081)	(0.135)
Mean dep. var.	1.4	1.6	1.0	1.4	1.6	1.0
Observations	40,952	25,548	9,666	40,952	25,548	9,666
Eligible sample	X	-	-	X	-	-
Border sample	-	X	-	-	X	-
Similar sample	-	-	X	-	-	X

Notes: Estimates of the Opportunity Zones designation on arrests. Robust standard errors clustered at the census tract level in parentheses. Columns (1), (2), and (3) use the difference-in-differences (DiD) estimation. Columns (4), (5) and (6) employ a propensity score weighting in a difference-in-differences (PSM-DiD) model. Columns (1) and (4) include the low-income, eligible and designated census tracts. Columns (2) and (5) consider the low-income designated and their bordering, eligible census tracts. Columns (3) and (6) use the low-income, eligible and similar tracts (with poverty rates between 5 and 35 percent and between 65 and 95 percent of the greater statewide or metropolitan area median family income). *p<0.1; **p<0.05; ***p<0.01.

Table E.4: Regression discontinuity estimates of the Opportunity Zones designation on arrest

	(1)	(2)	(3)	(4)
A. Violent crime				
Designated OZ	5.27	0.35	16.30	14.28
	(6.58)	(4.91)	(26.29)	(12.87)
Mean dep. var.	7.2	7.2	7.7	7.7
Observations	2,017	2,017	3,079	3,079
B. Murder				
Designated OZ	−0.17	−0.50	1.26	0.52
	(0.82)	(0.67)	(1.87)	(1.46)
Mean dep. var.	0.2	0.2	0.2	0.2
Observations	1,898	1,898	2,906	2,906
C. Robbery				
Designated OZ	−0.13	−0.37	0.64	2.26
	(2.85)	(2.17)	(7.98)	(4.96)
Mean dep. var.	2.2	2.2	2.4	2.4
Observations	2,017	2,017	3,079	3,079
D. Aggravated assault				
Designated OZ	5.58	1.18	13.63	11.61
	(4.82)	(3.33)	(21.09)	(9.74)
Mean dep. var.	4.8	4.8	5.2	5.2
Observations	2,017	2,017	3,079	3,079
E. Property crime				
Designated OZ	23.78	19.74	83.10	32.30
	(19.70)	(14.54)	(103.88)	(38.41)
Mean dep. var.	9.6	9.6	9.4	9.4
Observations	2,017	2,017	3,079	3,079
F. Burglary				
Designated OZ	2.41	1.47	5.23	5.38
	(2.80)	(2.17)	(11.48)	(5.53)
Mean dep. var.	1.8	1.8	1.8	1.8
Observations	2,017	2,017	3,079	3,079
G. Theft				
Designated OZ	20.05	16.63	80.97	26.28
	(18.83)	(13.95)	(100.46)	(36.52)
Mean dep. var.	6.8	6.8	6.6	6.6
Observations	2,017	2,017	3,079	3,079
H. Motor vehicle theft				
Designated OZ	1.33	1.64	−3.10	0.64
	(1.64)	(1.36)	(6.90)	(2.91)
Mean dep. var.	0.9	0.9	0.9	0.9
Observations	2,017	2,017	3,079	3,079
Bandwidth	IK: 0.25	IK: 0.25	CC: 0.40	CC: 0.40
Polynomial function	Linear	Quad	Linear	Quad

Notes: Fuzzy regression discontinuity estimates of the Opportunity Zones designation on arrests. Robust standard errors clustered at the census tract level in parentheses. All panels use a designated Opportunity Zone (OZ) indicator variable instrumented with an indicator of being above the eligiblity threshold. The optimal bandwidths (IK and CC) follow Imbens and Kalyanaraman (2012) and Calonico et al. (2015). *p<0.1; **p<0.05; ***p<0.01.