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

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Abstract

Objective: Relying solely on the criminal justice system and law enforcement to prevent crime is costly to society. One alternative is the role of place-based capital investment policies aiming to foster economic growth and job creation in distressed areas. However, there is limited research on the crime effects of such interventions. The Opportunity Zones program, created as part of the 2017 Tax Cuts and Jobs Act, allows assessing the public safety effects of a prominent place-based policy providing substantial tax benefits to capital investments in low-income census tracts. This research evaluates the early impacts of the Opportunity Zones program designation on economic conditions and public safety in 31 major US cities.

Methods: Regression discontinuity and difference-in-differences methods were used to address concerns that designated census tracts are different from non-selected areas in (un)observable characteristics.

Results: The causal reduced-form estimates suggest that the program has not caused neighborhood changes at least four years after its implementation, measured by urban development, property prices, poverty, employment, and income levels. Accordingly, it has not impacted public safety, comprised of calls for service, police stops, crimes, and arrests. The null effects do not mask city-specific improvements, and there are no impacts on detailed crime and arrest categories.

Conclusions: The evidence suggests that place-based capital investment policies are limited alternatives to influence short-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

Uneven economic growth and structural changes have restricted the economic mobility of the most disadvantaged, which, compounded with historical racism and neighborhood disinvestment, have led to the increase in inner-city poverty (Sampson et al., 2018; Wilson, 2003). Growing up in these areas has long-lasting adverse effects on residents' life outcomes and intergenerational mobility (Chetty and Hendren, 2018; Sharkey and Torrats-Espinosa, 2017). 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 should aim to improve the areas where people live (Sampson, 2016; Sharkey, 2013). Simultaneously, the widespread idea that crime is a consequence of material deprivation is one of the oldest and recurring topics on the precursors of crime literature (Agnew, 1992; Merton, 1938; Wright, 1893). Evidence supports that crime is an economic phenomenon as unemployment (Aaltonen et al., 2013; Machin and Meghir, 2004; Raphael and Winter-Ebmer, 2001), poverty (Chamberlain and Hipp, 2015; Sharkey et al., 2016), and inequality (Hipp and Kubrin, 2017; Kelly, 2000) influence criminal behaviors.[1] Furthermore, concentrated disadvantage is a common determinant explaining that neighborhoods experiencing most of the crime victimization are also the ones where most convicted offenders live and return after prison –the spatial concentration of crime and mass incarceration– (Sampson and Loeffler, 2010; Simes, 2018). Consequently, community investments are vital in improving public safety (Sharkey, 2018a,b), and their implementation have high-public support (Crabtree, 2020), so that crime prevention becomes a responsibility of the whole community and not only from police departments (Crawford and Evans, 2017).

Neighborhood investments encompass a diverse set of initiatives, and there is limited research on how to effectively target efforts at disadvantaged communities to reduce criminal involvement without requiring law enforcement presence. One approach deploys interventions on high-risk individuals and areas with a clear link to crime reducing components. The results of this approach are very promising. For example, a growing literature emphasizes the benefits of providing behavioral therapy among criminally involved and economically disadvantaged young adults (Blattman et al., 2017; Heller et al., 2017), offering summer jobs opportunities and mentoring to youth enrolled in high-violence schools (Davis and Heller, 2020; Heller, 2014), funding local nonprofits focusing on crime and community life (Sharkey, 2018a), and changing the neighborhood's built environment by greening and remediating the urban space (Branas et al., 2018; Kondo et al., 2015) and expanding street lighting (Chalfin et al., 2021a; Mitre-Becerril et al., 2022).

[1] There is a related literature on local labor policies benefiting the low-income and at-risk groups achieving crime reductions (Heller, 2014; Yang, 2017) or at least not compromising public safety (Mitre-Becerril and Chalfin, 2021).

Another approach centers on providing fiscal incentives (e.g., tax benefits, subsidies, cash grants) for new jobs, businesses, and capital investments to promote local economic growth in delimited areas. These policies, known as place-based interventions, can deal with pockets of distress by focusing on the vitality of a place and increasing the residents' well-being (Bartik, 2020c; Ladd, 1994; Neumark and Simpson, 2015). Crime reduction is not a primary goal of these interventions; still, they can influence public safety by modifying the socioeconomic context, reducing inequality and social disorganization, and changing the opportunity cost of crime. These spatially targeted economic development interventions 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 are the New Markets Tax Credit, the Enterprise Zones, and the Empowerment Zones. These policies have in common the provision of tax incentives to businesses and development projects to encourage economic growth in specific areas.[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), and there is disagreement about its features. Still 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 is 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., 2022; Corinth and Feldman, 2021; Freedman et al., 2021), but it seems to have heterogeneous effects (Arefeva et al., 2021; Atkins et al., 2021; Sage et al., 2021; 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, mainly covering data since the mid-2000s and 2010s, limiting measuring their crime effects. 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

[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.

and survey data from 31 of the largest US cities.

While this research uses similar econometric methods and data sources to previous Opportunity Zones studies (Chen et al., 2022; Corinth and Feldman, 2021; Freedman et al., 2021), it makes four contributions. First, this research includes up to four years of post-intervention coverage for several outcomes, providing more time to measure neighborhood impacts. A longer time horizon is essential for evaluating policies with medium- and long-term goals and programs deployed amidst an unprecedented global pandemic with considerable economic and sociodemographic pitfalls. Furthermore, the timeline of the Opportunity Zones program meant that 2019 was the latest year to make investments to receive most of the tax benefits, followed by 2021 as another relevant deadline to obtain a considerable tax reduction on the taxpayers' investment basis. Hence, 2019 and 2021 are key years to start seeing neighborhood changes (if any) caused by the policy. Second, this research includes outcomes from administrative records at the census tract level to assess other margins of the program not covered by previous studies, such as using construction and zoning permits and small business loans to measure local urban development changes without relying on survey data usually aggregated at higher geographical levels (e.g., city or zip code) and with more statistical uncertainty that could limit finding significant results. Next, this research is the first to evaluate the impact of the Opportunity Zones on public safety broadly understood (calls for service, police stops, crimes, and arrests). While assessing changes in serious crime is the most common metric in research, which relates to its availability and standardization at the agency level,[4] and the high cost of these crimes to society (Cohen and Piquero, 2009), other public safety outcomes are equally relevant to understand neighborhood changes. For instance, calls for service have been recognized as an alternative to measure crime and public safety demand, with fewer concerns about selective reporting from law enforcement agencies (Bursik Jr and Grasmick, 1993; Klinger and Bridges, 1997; Maxfield, 1982). Likewise, police stops and arrests can signal residents' increased demand for public safety, but also a police behavioral response to new urban developments and gentrification (Beck, 2020; Laniyonu, 2018). It also helps to identify the causal mechanisms at play. To the extent that crime and arrests decrease in similar magnitude, there could be evidence of deterrence rather than incapacitation effects. Accordingly, using a diverse set of public safety outcomes provides a better understanding of any neighborhood change caused by the program. Finally, by using data from 31 US cities to measure the Opportunity Zones' impacts on economic changes and crime effects,[5] this research faces fewer concerns about external validity than single-city case studies and provides

[4] The FBI's Uniform Crime Reporting, which began in 1929, compiles and reports agency-level crime data from nearly all law enforcement agencies in the US.

[5] Calls for service, police stops, and arrests outcomes use data from nine, ten, and eleven cities, respectively, due to data limitations.

more statistical power to detect small changes and estimate city-specific impacts.

Using regression discontinuity and difference-in-differences estimators, the results suggest that subsidizing capital tax investments does not cause neighborhood changes, at least four years after its implementation, measured by urban development, property prices, poverty, employment, and income levels. Moreover, it does not improve 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 improvements. Similarly, there are no effects on detailed crime and arrest categories. The evidence suggests that national place-based capital investment policies are a limited alternative to influence short-term community changes and public safety improvements. It also highlights the challenges of implementing these initiatives to deter crime.

The remaining of the article is organized as follows. Section 2 reviews the literature on place-based interventions, economic growth, and crime, including 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. But the private sector faces 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 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 that later became the Opportunity Zones program.

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 the 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 vehicle organized as a corporation or partnership 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, 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 provide relief for investors facing hardships meeting the 180 days deadline amidst the COVID pandemic, the IRS (2020, 2021) extended the deadlines up 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 at the latest by 2019 (2021). 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 the \$15,000 of the original investment; the investor would only pay taxes on the \$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.[6]

2.2 Prior literature

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). By encouraging new jobs in disadvantaged areas, these initiatives can also address market failures that partially explain the low Black employment rates to comparable White individuals (the spatial mismatch and racial mismatch hypotheses).[7] Even if these policies do not create new jobs, their redistribution to areas that lack them could benefit the most disadvantaged individuals, particularly racial minorities.

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). There is evidence that 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 to be 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 are having negative impacts (Bondonio and Greenbaum, 2007).

Possible explanations for these diverse findings on place-based initiatives could be that the tax incentives change across locations as the programs have different priorities (e.g., real estate vs. community development focused, business climate vs. residents' welfare). Also, the programs' expansion could dilute their impacts by including less distressed areas (Greenbaum and Bondonio, 2004; Greenbaum and Lan-

[6]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

[7]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 the 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.

ders, 2009). No research has studied the effects of these place-based initiatives on public safety, which is a relevant knowledge gap that this paper contributes to close.

Another related 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, 2021). Public-private investments in new mixed-income developments while impacting property prices 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.

Whether the Opportunity Zone program fosters public safety improvements is an empirical question. To the extent that the program encourages urban development, particularly on vacant lots, or decreases vacancy rates, evidence suggests a crime decrease (Branas et al., 2018; Cui and Walsh, 2015; Spader et al., 2016). New development projects that positively impact construction jobs would also reduce criminal offenses, especially for those with a criminal background (Schnepel, 2018), and overall lower unemployment rates relate to crime decreases (Aaltonen et al., 2013; Raphael and Winter-Ebmer, 2001). Although real estate and construction projects are the most likely candidates for new investments due to their high-intensity capital requirements, the Opportunity Zones program allows investing in any business so that manufacturing, retail, and professional service companies, among others, can also benefit from the tax incentives. Evidence suggests that business activities, particularly those that attract foot traffic, can reduce crime (Chang and Jacobson, 2017). Likewise, investments leading to more mixed-land use areas can reduce crime by creating natural surveillance mechanisms (Jacobs, 1961; Twinam, 2017). Local officials could complement the private investments with public investments in the built environment, such as street lighting or greening the urban space, which would also reduce criminal behaviors (Chalfin et al., 2021a;

Locke et al., 2017).

While the previous mechanisms imply changes in the built environment and urban development, there are other pathways to influence criminal behaviors. Such as persistent poverty and crime concentration can impact residents' expectations and behaviors by internalizing crime and disorder-prone behaviors (Sampson et al., 2018), a change in people's expectations about future improvements in the community can be equally effective as the investment to improve the economic conditions and reduce crime.[8] Specifically, the announcement of development projects signals the intention and commitment of investors to improving an area so that residents, developers, and business owners adjust their beliefs and behaviors about future neighborhood conditions before the investment takes place. This situation can translate into new housing units, property renovations, and higher property prices, as the announcement on new construction projects has shown (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 evidence that the announcement of a new transit option decreased crime before its construction and opening (Billings et al., 2011), despite its null effects on the local labor market (Canales et al., 2019).

Policy evaluations on the early impacts of the Opportunity Zones program have reported mixed results. Arefeva et al. (2021) found that the program created employment and establishments growth in metropolitan areas across different industries and subpopulation groups. Sage et al. (2021) showed that vacant lots and older properties had price increases compared to similar properties at eligible, not designated tracts. Atkins et al. (2021) reported no overall increase in job postings, but there were positive impacts in urban and high Black populated areas. Xu (2021) found an overall increase in private investments but at the expense of a decrease in entrepreneurship in the non-tradable sector (e.g., retail, restaurants). Others have found limited 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., 2022; Corinth and Feldman, 2021; Freedman et al., 2021). Accordingly, analyzing the Opportunity Zones program on a subset of high populated cities is relevant to better understand its impacts (if any) on economic changes and public safety.

[8] While the role of an announcement before the actual policy change is not a common mechanism explaining behavioral changes in criminology, it is common in other fields such as monetary policy and the stock market (Bomfim, 2003).

3 Data

3.1 Data sources

Academic 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 from the 51 states and the District of Columbia. These outcomes are available on public and private national repositories. Specifically, all the studies have used the American Community Survey, and some complement it with data from the Federal Housing Finance Agency (Chen et al., 2022), Real Capital Analytics Commercial Real Estate Database (Corinth and Feldman, 2021; Sage et al., 2021), Your-economy Time Series information (Arefeva et al., 2021), Burning Glass Technologies (Atkins et al., 2021), and OpenCorporates (Xu, 2021), among others.

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.[9] A current effort to create a more detailed national repository – the Criminal Justice Administrative Records System– has partial coverage of criminal justice cases (e.g., incarceration, probation, and parolee), available on a case-by-case basis, subject to approval, but does not identify the location of the incident.[10] Another recent initiative is the Stanford Open Policing Project that offers standardized, time-stamped, location police stop data for selected local jurisdictions (Pierson et al., 2020), but most of it is outdated for this research objective (e.g., no post-2018 data).[11]

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.[12] Data on calls for service, police stops, crime, and arrests come from each city's police department. The crimes

[9] The Federal Bureau of Investigation's UCR, replaced in 2021 with the National Incident-Based Reporting System, provides information at the county or agency level (police department or sheriff's office), and the Bureau of Justice Statistics National Crime Victimization Survey offers some subnational estimates with practical limitations.

[10] The geographical coverage includes 23 states representing 44% of the US population. See https://cjars.isr.umich.edu/introductory-webinar/

[11] See https://openpolicing.stanford.edu/

[12] 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.

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.[13] Non-major crimes are all the other incidents reported to the police departments.

To measure urban development, this research uses construction, zoning, and land-use change permits. In comparison to Chen et al. (2022) that 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, this research 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. Furthermore, 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 mortgages purchased or securitized by Fannie Mae or Freddie Mac.[14]

While investors need to 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.[15] 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 investments 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 private equity investments in operating businesses.[16] 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.[17] The equity investments are restricted to non-banking,

[13] Rape is excluded as several departments do not disclose its location to protect the victims' privacy.

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

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

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

[17] 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.

non-financial services companies.[18] 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 changing this restriction). The investments were aggregated at the census tract year level by geocoding the address of the operating business.[19]

The analysis also includes socioeconomic and demographic variables collected from the American Community Survey (ACS), which is 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 income. 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. The ACS data is only available up to 2020. Also, this year, 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.[20] Finally, this research relies on the list of designated and eligible census tracts compiled by the Urban Institute.[21]

3.2 Analytical database

This research follows pre-specified data collection criteria to avoid criticisms of strategically selecting data to show some desired effects. **Appendix A** provides a detailed explanation of the data collection process, but in summary, it goes as follows. The data review starts from the most to the least populated US cities based on the 2010 Census estimates, selecting a city if it satisfied at least two conditions. First, it must have public crime data from 2015 that could be aggregated to the census tract-year level. Then, it must have at least one dataset on arrests, calls for service, police stops, or planning permits that could also be computed at the tract-year level. Thirty-one cities satisfied these requirements among the 76 jurisdictions revised during the data collection process. Cities not providing the longitude/latitude values of the public

[18] It excludes companies in the banking and financial services (commercial banking, insurance, investing, investment banking, and pooled investment fund).

[19] 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.

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

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

safety incidents or planning permits were geocoded based on the street address.[22]

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.[23] **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 304 and 176 non-major and major crimes in any given year, 48 and 32 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 204 and 33 non-major and major crime arrests in the designated tracts, but the incidents decreased to 111 and 21 among the eligible tracts. These numbers translate to 52 to 67 arrests for every 100 non-major crimes and about 16 to 19 arrests per 100 major crimes among designated and eligible tracts. Arrests for aggravated assaults are the most common event, followed by thefts and robberies. 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, while this figure goes down to 82% in the eligible group. Eligible tracts raised nearly twice private equity investments as their designated counterparts. Still, such tracts have fewer planning permits (29.7 vs. 26.4). 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 (25.8% vs. 20.4%) and fewer college graduates (25.6% vs. 34%). The unemployment rate in the average 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 a 7.4 percent lower appreciation in the designated than the eligible tracts. The former tracts have received more small business loans too.[24]

[22] A manual revision of a random sample of the incidents revealed that the hit rate for geocoding crime data was above the minimum acceptable target indicated by Ratcliffe (2004).

[23] **Appendix Figure B.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.

[24] The sociodemographic and crime differences hold across cities (**Appendix Table B.1**). However, in some cities, the designated tracts are safer than the eligible ones, and they have more building permits, highlighting the importance of conducting a heterogeneity analysis across cities.

The descriptive statistics show that 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 Zones 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).[25] 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 the results are not driven by a methodological choice and allows measuring different margins of the policy. For example, the difference-in-differences method can estimate a heterogeneity analysis at the city level, while 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.

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.

[25] For example, Times Squares in New York City is a good 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.

The econometric specification is as follows:

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

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.[26] 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. Even though crime displacement is not common in small (Johnson et al., 2014) and large areas (Telep et al., 2014), if there are geographical spillovers, this group could underestimate the effects. 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).[27] This comparison reduces the sample size, but the treated and control groups are more similar while reducing any 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 (population 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

[26] 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.

[27] 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.

is common among the Opportunity Zones (Arefeva et al., 2021; Chen et al., 2022; Corinth and Feldman, 2021; Freedman et al., 2021; Sage et al., 2021) 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 exploits 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 the 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.[28] 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 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 (2018-2021) 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) 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

[28] 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.

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, restricting the sample 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 consist in combining the poverty and income ratio threshold into a single standardize 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 any of the two thresholds become binding.

The regression discontinuity design forms part of the quasi-experimental methods that strengthen the link between rigorous evidence and policy evaluation in criminology (Berk et al., 2010; Blumstein, 2013; Braga and Weisburd, 2013). In crime research, this method has been used to estimate treatment effects of processing juveniles as adults (Loeffler and Grunwald, 2015), private police (MacDonald et al., 2016), prison sentences (Mitchell et al., 2017), facility security classification (Tahamont, 2019), access to alcohol (Chalfin et al., 2019), and racial disparities (Pierson et al., 2020).

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. Both approaches lead to the same conclusions.

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 change, while the planning permits and house price index have 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 into 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, there is no evidence to suggest that the parallel trends do not hold for any of the 14 variables.[29]

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). The results suggest that the program significantly increased the small business loans by five percent (32 thousand dollars), the price of houses by around percent, and planning permits by 10.8 percent (2.7 additional permits). In comparison, it reduced the unemployment rate by around 10 percent (1.0 to 1.2 percentage points) and the family income by 2.3 to 4.7 percent (0.9 to 2.1 thousand dollars). However, as the parallel trends assumption does not hold for these specifications, Columns (1) and (2) estimates do not solely reflect the effect of the Opportunity Zones program but also self-selection intro treatment. Column (3) uses the baseline difference-in-differences on the similar tracts sample (±15 percentage points from the threshold), revealing that only the housing prices remained statistically significant.

Economic growth should translate into better quality of life outcomes. Also, to the extent that property owners and investors believe that the Opportunity Zones will foster economic growth in the future, we should expect an increase in rents, higher property prices, and more urban development measured through construction and zoning permits. However, the propensity score weighting in a difference-in-differences setting in Columns (4), (5), and (6) presents that most of the economic outcomes are no longer statistically significant. Only the family income significantly decreased using all the eligible tracts as a control group, but using the bordering (similar) sample, the 95% confidence interval rules out decreases greater than a 3.0

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

(5.9) percent change. Other outcomes, such as the planning permits and the unemployment rate, change their sign once the model down-weights observations that had different pre-intervention trends. Importantly, while the effects on equity investments capital are positive, there are far from being statistically significant, suggesting that the program had limited impacts on attracting private equity from individual and institutional investors for the mean Opportunity Zone tract. Overall, the evidence suggests that the effects of the Opportunity Zone designation on equity investments, business loans, urban permits, property prices and rents, family income, and poverty and unemployment rates are indistinguishable from zero.

As there is limited evidence of the Opportunity Zones impact on economic improvements, the only mechanism that could influence public safety outcomes is a change in the expectations of the residents, developers, and business owners about future neighborhood conditions even if the investments have not been deployed yet. **Table 3** provides the difference-in-differences point estimates on public safety.[30] Using the baseline model, Columns (1), (2), and (3) suggest a decrease in police stops between 10.5 to 17.4 percent (58 to 80 fewer stops), in non-major crime arrests in the range of 17.1 to 32.8 percent (around 32 fewer arrests), and in major crime arrests of about 4.2 to 7.2 percent (between 1.2 to 1.8 fewer arrests). These results have a self-selection bias as the parallel trends assumption does not hold in these specifications. Columns (4), (5), and (6) address this concern by using the propensity score weighted difference-in-differences design. Only two out of 18 estimates show a significant change (calls for service and non-major crimes). Still, given that the statistical significance does not hold across subsamples, these results are 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 the difference-in-differences specification is that the eligible but not selected tracts are different in unobservable characteristics even after down-weighting units violating the parallel trend assumption. Consequently, the lack of significant results after four years of enacting the Opportunity Zone program is due to unobserved bias. 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 exploits the poverty and income thresholds eligibility for the program. **Figure 5** presents how the poverty rate and median family income ratio thresholds change the probability

[30] 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.

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 were 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 D** 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 significant changes in socioeconomic and public safety changes at the neighborhood level. Consequently, the lack of null impacts is not due to a methodological choice, but rather the capital investment place-based intervention has not had meaningful effects on local conditions even after four years of its implementation.

5.3 Heterogeneity

One concern is that the difference-in-differences estimates mask city-specific improvements. **Appendix E** presents the difference-in-differences estimates by city.[31] The magnitude and sign of the coefficients are very similar across the two samples. While some cities may have experienced some changes, there are no consistent impacts on the economic and public safety outcomes, so the null results hold across individual cities.

Another concern is that the lack of significant results on aggregated crime and arrests could be hiding public safety impacts on specific UCR Part I crime categories that could be more prone to neighborhood conditions or changes in the residents' expectations about future conditions. **Appendix F** 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 there may have been some crime changes on specific outcomes, the lack of consistent, significant results suggests no impacts on any of the UCR Part I crime categories.

Finally, there is evidence of spatial concentration of the Opportunity Zone investments, which could explain the null impacts of most studies on this initiative (Kennedy and Wheeler, 2021). To explore

[31] The difference-in-differences using the similar sample and regression discontinuity city-level estimates were not estimated due to low sample size.

this idea, **Table 4** 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) using a triple differences framework. The 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 negligible impacts on economic and public safety conditions after four years of its implementation among the 31 cities included in this study. This section discusses possible explanations for these results.

One explanation could be that the unit of analysis was not the appropriate one to deliver a dosage high enough to cause local changes –a concern that experimental risk-focused crime prevention strategies also have experienced (Weisburd et al., 2008). While criminologists and sociologists usually use census tracts as the common scale to operationalize the neighborhood concept as it is a practical unit of analysis with consistent, available data, they can be insufficient to assess how the community affects the people within them (Sharkey and Faber, 2014).[32] Furthermore, the neighborhood mechanisms influencing criminal propensities are expected to operate at different geographical levels (Chyn and Katz, 2021; Sampson et al., 2002). Opportunity theory mechanisms happening at a small scale (e.g., blocks and streets segments), collective efficacy and disorganization processes situate at the mesoscale level (e.g., neighborhood) (Hipp and Williams, 2020; Kirk and Laub, 2010), while inequality and relative deprivation connect the mesoscale to macro units (e.g., large areas or city level) (Chamberlain and Hipp, 2015; Hipp and Kubrin, 2017). Consequently, policymakers should consider the underlying mechanisms that will influence behavioral changes, so the investments focus on such geographical level.

Economists also have 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– and they could cause gentrification and residents' displacement (Bartik, 2020a,b). Counterarguments say these concerns may be overestimated, and 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), but state-level interventions (Enterprise Zones) have been more flexible on their

[32] The American Community Survey five-year estimates are also available to the block group level, but due to privacy protections and statistical unreliability, several outcomes are not disclosed in low-populated areas.

geographical boundaries. In both cases, the evidence on economic impacts is mixed, suggesting that the unit of analysis is relevant. Still, it is unlikely the sole reason explaining the lack of significant results.

Another consideration related to the unit of analysis is the dosage and features of the intervention. On the one hand, 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 good investments, making them more competitive compared to profitable alternative opportunities (GAO, 2021). To compare the tax benefits among other policies, the New Markets Tax Credit provides 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. On the other hand, 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,[33] 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 foster new jobs and neighborhood change.

A third explanation could be that four years is too early to assess the economic and public safety conditions of a policy encouraging medium-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 days restriction to defer their capital gains taxes. Then, they use the 30 months

[33] 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

grace period to deploy the resources in a physical property. This process means that 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, planning permits, and property prices), likely, the investors and developers are not expecting a large impact on the local activity in the near future due to the program.

A fourth consideration of the null effects is the role of the unprecedented COVID pandemic that could have affected the influence of the program. The pandemic unlikely biased the estimates,[34] 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 is a reminder 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 key 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 of the investments made in all qualified opportunity funds, then it would be possible to 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 (Sampson, 2016). Accordingly, the idea that community investment is crucial

[34] 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.

to reducing crime echoes well in academia (Sharkey, 2018a,b) and the public opinion (Crabtree, 2020), but community investments comprise an extensive range of initiatives. While non-policing targeted efforts towards high-risk individuals (Blattman et al., 2017; Heller, 2014; Heller et al., 2017; Davis and Heller, 2020) and places (Branas et al., 2018; Chalfin et al., 2021a; Kondo et al., 2015; Mitre-Becerril et al., 2022) have shown effective crime reductions, scaling up these programs without diluting their benefits is an open challenge.[35] In contrast, 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 public safety impacts. Enacting the Opportunity Zones program at a moment when georeferenced, time-stamped public safety data has become more accessible to researchers allows providing some evidence on the topic.

The Opportunity Zones program provides capital tax benefits to investments in low-income census tracts. This research uses two alternative econometric specifications to overcome the selection into treatment bias. It 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 after four years of its implementation. The results suggest that subsidizing capital tax investments do not cause short-term neighborhood changes –measured by urban development, property prices, poverty, employment, and income– nor improve public safety –comprised of calls for service, police stops, crimes, and arrests. In addition, there are few heterogeneity impacts among the individual US cities, suggesting no city-specific improvements. Similarly, 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 important factor in the policy design. To be clear, from a policy evaluation perspective, only the new investments that the program encouraged are relevant, but from a budgetary approach, the new investments and those that would have occurred even in the absence of the program but now there are taxed at a lower rate are part of the program's fiscal cost.

Finally, this study is not without limitations. Place-based capital investments interventions may lead to economic growth, job creation, and public safety improvements in the long term if the infuse of financial

[35] The summer youth employment experiments show that the benefits persist as it scales and changes contexts (Heller, 2022), but enhancing street lighting (Chalfin et al., 2021b) and remediating vacant lots (Kondo et al., 2018) are not impervious initiatives to the local context and deployment.

resources are enough to leverage the agglomeration effects and unlock potential multipliers from local hiring and better physical infrastructure, or at least change the expectations of residents, developers, and business owners about future improvement in the neighborhood, and their willingness to intervene in solving common problems. However, this study suggests that national programs fostering private capital investments in low-income areas are a limited alternative to influence short-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 not a sufficient condition 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	303.9	335.5	212.8	262.0
Major crimes	175.8	169.2	133.3	170.6
Violent	46.8	45.4	28.5	32.2
Murder	0.9	1.5	0.5	1.1
Robbery	18.7	20.0	11.8	14.6
Aggravated assault	27.4	28.2	16.3	20.1
Property	129.0	139.2	104.8	149.7
Burglary	24.8	24.9	20.9	23.5
Theft	85.0	109.3	69.5	126.0
Motor vehicle theft	19.3	23.7	14.5	17.9
Non-major crime arrests	204.1	311.5	111.0	162.9
Major crime arrests	32.7	49.1	21.3	42.4
Violent	18.7	32.2	11.0	22.5
Murder	0.7	3.0	0.4	1.9
Robbery	5.8	13.5	3.5	10.1
Aggravated assault	12.3	18.8	7.1	11.8
Property	14.0	24.0	10.3	26.5
Burglary	3.1	6.1	2.3	5.7
Theft	9.0	20.1	6.9	22.6
Motor vehicle theft	1.9	2.9	1.2	2.1
Calls for service	2,827.1	3,108.3	1,907.6	1,722.9
Police stops	627.5	1,227.7	366.7	857.4
Low-income tract (%)	98.3	13.0	82.3	38.2
Contiguous tract (%)	1.7	13.0	17.7	38.2
Equity investments (millions)	0.8	5.3	1.6	21.7
Planning permits	29.7	50.7	26.4	49.4
Population (thousands)	3.6	2.0	3.8	1.8
White (%)	31.3	24.8	45.2	28.5
Black (%)	44.7	34.4	30.2	32.0
Hispanic (%)	28.4	28.1	26.2	26.4
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 (%)	23.6	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 (%)	25.8	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	18.7
Unemployment rate (%)	15.6	8.7	10.9	6.6
Family income (thousands)	39.0	18.1	53.9	25.6
Poverty rate (%)	34.3	13.7	24.8	12.9
Gross rent (thousands)	0.9	0.3	1.1	0.3
House price index (Y2000=100)	242.1	158.9	261.4	152.7
Small business loans (thousands)	848.5	1,923.4	751.8	1,521.0

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	0.161	0.172	0.672	0.279	−0.096	0.325
	(0.145)	(0.142)	(0.419)	(0.426)	(0.651)	(0.246)
Mean dep. var.	0.9	0.5	0.6	0.9	0.5	0.6
Observations	38,410	24,571	8,815	38,410	24,571	8,815
B. Small business loans (thousands)						
Treatment*Post	25.126	32.232*	55.480	−10.693	−31.957	42.173
	(20.081)	(18.152)	(57.910)	(31.368)	(43.539)	(50.129)
Mean dep. var.	688.2	636.0	723.6	688.2	636.0	723.6
Observations	33,606	21,498	7,713	33,606	21,498	7,713
C. Planning permits						
Treatment*Post	2.743**	1.857	4.014	1.603	−0.098	3.901
	(1.125)	(1.159)	(3.913)	(1.453)	(2.665)	(3.194)
Mean dep. var.	25.2	27.1	22.9	25.2	27.1	22.9
Observations	35,772	22,750	8,183	35,772	22,750	8,183
D. House price index (Y2000=100)						
Treatment*Post	9.848***	7.149*	15.961*	0.592	0.763	−0.712
	(3.650)	(3.942)	(8.712)	(3.524)	(3.951)	(8.305)
Mean dep. var.	241.5	237.1	272.5	241.5	237.1	272.5
Observations	13,155	7,751	4,282	13,155	7,751	4,282
E. Gross rent (thousands)						
Treatment*Post	−0.020***	−0.006	−0.016	−0.001	0.003	−0.021
	(0.004)	(0.005)	(0.013)	(0.006)	(0.006)	(0.018)
Mean dep. var.	1.0	0.9	1.1	1.0	0.9	1.1
Observations	33,396	21,370	7,692	33,396	21,370	7,692
F. Family income (thousands)						
Treatment*Post	−2.157***	−0.962**	−1.319	−1.072*	−0.250	−1.054
	(0.374)	(0.408)	(1.059)	(0.563)	(0.520)	(1.232)
Mean dep. var.	44.9	41.5	57.8	44.9	41.5	57.8
Observations	32,994	21,096	7,684	32,994	21,096	7,684
G. Poverty rate (0.01=1%)						
Treatment*Post	−0.003	−0.002	0.004	0.005	0.003	0.003
	(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	33,599	21,491	7,713	33,599	21,491	7,713
H. Unemployment rate (0.01=1%)						
Treatment*Post	−0.012***	−0.010***	−0.002	0.001	0.001	−0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)
Mean dep. var.	0.1	0.1	0.1	0.1	0.1	0.1
Observations	33,593	21,485	7,712	33,593	21,485	7,712
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	21.10	48.41	103.37	52.15	50.44	115.72**
	(35.58)	(37.85)	(106.92)	(35.26)	(45.25)	(57.86)
Mean dep. var.	2,179	2,407	1,797	2,179	2,407	1,797
Cities	9	9	9	9	9	9
Observations	9,607	6,711	2,173	9,607	6,711	2,173
B. Police stops						
Treatment*Post	−80.32***	−58.14**	−51.82	8.18	9.95	−9.34
	(23.82)	(25.00)	(36.35)	(24.83)	(28.23)	(20.41)
Mean dep. var.	461	553	277	461	553	277
Cities	10	10	10	10	10	10
Observations	23,029	14,256	5,554	23,029	14,256	5,554
C. Non-major crimes						
Treatment*Post	−2.99	−4.95	26.67	5.54	0.13	16.47*
	(5.17)	(5.41)	(21.89)	(5.51)	(5.12)	(8.63)
Mean dep. var.	244	268	187	244	268	187
Cities	29	29	29	29	29	29
Observations	36,658	23,258	8,618	36,658	23,258	8,618
D. Major crimes						
Treatment*Post	0.99	−0.66	5.21	6.56	−0.78	3.55
	(2.01)	(2.10)	(4.12)	(5.47)	(2.60)	(3.58)
Mean dep. var.	145	160	118	145	160	118
Cities	31	31	31	31	31	31
Observations	37,978	24,298	8,802	37,978	24,298	8,802
E. Non-major crime arrests						
Treatment*Post	−32.51***	−27.46***	−31.23***	−2.04	0.38	−3.69
	(5.26)	(5.45)	(11.73)	(5.49)	(6.58)	(5.39)
Mean dep. var.	139	160	95	139	160	95
Cities	11	11	11	11	11	11
Observations	26,302	16,331	6,104	26,302	16,331	6,104
F. Major crime arrests						
Treatment*Post	−1.80***	−1.21*	−2.08	1.24	−0.01	−0.84
	(0.67)	(0.71)	(1.48)	(1.57)	(0.72)	(1.46)
Mean dep. var.	25	28	16	25	28	16
Cities	11	11	11	11	11	11
Observations	26,302	16,331	6,104	26,302	16,331	6,104
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

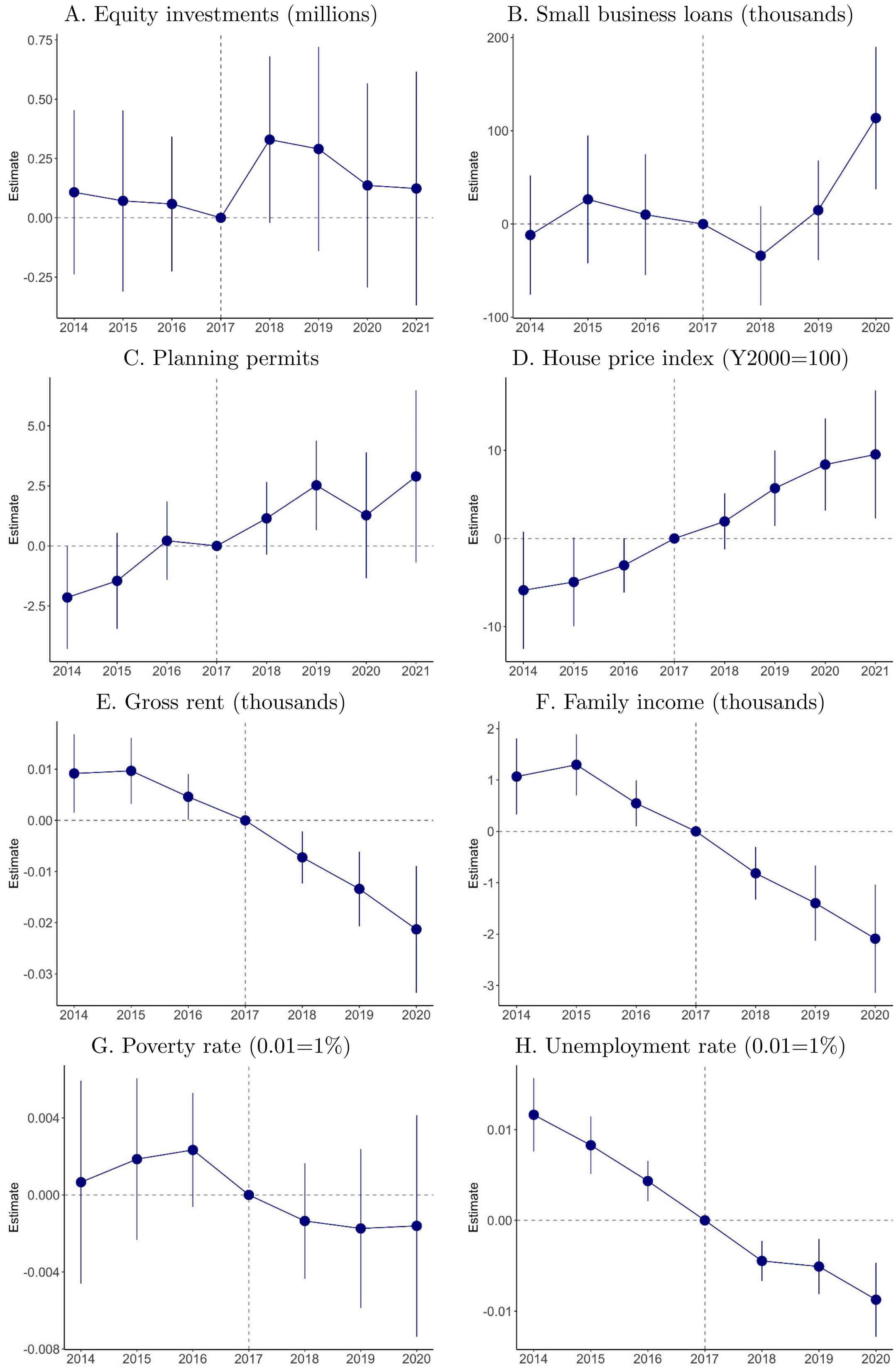
	PSM-TD (1)	PSM-TD (2)	PSM-TD (3)
A. Equity investments (millions)			
Treatment*Post*Top5	1.109	−2.791	1.427
	(3.933)	(6.750)	(1.736)
Mean dep. var.	0.9	0.5	0.6
Observations	24,571	38,410	8,815
B. Small business loans (thousands)			
Treatment*Post*Top5	−741.299***	−837.169**	104.888
	(244.227)	(358.757)	(216.493)
Mean dep. var.	688.2	636.0	723.6
Observations	21,498	33,606	7,713
C. Planning permits			
Treatment*Post*Top5	−5.693	−21.785	−6.194
	(9.081)	(25.783)	(9.565)
Mean dep. var.	25.2	27.1	22.9
Observations	22,750	35,772	8,183
D. House price index (Y2000=100)			
Treatment*Post*Top5	−2.047	5.938	4.038
	(10.236)	(12.491)	(18.364)
Mean dep. var.	241.5	237.1	272.5
Observations	7,751	13,155	4,282
E. Gross rent (thousands)			
Treatment*Post*Top5	−0.016	0.008	−0.026
	(0.024)	(0.030)	(0.044)
Mean dep. var.	1.0	0.9	1.1
Observations	21,370	33,396	7,692
F. Family income (thousands)			
Treatment*Post*Top5	−3.456	1.979	0.040
	(2.566)	(2.853)	(3.371)
Mean dep. var.	44.9	41.5	57.8
Observations	21,096	32,994	7,684
G. Poverty rate (0.01=1%)			
Treatment*Post*Top5	−0.003	−0.012	0.015
	(0.009)	(0.010)	(0.014)
Mean dep. var.	0.3	0.3	0.2
Observations	21,491	33,599	7,713
H. Unemployment rate (0.01=1%)			
Treatment*Post*Top5	−0.002	0.002	0.004
	(0.005)	(0.006)	(0.008)
Mean dep. var.	0.1	0.1	0.1
Observations	21,485	33,593	7,712
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

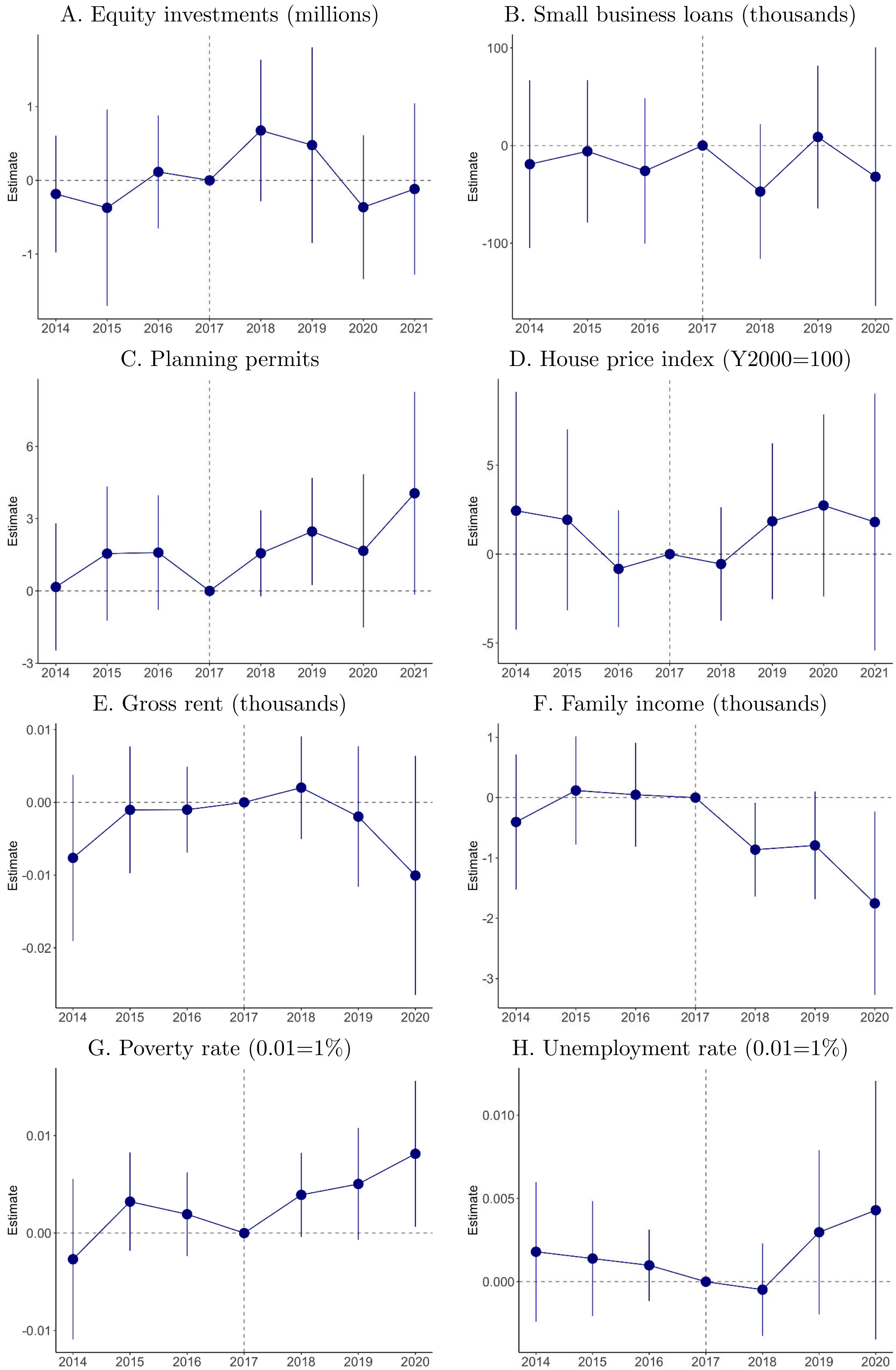
Notes: The map shows the location of the 31 US major cities included in this research. To include a city, they must have public crime data that could be aggregated to the census tract year level and at least one dataset on arrests, calls for service, police stops, or planning/construction permits that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

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



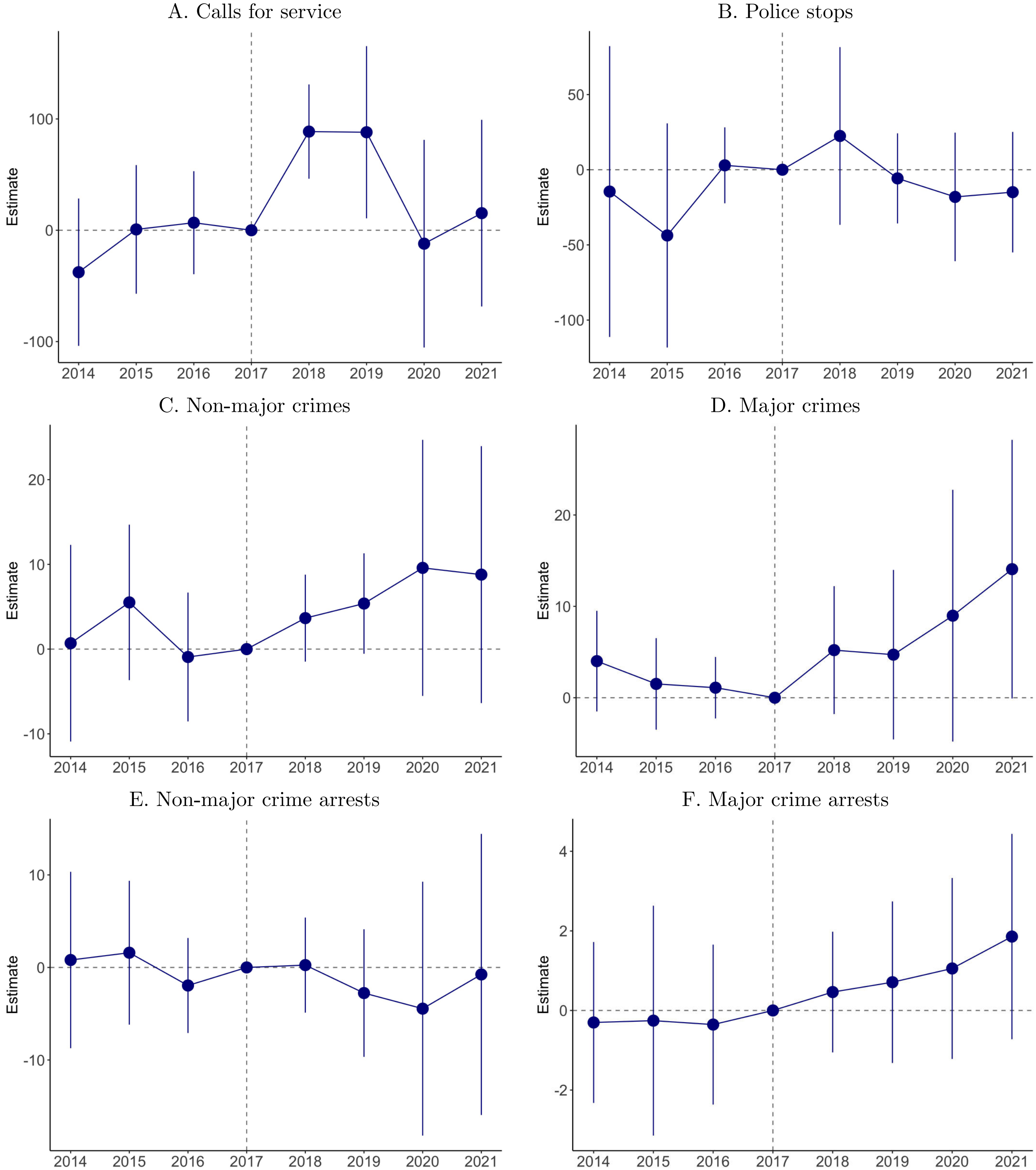
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model use the low-income, eligible census tracts sample. Gross rent, family income, poverty, and unemployment only have data up to 2020.

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 use the low-income, eligible census tracts sample. Gross rent, family income, poverty, and unemployment only have data up to 2020.

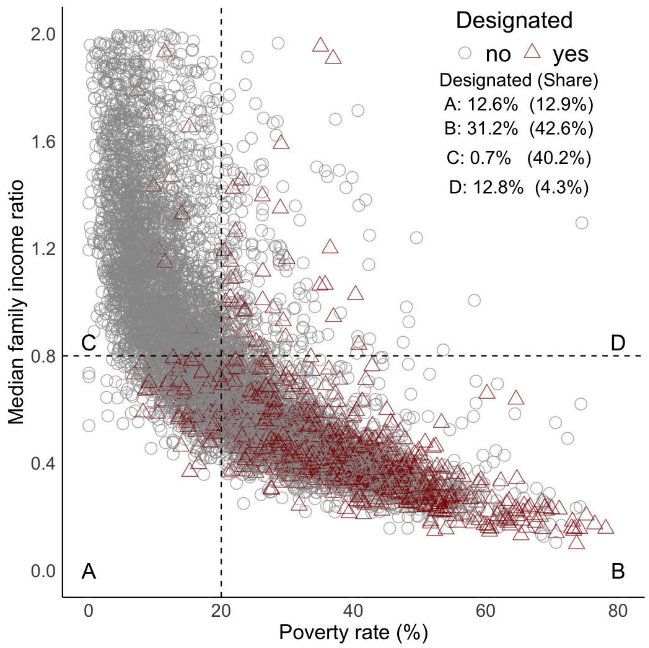
Figure 4: Propensity score weighted event study estimates on public safety, eligible sample



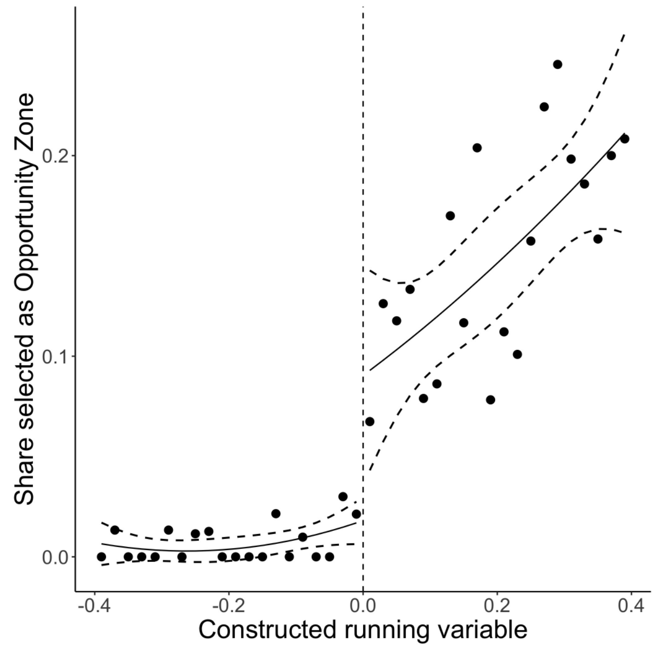
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity-score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic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

This research uses data from 31 US cities: 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. This appendix explains the process of selecting such cities and their data cleaning.

The first step was reviewing the data portals from the most to the least populated US cities based on the 2010 Census estimates. A city was chosen if it satisfied at least two conditions: 1) it must have had public crime data since 2015 that could be aggregated to the census tract-year level, but if available, the data was extracted from 2014 to 2021, and 2) the city must have had at least one dataset on arrests, calls for service, or police stops that could also be computed at the tract-year level.

The second step involved geocoding the microdata for the cities not providing incidents with longitude/latitude. Among these cities, it is common that they rounded the locations to the nearest hundred-block or blurred the last two digits of the address; such cases were replaced with a two-zero number (e.g., 12XX Street Name became 1200 Street Name). Then, this research used three geocoders to increase the probability of obtaining the XY coordinates of each incident. It first used the US Census geocoder, followed by the ArcGIS Online Geocoding Service and the Nominatim OpenStreetMap search engine. This process was done in a loop and included waiting time because geocoders block calls above their usage limit (e.g., maximum requests per second). A manual inspection of a random sample of incidents per outcome-city revealed that the hit rate for geocoding the public safety data was above the minimum acceptable hit rate indicated by Ratcliffe (2004).

Finally, this research grouped the incidents using the FBI's UCR categories: murder, rape, robbery, aggravated assault (classified as violent crimes) and burglary, theft, and motor vehicle theft (categorized as a property crime), defined as major crimes. Non-major crimes refer to all the other incidents known to law enforcement. To ensure its accuracy, the criminal offenses of this research were compared to ones reported directly to the FBI.[36] The crime categories matched well in levels and trends.

[36]Specifically, the comparison was made to the UCR data dashboard made available by Jacob Kaplan at https://jacobdkaplan.com/crime.html.

B Appendix: City-specific descriptive statistics

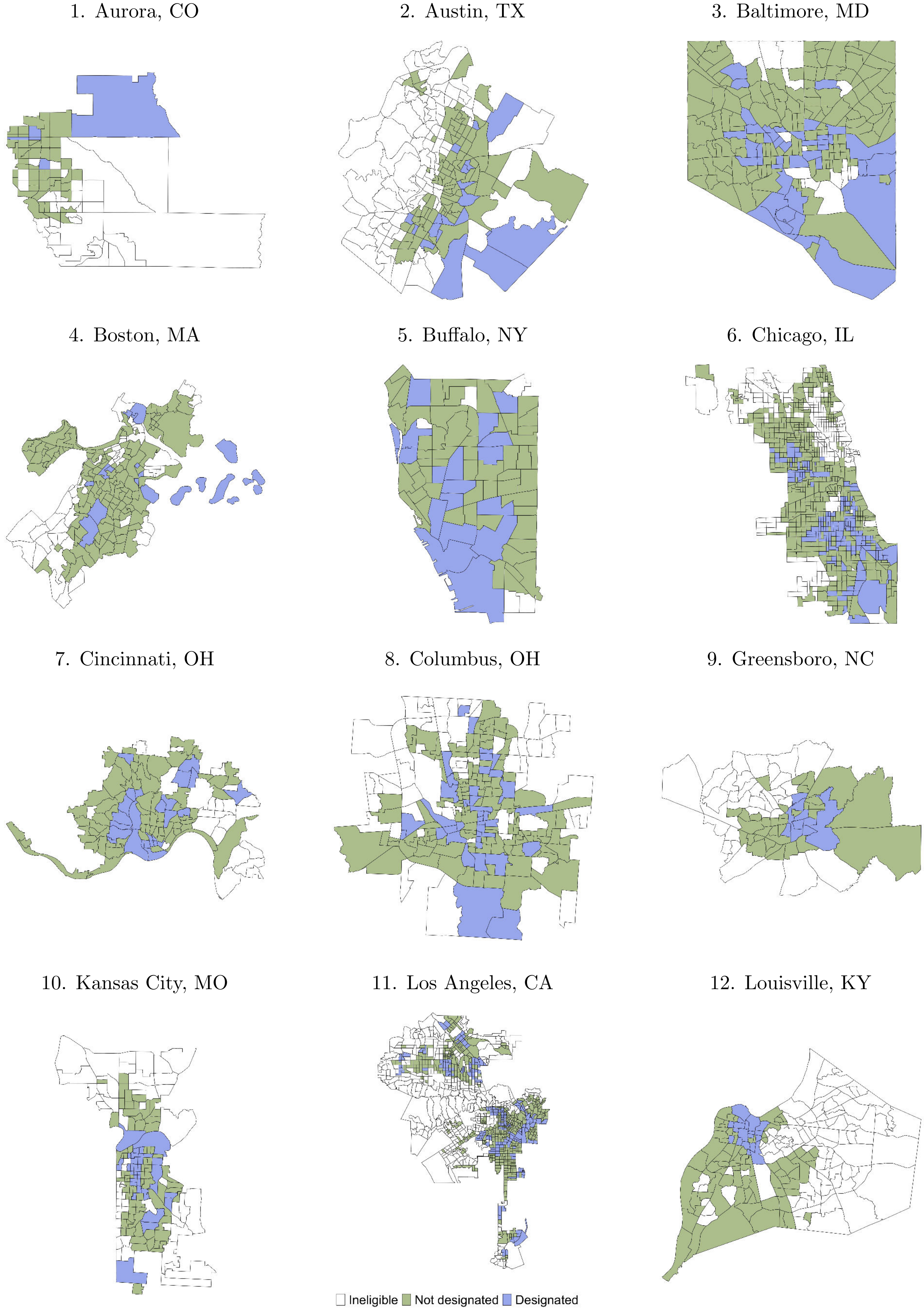
Table B.1: Descriptive statistics by city and treatment group

	Major crimes	Major crimes arrests	Calls for service	Police stops	Unempl rate (%)	Family income ($K)	Planning permits
Aurora, CO (E)	119.8				9.2	52.5	56.2
Aurora, CO (D)	218.1				8.0	48.6	107.9
Austin, TX (E)	241.0	7.6		559.3	6.2	55.8	85.8
Austin, TX (D)	188.9	3.3		499.6	10.3	45.6	82.4
Baltimore, MD (E)	182.1	13.6	3,175.1		13.2	56.4	182.8
Baltimore, MD (D)	269.3	27.0	5,452.4		18.0	38.6	191.6
Boston, MA (E)	95.8				9.7	62.4	29.4
Boston, MA (D)	72.4				19.3	39.0	11.8
Buffalo, NY (E)	194.6				11.9	45.1	13.5
Buffalo, NY (D)	283.2				13.3	36.4	25.9
Chicago, IL (E)	126.8	14.8		129.1	13.4	54.1	12.8
Chicago, IL (D)	181.0	20.2		294.1	27.0	31.4	11.3
Cincinnati, OH (E)	189.8	33.0	3,854.1		13.7	47.5	22.8
Cincinnati, OH (D)	265.2	46.7	6,130.8		19.4	34.3	41.7
Columbus, OH (E)	84.4				9.5	50.8	15.8
Columbus, OH (D)	88.4				14.4	33.8	18.6
Greensboro, NC (E)	174.6				9.1	47.5	13.7
Greensboro, NC (D)	226.3				15.5	36.6	35.2
Kansas City, MO (E)	425.3				9.5	50.0	8.5
Kansas City, MO (D)	487.8				13.9	36.5	12.4
Los Angeles, CA (E)	108.6	23.0	1,091.6	759.3	10.7	47.4	32.2
Los Angeles, CA (D)	148.7	36.6	1,325.3	1,045.8	12.1	38.7	31.4
Louisville, KY (E)	181.3	42.8		170.9	11.3	44.3	7.9
Louisville, KY (D)	283.9	64.0		636.8	18.3	26.9	16.7
Mesa, AZ (E)	104.1		1,190.7		9.5	47.8	14.3
Mesa, AZ (D)	144.6		2,132.7		9.3	46.5	59.4
Milwaukee, WI (E)	142.0				12.3	45.9	7.9
Milwaukee, WI (D)	186.3				16.6	31.4	9.6
Minneapolis, MN (E)	187.3				9.1	61.7	120.2
Minneapolis, MN (D)	259.8				12.7	37.2	67.5
Nashville, TN (E)	215.7			2,272.7	8.0	50.2	
Nashville, TN (D)	249.1			2,955.9	14.1	29.8	
New Orleans, LA (E)	94.6		2,242.9		11.5	49.3	22.9
New Orleans, LA (D)	183.1		4,476.1		16.4	35.7	27.5
New York, NY (E)	44.9	19.4		10.6	10.3	54.8	5.6
New York, NY (D)	68.4	34.7		18.2	12.0	45.0	8.7
Norfolk, VA (E)	108.6				9.5	57.4	31.9
Norfolk, VA (D)	148.1				15.3	40.1	30.6
Orlando, FL (E)	256.9		4,182.4		9.2	43.3	57.4
Orlando, FL (D)	242.4		5,400.7		18.5	33.7	55.1
Philadelphia, PA (E)	161.4	32.3		1,014.1	14.5	51.8	21.6
Philadelphia, PA (D)	194.7	37.9		1,925.9	17.3	38.0	38.6

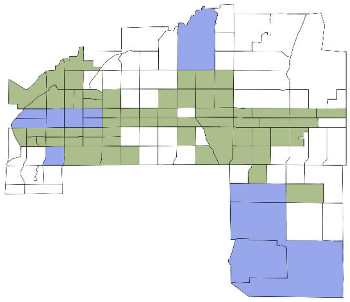
Pittsburgh, PA (E)	83.7			17.9	8.8	62.8	47.0
Pittsburgh, PA (D)	86.4			13.1	17.4	36.8	26.6
Portland, OR (E)	182.6		1,731.1		8.4	63.7	17.6
Portland, OR (D)	574.8		4,422.9		12.6	68.5	31.9
Raleigh, NC (E)	198.7				8.0	61.3	0.2
Raleigh, NC (D)	238.8				10.8	43.1	1.6
Sacramento, CA (E)	159.6		2,523.5	99.4	11.7	56.7	
Sacramento, CA (D)	231.2		4,074.9	168.7	18.2	34.4	
Saint Paul, MN (E)	98.2			338.4	9.2	59.0	77.4
Saint Paul, MN (D)	243.7			701.6	13.1	45.5	70.3
San Francisco, CA (E)	476.9	49.3			8.8	65.3	22.4
San Francisco, CA (D)	156.7	19.4			13.0	53.0	27.3
Seattle, WA (E)	378.1				7.2	91.4	59.8
Seattle, WA (D)	490.7				8.5	57.7	58.9
St. Louis, MO (E)	230.4				14.4	48.7	50.7
St. Louis, MO (D)	296.0				20.5	30.7	62.1
Tucson, AZ (E)	270.6	14.6	2,248.0		11.1	45.9	15.2
Tucson, AZ (D)	308.3	18.4	3,355.2		13.3	36.4	16.2
Washington, DC (E)	191.7	25.0			13.3	75.6	93.7
Washington, DC (D)	220.0	43.6			20.1	44.9	67.8

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.

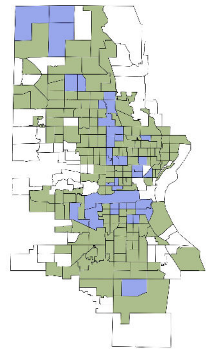
Figure B.1: Eligible and designated Opportunity Zones census tracts by city



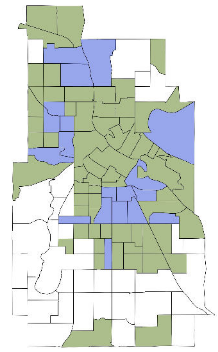
13. Mesa, AZ



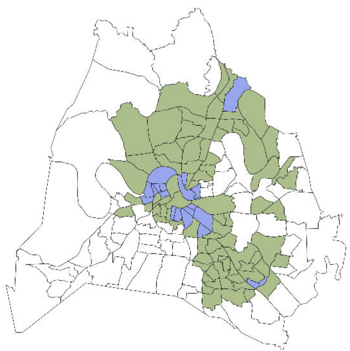
14. Milwaukee, WI



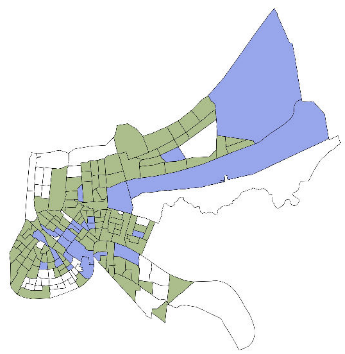
15. Minneapolis, MN



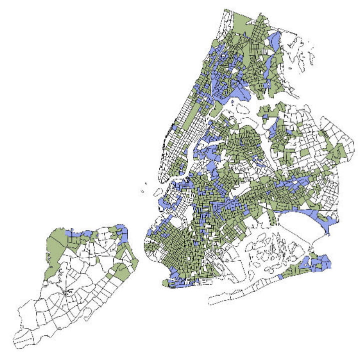
16. Nashville, TN



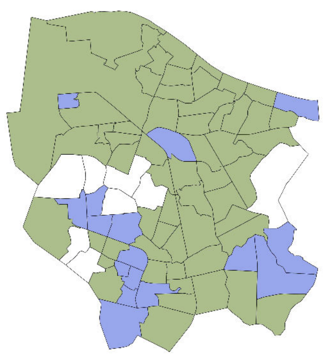
17. New Orleans, LA



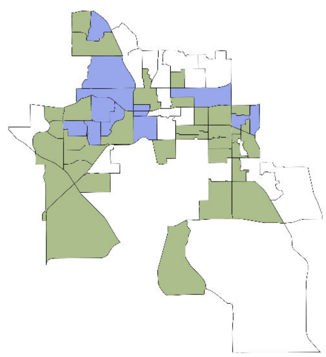
18. New York, NY



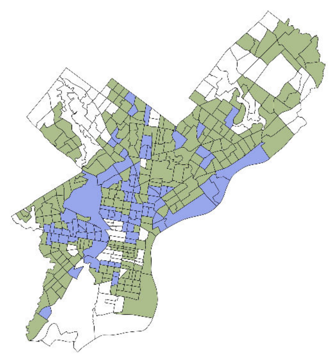
19. Norfolk, VA



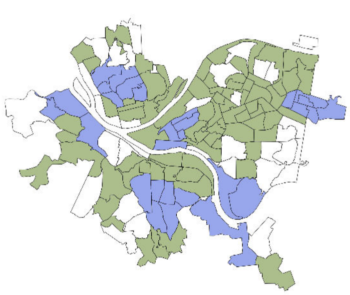
20. Orlando, FL



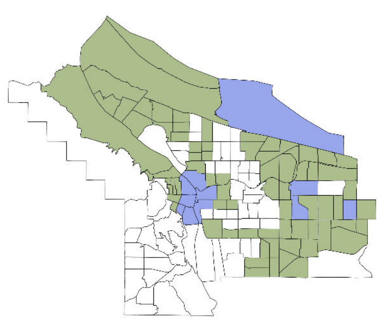
21. Philadelphia, PA



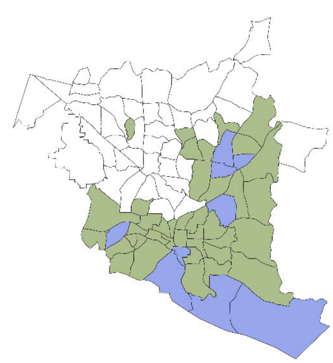
22. Pittsburgh, PA



23. Portland, OR

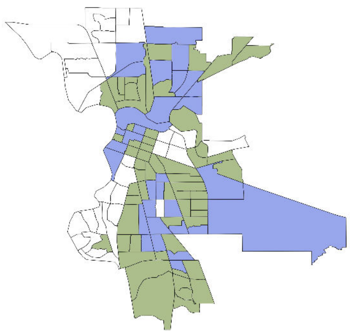


24. Raleigh, NC

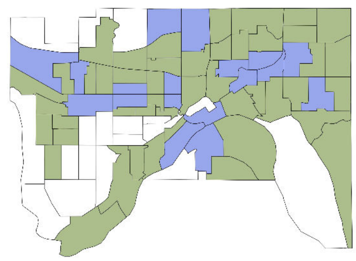


Ineligible Not designated Designated

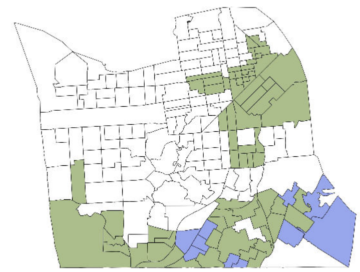
25. Sacramento, CA



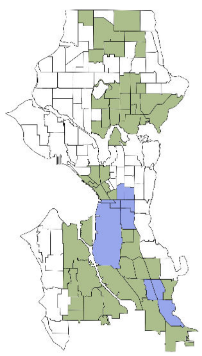
26. Saint Paul, MN



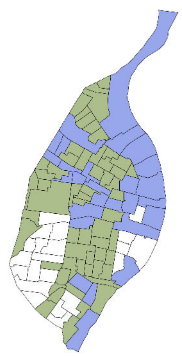
27. San Francisco, CA



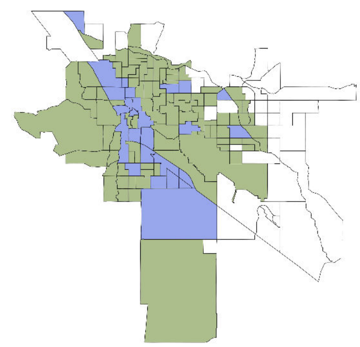
28. Seattle, WA



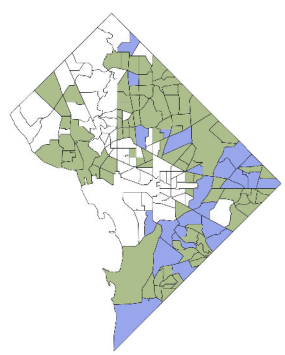
29. St. Louis, MO



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 a population of 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.

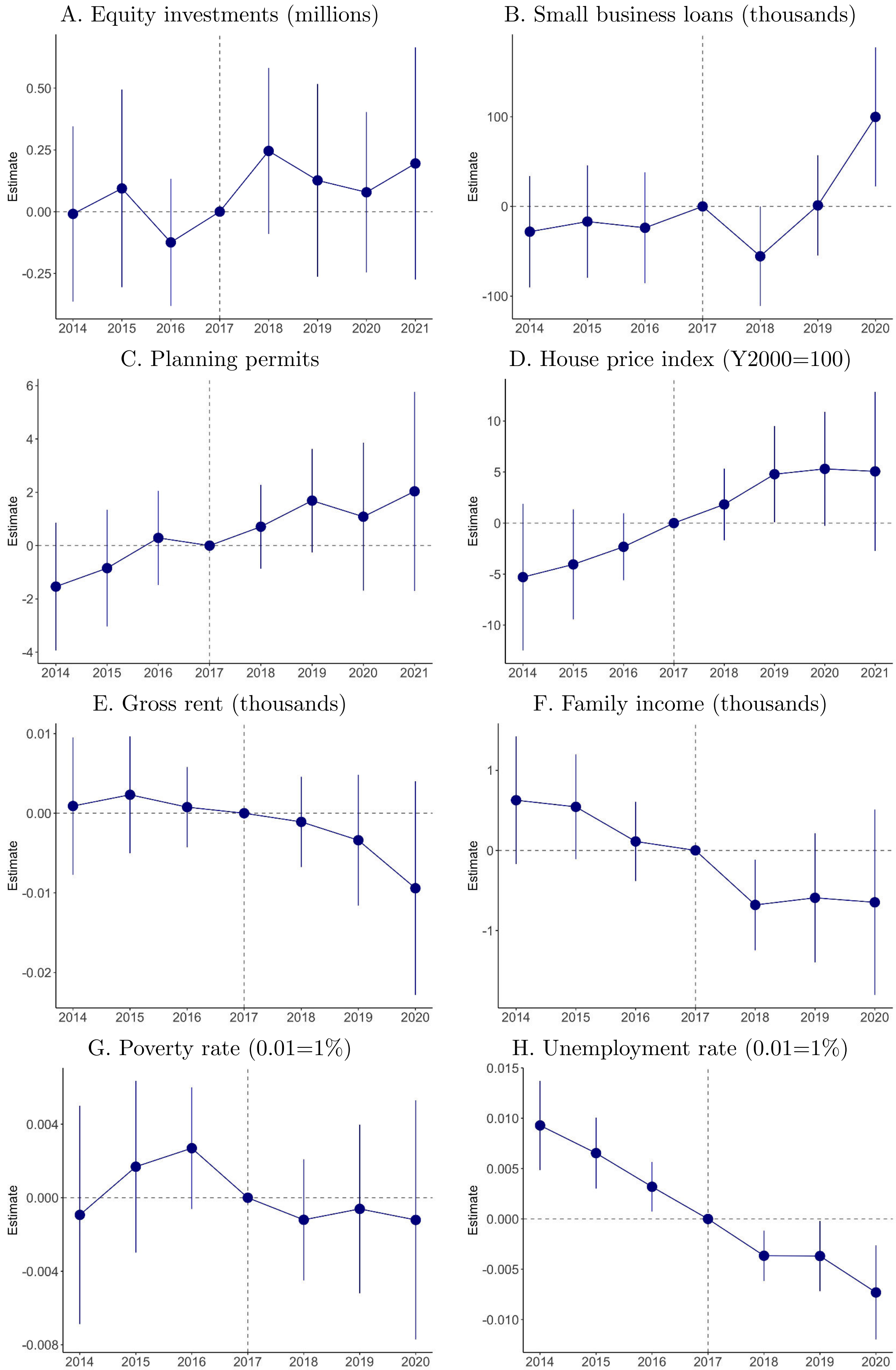
C Appendix: Event study design on alternative sample groups

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 C.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 C.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 C.3** and **C.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.

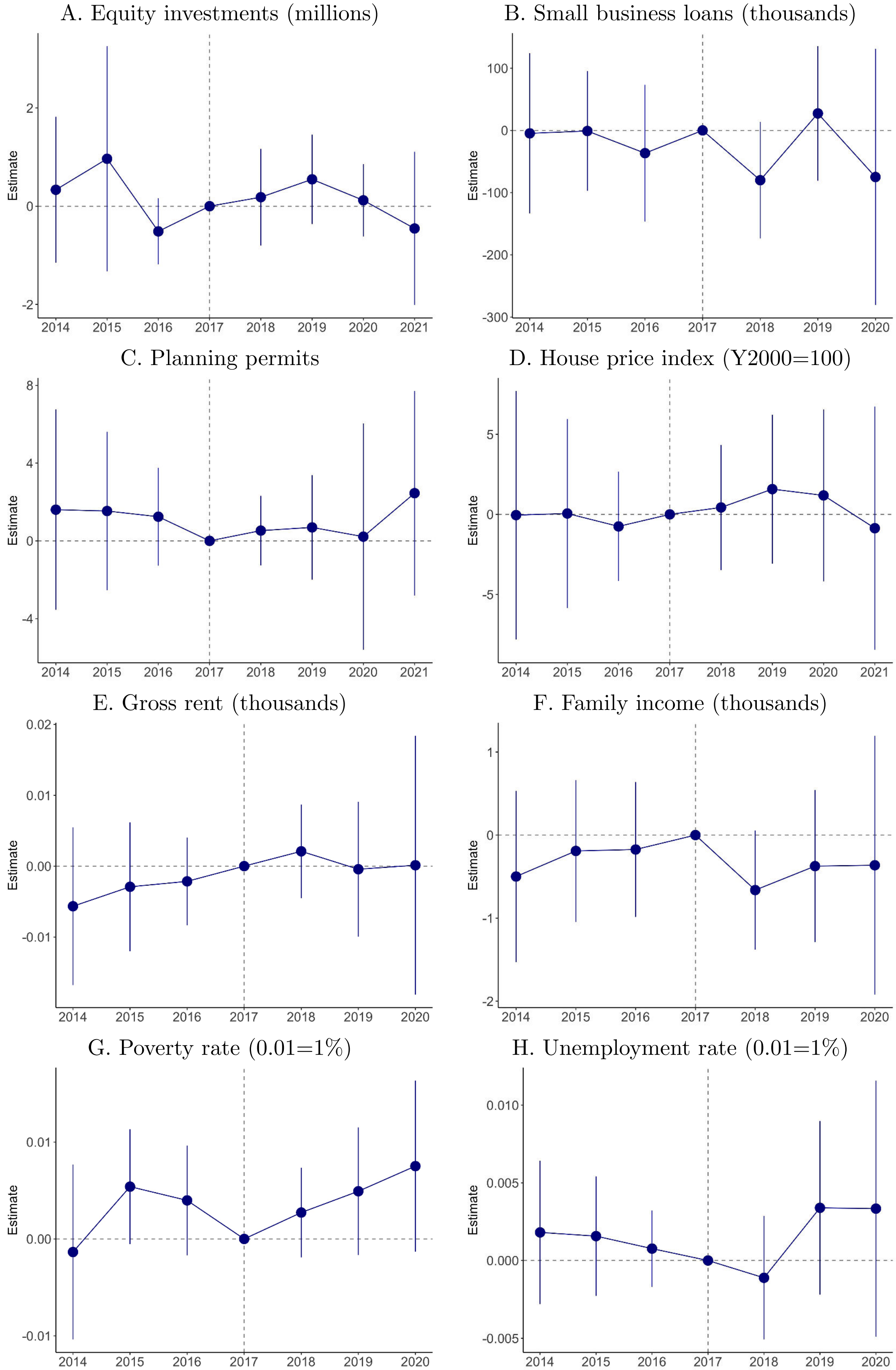
Appendix Figure C.5 presents the baseline event study design estimates for the six public safety outcomes. There are pre-trends using all the eligible tracts as a comparison group on calls for service, police stops, major crimes, and non-major and major crime arrests. **Appendix Figures C.6 and C.7** 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 C.8 and C.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 C.1: Event study estimates on economic outcomes, bordering sample



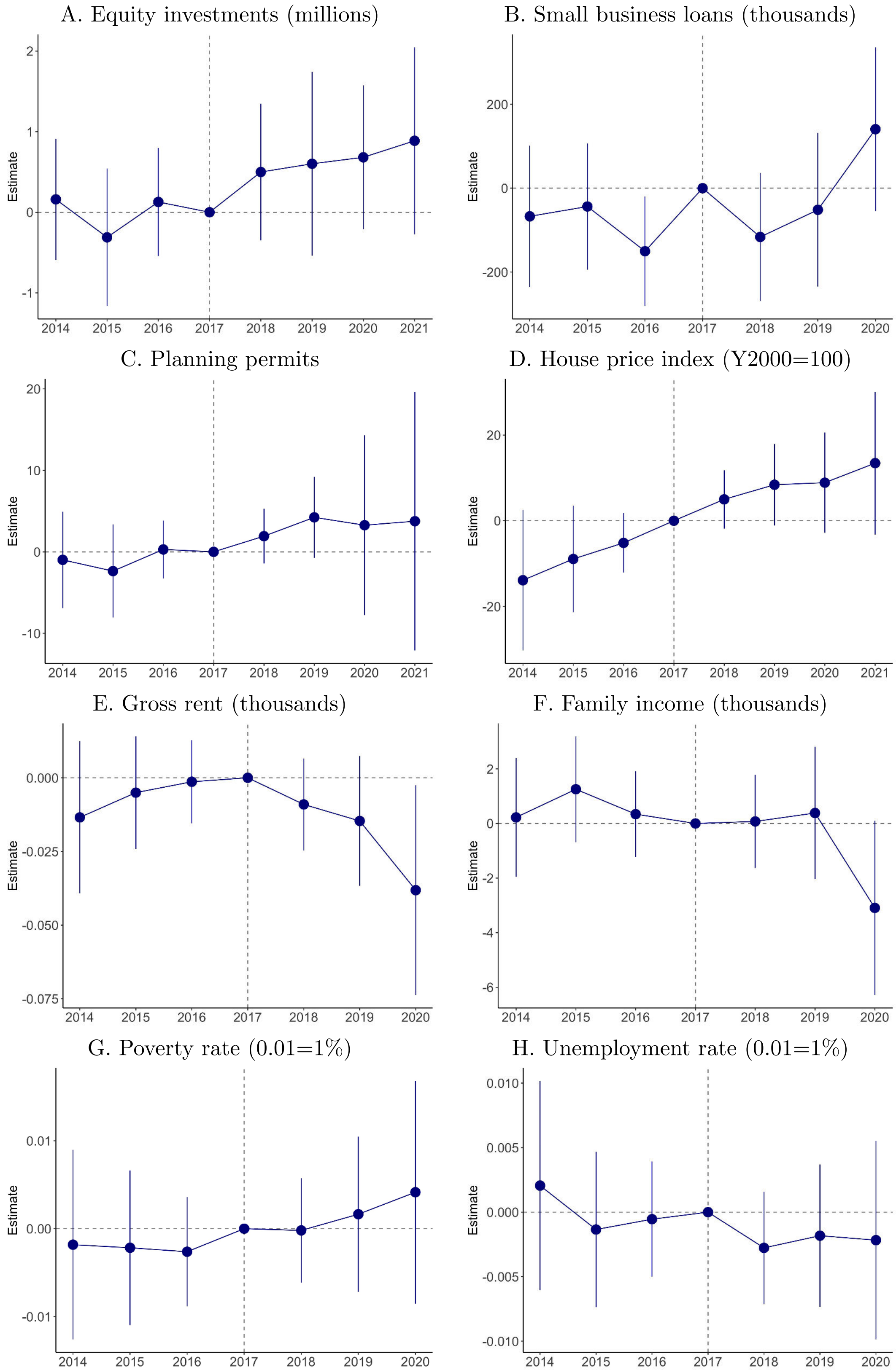
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model use the low-income designated and their bordering, eligible census tracts sample.

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



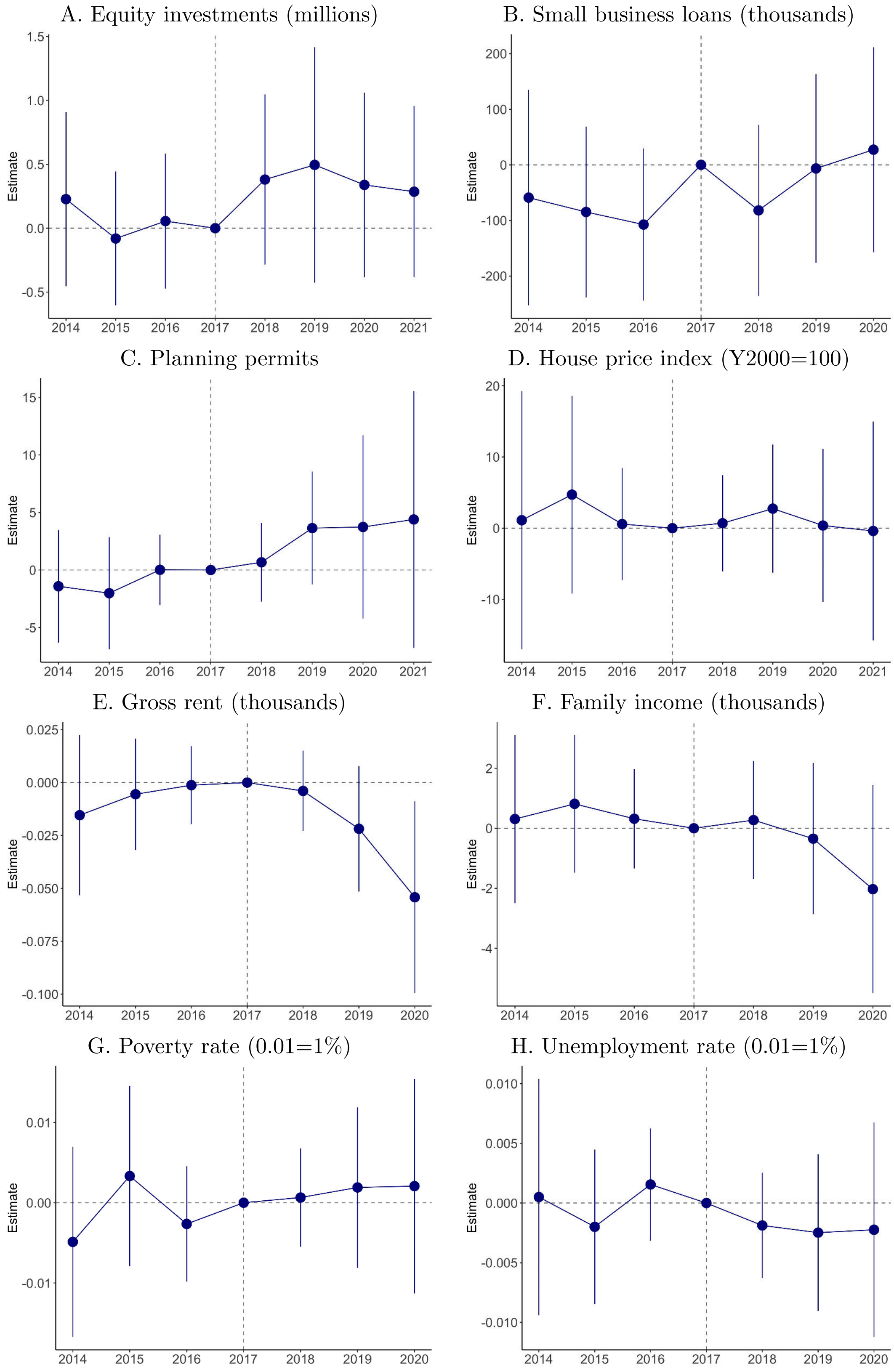
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity-score weights from a logit model that predicts Opportunity Zone designation using pre-intervention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

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



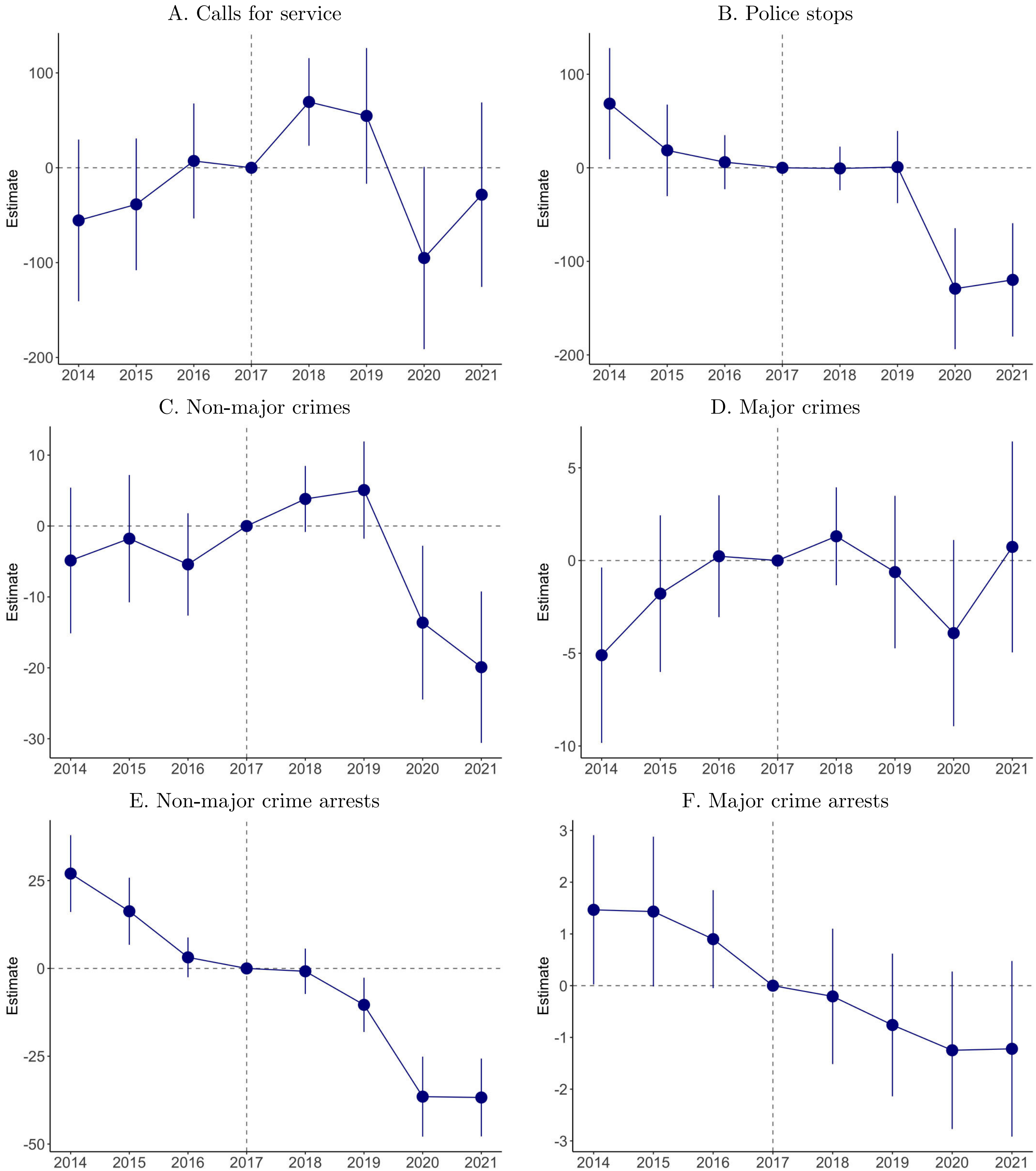
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model use the eligible, low-income, similar but not designated 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).

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



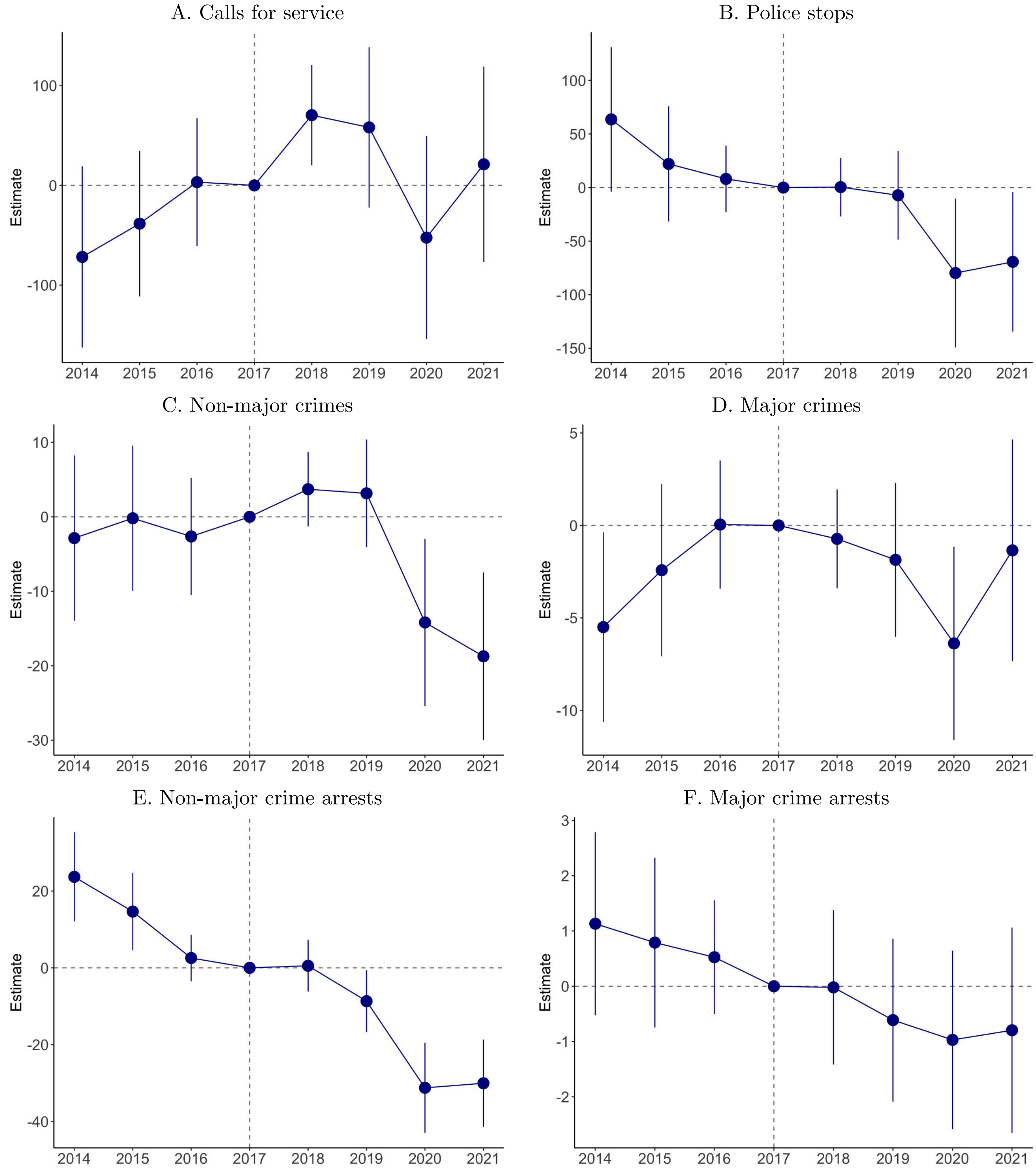
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \omega_i + \sigma_t + \sum_{\tau=-q}^{m} \beta_{1\tau} D_{it} + X_{it}\alpha_X + e_{it}$, where the regression uses inverse propensity-score weights from a logit model that predicts Opportunity Zone designation using pre-intervention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

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



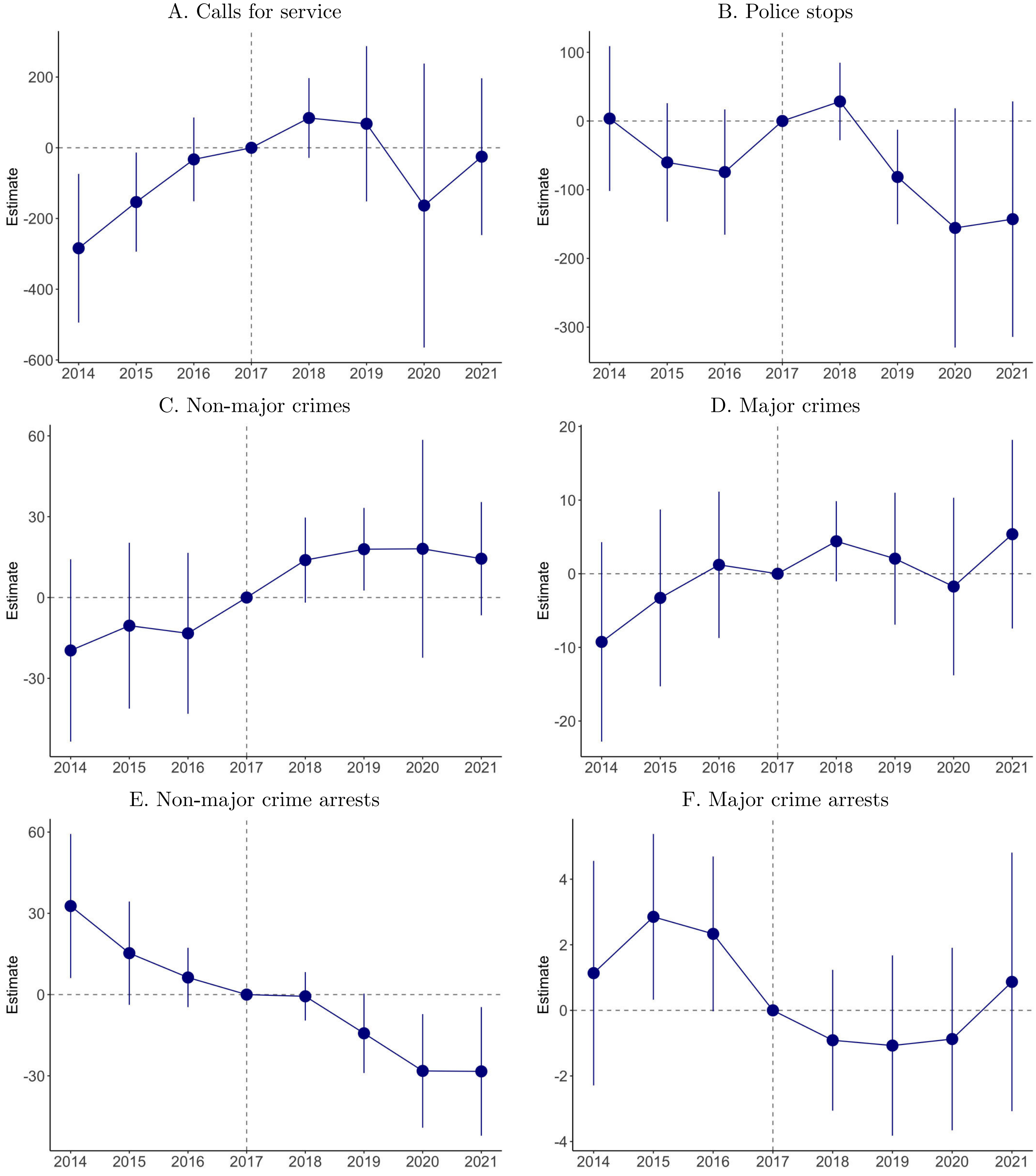
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model use the low-income, eligible census tracts sample.

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



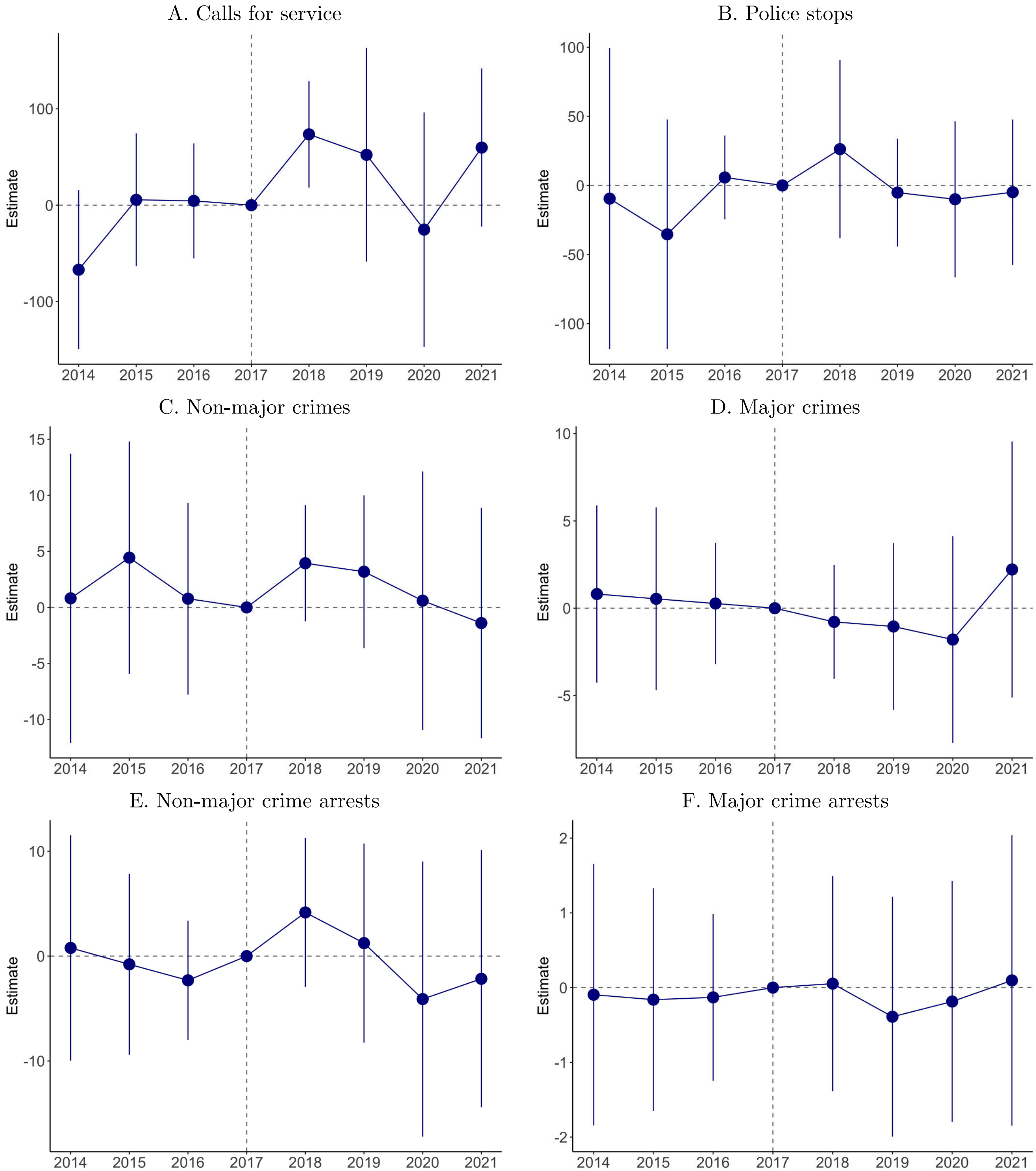
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model use the low-income designated and their bordering, eligible census tracts sample.

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



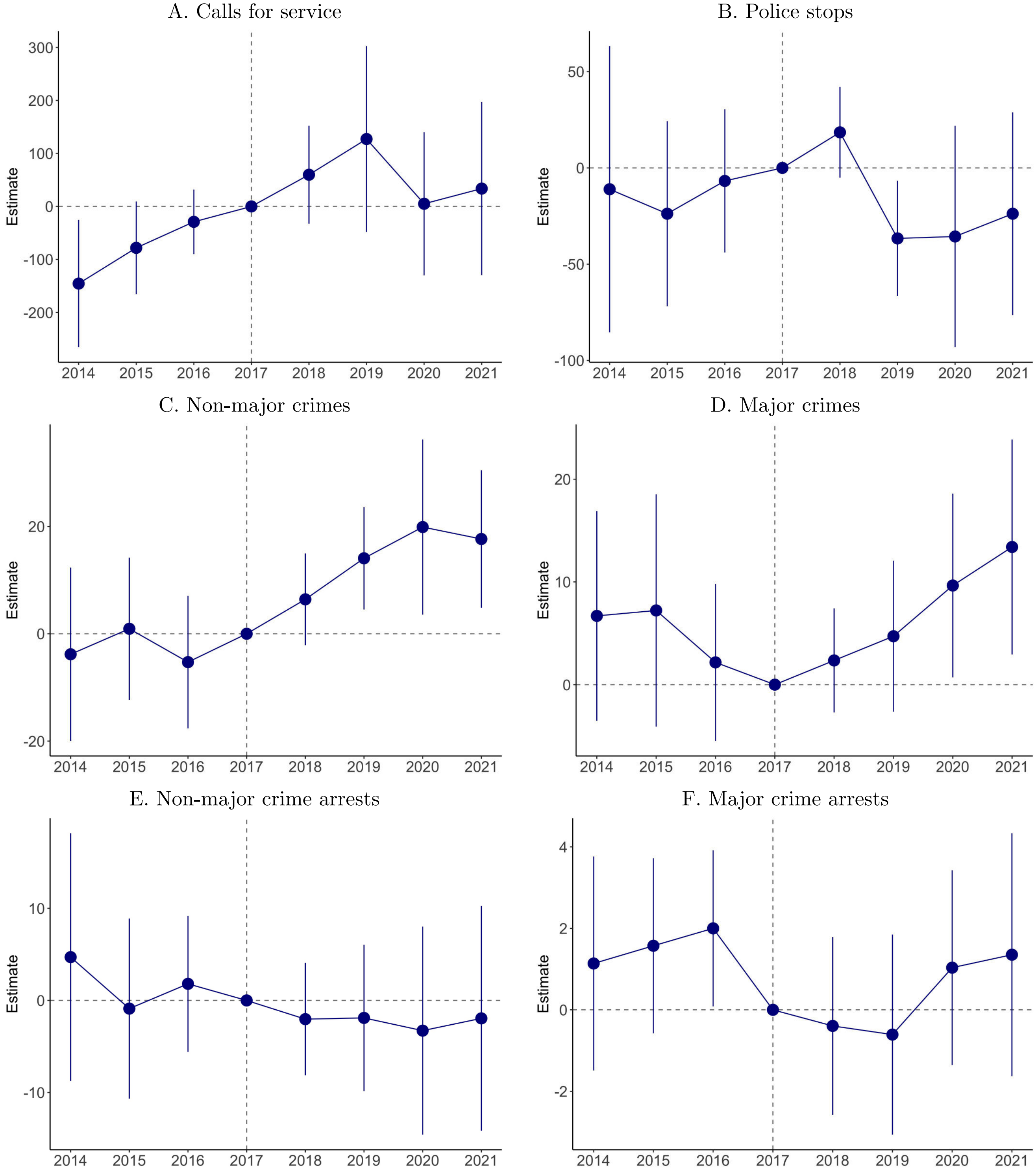
Notes: Event study design estimates following: $y_{it} = \gamma_0 + \gamma_i + \mu_t + \sum_{\tau=-q}^{m} \beta_\tau D_{it} + X_{it}\alpha_X + e_{it}$. The regression clusters the standard errors at the census tract level. The econometric model use the eligible, low-income, similar but not designated 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).

Figure C.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 use the low-income, eligible census tracts sample.

Figure C.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).

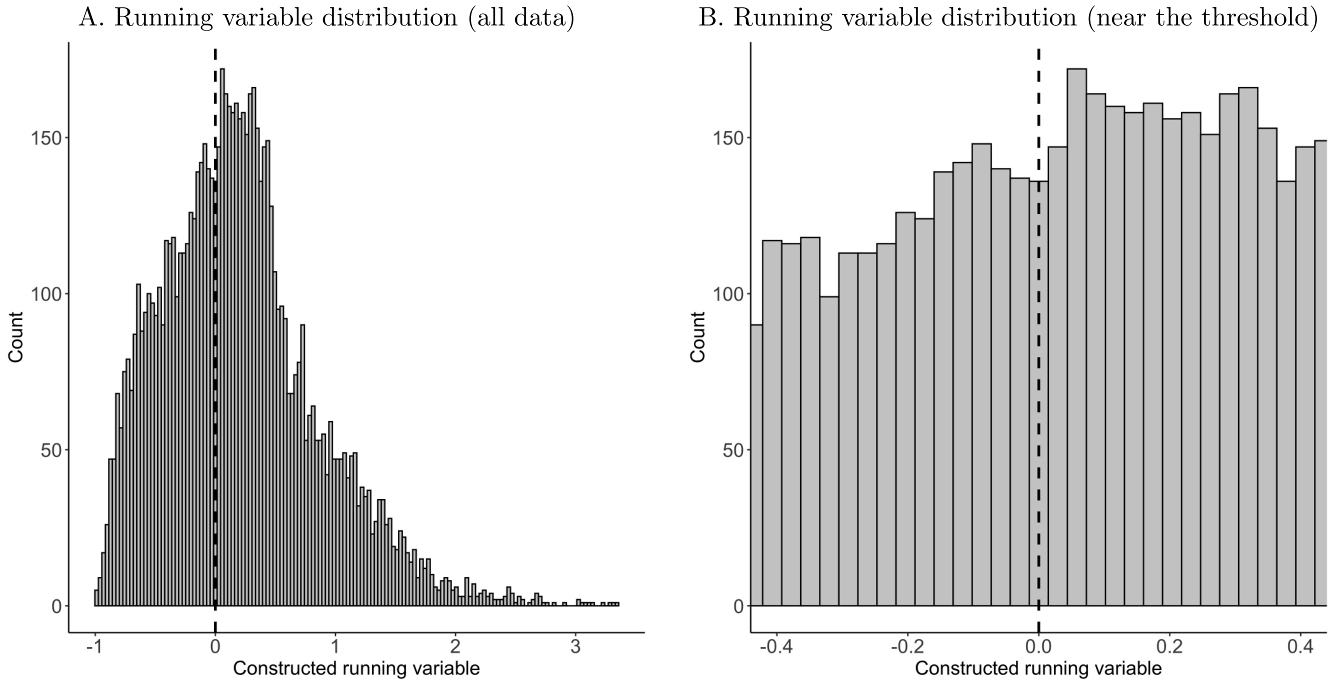
D Appendix: Regression discontinuity results

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 D.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 D.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 D.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.

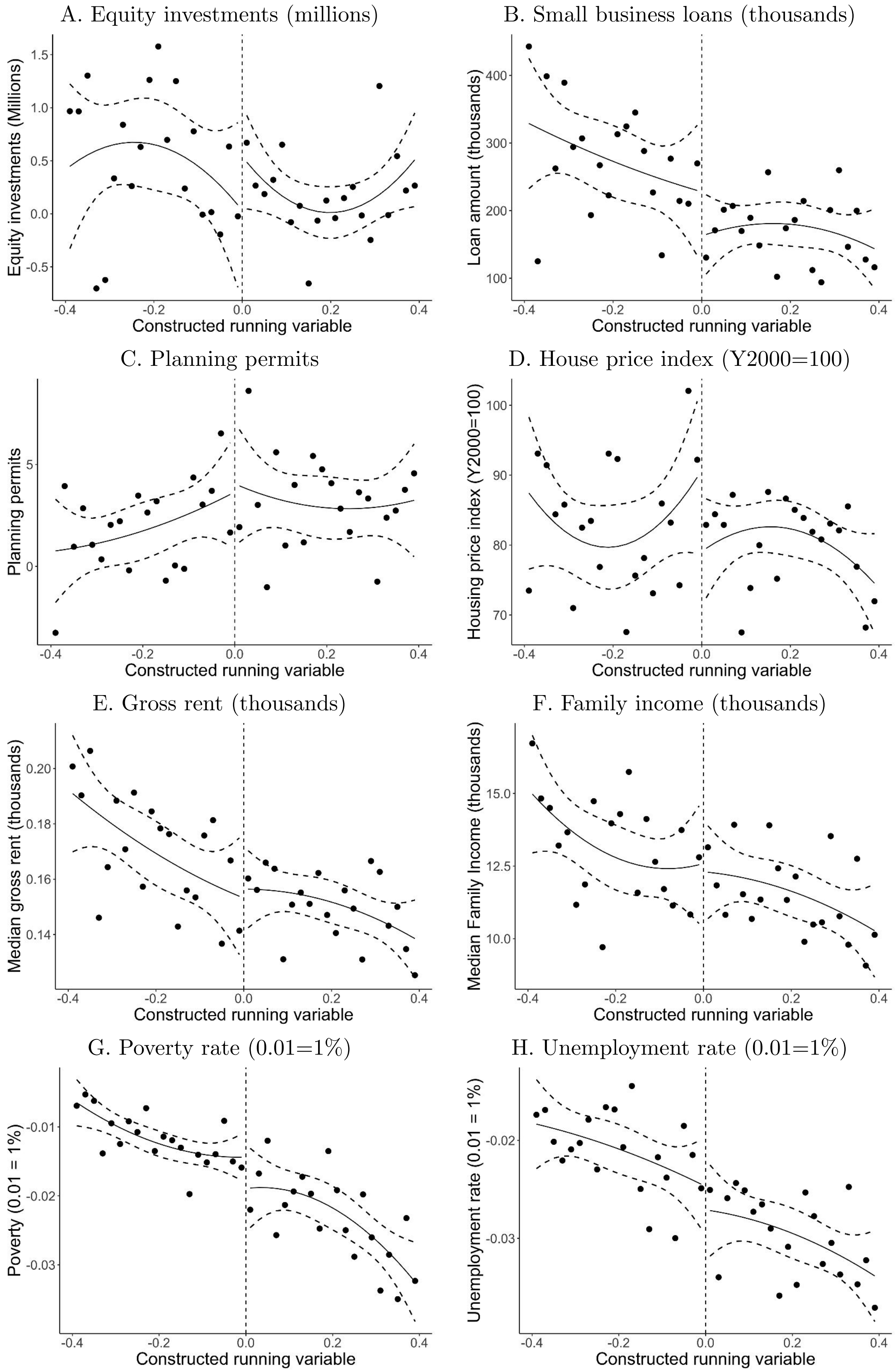
Consistent with no evidence of socioeconomic changes in the Opportunity Zone tracts, **Figure D.3** shows no effects on public safety. Police stops and major crime arrests seem to exhibit a differential change around the threshold, but there are no significant changes. Worth mentioning that in these 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 D.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 D.1: Running variable distribution



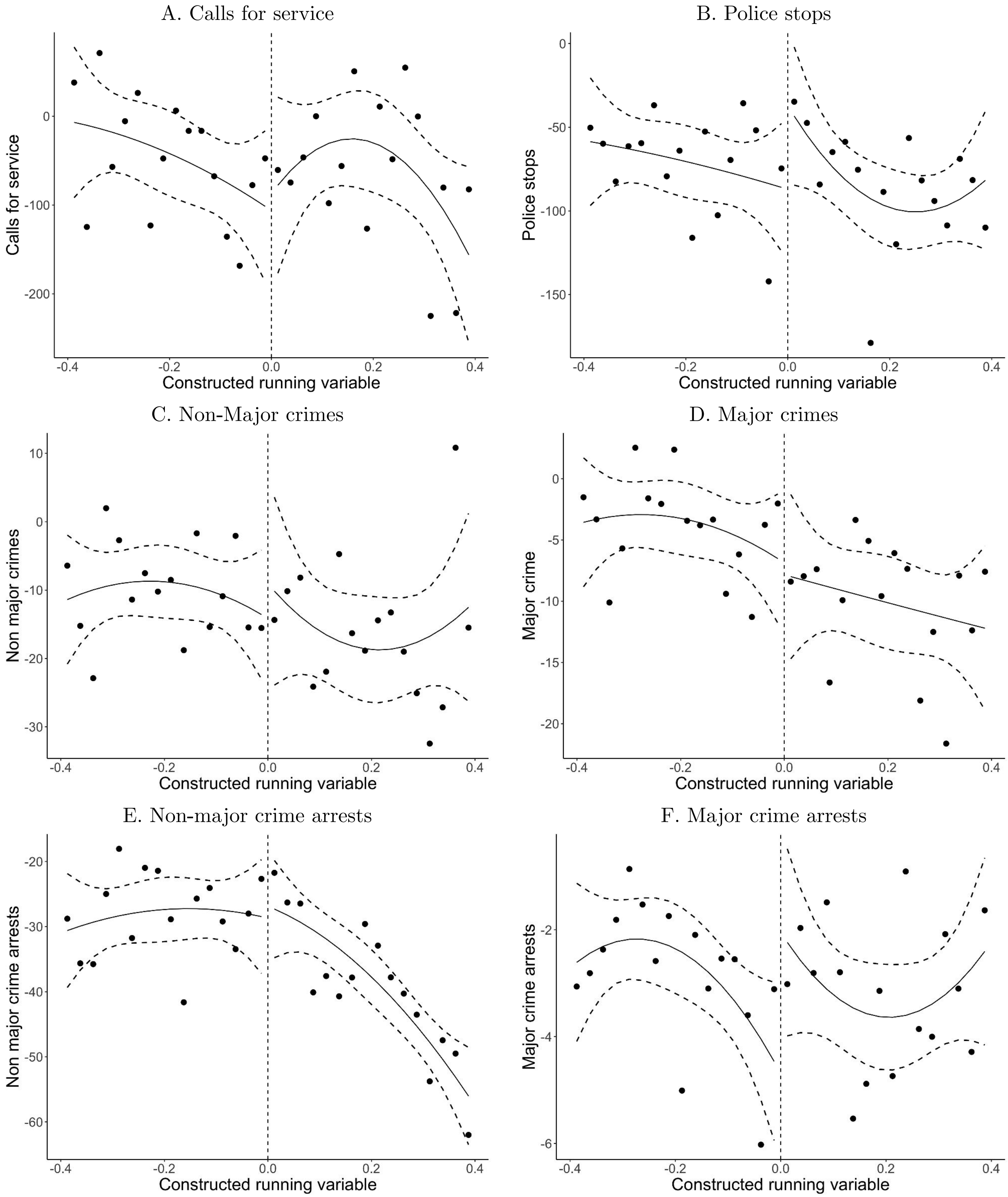
Notes: The panels show the distribution of tracts by the constructed running variable. Panel B presents the regression discontinuity of the parcels. The pvalues of the manipulation tests are 0.28 and 0.82 using the McCrary (2008) and Cattaneo et al. (2018) approaches.

Figure D.2: Regression discontinuity on economic outcomes



Notes: All panels present the mean difference of the outcome variable between the post (2018-2021) 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. Gross rent, family income, poverty, and unemployment only have data up to 2020.

Figure D.3: Regression discontinuity on public safety outcomes



Notes: All panels present the mean difference of the outcome variable between the post (2018-2021) 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 D.1: Regression discontinuity estimates of the Opportunity Zones designation on economic outcomes

	(1)	(2)	(3)	(4)
A. Designated Opportunity Zone				
Treatment effect	0.088***	0.059**	0.071***	0.072***
	(0.019)	(0.029)	(0.015)	(0.023)
Mean dep. var.	0.07	0.07	0.08	0.08
Observations	2,523	2,523	3,839	3,839
B. Equity investments (millions)				
Designated OZ	4.498	18.734	−1.361	7.305
	(3.520)	(20.987)	(3.566)	(5.895)
Mean dep. var.	1.55	1.55	1.53	1.53
Observations	2,523	2,523	3,839	3,839
C. Small business loans (thousands)				
Designated OZ	−554.626	−980.559	−202.411	−901.749
	(470.937)	(1,238.284)	(459.141)	(715.791)
Mean dep. var.	908.23	908.23	923.18	923.18
Observations	2,523	2,523	3,839	3,839
D. Planning permits				
Designated OZ	−3.300	−19.917	1.835	−7.316
	(26.238)	(77.284)	(21.616)	(39.959)
Mean dep. var.	30.35	30.35	31.98	31.98
Observations	2,424	2,424	3,701	3,701
E. House price index (Y2000=100)				
Designated OZ	−65.775	−226.727	−23.015	−141.114
	(68.851)	(388.928)	(60.559)	(122.358)
Mean dep. var.	285.90	285.90	286.60	286.60
Observations	1,561	1,561	2,306	2,306
F. Gross rent (thousands)				
Designated OZ	0.079	0.197	0.123	0.022
	(0.146)	(0.342)	(0.131)	(0.205)
Mean dep. var.	1.17	1.17	1.18	1.18
Observations	2,515	2,515	3,831	3,831
G. Family income (thousands)				
Designated OZ	5.415	2.611	7.605	−2.723
	(13.059)	(30.694)	(11.911)	(18.643)
Mean dep. var.	68.75	68.75	68.53	68.53
Observations	2,516	2,516	3,831	3,831
H. Poverty rate (0.01=1%)				
Designated OZ	−0.027	−0.098	−0.007	−0.041
	(0.040)	(0.137)	(0.034)	(0.058)
Mean dep. var.	0.15	0.15	0.16	0.16
Observations	2,521	2,521	3,837	3,837
I. Unemployment rate (0.01=1%)				
Designated OZ	−0.018	−0.072	−0.026	−0.034
	(0.031)	(0.117)	(0.025)	(0.044)
Mean dep. var.	0.08	0.08	0.09	0.09
Observations	2,521	2,521	3,837	3,837
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 D.2: Regression discontinuity estimates of the Opportunity Zones designation on public safety

	(1)	(2)	(3)	(4)
A. Calls for service				
Designated OZ	556.46	783.05	504.90	347.97
	(686.85)	(558.43)	(997.92)	(837.11)
Mean dep. var.	1,670.6	1,670.6	1,759.5	1,759.5
Cities	9	9	9	9
Observations	574	574	917	917
B. Police stops				
Designated OZ	320.97	235.06	11,665.66	786.77
	(345.02)	(308.52)	(113,370.70)	(744.92)
Mean dep. var.	245.8	245.8	258.4	258.4
Cities	10	10	10	10
Observations	1,667	1,667	2,518	2,518
C. Non-Major crimes				
Designated OZ	−10.60	−26.33	72.12	39.79
	(56.85)	(56.53)	(188.06)	(73.97)
Mean dep. var.	178.7	178.7	184.6	184.6
Cities	29	29	29	29
Observations	2,449	2,449	3,715	3,715
D. Major crimes				
Designated OZ	−23.87	−19.80	−100.79	−16.55
	(46.28)	(40.81)	(149.79)	(57.65)
Mean dep. var.	115.1	115.1	119.9	119.9
Cities	31	31	31	31
Observations	2,523	2,523	3,839	3,839
E. Non-major crime arrests				
Designated OZ	15.91	−20.96	222.12	84.66
	(77.94)	(76.94)	(517.26)	(194.56)
Mean dep. var.	84.6	84.6	89.6	89.6
Cities	11	11	11	11
Observations	1,704	1,704	2,626	2,626
F. Major crime arrests				
Designated OZ	23.38	16.21	146.37	63.04
	(20.52)	(14.38)	(384.82)	(60.31)
Mean dep. var.	16.5	16.5	16.9	16.9
Cities	11	11	11	11
Observations	1,704	1,704	2,626	2,626
Bandwidth	IK: 0.25	IK: 0.25	CC: 0.40	CC: 0.40
Polynomial function	Linear	Quad	Linear	Quad

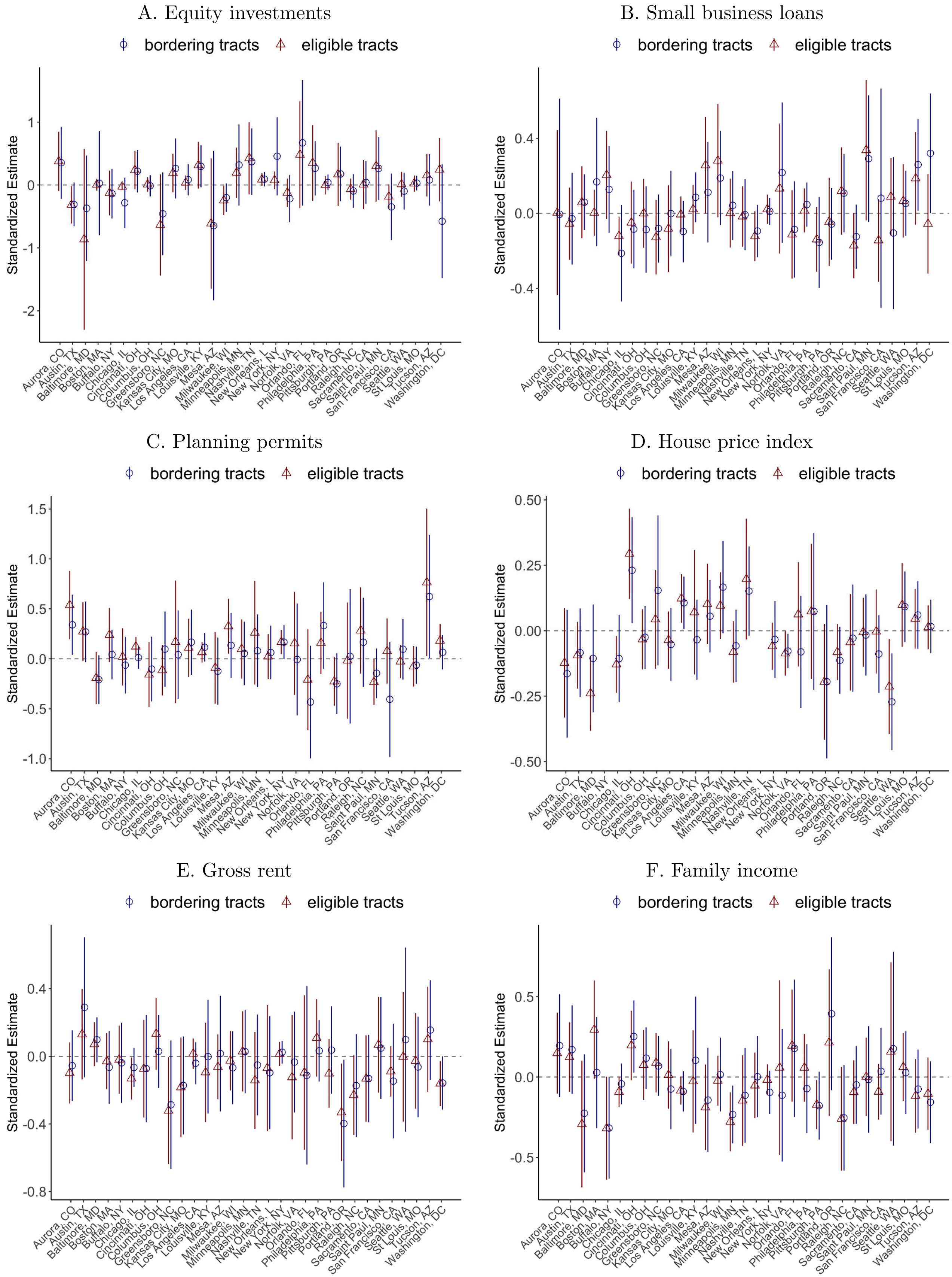
Notes: Fuzzy regression discontinuity estimates of the Opportunity Zones designation on public safety. Robust standard errors clustered at the census tract level in parentheses. The number of cities reporting the outcome is included. All panels use a designated Opportunity Zone (OZ) indicator variable 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.

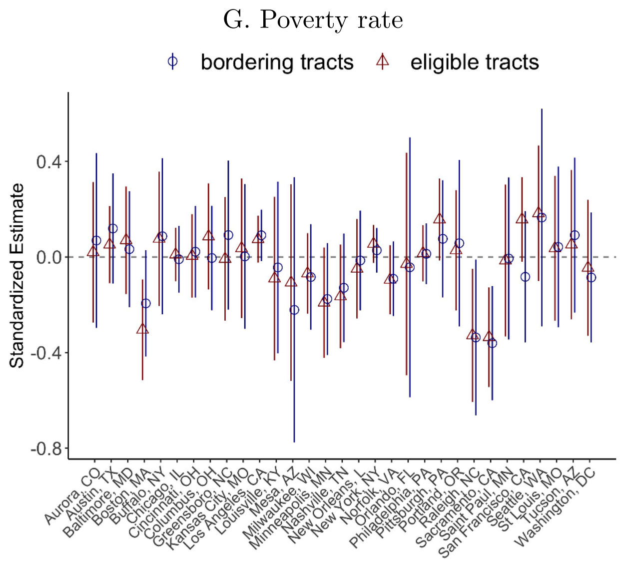
E Appendix: City-specific difference-in-differences estimates

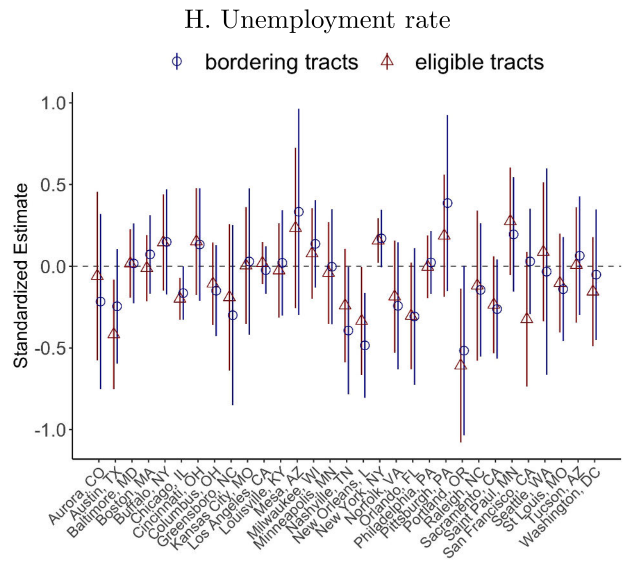
This appendix shows the propensity score weighted difference-in-differences estimates by city level on the eligible and bordering tracts samples. The similar tracts sample was not estimated because it drastically reduces the sample size, increasing the standard errors. Similarly, the regression discontinuity 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 E.1** and **E.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 E.1: Propensity score weighted difference-in-differences estimates on economic outcomes by city

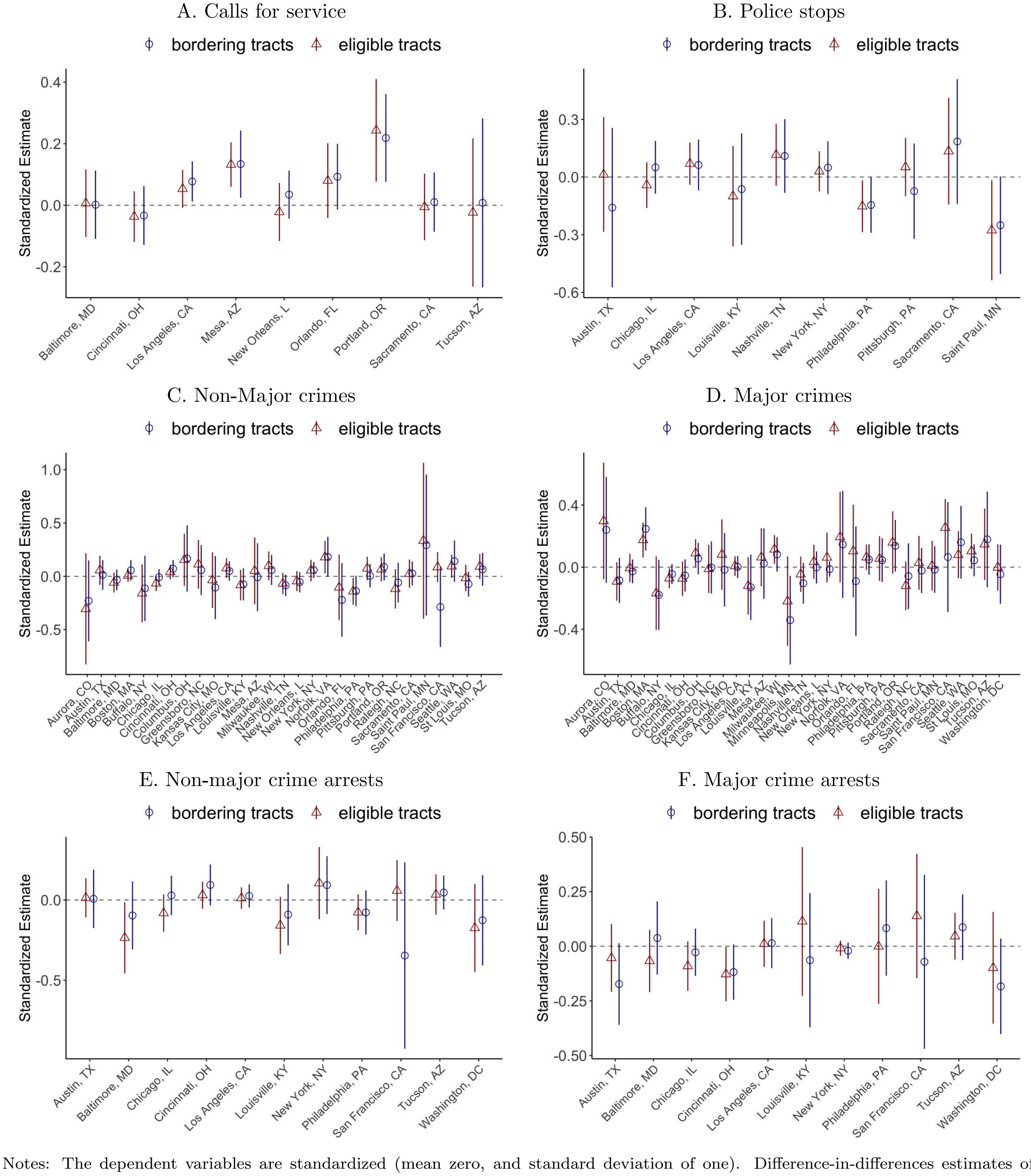






Notes: The dependent variables are standardized (mean zero, and standard deviation of one). Difference-in-differences estimates of the Opportunity Zones designation on economic outcomes where the regression uses inverse propensity score weights from a logit model that predicts Opportunity Zone designation using pre-intervention sociodemographic controls. The regression clusters the standard errors at the census tract level. The panels include estimates on two alternative samples: 1) the low-income, eligible tracts and 2) the low-income designated and their bordering, eligible census tracts.

Figure E.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 two alternative samples: 1) the low-income, eligible tracts and 2) the low-income designated and their bordering, eligible census tracts.

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

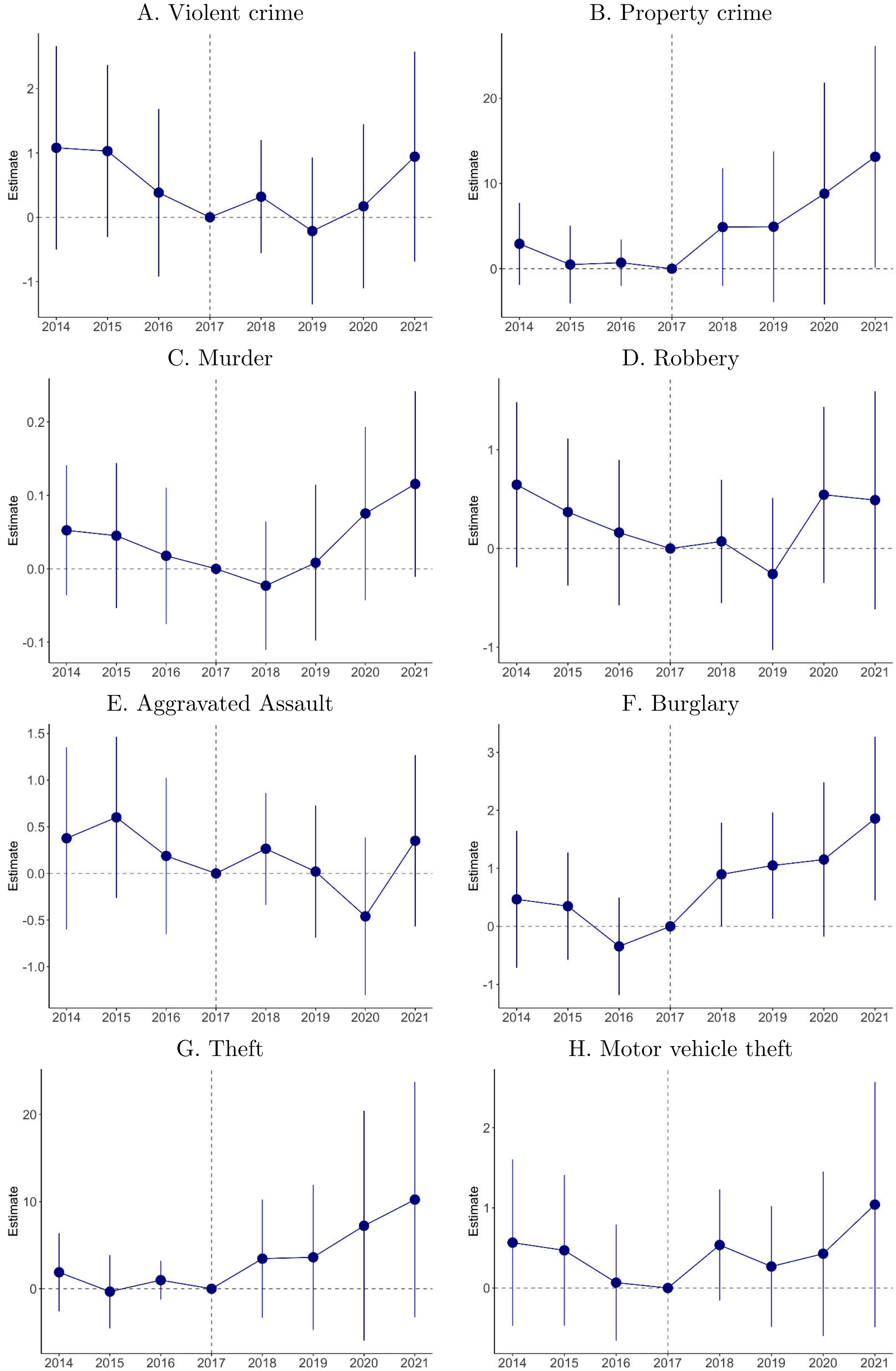
This appendix presents the regression discontinuity and propensity score weighted difference-in-differences estimators on the Uniform Crime Reporting Part I crime and arrest categories.

Appendix Figures F.1 and **F.2** indicate that the parallel trends assumption holds for the crime and arrests subcategories using the eligible sample. While the assumption also holds for the bordering and similar tracts samples, there are not presented for conciseness.

Appendix Table F.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 5 percent (an additional 1.1 burglaries per year).

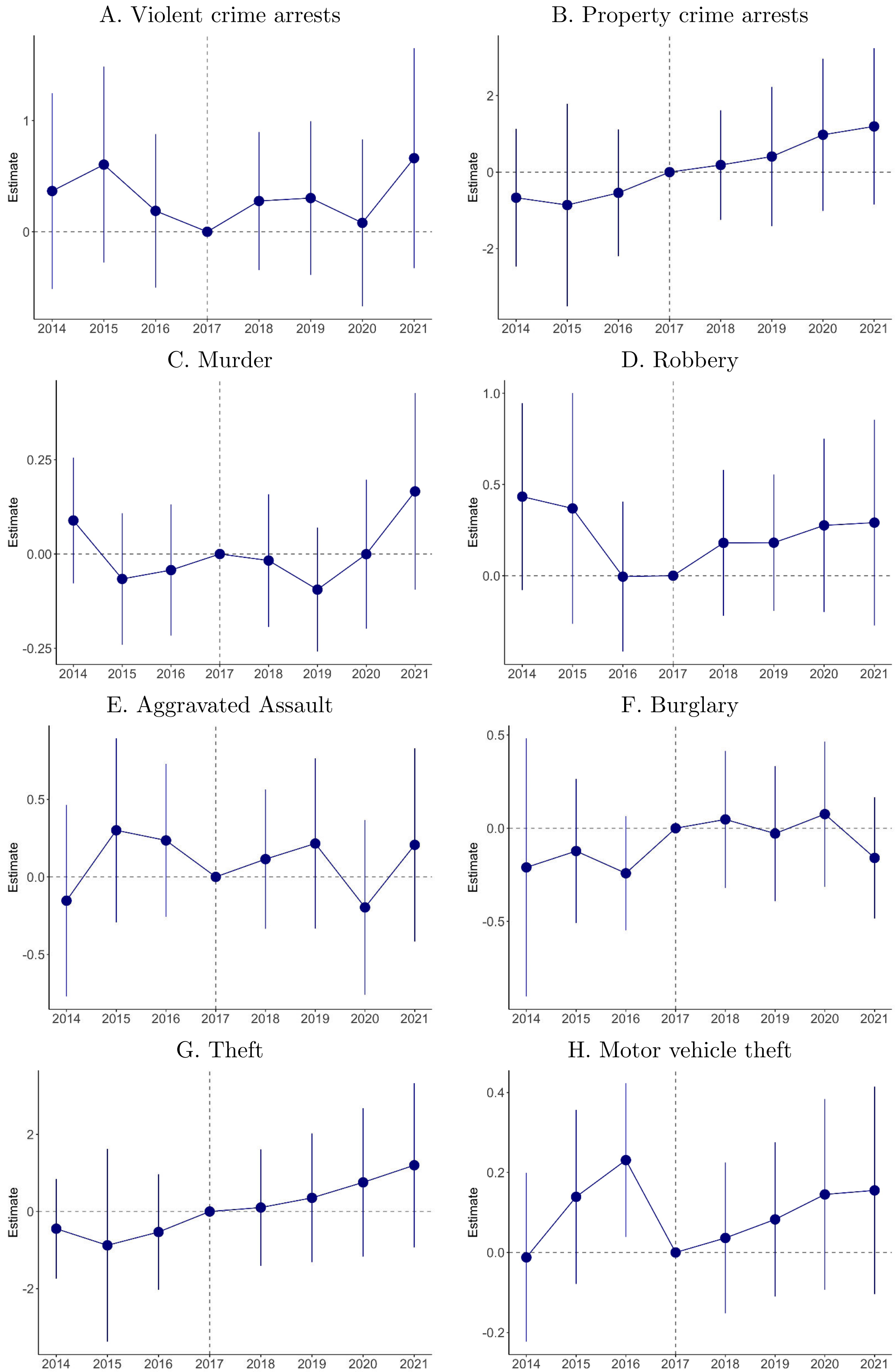
The regression discontinuity estimates (**Appendix Table F.2**) do not show any significant positive effects on burglary and there is a flip sign in two specifications. Consequently, overall there are no impacts on crimes. **Appendix Tables F.3** and **F.4** present no changes on the disaggregated arrests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

Figure F.1: Propensity score weighted event study estimates on crime, 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 use the low-income, eligible census tracts sample.

Figure F.2: Propensity Score weighted event study estimates on arrests, 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 use the low-income, eligible census tracts sample.

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

	DiD (1)	DiD (2)	DiD (3)	PSM-DiD (4)	PSM-DiD (5)	PSM-DiD (6)
A. Violent crime						
Treatment*Post	−0.022	−0.476	0.932	−0.315	−1.050	−0.019
	(0.571)	(0.605)	(0.929)	(0.629)	(0.651)	(0.702)
Mean dep. var.	35.2	40.9	22.0	35.2	40.9	22.0
Observations	37,978	24,298	8,802	37,978	24,298	8,802
B. Murder						
Treatment*Post	0.100***	0.052	−0.014	0.015	−0.010	0.002
	(0.036)	(0.040)	(0.075)	(0.034)	(0.040)	(0.067)
Mean dep. var.	0.6	0.8	0.3	0.6	0.8	0.3
Observations	36,691	23,696	8,331	36,691	23,696	8,331
C. Robbery						
Treatment*Post	−1.048***	−0.722**	−0.185	−0.080	−0.426	0.156
	(0.306)	(0.320)	(0.485)	(0.381)	(0.330)	(0.458)
Mean dep. var.	14.2	16.2	9.5	14.2	16.2	9.5
Observations	37,978	24,298	8,802	37,978	24,298	8,802
D. Aggravated assault						
Treatment*Post	0.947**	0.207	1.108	−0.246	−0.610	−0.197
	(0.373)	(0.403)	(0.698)	(0.383)	(0.432)	(0.489)
Mean dep. var.	20.5	24.1	12.3	20.5	24.1	12.3
Observations	37,978	24,298	8,802	37,978	24,298	8,802
E. Property crime						
Treatment*Post	1.011	−0.188	4.280	6.876	0.273	3.565
	(1.739)	(1.830)	(3.779)	(5.136)	(2.264)	(3.391)
Mean dep. var.	110.3	118.9	96.3	110.3	118.9	96.3
Observations	37,978	24,298	8,802	37,978	24,298	8,802
F. Burglary						
Treatment*Post	0.817*	1.642***	1.483	1.120**	1.532***	1.131
	(0.475)	(0.524)	(1.107)	(0.486)	(0.526)	(0.878)
Mean dep. var.	22.3	24.5	18.9	22.3	24.5	18.9
Observations	37,978	24,298	8,802	37,978	24,298	8,802
G. Theft						
Treatment*Post	−0.450	−1.706	0.611	5.465	−0.736	1.526
	(1.459)	(1.480)	(3.234)	(5.062)	(1.928)	(3.034)
Mean dep. var.	71.9	76.6	63.4	71.9	76.6	63.4
Observations	37,978	24,298	8,802	37,978	24,298	8,802
H. Motor vehicle theft						
Treatment*Post	0.647*	−0.121	2.188**	0.293	−0.520	0.908
	(0.385)	(0.447)	(0.877)	(0.399)	(0.460)	(0.703)
Mean dep. var.	16.1	17.8	13.9	16.1	17.8	13.9
Observations	37,978	24,298	8,802	37,978	24,298	8,802
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 F.2: Regression discontinuity estimates of the Opportunity Zones designation on crime

	(1)	(2)	(3)	(4)
A. Violent crime				
Designated OZ	0.32	−1.93	−22.97	2.41
	(5.73)	(5.45)	(28.80)	(8.23)
Mean dep. var.	18.3	18.3	19.9	19.9
Observations	2,523	2,523	3,839	3,839
B. Murder				
Designated OZ	−0.54	−0.87	−1.28	−1.14
	(0.56)	(0.54)	(1.09)	(0.81)
Mean dep. var.	0.2	0.2	0.3	0.3
Observations	2,366	2,366	3,605	3,605
C. Robbery				
Designated OZ	0.03	−0.17	−8.91	0.23
	(3.55)	(3.46)	(14.10)	(5.16)
Mean dep. var.	8.0	8.0	8.8	8.8
Observations	2,523	2,523	3,839	3,839
D. Aggravated assault				
Designated OZ	0.30	−1.48	−13.26	2.52
	(4.17)	(3.67)	(16.93)	(6.01)
Mean dep. var.	10.1	10.1	11.0	11.0
Observations	2,523	2,523	3,839	3,839
E. Property crime				
Designated OZ	−24.17	−17.84	−77.77	−18.92
	(44.17)	(38.69)	(126.83)	(54.94)
Mean dep. var.	96.8	96.8	100.0	100.0
Observations	2,523	2,523	3,839	3,839
F. Burglary				
Designated OZ	−2.13	3.78	−5.59	0.53
	(10.44)	(8.94)	(25.38)	(14.74)
Mean dep. var.	17.8	17.8	18.5	18.5
Observations	2,523	2,523	3,839	3,839
G. Theft				
Designated OZ	−15.43	−10.48	−53.74	−16.19
	(40.12)	(34.93)	(109.25)	(48.70)
Mean dep. var.	66.4	66.4	68.5	68.5
Observations	2,523	2,523	3,839	3,839
Designated OZ	−15.43	−10.48	−53.74	−16.19
H. Motor vehicle theft				
Designated OZ	−6.65	−11.19	−18.49	−3.29
	(7.89)	(7.39)	(20.86)	(11.15)
Mean dep. var.	12.6	12.6	13.0	13.0
Observations	2,523	2,523	3,839	3,839
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 F.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	−0.869**	−0.610*	0.032	0.040	−0.052	0.430
	(0.352)	(0.368)	(0.583)	(0.304)	(0.351)	(0.401)
Mean dep. var.	13.6	15.9	8.3	13.6	15.9	8.3
Observations	26,302	16,331	6,104	26,302	16,331	6,104
B. Murder						
Treatment*Post	0.022	−0.001	0.070	0.017	−0.046	0.012
	(0.041)	(0.049)	(0.067)	(0.043)	(0.058)	(0.046)
Mean dep. var.	0.5	0.5	0.3	0.5	0.5	0.3
Observations	25,782	16,155	5,968	25,782	16,155	5,968
C. Robbery						
Treatment*Post	−0.339**	−0.197	−0.226	0.033	0.026	0.098
	(0.141)	(0.150)	(0.234)	(0.169)	(0.158)	(0.200)
Mean dep. var.	4.3	4.9	2.6	4.3	4.9	2.6
Observations	26,302	16,331	6,104	26,302	16,331	6,104
D. Aggravated assault						
Treatment*Post	−0.552*	−0.412	0.191	−0.011	−0.033	0.322
	(0.293)	(0.302)	(0.436)	(0.217)	(0.281)	(0.330)
Mean dep. var.	8.9	10.4	5.5	8.9	10.4	5.5
Observations	26,302	16,331	6,104	26,302	16,331	6,104
E. Property crime						
Treatment*Post	−0.933*	−0.598	−2.111*	1.204	0.041	−1.269
	(0.509)	(0.556)	(1.271)	(1.456)	(0.580)	(1.375)
Mean dep. var.	11.1	12.1	7.9	11.1	12.1	7.9
Observations	26,302	16,331	6,104	26,302	16,331	6,104
F. Burglary						
Treatment*Post	0.074	0.070	0.082	0.128	0.016	0.023
	(0.119)	(0.124)	(0.178)	(0.161)	(0.131)	(0.161)
Mean dep. var.	2.5	2.7	1.8	2.5	2.7	1.8
Observations	26,302	16,331	6,104	26,302	16,331	6,104
G. Theft						
Treatment*Post	−1.062**	−0.746	−2.322*	1.061	−0.048	−1.399
	(0.465)	(0.521)	(1.260)	(1.368)	(0.552)	(1.389)
Mean dep. var.	7.2	7.8	5.0	7.2	7.8	5.0
Observations	26,302	16,331	6,104	26,302	16,331	6,104
H. Motor vehicle theft						
Treatment*Post	0.055	0.078	0.129	0.015	0.073	0.107
	(0.077)	(0.082)	(0.180)	(0.072)	(0.077)	(0.138)
Mean dep. var.	1.4	1.6	1.1	1.4	1.6	1.1
Observations	26,302	16,331	6,104	26,302	16,331	6,104
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 F.4: Regression discontinuity estimates of the Opportunity Zones designation on arrest

	(1)	(2)	(3)	(4)
A. Violent crime				
Designated OZ	−2.71	−1.14	9.28	2.70
	(6.03)	(4.46)	(42.80)	(12.06)
Mean dep. var.	7.7	7.7	8.2	8.2
Observations	1,704	1,704	2,626	2,626
B. Murder				
Designated OZ	−0.70	−0.29	2.71	0.06
	(1.00)	(0.75)	(6.68)	(1.89)
Mean dep. var.	0.2	0.2	0.3	0.3
Observations	1,646	1,646	2,535	2,535
C. Robbery				
Designated OZ	−0.58	1.79	−4.83	−0.63
	(3.17)	(2.38)	(18.94)	(6.23)
Mean dep. var.	2.5	2.5	2.6	2.6
Observations	1,704	1,704	2,626	2,626
D. Aggravated assault				
Designated OZ	−1.45	−2.65	9.16	3.25
	(4.49)	(3.23)	(37.60)	(9.47)
Mean dep. var.	5.0	5.0	5.3	5.3
Observations	1,704	1,704	2,626	2,626
E. Property crime				
Designated OZ	26.09	17.36	137.09	60.34
	(18.79)	(12.97)	(349.37)	(55.40)
Mean dep. var.	8.8	8.8	8.7	8.7
Observations	1,704	1,704	2,626	2,626
F. Burglary				
Designated OZ	1.64	2.15	−0.54	2.98
	(3.45)	(2.50)	(14.86)	(7.11)
Mean dep. var.	1.9	1.9	1.9	1.9
Observations	1,704	1,704	2,626	2,626
G. Theft				
Designated OZ	21.91	12.64	140.35	54.15
	(17.32)	(12.01)	(357.17)	(51.25)
Mean dep. var.	6.0	6.0	5.8	5.8
Observations	1,704	1,704	2,626	2,626
H. Motor vehicle theft				
Designated OZ	2.54	2.57	−2.72	3.20
	(2.11)	(1.67)	(14.66)	(4.04)
Mean dep. var.	0.9	0.9	1.0	1.0
Observations	1,704	1,704	2,626	2,626
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.