Street Vendor Evictions, Crime Reduction, and Gentrification: Evidence from Beijing's Urban Upgrading Campaign

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Abstract: We study the economic and social consequences of Beijing's 2017 Urban Upgrading Campaign, which sought to improve urban environments by removing unauthorized vendors and commercial activities from backstreets and alleys. Exploiting the staggered rollout of the policy across subdistricts between 2017 and 2019, we find that the campaign resulted in a 50.00% drop in the number of restaurants and a 46.58% drop in the number of retail establishments, particularly among low-price and non-chain businesses. We also show that the program reduced crime by around 42.42%, particularly property crime, along with evidence of crime spillovers to adjacent cities. Results on neighborhood composition are consistent with gentrification: housing prices rose modestly while transaction volumes declined, and treated subdistricts experienced population sorting toward higher-income, more educated, and more local residents. Overall, our results highlight a central trade-off of urban enforcement—improvements in public order accompanied by the displacement of informal economic activity and selective migration.

Keywords: urban upgrading campaign; street vendor eviction; crime; gentrification; neighborhood change

JEL Classification Codes: H76, K42, R12, R23, O17

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1 Introduction

Cities around the world struggle to balance public order with inclusiveness. Informal economic activities, such as street vending and unlicensed retail, provide livelihoods and affordable services, yet are often perceived as sources of congestion, disorder, and crime. In response, governments have periodically implemented coordinated "cleanup" or public-order campaigns that restrict informal activity at a fine spatial scale. Prominent examples include the systematic removal and resettlement of street hawkers into regulated markets in Singapore during the 1970s and 1980s, whose legacy of public hawker centers persists to this day, and Johannesburg's cleanup of street traders in 2013, which involved the mass eviction of informal traders from the city center. While such interventions are frequently justified on the grounds of improving urban order and safety, far less is known about their broader consequences for neighborhood composition, local economic activity, and urban inequality.

The relationship between informal commercial activity and urban crime is theoretically ambiguous. Traditional "broken windows" theory posits that visible disorder signals weak social control and invites criminal behavior (Wilson and Kelling, 1982). From this perspective, removing vendors should reduce crime by restoring order and demonstrating government capacity. However, an alternative view, rooted in Jane Jacobs' (1961) concept of "eyes on the street", suggests that commercial activity, even informal, provides natural surveillance that deters crime. Street vendors may also offer legitimate income opportunities that reduce incentives for property crime (Rosenthal and Ross, 2010). Existing empirical evidence is mixed and largely correlational. Some studies document crime reductions following urban cleanup interventions (Braga et al., 1999; Kondo et al., 2016), while others find null effects or raise concerns about measurement and displacement (Harcourt and Ludwig, 2006). Critically, prior work rarely examines whether crime reductions reflect improved security for incumbent residents or instead result from demographic shifts as neighborhoods gentrify and lower-income populations are displaced.

We address this question by studying a large-scale subdistrict-level enforcement cam-

paign launched by the municipal government of Beijing in 2017. The policy aimed to "clean up" backstreets and alleys by removing unauthorized construction, illegal parking, and most prominently, unlicensed street vendors and other informal commercial activities. Implementation began in the historic city core and was subsequently extended to other districts, with substantial variation in the timing of enforcement across subdistricts. We compile a novel subdistrict-by-month panel combining administrative records on policy implementation with data on business establishments, crime incidents, housing markets, population, and household consumption. Exploiting the staggered rollout across subdistricts, we estimate staggered difference-in-differences models with flexible event-study designs to trace the dynamic effects of the policy on local economic activity, public safety, and neighborhood composition.

Our analysis yields three main findings. First, as stylized facts, the policy dramatically transformed local economic landscapes. The number of restaurants in backstreets fell by 50.00% and shopping establishments by 46.58%, with disproportionate losses among low-price restaurants (dropped by 44.73%) and non-chain retailers (dropped by 44.12%). These patterns indicate that enforcement targeted precisely the informal and small-scale commercial activities that serve lower-income populations. Nighttime light intensity—a proxy for evening economic activity—declined by 8.61%, consistent with reduced subdistrict life and commerce. The magnitude of these changes substantially exceeds typical year-to-year variation and represents a fundamental reshaping of neighborhood commercial character.

Second, we document substantial and persistent reductions in crime following policy implementation. Treated subdistricts experience 0.070 fewer crimes per month, i.e., a 42.42% decline relative to the pre-treatment mean of 0.165 crimes. Effects are concentrated in property crimes, with particularly large reductions in burglary (dropped by 37.50%) and robbery (dropped by 100.00%). Event study estimates using multiple robust difference-in-differences estimators confirm parallel pre-trends and show that effects emerge immediately upon treatment and persist for at least two years. We find no evidence that crime is simply displaced to nearby untreated subdistricts or main avenues, suggesting genuine local crime reduction rather than spatial reshuffling.

Third, we find evidence of significant demographic change consistent with policy-induced gentrification. Population in backstreet areas fell by 24.35%, while the composition of remaining residents shifted markedly upward in socioeconomic status. Using household survey data, we show that treated areas experienced a 0.10 percentage point increase in the share of high-income residents, an 4.60 percentage point increase in high-education residents, and a 3.70 percentage point increase in local Beijing hukou holders, with corresponding declines in low-income and migrant populations. Housing market data reveal modest price increases (though estimates here are less precise due to pre-trend concerns) alongside declining transaction volumes, suggesting reduced affordability and market liquidity. Per capita household consumption in treated districts increased by 16.77%, further confirming an upward shift in resident socioeconomic status.

In sum, these results reveal a fundamental trade-off embedded in urban order policies. The Beijing campaign achieved its stated objective of reducing crime and disorder. However, it did so not only through enhanced surveillance and enforcement—we document a 1.51% increase in security camera installations—but also by fundamentally altering who lives and works in treated neighborhoods. The policy displaced lower-income residents and eliminated the informal businesses that catered to them, turning mixed-income neighborhoods into wealthier and more tightly regulated areas that register lower crime rates. Whether one views this as a successful urban policy or an inequitable exclusion depends critically on how one weighs public safety gains for remaining residents against the costs imposed on displaced vendors, shopkeepers, and lower-income households.

This paper makes several contributions. First, we provide the first high-granularity evidence on how large-scale vendor eviction policies affect crime in a developing-country context. While a substantial literature documents correlations between physical disorder and crime (Skogan, 1990; Sampson and Raudenbush, 1999), and scattered studies evaluate cleanup interventions in developed countries (Braga and Bond, 2008), rigorous causal evidence from developing cities, where informal economic activity is far more prevalent, remains scarce. More generally speaking, previous studies have delved into factors of crime occurrence in a public or urban perspective, such as housing (Khalil and Sanfelice, 2025; Kling et al., 2005;

Sandler, 2017; Aliprantis and Hartley, 2015), neighborhood conditions and urban development (Bellégo and Drouard, 2024; Borbely and Rossi, 2023; Branas et al., 2018; Mitre-Becerril and MacDonald, 2024), private investment (Lacoe et al., 2018), education (Berthelon and Kruger, 2011), social capital (Buonanno et al., 2009), financial assistance (Palmer et al., 2019), and transportation access (Phillips and Sandler, 2015), among others. The urban program in this study directly targets the clearance of illegal street vendors and operates at a very fine-grained backstreet and alley level, thereby offering a new perspective on the above literature on crime. Furthermore, our findings speak to debates about mechanisms underlying crime reductions in place-based interventions. Prior work often cannot fully distinguish whether crime falls due to deterrence, incapacitation, changed criminal opportunities, or demographic change, such as Ellen et al. (2013), MacDonald et al. (2016), among others. This study jointly examines crime, enforcement, commercial activity, and population composition. Therefore, we provide more comprehensive interpretations of between-factor relationships and explanations of crime dynamics.

Second, we contribute to the growing literature on gentrification and displacement in developing country cities (Lees et al., 2016; Ghertner, 2015). Related work has analyzed neighborhood change, gentrification, and residential sorting in terms of housing-market dynamics, amenities, and household mobility (Guerrieri et al., 2013; Lee and Lin, 2018; Couture and Handbury, 2023; Ferreira et al., 2023; Harari and Wong, 2025), though recent evidence also suggests that some forms of redevelopment do not necessarily induce gentrification (Boustan et al., 2023). Evidence from large-scale residential relocation programs likewise underscores the long-run importance of spatial reallocation for inequality and neighborhood outcomes (Chyn et al., 2025). In this study, we show that "public order" policies, often treated as distinct from housing or development policy, can have profound distributional consequences that rival those of explicit redevelopment programs. This matters for how we conceptualize urban inequality: exclusionary processes operate not only through housing markets and formal planning, but also through seemingly neutral enforcement of order and regulations.

Finally, our results have direct policy relevance for the hundreds of millions of people living in cities where informal economic activity remains widespread. Street vendor evictions

are politically popular, often enjoying support from formal businesses and middle-class residents (Anjaria, 2016; Meneses-Reyes and Caballero-Juárez, 2014; Bates and Santerre, 2001). Our evidence suggests that such policies can indeed reduce crime, but that a complete cost-benefit evaluation must account for costs borne by displaced vendors and residents who lose access to affordable goods, services, and housing. More broadly, our findings illustrate how ostensibly technical policies aimed at "order" and "safety" can function as instruments of spatial exclusion, raising normative questions about whose safety matters and who belongs in the urban core.

The remainder of this paper proceeds as follows. Section 2 provides institutional background on Beijing's street cleanup policy and documents the geographic and temporal variation in its implementation. Section 3 describes our data sources, variable construction, empirical strategy, and stylized facts of vendor eviction. Section 4 presents our main results on crime and gentrification. Sections 5 and 6 examine mechanisms and potential spillover effects, respectively. Section 7 concludes.

2 Background

In April 2017, the Beijing government launched the "Three-Year Action of Backstreet Environmental Improvement in the Capital Core Area (2017-2019)" to enhance urban quality of life by improving backstreet environments. The policy file explicitly states that 2,435 backstreets (without the full list of backstreets released) in the Capital Core Area, i.e., Dongcheng District and Xicheng District, are to be improved. In practice, other districts throughout the city have also successively carried out the backstreet improvement project. This urban upgrading campaign was framed as "ten prohibitions and one initiative". The ten prohibitive requirements include demolishing unauthorized construction, expelling wall-breaking vendors, and other improvements to the built environment, including facades, signage, road surfaces, and overhead wires. The one initiative aims to promote "civilized" streets and alleys, placing greater emphasis on social order, public governance, and moral requirements. In Table A1, we provide detailed contents of each regulation that we collect from the official

governmental website. Among the aforementioned measures, the eviction of wall-breaking vendors is one of the most strictly enforced and has received the most attention. Wall-breaking (*kai qiang da dong* in Chinese) refers to small informal businesses that are typically located in less prominent, less visible areas such as side alleys or backstreets.

As China's capital and a global mega-city, Beijing has a large population of 22 million and a substantial volume of commercial activity. The total retail sales of consumer goods of Beijing reached 1,367.7 billion yuan (nearly 200 billion US dollars) in 2025. Beijing has a multi-tier administrative system. Below the municipal level, Beijing is divided into 16 districts (*qu*), which are county-level units and supervise the next tier: subdistricts (*jiedao*) in urban areas and townships (*xiangzhen*) in more rural areas. Subdistrict offices serve as the district's grassroots administrative arm, coordinating community management, social assistance, public affairs, and law enforcement, among other functions. In practice, policy and budget authority flow from the municipal level to districts, while subdistricts focus on implementation and community-level service delivery. For simplicity, throughout this study, we refer to all urban subdistricts and rural townships as subdistricts. Beijing consists of more than 300 subdistricts, and the average area is approximately 50 square kilometers.

According to a 2019 government report, this campaign successfully upgraded 5,393 city-wide backstreets in Beijing, double the proposed number. The report states that the program effectively enhanced urban quality and cultural heritage over the three years of program implementation. However, unofficial evidence reflects contradictory voices (Fan et al., 2017; Myers, 2017). On the one hand, some residents praised the restoration of cleanliness and quietness to the backstreets. On the other hand, many informal businesses that previously offered convenient dining and shopping closed, leading to complaints from residents and tourists about reduced convenience. Moreover, small business owners faced relocation challenges.

A prominent case study that encapsulates these contradictory outcomes is the transformation of Sanlitun's (one of the busiest commercial centers in Beijing) infamous "dirty subdistrict". Before the backstreet upgrading campaign, the dirty subdistrict was a com-

mercial and pedestrian hub, home to many bars, restaurants, and small businesses. The subdistrict earned its name from the conditions created by these businesses: garbage and dirty water were dumped directly onto the pavement, and the area was plagued by noise and public nuisance from bars, leading residents to frequently call the police. In April 2017, as part of the citywide initiative, 33 illegal businesses along this subdistrict were dismantled. The impact on the local environment was immediate and profound. The building's original appearance was restored, the reclaimed public space was converted into green areas, and police-reported incidents in the vicinity dropped by a third. However, residents also said they lost the convenience of gathering and dining out (Zhang, 2017).

3 Data, Design, and Stylized Facts

3.1 Data

Rollout dates of the campaign. To understand how the urban upgrading campaign reshapes street businesses and crime dynamics, a challenge is to collect exact implementation dates at a granular spatial level. The government office of Beijing does not disclose consolidated implementation timelines. Hence, we manually collect exact implementation dates at the subdistrict level from subdistrict official websites and social media (e.g., WeChat) accounts, as well as from newspaper coverage. In Figure A1, we show an example of how we identify the subdistrict-level implementation dates from WeChat. The time of implementation at the backstreet or alley level is unavailable. Hence, all backstreets in a given subdistrict are assumed to have been treated simultaneously. Figure 1 Panel A displays the implementation batches of subdistricts across the city.

The map indicates a possible association between the distance to the city center and the starting date of the campaign. Here, we conduct a balance test to deal with the randomization concern in later sections. Figure 2 contains the results. We split the treated subdistricts into early-treated and late-treated based on the median of implementation dates. We then

regress six pre-determined characteristics in 2010 on the dummy of early-treated. The characteristics include the subdistrict-level ratio of aged 0-14, the ratio of aged 15-64, the ratio of aged above 65, the ratio of local residents, the total population, and the distance to the district center. Samples are restricted to only treated subdistricts. The regression includes district fixed effects, and we adopt robust standard errors. For simplicity reasons, we do not include a regression equation here. All population-related characteristics show balanced patterns between subdistricts, whether they were early or late treated. The average distance to the district center is smaller for early-treated ones, but the difference is statistically insignificant. In a robustness test of this study, we include the distance factor in models, and our results survive.

Another challenge is to identify backstreets from other types of streets, such as avenues, highways, trunks, among others. We collect the geographic dataset of all streets in Beijing. Using road types and road classes in the dataset, we identify backstreets by their functional classes. Our spatial data classifies roads into 27 levels, with smaller road-level numbers indicating higher classes. We exclude level 12 (trunk) and all higher classes, and identify the remaining lower-class roads as backstreets. Specifically, identified backstreets include those types labeled as cycleway, footway, living street, path, pedestrian, residential, service, steps, and track, among others. Figure 1 Panel B displays an example of identified backstreets (against other streets) in some neighborhoods. Finally, the analysis sample (2016–2019) covers approximately 40,000 backstreets and 339 subdistricts.

Crime data. The crime data come from *China Judgments Online* (CJO), the national repository of court decisions maintained by the Supreme People's Court. This database has been widely adopted across the social sciences disciplines due to its extensive case narratives, legal classifications, and procedural details, which provide multiple variations to analyze patterns of crime, enforcement, and sentencing, among others.

For each case, we extract the offense type, the incident date when available, and brief factual descriptions. Moreover, we retain only first-instance records of criminal offenses, and the crime types include robbery, traffic offense, burglary, drug, fraud, and murder, etc. We

exclude civil litigation and follow-up judgments issued after the first instance. To enhance consistency over time, we remove cases with missing or implausible incident dates. We then identify all criminal incidents that occurred within Beijing from the raw court judgment data. We expand each case into separate event records whenever the text specifies multiple offenses by the same defendant at different times. For example, one case documents two thefts on March 1, 2015 and February 1, 2016. We divide them into two separate criminal events.

A challenge is precisely locating where the offense occurred, as the crime descriptions only contain unstructured address information. We employ the large language model (LLM)-processed and geocoded criminal case locations provided by Zhang et al. (2025). They use LLM to standardize and extract location descriptions (e.g., streets, neighborhoods, buildings) from CJO. The extracted address strings are then geocoded into latitudes and longitudes using the Baidu Maps API, producing point locations for each incident.

The next step is to match crime with backstreet segments. Within each subdistrict, we only count crimes that happened within 50 meters of backstreets, the targets of the urban upgrading program. In robustness checks, we also apply 70 m and 100 m buffers of the backstreet crime definition. If a backstreet spans two subdistricts, we assign it to the subdistrict in which its sub-segment is longer. In the main analysis, we exclude crimes that occurred farther from the backstreets. In another test on crime patterns near non-backstreets (avenues), we count crime within a 50 m buffer around avenues. This study is primarily conducted at the subdistrict-year-month level. We have two outcome variables for crime: the subdistrict-year-month crime count and the crime ratio, defined as the number of crimes per subdistrict-level population. We collect the subdistrict population from the Sixth Chinese National Population census (2010). The baseline sample period is restricted to 2016-2019. Table 1 summarizes the statistics of criminal cases. We split the sample into the control group (never-treated subdistricts) and the treatment group (ever-treated subdistricts). The subdistrict-year-month counts of crimes for the control and treatment groups are 0.055 and 0.259, respectively, and the majority of crimes are robbery.

Restaurants and shopping places. To demonstrate stylized facts, such as whether the urban upgrading campaign led to the eviction of street businesses, we aim to investigate the spatial patterns of restaurants and stores. We collect lists of business establishments in Beijing in the form of points of interest (POI) with yearly variations, sourced from the *OpenStreetMap*. These data contain the name, location, and type (e.g., restaurant, shopping places, public amenities, enterprises, among others). Because the campaign primarily targeted street vendors, and other POI categories have many missing values, this study focuses on restaurants and stores. This study further distinguishes restaurants by market segment. We use Beijing order data from *Dianping*, one of China's largest online food-delivery platforms, and match restaurants by name, retaining venues with at least 85% name-match similarity. Based on the annual median of per-capita spending, we classify restaurant POIs into high-end and low-end categories. In the original data, shopping places include various types such as shopping malls, convenience stores, retail shops, and supermarkets. We manually identified chain shopping venues in Beijing to distinguish them from other non-chain establishments.

Housing prices and rents. Housing purchases and prices could reflect localized residential costs and underlying population characteristics. We collect resale housing transactions in Beijing between 2016 and 2019 from *Lianjia*, one of the top real estate brokerage companies in China. *Lianjia* is a common source of housing data that includes transaction dates, prices, flat areas, level ranges, geocoded locations, and other detailed information. We also obtain rental records in Beijing from *Lianjia* with property attributes, contract dates, and location information. Rental prices may reflect the housing burden faced primarily by the migrant population. The summary statistics for POIs and housing data are also provided in Table 1.

Supplementary datasets. Moreover, we consolidate supplementary datasets that contain control variables. From the 2015 China County Statistical Yearbook, we collect district-level annual statistics, such as GDP per capita, sex ratio, middle-aged (15-64 years old) ratio, the proportion of workers, the number of hospitals, and the pollution level (measured by PM2.5). For mechanism analyses, we employ datasets such as high-resolution grid-level population changes, satellite-based nightlight data, the China Household Finance Survey

(CHFS), surveillance monitors, and police expenses.These supplementary data serving as the outcome variables of the mechanism analysis, and detailed information is introduced in the section on mechanisms. The definitions of crime variables, gentrification proxies, and other supporting variables can be found in Table A2 in the Appendix.

3.2 Empirical Design

For the baseline analyses of crime patterns, this empirical work is primarily conducted at the subdistrict-year-month level. We adopt the staggered Difference-in-Differences (DID) strategy. As shown in Equation (1), the sample is a panel dataset indexed by subdistrict s and year-month t. The outcome $Crime_{st}$ is measured by (i) the number of crime cases or (ii) the crime ratio, defined as the number of crimes (subdistrict by year-month) divided by the district-level population. As discussed above, we only count crimes that occurred within 50 m buffers of the backstreets in a subdistrict.

The key independent variable, $Post_{st}$, denotes whether a subdistrict experienced the upgrading campaign in a given year-month. The model includes subdistrict fixed effects θ_s and year-month FE τ_t are included. Due to the granularity of the setting, an empirical challenge is the unavailability of time-varying variables, such as population compositions. To overcome this challenge, we collect cross-sectional attributes in 2015, including GDP per capita (in log-form), the proportion of the population aged 15–64, the male share of the population, the number of hospitals, the employment rate, and the pollution (PM2.5) level (in log-form), denoted by the variable vector X_{dy}. The error term is u_{st}. Robust standard errors are clustered at the subdistrict level. Besides the static version of DID estimation, we also conduct event studies to show dynamic patterns of outcome variables across the sample time window. When showing dynamic results, we adopt the pre-trend diagnostic strategies developed by (Roth, 2022) and multiple advanced estimators, such as De Chaisemartin and d'Haultfoeuille (2020), and Callaway and Sant'Anna (2021).

When analyzing mechanisms, we replace the outcome variable of crime with other socio-economic consequences, such as population and nightlight. Note that due to low frequencies

of population and nightlight, the analyses will be conducted at the subdistrict-year level. Other specifications remain the same, so we do not explicitly list the equations.

$$Crime_{st} = \alpha + \beta Post_{st} + X'_{dy}\Gamma + \theta_s + \tau_t + u_{st} \tag{1}$$

Another key focus of this study is gentrification, and we use housing measurements as main proxies. To be more specific, the housing outcomes include (resale) housing prices, transaction volumes, and rents. As prices and rents are collected at a pooled cross-sectional basis, the analyses are conducted at the transaction (or rental contract) level, indexed by property i in subdistrict s sold/rented in year-month t, as modeled in Equation (2). We also include transacted property attributes (has elevator or not, decoration level, property age, number of rooms, and floor level) or rented flat attributes (number of bedrooms, south-faced or not, rental type, and floor level), as denoted by H_{ist}. When testing patterns in transaction volumes, we aggregate the number of transactions at the subdistrict-year-month level, as shown in Equation (3). Other specifications are set the same as Equation (1).

$$HousePrice_{ist} = \alpha + \beta Post_{st} + H'_{ist}\Delta + X'_{dy}\Gamma + \theta_s + \tau_t + u_{st} \tag{2}$$

$$HouseVolume_{st} = \alpha + \beta Post_{st} + X'_{dy}\Gamma + \theta_s + \tau_t + u_{st} \tag{3}$$

3.3 Stylized Facts

Before delving into the baseline analysis of crime and gentrification, in this subsection, we present the stylized facts on how the street program influences street businesses. Using the annual POI data, we conduct the analysis by regressing numbers of restaurants and shopping places on the policy implementation indicator. The regression model is similar to Equation (1) but at a less granular subdistrict-year level. Results are shown in Table 2. The

results indicate that the street campaign is followed by sizable reductions in local commercial activity as proxied by POIs. The estimated coefficients on *Post* are negative and statistically significant across all specifications. The total number of restaurants declines by about 50.00% (1-exp(-0.693)), with both high-price and low-price restaurants contracting. Shopping places exhibit a similar pattern: total establishments fall by roughly 46.58% (1-exp(-0.627)), driven by decreases in both chain and non-chain venues.

Furthermore, Figure 3 reports dynamic effects of the program on local business counts using staggered-adoption event studies. Across panels, we generally show parallel trends using different estimators. The dynamic trends are consistent with the static estimates: total restaurants fall, with declines evident for both high-price and low-price segments, and shopping places contract, mainly driven by reductions in non-chain venues. We further conduct the pre-trend diagnose procedure, as shown in Appendix Figure A2, and our dynamic trends do not indicate pre-trend concerns to a large extent.

The stylized facts provide compelling evidence that this urban upgrading campaign generates a powerful and persistent impact on nearby commercial activity. Next, we will focus on how crime and gentrification factors change in response to the campaign and investigate the mechanisms that drive those effects.

4 Main Outcomes

This section presents the main outcomes of the backstreet cleanup campaign. We focus on two sets of effects that are central to the policy debate. As a main outcome, Section 4.1 examines the impact of the campaign on local crime, assessing whether intensified subdistrict-level enforcement translated into improvements in public order. As a subsequent outcome, in Section 4.2, we study gentrification, using housing market and consumption outcomes to assess whether the policy reshaped the communities. Together, these results provide a unified account of the consequences of subdistrict-level urban enforcement, including changes in public safety and broader patterns of neighborhood changes.

4.1 Crime

4.1.1 Results

After documenting a set of stylized facts on how the cleanup campaign reshaped subdistrict-level commercial activity, we turn to its implications for public safety. Specifically, we examine whether the staggered rollout of the backstreet cleanup policy led to changes in local crime outcomes. We follow equation 1 and replace the outcome variable with measures of crime at the subdistrict–month level. We consider two complementary outcomes. First, we use the number of crimes occurring on each subdistrict. Second, to account for differences in population size across locations, we construct a crime ratio by scaling crime counts by the district-level population in 2010. In both cases, the unit of analysis is the subdistrict-month, and identification comes from variation in the timing of policy implementation across subdistricts.

Table 3 reports the main estimates. Columns (1)–(3) use the number of crimes as the dependent variable, while Columns (4)–(6) use the crime ratio. Across specifications, we progressively incorporate a rich set of economic time-trend controls to account for differential pre-existing trends across subdistricts. Our preferred specifications, which include the full set of controls, are presented in Columns (3) and (6). The results consistently show that the cleanup policy led to a significant decline in crime. Using crime counts, we estimate that treated subdistricts experienced 0.070 fewer crimes per month following implementation, corresponding to a reduction of about 42.42% relative to the pre-treatment mean in 2016. Using the population-adjusted crime ratio yields a similar conclusion: the policy is associated with a statistically significant decline of 0.011 in the crime ratio, indicating that the finding is not driven by population scale.

To sum up, these estimates provide robust evidence that the backstreet cleanup campaign achieved its stated objective of improving public order by reducing crime at a very fine spatial scale. This finding speaks to a long-standing theoretical debate on the relationship between informal commercial activity and urban crime. While the "broken windows" view emphasizes

that visible disorder facilitates crime and predicts crime reductions following cleanup, the "eyes on the street" perspective highlights the crime-deterring role of active subdistrict-level commerce through informal surveillance. Our results suggest that, in this context, the crime patterns are more consistent with the predictions of the broken windows framework.

4.1.2 Heterogeneity

To further characterize the crime response to the cleanup campaign, we examine heterogeneity along two dimensions: across different types of crime and across different categories of nearby POIs. First, crime categories differ substantially in their prevalence, severity, and relationship to subdistrict-level environments. Some offenses, such as burglary and robbery, are relatively frequent and closely tied to backstreet conditions, while others—such as traffic offenses or serious crimes including drug-related offenses and homicide—are less common. Second, because crime locations can be precisely geocoded and the cleanup campaign explicitly targeted unregulated commercial activity, we examine whether the effects are more pronounced around commercially oriented POIs rather than around places such as public service facilities or urban government.

Heterogeneity by Crime Types. First, we examine heterogeneity across crime types. In our sample, crime incidents are classified into six categories: burglary, robbery, traffic offenses, drug-related offenses, fraud, and murder. For each category, we aggregate the number of incidents at the backstreet–year–month level and re-estimate our baseline specification separately by crime type.

As shown in Table 4, the cleanup campaign led to significant declines in burglary and robbery, both in terms of crime counts and population-adjusted ratios. These crimes are typically opportunistic and closely linked to local environmental conditions, making them particularly responsive to improvements in perceived order and enforcement. In contrast, we find no statistically significant effects on traffic offenses or more serious crimes, such as drug-related offenses and murder. One plausible explanation for the null effect on traffic offenses is that backstreets are often narrow and low-speed environments, where drivers

already exercise caution when driving. Similarly, serious crimes tend to be less frequent and less directly connected to subdistrict-level disorder. Additionally, this pattern is consistent with the idea that subdistrict-level cleanup primarily affects offenses that are more sensitive to visible order, a prediction consistent with the notion of the broken windows framework.

Heterogeneity by POI. We next examine heterogeneity across nearby POI categories. Specifically, for each category of POI, we draw a 50-meter-radius buffer around the POI locations, aggregate crime incidents occurring within these buffered areas, and construct a POI-specific panel at the year–month level. We then re-estimate our baseline specification separately for each POI category. This approach allows us to assess whether the crime-reducing effects of the cleanup campaign were concentrated in the types of urban spaces most directly affected by the policy.

The results, presented in Appendix Figure A3, show that the crime-reducing effects are particularly pronounced around food and beverage establishments, shopping-related POIs, commercial service locations, and infrastructure facilities. The first three categories are directly consistent with the primary targets of the cleanup campaign, which explicitly focused on informal and unregulated commercial activity in backstreet areas. The significant effects around these POIs therefore suggest that the policy was most effective in places where street-level commercial disorder had been most concentrated. Interestingly, we also observe significant crime reductions around infrastructure-related POIs. One possible interpretation is that cleanup efforts in these areas were accompanied by improvements in physical order and environmental visibility, such as the removal of obstructions or reinforcement of public space management, making it more difficult for opportunistic crimes to occur or hide.

By contrast, we do not find statistically significant effects around tourist attractions, convenience-oriented public facilities, government institutions, or other public-service-related locations. These null results are also intuitive, as such places are less directly associated with the informal commercial activities that the cleanup campaign primarily sought to regulate. Overall, the POI heterogeneity results reinforce the view that the observed crime reductions were spatially concentrated in the urban environments most directly exposed to the policy.

4.1.3 Parallel Trend

To assess the dynamic effects of the cleanup campaign and examine the parallel trends assumption, we first estimate a conventional two-way fixed effects (TWFE) event-study specification. Specifically, we replace the treatment indicator in Equation 1 with a series of leads and lags relative to the first quarter of cleanup implementation in each treated backstreet, while controlling for subdistrict and year-month fixed effects, as well as the full set of baseline controls.

The event-study estimates are plotted in Figure 4, with crime counts as the dependent variable in Panel A and the crime ratio in Panel B. There is little clear evidence of differential pre-trends between treated and yet-untreated backstreets prior to the cleanup campaign. The coefficients in the pre-treatment periods are generally small and statistically indistinguishable from zero, while the post-treatment coefficients become negative following implementation and remain persistently below zero thereafter.

At the same time, because the cleanup campaign was rolled out in a staggered manner across subdistricts, conventional TWFE event-study estimates may suffer from "negative weighting" problems when treatment effects are heterogeneous over time. To address this concern, we complement the TWFE estimates with event-study specifications based on more recent estimators designed for staggered adoption settings, including De Chaisemartin and d'Haultfoeuille (2020) and Callaway and Sant'Anna (2021). The dynamic patterns obtained from these alternative estimators are similar to those from the TWFE specification, again showing no meaningful pre-treatment differences and a clear decline in crime after treatment. As shown in Appendix Figure A4, when we adopt the pre-trend diagnoses, the fitted trend line lies well within the confidence intervals of the pre-treatment coefficients, while clearly deviating from the post-treatment estimates. This pattern suggests that the pre-treatment periods are broadly consistent with parallel trends, whereas the post-treatment divergence reflects a significant policy effect.

4.1.4 Robustness Checks on Crime Outcomes

We conduct a broad set of robustness checks to assess the validity of our identification strategy and the stability of the crime results. We summarize the key findings below and report the detailed analyses in Appendix Figure A5 and Tables A3 to A8.

These checks address several potential threats, including within-city spillovers that may contaminate the control group, nonrandom rollout of the cleanup campaign, and anticipation effects prior to implementation. In summary, we find little evidence that untreated nearby areas or major roads experienced meaningful increases in crime, suggesting that the baseline estimates are unlikely to be driven by simple within-city displacement. We also show that the main treatment effect remains stable after flexibly controlling for differential trends related to proximity to district government, and under alternative designs that explicitly account for possible anticipation effects.

In addition, we perform a range of placebo and specification checks. These include placebo assignments of treatment status and timing, alternative sample restrictions based on road type and traffic accessibility, alternative definitions and transformations of the crime outcome, and alternative spatial buffers used to assign crime incidents to backstreets. Across all of these exercises, the main findings remain qualitatively unchanged.

4.2 Gentrification

As a second-order outcome, we next examine whether the backstreet cleanup campaign was associated with gentrification. We use housing market outcomes and household consumption as proxies for gentrification, capturing both changes in asset prices and shifts in the socioeconomic status of residents (add literature on housing prices / consumption as standard proxies for gentrification).

Housing Market Outcomes. We first turn to housing market outcomes. Our housing data come from one of the largest real estate brokerage platforms in China, which provides

detailed transaction-level records for both second-hand housing sales and rental listings. The data include residential compound identifiers and geocoded information. To spatially match housing units to backstreets, we first calculate the distance between each residential compound and nearby road segments. Housing units are assigned to a given backstreet if the entrance of the corresponding residential compound falls within a 50-meter buffer of that road segment. We then retain only those housing units located within backstreet areas and assign each unit to the corresponding subdistrict. For housing prices, the outcome variable is the transaction unit price measured in RMB per square meter, and we also consider its logarithmic transformation. We also construct monthly transaction volume by aggregating the number of housing transactions within each subdistrict–month. All specifications include transaction year–month fixed effects and subdistrict fixed effects.

The results are presented in Panel A of Table 5. We find that housing prices in treated subdistricts increased modestly following policy implementation, with an estimated increase of approximately 646.60 RMB per square meter. However, this effect becomes statistically insignificant when we use log unit price as the dependent variable. At the same time, transaction volumes declined slightly, by approximately 1.90 units per month in treated backstreets. This pattern is broadly consistent with the decline in local commercial activity documented above. With reduced economic activity in treated areas, both housing demand and transaction activity may have weakened. In addition, we observe positive but statistically insignificant changes in rental prices.

Overall, the housing market evidence points to mild signs of neighborhood upgrading, but the results should be interpreted with caution. Relative to the crime outcomes, the housing estimates are less precisely estimated and exhibit weaker statistical power. One possible explanation is that unlike crime, which can respond quickly to changes in enforcement and local conditions, housing prices are rigid in the short term, adjusting more gradually as information diffuses and residents update their expectations about neighborhood quality.

Household Consumption. As complementary evidence on gentrification, we turn to household consumption outcomes. We use data from the China Household Finance Survey

(CHFS) for the years 2015, 2017, and 2019. Our main outcome is per capita household consumption, constructed as total household consumption expenditure divided by household size (measured in RMB per capita), and we also consider its logarithmic transformation. We then repeat the regression analysis in Equation 3, replacing the outcome variable with household consumption measures and including household, district, and survey-year fixed effects. The results, presented in Panel B of Table 5, show that per capita consumption increased in treated areas following the cleanup campaign. This rise in consumption provides additional support for an upward shift in the socioeconomic composition of residents, consistent with higher-income households becoming more prevalent in these neighborhoods.

The housing market and consumption results point to meaningful neighborhood change following the policy. While the housing evidence is relatively modest and should be interpreted with caution, the increase in household consumption provides additional support for the view that the cleanup campaign was associated with gentrification, characterized by rising housing values, reduced transaction activity, and a more affluent resident base.

5 Possible Mechanisms

Having documented the effects of the cleanup campaign on crime and gentrification, we next explore potential channels through which these outcomes may have emerged. We examine three broad sets of mechanisms. First, the campaign may have reduced overall economic activity at the backstreet, thereby changing criminal opportunities in treated areas. Second, the policy could be associated with shifts in population composition toward higher socioeconomic status, which may have shaped both crime patterns and gentrification dynamics. Third, the cleanup effort may have been accompanied by enhanced police presence and surveillance. These channels provide a conceptual framework for understanding how clear-up enforcement is associated with the main outcomes.

5.1 Nightlight and Population

The first channel we explore concerns changes in overall economic activity in treated backstreets. By clearing informal vendors and restricting unlicensed commercial operations, the cleanup campaign may have reduced economic intensity in the neighborhood. A contraction in local activity could, in turn, affect crime by reducing foot traffic, commercial transactions, and the density of potential targets, thereby decreasing criminal activities. To examine this channel, we use two complementary measures: backstreet-level population and nighttime light intensity between 2016 and 2019.

First, we construct a measure of subdistrict-level population using the LandScan Global Population Database, a widely used high-resolution gridded population dataset that provides annual estimates of population counts at fine spatial resolution (Dobson et al., 2000). LandScan distributes ambient population across geographic grid cells based on census data, land use, transportation networks, and other spatial covariates. Following our spatial matching procedure used for crime outcomes, we calculate the distance between each population grid cell and road segments. Grid cells whose centroids fall within a 50-meter buffer of a backstreet are classified as belonging to that street. We then retain only grid cells located

within backstreet areas and assign each grid cell to its corresponding administrative subdistrict unit. Subdistrict-level population is computed by summing the population counts across all relevant grid cells within each backstreet-year. In the regression analysis, we use the logarithm of population and include subdistrict and year fixed effects.

Second, we measure nighttime economic intensity using satellite-based nightlight data from Li et al. (2020), which provide harmonized and calibrated nighttime light intensity measures across years. Nighttime light has been widely used as a proxy for local economic activity in the absence of high-resolution administrative data (Henderson et al., 2012). For each subdistrict, we calculate the average nightlight brightness within the backstreet buffer area. We then estimate regressions of the logarithm of average nightlight intensity on the treatment indicator, controlling for subdistrict and year fixed effects.

The results are shown in Table 6. We observe substantial declines in both population and nighttime economic intensity following policy implementation. The coefficient on the treatment indicator is -0.279, significant at the 1% level, for log population, implying a sizable contraction in the number of residents in treated backstreets. Similarly, log nightlight intensity declines by 8.61% (1-exp(-0.09)), also significant at the 1% level. This indicates a measurable reduction in overall economic activities during nighttime hours. These findings suggest that the cleanup campaign was associated with a slowdown in street-level economic activity and a decline in local population density. Such reductions in economic intensity may have reduced criminal opportunities by lowering the concentration of potential targets and routine activities in treated areas.

5.2 Composition Change

A second potential channel is compositional change. Although the policy led to a decline in overall population and economic activity, the socioeconomic profile of the remaining residents may have shifted upward. If higher-income, more educated, and more locally rooted residents became more prevalent in treated areas, such demographic re-sorting could affect both crime patterns and gentrification outcomes.

We rely on two complementary data sources. First, we use individual-level data from the China Household Finance Survey (CHFS), a nationally representative household survey that collects detailed information on household income, assets, education, hukou status, and demographic characteristics. This biennial panel design allows us to track individuals across waves and assess whether residents remain in or exit a district, providing insight into population re-sorting over time. Second, we use complementary data from the Sixth (2010) and Seventh (2020) National Population Censuses. The population census is a universal enumeration with extensive micro-level demographic information, providing highly representative and consistent measures of population structure.

The results, reported in Table 7, reveal substantial compositional changes following the initiation of the cleanup campaign. In Panel A, we use CHFS individual-level data to construct indicator variables for whether an individual belongs to a given socioeconomic group, such as high-income, high-education, or local hukou status. Individuals are matched to their district of residence. We define a district-level treatment indicator, $Post_d$, which equals one beginning in the year when the first backstreet cleanup was implemented within a given district, and zero otherwise. The estimates show that treated districts experienced a 0.10%. increase in the share of high-income residents and an 4.60%. increase in the share of residents with higher education. The proportion of local Beijing hukou holders rose by 3.70%, accompanied by corresponding declines in low-income and low-education populations.

As complementary and robustness evidence, we also turn to population Census data, which yield consistent patterns. The Census has two key advantages. First, it offers stronger representativeness, as it is based on a large-scale nationally administered survey with near-universal coverage. Second, it provides finer geographic identifiers, allowing us to conduct the analysis at the subdistrict level. At the same time, however, the Census is not a longitudinal dataset that tracks individuals over time. As a result, we cannot follow individual-level transitions. Instead, we construct subdistrict-level demographic ratios. Specifically, using data from the Sixth (2010) and Seventh (2020) National Population Censuses, we compute the share of local residents and the shares of young, middle-aged, and elderly population at the subdistrict level (information on income and education is not available in the Census).

These ratios allow us to examine structural demographic shifts at the subdistrict level using a long-difference specification between 2010 and 2020. We define $I(2020)$ as a post indicator for the later Census wave and define treated subdistricts as those that had implemented at least one cleanup action by 2020. The estimation results, reported in Panel B, show that subdistricts exposed to the cleanup campaign experienced a 3.70%. increase in the share of local residents. The share of young population rose by about 1.70%, while the share of elderly residents declined by roughly 1.50%.

In sum, these findings suggest that the cleanup campaign was associated not only with population decline, but also with systematic upward re-sorting in the socioeconomic composition of residents. Such compositional shifts, combined with population decline, are closely associated with the simultaneous emergence of lower crime and neighborhood gentrification. First, population decline and composition change could contribute to the decline in crime. However, we do not interpret this causally, nor do we attempt to pin down the direction of timing between demographic change and crime reduction. It is equally possible that changes in population composition contributed to lower crime, or that declining crime made treated areas more attractive to higher-status residents. At the same time, the combination of population decline and an upward shift in resident socioeconomic composition aligns closely with the housing market patterns documented above: a smaller but more affluent resident base is consistent with modest price appreciation alongside lower transaction volume.

5.3 Monitor and Police

A third possible channel operates through enhanced monitoring and police enforcement. The cleanup campaign may have been accompanied by intensified public security efforts, either through increased installation of surveillance equipment or through greater allocation of police resources. Strengthened monitoring and enforcement could raise the perceived probability of crime detection and punishment, thereby deterring criminal activity and facilitating neighborhood upgrading.

To examine this channel, we use two measures of enforcement intensity. First, we col-

lect data on the number of newly installed surveillance monitors at the subdistrict level from the Beijing Traffic Municipal Bureau. [1] Second, we use information on annual police expenditures at the district level, obtained from the Annual Expenditure Settlement Announcements of the Public Security Sub-Bureaus.[2] These publicly released fiscal documents provide detailed records of realized spending, serving as a proxy for district-level policing intensity.

We estimate specifications similar to our baseline framework, replacing the outcome variables with measures of newly installed surveillance monitors at the subdistrict level and annual police expenditures at the district level. The results are reported in Table 8. In Columns (1) and (3), we use the levels of monitors and expenditures, respectively, while Columns (2) and (4) use their logarithmic transformations. The results show that treated areas experienced a statistically significant increase in the number of newly installed surveillance monitors at the subdistrict level following policy implementation. In contrast, district-level police expenditures exhibit a moderate but statistically insignificant increase. Overall, the evidence suggests that the cleanup campaign was associated with expanded monitoring infrastructure, while changes in broader fiscal police resources were less pronounced.

6 Possible Spillover Effects

A broader public concern following the large-scale cleanup of nonlocal residents is whether crime may have been displaced beyond Beijing. Our earlier analysis shows no significant within-city spillover across untreated streets. This absence of intra-city displacement further

[1] For surveillance intensity, we collect data on monitoring devices from periodic public announcements released by the Beijing Traffic Management Bureau, which report the locations of newly installed surveillance cameras. We geocode these reported locations into latitude-longitude coordinates and spatially match each camera to the nearest road segment. We then retain only those monitors located on backstreets. Based on this matching, we construct a monthly measure of newly installed monitors for each backstreet by counting the number of newly added surveillance devices within that backstreet in each month. Regressions include year–month and subdistrict fixed effects.

[2] For police resources, we use district-level data on annual public security expenditures published by local Public Security Bureaus. These expenditure data capture realized spending on public safety at the district level. Because expenditure information is unavailable for Shunyi and Miyun districts, we exclude these two districts from the analysis.

strengthens the credibility of our identification strategy. However, it remains possible that potential offenders may have relocated outside Beijing.

Conceptually, cross-city spillovers could operate in two directions. First, crime may shift to geographically proximate cities, as displaced individuals seek nearby locations with similar economic opportunities and living environments. Second, displaced nonlocal residents may return to their hometowns, potentially affecting crime patterns in their origin cities. However, the latter channel is not guaranteed. Many migrants move to Beijing precisely because of limited economic opportunities in their hometowns. Following displacement, they may be reluctant to return to their places of origin and instead choose to relocate to nearby cities with comparable labor markets and urban conditions.

We therefore empirically examine whether cities that are either geographically close to Beijing or historically more connected through migration flows exhibit differential changes in crime following the cleanup campaign. To conduct this analysis, we construct a monthly panel of crime rates for all prefecture-level cities nationwide. Crime rates are measured at the city-month level to allow for high-frequency variation around the policy period. In addition, we use data from the China Migrants Dynamic Survey (CMDS) 2017 to measure pre-existing migration linkages between each city and Beijing. The CMDS is a nationally representative survey of the migrating population, administered by the National Health Commission, and is specifically designed to capture migration histories, origin-destination patterns, and mobility intentions of migrant residents. Using CMDS, we compute migration rates from each city to Beijing prior to the cleanup campaign, which serve as a measure of migration exposure. This allows us to identify cities that historically sent more migrants to Beijing and are therefore more likely to experience potential return flows.

The estimation results are shown in Table 9. In general, the findings provide suggestive evidence of cross-city spillovers that are geographically concentrated in proximate areas. In particular, cities located in provinces neighboring Beijing experienced statistically significant increases in both crime levels and crime rates during the post period (Columns (1)–(2)). Here, $Post_b$ is defined as an indicator equal to one beginning in the year when the cleanup

policy was first implemented in Beijing, and $Nearby$ is a dummy variable identifying cities located in provinces adjacent to Beijing. The magnitude of the coefficients suggests that crime increased in neighboring provinces relative to more distant areas following the policy onset.

In contrast, the interaction between $Post_b$ and the migration exposure measure ($HighRatio$) yields weaker and less consistent patterns. When the dependent variable is crime number, the coefficient on $Post_b \times HighRatio$ is negative and statistically significant. When crime rate is used as the outcome, however, the coefficient becomes statistically insignificant. One possible interpretation, consistent with the discussion above, is that displaced individuals may be less inclined to return to their hometowns despite historical migration linkages, instead relocating to nearby cities with more comparable economic conditions.

Overall, the results point to the presence of geographically concentrated spillovers. Crime appears to have shifted toward cities in neighboring provinces following the cleanup campaign. These findings suggest that large-scale urban enforcement policies may generate displacement effects beyond city boundaries.

7 Conclusion

This paper studies the socioeconomic effects of a backstreet cleanup campaign in Beijing between 2017 and 2019. The policy targeted informal commercial activity and was rolled out gradually across subdistricts. Using this variation, we document several main findings. First, the policy led to large and persistent reductions in crime, particularly property crime, with little evidence of within-city displacement. Second, we find signs of neighborhood upgrading. Housing prices increase modestly, transaction volumes fall, and household consumption rises after the policy. Then, we document some changes in population composition. While the overall population declined, the remaining residents became more affluent.

Our study relates to several strands of the literature. We provide one of the first causal evidence on the effect of vendor eviction policies on crime in a developing country setting,

where informal activity is widespread but empirical evidence is still limited. We also contribute to the literature on gentrification and urban change. The results show that "public order" policies can affect both housing markets and household outcomes. More broadly, the findings suggest that policies aimed at improving order and safety can also change local economic activity and the composition of neighborhoods, which in turn serve as key channels for urban crime reduction.

This study sheds lights on several policy implications. First, it reveals a trade-off in urban policy between safety and local economic vitality: the campaign improved public order, but it did so alongside a contraction in informal economic activity that previously sustained dense, low-cost urban services. Another implication is that part of the observed crime reduction reflects compositional change rather than purely behavioral response. This implies that crime enforcement policies can operate not only through deterrence or opportunity reduction, but also through selection—altering who lives and works in a place. Finally, the results highlight the need to account for spatial spillovers when designing local policies. Although we find little evidence of within-city displacement, there is suggestive evidence that some effects extend beyond city boundaries. This implies that narrowly targeted interventions may shift, rather than fully eliminate, certain activities. A more coordinated approach across jurisdictions may therefore be important in addressing the underlying drivers of crime and informal activity.

Future work could extend our analysis by quantifying these trade-offs, incorporating both the benefits from improved safety and the costs borne by displaced individuals and businesses. Understanding how these effects evolve over the longer run, and how they differ across institutional settings, remains an important direction for research.

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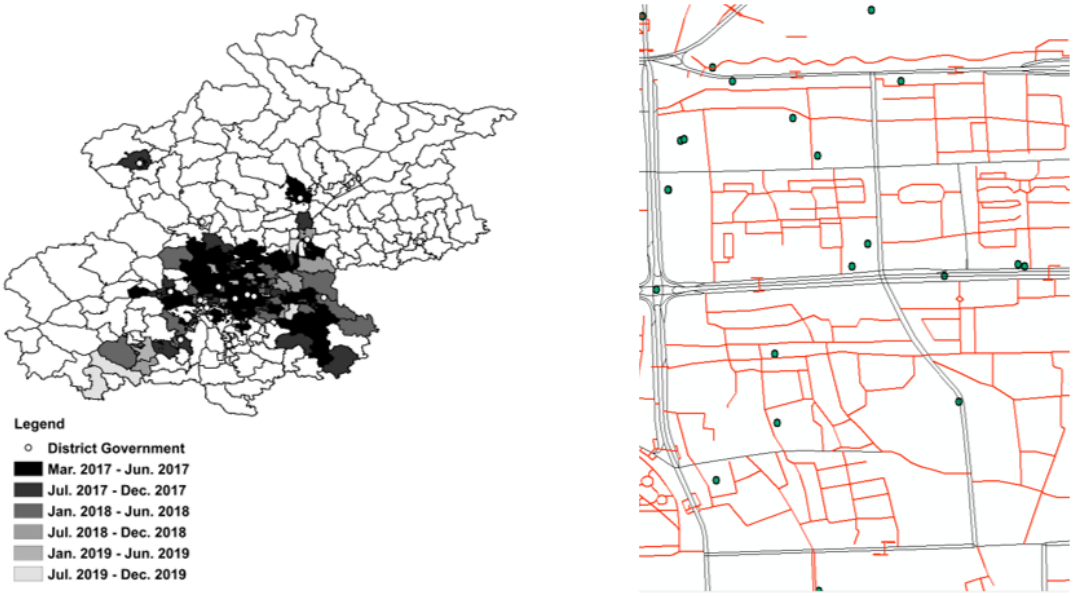
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(a) Subdistrict rollout

(a) Identified backstreets and crime

Figure 1: Subdistrict-level Rollout, Backstreets, and Crime

Notes: Panel (a) displays the spatial distributions of district governments, subdistricts, and the implementation dates of the campaign. Panel (b) shows the identified backstreets (red lines), other streets (black lines), and geo-coded crime cases (green dots). Due to space limitations, only several neighborhoods in a district are shown.

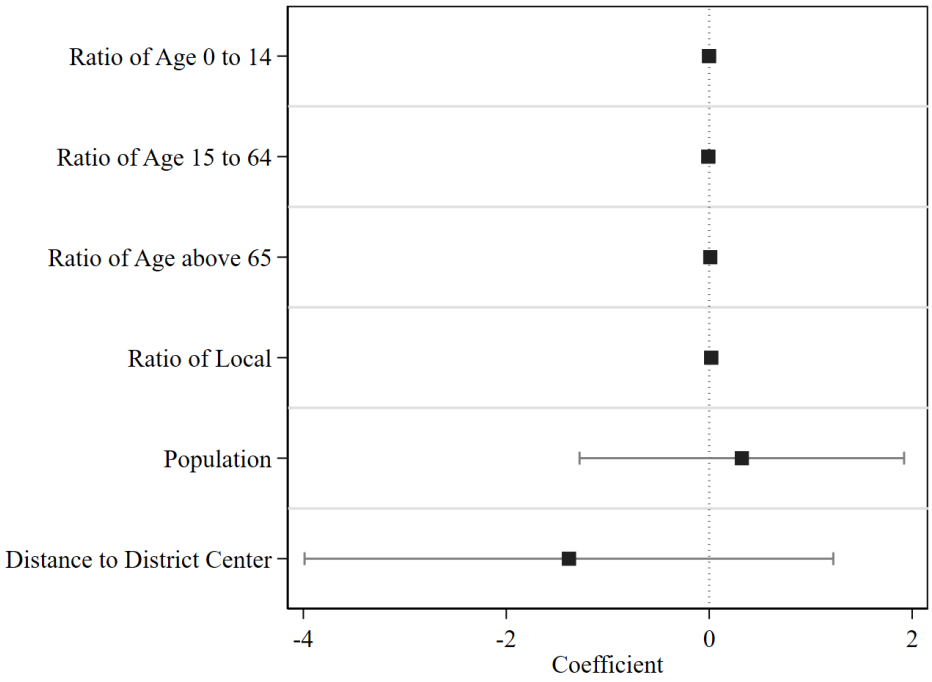
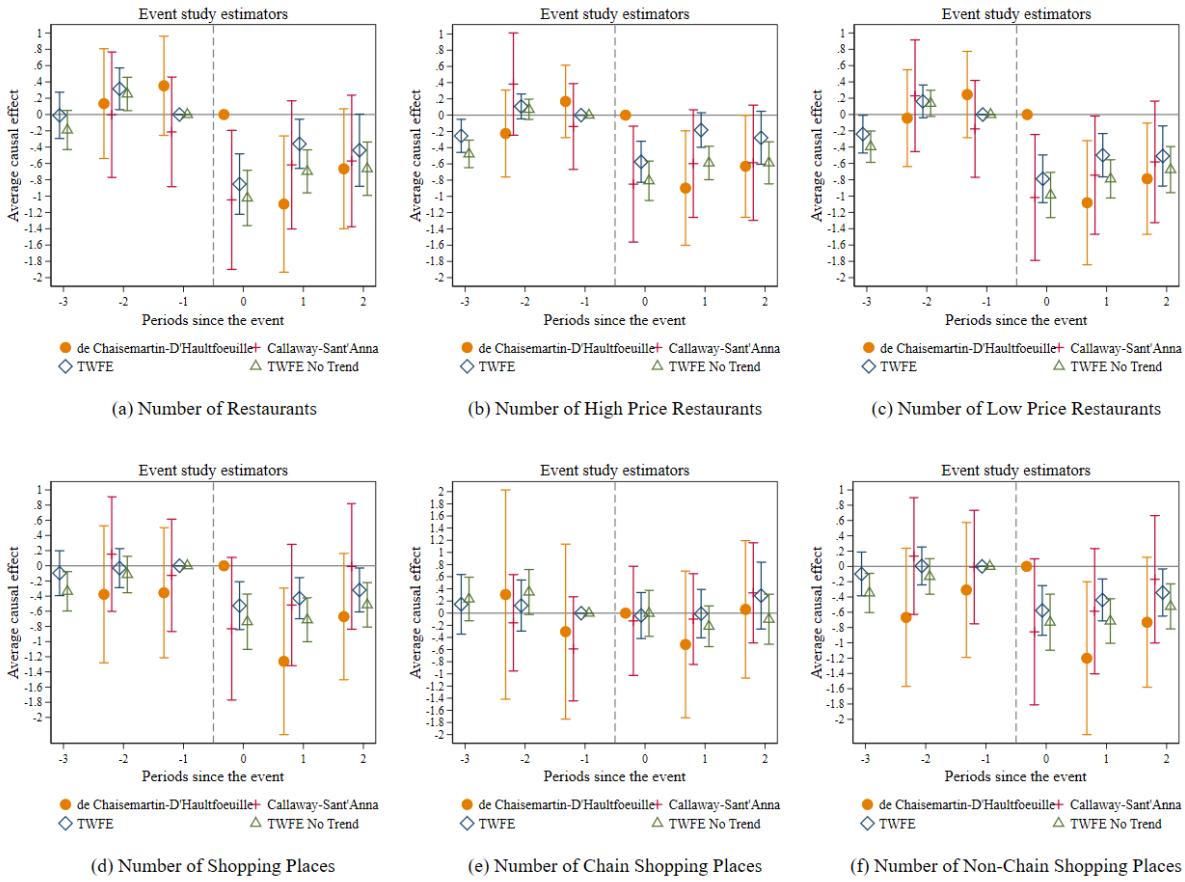


Figure 2: Balance Test

Notes: This figure displays the estimates and their 95% confidence intervals of pre-determined characteristics on treated status of a subdistrict. The dependent variables are six pre-determined characteristics in 2010. The first four rows show the share of the total subdistrict population for these groups: children (0-14), working-age adults (15-64), seniors (65+), and local residents. The fifth row provides the total population in units of 10,000 people. The final row indicates the distance in kilometers from the subdistrict's center to the district's center. We split the treated subdistricts into early- and late-treated groups based on the average treated year-month. The independent variable is a dummy taking a value of 1 if the treated subdistricts is in early-treated groups and 0 for late-treated groups. Samples are restricted to treated subdistricts. The regression includes only district fixed effects, and we adopt robust standard errors.

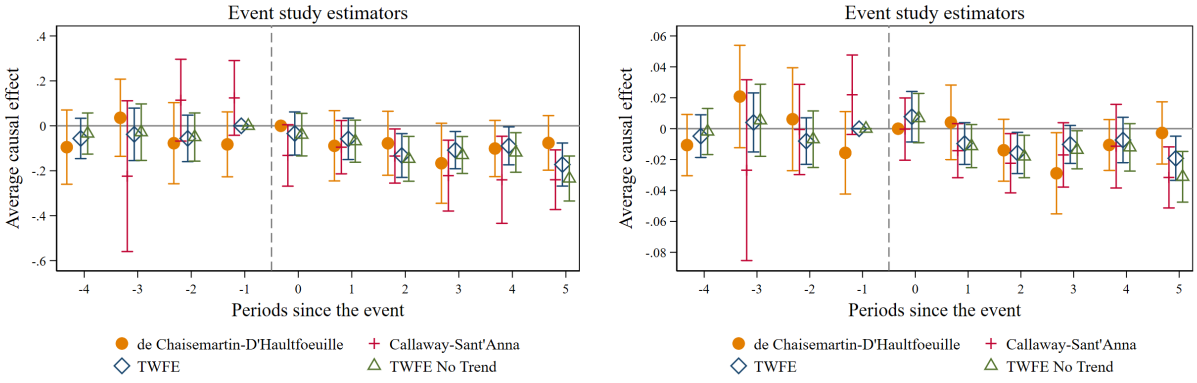


(a) Number of Restaurants (b) Number of High Price Restaurants (c) Number of Low Price Restaurants

(d) Number of Shopping Places (e) Number of Chain Shopping Places (f) Number of Non-Chain Shopping Places

Figure 3: Dynamic estimates for business establishments

Notes: The figures show point estimates and 95% confidence intervals of the dynamic effect of the cleanup campaign. The dependent variable of panels (a)-(c) is the number of restaurants (total, high-end, and low-end), and the dependent variable of panels (d)-(f) is the number of shopping places (total, chain, and non-chain). In each plot, we utilize several estimators: the standard TWFE estimator, TWFE estimator without linear trends, the DIDm estimator from De Chaisemartin and d'Haultfoeuille (2020), and the csdid estimator from Callaway and Sant'Anna (2021). Economic time trends include *ln(GDP per capita)*, *Middle Age percent*, *lnHospital*, *Worker percent*, and *lnPM2.5*. Standard errors are clustered at subdistrict level. Each period on the x-axis represents the number of year to the year of reform.



(a) Crime Number

(b) Crime Ratio

Figure 4: Dynamic estimates for Crime

Notes: The figures show point estimates and 95% confidence intervals of the dynamic effect of the cleanup campaign. The dependent variable of panel (a) is the number of crimes, and the dependent variable of panels (b) is the ratio of crimes. In each plot, we utilize several estimators: the standard TWFE estimator, TWFE estimator without linear trends, the DIDm estimator from De Chaisemartin and d'Haultfoeuille (2020), and the csdid estimator from Callaway and Sant'Anna (2021). Economic time trends include *ln(GDP per capita)*, *Middle Age percent*, *lnHospital*, *Worker percent*, and *lnPM2.5*. Standard errors are clustered at subdistrict level. Each period on the x-axis represents the number of quarter to the quarter of reform.

Table 1: Summary Statistics

	Control Group (Never Treated Subdistrict)			Treatment Group (Ever Treated Subdistrict)		
	Mean	Std. Dev	N	Mean	Std. Dev	N
Post	0.000	0.000	7,488	0.560	0.496	8,784
Crime Number	0.055	0.375	7,488	0.259	0.753	8,784
Robbery	0.039	0.280	7,488	0.205	0.650	8,784
Traffic Offense	0.000	0.012	7,488	0.000	0.015	8,784
Burglary	0.001	0.038	7,488	0.011	0.118	8,784
Drug	0.001	0.053	7,488	0.005	0.077	8,784
Fraud	0.003	0.134	7,488	0.004	0.071	8,784
Murder	0.000	0.000	7,488	0.001	0.024	8,784
Crime Ratio	0.010	0.075	7,008	0.040	0.144	8,208
Number of Restaurants	12.536	37.989	1,092	95.991	143.758	1,281
High Price	1.516	5.254	1,092	15.252	26.654	1,281
Low Price	4.060	13.614	1,092	30.248	44.782	1,281
Number of Stores	2.633	9.052	1,092	14.209	25.487	1,281
Chain	0.264	1.109	1,092	1.848	5.230	1,281
Non-Chain	2.369	8.221	1,092	12.361	21.436	1,281
Price per sqm ($yuan$)	41609.013	11537.740	11,821	69280.891	26400.923	107,346
Transaction Volume ($unit$)	1.579	5.694	7,488	12.221	18.376	8,784
Rent per sqm ($yuan$)	80.450	58.750	524	166.771	89.922	5,889
Night Light ($nanoWatts/sr/cm^2$)	29.935	21.224	608	58.731	9.192	712
Population ($person$)	12624.392	19635.935	577	40223.961	30067.346	687
GDP per capita (10,000 $yuan$)	5.421	3.090	7,488	11.234	6.397	8,784
% Male	0.512	0.006	7,488	0.510	0.011	8,784
% Middle Age	0.784	0.018	7,488	0.793	0.026	8,784
Number of Hospital	26.038	21.054	7,488	76.279	52.531	8,784
% Worker	2588.368	904.880	7,488	3937.014	1891.022	8,784
PM2.5 ($\mu g/m^3$)	79.631	12.082	7,488	83.211	6.506	8,784

Notes: This table summarizes variables that we use in regression analyses. The control group includes untreated subdistricts, while the treatment group includes treated subdistricts. Most variables are measured annually; however, GDP per capita, % Male, % Middle Age, Number of Hospitals, % Worker, and PM2.5 are based on 2015 County Statistical Yearbook data only.

Table 2: Stylized Facts

	Number of Restaurant			Number of Shopping Places		
	(1)	(2)	(3)	(4)	(5)	(6)
	lnTotal	lnHighPrice	lnLowPrice	lnTotal	lnChain	lnNon-Chain
Post	-0.693***	-0.326***	-0.593***	-0.627***	-0.329***	-0.582***
	(0.138)	(0.088)	(0.110)	(0.106)	(0.058)	(0.102)
R^2	0.867	0.853	0.860	0.764	0.588	0.759
Observations	2373	2373	2373	2373	2373	2373
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
ln(GDP per capita) × Year trend	Yes	Yes	Yes	Yes	Yes	Yes
Middle Age Percent × Year trend	Yes	Yes	Yes	Yes	Yes	Yes
ln(Hospital)× Year trend	Yes	Yes	Yes	Yes	Yes	Yes
Worker Percent × Year trend	Yes	Yes	Yes	Yes	Yes	Yes
lnPM2.5 × Year trend	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents the estimation results on the relation between the staggered rollout of cleanup campaign across Beijing and the POI counts. The sample includes all subdistricts in Beijing. All observations are aggregated to the subdistrict level. The dependent variable is the logarithm of the number of backstreet restaurants in columns (1) to (3) and the logarithm of the number of backstreet shopping places in columns (4) to (6). Standard errors, presented in parentheses, are clustered at the subdistrict level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 3: Crime and Crime Ratio

	(1)	(2)	(3)	(4)	(5)	(6)
	Crime Number			Crime Ratio		
Post	-0.108***	-0.062***	-0.070***	-0.018**	-0.011**	-0.011**
	(0.030)	(0.021)	(0.022)	(0.007)	(0.004)	(0.005)
R^2	0.272	0.277	0.282	0.244	0.250	0.255
Observations	16272	16272	16272	15216	15216	15216
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	No
Year-Month FE	No	No	Yes	No	No	Yes
ln(GDP per capita) × Year trend	No	Yes	Yes	No	Yes	Yes
Male Percent × Year trend	No	Yes	Yes	No	Yes	Yes
Middle Age Percent × Year trend	No	Yes	Yes	No	Yes	Yes
lnHospital × Year trend	No	Yes	Yes	No	Yes	Yes
Worker Percent × Year trend	No	Yes	Yes	No	Yes	Yes
lnPM2.5 × Year trend	No	Yes	Yes	No	Yes	Yes
Outcome mean	0.165	0.165	0.165	0.026	0.026	0.026

Notes: This table presents the estimation results on the relation between the staggered rollout of cleanup campaign across Beijing and crime. The sample includes all subdistricts in Beijing. All observations are aggregated at the subdistrict level. The dependent variable is number of crime take place in backstreets in columns (1) to (3) and the corresponding ratio in columns (4) to (6). The crime ratio is computed by crime count dividing the subdistrict's 2010 population. Columns (1) through (3) and (4) through (6) present results progressively incorporate additional economic time-trend controls. Our preferred specifications, which include the full set of controls, are shown in columns (3) and (6). The outcome mean is the average crime number and ratio in the pre-treatment year 2016. Standard errors, presented in parentheses, are clustered at the subdistrict level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 4: Number and Ratio of Different Types of Crime

Panel A

	(1) Burglary	(2) Robbery	(3) Traffic Offense	(4) Drug	(5) Fraud	(6) Murder
Post	-0.048**	-0.006*	-0.000	0.000	-0.003	-0.001
	(0.020)	(0.003)	(0.000)	(0.003)	(0.002)	(0.001)
R^2	0.243	0.056	0.025	0.038	0.042	0.024
Observations	16272	16272	16272	16272	16272	16272
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Outcome mean	0.128	0.006	0.000	0.003	0.003	0.000

Panel B

	(1) Burglary	(2) Robbery	(3) Traffic Offense	(4) Drug	(5) Fraud	(6) Murder
Post	-0.007*	-0.001*	-0.000	-0.000	-0.000	-0.000
	(0.004)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R^2	0.218	0.048	0.025	0.043	0.043	0.024
Observations	15216	15216	15216	15216	15216	15216
Subdistrict FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes	Yes	Yes
Outcome mean	0.020	0.001	0.000	0.000	0.000	0.000

Notes: This table presents the estimation results on the relation between the staggered rollout of cleanup campaign across Beijing and different types of crime. The dependent variable in Panel (A) is the number of crimes in backstreets for six crime types, while dependent variable in Panel (B) is the ratio. Crime ratio is computed from dividing the number of district population in 2010. Outcome mean is pre-treatment average crime number and crime ratio. Full economic time trends includes *ln(GDP per capita)*, *Middle Age percent*, *lnHospital*, and *Worker percent* as well as fixed effects. Standard errors are clustered at the subdistrict level. ***, **, and * indicate significance at the 1%, 5%, and 10% level, respectively.

Table 5: Gentrification

Panel A

	(1) Price	(2) lnPrice	(3) Transaction Volume	(4) Rents	(5) lnRents
Post	646.604*	0.005	-1.896**	6.867	0.024
	(359.663)	(0.007)	(0.749)	(6.020)	(0.033)
R^2	0.745	0.749	0.797	0.662	0.724
Observations	118784	118784	16272	6408	6408
Subdistrict FE	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes
Control	Yes	Yes	No	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes	Yes

Panel B

	(1) Per Capita Consumption	(2) ln(Per Capita Consumption)
$Post_d$	6890.432***	0.155***
	(26.650)	(0.001)
R^2	0.508	0.723
Observations	1323	1323
Subdistrict FE	Yes	Yes
Household FE	Yes	Yes
Year-Month FE	Yes	Yes
Control	Yes	Yes
Economic Time Trends	Yes	Yes

Notes: This table reports the estimated effects of cleanup campaign on several outcome variables regarding gentrification. Panel (A) analyzes housing market outcomes, including transaction prices in columns (1) and (2), transaction volume in column (3), and rental prices per square meter in columns (4) and (5). We control whether it has elevator, whether it has refined decoration, age of room, number of bedrooms, and floor of room in columns (1) and (2), and number of bedrooms, orientation of room, whether it is a shared room, and floor of room in columns (4) and (5). Panel (B) uses household per capita consumption from CHFS (2015, 2017, 2019) with subdistrict, household, and year-month fixed effects. $Post_d$ denotes the first date of the treatment date of a district. Base year of economic time trend is 2014. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6: Population and Nightlight

	(1) lnPopulation	(2) lnNightlight
Post	-0.279***	-0.090***
	(0.063)	(0.014)
R^2	0.952	0.980
Observations	1264	1320
Subdistrict FE	Yes	Yes
Year-Month FE	Yes	Yes
Economic Time Trends	Yes	Yes

Notes: This table reports the estimated effects of cleanup campaign on population and nighttime light. The outcome variables are the logarithm of backstreet population and nighttime light intensity at the subdistrict level. We calculated the total population in backstreet for each subdistrict by aggregating all grid cells that fall within a 50-meter buffer of the subdistrict. Subdistrict nighttime light is calculated using average nighttime light brightness within the backstreet buffer area. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Demographics

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
			Age			Income			Education	
	Local	Young	Middle Aged	Elder	Low	Middle	High	Low	Middle	High
$Post_d$	0.037***	0.025***	-0.007***	-0.017***	-0.110***	0.029***	0.001***	-0.016***	-0.064***	0.046***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
R^2	0.080	0.006	0.024	0.037	0.137	0.015	0.100	0.043	0.010	0.053
Observations	9997	9997	9997	9997	9997	9997	9997	9997	9997	9997
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B

	(1)	(2)	(3)	(4)
	Local Ratio	Young Ratio	Middle Aged Ratio	Elder Ratio
Treat × $Post_{2020}$	0.037**	0.017***	-0.002	-0.015***
	(0.014)	(0.002)	(0.005)	(0.005)
R^2	0.957	0.829	0.941	0.940
Observations	636	636	636	636
Subdistrict FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

Notes: This table reports the estimated effects of cleanup campaign on demographic changes in Beijing. In Panel (A), the dependent variable is a dummy variable taking 1 if the individual belongs to the populaion group and 0 otherwise with year and district fixed effects. Controls include age and age squared. Economic trends use 2014 as base year. Standard errors are clustered at the district level. In Panel (B), the dependent variables are ratios of each population group calculated using census from 6th (2010) and 7th (2020) National Population Census' ratio outcomes. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8: Number of Monitor and Police Expense

	(1) Number	(2) lnNumber	(3) Expense	(4) lnExpense
Post	0.131***	0.015**		
	(0.045)	(0.006)		
$Post_d$			34792.57	0.605
			(62931.42)	(0.736)
R^2	0.087	0.089	0.978	0.964
Observations	16272	15216	52	52
District FE	No	No	Yes	Yes
Subdistrict FE	Yes	Yes	No	No
Year FE	No	No	Yes	Yes
Year-Month FE	Yes	Yes	No	No
Economic Time Trends	Yes	Yes	Yes	Yes
Outcome mean	0.105	0.017	121648.3	11.515

Notes: This table reports the estimated effects of cleanup campaign on monitors and police expense. The dependent variables in columns (1) and (2) are the number of newly applied monitors and the logarithm value of it in backstreets. In columns (3) and (4), we use police expenses (in ten thousand yuan) and their logarithmic values. Shunyi and Miyun Districts are excluded due to missing data. Standard errors are clustered at the subdistrict level in columns 1 to 2 and at the district level in columns (3) to (4). ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 9: Spillover to Other Cities

	(1) Crime Number	(2) Crime Ratio	(3) Crime Number	(4) Crime Ratio
$Post_b \times$ Nearby	13.089***	0.015**		
	(3.442)	(0.007)		
$Post_b \times$ High Rate			-17.245***	-0.000
			(3.732)	(0.007)
R^2	0.798	0.741	0.807	0.773
Observations	15936	15936	13296	13296
City FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

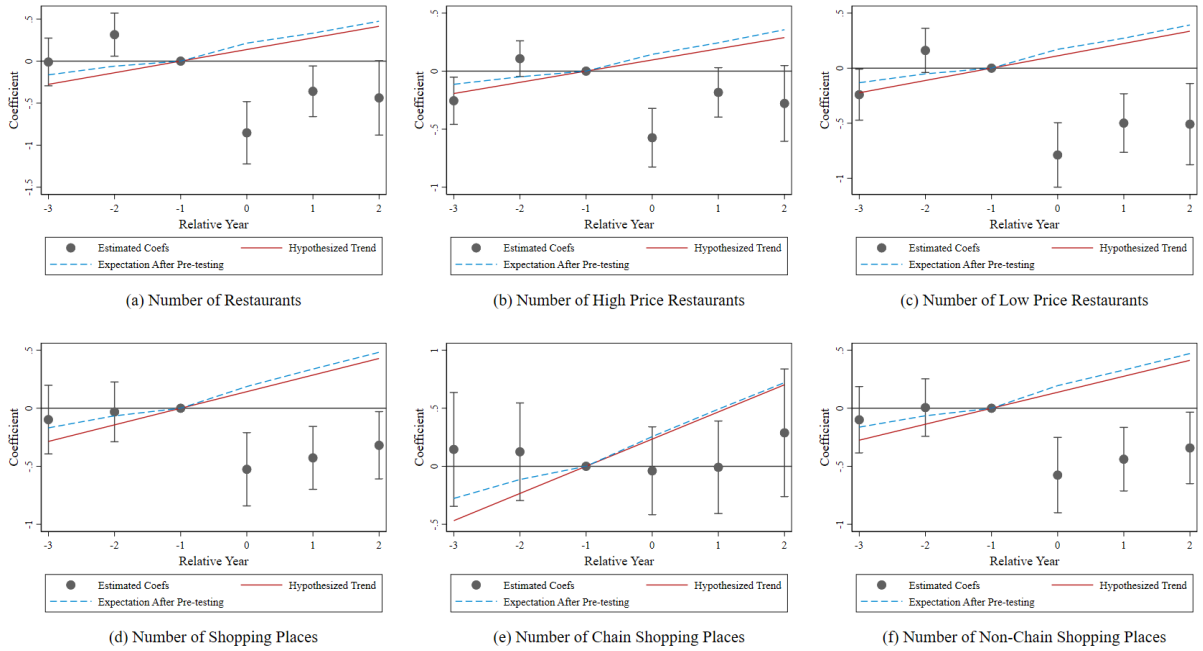
Notes: This table reports the estimated effects of cleanup campaign on spillover effects from Beijing to adjacent provinces or provinces with migrants to Beijing. The dependent variable is monthly crime number and crime ratio in cities other than Beijing. The independent variable *Nearby* is a dummy indicating the four adjacent provinces (Tianjin, Hebei, Inner Mongolia Autonomous Region, Shanxi). $Post_b$ indicates the initial policy implementation date in Beijing, which is April 2017. This table uses CMDS (2017) data to calculate migration rates from each city to Beijing, then divides cities into high and low migration rate groups. Samples include all cities with migrants to Beijing in China. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

A Appendix Tables and Figures



Figure A1: Subdistrict Example

Notes: The figures show the detailed contents of subdistrict government Official Accounts examples. Starting date, belonged district, and campaign name are included in the articles.

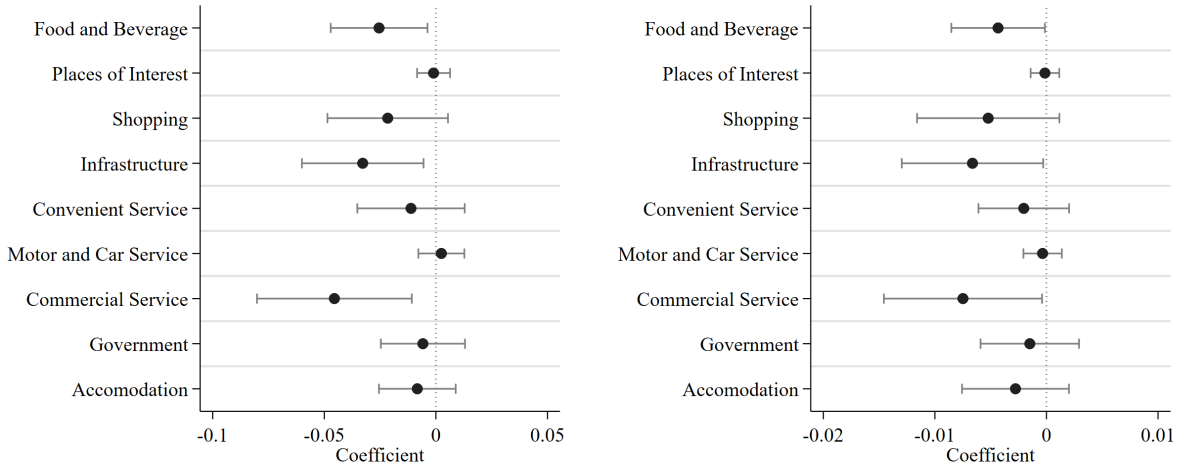


(a) Number of Restaurants (b) Number of High Price Restaurants (c) Number of Low Price Restaurants

(d) Number of Shopping Places (e) Number of Chain Shopping Places (f) Number of Non-Chain Shopping Places

Figure A2: Hypothesized Linear Trend of the Event-study for Business Establishments

Notes: The figures show the hypothesized linear trend of the TWFE event-study results with full controls in Figure 3 using Roth (2022) pretrends. The solid red line is the hypothesized linear trend with 50% power. The regression standard errors are clustered at the subdistrict level.

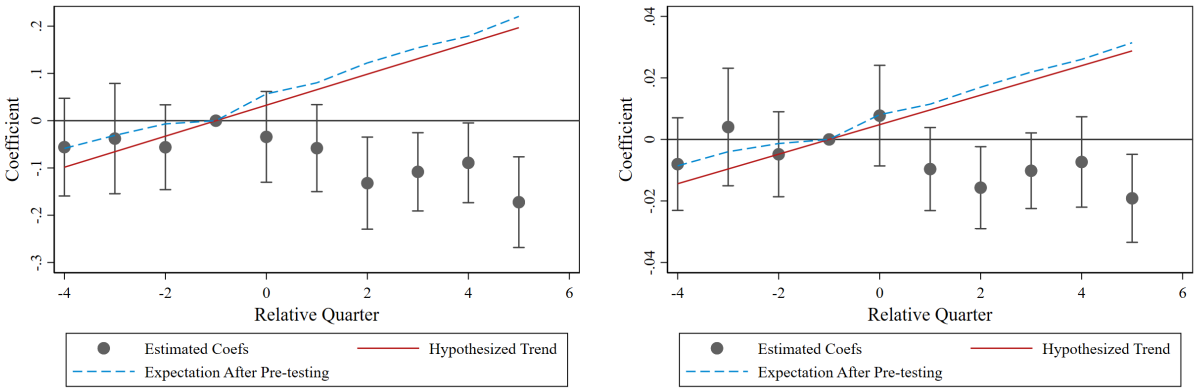


(a) Crime Number

(b) Crime Ratio

Figure A3: Crimes Happen Near POI

Notes: This figure plots the heterogeneous effects of cleanup campaign on number or ratio of crime take place around other types of POI. Crime near POI is number of crime happen near POI in backstreet of each subdistrict in each month. The estimates are produced using subdistrict fixed effects, year-month fixed effects and district-specific economic time trends. Standard errors are clustered at the subdistrict level.

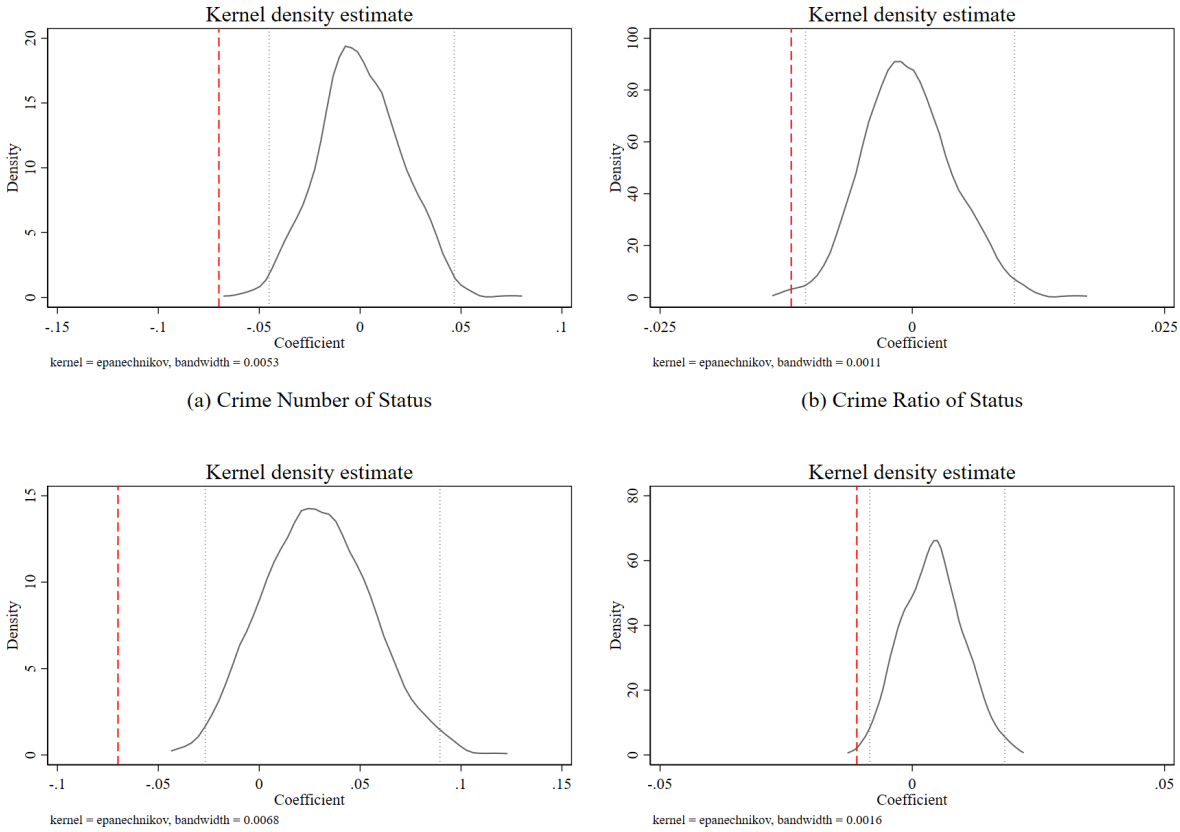


(a) Crime Number

(b) Crime Ratio

Figure A4: Diagnostic Parallel Trend for Crime

Notes: The figures show the hypothesized linear trend of the TWFE event-study results with full controls in Figure 4 using Roth (2022) pretrends. The solid red line is the hypothesized linear trend with 50% power. The regression standard errors are clustered at the subdistrict level.



(a) Crime Number of Status

(b) Crime Ratio of Status

(c) Crime Number of Timing

(d) Crime Ratio of Timing

Figure A5: Placebo Test

Notes: Figures A and B present estimates from a placebo test in which the treatment group is randomly assigned, while Figures C and D report results from a second type of placebo test that randomly assigns the timing of the treatment. In each figure, the leftmost red line indicates the point estimate of the actual policy effect on our primary crime number or crime ratio outcome, as reported in Table 3. The gray line delineates the 1% to 99% range of the distribution generated by placebo estimates. All placebo simulations are based on 1,000 replications and follow the full set of controls, time trends, and fixed effects as applied in Table 3.

Table A1: Backstreets Cleanup Campaign Policy Document

Ten prohibitions and one initiative	Specific Content
Ten Prohibitions	**1. No Illegal Construction:** Demolish unauthorized buildings such as sheds, walls, and fences **2. No Wall-breaking Vendors:** Seal "wall-breaking openings" to restore street appearance; Stop any business activities run from residential buildings that face the street; Use green fences or railings to separate properties from the street where needed **3. Facade Renovation:** Repair damaged walls, Paint building exteriors to ensure clean walls with consistent colors; Decorate or conceal outdoor air condition units, electrical meter boxes, junction boxes; Add visible house numbers where possible **4. Signage Standardization:** Remove illegally installed advertisements and signs; Ensure standardized design, precise implementation, and landscape aesthetics for approved installations **5. Road Surface Repair:** Repair damaged road surfaces to ensure they are flat and free of potholes; Check that all manhole covers and drainage grates are undamaged; Ensure rainwater and sewage pipes are smooth-running and clean **6. Facility Maintenance:** Standardize the installation of all public service facilities; Ensure facilities are intact, free of damage and rust, and kept clean and tidy **7. Greening Achievements:** Maximize green space in public areas and replant any missing vegetation; Use a variety of planting methods to create a good balance of flowers and trees; Protect all ancient trees in the area **8. Overhead Line Regulation:** Bury overhead lines underground wherever conditions permit; Organize and manage remaining lines neatly in areas where burial is not possible **9. Cleanliness and Hygiene:** Keep all ground surfaces clean and tidy; Remove any graffiti or illegal paper advertisements from walls; Maintain public toilets so they are clean, odor-free, and free of flies **10. Civilized Order:** Do not park vehicles in a disorderly manner; Do not install private ground locks or use stone piers to occupy parking spaces; Do not pile up materials on roadsides; Do not engage in roadside vending
One Creation	Carry out civilized street creation activities focusing on the following five aspects, creating a group of model civilized streets, including good public environment, good social order, good moral standards, good co-creation and co-building, and good promotional atmosphere.

Table A2: Variable Definition

Variable	Definition	Source
Post	Treatment variable of urban upgrading campaign regarding subdistricts in Beijing	Government documents and WeChat official accounts
Crime Number	Number of crime happens in backstreet	China Judgments Online
Crime Ratio	Ratio of crime happens in backstreet to subdistrict populuation in 2010	China Judgments Online and Census
Number of Restaurants	Number of restaurants in backstreet	Gaode Map and Dazhongdianping
Number of Stores	Number of stores in backstreet	Gaode Map and Dazhongdianping
Price per sqm	Selling prices of a house per sqm	Lianjia
Transaction Volume	Transaction volume of houses in backstreet	Lianjia
Rent per sqm	Rents of a house per sqm	Lianjia
Night Light	Night light density in backstreet	Li et al. (2020)
Population	Population in backstreet	LandScan Global Population Database

Table A3: Criminals to Untreated Subdistricts and Crimes on Avenue

	Untreated Subdistricts		Avenue	
	(1)	(2)	(3)	(4)
	Crime Number	Crime Ratio	Crime Number	Crime Ratio
$Post_u$	0.046	0.004		
	(0.031)	(0.006)		
Post			0.009	0.002
			(0.031)	(0.004)
R^2	0.138	0.069	0.305	0.150
Observations	3504	3264	16272	15216
Subdistrict FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

Notes: This table presents the results of two robustness tests. The first assigns an artificial treatment to never-treated subdistricts, while the second conducts a spatial test at the avenue level. We first divide untreated subdistricts into high and low pre-treatment crime ratio groups. An artificial treatment date is then assigned to each untreated subdistrict, based on the treatment date of the nearest, later-treated subdistrict with a high pre-treatment crime ratio. The treatment variable is denoted as $Post_u$. Columns (1) and (2) include the full sample of never-treated subdistricts. Columns (3) and (4) include all Beijing subdistricts but compute crime counts and ratios at the avenue level. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A4: Consider the Distance to District Government

	(1) Crime Number	(2) Crime Ratio	(3) Crime Number	(4) Crime Ratio
Post	-0.057**	-0.010**	-0.056**	-0.010**
	(0.023)	(0.004)	(0.023)	(0.004)
R^2	0.282	0.256	0.282	0.256
Observations	16224	15216	16224	15216
Subdistrict FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes
Distance × Year	Yes	Yes	No	No
Distance × Year-Month	No	No	Yes	Yes

Notes: This table presents the estimates verifying whether controlling for distance can lead to better balance between the early-treated group and late-treated group. Columns (1) and (3) control for Distance × Year interactions, while columns (2) and (4) control for Distance × Year-Month interactions. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A5: Anticipation from Beijing and District

Panel A

	(1) Crime Number	(2) Crime Ratio	(3) Crime Number	(4) Crime Ratio
$Post_b \times$ Treat	0.026	0.014**		
	(0.034)	(0.006)		
Post			-0.074***	-0.009**
			(0.022)	(0.005)
R^2	0.350	0.279	0.281	0.261
Observations	10450	9755	15454	14437
Subdistrict FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

Panel B

	(1) Crime Number	(2) Crime Ratio	(3) Crime Number	(4) Crime Ratio
$Post_d$	-0.006	0.002		
	(0.039)	(0.007)		
Post			-0.070***	-0.009*
			(0.022)	(0.005)
R^2	0.358	0.280	0.281	0.259
Observations	8238	7940	15439	14409
Subdistrict FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

Notes: This table reports the estimated effects of cleaning up backstreet policy on crime. In Panel (A), we consider whether the subdistrict is more or less attractive to offenders by affecting their risk assessment from first-treated subdistricts in Beijing, and in Panel (B), we consider from earliest-treated subdistricts in each district. $Post_b$ indicates the initial policy implementation date in Beijing, which is April 2017, while $Post_d$ indicates the earliest treatment date of each district. Columns (1) and (2) keep the period before each subdistrict's actual renovation date, while columns (3) and (4) exclude the period between the district's first renovation and subdistrict-level implementation. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A6: Change the Definition of Backstreet

	(1) Crime Number	(2) Crime Ratio	(3) Crime Number	(4) Crime Ratio
Post	-0.050***	-0.046***	-0.008**	-0.007**
	(0.017)	(0.004)	(0.015)	(0.003)
R^2	0.224	0.146	0.231	0.159
Observations	16272	15216	16272	15216
Subdistrict FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

Notes: This table reports the estimated effects of cleanup campaign on crime after changing the definition of backstreet. In columns (1) and (2), backstreet that are open to traffic are deleted. In columns (3) and (4), roads that are unclassified or unknown and open to traffic are deleted. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A7: Other Robustness Checks

	(1) Standardized Crime Number	(2) Crime Dummy	(3) Arcsin(Crime Number)	(4) ln(Crime+1)
Post	-0.073*	-0.036***	-0.044***	-0.034***
	(0.041)	(0.012)	(0.014)	(0.011)
R^2	0.159	0.238	0.279	0.279
Observations	16272	16272	16272	16272
Subdistrict FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

Notes: This table reports the estimated effects of the cleanup campaign on crime outcomes using alternative dependent variables. The dependent variable in column (1) is standardized crime number, dummy of crime taking a value of 1 if there happens a crime in this month and 0 otherwise in column (2), arcsin value of crime number in column (3), and logarithm value of crime number plus one in column (4). Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A8: Sensitivity

	(1) Crime Number	(2) Crime Ratio	(3) Crime Number	(4) Crime Ratio
Post	-0.076***	-0.010**	-0.078***	-0.010**
	(0.025)	(0.004)	(0.028)	(0.005)
R^2	0.420	0.410	0.453	0.413
Observations	16272	15216	16272	15216
Subdistrict FE	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes
Economic Time Trends	Yes	Yes	Yes	Yes

Notes: This table reports the estimated effects of cleaning up backstreet policy on crime. We look into different boundary levels of a backstreet. Column (1) and (2) are results from crime occurred within 70 meters from a backstreet. Column (3) and (4) are results from crime occurred within 100 meters from a backstreet. Standard errors are clustered at the subdistrict level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

B Additional Robustness Checks on Crime Outcomes

This appendix presents a series of additional robustness checks to assess the validity of our identification strategy and the sensitivity of the main results on crime outcomes. In particular, we address three sets of identification concerns: potential spillover effects that may contaminate the control group, nonrandom rollout of the cleanup campaign, and possible anticipation effects. We then present placebo tests and other robustness checks.

B.1 Identification Concerns

Spillover to Untreated Areas Within the City. A first concern is that the cleanup campaign may have displaced crime into untreated areas within Beijing, which would violate the stable control group assumption and attenuate our estimated treatment effects. To assess this possibility, we explicitly examine whether crime increased in untreated nearby areas.

The results show little evidence of meaningful within-city spillover. As reported in Columns (1) and (2) of Appendix Table A3, untreated backstreets exhibit only a very small and statistically insignificant increase in crime following nearby cleanup implementation. We further examine whether crime may have shifted from treated backstreets to larger nearby roads, which could plausibly absorb displaced activity. Columns (3) and (4) show no significant increase in crime on major roads either. Overall, these findings suggest that the observed crime reduction is unlikely to be driven by simple relocation to untreated areas within the city, thereby strengthening the credibility of our identification strategy.

Nonrandom Rollout of the Cleanup Campaign. A second concern is that policy rollout may not have been conditionally random across space. In our earlier balance tests (Section 3.1), we examined four pre-treatment covariates, including population, population composition, and distance to district government. Among these variables, backstreets located closer to district government offices appear somewhat more likely to be cleaned up

earlier, although the relationship is not statistically significant. Because the coefficient is non-negligible in magnitude, there remains a concern that distance to district government may proxy for unobserved policy prioritization or differential local governance trends.

To address this issue, we augment our baseline specification by interacting distance to district government with flexible time trends. Specifically, we interact distance to district government with either year trends or year–month trends. As shown in Appendix Table A4, the main treatment effect remains stable across these specifications. These results alleviate the concern that the estimated effects are driven by nonrandom policy rollout correlated with proximity to district administrative centers.

Anticipation Effects. A third concern is that backstreets scheduled for later cleanup may have adjusted in advance once the campaign became publicly visible. This concern is particularly relevant because Beijing's first documented backstreet cleanup took place in April 2017, which may have generated citywide expectations of future enforcement.

To assess anticipation effects, we conduct a test in two steps. In Panel A of Appendix Table A5, we first retain only the period prior to each subdistrict's actual cleanup date and redefine the post indicator as an indicator for the period after April 2017 (Post_b), when the first cleanup occurred in Beijing. If untreated backstreets had begun adjusting in anticipation of future cleanup, we would expect to observe a decline in crime even before their own treatment dates. The estimated coefficients, however, are positive and statistically unstable, providing little evidence of systematic anticipatory behavioral adjustment.

We then implement a stricter design in Columns (3) and (4) by excluding the entire period between Beijing's first cleanup date and each subdistrict's own treatment date. Under this trimmed sample, we effectively compare the post-treatment period to a pre-treatment window that is fully free of any possible anticipation generated by the citywide campaign. The estimated treatment effects remain robust.

We further repeat the same exercise in Panel B, replacing Beijing's first cleanup date with the first cleanup date within each district. This provides a more localized anticipation test

under the idea that expectations may have formed at the district level rather than citywide. The results are again similar, suggesting that anticipation effects are unlikely to explain our baseline findings.

B.2 Other Robustness and Placebo Checks

Placebo Tests. We next conduct placebo tests to examine whether our estimation strategy mechanically generates treatment effects in the absence of true policy variation. Appendix Figure A5, Panels A and B, present estimates from a placebo exercise in which treatment status is randomly assigned across backstreets while holding the original timing structure fixed. Panels C and D further randomize treatment timing. Across these placebo exercises, the estimated coefficients are centered around zero and display no systematic post-treatment decline. These placebo checks provide additional reassurance that the baseline results are not driven by spurious correlations or mechanical features of the empirical design.

Alternative Sample Restrictions. We also assess whether the results are sensitive to alternative sample definitions related to road type and traffic accessibility. One concern is that some backstreets may function more like ordinary traffic roads rather than the narrow, low-order neighborhood spaces targeted by the cleanup campaign.

To address this issue, we progressively restrict the sample. In Columns (1) and (2) of Appendix Table A6, we exclude backstreets that are open to traffic. In Columns (3) and (4), we further exclude roads that are unclassified or of unknown type and open to traffic. Across these alternative samples, the estimated treatment effects remain stable, suggesting that the baseline findings are not driven by ambiguous or non-backstreet road segments.

Alternative Outcome Definitions. We further test the robustness of our findings to alternative definitions and transformations of the crime outcome. Specifically, Appendix Table A7 reports estimates using standardized crime counts, a crime occurrence dummy, the arcsine transformation of crime counts, and $\ln(\text{Crime} + 1)$ as dependent variables. Across

these alternative outcome measures, the main results remain qualitatively unchanged. This suggests that the estimated decline in crime is not driven by a particular functional form or by the presence of zeros and skewness in the crime distribution.

Alternative Spatial Buffers. Finally, we examine whether the results are sensitive to how we define the spatial boundary of a backstreet. In the baseline specification, crime is assigned to a backstreet based on a fixed distance threshold. Appendix Table A8 reports results using alternative spatial buffers. Columns (1) and (2) define crime as incidents occurring within 70 meters of a backstreet, while Columns (3) and (4) use a 100-meter threshold. The estimated treatment effects remain negative and statistically significant across these alternative boundary definitions, suggesting that the main findings are not sensitive to the precise spatial cutoff used to assign crime incidents to backstreets.