Conspicuous Inequality: Speculative Housing Booms and Property Crime in China*

Hong Ma Yu Pan You Shan Mingzhi (Jimmy) Xu

November 18, 2025

Abstract

This paper identifies the causal effect of housing speculation on crime by exploiting spillovers from house purchase restrictions (HPR) in major Chinese cities. These policies redirected speculative demand to nearby unregulated markets, triggering sharp and exogenous housing booms. Using newly constructed measures of criminal activity derived from large-scale text analysis of court judgments, we show that speculative housing booms increase property crime but not violent crime. The rise is concentrated in financially motivated offenses—-larceny, fraud, and high-value robbery-—and is committed primarily by socioeconomically vulnerable individuals and first-time offenders. Mechanism analyses reveal that housing booms widen wealth inequality and make affluence more visible through conspicuous consumption, raising both the expected returns to crime and the ease of targeting. Alternative explanations such as deprivation, social strain, migration, or weakened deterrence find no support. The results highlight a new social cost of housing speculation: by amplifying visible inequality, it erodes cohesion and fuels crime.

Keywords: Crime; house purchase restriction policy; inequality; conspicuous consumption
JEL Code: G18, K42, R21

*Corresponding author: Hong Ma: Tsinghua University, email: mahong@sem.tsinghua.edu.cn, tel: (+86) 10 62794388; Yu Pan: Xiamen University, e-mail: panyu@xmu.edu.cn; You Shan: University of Science and Technology of China, e-mail: shanyou@ustc.edu.cn; Mingzhi Xu: Peking University, email: mingzhixu@nsd.pku.edu.cn. All authors contributed equally and are listed in alphabetical order. We are grateful for the financial support of the National Natural Science Foundation of China (Grant No. 72322007, 72425004 and 72503230), and the Social Science Foundation of China (Grant No.23&ZD046). All authors contribute equally and all errors are our own.

1 Introduction

"It is not scarcity but inequality that breeds discontent." This enduring intuition, first articulated by Confucius more than two millennia ago, resonates with the social repercussions of rising inequality in the modern world. Economists have repeatedly warned about the socioeconomic consequences of inequality, which—at its most extreme—can manifest as crime or broader social unrest (Stiglitz, 2012; Piketty, 2014; Case and Deaton, 2021). In particular, economic theories of crime, beginning with the seminal work of Becker (1968), posit that widening inequality raises the expected returns to criminal activity relative to lawful work, thereby increasing incentives to offend.

This paper provides causal evidence that widening inequality and its heightened visibility can increase criminal activity. Empirically establishing this causal link has proven difficult. Inequality and crime are jointly determined, and both respond to unobserved local conditions such as enforcement intensity, migration, and social norms.[1] Moreover, conventional measures of inequality evolve slowly and often fail to capture how disparities become visible or salient within communities.

Speculative housing booms make inequality both salient and measurable, providing an ideal setting to address these identification challenges. Unlike most sources of income or wealth disparity that accumulate gradually, surges in housing prices generate windfall gains for multi-property owners while leaving renters and first-time buyers behind, creating rapid and highly visible changes in the local distribution of wealth. These booms also reshape neighborhoods and patterns of conspicuous consumption, transforming private asset appreciation into visible signals of social status. At the same time, because speculative capital often moves for reasons exogenous to local labor markets or fundamentals, housing booms offer a quasi-experimental environment for identifying the broader social consequences of inequality.[2]

We leverage a recent quasi-natural experiment in China that generated precisely such speculative dynamics. Between 2016 and 2017, under clear central directives,[3] major cities implemented stringent Housing Purchase Restrictions (HPR) to curb speculation and cool overheated housing markets. While the policy succeeded in restraining local price growth, it also redirected speculative capital into nearby unregulated cities, triggering sharp housing booms largely disconnected from local economic fundamentals (Deng et al., 2022; Liu and Zhang, 2024). These policy-driven

[1] Most studies rely on panel correlations with fixed effects or dynamic panels with instruments for inequality, raising concerns about reverse causality, measurement, and cross-place comparability.

[2] A large literature highlights the economic costs of speculation: it distorts investment decisions (Bayer et al., 2021; Campello et al., 2022), misallocates capital (Gorback and Keys, 2020; Basco et al., 2025; Bednarek et al., 2021), and generates large efficiency losses (Gao et al., 2020; Favilukis and Van Nieuwerburgh, 2021). These dynamics undermine affordability and weaken financial stability (Glaeser, 2013; Martín et al., 2021; DeFusco et al., 2022; Mian and Sufi, 2022). This literature demonstrates how speculation distorts markets and redistributes wealth, but leaves open the question of its broader social consequences.

[3] President Xi Jinping first articulated the principle that *"housing is for living, not for speculation"* (*fang zhu bu chao*) at the Central Economic Work Conference in December 2016, later reaffirming it at the 19th Party Congress in 2017.

spillovers created large, sudden, and geographically concentrated increases in visible wealth, providing a rare opportunity to identify how rapid and conspicuous inequality shapes social outcomes such as crime.

Our empirical strategy exploits geographic spillovers induced by the HPR policy in a difference-in-differences framework. We define unregulated cities located within 250 kilometers of a regulated market as treated, while those farther away serve as controls. The intuition is straightforward: speculative capital does not travel far, so proximity to restricted markets predicts exposure to redirected investment. This spatial contrast generates exogenous variation in local housing booms and, consequently, in visible inequality. To measure crime, we follow Ma et al. (2025) and apply large-scale textual analysis to millions of judgment documents from all levels of Chinese courts. Since 2013, China's Supreme People's Court (SPC) has mandated that all judicial verdicts be made publicly available, enabling precise tracking of criminal cases by type across cities and over time.

We obtain robust evidence that speculative housing booms lead to higher crime. Following the HPR shock, treated cities experienced statistically and economically significant increases in crime—about 5.2% more cases and 6.5% more offenders per thousand residents than comparable control cities, equivalent to approximately nine additional cases and fifteen additional offenders per million. In monetary terms, we estimate direct property losses of approximately 8.9 billion RMB (1.4 billion USD) over 2014–2019, or about 0.014 percent of GDP in 2014, excluding indirect costs related to law enforcement and victimization.

The rise in crime is concentrated in offenses with clear pecuniary motives. Property crimes, such as larceny, fraud, and high-value robbery, increased significantly in treated cities, while violent and other non-pecuniary offenses remained unchanged.[4] This pattern underscores the economic nature of the response. The effects are strongest among socioeconomically vulnerable groups: younger cohorts, women, and the less educated, as well as individuals with weaker local networks. These groups benefited least from the housing boom and faced the greatest relative decline in opportunity, consistent with an inequality-driven mechanism. Notably, the policy shock also attracted first-time offenders, suggesting that speculation-induced inequality expanded criminal participation beyond traditional margins. At the same time, the increase in property crime was muted in cities with stronger cultural institutions and kinship networks, highlighting the moderating role of social norms.

We identify housing–driven inequality and its visibility as the principal mechanisms linking the HPR shock to rising crime. First, consistent with findings in Piketty and Zucman (2014) and Ioannides and Ngai (2025), the HPR shock widened local wealth disparities by increasing the value of housing assets held by multi-property owners while leaving renters and first-time buyers

[4]Under the Criminal Law of the People's Republic of China, larceny is defined as the unlawful taking of another's property with the intent to permanently possess it, including acts such as burglary, pickpocketing, and theft. In contrast, violent crimes, such as homicide, assault, and rape, involve the use of force intended to harm a victim's life or physical integrity.

behind.[5] Within Becker (1968)'s framework, this widening gap raises the expected returns to crime: potential targets become wealthier while legitimate earnings opportunities remained largely unchanged. Second, the boom made wealth more conspicuous. Housing gains translated into higher spending on visible consumption, such as cars, luxury goods, and status-oriented gifts, that lowered the cost of identifying valuable targets.[6] In line with Mejía and Restrepo (2016) and Hicks and Hicks (2014), the criminogenic effects of the housing boom are strongest in high-value incidents and in cities exhibiting more conspicuous consumption, directly supporting this mechanism.

We examine several alternative explanations and find them unlikely to be the main drivers of our results. First, economic deprivation arising from deteriorating labor market conditions can fuel crime (Dix-Carneiro et al., 2018; Britto et al., 2022). However, wages, employment, and firm activity in treated cities remained stable after the shock, indicating that the overall opportunity cost of crime did not decline. Second, sociological theories, beginning with Merton (1938)'s "strain theory" and extended by Kelly (2000), posit that inequality breeds frustration and norm breakdown, leading individuals to seek illegitimate means of success. Our findings suggest that economic incentives rather than psychological strain are the dominant channel linking inequality to crime. Consistent with this view, the HPR shock did not heighten such sentiments or increase violent crime. Third, compositional changes through migration (Bianchi et al., 2012; Bell et al., 2013; Street, 2025) can also be ruled out, since the HPR policy did not induce movement of low-income or crime-prone populations from restricted to unrestricted cities. Finally, deterrence mechanisms are unlikely to be responsible: expenditures on policing and justice did not fall (Levitt, 1997; Di Tella and Schargrodsky, 2004; Mello, 2019). Weaker safety nets, as emphasized by Fishback et al. (2010) and Melander and Miotto (2023), cannot explain the findings, as social transfers and the provision of public goods were unchanged. Taken together, these tests isolate inequality—and its visibility—as the primary causal mechanism linking speculative housing booms to crime.

To formalize this mechanism of conspicuous inequality, we draw on the model of Chiu and Madden (1998) by introducing heterogeneous wealth distributions and asymmetric information. In our framework, individual wealth is privately observed, but consumption choices are publicly visible. Individuals simultaneously decide on consumption and potential criminal behavior. The model yields a unique separating equilibrium in which high-wealth individuals engage in conspicuous consumption to signal status, while low-wealth individuals target these visible consumers, as conspicuous goods reveal high-value opportunities. We then analyze how exogenous increases in housing asset returns affect this equilibrium. Such shocks disproportionately enrich property owners, widening the wealth distribution and amplifying visibility through conspicuous spending.

[5] This divergence appears not only in objective measures such as the Gini coefficient, but also in perceptions of inequality and social discontent.

[6] As shown in Section 4, Chinese customs data reveal a surge in imports of luxury goods such as designer accessories and premium automobiles.

The combination of greater inequality and salient wealth signals strengthens the incentive for lower-wealth individuals to commit property crimes. These theoretical predictions align closely with our empirical evidence on the "criminal demography" of housing booms, showing that disadvantaged populations disproportionately drive the observed rise in property offenses.

Related Literature. This paper makes several contributions to the literature. First, we contribute to the long-standing literature on inequality and crime. Building on the seminal framework of Becker (1968), prior studies have shown that inequality can raise the expected returns to criminal activity, either by increasing the wealth of potential targets or by depressing the opportunity cost of offending for the poor (e.g., Ehrlich, 1973; Bourguignon, 2000; İmrohoroğlu et al., 2000; Demombynes and Özler, 2005; Enamorado et al., 2016).[7] A complementary strand emphasizes behavioral channels such as frustration or perceived injustice, echoing Merton (1938)'s "strain theory" and supported by empirical findings in both sociology and criminology (e.g, Stack, 1984; Fajnzylber et al., 1998, 2002*a*,*b*). Our paper contributes new causal evidence from China on how inequality, particularly when made salient by asset booms, affects crime. We distinguish between economic and psychological mechanisms, finding support for the former: speculative gains raised the returns to property crime without triggering broader violence or social unrest.

Second, we advance the literature on the broader social consequences of housing markets and speculation. Prior work links housing booms to affordability pressures, capital misallocation, and macroeconomic volatility (e.g., Gao et al., 2020; Favilukis and Van Nieuwerburgh, 2021; Bayer et al., 2021; DeFusco et al., 2022), as well as to educational attainment, fertility decisions, and health (e.g., Lovenheim, 2011; Dettling and Kearney, 2014; Daysal et al., 2021; Hu et al., 2024). We document a new externality: crime. Speculative booms amplified local inequality and spurred property offenses, particularly among disadvantaged populations. These findings identify an overlooked social cost of speculative housing dynamics and highlight how asset markets can reshape the local social contract.

Third, we extend emerging evidence on the role of visibility in the link between inequality and crime. Recent studies suggest that conspicuous consumption and visible affluence are key determinants of criminal targeting (Hicks and Hicks, 2014; Mejía and Restrepo, 2016). Building on the "broken windows" theory, a strand of research shows that poor housing conditions encourage criminal activity (Wilson and Kelling, 1982; Ellen et al., 2013; Cui and Walsh, 2015; South et al., 2021; Khalil and Sanfelice, 2025; Disney et al., 2023) while improving neighborhood environment

[7] Crime reflects a tradeoff between expected returns and costs (Raphael and Winter-Ebmer, 2001; Gould et al., 2002; Di Tella and Schargrodsky, 2004; Fougère et al., 2009; Draca et al., 2011; Dix-Carneiro et al., 2018; McMillen et al., 2019; Britto et al., 2022). The costs of crime are shaped by labor market opportunities and the effectiveness of law enforcement, while the returns depend on illicit earnings and the ease of identifying lucrative targets (Kirchmaier et al., 2020; Axbard et al., 2021; Braakmann et al., 2024). Additional research shows that migration and stress (Bianchi et al., 2012; Bell et al., 2013; Draca et al., 2019), as well as access to credit, transfers, and housing (Fishback et al., 2010; Che et al., 2018; Khanna et al., 2021; Khalil and Sanfelice, 2025), also influence the benefits from criminal activity.

reduces it (Aliprantis and Hartley, 2015; Sandler, 2017; Chyn, 2018; Autor et al., 2019; Diamond and McQuade, 2019; Khalil and Sanfelice, 2025). Our contribution is to provide causal evidence that housing booms make inequality both sharper and more visible by fueling luxury consumption, status signaling, and neighborhood change, thereby heightening the salience of economic disparities. This visibility lowers the cost of identifying lucrative targets and strengthens incentives in economic models of crime with incomplete information (Chiu and Madden, 1998).

Fourth, our paper contributes to a growing literature on the unintended consequences of China's Housing Purchase Restrictions (HPR). Existing research has documented how HPR reshaped local markets, by cooling prices in restricted cities (Du and Zhang, 2015), reducing local revenues (Zhao and Zhang, 2022), and influencing household behavior (Chen et al., 2024*a*; Tan et al., 2022; Rogoff and Yang, 2024). A separate body of work has traced its spillovers, showing that redirected speculative demand distorted markets in nearby unregulated cities (Deng et al., 2022; Liu and Zhang, 2024). We extend this literature by demonstrating that these spillovers also generated social costs, increasing crime through the channel of visible, speculation-driven inequality.

Finally, we contribute to the nascent literature on crime in China. Prior work has linked crime to demographic and economic shifts, including sex ratios, automation, and export slowdown (Edlund et al., 2013; Cameron et al., 2019; Zhang and Zhang, 2023; Fang and Miao, 2025; Ma et al., 2025). Furthermore, Li et al. (2019) and Dong et al. (2020) attribute crime to income polarization or poverty, respectively. Some research has grappled with measurement challenges due to underreporting in official statistics (He and Marshall, 1997; Yu and Zhang, 1999). We construct city-level crime indicators to overcome this limitation and leverage quasi-experimental variation to establish causality. Our findings offer new insight into how positive wealth shocks, when unevenly distributed, can destabilize communities and draw in first-time offenders.

The remainder of the paper is organized as follows. Section 2 provides an institutional background, describes the policy variation, and outlines the empirical strategy, data sources, and variable construction. Section 3 presents the main results on the effect of housing booms on property crime. Section 4 investigates the underlying mechanisms, focusing on inequality and its visibility. Section 5 introduces a theoretical framework that formalizes the incentives linking conspicuous wealth to crime. Section 6 concludes.

2 Policy Background, Data, and Empirical Strategy

This section introduces China's Housing Purchase Restriction (HPR) policies and explains how they create a quasi-natural experiment for identifying the causal effects of housing price appreciation on local crime. We begin by describing the institutional background and policy design, and then outline our difference-in-differences framework and the construction of key variables.

2.1 Housing Purchase Restriction Policy

Since the early 2000s, China's housing markets have seen sustained and rapid price growth (Fang et al., 2016; Liu and Xiong, 2020). This prolonged appreciation unfolded in a policy environment where real estate served as a key driver of economic expansion and a major source of local fiscal revenue through land sales. These institutional incentives encouraged tolerance of rising prices and, more importantly, shaped expectations of continued appreciation. As prices increased, the belief that they would keep rising strengthened, which stimulated speculative purchases, lengthened investment horizons, and expanded arbitrage activity. The result was a self-reinforcing expectations cycle that amplified demand, deepened market distortions, and raised risks to both economic and social stability.

Price growth was highly uneven across regions, with the run-up concentrated in major metropolitan areas while many smaller cities saw little movement. From 2012 to September 2016, tier-1 cities recorded average annual house price growth of 14.9 percent, compared with less than 3 percent in tier-3 cities (Deng et al., 2022).[8] Reflecting this divergence, all tier-1 and many tier-2 cities (hereafter, regulated cities) introduced HPR in September 2016 to cool overheated markets and restrain speculative demand. These measures were reinforced, and in some cases tightened, in March 2017. The policy package combined several tools: higher down-payment requirements, limits on purchases by multi-property households, higher mortgage rates, tighter credit supply, and price caps.[9] These interventions proved effective in restraining price growth. According to Deng et al. (2022), the average monthly growth rate of house prices in regulated cities declined from 4 percent in September 2016 to 1.8 percent in subsequent months, remaining below 1 percent thereafter. Following the second round of restrictions in March 2017, price growth dropped further from 0.7 percent to roughly 0.1 percent and stabilized at that subdued level.

Despite the HPR rollout, investors in regulated cities could still buy homes in nearby unregulated areas. With lighter oversight, these adjacent markets became natural outlets for displaced speculative capital. The resulting inflows of out-of-town buyers raised housing demand and pushed up local prices for reasons unrelated to local production, income, or employment. Lending patterns reinforced this channel: commercial banks, constrained in regulated markets, reallocated credit toward unregulated cities, which further amplified speculative activity. Consistent with this interpretation, Deng et al. (2022) also show that the HPR shock leaves GDP, GDP growth, real-estate investment, industrial output, employment, and population essentially unchanged, and on this basis treat the

[8] Chinese cities are commonly classified into several tiers by economic scale and population size. Tier-1 cities include Beijing, Shanghai, Guangzhou, and Shenzhen, which are the most populous and economically dominant metropolitan areas. Tier-2 cities are largely provincial capitals and major industrial or commercial centers, while tier-3 cities correspond to important regional hubs.

[9] Appendix Tables A.1 and A.2 summarize the first- and second-round restrictions across the 22 regulated cities, with effective dates, following Deng et al. (2022) and Liu and Zhang (2024).

induced shifts in nearby unregulated markets as exogenous to local fundamentals.

2.2 Data

Crime Rates. Our primary measure of crime is constructed from the China Judgments Online (CJO) database, the official judicial disclosure platform operated by the Supreme People's Court (SPC). As part of nationwide judicial transparency reforms initiated in late 2013, the SPC mandated that courts at all levels publish their rulings on the CJO website.[10] The database contains five main categories of judicial documents—criminal, civil, administrative, enforcement, and other rulings (e.g., payment orders and state compensation decisions)—each providing detailed case-level information, including titles, parties, facts, legal reasoning, and final judgments.

Our analysis draws on all first-instance criminal cases adjudicated by courts within each city.[11] We identify crime types through systematic textual analysis of each judgment's title and case description, using keyword searches for relevant offenses. For instance, cases containing the term "诈骗" ("fraud") are classified as fraud. If a case involves multiple offenses, such as both larceny and homicide, it is assigned to all corresponding categories. Appendix Table A.3 details the keyword mappings used in the classification.[12]

We focus on offenses that directly infringe upon the legally protected interests of individual victims, dividing them into two broad categories. The first comprises *property crimes*, defined as the unlawful appropriation or destruction of property, such as larceny, fraud, and robbery.[13] The second category covers *violent crimes*, which entail physical harm or threats to life, including homicide, assault, rape, abduction, and kidnapping.[14]

Between 2014 and 2019, we identify 1,449,969 property and violent crime cases, accounting for 39.76% of all criminal cases in our dataset. Property crimes make up 73.48% of this sample, with larceny alone representing 54.24%, while violent crimes account for the remaining 26.52%

[10]China's court system consists of four hierarchical levels: more than 3,000 county-level primary courts, over 400 city-level intermediate courts, 31 province-level high courts, and one Supreme People's Court. Although any level of court may serve as the court of first instance, jurisdiction depends on case type and severity. Higher courts supervise lower courts to ensure uniform application of law. See Appendix C for details. The CJO is available at https://wenshu.court.gov.cn. The mandated disclosure does not include cases involving national security, juvenile defendants, divorce litigation, or mediated settlements.

[11]Roughly 99% of first-instance criminal cases are heard by county-level and city-level courts; only 0.83% (30,152 out of 3,647,146 cases) are tried by provincial high courts or the Supreme People's Court.

[12]A potential concern is that certain offenses, such as illegal excavation ("盗掘") and deforestation ("盗伐"), may differ in motivation from conventional larceny, despite being legally classified as property crimes. To address this, we reclassify such offenses into a separate "other property crimes" category. These reclassifications leave our main estimates virtually unchanged, confirming that our results are not driven by category misclassification.

[13]Although robbery involves the use of violence and some studies classify it as a violent crime, we treat it as a property offense, because its primary intent is to obtain property. In subsequent analyses, we separately examine subcategories including larceny, fraud, and robbery.

[14]This classification follows the established empirical literature linking inequality to crime, which typically focuses on property and violent offense. As a robustness check, we confirm that housing booms do not significantly affect other crime categories (see Section 3.3 and Appendix Table A.10).

(Appendix Table A.4). For each case, we extract the date of offense and aggregate observations to the city–year level by crime type. We also record the number of offenders per incident and compute annual totals by city and category.

Because city size varies substantially, we normalize all counts using 2010 population figures from the 2010 Population Census, producing two primary outcome variables: (i) the number of criminal cases per thousand residents and (ii) the number of offenders per thousand residents. Finally, since the CJO platform was established only in late 2013, earlier cases may not be fully reported. To ensure consistency, we restrict the analysis to 2014–2019, thereby avoiding potential underreporting in early years and contamination from the COVID-19 pandemic beginning in early 2020.

Other City-level Data. Bilateral distances between cities are computed from precise latitude–longitude coordinates obtained via the Gaode Map API, a leading digital mapping platform. Prefecture-level housing prices come from CityRE, a national real estate data provider. CityRE reports a hedonic, constant-quality price index constructed from listings and transaction records collected from national and local brokerages, an approach widely used in recent research (Deng et al., 2022; Liu and Zhang, 2024). Additional city-level, time-varying variables, including GDP per capita, permanent population, fiscal revenue and expenditure, residential housing investment, and college enrollment rates, are drawn from the China City Statistical Yearbooks (various issues, 2014–2019), published by the National Bureau of Statistics of China.[15]

Household-level Data. To investigate the distributional and behavioral mechanisms underlying our results, we use microdata from the China Family Panel Studies (CFPS), a nationally representative longitudinal survey administered by the Institute of Social Science Survey at Peking University. The CFPS employs a stratified multistage sampling design, covering approximately 16,000 households across 25 provinces and municipalities (excluding Xinjiang, Tibet, Qinghai, Inner Mongolia, Ningxia, and Hainan), which together account for 94.2% of mainland China's population. We use data from the 2012–2022 waves to construct household-level measures of income, consumption, asset holdings, and homeownership.

Import Data. To examine the role of conspicuous consumption as a mechanism linking housing booms to crime, we analyze detailed data on luxury goods imports at the city level. These data are drawn from the China Customs Database maintained by the General Administration of Customs, which records the universe of international trade transactions by Chinese firms between 2014 and

[15] Because the China City Statistical Yearbooks do not directly report permanent population, we compute city-level resident population by dividing total GDP by GDP per capita.

2019. The dataset includes information at the eight-digit Harmonized System (HS) level for more than 7,000 product categories, reporting trade values (in U.S. dollars), partner countries, and the location of importing and exporting firms. We aggregate these data to construct city-year measures of luxury imports as proxies for visible wealth and status-oriented consumption.

Appendix Table A.5 reports summary statistics for all key variables. Panel A presents variables used in the baseline analysis (Section 3), while Panel B summarizes those used in the mechanism analysis (Section 4). Detailed variable definitions and construction procedures are provided in Section 4.

2.3 Empirical Strategy

A direct comparison between regulated and unregulated cities would be confounded by selection, as cities subject to HPR tend to be large metropolitan centers, provincial capitals, or major industrial and commercial hubs with stronger economic fundamentals. To address this concern, we exploit the unintended geographic spillovers of the HPR policy as a source of exogenous variation. These spillovers arise because speculative capital displaced from regulated cities flowed disproportionately into nearby unregulated markets, generating sharper housing booms relative to more distant cities. This spatial disparity provides a quasi-experimental setting to identify the causal impact of housing price appreciation on crime. Specifically, we adopt a difference-in-differences strategy and classify unregulated cities into treatment and control groups based on proximity to the nearest regulated city. Cities within 250 kilometers are assigned to the treatment group, while those beyond that threshold serve as controls. The 250-kilometer cutoff reflects practical investment behavior: high-speed rail allows travel between such cities in under two hours, making it feasible for investors to monitor markets and manage properties. This approach yields two roughly equal-sized groups: 154 treated and 160 control cities. Figure 1 illustrates the spatial distribution of regulated, treated, and control cities.

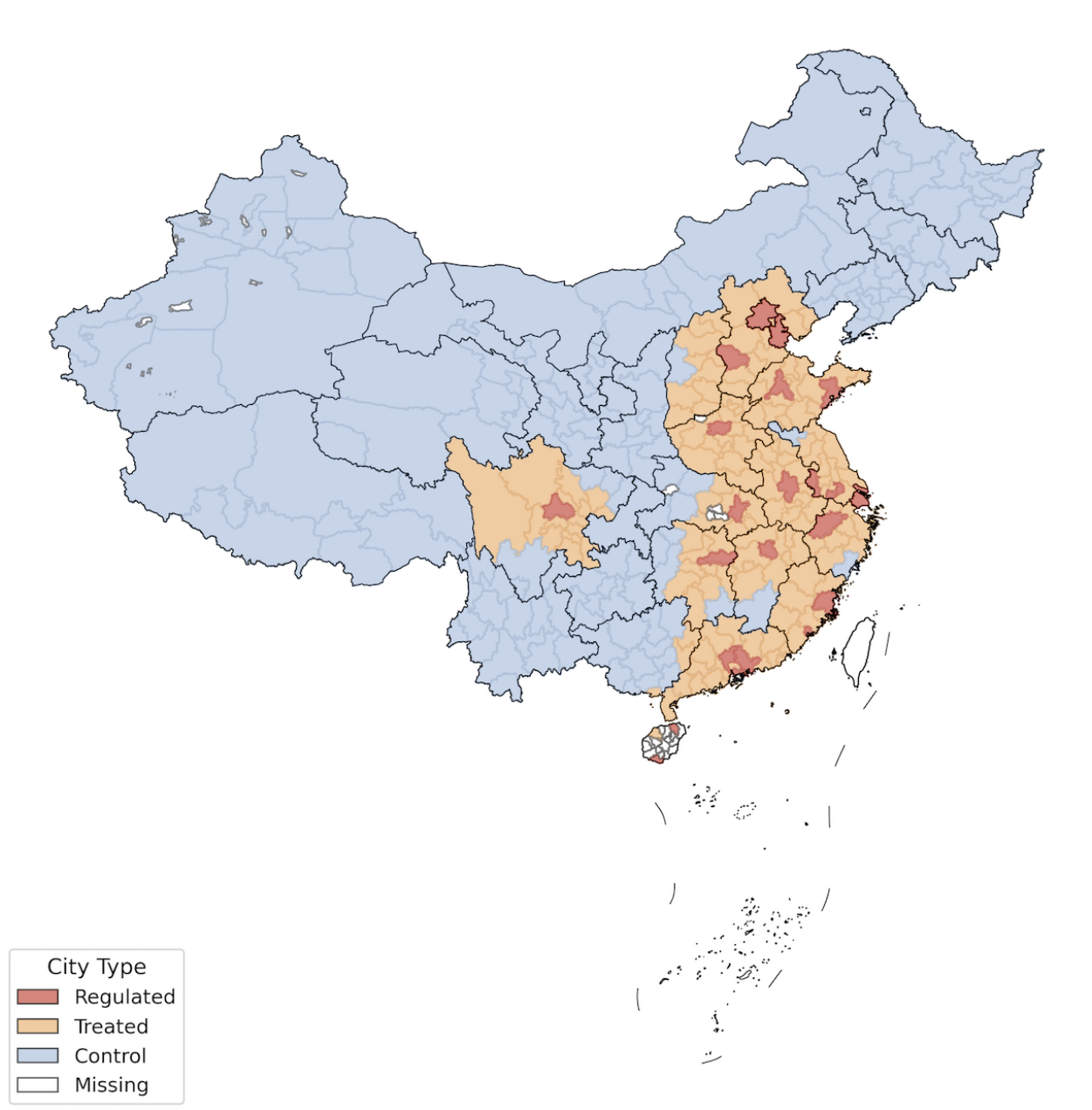
The key identifying assumption is that speculative pressure from investors in regulated cities is orthogonal to local crime-related fundamentals in the affected unregulated areas. Evidence in Deng et al. (2022) supports this assumption, showing that the HPR spillover shock had negligible effects on GDP growth, industrial output, employment, and population in nearby cities. As further emphasized by Tian and Wang (2025), this exogeneity is plausible in China's housing context, where deeply entrenched expectations of sustained price increases—underpinned by perceived policy support—have driven speculative investment largely independent of local economic conditions.

We estimate the following specification:

$$\log(Crime/L)_{ct} = \beta_1 \text{Treat}_c \times \text{Post}_t + \beta_X X_{ct} + \gamma_t + \gamma_c + \epsilon_{ct}. \tag{1}$$

The dependent variable $\log(Crime/L)_{ct}$ denotes the logarithm of the crime rate in city c and year

Figure 1: Geographic Distribution of Regulated, Treated, and Control Cities



Notes: This figure shows the geographic distribution of the cities included in our analysis. The red areas indicate the 22 cities that implemented house purchase restrictions (HPR) in 2016, including Beijing, Changsha, Chengdu, Fuzhou, Guangzhou, Haikou, Hangzhou, Hefei, Huizhou, Jinan, Nanchang, Nanjing, Qingdao, Sanya, Shanghai, Shenzhen, Shijiazhuang, Tianjin, Wuhan, Wuxi, Xiamen, and Zhengzhou. The yellow areas represent 154 cities located within 250 kilometers of the nearest HPR city; these cities are classified as treated. The blue areas represent the remaining 160 cities, which form the control group. Cities with missing data are shown in white.

t, measured either by the number of criminal cases per thousand residents or by the number of convicted offenders per thousand residents. Crime counts are standardized using each city's 2010 permanent resident population, as reported in the 2010 Population Census. The indicator Treat_c equals one for cities within 250 kilometers of the nearest HPR-regulated market, and Post_t equals one for all years from 2016 onward.

The vector X_{ct} controls for time-varying city characteristics that may influence crime, including the logarithm of GDP per capita, population size, fiscal revenue, residential housing investment, and the share of college-educated residents.[16] We include year fixed effects (γ_t) to absorb national shocks and macroeconomic trends, and city fixed effects (γ_c) to capture time-invariant local heterogeneity. Standard errors are clustered at the city level to account for serial correlation within cities over time.

3 Empirical Results

This section presents our main empirical findings on the effects of housing booms, triggered by the spillovers from Housing Purchase Restrictions (HPR), on crime rates across Chinese cities between 2014 and 2019. We begin by estimating the baseline difference-in-differences specification that exploits the quasi-experimental variation induced by these policy spillovers. We then explore heterogeneity in the effects across crime types and population groups, and conclude with a series of robustness checks that verify the validity and stability of our results.

3.1 Baseline Results

Table 1 presents the baseline estimates from equation (1), which capture the effects of HPR-induced housing booms on local crime rates in nearby unregulated cities. Columns (1)–(2) first validate the treatment mechanism: relative to control cities, treated cities experience significant house-price increases of 12.5 percent and 10.1 percent over the four years following the 2016 HPR rollout, with and without city-level controls, respectively. These magnitudes are consistent with prior evidence.[17]

Columns (3)–(6) summarize the effect of HPR-induced spillovers on crime. In the preferred specifications with city controls (cols. 4 and 6), exposure to speculative inflows raises criminal cases by 5.2% and offenders by 6.5% relative to control cities. At sample means, these magnitudes equal about 9 additional cases and 15 offenders per million residents. Thus, cities exposed to spillovers from nearby regulated housing markets exhibit materially higher crime. Columns (7)–(8) further quantify this link by instrumenting log house prices with $\text{Treat}_c \times \text{Post}_t$. The IV estimates indicate

[16] To avoid bias from potentially endogenous controls, we later replace X_{ct} with interactions between pre-treatment city characteristics and Post_t or year dummies, following Che et al. (2022) and Eppinger and Ma (2024). Results are unchanged.

[17] For instance, Deng et al. (2022) document price increases of 2.4 and 6.4 percent following the two policy rounds, while Liu and Zhang (2024) find that HPR policies raise prices in nearby unregulated cities by roughly 12 percent.

Table 1: Baseline Results: Crime Effects of Spillovers from Regulated Housing Markets

	(1) log House price	(2) log House price	(3) log #Case per thousand	(4) log #Case per thousand	(5) log #Criminal per thousand	(6) log #Criminal per thousand	(7) log #Case per thousand	(8) log #Criminal per thousand
$\text{Treat}_c \times \text{Post}_t$	0.125***	0.101***	0.072***	0.052**	0.086***	0.065**		
	(0.014)	(0.014)	(0.024)	(0.025)	(0.025)	(0.027)		
$\log(\text{House price})_{ct}$							0.542**	0.672***
							(0.241)	(0.258)
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
City controls	N	Y	N	Y	N	Y	Y	Y
Observations	1,740	1,588	1,884	1,614	1,884	1,614	1,588	1,588
R-squared	0.951	0.957	0.923	0.907	0.920	0.903	-	-
First-stage F-stat	-	-	-	-	-	-	51.40	51.40

Notes: The unit of observation is the city-year from 2014 to 2019. The dependent variables (all in log) are house price index in columns (1)-(2), the number of criminal cases per thousand population in columns (3), (4) and (7), and the number of criminals per thousand in columns (5), (6) and (8), respectively. Treat_c is an indicator equal to one if the city lies within 250 km of the nearest regulated city; Post_t equals one for years from 2016 onward. Control variables include log GDP per capita, log population, log fiscal revenue, log residential housing investment, and the share of college-educated population. Columns (1), (3), and (5) report estimates without time-varying city-level controls; these controls are included in the remaining columns. All specifications include city and year fixed effects. Robust standard errors, clustered at the city level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

that a 1% exogenous increase in house prices lifts criminal cases by 0.54% and offenders by 0.67%.

Types of Crime. We next examine how the effects of HPR-induced housing shocks differ across offense types, to shed light on the mechanisms underlying the aggregate relationship. Criminal cases are grouped into property crimes and violent crimes. Property crimes are further classified into larceny, fraud, robbery, and other property-related offenses using a keyword-based textual analysis of case titles and descriptions.[18]

Table 2 reports the results by crime category. Panel A presents estimates for the log number of criminal cases per thousand residents, and Panel B for the log number of offenders per thousand. The results show a clear and statistically significant rise in property crimes in treated cities, driven mainly by larceny and fraud.[19] Although the coefficient on robbery is positive but imprecisely estimated, the impact becomes both economically and statistically significant for high-value robberies, defined as cases with claim amounts above the 80th percentile of all property crimes.[20]

In contrast, we find no measurable effect on violent crimes. This asymmetry suggests that the rise in crime is not driven by impulsive or affective aggression, but by calculated, pecuniary motives.

[18]Appendix Table A.3 lists the full set of keywords in Chinese and their English translations. As shown in Appendix Table A.4, larceny accounts for the largest share of cases (54.24%), followed by fraud (11.62%), robbery (4.13%), and other property crimes (4.19%). Violent crimes make up the remaining 26.52%.

[19]To ensure that these effects are not driven by crimes against migrant populations such as out-of-town speculators, we further examine burglary—a subset of larceny typically targeting local residents. The estimates indicate that rising house prices lead to a 7.7 percent (s.e. = 0.034) increase in burglary cases and a 10 percent (s.e. = 0.037) increase in offenders, suggesting that the observed effects are not confined to non-locals.

[20]As shown in Table 9 and discussed in Section 4.2, the number of such cases rises by 20.4 percent (s.e. = 0.097).

Table 2: Heterogeneous Effects of HPR Spillovers by Crime Type

	Property crime					Violent crime	Property Elasticity
	(1) All	(2) Larceny	(3) Defraud	(4) Robbery	(5) Others	(6) All	(7) All
Panel A: Dep. Var.: log #Case per thousand							
$\text{Treat}_c \times \text{Post}_t$	0.090***	0.094***	0.096**	0.024	-0.022	-0.035	
	(0.027)	(0.028)	(0.038)	(0.052)	(0.042)	(0.028)	
$\log(\text{House price})_{ct}$							0.892***
							(0.271)
City FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,579	1,599	1,614	1,588
R-squared	0.904	0.901	0.831	0.820	0.801	0.900	-
First-stage F-stat	-	-	-	-	-	-	51.40
Panel B: Dep. Var.: log #Criminal per thousand							
$\text{Treat}_c \times \text{Post}_t$	0.098***	0.090***	0.139***	0.031	0.030	-0.016	
	(0.029)	(0.029)	(0.046)	(0.061)	(0.051)	(0.030)	
$\log(\text{House price})_{ct}$							0.972***
							(0.287)
City FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,579	1,599	1,614	1,588
R-squared	0.898	0.890	0.806	0.786	0.773	0.893	-
First-stage F-stat	-	-	-	-	-	-	51.40

Notes: The unit of observation is the city-year from 2014 to 2019. The dependent variable in Panel A is the number of criminal cases per thousand population (in log); in Panel B, it is the number of offenders per thousand (in log). Crime classifications are based on keyword matching, as detailed in Appendix Table A.3. Treat_c is an indicator equal to one if a city is located within 250 km of the nearest regulated city. Post_t equals one for years 2016 and onward. All regressions include time-varying city-level controls, city fixed effects, and year fixed effects. Robust standard errors, clustered at the city level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Rapid appreciation in housing prices appears to amplify perceptions of exclusion and economic disparity—particularly among individuals priced out of homeownership—thereby intensifying incentives for financially motivated offenses. Section 4 explores this mechanism in greater detail, linking housing-induced inequality and visible wealth disparities to the surge in property crime.

Social Costs of Crime To assess the broader welfare implications of HPR-induced shocks, we translate the estimated effects on crime into monetary terms, providing a measure of the social costs associated with rising house prices. We first estimate the elasticity of property crime with respect to urban house prices, instrumenting log house prices with $\text{Treat}_c \times \text{Post}_t$. As shown in Column (7) of Table A.3, a 1 percent exogenous increase in house prices raises property crime cases by 0.89 percent and property offenders by 0.97 percent. Using an average monetary loss per property crime of about 103,000 RMB (median 80,000 RMB) and the cumulative 50.3 percent increase in urban house prices between 2014 and 2019, this implies direct property losses of roughly 8.9 billion RMB ($\approx$ 1.4 billion USD at the 2014 exchange rate), equivalent to 0.014 percent of GDP in 2014.[21]

This estimate captures only direct monetary losses and omits broader social costs, including public expenditures on policing, justice, and incarceration, as well as private defensive spending and foregone labor income. It should therefore be viewed as a lower bound on the total cost of crime associated with housing booms. For comparison, Miller et al. (1996) report that in the United States, direct monetary losses account for about 65 percent of total victim costs across property crime types. Similarly, Cohen and Piquero (2009) find that victim losses comprise roughly 43 percent of total costs under a "bottom-up" approach (summing each cost component) and 18 percent under a "top-down" approach based on willingness-to-pay (WTP) estimates. Applying these benchmarks suggests that the total social cost in our setting ranges from 31.8 to 76.1 billion RMB (approximately 5–12 billion USD), or 0.05–0.12 percent of GDP in 2014.

3.2 Heterogeneity Analysis

We next investigating where these effects are most pronounced, across different offender characteristics, and city environments.

Criminal Demography. We begin by exploring how the effects of housing market booms vary across offenders with different socioeconomic and demographic characteristics. Because the increase in crime is concentrated in property-related offenses (as documented in Table 2), we focus on individuals involved in such crimes. [22]

[21]These calculations are obtained by multiplying the estimated elasticity of property crime cases to housing prices (0.892) by the cumulative rise in housing prices between 2014 and 2019 (50.3 percent), the total number of property crime cases nationwide in 2014 (191,944), and the average loss per property crime (103,000 RMB).

[22]Demographic information includes education, age, gender, Hukou registration, and birthplace, which is not available for all defendants. To address this limitation, we impute the number of offenders in each category based on the observed

Table 3: Heterogeneous Effects of HPR Spillovers on Property Crime by Defendant Demographics

Dep. Var:	log #Criminal per thousand						
	(1) Without record	(2) With record	(3) Low-educated	(4) High-educated	(5) Yound	(6) Middle	(7) Old
$\text{Treat}_c \times \text{Post}_t$	0.123***	0.027	0.108***	-0.008	0.175***	0.068**	0.063*
	(0.032)	(0.031)	(0.030)	(0.054)	(0.041)	(0.030)	(0.035)
City FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,594	1,607	1,614	1,611
R-squared	0.877	0.904	0.893	0.776	0.849	0.896	0.817
	(8) Female	(9) Male	(10) Non-top 3 surnames	(11) Top 3 surnames	(12) Migrants	(13) Locals	
$\text{Treat}_c \times \text{Post}_t$	0.237***	0.086***	0.055**	0.038	0.082**	0.104***	
	(0.048)	(0.029)	(0.027)	(0.031)	(0.036)	(0.032)	
City FE	Y	Y	Y	Y	Y	Y	
Year FE	Y	Y	Y	Y	Y	Y	
City controls	Y	Y	Y	Y	Y	Y	
Observations	1,610	1,614	1,542	1,542	1,614	1,613	
R-squared	0.784	0.898	0.911	0.858	0.913	0.874	

Notes: The unit of observation is the city-year from 2014 to 2019. The dependent variable in all columns is the number of individuals committing property crimes per thousand population (in log). Treat_c is an indicator equal to one if a city is located within 250 km of the nearest regulated city; Post_t equals one for years 2016 and onward. We estimate separate regressions for different demographic subgroups: individuals without and with prior criminal records (columns 1–2), with low and high educational attainment (columns 3–4), aged 15-24, 25-44, and 45-64 (columns 5–7), female and male (columns 8–9), with surnames outside or among the top three most common locally (columns 10–11), and migrants and locals (columns 12–13). All regressions include time-varying city-level controls, as well as city and year fixed effects. Robust standard errors, clustered at the city level, are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3 reports the results, where the dependent variable is the number of offenders involved in property crimes per thousand residents (in log). Columns (1) and (2) show that the increase in crime is driven primarily by individuals without prior criminal records, suggesting that housing market shocks induce entry into crime among first-time offenders rather than intensifying activity among habitual or professional criminals.

The remaining columns show that the rise in property crime is disproportionately concentrated among socioeconomically disadvantaged groups. That is, those with fewer housing assets and lower capacity to benefit from the housing boom. Specifically, Columns (3) and (4) divide offenders by education, using completion of high school as the threshold. The effect is significant only among the low-educated group and the magnitude is significantly larger for this subgroup (p-value = 0.061, Z-test), highlighting greater vulnerability among individuals with limited human capital.

Columns (5)–(7) examine heterogeneity by age. We find significant effects across all cohorts (youth (15–24), middle-aged (25–44), and older adults (45–64)) with the coefficient for young offenders significantly larger than for other groups (p-values = 0.035 and 0.038, Z-tests). Given that younger individuals are less likely to own housing assets or substantial savings, they appear more susceptible to shocks in asset prices.

Columns (8) and (9) report results by gender. The effects are positive and statistically significant for both groups, though the coefficient for female offenders is somewhat larger (p-value = 0.007, Z-test). This pattern may reflect that women, who generally hold fewer property assets and face tighter financial constraints, are more sensitive to inequality shocks arising from speculative housing booms.[23]

Columns (10) and (11) investigate how kinship networks mediate the relationship between housing booms and crime. We proxy for local lineage strength by identifying whether an offender's surname ranks among the three most common in the city. The estimates show significant increases in crime only among individuals with less common surnames. This pattern echoes findings by Freedman (2021*a*,*b*), who document that shared surnames often signal membership in extended kinship networks that facilitate intra-clan risk sharing and resource pooling. Such networks offer informal insurance that helps buffer economic shocks, thereby reducing reliance on criminal activity as a coping strategy (Chen et al., 2022, 2024*b*).

Finally, Columns (12) and (13) explore heterogeneity by residential registration status. We

distribution among cases with complete information. Specifically, we calculate the share of, for example, male and female offenders within the subsample with known gender, and apply those proportions to the full set of offenders in each city. The same procedure is used for all other demographic variables.

[23]While the estimated elasticity is larger for women, its aggregate impact is limited because female crime rates are much lower—-0.013 offenders per thousand for women versus 0.153 for men. This difference implies an increase of only about 3 additional female offenders per million residents, compared with 13 for men. Moreover, gender differences in reporting or enforcement may also contribute to the observed pattern, making it less central to the overall interpretation.

classify offenders as locals if they committed crimes in the city of their hukou registration or place of birth, and as migrants otherwise. Although migrants typically own fewer housing assets, we find positive and significant effects for both groups (p-value = 0.648, Z-test). [24]

Social and Cultural Institutions. A substantial literature highlights the role of cultural norms and social institutions in shaping behavior and sustaining social order in China..[25] Building on this insight, we examine whether such institutions moderate the impact of speculative housing booms on crime. In particular, we test whether places with stronger moral discipline and richer informal risk-sharing experience smaller increases in property crime.

We focus on three dimensions of informal institutions through which social norms may shape responses to economic shocks and influence criminal behavior. First, we measure kinship strength using the number of genealogies per thousand residents, drawn from Shanghai Library (2009)'s comprehensive catalogue of Chinese genealogies (Zhongguo Jiapu Zongmu). Kinship networks provide powerful informal enforcement mechanisms, fostering mutual monitoring, moral suasion, and community-based risk sharing. Second, we capture religious influence using the spatial density of traditional Chinese religious sites, specifically the number of Buddhist monasteries and Taoist temples per square kilometer.[26] Both Buddhism and Taoism have historically shaped ethical norms in China, emphasizing self-restraint and moral discipline that discourage deviant behavior. Third, following Kung and Ma (2014), Li et al. (2020), and Deng et al. (2024), we quantify Confucian influence by the number of historical Confucian academies and temples per square kilometer. These institutions, which are central to the dissemination of Confucian teachings on hierarchy, virtue, and civic responsibility, embedded moral and collective values in local communities and continue to exert lasting social influence.[27]

We classify cities into High and Low groups along each dimension of informal institutions (kinship strength, religious influence, and Confucian heritage) based on the median value of the respective measure. Table 4 reports the results, with the dependent variable defined as the number

[24]This result is consistent with evidence in Column (5) of Table 12, where we find the ratio of non-local residents to locals declined in treated cities after the HPR shock, implying that surging housing costs constrained in-migration. As a result, long-term residents, who are unable to relocate and facing worsening affordability, bore the brunt of inequality pressures, leaving them more exposed to the economic strains that foster financially motivated crime.

[25]Institutions such as religious traditions, Confucian values, and kinship networks not only impose moral constraints that deter deviant behavior (Xu et al., 2017; Kung and Ma, 2014), but also serve as informal insurance mechanisms by facilitating community-based financial support (Chen et al., 2022, 2024*b*).

[26]Data on religious sites are drawn from the Spatial Explorer of Religion system, developed jointly by Purdue University, the University of Michigan, and Wuhan University, which offers comprehensive spatial coverage of religious institutions across Chinese cities.

[27]Data on ancient academies are sourced from Ji (1996)'s *Dictionary of Academies in China (Zhongguo Shuyuan Cidian)*, which records over 7,000 academies with verified historical locations across Chinese cities from the Tang (618–907) to the Qing (1644–1912) dynasties. The data of the Confucian temples are compiled from local archives and historical sources, covering more than 500 verified sites throughout the country.

Table 4: Heterogeneous Effects of HPR Spillovers on Property Crime by Social and Cultural Institutions

	Kinship		Religion		Confucianism	
	(1) High	(2) Low	(3) High	(4) Low	(5) High	(6) Low
Panel A: Dep. Var.: log #Case per thousand						
$\text{Treat}_c \times \text{Post}_t$	0.018	0.158***	0.009	0.096***	0.016	0.082**
	(0.045)	(0.038)	(0.039)	(0.033)	(0.048)	(0.040)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Observations	924	690	810	804	816	798
R-squared	0.915	0.893	0.924	0.887	0.934	0.873
Panel B: Dep. Var.: log #Criminal per thousand						
$\text{Treat}_c \times \text{Post}_t$	0.030	0.155***	0.029	0.081**	0.036	0.085*
	(0.044)	(0.043)	(0.040)	(0.034)	(0.047)	(0.043)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Observations	924	690	810	804	816	798
R-squared	0.910	0.885	0.919	0.884	0.931	0.867

Notes: The unit of observation is the city-year from 2014 to 2019. The dependent variable in Panel A is the number of property crime cases per thousand population (in log); in Panel B, it is the number of offenders involved in property crimes per thousand (in log). We run separate regressions for the cities with 50% most and least the number of genealogies per thousand population in columns (1) and (2), the number of Buddhist monasteries and Taoist temples per square kilometer in columns (3) and (4), and the number of historical Confucian academies per square kilometer in columns (5) and (6). All regressions include time-varying city-level controls, city fixed effects, and year fixed effects. Robust standard errors, clustered at the city level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

of property crime cases per thousand residents (in log) in Panel A and the number of offenders involved per thousand (in log)in Panel B.

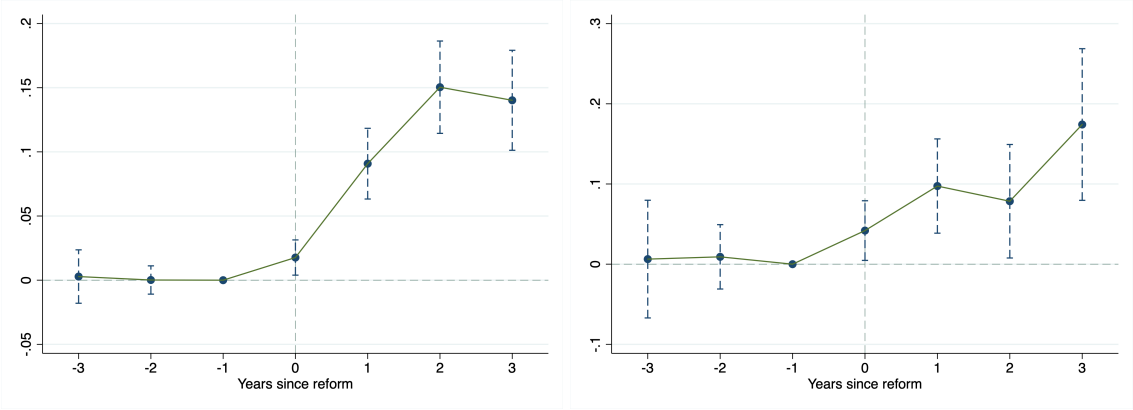
Across all three dimensions, a clear and consistent pattern emerges. Cities with weaker kinship ties, lower densities of religious institutions, or less Confucian influence experience significant increases in property crimes following the HPR shock. By contrast, cities with stronger social and cultural institutions show no significant crime response. Moreover, the estimated increase in property crimes is significantly smaller in cities with stronger informal institutions across all three measures.[28] These results suggest that kinship networks, religious traditions, and Confucian norms each operate as effective informal constraints, mitigating the criminogenic effects of speculative housing booms. More broadly, they highlight the critical role of embedded social norms and community-based institutions in mediating behavioral responses to economic shocks.

3.3 Validity and Robustness Checks

We perform a series of robustness checks to assess the credibility of our baseline results. Unless otherwise noted, all specifications include the same city-level controls as in equation (1). We present

[28] In both case and offender counts, p-values < 0.05 for Fisher's permutation test.

Figure 2: Pre-trends and Dynamic Responses to the HPR Shock on House Prices and Crime Rates



(A) House Prices (B) Property Cases

Notes: The figure plots the estimated dynamic effects of the HPR shock on house prices and property crime rates in nearby unregulated cities from 2013 to 2019. The dependent variables (all in log) are house price index in Panel (A) and the number of property crime cases per thousand residents in Panel (B). Each dot represents the estimated coefficient from the interaction terms in equation (2), and the dashed lines indicate 95 percent confidence intervals, with standard errors clustered at the city level. All coefficients are expressed relative to the baseline year, 2015. In the figure, "–1" denotes the year before the HPR shock, "0" corresponds to the year of policy implementation (2016), and "+1" indicates the first full post-treatment year.

results for the combined property-and-violent category and, separately, for property crimes alone.

Pre-Trends Tests. We begin by testing the validity of the parallel-trends assumption. Specifically, we examine whether treated and control cities exhibited similar pre-treatment trends in both house prices and crime rates. To do so, we replace the treatment indicator $\text{Treat}_c \times \text{Post}_t$ with interactions between treatment status and year dummies, estimating the following event-study specification:

$$Y_{ct} = \sum_k \beta_k \left(\text{Treat}_c \times \mathbb{I}_{t=k}\right) + \beta_X X_{ct} + \gamma_t + \gamma_c + \epsilon_{ct}, \tag{2}$$

where Y_{ct} is log outcome in city c and year t, either house prices or crime, and $\mathbb{I}_{t=k}$ indicates year k. Other covariates follow the baseline.

Figure 2 plots the event-study estimates β_k from equation (2), along with 95% confidence intervals. The two panels report dynamic responses for urban housing prices and property crime rates (both in logs), with the crime rate measured as the number of property crime cases per thousand residents.[29] All regressions are normalized to 2015, the year immediately preceding the HPR shock, as the reference period. The pre-treatment coefficients are small and statistically insignificant for both outcomes, confirming that treated and control cities followed parallel trajectories prior to the policy. Starting in 2016, however, housing prices and property crime rates in treated cities rise sharply and continue to diverge in subsequent years.[30] The close temporal alignment between the

[29]Because judicial disclosure coverage was incomplete before 2014, we restrict the crime pre-trend analysis to 2013–2019 to ensure consistent data quality.

[30]Appendix Figure B.1 reports analogous results for housing prices over 2010–2019 and alternative crime measures

onset of housing booms and the increase in property crime lends strong support to our identification strategy. Appendix Table A.6 reports the full set of coefficient estimates from equation (2).

Continuous and Alternative Distance Cutoffs. In the baseline analysis, cities located within 250 kilometers of the nearest regulated city are classified as exposed to the HPR spillover, while those beyond this threshold serve as controls. To assess the robustness of this distance-based definition, we implement two complementary strategies following Deng et al. (2022) and Liu and Zhang (2024): a continuous-distance specification and alternative binary distance cutoffs. Table A.7 summarizes the results: Panel A reports total property plus violent crimes, and Panel B focuses on property crimes only. We first estimate a continuous version of equation (1), replacing the binary treatment indicator ($\text{Treat}_c \times \text{Post}_t$) with the interaction term $\ln(\text{Distance}_c) \times \text{Post}_t$, where Distance_c denotes the straight-line distance between city c and the nearest regulated city. If speculative capital flows diminish with distance, as housing market dynamics suggest, then spillover effects on crime should likewise attenuate. Consistent with this prediction, Columns (1)–(2) show negative and statistically significant coefficients on the interaction term: cities farther from policy epicenters exhibit smaller post-HPR increases in crime.

Next, we re-estimate the baseline model using alternative binary distance thresholds. Specifically, Columns (3) through (6) of Table A.7 define the treatment group as cities located within 300 kilometers and 200 kilometers, respectively, of the nearest regulated city. In all cases, the estimated coefficients remain positive and statistically significant, confirming that our findings are not sensitive to the precise choice of spatial cutoff.

Addressing the Bad Control Problem. The baseline regressions include city-level time-varying covariates to capture confounders that might influence crime trends independently of the housing policy shock. A well-known concern, however, is that such controls may themselves respond to the treatment, generating post-treatment bias—commonly referred to as the "bad-control" problem. We adopt two complementary strategies to mitigate this issue.

First, following Federico et al. (2025) and Che et al. (2022), we interact baseline city characteristics measured in 2014 with the post indicator (Post_t). This keeps the controls fixed at pre-policy levels while allowing their effects to vary over time. As shown in Columns (1) and (2) of Table A.8, the estimated treatment effects for both combined property and violent crimes (Panel A) and property crimes alone (Panel B) remain highly consistent with the baseline. Second, following Eppinger and Ma (2024) and Lu et al. (2019), we replace contemporaneous controls with interactions between pre-treatment city characteristics and year dummies. This approach allows for flexible city-specific time trends while minimizing endogeneity concerns in post-policy controls. Columns (3) and (4) in

over 2013–2019, showing the same absence of pre-trend divergence and pronounced post-policy effects.

both panels again confirm robustness, showing that the estimated effects of the housing shock on crime remain positive and statistically significant across both outcomes.

Alternative Crime Measures. If the link between crime and HPR shocks reflects real behavioral change rather than reporting or classification artifacts, the intensity and economic magnitude of offenses should also rise. We therefore construct two complementary outcomes. First, we compute the number of criminal incidents per case to capture the frequency and persistence of offending. Second, for property crimes, we record the monetary value involved in each case as a proxy for the scale of criminal gains. We aggregate both measures to the city level and re-estimate the baseline difference-in-differences specification.[31]

Columns (1) and (2) of Appendix Table A.9 show that both measures increase significantly after the HPR shock in nearby unregulated cities, for total property plus violent crimes (Panel A) and for property crimes alone (Panel B). These results indicate that speculative housing booms raised not only the incidence of crime but also its economic stakes.

Robustness to Policy Timing and Comparison-City Matching. We further test the sensitivity of our findings to alternative coding of the policy timing. Because the first wave of HPR measures began in September 2016, we follow Liu and Lu (2015) and assign a one-third weight to that year in defining the post-policy period. As shown in Columns (3) and (4) of Table A.9, the estimates are virtually unchanged for both combined property and violent crimes (Panel A) and property crimes only (Panel B).

We also address potential differences in pre-policy characteristics using propensity-score matching. Cities are matched on a rich set of 2014 indicators, including GDP growth, registered population, fiscal expenditure per capita, average wage, export volume, and house prices. Results from the matched sample, reported in Columns (5) and (6) in both panels, closely track the baseline. This consistency indicates that the post-policy rise in crime is not driven by pre-existing structural differences across cities.

Falsification Test. To verify that our baseline results are not driven by spurious correlations or unobserved contemporaneous shocks, we conduct a falsification test by randomly assigning treatment status across cities and re-estimating the baseline specification 500 times. Figure B.2 plots the distribution of estimated coefficients from these placebo regressions. In each panel, the red vertical line marks the corresponding baseline estimate. The placebo distributions are tightly centered at zero, indicating no systematic effect under random assignment. By contrast, the actual

[31] To mitigate the influence of extreme outliers, we winsorize case-level monetary values at the 99.9th percentile. The results remain robust when alternatively winsorized at the 99.5th or 99th percentile.

coefficients from the baseline lie far in the upper tails, beyond the 95th percentile in all four panels. This pattern provides strong evidence that the post-HPR increase in crime reflects a genuine causal effect rather than statistical noise or coincident shocks.

Placebo Test: Offenses Unrelated to Inequality. To further verify that the observed rise in property crime stems from housing boom, rather than from broader social changes or shifts in general crime trends, we examine whether other categories of offenses respond similarly. Using keyword-based text analysis of case titles, we classify all remaining offenses into four groups: (i) endangering public security, (ii) felony traffic violations, (iii) drug-related crimes, prostitution, and gambling, and (iv) miscellaneous offenses. The first three categories together account for approximately 74.7% of all non-property cases.[32] Appendix Table A.10 reports the results. Across all categories, we find no statistically significant changes in crime rates following the housing boom. The absence of effects among these unrelated offenses strengthens our interpretation that the HPR-induced housing shocks specifically fueled economically motivated property crimes.

In summary, these findings reinforce our interpretation that the rise in crime reflects speculative housing spillovers rather than improvements in economic fundamentals or broader macroeconomic trends. Crime rates diverge sharply only after the policy, with no evidence of pre-existing differences or concurrent changes in local labor markets. The pattern is clear and robust: where housing speculation spilled over, crime followed.

4 Mechanism Analysis

Having established the causal link between speculative housing booms and rising property crime, we now turn to the underlying mechanisms. A rich body of research, as discussed in the literature review, identifies several channels through which economic conditions shape criminal behavior. Crime may rise when legitimate opportunities deteriorate—a deprivation channel driven by falling income, wages, or employment that lowers the opportunity cost of offending (Gould et al., 2002; Dix-Carneiro et al., 2018). It may also respond to relative inequality, where visible wealth disparities alter perceived returns to crime or erode social cohesion (Fajnzylber et al., 2002*a*; Enamorado et al., 2016; Buonanno and Vargas, 2019). Other mechanisms proposed in the literature include psychological strain from social stress or frustration, shifts in population composition due to migration, and weakened deterrence arising from declining law enforcement capacity.

Our empirical evidence allows us to distinguish among these competing explanations. We find no evidence of declining income, employment, or enforcement, ruling out deprivation and deterrence effects. Nor do we observe changes in migration or violent crime, rejecting composition-based and psychological-strain channels. Instead, the results point to two interrelated mechanisms—*widened*

[32] Complete keyword lists and English translations are provided in Appendix Table A.3.

inequality and *wealth visibility*—as the dominant forces linking housing booms to crime. The surge in housing prices expanded local wealth disparities by enriching multi-property owners while leaving renters and first-time buyers behind, thereby raising the relative payoff from property crime. At the same time, growing inequality fueled conspicuous consumption: wealthier households converted housing gains into visible displays of affluence, lowering the informational cost of identifying lucrative targets and heightening incentives for opportunistic crime.

4.1 Widened Inequality

We begin by testing whether speculative housing booms widened wealth disparities across households with different exposure to property markets. If rising house prices primarily benefit multi-property owners, the resulting divergence in asset appreciation should magnify inequality and strengthen incentives for property crime.

Household-level Evidence. We explore this mechanism by comparing households with multiple properties to those owning a single property or renting, focusing on how their wealth and investment behavior changed following the housing shock. The analysis draws on microdata from the CFPS. Because nearly 89% of households in our sample already own at least one property, the key distinction lies between those able to accumulate multiple properties and those who cannot. Our empirical strategy estimates the following specification:

$$Y_{hct} = \beta_m \text{More}_h \times \text{Treat}_c \times \text{Post}_t + \beta_o \text{One}_h \times \text{Treat}_c \times \text{Post}_t + \beta X_{ct} + \gamma_h + \gamma_t + \gamma_c + \epsilon_{hct}, \quad (3)$$

where the dependent variable Y_{hct} includes a range of household-level outcomes, including housing assets, financial holdings, investment returns, property income, and total wealth. The indicator More_h equals one if household h owned multiple properties after 2016, while One_h equals one if the household owned no more than one. The coefficients β_m and β_o capture the differential impacts of the housing boom across these groups.

Table 5 summarizes the results. Column (1) shows that, following the HPR shock, housing assets rose sharply among multi-property households in treated cities, while those with one or no property experienced a decline. To rule out the influence of non-tradable rural dwellings, Column (2) excludes rural owner-occupied housing. The results indicate that households owning multiple urban properties experienced a significant increase in housing assets, whereas single-property owners and renters show no meaningful change.[33] Overall, the housing boom disproportionately benefited

[33]This suggests that the decline in housing assets among single- or no-property households in Column (1) is primarily driven by a drop in the implicit market value of rural dwellings. One possible reason is that rising urban house prices raises the difficulty for rural households to upgrade to urban properties. In addition, Liu and Zhang (2024) find that higher urban house prices reduce marriage rates among rural residents without urban housing. This may further diminish incentives to repair and maintain dwellings, thereby accelerating depreciation and lowering their potential market value.

Table 5: Mechanism: Divergent Household Responses to House Price Gains

	(1) log Housing asset	(2) log Housing asset (tradable)	(3) log Financial asset	(4) log Financial invest. income	(5) log Property income	(6) Property income share	(7) log Household wealth
$\text{More}_h \times \text{Treat}_c \times \text{Post}_t$	0.240***	0.277***	0.561**	0.130***	0.416***	0.006***	0.190***
	(0.058)	(0.071)	(0.222)	(0.045)	(0.154)	(0.002)	(0.065)
$\text{One}_h \times \text{Treat}_c \times \text{Post}_t$	-0.245***	-0.101	0.314	0.023	-0.037	0.000	-0.066
	(0.056)	(0.064)	(0.224)	(0.024)	(0.111)	(0.001)	(0.067)
City FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y
Observations	43,728	12,444	48,313	39,072	39,463	38,221	47,433
R-squared	0.731	0.772	0.471	0.505	0.547	0.509	0.652

Notes: Observations are at the household-year level. The dependent variables are: log housing assets (column 1); log tradable housing assets (column 2); inverse hyperbolic sine (IHS) transformation of financial assets (column 3), financial investment income (column 4), and property income (column 5); property income share, winsorized at the 99th percentile (column 6); and log household wealth (column 7). Compared to column (1), column (2) further excludes rural owner-occupied dwellings. Financial assets include cash, deposits, government bonds, stocks, mutual funds, derivatives, other instruments, and household credit claims. Property income comprises financial investment returns, rental income from housing, land lease income, and income from other rental assets. Household wealth includes cash, deposits, financial assets, credit, durable goods, land, and productive capital. Because the 2012 CFPS did not collect financial investment income, columns (3)–(5) use data from 2014–2022, while the others cover 2012–2022. More_h is an indicator for households with more than one property after 2016; One_h identifies households with at most one property. Treat_c equals one for cities within 250 km of the nearest regulated city; Post_t equals one for years from 2016 onward. All regressions include city-by-year controls, city fixed effects, year fixed effects, and household fixed effects. Robust standard errors clustered at the city level are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

households already holding multiple properties, while single-property owners and renters were largely excluded from these wealth gains.

The inequality extends beyond housing. In Column (3), we find that rising collateral values spurred a significant increase in financial asset holdings, such as deposits, stocks, bonds, private credit claims, and other investments, among multi-property owners, while there is no comparable change for others. This reallocation translates into higher financial income, as shown in Column (4). The boom also stimulated rental demand and prices, generating additional income streams for landlords. Columns (5) and (6) show that both the level and share of property income (including returns from rent, land leases, and financial investments) increased for multi-property households but remained flat for others.[34] Finally, Column (7) confirms that these cumulative advantage significantly boosted overall household wealth for multi-property owners. These results demonstrate that speculative housing booms can amplify wealth concentration and widen inequality even in cities not directly subject to regulation.

City-level Evidence. Building on the household-level findings in Table 5, we next examine whether the HPR-induced housing boom translated into broader inequality at the city level. Using household data from the CFPS, we compute five standard inequality measures for each city-year:

[34]Because of a large number of zero values, we apply the inverse hyperbolic sine transformation to financial assets, investment income, and property income in Columns (3)–(5), following Chen (2013); McKenzie (2017); Londoño-Vélez and Ávila-Mahecha (2021); Johannesen et al. (2020).

Table 6: Mechanism: HPR Spillovers and Rising Inequality in Nearby Cities

	(1) Gini	(2) RMD	(3) MLD	(4) Theil	(5) COV
Panel A: Housing asset inequality					
$\text{Treat}_c \times \text{Post}_t$	0.041***	0.037***	0.117***	0.118***	0.226***
	(0.010)	(0.008)	(0.033)	(0.032)	(0.083)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	598	598	598	598	598
R-squared	0.735	0.736	0.750	0.521	0.381
Panel B: Property income inequality					
$\text{Treat}_c \times \text{Post}_t$	0.054**	0.047*	0.183**	0.101**	0.129*
	(0.026)	(0.025)	(0.078)	(0.046)	(0.071)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	477	477	477	477	477
R-squared	0.484	0.478	0.512	0.488	0.485
Panel C: Household wealth inequality					
$\text{Treat}_c \times \text{Post}_t$	0.044***	0.038***	0.132***	0.138***	0.296***
	(0.010)	(0.008)	(0.031)	(0.031)	(0.083)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	598	598	598	598	598
R-squared	0.659	0.676	0.628	0.460	0.345

Notes: Observations are at the city-year level. Panel A reports results for housing asset inequality, Panel B for property income inequality, and Panel C for household wealth inequality. Columns (1)–(5) correspond to five standard inequality measures: Gini index, relative mean deviation, mean log deviation, Theil entropy index, and coefficient of variation. Due to the absence of financial investment income data in the 2012 CFPS, Panel B uses data from 2014–2022, while Panels A and C use the full 2012–2022 sample. Property income is winsorized at the 99th percentile within each year. Treat_c is an indicator equal to one if a city is located within 250 km of the nearest regulated city. Post_t equals one for years 2016 and after. All regressions include city fixed effects, year fixed effects, and time-varying city-level controls. Robust standard errors, clustered at the city level, are reported in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

the Gini coefficient, relative mean deviation (RMD), mean log deviation (MLD), Theil index, and coefficient of variation (COV), following established practices in the inequality literature (Attanasio and Pistaferri, 2014; Coibion et al., 2017, 2021). We then re-estimate equation (1) using these measures as dependent variables.

Table 6 presents the results. Cities more exposed to HPR spillovers exhibit significantly larger increases in inequality across all dimensions. In Panel A, we compute inequality based on housing assets; in Panel B, on property income; and in Panel C, on total household wealth. The estimated effects are uniformly positive and statistically significant. Moreover, the magnitudes in Panel C exceed those in Panel A, suggesting that the initial disparities from housing appreciation were subsequently amplified through differential financial accumulation and investment returns.

Subjective Inequality. To further substantiate our argument that housing booms exacerbate inequality, we examine whether individuals *perceive* a widening social gap using micro-level data from the CFPS. Table 7 presents the results for individuals in urban areas, with all regressions including individual fixed effects. In Columns (1) and (2), we measure perceived inequality as the gap between each respondent's self-reported income or social status and the maximum scale (five points). Following the HPR shock, individuals in treated cities report significantly larger perceived gaps in both dimensions, indicating a pronounced rise in subjective inequality. In Column (3), we use respondents' assessments of the adequacy of the social security system as a proxy for redistributive preferences. Residents in treated cities express significantly stronger demand for enhanced social protection, consistent with growing perceptions of unfairness.

Columns (4)–(6) explore heterogeneity across asset-holding groups by estimating an individual-level version of equation (3). Both multi-property owners and those with a single or no property report higher perceptions of inequality, but only the latter group—those left out of the housing boom—show a significant increase in demand for redistribution (Column 6). These findings suggest that as inequality objectively rises, residents feel increasingly left behind and call for institutional responses. This finding aligns well with our broader mechanism: perceived inequality, not merely objective disparities, can erode social cohesion and ultimately fuel economically motivated crime, especially in settings where conspicuous wealth is highly visible.

Crime Follows Inequality. Finally, we provide complementary evidence linking inequality to the rise in crime following the HPR shock. If speculative housing activity exacerbates local disparities, crime should increase more sharply in cities where inequality widened the most. Appendix Table A.11 supports this prediction. We classify cities into high- and low-inequality groups based on whether the post-HPR change in the housing-asset Gini coefficient (2012–2020) is above or below the median.

Table 7: Mechanism: HPR Spillovers, Perceived Inequality, and Individual Attitudes

	(1) Income gap	(2) Status gap	(3) Social security	(4) Income rank	(5) Social status	(6) Social security
$\text{Treat}_c \times \text{Post}_t$	0.099**	0.113**	0.330*			
	(0.045)	(0.056)	(0.175)			
$\text{More}_h \times \text{Treat}_c \times \text{Post}_t$				0.107**	0.113*	0.228
				(0.051)	(0.062)	(0.176)
$\text{One}_h \times \text{Treat}_c \times \text{Post}_t$				0.105*	0.125**	0.424**
				(0.053)	(0.062)	(0.201)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Observations	14,203	14,277	12,283	14,054	14,129	12,164
R-squared	0.588	0.591	0.516	0.589	0.592	0.516

Notes: Observations are at the individual-year level from 2012 to 2022. The dependent variables in columns (1) and (2) measure the respondent's perceived gap between their self-assessed income and social status and the maximum score (5 points). Column (3) uses the respondent's perceived severity of the social security system as a proxy for concerns about redistribution and social protection. More_h is a dummy equal to one if the household owns more than one property after 2016; One_h equals one if the household owns at most one property. Treat_c indicates cities within 250 km of the nearest regulated city, and Post_t equals one for years 2016 and onward. All specifications include time-varying city-level controls, city fixed effects, year fixed effects, and individual fixed effects. Robust standard errors are clustered at the city level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Consistent with the inequality mechanism, we find that the HPR shock significantly raises both total and property crimes in cities inequality rose more sharply, while the effects in low-Gini-change cities are statistically insignificant. A similar pattern holds when we use changes in perceived social-status gaps as a proxy for subjective inequality. Moreover, the estimated treatment effects are significantly larger in high-inequality cities than low-inequality counterparts.

4.2 Visible Inequality through Conspicuous Consumption

Our discussion of perceived inequality already points to the importance of visibility in driving property crime. Housing booms made wealth more conspicuous, which, as shown by Chiu and Madden (1998), reduces the information costs of identifying affluent targets and thereby increases crime (Hicks and Hicks, 2014; Mejía and Restrepo, 2016).[35] Drawing on household consumption data from the CFPS and city-level import records from Chinese Customs, we provide direct evidence that speculative housing booms triggered a surge in conspicuous consumption and luxury imports, making wealth more visible and amplifying the perceived returns to property crime.

Conspicuous Consumption. Following Charles et al. (2009), Brown et al. (2011), and Kaus (2013), we measure conspicuous consumption across categories typically associated with social signaling—apparel, automobiles, durable goods, personal care (e.g., health clubs, salons), and

[35] Rising house prices have been shown to stimulate consumption through housing wealth effects, particularly among property-rich households (Berger et al., 2018; Stroebel and Vavra, 2019; Kaplan et al., 2020; Painter et al., 2022).

Table 8: Mechanism: HPR Spillovers and Conspicuous Consumption

Panel A: Conspicuous consumption	(1) log Cons. Cons.	(2) log Cons. Cons. p.c.	(3) log Other Cons.	(4) Apparel	(5) Automobile	(6) Durable	(7) Healthcare	(8) Beauty	(9) Social gift
$\text{More}_h \times \text{Treat}_c \times \text{Post}_t$	0.121**	0.127**	0.010	0.061	0.882***	0.186	0.172*	0.332**	0.375**
	(0.057)	(0.060)	(0.038)	(0.083)	(0.150)	(0.164)	(0.089)	(0.137)	(0.176)
$\text{One}_h \times \text{Treat}_c \times \text{Post}_t$	-0.012	0.016	-0.024	-0.118	0.020	-0.105	-0.090	0.222	0.085
	(0.051)	(0.056)	(0.032)	(0.073)	(0.111)	(0.133)	(0.060)	(0.138)	(0.168)
City FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Household FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	46,517	41,733	48,126	48,313	48,313	48,313	48,313	48,313	48,313
R-squared	0.597	0.563	0.590	0.535	0.541	0.315	0.448	0.583	0.448

Panel B: Luxury imports	(10) Luxury import	(11) Leather goods	(12) Jewelry	(13) Car & watch	(14) Luxury import	(15) Luxury import	(16) Product import
$\text{Treat}_c \times \text{Post}_t$	1.392***	1.092***	2.836***	1.535***	1.220**	1.487**	0.386
	(0.383)	(0.412)	(0.778)	(0.501)	(0.588)	(0.632)	(0.261)
$\text{Luxury}_k \times \text{Treat}_c \times \text{Post}_t$							1.407**
							(0.699)
City FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y
Observations	1,288	1,246	882	354	1,496	1,547	14,712

Notes: Observations are at the household-year level for years from 2012 to 2022. Panel A examines the change in conspicuous consumption across multi-property owners and households with at most one property. The dependent variables (all in log) are total conspicuous consumption in column (1), conspicuous consumption divided by the number of working-age household members (aged 15-64) in column (2), and non-conspicuous expenditures in column (3). Columns (4)-(9) disaggregate conspicuous consumption into six categories: apparel, automobiles, durable goods, healthcare, personal grooming and social gift expenditure, where the dependent variables are transformed using the inverse hyperbolic sine function to account for zero values. Column (10) adopts a narrow definition of luxury goods, comprising selected products from HS 2-digit codes 42, 43, 71, 87 and 91. Columns (11)-(13) decompose luxury goods into three categories: leather goods, jewelry, car and watch. Columns (14) and (15) use broader definitions of luxury goods, with column (14) incorporating all products from HS2-digit codes 33, 41, 42, 71 and 91, and column (15) further includes products from 43, 50, 61, 62, 64, 65, 66 and 67. Column (16) considers a product-level specification in which we interact the treatment and post indicators with a luxury goods dummy variable. More_h is a dummy that takes the value of one if the household owns more than one property after 2016, while One_h is an indicator equal to one if the household owns at most one property. Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. All columns include time-varying city-level controls, city fixed effects and year fixed effects. All columns in panel A further add household fixed effects. All columns in panel B use PPML regressions. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

social gifts (e.g., financial transfers to relatives or acquaintances). We then estimate equation (3), comparing multi-property households with those owning one or no property.

Panel A of Table 8 reports the results. Columns (1) and (2) show that only multi-property households significantly increase their conspicuous spending after the HPR shock, both in total and on a per–working-age–member basis. Households with a single or no property display no significant change. This pattern, however, does not reflect a broad rise in consumption: Column (3) shows that non-conspicuous expenditures, such as basic necessities, commuting, education, and basic healthcare, remain flat for both groups.

Columns (4)–(9) break down the response by category. The increase is concentrated in automobiles, personal care (health services and grooming), and social gifts—categories that are highly visible in everyday interactions and public settings.[36] The surge in automobile spending is consistent with Deng et al. (2022), who show that real estate windfalls shift demand toward durable, higher-end purchases.

Luxury Imports. Panel B of Table 8 analyzes city-level luxury imports as a direct proxy for high-end consumption.We assemble highly disaggregated product-level import data from Chinese Customs and classify luxury items at the HS 8-digit level within the following HS 2-digit chapters: 42 (leather), 43 (fur), 71 (jewelry and precious metals), 87 (automobiles), and 91 (watches).[37]

We re-estimate equation (1) using luxury imports as the dependent variable, applying Poisson Pseudo-Maximum Likelihood (PPML) estimation and including the log of total imports as a control to isolate the intensive margin. Column (10) shows that treated cities experience a significant increase in luxury imports relative to controls following the HPR shock, consistent with the CFPS-based evidence. Columns (11)–(13) disaggregate the effects by product type: leather and fur, jewelry, and automobiles and watches, with all categories showing significant increases.

This pattern remains robust when using broader definitions of luxury goods. Columns (14) and (15) adopt alternative classifications from Warby and Bailey (2025) and Sun (2018), respectively,

[36] We apply the inverse hyperbolic sine transformation to each consumption category in Columns (4)–(9) to account for zero values.

[37] For instance, HS 43031010 ("fur apparel"), HS 71131910 ("gold jewellery and parts thereof"), and HS 91012100 ("automatic wrist-watches with precious-metal cases") fall into product headings universally treated as luxury segments: fur apparel (HS 43/4303), precious-metal jewellery (HS 71/7113) and precious-metal watches (HS 91/9101). Industry benchmarks classify jewellery and watches among core "personal luxury goods" categories (see Boston Consulting Group & Altagamma, *True-Luxury Global Consumer Insights 2025*, https://web-assets.bcg.com/51/92/68b6af8346629c05e586a734d857/bcg-altagamma-true-luxury-global-consumer-insights-report-2025-vpress-final.pdf). Moreover, the Organisation for Economic Co-operation and Development–European Union Intellectual Property Office joint report flags sectors such as leather goods (HS 42), jewellery (HS 71) and watches (HS 91) as among the most IP-intensive and counterfeit-prone, reflecting their high-end, status-signalling nature (https://www.oecd.org/content/dam/oecd/en/publications/reports/2025/05/mapping-global-trade-in-fakes-2025_5c812e3c/94d3b29f-en.pdf). The full list of HS 8-digit items is provided in Appendix Table A.12.

and yield similar results.[38] Columns (16) further implement a product-level difference-in-differences design following Lan and Li (2018), pairing each luxury item with a non-luxury counterpart within the same HS 2-digit category (e.g., leather vs. textile suitcases) and interacting the treatment and post-period indicators with a luxury-good dummy, Luxury_k.[39] The results show that luxury goods imports rise significantly more than comparable non-luxury imports in treated cities, while non-luxury imports remain unchanged. In summary, these findings reinforce the evidence that speculative housing booms heightened conspicuous consumption and made wealth more visible at the city level.

High-Value Targets. If conspicuous consumption makes wealth more visible, offenders may increasingly select affluent victims, generating larger monetary losses. To test this, we sort property crime incidents into quintiles by claim amount and re-estimate equation (1) for each quintile. We repeat the exercise by crime type (larceny, fraud, robbery) using the same value cutoffs.

Table 9 summarizes the results. For property crime overall, the effects of rising house prices are concentrated in higher-value incidents: coefficients are significant only for cases above the 40th percentile of claim amounts, while lower-value bins are small and statistically insignificant. The same pattern holds within crime types. For larceny, significance appears only above the 40th percentile, with increases of 8.9–14.2%. For fraud, significance emerges above the 60th percentile (8.0–13.2%). For robbery, effects are strongest above the 80th percentile, reaching 21.3%. In most cases (20 out of 23), the coefficients for the significant high-value incidents are significantly larger than those for low-value incidents ($p < 0.1$), except in Column (2) and Column (5) of Panel A, Column (2) and Column (4) of Panel B, and Column (3) and Column (5) of Panel D. [40]

We next examine whether the rise in crime is more pronounced in cities experiencing faster growth in conspicuous consumption. Appendix Table A.14 presents the results, where cities are grouped by whether their growth in conspicuous consumption from 2012 to 2020 is above or below the median (Columns 1–2 and 5–6), and by whether their growth in social-gift spending, which is a particularly visible component, is above or below the median (Columns 3–4 and 7–8). We find that the HPR shock significantly increases property crimes in cities with faster growth in conspicuous consumption. The insignificant effect on total crime likely reflects the broad definition of conspicuous consumption, which includes both visible and less visible goods. When focusing on social-gift spending, the effects strengthen: the HPR shock significantly raises both total and

[38] Specifically, Column (13) includes HS 2-digit codes 33, 41, 42, 71, and 91 following Warby and Bailey (2025), and Column (14) expands to 33, 42, 43, 50, 61, 62, 64, 65, 66, 67, 71, and 91 following Sun (2018).

[39] For instance, the non-luxury counterpart to "42021110—trunks and suitcases with outer surface of leather, composition leather, or patent leather" is "42021210—trunks and suitcases with outer surface of plastics or textile materials."

[40] Results remain robust when we employ Poisson Pseudo-Maximum Likelihood estimation to account for zero crime counts (see Appendix Table A.13).

Table 9: Mechanism: House Prices and Crime Return

Dep. Var:	log #Case per thousand				
	(1) 0-20th of amount	(2) 20-40th of amount	(3) 40-60th of amount	(4) 60-80th of amount	(5) 80-100th of amount
Panel A: All property crimes					
$\text{Treat}_c \times \text{Post}_t$	0.038	0.033	0.124***	0.102***	0.099***
	(0.034)	(0.040)	(0.034)	(0.031)	(0.033)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,611	1,613	1,612	1,614	1,614
R-squared	0.886	0.861	0.852	0.835	0.836
Panel B: Larceny					
$\text{Treat}_c \times \text{Post}_t$	0.033	0.039	0.146***	0.090***	0.149***
	(0.037)	(0.040)	(0.036)	(0.033)	(0.039)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,611	1,613	1,610	1,612	1,607
R-squared	0.886	0.857	0.844	0.817	0.695
Panel C: Fraud					
$\text{Treat}_c \times \text{Post}_t$	0.001	0.004	-0.009	0.130**	0.079*
	(0.001)	(0.003)	(0.063)	(0.054)	(0.044)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,442	1,593	1,612
R-squared	0.223	0.411	0.653	0.711	0.786
Panel D: Robbery					
$\text{Treat}_c \times \text{Post}_t$	-0.067	0.030	0.057	-0.004	0.204**
	(0.061)	(0.069)	(0.063)	(0.076)	(0.097)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,493	1,276	1,295	1,254	983
R-squared	0.703	0.710	0.687	0.647	0.549

Notes: Observations are at the city-year level for years from 2014 to 2019. The dependent variable is the number of property crime cases per thousand population (in log), where panels A-D consider all property crimes, larceny, fraud and robbery, respectively. Columns (1) to (5) display separate estimates for cases with claim amounts below the 20th percentile, 20th-40th percentile, 40th-60th percentile, 60th-80th percentile, and above the 80th percentile of claim amounts of all property crimes for each year. Given the small number of fraud cases in the 0-20th and 20th-40th percentiles, we apply the inverse hyperbolic sine transformation to the dependent variable to address skewness and accommodate zero observations. For readability, the coefficients and standard errors in columns (1) and (2) of panel C are scaled by 100. Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. All columns include time-varying city-level controls, city fixed effects and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

property crimes in high-growth cities, while effects in low-growth cities are smaller and statistically insignificant.

In summary, these results provide compelling evidence that conspicuous consumption following housing wealth shocks makes lucrative opportunities more visible to potential offenders, reinforcing the visibility channel emphasized in this paper. As housing booms make affluence more observable, they reduce the information costs of identifying high-value targets and increase the expected returns to property crime, consistent with the evidence in Draca et al. (2019), Kirchmaier et al. (2020), and Braakmann et al. (2024).

4.3 Alternative Mechanisms

Our mechanism analysis has identified widened inequality and its visibility as the primary channels through which speculative housing booms increase property crime. In this subsection, we assess several alternative explanations. Specifically, we examine (i) a *deprivation* channel driven by declines in income, wages, or employment; (ii) a *social-strain* channel emphasizing psychological stress and frustration; (iii) a *composition* channel arising from migration-induced demographic shifts; and (iv) a *deterrence* channel reflecting potential reductions in law-enforcement capacity.

The Deprivation Channel. While Confucius regarded inequality rather than deprivation as the root of social unrest, both economic research and early moral philosophy have long emphasized absolute poverty as a key driver of crime.[41] In Becker (1968)'s framework, poverty lowers the opportunity cost of crime: when legitimate opportunities are limited, the relative payoff to crime rises.[42] To explore the deprivation channel, we test whether deteriorating labor markets or rising living costs could explain the observed increase in crime.

At the individual level, Table 10, Columns (1)–(3) re-estimate equation (3) using labor market outcomes as dependent variables. Specifically, Column (1) employs a binary employment indicator equal to one if the individual is employed; Columns (2) and (3) use the log of wage income from the primary job and the log of total income from all jobs, respectively. Across all specifications, the coefficients are small and statistically insignificant, indicating that the HPR-induced housing shock did not affect employment status or earnings for either multi-property owners or those with fewer or no properties.

We further test whether rising house prices eroded real income through higher living costs, following Chen et al. (2025). Columns (4) of Table 10 uses household real income per working-age member net of housing-related expenses (rent, utilities, property management fees, and mainte-

[41] Marcus Aurelius, the famous Roman emperor and philosopher who lived in 121-180AD, once reflected "Poverty is the mother of crime".

[42] Recent evidence by Ma et al. (2025) shows that export slowdowns reduce labor demand and lower the opportunity cost of crime, leading to higher crime rates in Chinese regions more exposed to trade contraction.

Table 10: Mechanism: HPR Spillovers and Labor Market

	(1) Employment status	(2) log main job wage	(3) log total employment income	(4) log Household real income p.c.	(5) log Household real income p.c.
$\text{More}_h \times \text{Treat}_c \times \text{Post}_t$	0.0002	0.048	-0.029	0.101*	0.092
	(0.003)	(0.052)	(0.047)	(0.059)	(0.059)
$\text{One}_h \times \text{Treat}_c \times \text{Post}_t$	0.0005	0.004	-0.039	-0.007	0.012
	(0.003)	(0.049)	(0.047)	(0.054)	(0.054)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	N	N
Household FE	N	N	N	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	69,551	23,294	27,365	38,917	38,219
R-squared	0.395	0.643	0.646	0.553	0.542

Notes: The dependent variables are individual employment status in column (1), log main job wage in column (2), log total employment income in column (3), and log household real income divided by the number of working-age household members (aged 15-64) in column (4) and (5). Household real income in column (4) is defined as household total income net of rents and housing-related expenses, including heating costs, property management fees, housing repairs, and utilities (water, electricity, and fuel), while column (5) further subtracts mortgage and housing loan payments. More_h is a dummy that takes the value of one if the household owns more than one property after 2016, while One_h is an indicator equal to one if the household owns at most one property. Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. All columns include time-varying city-level controls, city fixed effects, year fixed effects and individual fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

nance). Alternatively, Column (5) uses household real income per working-age member net of both expenses and mortgage payments. Again, we find no significant effects in both cases. Hence, the rise in property crime cannot be attributed to worsening opportunity costs: neither labor market conditions nor real purchasing power changed materially after the housing shock.

Finally, Deng et al. (2022) provide additional city-level evidence that the HPR policy had no discernible effects on GDP, GDP growth, real estate investment, industrial output, employment growth, or population.[43] These findings reinforce that the post-HPR surge in crime is unlikely to reflect economic hardship or weakening fundamentals. Instead, it appears to stem from speculative housing-market spillovers that heightened inequality and altered social incentives, rather than from deprivation itself.

The Social Strain Channel. Rising inequality may foster crime not only by changing economic incentives but also through its psychological toll on individuals in lower socioeconomic positions. As wealth gaps widen, exposure to others' affluence can intensify feelings of frustration and relative deprivation. These emotional responses, rooted in perceived unfairness and unfulfilled aspirations, are central to sociological theories such as Merton (1938), which emphasize the psychological strain

[43]The muted response of real estate investment reflects institutional characteristics of China's land market, where land supply is heavily influenced by local government strategies and fiscal objectives rather than by market prices (Chang et al., 2025; He et al., 2022). For instance, Chang et al. (2025) show that during the COVID-19 housing downturn, local governments stabilized land and housing prices through supply controls, higher-priced land acquisitions by local government financing vehicles, and restrictions on new housing permits.

Table 11: Mechanism: HPR Spillovers and Psychological Strain

	(1) Depression or not	(2) Happiness	(3) Future confidence	(4) Word test	(5) Math test
$\text{More}_h \times \text{Treat}_c \times \text{Post}_t$	0.010	-0.127	-0.039	0.574	-0.192
	(0.017)	(0.099)	(0.033)	(0.802)	(0.352)
$\text{One}_h \times \text{Treat}_c \times \text{Post}_t$	0.011	-0.100	0.015	0.736	-0.238
	(0.016)	(0.092)	(0.034)	(0.686)	(0.256)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Individual FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	93,979	49,270	90,044	20,472	20,796
R-squared	0.446	0.593	0.491	0.864	0.842

Notes: Observations are at the individual-year level for years from 2012 to 2022. The dependent variables are depression indicator in column (1), happiness in column (2), confidence in the future in column (3), word test scores in column (4), and math test scores in column (5). More_h is a dummy that takes the value of one if the household owns more than one property after 2016, while One_h is an indicator equal to one if the household owns at most one property. Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. All columns include time-varying city-level controls, city fixed effects, year fixed effects and individual fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

induced by unequal opportunities. In this view, crime may arise not from material incentives but from emotional stress and resentment (Kelly, 2000).

To test whether housing-induced inequality affects psychological well-being, we construct a set of mental and cognitive health indicatorsfrom the China Family Panel Studies (CFPS). Specifically, we measure depression risk using the Center for Epidemiologic Studies Depression Scale (CES-D20) and the Kessler Psychological Distress Scale (K6). Following established thresholds in Vilagut et al. (2016) and Prochaska et al. (2012), individuals scoring above 20 on the CES-D20 or 13 on the K6 are classified as being at risk of depression or severe psychological distress.[44] We then re-estimate equation (3), replacing household fixed effects with individual fixed effects and using these measures as dependent variables.

Table 11 reports the results. Column (1) shows that house price appreciation has no significant effect on depression risk, regardless of housing ownership. Columns (2) and (3) examine self-reported happiness (ten-point scale) and confidence in the future (five-point scale); both remain unaffected. To explore cognitive effects, Columns (4) and (5) analyze standardized word and math test scores following Zhang et al. (2025). The results show no significant changes in either domain.[45]

Taken together, these findings suggest that housing booms, despite widening inequality, do not measurably affect mental health or cognitive performance in the short run. This aligns with our

[44]The 2012, 2016, 2018, 2020, and 2022 CFPS waves report CES-D20 scores ranging from 0 to 60, while the 2014 wave uses the K6 scale ranging from 0 to 24.

[45]The word test measures Mandarin character recognition, while the math test assesses logical and numerical reasoning. Each respondent is randomly assigned one word and one math test, with maximum scores of 34 and 24, respectively.

Table 12: Mechanism: HPR Spillovers and Migration

	(1) log #Case per thousand	(2) log #Case per thousand	(3) log #Criminal per thousand	(4) log #Criminal per thousand	(5) Migrant ratio
$\text{Migrant share}_c \times \text{Treat}_c \times \text{Post}_t$	0.003	-0.014	-0.022	-0.029	
	(0.200)	(0.191)	(0.225)	(0.212)	
$\text{Treat}_c \times \text{Post}_t$	0.052**	0.052**	0.066**	0.067**	-0.017**
	(0.026)	(0.026)	(0.027)	(0.027)	(0.007)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,602	1,602	1,602	1,602	1,602
R-squared	0.895	0.895	0.887	0.887	0.981

Notes: Observations are at the city-year level for years from 2014 to 2019. The dependent variables are the log of the number of criminal cases per thousand population in columns (1) and (2), the log of the number of criminals per thousand in columns (3) and (4), and non-Hukou residents divided by population with Hukou in column (5). Migrant share_c is the population share of migrants in city c calculated using micro-data from the 2015 Chinese Population Census in columns (1) and (3), and from the 2010 Census in columns (2) and (4). Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. All columns include time-varying city-level controls, city fixed effects and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

earlier evidence that house price shocks raise property but not violent crime (Table 2), reinforcing that the mechanism operates through economic incentives rather than emotional strain.

The Composition Channel. A third possible explanation for the rise in crime is changes in population composition following the HPR shock. Tighter purchase restrictions in regulated cities could have displaced potential buyers toward nearby unregulated areas, particularly low-income or credit-constrained households. If these inflows consisted disproportionately of low-skilled migrants with weaker local ties or limited access to public services, they could plausibly have raised crime rates in receiving cities.

Table 12 evaluates this hypothesis. We first test whether cities with historically greater capacity to absorb migrants experienced larger crime effects. Using migrant shares from the 2010 and 2015 Population Censuses to proxy each city's predisposition to attract newcomers, we interact these shares with the treatment and post dummies in equation (1). Across all specifications (Columns 1–4) the triple-interaction terms are small and statistically insignificant, indicating that crime increases are not systematically larger in cities with higher pre-HPR migrant shares.

Next, we examine actual changes in population composition using city-level data from the China City Statistical Yearbook. We compute the ratio of non-Hukou residents to locally registered residents as a proxy for migrant presence. Column (5) shows that this ratio declines significantly in treated cities relative to controls, suggesting that rising housing costs deterred, rather than attracted, in-migration. Overall, migration patterns cannot explain the observed increase in crime. If anything, speculative housing booms appear to have reduced the presence of non-local residents.[46]

[46]This interpretation aligns with Table 3, which shows similar post-HPR crime surges among both migrants and

Table 13: Mechanism: HPR Spillovers, Fiscal Revenue and Expenditure

	(1) log Total fiscal revenue	(2) log Total fiscal expenditure	(3) log Public security spending	(4) log Social spending	(5) log Stability spending
$\text{Treat}_c \times \text{Post}_t$	0.137***	0.060***	0.091***	0.062***	0.066***
	(0.024)	(0.016)	(0.019)	(0.018)	(0.018)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,608	1,608	1,344	1,020	996
R-squared	0.978	0.980	0.975	0.984	0.984

Notes: Observations are at the city-year level for years from 2014 to 2019. The dependent variables are fiscal measures in logs. The fiscal measures we consider include total fiscal revenue (column 1), total fiscal expenditure (column 2), general public budget expenditure on public security (column 3), general public budget expenditure on "social spending" (column 4), and general public budget expenditure on "stability spending" (column 5), where total fiscal revenue and expenditure are the sum of revenue and expenditure from the general public budget and the government-managed fund budget, and "stability spending" is the sum of public security expenditure (which includes all expenses by the People's Armed Police, prosecutorial system, the court system, judicial system, and public security organs) and "social spending" (which is the sum of expenditure on public services, education, social security, medical services, urban and rural affairs and housing). Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2017. All columns include time-varying city-level controls, city fixed effects and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

The Deterrence Channel Housing booms may also affect crime deterrence by changing law-enforcement capacity. Soaring house prices can incentivize local governments to channel more resources toward real estate development, potentially at the expense of public goods and social programs that help prevent crime and maintain stability.

We examine how house price shocks affect local fiscal revenue and expenditure.[47] Our analysis focuses on the two largest components of local public finance in China: the general public budget and the government-managed fund budget.[48] To evaluate spending on stability-related outlays, we further examine two subcategories within the general public budget: (i) public security spending, which covers the People's Armed Police, judicial and prosecutorial systems, and public security bureaus; and (ii) social spending, encompassing education, healthcare, social security, urban and rural development, and public housing. Since local budgets are typically set one year in advance, we define the post-treatment period as 2017 and onward.

Table 13 reports the results. Columns (1)–(2) show that housing booms significantly raise both total fiscal revenue and total expenditure. Turning to stability-linked categories, Columns (3)–(5) show that spending on public security, social services, and their combined total all rise significantly

long-term locals: Despite being more vulnerable, mobile migrants likely left high-cost cities.

[47] Given the absence of a unified municipal fiscal database, we compile detailed fiscal data from multiple sources, including the Fiscal Statistical Yearbooks published by provincial finance bureaus, the Statistical Yearbooks of provinces and cities, and balance sheets from municipal government websites.

[48] Under China's Budget Law, the government budget system comprises four components: (1) the general public budget, financed primarily through taxes and used for public welfare, security, and social development; (2) the government-managed fund budget, funded largely through land-use right transfers and project-specific levies; (3) the state-owned capital operation budget; and (4) the social insurance fund budget. Together, the first two budgets account for roughly 70% of total fiscal revenue and 78% of total expenditure nationwide as of 2024.

in treated cities after the HPR shock. These findings suggest that local governments responded to rising crime not by retrenching, but by expanding expenditure on public security and welfare. In this sense, our estimates of the HPR shock's effect on crime are likely conservative: part of the potential increase in social unrest may have been mitigated by these policy responses.

Section Summary. The evidence discussed in this section points to a coherent mechanism. Speculative housing booms did not raise crime by eroding legitimate opportunities, straining mental health, reshaping migration, or weakening deterrence. Rather, they operated through distributional and visibility channels, by widening wealth inequality and making affluence more conspicuous. As housing gains accrued to multi-property owners, visible displays of wealth amplified perceived disparities and lowered the information costs of targeting lucrative victims. In this sense, property crime followed the fault lines of speculative inequality, not the shadows of deprivation.

5 Theoretical Analysis

To interpret the empirical results and clarify the underlying mechanism, we develop a simple theoretical framework based on the seminal model of Chiu and Madden (1998). The model is deliberately kept parsimonious to highlight the central channel of *widening inequality and its visibility*. We incorporate exogenous increases in housing asset returns, which are driven by spillovers from HPR policies in neighboring cities, and trace how these shocks reshape local wealth distribution. Rising inequality not only widens the wealth gap but also induces conspicuous consumption that makes affluence more visible, thereby signaling high-value targets to potential offenders.

5.1 Model Setup

In the model, individuals start with different wealth, borrow to invest in housing, and see those differences magnified as returns accrue. They then choose whether to purchase conspicuous goods (e.g., luxury items), with consumption publicly observed and serving as a signal of status. Finally, individuals decide whether to commit crime, balancing risks against the expected payoff from targeting different types of consumers.

5.1.1 Borrowing and Investment Decisions

We consider a continuum of individuals indexed by $i \in [0, 1]$ with unit mass. Each individual is endowed with initial wealth m_i, independently drawn from a uniform distribution on $[0, 1]$. First, individuals make investment and borrowing decisions to maximize their wealth; second, they determine consumption and crime choices. Individuals can invest in a housing asset priced at P per unit, normalized to one without loss of generality, which yields a gross return R. They may also

borrow up to a fraction θ of their initial wealth at an exogenous interest rate r_b, following Banerjee and Duflo (2014). Each individual chooses housing investment a_i and borrowing b_i to maximize wealth:

$$\begin{aligned} \max_{a_i, b_i} \quad & w_i = m_i + b_i - a_i + a_i R - b_i(1 + r_b) \\ \text{s.t.} \quad & 0 \leq a_i \leq m_i + b_i, \\ & 0 \leq b_i \leq \theta m_i. \end{aligned}$$

When the housing return exceeds both the asset price and the cost of borrowing ($R > 1 + r_b$), the optimal strategy is to borrow up to the limit and invest all available resources in housing.[49] Individual i's post-investment wealth is then:

$$w_i = (1 + \theta) m_i R - \theta m_i (1 + r_b).$$

which follows a uniform distribution over $[0, R + \theta(R - 1 - r_b)]$. We denote the cumulative distribution function of this wealth distribution by $G(w) : [0,\ R + \theta(R - 1 - r_b)] \ \to \ [0, 1]$.

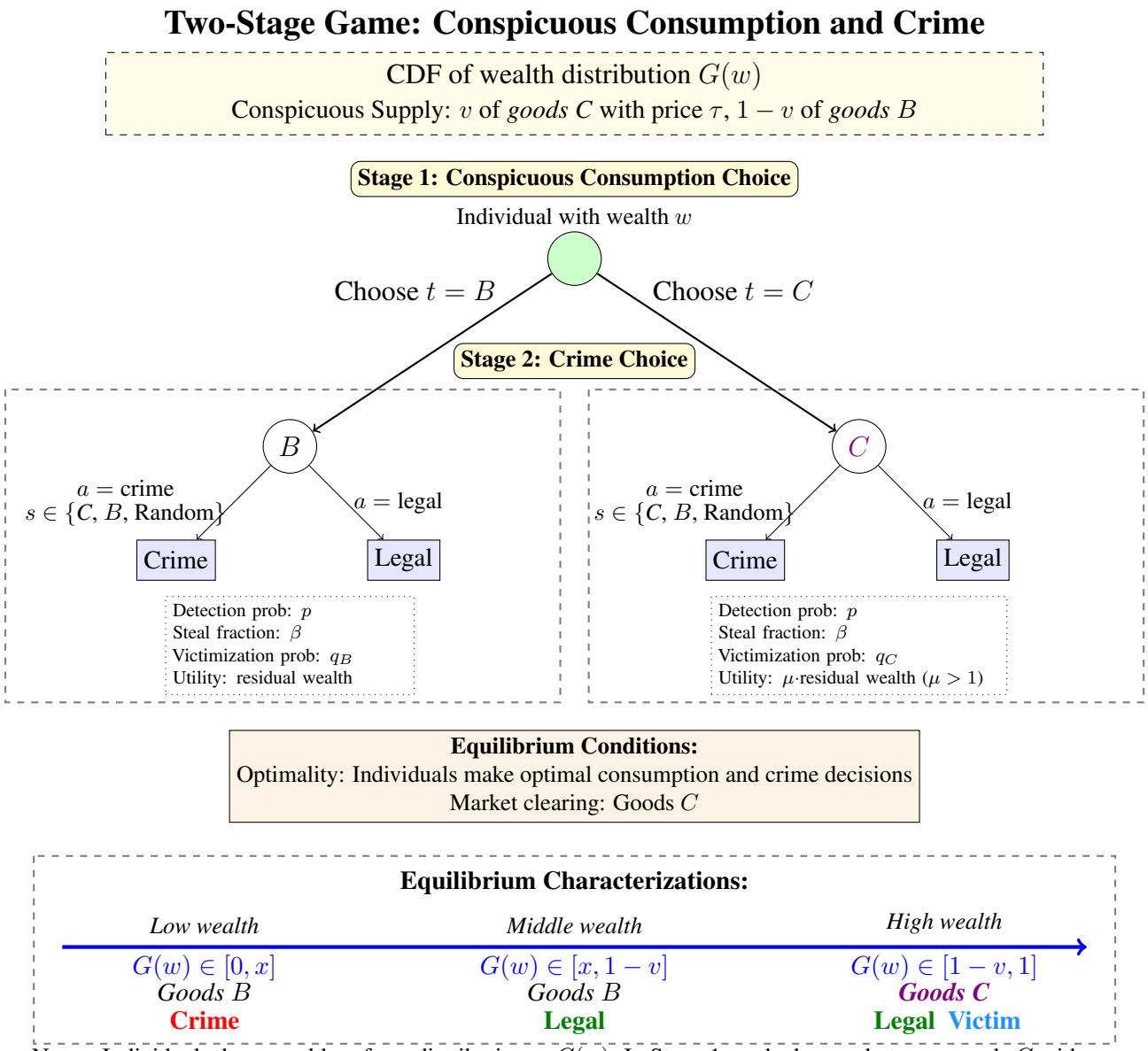
After these investment choices, we introduce a two-stage game of consumption and crime, following the conceptual framework of Chiu and Madden (1998). In the first stage, individuals with heterogeneous wealth levels w_i choose whether to purchase conspicuous goods that provide additional utility, while allocating their remaining wealth to basic consumption. Because such spending is publicly observable, conspicuous consumption also serves as a signal of affluence. In the second stage, individuals weigh the expected benefits and risks of crime, deciding both whether to offend and whom to target. The equilibrium features a clean stratification by wealth: the rich signal affluence through conspicuous consumption and remain law-abiding; the middle class consume modestly and abstain from crime; and the poorest consume modestly but may choose to offend. Figure 3 illustrates the timeline and structure of this two-stage interaction.

5.1.2 Conspicuous Consumption and Crime Decisions in a Two-Stage Game

Conspicuous Consumption Stage. A unit mass of goods is available, with a fraction $v \in (0, 1/2)$ labeled conspicuous (C) and the remainder basic (B). Each individual is matched one-to-one with a good and chooses $t \in \{C, B\}$. Normalize the cost of B to zero; the price premium for C is the endogenous amount τ. Utility reflects both the signaling value of the good and residual wealth: it equals $\mu \cdot m_C$ if $t = C$ and m_B if $t = B$, where $\mu > 1$ captures the added utility from conspicuous display. Market clearing requires exactly a fraction v of individuals to purchase C. Because choices are publicly observable, conspicuous goods serve as visible markers of affluence.

[49] This assumption rests on a simple intuition: individuals borrow to invest in housing only when the expected return exceeds the cost of financing. In periods of rapid price appreciation, such as China's housing boom, this condition is naturally satisfied, making leveraged investment in real estate an attractive choice.

Figure 3: Sequential Decision Tree



Notes: Individuals draw wealth w from distributionon $G(w)$. In Stage 1, each chooses between goods C with an endogenous price τ and B. The supply of goods C equals $v \in (0, 1/2)$. Consumption choices are publicly observable. In Stage 2, after observing goods, individuals decide whether to commit a property crime and select a victim type $s \in \{C, B, \text{Random}\}$. With probability p a crime is detected and the offender loses all wealth; otherwise, the offender appropriates a fraction β of the victim's residual wealth. In the separating equilibrium, high-wealth individuals ($G(w) \in [1-v, 1]$) choose C and do not offend; middle-wealth individuals ($G(w) \in [x, 1-v]$) choose B and do not offend; low-wealth individuals ($G(w) \in [0, x]$) choose B and offend. The equilibrium price τ clears the goods C market, and criminals target only individuals consuming C.

Crime Stage. Individuals then decide whether to engage in property crime, following the framework in Becker (1968). A potential offender chooses a target type $s \in \{C, B, \text{random}\}$, while each person can commit at most one crime and be victimized at most once. Consumption type is publicly observed, but wealth is private; only its distribution is known. Crime is detected with probability $p \in (0,1)$, in which case the offender forfeits all remaining wealth. If undetected, the offender obtains a fraction $\beta \in (0,1)$ of the victim's residual wealth after consumption. Let $a \in \{\text{legal}, \text{crime}\}$ denote the individual's activity choice, and let q_t be the endogenous probability of being victimized when consuming good $t \in \{C, B\}$. Expected utility is

$$U(a, s; t, w, \tau) = \begin{cases} \big(1 - \beta q_B\big) w & \text{if } a = \text{legal}, t = B \\ \mu\big(1 - \beta q_C\big)(w - \tau) & \text{if } a = \text{legal}, t = C \\ (1-p)\left\{\big(1 - \beta q_B\big) w + \beta \int m_s(w', r)\, dF_s(w')\right\} & \text{if } a = \text{crime}, t = B \\ \mu(1-p)\left\{\big(1 - \beta q_C\big)(w - \tau) + \beta \int m_s(w', r)\, dF_s(w')\right\} & \text{if } a = \text{crime}, t = C \end{cases}$$

where $m_s(w', r)$ denotes the residual wealth of a victim of type s with initial wealth w', and F_s is the corresponding distribution.

Equilibrium Characterization: The equilibrium of the two-stage game is defined by a set of behavioral rules and consistency conditions that jointly determine individuals' consumption choices, crime decisions, and market outcomes. Formally, an equilibrium consists of the following components: (1) a consumption choice function maps each wealth level to a consumption decision, determining which type of goods an individual purchases, $t \in \{C, B\}$; (2) a crime participation function assigns an activity choice, whether to offend or not, $a \in \{\text{legal}, \text{crime}\}$; (3) a victim selection strategy governs how criminals choose their targets based on observable consumption, $s \in \{C, B, \text{random}\}$; (4) a price premium τ for the conspicuous goods C adjusts to clear the market, ensuring that the demand matches supply. Each individual acts optimally given these strategies and all markets are clear in equilibrium. Following Chiu and Madden (1998), and under mild regularity conditions, the model yields a unique equilibrium characterized by complete separation across the wealth distribution. Appendix D.1 provides a complete set of formal definitions, proofs, and details.

The equilibrium features three distinct groups sorted by wealth, with the endogenous cutoff x directly determining the measure of individuals who engage in criminal activity, while v determines the measure of conspicuous consumers who are also potential victims. Individuals at the top, with $G(w) \in [1 - v, 1]$, purchase the conspicuous goods C, accepting a positive probability of victimization, $q(L) = x/v$. The marginal conspicuous consumer at $G(w) = 1 - v$ is indifferent between purchasing C and the alternative B, as the utility gain from signaling exactly offsets the

expected victimization cost. Individuals with intermediate wealth, $G(w) \in [x, 1-v]$, choose B, avoid being targeted, and refrain from criminal activity. At the lower end of the distribution, those with $G(w) \in [0, x]$ also consume B without risk of victimization, but choose to commit crimes, targeting only conspicuous consumers. The marginal offender at $G(w) = x$ is indifferent: the expected gain from crime equals the expected cost given detection probabilities. The price premium τ adjusts to clear the market for C by equating demand with the supply.

5.2 Comparative Statics: Housing, Inequality, and Crime

We then examine how an exogenous increase in housing asset returns—driven in our context by the HPR policies imposed in neighboring cities—translates into greater local wealth inequality and, ultimately, higher property crime rates. Let $G^{-1}(i)$ denotes the wealth at the i-th percentile of the distribution. We say that wealth distribution G_2 exhibits greater relative differential inequality than G_1 if, for all $i > j$ in $[0, 1]$, $G_2^{-1}(i)/G_2^{-1}(j) > G_1^{-1}(i)/G_1^{-1}(j)$. In other words, the ratio of wealth between any two percentiles is larger under G_2 than under G_1, reflecting a wider dispersion across the distribution.

Proposition 1. ***(Housing Returns Amplify Inequality)*** *An increase in housing asset returns from R_1 to R_2 shifts the wealth distribution from $G_1(w)$ to $G_2(w)$, where G_2 exhibits greater relative differential inequality than G_1.*

The formal proof of Proposition 1 is provided in Appendix D.2. Intuitively, higher housing returns disproportionately benefit those already holding substantial assets. The wealthy accumulate large capital gains, while individuals with little or no housing wealth see minimal improvement. This asymmetric effect stretches the distribution: the rich grow significantly richer, while the poor remain largely unaffected, amplifying relative inequality.

The spillover effects of the HPR policies generate substantial increases in housing asset returns in neighboring areas, thereby exacerbating wealth inequality. This widening wealth disparity raises the expected returns from targeting the wealthy as conspicuous consumption acts as a visible signal that facilitates criminal targeting. These contribute to an equilibrium increase in property crime rates. We formally summarize this logic in the following proposition:

Proposition 2. ***(Wealth Inequality Fuels Crime)*** *An increase in wealth inequality, as measured by greater relative differential inequality in the wealth distribution, leads to a higher equilibrium incidence of property crime.*

The proof of the proposition follows directly from Theorem 3 of Chiu and Madden (1998), which establishes that if wealth distribution G_2 exhibits greater relative differential inequality than G_1,

then the equilibrium crime rate under G_2 is strictly higher.[50] The mechanism can be understood as follows. Property crime takes place under imperfect information: potential offenders must form expectations about the likely payoff from targeting a victim. When inequality widens, visible signals like conspicuous consumption become more revealing of underlying wealth.[51] Because such goods are consumed disproportionately by the rich, a wider wealth gap sharpens the contrast between rich and poor. This raises the expected return from crime, making criminal activity more attractive in equilibrium and leading to higher crime rates.[52]

6 Conclusion

This paper identifies the causal effect of housing speculation on crime by exploiting spillovers from house purchase restrictions (HPR) in major Chinese cities. The analysis combines quasi-experimental variation with newly constructed measures of criminal activity derived from large-scale text analysis of court judgments across all levels of the judiciary. To our knowledge, this is the first study to establish a causal link between housing speculation and crime in China using judicial microdata.

Speculative housing booms significantly increase property crime but not violent crime. The effects are concentrated in financially motivated offenses—larceny, fraud, and high-value robbery—and are committed primarily by socioeconomically disadvantaged groups and first-time offenders. The evidence points to visible inequality as the key mechanism: housing booms widen wealth gaps and, through conspicuous consumption, make affluence more observable, thereby raising both the expected returns to crime and the ease of identifying high-value targets. In contrast, alternative channels, such as deprivation, psychological strain, migration, or weakened deterrence, find no support. The effects are markedly weaker in cities with stronger kinship ties and cultural institutions, underscoring the stabilizing role of social cohesion.

More broadly, these findings highlight a neglected consequence of speculative housing markets. When rising property values translate into visible inequality, they can erode social stability by turning wealth gaps into perceived opportunity. Managing housing speculation is thus not only a matter of price or financial regulation, it is also essential for maintaining social order in an era when inequality is increasingly visible.

[50] See Chiu and Madden (1998), Theorem 3, for the full derivation. In their model, greater relative differential inequality shifts the distribution of potential payoffs in a way that raises the marginal return to crime, thereby yielding a strictly higher equilibrium crime rate.

[51] As wealth distribution shifts from $G_1(w)$ to $G_2(w)$, the endogenous equilibrium price of C rises with a supply of v, resulting in higher expenditure.

[52] The result also accords with the incomplete-information framework of Mejía and Restrepo (2016), in which conspicuous consumption serves as a credible signal of wealth.

References

Aliprantis, Dionissi, and Daniel Hartley. 2015. "Blowing it up and knocking it down: The local and city-wide effects of demolishing high concentration public housing on crime." *Journal of Urban Economics*, 88: 67–81.

Attanasio, Orazio, and Luigi Pistaferri. 2014. "Consumption inequality over the last half century: Some evidence using the new PSID consumption measure." *American Economic Review*, 104(5): 122–126.

Autor, David H., Christopher J. Palmer, and Parag A. Pathak. 2019. "Ending Rent Control Reduced Crime in Cambridge." *AEA Papers and Proceedings*, 109: 381–84.

Axbard, Sebastian, Anja Benshaul-Tolonen, and Jonas Poulsen. 2021. "Natural resource wealth and crime: The role of international price shocks and public policy." *Journal of Environmental Economics and Management*, 110: 102527.

Banerjee, Abhijit V, and Esther Duflo. 2014. "Do firms want to borrow more? Testing credit constraints using a directed lending program." *Review of Economic Studies*, 81(2): 572–607.

Basco, Sergi, David Lopez-Rodriguez, and Enrique Moral-Benito. 2025. "House prices and misallocation: The impact of the collateral channel on productivity." *The Economic Journal*, 135(665): 1–35.

Bayer, Patrick, Kyle Mangum, and James W Roberts. 2021. "Speculative fever: Investor contagion in the housing bubble." *American Economic Review*, 111(2): 609–651.

Becker, Gary S. 1968. "Crime and punishment: An economic approach." *Journal of Political Economy*, 76(2): 169–217.

Bednarek, Peter, Daniel Marcel te Kaat, Chang Ma, and Alessandro Rebucci. 2021. "Capital flows, real estate, and local cycles: Evidence from German cities, banks, and firms." *The Review of Financial Studies*, 34(10): 5077–5134.

Bell, Brian, Francesco Fasani, and Stephen Machin. 2013. "Crime and immigration: Evidence from large immigrant waves." *Review of Economics and Statistics*, 95(4): 1278–1290.

Berger, David, Veronica Guerrieri, Guido Lorenzoni, and Joseph Vavra. 2018. "House prices and consumer spending." *The Review of Economic Studies*, 85(3): 1502–1542.

Bianchi, Milo, Paolo Buonanno, and Paolo Pinotti. 2012. "Do immigrants cause crime?" *Journal of the European Economic Association*, 10(6): 1318–1347.

Bourguignon, François. 2000. "Crime, violence and inequitable development." 199–220, World Bank Washington, DC.

Braakmann, Nils, Arnaud Chevalier, and Tanya Wilson. 2024. "Expected returns to crime and crime location." *American Economic Journal: Applied Economics*, 16(4): 144–160.

Britto, Diogo GC, Paolo Pinotti, and Breno Sampaio. 2022. "The effect of job loss and unemployment insurance on crime in Brazil." *Econometrica*, 90(4): 1393–1423.

Brown, Philip H, Erwin Bulte, and Xiaobo Zhang. 2011. "Positional spending and status seeking in rural China." *Journal of Development Economics*, 96(1): 139–149.

Buonanno, Paolo, and Juan F Vargas. 2019. "Inequality, crime, and the long run legacy of slavery." *Journal of Economic Behavior & Organization*, 159: 539–552.

Cameron, Lisa, Xin Meng, and Dandan Zhang. 2019. "China's sex ratio and crime: Behavioural change or financial necessity?" *The Economic Journal*, 129(618): 790–820.

Campello, Murillo, Robert A Connolly, Gaurav Kankanhalli, and Eva Steiner. 2022. "Do real estate values boost corporate borrowing? Evidence from contract-level data." *Journal of Financial Economics*, 144(2): 611–644.

Case, Anne, and Angus Deaton. 2021. *Deaths of Despair and the Future of Capitalism.* Princeton University Press.

Chang, Jeffery, Yuheng Wang, and Wei Xiong. 2025. "Price and volume divergence in China's real estate markets: the role of local governments." *The Review of Financial Studies*, hhaf089.

Charles, Kerwin Kofi, Erik Hurst, and Nikolai Roussanov. 2009. "Conspicuous consumption and race." *The Quarterly Journal of Economics*, 124(2): 425–467.

Chen, M Keith. 2013. "The effect of language on economic behavior: Evidence from savings rates, health behaviors, and retirement assets." *American Economic Review*, 103(2): 690–731.

Chen, Yunjie, Yongzheng Liu, and Hailun Wei. 2024*a*. "The unintended consequences of home-buying restriction in China." *Review of Economics and Statistics*, 1–45.

Chen, Zhiwu, Chicheng Ma, and Andrew J Sinclair. 2022. "Banking on the Confucian clan: Why China developed financial markets so late." *The Economic Journal*, 132(644): 1378–1413.

Chen, Zhiwu, Zhan Lin, and Xiaoming Zhang. 2024*b*. "Hedging desperation: How kinship networks reduced cannibalism in historical China." *Journal of Comparative Economics*, 52(2): 361–382.

Chen, Ziyang, Pierre-Philippe Combes, Sylvie Démurger, and Xiuyan Liu. 2025. "Housing Costs and Real Income Differences across Chinese Cities." *Journal of Development Economics*, 176: 103527.

Che, Yi, Xun Xu, and Yan Zhang. 2018. "Chinese import competition, crime, and government transfers in us." *Journal of Comparative Economics*, 46(2): 544–567.

Che, Yi, Yi Lu, Justin R Pierce, Peter K Schott, and Zhigang Tao. 2022. "Did trade liberalization with China influence US elections?" *Journal of International Economics*, 139: 103652.

Chiu, W Henry, and Paul Madden. 1998. "Burglary and income inequality." *Journal of Public Economics*, 69(1): 123–141.

Chyn, Eric. 2018. "Moved to opportunity: The long-run effects of public housing demolition on children." *American Economic Review*, 108(10): 3028–3056.

Cohen, Mark A, and Alex R Piquero. 2009. "New evidence on the monetary value of saving a high risk youth." *Journal of Quantitative Criminology*, 25(1): 25–49.

Coibion, Olivier, Yuriy Gorodnichenko, and Dmitri Koustas. 2021. "Consumption inequality and the frequency of purchases." *American Economic Journal: Macroeconomics*, 13(4): 449–482.

Coibion, Olivier, Yuriy Gorodnichenko, Lorenz Kueng, and John Silvia. 2017. "Innocent Bystanders? Monetary policy and inequality." *Journal of Monetary Economics*, 88: 70–89.

Cui, Lin, and Randall Walsh. 2015. "Foreclosure, vacancy and crime." *Journal of Urban Economics*, 87: 72–84.

Daysal, N Meltem, Michael Lovenheim, Nikolaj Siersbæk, and David N Wasser. 2021. "Home prices, fertility, and early-life health outcomes." *Journal of Public Economics*, 198: 104366.

DeFusco, Anthony A, Charles G Nathanson, and Eric Zwick. 2022. "Speculative dynamics of prices and volume." *Journal of Financial Economics*, 146(1): 205–229.

Demombynes, Gabriel, and Berk Özler. 2005. "Crime and local inequality in South Africa." *Journal of Development Economics*, 76(2): 265–292.

Deng, Yinglu, Li Liao, Jiaheng Yu, and Yu Zhang. 2022. "Capital spillover, house prices, and consumer spending: Quasi-experimental evidence from house purchase restrictions." *The Review of Financial Studies*, 35(6): 3060–3099.

Deng, Yingwen, Yongliang Wu, Changli Zeng, and Min Zhang. 2024. "The impact of Confucianism on auditor judgment." *Journal of Business Finance & Accounting*, 51(1-2): 398–430.

Dettling, Lisa J, and Melissa S Kearney. 2014. "House prices and birth rates: The impact of the real estate market on the decision to have a baby." *Journal of Public Economics*, 110: 82–100.

Diamond, Rebecca, and Tim McQuade. 2019. "Who wants affordable housing in their backyard? An equilibrium analysis of low-income property development." *Journal of Political Economy*, 127(3): 1063–1117.

Disney, Richard, John Gathergood, Stephen Machin, and Matteo Sandi. 2023. "Does homeownership reduce crime? A radical housing reform from the UK." *The Economic Journal*, 133(655): 2640–2675.

Di Tella, Rafael, and Ernesto Schargrodsky. 2004. "Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack." *American Economic Review*, 94(1): 115–133.

Dix-Carneiro, Rafael, Rodrigo R Soares, and Gabriel Ulyssea. 2018. "Economic shocks and crime: Evidence from the Brazilian trade liberalization." *American Economic Journal: Applied Economics*, 10(4): 158–95.

Dong, Baomin, Peter H Egger, and Yibei Guo. 2020. "Is poverty the mother of crime? Evidence from homicide rates in China." *PLoS One*, 15(5): e0233034.

Draca, Mirko, Stephen Machin, and Robert Witt. 2011. "Panic on the streets of London: Police, crime, and the July 2005 terror attacks." *American Economic Review*, 101(5): 2157–2181.

Draca, Mirko, Theodore Koutmeridis, and Stephen Machin. 2019. "The changing returns to crime: Do criminals respond to prices?" *The Review of Economic Studies*, 86(3): 1228–1257.

Du, Zaichao, and Lin Zhang. 2015. "Home-purchase restriction, property tax and housing price in China: A counterfactual analysis." *Journal of Econometrics*, 188(2): 558–568.

Edlund, Lena, Hongbin Li, Junjian Yi, and Junsen Zhang. 2013. "Sex ratios and crime: Evidence from China." *Review of Economics and Statistics*, 95(5): 1520–1534.

Ehrlich, Isaac. 1973. "Participation in illegitimate activities: A theoretical and empirical investigation." *Journal of Political Economy*, 81(3): 521–565.

Ellen, Ingrid Gould, Johanna Lacoe, and Claudia Ayanna Sharygin. 2013. "Do foreclosures cause crime?" *Journal of Urban Economics*, 74: 59–70.

Enamorado, Ted, Luis F López-Calva, Carlos Rodríguez-Castelán, and Hernán Winkler. 2016. "Income inequality and violent crime: Evidence from Mexico's drug war." *Journal of Development Economics*, 120: 128–143.

Eppinger, Peter, and Hong Ma. 2024. "Optimal ownership and firm performance: An analysis of China's FDI liberalization." *Review of Economics and Statistics*, 1–46.

Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza. 1998. *Determinants of crime rates in Latin America and the world: An empirical assessment.* World Bank Publications.

Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza. 2002*a*. "Inequality and violent crime." *The Journal of Law and Economics*, 45(1): 1–39.

Fajnzylber, Pablo, Daniel Lederman, and Norman Loayza. 2002*b*. "What causes violent crime?" *European Economic Review*, 46(7): 1323–1357.

Fang, Guanfu, and Liya Miao. 2025. "Robot and crime: Evidence from China." *World Development*, 188: 106921.

Fang, Hanming, Quanlin Gu, Wei Xiong, and Li-An Zhou. 2016. "Demystifying the Chinese housing boom." *NBER Macroeconomics Annual*, 30(1): 105–166.

Favilukis, Jack, and Stijn Van Nieuwerburgh. 2021. "Out-of-town home buyers and city welfare." *The Journal of Finance*, 76(5): 2577–2638.

Federico, Stefano, Fadi Hassan, and Veronica Rappoport. 2025. "Trade shocks and credit reallocation." *American Economic Review*, 115(4): 1142–1169.

Fishback, Price V, Ryan S Johnson, and Shawn Kantor. 2010. "Striking at the roots of crime: The impact of welfare spending on crime during the great depression." *The Journal of Law and Economics*, 53(4): 715–740.

Fougère, Denis, Francis Kramarz, and Julien Pouget. 2009. "Youth unemployment and crime in France." *Journal of the European Economic Association*, 7(5): 909–938.

Freedman, Maurice. 2021*a*. *Chinese Lineage and Society: Fukien and Kwantung Volume 33*. Routledge.

Freedman, Maurice. 2021*b*. *Lineage organisation in South-eastern China.* Routledge.

Gao, Zhenyu, Michael Sockin, and Wei Xiong. 2020. "Economic consequences of housing speculation." *The Review of Financial Studies*, 33(11): 5248–5287.

Glaeser, Edward L. 2013. "A nation of gamblers: Real estate speculation and American history." *American Economic Review*, 103(3): 1–42.

Gorback, Caitlin S, and Benjamin J Keys. 2020. "Global capital and local assets: House prices, quantities, and elasticities." *Working Paper*.

Gould, Eric D, Bruce A Weinberg, and David B Mustard. 2002. "Crime rates and local labor market opportunities in the United States: 1979–1997." *Review of Economics and Statistics*, 84(1): 45–61.

He, Ni, and Ineke H Marshall. 1997. "Social production of crime data: A critical examination of Chinese crime statistics." *International Criminal Justice Review*, 7(1): 46–64.

He, Zhiguo, Scott T Nelson, Yang Su, Anthony Lee Zhang, and Fudong Zhang. 2022. *Industrial land discount in China: a public finance perspective.* National Bureau of Economic Research.

Hicks, Daniel L, and Joan Hamory Hicks. 2014. "Jealous of the Joneses: Conspicuous consumption, inequality, and crime." *Oxford Economic Papers*, 66(4): 1090–1120.

Hu, Yushan, Penglong Zhang, and Guozhong Zhu. 2024. "Housing, health and life expectancy inequality in China." *Working Paper*.

İmrohoroğlu, Ayşe, Antonio Merlo, and Peter Rupert. 2000. "On the political economy of income redistribution and crime." *International Economic Review*, 41(1): 1–26.

Ioannides, Yannis M, and L Rachel Ngai. 2025. "Housing and Inequality." *Journal of Economic Literature*.

Ji, Xiaofeng. 1996. *A Dictionary of Academies in China.* Zhejiang Education Publishing House.

Johannesen, Niels, Patrick Langetieg, Daniel Reck, Max Risch, and Joel Slemrod. 2020. "Taxing hidden wealth: The consequences of us enforcement initiatives on evasive foreign accounts." *American Economic Journal: Economic Policy*, 12(3): 312–346.

Kaplan, Greg, Kurt Mitman, and Giovanni L Violante. 2020. "The housing boom and bust: Model meets evidence." *Journal of Political Economy*, 128(9): 3285–3345.

Kaus, Wolfhard. 2013. "Conspicuous consumption and "race": Evidence from South Africa." *Journal of Development Economics*, 100(1): 63–73.

Kelly, Morgan. 2000. "Inequality and crime." *Review of Economics and Statistics*, 82(4): 530–539.

Khalil, Umair, and Viviane Sanfelice. 2025. "Housing improvement and crime." *Journal of Public Economics*, 242: 105316.

Khanna, Gaurav, Carlos Medina, Anant Nyshadham, Christian Posso, and Jorge Tamayo. 2021. "Job Loss, Credit, and Crime in Colombia." *American Economic Review: Insights*, 3(1): 97–114.

Kirchmaier, Tom, Stephen Machin, Matteo Sandi, and Robert Witt. 2020. "Prices, policing and policy: The dynamics of crime booms and busts." *Journal of the European Economic Association*, 18(2): 1040–1077.

Kung, James Kai-sing, and Chicheng Ma. 2014. "Can cultural norms reduce conflicts? Confucianism and peasant rebellions in Qing China." *Journal of Development Economics*, 111: 132–149.

Lan, Xiaohuan, and Wei Li. 2018. "Swiss watch cycles: Evidence of corruption during leadership transition in China." *Journal of Comparative Economics*, 46(4): 1234–1252.

Levitt, Steven D. 1997. "Using electoral cycles in police hiring to estimate the effect of police on crime." *American Economic Review*, 87(3): 270–290.

Li, Jing, Guanghua Wan, Chen Wang, and Xueliang Zhang. 2019. "Which indicator of income distribution explains crime better? Evidence from China." *China Economic Review*, 54: 51–72.

Liu, Chang, and Wei Xiong. 2020. "China's Real Estate Market." In *The Handbook of China's Financial System*. Princeton University Press.

Liu, Qing, and Yi Lu. 2015. "Firm investment and exporting: Evidence from China's value-added tax reform." *Journal of International Economics*, 97(2): 392–403.

Liu, Ziqian, and Yu Zhang. 2024. "The Effect of House Prices on Fertility: Evidence from House Purchase Restrictions." *Working Paper*.

Li, Wanli, Xixiong Xu, and Zhineng Long. 2020. "Confucian culture and trade credit: Evidence from Chinese listed companies." *Research in International Business and Finance*, 53: 101232.

Londoño-Vélez, Juliana, and Javier Ávila-Mahecha. 2021. "Enforcing wealth taxes in the developing world: Quasi-experimental evidence from Colombia." *American Economic Review: Insights*, 3(2): 131–148.

Lovenheim, Michael F. 2011. "The effect of liquid housing wealth on college enrollment." *Journal of Labor Economics*, 29(4): 741–771.

Lu, Yi, Jin Wang, and Lianming Zhu. 2019. "Place-based policies, creation, and agglomeration economies: Evidence from China's economic zone program." *American Economic Journal: Economic Policy*, 11(3): 325–360.

Ma, Hong, Yu Pan, and Mingzhi (Jimmy) Xu. 2025. "The criminogenic consequence of export slowdown: evidence from millions of court judgement documents in China." *The Economic Journal*, 135(671): 2242–2275.

Martín, Alberto, Enrique Moral-Benito, and Tom Schmitz. 2021. "The financial transmission of housing booms: evidence from Spain." *American Economic Review*, 111(3): 1013–1053.

McKenzie, David. 2017. "Identifying and spurring high-growth entrepreneurship: Experimental evidence from a business plan competition." *American Economic Review*, 107(8): 2278–2307.

McMillen, Daniel, Ignacio Sarmiento-Barbieri, and Ruchi Singh. 2019. "Do more eyes on the street reduce Crime? Evidence from Chicago's safe passage program." *Journal of Urban Economics*, 110: 1–25.

Mejía, Daniel, and Pascual Restrepo. 2016. "Crime and conspicuous consumption." *Journal of Public Economics*, 135: 1–14.

Melander, Eric, and Martina Miotto. 2023. "Welfare cuts and crime: Evidence from the New Poor Law." *The Economic Journal*, 133(651): 1248–1264.

Mello, Steven. 2019. "More COPS, less crime." *Journal of Public Economics*, 172: 174–200.

Merton, Robert K. 1938. "Social structure and anomie." *American Sociological Review*, 3(5): 672–682.

Mian, Atif, and Amir Sufi. 2022. "Credit supply and housing speculation." *The Review of Financial Studies*, 35(2): 680–719.

Miller, Ted R., Mark A. Cohen, and Brian Wiersema. 1996. "Victim costs and consequences: A new look." National Institute of Justice, Washington, DC.

Painter, Gary, Xi Yang, and Ninghua Zhong. 2022. "Housing wealth as precautionary saving: Evidence from urban China." *Journal of Financial and Quantitative Analysis*, 57(2): 761–789.

Piketty, Thomas. 2014. *Capital in the twenty-first century.* Harvard University Press.

Piketty, Thomas, and Gabriel Zucman. 2014. "Capital is back: Wealth-income ratios in rich countries 1700–2010." *The Quarterly Journal of Economics*, 129(3): 1255–1310.

Prochaska, Judith J, Hai-Yen Sung, Wendy Max, Yanling Shi, and Michael Ong. 2012. "Validity study of the K6 scale as a measure of moderate mental distress based on mental health treatment need and utilization." *International Journal of Methods in Psychiatric Research*, 21(2): 88–97.

Raphael, Steven, and Rudolf Winter-Ebmer. 2001. "Identifying the effect of unemployment on crime." *The Journal of Law and Economics*, 44(1): 259–283.

Rogoff, Kenneth S, and Yuanchen Yang. 2024. "A tale of tier 3 cities." *Journal of International Economics*, 152: 103989.

Sandler, Danielle H. 2017. "Externalities of public housing: The effect of public housing demolitions on local crime." *Regional Science and Urban Economics*, 62: 24–35.

Shanghai Library. 2009. "Zhongguo jiapu zongmu."

South, Eugenia C, John MacDonald, and Vincent Reina. 2021. "Association between structural housing repairs for low-income homeowners and neighborhood crime." *JAMA Network Open*, 4(7): e2117067–e2117067.

Stack, Steven. 1984. "Income inequality and property crime: A cross-national analysis of relative deprivation theory." *Criminology*, 22(2): 229–256.

Stiglitz, Joseph E. 2012. *The price of inequality: How today's divided society endangers our future.* WW Norton & Company.

Street, Brittany. 2025. "The impact of economic opportunity on criminal behavior: Evidence from the fracking boom." *Journal of Public Economics*, 248: 105402.

Stroebel, Johannes, and Joseph Vavra. 2019. "House prices, local demand, and retail prices." *Journal of Political Economy*, 127(3): 1391–1436.

Sun, Danyang. 2018. "Literature Review on Importing Tax Policy of China's Luxury Goods." *Modern Economy*, 9(3): 510–527.

Tan, Jing, Hao Xu, and Jingwen Yu. 2022. "The effect of homeownership on migrant household savings: Evidence from the removal of home purchase restrictions in China." *Economic Modelling*, 106: 105679.

Tian, Xuan, and Yichu Wang. 2025. "Housing speculation and entrepreneurship." *Review of Finance*. Forthcoming.

Vilagut, Gemma, Carlos G Forero, Gabriela Barbaglia, and Jordi Alonso. 2016. "Screening for depression in the general population with the Center for Epidemiologic Studies Depression (CES-D): A systematic review with meta-analysis." *PloS One*, 11(5): e0155431.

Warby, Brian, and Nicholas Bailey. 2025. "Are populist leaders playing the public? Examining the effect of populism on common and luxury good imports." *Political Research Exchange*, 7(1): 2476407.

Wilson, James Q, and George L Kelling. 1982. "Broken windows." *Atlantic Monthly*, 249(3): 29–38.

Xu, Xixiong, Yaoqin Li, Xing Liu, and Weiyu Gan. 2017. "Does religion matter to corruption? Evidence from China." *China Economic Review*, 42: 34–49.

Yu, Olivia, and Lening Zhang. 1999. "The under-recording of crime by police in China: A case study." *Policing: An International Journal of Police Strategies & Management*, 22(3): 252–264.

Zhang, Shiying, and Peng Zhang. 2023. "The Dark Side of Automation: Robot and Crime." *Working Paper*.

Zhang, Zheyuan, Hui Xu, Ruilin Liu, and Zhong Zhao. 2025. "Free education and the intergenerational transmission of cognitive skills in rural China." *Journal of Population Economics*, 38(1): 1–30.

Zhao, Renjie, and Jiakai Zhang. 2022. "Rent-tax substitution and its impact on firms: Evidence from housing purchase limits policy in China." *Regional Science and Urban Economics*, 96(103804).

Online Appendix

A Table Appendix

Table A.1: First Round of the House Purchase Restriction Policy

City	Policy Shock	Data Effective
Beijing	• Raise the down payment: from 35% to 40% for the first house; from 35% to 50%-70% for the second house.	2016.9.30
Changsha	• Price-cap regulation: the average transaction price cannot increase further.	2016.11.25
Chengdu	• Raise the down payment: from 35% to 40% for the second house.	2016.10.9
Fuzhou	• Raise the down payment: to 30% for the second house	2016.10.14
Guangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.1
Haikou	N/A	N/A
Hangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house in city center areas. • Raise the down payment: from 30%-40% to 50% for the second house.	2016.9.20
Hefei	• Restrictions on resident purchases: cannot own more than 2 houses. • Raise the down payment: to 40%-50% for the second house.	2016.10.1
Huizhou	N/A	N/A
Jinan	• Raise the down payment: from 20% to 30% for the first house; from 20% to 30%-40% for the second house.	2016.10.2
Nanchang	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.8
Nanjing	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.9.25
Qingdao	N/A	N/A
Sanya	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses.	2016.10.1
Shanghai	• Decrease credit supply (by rationing).	2016.10.19
Shenzhen	• Restrictions on purchases: cannot own more than 1 house. • Raise the down payment: to 30%-50% for the first house.	2016.10.4
Shijiazhuang	• Raise the land tax: to 3% for the second house.	2016.10.1
Tianjin	• Restrictions on non-resident purchases: cannot own more than 1 house. • Raise the down payment for non-residents: to 40% for the first house purchase.	2016.9.30
Wuhan	• Restrictions on non-resident purchases: cannot own more than 1 house. • Raise the down payment: to 25% for the first house; to 50% for the second house.	2016.10.3
Wuxi	• Raise the down payment: to 40% for the second house.	2016.10.2
Xiamen	• Restrictions on non-resident purchases: those who own 1 house can only purchase additional houses with areas larger than 180 m^2. • Restrictions on resident purchases: those who own 2 houses can only purchase additional houses with areas larger than 180 m^2. • Raise down payment: to 30% for the first house; to 40% for the second house.	2016.10.5
Zhengzhou	• Restrictions on non-resident purchases: those who own 1 house can only purchase additional houses with areas larger than 180 m^2. • Restrictions on resident purchases: those who own 2 houses can only purchase additional houses with areas larger than 180 m^2. • Raise down payment: to 30% for the first house; to 40% for the second house.	2016.10.2

Notes: This table summarizes the first-round house purchase restriction policies implemented in 19 regulated cities and year 2016, as in Deng et al. (2022) and Liu and Zhang (2024). Policy information is sourced from city government announcements and the China Index Academy (a China's real estate market information provider). The restrictions on resident and non-resident purchases mentioned above are at the household level. Non-residents need to satisfy local residency and tax payment requirements for a certain period before qualifying for home purchases.

Table A.2: Second Round of the House Purchase Restriction Policy

City	Policy Shock	Data Effective
Beijing	• Raise the down payment: to 60%-80% for the second house. • Decrease credit supply: stop providing mortgages lasting longer than 25 years.	2017.3.17
Changsha	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resident purchases: cannot own more than 2 houses. • Raise the down payment: to 30% for the first house; to 35%-40% for the second house.	2017.3.18
Chengdu	• Restrictions on purchases: each household can only own 1 house.	2017.3.23
Fuzhou	• Raise the down payment: to 50% for the second house. • Restrictions on resale: owner needs to hold a house for 2 years before resale.	2017.3.28
Guangzhou	• Raise the down payment: from 30% to 40%-70% for households that ever applied for mortgages.	2017.3.17
Haikou	• Restrictions on non-resident purchases: cannot own more than 1 house. • Restrictions on resale: owner needs to hold a house for 2 years before resale.	2017.4.14
Hangzhou	• Restrictions on non-resident purchases: cannot own more than 1 house in the city area. • Restrictions on resident purchases: cannot own more than 2 houses in the city area.	2017.3.3
Hefei	• Increase mortgage rate by 10%.	2017.3.20
Huizhou	• Increase mortgage rate by 10%.	2017.3.20
Jinan	• Raise the down payment: to 60% for the second house. • Increase the mortgage rate by 10%. • Restrictions on resale: owner needs to hold a house for 2 years before resale.	2017.4.19
Nanchang	• Restrictions on non-resident purchases: raise the criteria for the purchases. • Restrictions on resident purchases: cannot own more than 1 house.	2017.3.8
Nanjing	• Restrictions on non-resident purchases: raise the criteria for the purchases. • Raise the down payment: from 30%-40% to 50% for the second house.	2017.3.15
Qingdao	• Raise the down payment: from 20% to 30% for the first house; from 30% to 40% for the second house.	2017.3.16
Sanya	• Raise the down payment: from 30%-40% to 50% for the second house.	2017.3.11
Shanghai	• Decrease credit supply (by stricter rationing).	2016.3.17
Shenzhen	• Increase mortgage rate by 10%.	2017.3.20
Shijiazhuang	• Raise the down payment: to 30%-40% for the first house; to 50%-60% for the second house.	2017.3.17
Tianjin	• Restrictions on non-resident purchases: raise the criteria for the purchases. • Restrictions on resident purchases: each individual cannot own more than 1 house. • Raise the down payment for non-residents: to 40% for the first house purchase.	2017.3.31
Wuhan	• Increase mortgage rate by 10%.	2017.3.20
Wuxi	• Increase mortgage rate by 10%.	2017.3.20
Xiamen	• Restrictions on resident purchases: an individual can only own 1 house.	2017.3.24
Zhenzhou	• Restrictions on non-resident purchases: raise the criteria for the purchases.	2017.3.17

Notes: This table summarizes the second-round house purchase restriction policies implemented in 22 regulated cities and year 2017, as in Deng et al. (2022) and Liu and Zhang (2024). Policy information is sourced from city government announcements and the China Index Academy (a China's real estate market information provider). The restrictions on resident and non-resident purchases mentioned above are at the household level. Non-residents need to satisfy local residency and tax payment requirements for a certain period before qualifying for home purchases.

Table A.3: Keywords in Criminal Case Classification

Types	Chinese	English
1 Stealing	盗窃	stealing a relatively large amount of public or private property or committing theft repeatedly
	盗掘	excavating a site of ancient culture or ancient tomb of historical, artistic or scientific value
	盗伐	stealthily felling trees, bamboo, etc. in forest or woods
2 Defraud	诈骗	swindling public or private property
3 Robbery	抢劫	robbing public or private property
	抢夺	forcibly seizing public or private property
	聚众哄抢	gathering people to forcibly seize public or private property
4 Other property	敲诈勒索	extorting public or private money or property by blackmail
	侵占	unlawfully taking possession of another person's money or property under his custody and refusing to return it
	挪用资金，挪用公款	misappropriating the funds of his own unit for personal use or for loaningthem to another person
	挪用特定款物	misappropriating of funds or materials that are allocated for specific purposes
	故意毁坏财物	intentionally destroying or damaging public or private money or property
	破坏生产经营	sabotaging production and business operation
5 Violence	故意杀人	intentionally committing homicide
	故意伤害	intentionally inflicting injury upon another person
	过失致人死亡	negligently causing death to another person
	过失致人重伤	negligently inflicting injury upon another person
	强奸	raping a woman or a child by violence, coercion or any other means
	猥亵	acting indecently against a woman or a child by violence, coercion or any other forcible means
	拐卖妇女，儿童	abducting and trafficking in a woman or a child
	收买被拐卖的妇女，儿童	purchasing trafficked women and children
	聚众阻碍解救被收买的妇女，儿童	organizing crowds to obstruct the rescue of purchased women and children
	绑架	kidnapping another person
6 Public security	放火	arson
	失火	negligent arson
	决水	breaching a dike
	爆炸	causing explosion
	投放危险物质	spreading dangerous materials
	以危险方法危害公共安全	using other dangerous means to endanger public security
	破坏，损坏交通工具	sabotaging transportation vehicles
	破坏，损坏交通设施	sabotaging transportation facility
	破坏，损坏电力设备	sabotaging any electric power or gas facility
	破坏，损坏易燃易爆设备	sabotaging any inflammable or explosive equipment
	恐怖组织	terrorist organization
	恐怖活动	terrorist activities
	恐怖主义	terrorism
	极端主义	extremism
	劫持航空器	hijacking an aircraft
	劫持船只	hijacking a ship
	劫持汽车	hijacking a motor
	暴力危及飞行安全	using violence against any person on board an aircraft and thereby endangers air safety
	破坏，损坏广播电视设施, 公用电信设施	sabotaging any broadcasting, television or public telecommunications facility
	重大飞行事故	causing a grave air accident
	铁路运营安全事故	causing a railway operational accident
	重大责任事故	major liability accidents
	强令，组织他人违章冒险作业，危险作业	compelling or organizing others to engage in unsafe operations in violation of safety regulations
	重大劳动安全事故	major workplace safety accidents
	大型群众性活动重大安全事故	major safety accidents at large-scale public events
	危险物品肇事	violating the regulations on the control of dangerous materials and thereby causing a serious accident

Types	Chinese	English
	工程重大安全事故	lowering the quality standard of a construction project and thereby causing a serious accident
	教育设施重大安全事故	failing to adopt measures against dangers in educational facilities and thereby causing a serious accident
	消防责任事故	violating the regulations on fire prevention and causing a serious accident
	不报, 谎报安全事故	not reporting or not truly reporting when accident happens
	非法制造, 买卖, 运输, 邮寄, 储存枪支, 弹药, 爆炸物	illegally manufacturing, trading in, transporting, mailing or storing any guns, ammunition or explosives
	非法制造, 买卖, 运输, 储存危险物质	illegally manufacturing, trading in, transporting, mailing or storing any dangerous materials
	违规制造, 销售枪支	violating the regulations on manufacturing or selling guns
	非法持有, 私藏枪支, 弹药	illegally possessing or concealing any guns or ammunition
	非法出租, 出借枪支	illegally leasing or loaning guns
	丢失枪支不报	failing to report about the lost guns
	非法携带枪支, 弹药, 管制刀具, 危险物品危及公共安全	illegally entering a public place with any gun, ammunition, controlled cutting tool or any other dangerous tools and thereby endangering public security
7 Traffic felony	交通肇事	violating regulations governing traffic and transportation and thereby causing a serious accident
	危险驾驶	driving dangerously
	妨害安全驾驶	interfering with safe driving
8 Drugs	毒品	drugs related
	非法提供麻醉药品	illegal provision of narcotic drugs
	非法提供精神药品	illegal provision of psychotropic drugs
	妨害兴奋剂管理	interfering with doping control management
9 Prostitution	聚众淫乱	gathering to engage in licentious activities
	组织卖淫	arranging for another to engage in prostitution
	强迫卖淫	forcing another person to engage in prostitution
	引诱, 容留, 介绍卖淫	luring other persons into or sheltering prostitution or procuring other persons to engage in prostitution
	引诱幼女卖淫	luring a girl under the age of 14 to engage in prostitution
	卖淫, 嫖娼	prostitution
	传播性病	spreading serious venereal diseases such as syphilis and gonorrhea
	淫秽	pornography
10 Gambling	赌博	gambling

Table A.4: Summary Statistics for Different Types of Crime

Crime Type	Number	Percentage
Property	1,065,466	73.48%
Larceny	786,409	54.24%
Defraud	168,517	11.62%
Robbery	59,869	4.13%
Other property	60,817	4.19%
Violence	384,503	26.52%

Notes: The table lists the number and share of different types of criminal cases in our sample. We consider two main categories of criminal cases (property and violence) by matching the keywords in the case titles and contents. The full list of keywords we use and their translations are reported in Appendix Table A.3. The shares does not add up to 100% since some cases involve multiple offenses.

Table A.5: Summary Statistics: Variables Used in Regressions

	Obs	Mean	Std	Min	Max
Panel A: Baseline Variables					
log House price	1513	8.473	0.369	7.496	10.054
log Number of criminal cases per thou. population	1614	-1.890	0.493	-4.339	-0.377
log Number of larceny cases per thou. population	1614	-2.594	0.580	-5.255	-0.763
log Number of fraud cases per thou. population	1614	-4.103	0.620	-6.935	-1.938
log Number of robbery cases per thou. population	1580	-5.333	0.827	-8.512	-2.989
log Number of other property cases per thou. population	1599	-5.115	0.705	-8.488	-3.518
log Number of violence cases per thou. population	1614	-3.168	0.575	-6.213	-1.655
log Number of criminals per thou. population	1614	-1.617	0.514	-4.339	-0.071
log Number of criminals committing larceny per thou. population	1614	-2.314	0.576	-5.255	-0.531
log Number of criminals committing fraud per thou. population	1614	-3.741	0.686	-6.450	-1.656
log Number of criminals committing robbery per thou. population	1580	-4.998	0.903	-8.512	-2.539
log Number of criminals involving other property crimes per thou. population	1599	-4.704	0.812	-8.488	-2.674
log Number of criminals committing violence per thou. population	1614	-2.985	0.602	-5.877	-1.347
log Number of criminal acts per thou. population	1614	-1.113	0.521	-3.646	0.551
log Claim amount of property cases	1614	17.352	1.629	12.122	27.102
log GDP per capita	1614	10.726	0.506	7.806	12.281
log Permanent population	1614	5.791	0.676	2.953	8.043
log Fiscal revenue	1614	13.903	0.888	11.509	16.936
log Housing investment	1614	13.807	1.154	7.984	17.296
Population share of college enrollment (%)	1614	1.553	1.919	$1.27*10^{-7}$	13.112
log Number of criminals without prior records per thou. population	1614	-2.527	0.570	-5.948	-0.824
log Number of criminals with prior records per thou. population	1614	-2.843	0.599	-5.255	-0.973
log Number of low-educated criminals per thou. population	1614	-2	0.557	-5.255	-0.244
log Number of high-educated criminals per thou. population	1594	-5.089	0.804	-8.385	-1.878
log Number of young criminals per thou. population	1607	-3.659	0.722	-6.646	-1.324
log Number of middle-aged per thou. population	1614	-2.472	0.571	-4.850	-0.643
log Number of old criminals per thou. population	1611	-3.547	0.565	-5.779	-1.860
log Number of male criminals per thou. population	1614	-2.031	0.557	-4.850	-0.224
log Number of female criminals per thou. population	1610	-4.556	0.674	-7.563	-2.808
log Number of criminals with top 3 surnames per thou. population	1542	-3.459	0.524	-5.799	-1.805
log Number of criminals with non-top 3 surnames per thou. population	1542	-2.224	0.569	-4.196	-0.372
log Number of local criminals per thou. population	1613	-2.548	0.558	-5.661	-1.282
log Number of non-local criminals per thou. population	1614	-2.901	0.773	-5.183	-0.416
Panel B: Variables used in mechanism analysis					
log Housing asset	47766	11.838	1.441	4.605	18.198
log Financial asset (hyperbolic sine transformation)	52642	7.544	4.891	0	18.270
log Financial investment income (hyperbolic sine transformation)	51285	0.124	1.073	0	14.509
log Property income (hyperbolic sine transformation)	52411	1.161	2.971	0	14.519
Property income share	49988	0.011	0.045	0	0.444
log Household wealth except for housing asset	51748	10.679	1.613	-8.436	17.729
Gini index of housing asset	600	0.519	0.096	0.262	0.895
Gini index of household wealth	600	0.536	0.086	0.282	0.883
Gini index of household property income	477	0.467	0.132	0.048	0.792
Income gap	86660	2.553	0.897	1	4
Status gap	92063	2.315	0.879	1	4
Social security	86832	6.168	2.404	1	10
log Conspicuous consumption	50781	7.986	1.570	0	15.322
log Conspicuous consumption per capita	44022	7.276	1.500	-0.693	14.459
Depression or not	109749	0.248	0.432	0	1
Happiness	60656	7.425	2.147	0	10
Future confidence	99608	3.997	1.014	1	5
Word test scores	33229	18.934	10.535	0	34
Math test scores	33521	8.562	5.827	0	24
Employment status	79695	0.982	0.131	0	1
log Main job wage	33781	9.952	1.039	0	13.816
log total employment income	36580	9.912	1.121	0	16.148
log Household real income per capita	39978	9.664	1.138	0.916	15.554
log Household real income per capita (net mortgage)	39373	9.643	1.133	0.916	15.554
log Government public budget revenue	1614	13.903	0.888	11.509	16.936
log Government public budget expenditure	1614	14.905	0.595	12.304	17.697
log Government managed fund revenue	1608	13.188	1.354	7.689	16.958
log Government managed fund expenditure	1608	13.334	1.276	6.071	17.103
log Public security expenditure	1349	21.170	0.677	19.195	24.014
log Social spending expenditure	1032	23.752	0.599	21.654	26.511
log Stability expenditure	1007	23.820	0.595	21.747	26.590

Notes: The table reports the summary statistics of variables we use in the empirical analysis. We report the number of observations, the average and the standard deviation, and the minimum and the maximum values across prefectures and years.

Table A.6: Event Study Estimates of the HPR Spillover Effect on House Prices and Crime Rates

	House price	Property & violent crimes		Property crime	
	(1) log House price	(2) log #Case per thousand	(3) log #Criminal per thousand	(4) log #Case per thousand	(5) log #Criminal per thousand
$\text{Treat}_c \times \mathbb{I}_{t=2013}$	0.003	0.008	0.006	0.007	-0.001
	(0.011)	(0.034)	(0.035)	(0.037)	(0.038)
$\text{Treat}_c \times \mathbb{I}_{t=2014}$	0.0001	0.007	0.004	0.009	0.005
	(0.006)	(0.019)	(0.021)	(0.020)	(0.023)
$\text{Treat}_c \times \mathbb{I}_{t=2016}$	0.018**	0.023	0.038**	0.042**	0.058***
	(0.007)	(0.016)	(0.018)	(0.019)	(0.020)
$\text{Treat}_c \times \mathbb{I}_{t=2017}$	0.091***	0.065**	0.080***	0.098***	0.109***
	(0.014)	(0.025)	(0.027)	(0.030)	(0.031)
$\text{Treat}_c \times \mathbb{I}_{t=2018}$	0.150***	0.041	0.053	0.079**	0.086**
	(0.018)	(0.032)	(0.034)	(0.036)	(0.037)
$\text{Treat}_c \times \mathbb{I}_{t=2019}$	0.140***	0.115***	0.124***	0.174***	0.170***
	(0.020)	(0.044)	(0.048)	(0.048)	(0.051)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,850	1,883	1,883	1,883	1,883
R-squared	0.955	0.882	0.878	0.880	0.872

Notes: This table reports estimates from an event study analysis of the impact of HPR spillovers on house prices and crime rates. The unit of observation is the city-year from 2013 to 2019. The dependent variables (all in log) are house price index in column (1), the number of total property and violent crime cases per thousand population in column (2), the number of property crime cases per thousand in column (3), the number of total offenders involved in property and violent crimes per thousand in column (4), and the number of property crime offenders per thousand in column (5). Treat_c is an indicator equal to one if the city is located within 250 km of the nearest regulated city. $\mathbb{I}_{t=k}$ denotes a year-specific dummy for year k. All regressions control for city fixed effects, year fixed effects, and a set of time-varying city-level controls. Robust standard errors, clustered at the city level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.7: Robustness: Continuous-distance Specification and Alternative Distance Cut-offs

	Continuous		300 km		200 km	
	(1) log #Case per thousand	(2) log #Criminal per thousand	(3) log #Case per thousand	(4) log #Criminal per thousand	(5) log #Case per thousand	(6) log #Criminal per thousand
Panel A: Property & violent crimes						
$\ln(\text{Distance}_c) \times \text{Post}_t$	-0.040**	-0.046**				
	(0.016)	(0.018)				
$\text{Treat}_c \times \text{Post}_t$			0.066**	0.081***	0.044*	0.057**
			(0.026)	(0.028)	(0.024)	(0.026)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614	1,614	1,614
R-squared	0.908	0.903	0.907	0.903	0.907	0.903
Panel B: Property crime						
$\ln(\text{Distance}_c) \times \text{Post}_t$	-0.057***	-0.059***				
	(0.018)	(0.020)				
$\text{Treat}_c \times \text{Post}_t$			0.107***	0.116***	0.074***	0.084***
			(0.028)	(0.030)	(0.026)	(0.028)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614	1,614	1,614
R-squared	0.905	0.899	0.905	0.899	0.904	0.898

Notes: The unit of observation is at the city-year level from 2014 to 2019. The dependent variables (all in log) are the number of criminal cases per thousand population in the odd-numbered columns, and the number of criminals per thousand population in the even-numbered columns. Panel A considers property and violent crimes, while panel B focuses on property crime only. In columns (1) and (2), treatment intensity is captured continuously using $\log(\text{Distance}_c)$, where Distance_c denotes the distance to the nearest regulated city, instead of the baseline binary treatment indicator. In columns (3) and (4), cities located within 300 km of the nearest regulated city are designated as treated, and those beyond serve as the comparison group. Columns (5) and (6) apply a 200 km cutoff. The variable Post_t is an indicator equal to one for years 2016 and later. All regressions include time-varying city-level controls, city fixed effects, and year fixed effects. Robust standard errors clustered at the city level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.8: Robustness: Alternative Control Strategies for Crime Outcomes

	Initial×Post_t		Initial×Year FE	
	(1) log #Case per thousand	(2) log #Criminal per thousand	(3) log #Case per thousand	(4) log #Criminal per thousand
Panel A: Property & violent crimes				
$\text{Treat}_c \times \text{Post}_t$	0.059**	0.071***	0.059**	0.071***
	(0.024)	(0.025)	(0.024)	(0.025)
City FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614
R-squared	0.909	0.905	0.918	0.913
Panel B: Property crime				
$\text{Treat}_c \times \text{Post}_t$	0.087***	0.097***	0.087***	0.097***
	(0.026)	(0.027)	(0.026)	(0.027)
City FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614
R-squared	0.907	0.901	0.915	0.908

Notes: This table assesses the robustness of the estimated effects to alternative approaches addressing potential endogeneity of time-varying controls ("bad controls"). The sample includes city-year observations from 2014 to 2019. The dependent variables (all in log) are the number of criminal cases per thousand population (columns 1 and 3) and the number of criminals per thousand population (columns 2 and 4). Panel A considers property and violent crimes, while panel B focuses on property crime only. Treat_c is an indicator equal to one for cities located within 250 kilometers of the nearest regulated city. Post_t is a post-policy dummy equal to one for years 2016 and after. Columns (1) and (2) interact baseline (2014) city-level characteristics with the post-policy dummy, following Federico et al. (2025) and Che et al. (2022). Columns (3) and (4) interact baseline controls with year fixed effects instead, following Eppinger and Ma (2024) and Lu et al. (2019). All regressions include city and year fixed effects. Standard errors clustered at the city level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.9: Robustness: Alternative Crime Measures and Specifications

	Alternative crime measures		$\text{Post}_{2016} = 1/3$		PSM-DiD	
	(1) log #Incident per thousand	(2) log Amount per thousand	(3) log #Case per thousand	(4) log #Criminal per thousand	(5) log #Case per thousand	(6) log #Criminal per thousand
Panel A: Property & violent crimes						
$\text{Treat}_c \times \text{Post}_t$	0.089***	0.102**	0.052**	0.065**	0.042*	0.055**
	(0.029)	(0.047)	(0.025)	(0.027)	(0.023)	(0.024)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614	1,524	1,524
R-squared	0.886	0.831	0.907	0.903	0.913	0.911
Panel B: Property crime						
$\text{Treat}_c \times \text{Post}_t$	0.114***	0.102**	0.090***	0.098***	0.075***	0.082***
	(0.031)	(0.047)	(0.027)	(0.029)	(0.026)	(0.027)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614	1,524	1,524
R-squared	0.878	0.831	0.904	0.898	0.910	0.907

Notes: This table presents robustness checks using alternative outcome measures and estimation specifications. The unit of observation is the city-year from 2014 to 2019. The dependent variables (all in log) are: the number of criminal incidents per thousand population (column 1); total monetary value involved in property crimes per thousand population (column 2, winsorized at 99.9th percentile to avoid outliers); the number of criminal cases per thousand population (columns 3 and 5); and the number of offenders per thousand population (columns 4 and 6). Panel A considers property and violent crimes, while panel B focuses on property crime only. Treat_c is an indicator equal to one if the city is within 250 km of the nearest regulated city. Post_t is a post-treatment indicator equal to one for years 2016 and after (columns 1, 2, 5, and 6), while Post_{2016} is coded as 1/3 to reflect partial treatment timing in 2016 (columns 3 and 4), following Liu and Lu (2015). In column (2), we additionally control for the share of cases with claim amounts exceeding 1 million RMB to further mitigate the influence of extreme observations. Columns 5 and 6 use a PSM-DiD approach to restrict the control group to more comparable cities. All regressions include city fixed effects, year fixed effects, and time-varying city-level controls. Standard errors are clustered at the city level and reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.10: Robustness: House Prices and Other Crime Types

Dep. Var.:	log #Case per thousand				log #Criminal per thousand			
	(1) Public security	(2) Traffic felony	(3) Drugs, Prostitution and gamble	(4) Others	(5) Public security	(6) Traffic felony	(7) Drugs, Prostitution and gamble	(8) Others
$\text{Treat}_c \times \text{Post}_t$	-0.022	-0.002	0.043	-0.018	-0.021	0.0002	0.043	-0.025
	(0.052)	(0.047)	(0.048)	(0.036)	(0.055)	(0.047)	(0.047)	(0.037)
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,593	1,613	1,611	1,614	1,593	1,613	1,611	1,614
R-squared	0.785	0.857	0.866	0.789	0.752	0.857	0.860	0.807

Notes: The unit of observation is the city-year from 2014 to 2019. The dependent variable (all in log) are the number of criminal cases per thousand population in columns (1) to (4), and the number of offenders per thousand population in columns (5) to (8). Crime classifications are based on keyword matching, as detailed in Appendix Table A.3. Treat_c is an indicator equal to one if a city is located within 250 km of the nearest regulated city. Post_t equals one for years 2016 and onward. All regressions include time-varying city-level controls, city fixed effects, and year fixed effects. Robust standard errors, clustered at the city level, are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.11: Mechanism: Inequality and Crime

Dep. Var.:	log #Case per thousand							
	All crime				Property crime			
	(1) High Gini change	(2) Low Gini change	(3) High status change	(4) Low status change	(5) High Gini change	(6) Low Gini change	(7) High status change	(8) Low status change
$\text{Treat}_c \times \text{Post}_t$	0.134**	0.037	0.090*	0.007	0.152**	0.079	0.144**	0.022
	(0.061)	(0.054)	(0.053)	(0.068)	(0.072)	(0.058)	(0.057)	(0.071)
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	306	276	288	300	306	276	288	300
R-squared	0.890	0.905	0.903	0.892	0.883	0.907	0.901	0.892

Notes: Observations are at the city-year level for years from 2014 to 2019. The dependent variable is the number of criminal cases per thousand population (in log). Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. We run separate regressions for the cities with 50% higher and lower change of Gini index calculated according to household house asset between 2012 and 2020 in columns (1)-(2) and columns (5)-(6), and for the cities with 50% higher and lower change of average status gap calculated based on individuals' perceived gap between self-assessed social status and the highest score between 2012 and 2020 in columns (3)-(4) and columns (7)-(8). All columns include time-varying city-level controls, city fixed effects and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

Table A.12: Luxury Goods Lists

HS2-digit	HS8-digit	Product name
42 Leather products	42021110	Trunks and suitcases with outer surface of leather, composition leather, or patent leather
	42021190	Vanity cases, briefcases, and similar containers with outer surface of leather, composition leather, or patent leather
	42022100	Handbags with outer surface of leather, composition leather, or patent leather
	42029100	Other similar containers with outer surface of leather, composition leather, or patent leather
	42031000	Articles of apparel of leather or composition leather
	42032990	Gloves of leather or composition leather, not elsewhere specified
	42033000	Belts and bandoliers of leather or composition leather
	42033010	Belts of leather or composition leather
	42033020	Bandoliers of leather or composition leather
	42034000	Other articles of apparel and clothing accessories of leather or composition leather
	42050000	Other articles of leather or composition leather
	42050010	Seat covers of leather or composition leather
	42050090	Other articles of leather or composition leather
43 Fur products	43031010	Fur clothing
	43031020	Fur clothing accessories
	43039000	Other fur products
71 Pearl, gem, or precious metal products	71131100	Silver jewelry and parts thereof
	71131110	Silver jewelry set with diamonds and parts thereof
	71131190	Other silver jewelry and parts thereof
	71131910	Gold jewelry and parts thereof
	71131911	Gold jewelry set with diamonds and parts thereof
	71131919	Other gold jewelry and parts thereof
	71131921	Platinum jewelry set with diamonds and parts thereof
	71131929	Other platinum jewelry and parts thereof
	71131990	Jewelry of other precious metals, n.e.s., and parts thereof
	71131991	Jewelry of other precious metals set with diamonds and parts thereof
	71131999	Other jewelry of precious metals and parts thereof
	71132000	Jewelry of base metals clad with precious metals and parts thereof
	71132010	Jewelry of base metals clad with precious metals, set with diamonds, and parts thereof
	71132090	Other jewelry of base metals clad with precious metals and parts thereof
	71141100	Silver articles and parts thereof
	71141900	Articles of other precious metals and parts thereof
	71142000	Articles of base metals clad with precious metals and parts thereof
	71161000	Articles of natural or cultured pearls
	71162000	Articles of precious or semi-precious stones (natural, synthetic, or reconstructed)
87 Automobiles	87032411	Passenger cars with spark-ignition reciprocating piston internal combustion engines, cylinder capacity $>$3L but $\leq$4L, and CKD kits thereof
	87032412	Off-road vehicles with spark-ignition reciprocating piston internal combustion engines, cylinder capacity $>$3L but $\leq$4L, and CKD kits thereof)
	87032421	Passenger cars with spark-ignition reciprocating piston internal combustion engines, cylinder capacity $>$4L, and CKD kits thereof
	87032422	Off-road vehicles with spark-ignition reciprocating piston internal combustion engines, cylinder capacity $>$4L, and CKD kits thereof
	87032430	Gasoline passenger cars, cylinder capacity $>$3000ml
	87032440	Gasoline off-road vehicles, cylinder capacity $>$3000ml
	87033321	Passenger cars with compression-ignition reciprocating piston internal combustion engines, cylinder capacity $>$3L but $\leq$4L, and CKD kits thereof
	87033322	Off-road vehicles with compression-ignition reciprocating piston internal combustion engines, cylinder capacity $>$3L but $\leq$4L, and CKD kits thereof
	87033330	Diesel passenger cars, cylinder capacity $>$2500ml
	87033340	Diesel off-road vehicles, cylinder capacity $>$2500ml
	87033361	Passenger cars with compression-ignition reciprocating piston internal combustion engines, cylinder capacity $>$4L, and CKD kits thereof
	87033362	Off-road vehicles with compression-ignition reciprocating piston internal combustion engines, cylinder capacity $>$4L, and CKD kits thereof
91 Watches and clocks	91012100	Automatic luxury watches with precious metal cases
	91012900	Other luxury watches with precious metal cases

Table A.13: Mechanism: House Prices and Crime Return (PPML Estimation)

Dep. Var:	#Case per thousand				
	(1) 0-20th of amount	(2) 20-40th of amount	(3) 40-60th of amount	(4) 60-80th of amount	(5) 80-100th of amount
Panel A: All property crimes					
$\text{Treat}_c \times \text{Post}_t$	0.005	-0.033	0.046	0.076**	0.099***
	(0.039)	(0.040)	(0.036)	(0.032)	(0.030)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614	1,614
Panel B: Larceny					
$\text{Treat}_c \times \text{Post}_t$	0.004	-0.032	0.059	0.049	0.117***
	(0.043)	(0.041)	(0.038)	(0.038)	(0.035)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,614	1,614	1,614	1,614	1,614
Panel C: Fraud					
$\text{Treat}_c \times \text{Post}_t$	0.469	0.446	-0.141	0.150**	0.118***
	(0.800)	(0.290)	(0.162)	(0.059)	(0.040)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	318	900	1,608	1,614	1,614
Panel D: Robbery					
$\text{Treat}_c \times \text{Post}_t$	-0.034	-0.032	0.145**	0.100	0.391***
	(0.056)	(0.084)	(0.073)	(0.100)	(0.144)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y
Observations	1,608	1,602	1,596	1,584	1,554

Notes: Observations are at the city-year level for years from 2014 to 2019. All reported estimates are obtained from Poisson Pseudo-Maximum Likelihood (PPML) regressions. The dependent variable is the number of property crime cases per thousand population, where panel A uses all property crimes, panel B uses larceny cases, panel C uses fraud cases, and panel D uses robbery cases, respectively. Columns (1) to (5) display separate estimates for cases with claim amounts below the 20th percentile, 20th-40th percentile, 40th-60th percentile, 60th-80th percentile, and above the 80th percentile of claim amounts of all property crimes for each year. Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. All columns include time-varying city-level controls, city fixed effects and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

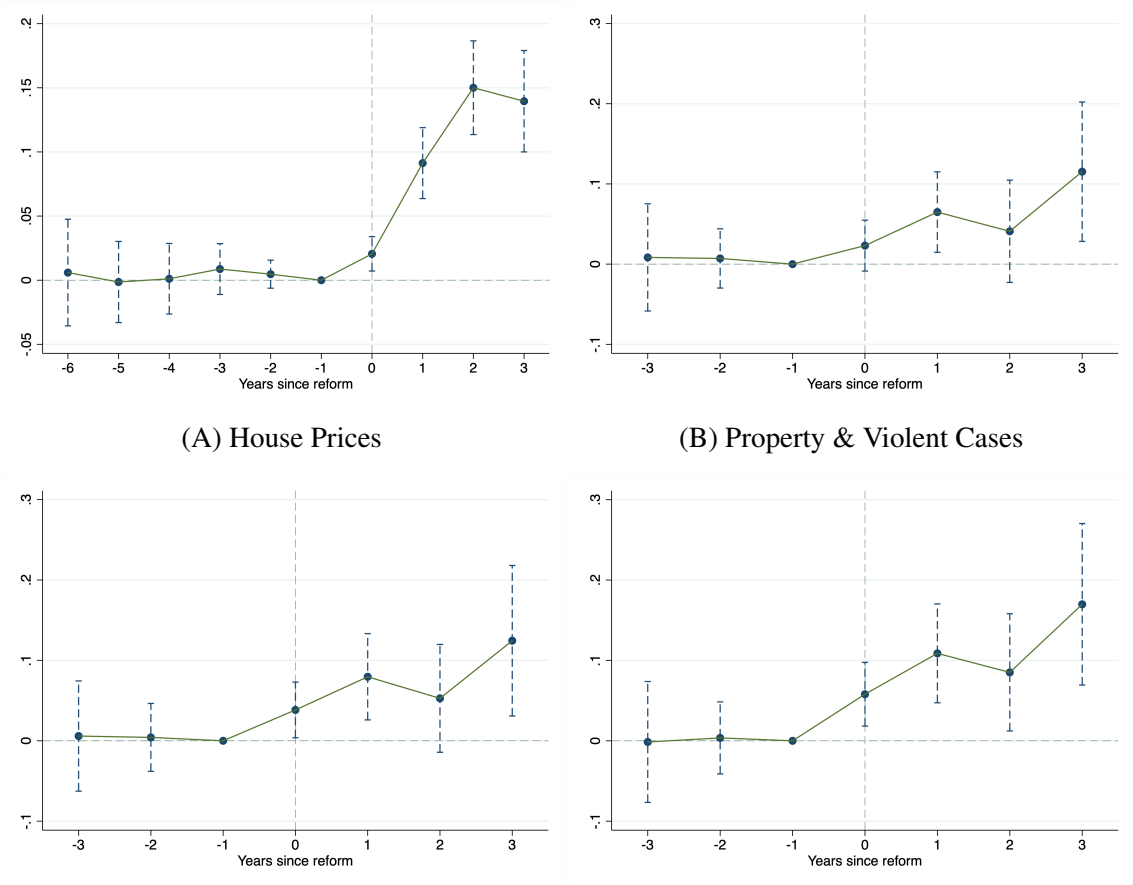
Table A.14: Mechanism: Conspicuous Consumption and Crime

Dep. Var.:	log #Case per thousand							
	All crime				Property crime			
	(1) High cons. cons. growth	(2) Low cons. cons. growth	(3) High social gift growth	(4) Low social gift growth	(5) High cons. cons. growth	(6) Low cons. cons. growth	(7) High social gift growth	(8) Low social gift growth
$\text{Treat}_c \times \text{Post}_t$	0.071	0.017	0.101**	0.004	0.104*	0.051	0.134**	0.039
	(0.051)	(0.073)	(0.049)	(0.072)	(0.056)	(0.074)	(0.058)	(0.074)
City FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
City controls	Y	Y	Y	Y	Y	Y	Y	Y
Observations	288	300	282	306	288	300	282	306
R-squared	0.921	0.863	0.914	0.875	0.913	0.876	0.906	0.881

Notes: Observations are at the city-year level for years from 2014 to 2019. The dependent variable is the number of criminal cases per thousand population (in log). Treat_c is a dummy that takes the value of one if the city is within 250 km of the nearest regulated city. Post_t is a dummy that takes the value of one if the time is after or equal to year 2016. We run separate regressions for the cities with 50% higher and lower growth of average conspicuous consumption between 2012 and 2020 in columns (1)-(2) and columns (5)-(6), and for the cities with 50% higher and lower growth of average spending on social gifts between 2012 and 2020 in columns (3)-(4) and columns (7)-(8). All columns include time-varying city-level controls, city fixed effects and year fixed effects. Robust standard errors are reported in parentheses and are clustered at the city level. *** p<0.01, ** p<0.05, * p<0.1.

B Figure Appendix

Figure B.1: Pre-trends and Dynamic Responses to the HPR Shock on Other Crime Measures



(A) House Prices

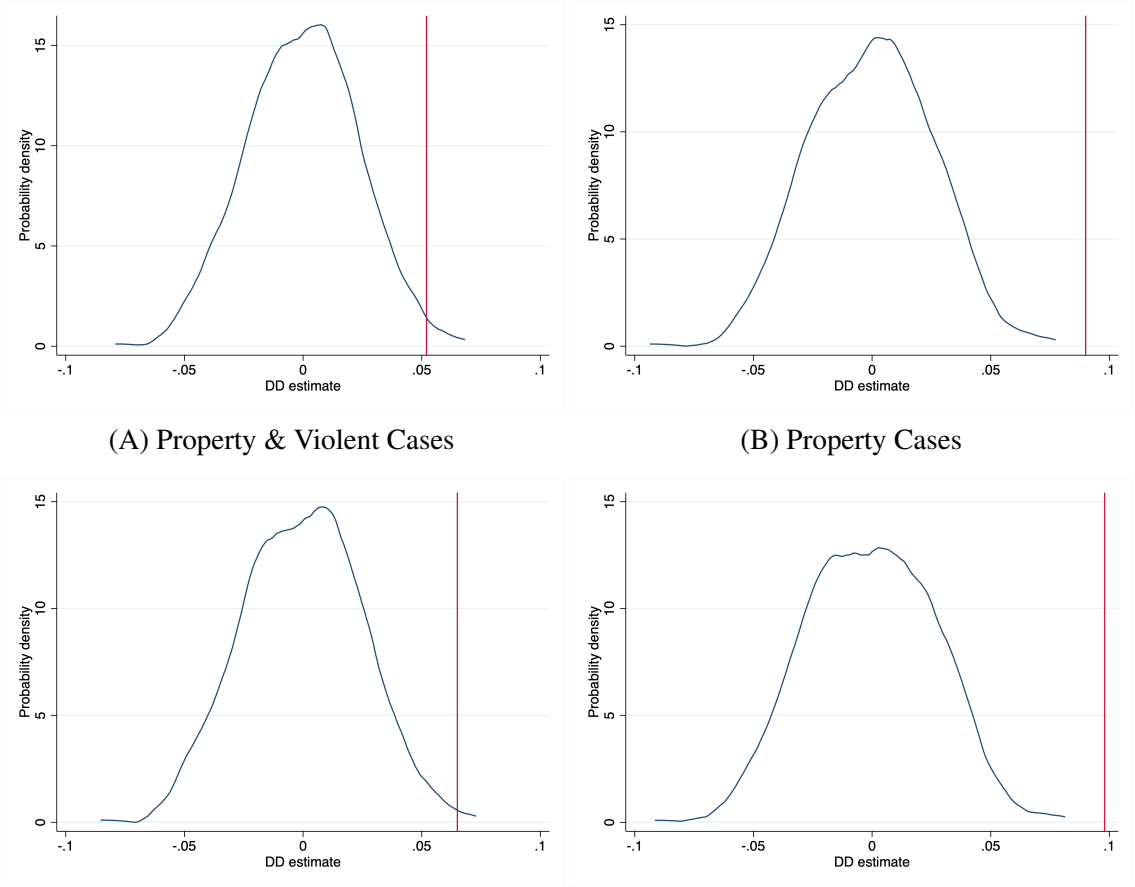
(B) Property & Violent Cases

(C) Property & Violent Offenders

(D) Property Offenders

Notes: The figure plots the estimated dynamic effects of the HPR shock on house prices in nearby unregulated cities from 2010 to 2019 and crime rates from 2013 to 2019. The dependent variables (all in logs) in panels (A)-(D) are: (A) urban house prices, (B) the number of total property and violent crime cases per thousand residents, (C) the number of total offenders involved in property and violent crimes per thousand, and (D) the number of property crime offenders per thousand. Each dot represents the estimated coefficient from the interaction terms in equation (2), and the dashed lines indicate 95 percent confidence intervals, with standard errors clustered at the city level. All coefficients are expressed relative to the baseline year, 2015. In the figure, "–1" denotes the year before the HPR shock, "0" corresponds to the year of policy implementation (2016), and "+1" indicates the first full post-treatment year.

Figure B.2: Distribution of Placebo Estimates from the Falsification Test



(A) Property & Violent Cases

(B) Property Cases

(C) Property & Violent Offenders

(D) Property Offenders

Notes: This figure presents the cumulative distribution of estimated coefficients from 500 placebo simulations in which treatment status is randomly assigned across cities. The dependent variables (all in logs) in panels (A)-(D) are: (A) the number of total property and violent crime cases per thousand residents, (B) the number of property crime cases per thousand, (C) the number of total offenders involved in property and violent crimes per thousand, and (D) the number of property crime offenders per thousand. The vertical lines in panels (A) and (C) indicate the actual baseline estimates reported in columns (4) and (6) of Table 1, and the vertical lines in panels (B) and (D) refer to the estimates displayed in column (1) of Table 2, respectively.

C Background: Legal System in China

This section provides institutional background on China's criminal justice system to help readers better understand the database we use.

According to the Criminal Procedure Law of the People's Republic of China, a criminal case is handled by three institutional authorities: the public security organs, the People's Procuratorates, and the People's Courts. The public security organs hold investigative prerogatives, including preliminary examination, detention procedures, and arrest execution. The People's Procuratorates exercise prosecutorial jurisdiction and maintain authority over public prosecution initiation. The People's Courts discharge adjudicative responsibilities. Different levels of People's Courts have jurisdiction rights over distinct categories of criminal cases as courts of the first instance: Primary People's Courts possess competence over routine criminal proceedings, Intermediate People's Courts are responsible for national security infractions and cases punishable by life imprisonment or the death penalty, while Higher People's Courts and the Supreme People's Court maintain original jurisdiction over cases of provincial and national significance.

Public-prosecution criminal cases proceed through three sequential stages: (i) filing and investigation, (ii) examination and prosecution, and (iii) trial. Initially, the public security organs or the People's Procuratorates make case filing decisions following prompt examination of materials submitted by reporters or informants. For cases that proceed to filing, the public security organs undertake investigations involving evidence collection and acquisition to establish criminal suspects' guilt or innocence and assess crime severity. Active or primary suspects may face detention or even arrest procedures during investigation. When the public security organs seek to arrest a criminal suspect, they must submit written requests accompanied by case files and evidence to the People's Procuratorate for arrest approval within three days after the detention.[53] The People's Procuratorate must render an arrest decision within seven days of receiving the arrest application. The duration for detaining a criminal suspect during post-arrest investigation shall not exceed two months, except for some complex cases.[54] Upon investigation finished by the public security organs, the case documentation, together with written prosecution recommendations and all evidence, is submitted to the People's Procuratorate for further examination. The People's Procuratorate should make a determination within one month on whether to commence prosecution in People's Courts.[55] When the People's Procuratorate finds that the evidence is reliable and sufficient and that criminal

[53] Under special circumstances, the time limit for submitting arrest requests can be extended by one to four days. For arrests of primary suspects involved in crimes committed across multiple locations, repeatedly, or within criminal organizations, the time limit can be extended to 30 days.

[54] When cases are complex and investigations cannot be completed within the standard time limit, a one-month extension may be granted with approval from the People's Procuratorate at the next higher level. Additionally, for particularly grave and complex cases, a further extension of 2 to 4 months may be authorized by the Standing Committee of the National People's Congress.

[55] An extension of a half month may be allowed for major or complex cases.

responsibility should be pursued, it would decide to prosecute. However, prosecution shall not be initiated if a criminal suspect falls under any of the following circumstances: (i) the act is obviously minor, causes no serious harm, and does not constitute a crime; (ii) the limitation period for criminal prosecution has expired; (iii) criminal punishment exemption has been granted via special amnesty decree; (iv) the crime requires complaint-based handling under Criminal Law, but no complaint has been filed or the complaint has been withdrawn; (v) the criminal suspect is deceased; or (vi) other laws exempt the investigation of criminal responsibility. Upon prosecution initiation, the People's Courts must commence trial proceedings within three months.[56] Thus, based on the procedures outlined above, our sample encompasses cases in which the People's Procuratorate initiates public prosecution in the People's Courts and private prosecution cases where victims may sue directly to a People's Court.[57]

D Model Appendix

D.1 Equilibrium Characterization

This appendix provides a formal characterization of the equilibrium in the two-stage game introduced in Section 5.1.2. We show that, under empirically plausible parameter restrictions, the economy admits a unique *separating equilibrium*

D.1.1 Definitions and Preliminaries

Let $G(w)$ denotes the cumulative distribution function of wealth: $G(w) : [\underline{w}, \overline{w}] \to [0, 1]$. The model consists of two stages. In Stage 1, individuals choose consuming X. Let $H \subset [0, 1]$ denotes agents who select goods C. In Stage 2, after observing consumption choices, some individuals decide whether to commit property crimes. Let $X \subset [0, 1]$ be the set of criminals, each matched one-to-one with a victim other than themselves. Denote the set of potential victims as V. Then $q = \frac{\lambda(X)}{\lambda(V)}$ represent the probability that a given member of V is victimized, where $\lambda(\cdot)$ is Lebesgue measure. Given consumption and crime choices, the population partitions naturally into four classes, with the associated expected utilities (U_1, U_2, U_3, U_4)[58]:

- **Class 1:** Noncriminal conspicuous consumers ($H \cap \overline{X}$), with utility U_1.
- **Class 2:** Noncriminal basic consumers ($\overline{H} \cap \overline{X}$), with utility U_2.
- **Class 3:** Criminal basic consumers ($\overline{H} \cap X$), with utility U_3.

[56] If it is necessary to extend the time limit, the period shall not exceed six months.

[57] Cases of private prosecution comprise: (i) complaint-based cases that require victim initiation under law; (ii) minor criminal cases; and (iii) cases for which victims possess evidence demonstrating defendants' criminal responsibility, but the public security organs or the People's Procuratorates decline to investigate.

[58] While Class 4 is theoretically possible, we will show below that this class is empty in equilibrium.

- **Class 4:** Conspicuous consumers who also commit crime ($H \cap X$), with utility U_4.

D.1.2 Equilibrium Definition

We now formalize the equilibrium described in Section 5.1.2.

Definition 1 (Equilibrium)**.** *An* equilibrium *is H, X, τ with victim set V such that:*

(i) ***Optimality:*** *For each wealth realization w with CDF value $G(w) \in [0,1]$, the individual's decisions maximize expected utility:*

$$\begin{aligned}
&(E1) \quad U_1 \geq \max\{U_2, U_3, U_4\} && \text{for } G(w) \in H \cap \overline{X},\\
&(E2) \quad U_2 \geq \max\{U_1, U_3, U_4\} && \text{for } G(w) \in \overline{H} \cap \overline{X},\\
&(E3) \quad U_3 \geq \max\{U_1, U_2, U_4\} && \text{for } G(w) \in \overline{H} \cap X,\\
&(E4) \quad U_4 \geq \max\{U_1, U_2, U_3\} && \text{for } G(w) \in H \cap X,\\
&(E5) \quad m'(V) > m'(\overline{V}).
\end{aligned}$$

where $m'(\cdot)$ denotes the average wealth after consumption of some group.

(ii) ***Market clearing:*** *The conspicuous goods market clears.*

Condition (i) guarantees individual optimality, and (iii) ensures the market for conspicuous goods clears.

D.1.3 Equilibrium Assumptions

Building on Chiu and Madden (1998), we restrict attention to empirically plausible environments by imposing the following conditions on preferences, wealth distribution, and crime technology.

Assumption 1.

$$\mu < \delta \quad \textit{and} \quad \beta < \gamma,$$

where

$$\begin{aligned}
\delta &= \min\left\{ \frac{y_2}{y_2} \,\middle|\, G(y_2) = G(y_1) + 1 - 2v,\ y_1 \geq \underline{w},\ y_2 \leq \overline{w} \right\},\\
\gamma &= \frac{\sigma(\delta - 1)(\mu - 1)}{(\mu - 1) + \mu\sigma(\delta - 1)},\\
\sigma &= \frac{\beta(1-p)}{p}.
\end{aligned}$$

It ensures that the utility gained from conspicuous consumption remains limited and prevents crime from becoming highly profitable.

Assumption 2. *The assumption ensures that equilibrium exists and is unique.*

(a)

$$\mu < \frac{G^{-1}(1-v)}{G^{-1}(1-v) - \left[m'(V) - m'(\overline{V})\right]}.$$

This ensures the wealth gap between conspicuous and basic consumers exceeds the price premium of conspicuous goods in a crime-free setting, making it more profitable for criminals to target conspicuous consumers rather than choosing victims at random.

(b)

$$\frac{G^{-1}(v)}{m'(V) - \frac{(\mu-1)G^{-1}(1-v)}{\mu}} > \sigma > \frac{\underline{w}}{m'(V) - \frac{(\mu-1)G^{-1}(1-v)}{\mu}},$$

The lower bound ensures that crime is profitable enough for the poorest individuals, preventing a zero-crime equilibrium, while the upper bound prevents crime from becoming too profitable.

We further require that β be sufficiently small, which prevents limiting cases in which criminals can appropriate an arbitrarily large fraction of victim wealth.

D.1.4 Existence and Uniqueness of Equilibrium

Given Definition 1 and assumptions above, we apply two key Lemmas from Chiu and Madden (1998).

Lemma 1 (Lemma 1, Chiu and Madden (1998))**.** *Under Assumptions 1, any equilibrium must exhibit perfect sorting by wealth:*

- *Low-wealth individuals consume basic goods and commit crime (Class 3).*
- *Middle-wealth individuals consume basic goods and abstain from crime (Class 2).*
- *High-wealth individuals consume conspicuous goods and abstain from crime but serve as victims (Class 1).*
- *Criminal conspicuous consumers (Class 4) do not exist in equilibrium.*

Lemma 2 (Lemma 2, Chiu and Madden (1998))**.** *Under Assumptions 1–2, and provided β is sufficiently small:*

(a) There exists a unique equilibrium with victim set $V = H$ and criminal set $X = [0, x]$, where x is determined by the marginal indifference conditions.

(b) No other equilibriums exist.

Main Result. Combining Lemmas 1 and 2, we establish the existence of a *unique separating equilibrium* featuring three distinct wealth-based behavioral types: (i) individuals with low wealth engage in property crime, (ii) those with intermediate wealth abstain from offending and consume basic goods, and (iii) the wealthy engage in conspicuous consumption and become targets of crime. This equilibrium highlights the model's central mechanism: rising housing wealth amplifies inequality, thereby making economic status more visible—and more susceptible—to criminal targeting.

D.2 Proof of Proposition 1

Let w_i and w_j denote the wealth levels at percentiles $i > j$ under return R. The *relative differential inequality* $I(R)$ is given by

$$I(R) = \frac{w_i}{w_j} = \frac{(\lambda m_i + m_i)R - \lambda m_i(1+r)}{(\lambda m_j + m_j)R - \lambda m_j(1+r)}.$$

We show that $I(R)$ is strictly increasing in R. Differentiating $I(R)$ with respect to R yields

$$I'(R) = \frac{(\lambda m_i + m_i) - (\lambda m_j + m_j)}{[(\lambda m_j + m_j)R - \lambda m_j(1+r)]^2}.$$

For $m_i > m_j$, the numerator is positive, implying $I'(R) > 0$. Thus, as R increases from R_1 to R_2, the wealth distribution G_2 exhibits a greater *relative differential inequality* than G_1.