The Scar of Political Conflict: Evidence from Tear Gas Deployments in Hong Kong*

HENG CHEN
University of Hong Kong
DEREK HUO
University of Hong Kong

September 12, 2025

Abstract. This study examines how exposure to political violence reshapes market behavior by leveraging Hong Kong's 2019-2020 protests. Exploiting local variation in tear gas deployment–the primary crowd-control weapon marking sites of violent confrontations–we use a difference-in-differences approach comparing housing prices in buildings within 50 meters of tear gas sites to those 50-1,000 meters away, before the protests and after the National Security Law (NSL) ended the movement. We find that homeowners near violence sites sold their apartments at a significant discount following the NSL, persisting for about six months. The discount varies systematically with properties' potential sensory exposure to violence, neighborhood political ideology, and residents' emigration opportunity – patterns inconsistent with local amenity deterioration from tear gas exposure. Our research design isolates the impact of past experiences with violence from expectations of future unrest, underscoring how extreme political experiences can drive high-stakes economic decisions.

Keywords: political violence, housing price, protests, Hong Kong, market behavior

*Heng CHEN: The University of Hong Kong, Email: hengchen@hku.hk; and Derek HUO: The University of Hong Kong, Email: derekhuo@connect.hku.hk.

1. Introduction

Political violence has been on the rise globally. Confrontations between civilians and authorities have become commonplace, erupting in public spaces from city streets to university campuses. Recent examples include Chile's mass demonstrations against inequality (2019-2020), the nationwide protests after George Floyd's murder in Minneapolis (2020), global protests over the Israel-Hamas war (2023-2024), and the violent confrontations during 'No Kings' protests in Los Angeles (2025). The increasing prevalence of political violence makes it crucial to understand its impacts.

Political economists have shown that exposure to violence can reshape individuals' political attitudes. For instance, research on the Second Palestinian Uprising (2000–2005) finds that politically motivated violence led to persistent shifts toward right-wing positions (Klor, Lahad, and Zussman 2025), while studies of the Khmer Rouge genocide in Cambodia demonstrate that state violence can simultaneously foster support for political pluralism and instill fear of the regime (Bühler and Madestam 2023). Similarly, Campedelli et al. (2023) show that exposure to images of organized crime-related violence increases trust in state institutions.

While existing research shows that violence shapes attitudes, whether these experiences translate into high-stakes market behavior remains largely unexplored. This research gap stems from two challenges. First, exposure to violence simultaneously constitutes a *direct experience* and reshapes *expectations of future violence*, making it difficult to disentangle these two effects. For example, when Daniele, Le Moglie, and Masera (2023) document residents fleeing from drug cartel conflicts in Mexico's opium-producing regions, the migration could have been driven by lived experiences of violence, fears of future conflict, or both. Second, researchers rarely have access to individual-level data linking violence exposure to subsequent economic behavior, particularly in conflict zones.

We tackle these challenges in the context of Hong Kong's 2019 Anti-Extradition Law Amendment Bill (Anti-ELAB) Movement–the city's largest and most violent civil unrest–which provides a unique setting for our analysis. The movement began in June 2019 when protests erupted over a proposed bill allowing criminal suspects to be sent to mainland China. What followed were intense months of escalating violence, with multiple protests occurring each week, and tear gas–the police's primary crowd-control weapon–deployed across the whole city. The movement continued until Beijing's imposition of the National Security Law (NSL) in May 2020, which brought the protests to a definitive end.

First, the NSL's imposition helps address the challenge of disentangling past experiences from expectations of future violence. The law imposes severe penalties, including life sentences for antigovernment activities, and has been widely criticized for restricting political freedoms, particularly speech and assembly. Hong Kong citizens anticipated that the NSL would eliminate future anti-government protests, whether violent or peaceful–an expectation confirmed by the absence of subsequent demonstrations requiring police intervention. This clean break in protest dynamics provides a rare opportunity to isolate the effect of past experiences with violence from expectations of future violence.

Second, as the largest protest movement in Hong Kong's history, its spatial pattern helps overcome the data limitation. Unlike previous protests that concentrated in central areas, the 2019 protests were widespread, reaching all 18 districts and spreading from business districts into residential neighborhoods. This spatial penetration into residential areas enables us to leverage the comprehensive housing transaction records to examine homeowners' response after witnessing violence.

In this paper, we ask whether and under what conditions homeowners' exposure to violence *during* the movement influenced their property transactions *after* it ended. Our identification strategy leverages local-level variations in violence exposure, rather than regional-level violence intensity. To this end, we construct a novel measure of violence exposure using tear gas deployment data. Tear gas was the most common crowd-control weapon during the movement, deployed at nearly 2,000 locations, and these deployments mark sites of violent confrontations between protesters and police. We construct our building-level measure by geocoding each deployment location and calculating residential buildings' proximity to the nearest tear gas site, allowing us to capture variation in homeowners' exposure to violence.

Specifically, we define treated buildings as those within 50 meters of tear gas sites–approximating the typical tear gas dispersal area–and control buildings as those between 50 and 1,000 meters away, with 1,000 meters representing a typical neighborhood size. This classification is justified because homeowners in buildings near tear gas sites likely had greater direct exposure to conflicts, potentially witnessing confrontations and experiencing tear gas effects firsthand.

Using a difference-in-differences approach, we examine housing price changes across two distinct periods: before the movement and after the national security law's implementation. We compare price changes between treated buildings (within 50 meters of tear gas sites) and control buildings (50-1,000 meters away).

Our identification strategy relies on the assumption that tear gas deployment locations were conditionally random. At the aggregate level, tear gas was not randomly deployed *across communities*–protest activities and subsequent police responses could occur more frequently in certain locations. However, *within these communities*, the precise locations of tear gas deployment were largely determined by idiosyncratic factors: unexpected crowd movements, sudden escalations in protest intensity, and the divergence between police-intended and actual deployment locations due to environmental conditions. Supporting this assumption, treated and control buildings within the same community were comparable before the protests in two key dimensions: they showed no significant differences in pre-movement housing prices, and their residents exhibited similar political preferences.

Based on this research design, we find that after the national security law, apartments in treated buildings sold at a significant discount of 1.6 percentage points compared to those in control buildings, controlling for community-specific price trends as well as building and year-month fixed effects. This discount is economically meaningful: given the average apartment price of 7.1 million HKD (0.9 million USD) during our sample period, it represents approximately 0.1 million HKD (15,000 USD). Using an event study model, we validate the assumption of parallel pre-trends: treated and control buildings showed no significant price differentials before June 2019.

One might worry that our findings are driven by observable and unobserved variables correlated with tear gas deployment locations, rather than by the tear gas exposure itself. For instance, tear gas was more likely to be deployed along main roads, near transit stations, and in commercially dense areas, and apartments in such locations might have experienced different price trajectories after the protests regardless of tear gas exposure. To address these concerns, we conduct two tests. First, we employ a horse race analysis that allows both apartment-level characteristics (size, floor level, orientation) and building-level features (distance to amenities, transportation access, commercial density) to have time-varying effects; our main findings remain robust to these controls, suggesting that differential valuation of property characteristics does not explain our results. Second, we conduct randomization inference for a placebo test to further mitigate concerns about unobserved confounders.

When interpreting our results, two points merit attention. First, our difference-in-differences approach captures impacts of relative–rather than absolute–exposure to violence. While residents throughout Hong Kong experienced protest-related violence, those in treated buildings were more likely to face higher-intensity exposure through direct encounters with tear gas and confrontations. We leverage this local variation in exposure across locations to identify the impact of violence. Second, while one might argue that the National Security Law (NSL) likely had impacts comparable to protest violence on Hong Kong residents, the NSL affected all citizens uniformly. Our identification strategy exploits variation in individuals' exposure to protest violence, allowing us to isolate these effects from the common impact of the NSL on all properties.

Building on the baseline results, we conduct a series of heterogeneity analyses to understand the channels driving the housing price effects. Specifically, we examine *where*, *for whom*, and through *what actions* the discounts were more pronounced. First, the discount was even larger for a subset of apartments with greater potential for direct sensory exposure–such as lower-floor apartments, apartments with unobstructed city-facing views, and those affected by tear gas incidents during prime evening hours (6-9pm) when residents were most likely to witness events firsthand. Second, the effects were stronger in pro-democracy neighborhoods and absent in pro-establishment neighborhoods, suggesting that political leaning may shape how residents interpreted and responded to the same violent events.

Third, the discount only emerged following a new emigration pathway. In response to the NSL, the British government launched the British National (Overseas) [BNO] visa scheme (implemented in February 2021), offering Hong Kong residents a route to UK citizenship. An exodus followed: by March 2024, over 172,000 Hong Kong residents had applied for BNO visas, with an estimated 150,000 applicants relocating to the UK. The scheme's inclusion of dependents further amplified emigration numbers. The Hong Kong government estimated that approximately 410,000 residents–about 5.5% of Hong Kong's 7.4 million population–had emigrated by February 2024.

We find that turnover rates were higher in buildings near tear gas sites after the BNO scheme became available, accompanied by significant price discounts. Notably, these effects did not exist in the period between NSL implementation and BNO availability. This pattern suggests a fire-sale interpretation: residents in treated buildings were more eager to liquidate their properties and willing to accept lower prices to expedite sales so that they could take advantage of this BNO scheme.

To further validate this mechanism, we exploit variation in BNO visa eligibility. We find that BNO-eligible local sellers of apartments in the treated buildings offered price discounts after the scheme's implementation, relative to non-eligible sellers of apartments in the treated buildings. Moreover, BNO visa eligibility of buyers had no effect on prices, suggesting that the observed price effects were driven specifically by the combination of violence exposure and emigration opportunities.

Taken together, these results support a conjecture in which experiences of political violence eroded confidence in Hong Kong's future, prompting affected residents to liquidate housing assets when emigration opportunities arose. This interpretation is consistent with the shattered assumptions theory, which posits that traumatic political events can disrupt individuals' core beliefs about societal security and governance (Janoff-Bulman 1989).[1]

[1] Extensive psychological and economic research demonstrating that significant negative shocks can generate lasting effects on individuals' perceptions and behavior (Carmil and Breznitz 1991, Punamäki

Our event study reveals that property price discounts peak around the BNO policy's enactment and gradually diminish over time. This temporal pattern aligns with this particular mechanism: as the initial pool of emigration-motivated sellers depleted, the price discount dissipated. The transitory nature of the discount effect suggests that our findings reflect temporary selling pressure from migration-motivated homeowners rather than a permanent devaluation of these properties.

By contrast, demand-side explanations–particularly those based on neighborhood amenity deterioration–are inconsistent with our findings for several reasons. First, if neighborhood deterioration were driving the price effects, we would expect uniform discounts across all apartments within the same building. Second, we observe stark differences in discounts between pro-democracy and pro-establishment neighborhoods, despite similar levels of physical disruption, suggesting that neighborhood quality deterioration cannot explain the price patterns. Third, selling pressure concentrated among BNO-eligible homeowners, indicating that the discounts were not driven by declining neighborhood amenities. Furthermore, our analysis of rental and Airbnb prices, which should reflect any deterioration in neighborhood quality, shows no differences between treated and control buildings in the post-movement period. This absence of rental market effects further suggests that neighborhood deterioration cannot explain the documented price discount.

The broader message of our paper is that individuals' political experiences–particularly exposure to violence–can shape market behavior when conditions are suitable. In doing so, the study makes several contributions to the literature. First, we extend research on the impact of violence beyond its common focus on attitudes and mental health. For example, Cassar, Grosjean, and Whitt (2013) show that exposure to the Tajik civil war (1992-1997) undermined social trust; Shayo and Zussman (2017) find that violence during the Second Intifada increased Israeli judges' in-group bias; Ang (2021) demonstrate that police killings reduce academic performance in nearby schools, particularly among Black and Hispanic students; and Vu et al. (2023) show that prenatal exposure to lynchings in the American South worsened later-life mortality for African American males.[2]

We examine how exposure to political violence influences market behavior, thereby

et al. 1997, Tedeschi and Calhoun 2004, Voors *et al.* 2012 and Callen *et al.* 2014). While both Voors *et al.* (2012) and Callen *et al.* (2014) use experimental methods to study how exposure to violence affects preferences, the former shows that individuals who were exposed to violence in Burundi display more risk-seeking behavior and have higher discount rates in incentivized experiments, and the latter finds that priming subjects to recall fearful situations related to violence in Afghanistan increases their preference for certainty.

[2]Bor et al. (2018) and Curtis et al. (2021) find that police killings of unarmed Black Americans and highly publicized anti-Black violence increase the number of poor mental health days among Black Americans.

moving beyond prior work that has focused primarily on psychological, health, and political outcomes. Our context is also distinct: we study violence in the setting of protest-police confrontations during episodes of collective action, rather than civil wars, terrorism, or systematic racial violence examined in previous research.

Second, our setting addresses a common challenge in the literature by isolating the impact of past violence exposure from expectations of future unrest. While existing research shows that expected violence and terrorism risk affect property prices (Besley and Mueller 2012) and vacancy rates (Abadie and Dermisi 2008), the implementation of Hong Kong's National Security Law created a unique context where future protests were effectively precluded. This allows us to show that prior political experiences influenced property owners' behavior even after the possibility of future unrest had been eliminated.

Moreover, our detailed transaction data enables us to identify the underlying mechanisms driving price changes. The asymmetric effects we find between sellers' and buyers' emigration eligibility provide evidence that supply-side factors, rather than demand-side considerations, drive the observed housing price discounts.

Third, we contribute to research on protests and their consequences by examining violence during these events. Prior work has studied protest participation dynamics (Guiso, Sapienza, and Zingales 2006), long-term political engagement (Bursztyn et al. 2021), social network influences (González 2020), effects on voting and political contributions (Madestam et al. 2013), and impacts on stock returns (Acemoglu, Hassan, and Tahoun 2018). We extend this literature by documenting how violence during protests affects witnesses' market behaviors, providing new evidence on the economic consequences of political unrest through the housing market channel.

Finally, our study is related to two recent papers examining the Hong Kong property market from different angles. Luo, Yang, and Olken (2024) study how real estate wealth affects households' ability to emigrate and their economic costs of leaving, analyzing historical cases of Shanghai firms moving to Hong Kong (1930s-40s) and post-1997 Hong Kong emigration. He et al. (2024) examine how uncertainty about land lease extensions beyond 2047 affects housing prices, finding that properties with uncertain extension status sell at a discount. While these studies focus on wealth effects and institutional uncertainty respectively, we identify how direct exposure to political violence shapes real estate transactions through a quasi-experimental design based on tear gas deployment locations.

2. Background

2.1. City of Protest: The 2019 Anti-ELAB Movement and the Aftermath

Hong Kong's unique political status derives from its British colonial history (1842-1997). The 1997 handover to China established a "one country, two systems" framework through the Sino-British Joint Declaration, guaranteeing Hong Kong's capitalist system and autonomy for 50 years.

Post-handover Hong Kong saw multiple protests defending its promised autonomy, including opposition to Article 23 legislation (2003), the National Education curriculum (2012), and the Umbrella Movement demanding democratic elections (2014). These protests were predominantly peaceful, with only isolated incidents of violence. The 2019 Anti-Extradition Law Amendment Bill (Anti-ELAB) Movement marked the most significant protests since the handover. Initially triggered by a proposed extradition bill allowing transfers to mainland China, the movement evolved into broader demands for democratic reforms and drew unprecedented public participation.

While this movement is part of a continuum of political protests since the handover, the 2019 Anti-ELAB Movement stands out for its unprecedented scale and duration. Beginning in June 2019, it mobilized a diverse cross-section of Hong Kong society, with protests organized in a largely decentralized manner. Some demonstrations reportedly drew an astounding two million participants in a city of seven million, and gatherings exceeding 100,000 people became commonplace. This period of sustained unrest lasted until December 2019. During these seven months, multiple protests occurred each week, sometimes even on weekdays. The COVID-19 pandemic abruptly interrupted its momentum, though protesters returned for sporadic demonstrations after restrictions eased later in the year.

The unparalleled magnitude and impact of the 2019 protests, which represented the most serious challenge to Beijing's rule since 1989, prompted a significant response from the Chinese central government. Seeking to reassert control and quell dissent, Beijing introduced the National Security Law (NSL), a controversial piece of legislation that marked a turning point in Hong Kong's political landscape. On May 21, 2020, the National Security Law was announced. Subsequently, its details and provisions underwent thorough deliberation and finalization. The culmination of this process was its formal enactment on June 30, 2020, by the Standing Committee of the National People's Congress of China (NPCSC), effectively bypassing Hong Kong's Legislative Council (LegCo).

The NSL was seen as a direct reaction to the unrest and demands for greater autonomy. The stated purpose of the NSL is to criminalize acts of secession, subversion, ter-

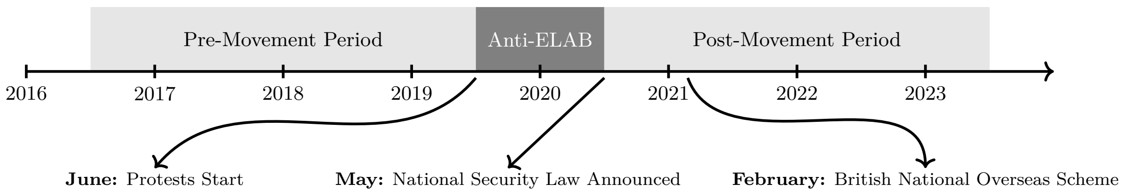


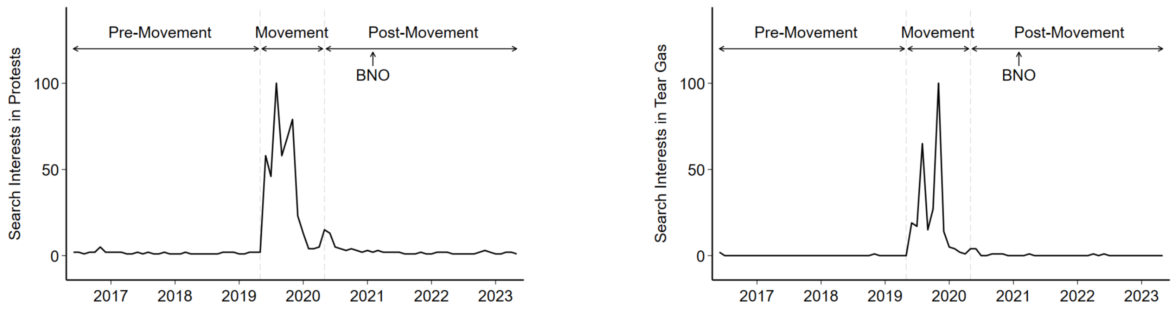
Figure 1. *Timeline of Events. This figure highlights the timeline of key events in our study. It marks the start of the Anti-ELAB Movement in June 2019, the introduction of the National Security Law (NSL) in May 2020, and the BNO Visa Scheme's implementation in February 2021.*

rorism, and collusion with foreign forces. However, critics argue that the law's broad and vaguely defined terms have created a chilling effect on the freedoms of speech, assembly, and press. Concerns have been raised by international organizations, foreign governments, and human rights groups, who view the NSL as a threat to the freedoms promised under the Sino-British Joint Declaration.[3] Since its implementation, it was widely expected that anti-government civil movements would cease in Hong Kong given the severe punishments, and indeed, Hong Kong has seen no major anti-government protests.

To demonstrate the NSL's effectiveness in suppressing protests, we present Google search intensity data for protest-related terms in Figure 2. Panel A shows search intensity for "protest" and B for "tear gas" in Hong Kong. The figures display near-zero search activity during 2016-2019, dramatic spikes during the 2019-2020 protests, and an abrupt return to zero after the NSL's implementation. Notably, search intensity remains at zero through the introduction of the BNO visa scheme and continues at zero through 2021-2023.

The British government, perceiving the enactment of the National Security Law (NSL) in Hong Kong as a breach of the Sino-British Joint Declaration, introduced the British National Overseas (BNO) policy as a counter-response. This policy was designed to facilitate the migration of Hong Kong residents to the UK by offering them a pathway to citizenship. The BNO status, originally created in 1987 due to Hong Kong's status as a former British colony, served as a form of nationality for Hong Kong residents without granting them the right to reside or work in the United Kingdom. However, the revised BNO policy significantly broadened these rights, allowing BNO holders to apply for British citizenship after residing in the UK for five years. Many viewed this policy as a lifeline for those seeking to leave Hong Kong following the NSL's enactment due to ensuing political uncertainties. The UK government an-

[3]The NSL has had a significant impact on Hong Kong's political landscape, leading to the arrest and prosecution of pro-democracy activists, the disqualification of opposition lawmakers, and the closure of independent media outlets. See United Nations Human Rights Office, "Hong Kong's National Security Law: Implications for Human Rights and the Rule of Law," June 2021.



(a) Search Index for Protests

(b) Search Index for Tear Gas

Figure 2. *Protests and Tear Gas in Google Search Index. This figure illustrates Google search intensity in Hong Kong for protest-related terms from 2017 to 2023. Both panels show near-zero search activity before June 2019, dramatic spikes during the movement period (June 2019-April 2020), and an immediate return to zero after the NSL's implementation, remaining at zero through the BNO visa scheme enactment (marked by arrow) and beyond. The vertical lines divide the sample into pre-movement, movement, and post-movement periods.*

nounced revisions to the BNO policy on July 1, 2020, and formally introduced it on January 31, 2021. The timeline of these events is captured in Figure 1.

The introduction of the British National (Overseas) (BNO) visa scheme in January 2021 sparked a significant exodus from Hong Kong. UK Home Office data shows that by March 2024, Hong Kong residents had submitted over 172,000 BNO visa applications, with a 96% approval rate. An estimated 150,000 successful applicants had moved to the UK by March 2024, with the actual number of arrivals being higher as the scheme allows dependents (spouses, children, and parents) to accompany main applicants.

2.2. Protests, Violence and Tear Gas Deployments

While the 2019 Anti-ELAB Movement in Hong Kong serves as a compelling backdrop for our study, two key aspects of this prolonged period of unrest are particularly relevant to our empirical design and warrant further discussion. First, the protests geographically spread across all 18 districts of Hong Kong, reaching streets and neighborhoods that had never experienced protests before. This contrasted with earlier events, which were confined to a few central areas. Second, clashes between protesters and the police frequently erupted, escalating into violence. These confrontations were significantly more intense and violent than those in previous demonstrations.

These outbreaks of violence often occurred during marches, which typically followed a similar pattern. Protesters would gather at a predetermined location–often a shopping mall or busy intersection–and march toward a specific destination, such as a government building or symbolic landmark. Along the march route, protesters engaged in various forms of dissent, from chanting slogans and displaying banners to performing acts of civil disobedience. Upon reaching their destination, they might hold rallies or engage in other forms of protest before dispersing.

Figure 1(a) illustrates one of the most frequently used protest routes in Hong Kong's history, commonly utilized both before and during 2019. This traditional route typically began in Victoria Park and proceeded through major thoroughfares in Hong Kong Island's central districts. However, the 2019 protests marked a significant departure from these established patterns, with demonstrations spreading throughout Hong Kong's territory. Figure 1(b) demonstrates this expansion by showing a protest route in the Kowloon area - one of many new locations that had not previously experienced protest activity.

However, it was during these marches that violent confrontations with the police force often erupted unpredictably. These eruptions of violence typically stemmed from either aggressive actions by protesters or excessive force by the police. Frustration with the government's perceived intransigence, coupled with anger and desperation, drove some protesters to escalate their tactics. These escalations included throwing projectiles such as bricks and Molotov cocktails, vandalizing property, and directly confronting police. Conversely, critics condemned instances of what they deemed excessive force by police, arguing that officers employed disproportionate responses to often-minor acts of civil disobedience.

During the 2019 Anti-ELAB movements, tear gas was frequently used by law enforcement to manage intense clashes with protesters. Its primary purpose was to quickly disperse crowds by causing temporary discomfort, such as severe eye irritation, coughing, and difficulty breathing. Intended to protect police officers from harm, tear gas can disperse over a range of 30-50 meters (100-160 feet). Being heavier than air, it tends to stay close to the ground, typically rising only 6-10 feet. This characteristic makes it effective in controlling crowds and preventing protesters from advancing. It is worth noting that its use was often unpredictable, deployed in response to escalating conflicts.

Among various weapons deployed, tear gas usage was particularly closely monitored and documented during the 2019 protests in Hong Kong by both the media and the public. This focused attention stemmed from two primary reasons. First, the use of tear gas as a crowd control weapon during the 2014 Umbrella Movement–its first deployment in half a century–left a lasting legacy. The citywide controversy surrounding tear gas use in 2014 made monitoring its deployment in 2019 especially salient.

Second, Hong Kong's exceptionally high population density, with approximately 6,940 people per square kilometer, meant that the impact of tear gas deployment was particularly severe. The frequent use of tear gas in these densely populated areas not only affected protesters' health but also impacted nearby residents both physically and psychologically. Consequently, tear gas deployment became a flashpoint for criticism and raised significant concerns about police violence. These factors combined to make tear gas monitoring a critical aspect of public and media scrutiny during the 2019 protests. The intense public interest in tear gas deployment led to comprehensive monitoring and documenting efforts throughout the 2019 protest movement.

3. Data

Our analysis draws upon a number of data sources. We combine information on the geographical locations of tear gas deployments with detailed records of residential sale and rental transactions as well as data from Airbnb listings. To explore the heterogeneity in political preferences across residential areas, we utilize the 2015 and 2019 Hong Kong District Council election data.

3.1. Tear Gas Deployments

During the 2019 Hong Kong protests, public attention focused intensely on protest activities and law enforcement responses, particularly the use of weapons. The Hong Kong government's withholding of detailed records on weapon deployments created a significant demand to fill this information gap. In response, numerous non-governmental organizations developed online platforms providing real-time geolocation information throughout the movement.[4]

The most successful initiative emerged from LIHKG, a multi-category forum website launched in Hong Kong in 2016, often likened to a Hong Kong version of Reddit. During the 2019 protests, LIHKG became a central hub for protest-related discussions, coordination, and information sharing.[5]

LIHKG leveraged its popularity to introduce a web mapping service called HKMap. This platform gathers data from user submissions and verifies the information using trusted ground crews, live broadcasts, Telegram channels, and other sources. HKMap is accessible through both its webpage and smartphone apps, ensuring widespread availability. According to its developer, the service has covered nearly every protest across Hong Kong. The platform's impact was immediate and substantial: when

[4] The heightened sensitivity to weapon deployment, especially tear gas, stemmed from Hong Kong's 2014 Umbrella Movement, when police use of tear gas sparked widespread public controversy and became a defining moment in the city's civil discourse. This historical context explains why civil society organizations and citizens were particularly vigilant in monitoring and documenting weapon deployments during the 2019 protests, leading to extensive crowd-sourced documentation efforts.

[5] At its peak, LIHKG reported over 120,000 active users and millions of daily page views, establishing itself as one of Hong Kong's most influential online platforms during this period.

launched in early August 2019, it attracted over 10,000 unique visitors on its first day. By October 2019, HKMap had reached a peak of more than 200,000 unique daily users.

Figure A2 shows how real-time protest information and tear gas deployment data were disseminated on HKMap during an intense conflict in the Sheung Wan area on November 2, 2019. The map displays white flashes indicating tear gas locations, labeled as "TG" in English. This platform was continuously updated with precise coordinates and timings of each tear gas deployment, enabling us to obtain reliable geographical data for our analysis. The real-time nature and accuracy of this information allowed for a comprehensive tracking of tear gas incidents throughout the protests.

Our dataset documents 1,789 tear gas deployments across Hong Kong during the Anti-ELAB Movement. Figure A3 maps the spatial intensity of these deployments across Hong Kong's 18 administrative districts, revealing substantial variation in tear gas deployments across different areas of the city.

3.2. Residential Sale Transactions

We acquire the universe of residential housing transactions in Hong Kong from EPRC Ltd., covering the period from June 2016 to May 2023. This dataset provides detailed information about each housing transaction, including the transaction date, price, building name and address, floor size, floor level, flat number, flat orientation, names of buyers and sellers, as well as some additional remarks.[6]

Based on the full official names of sellers and buyers in the dataset, we could infer whether the transactors were originally from Hong Kong. This was because Hong Kong locals use a unique spelling system of their names, which is even different from mainland Chinese and Taiwanese. This identification method was also used in Fan et al. (2023). However, due to the Personal Data (Privacy) (Amendment) Ordinance that came into effect on October 8, 2021, name information was not available in the dataset after that date, and we were unable to identify the origins of transactors for transactions after that time point.

We constructed the final dataset on residential sale transactions through several steps. First, we focused exclusively on transactions of residential apartments whose prices were determined in the private market. We excluded government projects because their prices are largely determined by government regulations. We also dropped transactions with special contractual arrangements, such as deeds of gift and name

[6]In Hong Kong's property transactions, the Agreement for Sale and Purchase (ASP) and Deed of Assignment are the key legal documents. The ASP establishes the terms and conditions of the sale, including the agreed price, while the Deed of Assignment formally transfers the property's legal title from seller to buyer. We use the ASP date in our analysis as it represents when the transaction decision is made and the sale price is determined.

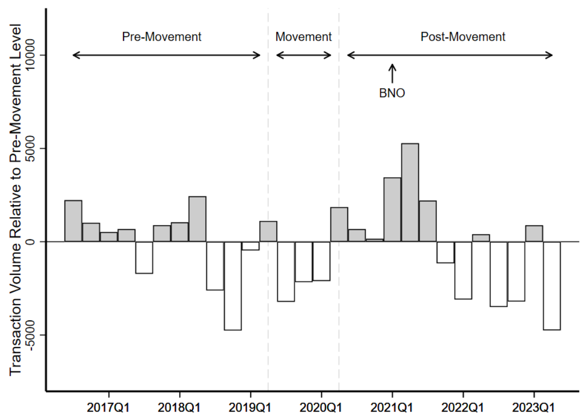


Figure 3. *Housing Market Transaction Volumes in Hong Kong, 2016-2023. The figure shows quarterly transaction volumes as deviations from the pre-movement quarterly average of 9,652 transactions. The timeline is divided into three periods: Pre-Movement (before June 2019), Movement (June 2019-April 2020), and Post-Movement (after May 2020). Gray bars indicate positive deviations while white bars represent negative deviations from the baseline. A notable point marked as "BNO" (British National Overseas visa scheme) in the Post-Movement period coincides with substantial increases in transaction volumes.*

changes. Second, we concentrated on the secondary market involving individual sellers and buyers, excluding primary market sales by real estate developers. Third, we exclude transactions that occurred during the protest period and the initial phase of the COVID-19 pandemic, specifically those before the announcement of the National Security Law (NSL). We do not include this period in the benchmark analysis as it is not the focus of our study and discuss about it in section 6.5. Finally, we excluded buildings located beyond the 1000-meter-circle area of any tear gas location documented in our tear gas dataset, given that they are irrelevant for our empirical analysis. Through this process, our final dataset comprised 188,511 transaction records from 9,654 residential buildings.

Figure 3 presents the quarterly transaction volumes in Hong Kong's housing market during our study period. The figure displays deviations from the pre-movement quarterly average of 9,652 transactions. The transaction volume generally fluctuated around this baseline throughout the pre-movement period. Notably, there was a significant spike in transactions when the BNO (British National Overseas) visa scheme was officially implemented by the UK government in early 2021, with volumes exceeding the baseline by approximately 5,000 transactions. This spike might be partially attributed to increased supply from Hong Kong emigrants to the UK.

Panel A of Table A1 reports the summary statistics of the sale transaction dataset. Consistent with the official price index, the transaction prices in the post-movement period (May 2020 – May 2023) are slightly higher than those in the pre-movement period (June 2016 – May 2019). The median (average) floor level in the dataset is

14, indicating a key feature of Hong Kong residential buildings: many are high-rise structures due to the city's high population density. Another notable characteristic is the prevalence of south-facing apartments, which are considered desirable in Hong Kong because they receive more sunlight. These apartments comprise approximately 30% of total transactions.

Hong Kong's official Tertiary Planning Units (TPUs) provide an ideal definition of communities for our analysis. These administrative zones, which divide the city into 214 areas, are specifically designed to capture spatially homogeneous characteristics in terms of population, land use, and socio-economic factors. The TPU scale–more granular than districts but broader than individual streets–effectively captures communities where households share similar characteristics. Our final dataset includes 9,654 buildings distributed across 131 TPUs.

3.3. Residential Rental Transactions and Airbnb Listings

In addition to sale transactions, we also collected rental transaction records in Hong Kong to further investigate whether the intense political conflicts altered the character and quality of local neighborhoods. Specifically, we aimed to examine potential effects on amenities, livability, and consequently, rental values. We gathered two types of rental transactions in Hong Kong.

The first type is traditional residential rental transactions, where residential property owners rent their apartments to prospective tenants. We collected residential rental records from Centaline Properties, one of the largest real estate brokers in Hong Kong, ensuring coverage of all major residential areas in the city. Similar to the sale transaction dataset, we excluded buildings located beyond the 1000-meter-circle area of any tear gas location. Our rental transaction dataset consists of 51,859 rental contracts from this broker, covering 5,000 unique residential buildings over the period from January 2019 to June 2023. The summary statistics of this rental transaction dataset are presented in Panel B of Table A1.

The second type of rental transactions comes from Airbnb listings in Hong Kong. Widely recognized as a pioneer of the sharing economy, Airbnb is a peer-to-peer marketplace for short-term rentals where suppliers (hosts) offer various accommodations to prospective renters (guests). Following the data collection method used in previous studies on Airbnb, we obtained information on Airbnb listings from Inside Airbnb.[7] This dataset consists of information about all Airbnb listings in Hong Kong, collected quarterly from 2018 Q3 to 2023 Q3. Airbnb assigns a unique ID number to each list-

[7]See prior works on Airbnb listings such as Barron, Kung, and Proserpio (2021), Farronato and Fradkin (2022), Garcia-López et al. (2020), and Koster, Van Ommeren, and Volkhausen (2021).

ing, allowing us to observe price changes for individual listings over time.[8] The final Airbnb listing dataset consists of 8,127 observations from 1,809 unique listings.

This dataset includes listing characteristics such as the price per night, the average guest review rating (ranging from 0 to 5), and the number of available days for booking in the coming 90 days. The summary statistics of the Airbnb listing dataset are presented in Panel C of Table A1.

3.4. Hong Kong District Council Election Data

We also examine the political ideologies and preferences of housing market participants. For this purpose, we utilize voting results from Hong Kong's District Council elections. The District Council elections, initiated in 1982 and held quadrennially. These councils advise the government on local matters such as public facilities, community activities, and environmental improvements. Notably, unlike some higher-level elections in Hong Kong, District Council elections have been conducted by universal suffrage since 1999, allowing all eligible residents to vote directly for their representatives. The electoral system divides each district into constituencies, with each constituency electing one council member. This structure ensures granular representation at the neighborhood level. Up to and including 2019, these elections were generally considered to be free and fair, providing an authentic representation of local political preferences.

In the 2019 local election, Hong Kong was divided into 452 District Council Constituency Areas (DCCAs), each electing one District Council member. All 452 seats from these directly elected constituencies were contested. The spatial distribution of vote shares for the anti-establishment camp in Hong Kong reveals significant variations. While approximately 57% of voters overall supported the anti-establishment camp, exceeding the 50% threshold, this proportion varied substantially across DCCAs. In some areas, nearly 90% of voters supported the anti-establishment (yellow) camp, while in others, about 70% of voters backed the pro-establishment (blue) camp.

4. Empirical Strategies

4.1. Treatment, Control and Conditional Randomness of Violence Exposure

A crucial aspect of our empirical strategy is developing a measure that captures residents' varying exposure to political violence, allowing us to identify its impact on high-stakes economic decisions. While this is generally challenging, we propose a

[8] We applied the data cleaning method used by Garcia-López et al. (2020), dropping observations of listings that did not receive any guest reviews in six months, as these were unlikely to be actively operating. We also excluded listings located beyond the 1000-meter-circle area of any tear gas location.

novel approach utilizing the distance to locations of tear gas deployment within the same local neighborhood as a proxy for exposure to political violence.

To operationalize this measure, we define our treated group as residential buildings within a 50-meter radius of tear gas deployment locations, reflecting the typical tear gas dispersal area, while our control group includes buildings between 50 and 1000 meters from these sites, approximating a typical neighborhood size in Hong Kong's dense urban environment. Our treatment indicator, TG-50, equals 1 for buildings within the 50-meter radius and 0 for those in the control area. We exclude transactions beyond the 1000-meter radius to maintain comparability. In our sale transaction dataset, approximately 6% of the samples fall within the treated group.

To illustrate our methodology, Figure 4 provides an example of our treated and control group construction using data from the Taikoo area of Hong Kong Island. The figure maps residential buildings (blue dots) and tear gas deployment locations (red crosses). Buildings within small blue circles centered at tear gas sites constitute our treatment group, while those located within the larger green circular area but outside the blue circles form our control group. This visualization demonstrates how we spatially defined our treated and control groups based on proximity to tear gas deployment sites. Buildings falling outside both boundaries are excluded from our analysis.

This construction offers several advantages. First, this measure effectively proxies exposure to violence. Tear gas deployment typically occurred in response to escalating tensions between protesters and police, and its use itself constitutes a form of violence. Consequently, residents living in buildings near tear gas deployment sites were more likely to have been exposed to conflicts, potentially witnessing confrontations firsthand or experiencing the effects of tear gas. While all residents in the same neighborhood would have similar probabilities of learning about conflicts and violence indirectly through media or social networks, those living closer to tear gas deployment sites had higher chances of direct exposure to violence and confrontations.

Second, our measure leverages the conditional randomness in the locations of tear gas deployment. We do not suggest tear gas deployment was entirely random spatially; it occurred along protest routes, which were not random. However, conditional on tear gas being deployed in a neighborhood, the exact locations were primarily idiosyncratic due to various incidental factors. These factors include fluctuating crowd density, unpredictable outbreaks of sentiment, and spontaneous conflicts that could trigger tension and subsequent tear gas use. Consequently, the conditional assignment of buildings to be very close to a tear gas deployment site versus those reasonably close is almost random. This conditional randomness provides a quasi-experimental setting

Figure 4. *Construction of Treated and Control Groups. This figure illustrates our spatial classification methodology using the Taikoo area as an example. We classify residential buildings (blue dots) based on their proximity to tear gas deployment sites (marked by red crosses). The treatment group consists of buildings within the small blue circles centered at tear gas sites, while the control group comprises buildings that fall within the larger green circle but outside the blue circles. Buildings outside both boundaries are excluded from our analysis.*

within neighborhoods, allowing us to compare outcomes for buildings with different levels of exposure to tear gas.

To strengthen the credibility of our quasi-experimental design, we conduct tests to corroborate the conditional randomness assumption underlying our treatment assignment. Specifically, we examine whether buildings' treatment status, determined by their proximity to tear gas sites, correlates with their pre-movement sale prices (June 2016 - May 2019). We regress apartment sale prices per square foot from this period on their buildings' TG-50 status, controlling for Tertiary Planning Unit (TPU) fixed effects, where TPUs are local communities defined in Section 3.2.

Table 1 presents these regression results. In column 1, conditional on TPU fixed effects, we find no statistically or economically significant difference in pre-movement sale prices between treated and control group buildings. Column 2, which allows each TPU to have its own time trend by controlling for TPU-month fixed effects, shows consistent results, further supporting our identification strategy.

Column 3 incorporates controls for six crucial building-level attributes: number of apartments (proxying living density), presence of a clubhouse or swimming pool (capturing amenities), and distances to the nearest MTR station, shopping mall, and

Table 1. Pre-movement Sale Prices and Tear Gas Deployment (apartment level)

	Dependent Variable: Sale Price per Square Foot (in log)			
	(1)	(2)	(3)	(4)
TG-50	0.000	0.003	-0.013	-0.013
	(0.024)	(0.025)	(0.017)	(0.016)
Area (in log)				-0.241***
				(0.017)
Floor (in log)				0.059***
				(0.003)
South				0.051***
				(0.005)
# of Apartments in Building (in log)			-0.019*	-0.034***
			(0.011)	(0.010)
Clubhouse			0.180***	0.200***
			(0.022)	(0.016)
Swimming Pool			0.074***	0.069***
			(0.019)	(0.016)
Distance to MTR Station (in log)			-0.024***	-0.028***
			(0.007)	(0.007)
Distance to Shopping Mall (in log)			-0.033***	-0.027***
			(0.011)	(0.008)
Distance to Main Road (in log)			-0.008	-0.007
			(0.007)	(0.006)
TPU FE	Yes	No	No	No
TPU*Year-Month FE	No	Yes	Yes	Yes
Observations	79,323	78,998	78,998	78,998
R-squared	0.284	0.422	0.494	0.550

Note: This table presents the results of an analysis examining pre-movement house sale prices in relation to tear gas deployment. The dependent variable is the logarithm of sale price per square foot, and the key independent variable is a binary indicator for buildings within a 50-meter radius of tear gas deployment sites (TG-50) during the movement. Across all specifications, there is no statistically significant difference in pre-movement prices between treated and control groups, indicating that tear gas deployment locations were conditionally random and not correlated with housing prices before the 2019 movement. Standard errors in parentheses clustered at TPU level; * p<0.1, ** p<0.05, *** p<0.01.

main roads (capturing location advantages). Column 4 adds housing apartment-level characteristics: flat area, floor level, and south-facing status. In both specifications, we find that proximity to future tear gas deployment sites does not predict pre-movement sale prices, supporting our conditional randomness assumption.

To further validate our research design, we examine whether tear gas deployment correlated with residents' political preferences. We use District Council Constituency Area (DCCA) election results as granular proxies for local political leanings, assigning each building the pro-democracy (yellow) voting share of its DCCA (discussed in section 3.4). With 452 DCCAs across Hong Kong serving 4.13 million registered voters, and a median DCCA size of 250,000 square meters (equivalent to a 500-meter square), these electoral data provide highly localized measures of political attitudes.

Table 2. Pro-Democracy Support and Tear Gas Deployments (building level)

	Dependent Variable: Pro-Democracy Vote Share			
	2019 Local Election		2015 Local Election	
	(1)	(2)	(3)	(4)
TG-50	-0.005	-0.005	-0.021	-0.018
	(0.004)	(0.003)	(0.017)	(0.014)
# of Apartments in Building (in log)		-0.001		0.000
		(0.001)		(0.007)
Clubhouse		0.000		-0.010
		(0.006)		(0.015)
Swimming Pool		0.007		-0.000
		(0.007)		(0.018)
Distance to MTR Station (in log)		0.003		0.012
		(0.004)		(0.016)
Distance to Shopping Mall (in log)		-0.002		-0.008
		(0.004)		(0.022)
Distance to Main Road (in log)		-0.001		-0.008
		(0.003)		(0.011)
TPU FE	Yes	Yes	Yes	Yes
Observations	9,653	9,653	8,237	8,237
R-squared	0.558	0.560	0.591	0.593

Note: This table examines the relationship between tear gas deployment during the 2019 protests and local voting patterns. The dependent variable is pro-democracy vote share at the building level. The key independent variable is a binary indicator for buildings within a 50-meter radius of tear gas deployment sites (TG-50). Across all specifications, no statistically significant differences in pro-democracy vote share are observed between treated and control groups. This suggests that tear gas deployment locations were conditionally random and not correlated with political preferences as revealed in both the 2015 and 2019 elections. Standard errors in parentheses clustered at TPU level; * p<0.1, ** p<0.05, *** p<0.01.

We regress the pro-democracy vote share on the building's TG-50 status, with results presented in Table 2. Using TPU fixed effects to control for unobserved local neighborhood characteristics (Column 1), we find no significant correlation between treatment status and pro-democracy vote share in a building's DCCA. This result persists after controlling for the six building-level attributes (Column 2), suggesting tear gas deployment was not systematically related to local pro-democracy support.

In Columns 3 and 4, we conduct a robustness check by examining vote shares from the 2015 local election. This election, occurring well before the 2019 protests, provides a baseline measure of political preferences unaffected by the protest movement. The results remain qualitatively consistent, showing no significant association between tear gas deployment locations and pre-existing political preferences of local residents.

4.2. The Difference-in-Differences Specification

Using the spatial variation in tear gas exposure, we implement a difference-in-differences strategy by estimating the following specification at transaction level:

$$y_{ijt} = \beta_0 + \beta^{DiD}\text{TG-50}_j \times \text{NSL}_t + \lambda_j + \delta_t^{TPU} + X_i\zeta + \varepsilon_{ijt}, \tag{1}$$

where y_{ijt} is the sale price per square foot of apartment i that is located in building j, transacted in month t. The treatment variable, TG-50_j, is a binary indicator equal to 1 for treated buildings (within 50 meters of a tear gas deployment site) and 0 for control buildings. NSL_t is a time dummy equal to 1 for the period after May 2020 and 0 for the period before June 2019. We include building fixed effects (λ_j) and TPU×year-month fixed effects (δ_t^{TPU}) to control for time-invariant building characteristics and time-varying TPU-level factors, respectively. The vector X_i represents apartment-level controls including size, orientation, and floor. To account for potential spatial correlation in the error terms, we cluster the standard errors at the Tertiary Planning Unit (TPU) level.

The coefficient of interest, β^{DiD}, captures the differential change in housing prices for buildings close to tear gas deployment sites (treated group) compared to those farther away (control group), from the pre-movement period (before June 2019) to the post-movement period (after May 2020), the introduction of the National Security Law.

Conditional Randomness Several aspects of this specification warrant mention. Our earlier analysis (Section 4.1) demonstrated that proximity to tear gas deployment sites within the same community did not predict sale prices before the movement. Consequently, we can more confidently attribute any systematic differences in post-movement transaction prices between treated and control buildings to the differential exposure to tear gas deployment, rather than to pre-existing group differences.

Targeted Time Frame We exclude sale transactions during the Anti-ELAB movement (June 2019 - April 2020). This exclusion allows for a cleaner interpretation of the estimated coefficient by comparing the pre-movement period with the period following the National Security Law's implementation. During the Anti-ELAB movement, numerous protest-related confounding factors (e.g., temporary road closures affecting house visits) obscured the short-term impact of tear gas exposure. In contrast, the post-movement period saw protest activities completely disappeared, with neighborhood living environments returning to normal. By this time, the immediate physical effects of tear gas (e.g., lingering smell, shattered windows) had been remediated, and protest-related confounding factors had dissipated.

Granular Fixed Effects We incorporate TPU×month fixed effects in this specification to control for the temporal trend of each TPU's housing price level. This stringent control ensures that our coefficient of interest, β^{DiD}, captures the differential changes in housing prices between the treated and control buildings within the same neighborhood, while mitigating potential bias from uncontrolled price trends at the TPU level. This approach strengthens the internal validity of our estimates by isolating the treatment effect from local market dynamics and time-varying neighborhood characteristics.

4.3. Dealing with Observable and Unobservable Confounders

Observable Confounders A potential concern with our identification is that fixed apartment-level and building-level characteristics might have differential impacts on housing prices before and after the protest period, potentially driving the differential price changes between treated and control groups. For instance, buildings near main roads were more likely to experience tear gas during the protests. If buyers and sellers subsequently re-evaluated the value of proximity to main roads, this could explain the observed price discount rather than the direct effect of tear gas exposure.

To address this concern, we allow apartment-level and building-level characteristics to have differential impacts over time. Specifically, we conduct a horse-race analysis by extending our baseline difference-in-differences model (Equation (3)) to include interaction terms between these characteristics and the treatment indicator (TG-50). We consider two main categories of characteristics. First, apartment-level characteristics include physical size (logged area), vertical position (logged floor level), and orientation (south-facing). Second, building-level characteristics encompass the scale of development (number of apartments), amenities (presence of clubhouse and swimming pool), and location advantages such as transportation (distance to MTR stations), commercial convenience (distance to shopping malls), and accessibility (distance to main roads). The analysis interacts each of these characteristics with the NSL period indicator to capture any differential valuation of these attributes during the post-movement period.

Unobservable Confounders One may also worry that some unobserved variables might be correlated with the locations of tear gas deployment, and these variables could also lead to inter-temporal sale price changes of housing apartments before and after the movement. To address this concern, we employ a randomization inference approach, which allows us to quantify the likelihood that other unobserved variables correlated with tear gas deployment sites indeed drive our findings.

The core idea of this approach is to construct pseudo-treated groups from buildings that are as likely to be close to tear gas deployment sites as those that are actually treated. If some unobserved variables largely influenced the locations of tear gas deployment, then it would be commonplace for the pseudo-treatment to yield similar effects to those estimated from the actual treatment in our difference-in-differences design. The key assumption of this approach is that conditional on the impact of the unobserved variable, there is still some randomness in where the actual sites of tear gas deployment occur.

To gauge the likelihood of our results being driven by unobserved variables, we conduct the pseudo experiments many times. This process allows us to obtain a distribution of the pseudo-estimated effects. By comparing the effect from the actual treatment to this distribution, we can reveal how likely it is that the estimated effect we found was driven by some unobserved variables rather than the treatment itself.

Specifically, the randomization inference estimation was conducted in the following steps. First, our original sample consisted of 724 buildings in the treated group and 8,930 in the control group. To construct pseudo-treated buildings, we focused on buildings located within a 200-meter-radius circle of tear gas deployment sites. These buildings were likely to be treated due to close proximity but did not receive treatment by chance. Given the narrow 200-meter radius, we assumed that all 3,632 buildings in this subsample had similar chances of being exposed to tear gas, although only some actually became part of the treated group. From this subsample of 3,632 unique buildings, we randomly chose a new group of 724 buildings to form the pseudo-treated group. The remaining 2,908 buildings, together with those beyond the 200-meter-radius circle but within the 1000-meter-radius circle, were classified as the pseudo-control group.

Second, using the pseudo-treated and control groups, we re-estimated the same difference-in-differences model from our main specifications to obtain a pseudo-treatment effect. If unobserved variables were driving both pseudo and actual treated groups, we would expect pseudo-treated and actual treated buildings to experience similar price changes in the post-movement period. However, if the pseudo-treatment effect was significantly different from the actual treatment effect, it would suggest that differential exposure to tear gas deployment, rather than unobserved variables, was driving our findings.

Finally, we repeated this procedure 2,000 times, allowing us to calculate the p-value representing how likely we were to find a pseudo-effect similar to or larger than the actual treatment effects in magnitude. This p-value quantifies the likelihood that any systematic differences in price changes between the treated and control groups could be attributed to unobserved factors rather than the treatment itself.

Table 3. Difference-in-Differences Estimation

	Dependent Variable: Sale Price per Square Foot (in log)		
	(1)	(2)	(3)
TG-50×NSL	-0.019**	-0.019**	-0.016**
	(0.008)	(0.008)	(0.006)
Apt Chars.	No	Yes	Yes
Building FE	Yes	Yes	Yes
Year-Month FE	Yes	Yes	No
TPU×Year-Month FE	No	No	Yes
Observations	162,369	162,369	161,687
R-squared	0.661	0.678	0.703

Note: This table presents the main results of the DID estimation from Equation (3), examining the impact of proximity to tear gas deployment sites on post-movement housing prices. The dependent variable is the logarithm of sale price per square foot. Column (1) reports the estimated effect without apartment-level controls, while Column (2) adds controls for apartment characteristics such as size, orientation, and floor level. Column (3) includes TPU × year-month fixed effects to control for time-varying TPU-level factors. Standard errors in parentheses clustered at TPU level; * p<0.1, ** p<0.05, *** p<0.01.

4.4. Dynamic Impacts

While the difference-in-differences strategy provides an estimate of the average differential effects in housing price changes across the treated and control groups after the introduction of the National Security Law (NSL), we are also interested in the dynamic pattern of these effects over time.

To achieve this objective, we employ an event study model to investigate the dynamic impacts of differential exposure to violence following the announcement of the National Security Law (NSL). We estimate the following specification:

$$y_{ijt} = \beta_0 + \sum_{\tau=-8}^{8} \theta_\tau \text{TG-50}_j \times \text{Period}_\tau + \lambda_j + \delta_t^d + X_i\zeta + \varepsilon_{ijt}, \tag{2}$$

where y_{ijt} is the sale price for apartment i in building j at month t. Our treatment variable TG-50$_j$ interacts with quarterly time indicators Period$_\tau$. For the pre-movement period ($\tau < 0$), Period$_\tau$ counts quarters before the movement's start. For the post-movement period ($\tau \geq 0$), it counts quarters after the NSL announcement. We set Period$_{-1}$ as our reference period (March-May 2019). The model includes apartment-level controls X_i, building fixed effects λ_j, and TPU×month fixed effects δ_t^d. Standard errors are clustered at the Tertiary Planning Unit (TPU) level.

5. Results

5.1. The Difference-in-Differences Analysis

We begin by estimating a specification with building and year-month fixed effects. Columns (1) and (2) of Table 3 report the results without and with apartment-level characteristics, respectively. The estimated coefficients on the interaction term remain consistently negative and statistically significant, with similar magnitudes across specifications. This consistency suggests that our findings are not driven by differences in apartment characteristics.

Column (3) presents our main specification from Equation (3), which includes TPU $\times$ Year-Month fixed effects. This result suggests that in the post-movement period, buildings closer to tear gas deployment sites (treated group) were sold on average 1.6% less in sale prices compared to buildings relatively further away (control group), relative to the price differences between these groups in the pre-movement period.

Sensitivity Analysis Having established our main finding, we now examine its robustness through sensitivity analyses that exploit the spatial nature of our research design. By systematically varying the treatment radius, we can test whether the price effects exhibit a theoretically consistent spatial pattern. If exposure to political violence indeed drives our results, we would expect the magnitude of the price discount to follow a distance-decay pattern: the effects should be strongest within our baseline 50-meter radius and gradually attenuate as we expand the treatment zone. Such a pattern would not only validate our main findings but also provide additional support for our hypothesis that proximity to violence, rather than unobserved neighborhood characteristics, generates the observed price discounts.

To assess the spatial decay of treatment effects, we re-estimate Equation (3) using progressively larger treatment radii. We define four treatment zones: 50, 100, 150, and 200 meters from tear gas deployment sites. For each radius, we reconstruct the treated and control groups and estimate our main specification. Columns (1) through (4) of Table 4 present these results, corresponding to the 50-, 100-, 150-, and 200-meter treatment definitions, respectively.

Column (1) replicates our main finding, shown here for comparison purposes, with a statistically significant effect of -1.6%. Expanding the radius of the treatment group definition to 100 meters (Column 2) yields a smaller but still significant effect of -0.9%. However, when further extending the treatment radius to 150 meters (Column 3) and 200 meters (Column 4), the effect becomes statistically indistinguishable from zero.

In Column (5), we introduce a new dummy variable, TG-50-100, representing transactions from buildings located 50 to 100 meters from tear gas deployment sites. We add this variable's interaction term with NSL to Equation (3). The results show that our key estimate only marginally increases in magnitude, while the coefficient on the new interaction term is negligible and statistically insignificant. This finding suggests that the discount effect observed in columns (1) and (2) is primarily driven by buildings within 50 meters of tear gas deployment sites, with properties in the 50-100 meter

__Table 4.__ Difference-in-Differences: Varying the Definition of Treatment Group

	Dependent Variable: Sale Price per Square Foot (in log)				
	(1)	(2)	(3)	(4)	(5)
TG-50 × NSL	-0.016**				-0.017**
	(0.006)				(0.007)
TG-100 × NSL		-0.009**			
		(0.004)			
TG-150 × NSL			0.001		
			(0.004)		
TG-200 × NSL				0.000	
				(0.004)	
TG-50-100 × NSL					-0.004
					(0.004)
Apt Chars.	Yes	Yes	Yes	Yes	Yes
Building FE	Yes	Yes	Yes	Yes	Yes
TPU×Year-Month FE	Yes	Yes	Yes	Yes	Yes
Observations	161,687	161,687	161,687	161,687	161,687
R-squared	0.703	0.703	0.703	0.703	0.703

Note: This table reports the DID results using various treatment radius definitions. The dependent variable is the logarithm of sale price per square foot, while the treatment variables represent different treatment radius to tear gas deployment sites: 50 meters, 100 meters, 150 meters, and 200 meters, corresponding to Columns (1), (2), (3), and (4), respectively. In all specifications, we include controls for apartment characteristics such as size, orientation, and floor level. Standard errors in parentheses clustered at TPU level; * p<0.1, ** p<0.05, *** p<0.01.

range having minimal impact.

This spatial pattern aligns precisely with our expectations. Those residing closest to areas of tear gas use – and thus most directly exposed to the violence and disruption – appear to have been the most likely to consider selling their houses at a discount, potentially engaging in a fire sale to facilitate emigration.

Our research design permits a complementary sensitivity test by varying the control group definition while maintaining a fixed 50-meter treatment radius. We systematically narrow the control group from buildings within 50-1000 meters to those within 50-200 meters of tear gas sites. Table A2 presents these results. The estimated price discount remains remarkably stable, ranging from -1.6% to -1.8% and retaining statistical significance across all specifications. Notably, this consistency persists even when restricting the control group to buildings within 200 meters of tear gas sites (Column 5). The robustness of our estimates to control group definition suggests that buildings beyond the 50-meter treatment radius experienced similarly low exposure to violence, forming a homogeneous control group.

5.2. Horse-race Analysis and Randomization Inferences

Table A3 presents the results of our horse race analysis, examining the interaction effects of various apartment-level and building-level characteristics with the treatment indicator (TG-50). Columns (1) to (3) focus on individual apartment characteristics including apartment size, floor level, and orientation, while column (4) incorporates all these apartment-level characteristics simultaneously. Columns (5) to (11) examine building characteristics, starting with the scale of the building (number of apartments), presence of amenities (clubhouse and swimming pool), and proximity measures to key facilities (MTR stations, shopping malls, and main roads), with column (11) incorporating all building characteristics simultaneously. Several interactions show statistical significance, indicating that certain property attributes do have time varying impacts on housing prices.

However, the most crucial observation from this analysis is the robustness of the difference-in-differences estimators. Despite the inclusion of these various interaction terms, the coefficient on the interaction between TG-50 and NSL remains consistently negative and statistically significant across all specifications, ranging from -0.015 to -0.020 ($p < 0.05$ or $p < 0.01$). This stability persists even in the most demanding specifications - Column (4), which controls for all apartment characteristics simultaneously, and Column (11), which includes all building characteristics together. The consistency of the TG-50 $\times$ NSL coefficient implies that while certain apartment characteristics do contribute to differential price changes, they do not account for or undermine the key discount effect we identified.

After addressing concerns about observable apartment-level and building-level characteristics, we examine whether unobserved variables correlated with tear gas deployment sites might drive our findings. To address this concern, we employ the randomization inference approach discussed in Section 4.3 to explore whether our results are likely driven by such unobserved confounders.

Figure 4(a) presents the distribution of the effects of pseudo-treatments constructed from buildings within a 200-meter radius of tear gas deployment locations. In this circle, there are 3,632 buildings that are used to construct pseudo-treatment group. Among 2,000 random pseudo-treatments, most of the estimated coefficients are close to zero, with the distribution having a mean of -0.00007 and a standard deviation of 0.005. Given that the actual treatment effect of a -0.016 price discount is larger in magnitude than the 2.5th percentile of the coefficient distribution (-0.01), we can reject the null hypothesis that the observed discount effect is due to unobserved characteristics of buildings located near each other.

To conduct a more stringent randomization inference test, we narrow the radius for constructing pseudo-treatments from 200 meters to 150 meters. In the stricter 150-meter radius area, there are only 2,747 buildings could potentially be pseudo-treated which consists of the 724 actually treated buildings. These buildings are expected to share more similar characteristics with the actual treated group. In this exercise, we obtain the distribution of coefficients from estimating the pseudo-treatment effects and present them in Figure 4(b). This distribution has a mean of 0.0003 and a standard deviation of 0.005, and the actual treatment effect of -0.016 falls beyond the 95% confidence interval of the pseudo-treatment effects. The results are still similar to those of 200-meter randomization inferences, leading us to be more confident that it is highly unlikely to observe a significant discount effect without considering the buildings' varying levels of exposure to tear gas deployments.

5.3. Dynamic Effects

Now we turn to another dimension of our empirical explorations: the dynamic effects. Towards this end, we estimate the event study model, specified by Equation (2). The results of this analysis are presented in Figure 5. The x-axis represents the number of quarters relative to the event of interest, with 0 marking the announcement of the NSL. Negative numbers indicate quarters before the 2019 movement, while positive numbers show quarters after the NSL announcement. The y-axis displays the estimated coefficients of the interaction terms, representing the differential effects on housing prices in treated buildings compared to control areas over time. The 95% confidence intervals are represented by vertical dashed lines in the figure.

In the pre-movement period, from quarters -12 to -1, we find that the estimated coefficients fluctuate around zero, with all confidence intervals including zero. The pattern of coefficients and their confidence intervals in this period suggests no significant pre-trends, supporting the parallel trends assumption crucial for difference-in-differences analysis.

The event study results reveal a compelling temporal pattern. A noticeable trend occurs starting from quarter 0, coinciding with the NSL announcement. The estimated coefficients, now marked by solid circles, show a clear downward trend. This negative effect appears to intensify over time, reaching its largest magnitude in quarter 3. Importantly, quarter 3 coincides with the enactment of the BNO policy. The difference-in-differences estimates are statistically significant for a period of half a year, specifically in quarters 3 and 4. Following this period of pronounced effect, we observe a tapering off of the estimated impacts. While the coefficients remain consistently negative in subsequent quarters, they are no longer statistically significant.

The smooth downward trend in the estimated effects right after quarter 0 is intriguing and will be informative for understanding the mechanism underlying the

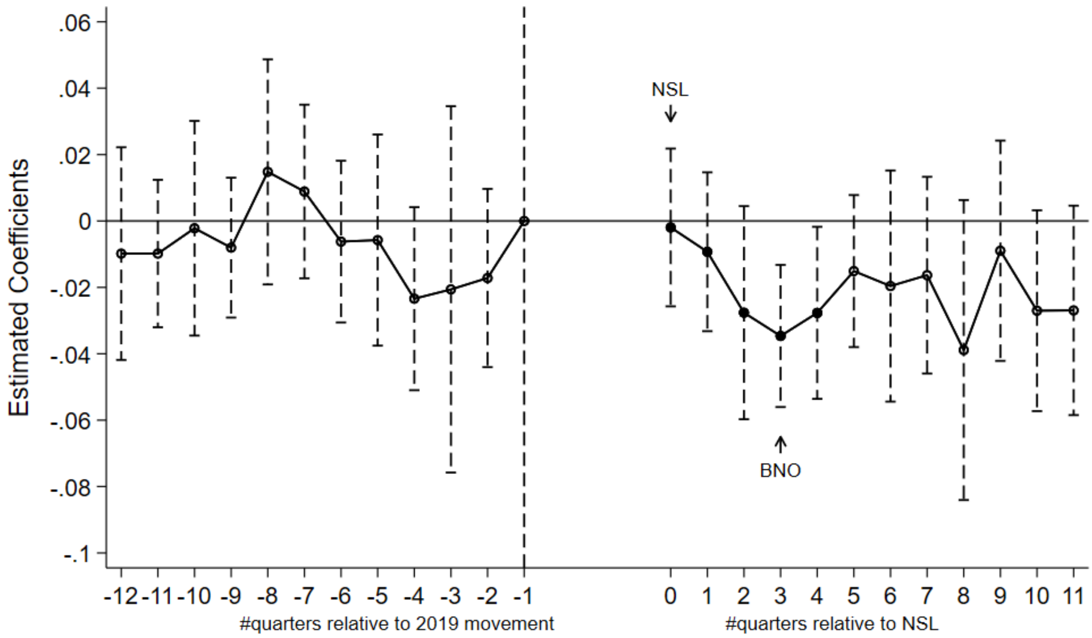


Figure 5. *Event Study. This figure shows the dynamic effects on housing prices in treated buildings relative to control buildings. The x-axis represents the number of quarters before and after the event, with negative values indicating quarters prior to the 2019 movement and positive values reflecting quarters after the NSL announcement. The dots denote the estimated coefficients for each period, while the vertical dashed lines represent the 95% confidence intervals.*

temporary but significant discount effects we discover. We return to this pattern in Section 6.

6. Exploring Mechanisms

We next examine where, for whom, and through what actions the housing price effects were more pronounced to understand the underlying mechanisms. Our analysis collectively suggests that exposure to political violence altered individuals' political beliefs about Hong Kong society, influencing their market behavior once emigration became possible. This aligns with the shattered assumptions theory (Janoff-Bulman 1989), where traumatic events can disrupt core beliefs about societal governance and justice, leading to worldview revisions. In Hong Kong's context, direct exposure to police violence likely eroded residents' trust in institutional protection and local governance. Alternative explanations, such as neighborhood amenity damage, are inconsistent with our findings, particularly given the absence of effects in rental markets.

6.1. Experiencing Political Violence

We conjecture that direct exposure to political violence, proxied by tear gas deployment, affected homeowners through their personal experiences. Such experiences

Table 5. *Heterogeneous Effects by Varying Potential for Direct Sensory Exposure*

	Dependent Variable: Sale Price per Square Foot (in log)					
	Floor Level		Window View		TG Time	
	Low	High	City View	Others	18:00-20:59	00:00-02:59
	(1)	(2)	(3)	(4)	(5)	(6)
TG-50$\times$NSL	-0.022**	-0.007	-0.026**	-0.001	-0.029**	-0.020
	(0.009)	(0.007)	(0.011)	(0.013)	(0.014)	(0.012)
Apt Chars.	Yes	Yes	Yes	Yes	Yes	Yes
Building FE	Yes	Yes	Yes	Yes	Yes	Yes
TPU$\times$Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	79,569	80,281	21,785	33,443	92,423	90,500
R-squared	0.721	0.725	0.622	0.648	0.700	0.712

Note: This table presents heterogeneous effects of tear gas exposure on housing prices across different property and tear gas incident characteristics. The dependent variable is the logarithm of sale price per square foot. Columns (1) and (2) compare effects between lower and higher floors. Columns (3) and (4) examine differences between apartments with city views versus other views. Columns (5) and (6) contrast effects by the timing of tear gas incidents. All specifications include apartment characteristics controls, building fixed effects, and TPU $\times$ year-month fixed effects. Standard errors in parentheses clustered at TPU level; * p<0.1, ** p<0.05, *** p<0.01.

likely operate through multiple sensory channels - most notably visual exposure to violent confrontations and police interventions, and olfactory exposure to tear gas residue. Following this logic, we would expect the impact of political violence to be stronger where sensory experiences were more intense. To test this mechanism, we conduct a series of heterogeneity analyses that exploit variations in potential exposure intensity across different apartments.

Vertical Heterogeneity To test our conjecture about sensory experiences, we first exploit variation in vertical exposure to political violence. The vertical dimension offers a unique advantage in identifying the role of direct experiences: apartments within the same building share identical horizontal locations but differ markedly in their exposure to street-level events. This variation is particularly relevant in Hong Kong's dense urban environment, where high-rise residential buildings commonly exceed 30 floors. If direct sensory experiences indeed drive the price effects, we would expect the impact to vary with vertical distance from the street level, where the political violence occurred.

To implement this test, we partition our sample based on vertical location within buildings, using the median floor (14th floor) as the cutoff. This creates two subsamples: one comprising transactions of apartments above the 14th floor and another for those on or below the 14th floor. We then re-estimate Equation (3) separately for each subsample and report the results in Columns (1) and (2) of Table 5.

We find that the estimated coefficient in the lower floor sample (Column 1) is still significant and the magnitude becomes larger (-2.2%) than that in the full sample. Interestingly, the estimated coefficient in the higher floor sample (Column 2) is negative but with a much smaller magnitude (-0.7%) and is not statistically significant. The contrast between the two samples suggests that the effects we find from our full sample are primarily driven by transactions of apartments located on lower floors, while there is no statistically significant effect for those located on higher floors.

Three points related to this result deserve mention. First, this vertical pattern suggests that the spatial differences across treated and control groups are less meaningful for homeowners who resided on higher floors: both treated and control groups could have much less exposure to violence given their vertical distance from the site, even though there are spatial variations between them horizontally. It suggests that the negative impact is not uniform across all apartments in treated buildings, but rather concentrates on those more directly exposed to street-level disturbances. This is consistent with our conjecture that it is the exposure to violence that leaves a mark.

Second, this pattern is consistent with the physical properties of tear gas dispersion. Tear gas typically spreads 30-50 meters horizontally but, being heavier than air, rises only 6-10 feet vertically. This chemical property explains why our 50-meter horizontal treatment radius captures exposure effects, while vertical distance sharply moderates the impact.

Third, the vertical heterogeneity helps mitigate potential confounding factors. If unobserved spatial characteristics correlated with tear gas deployment were driving our results, we would expect similar effects across all floors. The concentration of discount effects in lower floors suggests our findings were not driven by location specific factors.

Window View Heterogeneity While vertical distance captures one dimension of exposure variation, window views offer another way to test our sensory experience hypothesis. Apartments with different window orientations within the same building can experience varying degrees of visual exposure to political violence, even if they are on the same floor. If experiences with violence drive the price effects, we would expect stronger impacts for apartments with unobstructed views of protest areas compared to those with alternative views.

Using detailed property information from available floor plans and real estate agencies, we classify apartment views into four categories: sea view (ocean visible from windows), city view (urban landscape including streets and businesses), nature view (greenery or natural landscapes), and no view (no significant external view).[9]

[9] We implemented a three-step classification process: (1) analyzing digital maps and architectural floor plans, (2) examining photographic evidence from real estate listings, and (3) conducting physical site inspections and consulting licensed agents for ambiguous cases. This systematic approach enabled reliable view categorization for apartments in 1,291 buildings (72 treated and 1,219 control).

To implement this test, we separate our sample into two groups based on window views: apartments with city views that overlook streets and buildings, and those with alternative views (sea, nature, or no view). We then re-estimate Equation (3) separately for each subsample and report the results in Columns (3) and (4) of Table 5. For apartments with city views, which provide direct visual exposure to street-level events, we find a significant price discount of 2.6%. In contrast, apartments with alternative views show no significant price effect, with the estimated coefficient being close to zero (-0.1%) and statistically insignificant.

This stark contrast is particularly telling. Apartments with city views would have provided occupants with direct visual exposure to protest activities and police interventions, while those with alternative views would have been relatively shielded from these disturbing scenes despite being in the same physical location. The fact that price effects concentrate in apartments with city views, even after controlling for building fixed effects, suggests that visual exposure to political violence played a crucial role in shaping residents' experiences.

Timing Heterogeneity To further explore the role of direct sensory experiences, we examine heterogeneous effects based on the timing of tear gas deployment. Among the 1,789 tear gas deployment locations, 333 occurred between 18:00 and 20:59 and 269 took place between 00:00 and 02:59. We separate tear gas incidents into these two time windows: prime time (18:00-20:59) when residents are typically at home and awake, and late night (00:00-02:59) when most residents are likely asleep. We then re-estimate Equation (3) separately for each subsample and report the results in Columns (5) and (6) of Table 5. Properties exposed to tear gas during prime time show a significant price discount of 2.9%, while those affected during late night hours exhibit a smaller and statistically insignificant effect of -2.0%.

This temporal contrast provides additional support for our sensory experience hypothesis. Residents were more likely to directly witness police interventions and experience tear gas effects during prime time hours, when they were awake and at home. In contrast, residents were largely unaware of incidents occurring in the middle of the night while they were asleep, despite the same level of tear gas deployment. The stronger price response to prime time incidents reinforces the importance of direct sensory exposure in shaping how residents experience and value properties affected by political violence.

6.2. Interpreting Violence: Political Leaning

Our previous analysis suggests the importance of direct exposure to political violence. Building on this finding, we now examine how the impact of protest-related violence on housing prices differs across neighborhoods with varying political preferences. Al-

Table 6. Heterogeneous Effects Across Political Districts (District Council Election)

	Dependent Variable: Sale Price per Square Foot (in log)			
	Districts in 2019 Election		Districts in 2015 and 2019 Elections	
	Yellow	Blue	Yellow	Blue
	(1)	(2)	(3)	(4)
TG-50 × NSL	-0.021***	0.002	-0.017**	-0.006
	(0.007)	(0.010)	(0.008)	(0.010)
Apartment Characteristics	Yes	Yes	Yes	Yes
Building FE	Yes	Yes	Yes	Yes
TPU×Year-Month FE	Yes	Yes	Yes	Yes
Observations	97,737	63,043	69,767	47,123
R-squared	0.697	0.729	0.682	0.736

Note: This table presents heterogeneous effects across districts with different political leanings. In columns 1 and 2, districts are classified based on their pro-democracy vote share in the 2019 election relative to the median, with 4,747 buildings in yellow districts (above median) and 4,907 in blue districts (below median). Columns 3 and 4 use a stricter classification based on consistent voting patterns across both 2019 and 2015 elections, identifying 3,662 buildings in consistently yellow districts and 3,888 in consistently blue districts. TG-50 is an indicator for buildings within 50 meters of tear gas deployment sites, and NSL is an indicator for the post-movement period. * p<0.1, ** p<0.05, *** p<0.01.

though exposure to violence itself is significant, how different groups interpret these events may equally influence their belief formation.

We expect that residents with different political preferences may interpret the same experiences through distinct lenses. Specifically, we hypothesize that pro-democracy areas may exhibit stronger price discounts compared to pro-establishment areas, even when exposed to similar levels of violence. This hypothesis builds on social identity theory (Tajfel and Turner 1979), which suggests that individuals' ideological preferences significantly shape their interpretation of political events and subsequent responses.

In Hong Kong's context, residents likely interpreted tear gas deployments through different ideological lenses. Pro-establishment residents might have viewed these incidents as necessary law enforcement actions, while pro-democracy supporters might have interpreted them as state suppression of legitimate political expression. These divergent interpretations could lead to different assessments of Hong Kong's institutional trajectory: pro-democracy supporters might grow increasingly pessimistic upon witnessing political violence in their neighborhoods, while pro-establishment supporters might remain relatively unaffected. Consequently, we expect to observe stronger price discounts in pro-democracy neighborhoods, where residents may interpret protest-related violence negatively.

To measure the political leaning of each location, we utilize data from the 2019 District Council election, held on November 24, 2019. As discussed in Section 3.4, Hong Kong's political landscape was characterized by two major camps: the pro-establishment camp and the pro-democracy opposition. The primary political cleavage centered on attitudes toward Beijing's governance. With an unprecedented voter turnout of 71.2% and nearly 3 million votes cast, this election was widely regarded as a genuine expression of public opinion. We use the vote share of the pro-democracy (yellow) camp within each District Council Constituency Area (DCCA) as our measure of local political preferences.

Using the pro-democracy camp's vote share, we rank all 452 DCCAs and partition them into two subsamples based on the median. DCCAs with above-median vote shares are classified as "yellow districts," and those below the median as "blue districts." We estimate our difference-in-differences specification (Equation (3)) separately for each subsample.

Columns (1) and (2) in Table 6 present our results. In column (1), which shows estimates for districts with high (yellow) pro-democracy vote shares, we find a significant negative coefficient of -0.021 ($p < 0.01$) for the interaction between proximity to tear gas sites (TG-50) and the NSL period. This indicates that in strongly pro-democracy areas, properties within 50 meters of tear gas deployment sites experienced a 2.1 percentage point price discount following the NSL implementation, relative to more distant properties. In contrast, column (2), which presents estimates for districts with low (blue) pro-democracy vote shares, shows a small and statistically insignificant coefficient. This suggests that in pro-establishment areas, proximity to tear gas sites had no discernible effect on property prices in the post-NSL period.

One might worry that exposure to political violence could impact residents' political leanings, potentially blurring the interpretation of the findings. To address this concern, we define yellow and blue areas more stringently by focusing on areas that have consistently maintained their political orientation. Specifically, districts are classified as yellow if their pro-democracy vote share exceeded the median in both the 2019 and 2015 elections, and blue if their pro-democracy vote share was below the median in both elections. Districts that shifted classifications between the two elections are excluded from our sample. We conduct the same analysis and present the results in Columns (3) and (4) in Table 6, which are very similar to those reported in Columns (1) and (2).

These findings support our hypothesis that residents in pro-democracy areas respond more strongly to exposure to protest-related violence. The systematic variation in price effects across districts, from significant discounts in pro-democracy areas to negligible impacts in pro-establishment neighborhoods, suggests that political preferences likely shape how residents interpret and react to political violence in their communities.

6.3. From Violence Exposure to Economic Decisions: The British National (Overseas) Visa Scheme (BNO)

Our previous analyses demonstrated how direct exposure to political conflict influenced housing market outcomes both spatially (areas with visual exposure to violence) and politically (neighborhoods with pro-democracy leanings). However, the underlying considerations driving property owners' decisions require further investigation. This section extends our analysis by examining the impacts of a new emigration opportunity. Specifically, we analyze how the introduction of the British National (Overseas) [BNO] visa scheme and its eligibility requirements shaped market dynamics during this period.

As discussed in Section 2.1, the UK government's BNO scheme significantly reduced emigration barriers for Hong Kong residents seeking relocation to the UK. We argue that exposure to political violence influenced residents' likelihood of utilizing this emigration pathway. Specifically, those who experienced intense political violence in their neighborhoods were more likely to develop pessimistic views about Hong Kong's future, increasing their propensity to emigrate once the BNO scheme became available.

6.3.1. The Timing of BNO

To test this hypothesis, we examine building turnover patterns and investigate whether homeowners more exposed to political violence showed a higher propensity to sell their properties after the BNO scheme's enactment. We analyze the extensive margin by constructing monthly annualized turnover rates for each building, calculated as the percentage of apartments sold per month and annualized for temporal standardization. We modify our baseline specification (Equation 3) by using the building-level turnover rate as the dependent variable and removing apartment-level characteristic controls.

Column (1) of Table 7 reproduces the price effect for comparison purpose. Column (3) presents the turnover rate pattern: treated buildings experienced a 0.4 percentage point higher turnover rate during the post-NSL period compared to control buildings, relative to the pre-movement period. Given the sample's mean turnover rate of 3%, this represents a 13.3% increase in market activity. This rise in turnover among buildings more exposed to political violence aligns with our hypothesis about accelerated liquidation of housing assets.

The combination of price discounts and higher turnover rates supports a fire-sale mechanism: the observed pattern of price discounts coupled with higher turnover rates suggests that sellers in affected buildings appear more eager to dispose of their

Table 7. BNO Timing: Price and Turnover Rate Analysis

	Dependent Variable:			
	Price		Turnover Rate	
	(1)	(2)	(3)	(4)
TG-50 × NSL	-0.016**		0.004**	
	(0.006)		(0.002)	
TG-50 × Interim Period		-0.007		0.002
		(0.008)		(0.003)
TG-50 × BNO		-0.019***		0.005**
		(0.007)		(0.002)
Apartment Characteristics	Yes	Yes	-	-
Building FE	Yes	Yes	Yes	Yes
TPU×Year-Month FE	Yes	Yes	Yes	Yes
Observations	161,687	161,687	704,669	704,669
R-squared	0.703	0.703	0.048	0.048

Note: This table examines how proximity to tear gas deployment sites affects housing prices and transaction volumes. Columns 1-2 present the effects on housing prices, where Column 1 shows the overall discount effect and Column 2 separates the effects into the interim period and the period after official BNO announcement. Columns 3-4 present parallel analyses for turnover rates, calculated as the annualized percentage of apartments sold in a building per month. Standard errors (in parentheses) are clustered at the TPU-month level. * p<0.1, ** p<0.05, *** p<0.01.

properties, accepting lower prices to facilitate quicker sales.

To examine the BNO scheme's timing effects, we divide the post-movement period into two phases: the Interim Period (May 2020 to January 2021, between NSL announcement and BNO enactment) and the BNO Period (after February 2021). We create corresponding dummy variables Interim Period and BNO for these phases. We then modify our main specifications from Section 5.1 (Table 3) for both price and turnover effects by replacing the single interaction term TG-50 × NSL with two separate interactions: TG-50 × Interim Period and TG-50 × BNO. This specification allows us to isolate the effects during each phase.

Column (2) of Table 7 shows the housing price effects. While the coefficient on TG-50 × Interim Period is statistically insignificant, TG-50 × BNO reveals a larger and significant effect. Similarly, Column (4) demonstrates that for turnover rates, the primary effect emerged after the BNO's enactment. These patterns suggest that residents with greater exposure to violence were more inclined to utilize the emigration pathway once the BNO scheme was implemented.

6.3.2. The Eligibility of BNO: Local v.s. Non-local

We next exploit variation in BNO visa scheme eligibility to further validate our mechanism. The scheme's eligibility criteria create a natural distinction: only individuals born in Hong Kong before 1997 ("local residents") qualify, while those born elsewhere

Table 8. *Effect of Seller and Buyer Identity*

	Dependent variable: Sale Price per Square Foot (in log)								
	Pre-Movement Period			Interim Period			BNO Period		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TG-50 × Local Seller	-0.009		-0.009	0.022		0.022	-0.083***		-0.078***
	(0.013)		(0.014)	(0.023)		(0.023)	(0.029)		(0.027)
TG-50 × Local Buyer		-0.006	-0.005		-0.012	-0.012		-0.032	-0.025
		(0.010)	(0.010)		(0.011)	(0.011)		(0.023)	(0.021)
Apt Char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Building FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TPU×Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	67,570	67,570	67,570	18,952	18,952	18,952	20,474	20,474	20,474
R-squared	0.707	0.707	0.707	0.750	0.750	0.750	0.757	0.757	0.758

Note: This table presents DID estimates of the impact of local versus non-local seller and buyer identities on housing prices over three periods: Pre-Movement, Interim, and BNO Periods. Columns (1), (4), and (7) display the effects of local sellers, while columns (2), (5), and (8) show the effects of local buyers. Columns (3), (6), and (9) account for interactions between both seller and buyer identities. In all specifications, we include controls for apartment characteristics such as size, orientation, and floor level. Standard errors in parentheses clustered at TPU level; * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

("non-local residents") must pursue alternative emigration pathways. This institutional feature provides an additional test of our hypothesis. If exposure to political violence indeed shapes emigration decisions through belief changes, we would expect stronger selling pressure in treated buildings specifically from BNO-eligible local residents, who face lower emigration costs through the scheme, compared to non-local residents who experienced similar levels of violence but lack BNO eligibility.

To implement this test, we analyze how seller and buyer identity affects house prices across three periods: the Pre-Movement Period (before the protests began), the Interim Period (between NSL announcement and BNO enactment), and the BNO Period (after BNO enactment). We define two indicator variables: Local Seller and Local Buyer, which take the value of 1 for local residents and 0 for non-local residents. Using these indicators, we modify our baseline difference-in-differences specification (Equation (3)) by substituting NSL with Local Seller and estimate this modified specification separately for each sub-period.

Table 8 presents our analysis across the three periods. We first examine the Pre-Movement Period in Column (1), where the coefficient on TG-50 × Local Seller is economically small and statistically insignificant. This absence of price differentials between local and non-local sellers in buildings that would later experience varying levels of tear gas exposure (captured by TG-50) is informative for our identification. It confirms that prior to the protests, properties in areas that would later experience violence did not exhibit systematic price differences based on seller identity. The absence of pre-existing price differentials based on seller identity in buildings closer to violence sites strengthens our interpretation that subsequent changes in price patterns reflect the impact of violence exposure rather than pre-existing trends or sorting.

During the Interim Period, when the BNO scheme was not yet enacted, the coefficient in question remaining statistically insignificant (column 4). However, a marked shift emerges in the BNO Period (column 7), where we observe a significant negative coefficient of -0.083 ($p < 0.01$). This implies that local sellers offer an additional 8.3 percentage point discount relative to non-local sellers if they reside close to the tear gas deployment sites during the movement, compared to those who resided further away. This finding suggests that the official enactment of the BNO scheme had a stronger impact on local sellers relative to non-local sellers, particularly in buildings closer to areas that experienced violence during the movement.

Examining the buyer-side results in columns (2), (5), and (8), we observe a distinctly different pattern. The coefficients for TG-50 $\times$ Local Buyer remain statistically insignificant across all three periods, providing an important placebo test. This null result is intuitive: buyers of properties near tear gas sites did not necessarily experience the protests in these locations, and thus their purchasing behavior should not systematically vary based on the property's exposure to protest violence.

Finally, examining columns (3), (6), and (9), which simultaneously incorporate both seller and buyer identity interactions, we find patterns consistent with our separate analyses. In the BNO Period (column 9), the coefficient for TG-50 $\times$ Local Seller remains significantly negative (-0.078, $p < 0.01$), while the coefficient for TG-50 $\times$ Local Buyer remains statistically insignificant. The persistence of the seller effect in this joint specification, which controls for potential buyer-side heterogeneity, reinforces our interpretation that the BNO scheme primarily influenced the pricing decisions of local sellers who experienced greater exposure to political violence.

This stark contrast between seller and buyer behavior–significant discounts from local sellers but no systematic response from local buyers–supports our hypothesis that the price effects stem from the interaction between personal violence exposure and newly available emigration opportunities, rather than broader market conditions or location-specific factors.

6.4. Demand Side Factors?

The evidence suggests our findings stem from supply-side factors - specifically residents' exposure to violence and their subsequent housing decisions following the BNO scheme's introduction. Alternative demand-side explanations, particularly those based on neighborhood amenity deterioration, appear inconsistent with several key patterns.

First, our analysis focuses on the post-movement period when intense conflicts had already subsided, making it unlikely that temporary physical damage from tear gas drove the price effects.

Second, the systematic heterogeneity in discounts we documented in section 6.1, 6.2 and 6.3.1 further challenges the amenity-based interpretation. The price discounts vary systematically with exposure - diminishing for higher floors, apartments without city views, and those affected by early morning tear gas - a pattern unlikely under neighborhood-wide amenity changes. The differential effects across districts' political leanings also contradict an amenities-based explanation, as physical damage should affect prices regardless of political orientation. Finally, the timing of effects undermines this interpretation - rather than appearing immediately after the protests, the price effects emerged primarily after the BNO enactment.

Third, the event study reveals a temporal pattern of price effects that contradicts permanent amenity deterioration. Property price discounts peaked around the BNO policy's enactment and subsequently declined - a pattern consistent with a depleting pool of emigration-motivated sellers rather than lasting neighborhood damage. The transitory rather than persistent nature of these discounts suggests temporary selling pressure from migration-motivated homeowners.

Nevertheless, we can empirically test this hypothesis by examining rental prices and Airbnb rental prices across the pre-movement and post-movement periods. This test is particularly informative because rental markets primarily reflect the current value of housing services and locational amenities. Unlike housing prices, which capitalize both current housing services and expected future changes, rental rates directly reflect current neighborhood quality. If tear gas deployments had degraded neighborhood amenities, we would expect to see lower rental prices. Airbnb prices are especially sensitive to local amenities, as short-term visitors highly value access to shops, restaurants, and environmental quality. The absence of effects in these markets would therefore provide evidence against the amenity-based explanation.

Employing the same difference-in-differences design, we re-estimate Equation (3) using rental price and Airbnb rental price as dependent variables. Table A4 presents the results of this analysis. The results show no statistically significant effects on either rental prices (columns 1 and 2) or Airbnb room prices (columns 3 and 4), with coefficients close to zero. The absence of price adjustments in both the rental and short-term rental markets suggests that changes in neighborhood amenities due to tear gas exposure are unlikely to be the key driving factor behind our main findings.

6.5. Tear Gas and Housing Prices During the Movement

In our main analysis, we exclude the protest period to ensure cleaner identification. This choice stems from several considerations. First, during active protests, the effects of tear gas exposure were contemporaneous with ongoing unrest, making it difficult to disentangle the immediate impact of tear gas from expectations of future protest activities. Second, both violence and tear gas exposure could have had persistent effects on neighborhood amenities during the protest movement, though these effects might have dissipated after the protests ended. Third, protest activities disrupted normal market operations through altered foot traffic patterns and forced cancellations of housing visits, potentially affecting transaction prices through channels beyond tear gas exposure. Most importantly, during this period, the emigration pathway through the BNO scheme had not yet emerged. As our findings indicate, the price effects were concentrated after the BNO scheme's introduction, when emigration to the UK became a viable option.

Nevertheless, one might argue that the price decline in treated buildings originated during the protests, and our observed post-protest discount effects merely reflect the continuation of this pre-existing trend. This conjecture is inconsistent with our empirical evidence. In Section 6.3.1, we show that the discount effects only materialized after the implementation of the BNO scheme in February 2021, suggesting the combination of experiences with political violence and emigration option produced the discount.

To further address potential concerns about price dynamics during the protests, we examine the relationship between housing prices and tear gas exposure during the protest period using a two-way fixed effects model. The specification is as follows:

$$y_{ijt} = \beta_0 + \beta \times \text{TG-50}_j^t + \lambda_j + \delta_t^{TPU} + X_i\zeta + \varepsilon_{ijt}, \tag{3}$$

where TG-50_j^t is a time-varying treatment indicator equal to 1 if building j has experienced tear gas deployment within 50 meters prior to time t, and 0 otherwise. This treatment definition differs from that in Section 4.2 to account for the dynamic nature of tear gas exposure during the protest period. The model includes building fixed effects (λ_j), TPU-by-year-month fixed effects (δ_t^{TPU}), and apartment characteristics (X_i). Consistent with our main analysis, we exclude all buildings located beyond 1,000 meters from tear gas deployment sites.

Given the contemporaneous nature of protests and the numerous confounding factors previously discussed, β captures the correlation between tear gas exposure and housing prices, rather than identifying a causal effect of tear gas exposure on property values.

Table A5 in Appendix presents the estimation results using housing prices and turnover as dependent variables. In both cases, we find no significant correlation between tear gas exposure and housing prices or turnover rate during the protest period. These results suggest that the differences in both prices and transaction volumes between treated and control buildings did not change significantly after being exposed to tear gas.

This absence of correlation during the active protest period contrasts markedly with our main findings in the post-movement period, particularly the effects observed after the BNO enactment. This pattern supports our interpretation that while exposure to political violence may have shaped individuals' political beliefs, these changes only translated into economic decisions when suitable conditions and opportunities emerged. Specifically, while individuals' experiences during the protests may have shifted their perspectives on Hong Kong's future, they only acted on these beliefs when tangible exit options, such as the new BNO visa scheme, became available.

7. Conclusion

Political violence represents a distinct category of social disruption that differs from criminal violence (e.g., Campedelli et al. 2023), war crimes (e.g., Jung, Lim, and Park 2025), terrorist attacks (e.g., Bharadwaj et al. 2021), or domestic violence (e.g., Adams-Prassl et al. 2024 and Bhuller et al. 2023). Unlike these other forms of violence, exposure to state violence can reshape individuals' political beliefs and economic decision-making. The increasing polarization in many societies has made political violence more prevalent. Understanding how state violence shapes market behavior–beyond attitudes and stated preferences–becomes crucial for assessing its economic impact.

Our research contributes to this understanding by examining the consequences of Hong Kong's Anti-ELAB movement, a period marked by intense political violence. This setting combines three elements that make it particularly valuable for studying how experiences with political violence affects economic behavior: (1) varying exposure to violence, (2) high-stakes decisions through housing transactions, and (3) the NSL's suppression of protests provides a unique natural experiment to isolate the effect of past experiences. These features allow us to examine how extreme political experiences influence major financial decisions at the household level, with implications for understanding the broader economic consequences of political violence.

The future research agenda in this field could extend in several directions. First, the long-term persistence of these effects remains an open question - whether the impact of political violence on economic decision-making attenuates over time or creates permanent shifts in behavior. Second, while our study leverages the impact of direct exposure to political violence, it will be fruitful to examine the role of social media in amplifying these effects. We leave these important questions for future studies.

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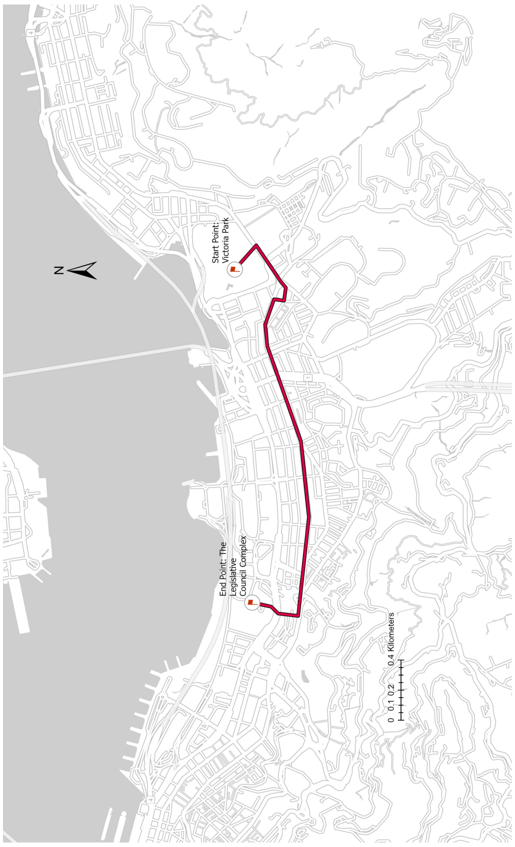
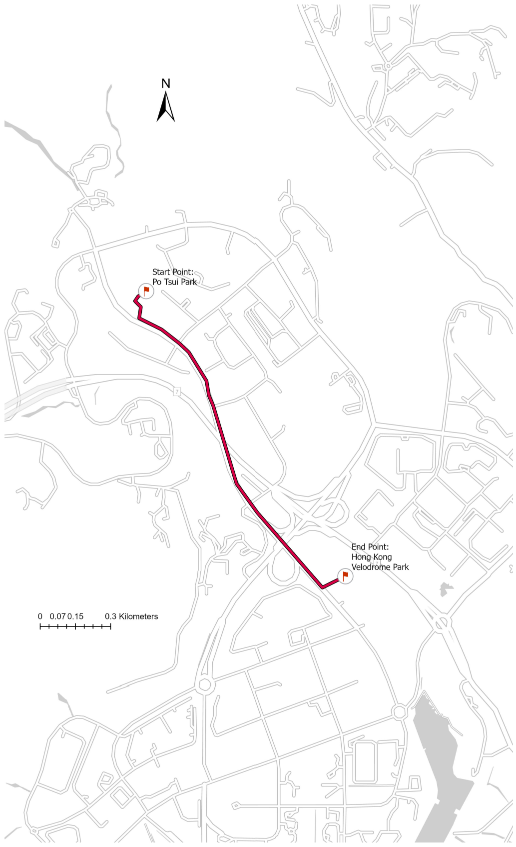
Online Appendix

(Not intended for publication)

A. Additional Figures



(a) Traditional Route (Hong Kong Island)



(b) Emerging Protest Route (Kowloon)

Figure A1. *Example Routes for Protests. This figure illustrates two distinct protest routes during the 2019 Anti-ELAB Movement. Panel (a) shows one of the most frequently used traditional protest routes on Hong Kong Island, which typically began at Victoria Park and was commonly used both before and during 2019. Panel (b) displays a route in Kowloon, representing one of many new protest locations that emerged during 2019 as demonstrations spread beyond traditional areas. The protesters' marching paths are marked by solid red lines.*

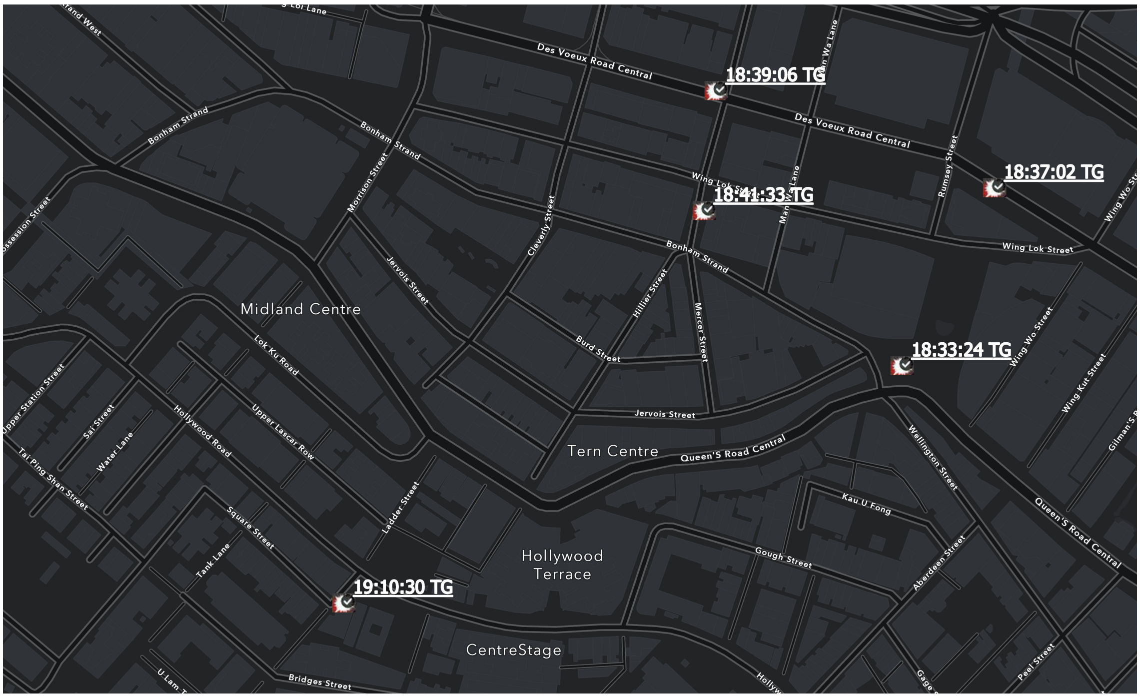


Figure A2. *Real-time Tear Gas Deployment Example. This figure captures a snapshot of tear gas deployments in Sheung Wan on November 2, 2019, demonstrating the precise spatial documentation of tear gas incidents during the 2019 Anti-ELAB Movement.*

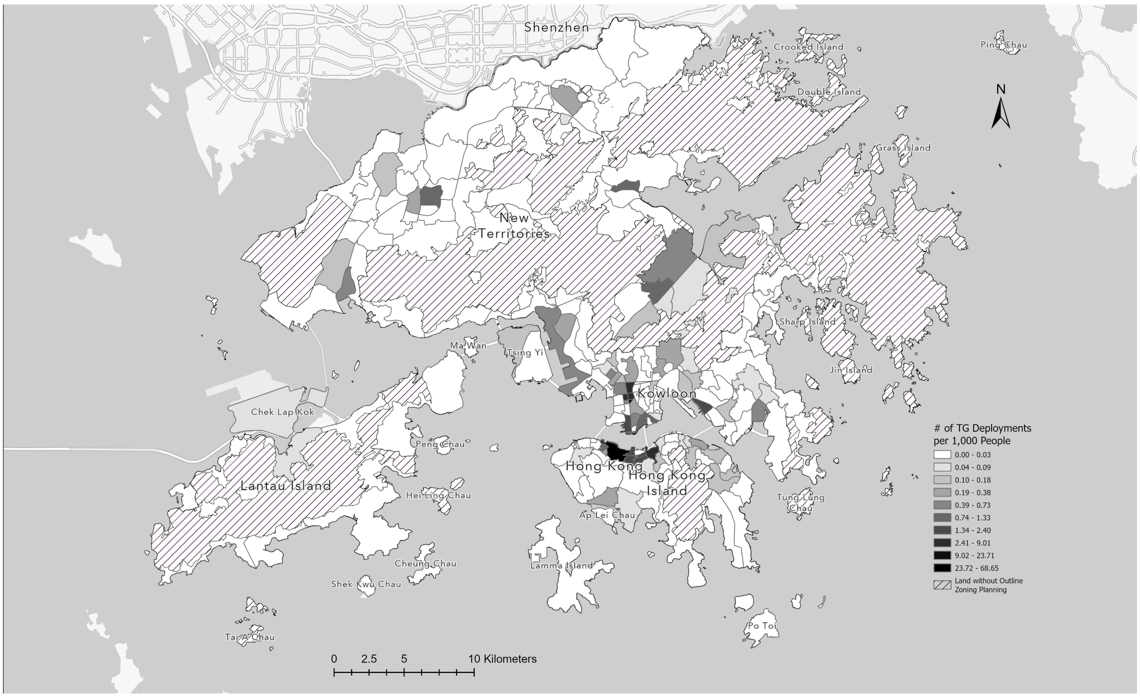
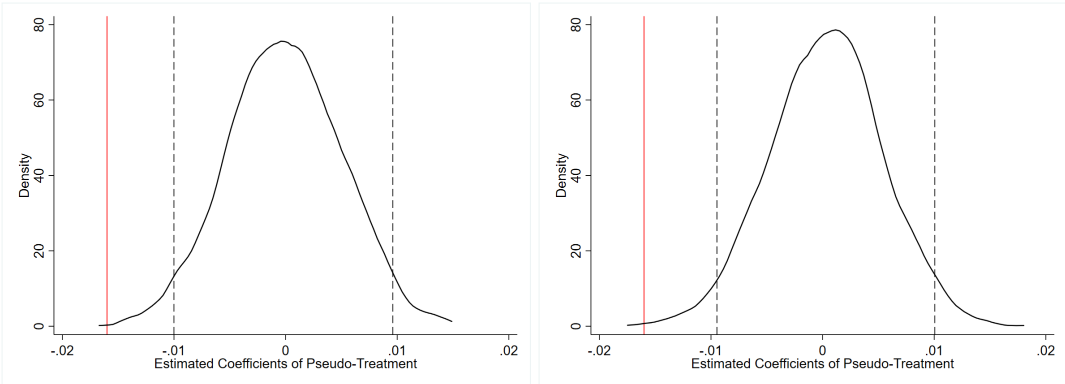


Figure A3. *Spatial Distribution of Tear Gas Deployment Intensity. This map displays tear gas deployment intensity across Hong Kong's 214 Tertiary Planning Units (TPUs), with darker shading indicating higher deployment rates per 1,000 residents. Diagonal lines mark areas without Outline Zoning Planning, which contain no residential buildings.*



(a) 200-meter radius

(b) 150-meter radius

Figure A4. *Results of Randomization Inference Tests. This figure presents placebo tests examining the effects of pseudo-treatments on buildings near tear gas deployment sites. Panel 4(a) shows the distribution of pseudo-treatment effects for buildings within a 200-meter radius of deployment sites, while Panel 4(b) narrows the radius to 150 meters, refining the comparison group. The randomization exercise is repeated 2,000 times. In both panels, the kernel density distributions (black solid lines) are nearly symmetric and around zero. The actual estimated effect, marked by the red vertical solid line, falls well outside the 95% confidence intervals (vertical dashed lines).*

B. Additional Tables

Table A1. *Summary Statistics*

	Mean	SD	p25	p50	p75
Panel A: Residential Property Sale Transactions					
Total Transaction Price (thousand HKD)	7139.74	4622.61	4450	5950	8250
Unit Transaction Price (thousand HKD per square feet)	13.78	4.08	11.05	13.68	16.40
Net Living Area (square feet)	489.36	170.64	370	465	577
Floor Level	16.23	11.95	7	14	23
Orientation: South	0.26	0.44	0	0	1
Local Seller	0.92	0.27	1	1	1
Local Buyer	0.90	0.30	1	1	1
Panel B: Residential Property Rental Transactions					
Total Transaction Price (HKD)	20526.79	10573.61	14000	17200	23000
Unit Transaction Price (HKD per square feet)	38.38	9.72	31.75	36.75	43.66
Net Living Area (square feet)	545.62	244.42	385	493	639
Panel C: Airbnb Listings					
Listing Price (HKD per night)	636.23	336.53	381	520	840
Guest Review Rating	4.63	0.29	4.5	4.7	4.8
Availability in 90 Days	48.92	29.84	23	52	76

Note: This table presents a detailed summary of the statistical properties of the main characteristics of sample apartments used in our analysis. Panel A outlines characteristics relevant to residential property sale transactions. Panel B focuses on residential property rental transactions. Panel C details the characteristics of Airbnb listings.

Table A2. *Difference-in-Differences: Varying the Definition of Control Group*

	Dependent Variable: Sale Price per Square Foot (in log)				
	Choice of Control Group:				
	50-1km	50-800m	50-600m	50-400m	50-200m
	(1)	(2)	(3)	(4)	(5)
TG-50 × NSL	-0.016**	-0.016**	-0.018***	-0.018***	-0.018***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)
Apt Char.	Yes	Yes	Yes	Yes	Yes
Building FE	Yes	Yes	Yes	Yes	Yes
TPU×Year-Month FE	Yes	Yes	Yes	Yes	Yes
Observations	161,687	147,655	122,900	95,013	52,428
R-squared	0.703	0.704	0.703	0.697	0.704

Note: This table reports the DID results using various control group definitions. The dependent variable is the logarithm of sale price per square foot. The treatment group consists of buildings within 50 meters of tear gas deployment sites, while the control group varies across columns, including buildings between 50 meters and different maximum distances (1000m, 800m, 600m, 400m, and 200m). In all specifications, we include controls for apartment characteristics such as size, orientation, and floor level. Standard errors in parentheses clustered at TPU level; * p<0.1, ** p<0.05, *** p<0.01.

Table A3. *Horse Race Analysis*

	Dependent Variable: Sale Price per Square Foot (in log)										
	Apartment Characteristics				Building Characteristics						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
TG-50 × NSL	-0.016**	-0.016**	-0.016**	-0.016**	-0.016**	-0.015**	-0.015**	-0.017***	-0.020***	-0.016**	-0.019***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Area (in log) × NSL	0.000			-0.003							
	(0.006)			(0.006)							
Floor (in log) × NSL		0.012***		0.011***							
		(0.002)		(0.002)							
South × NSL			0.007***	0.006**							
			(0.003)	(0.003)							
# of Apartments in Building (in log) × NSL					0.002						0.001
					(0.003)						(0.003)
Clubhouse × NSL						0.016***					0.012**
						(0.004)					(0.005)
Swimming Pool × NSL							0.015***				0.008*
							(0.004)				(0.005)
Distance to MTR Station (in log) × NSL								-0.002			0.002
								(0.002)			(0.002)
Distance to Shopping Mall (in log) × NSL									-0.009***		-0.009***
									(0.002)		(0.002)
Distance to Main Road (in log) × NSL										-0.001	-0.000
										(0.003)	(0.003)
Apt Char.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Building FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
TPU × Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	161,687	161,687	161,687	161,687	161,687	161,687	161,687	161,687	161,687	161,687	161,687
R-squared	0.703	0.703	0.703	0.703	0.703	0.703	0.703	0.703	0.703	0.703	0.703

Note: This table presents the results of horse race analysis, exploring the interaction effects of specific apartment-level and building-level characteristics with the treatment indicator TG-50. Columns (1) to (4) incorporate apartment characteristics such as size, floor level, orientation, among others. Columns (5) to (11) investigate building characteristics, including the number of apartments in a building, presence of a clubhouse, swimming pool, and the distance to the nearest MTR station, shopping mall, and main road. In all specifications, we include controls for apartment characteristics such as size, orientation, and floor level. Standard errors in parentheses clustered at TPU level; * p<0.1, ** p<0.05, *** p<0.01.

Table A4. *Effects on Rental and Airbnb Room Prices*

	Dependent variable:			
	Rental price		Airbnb Room price	
	(1)	(2)	(3)	(4)
TG-50 × NSL	0.008	0.006	-0.016	-0.016
	(0.007)	(0.007)	(0.024)	(0.024)
Apt Char.	No	Yes	-	-
Building FE	Yes	Yes	-	-
TPU×Year-Month FE	Yes	Yes	-	-
Listing Char.	-	-	No	Yes
Listing FE	-	-	Yes	Yes
TPU×Year-Quarter FE	-	-	Yes	Yes
Observations	47,446	47,446	7,706	7,698
R-squared	0.902	0.916	0.911	0.911

Note: This table presents placebo tests examining whether tear gas deployments affected local amenities, using rental and Airbnb prices as outcome variables. Columns (1) and (2) analyze rental prices at the apartment level, with Column (2) including controls for apartment characteristics such as size. Columns (3) and (4) examine Airbnb room prices at the listing-quarter level, with Column (4) controlling for time-variant listing characteristics such as guest review rating. The results show no significant changes across all specifications. Standard errors in parentheses are clustered at the TPU level; * p<0.1, ** p<0.05, *** p<0.01.

Table A5. *Effects of Tear Gas Exposure During the Movement*

	Dependent Variable:	
	Sale Price per Square Foot (in log)	Turnover Rate
	(1)	(2)
TG-50_j^t	-0.006	0.008
	(0.009)	(0.005)
Apt Char.	Yes	-
Building FE	Yes	Yes
TPU×Year-Month FE	Yes	Yes
Observations	97,333	436,066
R-squared	0.695	0.056

Note: This table presents the result of the two-way fixed effect model using data from both pre-movement and during-movement periods. Column 1 presents apartment-level estimates, using the logarithm of the sale price per square foot as the dependent variable, following the specification in Table 3, except the key independent variable. Column 2 presents building-month-level estimates, using the monthly turnover rate as the dependent variable, following the specification in Table 7, except the key independent variable. * p<0.1, ** p<0.05, *** p<0.01.