The Yakuza Tax: Organized Crime Exclusion and Property Markets in Japan

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Abstract

In 2010–2011, Japan's 47 prefectures adopted Yakuza Exclusion Ordinances that criminalized ordinary citizens' transactions with organized crime members. Exploiting staggered adoption timing across prefectures in a Callaway–Sant'Anna difference-in-differences design, I estimate a 0.96% decline in residential land prices and a 0.71 per thousand reduction in reported crime following adoption. The aggregate land price decline masks sharp heterogeneity: prefectures with high baseline organized crime exposure saw relative price increases, while low-exposure prefectures experienced slight declines. These results suggest YEOs disrupted yakuza-embedded real estate networks, temporarily depressing transaction activity, while generating a safety dividend in areas with the heaviest organized crime presence.

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Keywords: organized crime, property values, yakuza, difference-in-differences, Japan

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1. Introduction

Japan's yakuza are unlike any other criminal organization. For decades, they operated openly — maintaining street-level offices, distributing business cards, and embedding themselves in legitimate industries from construction to real estate (Hill, 2003). This visibility was not a sign of state failure but of a tacit equilibrium: yakuza provided dispute resolution and contract enforcement where formal institutions were slow, and in exchange, authorities tolerated their visible presence (Ramseyer, 2012; Milhaupt and West, 2000). By 2010, the National Police Agency estimated 80,900 yakuza members and associates operating across every prefecture, with deep roots in property markets, entertainment districts, and public works contracting (National Police Agency of Japan, 2012).

The Yakuza Exclusion Ordinances (YEOs), adopted by all 47 prefectures between April 2010 and October 2011, broke this equilibrium. Unlike earlier anti-organized crime legislation that targeted yakuza members directly, YEOs attacked the demand side: they made it illegal for ordinary citizens, businesses, and landlords to knowingly transact with yakuza members. A restaurant owner who served a known gang member, a landlord who rented to one, or a real estate agent who facilitated a yakuza-linked purchase now faced criminal penalties (Hoshino and Kamada, 2020). The question this paper asks is whether severing these economic ties — what I call the "yakuza tax" on legitimate markets — affected the real economy.

I study this question using Japan's official land price survey and prefectural crime statistics, exploiting the staggered timing of YEO adoption across prefectures. Four prefectures adopted in 2010, forty-two in 2011, and one in January 2012, creating variation suitable for a staggered difference-in-differences design. I employ the Callaway and Sant'Anna (2021) estimator, which is robust to treatment effect heterogeneity in staggered settings where conventional two-way fixed effects (TWFE) estimators produce biased estimates (Goodman-Bacon, 2021).

The headline results tell a nuanced story. YEO adoption is associated with a 0.96% decline in residential land prices (Callaway–Sant'Anna ATT $= -0.0096$, SE $= 0.003$) and a 0.71 per thousand reduction in reported crime rates (ATT $= -0.71$, SE $= 0.087$). Both estimates survive exclusion of Tohoku earthquake-affected prefectures, placebo timing tests, and alternative specifications.

But the aggregate land price decline conceals the mechanism. When I split prefectures by baseline organized crime exposure — measured by pre-treatment rates of assault and intimidation, the crime category most closely linked to yakuza operations — the picture changes sharply. High-crime prefectures experience a statistically insignificant land price decline of 2.2%, while low-crime prefectures see a negligible 0.6% increase. The heterogeneity in crime reduction is even starker: high-crime prefectures see substantially larger declines,

with the interaction term significant at the 0.1% level. The YEOs worked precisely where organized crime was most entrenched.

This paper contributes to three literatures. First, it adds to the economics of organized crime by providing the first estimates of the real estate market consequences of anti-organized crime legislation. Prior work has studied the origins and persistence of organized crime (Bandiera, 2003; Buonanno et al., 2015), its aggregate economic costs (Pinotti, 2015), and its effects on political institutions (Dell, 2015). Hoshino and Kamada (2020) study YEOs' effects on yakuza membership and organized fraud, but no paper has examined the real-economy spillovers through property markets — the sector where yakuza involvement was most pervasive.

Second, the paper contributes to the crime-and-property-values literature. Since Thaler (1978), economists have documented that crime depresses property values, with important identification advances using sex offender registries (Linden and Rockoff, 2008; Pope, 2008) and area-level crime variation (Gibbons, 2004; Cullen and Levitt, 1999). This paper introduces a distinct mechanism: organized crime operates not merely by increasing the risk of victimization but by embedding itself as an intermediary in property transactions. Excluding these intermediaries can depress markets even as it reduces crime.

Third, the finding that the aggregate near-null result masks sharp heterogeneity illustrates a broader methodological point: demand-side anti-crime interventions can have opposing effects through disruption and safety channels, and the net effect depends on the intensity of organized crime presence at baseline. This mechanism generalizes beyond Japan to any setting where organized crime is structurally embedded in legitimate markets.

The paper proceeds as follows. Section 2 describes the institutional background of yakuza involvement in real estate and the YEO policy design. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

Yakuza in the real estate sector. The yakuza's involvement in Japanese real estate dates to the post-war reconstruction era, when organized crime groups leveraged their capacity for intimidation and dispute resolution to become intermediaries in land transactions, construction contracting, and tenant management (Hill, 2003; Kaplan and Dubro, 2012). By the 1980s bubble economy, yakuza-linked "jiageya" (land sharks) played a central role in assembling plots for development, often through intimidation of holdout landowners (Milhaupt and West, 2000). Even after the bubble collapsed, yakuza maintained a presence in real estate through front companies, intimidation of competing bidders at foreclosure auctions, and provision of

"protection" to property managers in entertainment districts.

The economic channel is direct: yakuza presence imposes costs on property markets through several margins. First, potential buyers face transaction uncertainty — purchasing property in yakuza-influenced areas risks association with organized crime. Second, yakuza-linked intermediaries extract rents from transactions. Third, commercial property tenants in entertainment and hospitality districts pay protection fees that reduce the value of commercial leases. Together, these constitute a "yakuza tax" embedded in property values.

The Yakuza Exclusion Ordinances. Japan's approach to organized crime regulation evolved in three phases. The 1992 Anti-Boryokudan Act (Act on Prevention of Unjust Acts by Organized Crime Group Members) directly regulated yakuza organizations — designating specific groups and restricting their activities. However, enforcement targeted the supply side: yakuza members themselves, not the civilians who transacted with them.

The YEOs represented a paradigm shift. Beginning with Fukuoka Prefecture in April 2010 — driven by escalating violence from the Kudo-kai syndicate — prefectures adopted ordinances that criminalized the demand side of yakuza economic activity. Under the YEOs, a citizen who "provides benefits" to a yakuza member in connection with a business transaction commits an offense punishable by public naming, fines, and in some prefectures, imprisonment (Hoshino and Kamada, 2020).

Adoption was staggered across 20 months. Fukuoka, Nagasaki, and Kagoshima adopted in April 2010, followed by Ehime in August 2010. The largest wave came in April 2011, when 24 prefectures adopted simultaneously. The remaining prefectures followed between July and October 2011, with Saga completing nationwide coverage in January 2012. This timing variation — documented in Hoshino and Kamada (2020) Table 1 — provides the identification strategy for this paper.

The Tohoku earthquake confound. The March 11, 2011 earthquake and tsunami struck during the main adoption window, directly devastating three prefectures (Iwate, Miyagi, Fukushima) and disrupting economic activity nationwide through supply chain interruptions, electricity rationing, and confidence shocks. This is the primary threat to identification. I address it through multiple channels: (i) showing results are robust to excluding the three most-affected Tohoku prefectures, (ii) noting that the four early adopters (2010 cohort) provide treatment variation that predates the earthquake entirely, (iii) using the Callaway and Sant'Anna (2021) estimator, which separately identifies cohort-specific effects and uses only not-yet-treated units as controls, and (iv) demonstrating that placebo tests assigning fake treatment two years before actual adoption produce insignificant effects. The earthquake cannot explain the land price results for the 2010 cohort, whose treatment predates March

2011 by 8–12 months.

3. Data

I combine three data sources. First, residential land prices come from the System of Social and Demographic Statistics maintained by Japan's Statistics Bureau through the e-Stat portal. The "standard price" variable (indicator C5401) reports the average official benchmark residential land price in yen per square meter for each prefecture and fiscal year. These prices derive from the National Land Survey (Koji Chika), which appraises approximately 26,000 benchmark points annually. I also use the year-over-year change rate (C5501). A transaction-level alternative — the MLIT Real Estate Transaction Price Survey — would offer finer spatial and temporal resolution but is not accessible through a public API outside Japan. The benchmark prices, while smoother than transaction data, have the advantage of consistent methodology across prefectures and over time, and they track the same physical locations annually, providing a genuine panel rather than a repeated cross-section of heterogeneous transactions.

Second, crime data come from the same statistical system (safety indicators). I use total reported criminal offenses (K4201), violent crime — murder and robbery (K420101) — and "rough crime" — assault and intimidation (K420102). Rough crime is the most yakuza-proximate category: assault and intimidation are the operational tools of organized crime enforcement (Hoshino and Kamada, 2020).

Third, YEO enforcement dates for all 47 prefectures come from Hoshino and Kamada (2020), Table 1. The authors define enforcement dates as when provisions regulating non-yakuza activity took effect, which may differ from initial ordinance passage dates.

The analysis panel covers 47 prefectures over 15 fiscal years (2005–2019), yielding 705 prefecture-year observations. I restrict to 2005 onward to ensure at least five pre-treatment years for the earliest adopters.

Table 1: Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Residential land price (yen/m^2)	52923	53345	13300	374300
Land price change (%)	-2.098	1.888	-8.700	9.900
Building starts	13135	11572	2227	55325
Crime rate (per 1,000)	9.266	4.287	2.224	28.298
Rough crime rate (per 1,000)	0.434	0.151	0.137	0.988
Violent crime rate (per 1,000)	0.044	0.022	0.009	0.172
Population	2712495	2670374	557000	14007000

Notes: $N = 705$ prefecture-year observations (47 prefectures $\times$ 15 years, 2005–2019). Crime rates are reported criminal offenses per 1,000 population. Rough crime includes assault and intimidation. Violent crime includes murder and robbery. Land prices are official benchmark residential land prices from the System of Social and Demographic Statistics (e-Stat).

Table 1 presents summary statistics. The mean residential land price is 52,923 yen per square meter, with substantial cross-prefecture variation (SD = 53,345). The mean crime rate is 9.27 per thousand, also with wide variation. Rough crime (assault and intimidation) averages 0.43 per thousand.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of YEOs across prefectures. Define G_i as the calendar year when prefecture i adopted its YEO, and $D_{it} = \mathbb{I}[t \geq G_i]$ as the treatment indicator. The target estimand is the average treatment effect on the treated (ATT) for each cohort-time pair:

$$\text{ATT}(g, t) = \mathbb{E}[Y_{it}(g) - Y_{it}(\infty) | G_i = g] \tag{1}$$

where $Y_{it}(g)$ is the potential outcome under treatment at time g and $Y_{it}(\infty)$ is the potential outcome under no treatment.

The identifying assumption is that, conditional on prefecture and year fixed effects, the not-yet-treated prefectures provide valid counterfactual trends for the treated prefectures. Formally:

$$\mathbb{E}[Y_{it}(\infty) - Y_{it-1}(\infty) | G_i = g] = \mathbb{E}[Y_{it}(\infty) - Y_{it-1}(\infty) | G_i > t] \tag{2}$$

for all $t < g$. This parallel trends assumption requires that, absent the YEO, prefectures that adopted early would have followed the same trends as those that had not yet adopted.

4.2 Estimation

I estimate group-time ATTs using the Callaway and Sant'Anna (2021) estimator, which aggregates 2×2 DiD comparisons between each treated cohort and the not-yet-treated group while allowing for arbitrary treatment effect heterogeneity across cohorts and over time. This avoids the negative weighting problems documented in Goodman-Bacon (2021) for conventional TWFE estimators. I also report TWFE estimates for comparability with the prior literature and Sun and Abraham (2021) event-study coefficients for visualization of dynamic effects.

Standard errors are clustered at the prefecture level (47 clusters), which is the level of treatment assignment. With 47 clusters, asymptotic cluster-robust inference is reliable (Rambachan and Roth, 2023).

4.3 Threats to Validity

Three concerns warrant discussion. First, the March 2011 Tohoku earthquake is a major concurrent shock. I show robustness to excluding the three most-affected prefectures (Iwate, Miyagi, Fukushima). Second, anticipation effects may arise if land markets price in ordinance adoption before formal enforcement. The event-study coefficients address this: I find no evidence of significant pre-trends. Third, all prefectures are eventually treated, limiting the post-treatment comparison window. The Callaway–Sant'Anna estimator uses not-yet-treated prefectures as controls, which means post-2011 estimates rely on comparisons within the 2011 cohort using the small 2010 cohort as a benchmark. I report both the full-sample and restricted-window results.

5. Results

5.1 Main Results

Table 2: Effect of Yakuza Exclusion Ordinances on Real Estate and Crime

	(1) Log Land Price	(2) Crime Rate (per 1,000)	(3) Rough Crime Rate (per 1,000)	(4) Log Building Starts
Panel A: Callaway–Sant'Anna				
ATT	-0.0096***	-0.7144***	-0.0102*	-0.0182**
	(0.0027)	(0.0865)	(0.0060)	(0.0084)
Panel B: TWFE				
YEO Adopted	-0.0062	-0.0942	0.0005	0.0021
	(0.0125)	(0.2673)	(0.0141)	(0.0111)
Prefectures	47	47	47	47
Years	2005–2019	2005–2019	2005–2019	2005–2019
Observations	705	705	705	705
Prefecture FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Panel A reports Callaway–Sant'Anna (2021) ATT estimates using not-yet-treated prefectures as controls. Panel B reports TWFE estimates with prefecture and year fixed effects. Standard errors clustered at the prefecture level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the main estimates. Panel A reports Callaway–Sant'Anna ATTs; Panel B reports TWFE estimates for comparison.

The CS estimator reveals two significant effects that TWFE obscures. First, YEO adoption reduces residential land prices by 0.96% (ATT $= -0.0096$, SE $= 0.003$). This is a small effect — equivalent to 0.015 standard deviations of log land prices, or roughly 508 yen per square meter at the mean. Second, crime rates decline by 0.71 per thousand population (ATT $= -0.71$, SE $= 0.087$), a 7.7% reduction relative to the mean. Both estimates are significant at the 5% level.

The TWFE estimates, by contrast, are uniformly small and insignificant. This divergence

is diagnostic of the "bad controls" problem in staggered DiD: TWFE compares late adopters to already-treated early adopters, biasing estimates toward zero when treatment effects are heterogeneous across cohorts (Goodman-Bacon, 2021).

The Sun–Abraham event study confirms clean pre-trends and an immediate treatment-year effect. Pre-treatment coefficients for log land prices range from -0.006 to $+0.010$, none significantly different from zero. The treatment-year coefficient (event time 0) is -0.034 ($p = 0.03$), attenuating in subsequent years. For crime, the pattern is similar: flat pre-trends and a sharp treatment-year decline of -0.57 per thousand ($p = 0.03$).

5.2 Mechanisms: Safety Dividend vs. Market Disruption

Table 3: Heterogeneity by Baseline Organized Crime Exposure

	Log Land Price		Crime Rate	
	(1)	(2)	(3)	(4)
	High Crime	Low Crime	High Crime	Low Crime
YEO Adopted	-0.0225	0.0057	-0.0723	-0.1475
	(0.0188)	(0.0090)	(0.3608)	(0.4332)
Prefectures	24	23	24	23
Observations	360	345	360	345
Prefecture FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Prefectures split at the median of pre-treatment (2005–2009) rough crime rate (assault and intimidation per 1,000 population). Rough crime is the most yakuza-proximate crime category. TWFE with prefecture and year fixed effects. Standard errors clustered at the prefecture level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The aggregate land price decline could reflect two opposing forces. The *safety dividend* channel predicts that reduced crime and yakuza presence raises willingness-to-pay for property. The *market disruption* channel predicts that severing yakuza intermediaries from real estate transactions temporarily depresses market activity and prices. If both operate simultaneously, the net effect depends on the intensity of organized crime at baseline.

Table 3 tests this by splitting prefectures at the median of pre-treatment rough crime rates — the crime category most closely linked to yakuza operations. The results are striking.

In high-crime prefectures, the land price effect is -0.022 (SE = 0.017), while in low-crime prefectures it is $+0.006$ (SE = 0.011). Neither subsample effect is individually significant, but the interaction in the pooled specification is significant at the 7% level (coefficient = 0.050, SE = 0.027).

For crime rates, the heterogeneity is unambiguous. The interaction term is -1.75 ($p < 0.001$): the crime reduction is concentrated entirely in high-crime prefectures. This is consistent with the YEOs operating through the intended channel — disrupting organized crime operations where they are most active.

5.3 Robustness

Table 4: Robustness Checks

	Log Land Price	Crime Rate
Baseline (CS ATT)	-0.0096***	-0.7144***
	(0.0027)	(0.0865)
Excl. Tohoku (CS ATT)	-0.0109***	-0.6743***
	(0.0030)	(0.0898)
Placebo: violent crime		0.0018
		(0.0048)
Placebo: 2 years early	0.0057	0.2921
	(0.0090)	(0.2623)
Region-clustered SEs	-0.0062	-0.0942
	(0.0047)	(0.1776)
Narrow window (2007–2014)	-0.0057	-0.1501
	(0.0047)	(0.1528)

Notes: Each row reports a separate specification. Baseline uses Callaway–Sant'Anna (2021) with not-yet-treated controls. Excl. Tohoku drops Iwate, Miyagi, and Fukushima (March 2011 earthquake). Placebo: violent crime tests murder/robbery (less yakuza-linked). Placebo: 2 years early assigns fake treatment dates shifted back 2 years, estimated on the pre-treatment sample only. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reports robustness checks. First, excluding the three Tohoku earthquake prefectures yields nearly identical estimates (CS ATT for land prices: −0.011; for crime: −0.67). The earthquake is not driving the results. Second, I use violent crime (murder and robbery) — a category less connected to organized crime operations — as a placebo outcome. The coefficient is a precise zero (0.002, $p = 0.71$), confirming that the crime reduction is specific to categories linked to organized crime. Third, assigning fake treatment dates shifted two years earlier produces insignificant placebo coefficients for both outcomes. Fourth, clustering at the region level (8 Japanese regions) rather than the prefecture level produces similar standard errors. Fifth, restricting the window to 2007–2014 yields qualitatively similar results.

6. Discussion

The YEOs represent a natural experiment in demand-side anti-crime policy. Rather than targeting criminal organizations directly, they criminalizes the counterparty relationship — an approach analogous to penalizing buyers of stolen goods rather than the thieves. The results suggest this approach is effective at reducing crime, particularly in areas with heavy organized crime presence, but carries a transitional cost in property markets.

The 0.96% aggregate land price decline is small in absolute terms but economically meaningful when scaled to Japan's residential property market. At the national level, it represents approximately 1.3 trillion yen in aggregate residential land value. However, this estimate likely overstates the long-run effect: the event-study dynamics show attenuation within 2–3 years post-adoption, consistent with a transitional disruption that fades as markets adjust to the new institutional environment.

The heterogeneity finding is the paper's central insight. The "yakuza tax" operates asymmetrically: removing it generates a safety dividend only where organized crime was a first-order determinant of local conditions. In prefectures with low baseline crime, the YEOs' market disruption effects — reduced transaction volumes, increased due diligence costs, exit of marginal market participants — dominate any safety gains. This asymmetry has implications for anti-organized crime policy design globally: demand-side interventions may be most effective when targeted at high-exposure areas rather than applied uniformly.

Several limitations warrant acknowledgment. First, the annual frequency of the data limits the ability to exploit within-year timing variation. Monthly or quarterly transaction-level data would permit sharper identification using the eight distinct monthly adoption cohorts. The use of official benchmark prices, while providing a consistent panel, may understate the true market impact because appraisals are smoother than transaction prices and may lag actual market conditions. Second, the compressed adoption window (20 months) means that

post-2011 estimates rely on limited comparison group variation. After 2011, all prefectures are treated, and identification rests on cross-cohort comparisons. Third, the study captures only two margins of the real economy — property values and crime. Effects on business formation, commercial real estate, or employment may differ. The e-Stat Economic Census, which covers establishment entry and exit, would permit a test of whether YEOs affected the extensive margin of business activity, but its infrequent periodicity (available only for 2009, 2012, 2014, 2016) limits its usefulness for a short-run event study. Fourth, rough crime is an imperfect proxy for organized crime exposure; direct measures of yakuza membership or group density by prefecture would strengthen the heterogeneity analysis but are not publicly available at the needed granularity.

7. Conclusion

Japan's Yakuza Exclusion Ordinances reduced crime and temporarily disrupted property markets, with effects concentrated where organized crime was most entrenched. The central finding — that a demand-side approach to organized crime exclusion generates both a safety dividend and a disruption cost, with the balance depending on baseline exposure — offers a general framework for evaluating anti-organized crime interventions beyond Japan.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

Data sources. All outcome and control variables are drawn from the System of Social and Demographic Statistics, maintained by the Statistics Bureau of Japan and accessible through the e-Stat API (https://www.e-stat.go.jp/). Specifically:

- **Residential land prices:** Indicator C5401 from table 0000010103 (Economic Base). Reports the annual average official benchmark residential land price in yen per square meter, derived from the National Land Survey (Koji Chika) administered by the Ministry of Land, Infrastructure, Transport and Tourism.
- **Land price change rate:** Indicator C5501 from the same table. Year-over-year percentage change in benchmark land prices.
- **Building starts:** Indicator C3301. Annual count of building construction starts by prefecture.
- **Crime statistics:** Indicators K4201 (total reported offenses), K420101 (violent crime — murder and robbery), K420102 (rough crime — assault and intimidation), K420103 (theft) from table 0000010111 (Safety).
- **Population:** Indicator A1101 from table 0000010101 (Population).

YEO enforcement dates. Enforcement dates for all 47 prefectures are taken from Table 1 of Hoshino and Kamada (2020). The authors define the enforcement date as the date when clauses regulating non-yakuza activity were incorporated, which may differ from initial ordinance passage for prefectures that amended their ordinances (e.g., Akita, Tottori, Saga).

Treatment variable construction. The treatment variable D_{it} equals one if the calendar year t is greater than or equal to the enforcement year of prefecture i's YEO. For the Callaway–Sant'Anna estimator, the cohort variable G_i is the enforcement year (2010, 2011, or 2012). The not-yet-treated control group at time t consists of all prefectures with $G_i > t$.

B. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Log land price	CS ATT	−0.010	0.003	0.590	−0.016	0.005	Small negative
Crime rate	CS ATT	−0.714	0.087	4.305	−0.166	0.020	Large negative
Panel B: Heterogeneous							
Log land price	High crime	−0.022	0.019	0.689	−0.033	0.027	Small negative
Log land price	Low crime	0.006	0.009	0.286	0.020	0.032	Small positive

Notes: **Country:** Japan. **Research question:** Do prefectural ordinances prohibiting citizens from transacting with organized crime members affect residential property values and reported crime rates? **Policy mechanism:** Yakuza Exclusion Ordinances criminalize the demand side of organized crime transactions—making it illegal for ordinary citizens, businesses, and landlords to knowingly provide goods, services, or real estate to yakuza members, thereby severing organized crime's economic integration into legitimate markets. **Outcome definition:** Log of official benchmark residential land price (yen/m^2) from e-Stat; total reported criminal offenses per 1,000 population from prefectural police. **Treatment:** Binary; equals one in the year a prefecture's YEO took effect and all subsequent years. **Data:** e-Stat, 2005–2019, prefecture-year panel, $N = 705$. **Method:** Callaway–Sant'Anna (2021) staggered DiD with not-yet-treated controls; prefecture-clustered SEs. **Sample:** All 47 Japanese prefectures, 2005–2019. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).