

Enforcement against Organized Crime Increases Organized Fraud: Evidence from the Yakuza^{*}

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Abstract

Does enforcement against organized crime deter illegal activities? Previous studies mainly focus on direct enforcement, where law enforcement officers play a primary role. We study indirect enforcement on organized crime. The Yakuza Exclusion Ordinances (YEOs) regulate the criminal organizations in Japan, called the yakuza, by prohibiting non-yakuza citizens from providing benefits to them. We study the unintended consequences of the YEOs on a certain type of white-collar crime, organized fraud, which accounts for almost half of the total damage of all property crimes in Japan and is an emerging revenue for today's yakuza. The difference-in-differences estimates indicate that (i) the YEOs increase the financial damage of organized fraud and (ii) the effects are greater in regions with less yakuza competition. Additional evidence suggests that under the YEOs, former yakuza members engage in fraud, and current yakuza switch from traditional crimes to organized fraud.

Keywords: Indirect Enforcement; Organized crime; Yakuza; White-collar crime; Organized fraud

JEL codes: K4, K14, K42.

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Were the Yamaguchi-gumi broken up now, public order would become much worse. Young members would be destitute. ... [W]ithout our discipline, they would do as they like. ... [T]hey would be driven to income-generating crimes.

Shinobu Tsukasa, the leader of the Yamaguchi-gumi¹

1 Introduction

Organized crime imposes costs on society in terms of not only crime but also economic (Pinotti, 2015) and political outcomes (Alesina et al., 2019; Daniele and Geys, 2015). It is therefore of primary importance for policy makers to understand the effects of policies that aim to combat organized crime. However, the effects of enforcement against organized crime are often ex-ante ambiguous. While law enforcement increases the cost of illegal activities, it may stimulate conflict within or between criminal organizations. Existing research provides mixed evidence on the effectiveness of direct enforcement against criminal organizations. Some studies show that deterrence strategies targeting key gang members reduces gang-related violence (Braga et al., 2001; Chalfin et al., 2021; Piehl et al., 2003), while others show that countermeasures such as removing organization leaders and deploying troops, increases violence, especially in the context of Latin America (Calderón et al., 2015; Dell, 2015; Dickenson, 2014; Lindo and Padilla-Romo, 2018).

We study *indirect* enforcement on Japanese criminal organizations, called the *yakuza*, which have historically engaged in various crimes to gain money from non-yakuza citizens (individuals and businesses).² For example, the yakuza engage in extortion, coerce non-yakuza citizens into paying protection, interfere in civil disputes (especially when a lawful resolution is difficult), and intervene in the construction of public works projects. As in other organized crime groups, like the mafia, the yakuza’s economic activities are based relationships with non-yakuza citizens.

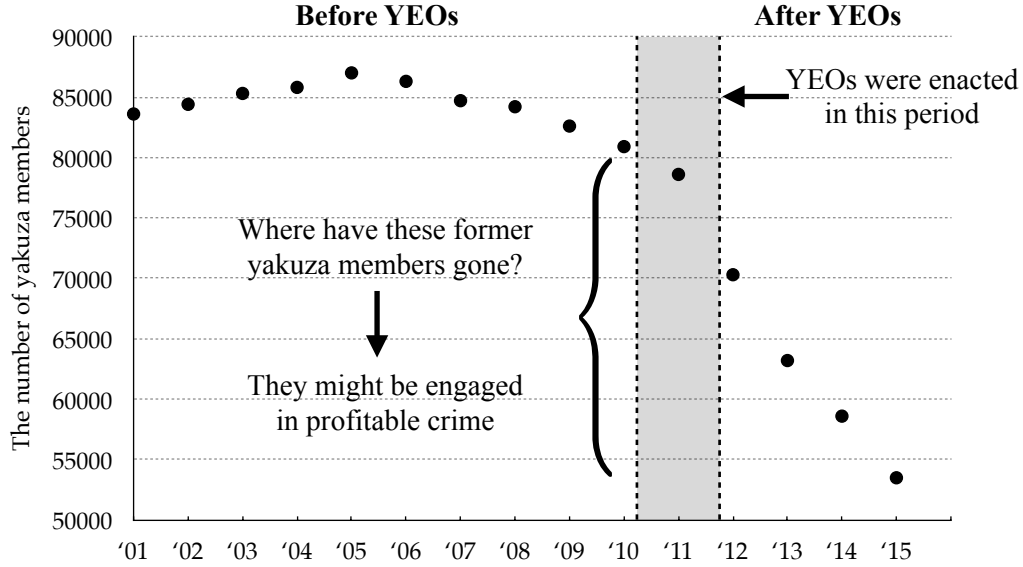
To combat the yakuza and maintain public safety, Japan introduced the Yakuza Exclusion Ordinances (YEOs). In contrast to the aforementioned *direct* enforcement, the YEOs are *indirect* enforcement that prohibits non-yakuza citizens from associating with yakuza members and providing any benefit to them.³ The YEOs aim to cut off the money flow from citizens by breaking the (coercive) relationship. The principals of the YEOs are analogous to third-party policing in the criminology literature; that is, police efforts to persuade or coerce third parties such as non-offending individuals who the police believe influence those participating in the target crime, to take some responsibility for crime control and prevention (Mazerolle and Ransley, 2004). When only the police address crime, their capacity to control crime may be limited; third-party policing strategies may expand their capacity by mobilizing third parties. Indeed, Hoshino and Kamada (2020) find that the YEOs had an immediate, strong impact on the yakuza. After their enactment, the number of yakuza members sharply declined (Figure 1), and the YEOs account for an approximate 26% of the reduction.

¹Interview with a Japanese newspaper (Sankei Shimbun, 2011).

²In this paper, yakuza refers to Japanese criminal organizations or their members.

³As we describe below, the YEOs penalize citizens for violations to ensure their compliance.

Figure 1: Change in Number of Yakuza Members Over Time



Notes: The YEOs were enacted in the shaded period. As they are prefecture-level local ordinances, the enactment dates vary by prefecture. Prefectures form the first level of a jurisdiction and represent an administrative division. Japan consists of forty-seven prefectures. To obtain reliable data on the number of yakuza members, the police draw on many sources to determine who joined and left yakuza organizations (footnote 14).

In this paper, we study the unintended, collateral consequences of the YEOs. We argue that the YEOs might have resulted in an increase in certain illegal activities. We consider two potential channels for this consequence. First, the YEOs decrease the number of yakuza members, but many of these now *former* yakuza members are poor and unable to find legitimate jobs (partly because of their yakuza histories). Indeed, only 1-2% of former yakuza members found legitimate jobs between 2010 and 2013 (Hirosue, 2016). This economic difficulty might drive them back toward crime. Second, the YEOs cripple the traditional income sources of current yakuza members, which likely affect how they make profit.

We argue that the YEOs might have encouraged current and former yakuza members to engage in a certain type of white-collar crime: *organized fraud*. Our motivations for examining organized fraud rather than violent crime are three-fold. First, the yakuza’s economic activities have become extensive in ways that do not necessarily entail violence. Organized fraud is an emerging revenue source for today’s yakuza (National Police Agency, 2015). Journalists document the involvement of former yakuza members in groups committing organized fraud (NHK, 2014). Second, violent crimes are not “profitable” in Japan. The crime-arrest ratio for violent crimes is high, ranging from 80 to nearly 100%, but that for organized fraud is about 30% during the study period (National Police Agency, 2015). These statistics suggest that current and former yakuza members may have an incentive to commit organized fraud rather than violent crimes. Third, organized fraud is a serious issue in Japan, accounting for *almost half* of the total financial damage of all types of property crimes (National Police Agency, 2012, 2014b). The financial damage per organized fraud

case increased by 85% in our sample period from 2011 to 2013.

Yakuza members, whether current or former, do not conduct organized fraud alone but usually collaborate with non-yakuza criminals. Organized fraud requires both “labor” and “capital” for efficient and profitable operations. A fraud organization is usually hierarchical, with various roles such as investors and fraud perpetrators. The organization needs crime labor to make scam calls or to receive scam money from victims. This crime labor is usually supplied by non-yakuza criminals, many of whom are poor young gangsters. The organization also needs a large initial investment, such as to prepare hideouts (where perpetrators make phone calls), purchase many prepaid mobile phones, and obtain lists of potential targets, which they share with other fraud organizations. High-ranking, and thus wealthy, members of yakuza organizations often provide the capital.⁴ Some descriptive evidence suggests that the yakuza are involved in organized fraud, which provides a source of funding for yakuza organizations (National Police Agency, 2014a, 2018).

To estimate the YEOs’ effects on organized fraud, we take a difference-in-differences (DiD) approach, exploiting time variation in the YEOs enactment dates across regions. We find that the YEOs increase the financial damage per case of organized fraud, accounting for 30-35% of the increase during the sample period. Furthermore, we account for local yakuza competition by constructing regional yakuza competition indices (YCIs), defined analogously to the Herfindahl–Hirschman index. The study period has 21 major yakuza organizations who compete rather than cooperate with each other, thereby potentially being in a state of economic or violent warfare. We find that the YEOs’ effects on the financial damage per case of organized fraud are *smaller* in regions with *higher* yakuza competition (*higher* YCIs).⁵

We also show that yakuza members, whether current or former, are involved in organized fraud in the presence of the YEOs. First, former yakuza members engage in organized fraud. Using the change in the number of (current) yakuza members as a proxy for the number of former yakuza members, our evidence suggests that the cumulative change in the number of yakuza members is negatively correlated with the financial damage of organized fraud, and this effect is smaller in areas with lower YCIs. Second, current yakuza members engage in organized fraud. The arrest data of yakuza members in Tokyo indicate that the YEOs increase the number of arrests of yakuza members for fraud, while the YEOs decrease the number of arrests of yakuza members for extortion, a typical crime from which they would earn income in the absence of the YEOs. We note that the arrest data may reflect unobserved levels of enforcement, and it is thus less clear whether this increased because yakuza members’ behavior change, or because police officers’ behavior change. To partially address this concern, we run a placebo regression using a crime common among the yakuza but that the YEOs should not affect. We find null effects of the YEOs on this crime.

⁴The YEOs hurt the yakuza’s economy, but not all yakuza became poor. In hierarchical yakuza organizations, low-ranking (poor) members must pay part of the money they earn from legal and illegal activities to higher-ranking members, thus contributing to the wealth of high-ranking members.

⁵We do not observe the same patterns for the number of cases of organized fraud. The null effects on the number of the cases of organized fraud but positive effects on the financial damage may be consistent with the idea that the increase in organized fraud prevents others from being deceived in the first place, but for those who are deceived, more money is stolen, as yakuza and non-yakuza criminals cooperate and become more sophisticated.

We draw two policy implications. Our finding that the YEOs increase organized fraud suggests that rehabilitation assistance for former yakuza members can improve public safety because it can discourage former members from committing crime such as organized fraud. The other implication, which is admittedly informal, is that focused regulation targeting a few yakuza organizations might be more effective than regulation targeting many. The YEOs' effect depends on local competition among yakuza organizations, which we measure using YCIs. This result suggests that the relationship between yakuza organizations is as strategic complements, indicating that if a yakuza organization is weakened (by intensive regulation), competing organizations will have less incentives to keep their power, especially their membership. In other words, a policy may be able to leverage the yakuza's strategic interaction to maximize its impact.

This study contributes to three strands of the literature. First, the literature on enforcement against organized crime focuses mainly on direct interventions, where law enforcement officers play a primary role (Braga et al., 2001; Calderón et al., 2015; Chalfin et al., 2021; Dell, 2015; Dickenson, 2014; Lindo and Padilla-Romo, 2018; Piehl et al., 2003). In contrast, our policy experiment studies an indirect intervention. To the best of our knowledge, only three empirical studies examine indirect interventions against organized crime. Grogger (2002) and Ridgeway et al. (2019) study the effect of gang injunctions on gang-related violence, finding a reduction in violence. In the context of the yakuza, Hoshino and Kamada (2020) provide evidence suggesting that the YEOs reduce the number of yakuza members, especially in regions with lower yakuza competition.

Second, this study contributes to the literature on organized crime. The recent literature examines the causes and consequences of organized crime, particularly focusing on Italy and Latin America. Such studies reveal connections between organized crime and citizens, firms, or politicians (e.g., Acemoglu et al., 2020; Blattman et al., 2021; Buonanno et al., 2015; Di Cataldo and Mastro-rocco, 2021; Daniele and Geys, 2015; Daniele and Dipoppa, 2022; Dimico et al., 2017; Le Moglie and Sorrenti, 2022; Pinotti, 2015; Slutzky and Zeume, 2020). For example, Brown et al. (2021) study how collusion between gangs facilitates extortion in El Salvador. They leverage a non-aggression pact between gangs, finding that the pact reduces violence in areas with initially high competition among gangs but increases extortion. Our study complements these studies by investigating a new context and focusing on Japanese organized crime. We show that the YEOs, an indirect intervention to cut ties between yakuza and non-yakuza citizens, decrease extortion but increase lucrative white-collar crime. Additionally, we show that the YEOs' effects depend on the strength of regional yakuza competition, suggesting the importance of the yakuza's strategic relationships.

Lastly, Becker (1968) predicts that participation in illegal activities depends on the accessibility of (legal) economic opportunities. Dell et al. (2019) find that a decline in manufacturing job opportunities caused by global trade with China increase drug-related violence. We can interpret their result as evidence suggesting that the decrease in (legal) market opportunities encouraged participation in lucrative drug markets, resulting in an increase in violence. To complement this, we argue and provide suggestive evidence that reducing the yakuza's market opportunities (by the YEOs) induces former yakuza members to commit lucrative white-collar crime.

The rest of this paper is organized as follows. Section 2 provides the background of the yakuza. Section 3 describes our data and identification strategy. Section 4 presents our main results, and Section 5 examines two potential channels. Section 6 concludes.

2 Yakuza: Institutional Background

2.1 Background on Yakuza

Here, we provide the background information about the yakuza necessary to understand our research design. This review is not comprehensive. We refer interested readers to Hill (2003, 2004), for example, for a more comprehensive review.

Yakuza Organizations It is *not* illegal to organize or join yakuza organizations in Japan due to the Freedom of Association (the Constitution of Japan, Article 21). This is in contrast to many other countries where organizing or joining a criminal organization is illegal. While the yakuza were traditionally socially tolerated in Japan to some extent, the victimization of non-yakuza citizens by the yakuza led the government to tighten its restrictions on the yakuza.

One such restriction was enacted by the Anti-Yakuza Laws (*Bōryokudan Taisaku Hō*) in 1992. These laws “designated” 21 yakuza organizations according to several criteria during the period of this study. These criteria are: (i) the yakuza organization has a hierarchical organizational structure, (ii) its members use violence to make money, and (iii) its members have criminal records. As of 2009 (before the YEOs), there were 82,600 yakuza members, and approximately 96% of them belonged to one of the designated yakuza organizations. This proportion is the same as in our sample period (National Police Agency, 2009, 2014a). Moreover, data on the yakuza are usually restricted to the designated organizations. We focus on such designated yakuza organizations, which we refer to simply as the yakuza hereafter.⁶ Given that approximately 96% of yakuza members belong to designated yakuza organizations, restricting our focus to only designated yakuza organizations is not very likely to affect our analyses.

Economic Activities of Yakuza The yakuza engage in several types of legal or illegal economic activities. Among the illegal economic activities, the yakuza’s traditional main income sources are extortion and the distribution of methamphetamine, gambling, and illegal bookmaking (National Police Agency, 2009). However, the yakuza’s economic activities, whether legal or illegal, vary according to the social landscape and time period. The yakuza’s contemporary economic activities are more diverse than they were previously (National Police Agency, 2015).

Since Japan’s economic boom in the 1950s, the yakuza engaged in protection racketeering as an illegal source of money (*mikajimeryō*), targeting business entities such as the sex industry. They also made money illegally from gambling and extortion. Since the economic boom, the yakuza had enormous influence over the Japanese infrastructure and construction sectors. The yakuza’s

⁶The law designated 22 yakuza organizations, but two organizations operating only in Okinawa merged in 2011.

economic activity grew in the 1970s and 1980s, and since then, the yakuza are referred to as the economic yakuza (Hill, 2003, 2004). For instance, they benefit from lending money at illegally high interest rates and from serving as corporate blackmailers (*sōkaiya*) who abuse the rights of stakeholders to extort money. They also interfere with civil disputes (*minbō*). As official legal processes are often slow and costly, non-yakuza citizens sometimes choose to have the yakuza resolve their problems (Milhaupt and West, 2000). The yakuza also target nuclear reactor construction. Nuclear reactors entail large site-specific investments, offering the yakuza a lucrative target. The level of extortion increases when a utility announces plans to build a reactor (Ramseyer, 2016). Behind all these activities is the threat of violence.

In more recent years, however, as enforcement against the yakuza increased, their use of violence became less common, and accordingly, their income sources changed (National Police Agency, 2015). They began running (legal) front companies, which reduces the costs of using violence. Moreover, they also expanded their involvement in illegal activities that do not rely on violence, particularly, organized fraud.

2.2 Yakuza Involvement in Organized Fraud

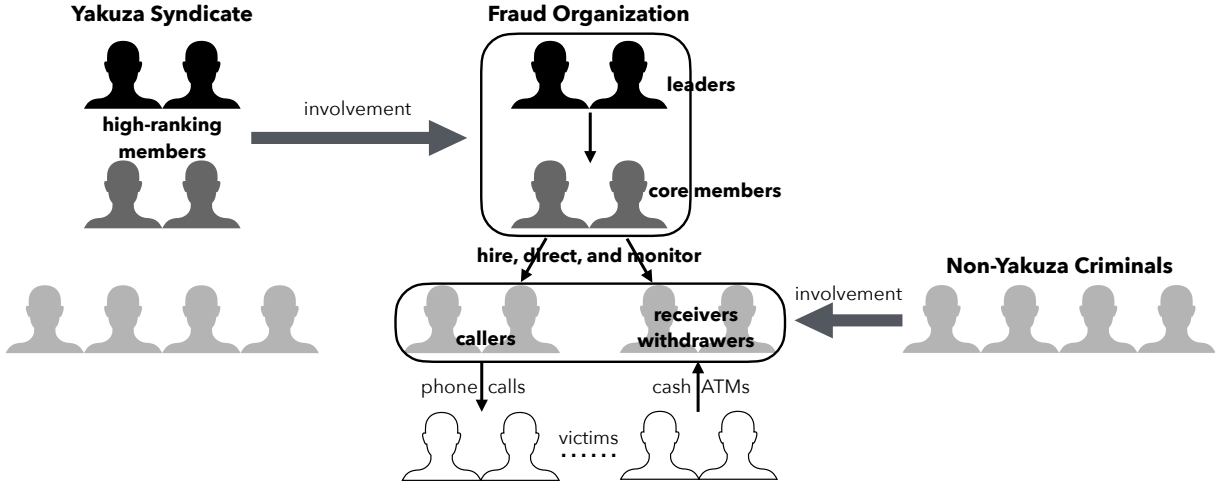
We examine the unintended effects of the YEOs on organized fraud. In this section, we explain this type of crime, with emphasis on its financial damage to society and the incentives for current and former yakuza members to engage in such crime under the YEOs.

Organized fraud in Japan refers mainly to fraud that uses telecommunications (e.g., phones, invoices, and emails) to acquire money illegally. It is also called communications fraud. The main target is the elderly: nearly 80% of the victims are above 60 years old (National Police Agency, 2014b). This kind of fraud has been seriously damaging Japan over the past 15 years. In 2014, the total financial damage amounted to 56 billion yen (approximately 530 million US dollars), accounting for *nearly half* of the total financial damage of property crimes in Japan (National Police Agency, 2014b).

Groups of fraudsters commit this type of fraud. The yakuza involved in organized fraud collaborate with non-yakuza criminals (National Police Agency, 2014a, 2018). Organized fraud entails “capital” and “labor” for efficient and profitable operations. It requires a large initial investment to change hideouts frequently, prepare lists of potential targets, and purchase a large number of pre-paid mobile phones. The organizations perpetrating the fraud are usually hierarchical and include various roles such as investors and fraud perpetrators (Figure 2). Organized fraud also requires (criminal) labor who actually perpetrate the crime (e.g., making phone calls and receiving money from victims). A fraud organization may obtain capital from high-ranking yakuza members and (criminal) labor from non-yakuza criminals, who are typically poor young gangsters.

In the hierarchical structure of a fraud organization (Figure 2), high-ranking members have power over the mid-ranking ones, and the mid-ranking ones hire and/or instruct low-ranking ones. The organization usually consists of (i) leaders and core members; (ii) callers, who trick victims over the phone; (iii) receivers, who receive cash from the victims; and (iv) withdrawers, who

Figure 2: Fraud Organization



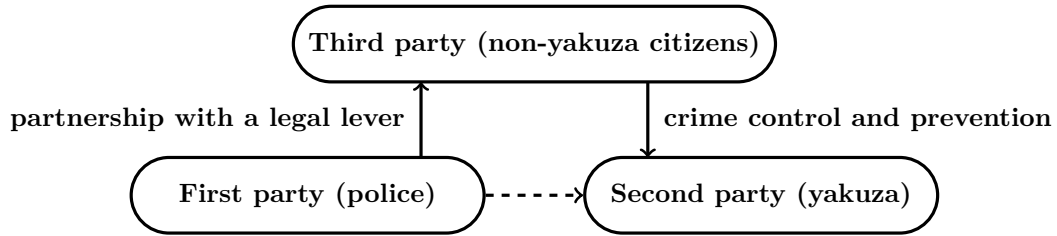
Notes: Organized fraud groups have hierarchical structures. Yakuza members, current or former, occupy the mid- or high-ranking positions. They hire, direct, and monitor the lower-level members. The low-ranking members have different roles: callers, receivers, and withdrawers. They face the highest risk of apprehension.

withdraw cash deposited by the victims from ATMs. Yakuza members usually become leaders or core members; non-yakuza criminals join, in many cases, as subordinate members.⁷ Organized fraud is systematic and profitable when (current) yakuza members collude with non-yakuza criminals and share crime resources. Typically, perpetrators make phone calls all day from hideouts (e.g., apartment rooms), mainly targeting the elderly. Posing as victim’s grandchildren, they deceive the victim out of money by claiming that they had an accident and needed to pay a settlement. The fraud needs low-end perpetrators who make phone calls (*kakeko*), receive money directly from victims (*ukeko*), or withdraw money deposited by the victims from an ATM (*dashiko*). Regardless of their role, the low-ranking members face the highest risk of apprehension. By contrast, yakuza members, whether current or former, typically occupy mid- or high-ranking layers (National Police Agency, 2018). They provide the initial investment required to run the scheme. Since low-ranking members are uninformed about the details of mid- or high-ranking members and the organization, even if they are arrested, the mid- or high-ranking members have less risk of apprehension.

We point out that organized fraud is a less costly crime than other income-generating crimes, such as drug trafficking, for (current or former) yakuza members. Under the Anti-Yakuza Laws, it is difficult and risky for current yakuza members to use violence to earn illegal money. While the yakuza may resort to violence to control their fraud organizations, it is not directed toward (non-yakuza) victims of fraud and thus the violence is less likely to be reported. Moreover, organized fraud pays well for former yakuza members. For instance, yakuza members who defect from an organization loses access to the yakuza’s networks, through which illegal drugs, such as methamphetamine (the most widely abused drug in Japan), are smuggled. Therefore, a former

⁷The term non-yakuza criminals refers to criminals who have never joined a yakuza organization. We distinguish them from non-yakuza citizens.

Figure 3: YEOs as Third-Party Policing



yakuza member likely finds organized fraud to be a relatively accessible potential income source.

2.3 Yakuza Exclusion Ordinances (YEOs)

The concept of the YEOs is “society versus yakuza.”⁸ This contrasts with the traditional approach, “police versus yakuza.” Indeed, the YEOs involve non-yakuza citizens to address the yakuza problem, while the traditional approach attempts to regulate the yakuza directly, such as by severely punishing yakuza members committing crimes.

The YEOs fall into third-party policing in the criminology literature. Third-party policing is a policing strategy that relies on resources and institutions besides the police to address crime problems. Specifically, it refers to police efforts to persuade or coerce third parties to take some responsibility for crime control and prevention (Mazerolle and Ransley, 2006). Third-party policing involves three parties—the police, a targeted crime, and a third party. Similarly, the YEOs involve the police, the yakuza as a target, and non-yakuza citizens as a third party (Figure 3).

The YEOs prohibit (non-yakuza) citizens from providing any benefit to the yakuza, aiming to weaken the yakuza by damaging their income flow. For example, the YEOs ban citizens from receiving extra-legal protection from yakuza members. They also prohibit citizens from providing food, drink, or office space to them; opening bank accounts or issuing credit cards to them, leasing office space to them, or selling insurance to them.

A potential concern is that citizens may not be willing to comply with the YEOs because they face the risk of retaliation from yakuza members (if they comply with the YEOs). For better enforcement, the YEOs impose penalties on non-yakuza citizens who associate with yakuza members for violations. For example, the (local) authorities may publish the names of offending businesses, ruining their reputation, and making it difficult for them to do business with banks and other financial institutions. Alternatively, the authorities may prohibit offending businesses from bidding on public construction projects. Violating the YEOs may also result in imprisonment or a fine. Thus, non-yakuza citizens face a dilemma between the yakuza and the authorities due to the YEOs.

The authorities use coercive methods to implement the YEOs. For example, the Onigiri Club is a golf club consisting of 70 construction companies in Fukuoka prefecture. In 2011, it was revealed

⁸In 2007, the Minister of Justice announced guidelines to reduce victimization (e.g., of citizens, corporations, and governments) by the yakuza. The guidelines emphasize the importance of avoiding associating with the yakuza in any way, given that the association may provide a source of income to the yakuza.

that the managers of nine companies in the Onigiri Club played golf with yakuza executives, and the Fukuoka Police Department reported this incident to the local government and published the names of these companies on its website. The nine companies were barred from public works, resulting in the bankruptcy of two of the nine companies.

In analyzing the effects of the YEOs, the effects may depend on the actual level of enforcement. Although it is difficult to quantify the level of enforcement, we argue that this is not a serious problem. The National Police Agency (NPA) reports the number of YEO offenders, which remained stable during this study’s sample period (increasing slightly from 62 to 71) ([National Police Agency, 2014a](#)). This statistic suggests that the YEOs were well enforced to at least some extent. Of course, the actual number of YEO offenders may be higher because as dark figures may exist, though this possibility does not invalidate the effectiveness of the YEOs. Rather, if the YEOs were not implemented properly and only a small number of non-yakuza citizens were complying with the YEOs, then the effects of the YEOs would be negligible.

2.4 Collateral Effects of YEOs

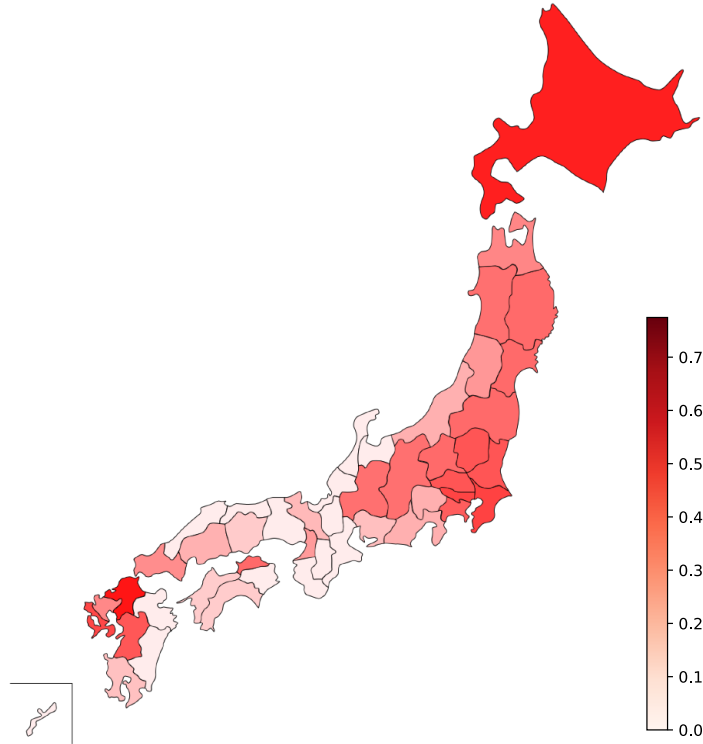
The YEOs may lead to an increase in organized fraud, because they reduce the economic opportunities for current and former yakuza members. First, former yakuza members likely cooperate with non-yakuza criminals when they commit organized fraud under the YEOs. As the number of yakuza members decreases, the number of former members inevitably increases. Many former members are poor and have difficulty finding lawful jobs, mostly because of their yakuza histories (e.g., tattoos and finger cuts may indicate yakuza connections) and lack of human capital. In an interview with [NHK \(2018\)](#), a former yakuza member said, “I was starving to death [...] [W]hen I was approached [about joining a non-yakuza fraud group], I thought I might be able to earn a high income.” Another interview with [TV Asahi \(2021\)](#) reveals a similar story. A former executive of the Yamaguchi-gumi said that “in a yakuza organization, I cannot make a good living (by the enforcement) [...] But in a non-yakuza criminal group, I can make enough money to buy a house in just one or two years.”

Second, current yakuza members also likely collude with non-yakuza criminals because the YEOs deprive their traditional sources of income. Organized fraud is representative of such crimes. Both of current yakuza members and non-yakuza criminals have certain roles in organized fraud. For example, the yakuza members invest seed money and occupy upper echelons in the hierarchies to hire and/or instruct low-ranking members, while non-yakuza criminals provide labor; that is, engage in the practical act of fraud. Consequently, the yakuza members in the upper echelons have a small risk of apprehension but the non-yakuza criminals have a high risk of apprehension.

To examine the YEOs’ effects, we consider the regional variation in yakuza competition levels. [Figure 4](#) indicates that yakuza competition levels are regionally clustered, with darker colored prefectures indicating higher competition among yakuza organizations. The yakuza competition levels are quantified using the YCI ([Section 3.1](#)).

We argue that the YEOs’ effects may depend on (local) yakuza competition, but it is not

Figure 4: Prefecture-Level Yakuza Competition Index



clear whether yakuza competition strengthens or weakens the effects. On the one hand, yakuza competition may strengthen the YEOs' effect on fraud. In areas with intense yakuza competition, non-yakuza criminals can more easily obtain capital and collude with yakuza members as many yakuza organizations exist. On the other hand, yakuza competition may weaken the YEOs' effect on fraud because in areas with less intense yakuza competition, yakuza members can more easily recruit non-yakuza criminals. Under lower competition, a yakuza organization is less likely to face competition or conflict with competing organizations and thus has less incentives to maintain its membership. Therefore, more yakuza members would leave their organizations in areas with lower yakuza competition in the presence of the YEOs. In this interpretation, the YEOs decrease the number of yakuza members in areas with lower YCIs (Hoshino and Kamada, 2020). This implies that there are more former yakuza members in lower yakuza competition areas. As such people have scarce lawful economic opportunities and thus may be driven to income-generating crime, we argue that organized fraud should increase more in such areas. Appendix E provides indirect evidence suggesting that other channels such as the demand for illegal markets, level of enforcement, or economic conditions do not explain the YCIs.

3 Data and Identification Strategy

3.1 Data

YEO Enactment Dates The YEOs are prefecture-level ordinances. Japan consists of 47 prefectures, which are the first level of jurisdiction and administrative division. The enactment dates vary across prefectures (Table 1).⁹ While many prefectures enacted the YEOs in April 2011, other prefectures enacted the YEOs between April 2010 and January 2012. To identify the effects of the YEOs, we exploit the staggered enactment of the YEOs across prefectures, as discussed in Section 3.2.¹⁰

A potential concern about the timings of the YEOs enactment is that a local government's decision on the YEOs may not be exogenous. Its decision may be affected by local yakuza activity. Fukuoka, with conflicts between competing yakuza organizations, was the first to enact the YEOs. Nevertheless, there has been a nationwide movement to strengthen regulations on the yakuza, so a local government should have had little discretion. In our view, conditional on prefecture fixed effects and time fixed effects, the timing of the YEOs enactment is plausibly exogeneous to the outcome of interest. In support of this view, the lead treatment indicators in the event study design suggest that the treatment effects are not driven by the pre-existing trends in the financial damage of organized fraud. Furthermore, the inclusion of the observable covariates hardly changes the magnitudes of the estimated coefficients, suggesting that the selection of observables is unlikely to be a concern.

Organized Fraud We use prefecture-level monthly panel data on organized fraud provided by the NPA. The data cover the period from 2011 to 2013. Due to this data restriction, the YEOs' treatment dummies are constant in the four prefectures that enacted the YEOs before 2011 (Ehime, Fukuoka, Nagasaki, and Kagoshima).

Because we argue that the profitability of crime increases when (current or former) yakuza members and non-yakuza criminals cooperate in the presence of the YEOs, the outcome variable of interest is the amount of financial damage per case of organized fraud, which we define as the total amount of financial damage divided by the number of cases of organized fraud. This number and the total financial damage are based on reports from fraud victims.

One may be concerned that the data may reflect changes in the reporting behaviors of victims, but we argue that this is less of a concern for estimating the effects of the YEOs. It is true that with an increase in organized fraud, the police attempt to alert the elderly to organized fraud. Such

⁹In several prefectures, the YEOs were amended after their introduction. For example, Akita enacted YEOs in March 2011 and then amended them in July 2011. As we are interested in the third-party policing-like aspect of the YEOs, we define the enactment date as the date at which the clauses to regulate non-yakuza activity were incorporated. Accordingly, we define the Akita enactment date as July 2011. The YEOs were similarly amended in Tottori, Fukuoka, and Saga.

¹⁰The contents of the YEOs differ slightly across prefectures, but the basic concept is common across them. When making contracts, non-yakuza citizens must check whether the counterparties are related to the yakuza. What non-yakuza citizens must check also differs across prefectures. In a few prefectures, they need to check only whether the counterparties are yakuza members, while in Tokyo, they need to check whether the counterparties have been closely associated with the yakuza. This requirement is stringent, but in many other prefectures, non-yakuza citizens are still required to ask the counterparties if at least five years have passed since they retired from their yakuza organizations.

Table 1: Enactment Dates of YEOs

Prefecture	Enactment	Prefecture	Enactment	Prefecture	Enactment
Hokkaido	Apr. 2011	Nagano	Sep. 2011	Okayama	Apr. 2011
Aomori	Jul. 2011	Shizuoka	Aug. 2011	Hiroshima	Apr. 2011
Iwate	Jul. 2011	Toyama	Aug. 2011	Yamaguchi	Apr. 2011
Miyagi	Apr. 2011	Ishikawa	Aug. 2011	Tokushima	Apr. 2011
Akita	Jul. 2011	Fukui	Apr. 2011	Kagawa	Apr. 2011
Yamagata	Aug. 2011	Gifu	Apr. 2011	Ehime	Aug. 2010
Fukushima	Jul. 2011	Aichi	Apr. 2011	Kochi	Apr. 2011
Tokyo	Oct. 2011	Mie	Apr. 2011	Fukuoka	Apr. 2010
Ibaraki	Apr. 2011	Shiga	Aug. 2011	Saga	Jan. 2012
Tochigi	Apr. 2011	Kyoto	Apr. 2011	Nagasaki	Apr. 2010
Gunma	Apr. 2011	Osaka	Apr. 2011	Kumamoto	Apr. 2011
Saitama	Aug. 2011	Hyogo	Apr. 2011	Oita	Apr. 2011
Chiba	Sep. 2011	Nara	Jul. 2011	Miyazaki	Aug. 2011
Kanagawa	Apr. 2011	Wakayama	Jul. 2011	Kagoshima	Apr. 2010
Niigata	Aug. 2011	Tottori	Apr. 2011	Okinawa	Oct. 2011
Yamanashi	Apr. 2011	Shimane	Apr. 2011		

an effort may prevent the elderly from being deceived, but that alone does not change victims’ reporting behaviors (conditional on being deceived).

The financial damage per case of organized fraud is 4 million yen (equivalent to 36,500 US dollars), suggesting the profitability of this crime. From 2011 to 2013, this average damage increased from 2.7 million yen to 5 million yen, or by roughly 85%. We have 1,613 observations (47 prefectures \times 36 months = 1,692 less 79, which we drop when taking the natural logarithm because of zeros).

Yakuza Competition Index To construct prefecture-level YCIs, we use two kinds of data: (i) the number of yakuza organizations operating in each prefecture and (ii) the number of members in each yakuza organization.

We define a prefecture-level YCI in a manner similar to the Hirschman-Herfindahl index.¹¹ Let s_{pg} be the “share” of a (designated) yakuza organization g in prefecture p , which we will define. It would be desirable to know the number of members of each yakuza organization in each prefecture to calculate the share, but such data do not exist. We need to estimate these figures. For now, we assume that the members of yakuza organization g are distributed over all the prefectures in which g operates, proportionally to the population of these prefectures. (To examine the robustness of our estimation, we modify this assumption in Appendix G.) As the total number of members of each yakuza organization is known, we can calculate the share s_{pg} under this assumption. Let q_p is prefecture p ’s population for each p , and then the ratio of prefecture p ’s population among all prefectures where a yakuza organization g operates is equal to $q_p / \sum_{p'} q_{p'}$, where $\sum_{p'}$ runs over all prefectures where g operates. Hence, the number of g ’s members in p

¹¹See [Brown et al. \(2021\)](#) for a similar construction of the HHI-based gang competition measure.

is estimated to be $N_{gp} = (g\text{'s total number of members}) \times (q_p / \sum_{p'} q_{p'})$. We can then define the share $s_{pg} = N_{gp} / \sum_{g'} N_{g'p}$ if g operates in p and $s_{pg} = 0$ otherwise, where $\sum_{g'}$ runs over all yakuza organizations operating in p . By definition, $0 \leq s_{pg} \leq 1$ and $\sum_g s_{pg} = 1$ (since each prefecture has at least one yakuza organization). We define the YCI of prefecture p as $YCI_p = 1 - \sum_g s_{pg}^2$. Note that $YCI_p = 0$ if there is only one (monopoly) yakuza organization in p and YCI_p increases in the number of yakuza organizations in p .

An alternative index for yakuza competition is simply the number of yakuza organizations at each prefecture. However, this alternative measure has an issue. For example, both Tokyo and Kumamoto have six yakuza organizations, but the number of yakuza members in Tokyo is about 15 times that in Kumamoto; hence, the alternative measure ignores differences of the number of yakuza members. By using the YCI, which depends on the number of yakuza members, we can capture the differences in the number of yakuza members.¹² By the reasons, we do not use the alternative measure for our main analysis, but our result is robust to the alternative index.

We treat the YCI as time-independent and measure it using the data prior to the YEOs' enactment (2009 data), because if the YEOs affect the YCIs then the YCI is a post-treatment covariate inducing post-treatment bias.

Control Variables We control for the following demographic and socioeconomic variables: the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP. The data on demographic and socioeconomic variables are publicly available from [e-Stat](#) (a portal site for Japanese Government Statistics). See Appendix I for the summary statistics of these control variables.

3.2 Identification Strategy

Difference-in-Differences Strategy We use a DiD approach by exploiting monthly variations in the YEO enactment dates. Moreover, we allow for treatment effect heterogeneity by a cross-sectional variation of YCI. Taken together, our main regression model is

$$Fraud_{pt} = \beta_1 YEO_{pt} + \beta_2 YEO_{pt} \times YCI_p + \eta X_{py} + \mu_p + \xi_m + \xi_y + \sum_r \delta_{pr} \rho_r t + \varepsilon_{pt}. \quad (1)$$

$Fraud_{pt}$ is the log of the financial damage per case of organized fraud in prefecture p at time t . YEO_{pt} is a treatment dummy: $YEO_{pt} = 1$ if the YEOs are in effect in prefecture p at time t ; and $YEO_{pt} = 0$ otherwise.¹³ YCI_p is the YCI of prefecture p . $YEO_{pt} \times YCI_p$ allows for heterogeneous effects of the YEOs with respect to the YCIs. X_{py} is the controls. μ_p is a prefecture fixed effect controlling for time-invariant unobservables across prefectures. ξ_m is a month fixed effect and ξ_y

¹²In Appendix G, we conduct several robustness checks using alternative indices. We adopt alternative definitions for yakuza shares s_{pg} and the resulting YCIs. We also use alternative definitions for yakuza competition levels. Our empirical results are robust to all these specifications.

¹³We use monthly panel data. Time t refers to the t -th month in our dataset. Using a dataset beginning in January 2011, for example, we refer to January 2011 as $t = 1$, January 2012 as $t = 13$, January 2013 as $t = 25$, and so on.

is a year fixed effect. Specifically, month m of the month fixed effect ξ_m appears only when time t corresponds to its month. For example, time index $t = 1, 13, 25$ corresponds to January. Year y of the year fixed effect ξ_y is defined analogously. $\sum_r \delta_{pr} \rho_r t$ captures region-specific time trends, which capture regional unobservables that vary over time. It is important to capture such regional unobservables as the YCIs are regionally clustered (Figure 4). Japan is divided into seven regions: Hokkaido-Tohoku, Kanto, Chubu, Kinki, Chugoku, Shikoku, and Kyushu-Okinawa. Let R be the set of these regions. δ_{pr} is a treatment dummy that equals 1 if prefecture p is in region r and 0 otherwise. ρ_r is a region-specific time trend. ε_{pt} is an idiosyncratic error. We use weighted least squares using the prefectural populations as weights. Standard errors are clustered at the prefecture level to adjust for serial correlation within the same prefecture.

Event Study Analysis We study the dynamic effects of the YEOs on organized fraud by examining months before and after the enactment of the YEOs in an event study framework. The analysis has two purposes. First, we indirectly assess the parallel trends assumption, the identifying assumption for the DiD strategy, by including the lead treatment indicators. The parallel trends assumption states that in the absence of a treatment, the treatment and control groups follow similar trends concerning the outcome of interest (e.g., Angrist and Pischke, 2008). This assumption could be violated, however, if the YEO enactment timings might reflect public concern about a recent surge in organized fraud. If the assumption is satisfied, then the coefficients of the lead treatment indicators should be close to zero. As the future treatment status (i.e., the lead treatment indicators) would have no significant effects if the treatments had causal effects, the null effects of the lead treatment indicators serve as a placebo test. In addition, the analysis explores whether the YEOs' effects persist for the ten months following their enactment. We include lag treatment indicators to explore whether an increase in the financial damage per case of organized fraud is persistent after the YEOs' enactment. If the YEOs' effects are persistent, then the coefficients for the months following the YEOs' enactment should be large in magnitude and statistically significant.

In this analysis, we normalize prefecture p 's time of observation such that the YEO's enactment time in prefecture p is zero: $t = (\text{calendar time}) - (\text{YEO enactment month for prefecture } p)$. The regression model for the event study design is

$$Fraud_{pt} = \sum_{\tau=-5}^{10} \alpha_{\tau} YEO_{pt}^{\tau} + \gamma YEO_{pt} \times YCI_p + \eta X_{py} + \mu_p + \xi_m + \xi_y + \sum_r \delta_{pr} \rho_r t + \varepsilon_{pt}, \quad (2)$$

with normalization $\alpha_{-1} = 0$. YEO_{pt}^{τ} is the treatment indicator: $YEO_{pt}^{\tau} = 1$ if the difference between dates t and t_p is either (i) equal to τ , (ii) greater than or equal to 10, or (iii) less than or equal to -5 , where t_p is the YEOs' enactment date in prefecture p . The coefficient α_{τ} indicates the τ -month lag effect if $\tau \geq 1$ and the $(-\tau)$ -month lead effect if $\tau \leq -1$. Appendix B shows that our result is robust when the lead and lag treatment indicators are balanced in event time, which ensures that the results are not driven by compositional imbalance in the sample.

4 Main Results

4.1 YEOs' Effects on Organized Fraud

Table 2 reports the YEOs' effects on the log of the financial damage per case of organized fraud. Panels A and B, respectively, report the estimates without and with heterogeneous effects of the YEOs ($\beta_2 = 0$ and $\beta_2 \neq 0$). In each panel, we have the following five specifications. (1) The baseline specification includes prefecture fixed effects, month fixed effects, and year fixed effects. (2) We add control variables. (3) We include month-by-year fixed effects, instead of month fixed effects and year fixed effects. (4) We include treatment timing group fixed effects, instead of prefecture fixed effects. (5) We add region-specific trends to specification (2). We define a treatment timing group of time t as the set of prefectures that enacted the YEOs in the same month. For instance, the treatment timing group of August 2011 consists of seven prefectures: Yamagata, Saitama, Niigata, Shizuoka, Toyama, Ishikawa, and Miyazaki (Table 1). We have eight treatment timing groups in total.

In Panel A, the YEOs' effects are significantly positive for all the specifications, which suggests that the YEOs increase the financial damage per case of organized fraud. The estimates are similar across the specifications, suggesting that the YEOs enactment is not confounded with covariates, various time fixed effects, or region-specific time trends. The estimate indicates that the YEOs increase the damage by approximately 27-30%, depending on the specification. Given that the financial damage per case of organized fraud grew, on average, by 85% from 2011 to 2013, our estimates indicate that the YEOs account for 31-35% of the recent increase. Note that given victims who may not be aware of being victimized, the financial damage of organized fraud may be underreported. In this case, there may be a downward bias in the estimated effects. In Panel B, consistent with the estimates in Panel A, the YEOs' effects are significantly positive, indicating that the YEOs increase the financial damage per case of organized fraud by approximately 99-113% in prefectures with a zero YCI (the estimates in Column (4) are relatively smaller (a 53% increase) than those in the other columns). The interaction term $YEO_{pt} \times YCI_p$ is significantly negative. That is, the strength of the YEOs' effects decreases as the YCI increases.

Figure 5 is based on specification (5). The downward slope illustrates the heterogeneity of the YEOs' effects according to the YCI in two ways. First, the solid line and gray band indicate the marginal effect of the YEOs with the 95% confidence intervals when we treat the YCI as a continuous variable. Second, the three dots with the black dashed lines indicate the binned estimates suggested by Hainmueller et al. (2019); the marginal effect of the YEOs evaluated at the low, medium, and high YCI, respectively. These results are consistent in the sense that the effect of the YEOs is greater in prefectures with lower YCIs. Taken together, we find that the YEOs increase the financial damage per case of organized fraud by roughly 32-45% when evaluated at the YCI mean.

Table 2: YEOs' Effects on Financial Damage of Organized Fraud per Case

Panel A:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: ln Financial Damage of Organized Fraud per Case					
YEO	0.241** (0.111)	0.248** (0.098)	0.251* (0.134)	0.240** (0.108)	0.262** (0.099)
R-squared	0.226	0.244	0.253	0.187	0.255
Panel B:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: ln Financial Damage of Organized Fraud per Case					
YEO	0.759** (0.289)	0.695** (0.288)	0.703** (0.308)	0.434*** (0.119)	0.750*** (0.275)
YEO × YCI	-0.980** (0.473)	-0.887* (0.461)	-0.862* (0.466)	-0.375*** (0.124)	-0.989** (0.448)
Overall Effects (Evaluated at Mean)	0.375*** (0.123)	0.364*** (0.150)	0.364** (0.150)	0.286*** (0.102)	0.362** (0.119)
R-squared	0.235	0.251	0.259	0.195	0.263
Observations	1,613	1,613	1,613	1,613	1,613
Prefecture Fixed Effects	✓	✓	✓	—	✓
Treatment Timing Group Fixed Effects	—	—	—	✓	—
Month Fixed Effects	✓	✓	—	✓	✓
Year Fixed Effects	✓	✓	—	✓	✓
Month × Year Fixed Effects	—	—	✓	—	—
Control Variables	—	✓	✓	✓	✓
Region-specific Trends	—	—	—	—	✓

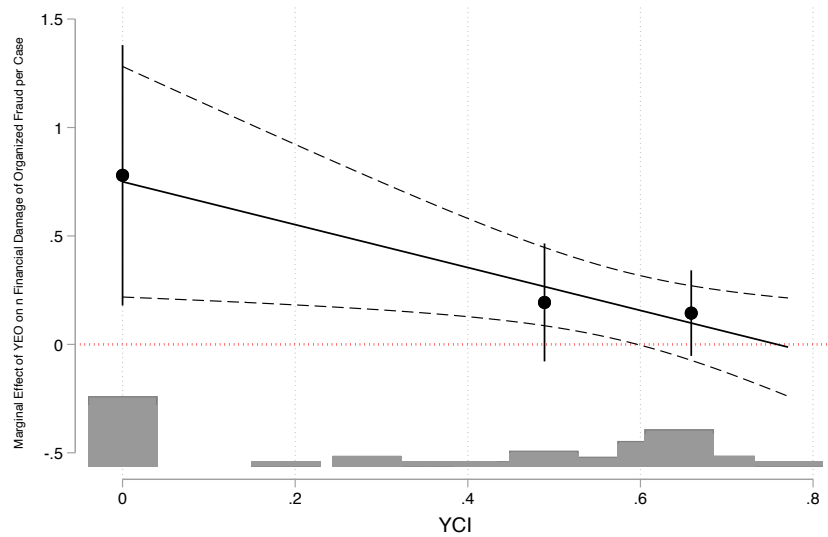
Outcome: the log of the financial damage of organized fraud per case. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP. Standard errors in parentheses are clustered at the prefecture level. Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

4.2 YEOs' Effects on Total Financial Damage and Number of Cases of Organized Fraud

Next, we examine the effects on the total financial damage and the number of cases of organized fraud per 100,000 residents. This analysis helps us better understand whether our main result is driven by the increase in the number of cases or the increase in the financial damage.

Panels A and B in Table 3 respectively present the results for the total financial damage of organized fraud and the number of organized fraud cases. Panel A indicates that the YEOs increase the total financial damages, and the effect diminishes with YCI in all the specifications. Panel B indicates that the magnitudes are small and the results are less robust to different specifications, while the patterns are consistent across the specifications except for the estimate in Column (3). Null effects on the number of the cases of organized fraud but positive effects on the financial damage may be consistent with the idea that with an increase in organized fraud, the police attempt to

Figure 5: Marginal Effects of YEOs on ln Financial Damage of Organized Fraud per Case with 95% CIs



Notes: The unit of the financial damage of organized fraud is per case. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP.

draw the elderly's attention to organized fraud, which prevents them from being deceived in the first place, but for those who are deceived, more money is exploited, as yakuza and non-yakuza criminals cooperate and become more sophisticated.

Table 3: YEOs' Effects on Total Financial Damage by Organized Fraud and on Number of Organized Fraud Cases

Panel A:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: ln Total Financial Damage of Organized Fraud					
YEO	0.982*** (0.306)	0.890*** (0.294)	0.852** (0.355)	0.398* (0.205)	0.935*** (0.288)
YEO × YCI	-1.361** (0.515)	-1.095** (0.494)	-1.017** (0.496)	-0.277 (0.311)	-1.222** (0.509)
R-squared	0.750	0.753	0.758	0.756	0.761
Panel B:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: ln # of Organized Fraud Cases per 100,000 Residents					
YEO	0.223* (0.120)	0.193 (0.117)	0.149 (0.179)	-0.036 (0.136)	0.184 (0.132)
YEO × YCI	-0.384* (0.194)	-0.204 (0.217)	-0.153 (0.227)	0.099 (0.220)	-0.231 (0.247)
R-squared	0.653	0.657	0.661	0.671	0.682
Observations	1,613	1,613	1,613	1,613	1,613
Prefecture Fixed Effects	✓	✓	✓	—	✓
Treatment Timing Group Fixed Effects	—	—	—	✓	—
Month Fixed Effects	✓	✓	—	✓	✓
Year Fixed Effects	✓	✓	—	✓	✓
Month × Year Fixed Effects	—	—	✓	—	—
Control Variables	—	✓	✓	✓	✓
Region-specific Trends	—	—	—	—	✓

Panel A Outcome: the log of the total financial damage of organized fraud. **Panel B Outcome:** the log of the number of organized fraud cases per 100,000 residents. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP. Standard errors in parentheses are clustered at the prefecture level.

Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

4.3 Event Study Analysis

Figure 6 reports the event study estimates based on regression model (2) with 90% confidence intervals. It provides two event study estimates: (i) The estimates in green are based on the baseline specification with prefecture fixed effects, month fixed effects, and year fixed effects. (ii) The estimates in purple are based on the specification that additionally includes the control variables and region-specific time trends. The top panel illustrates the YEOs' effects on the financial damage

per case of organized fraud when we assume no heterogeneity in the YEOs’ effect with respect to the YCI. The bottom left (resp. right) panel illustrates the YEOs’ effect on the same outcome conditional on the YCI equal to zero (resp. the mean). As in Figure 5, the YEOs’ effects decrease in the YCI and thus are not statistically significant for high YCIs. Therefore, we provide graphical evidence up to the mean value of the YCI.

In all the panels of Figure 6, the estimated coefficients before the YEOs enactment are virtually zero and not statistically significant, but after the enactment, the coefficients are large and statistically significant. Moreover, both green (baseline specification) and purple (baseline specification plus the control variables and region-specific time trends) behave similarly, which suggests that the YEOs enactment timing is not confounded with covariates or time trends.

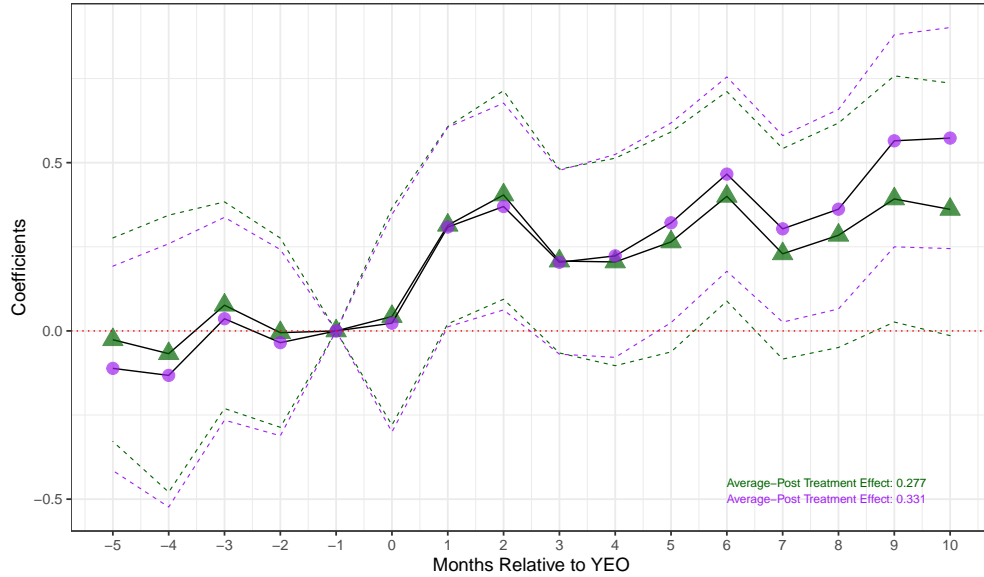
Our identification uses the differential timing of the YEOs’ enactment. A potential concern with the two-way fixed effects estimator when treatment timings differ across prefectures is that the estimated effects can be biased in the presence of treatment effect heterogeneity (Goodman-Bacon, 2021). For staggered DiD settings, the two-way fixed effect estimation requires that both treatment effects be constant and satisfy the parallel trends assumption.

We deal with this concern in the following five ways.

- (i) We calculate the average post-treatment effects from our event study analysis. If the estimates are considerably different, we cannot expect the treatment effects to be constant over time; in contrast, if they are comparable, we expect the treatment effects to be constant over time.
- (ii) We run a joint-significance F -test with the null hypothesis that all the coefficients on the *lag* treatment indicators are equal. If we cannot reject the null hypothesis, it cannot be statistically ruled out that the treatment effects are constant over time.
- (iii) We run a joint-significance F -test with the null hypothesis that all the coefficients on the *lead* treatment indicators are equal. If we cannot reject the null hypothesis, it cannot be statistically ruled out that the parallel trends assumption holds.
- (iv) We examine the robustness of our event study result, using Sun and Abraham’s (2021) interaction-weighted estimator. We report the results in Appendix A.
- (v) We examine whether the YEOs’ effects are driven by particular treatment timing groups. We report the results in Appendix C.

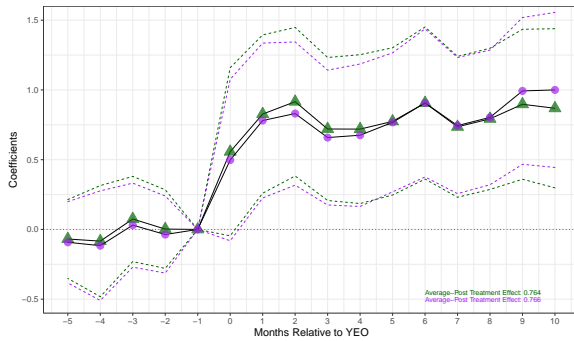
Here, we report the results of (i) to (iii). Note that there are three specifications (the top, left, and right panels in Figure 6). First, we focus on the top panel case. For (i), the average post-treatment effects is 0.277 (green), while the corresponding constant DiD estimates 0.241 (Column (1) in Table 2); meanwhile, the average post-treatment effects is 0.331 (purple), while the corresponding constant DiD estimate is 0.262 (Column (5) in Table 2). For (ii), we do not reject the null hypothesis (that all the coefficients on the lag treatment indicators are equal), suggesting that the YEOs’ effects are not heterogeneous over time. For (iii), we do not reject the null hypothesis (that all the coefficients on the lead treatment indicators are equal), suggesting that the parallel trends assumption likely holds. Second, we focus on the left panel case (where the YEOs are evaluated at the zero YCI). For (i), the average post-treatment effects is 0.764 (green), while the correspond-

Figure 6: Event Study Analysis of YEOs' Effects on Financial Damage of Organized Fraud



P-value of Joint Significance Test of Lead Treatment Coefficient: 0.887 (green triangle), 0.699 (purple circle)

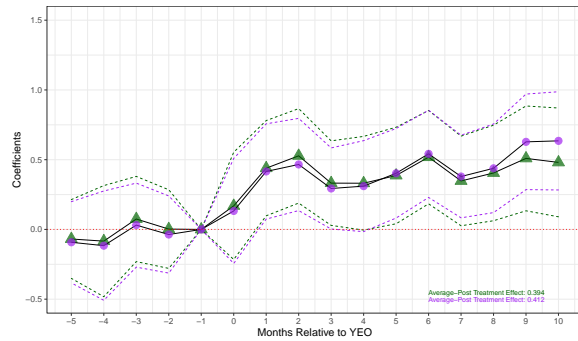
P-value of Joint Significance Test of Lag Treatment Coefficient: 0.344 (red triangle) 0.111 (blue circle)



YEOs' Effects Evaluated at YCI = Zero

P-value of Joint Significance Test of Lead Treatment Coefficient: 0.672 (green triangle), 0.640 (purple circle)

P-value of Joint Significance Test of Lag Treatment Coefficient: 0.346 (green triangle) 0.265 (purple circle)



YEOs' Effects Evaluated at YCI = Mean

P-value of Joint Significance Test of Lead Treatment Coefficient: 0.678 (green triangle), 0.629 (purple circle)

P-value of Joint Significance Test of Lag Treatment Coefficient: 0.346 (green triangle) 0.130 (purple circle)

Notes: Event study estimates and 90% confidence intervals. Estimates indicated in red triangle: baseline specification with prefecture fixed effects, month fixed effects, and year fixed effects. Estimates indicated in blue circle: baseline specification + control variables and region-specific time trends. **Outcome:** The unit of the financial damage of organized fraud is per case. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP.

ing constant DiD estimates 0.759; meanwhile, the average post-treatment effects is 0.766 (purple), while the corresponding constant DiD estimate is 0.750. For (ii) and (iii), we do not reject the null hypothesis (that all the coefficients on the lag/lead treatment indicators are equal). Third, we focus on the right panel case (where the YEOs are evaluated at the mean YCI). For (i), the

average post-treatment effects is 0.395 (green), while the corresponding constant DiD estimates 0.392; meanwhile, the average post-treatment effects is 0.412 (purple), while the corresponding constant DiD estimate is 0.362. For (ii) and (iii), we do not reject the null hypothesis (that all the coefficients on the lag/lead treatment indicators are equal). Overall, we have seen that the magnitudes across the event study and constant DiD estimates are comparable and that the YEOs' effects are constant over time, suggesting that the above concern may be less problematic.

The event study analysis reveals that the financial damage per case of organized fraud jumped immediately after the YEOs' enactment. It is true that the prefectural authorities announced the YEOs' enactment in advance, but the damage had been stably close to zero before the enactment, which suggests no announcement effect. To understand the immediacy, we note that the YEOs were well publicized prior to the enactment, as they were widely covered by media. For instance, two months before the YEOs enactment in Tokyo, Shinsuke Shimada, one of the most popular comedians and TV show hosts, retired from the show business after his tie with yakuza members was discovered. This scandal caused a media frenzy, impressing upon the public that an association with the yakuza ruins one's reputation. In addition, the interview with a yakuza leader (quoted in the beginning) was released on the same day that the YEOs were enacted in Tokyo. Another important aspect is that large businesses, such as banks, were expected to, and indeed did, comply with the YEOs, which require banks not to open bank accounts for yakuza members. Another possible reason for no announcement effect is that organized fraud requires a variety of criminal skills ([National Police Agency, 2015](#)). Yakuza members could not start organized fraud even if they predicted their economic difficulties in the near future due to the YEOs, because they needed to be prepared, such as by looking for non-yakuza criminals to do organized fraud. Taken together, the interval between the announcement and enactment of the YEOs serves as a period to prepare for a new type of crime—organized fraud.

5 Channels

We turn our attention to two potential channels through which the YEOs increase organized fraud. First, former yakuza members might commit organized fraud as they have few legitimate economic opportunities. Second, current yakuza members might engage in organized fraud because the YEOs financially damage the yakuza organizations.

5.1 Fraud by Former Yakuza Members

We argue that former yakuza members, whose number has increased by the YEOs, are involved in crime. [Hoshino and Kamada \(2020\)](#) find that the YEOs decrease the number of yakuza members, using DiD and event study designs. Given this result, our task is to show that a decrease in yakuza members is associated with an increase in crime.

For this purpose, we regress organized fraud on the change in the number of yakuza members, although we admit that this analysis is only suggestive. We use the number of yakuza members

as a proxy for the number of former yakuza members. The number of former yakuza members is unknown, but the number of current ones is known. The YEOs reduce the number of (current) yakuza members, which implies an increase in the number of former yakuza members. Therefore, we can use the change in the number of current members as a proxy for the change in the number of former ones. Our data do not distinguish the out-flow from yakuza from the in-flow, but we believe that this does not cause a serious problem for our empirical analysis. In 2010, the year before the enactment of the YEOs, 67% of yakuza members were above 40 years old (NPA, 2015). Given that the initiation with the yakuza usually begins at an earlier age (Hirose, 2014), the age composition in the yakuza suggests that the number of people joining yakuza organizations during our study period was small. Therefore, a decline in the number of yakuza members likely captures an increase in the out-flow from yakuza organizations, rather than a decrease in the in-flow.

We add a note on the data on yakuza members. Since the NPA reports the number of yakuza members only at the national level, we requested prefecture-level data from the Prefectural Police Departments. We obtained data on 37 out of 47 prefectures from 2008 to 2013.¹⁴ The data contain the number of yakuza members in each of these prefectures. As they are yearly panel data, we impute monthly values using linear interpolation. We obtained data on the number of yakuza members on 37 out of 47 prefectures from 2008 to 2013 at the yearly level. We define yakuza rates as the number of yakuza members per 100,000 residents.

We capture the transition of former yakuza members by calculating the cumulative change in the yakuza rates since the enactment of the YEOs. Our hypothesis is that former yakuza members (due to the YEOs) engage in organized fraud. If true, we should observe a negative association between the cumulative change in the yakuza rates since the enactment of the YEOs and organized fraud. Thus, we are interested in the *cumulative change* in the independent variable and the level of the dependent variable. Our regression model is:

$$Fraud_{pt} = \gamma_1 \Delta_{pt}^d + \gamma_2 \Delta_{pt}^0 \times YCI_p + \eta X_{py} + \mu_p + \xi_m + \xi_y + \sum_{r \in R} \delta_{pr} \rho_r t + \varepsilon_{pt}, \quad (3)$$

where $Fraud_{pt}$ is the log of the financial damage per case of organized fraud. Let $Yakuza_p$ denote the yakuza rate in prefecture p at the time of the YEOs' enactment. Let $Yakuza_{pt}$ denote the yakuza rate in prefecture p at time t . Let $\Delta_{pt}^0 = \ln Yakuza_{pt} - \ln Yakuza_p$ be the cumulative change in the log of the yakuza rates after the YEOs' enactment. Let $\Delta_{pt}^1 = \Delta_{pt}^0 - \Delta_{pt-1}^0$, $\Delta_{pt}^2 = \Delta_{pt}^0 - \Delta_{pt-2}^0$, etc. denote higher-order differences. We allow the change in the log of yakuza rates to depend on the YCI by including interaction term $\Delta_{pt}^0 \times YCI_p$. As we have seen that the YEOs' effects can depend

¹⁴According to the Organized Crime Division of the NPA, the police draw on many sources to identify whether a yakuza member leaves his yakuza organization. For example, the police judge based on expulsion letters and interviews with relevant members and arrested members. A yakuza organization sends an expulsion letter to other yakuza organizations when its member leaves. This letter prevents the defecting member from transferring to another yakuza organization. However, this notification may not be sufficient for the police to count the one as a yakuza dropout. The police need supporting evidence, which they can obtain by investigating yakuza offices and interviewing others. These processes allow the police to track the actual number of yakuza members, reducing the possibility of miscounting the number of yakuza members who hide their identities in the underground.

Table 4: Changes in Yakuza Rates and Financial Damage of Organized Fraud per Case

Panel A:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: ln Financial Damage of Organized Fraud per Case					
Δ ln Yakuza Rates	0.057 (0.053)	-0.033* (0.017)	-0.035** (0.015)	-0.029** (0.011)	-0.026*** (0.009)
R-squared	0.244	0.238	0.229	0.216	0.203
Panel B:	(1)	(2)	(3)	(4)	(5)
Dependent Variable: ln Financial Damage of Organized Fraud per Case					
Δ ln Yakuza Rates	-0.869 (0.815)	-0.157** (0.064)	-0.072** (0.028)	-0.047** (0.017)	-0.037*** (0.012)
Δ ln Yakuza Rates \times YCI	1.413 (1.222)	0.367** (0.179)	0.243* (0.132)	0.209* (0.122)	0.185* (0.097)
R-squared	0.245	0.239	0.230	0.217	0.204
Observations	1,177	1,148	1,115	1,080	1,046
Time Difference d	0	1	2	3	4
Prefecture Fixed Effects	✓	✓	✓	✓	✓
Month Fixed Effects	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓
Control Variables	✓	✓	✓	✓	✓
Region-specific Trends	✓	✓	✓	✓	✓

Outcome: the log of the financial damage of organized fraud per case. **Independent Variable:** the log of the number of yakuza members per 100,000 inhabitants. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP.

Standard errors in parentheses are clustered at the prefecture level. Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

on the YCI, our favored specification includes this interaction term, which allows for heterogeneity. The previous results suggest that the YEOs' effects are greater in prefectures with lower YCIs. Thus, if former yakuza members engage in organized fraud more often in such prefectures, then the coefficient γ_2 on the interaction term should be positive.

In Table 4, we report our estimates. Panel A does not allow for heterogeneity of the YEOs ($\gamma_2 = 0$), but Panel B allows for heterogeneity. Columns (1) to (5) correspond, respectively, to the regression model (3) with $d = 0, 1, \dots, 4$. As the number of yakuza members is on the decline, Δ_{pt}^0 is negative. The negative coefficients thus imply that as the number of yakuza members decreases (the number of former yakuza members increases), the financial damage per case of organized fraud increases. Furthermore, the coefficients of the interaction term are positive and statistically significant. Thus, the effect of the change in yakuza members on the financial damage is smaller in prefectures with lower YCIs. The results therefore suggest that an increase in former yakuza

members is associated with the financial damage, and its association is greater in prefectures with lower yakuza competition.

5.2 Fraud by Current Yakuza Members

We study the hypothesis that under the YEOs, yakuza members are likely to switch their income sources from traditional crime to organized fraud. From the data used for the previous analyses, we cannot determine whether current yakuza members commit organized fraud. We use another set of data on the arrests of yakuza members in Tokyo. In particular, we use the number of arrests of current yakuza members for fraud as a proxy for the yakuza's involvement in organized fraud. Furthermore, the yakuza often earn income by providing extra-legal protection to non-yakuza citizens, which is classified as extortion. Because the YEOs cripple traditional income sources for the yakuza, the YEOs should decrease the number of arrests of yakuza members for extortion.

Note that arrest data may reflect the unobservable level of law enforcement. If the YEOs are associated with increased law enforcement by the police, it is difficult to interpret the YEOs' effects. There are two possible interpretations of an increase in the arrests of yakuza members. One is that, as we hypothesize, the offenses by yakuza members change, while the other is that yakuza members do not change their illegal behavior but the police arrest more. We test the latter indirectly, using a placebo test. If the latter is true, not only would the number of yakuza arrests for fraud increase, but the number of yakuza arrests for other crimes would also increase under the YEOs. Specifically, the number of yakuza arrests should increase for crimes that (i) yakuza members occasionally commit but the YEOs do not attempt to regulate and (ii) the police may find it easy to engage in discretion. An example of such crime is obstruction of public order. When a police officer finds that a yakuza member interfere with the officer's duties, he or she may judge it to be obstruction of public order. The arrest data shows that this crime is more common than robbery. If the change in the police attitudes were the primary driver of the increase in yakuza arrests, we should see an increase in the number of yakuza arrests for obstruction of public order; however, if there is no change in yakuza arrests for obstruction of public order but there is an increase in yakuza arrests for fraud and a decrease in yakuza arrests for extortion, then this result should be interpreted as a change in the yakuza's behavior rather than a change in the police's behavior.

We use ward-level data on the arrests of yakuza members. The dataset is published annually by the Tokyo Metropolitan Police Department, which covers the period from 2004 to 2013. We restrict our sample to 2008 to 2013. The Tokyo metropolis consists of 23 special wards, 26 cities, 5 towns, and 8 villages. These types of jurisdictions differ from one another with respect to lifestyles, population compositions, and socioeconomic conditions. To reduce geographical heterogeneity across municipalities, we focus on the 23 special wards. Our research design is similar to an event study analysis, which exploits the introduction of the YEOs in Tokyo in 2011. A caveat of this approach is that as there is no cross-sectional variation, the effects of the YEOs in Tokyo are indistinguishable from other macro shocks that may affect the number of yakuza arrests. For this reason, we need

to be cautious about interpreting the magnitudes of the empirical results. As the arrest data are count data, we use the Poisson pseudo-maximum likelihood (PPML) estimation.¹⁵ We regress the conditional expectation as follows:

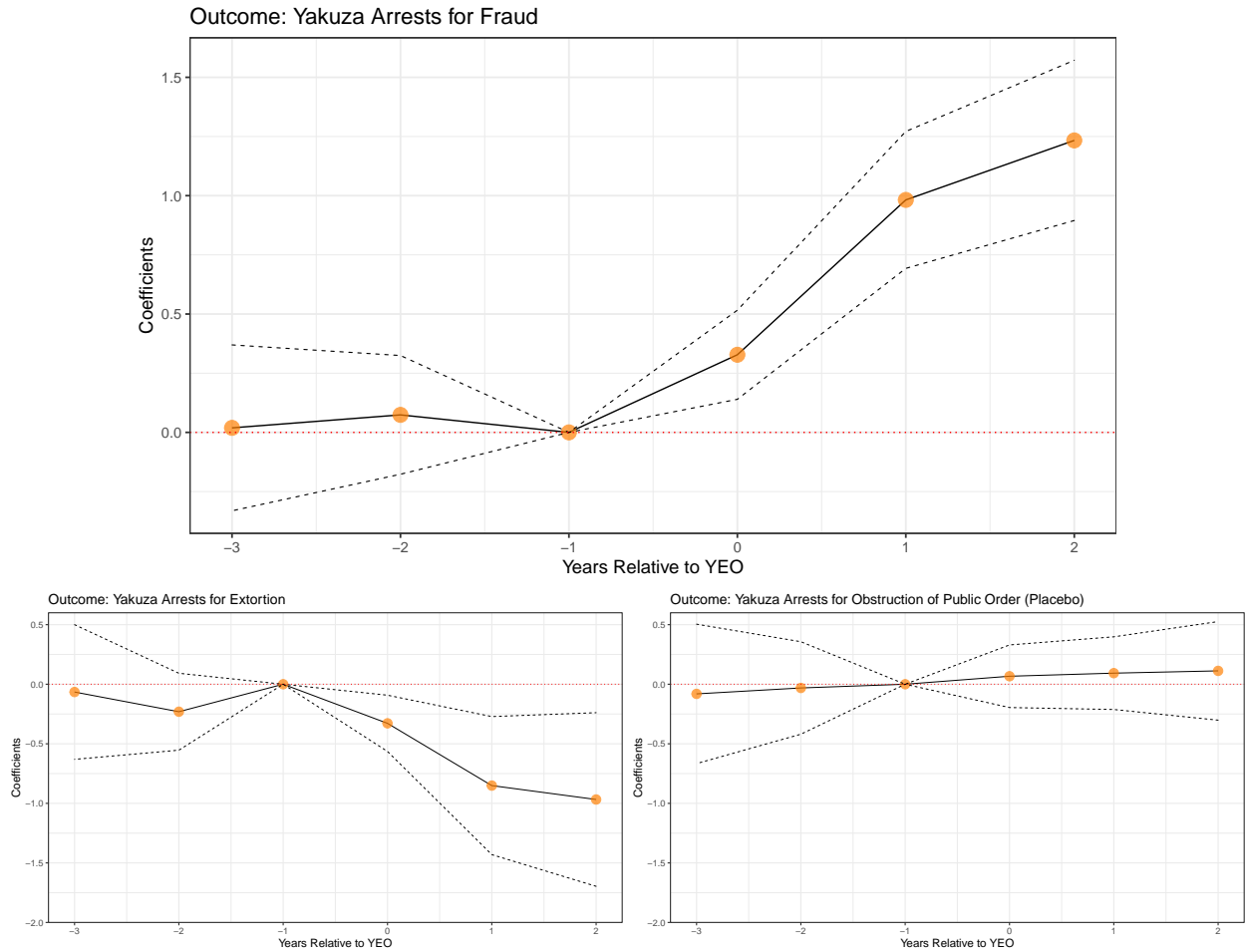
$$\ln \mathbb{E} \left[\text{Arrest}_{wy} \mid YEO_y^\tau, X_{wy} \right] = \sum_{\tau=-3}^2 \alpha_\tau YEO_y^\tau + \eta X_{wy} + \mu_w + \varepsilon_{wy}, \quad (4)$$

where Arrest_{wy} is the number of arrests of (current) yakuza members for fraud, extortion, or obstruction of public duties in ward w in year y . Let YEO_y^τ be the treatment indicator, which is equal to 1 if date t is away from date y_{Tokyo} by τ units of date, where y_{Tokyo} denotes the YEOs' enactment year in Tokyo. That is, $YEO_y^\tau = \mathbf{1}(y - y_{\text{Tokyo}} = \tau)$. X_{wy} is the control variables, including the log of overall population, the proportion of foreign people, the welfare recipients per 1,000 inhabitants, and the log of taxable income. We obtain these data from the Tokyo Statistical Yearbook (Table A6). Then, $\mathbb{E}[\text{Arrest}_{wy} \mid YEO_y^\tau, X_{wy}]$ is the expected value of Arrest_{wy} conditional on YEO_y^τ and X_{wy} . μ_w is a ward-level fixed effect. As we have treatment indicators of two years leading to and two years following the YEOs' enactment in Tokyo with a one-year lead treatment indicator (i.e., 2010) as a reference period, the year fixed effects are perfectly collinear with these treatment indicators. ε_{wy} is an idiosyncratic error. All regressors are weighted by the ward-level population. Standard errors are clustered at the ward level.

Figure 7 reports our estimation results with 90% confidence intervals. In Tokyo, the number of arrests of yakuza for fraud increase by more than 100% compared to the year prior to the enforcement of the YEOs. By contrast, the number of arrests for extortion decrease, down more than 100% compared to the reference year. Lastly, our placebo outcome, yakuza arrests for obstruction of public order, remained unchanged before and after the enactment of the YEOs. Our analysis provides evidence suggesting that the YEOs do not increase the level of law enforcement but increase fraud committed by current yakuza members and decreases extortion, the type of crime that the yakuza would have exploited in the absence of the YEOs.

¹⁵The Poisson model has several advantages (Wooldridge, 2010). It is consistent under mild distributional assumptions. It also does not suffer from the incidental parameters problem, and we can thus control for ward-level fixed effects.

Figure 7: YEOs's Effects on Arrests of Yakuza Members in Tokyo with 90% CI



Outcomes: The number of yakuza arrests for fraud; the number of yakuza arrests for extortion, the number of yakuza arrests for obstruction of public order. **Control Variables:** The log of overall population, the proportion of foreign people, the welfare recipients per 1,000 inhabitants, and taxable income **Data Sources:** The yakuza arrest data from the Tokyo Metropolitan Police Department; and the control variable data from [e-Stat](#).

5.3 Other Outcomes

To better understand the YEOs' impact on welfare, we examine other outcomes that the YEOs may affect. First, we examine the effects on overall violent crime and on overall property crime. Violent crime includes murder, robbery, arson, rape, abduction, human trafficking, and sexual assault, while property crime includes burglary, motor vehicle theft, snatching, and pickpocketing. The relevant data is available from the NPA. We define the outcomes here as the log of the number of each type of crime per 100,000 residents. We use month-level data from 2010 to 2013. Columns (1) and (2) report the results, and neither the coefficient of the YEOs nor the coefficient of the interaction term of the YEOs and YCI are statistically significant and are small in magnitude. Second, we examine the impact on legitimate business by examining the number of business bankruptcies and

the amounts of business debt. We define the outcomes here as the log of the number of business bankruptcies per 100,000 residents and the log of the debt amount; both measures are available from the Business Mutual Aid Association research and investigation reports. We use monthly-level data from 2010 to 2013. Column (3) and (4) show that the neither the coefficient of the YEOs nor the coefficient of the interaction term of the YEOs and YCI are statistically significant.

Table 5: YEOs' Effects on Other Outcomes

	(1)	(2)	(3)	(4)
	Violent Crime	Property Crime	Bankruptcy	Debt
YEO	0.009 (0.015)	0.027 (0.030)	0.051 (0.044)	0.204 (0.137)
YEO \times YCI	0.009 (0.028)	0.007 (0.052)	-0.090 (0.072)	-0.248 (0.220)
Observations	2,256	2,256	2,256	2,256
R-squared	0.751	0.886	0.734	0.736
Prefecture Fixed Effects	✓	✓	✓	✓
Month Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Control Variables	✓	✓	✓	✓
Region-specific Trends	✓	✓	✓	✓

Outcomes: the log of violent crime per 100,000 inhabitants: an aggregate variable including murder, robbery, arson, rape, abduction, human trafficking and sexual assault. The log of property crime per 100,000 inhabitants: an aggregate variable including burglary, motor vehicle theft, snatching, and pickpocketing. **Bankruptcy:** the log of number of firm bankruptcies per 100,000 residents. **Debt:** the log of the debt amount. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP.

Standard errors in parentheses are clustered at the prefecture level. Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

6 Concluding Remarks

We examine the effects of the crackdown on the yakuza through the YEOs. These ordinances apply indirect economic sanctions on the yakuza by prohibiting non-yakuza citizens from assisting the yakuza. We shed light on their unintended consequences. We argue that the YEOs might have increased the a new kind of white-collar crime, organized fraud, because they cripple existing economic opportunities for the yakuza. Such organized fraud surged recently, accounting for nearly half of the total financial damage of property crimes in Japan.

Our main results are two-fold: (i) the YEOs increase the financial damage per case of organized fraud and (ii) the strength of the YEOs' effects decreases as competition increases, as measured by the YCI. Given that the per-case damage increased by 85% during our sample period, our estimates suggest that the YEOs account for 30-35% of the recent surge in organized fraud.

We then examine two potential channels, finding that both former and current yakuza members engage in organized fraud in the presence of the YEOs. First, using the change in the number of yakuza members as a proxy for former yakuza members, we show that a cumulative change in yakuza members since the enactment of the YEOs is negatively correlated with the financial damage per case of organized fraud. Its association is greater in prefectures with less yakuza competition. We may explain this result as follows. After the YEOs were enacted, the number of former yakuza members increased, especially in prefectures with lower YCIs (Hoshino and Kamada, 2020). As former yakuza members in those prefectures face economic difficulties, they may be driven to organized fraud. Second, using arrest data of current yakuza members in Tokyo, we provide evidence suggesting that the YEOs in Tokyo increase yakuza arrests for fraud but decrease yakuza arrests for extortion. Our placebo test rules out the possibility that the observed increase in arrests is due to the increase in the level of policing enforcement. We interpret this result as follows. The YEOs make it difficult for (current) yakuza members to exploit traditional income sources from non-yakuza citizens. The yakuza thus need to substitute the traditional income sources with new ones, in particular, organized fraud.

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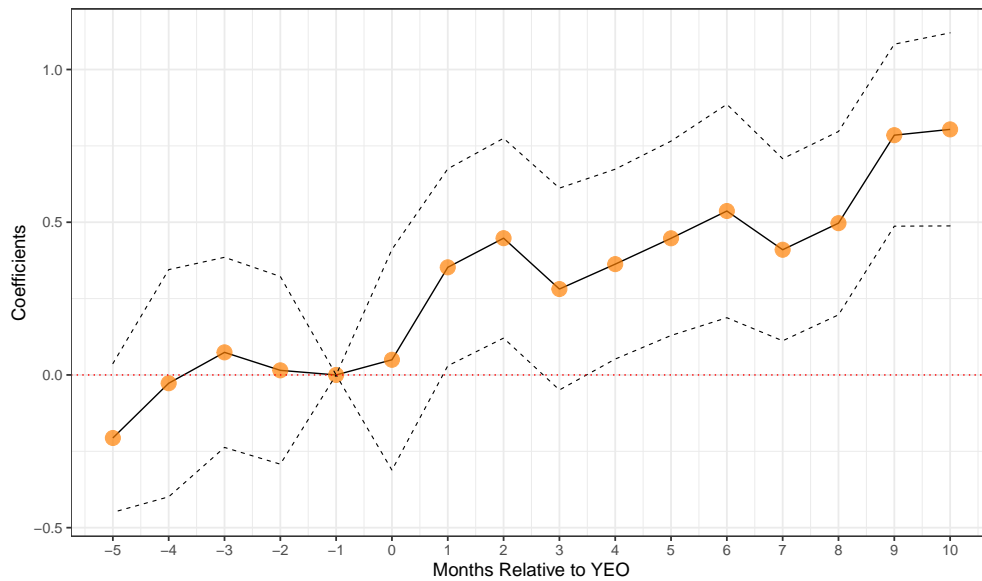
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ONLINE APPENDIX

A Event Study Analysis Using Sun and Abraham's (2021) Estimator

Recent development in the DiD literature reveals that treatment effects may be biased when treatment timing is staggered across units and treatment effects are heterogeneous (Goodman-Bacon, 2021). This section examines the robustness of the event study result, using Sun and Abraham's (2021) interaction-weighted estimator. In our setting, all prefectures eventually enacted the YEOs; in the interaction-weighted estimator, the last-treated prefecture serves as a control unit, and the time period that the last prefecture enacted the YEO is excluded. Figure A1 shows the result, revealing that the effects are similar to the main result.

Figure A1: Event Study Analysis of YEOs' Effects on Financial Damage of Organized Fraud Using Sun and Abraham's (2021) Estimator

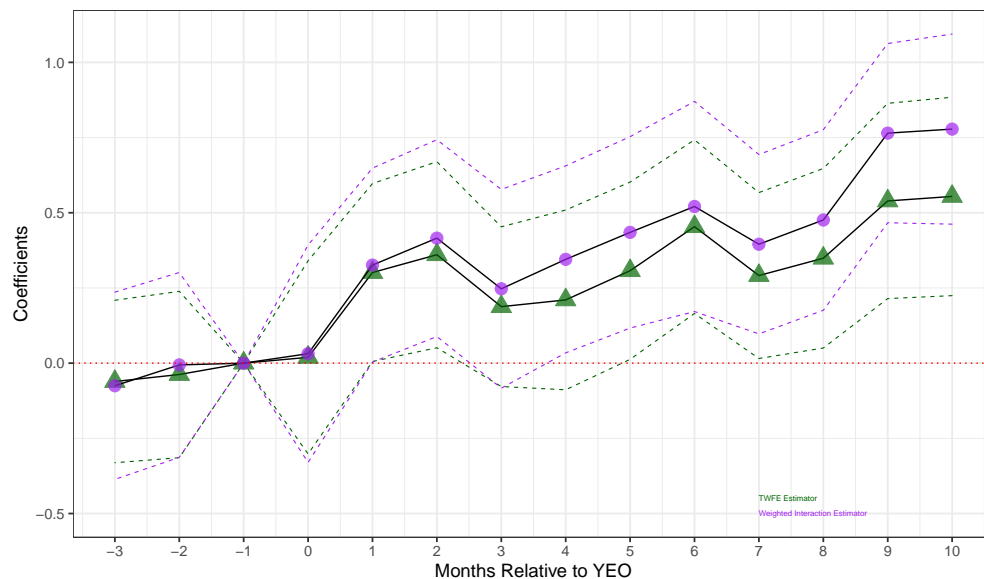


Notes: Event study estimates and 90% confidence intervals. Specification with prefecture fixed effects, month fixed effects, and year fixed effects + control variables and region-specific time trends. **Outcome:** The unit of the financial damage of organized fraud is per case. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP.

B Event study Analysis with Lead Treatment Indicators Balanced in Event Time

Our main event study specification uses treatment indicators five months prior to the enactment of the YEOs. One concern for this specification is that a large portion of the prefectures enacted the YEOs in April, 2011, and thus we might have missed longer lead treatment indicators for these prefectures. This appendix examines the robustness of the event study result, using a sample balanced in event time. Figure A2 provides the results using the two-way fixed effects estimator and Sun and Abraham's (2021) interaction-weighted estimator. The results are not driven by compositional imbalance in the sample.

Figure A2: event study Analysis of YEOs' Effects on Financial Damage of Organized Fraud Using Sun and Abraham's (2021) Estimator



Notes: Event study estimates and 90% confidence intervals. Specification with prefecture fixed effects, month fixed effects, and year fixed effects + control variables and region-specific time trends. **Outcome:** The unit of the financial damage of organized fraud is per case. **Control Variables:** the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP.

C YEOs' Effects by Treatment Timing Group

We run an additional test to see if the YEOs' effects are driven by particular treatment timing groups. Recall that we have eight treatment timing groups. We use the variation in treatment timing groups for identification in a staggered DiD specification (Goodman-Bacon, 2021). Because we are concerned with potential bias in the presence of treatment effect heterogeneity in staggered DiD settings, we run regression model (1) with $\beta_2 = 0$, which excludes the heterogeneity with respect to YCI.

Table A1: YEO Estimates by Treatment Timing Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: ln Financial Damage of Organized Fraud Per Case								
YEO	0.271** (0.104)	0.261** (0.105)	0.162 (0.152)	0.191* (0.100)	0.323*** (0.115)	0.316*** (0.113)	0.321** (0.122)	0.282** (0.107)
Observations	1,546	1,577	820	1,418	1,297	1,541	1,546	1,582
R-squared	0.253	0.248	0.241	0.258	0.234	0.246	0.246	0.249
Treatment Timing Group	1	2	3	4	5	6	7	8
Prefecture Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Month Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓
Control Variables	✓	✓	✓	✓	✓	✓	✓	✓
Region-specific Trends	✓	✓	✓	✓	✓	✓	✓	✓

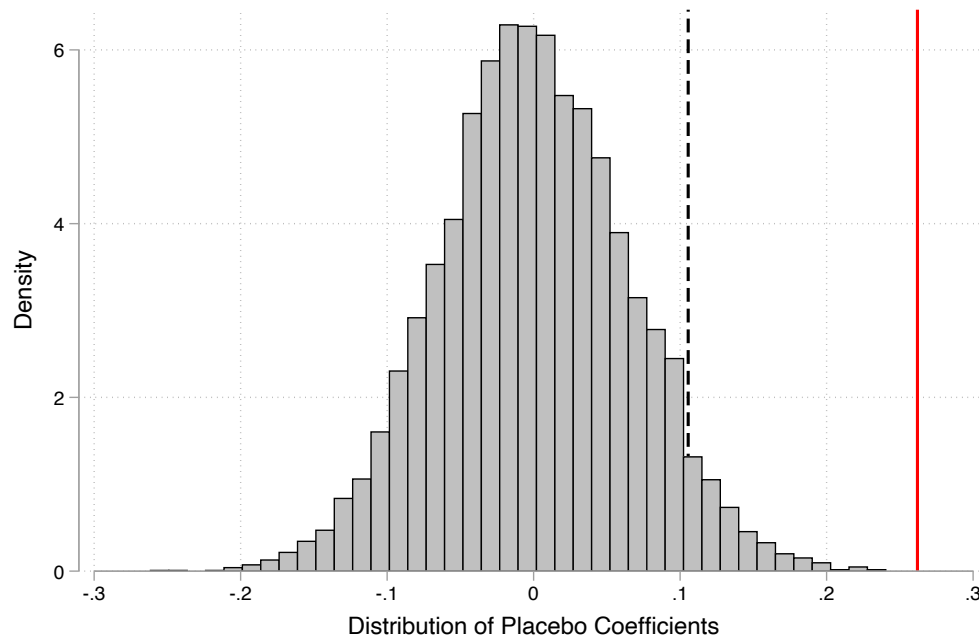
Treatment timing group: Group 1 (Fukuoka, Nagasaki, Kagoshima); Group 2 (Ehime); Group 3 (Hokkaido, Miyagi, Ibaraki, Tochigi, Gunma, Kanagawa, Yamanashi, Fukui, Gifu, Aichi, Mie, Kyoto, Osaka, Hyogo, Tottori, Shimane, Okayama, Hiroshima, Yamaguchi, Tokushima, Kagawa, Kochi, Kumamoto, Oita); Group 4 (Aomori, Iwate, Akita, Fukushima, Nara, Wakayama); Group 5 (Yamagata, Saitama, Niigata, Shizuoka, Toyama, Ishikawa, Shiga, Miyazaki); Group 6 (Chiba, Nagano); Group 7 (Tokyo, Okinawa); and Group 8 (Saga). **Outcome:** the log of financial damage of organized fraud per case. **Control Variables:** The log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP. Standard errors in parentheses are clustered at the prefecture level. Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

Table A1 reports the estimation results. Each column corresponds to a specification that excludes one treatment timing group from the sample (and uses the remaining seven). The exclusion of any treatment timing group hardly changes the YEO effects estimated in Table 2. The only exception is Column (3), but this case excludes the largest group, which includes nearly half the sample. These results, therefore, suggest that the YEOs' effects are hardly driven by any particular treatment timing group.

D Permutation Test

Since pre-treatment periods for data on communication frauds are short, we run an additional placebo test to ensure that the estimated effects of the YEOs are causal (e.g., [Abadie et al., 2010, 2015](#)). In this exercise, we randomly vary the enactment dates of the YEOs across prefectures, and we call the randomly assigned enactment dates placebo treatments. The intuition behind this exercise is that if the estimated effects of the YEOs are causal then the estimates that are derived from the placebo treatments should not have significant effects. For this test, we redraw placebo treatments randomly and reestimate placebo treatment effects. We iterate this process 10,000 times. Figure [A3](#) shows the resulting distribution of the estimated placebo coefficients. In the figure, the dashed line indicates the upper 95% of the distribution of the placebo coefficients; the red line represents the estimated effect from Column 4 in Panel A in Table 2. If the YEOs' effects are causal, then the estimated coefficients indicated by the red line, should be at the right (or left) tail of the distribution of placebo coefficients, indicated by the dashed line. Indeed, this is exactly what Figure [A3](#) shows. Hence, we conclude that it is *less* likely that the estimated effects of the YEOs are driven by randomness or idiosyncrasy.

Figure A3: Permutation Test: ln Financial Damage of Organized Fraud per Case



Red line: baseline estimates taken from Column (5) in Panel A Table 2. **Dashed line:** estimates derived from placebo treatments. **Permutation tests:** 10,000 times.

E Alternative Channels for Heterogeneity of YEO Effects by YCI

In the main text, we have shown that the YEO effects are stronger in areas with higher YCI, and interpreted this heterogeneity as a result of strategic interaction between yakuza organizations. Here, we assess alternative channels for the heterogeneity.

The first alternative channel supposes that illegal markets in more yakuza competitive areas are more profitable. Since yakuza members have better illegal economic opportunities in such areas, they are less likely to resign from their organizations and thus current and former yakuza members are less likely to engage in organized fraud. We indirectly assess this channel by looking at proxies for illegal markets, the number of the total arrests (not restricted to yakuza members) for one of the following: the use or distribution of methamphetamine per 100,000 inhabitants;¹⁶ prostitution per 100,000 inhabitants; and gun-related incidents per 100,000 inhabitants. That is, we regress the YCI on these measures, using the 2009 data. If the illegal market channel explains the heterogeneity in the YEOs' effects, we should observe higher arrest rates for these crimes in more yakuza competitive areas. As in Table A2, however, we find no statistically significant relevance.

The second alternative channel supposes that the enforcement level on yakuza is lower in prefectures with less yakuza competition. We indirectly assess this channel by regressing the YCI on the number of police officers per 1,000 residents (from the 2009 data). If the policing channel explains the heterogeneity in the YEOs' effects, we should observe that higher presence of police officers is significantly related with less yakuza competition. As in Table A2, however, we find no statistically significant relevance.

Lastly, our interpretation may be confounded with other prefecture-level characteristics that may be correlated with YCI. The lower YEOs' effectiveness in higher YCI areas could be because of regional differences in economic conditions. For instance, if prefectures with higher YCI are relatively poor regions, there can be fewer criminal opportunities in these regions, as suggested in the criminology literature (Cantor and Land, 1985). To assess this possibility, we regress the YCI on the GDP and welfare recipients per 1,000 residents (from the 2009 data). We also examine the relationship between YCI and other covariates used in the main analysis. As in Table A2, however, we find no statistically significant relevance. Furthermore, none of the coefficients for observed covariates, except for the ones of the size of population and of the proportions of foreign-born individuals, are statistically significant. The result provides suggestive evidence that these alternative channels explain the heterogeneous effect of the YEO by YCI.

¹⁶Methamphetamine is an illegal drug that is most frequently abused in Japan. In 2008, 77% of all drug-related arrests is related to methamphetamine. Yakuza organizations are largely involved in smuggling and distributing it, earning a considerable amount of money.

Table A2: Assessment of Alternative Channels for YCI

Dependent Variable: YCI					
	(1)	(2)	(3)	(4)	(5)
ln Methamphetamine Arrest per 100,000		0.124 (0.093)			
ln Prostitution Arrests per 100,000			0.089 (0.153)		
ln Gun Incident per 100,000				-0.053 (0.071)	
ln # of Police Officers per 1,000					-0.162 (0.560)
ln Population	0.206* (0.103)	0.215** (0.099)	0.223* (0.113)	0.203 (0.122)	0.211 (0.137)
Population Density	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Proportions of Aged 65 and Over	1.170 (2.314)	12.096 (8.024)	10.506 (7.984)	10.912 (8.095)	10.538 (8.052)
Proportions of Foreign-born Individuals	-11.303* (5.865)	-24.512** (10.101)	-18.795** (7.517)	-20.473** (8.501)	-18.228* (9.456)
GDP	0.001 (0.001)	0.003 (0.004)	0.001 (0.005)	0.003 (0.005)	0.004 (0.006)
Constant	-2.742 (1.960)	-17.273* (9.560)	-15.217 (9.315)	-16.315 (10.027)	-14.858 (9.087)
Observations	47	47	47	47	47
R-squared	0.312	0.438	0.412	0.408	0.406

Notes: Standard error: Robust standard errors. Estimator: weighted least squares, using the prefectural populations as weights.

Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

F Designated Yakuza Organizations

The Anti-Yakuza Laws “designate” 21 yakuza organizations in the period of our study. The criteria for the designation are that (i) the yakuza organization has a hierarchical organizational structure, (ii) its members use violence to make money, and (iii) its members have criminal records. Table A3 lists up all the designated yakuza organizations. In the table, “#pref. of influence” denotes the number of prefectures in which the corresponding yakuza organization operates.¹⁷

Table A3: Designated Yakuza organizations

yakuza organization	#pref. of influence	yakuza organization	#pref. of influence
Yamaguchi-gumi	45	Shinwa-kai	1
Inagawa-kai	19	Sōai-kai	2
Sumiyoshi-kai	18	Kyokudō-kai	5
Kudō-kai	3	Taishū-kai	1
Kyokuryu-kai	1	Sakaume-gumi	1
Aizukotetsu-kai	2	Kyokutō-kai	15
Kyōsei-kai	1	Azuma-gumi	1
Gōda-ikka	3	Matsuba-kai	10
Kozakura-ikka	1	Fukuhaku-kai	4
Asano-gumi	2	Namikawamutsumi-kai	6
Dōjin-kai	4		

¹⁷There were 22 designated yakuza organizations, but in 2011, two of them, both of which operated only in Okinawa, merged.

G Robustness Check

We examine the robustness of our empirical results. Table A4 reports all results to the following cases.

Outcome: Column (1) In the main analysis, our outcome measure is the log of (per-case) financial damage of organized fraud. Instead of this, we take, as our outcome measure, the financial damage of organized fraud per 100,000 residents. Column (1) reports the robustness to this alternative measure.

Estimation Method: Columns (2) and (3) We test the robustness to estimation methods. Column (1) reports the OLS estimation results. Regressors are no longer weighted by the prefecture population. Column (2) reports the WLS estimation result, using the GDP as weights. We obtain qualitatively similar results.

Alternative Identification Assumptions: Column (4) We check the robustness of our main results by using an alternative identification assumption. Instead of region-specific linear time trends, Column (4) controls for prefecture-specific linear time trends, controlling for unobserved heterogeneity across prefectures that evolve over time linearly. The result remain qualitatively similar.

Alternative Definitions of YCI: Columns (5) to (8) We check the robustness by using alternative definitions of YCI. To compute YCI, we compute the share s_{pg} of yakuza organization g in prefecture p . In the main text, we assume that the members of yakuza organization g are distributed proportionally to the population in prefecture p where g operates. We write YCI^{pop} for the YCI that we have used so far.

We recompute the share s_{pg} under alternative assumptions. First, suppose that the members of yakuza organization g are distributed equally among all prefectures where g operates. We write s_{pg}^{naive} for the share calculated under this assumption and YCI_p^{naive} for the resulting YCI. Second, suppose that the members of yakuza organization g are distributed proportionally to the population density of prefecture p where g operates. We write $s_{pg}^{density}$ for the share calculated under this assumption and $YCI_p^{density}$ for the resulting YCI. Using these two variants of YCI, we redo our estimation. As shown in Columns (4) and (5), our results are robust to these specifications.

Lastly, we use an alternative definition for the YCI. We redefine the YCI analogously to the Ethnic Polarization index (Montalvo and Reynal-Querol, 2005) for Column (6). Also we use the raw number of yakuza organizations operating in each prefecture for the YCI for Column (7). Our results are qualitatively similar and robust.

Amendment of Anti-Yakuza Laws: Column (9) The Anti-Yakuza Laws, which are nationwide laws, were amended in October 2012. Since our estimation strategy is based on the DiD

Table A4: Robustness of Effects of YEOs on Financial Damage of Organized Fraud

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
YEO	0.905** (0.420)	0.798*** (0.275)	0.736*** (0.246)	0.704** (0.289)	0.807*** (0.279)	0.805*** (0.285)	0.700** (0.286)	0.811*** (0.261)	0.807*** (0.261)	0.811*** (0.269)
YEO \times YCI ^{pop}	-1.385** (0.685)	-1.102** (0.511)	-0.992** (0.389)	-0.871* (0.483)					-1.028** (0.413)	-0.990** (0.476)
YEO \times YCI ^{naive}					-0.977** (0.404)					
YEO \times YCI ^{dens}						-1.055** (0.451)				
YEO \times EP							-0.692* (0.376)			
YEO \times # of Syndicates								-0.138** (0.052)		
AYL									0.309** (0.136)	
AYL \times YCI ^{pop}									-0.316 (0.209)	
Observations	1,613	1,613	1,613	1,613	1,613	1,613	1,613	1,613	1,613	1,334
R-squared	0.421	0.252	0.262	0.274	0.261	0.261	0.258	0.260	0.264	0.266

Control Variables: the log of overall population, population density, the proportion of individuals aged over 65, the proportion of foreign people, and GDP.

Standard errors in parentheses are clustered at the prefecture level. Significance at the 1%, 5%, and 10% levels indicated by ***, **, and *, respectively.

approach which exploits the prefectural variation, the amendment of the nationwide laws should not affect our results, and this is why we did not take into account.

Here we examine this assumption. Let the treatment dummy AYL_t be equal to 1 before the amendment and to 0 after the amendment. We add, to regression model (1), a non-interaction term AYL_t and an interaction term $AYL_t \times YCI_p^{pop}$. Either term is insignificant but the two elements of interest, YEO_{pt} and $YEO_{pt} \times YCI_p^{pop}$, remain significant.

Tohoku Earthquake: Column (10) Japan experienced the Tohoku Earthquake on March 11, 2011. It is often said that yakuza members engaged in business in the affected areas after the earthquake. For example, some of them participated in the repair of nuclear reactors (Ramseyer, 2016) and in the restoration of the affected areas. These economic opportunities might affect yakuza activities. A simple way to eliminate potential effects of the earthquake is to exclude prefectures that were affected by this earthquake (i.e., Miyagi, Iwate, Fukushima, Ibaraki, Tochigi, Chiba, and Kanagawa) from our dataset. Even using this subsample, we still obtain qualitatively the same results.

H Summary Statistics of Outcome and Control Variables: Prefecture-Level Analysis

Table A5: Summary Statistics of Outcome and Control Variables: Prefecture-Level Analysis

	Mean	Std. Dev.
Financial Damage of Organized Fraud per Case (million)	4.249	5.125
Yakuza Rates	43.080	19.320
Violent Crime Rates	0.775	0.329
Property Crime Rates	10.324	5.051
Bankruptcy	0.597	0.338
Debt (million)	4412.198	18593.94
Population	2717541	2644111
Proportion Aged 65+	0.251	0.028
Proportion of Foreign People	0.012	0.007
Population Density	653.677	1159.631
GDP (trillion)	11.253	15.594

Notes: The yakuza rates are the number of yakuza members per 100,000 inhabitants; the violent crime rates and the property crime rates are the number of the respective crime cases per 100,000 inhabitants; the bankruptcy is the number of business bankruptcies per 100,000 inhabitants. **Data Sources:** The yakuza data are obtained from prefectural police departments; the crime data (fraud, overall violent and property crime) from the NPA; the bankruptcy and debt data from the business mutual aid association research and investigation reports. The control variables are obtained from [e-Stat](#).

I Summary Statistics of Outcome and Control Variables: Ward-Level Analysis

Table A6: Summary Statistics of Outcome and Control Variables: Ward-level Analysis

	Mean	Std. Dev.
Fraud	16.746	8.191
Extortion	10.826	7.440
Obstruction of Public Order	4.789	3.276
Population	377092.2	207346.7
Proportion of Foreign Residents	0.046	0.025
Welfare Recipients per 1,000 Inhabitants	16.411	8.141
Taxable Income (billion)	847.000	455.000

Outcomes: the numbers of yakuza arrests. **Data Sources:** the data on yakuza arrests are obtained from the Tokyo Metropolitan Police Department; the other control variables are obtained from [e-Stat](#).