Distributive Politics and Crime*

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Abstract

We examine whether and how intergovernmental fiscal transfers reduce crime, an important but understudied aspect of distributive politics. Estimating the causal effect of redistribution on crime is complicated by the problem of simultaneity: transfers may be targeted precisely where crime is a problem. Our research design takes advantage of municipality-level panel data from Japan spanning a major electoral system reform that reduced the level of malapportionment across districts. This provides an opportunity to use the change in malapportionment as an instrumental variable, as malapportionment affects redistribution outcomes, but the change caused by the reform is orthogonal to local crime rates. Naïve OLS estimates show negligible (near zero) effects of transfers on crime, whereas the IV results reveal larger negative effects. This finding supports the argument that redistribution can reduce crime, and introduces a new perspective on the relationship between Japan's well-known pattern of distributive politics and its comparatively low crime rates.

Keywords: distributive politics, crime, malapportionment, instrumental variable, Japan

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An emerging literature in political science has begun to unpack and interrogate the relationship between distributive politics and crime (e.g., Holland, 2017; Rivera and Zarate-Tenorio, 2016; Rueda and Stegmueller, 2016). Rueda and Stegmueller (2016), for example, study voters' preferences for redistribution in Western Europe, finding that wealthy citizens in areas of high income inequality often prefer a higher level of redistribution due to concerns about crime. Approaching the topic from a different angle, Holland (2017) argues that the intentional leniency in the enforcement of some crimes, such as street vending or squatting, can be viewed as a form of strategic redistribution to the poor by politicians.

Although it may seem obvious, the theoretical and empirical links between redistribution and crime have been largely overlooked in studies of distributive politics (cf. Golden and Min, 2013). Intergovernmental transfers are frequently viewed as inefficient fiscal policy, characterized as "pork barrel" politics, and considered to be more relevant to the reelection interests of legislators than to programmatic policy outcomes or responsiveness to voters' needs (e.g., Acemoglu and Robinson, 2001; Alesina and Rodrik, 1994; DeBacker, 2011; Ferejohn, 1974; Stein and Bickers, 1994; Weingast, Shepsle and Johnsen, 1981).[1] Much of the research on crime, meanwhile, focuses on violence, drug-trafficking, or organized criminal groups (e.g., Magaloni, Franco-Vivanco and Melo, 2020), without directly exploring the relationship to distributive politics.[2]

Studies that do examine the impact of transfers or financial aid on the social, political, and economic conditions of localities have often found negative outcomes, including increased levels of corruption, which has raised concerns that transfers might contribute to the so-called "resource curse," particularly in developing contexts (e.g., Brollo et al., 2013;

[1]Our focus is central-to-local intergovernmental fiscal transfers, not direct payments to individuals or households, as in cash transfer programs (e.g., Blattman and Annan, 2016; Imai, King and Rivera, 2020; Labonne, 2013). Much of this literature focuses on countries with single-member district electoral systems; the political dynamics of redistribution may be different in countries with party-centered proportional representation elections (e.g., Helland and Sørensen, 2009; Rickard, 2018; Fiva, Halse and Smith, 2021).

[2]In some cases, weak state capacity might mean that organized criminal groups provide assistance to local citizens in place of distributive policies (e.g., Magaloni et al., 2019).

Nikolova and Marinov, 2017). However, other recent studies reveal that transfers can have positive impacts on several socioeconomic outcomes. For example, Litschig and Morrison (2013) find positive effects on education outcomes (schooling and literacy) and income for Brazilian municipalities receiving more transfers. Moreover, the overall economic and social costs of crimes may be higher than previously thought—McCollister, French and Fang (2010) report that the cost of a single murder in the United States is about \$9.8 million, while the average cost of household burglary is \$7,044. If transfers can reduce the direct and indirect costs associated with crime, such effects may also increase political support for redistribution.

In this study, we test the hypothesis that fiscal transfers from central to local governments, which play a significant policy role in all democracies, make a non-negligible contribution to the reduction of crime. Potential channels through which this effect operates include the provision of alternative sources of income to low-skilled workers and the unemployed through local welfare programs, government employment, public works projects, and subsidies to local companies. In line with the economic theory of crime introduced by Becker (1968), recipients of such assistance may hold back from committing a crime because their economic situation is improved.[3]

Despite the clear potential for redistribution to produce a reduction in crime, identifying the *causal relationship* between fiscal transfers and crime is a challenge due to the simultaneity bias inherent in the analysis of any public policy. Governments implement a policy to address some real or perceived social problem. If a given policy works, more application of that policy should result in lesser amounts of the problem—hence, we should expect to observe a negative association between the public policy and the social problem. However, policies are generally implemented precisely where the problem already looms

[3] Extensions and alternatives to Becker's theory include Benoît and Osborne (1995), Burdett, Lagos and Wright (2003), İmrohoroğlu, Merlo and Rupert (2000), Kelly (2000), Machin and Meghir (2004), and van Winden and Ash (2012).

large. Therefore, greater amounts of a social problem necessitate greater amounts of the public policy; as a result, we might actually observe a positive association between the two.

We overcome this challenge through an instrumental variable (IV) approach that takes advantage of the well-documented association between redistribution and legislative malapportionment (e.g., Ansolabehere, Gerber and Snyder, 2002; Pitlik, Schneider and Strotmann, 2006; Tamada, 2009). Our empirical case is Japan, where a major electoral reform in 1994 resulted in redistricting that significantly reduced the level of malapportionment for the House of Representatives (the lower and more important chamber of the National Diet) in a depoliticized process. Horiuchi and Saito (2003) show that the *change* in malapportionment due to the reform is related to a *change* in the amount of per capita transfers allocated to local municipalities. Importantly for our purposes, changes in malapportionment affect changes in crime rates only through changes in the amount of per capita transfers, so we can use *within-municipality* changes in the level of malapportionment as an IV to uncover more accurate estimates of the causal effect of redistribution on crime.[4]

The case of Japan also provides a useful setting for our identification strategy for its sociopolitical context. Distributive politics have played an important role in elections, with incumbents—especially those in the long-ruling Liberal Democratic Party (LDP)—using transfers to facilitate credit-claiming and reward supporters (Catalinac, Bueno de Mesquita and Smith, 2020; Hirose, 1981; Scheiner, 2005). Japan has a unitary structure of government, in which the budgets of municipalities are supported by local taxes as well as transfers, while policing is separately administered at the prefectural level. Although transfer allocations are in large part based on programmatic formulas, political influence frequently seeps into budgetary negotiations between bureaucrats and politicians (Campbell, 1977; McMichael, 2017), local municipalities lobby both local and national politicians with

[4]Similar IV approaches have been used to study the effect of unemployment on crime in the U.S. (e.g., Gould, Weinberg and Mustard, 2002; Lin, 2008; Raphael and Winter-Ebmer, 2001), generally finding that unemployment increases property crimes.

appeals for extra funds (Muramatsu, 1997), and politicians' home municipalities within districts tend to receive more transfers (Hirano, 2006), suggesting that representation does indeed have an effect on distributive policy outcomes. Finally, most crimes in Japan are property crimes such as larceny (theft), which makes it an especially suitable environment for evaluating economic explanations for crime.

We use a short panel data set of municipality-level population demographics, election outcomes, and policy outcomes, which we measure in the years immediately before and after the new electoral rules were first used in 1996. When the effects of per capita transfers, measured by local allocation taxes (henceforth, LATs), on crime rates are estimated through a naïve OLS regression model without addressing the issue of simultaneity, we find very small (near zero) effect sizes. However, when the change in malapportionment due to the reform is used to instrument for changes in per capita transfers, the estimated negative effect of transfers on crime is larger. Exploring two possible causal pathways, we find that transfers decrease unemployment rates but do not affect the level of taxable income per capita, in line with the economic mechanisms of our theoretical logic.

Our study contributes to several interdisciplinary research streams of political science, economics, and criminology. The main empirical finding that fiscal transfers reduce crime represents a novel contribution to the political science literature on distributive politics, particularly the emerging body of research exploring the connection between redistribution and crime (e.g., Holland, 2017; Rivera and Zarate-Tenorio, 2016; Rueda and Stegmueller, 2016). The fact that employment appears to be a likely channel through which transfers reduce crime also speaks to important public policy questions about the effectiveness of public employment programs versus alternative policy solutions, such as direct cash transfers. Methodologically, our IV approach connects to a growing set of studies in economics and criminology that use quasi-experimental sources of variation—such as the rolling introduction of daylight savings, random allocation of streetlights, or instrumental variables to measure unemployment or police hiring—to better identify various situational factors that contribute to crime (e.g., Chalfin et al., 2021; Doleac and Sanders, 2015; Gould, Weinberg and Mustard, 2002; Levitt, 2002; Lin, 2008; Raphael and Winter-Ebmer, 2001). Finally, our findings suggest that central-to-local transfers in Japan, often characterized in the existing literature as a scheme aimed at helping LDP politicians secure reelection or reward loyal supporters (Catalinac, Bueno de Mesquita and Smith, 2020; Hirose, 1981; Scheiner, 2005), may have also contributed to decades of remarkably low crime during a period of major economic and societal changes. We highlight the need for scholars of comparative politics and public policy to pay closer attention to the potential positive effects of redistribution, in addition to the politics behind it.

Economic Theories of Crime and the Case of Japan

Crime is one of the most important, and yet seemingly intractable, public policy issues facing many governments. Numerous studies in economics and criminology have investigated the determinants of crime, but evidence for the effectiveness of various policy solutions remains mixed (for a review, see Wilson and Petersilia, 2011). A general pattern is that property crimes, such as theft and burglary, are correlated with greater poverty and lower levels of policing, whereas violent crimes, such as rape and murder, tend to increase with inequality and social strain (e.g., Di Tella and Schargrodsky, 2004; Fajnzylber, Lederman and Loayza, 2002; Levitt, 2002).

Our theoretical logic relies primarily on the economic theory of crime initiated by Becker (1968), because it nicely models criminal behavior when the primary motivation is pecuniary gain. According to Becker's model, would-be offenders engage in criminal activity if the benefit from committing the crime exceeds the cost. Cost is modeled as a product of the likelihood of being caught,[5] the severity of punishment (e.g., length of imprisonment) if caught, and the unit cost of the punishment. Policing and sentencing have been identified as primary policy factors affecting the first two factors, and previous studies have shown that the former is more effective than the latter. In contrast, the unit cost of imprisonment is largely determined by the opportunity cost of imprisonment, or the alternative source of income that an offender would be able to earn if he or she were not arrested.

Building on this straightforward economic model for crime, we contend that transfers can be an important factor in reducing crime because they provide (low-skilled) workers with an alternative source of income through government employment, public works projects, welfare benefits, and subsidies to local companies—thereby increasing the opportunity cost of imprisonment.[6]

The case of Japan satisfies three implicit assumptions in the economic theory of crime, which makes it an ideal test case. The first assumption is that crime is related to economic conditions. As with studies from the U.S. and elsewhere (e.g., Gould, Weinberg and Mustard, 2002; Lin, 2008; Raphael and Winter-Ebmer, 2001), the existing research on Japan consistently finds that crime rates are positively correlated with unemployment (e.g., Tsushima, 1996; Kakamu, Polasek and Wago, 2008). Moreover, violent crime is rare in Japan—roughly 88 percent of all crimes are property crimes.[7] In contrast, the general logic of the economic model of crime might be less applicable to countries where violence, narcotics, or other forms of criminal activity are more common.

The second assumption is that transfers are directed toward projects and services that improve economic conditions, primarily through increased employment.[8] In Japan, while

[5] In Japan, the proportion of crimes resulting in an arrest was 42.2% in 1996 (Ministry of Justice, 1996), and conviction rates are high (Johnson, 2002).

[6] Transfers might also effectively consume the disposable time of would-be criminals, increase their self-esteem, or increase community-level social ties through government-sponsored activities and events. We lack the data to test these potential mechanisms, so cannot rule them out.

[7] Appendix Table A.1 gives detailed country-level data on types of reported crimes from 1993 to 1999. Note, however, that municipality-level statistics do not exist.

[8] Positive externalities through increased happiness or social stability are also possible.

investment in public services represents only about 5 percent of GDP, 70 percent of this spending is implemented by local governments using intergovernmental grants or local bonds (the majority of which are financed by transfers based on fiscal demands used in calculations for transfer allocations). Meanwhile, the share of local government revenue that comes from local taxation is low, at roughly 30-40 percent.[9] In other words, most of the operating budget for local government services depends on intergovernmental transfers (Mochida, 2008). As such, it is reasonable to expect that transfers are indeed used to provide basic services and public works projects that contribute directly to local employment.

Finally, the third assumption is that the money from transfers is actually spent on these projects, rather than being funneled into corrupt uses or administrative waste. The potential for misuse of transfer funds may be higher in developing contexts than in developed democracies like Japan. Although we cannot perfectly observe how much of a given municipality's budget is spent to benefit the local population, it is reasonable to expect that only an insignificant amount will go unaccounted for, or be used for projects that do not affect the local economy in some way or another. Moreover, our identification strategy means that any such inefficiencies would only affect our findings if there were systematic differences across municipalities depending on the change in malappportionment induced by redistricting, which is unlikely to be the case.

The intergovernmental system of Japan also provides an important structural environment for evaluating the effect of transfers separately from the effect of policing budgets. Policing is naturally one of the primary means of reducing crime, so it is important to consider its potential impact. Fortunately for our purposes, prefectural police departments directly administer all district-level police stations in a strictly bureaucratic manner, such that municipalities have neither legal, political, or financial control over the police (National Police Agency, 2004), nor the ability to run their own police systems. Thus, even if

[9]This is similar to the United Kingdom and France, but much lower than in the U.S., Canada, Germany, and Sweden (Mochida, 2008, p. 17).

we could aggregate police budgets at the municipality level, such budgets would be unlikely to be correlated with our IV (the change in malapportionment).[10] Our focus on within-unit changes in municipalities within a short time window also means that our estimated effects are unlikely to be confounded by broader patterns in aggregate crime in Japan during the period we study (e.g., Halicioglu, Andrés and Yamamura, 2012).[11]

With these background characteristics of the Japanese case in mind, we can turn our attention to investigating the empirical relationship between transfers and crime. Only a handful of studies have investigated the determinants of crime in Japan, often with a focus on explaining the country's remarkably low levels. Despite rapid economic growth and urbanization since the 1950s, crime rates in Japan have consistently been among the lowest of the OECD countries, and about one tenth the crime rate in the U.S. (OECD, 2014). Most explanations for Japan's relatively low crime rate focus on unique features of society, such as "shame culture" (e.g., Becker, 1988; Hamai and Ellis, 2006), or public safety institutions like the neighborhood police substation (*kōban*) system (e.g., Parker, 2001). We do not reject these broader determinants of crime in Japan, but aim to shine a spotlight on the contribution of distributive policies to variation in crime rates across municipalities, positing the following straightforward hypothesis:

> *Increases in central-to-local fiscal transfers per capita will decrease municipality-level crime.*

[10] Municipalities without policing capabilities can still potentially implement various crime-prevention measures such as the installation of security cameras in public spaces. However, these measures would likely be funded through fiscal allocations (contributing to the overall effect on crime), so do not pose a problem for our identification strategy, but may be important (but untestable) causal pathways.

[11] We discuss potential scope conditions and limitations of using the Japanese case in subsequent sections.

Methodological Approach and Data

As noted earlier, a problem of simultaneity bias may arise in assessing policy effects when greater amounts of a social problem necessitate greater amounts of the policy to address it. These relationships are inseparable in most observational data, which makes it difficult to identify the causal effect of the policy.[12] Field experiments based on a randomized controlled trial (RCT) are often an ideal approach to causal identification of policy effects in real-world settings, but are a challenge to implement in the case of distributive politics, since politicians are loath to abdicate control over the "who gets what, when, how" (Lasswell, 1936) decisions of politics. Although there are some notable exceptions (e.g., Blattman and Annan, 2016; Imai, King and Rivera, 2020; Labonne, 2013), these studies are focused mostly on developing democracies and often involve conditional cash transfers to individuals or households, and programs funded by nongovernmental organizations, rather than routine intergovernmental transfers. RCTs are much less common (and often less feasible to implement) in developed democracies.

Malapportionment as an Instrumental Variable

To overcome the simultaneity problem in the relationship between crime and transfers, we use an IV approach. The political economy literature has identified a number of factors that affect the amount of central-to-local transfers. Among them, we focus on malapportionment, as its relationship to distributive policies has been confirmed in numerous empirical studies, and because it is conceivably conditionally independent of crime rates and potential confounders.[13] More precisely, we focus on the *change in malapportionment*

[12]Moreover, geographically targeted policies may produce spillover effects into neighboring areas. In the case of crime, intensive policing in one area may increase crime in neighboring areas if potential criminals simply move their activity.

[13]In our analysis, crime rates are conditioned on political variables, socioeconomic variables, and municipality and year fixed effects.

caused by Japan's 1994 electoral system reform and subsequent redistricting.

It is easy to understand why overrepresented districts receive more transfers per capita. Incumbents with geographically delimited districts often use their power to allocate transfers strategically to local governments for electoral gain. If we assume that all incumbents have equal power to direct funds toward their home districts, then the amount of transfers will be equal across districts in single-member district (SMD) systems and proportionally increase with district magnitude in multimember district (MMD) systems. If this assumption (i.e., all incumbents have equal power) is at least partly true empirically, then overrepresented districts can be expected to receive greater amounts of subsidies on a per capita basis. Empirical evidence that overrepresented districts receive more subsidies per capita has been found in subnational analyses of the U.S. (Ansolabehere, Gerber and Snyder, 2002; Hauk and Wacziarg, 2007), Germany (Pitlik, Schneider and Strotmann, 2006), Norway (Helland and Sørensen, 2009), and Japan (Horiuchi and Saito, 2003; Tamada, 2009), as well as in crossnational analyses (Rodden, 2002).

Horiuchi and Saito (2003), whose research design also leverages the 1994 Japanese electoral reform, offer one of the strongest identification strategies by using municipality fixed effects in a short panel. Prior to 1994, the House of Representatives used the single nontransferable vote (SNTV) system with MMDs, and districts were notoriously malapportioned in favor of rural areas. The reform introduced a mixed-member majoritarian system—in effect since the 1996 election—featuring one tier of 300 seats filled by plurality rule in SMDs and a separate tier of 200 seats filled by closed-list proportional representation in eleven regional MMDs.[14] The overall level of malapportionment decreased from 2.82 to 2.32 using the Maximin Ratio—the ratio of the maximum number of representatives per capita over the minimum)—or from 0.131 to 0.078 using the Loosemore-Hanby Index—defined as $0.5 \cdot \Sigma|s_i - p_i|$, where s_i is the seat share and p_i is the share of persons in

[14]The number of seats in each tier has subsequently been reduced over time.

the total (national) population residing in the ith district. This index takes a value of zero with a perfectly proportional allocation of representatives across districts and approaches one as the distribution becomes more unequal (Horiuchi and Saito, 2003, p. 671).

Importantly for our purposes, redistricting was implemented in a depoliticized process. The electoral reform was driven by political demands at the national level, and was never intended to change the allocation of central-to-local transfers per capita between municipalities (Narita, 1997). In addition, the new SMD boundaries were drawn based on population criteria by an independent and nonpartisan committee (Ministry of Internal Affairs and Communications, 1994).[15] Thus, no other political factor can explain the significant change in the degree of malapportionment within a municipality between 1996 and 1997, the fiscal years before and after the first election under the new SMD boundaries. Horiuchi and Saito (2003) find that changes in malapportionment due to the reform resulted in significant changes in transfers per capita. Our IV approach is based on the same identification strategy in that it utilizes the electoral reform and the change in malapportionment, transfers, and crime rates. This, combined with municipality fixed effects to subsume all time-invariant unit heterogeneities, allows us to separate the impact of malapportionment from other confounders in the first-stage regression and helps us to claim the conditional orthogonality of the relationship between malapportionment and crime rates. After presenting our results, we will further discuss potential concerns related to the exclusion restriction criterion for the validity of our instrument.

Data and Variables

We use municipalities as our unit of analysis because crime rates and electoral environments all vary significantly within prefectures. However, we exclude towns and villages (leaving

[15]The first committee consisted of two jurists, two political scientists, two former bureaucrats, and one journalist.

only cities in the sample) to control for the range of functions of local governments, ensure greater homogeneity across observations, and minimize concerns about spatial correlation.[16] We limit the temporal scope of our main analysis to the years 1996 and 1997, immediately before and after the 1996 election.

Our main identification strategy is a panel IV approach with unit and time fixed effects. For this approach to work, our IV must have sufficient intertemporal variation. In our data sample, 1996 is the last fiscal year in which the incumbents elected in the 1993 election in MMDs could affect the allocation of transfers. In contrast, those elected in the 1996 election in SMDs could influence the counterpart allocations in 1997.[17] The change in transfers between 1996 and 1997 therefore reflects the change in malapportionment, providing an ideal opportunity to employ the IV approach under a short panel setting. With two years and 688 cities per year, our total sample in the main model includes 1,376 municipality-year observations.[18]

Our main dependent variable is the logged reported crime rate (from National Police Agency records), calculated as the number of reported crimes (of all categories) in a municipality divided by the municipality's population. Unfortunately, crime subcategories are unavailable at the municipality level. However, data on subcategories at the prefectural level allow us to get a general sense of the types of crime that account for the total number of reported crimes. Table 1 shows the results of regression analyses of the reported number of all penal code offenses on subcategories for larceny, felonious, violent, and moral offenses

[16] Specifically, the amount of transfers for a given municipality might be positively or negatively correlated with that of neighboring municipalities. Since larger cities have fewer neighboring municipalities within districts, our results are less likely to be affected by the outcomes of neighboring municipalities. We also control for spatial correlations by clustering standard errors by SMD, which results in 278 clusters in the main regression.

[17] The budget for fiscal year 1996 was determined between September 1995 and February 1996; the 1996 election took place in October, so legislators elected in 1996 would not have had any influence over the 1996 budget. It is possible that incumbents elected in 1993 might have already started to focus their strategies on the new districts as early as 1994, with the release of a recommended map for the new SMDs. However, this possibility only leads to conservative estimates of the reform's impact.

[18] Descriptive statistics on the complete set of variables are presented in Appendix Table A.2.

in 1996 and 1997. All models include year and prefecture fixed effects. Model (1) shows that the fixed effects account for roughly 32% of within-unit variation. Once the number of larceny offenses is included as an explanatory variable in model (2), the within R^2 jumps to 0.943, explaining almost all variation. On the other hand, when the other three crime categories are added in model (3), the within R^2 is barely changed, and these variables are statistically insignificant. Hence, it is safe to say that our dependent variable primarily reflects the number of larcenies, the most common form of property crime.[19]

Table 1: Fixed effects panel regressions of total prefecture-level crimes on subcategories

DV: Number of Penal Code Offenses	(1)	(2)	(3)
Number of Larceny Offenses		.9945***	.9886***
		(.0856)	(.0408)
Number of Felonious Offenses			3.883
			(4.227)
Number of Violent Offenses			-.6571
			(2.748)
Number of Moral Offenses			1.244
			(3.680)
Year fixed effects	✓	✓	✓
Prefectural fixed effects	✓	✓	✓
Within R^2	.3188	.9427	.9446
Number of units (prefectures)	47	47	47
Number of observations	94	94	94

Notes: Prefectural panel data, 1996–1997, are from the National Police Agency (N.d.). Standard errors in parentheses are clustered by prefecture.

Our key explanatory variable is the logged per capita LAT transfers (*chihō kōfuzei kōfukin*), which are composed of block grants from the national government to municipalities. We do not include categorical grants (national treasury disbursement, *kokko shishutsukin*) in order to separate out the effects of social security spending, which is solely financed with these grants. Because transfers per capita are endogenous to reported crime rates, we first

[19] Police administration is managed at the prefectural level, and the criteria for what is reported as a crime might idiosyncratically vary by prefecture. However, the municipality fixed effects subsume any potential prefecture-level differences in counting crimes.

regress the logged transfers per capita on the IV, as well as control variables, and use the predicted values of the logged transfers per capita as an instrumented explanatory variable.

We operationalize our IV as an *index of malapportionment.* This variable is calculated by dividing district magnitude by the district population (in logs), and was used as the key explanatory variable in Horiuchi and Saito (2003). Although the extent of malapportionment reflects regional characteristics (such as the overrepresentation of rural areas), such spurious associations are controlled for by the municipality fixed effects as well as other covariates. Our identification strategy relies on the within-unit variation caused by the depoliticized redistricting that followed the electoral reform. Municipality fixed effects control for all observed and unobserved factors that are unchanged between 1996 and 1997, which includes any effect of an event that occurred before 1996 as well as any heterogeneities across units.

To avoid post-treatment bias, we select control variables so that covariates in each period precede the treatment variable, and time-varying covariates in the post-treatment period are not affected by the pre-treatment period (Sobel, 2012). We therefore use only the following as covariates: population (in logs), ratio of the population aged 15 and below, ratio of the population aged 65 and over, and population density (in logs).[20] Finally, our identification assumption is that the malapportionment index is independent of both crime rates and unobserved confounders that are associated with either government transfers or crime rates after conditioning on the four demographic covariates, year trends, and municipality fixed effects.[21]

[20] Performing the IV estimations without control variables, or with the full set of covariates used by Horiuchi and Saito (2003), produces substantively and statistically similar results.

[21] Since our panel consists of two time periods, the identification assumption can be understood as the independence of the change in the malapportionment index from the change in crime rates and the change in unobserved confounders after conditioning on the change in the demographic covariates.

Estimation Framework and First-Stage Results

For our estimation models, the dependent variable is the logged reported crime rate in municipality i in year t (crime_{it}). We use the logged index of malapportionment ($\mathsf{malapportionment}_{it}$) to instrument for the logged per capita local allocation taxes (LAT_{it}), conditional on other municipality characteristics ($X_{k_{it}}$) as well as year fixed effects (T_{1t}, T_{2t}) and municipality fixed effects (α_{1i}, α_{2i}) where subscripts 1 and 2 denote the first and the second stages of the IV estimation. The final terms in these equations ($\epsilon_{1_{it}}$, $\epsilon_{2_{it}}$) are disturbance terms.[22] Our model has the same number of endogenous regressors and IVs, and is thus an example of a "just-identified" IV model, which is known to yield one of the least biased estimates (Angrist and Pischke, 2008). Our estimation models are formally written as follows:

$$\mathsf{LAT}_{it} = \pi \mathsf{malapportionment}_{it} + \Sigma\beta_{1k}X_{k_{it}} + T_{1t} + \alpha_{1i} + \epsilon_{1_{it}} \tag{1}$$

$$\mathsf{crime}_{it} = \tau\widehat{\mathsf{LAT}}_{it} + \Sigma\beta_{2k}X_{k_{it}} + T_{2t} + \alpha_{2i} + \epsilon_{2_{it}} \tag{2}$$

The results of the first-stage estimation are presented in Table 2. The models are estimated without (Column 1) and with (Column 2) control variables. The results show that the index of malapportionment has a positive and statistically significant relationship (at the 1 percent level) to per capita transfers. This is consistent with the results reported by Horiuchi and Saito (2003). The within R^2 also increases by only 0.015 after adding control variables, indicating that malapportionment has an independent explanatory power. The Kleibergen-Paap rk Wald F-statistics are 21.7 and 15.1, which exceed the traditional benchmark of 10 for ruling out a weak instrument (Stock and Yogo, 2005). Later, we show that these F-statistics, combined with the statistical significance of estimates in the second-stage regression, provide a weak-IV robust confidence set to support our hypothesis

[22]Since disturbance terms are suspected to be heteroskedastic and autocorrelated, we use two-way clustered standard errors based on SMD and year (Cameron, Gelbach and Miller, 2011).

(cf. Anderson and Rubin, 1949).

Table 2: First-stage results: regression of per capita local allocation tax on malapportionment

DV: Local allocation tax per capita (log)	(1)	(2)
Malapportionment (log)	.248	.206
	(.0533)	(.0530)
Control variables		✓
Municipality fixed effects	✓	✓
Within R^2	.157	.172
Cragg-Donald Wald F statistic	41.5	26.3
Kleibergen-Paap rk Wald F statistic	21.7	15.1
Number of units (municipalities)	688	688
Number of observations	1,376	1,376

Notes: Standard errors in parentheses are clustered by single-member district (SMD) and year. Within R^2 estimated separately with `xtreg` command. See Appendix Table A.3 for the complete results.

Before moving to our second-stage results, we discuss two checks on the quality of our first-stage estimation. First, we need to minimize the possibility that the observed effects are caused by differences in known or unknown characteristics among municipalities at different levels of the instrumented treatment. That is, the instrumented variable should not be correlated with other covariates in the panel setting. We therefore checked the balance for the *first differences* of the pre-treatment covariates by dividing municipalities into four groups according to the first differences of the malapportionment index, our IV, so that the balance check captures differences in the within-unit variations of our panel data. We find that the distributions of the differentials of the pre-treatment covariates are quite similar.[23] This indicates that cities that experienced different changes in malapportionment have similar observed characteristics.

Second, we performed a trend check in which the regression analysis is performed using variables measured in 1995 and 1996 instead of 1996 and 1997. This falsification test is performed to rule out the possibility that our estimation model picks up some preceding

[23]See Appendix Figure A.1.

time-trend rather than the change in the malapportionment. The results show no sign of a trend effect: the coefficient of the instrument is close to zero and the first-stage F-statistic is 0.056.[24]

Main Results

Table 3 reports the estimates of the second-stage regression. Control variables are excluded from estimations in models 1 and 3, but included in models 2 and 4. The first two columns report the results from naïve OLS models, where the coefficients of the *uninstrumented* per capita LAT are near zero even though they are negative and statistically significant. We argue that this is an indication of endogeneity: even though transfers decrease crime, they tend to be distributed where potential offenders might commit a crime without additional financial assistance, and these offsetting mechanisms push the treatment effects toward zero. The latter two columns report the results from the IV specification. The coefficients of the instrumented per capita LAT in Column 3 and 4 are both negative and fall within 95% confidence sets based on the AR test, which is known to be weak-IV-robust (Anderson and Rubin, 1949; Mikusheva, 2010).[25]

According to Column 4 in Table 3, a one-percentage-point increase in logged per capita transfers is estimated to decrease the average reported crime rate by 0.249 percentage points [CI from 0.010 to 0.488]. To interpret the substantive meaning of these effects, consider that an average city with a population of 141,234 citizens receives a yearly LAT transfer of about ¥6.2 billion (about $54 million), which comes to ¥42,247 (about $384) per capita.[26] The average city reports 2,353 crimes per year. Thus, a single percentage-point increase in LAT transfers for the average city (about ¥59.7 million or $542,000) reduces crime by

[24] See Appendix Table A.4 for details.

[25] The AR test in our case jointly tests the null hypothesis $H_0 : \tau = 0$ *and* $\mathbb{E}(\mathsf{malapportionment} \cdot \epsilon_{2_{it}}) = 0$.

[26] Pecuniary figures are not GDP-adjusted. We set the currency exchange rate from U.S. dollar ($) to Japanese yen (¥) as $1 = ¥110.

about six ($\approx 2,353 \times 0.249$ percent) cases. To put these effects on a different scale, the estimated average cost of preventing a single crime is about ¥10.2 million ($92,600) in transfers. These effects may seem small, but it is important to keep in mind that the purpose of public works projects and other local investments goes beyond reducing crime—the negative effect on crime can be considered a by-product of local government spending, and one that is frequently overlooked in discussions and analyses of distributive politics.

Table 3: Second-stage results: regression of logged crime rates on per capita local allocation tax using malapportionment as an IV (with comparison to OLS)

DV: Crimes per 1,000 residents (log)	OLS		IV	
	(1)	(2)	(3)	(4)
Local allocation tax per capita (log)	-.0348	-.0334	-.220	-.249
	(.0122)	(.0121)	(.103)	(.122)
Control variables		✓		✓
Municipality fixed effects	✓	✓	✓	✓
AR 95% Confidence Set			[-.453,-.030]	[-.541,-.026]
Number of units (municipalities)	688	688	688	688
Number of observations	1,376	1,376	1,376	1,376

Notes: Standard errors in parentheses are clustered by SMD and year. The AR α% confidence set is calculated with Stata's ado program `weakiv` (Finlay, Magnusson and Schaffer, 2013), originally based on Anderson and Rubin (1949), where the confidence sets are estimated with Wald/Minimum Distance tests with a grid search of 2,000 times. See Appendix Table A.5 for the complete table.

Robustness Checks

In this section, we address several potential threats to the validity of our findings. First, one limitation of the IV approach is that it estimates a class of local average treatment effects (LATE) for a sample within the population (Angrist, Imbens and Rubin, 1996). Specifically, our estimates are valid only for the cities in districts that would receive an increased (decreased) amount of transfers if the level of malapportionment between the 1993 and 1996 elections deteriorated (ameliorated)—in other words, the cities within districts that are "compliers" in our design. Therefore, the observed difference in the coefficients may simply reflect a difference in the quantities estimated.

It is not possible to know which districts have such traits, and no standard test is available for a continuous IV. However, if we can show the linearity in the relationship between the instrument and the endogenous treatment, as well as the dependent variable, an observation with any value of the instrument could be a potential complier, thereby making it more plausible that our estimated LATEs are similar to the global ATEs.[27]

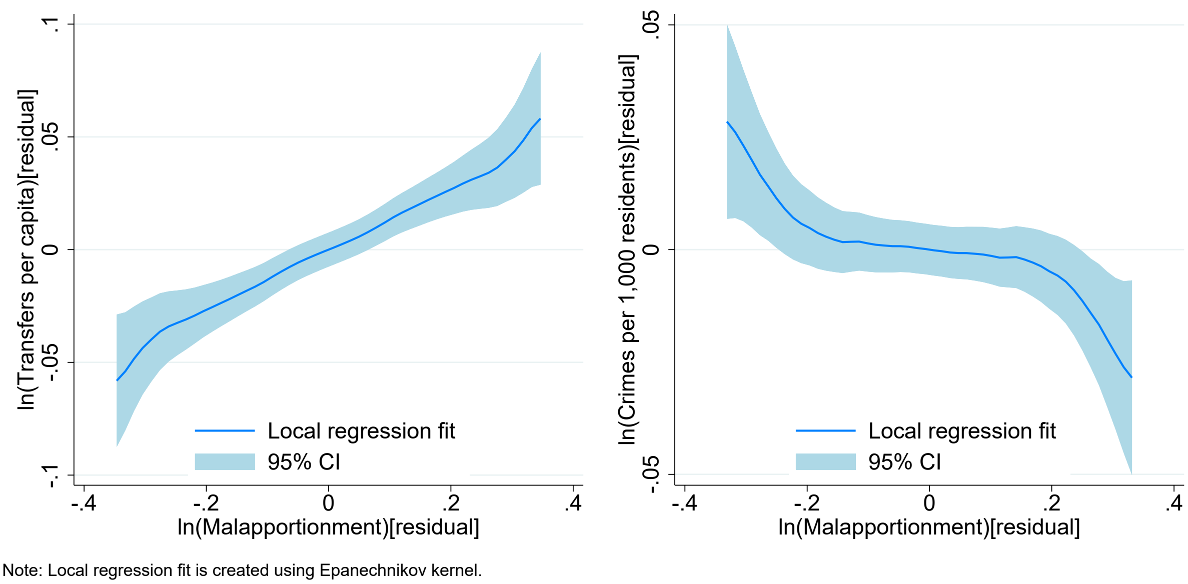

Figure 1: Nonparametric local regressions of the logged per capita transfers (left panel) and the logged reported crime rates (right panel) on the logged malapportionment index, conditional on control variables and municipality and year fixed effects. Both graphs are plotted with Epanechnikov kernel and rule-of-thumb bandwidth.

Following Miguel, Satyanath and Sergenti (2004), we present the results of nonparametric local regressions and their 95% CIs in Figure 1. The left panel shows the first stage and the right panel shows the reduced-form regression. That is, the coefficients in

[27] In other words, if we were to observe some hike in the local regressions rather than linearity, potential compliers could be limited to those where the instrumental variable takes a value near the hike. In that case, the observations clustered around the hike probably share some unobserved traits.

these equations correspond to the denominator and numerator of the Wald estimator of the IV. The plotted variables are residualized with all variables not used in the figure.[28] The left panel shows a positive linear relationship of changes in malapportionment with transfers per capita, while the right panel shows a negative linear relationship with crime rates, across the entire domain for the index of malapportionment, indicating that our IV estimates are not just picking up local associations among the key variables.

We also performed several additional robustness checks, which we summarize in Table 4.[29] First, we performed a simple placebo test by running the model with a one-year lagged dependent variable. If unknown trends other than the electoral reform affect the change in crime rates, we might observe a similar effect estimate with the lagged dependent variable for the years 1995 and 1996. Yet, the first column shows that the estimated effect is not substantively different from zero ($\tau = .061, s.e._{\tau} = .090$).

Second, to show that our results do not depend on an arbitrary selection of covariates, the model is estimated with an extended set of covariates. Specifically, we add all of the control variables used in Horiuchi and Saito (2003), apart from the LDP's seat share (a post-treatment covariate), with the same functional forms: municipality fiscal strength index (*zaisei-ryoku shisū*), per capita income (in logs), ratio of workers in the primary sector (agriculture, forestry, and fisheries), ratio of workers in the tertiary sector (primarily the service industry), and ratio of the population living in census-defined "densely inhabited

[28]Specifically, the variables in the left panel are residualized by the control variables, municipality fixed effects and year fixed effects. The transfers per capita variable is added to the right-hand side in residualizing the variables in the right panel.

[29]Full results of these robustness checks are presented in Appendix Tables A.6–A.9. Appendix Table A.10 reports the results of two additional checks based on sample restrictions. First, to explore the potential role of organized crime groups (yakuza), we exclude 17 cities where a group is headquartered. Second, to explore the role of local-level politicians' strategic spending prior to elections, we exclude 9 cities that held mayoral or assembly elections between the fiscal years of 1996 and 1997 (Senkyo.com, 2021). Neither of these sample restrictions substantially affects our results. Finally, Appendix Table A.11 reports the results with towns and villages included. For this sample, the estimate is large and negative, but far from statistically significant, so we cannot rule out a null effect. However, this analysis may suffer from spatial correlation. Moreover, unemployed persons may move to cities, and would-be criminals may avoid small towns and villages where crimes like theft are harder to conceal, so it is unsurprising that the effects might be limited to cities.

Table 4: Summary of the robustness checks on the second-stage results: estimations with a lagged dependent variable as a placebo test (column 1), with an extended set of control variables (column 2), and using the indicator of battleground district as a second IV (column 3)

DV: Crimes per 1,000 residents (log) Type of robustness check:	Using lagged dependent variable (1)	Extended set of control variables (2)	Battleground dummy as a 2nd IV (3)
Local allocation tax per capita (log)	.061	-.325	-.326
	(.090)	(.175)	(.115)
Municipality fixed effects	✓	✓	✓
Cragg-Donald Wald F statistic	42.6	14.1	16.6
Kleibergen-Paap rk Wald F statistic	16.7	10.8	9.68
AR 95% Confidence Set	[-.116, .261]	[-.778,-.011]	[-.634,-.118]
P-value for Hansen J statistic	n/a	n/a	.316
Number of units (municipalities)	686	686	686
Number of observations	1,372	1,372	1,372

Notes: Estimates are obtained using Stata's ado program `xtivreg2` (Schaffer, 2010). Standard errors in parentheses are clustered by SMD and year. The AR α% confidence set is calculated with Stata's ado program `weakiv` (Finlay, Magnusson and Schaffer, 2013), originally based on Anderson and Rubin (1949), where the confidence sets are estimated with Wald/Minimum Distance tests with a grid search of 2,000 times. The dummy for battleground district is coded as 1 if the seat-adjusted difference in vote share (vote share difference $\times$ seat) between a marginal candidate of the governing party coalition and an opposition party candidate is less than 1%.

districts" (DID).[30] We also include district magnitude, total number of previous wins for ruling coalition candidates, and prior cabinet experience for ruling coalition candidates, to capture the characteristics of electoral districts. The results in Column 2 of Table 4 indicate that the estimated effects are larger in magnitude with somewhat weaker statistical association.

Third, we estimated our main model with an additional IV: an indicator for the municipality being located within a "battleground" district. The choice of this second instrument is empirically underpinned by multiple studies in various contexts showing a positive relationship between closely contested districts and the amount of transfers allocated (e.g., Bickers and Stein, 1996; Gordon, 2011; Hirano, 2011). Here, we regard districts as competitive if the vote share margin separating one of the governing parties' candidates from victory or defeat was within 1 percentage point.[31] The results in Column 3 of Table 4 indicate that closely contested district races and per capita total transfers are strongly and positively associated (with a first-stage F-statistics of 9.68), that the estimated effect is negative and statistically significant at the 1% level, and that the model passes the overidentification test.[32] Although we prefer our "just-identified" single-IV model based on malapportionment, these results with the inclusion of a second IV based on competitiveness lend further support to our evidence of a negative effect of transfers on crime.

Altogether, these robustness checks provide reassuring support for our argument that

[30]These variables are excluded from the original models because these covariates measured in $t+1$ can be the results of the dependent variable in t, which also invites post-treatment bias. For example, the LAT per capita in t would affect the ratio of primary sector workers since part of the LAT is spent to support their industry.

[31]Using this definition, the number of battleground districts was six in 1993 (the election preceding budget allocations in 1996) and 15 in 1996 (the election preceding budget allocations in 1997), which amounts to 34 municipalities in 1993 (1996 budget) and 35 municipalities in 1996 (1997 budget). Governing parties before the 1996 election were the LDP, New Party Sakigake, and the Social Democratic Party (SDP). The latter two parties withdrew from the coalition in 1998, after the 1997 fiscal transfer allocation on which we focus.

[32]Specifically, the p-value of the Hansen's J statistic is .316, which is far from rejecting the null hypothesis that the residualized dependent variable is uncorrelated with the instruments. The validity of this instrument remains unchanged whether we narrow or broaden the definition of "battleground" to include narrower or wider margins of victory (0.5% and 2%). See Appendix Tables A.8 and A.9 for details.

transfers help to reduce crime. Nevertheless, a skeptical reader may still have some concerns about possible violations of the exclusion restriction. In short, for our finding that transfers affect crime rates to be credible, we need to believe that apart from the change in LAT transfers per capita, there were no other important changes *within municipalities* between 1996 and 1997 that could have caused the observed change in crime (and that we do not already control for in our analyses), and that changes in apportionment could not have affected crime through some other unobserved mechanism (the exclusion restriction).

For example, a reduction in the number constituents being represented might encourage a politician to become more responsive to local concerns, including public safety. If politicians' behavior could affect crime rates through some other mechanism apart from transfers, it would potentially violate the exclusion restriction. However, even if the politicians who were affected by the change in malapportionment had new incentives to concern themselves with public safety, the most obvious tool at their disposal would be to increase central government funds coming into their districts to improve their constituents' financial welfare. While politicians do have the ability to influence such distributive outcomes, they have considerably less power to influence other factors that might be correlated with crime, such as policing budgets (as explained earlier) or the strength of social ties in the community. Although we lack the systematic data on other forms of politicians' intentions and behavior to entirely rule out this possibility, it seems less plausible that changes in behavior would affect crime other than through the distributive politics channel we have proposed.

A related concern might be that the change in malapportionment could have shifted the balance of partisan representation for municipalities in affected districts, bringing in new incumbents from parties that care more about crime. In this scenario, fewer crimes in overrepresented municipalities might result not from greater fiscal transfers, but rather from some other political activity directed at the issue. This scenario is not worrisome in our case for two reasons. First, as noted, even if certain parties cared more about crime, the most direct way to affect crime rates would be through fiscal transfers—so, any change in partisan representation serves as a mediating variable connecting changes in malapportionment to changes in transfers, but has no direct connection to the relationship between transfers and crime. Second, Japan's major parties do not hold divergent policy positions on questions related to employment and crime—the most salient cleavage separating left and right during this time period, and for most of Japan's postwar history, has been security policy (e.g., Otake, 1999; Horiuchi, Smith and Yamamoto, 2018).

Mechanisms and Broader Implications

We have proposed that transfers allow local governments to provide low-skilled workers, who are at a higher risk of committing a crime, with alternative sources of income. If this is the case, the impact of the transfers should first appear as improvements in the fundamentals of the local economy, such as unemployment rates or personal income. Our strategy to test this mechanism is to replace the dependent variable in the main estimation model in Equation 2, the logged reported crime rate, with total unemployment rates, male unemployment rates, female unemployment rates, and taxable income per capita.[33] These alternative dependent variables are important for exploring some of the potential causal pathways connecting redistribution and crime, but can also be viewed as important to understanding the potential positive effects of transfers more generally.

First, we estimate the impact on unemployment. Since a portion of LAT transfers is spent on economic stimulus and public works projects that create jobs, unemployment rates should fall as the amount of transfers increases. Models (1) to (6) in Table 5 show the estimated coefficients and standard errors for the effect of transfers on unemployment rates.

[33] We also investigated the effects on three additional socioeconomic factors—marriage rates, divorce rates, and birth rates—and found no systematic associations with the instrumented government transfers.

Models (1) and (2) report the naïve OLS and IV estimates for total unemployment rates, while models (3) and (4), and models (5) and (6), do so for male and female unemployment rates, respectively.[34]

The results show that the coefficients estimated with both types of model are all negative and statistically significant. However, the coefficients with the IV estimation are several times larger than those with the naïve OLS estimation. Overall, these results look quite similar to those for crime rates. Since the unemployment rate of an average city during the period of analysis was about 4.3 percent, the results of the estimation for the total unemployment rate indicate that a 1% increase in transfers (about ¥59.7 million or \$542,000) reduces the unemployment rate by 0.067%, or 0.00289 (= 4.31×0.00067) percentage points. To translate this to a more intuitive scale, the estimated average cost to employ one unemployed person through transfers is about ¥14.6 million or \$133,000.[35]

Next, we consider the effects of transfers on income, using taxable income per capita as the dependent variable. Local residents may earn wages from a public works project that is partially financed by grants from the central government,[36] or might be hired by a private company that benefits from such subsidies. Regardless of how transfers are ultimately spent at the local level, a significant portion of the funds will eventually reach local residents. An important caveat, however, is that this process might not necessarily raise income levels—for example, if policymakers care about public safety more than improvement in living standards, they might distribute the minimum amount of money needed to dissuade crime to as many recipients as possible. The naïve OLS results in model (7) reported in Table 5

[34] One caveat of this analysis is that the unemployment rates come from the census, which is conducted once every five years and asks all those who have lived in Japan for more than three months to give their employment status as of October 1. Therefore, our data for unemployment rates in 1996 and 1997 are created through interpolation using the censuses conducted in 1995 and 2000. The dependent variable created this way is still able to reflect the impact of the change in malapportionment, but we cannot examine how the impact on unemployment rates changed over time.

[35] Since the unemployment rate of 4.3 percent implies that the unemployed population in an average city is about 6,088 persons, 0.00289 percentage points is equivalent to about four unemployed persons.

[36] Note that even if cities finance a public works project by issuing local bonds, about one third of such debts are "reimbursed" by LAT.

Table 5: Second-stage results: regression of logged unemployment rates (sub-classified by gender) and logged per capita taxable income on logged per capita local allocation tax using malapportionment as an IV (with comparison to naïve OLS)

DV:	Total Unemp. Rate (log)		Male Unemp. Rate (log)	
Estimation method:	OLS (1)	IV (2)	OLS (3)	IV (4)
Local allocation tax per capita (log)	-.00708 (.00191)	-.0670 (.0241)	-.00452 (.00196)	-.0489 (.0234)
Municipality fixed effects	✓	✓	✓	✓
Cragg-Donald Wald F statistic	n/a	26.4	n/a	26.4
Kleibergen-Paap rk Wald F statistic	n/a	15.2	n/a	15.2
AR 95% Confidence Set	n/a	[-.129,-.026]	n/a	[-.107,-.008]
Number of units (municipalities)	682	682	682	682
Number of observations	1,364	1,364	1,364	1,364

DV:	Female Unemp. Rate (log)		Taxable Income P.C. (log)	
Estimation method:	OLS (5)	IV (6)	OLS (7)	IV (8)
Local allocation tax per capita (log)	-.0124 (.00259)	-.107 (.0290)	-.0127 (.00226)	-.0126 (.0100)
Municipality fixed effects	✓	✓	✓	✓
Cragg-Donald Wald F statistic	n/a	26.4	n/a	26.3
Kleibergen-Paap rk Wald F statistic	n/a	15.2	n/a	15.1
AR 95% Confidence Set	n/a	[-.184,-.059]	n/a	[-.033,.008]
Number of units (municipalities)	682	682	688	688
Number of observations	1,364	1,364	1,376	1,376

Notes: Estimates are obtained using Stata's ado program `xtivreg2` (Schaffer, 2010). Standard errors in parentheses are clustered by SMD and year. The AR α% confidence set is calculated with Stata's ado program `weakiv` (Finlay, Magnusson and Schaffer, 2013), originally based on Anderson and Rubin (1949), where the confidence sets are estimated with Wald/Minimum Distance tests with a grid search of 2,000 times. Unemployment rates are interpolated from the 1995 and 2000 censuses. See Appendix Tables A.12 and A.13 for the complete results.

show a negative and statistically significant coefficient. Since government transfers partly aim to address the disparity in local finances, LAT is more heavily allocated to poor cities. Interestingly, the IV estimate in model (8) has a similarly sized coefficient. This indicates that the allocation of LAT transfers has no impact on the improvement of living standards through increasing income.

To summarize, our results indicate that transfers produce non-negligible negative effects on crime and unemployment, but have no impact on income. Although there are other potential channels through which transfers could produce the negative effect on crime (such as changes in overall happiness, increased social ties through community events sponsored by local governments, and so on), the findings from this analysis provide some support for the idea that the reduction in crime through transfers can at least partially be attributed to employment.[37] These findings paint a more nuanced picture than the generally negative characterization in the existing literature of intergovernmental fiscal transfers—namely that transfers mainly represent politicians' efforts to seek reelection through patronage and pork. Yes, distributing more funds to local areas may help win votes, but it is important to keep in mind that those votes are in part based on real improvements in voters' lives as a result of the transfers. Even voters who do not directly benefit in terms of employment or other payouts may indirectly benefit by living in a safer community. Researchers interested in the costs and benefits of redistribution in various applications should therefore remember to consider the relationship between transfers and crime.

Our findings also speak to broader issues in Japanese politics, which have been characterized by locally-oriented campaigning, pork, and clientelism, especially during the long period of LDP single-party dominance from 1955 to 1993 (e.g., Catalinac, Bueno de Mesquita and Smith, 2020; Hirose, 1981; Scheiner, 2005). During the same period that pork was being heavily distributed across Japan, crime was remarkably low (e.g., Tsushima,

[37]In a working paper, Imai (2020) adopts the same identification strategy we use, and finds that transfers affect construction and public sector jobs, but that these job gains might be offset by losses in other sectors.

1996). The patterns of distributive politics during this period have often been criticized as inefficient policy. At the same time, however, these policies arguably slowed economic deterioration in peripheral areas and kept crime rates down.

Since the 1990s, pork-barrel politics has declined, but inequality has risen (Chiavacci and Hommerich, 2017). From this perspective, the negative view of the Japan's distributive politics might be worth reassessing—at least to a degree. It is important to keep in mind that the effects we have shown pertain to a short time period for the set of municipalities that experienced a change in malapportionment as a result of redistricting. While this context provides the necessary conditions for our research design, and allows us to overcome the simultaneity problem in assessing the relationship between transfers and crime, it may also place some scope limitations on the generalizability of our findings to other time periods or cases. Further explorations of our findings in other contexts should be an important goal for future research.

Another important caveat is that our measure crime is based on official reported crimes, so our analysis does not fully capture the effect of distributive politics on forms of crime or malfeasance such as corruption, which may be harder to observe but closely related to the politics of redistribution and sudden fiscal windfalls for local governments. Japan's construction industry, for example, has been front and center in many corruption scandals (Woodall, 1996, pp. 84–94), and is also a recipient of central-to-local transfers through public works projects. In short, our data do not allow us to measure whether reductions in some observed forms of crime due to fiscal transfers are offset by other, less observable, forms of crime, such as bribery for construction contracts. We believe these are important possibilities for future research to explore.

Conclusion

Redistribution is a ubiquitous part of politics in all democracies. Yet whether distributive policies have any effect on important socioeconomic outcomes like crime is an understudied topic in the political science and political economy literatures, and one that oftentimes faces daunting challenges in terms of causal identification. Much of the existing literature emphasizes the negative, or purely political, aspects of distributive politics, with a focus on how fiscal transfers from central to local governments represent forms of elites' electoral mobilization strategies, clientelism, and inefficient public policy. This view has also been the conventional wisdom among scholars of Japanese politics analyzing the long-term dominance of the LDP. The LDP stayed in power for so long, it has often been argued, in large part by buying off voters and special interests with government largesse.

The results of our study suggest a partial revision to this negative view of distributive politics. Our findings show clear empirical evidence that transfers can have a non-negligible deterrent effect on crime, one of the most important public policy issues facing governments around the world. Our IV approach, which takes advantage of the well-known relationship between legislative malapportionment and budget allocation decisions, addresses the issue of simultaneity—a persistent problem of non-experimental evaluation studies of public policies. In addition, our exploration of the mechanisms behind the relationship provides novel evidence supporting the economic theory of crime introduced by Becker (1968): transfers reduce unemployment, without significantly changing levels of income. This finding is also important because it speaks to the dignity of work, and government's possible role in providing it as a way to prevent crime.

Together, our findings suggest that central-to-local transfers, far from being simply a wasteful redistribution scheme aimed at helping politicians get reelected, may have been partly responsible for the decades of low crime rates enjoyed by Japanese citizens in a period of rapid urbanization and societal changes. Japan's redistribution policies likely helped to keep citizens employed and deter economic incentives to engage in criminal activity. Analogs in distributive politics in the U.S. and elsewhere might expand the scope of inquiry to include the positive effects of government spending on local infrastructure or military bases in rural areas. Future studies might also consider how the effects of redistribution on crime are conditioned by other institutional and societal factors that are beyond the scope of our analysis of the case of Japan, such as federalism or ethnic and linguistic diversity.

Our findings highlight the possibility that even negatively perceived public spending can have non-negligible and underappreciated positive benefits. It is important to consider such possibilities when evaluating the total impact of a public policy on societal outcomes. We believe that scholars of distributive politics should further explore the effects of redistribution on crime and other socioeconomic outcomes, and that scholars of the politics of crime would be well advised to take distributive politics seriously as well.

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