Shaking Criminal Incentives*

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Abstract

We study criminal incentives exploiting the devastating shock of the 1995 Kobe earthquake. Evidence shows that the earthquake decreased burglaries but left other crime types unaffected. The effect stays significant even after controlling for unemployment, policing and income. We corroborate this by instrumenting damages with the distance from the earthquake epicentre. These findings survive various robustness checks under different specifications. The evidence is consistent with a simple theory of crime, value and specialization. We conclude that burglars respond to damages that devaluate their prospective takings. Yet, they cannot shift their specialization and substitute burglaries with other crime types.

JEL Keywords: crime, burglary, value, housing damage, specialization.

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1. Introduction

Understanding how criminal incentives function is key for the economic approach to the study of illegal behavior. In contrast to hot-blooded crimes, which are impulsive and may be driven by emotion, for sophisticated crimes prospective criminals calculate, premeditate and then decide whether to engage in an illegal activity. Incentives on whether to commit a crime are typically studied in relation to earning potentials in the formal labor market, and in relation to the cost of committing a crime, as these are approximated by the intensity of policing and the severity of punishment. This study is one of the first to systematically analyze whether criminals respond to changes in the value of the potential takings, which is the direct economic benefit from crime, and the first to investigate in this context the importance of criminal specialization, focusing primarily on whether criminals substitute less profitable crime types with other more lucrative ones.

There is a dense literature in the economics of crime and in criminology that focuses on incentives following the rational economic model of crime, which goes back to the seminal contributions of Becker (1968) and Ehrlich (1973). According to this approach a prospective criminal weighs up the expected benefits of illegal activity against the benefits of staying within the law. The vast majority of the existing studies within this approach focus on indirect alternatives, such as labor market conditions (e.g., unemployment, wages),[1] policing (e.g., number of police officers, policing innovations),[2] or punishments (e.g., changes in the length or the severity of sanctions).[3]

A small number of studies focus on the direct benefits for criminals, examining the impact of price changes on crime for one or more goods within a particular crime type.[4] In the economics of

[1]The large literature on crime and labor market conditions concentrate primarily on unemployment and wages. Freeman (1999) reviews the literature on crime and unemployment and finds that the evidence is "fragile, at best" (this is confirmed by subsequent studies by Fougere, Kramarz and Pouget, 2009; Gronqvist, 2013; and Bell, Jaitman and Machin, 2014). The smaller body of work on crime and wages, typically low wages or unskilled wages for males, reports significant associations (see Grogger, 1998; Gould, Weinberg and Mustard, 2002; and Machin and Meghir, 2004).

[2]There is a number of important contributions that report a strong relationship between crime and policing, such as Di Tella and Schargrodsky (2004); and Draca, Machin and Witt (2011). Fewer studies focus on policing innovations, such as Di Tella and Schargrodsky (2013), and Mastrobuoni (2017). Some studies, such as Ayres and Levitt (1998), and Van Ours and Vollaard (2016), also emphasize the role of technological innovations in protection.

[3]The various studies on crime and punishments has been recently reviewed by Chalfin and McCrary (2017), who conclude that the "evidence in favor of deterrence effects is mixed", as crime seems to respond to police but not to the severity of criminal sanctions.

[4]Related is also the literature on criminal earnings, such as Levitt and Venkatesh (2000), while Mastrobuoni and Rivers (2019) explore the disutility of prison, concentrating on robbers' behavior when they decide to leave or collect

crime literature Reilly and Witt (2008) show that the decline in the price of audio-visual goods led to a reduction in burglary. D'Este (2014) presents evidence from US pawnshops on the responsiveness of burglars to changes in prices. Braakmann, Chevalier and Wilson (2017) show that increases in the price of gold increase property crime in areas that hold disproportionally large amounts of jewellery. In criminology, several studies follow Clarke's (1999, 2000) approach and focus on particular goods, such as copper cable (Sidebottom, Belur, Bowers, Tompson and Johnson, 2011; Sidebottom, Ashby, and Johnson, 2014; and Brabenec and Montag, 2014), electrical equipment (Wellsmith and Burrell, 2005) and livestock (Sidebottom, 2013).[5]

Draca, Koutmeridis and Machin (2019) examine a panel of various stolen goods and reveal strong and significant crime-price elasticities. For specific commodity-related crimes, metals and particularly for copper they develop an instrumental variables structure, instrumenting local scrap copper prices with global copper prices, revealing an even stronger causal relationship. Importantly, their analysis indicates that the estimated crime-price elasticity retains its strong and statistically significant effect even after controlling for the impact of unemployment and wages.[6] This study shows that changing prices explain a significant proportion of the falling trend in crime.[7]

All the above-mentioned important studies concentrate on how prices influence crime. Nevertheless, prices of prospective takings are related to other factors that influence legal and illegal behavior. For instance, prices might influence all of the following: wages and unemployment, how protective people are of particular goods that have become more or less pricy, and how police advertises and investigates incidences of high-price goods that are particularly targeted by criminals. These are some of the many factors that could bias the estimated results. Also, market retail prices are different from street resale prices, which is the return to a criminal, leading to measurement error biases. Overall, despite their importance and their availability in various data sources, the use of prices solely to capture criminal returns might give raise to some reasonable concerns.

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more money from the bank for an additional minute.

[5]According to Clarke's (1999) influential contribution the rate of theft of one item is determined by the extent that it is "CRAVED" in terms of the attributes of: Concealability, Removability, Availability, Value, Enjoyability, and Disposability.

[6]Kirchmaier, Machin, Sandi and Witt (2018) also focus on metal crime and confirm these results by comparing the impact of prices to the impact of policing and policy.

[7]For crime trends see also the benchmark review by Levitt (2004) and the more recent review on crime and economic incentives by Draca and Machin (2015).

This paper contributes to the existing literature by adding something new to the picture. In particular, we focus on the shift in the economic incentives of criminals, caused by a largely unanticipated historical event, the 1995 Great Hanshin-Awaji earthquake or the so called Kobe earthquake, which caused severe damages in some municipalities, while it left other municipalities utterly unaffected (see Figures 1 and 2, and Table 1). An earthquake may have conflicting effects on crime. In fact, it can decrease crime by reducing the stakes, through damaging and devaluing potential stolen properties, especially for burglaries which relate more closely to housing damages compared to other crime types. Whereas, it may also boost crime by increasing the need or by making it easier to burgle goods from unprotected damaged buildings.[8] We investigate thoroughly this open empirical question and offer comprehensive evidence that criminal incentives are primarily driven by the stakes.

Damages in houses that devaluate the prospective loot inside them, by damaging valuable goods and leading people to remove them from damaged buildings, offer a new way to approximate changes in criminal returns. Also, the nature of the shock removes potential concerns in relation to endogeneity, especially since the entire area around Kobe was considered to be relatively safe from the threat of earthquakes, as it is discussed in detail in the sections to follow. The hypothesis that we advance and empirically establish is that burglars respond to changes that reduce the value of their prospective takings after the earthquake damages but they do not substitute the lost income due to the decrease in burglaries with increases in other crime types, implying that there are adjustment costs to changes in criminal specialization.

We construct a unique dataset by combining detailed data sources on crime, housing damage due to the earthquake, and various municipality-level socioeconomic characteristics. Using this matched dataset, we find strong and significant crime-value elasticities even after controlling for other potentially important confounding factors, such as labor market conditions and police forces. Intuitively, burglars avoid municipalities with reduced value of prospective takings, those that have been hit by the earthquake, but they continue to target houses in unaffected municipalities where

[8] The impact of the Kobe earthquake on crime has been understudied among scholars but has also been overlooked by media too, as the devastating damage has received most of the attention. Murasawa (2015) offers one of the very few anecdotes according to which there is a consensus that crime did not sharply increase after the Kobe earthquake.

the value of loot remains relatively high. Evidence from various countries around the same period suggests that among the top targets for burglars are sensitive electronic goods which can get devaluated significantly by a damaging earthquake, such as video, stereo/Hi-fi equipment, television, camera, computer equipment and mobile phones. Also, after the earthquake, victims are less likely to keep cash or precious products, such as jewellery, inside their damaged properties.[9] These reasons reduce the value of prospective criminal takings, especially for burglars in the municipalities that have been affected by the earthquake. Yet, the decline in crime occurs only for burglaries and not for other property crime types, such as theft, excluding a generic sentiment of sympathy from the side of criminals, which would have reduced not only burglaries but also other crime types in earthquake-affected areas (see Table 2 and Figure 3).

Apart from confounding factors, there are other issues that we tackle by collecting more detailed data. For instance, it might be the case that people are less likely to report a burglary after their place has been damaged by an earthquake. To tackle such measurement issues we use data not only for recorded crime but also for actual arrests. Using this additional data we can overcome measurement issues due to reporting, as offenders can get arrested even if their committed crimes are not reported by all their victims but by some of them only. Arguable, issues of endogenous reporting should be more important for recorded crime and of secondary importance for actual arrests.

Another concern relates to the fact that earthquake damages might mechanically decrease burglaries by simply reducing the number of houses and therefore of potential targets for burglars. To overcome this problem we measure housing damage using both fully and partly destroyed houses, as this concern is valid only for the former. For partly destroyed houses, the properties are still there and they form potential targets for burglars. Also, property crimes inside partly destroyed houses will still be recorded as burglaries, while it is possible some of the incidences in fully destroyed houses not to be recorded as burglaries. A related issue is that these partly damaged houses might become an easier target as they are less well protected precisely due to the earthquake damage.

[9] Clarke's (1999) analysis of "hot products" offers detailed evidence on what is stolen in various crime categories using sources from different countries. For residential burglaries in particular he highlights that among the hottest products is cash, electronic goods that we mentioned above, jewellery and purse/wallet.

Our results show that the change in the value of prospective loot after the earthquake is stronger and concerns related to decreased protection in partly destroyed houses can only underestimate our results by biasing our estimates downwards.

Even though we collect further data to account for several confounding factors, such as policing, unemployment, income per capita and population at the municipality level, there might still be concerns regarding the endogeneity of housing damage. To address this concern, apart from our Difference-in-Differences (DiD) approach we also employ an Instrumental Variables (IV) strategy, instrumenting the intensity of housing damage with the distance from the earthquake epicentre. The closer a municipality is to the earthquake epicentre, the more severe the housing damage due to the earthquake is (see Figure 4). In addition, since we control for many other confounding factors, it is unlikely the distance of each municipality from the earthquake epicentre to influence burglaries through any other channel apart from the change in the value of loot for housebreakers due to the earthquake damages. Our IV results confirm the DiD results and indicate a strong and statistically significant crime-value elasticity.

To offer an example, the year after the earthquake the heavily influenced municipality of Awaji, located around six kilometres away from the earthquake epicentre, experienced an annual decrease in burglaries of 57 percent. In contrast, municipalities further away, such as Kadoma, located around fifty-five kilometres away from the earthquake epicentre, have not encountered significant changes, as burglaries barely increased by less than 0.5 percent.[10] The full details for all the municipalities appear in Figure 5, which illustrates the correlation between the annual differences in logged burglary and the logged distance from the earthquake epicentre. Before the earthquake in 1994 this correlation is insignificant and equal to 0, while after the earthquake in 1995 the correlation becomes highly significant and equal to 0.46. This key result of our study clearly highlights that from a non-existent link just before the earthquake, we move to a clear, strong and significant connection between damage and burglaries the year just after the earthquake. This finding offers further evidence that housebreakers respond to devaluations in the value of prospective takings caused by the damaging earthquake, reducing the number of burglaries in quake-affected areas.

[10]In fact, in Awaji burglaries dropped from 23 in 1994 to just 10 in 1995. Whereas, in Kadoma they remained almost unaltered from 241 in 1994 to 242 in 1995.

To tackle concerns in relation to the validity of our instrument, we perform falsification checks with alternative IV models using the distances from other key locations, such as some of the largest metropolitan areas or the epicentres from some of the deadliest earthquakes in Japan. Nonetheless, all these placebo locations offer insignificant estimates, increasing our confidence in the results.

Importantly, other crime types apart from burglaries remain unaffected, excluding potential generalized effects, which could have been the case if the devastating earthquake had led people to commit crimes for their survival.[11] A related concern could have been that in areas with large damages the police had to deal with other issues apart from crime and this might have been the reason why burglaries dropped. We can exclude this possibility, as the evidence indicates that the police kept on dealing with crime issues and for this reason there is no reduction in arrests for other crime types except for burglaries (see Table 2). The fact that criminals do not replace the income loss due to the decline in burglaries with other crime types, implies that criminals might face adjustment costs that do not allow them to switch easily their specialization from burglaries, for which the returns have declined due to the earthquake damage, to other crime types. The importance of adjustment costs to changes in criminal specialization is a fundamental aspect of criminal behavior, which however has been overlooked in the related existing literature.

As the founder of the economics of crime, Gary Becker (2005), puts it: "The Kobe earthquake of 1995 killed over 6,000 persons, and destroyed more than 100,000 homes, still the economic recovery not only of Japan but also of the Kobe economy was rapid." In fact Kobe recovered from the damaging earthquake in the first couple of years (see Horwich, 2000). Consistent with this we find that the impact of the damage on crime occurred also in the very beginning of the post-earthquake period.

Our approach relates also to other branches of the existing literature. There is a connection, for instance, with studies that focus on the impact of natural disasters on the economy.[12] For the Kobe earthquake duPont and Noy (2015) highlight the speed of recovery, while Noy, Okuyama and Sawada (2015) show that population size and especially average income have remained lower due to the earthquake for a period of fifteen years. There are also some studies that examine the effects of

[11]See for instance Bignon, Caroli and Galbiati (2017).
[12]See Barro (2006 and 2009). Cavallo and Noy (2009) review this literature.

earthquakes on key socio-economic variables (see Porcelli and Trezzi, 2016 and Kirchberger, 2017). Hombrados (2018) focuses on a Chilean earthquake and finds a crime-reducing effect for property offences and particularly for burglaries, attributing this to the strengthening of community life post-earthquake.

Some recent studies have also focused on behavioral aspects that influence criminal behavior. These include the field experimental approaches of Blattman, Jamison and Sheridan (2017), and Heller, Shah, Guryan, Ludwig, Mullainathan and Pollack (2017), which reveal strong crime reduction effects due to the functioning of cognitive programmes. This approach has been particularly effective in reducing violent crimes. We consider that this behavioral approach complements the Beckerian approach as it seems to be successful in explaining hot-blooded crimes, while the rational economic model of crime is particularly successful in accounting for sophisticated illegal activities, such as property crimes. Despite the importance of the behavioral approach, the analysis of impulsive crime goes beyond the scope of our study, as we focus on sophisticated property crimes and primarily on burglaries, for which economic incentives play the predominant role.

After this introductory section the rest of the paper is structured as follows. In Section 2 we model crime, value and adjustment costs to changes in criminal specialization. In Section 3 we describe the datasets we use on crime, damages and municipality-level characteristics. Section 4 offers the empirical modelling approaches to analyze the impact of a drop in criminal returns on the occurrence of crime using DiD and IV approaches. Section 5 gives the results from the descriptive DiD, the DiD estimates and the IV estimates. Section 6 reports the several robustness checks that we perform. In Section 7 we discuss our results, together with the importance of adjustment costs to changes in criminal specialization and their link to substitution effects across different crime types, while Section 8 offers some concluding remarks.

2. Modelling Crime, Value and Specialization

We first consider the standard economic model of crime and incentives based on Freeman (1999), Machin and Meghir (2004) and Draca, Koutmeridis and Machin (2019), who extend the seminal model of Becker (1968) and Ehrlich (1973) to test empirically its theoretical predictions. We primarily examine choices of stolen quantities across different crime types when the value of prospective takings changes. In our study, for instance, the count of burglaries is expected to decline, as the loot devaluates due to the damaging earthquake. We derive a crime-value elasticity that can be empirically estimated and we also clarify the role of other crime determinants, such as wages, sanctions, the probability of detection and adjustment costs to changes in criminal specialization, relating them to the crime-value elasticity.

Initially we employ a simplifying version of the model. If V is defined as the resale value of prospective takings from successful crime, π the probability of being caught, S the punishment if caught and W the gain from legitimate labor, then the decision-maker will choose to commit crimes when the following inequality, comparing the expected returns from crime to the legal labor market wage, holds:

$$(1-\pi_{i\tau})V_\tau - \pi_{i\tau}S_\tau > W_i \tag{1}$$

In (1) the subscript $i\tau$ on the probability of being caught π reflects heterogeneity for individual i when he commits a crime of type τ (e.g., burglary, theft). The illegal gain V reflects the resale value of the stolen loot from crime type τ when the criminal resales it, which does not depend on the characteristics of the individual criminal. Legislation punishes individuals equally for crimes of a given type τ that is why S does not depend on the individual criminal either, while legitimate wages depend on the characteristics of individual i. Individuals might specialize only on one crime type, say burglary, or they might commit crimes of different types, for instance a particular criminal might mainly specialize in burglary but also engage in some theft. Our model takes into account this and examines criminals' potential specialization.

2.1. Deriving the Crime-Value Elasticity

When there is no individual or crime-type heterogeneity, the only decision is between legal and illegal work. The decision to commit a crime with a value of prospective takings V, occurs when the inequality below holds, which is the standard one from the Becker (1968) and Ehrlich (1973) economic model of crime:

$$(1-\pi)V - \pi S > W \tag{2}$$

The most relevant case for our empirical analysis is that of homogeneous individuals and heterogeneous crime types, as the data available about the type of criminal is limited, but there is a lot of detail identifying different crime types. Now the basic inequality becomes:

$$(1 - \pi_\tau)V_\tau - \pi_\tau S_\tau > W \tag{3}$$

In (3), the individual criminal not only decides between legal and illegal work but also between alternative types, τ, of illegal work.

Now consider that there are two crime types, indexed by 1 and 2 respectively. For example, we can consider crime type $\tau = 1$ to be burglary and $\tau = 2$ to be theft. If inequality (3) holds for both 1 and 2, the thief has the choice between the two crime types. Conditional upon doing crime, an individual chooses to commit crime type 1 rather than 2 as long as the following inequality strictly holds.

$$(1 - \pi_1)V_1 - \pi_1 S_1 > (1 - \pi_2)V_2 - \pi_2 S_2 \tag{4}$$

In our example of only two different crime types, in inequality (3), τ takes the value 1 or 2. However, as crime type 1 increases, the gap between the left and the right hand side of (3) declines and an equilibrium occurs when it holds with equality. A natural way to model crime in this setting is to allow the probability of being caught to change. For instance, π_1 should increase as opportunities for crime type 1 become scarce in the short run. This means that committing crime type 1 is less risky in the beginning. Nevertheless, as more criminals choose to commit the same crime type, the probability of being caught increases.[13] π_1 can be written as an increasing function of the stolen quantity for crime type 1: $\pi_1 = kC_1$ with $k > 0$ and C_1 denoting the count of offences for crime type 1.

Replacing this in (3) gives the following inequality in which both the count of crime type 1 and the prospective value of its takings appear:

$$(1 - kC_1)V_1 - kC_1 S_1 > W \tag{5}$$

[13] According to Ehrlich (1996) this is the demand side of the market for offences.

In equilibrium the above relationship holds with equality. We also rearrange it and solve for C_1 to start working towards deriving the crime-value elasticity:

$$C_1 = (V_1 - W)/[k(V_1 + S_1)] \tag{6}$$

Taking the partial derivative of (6) with respect to value and multiplying by (V_1/C_1) gives the crime-value elasticity:

$$(\partial C_1/\partial V_1)(V_1/C_1) = \{(S_1 + W)/[k(V_1 + S_1)^2]\} \tag{7}$$

In equation (7), the elasticity is always positive, so that decreases in the value of prospective takings for crime type 1, generate decreases in the quantity of committed crimes of type 1.

The elasticity in (7) describes the response of crime to changes in stolen value for a single crime type. To make the role of relative returns between crime types explicit, we can augment the model with the inclusion of a second crime type. We first assume that both wages and sentences are the same for potential criminals choosing between the two crime types. That is, the punishment for crime type 1 or 2 is the same $S = S_1 = S_2$ and the legal wage is W. This makes the decision of whether to commit crime type 1 as opposed to 2 to depend on the relative values of prospective takings from the two alternative crime types. The point about equal punishments across crime types is a plausible assumption across similar property crime types.[14] A crime-value elasticity for crime type 1 can then be derived as follows:

$$(\partial C_1/\partial V_1)(V_1/C_1) = \{(S + V_2)(1 - kC_2)/[k(V_1 + S)^2]\}(V_1/C_1) \tag{8}$$

In the two-crime-type model, again the crime-value elasticity is always positive (recall that $1 - kC_2$ is always positive as kC_2 replaces the probability π_2, which is always less than or equal to 1). In subsequent sections we examine empirically this theoretical prediction, deriving statistically significant elasticities that confirm this result.

[14] The norm is sentences to be the same across similar crimes but there are exceptions where they vary. In analyses that go beyond the scope of our study, Kessler and Levitt (1999) and Bell, Jaitman and Machin (2014) exploit such exceptions of changes in the severity of punishment to examine how it influenced criminal behavior.

2.2. Criminal Specialization and Adjustment Costs

Equation (8) offers the own value elasticity of crime, which is always positive. To consider adjustments in criminal specialization it would be important to derive the cross-value elasticity of crime, which is the change in crime type 2 when the value of the prospective takings for crime type 1 change. To derive this take (4) with equality, replace the probability of being caught with the crime quantities, solve for C_2, take the partial derivative with respect to $V1$ and multiply both sides by V_1/C_2. This gives the following relationship:

$$(\partial C_2/\partial V_1)(V_1/C_2) = \{-(1-kC_1)/[k(V_2+S)]\}(V_1/C_2) \tag{9}$$

This cross-value elasticity of crime is always negative. One important feature of equations (8) and (9) is that a decline in the value of prospective takings for crime type V_1, say burglary, decreases the quantity of burglaries from (8), while it increases the quantity of offences for the other crime type C_2, say theft, from (9). This results indicates that changes in the returns from crime might generate switching from one to another crime type.[15]

Importantly, a more realistic model should consider that criminals are not fully flexible to switch across different crime types due to adjustment costs when changing their criminal specialization. For instance, when the returns for burglaries go down, a frictionless model would predict that burglaries drop and other crime types, such as theft, increase. However, when there are switching frictions to changes in criminal specialization, which means that it is difficult a burglar to switch to a thief, this might not be the case. Without loss of generality we model such adjustment costs to changes in criminal specialization through increases in the probability of being caught for the newly specialized criminal.

We examine the behavior of a criminal who is examining whether and which type of crime he should commit next. The easiest way to see this is to consider inequality (11) below, which is (5) for crime type 1 and consider that this also holds for crime type 2 (also recall from above that

[15] Crime switching is unexplored in the economics of crime literature. A related paper that indirectly examines this, is Levitt (1998), which however focuses on the rather different topic of distinguishing incapacitation from deterrence effects – issues that go beyond the scope of the present study – through examining whether changes in arrest rates for one crime type make criminals switch to other crime types.

$S = S_1 = S_2$).

$$(1 - \pi_1)V_1 - \pi_1 S > W \tag{10}$$

Initially assume that crime type 1 is more profitable, so the criminal fully specializes in this crime type. Now, assume that the value of prospective takings V_1 drop and the left-hand side of the inequality (10) is no longer larger than legal wages. This means that some individuals with relatively low legal wages will no longer commit property crime of type 1. This is also the positive crime-value elasticity result of equation (7). In the absence of adjustment costs to changes in criminal specialization, ceteris paribus the criminal will switch to committing crime type 2, as inequality (10) still holds for this crime type. This is the negative cross-value elasticity of crime expressed in equation (9). Yet, when there exist adjustment costs when criminals switch from crime type 1 to type 2, inequality (10) for crime type 2 can be modified to take into account such frictions, as follows:

$$(1 - \overline{\pi_2})V_2 - \overline{\pi_2} S > W \tag{11}$$

One way to model adjustment costs to changes in criminal specialization is to allow the probability of being caught to be higher under adjustment costs, $\overline{\pi_2} = \pi_2 + \xi$, where ξ is the adjustment cost when switching from 1 to crime type 2.[16] Intuitively, this means that under adjustment costs somebody who specializes in crime type 1, say burglary, might have a larger probability to get caught when he switches to the new crime type 2, say theft. If such adjustment costs are sizeable, the inequality above might no longer hold. The result in this case will be the following: a drop in the value of criminal takings for type 1, V_1, decreases the count of crimes for this crime type C_1, say burglaries, without affecting the count of crimes for the other crime type C_2, say theft. This is an important theoretical result that is tested empirically in the sections that follow.

[16] The most accurate way to denote the adjustment cost when switching from crime type 1 to 2 would be ξ_{12} and its general form would be $\xi_{\tau\tau'}$, denoting a switch from crime type τ to another crime type τ'. Also, $\xi_{\tau\tau'}$ does not need to be equal to $\xi_{\tau'\tau}$, and therefore ξ_{12} does not need to be equal to ξ_{21}, implying that switching from a burglar to a thief might be harder or easier than switching from a thief to a burglar. For our purposes denoting the adjustment cost simply as ξ suffices, though modelling more complicated specialization switches remains a promising avenue for future studies.

3. Data on Crime and Damage from the Kobe Earthquake

The Kobe Earthquake

The Great Hanshin earthquake, or Kobe earthquake, occurred on 17 January 1995 in the southern part of Hyogo Prefecture, Japan, which combined with Osaka, is known as Hanshin. The square in Panel A of Figure 1 indicates the location of Hyogo Prefecture, while the star in Panel B indicates the earthquake epicentre. Beginning at 5:46am, the earthquake measured 6.9 on the moment magnitude scale and 7 on the Japan Meteorological Agency (JMA) Shindo intensity scale (The City of Kobe, 2009).[17] The core of the earthquake was located 17km beneath its epicentre, on the northern end of Awaji Island, just 20km away from the centre of the city of Kobe (see Figure 2).

The earthquake caused the second largest loss of lives in post-war Japan: 6,434 people were killed, 40,092 people were injured, and more than 300,000 people evacuated. More than 682,000 homes, factories, and shops were destroyed or burnt down and infrastructure such as water, electricity, gas supplies and phone lines were seriously disrupted. About 4,600 deaths were recorded in Kobe. Among major cities, Kobe, with its population of 1.5 million, was the closest to the epicentre and hit by the strongest tremors. The Kobe earthquake was Japan's worst earthquake in the 20th century after the Great Kanto earthquake in 1923, which killed more than 105,000 people.

The earthquake was caused by a sudden movement of the Nojima Fault, which up until then was not considered a dangerous fault, indicating that this was an unanticipated shock. During the quake the sides of the fault shifted 2 to 3 metres in opposite directions. There is a large list of reasons why the earthquake caused such a devastating loss in lives and housing damage. First of all, the earthquake was very shallow, which means it is more likely to cause extensive damage, compared to relatively deeper earthquakes. Second, the timing of the earthquake (5:46am) found many inside their homes sleeping, increasing the number of casualties. Third, Kobe lies in area that many Japanese thought was unlikely to be hit by a major earthquake and thus the residents there were not prepared for such a shock. Kobe is considered one of the most attractive cities in Japan

[17] The seismic intensity of 7 on the JMA Shindo intensity scale is assigned to an earthquake strong enough to alter a landform or cause a landslide.

and ironically some people even moved there to escape earthquakes (see Hays, 2009).

Crime and Police Data

Crime and police data come from the police reports and are available for 115 police offices, whereas housing damage and socio-economic characteristics come from different datasets which are available for 163 municipalities. Our aim is to estimate the response in the behavior of housebreakers to damages that reduce the value of prospective takings from burglaries. To analyze the impact of housing damage on crime, and burglaries in particular, we combine data on crime, police forces, earthquake damage, and municipality characteristics. The crime data are obtained from the Statistical Crime Report (*Hanzai Tokeisyo*) which provides information on annual criminal activity at the police-office level in two prefectures: the Hyogo Prefecture and the Osaka Prefecture. Information is available for the violations of criminal law and civil law. Within the violations of criminal law, data are available for total crime, serious offense, and total theft, where total theft is further divided into burglary and non-burglary theft.

We measure police forces using the number of police officers, which was provided by the Hyogo and Osaka Prefecture Police upon request. The documents report the head counts of police officers at the police-office level. We employ the data on criminal activity and police forces for 115 police offices in Hyogo and Osaka Prefectures from 1990 to 2000.

For each type of crime, the data report the numbers of recorded crime events and arrested persons. To understand each measure, consider the following hypothetical situation: there were two recorded criminal events, each of which involved three offenders. In this case, the numbers of recorded events will be two and the arrested persons will be three. Each of these measures captures different dimensions of criminal activity. The number of recorded events reflects incidences of criminal events with potential reporting biases, whereas the numbers of arrested persons reflect incidences of crime and police effort. It should be noted that these crime measures understate the actual incidences of crime as not all the incidences are recorded by the police nor all the criminals are successfully arrested. Thus, if there is a unit change, say, in recorded crimes, it is likely that actual crime incidences have changed by more than a unit.

Data on Housing Damage and Municipality Characteristics

The crime and policing data are matched at the municipality level with data on housing damage and socio-economic characteristics. Information on housing damage due to the earthquake is obtained from the Disaster Prevention Divisions of Hyogo and Osaka Prefectures (Hyogo Prefecture, 2005; and Osaka Prefecture, 1997). The documents report the number of houses damaged by the earthquake, and this number is further divided into sub-categories for fully and partly damaged houses, according to the extent of the damage.[18]

Data on municipality characteristics are obtained from the survey report, the System of Social and Demographic Statistics (*Syakai Jinko Tokei Taikei*). The survey covers every municipality in Japan and collects information on socio-economic and demographic characteristics. The survey has been conducted since 1976 and provides 1,500 social and demographic variables by municipality. As socio-economic indicators, we use logged income per capita, logged population size, unemployment rate, and proportion of foreigners. As the information on numbers of unemployed, foreigners and those in the labor force are available in census years (i.e., 1995, 2000 and 2005), we linearly interpolate the required variables between census years to compute the unemployment rate and the proportion of foreigners. Data on housing damage and socio-economic indicators are available for 163 municipalities located in Hyogo and Osaka Prefectures.

Matching Crime Data to Damage Data

As the data on crime and police forces were recorded at the police-office level, and those on housing damage and socio-economic indicators were recorded at the municipality-level, these datasets with different units of observations have to be matched. There are three cases for matching: (i) one police office covers multiple municipalities; (ii) one municipality is covered by multiple police offices; and (iii) multiple police offices cover multiple municipalities. In case (i), a weighted mean of

[18] In the rest of the paper a "fully damaged house" is a property which has lost its fundamental value as residence, i.e., either the home is completely collapsed or the home has no possibility to be used again by repairing. This is defined as a home whose total floor space is damaged by 70% or over, or the economic damage to main structure of the home is 50% or more of the value of the home. We refer to a "partly damaged house" for a property which has partly lost its fundamental value as residence, i.e., the home is damaged but can be used again by repairing. This is defined as a home whose total floor space is damaged by 20% or over but less than 70%, or the economic damage to main structure of the home is 20% or more but less than 50% of the value of the home.

each municipality level variable is computed, and subsequently the weighted mean is matched with the crime and police data. In case (ii), a weighted mean of each police-office level crime variable is computed, and the weighted mean is matched with the data on earthquake damage and municipality characteristics. In case (iii), the weighted means of each municipality level variable and each police-office level crime variable are computed. Subsequently, the weighted means of municipality level and police-office level variables are matched. The weight used is population size, except for unemployment rate for which the size of labor force population is used as weight.

After matching, the number of municipality-police-office pairs becomes 94. As a result, our final sample consists of observations for 94 municipality-police-office pairs from 1990 to 2000.

4. Empirical Models of Burglary and Value

We analyze the impact of a shock to the value of criminal takings on the occurrence of crime by estimating the effects of housing damage caused by the earthquake on crime. Our starting point is a DiD model for the determinants of crime, and particularly for burglary, which is specified as follows:

$$Log(Crime_{mt}) = \alpha_1 + \beta_1(Log(Damage_m) \times Post_t) + X'_{mt}\zeta + \mu_m + \rho_t + \epsilon_{mt} \tag{12}$$

where $Log(Crime_{mt})$ represents logged number of recorded crime or of arrests for the following crime types: total crime, serious offence, total theft, burglary and non-burglary theft, in municipality m in year t.[19] We are primarily interested in burglary but we examine other crime types to identify potential substitution effects, as a decrease in burglaries may increase other crime types. The term $Log(Damage_m) \times Post_t$ is the interaction of $Log(Damage_m)$ corresponding to the logged number of destroyed houses and $Post_t$ corresponding to the dummy variable that equals one if the year is 1995 or after (i.e., post-earthquake period). In treated municipalities, namely the ones influenced by the earthquake, the housing damage is positive, while in control municipalities the number of

[19] As this study requires the matching of police office level data and municipality level data, the unit of observation is the municipality-police-office pair instead of the municipality. However, for the sake of brevity, we hereafter call a municipality-police-office pair a municipality unless otherwise explicitly stated.

fully damaged houses is equal to zero. The municipality characteristics, X_{mt}, and the parameter ζ are $K \times 1$ vectors, where K is the number of time-varying variables capturing municipality characteristics, such as population size, the rates of police forces, unemployment, foreigners and income per capita. μ_m and ρ_t are municipality fixed effects and year fixed effects, respectively, and ϵ_{mt} is the disturbance term.

Our coefficient of interest is β_1 which measures the impact of housing damage, and therefore of the decline in the value of prospective takings for burglars, on the number of burglaries. In our research design, we define the treatment group as the set of municipalities with at least one fully damaged house due to the earthquake, while the control group is defined as the set of municipalities with no fully damaged house.[20] It should be noted that, within the treatment group, there is variation in the extent of housing damage. Figure 2 and Table 1 illustrate this point. Figure 2 presents the approximate locations of the epicentre that caused the Kobe earthquake, and seismic intensities. Each block corresponds to a municipality. Figure 2 indicates that the highest seismic intensity of 7 on the JMA scale was observed close to the epicentre along the fault line. Not surprisingly, the closer a municipality was located to the epicentre, the higher the seismic intensity was (see Figure 4). In the treatment group, approximately 1.8 homes were fully destroyed per 100 persons with a standard deviation of around 3, while the corresponding figure for partly destroyed homes is 2.4 with a standard deviation of 3.9, indicating that there is heterogeneity in the extent of treatment (see Panel A of Table 1).

For β_1 in equation (12) to capture the effect of housing damage on burglary, we require the assumption that the trends in the treatment and control groups were similar prior to the earthquake. Figure 3 plots the burglary rate by treatment status, where the burglary rate is expressed as the number of burglary per 1,000 individuals. Panel A reports the rate for recorded burglary events, while Panel C reports the rate for arrests due to burglary. Prior to the earthquake, the burglary rates between the treatment and control groups exhibited similar trends for both recorded event and arrest rates. After the earthquake, the burglary rate in the treatment group appears to have

[20] We considered this as a reasonable way to distinguish the treatment from the control group. Nevertheless, the main results of our study do not depend on this distinction, as they hold even we define the treatment municipalities as those with at least one partly destroyed house.

decreased much more in both panels. In particular, Panel C indicates that the arrest rate in the treatment group sharply decreased after the earthquake, indicating that the earthquake-induced housing damage reduced the rate of burglary. Panels B and D of Figure 3 display total crime excluding burglaries and in these two panels the trends in the treatment and control groups were also similar prior to the earthquake and they have not changed much after the earthquake, indicating that the effect is present only for burglaries.

To estimate the impact of housing damage on burglary, it is important that the treatment and control municipalities had similar characteristics prior to the earthquake. Panels B to D of Table 1 present the pre-earthquake characteristics of the two groups. Panel B compares the occurrences of crime measured by recorded criminal events and arrests, and police resources measured by number of police officers. All measures are expressed per 1,000 population. Panel B indicates that the control group exhibits a slightly higher occurrence of crime, more arrests, and fewer police officers per 1,000 population. However, the differences are not statistically significant. Panel C reports the characteristics related to labor market and the economy. The proportions of those employed and unemployed are similar in the two groups. Although the treatment group displays higher per capita income, the difference across the two groups is not statistically significant. Overall, we find no important differences between the two groups regarding crime, police forces, labor market and the economy.

Turning to demographic characteristics, Panel D indicates that there are no important differences in the sex ratio, the age composition or the proportion of foreigners. Importantly for the purpose of estimation, the treatment municipalities display higher in- and out-migration rates prior to the earthquake. The different migration patterns could bias the results if the patterns were correlated with occurrences of burglary or damage and differed between the two groups. It is possible that, for example, more affluent people, who are likely to hold more valuable possessions that may potentially be burgled, moved out from the treatment group after the earthquake to live in safer areas. To investigate whether there were different migration patterns across the two groups over time, we compare the difference in in-migration rates between the two groups before and after the earthquake. Although the in-migration rate was higher in the treatment group throughout the sample period, the difference in in-migration rates across the two groups was not significantly different before and after the earthquake. Thus, this systematic difference across the two groups is likely to be absorbed by the municipality fixed effects. The same exercise is repeated for the out-migration rate and we obtain the same result.

Endogeneity Concerns

For β_1 in equation (12) to identify the causal effect of housing damage, and therefore of the change in the value of criminal takings, on the number of burglaries, the earthquake shock on housing must be exogenous. The nature of the shock makes this assumption highly plausible. A shock caused by natural disasters, such as an earthquake, is likely to be exogenous to socio-economic variables. In particular, Kobe had never been hit by major earthquakes for over 1,500 years that earthquakes had been recorded in Japan, which made it credible that the city was safe from seismic activity (United Nations Centre for Regional Development, 1995). Private households, businesses and local governments did not expect a large earthquake in this area and were not prepared for the large shock. For instance, in terms of home insurance only 3 percent of property in the Kobe area was covered by earthquake indemnity, as opposed to 16 percent in Tokyo (Edgington, 2010).

However, one might still be concerned that housing damage caused by the earthquake was correlated with some unobservables correlated with the occurrence of burglary. For example, it is plausible that in the wake of the earthquake, more police forces were relocated to the areas with more earthquake damage, which can subsequently affect the occurrence of crime. We account directly for this by controlling for the number of police officers. It could also be the case that residences in affluent areas, which likely contain more valuables to burgle, are more resistant to the earthquake shocks because they are newer or built with earthquake-resistant materials. Looking at Table 1, it indicates that the control group, which recorded no fully damaged house, has lower mean income per capita, thereby not supporting the possibility that the homes located in the affluent areas were more earthquake resistant. Furthermore, to account for possible endogeneity of housing damage, we control for a range of socio-economic controls. Nevertheless, if there are some unobservables correlated with housing damage that are also correlated with burglary, the estimator β_1 in equation

(12) will be biased.

Even though there is strong evidence justifying that the extent of housing damage is exogenous in the simple DiD, there might still be concerns regarding endogeneity. In particular, there might be issues of omitted variable bias, as for instance the damage to buildings might be more predominant in disadvantage high-crime areas. To account for this, among other variables, we also control for per capita income and unemployment rate for each municipality.

Another concern relates to measurement error, as in destroyed areas people might not report every single burglary, as they have already lost a large portion of their assets and losing few more possessions will not influence them much. We account for this possibility of measurement error in crime using not only recorded burglaries but also actual arrests for burglaries. Reverse causality might be a concern too, as burglars might destroy further the already damaged buildings before the authorities measure the damages. In addition to employing various datasets, using different measures of crime and controlling for potential confounding factors, to further address potential endogeneity issues, we also establish a suitable IV strategy.

Instrumental Variables

We address potential endogeneity concerns by instrumenting the interaction of the log of housing damage with the post-earthquake dummy, $Log(Damage_m) \times Post_t$ in equation (12), with the interaction of the log of distance from the epicentre with the post-earthquake dummy, $Log(Epicentre_m) \times Post_t$. For this interaction term to be a valid instrument, we require that $Log(Epicentre_m) \times Post_t$ does not appear in equation (12), is correlated with housing damage variable and is not correlated with any other determinant of burglary other than housing damage. Intuitively, we require that our proposed instrument, the distance from the earthquake epicentre, influences burglaries exclusively through damaging properties and therefore through the devaluation of the potential loot. As the location of the epicentre is random, so does the distance of each municipality from the epicentre. The exclusion restriction is likely to be satisfied as it is hard to consider another channel apart from the extent of housing damages through which the distance from the earthquake epicentre influences burglaries, conditional on various municipality characteristics. In contrast, distance from the epicentre is highly correlated with seismic intensity (see Figure 2), and therefore also with the extent of damage to housing. Figure 4 shows the correlation between the logged distance of each municipality from the earthquake epicentre and the logged number of damaged houses both fully and partly destroyed (see Panels A and B, where larger circles correspond to municipalities with larger population).This relation can be tested using the following equations:

$$Log(Damage_m) \times Post_t = \alpha_2 + \beta_2(Log(Epicentre_m) \times Post_t) + X^{'}_{mt}\chi + \eta_m + \theta_t + u_{mt} \quad (13)$$

$$Log(Crime_{mt}) = \alpha_3 + \beta_3(Log(Epicentre_m) \times Post_t) + X^{'}_{mt}\psi + \iota_m + \kappa_t + \nu_{mt} \quad (14)$$

Equation (13) corresponds to our first-stage equation where $Log(Epicentre_m)$ is the distance from the epicentre to municipality m in kilometres. The individual characteristics, X_{mt}, and the parameter χ are $K \times 1$ vectors, where K is the number of time-varying observables capturing municipality characteristics. η_m and θ_t are municipality fixed effects and year fixed effects, respectively, and u_{mt} is the disturbance term. Equation (14) is the reduced form equation of log crime on the instrument. The IV local average treatment effect (LATE) estimate is then the ratio of the reduced form to the first stage coefficient, β_3/β_2.

The relevance condition is satisfied, as clearly the instrument is correlated with the potentially problematic variable and this is visible in Figure 2, which shows that the seismic intensity is higher closer to the earthquake epicentre and in Figure 4, which shows the correlation between the logged distance of each municipality from the earthquake epicentre and the logged number of damaged houses both fully and partly destroyed (see Panels A and B, where larger circles correspond to municipalities with larger population).[21] Importantly, Figure 5, which is a visual representation of the key relationship in the reduced form equation (14), shows that before the earthquake there is no correlation between changes in burglaries and the distance from the epicentre, while after the earthquake there is a strong and clear correlation, with municipalities closer to the epicentre experiencing the largest decline in burglaries (in Figure 5 compare Panel A to Panel B). The exclusion restriction is also satisfied as it is hard to find another channel apart from the extent of

[21] As we will see later this point is also confirmed by the significant effect in the first-stage regressions of Tables 5 and 6.

housing damages through which the distance from the earthquake epicentre influences burglaries. It is important to note that we control for other variables, such as per capita income, which might influence crime at large. Also, we compare burglaries to other crime types and we find that the effect is present only for burglaries (see Tables 2 and 7). Last but not least, housing damages might decrease the returns for housebreakers by devaluing the loot but they might also make it easier to break into properties. It is hard to account for this but the significant negative effect that we find indicates that the impact on the returns overshadows the ease with which burglars can enter partly destroyed houses after the earthquake. This also means that if something the effect we estimate is downward biased.

5. Results

Descriptive Difference-in-Differences

Table 1 offers descriptive statistics for the treatment and the control group, broken down in Panels A to D, which relate to earthquake damage, crime and police, labor market and the economy, and demographics. In the municipalities of the treatment group the earthquake has generated at least one fully damaged house, while in the nearby municipalities of the control group there was no completely damaged house. The table clearly illustrates that apart from the large differences in earthquake damage (Panel A), the control and treated municipalities are very similar with respect to all other characteristics (see Panels B to D).

Table 2 displays arrests for different crime types and offers an illustrative description of the key variables in the control and treated municipalities, before and after the 1995 Kobe earthquake. This descriptive DiD comparison of means offers some initial descriptive results. Columns (1) to (7) display simple means, notably even for simple differences in means, the largest difference when we compare the treatment to the control group before and after, is for burglaries.

Intuitively, an earthquake that damages the value of houses and the possessions inside them, which is the loot for housebreakers, influences primarily burglaries and not other crime types. This is a key point that appears continuously in our analysis, which follows in the subsequent sections, and it is consistent even when we account for several other factors, such as changes in the level of unemployment or in police forces. This is displayed in columns (8) and (9) of Table 2.[22]

Figure 3 illustrates the drop in both recorded burglaries and arrests for burglaries only in treated municipalities just after the 1995 Kobe earthquake (Panels A and C). It is important to notice the pre-earthquake similar trends in the treatment and control groups. Another key point is that after the first couple of years the trends become very similar again for the treated and the control municipalities. In Panel C the drop in the number of arrests for burglaries drops significantly for two years, whereas in Panel A most of the decline in recorded burglaries occurs in the first year after the earthquake. The most likely explanation for this difference is that records for burglaries happen almost simultaneously with the burglaries, while arrests for burglaries might delay due to the investigations of the police, the functioning of the law enforcement institutions or of the court. These factors can play a role in explaining this lag in Panel C. At the same time total crime (excluding burglary) remains largely unchanged by the earthquake as this is displayed in Panels B and D.

Difference-in-Differences Estimates

After the descriptive DiD, we focus initially on burglaries and we present DiD estimates using variations of the model in equation (12). Table 3 displays the effect of housing damage on *recorded crimes for burglaries*. There are two types of housing damage. In the first three columns the key independent variable is the number of *fully* destroyed houses, interacted with the post-earthquake dummy that takes the value of unity for the post-earthquake years, while in the last three columns we have the number of *partly* destroyed houses interacted with the same dummy. Columns (1), (2), and (3) display DiD regression estimates for the interaction of the post-earthquake dummy with fully destroyed houses without control variables, with socio-economic control variables (e.g., rate of unemployment, police forces, income per capita), and with socio-economic controls plus municipality specific time trends, respectively. Throughout our analysis all regressions control for population and include year fixed effects. The same exercises are repeated using the interaction with

[22] For comparison we include column (9) in Table 2 which reports the IV estimates when we instrument $Log(Damage_m) \times Post_t$ using the distance from the Kobe earthquake epicentre, which is $Log(Epicentre_m) \times Post_t$.

partly destroyed houses in columns (4) to (6). Our preferred specification is displayed in columns (3) and (6) with the full set of control variables. Broadly, the results indicate that an increase in the number of damaged houses (either fully or partly destroyed houses) decreases the number of recorded burglaries and this effect is statistically significant at the 5 or 10 percent significance level.

Corresponding results using *actual arrests for burglaries* as the dependent variable, to account for endogenous reporting, are displayed in Table 4. Table 4 also shows that an increase in the number of damaged houses (either fully or partly destroyed houses) decreases the number of arrests for burglaries and this effect is strong and significant. In fact, throughout columns (1) to (6) all estimated elasticities are statistically significant at any conventional significance level, strengthening the results of Table 3.

Intuitively, the results from Tables 3 and 4 indicate that as housebreakers realize that after the earthquake damages the returns from burglaries are lower, they decide to break into fewer properties. These results do not depend on the log-log specification as they also hold when we use level-level specifications, which offer estimates that are statistically significant at any conventional level (see Table A1 in the Appendix).

Instrumental Variables (IV) Estimates

The results from the simple DiD give us confidence that there is a strong and significant relationship between burglary and the devaluation of prospective takings measured by housing damage.

Even though we include several control variable to account directly for potential confounding factors, such as police forces, unemployment and income per capita, the endogeneity of housing damage might still be a concern. To overcome this concern, we employ an IV strategy, where the distance of each municipality from the earthquake epicentre is used as an instrument for the extent of housing damage. We argue that our proposed instrument, the distance from the earthquake epicentre, influences burglaries exclusively through damaging properties and therefore through devaluing the potential loot.

Figure 2 offers a description of the relevance of our instrument. In fact this map illustrates that municipalities closer to the earthquake epicentre suffered more from the higher seismic intensity.

Figure 4, also illustrates the correlation between the logged distance of each municipality from the earthquake epicentre and the logged number of damaged houses both fully and partly destroyed (see Panels A and B). It also confirms that the closer a municipality to the earthquake epicentre is, the higher the number of damaged houses is. The key relationship of the reduced form equation (14) is illustrated visually in Figure 5, which shows that before the earthquake in 1994 there is no correlation between changes in burglaries and the distance from epicentre, while after the earthquake in 1995 there is a very clear and strong relationship, with the municipalities closer to the epicentre experiencing the largest decline in burglaries, as we argue in this study primarily due to the declining returns for burglars from the earthquake damages. In fact, for recorded burglaries the coefficient changes from an insignificant 0 in 1994 to a significant 0.46 in 1995.

Table 5 offers the IV results for recorded burglaries. As in previous tables, the first four columns correspond to fully destroyed houses, while the last four columns display results using partly destroyed houses as the key independent variable. Column (1) shows estimates for the simple Ordinary Least Squares (OLS) regression. Column (2) displays the reduced form estimate of the effect of the instrument on burglaries, as this is specified in equation (14). Column (3) displays the estimate for the first-stage effect of the instrument on housing damage, as this is shown in equation (13). Lastly, column (4) offers the IV estimate of the effect of housing damage on burglaries. For recorded burglaries Table 5 shows that the OLS, the reduced form, the first stage and the IV estimates are statistically significant at least at the 10 percent significance level, with high F-statistics, for both fully and partly destroyed houses.[23]

Table 6 reports the corresponding results for actual arrests for burglaries, instead of recorded burglaries which are displayed in Table 5. All the estimates obtained in OLS, reduced form, first stage and IV regressions in Table 6 are strong and significant even at the 1 percent significance level for both fully and partly damaged houses. The F-statistics on the excluded instrument from the

[23]The model is just identified, implying that there are concerns regarding bias due to weak correlation or weak instruments. The F-statistics reported in Table 5 and 6 are always larger than 10, indicating that there is no such problem. When we have one endogenous regressor this simple F-statistic is enough. The Cragg-Donald EV statistics are used when we have multiple endogenous regressors. Stock and Yogo (2005) characterize instruments to be weak if the values of the Cragg and Donald (1993) minimum eigenvalue (EV) statistic for which a Wald test at the 5 percent level have an actual rejection rate of no more than 10 percent. We would have used the Cragg-Donald EV statistics, if instead of just one post dummy for every year after 1995, we have used different dummies for each year after 1995, as in that case we would have had more than one endogenous regressors.

first stage regressions are large, implying that our instrument is not weak. Table 6 shows that when we use the distance from the earthquake epicentre as an instrument for the extent of damage, we derive a strong and significant crime-damage or crime-value elasticity of 0.1. Intuitively, an increase in the number of damaged houses by 10 per cent decreases the number of arrests for burglaries by around 1 per cent. Importantly, the estimated IV elasticities have the expected sign and are statistically significant at the 1 percent significance level. Similarly the IV results do not depend on the log-log specification either, as they also hold when we use level-level specifications (see Table A2 in the Appendix).

6. Robustness Checks

The results from the DiD and the IV regressions show a clear connection between housing damage and burglary or to put it differently of value and burglary. Throughout this study we interpret housing damage due to the earthquake as a decline in the value of the property and the interior possessions, which is the potential loot for housebreakers. The analysis so far indicates that there is a clear connection between crime and its returns. Nevertheless, in this section we perform several robustness checks to re-confirm the validity of our main result.

Recorded Crimes vs Actual Crime Arrestss

There are concerns in relation to endogenous reporting, which is the fact that victims report a burglary only when the stolen properties are valuable enough. This might bias our results downwards as several largely damaged houses might get burgled without being recorded.

We tackle this issue using data for both recorded crimes and actual crime arrests, as this concern of endogenous reporting could be valid primarily for the former. However, in Tables 3-6 we find strong and significant results for both recorded crimes and actual crime arrests (for both fully and partly destroyed houses), which make this concern unlikely to threaten our analysis.

Housing Damage, Protection and Susceptibility to Burglary

Another related concern is that the destruction of houses might mechanically reduce the number of burglaries by simply reducing the number of houses and therefore of potential targets for burglars. This is a valid issue for fully destroyed houses.

However, this concern is largely eliminated when we use data for partly destroyed houses, where the targets exist even after the earthquake. For these partly destroyed houses, there is a possibility that burglaries might even increase after the earthquake, despite the decline in the returns for burglars, as the destruction makes such buildings less well protected and therefore more easily susceptible to housebreakers. The negative estimated coefficients in Tables 3-6 indicate that the impact of the declining returns is larger. This implies that either such effects, related to less well-protected properties after an earthquake damage, are nonexistent or they are smaller than the impact of declining returns, and therefore they only bias our estimates downwards.

Instrumental Variables using Placebo Locations

The most convincing results come from the IV approach in Tables 5 and 6, where we instrument the extent of damage with the distance from the Kobe earthquake epicentre. However, there might be concerns that this is not a valid instrument and even the use of distances from other related locations could generate similar results. This could be a threat to our analysis that is why we perform a placebo analysis using locations other than the Kobe earthquake epicentre.

For instance, distances from locations that have nothing to do with earthquakes might generate similar results. That is why we first use placebo distances of each municipality from some of the largest cities in Japan. In particular, we use distance from the following locations: Tokyo, Yokohama, Nagoya, Sapporo and Fukuoka.[24]

Also, one might be concerned that people were aware of the risk of the earthquake. Those who lived closer to the epicentre might have been less risk averse, which could influence how much protection they give to their properties. If this is the case, the exclusion restriction will be violated.

[24]The list with the largest cities in 1995 comes from this source: http://demographia.com/db-jp-city1940.htm. From the list we exclude the very nearby cities to the earthquake epicentre, such as Osaka, Kobe and Kyoto.

As Kobe was considered safe from seismic activity (United Nations Centre for Regional Development, 1995) and the earthquake was largely unanticipated, this concern is unlikely to be serious. Nevertheless, to address this type of concerns, we perform a placebo analysis using distances from epicentres other than that of the Kobe earthquake. If the location of housing and the extent of protection people give to their housing are both affected by their level of risk averseness, the distances from the epicentres of other past earthquakes could also yield the same results. That is why we also use placebo distances from some of the following deadliest earthquakes in Japan, located in: Gifu, Kanagawa, Miyagi, Fukui and Iwate.[25]

Table 7 displays IV results for all these different models. Panel A offers the real effect using the distance from the Kobe earthquake epicentre as an instrument for housing damage and the estimates are always statistically significant. One may be concerned that the main results are driven by a change in the provision of public resources subsequent to the earthquake. For example, government spending increased in the treatment municipalities after the earthquake, and some of this increased spending may have been used to improve the safety of these communities. Burglaries in the treatment municipalities may have decreased because treatment municipalities benefited from such an additional spending. We tackle this identification issue in our robustness exercise. To confirm that the main findings are not driven by omitting this potentially relevant variable, possibly correlated with housing damage, we control for public resources, captured by a dummy variable that equals one if a municipality was located in the areas where disaster relief act was applied, and zero otherwise. The act was applied to areas defined by the government to be damaged and these areas received public support as a relief from the disaster. When in our main model we add a control dummy variable to account for the impact of governmental disaster relief, the estimated crime-value elasticities retain their strong and significant effect, as they are displayed in the second row of Panel A.

Panel B offers the estimates using placebo distances from the largest metropolitan areas as IVs. None of the results is statistically significant. Similarly, Panel C offers the estimates using placebo distances from the epicentres of other deadliest earthquakes as IVs. Again none of the results is

[25] These are the earthquakes with some of the highest death toll that occurred after the 1868 Meiji Restoration which is considered to be the beginning of Modern Japan.

statistically significant at any conventional significance level. This increases further our confidence that the IV we use is valid and makes concerns that question its validity less likely to pose a justifiable threat to our identification strategy.

Burglaries and Other Crime Types

The natural question to ask is whether other crime types, apart from burglaries, are also influenced by the earthquake damage too. This could be a key criticism, as housing damage and burglaries are very closely linked through criminal returns but the same is not true for other crime types. If we find significant effects for other crime types too, beyond and above burglaries, then different potential mechanisms other than reduced criminal returns might be in operation, such as generalized effects that reduce income due to the earthquake damage and influence all crime types.[26]

Also, it is essential to understand whether criminals switch from one crime type to another when new opportunities or threats appear. In our case when burglars realize that the returns from burglaries go down, they might decide to switch to other crime types. This key aspect of substitution effects remains almost completely overlooked, despite being central for understanding criminal behavior.

Panel D of Table 7 offers results for other crime types, such as total crime (excluding burglaries), total serious offences and non-burglary thefts. For both recorded crimes and actual crime arrests none of the estimates is significant (see columns (1)-(4) and (5)-(8), respectively). A comparison of these results from Panel D to the effect on burglaries from Panel A reveals that the damaging earthquake influences only the number of burglaries, as the prospective takings for housebreakers have been devaluated, while other crime types remain unaffected.

Results using Different Sample Periods

Another way to corroborate our results is to narrow our sample from 1990-2000 to shorter time periods. The main argument is that we can test how our results are influenced if we restrict our

[26] Such influences caused by a reduction in income are expected to increase crime at large, rather than reducing it, as we find for burglaries. This is the case as a reduction in legal income may incentivise individuals to increase their illegal income. Notice that in any case we control directly for income per capita in our regressions, which accounts for the potential link between income and crime.

sample to just a few year before and after the 1995 Kobe earthquake.

Table 8 displays these results when we use the full sample of 5, 4 and 3-year windows before and after the earthquake, which corresponds to the time periods 1990-2000, 1991-1999 and 1992-1998, respectively. Panel A illustrates the IV results for recorded crimes, using the logged distance of each municipality from the 1995 Kobe earthquake epicentre as an instrument. This is the same specification as in Table 5. All the estimated burglary-damage elasticities have the expected negative sign and most of them are statistically significant at least at the 10 percent significance level. For recorded crimes the largest magnitudes are observed when we narrow the time period to just 3 years before and after 1995.

Panel B displays the IV estimates for actual crime arrests for burglaries for fully and partly damaged houses, using the same specification as in Table 6. For arrests the coefficients decrease in magnitude when the time period narrows, while we also lose significance by restricting the sample, due to the fewer observations and the smaller number of arrests compared to recorded crimes.

Overall, most of the estimates offer a strong elasticity that ranges from -0.05 to -0.1. This consistently negative elasticity between burglary and damage encapsulates precisely the fact that post-earthquake in affected municipalities the devaluation of potential takings from burglaries discouraged housebreakers, who responded to changes in economic incentives and broke into fewer properties.

Short and Longer Run Effects

Our analysis so far is using one post-earthquake dummy for the entire period between 1995 and 2000. This specification might mask heterogeneous effects for different years. For instance, the immediate years after the 1995 Kobe earthquake might have had a different effect compared to the years at the end of our sample period. In order to examine such short and longer run effects, we modify the estimated equation (12) to the following one:

$$Log(Crime_{mt}) = \alpha_1 + \sum_{k=q}^{q+w} \beta_k (Log(Damage_m) \times Year_k) + X_{mt}^{'}\zeta + M_m + T_t + \epsilon_{mt} \qquad (15)$$

In (15) q stands for the quake year, for the Kobe earthquake $q = 1995$; w stands for the time window, which is the years before and after the shock, for the full time sample 1990-2000 $w = 5$; the rest is similar to estimating equation (12).[27]

Table 9 displays these effects for different years. The IV estimation shows that almost the entire effect occurs in the first post-earthquake year 1995. This is consistent with Becker's (2005) argument that after the earthquake recovery in Kobe was rapid. This elasticity is strong, has the expected negative sign, and for both fully and partly destroyed houses is very similar to the elasticity we derive when we pool all post-earthquake years together (see Table 5). The main results still hold when instead of a log-log specification, we employ a level-level one (see Table A3 in the Appendix). The combination of these facts implies that the change in the value of prospective takings due to the earthquake damage affected burglaries almost simultaneously.

7. Discussion

The main argument of our analysis is that criminals respond to changes in the value of loot. In particular, housebreakers react when the value of their prospective takings drop after a damaging earthquake. We find that criminals commit fewer burglaries after the earthquake in the affected municipalities. We also find that other crime types remain unaffected and there appears to be no substitution effect. Namely, the decrease in burglaries does not lead criminals to commit other types of crime. This is a key result of our analysis, which is however overlooked in the related literature, as it relates to the way criminals respond to changing economic incentives.

We show that a devaluation of criminal prospective takings by 10 percent, decreases burglaries by 1 percent and this result is statistically significant. Our derived elasticity of 0.1 is also significant in size, when we compare it to other key determinants of crime. For instance, for property crimes the elasticity with respect to the police force size is -0.17 (Chalfin and McCrary, 2018), while the elasticity with respect to prison population is -0.2 (Johnson and Raphael, 2012). This means that a 10 percent increase in damaged houses due to the earthquake has an equivalent burglary-reduction

[27]Similarly we modify equations (13), (14) and the corresponding IV structural equation to include multiple year dummies interacted with the extent of earthquake damage and therefore multiple instruments.

effect to a 5 percent increase in prison population or a 6 percent increase in police force size.

Our results show that there is no evidence for generalized effects of the earthquake on crime at large. For instance, a change in overall unemployment or in migration due to the earthquake might influence several different crime types. In our analysis we collect further data and we control directly for various factors, such as the level of unemployment, police forces and income per capita, finding no evidence of such generalized effects. Similarly, if the decline in burglaries is driven primarily by the strengthening of community life post-earthquake, as Hombrados (2018) argues, the crime-reducing effect would be evident also for other crime types but in both his and our study the effect is significant primarily for burglaries. More broadly, a generic "feeling-sorry" sentiment from the side of the criminals would have reduced not only burglaries but also other crime types, such as theft and other serious offences, in the earthquake-affected areas. We have explored this in detail and we have found no evidence for such generic effects. In our several different robustness checks the crime-reducing effect consistently appears only for burglaries.

The fact that other crime types, apart from burglaries, remain unaffected implies that there are no substitution effects either. Intuitively, a reduction in burglaries might shift criminals to other crime types in order to replace the lost income. We find no evidence for such substitution effects in either our DiD or our IV research designs, implying adjustment costs to changes in criminal specialization.

These substitution effects relate to the literature on indirect effects of crime when incentives change. One branch of the economics of crime and criminology literature focuses on *displacement across space*.[28] An earthquake might indeed shift police forces to the areas with the largest damage and this might reduce crime there by increasing crime in the areas that are under-policed. This effect due to changes in policing offers an alternative channel that leads to similar results to our approach, which however does not relate to criminal returns. We tackle this in two ways. First, we control for changes in the level of police forces to account for such policing effects. This means that the estimated elasticities are clean from such biases due to shifts in police forces. Second, we tackle the question of whether criminals relocate to the under-policed areas, those that have not

[28] See, for instance, Di Tella and Schargrodsky (2004) and Draca, Machin, Witt (2011) for displacement effects.

been influenced by the earthquake. In relative terms burglars avoid municipalities with reduced value of prospective takings, those that have been hit by the Kobe earthquake, but they continue to target houses in unaffected municipalities where the value of loot remains relatively high. This is the main result of the DiD and the IV approaches. We can also examine absolute changes and the displacement of crime by comparing burglary rates before and after in the control municipalities. Columns (2) and (5) of Table 2 shows that housebreaks have not increased in control areas, as burglary arrests per 1,000 people have actually decreased slightly even in control municipalities from 0.297 to 0.208.[29] This is evidence that in absolute terms housebreakers have not relocated to nearby areas were the returns to burglary have remained relatively high. A potential explanation for this might be that burglars are different from other criminals when relocating, as they need to have good knowledge of the characteristics of the neighbourhoods and of the particular houses that they wish to burgle. In contrast, for thieves, such as pickpockets for example, it might be relatively easier to relocate to new areas, as the characteristics of the location are not equally crucial.

Apart from displacement across space, there is also *substitution across goods* within one crime type. Draca, Koutmeridis and Machin (2019) offer the first evidence using a wide range of products that criminals respond to price changes and shift to the goods that yield higher returns within a particular crime type. We recalculated some of the key results of this paper and the crime-price elasticity for different burglaries is 0.272, which is significant at the 5 percent significance level. An important observation is that this crime-price elasticity for burglaries that is derived using a completely different setting, research design and dataset, points to the same direction as the burglary-value elasticity we derive in column (4) of Table 5 (both elasticities relate to recorded crimes and our estimated one is negative as it relates to declines in value due to earthquake damages). Returning to substitution effects, consider for example that burglars might move away from DVD players as their prices drop but they might target more jewellery items as the increase in the price of gold boosts their resale value for a burglar. One issue with this approach is that a change in the value of one product shifts criminal behavior towards this particular product for every crime type, such as theft, burglary and robbery, which makes it difficult to examine shifts of crime across crime

[29]Notice that the drop in burglaries in the treated municipalities is much larger, from 0.416 to 0.207.

types, say from burglary to theft. We overcome this issue with this paper as the earthquake is an exogenous shock that influences primarily burglaries.

Our analysis captures different perspectives and it allows us to examine precisely how criminals choose their *specialization across different crime types*. In particular, the 1995 Kobe earthquake decreases primarily the returns to burglary, as the building damages together with the damages in the properties inside these buildings, reduce the prospective takings predominantly for housebreakers. Even though there could be effects for other crime types too, undoubtedly their influence, if such an influence exists, would be of lesser importance.[30] Our IV results in Table 7 clearly indicate that there are strong and significant effects for burglaries but there are no effects for other crime types (compare Panels A and D).[31]

A potential explanation for this finding is the existence of adjustment costs, which do not allow criminals to switch easily their specialization from one crime type, say burglary, to other crime types. There are reasons to believe that for illegal work specialization, the accumulation of specific human capital and learning-by-doing, are crucial as they are for legal work.[32] Even though this new insight appears to be only loosely related to the existing strands in the economics of crime literature, it actually constructively reconciles seemingly conflicting findings and offers a unified approach of criminal "displacement" across space, goods and specializations in response to changing economic incentives.

Importantly, even though it is relatively easy for a criminal to perform the same theft in a different location (displacement across space) or to decide which goods to target within a crime type (substitution across goods), there are frictional adjustment costs that do not allow criminals to effortlessly switch their expertise from one activity to another (specialization across different crime types). For instance, it is complicated for a burglar to switch to a robber, as the criminal has to sacrifice a set of skills he already possesses, such as the ability to unlock doors, while at the same time he has to acquire a new set of skills, such as the ability to use weapons. This example indicates

[30] For instance, individuals might carry some damaged products outdoors too and this might decrease the returns for thefts too. Nevertheless, post earthquake the potential takings for burglars drop much more compared to other criminals, such as thieves, as the devaluation of damaged products inside houses should be much larger compared to the decline in the value of the goods that citizens carry with them on the streets.

[31] Recall that for both recorded crimes and for actual crime arrests in Table 7, only burglaries are influenced, while all other crime types remain utterly unaffected.

[32] For instance, see Lochner (2004) and Bayer, Hjalmarsson and Pozen (2009).

that such changes in expertise are costly, even within similar property crime types (e.g., theft, burglary, robbery), let alone shifts across dissimilar crimes (e.g., switches from fraud to smuggling or from bribery to arson). Such adjustment costs make changes to criminal specializations infrequent and less responsive to changes in economic incentives, compared to changes in locations or targeted goods. The equivalent in the labor market could be the inability of, say, a construction worker to switch to a nurse, when relatively better opportunities appear for a career in the nursing occupation.

This study examines theoretically and offers the first piece of evidence of such adjustment costs in criminal specialization that do not allow criminals to substitute out burglaries, when the associated returns drop, with other crime types, for which the returns remain largely unaffected. The natural experimental research design makes this argument even more convincing as we compare differences between treated and control areas, where it is unlikely changes in common factors to influence our estimates. For instance, a decline in the prices of indoor electric devices, such as DVD and audio players, would have discouraged burglars in both the control and the treated municipalities. Our DiD design allows us to account for such common trends and derive estimates that are clean from such biases. Nevertheless, the context of our approach is specific and future studies in different settings are required to fully understand how incentives interact with criminal returns and specialization in an attempt to build informed evidence-based crime-reduction interventions.

8. Conclusion

This study offers the first comprehensive natural experimental evidence that reveals the response of criminals to changing economic value, a key but understudied determinant of illegal behavior. It also reveals for the first time the non-response of criminals to substituting one crime type, and particular burglary, with other crime types, indicating the existence of adjustment costs to changes in criminal specialization.

We exploit a historically unique source of exogenous variation, the unanticipated 1995 Kobe earthquake, which influenced several Japanese municipalities with thousands of deaths and building damages, while it left others utterly unaffected, to show the response of housebreakers to damages that reduce the value of their prospective takings. Importantly, other crime types remain unchanged, eliminating the possibility of generalized effects that influence crime at large or of substitutions across different crime types. We perform several robustness checks. First, we use both fully and partly destroyed houses, as the destruction of houses may decrease burglaries by simply reducing the number of houses, and therefore of potential targets. Second, we control for endogenous reporting, the fact that people might report a burglary only when the stolen value is large enough, by using both reported crimes and actual arrests. Third, we tackle further endogeneity concerns instrumenting the extent of housing damage with the distance from the earthquake epicentre, which influences burglaries exclusively through devaluing the potential loot. Fourth, we also perform placebo analysis for our IV regressions using distances from other key locations, such as from some of the biggest metropolitan areas or from some of the deadliest earthquakes in Japan. Importantly, our estimated elasticity is robust and points to the same direction as burglary-value elasticities derived in the non-experimental related literature.

Overall, this study contributes significantly to the related literature as it is the first to examine, in a natural experimental setting, the connection between crime, economic value and substitution effects across different crime types. Despite the importance of this study, future research is required to shed more light to these fundamental but largely underexplored facets of criminal behavior.

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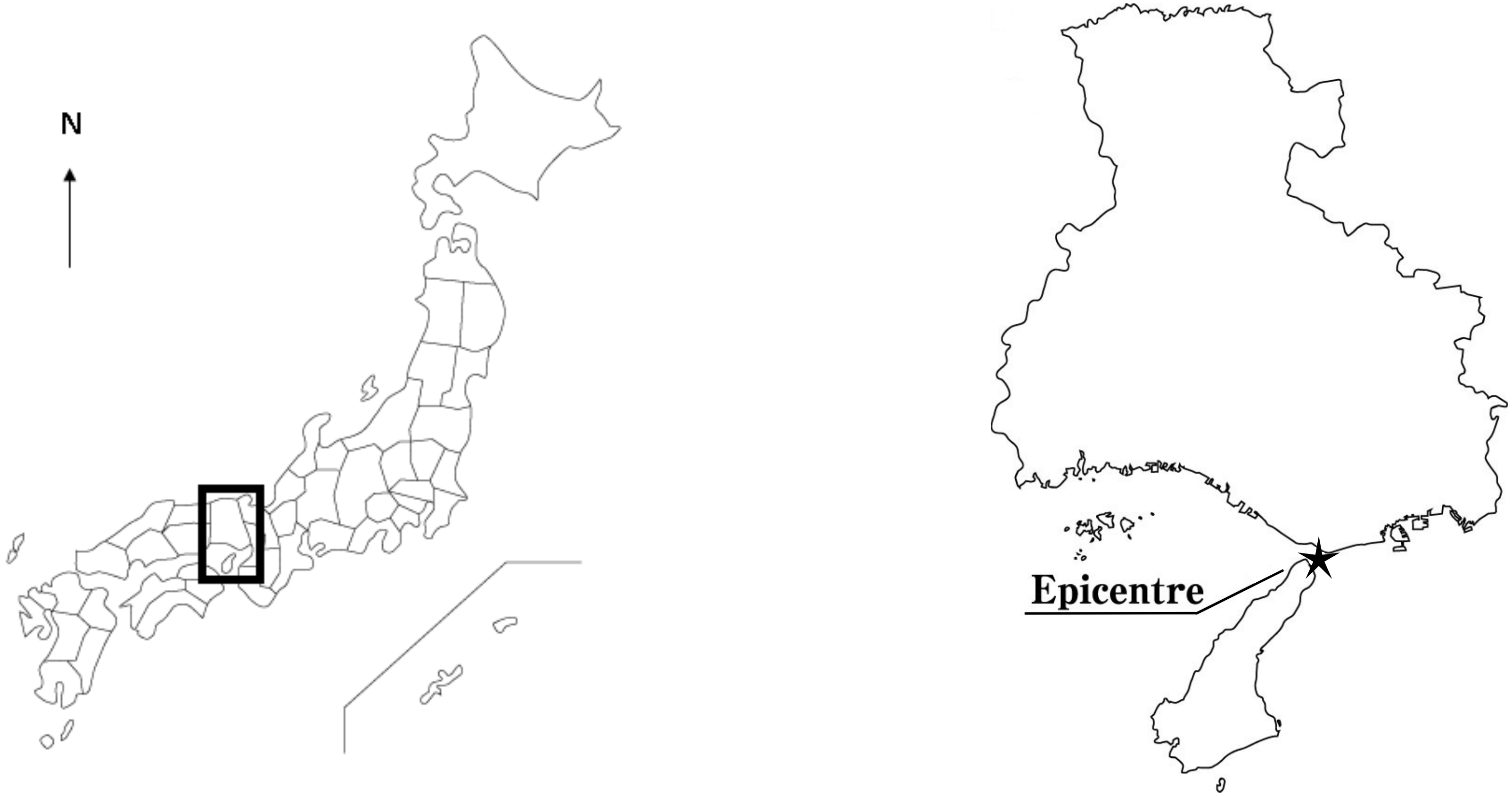
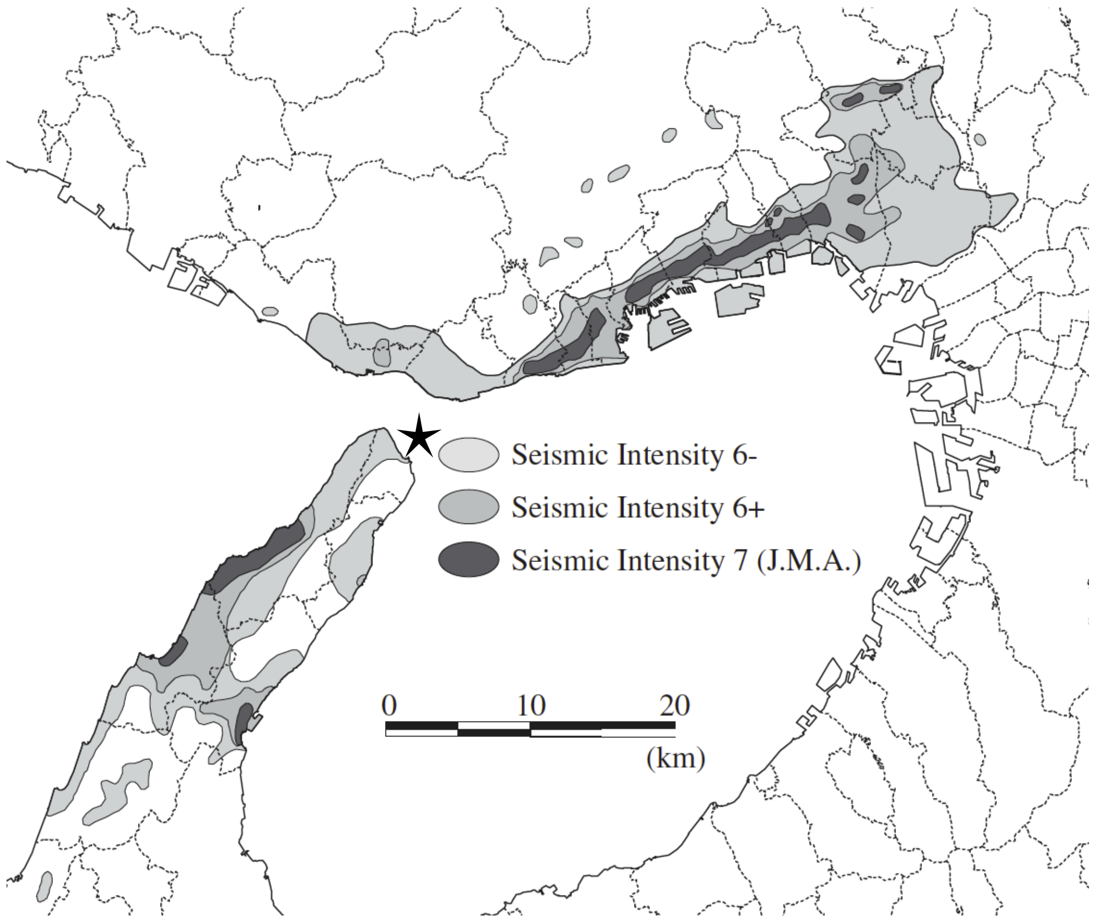


Panel A

Panel B

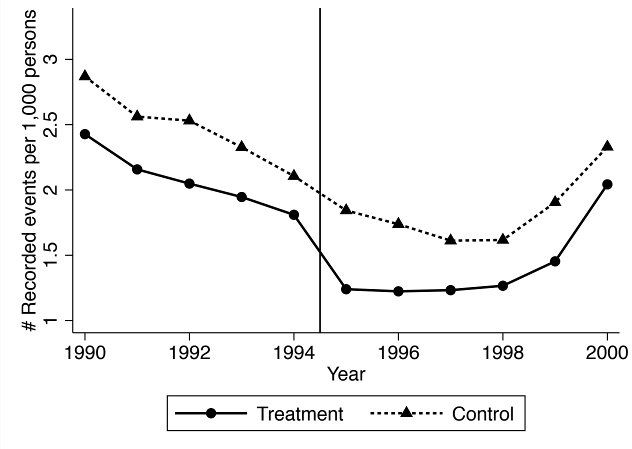
Notes: The square in panel A indicates the location of Hyogo Prefecture in Japan. Panel B focuses on the Hyogo Prefecture and the star shows the location of the earthquake epicentre.

Figure 1: Hyogo Prefecture and the Earthquake Epicentre.

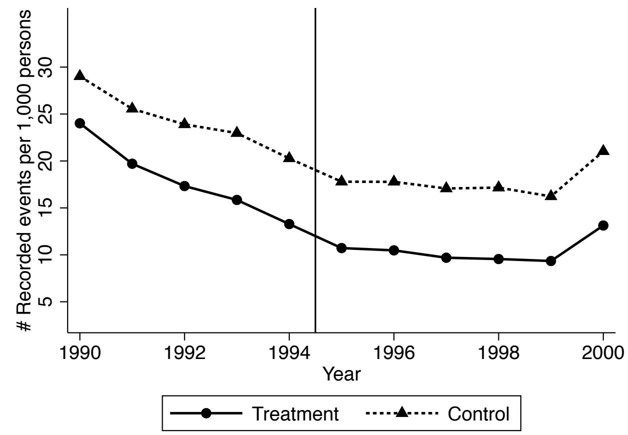


Notes: The figure is obtained from Fujimoto and Midorikawa (2002) and edited by the authors. The star indicates the earthquake epicentre.

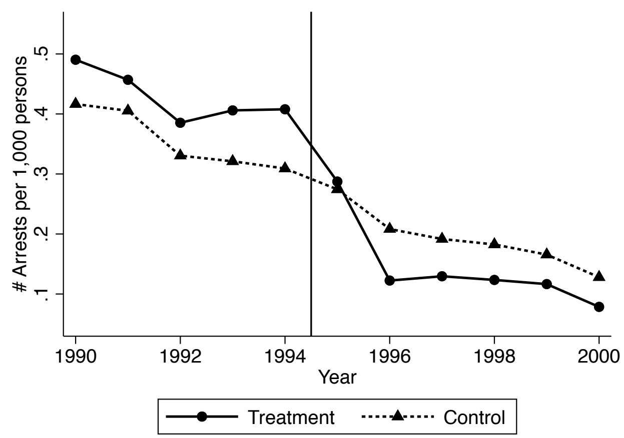
Figure 2: Seismic Intensity measured in the Japan Meteorological Agency (JMA) Scale.



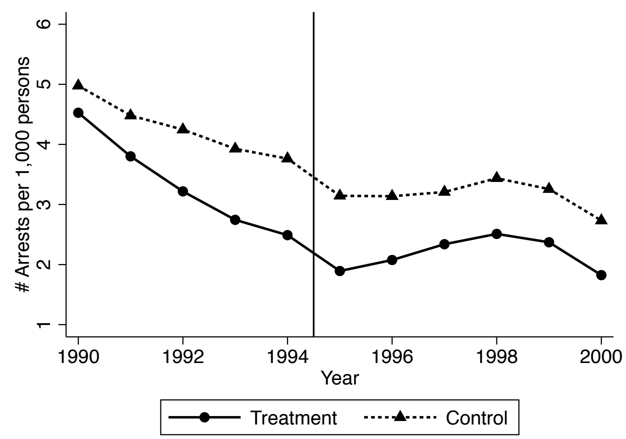
Panel A: Recorded Events for Burglary



Panel B: Recorded Events for Total Crime (excluding Burglary)



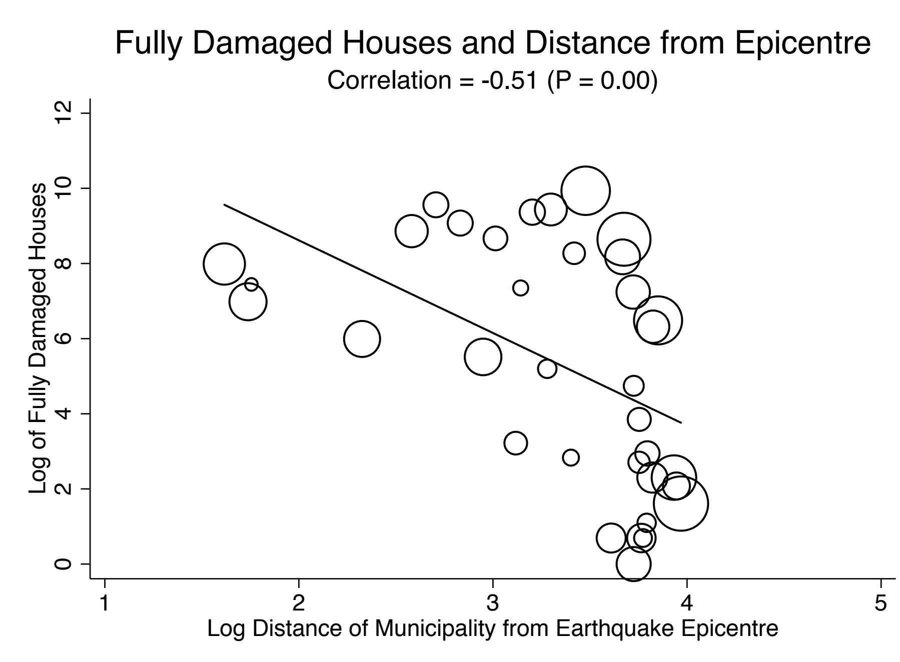
Panel C: Actual Arrests for Burglary



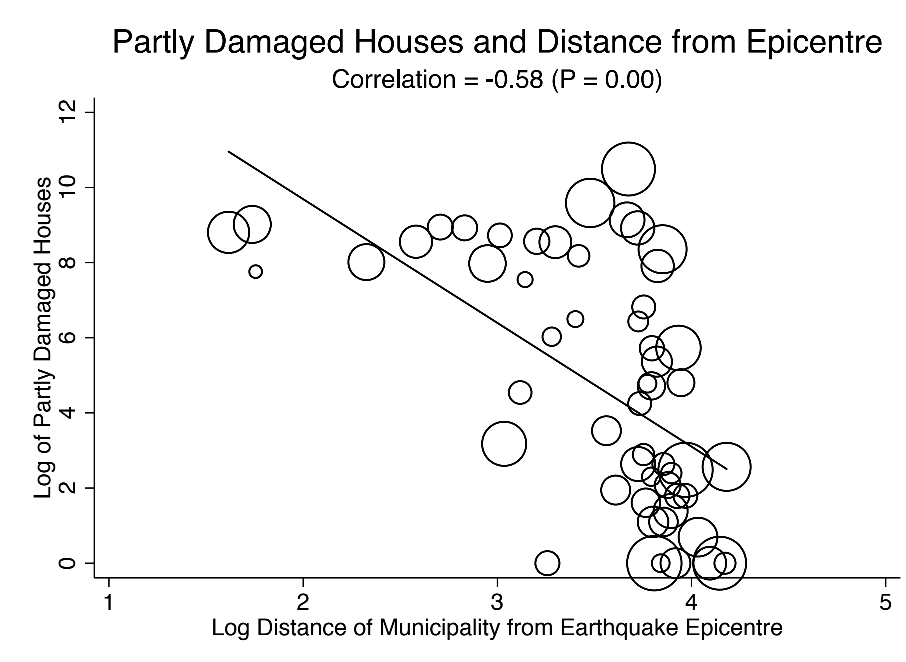
Panel D: Actual Arrests for Total Crime (excluding Burglary)

Notes: Panels A-D present the average numbers of recorded events and arrests for quake affected (treatment) and unaffected (control) areas weighted by population.

Figure 3: Difference-in-Differences with simple weighted averages.



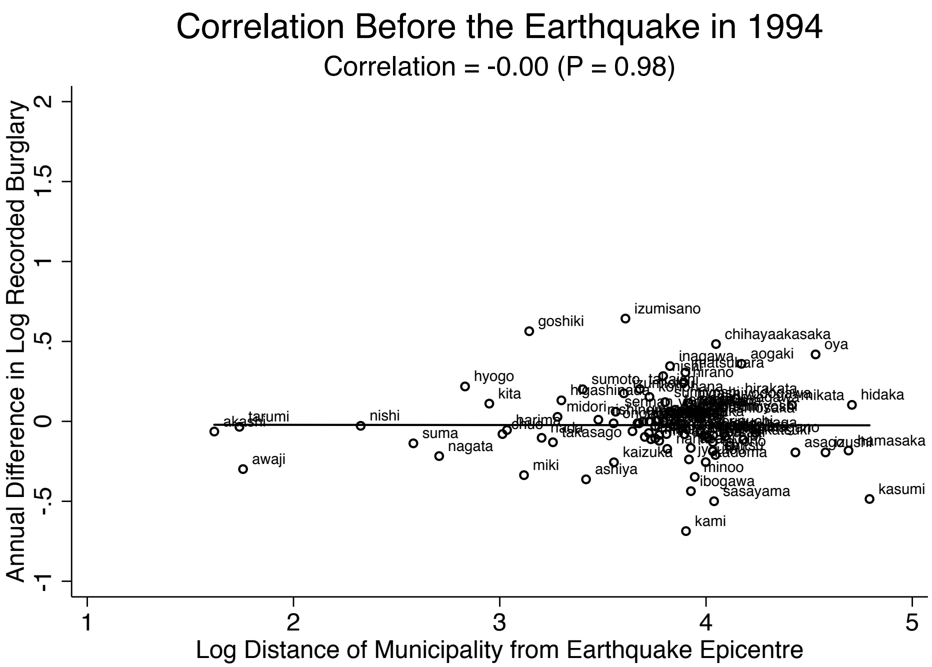
Panel A: Fully Destroyed Houses



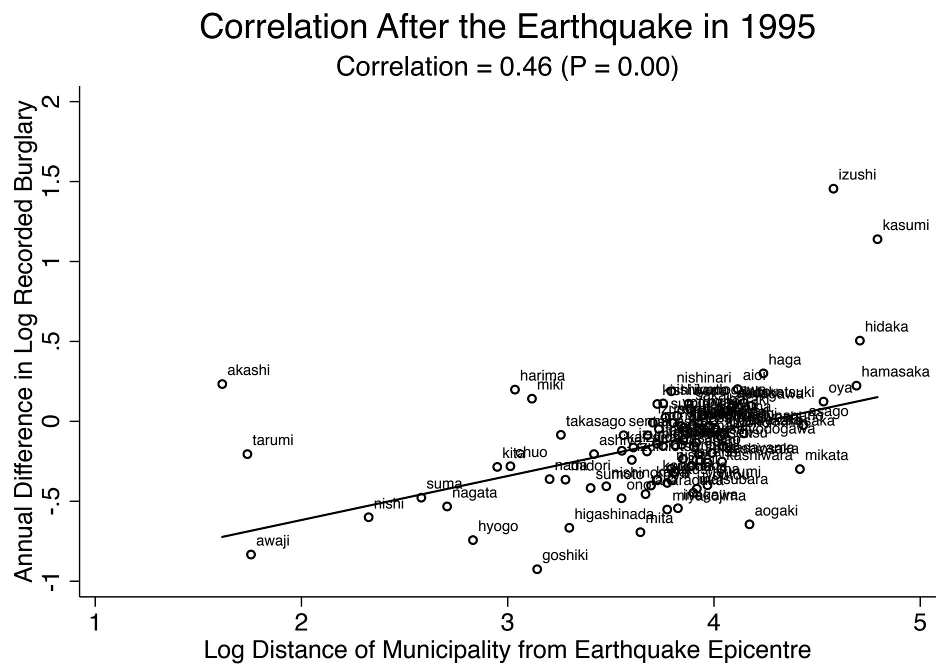
Panel B: Partly Destroyed Houses

Notes: Panels A and B present the correlation between the log of the number of fully and partly destroyed houses to the logged distance of each municipality from the earthquake epicentre in kilometres. Bigger circles display municipalities with larger population.

Figure 4: Correlation between House Damage and Municipality Distance from Earthquake Epicentre.



Panel A: Recorded Events for Burglaries in 1994



Panel B: Recorded Events for Burglaries in 1995

Notes: Panels A and B present correlations between the annual differences in logged burglary against the logged distance of each municipality from the earthquake epicenter in kilometres.

Figure 5: Correlations between Changes in Burglary and Distance from Epicentre Before and After the Earthquake.

Table 1: Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)
	Control 59 Municipalities		Treatment 35 Municipalities		
	Mean	S.D.	Mean	S.D.	P-Value
Panel A: Earthquake Damage (year=1995)					
% Fully-Destroyed houses	0.00	0.00	1.78	2.96	0.00
% Semi-Destroyed houses	0.00	0.01	2.44	3.90	0.00
% Deaths	0.00	0.00	0.11	0.22	0.00
% Injured	0.01	0.02	0.73	1.05	0.00
Panel B: Crime and Police (year<1995)					
Criminal Events per 1,000 persons	18.75	20.01	16.94	15.37	0.31
Criminal Arrests per 1,000 persons	3.06	3.14	2.84	2.24	0.41
Police Officers per 1,000 persons	1.52	1.32	1.53	1.12	0.98
Panel C: Labor and Economy (year<1995)					
% Employed	95.99	94.92	96.25	71.92	0.99
% Unemployed	4.01	3.89	3.75	2.77	0.73
Income per capita (in ¥1,000)	1,446.59	1,589.05	1,654.22	1,260.45	0.14
Panel D: Demography (year<1995)					
Sex Ratio (Males per 100 Females)	96.96	99.68	95.90	69.43	0.90
% Aged under 15	18.38	22.97	17.64	11.64	0.86
% Aged 15 to 64	69.88	82.41	71.42	48.19	0.92
% Aged 65 over	11.74	13.16	10.94	6.36	0.74
% Foreign	1.20	2.39	1.66	2.09	0.34
In-Migration per 1,000 persons	48.97	44.66	56.41	40.17	0.07
Out-Migration per 1,000 persons	51.68	50.25	61.15	46.22	0.04

Notes: This table displays, means, standard deviations (s.d.) and p-values. Mean refers to weighted means where weights are population, except for % Employed and % Unemployed for which labor-force population is used. Figures in panel A are damage rates due to the Kobe earthquake. Figures in panels B to D are the average over the pre-earthquake period (i.e., year<1995) except for the variables available only in census years for which the averages in 1990 are reported. The p-value corresponds to p-values of the test under the null hypothesis of the equality of means between the treatment and control groups. The treatment group is defined as municipalities with at least one fully destroyed house, while the control municipalities have no fully destroyed house. Per capita taxable income is measured in ¥1,000.

Table 2: Descriptive Difference-in-Differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Treatment Before, 35mun., 1990-94	Control Before, 59mun., 1990-94	Difference Treat-Ctrl Before	Treatment After, 35mun., 1995-2000	Control After, 59mun., 1995-2000	Difference Treat-Ctrl After	DiD Means Only	DiD with Controls	DiD IV with Controls
Total Crime Arrests per 1,000	3.678	3.248	0.430	3.929	3.583	0.346	-0.084	-0.006	0.004
								(0.012)	(0.026)
Serious Offence Arrests per 1,000	0.053	0.052	0.001	0.073	0.079	-0.006	-0.007	-0.007	0.014
								(0.030)	(0.053)
Total Theft Arrests per 1,000	2.026	1.643	0.383	1.979	1.663	0.316	-0.067	-0.017	-0.018
								(0.012)	(0.021)
Burglary Arrests per 1,000	0.416	0.297	0.119	0.207	0.208	-0.001	-0.120	-0.082***	-0.113***
								(0.022)	(0.037)
Non-Burglary Arrests per 1,000	1.610	1.347	0.264	1.771	1.455	0.317	0.053	0.001	0.005
								(0.015)	(0.034)

Notes: The table displays simple means in columns (1)-(7). Column (8) displays Difference-in-Differences estimates with standard errors in parentheses, as in Table 4 below. Similarly, column (9) displays IV estimates, with standard errors in parentheses, as in Table 6 below. Standard errors are clustered by municipality code. Statistical significance at the 10, 5 and 1 percent level is displayed by *, ** and ***, respectively.

Table 3: Recorded Crimes - Difference-in-Differences Estimates of the Effect of Housing Damage on Burglary, 1990-2000

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Burglary)
Difference-in-Differences Log(fully damaged houses)*post	-0.014**	-0.016**	-0.031**			
	(0.006)	(0.007)	(0.014)			
Difference-in-Differences Log(partly damaged houses)*post				-0.009	-0.010*	-0.024*
				(0.006)	(0.006)	(0.012)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls (6)	No	Yes	Yes	No	Yes	Yes
Municipality Time Trends (93)	No	No	Yes	No	No	Yes
Observations	1,034	1,034	1,034	1,034	1,034	1,034

Notes: The table displays difference-in-differences results for the period 1990-2000, where the dependent variable is recorded Log(Burglary), the key regressor is the Log(Damaged Houses), either fully damaged or partly damaged, interacted with a dummy variable that takes value 1 after the earthquake year 1995, otherwise 0. All regressions also control for the log of population. Other controls include the logged income per capita, logged number of police officers, logged number of foreigners, sex ratio, unemployment rate and share of population under 15. Standard errors are clustered at the municipality level and are displayed in parenthesis. Statistical significance at the 10, 5 and 1 percent level is displayed by *, ** and ***, respectively.

Table 4: Actual Crime Arrests - Difference-in-Differences Estimates of the Effect of Housing Damage on Burglary, 1990-2000

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Burglary)
Difference-in-Differences Log(fully damaged houses)*post	-0.088***	-0.090***	-0.082***			
	(0.011)	(0.012)	(0.022)			
Difference-in-Differences Log(partly damaged houses)*post				-0.075***	-0.078***	-0.065***
				(0.012)	(0.012)	(0.020)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls (6)	No	Yes	Yes	No	Yes	Yes
Municipality Time Trends (93)	No	No	Yes	No	No	Yes
Observations	1,030	1,030	1,030	1,030	1,030	1,030

Notes: Dependent variable is the log of the number of actual arrested people for burglaries. The rest as in Table 3.

Table 5: Recorded Crimes - Instrumental Variables Estimates of the Effect of Housing Damage on Burglary, 1990-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	Reduced Form	1st Stage	IV structure	OLS	Reduced Form	1st Stage	IV structure
	Log(Burglary)	Log(Burglary)	Log(Destroyed Houses)	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Destroyed Houses)	Log(Burglary)
Difference-in-Differences								
Log(fully destroyed houses)*post	-0.031**			-0.083**				
	(0.014)			(0.036)				
Log Distance from Epicentre		0.228*	-2.768***					
		(0.116)	(0.354)					
F-Statistic			60.84					
Difference-in-Differences								
Log(partly destroyed houses)*post					-0.024*			-0.072**
					(0.012)			(0.031)
Log Distance from Epicentre						0.228*	-3.156***	
						(0.116)	(0.353)	
F-Statistic							79.57	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trends (93)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,034	1,034	1,034	1,034	1,034	1,034	1,034	1,034

Notes: The table displays the results from 2SLS, where the dependent variable is the log of recorded burglaries, the endogenous regressor is the log of damaged houses (either fully destroyed or partly destroyed) interacted with a dummy variable that takes value 1 after the earthquake year 1995, otherwise 0, and the instrumental variable used is the log distance of each municipality from the earthquake epicentre measured in kilometres. The rest as in Table 3.

Table 6: Actual Crime Arrests - Instrumental Variables Estimates of the Effect of Housing Damage on Burglary, 1990-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	Reduced Form	1st Stage	IV structure	OLS	Reduced Form	1st Stage	IV structure
	Log(Burglary)	Log(Burglary)	Log(Destroyed Houses)	Log(Burglary)	Log(Burglary)	Log(Burglary)	Log(Destroyed Houses)	Log(Burglary)
Difference-in-Differences								
Log(fully destroyed houses)*post	-0.082***			-0.113***				
	(0.022)			(0.037)				
Log Distance from Epicentre		0.313***	-2.768***					
		(0.118)	(0.354)					
F-Statistic			60.84					
Difference-in-Differences								
Log(partly destroyed houses)*post					-0.065***			-0.099***
					(0.020)			(0.031)
Log Distance from Epicentre						0.313***	-3.156***	
						(0.118)	(0.353)	
F-Statistic							79.57	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trends (93)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,034	1,034	1,034	1,034	1,034	1,034	1,034	1,034

Notes: The table displays the results from 2SLS, where the dependent variable is the log of actual crime arrests for burglaries. The rest as in Table 5.

Table 7: The Effect of Housing Damage on Burglaries and Other Crime Types
Comparisons of Difference-in-Differences Estimates using Instrumental Variables between Real and Placebo Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Recorded Crimes				Actual Crime Arrests			
	Fully Destroyed Houses		Partly Destroyed Houses		Fully Destroyed Houses		Partly Destroyed Houses	
	IV Estimate	Standard Error	IV Estimate	Standard Error	IV Estimate	Standard Error	IV Estimate	Standard Error
Panel A: Real IV Effect for Burglaries								
Real IV = Log Distance from Epicentre	-0.083**	(0.036)	-0.072**	(0.031)	-0.113***	(0.037)	-0.099***	(0.031)
Real IV + Earthquake Relief Dummy	-0.077**	(0.036)	-0.069**	(0.031)	-0.117***	(0.037)	-0.106***	(0.032)
Panel B: Placebo Largest Cities in 1995 (in 1,000)								
IV = Log Distance from Tokyo (7,968)	0.921	(1.588)	-0.968	(2.048)	-0.251	(0.605)	0.263	(0.983)
IV = Log Distance from Yokohama (3,307)	1.141	(2.260)	-0.810	(1.345)	-0.268	(0.730)	0.191	(0.687)
IV = Log Distance from Nagoya (2,152)	1.331	(2.959)	-0.737	(1.082)	-0.293	(0.831)	0.162	(0.564)
IV = Log Distance from Sapporo (1,757)	-0.146	(0.122)	-0.113	(0.093)	-0.163	(0.126)	-0.126	(0.092)
IV = Log Distance from Fukuoka (1,285)	0.500	(0.648)	-3.432	(34.088)	-0.220	(0.400)	1.508	(16.409)
Panel C: Placebo Deadliest Earthquakes (year)								
IV = Log Distance from Gifu (1858)	1.050	(2.113)	-0.837	(1.594)	-0.274	(0.706)	0.219	(0.784)
IV = Log Distance from Kanagawa (1923)	1.137	(2.248)	-0.811	(1.352)	-0.268	(0.727)	0.191	(0.690)
IV = Log Distance from Miyagi (1936)	0.141	(0.170)	0.208	(0.370)	-0.187	(0.207)	-0.276	(0.372)
IV = Log Distance from Fukui (1948)	-0.024	(0.157)	-0.024	(0.156)	-0.173	(0.224)	0.173	(0.230)
IV = Log Distance from Iwate (2008)	0.005	(0.112)	0.005	(0.116)	-0.176	(0.155)	-0.182	(0.161)
Panel D: Placebo Crime Types with Real IV								
Dep. Var. = Total Crime (without Burglary)	-0.029	(0.018)	-0.026	(0.016)	0.015	(0.030)	0.013	(0.027)
Dep. Var. = Total Serious Offenses	-0.010	(0.052)	-0.009	(0.045)	0.014	(0.053)	0.012	(0.046)
Dep. Var. = Total Theft (without Burglary)	-0.024	(0.023)	-0.021	(0.020)	0.005	(0.034)	0.004	(0.030)

Notes: The table displays the results from 2SLS, where the dependent variable is Log(Burglary) if not specified differently in Panel D, the endogenous regressor is the log of the number of damaged houses (fully destroyed or partly destroyed) interacted with a dummy variable that takes value 1 after the earthquake year 1995 otherwise 0. The instrumental variable used is specified in Panels A-D. All regressions include year effects, municipality effects, municipality specific time trends and other controls (income per capita, police officers, foreigners, sex ratio, unemployment rate and share of population under 15). The specification is as in Tables 5 and 6 but this table displays only the IV results.

Table 8: Instrumental Variables Estimates of the Effect of Housing Damage on Burglary for Various Sample Periods

	(1)	(2)	(3)	(4)	(5)	(6)
		Log(Burglary)			Log(Burglary)	
Sample Time Periods	1990–2000	1991–1999	1992–1998	1990–2000	1991–1999	1992–1998
Panel A: Recorded Crimes						
Log(fully destroyed houses)*post	-0.083**	-0.067	-0.095*			
	(0.036)	(0.042)	(0.054)			
Log(partly destroyed houses)*post				-0.072***	-0.058***	-0.080**
				(0.031)	(0.036)	(0.044)
Panel B: Actual Crime Arrests						
Log(fully destroyed houses)*post	-0.113***	-0.091*	-0.059			
	(0.037)	(0.049)	(0.068)			
Log(partly destroyed houses)*post				-0.099***	-0.078*	-0.049
				(0.031)	(0.041)	(0.055)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls (6)	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trends (93)	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,034	846	658	1,034	846	658

Notes: The table displays Instrumental Variables estimates for various sample periods. The dependent variable is the logged number of burglaries and the IV is the logged distance of each municipality from the 1995 Kobe earthquake epicentre. The rest as in Tables 5 and 6.

Table 9: The Effects of Housing Damage on Burglary by Year

Dependent Variable: Log(Burglary)	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
	Log(Burglary)		Log(Burglary)	
Log(fully destroyed houses)*1995	-0.043***	-0.123**		
	(0.012)	(0.051)		
Log(fully destroyed houses)*1996	-0.032**	-0.072*		
	(0.015)	(0.042)		
Log(fully destroyed houses)*1997	-0.018	-0.018		
	(0.020)	(0.035)		
Log(fully destroyed houses)*1998	-0.018	-0.009		
	(0.020)	(0.027)		
Log(fully destroyed houses)*1999	-0.041***	-0.042		
	(0.015)	(0.028)		
Log(fully destroyed houses)*2000	-0.025	0.013		
	(0.023)	(0.035)		
Log(partly destroyed houses)*1995			-0.034***	-0.107**
			(0.013)	(0.042)
Log(partly destroyed houses)*1996			-0.022	-0.060*
			(0.014)	(0.036)
Log(partly destroyed houses)*1997			-0.010	-0.013
			(0.017)	(0.032)
Log(partly destroyed houses)*1998			-0.010	-0.006
			(0.017)	(0.025)
Log(partly destroyed houses)*1999			-0.030*	-0.039
			(0.016)	(0.026)
Log(partly destroyed houses)*2000			-0.013	0.012
			(0.020)	(0.033)
Fixed Effects, Controls & Municipality Trends	Yes	Yes	Yes	Yes
Observations	1034	1034	1034	1034

Notes: This table displays the effect of the earthquake damage on recorded burglary for each post-earthquake year from 1995 to 2000. The rest as in Tables 3 and 5.

APPENDIX FOR ONLINE PUBLICATION

Table A1: Recorded Crimes (Level-Level Specification) – DiD Estimates of the Effect of Housing Damage on Burglary, 1990-2000

	(1)	(2)	(3)	(4)	(5)	(6)
	Burglary	Burglary	Burglary	Burglary	Burglary	Burglary
Difference-in-Differences						
Fully damaged houses*post	-0.006***	-0.008***	-0.010***			
	(0.002)	(0.002)	(0.003)			
Difference-in-Differences						
Partly damaged houses*post				-0.004***	-0.005***	-0.007***
				(0.001)	(0.002)	(0.002)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls (6)	No	Yes	Yes	No	Yes	Yes
Municipality Time Trends (93)	No	No	Yes	No	No	Yes
Observations	1034	1034	1034	1034	1034	1034

Notes: The table displays difference-in-differences results for the period 1990-2000, where the dependent variable is the count of recorded Burglary, the key regressor is the count of Damaged Houses, either fully damaged or partly damaged, interacted with a dummy variable that takes value 1 after the earthquake year 1995 otherwise 0. All regressions also control for the log of population. Other controls include the logged income per capita, logged number of police officers, logged number of foreigners, sex ratio, unemployment rate and share of population under 15. Standard errors are clustered at the municipality level and are displayed in parenthesis. Statistical significance at the 10, 5 and 1 percent level is displayed by *, ** and ***, respectively.

Table A2: Recorded Crimes (Level-Level Specification) - IV Estimates of the Effect of Housing Damage on Burglary, 1990-2000

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	Reduced Form	1st Stage	IV structure	OLS	Reduced Form	1st Stage	IV structure
	Burglary	Burglary	Destroyed Houses	Burglary	Burglary	Burglary	Destroyed Houses	Burglary
Difference-in-Differences								
Fully destroyed houses*post	-0.010***			-0.024**				
	(0.003)			(0.011)				
Log Distance from Epicentre		0.815**	-34.584***					
		(0.362)	(11.840)					
F-Statistic			8.29					
Difference-in-Differences								
Partly destroyed houses*post					-0.007***			-0.020**
					(0.002)			(0.009)
Log Distance from Epicentre						0.815**	-40.248***	
						(0.362)	(10.991)	
F-Statistic							12.67	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls (6)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Time Trends (93)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1034	1034	1034	1034	1034	1034	1034	1034

Notes: The table displays the results from 2SLS, where the dependent variable is the count of recorded burglaries, the endogenous regressor is the count of damaged houses (either fully destroyed or partly destroyed) interacted with a dummy variable that takes value 1 after the earthquake year 1995 otherwise 0, and the instrumental variable used is the log distance of each municipality from the earthquake epicentre measured in kilometres. All regressions include year effects, municipality effects, municipality specific time trends and other controls (income per capita, police officers, foreigners, sex ratio, unemployment rate and share of population under 15). The rest as in Table A1.

Table A3: The Effects of Housing Damage on Recorded Burglary by Year in Level-Level Specification

Dependent Variable: Burglary	(1)	(2)	(3)	(4)
	OLS	IV	OLS	IV
	Burglary		Burglary	
Fully destroyed houses*1995	-0.012***	-0.032**		
	(0.003)	(0.013)		
Fully destroyed houses*1996	-0.010***	-0.021**		
	(0.003)	(0.010)		
Fully destroyed houses*1997	-0.010**	-0.012		
	(0.004)	(0.011)		
Fully destroyed houses*1998	-0.012**	-0.013		
	(0.005)	(0.010)		
Fully destroyed houses*1999	-0.014**	-0.010		
	(0.006)	(0.010)		
Fully destroyed houses*2000	-0.015*	0.004		
	(0.008)	(0.016)		
Partly destroyed houses*1995			-0.008**	-0.026**
			(0.004)	(0.011)
Partly destroyed houses*1996			-0.005	-0.017*
			(0.004)	(0.009)
Partly destroyed houses*1997			-0.007*	-0.009
			(0.004)	(0.009)
Partly destroyed houses*1998			-0.006	-0.011
			(0.005)	(0.008)
Partly destroyed houses*1999			-0.008	-0.009
			(0.005)	(0.009)
Partly destroyed houses*2000			-0.004	0.004
			(0.007)	(0.014)
Fixed Effects, Controls & Municipality Trends	Yes	Yes	Yes	Yes
Observations	1034	1034	1034	1034

Notes: This table displays the effect of the earthquake damage on burglary for each post-earthquake year for the period 1995-2000. The rest as in Tables A1 and A2.