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ABSTRACT

More Schooling, Less Youth Crime? Learning from an Earthquake in Japan*

This paper aims to identify the causal effect of schooling on youth crime. To identify the causal effect, I use the policy interventions that occurred after the Kobe earthquake that hit Japan in 1995 as a natural experiment inducing exogenous variation in schooling. Based on a comparison of the arrest rates between municipalities exposed to similar degrees of earthquake damage but with and without the policy interventions, I find that a higher high school participation rate reduces juvenile arrest rates for violent crime but not for property crime. The estimates of social benefits show that it is less expensive to reach a target level of social benefits by improving schooling than by strengthening the police force.

JEL Classification: H52, I28, K42

Keywords: schooling, youth crime, social externality

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1. Introduction

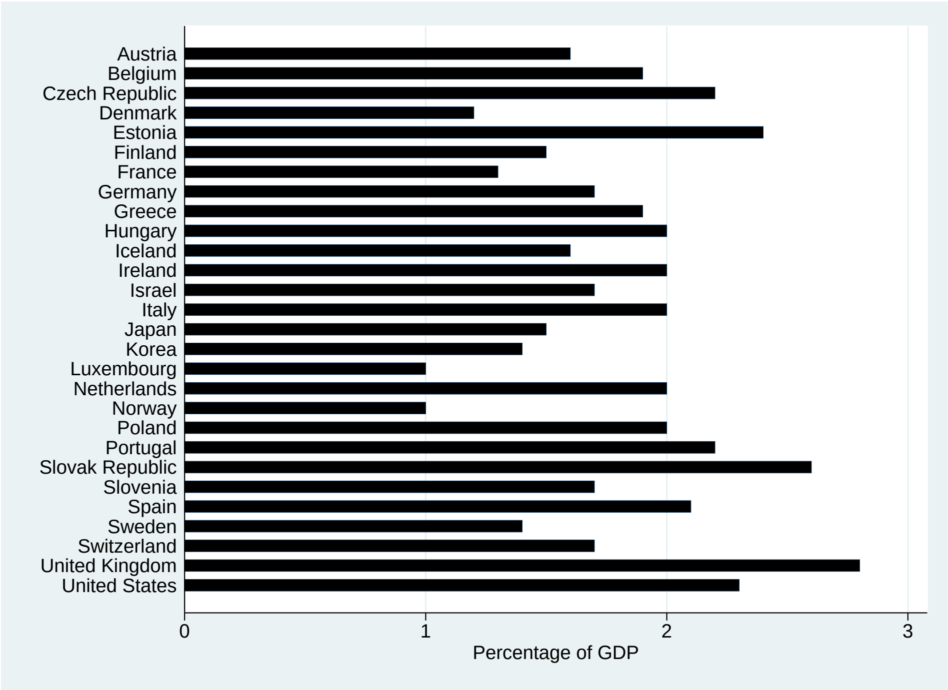
The welfare of individuals has been traditionally measured by indicators of wealth such as income. During recent decades, however, non-monetary indicators of welfare, such as health and happiness, have started to receive more attention (Organisation for Economic Cooperation and Development (OECD), 2010). Among various non-monetary outcomes, this paper focuses on the effect of education on crime. Crime not only causes physical damage or monetary losses to victims but also compels governments to expend large amounts of money on law enforcement. Figure 1 shows that the spending on public order and safety corresponds to two per cent or more of gross domestic product (GDP) in numerous OECD countries including the United Kingdom (2.8 per cent) and the United States (2.3 per cent) (OECD, 2011). Furthermore, police expenditure in England and Wales grew by nearly 50 per cent in real terms between 1999 and 2009, from £9.83 billion to £14.55 billion (Mills et al., 2010).

This paper, among the different age groups involved in crime, specifically focuses on juvenile crime. Juveniles are the predominant group in the crime-age profile: criminological statistics commonly show that crime rates rapidly increase over the teenage years, peak at the mid to late teenage years, and decrease over the remaining life course (Bosick, 2009; Hansen, 2003). Juvenile crime differs from adult crime in various ways. For example, juvenile crime may have a longer-lasting effect than adult crime on the criminals themselves as well as society because involvement in crime at early age can hinder juveniles from attaining an education (Hjalmarsson, 2008; Webbink et al., 2013). The low educational attainment of these juveniles may trigger them to be involved in adult crime in the future, which incurs direct costs (e.g., law enforcement expenditure) and indirect costs (e.g., low level of human capital) to society.

A conventional policy to reduce crime is to increase the severity of punishments or police resources. However, the effectiveness of these conventional policies seems to be limited given the underlying complexity of criminal behaviour. Many criminals commit crimes due to limited knowledge, peer pressure, other psychological reasons or the lack of formal labour market prospects. If people use drugs because they feel peer pressure to use them or because they are not fully aware of the adverse health effects of drugs, introducing tougher sanctions against the use of a certain drug may just cause people to use other drugs but does not necessarily reduce the use of drugs.

Traditionally, education has not been regarded as a measure for combating crime. However, reducing crime by improving education may help overcome the limitations of conventional policies by providing potential criminals with, for instance, knowledge about the consequences of their behaviour, psychosocial skills such as self-control, or better job opportunities in the formal labour market. Furthermore, education has other positive effects on society not necessarily brought about by other crime fighting measures. For example, edu-

Figure 1: Spending on public order and safety in OECD countries



The graph indicates spending on public order and safety as a percentage of GDP in 2009.

cation contributes to better health and political awareness and improves long-run economic growth (Barro, 2001; Dee, 2004; Grossman, 2006). These effects may in turn indirectly contribute to crime reduction.

Statistical evidence indicates that there is a negative correlation between education and crime. For example, amongst black men in the United States in 1980, 4.11 per cent of high school dropouts were imprisoned, compared to only 0.75 per cent of college graduates (Lochner and Moretti, 2004). Mere correlations, however, do not necessarily imply causality. In fact, to establish that education is an effective measure for combating crime, one needs to illustrate that a better education system does "cause" a reduction in crime. Only then can governments consider using educational policies as a crime-reduction measure. This paper aims to establish this causation and computes social benefits brought about by reducing crime through education.

To identify the causal effect of schooling on crime, I exploit the public support granted to students and teachers in specific municipalities after the Kobe earthquake, which hit Japan in 1995, as instrumental variables (IVs) for schooling. Using the IVs is important because the causal variable considered, schooling, is potentially endogenous. Specifically, unobserved heterogeneity across municipalities, such as local labour market conditions, may affect local education and crime levels at the same time. People living in a high-crime area may not invest much in schooling because they obtain a higher return from participating in crime than from working in the formal sector. If this is the case, the crime level can be negatively correlated with the level of schooling even if there is no causal effect of education on crime. Based on a comparison of the arrest rates between municipalities exposed to similar degrees of earthquake damage but with and without the policy interventions, IV estimates suggest that high school participation reduces juvenile arrest rates for violent crime but not for property crime. Various theoretical predictions regarding the relationship between education and crime are discussed to interpret the findings.

The early literature shows a correlation between education and crime (Grogger, 1998; Jarjoura, 1993; Witte and Tauchen, 1994), while the more recent literature uses exogenous variation in education to estimate its causal effect on crime (Lochner and Moretti, 2004; Machin et al., 2011). Among the early studies, I restrict my attention to the literature that specifically focuses on juvenile crime.[1] Although it is not extensive, the literature in this field has been growing over recent years. The studies on juvenile crime investigate the short-run effects of education on crime, and offenders may well be at school age when the crime is committed, unlike the studies on adult crime, which examine the effects of a completed education on subsequent criminal behaviour. The first stream of literature examines the causal effects of day-to-day variation in school attendance on day-to-day variation in youth crime. Jacob and Lefgren (2003) and Luallen (2006), using days on which teachers are in training and on strike, respectively, as sources of exogenous variation in school attendance, find that the level of reported property crimes committed by juveniles decreases and that of violent crimes increases on school days in the United States. The authors suggest that school incapacitates juveniles and lowers the opportunity to commit property crimes, whereas school attendance increases violent crimes by increasing the opportunity to interact with peers.

The second stream of literature attempts to address a potential endogeneity issue using variations in schooling created by educational systems. Machin et al. (2012), using variations in educational attainment caused by changes in the post-compulsory education system that occurred in the United Kingdom, find that higher proportions of young men in full time education and who stay on after compulsory education reduce both property and violent crime convictions.[2] Landerso et al. (2013) and Anderson (2014) investigate the causal effects of school starting age and minimum dropout age, respectively, on youth crime. Landerso et al. (2013) find that a higher age at school start lowers the propensity to be charged with property crime but not with violent crime for young males in Denmark, whereas An-

[1] Refer to Rud et al. (2013) for a detailed summary of the literature on the effects of education on youth crime.

[2] Similar in spirit to Machin et al. (2012), Machin et al. (2011) analyse the causal effect of educational attainment on adult crime in the United Kingdom, using variations in schooling caused by changes in compulsory school leaving age laws. They find that educational attainment reduces property crime convictions of male adults, while this effect is not found for violent crime convictions. Their findings indicate the differential effects of education on adult and juvenile violent crime, implying that the mechanisms through which education affects crime may be different for adult and juvenile crime.

derson (2014) finds that minimum dropout age requirements reduce the juvenile arrest rates for both violent and property crime in the United States. To summarise, the empirical literature on juvenile crime to date has shown negative effects of education on property crime for males, while its effects on violent crime are mixed. Some studies find positive effects (Jacob and Lefgren, 2003; Luallen, 2006), some find no effects (Landerso et al., 2013), and others find negative effects (Anderson, 2014; Machin et al., 2012), indicating a possibility that education has heterogeneous effects on different types of juvenile crime for males.[3]

The remainder of this paper is structured as follows. Section 2 explains the earthquake, the policy interventions in focus and their effects on schooling. Section 3 presents the econometric specification and discusses the empirical problems and the IV strategy. Section 4 describes the data on crime and schooling to be analysed, while Section 5 reports the main empirical findings and investigates the type of crime that is affected by schooling. Furthermore, Section 5 discusses theoretical predictions concerning the relationship between education and crime. A series of robustness checks and falsification exercises are conducted in Section 6, in particular addressing a concern that earthquake-related shocks drive the main results. Because crime generates huge social costs, Section 7 computes estimates of the social benefits due to reductions in crime through education. Finally, Section 8 discusses policy implications and concludes the paper.

2. The earthquake and educational policies

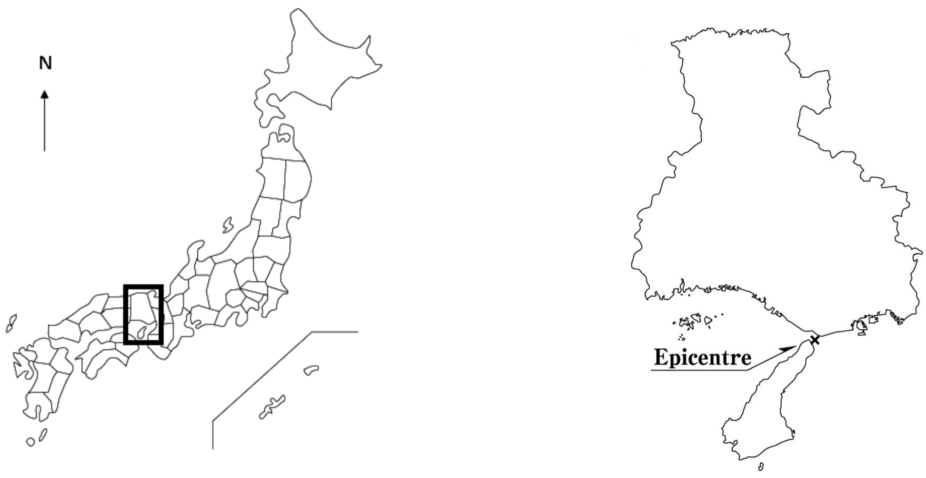
2.1. *The Kobe earthquake*

The Kobe earthquake, officially called the great Hanshin-Awaji earthquake, occurred on 17 January 1995 with its epicentre in Hyogo Prefecture. The square in Panel A of Figure 2 indicates the location of Hyogo Prefecture, while the cross in Panel B indicates the epicentre. The earthquake recorded a magnitude of 7.3 and the maximum possible intensity of 7 on the Japanese intensity scale in the southern part of Hyogo Prefecture (cf., Figure 3).[4] The earthquake caused the second largest loss of life in post-war Japan: 6,434 people were killed, 40,092 people were injured, and more than 300,000 people evacuated. More than 682,182 homes, factories, and shops were destroyed or burnt down and infrastructure such as water, electricity, gas supplies and phone lines were seriously disrupted. Not surprisingly, the earthquake also severely disrupted schooling: 3,883 schools were damaged, leading to a suspension of classes in the disaster area. A large number of students lost their homes and guardians, and schoolteachers were injured. As a result, various relief measures were adopted to support students and teachers.

[3] Interestingly, early literature shows a very different picture for female juvenile crime: education lowers violent crime and has no significant effect on property crime (Anderson, 2014; Jacob and Lefgren, 2003; Landerso et al., 2013), implying that there are heterogeneous effects of education by gender.

[4] The seismic intensity of 7 is assigned to an earthquake strong enough to alter a landform or cause a landslide.

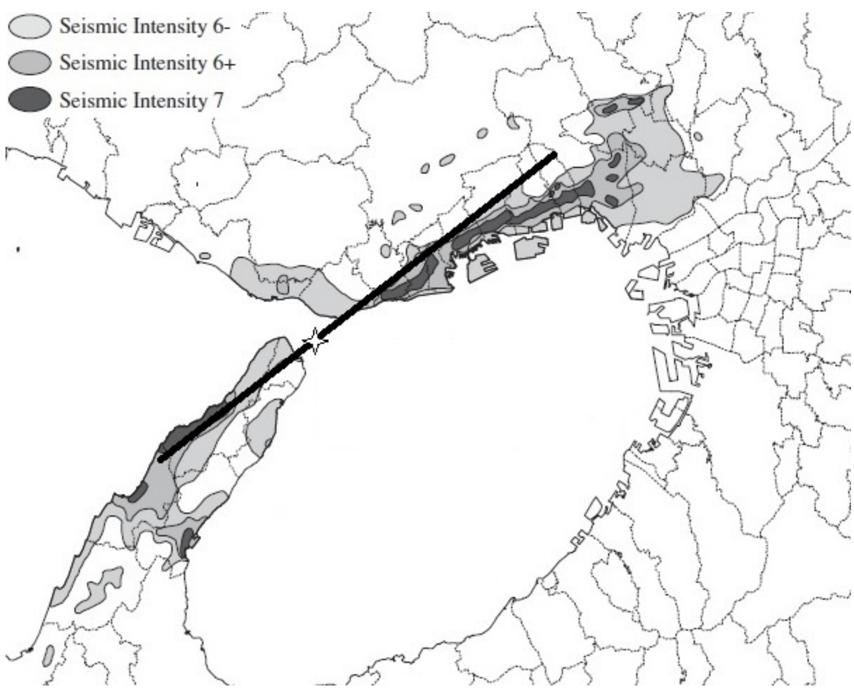
Figure 2: Hyogo Prefecture and the epicentre



A. Hyogo Prefecture B. The epicentre

The square in panel A indicates the location of Hyogo Prefecture in Japan. The cross in panel B shows the location of the epicentre within Hyogo Prefecture.

Figure 3: Seismic intensity



The figure is obtained from Fujimoto and Midorikawa (2002) and edited by the author. The line indicates an approximate location of the faults that caused the earthquake.

Figure 4: Disaster relief act

The shaded area indicates the municipalities where the disaster relief act was applied.

2.2. *The policies in focus*

Shortly after the earthquake, the Japanese government applied the Disaster Relief Act (DRA) to a set of municipalities in Hyogo Prefecture and Osaka Prefecture that the government defined as damaged (c.f., Figure 4). Although the areas that experienced high seismic intensity concentrate on the southern part of Hyogo Prefecture, the DRA was applied to a wider area than southern Hyogo and these areas were targeted for various forms of support.[5]

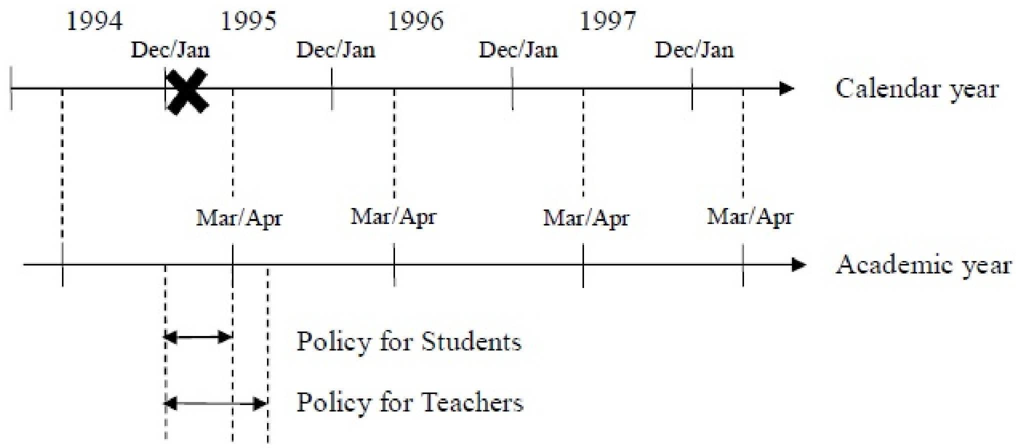
Firstly, the government provided textbooks and school supplies to junior high school students in need of emergency assistance from January 1995 to March 1995.[6] Secondly, schoolteachers and their families were also targeted for relief. Schoolteachers and their families residing in the DRA applied area whose (i) residences were more than 20 per cent destroyed in terms of the total area or (ii) family members were dead or injured were exempted from paying medical bills between January 1995 and May 1995.

To understand the effects of the policy interventions on schooling, measured by the high school participation rates, it is necessary to understand the Japanese educational system. Upon completion of primary school at age 12, it is compulsory to go on to junior high school

[5] The types of support are divided into two categories: emergency support and educational support. Emergency support includes rescue operations, emergency medical support, the burial of corpses and the provision of emergency shelters, food, water and clothes.

[6] Tuition fees were not an issue here because junior high school does not charge tuition fees (at least for public schools) as it is compulsory education.

Figure 5: Timing of the policy interventions



The top arrow indicates the calender year, while the bottom arrow shows the academic year starting from April and ending in March. The cross points the timing at which the earthquake occurred.

for three years. After graduating from junior high school at age 15, students can choose whether to go on to high school. These policies targeted students and teachers at a junior high school level. Thus, the policies are expected to have raised high school participation rates or prevented them from dropping by mitigating the shocks of the earthquake on high school participation.

The timing of the policy interventions and their effects on high school participation are discussed below. The public support for (i) students and (ii) teachers was provided from (i) January 1995 to March 1995 and (ii) January 1995 to May 1995, respectively. Figure 5 summarises the timing of the policy interventions. The top arrow indicates the calendar year, while the bottom arrow shows the academic year starting in April and ending in March. As indicated by the cross, the earthquake occurred in January 1995 (i.e., the end of the 1994 academic year). Students in junior high school at the time of the earthquake were potentially affected by the policy interventions. Recalling that junior high school is a three-year course, students affected by the policies went on to high school (if they choose to) in April 1995, 1996 or 1997. Thus, the policies potentially affected high school participation rates in years 1995, 1996 and 1997.

3. Identification strategy

I now turn to a discussion of the identification strategy. This study investigates the causal effect of schooling on juvenile crime by regressing a measure of juvenile crime on the high school participation rate, controlling for various socio-economic indicators. The following model is specified:

$$arrest\ rate_{mt} = \beta_0 + \beta_1(\%\,highschool)_{mt-3} + X_{mt}^{'}\gamma + \nu_m + \tau_t + u_{mt} \qquad (1)$$

where $arrest\ rate_{mt}$ represents the number of juvenile arrests per 1,000 juvenile population (hereafter, arrest rate) in municipality-police office pair m in year t, where juvenile refers to individuals aged between 18 and 19.[7] $\%\,highschool$ is the fraction of junior high school students who went on to high school. Equation (1) examines the effect of high school participation on the arrest rates three years after high school participation (i.e., the period immediately after the completion of high school for those who went on to and successfully completed high school).[8] γ is the vector of parameters for the time varying covariates, X_{mt}. ν_m and τ_t are the municipality and year fixed effects, respectively, and u_{mt} represents the disturbance terms.

The main coefficient of interest is β_1, which measures the effect of schooling on juvenile arrests. An econometric issue in the estimation of equation (1) is that schooling is potentially endogenous. Specifically, unobserved heterogeneity across municipalities, such as local labour market conditions, may simultaneously affect local schooling and crime levels. For example, people living in a high crime area may not invest much in schooling because they obtain a higher return from participating in crime than from working in the formal sector. If this is the case, β_1 can be negative even if there is no causal effect of schooling on crime. To address the potential endogeneity issue, the policy interventions outlined in Section 2 are used as IVs for schooling.

However, the nature of the policy interventions raises some empirical concerns. Namely, the set of municipalities affected by the policies (hereafter, the supported group) presumably suffered a greater extent of earthquake damage than those not eligible to receive public support (hereafter, the not-supported group). Thus, the effect of the policy interventions may be confounded by earthquake-related shocks that happened to affect the schooling and criminal decisions of adolescents in the disaster areas. For example, earthquake shocks may have affected youth labour market conditions, which might have subsequently affected the schooling and criminal decisions of adolescents. It may also be possible that there was a relocation of police resources after the earthquake and more resources were assigned to the supported group.

To control for any earthquake shocks that might have affected the arrest rates and the

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

[8] Schooling in period t can potentially affect the criminal behaviour in period $t+x$ where $x \geq 4$. However, those who entered high school in period t are aged 20 or above at period $t+x$ where $x \geq 4$. Crimes committed by those aged 20 or above are classified as adult crimes in the Japanese criminal law, and unfortunately, the detailed enough information on age-specific number of arrests is not available for adults.

high school participation rates differently across the supported group and the not-supported group, I omit a subset of municipalities in the supported group that were severely damaged and in the not-supported group that were the least damaged by the earthquake. Omitting these municipalities leaves in the sample (i) the set of municipalities that were the least damaged within the supported group (the treatment group) and (ii) those in the not-supported group that suffered similar degrees of earthquake damage to the treatment group (the control group). The earthquake damage is measured by the percentage of people who died or were injured and of households that had their homes destroyed. These damage measures were reported by the government according to the official evaluation performed after the earthquake.

A municipality is classified as severely damaged if one or more damage measures were recorded above the top 25th percentile value of damage distribution within the supported group and omitted from the sample.[9] Because the distribution of the earthquake damage is negatively skewed, this rule drops most of the municipalities that recorded high degrees of damage. As a result, 16 municipalities are retained as the treatment group. Likewise, a municipality in the not-supported group is classified as less damaged if all damage indicators were recorded below the bottom 25th percentile value of damage distribution within the not-supported group and dropped from the sample.[9] This rule selects 30 municipalities as the control group. Thus, the final sample consists of 46 municipalities presented in Figure 6.[10][11] The cross in Figure 6 indicates the epicentre, and the faults that caused the earthquake are located along the line lying from the southwest to the northeast. Not surprisingly, the municipalities lying along the faults that recorded high proportions of earthquake damage are dropped from the sample.

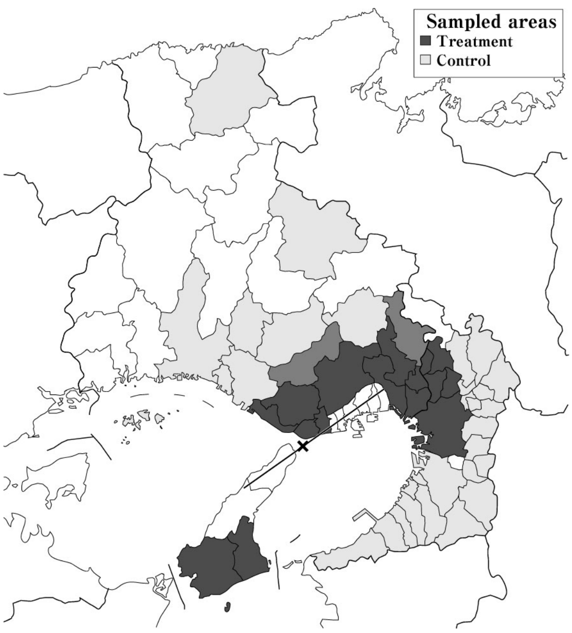
Table 1 summarises the extent of the earthquake damage in the treatment and control municipalities. For the comparison purpose, the degree of earthquake damage in the omitted municipalities in the supported group is also reported. Panel A indicates that very few people died or were severely injured in the treatment and control groups. Panel A shows that only 0.03 per cent and 0.07 per cent of people on average died and experienced serious injury, respectively, in the treatment group, implying that presumably the direct effects of the earthquake were limited, although a higher proportion of people appears to have experienced minor injury in the treatment group. Regarding the damage to homes reported in Panel B, more households had their homes damaged in the treatment group than in the control group. However, a closer look at data on the extent of earthquake damage shows that a higher proportion of households in the treatment group experienced minor damage instead of the full destruction of homes.

[9] The robustness of the results to this particular cut-off value is examined in Section 6.

[10] The two areas highlighted by dark grey in Figure 6 cannot be classified into the treatment group or the control group because police offices that administer these areas cover both municipalities with and without the policy interventions. Thus, the two areas highlighted by dark grey are omitted from the sample.

[11] Figure 6 indicates that there are two areas in the control group that are geographically detached from the rest of sampled areas. The results are not sensitive to the omission of these two areas.

Figure 6: Sampled areas



The line indicates an approximate location of the faults that caused the earthquake. The black area indicates the least damaged municipalities within the supported group (the treatment group). The light grey area corresponds to the municipalities in the not-supported group that experienced similar degrees of earthquake damage as the treatment group (the control group).

Table 1: Earthquake damage

	Omitted		Treatment		Control		
	Mean	Std. err.	Mean	Std. err.	Mean	Std. err.	p-value
A. Loss of life and injury							
% dead	0.67	3.08	0.03	0.43	0.00	0.01	0.93
% severely injured	0.92	3.61	0.07	0.65	0.00	0.05	0.89
% lightly injured	1.23	4.17	0.38	1.55	0.01	0.16	0.74
B. Damage to homes							
% fully destroyed	38.10	18.36	3.22	4.41	0.00	0.04	0.32
% half destroyed	31.08	17.49	7.61	6.63	0.01	0.14	0.13

Table reports weighted means in percentage. Weights are total population in Panel A and total households in Panel B. P-value corresponds to p-values from the test for the equality of each damage measure between the treatment and control groups.

In contrast, the extent of earthquake damage in the omitted municipalities appear to be much more severe than the sampled municipalities. Particularly, damage to homes was severe: approximately 40 per cent and 30 per cent of households had their homes fully and half destroyed, respectively, in the omitted municipalities. The last column of Table 1 reports p-values from the test for the equality of each damage measure between the treatment and control groups. The p-values indicate that degrees of damage are not significantly different between the two groups irrespective of the damage measures used. Because the retained municipalities were exposed to similar degrees of damage, the effect of the earthquake or the earthquake-related shocks would have been similar across the two groups. For example, if demand for construction work increased after the earthquake in the disaster areas, it would have similarly affected the two groups. Similarly, if there was increased concern for security after the earthquake and police resources were relocated to the disaster areas, the two groups would have been exposed to similar changes.

Table 2 reports the pre-earthquake characteristics of the treatment and control groups. Panel A compares the occurrences of crime measured by recorded crime rates and arrest rates and police resources measured by number of population covered per police office. Panel A indicates that the treatment group exhibits a higher occurrence of crime, more arrests per 1,000 population, and fewer citizens served per police officer. However, the differences are statistically insignificant. Regarding the educational characteristics reported in Panel B, individuals in the treatment group appear to be more educated (i.e., higher high school participation rates and more individuals with high school diplomas or university degrees), but the differences are not statistically significant. In terms of educational resources, the treatment group shows higher educational expenditures, but the difference is also not significant. The characteristics related to the labour market and the economy reported in Panel C indicate that the proportions of those employed and unemployed and per capita income appear to be similar in the two groups.

Turning to demographic characteristics, Panel D indicates that there are no important differences in sex ratio, age composition, and proportions of single mother households, although the treatment group shows a slightly higher proportion of foreigners. Importantly for the purpose of identification, both in- and out-migration rates are not significantly different between the two groups. However, the treated municipalities display higher in- and out-migration rates. The different migration patterns would bias the results if the pattern were correlated with arrest rates and differed between the two groups. For example, less crime prone adolescents in the treatment group may have moved out after the earthquake in search of better educational opportunities. To investigate whether there were different migration patterns in the two groups following the earthquake, in- and out-migration rates *after* the earthquake are compared. Although both in- and out-migration rates are higher in the treatment group after the earthquake, the differences are not statistically significant.[12]

[12] In-migration per 1,000 population in the treatment group and the control group are 58.35 (58.60) and

Table 2: Municipality characteristics

	Control		Treatment		
	Mean	Std. Err.	Mean	Std. Err.	p-value
A. Crime and Police					
Recorded crime per 1,000 pop.	16.33	23.14	18.70	33.87	0.95
Arrests per 1,000 pop.	4.69	12.48	7.15	21.06	0.91
Population per police office	153,593	24,980	123,207	48,805	0.54
B. Education					
% High school participation	94.81	4.05	95.91	4.95	0.87
% Compulsory	24.87	7.89	21.82	10.33	0.82
% High school	41.72	9.00	42.37	12.35	0.97
% University	10.81	5.67	13.14	8.45	0.81
Student-teacher ratio	19.80	3.36	19.53	7.20	0.97
Education expenditure per capita	222.54	34.34	287.55	145.67	0.58
C. Economy					
% Unemployed	2.17	2.66	2.67	4.03	0.92
% Employed	58.19	9.01	59.15	12.29	0.95
Income per capita	1,324.23	229.17	1,350.21	563.87	0.96
D. Demography					
Sex ratio	0.97	0.16	0.97	0.39	1.00
% Population under 15	18.65	7.11	17.39	9.48	0.92
% Population 15 to 65	72.29	8.17	73.92	10.98	0.91
% Foreigners	1.22	2.00	2.50	3.91	0.75
% Single mother	1.45	2.19	1.43	2.96	0.99
In-migration per 1,000 pop.	46.09	38.28	59.84	59.30	0.84
Out-migration per 1,000 pop.	47.21	38.72	64.27	61.31	0.81

Mean refers to weighted means where weights are population, except for % high school participation (population in junior high school), education expenditure (population under 15), % compulsory, % high school, % university, % unemployed, and % employed (population aged 15 or above), and % single mother (household). Figures are the values in the census year 1990. The p-value corresponds to p-values of tests under the null hypothesis of equality of means or proportions between the treatment and control groups. The sex ratio is the number of males relative to females. Per capita taxable income and education expenditure are measured in ¥1,000.

Figure 7: High school participation rates

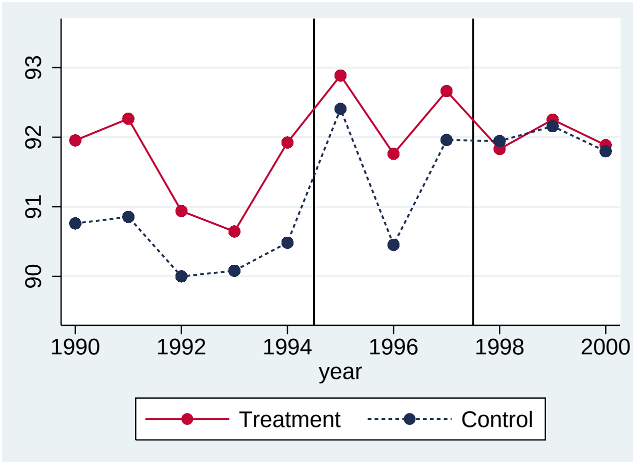


Figure plots the average high school participation rates. The average rates are weighted by the number of junior high school graduates, and regression-adjusted for sex ratio. Solid line: least damaged municipalities eligible for the public support (treatment group). Dotted line: municipalities not eligible for the public support that experienced similar degrees of earthquake damage to the treatment municipalities (control group).

Nevertheless, one could be concerned that the migration patterns of adolescents might be different between the two groups. Unfortunately, it is not possible to examine the characteristics of migrants due to data limitations. However, if the migration pattern were correlated with age, the migration would have affected the age compositions of the two groups. To examine this possibility, I conduct t-tests under the null hypotheses of constant differences in the percentages of individuals under 15, between 15 and 65, and in junior high school between the two groups before and after the earthquake. Because junior high school is compulsory education, the percentage of individuals in junior high school should correspond to the percentage of those aged between 13 and 15. The null hypothesis is not rejected for every case, meaning that the results do not support the possibility that different migration rates altered the age compositions or the proportion of junior high school students in the two groups. Overall, I find no important differences between the two groups regarding crime, education, demography, and economy.

Figure 7 plots the fraction of junior high school students who went on to high school. The solid and dotted lines represent the treatment and control groups, respectively, and the years between the two vertical lines correspond to the years in which high school participation rates are potentially affected by the policy interventions. Figure 7 indicates that the average high school participation rates of both groups show similar trends prior to the earthquake

41.55 (36.43), respectively, where standard errors are reported in parentheses. The p-value of the test under the null hypothesis of equality of means is 0.80. The corresponding figures for out-migration per 1,000 population are 61.33 (59.99) and 45.74 (38.14), respectively, and the p-value is 0.82. Figures are the values in the census year 2000.

in 1995, although the treatment group shows higher average high school participation rates. Note that the systematic difference between the two groups is absorbed by municipality fixed effects in the econometric specification.

Shortly after the earthquake, the average high school participation rates increased in both groups, which is puzzling at a glance. However, supporting materials suggest that there was educational support provided to those who entered high school in 1995 (only) in addition to the public support outlined in Section 2.2. For example, students who suffered earthquake damage and went on to high school in 1995 were admitted to high school even if the high schools had already met their quota. The eligibility for these forms of support was determined by the extent of earthquake damage that the students incurred. Because the treatment and control groups suffered similar degrees of damage, students in both groups would have similarly benefited from this type of support. The sharp increase in high school participation in 1995 implies that presumably the special support offered to those who entered high school in 1995 more than compensated for the earthquake damage. Note that although the high school participation rates were affected by this support, it does not invalidate the identification because common shocks that affect both groups are absorbed by year fixed effects.

In 1996, it appears that the high school participation rates dropped in both groups, but to a smaller extent in the treatment group. The smaller dip in the treatment group suggests that presumably the public support described in Section 2.2 prevented high school participation from dropping in the treatment group. In other words, high school participation would have dropped even more in the absence of public support.

4. Data and variables

This study combines data on crime and schooling. For data on youth crime, I rely on the crime data in the Statistical Crime Report (SCR, *Hanzai Tokeisyo*). Data on criminal activity for 115 police offices in Hyogo Prefecture and Osaka Prefecture from 1996 to 2005 are obtained from the SCR. To the best of my knowledge, the SCR is the only available dataset that provides information on criminal activity at a police-office level in Hyogo Prefecture and Osaka Prefecture. From 1996 onwards, the SCR provides detailed information on the nature of the crime, which allows me to investigate the effect of high school participation on different types of crime. This study broadly divides crime into two categories, violent crime and property crime. Violent crime consists of murder, assault and sexual offenses, while property crime consists of theft, robbery and extortion. One of the disadvantages of the SCR is that criminal activity is recorded by police office but not by age. However, the age-specific criminal activities of juveniles are available at a prefecture level. Thus, the age-specific criminal activities of juveniles in a police office are computed using information on total criminal activities (i.e., the sum of criminal activities committed by juveniles and adults) at a police-office level and the age-specific criminal activities of juveniles at a prefecture level. Details of the computation can be found in Appendix A.

As measures of criminal activities, the SCR provides information on recorded crimes and arrests. Because recorded crimes reflect incidents of crime more accurately than arrests, they are a better measure of criminal activity. However, recorded crimes cannot be used for the purpose of this study because the identity of offenders including age is not available. Thus, arrests are used as a measure of criminal activity. One of the disadvantages of using arrests is that it understates the actual incidents of crime because not all incidents result in arrests. Thus, if a unit change in arrests is observed, it is likely that actual incidents changed more than a unit. Another possible disadvantage is that using arrests may overstate the effect of schooling on criminal activity to the extent that more schooling makes adolescents more skilful at avoiding arrests. This concern is unlikely to be severe for serious offences because the clear-up rates for serious offences are very high (e.g., 96 and 67 per cent for murder and rape, respectively), although it remains a concern for less serious offences (e.g., 12 and 41 per cent for theft and extortion, respectively).

The crime data are merged with the data on high school participation rates, defined as the share of junior high school graduates who go on to high school, obtained from the School Basic Survey Report 1993 to 2002 (*Gakko Kihon Chosa Hokokusyo*). The survey covers every school in Japan and has been conducted every year since 1948. The survey is a good source of community-level information on schooling and provides a rich set of information including the number of classes, students and teachers, school budgets, and the careers of graduates. For data on other covariates, I rely on 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 conditions at a municipality level. The survey has been conducted since 1976 and provides 1,500 social and demographic variables by municipality. As socio-economic indicators, per capita income, unemployment rate, sex ratio, proportion of foreigners and single mother households are included in the analysis. Information on number of households, unemployed, foreigners and single mothers and those in the labor force are available in census years, 1995, 2000 and 2005. To compute the unemployment rate and the proportion of foreigners and single mother households, I linearly interpolate the required variables between census years.

Information on the damage due to the earthquake is obtained from the Disaster Prevention Divisions of Hyogo Prefecture and Osaka Prefecture. The documents report earthquake damage that is categorised into injuries, deaths and damage to buildings. Each category is further divided into sub-categories according to the extent of the damage.

Data on covariates are available for 163 municipalities in Hyogo Prefecture and Osaka Prefecture. As the data on crime and covariates have different units of observations, these data need 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 each municipality level variable is computed, and subsequently the weighted mean is matched with the crime data. In case (ii), a weighted mean of each police-office level crime variable is computed, and then the weighted mean is matched with the data on covariates. 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. For crime data, the population of individuals aged between 18 and 19 is used as a weight. For the data on covariates, population is used as a weight, except for high school participation rate, unemployment rate and single mother households, for which the number of junior high school graduates, labour force population and number of households, respectively, are used as weights. As a result of matching, the number of municipality-police office pairs becomes 94. After retaining in the sample only the municipalities that experienced similar degrees of earthquake damage as described in Section 3, the sample consists of observations for 46 municipality-police office pairs from 1996 to 2005 except for the high school participation rates that appear in equation (1) with a three period lag (i.e., from 1993 to 2002).

5. Results

I begin by estimating equation (1) using the Ordinary Least Squares (OLS) estimator. Table 3 reports the estimates of regressions where the arrest rates between 1996 and 2005 are regressed on the high school participation rate and covariates that absorb unobserved heterogeneity at the municipality and year levels and municipality-specific time trends.

Panel A of Table 3 presents the OLS results for violent and property crimes. The first column reveals that the effects of schooling vary considerably by crime category: high school participation is negatively associated with violent crimes and positively associated with property crimes. To further allow for the heterogeneous effects of schooling across different types of crime, Panel B decomposes violent crimes and property crimes into detailed types of crime.[13] The first column in rows B1 and B2 shows that high school participation is negatively and significantly associated with assault and sexual offense. Turning to property crimes, schooling appears to have a positive association with theft and no significant association with robbery and extortion (cf. the first column in rows B3 to B5).

The first column in Table 3 is correctly specified if every difference across municipalities is time invariant or captured by municipality-specific time trends. However, the first column is misspecified if the time varying socio-economic characteristics also affect the arrest rates. Another possible issue with the first column is selection bias. Namely, the assignment of municipalities to the treatment group might be endogenous. For example, poor municipalities with fragile infrastructures may be severely damaged by the earthquake

[13] Table 3 does not present the effect on murder because the number of arrests due to murder is extremely low and there is too little variation in the data.

Table 3: The effect of high school participation on youth crime (OLS)

Dependent variable:	# Arrested juveniles per 1,000 youth population			
	% High school	Std. err.	% High school	Std. err.
A. Violent vs. property				
A1. Violent crime	-0.035***	(0.009)	-0.049***	(0.010)
A2. Property crime	0.131***	(0.043)	0.148***	(0.050)
B. By crime type				
B1. Assault	-0.030***	(0.009)	-0.042***	(0.009)
B2. Sexual offense	-0.006**	(0.002)	-0.007**	(0.003)
B3. Theft	0.137***	(0.041)	0.155***	(0.048)
B4. Robbery	0.002	(0.004)	0.002	(0.004)
B5. Extortion	-0.008	(0.005)	-0.008*	(0.005)
Controls	No		Yes	

Table reports estimated coefficients on β_1 in equation (1). Standard errors in parentheses are clustered by municipality. *** denotes p<0.01, ** refers to p<0.05, and * refers to p<0.1. Labels of the dependent variables indicate the following. Violent crime: murder, assault, and sexual offense; property crime: theft, robbery, and extortion. Controls included are the municipality fixed effects, year fixed effects, municipality-specific time trends, per capita income, unemployment rate, sex ratio, proportions of foreigners and single mother households. The effect on murder is not reported because the number of arrests due to murder is extremely low.

and, consequently, more likely to be targeted by the policies. The level of crime in those poor municipalities may be high due to, for example, few opportunities in the formal labour market. To allow for the possibility that time varying socio-economic characteristics affect the arrest rates, I further control for per capita income, unemployment rate, sex ratio, and proportions of foreigners and single mother households in the second column. The results are not sensitive to the inclusion of these variables: high school participation is negatively correlated with violent crime and positively correlated with property crime. The second column in row A1 indicates that a one-percentage-point increase in the high school participation rate is associated with a lower number of arrests due to violent crime per 1,000 youth population by approximately 0.05, corresponding to an approximately 11 per cent reduction relative to its mean value, 0.47. Regarding property crime, row A2 indicates that a one-percentage-point increase in the high school participation rate is associated with a higher number of arrests per 1,000 youth population by approximately 0.15 on average, corresponding to an approximately 4 per cent reduction relative to its mean, 3.44.

The OLS estimator, even after controlling for municipality fixed effects, is inconsistent if the disturbances, u_{mt}, and the high school participation rate are correlated. To address the possible endogeneity of schooling, equation (1) is estimated using the IV estimator. I begin by reporting the first-stage estimates of equation (1) in which the high school participation rate is instrumented with the policy interventions using dummies that equal one if municipality m belongs to the treatment group and the year is 1995, 1996 or 1997, respectively, and zero otherwise. The treatment dummies reflect the effects of the policies on the high school participation rate and are therefore expected to be positive. The estimated first-stage effects of the policy interventions on the high school participation rate in years 1995, 1996 and 1997 are -0.140 (0.61), 1.179 (0.00) and 0.662 (0.02), respectively, where the p-values are reported in parentheses. The estimate shows that the high school participation rates in the treatment and control groups were not significantly different in 1995. This result is not surprising because there was educational support provided specifically to those who entered high school in 1995 in addition to the policies outlined in Section 2.2. For example, students who suffered earthquake damage and went on to high school in 1995 were admitted to high school even if the high school had already met its quota. The eligibility for these types of support was determined by the extent of earthquake damage incurred by the students. Because the treatment and control groups suffered similar degrees of damage, students in both groups would have similarly benefited from these supports.

In contrast, the high school participation rates were higher in the treatment group in years 1996 and 1997. In particular, the treatment dummy 1996 shows the greatest magnitude of coefficient and is statistically significant at any conventional significance level. Because the IV estimator with uninformative instruments is known to be biased toward OLS, I only use the treatment dummy 1996 as an instrument because it has the strongest predictive power in terms of the effect size and its statistical significance. After omitting the treatment dummies

1995 and 1997 from the first-stage equation, the coefficient on the treatment dummy 1996 is 1.109 with standard errors 0.349 (F-statistic 10.1) reported in Table 4. The coefficient estimate implies that the high school participation rate was higher in the treated municipalities by approximately 1.109 percentage points in 1996.

It is important for the identification that the instrument is not weak, meaning that the instrument and the high school participation rate are highly correlated. A weak instrument is known to bias the IV estimator toward the probability limit of the corresponding OLS estimator. Stock et al. (2002) compute the critical value for the weak instrument test based on the first-stage F-statistic and suggest that an F-statistic above approximately 10 makes IV inferences reliable. As the first-stage F statistic is 10.1, the instrument is not weak according to their test for weak instruments. However, one might be concerned that a weak instrument problem is still an issue because the first-stage F statistic is almost equal to the threshold value of 10. With a weak instrument, IV estimates from a just-identified model tend to be too imprecise (Angrist and Pischke, 2009). Nevertheless, the IV estimator in a just-identified model is known to be approximately unbiased. In other words, even with a weak instrument, the IV estimator in a just-identified model is approximately centred around the true parameter.

Table 4, in addition to the first-stage estimates, presents the reduced-form estimates for the effects of the policy interventions on the arrest rates and the IV estimates of the effect of high school participation on the arrest rates (i.e., β_1 in equation (1)). Panels A and B present the results for violent and property crime, respectively. The reduced-form estimates in row A1 show that the policy interventions are significantly associated with lower arrest rates for violent crimes. Finally, the causal effect of interest reported in row A1 indicates that high school participation significantly reduced the arrest rates for violent crimes. The effect size is now greater (in absolute terms) than the corresponding estimate in Table 3. The estimated causal effect for violent crime is -0.114, meaning that a one-percentage-point increase in the high school participation rate reduces violent crime arrest rates by approximately 0.114 on average, corresponding to an approximately 24 per cent reduction evaluated at its mean value. Rows A2 and A3 allow for the differential effects of schooling on different types of violent crimes, and indicate that schooling significantly reduces both assault and sexual offenses. Again, the effect sizes are greater (in absolute terms) than the corresponding estimates in Table 3.

Turning to property crime, the reduced-form estimate in row B1 indicates that there is no significant association between the policy interventions and property crime arrest rates. Consistent with the reduced-form result, the IV estimate in row B1 shows that the effect of schooling on property crime arrest rates is positive, but no longer statistically significant. Decomposing property crimes into more detailed types of crime does not change the findings: I find no significant effect of high school participation on theft, robbery, and extortion (cf., rows B2 to B4). It is not surprising that the results for total property crime and theft are

Table 4: First-stage, reduced-form, and IV estimates of crime regressions

Dependent variable:	% High school		# Arrested juveniles per 1,000 youth population			
	First-stage		Reduced-form		IV	
	Policy	Std. err.	Policy	Std. err.	% High school	Std. err.
A. Violent crime						
A1. Total violent crime	1.109***	(0.349)	-0.127**	(0.061)	-0.114**	(0.047)
A2. Assault	1.109***	(0.349)	-0.095*	(0.053)	-0.085*	(0.044)
A3. Sexual Offense	1.109***	(0.349)	-0.028*	(0.017)	-0.026**	(0.012)
B. Property crime						
B1. Total property crime	1.109***	(0.349)	0.290	(0.264)	0.261	(0.213)
B2. Theft	1.109***	(0.349)	0.289	(0.271)	0.261	(0.217)
B3. Robbery	1.109***	(0.349)	-0.010	(0.026)	-0.009	(0.021)
B4. Extortion	1.109***	(0.349)	0.022	(0.025)	0.020	(0.021)

* $p<.10$, ** $p<.05$, and *** $p<.01$. Standard errors are clustered by municipality. First-stage and reduced-form estimates are the estimated coefficients on the high school participation rate. The IV estimates are the estimates of β_1 in equation (1). Rows in each panel correspond to the regressions for the different types of crime. Refer to Table 3 for the controls included and classification of crime. The result for murder is not reported because the number of arrests due to murder is extremely low.

very similar because approximately 90 per cent of property crime arrests among juveniles are due to theft.

To summarise, I find that that schooling significantly reduced arrest rates for different types of violent crimes but not for property crimes. The effects on violent crime arrest rates are negative and statistically significant using both OLS and IV estimators after controlling for covariates that absorb municipality level heterogeneity, year level heterogeneity, and municipality-specific time trends. The magnitude of coefficients is greater (in absolute terms) for the IV estimates. Why is this the case? One possible interpretation is that juveniles may not invest much in schooling if there are rich (legitimate) local labour market opportunities for unskilled juveniles such as unskilled manual work. At the same time, unskilled juveniles may not participate in crime because they can obtain a job in the formal sector. If this is the case, the OLS estimator can be biased upward. Another possible reason is that the IV estimates are obtained from juveniles affected by the policy interventions that raised the high school participation rate from approximately 96 per cent to 97 per cent. These juveniles are from the bottom end of the educational distribution and might be most responsive to a change in schooling in terms of criminal activities.

Potential mechanisms of crime reduction

Having estimated the causal effect of schooling on different types of crime, it would be informative to know the potential operating mechanisms that drive the main results. Because Table 4 provides the net effects of a change in schooling on crime, it is not possible to pin down the exact mechanisms for a reduction in violent crime. Nevertheless, I attempt to narrow down the potential mechanisms through which schooling reduces crime that are suggested by existing literature.

Firstly, rational choice models suggest that education reduces crime by raising the return in the formal labour market and thereby increasing the opportunity cost of committing a crime (Ehrlich, 1975; Huang et al., 2004; Lochner, 2004). This operating path may be plausible because violent crime is typically associated with a longer sentence than property crime, meaning that criminals must pay a higher opportunity cost when they commit violent crimes. Secondly, the results are also consistent with behavioural economic models, suggesting that excessive myopia and projection bias play a role in explaining risky behaviour by adolescents (O'Donoghue and Rabin, 2001). Participation in risky behaviour involves a trade-off between short-run benefits (e.g., immediate gratification) and long-run costs (e.g., losing a job opportunity in the future). Education may reduce crime by affecting the time or risk preferences of individuals or by enabling them to more accurately compute the cost of engaging in crimes. This path is plausible because violent crime may be perceived as more risky than property crime. In addition, because violent crime typically entails a higher associated cost, the ability to accurately compute the associated future cost would discourage adolescents from committing a serious offense.

Thirdly, more schooling might reduce violent crime by providing adolescents with less crime-prone peers (Witte, 1997). This channel is potentially important because adolescents tend to commit crime in groups and peer relationships, such as exposure to deviant peers, time spent with peers and attachment to peers, are strongly related to the delinquent behaviour of adolescents (Panel on Juvenile Crime et al., 2001, Thornberry et al., 1994, and Warr, 1993). Lastly, some literature suggests that "idle hands are the devil's workshop" and more schooling reduces crime because adolescents have less time to be involved in criminal activity (Jacob and Lefgren, 2003; Witte and Tauchen, 1994). However, the estimates in Table 4 capture the effect of high school participation on arrest rates three years after high school participation (cf. equation (1)), implying that most individuals are no longer at high school at the time the arrest rates are observed because high school is a three-year-course. Therefore, educational incapacitation is unlikely to be the dominant path for a reduction in crime in the current context.

6. Robustness checks

This section conducts a set of robustness checks and falsification exercises. Firstly, the robustness of the results to different sample specifications is examined. Secondly, to establish that the main results are not driven by earthquake-related shocks that happened to affect schooling and crime, a set of falsification exercises are performed.

Firstly, a series of robustness checks are conducted using different sample specifications. Recall that, to address a concern that the effect of the policy interventions may be confounded by earthquake-related shocks that happened to affect the schooling and criminal activity in the disaster areas, I omitted from the sample a set of municipalities (i) within the supported group that are severely damaged and (ii) within the not-supported group that are the least damaged. A municipality is classified as severely damaged if one or more damage measures were recorded above the top 25th percentile value of damage distribution within the supported group. Likewise, a municipality in the not-supported group is classified as least damaged if all damage indicators were recorded below the bottom 25th percentile value of damage distribution within the not-supported group. Because the sampled municipalities suffered similar degrees of earthquake damage, the municipalities would have been similarly affected by earthquake-related shocks. However, one might be concerned that the main results are sensitive to sample specification.

To address this concern, I shift the cut-off values of damage distribution within the supported group and the not-supported group, respectively, from the top and bottom 25th percentile values to the top and bottom 10th, 15th, 20th, 30th, and 35th percentile values. The greater the percentile used, the more restrictive the sample becomes: the sample sizes correspond to 490, 470, 460, 450, and 420 when the top and bottom 10th, 15th, 20th, 30th, and 35th percentile values, respectively, are used as the cut-off values.

Table 5: Alternative sample specifications

		First-stage		IV	
Cut-off values		(1)		(2)	
A.	10th percentile value				
A1.	Violent crime	1.218***	(0.327)	-0.127***	(0.041)
A2.	Assault	1.218***	(0.327)	-0.095**	(0.037)
A3.	Sexual offense	1.218***	(0.327)	-0.033***	(0.013)
A4.	Property crime	1.218***	(0.327)	0.108	(0.195)
A5.	Theft	1.218***	(0.327)	0.136	(0.191)
A6.	Robbery	1.218***	(0.327)	-0.022	(0.021)
A7.	Extortion	1.218***	(0.327)	-0.005	(0.027)
B.	15th percentile value				
B1.	Violent crime	1.160***	(0.342)	-0.113***	(0.043)
B2.	Assault	1.160***	(0.342)	-0.086**	(0.039)
B3.	Sexual offense	1.160***	(0.342)	-0.023**	(0.011)
B4.	Property crime	1.160***	(0.342)	0.221	(0.200)
B5.	Theft	1.160***	(0.342)	0.219	(0.204)
B6.	Robbery	1.160***	(0.342)	-0.007	(0.019)
B7.	Extortion	1.160***	(0.342)	0.018	(0.019)
C.	20th percentile value				
C1.	Violent crime	1.109***	(0.349)	-0.114**	(0.047)
C2.	Assault	1.109***	(0.349)	-0.085*	(0.044)
C3.	Sexual offense	1.109***	(0.349)	-0.026**	(0.012)
C4.	Property crime	1.109***	(0.349)	0.261	(0.213)
C5.	Theft	1.109***	(0.349)	0.261	(0.217)
C6.	Robbery	1.109***	(0.349)	-0.009	(0.021)
C7.	Extortion	1.109***	(0.349)	0.020	(0.021)
D.	25th percentile value				
D1.	Violent crime	1.109***	(0.349)	-0.114**	(0.047)
D2.	Assault	1.109***	(0.349)	-0.085*	(0.044)
D3.	Sexual offense	1.109***	(0.349)	-0.026**	(0.012)
D4.	Property crime	1.109***	(0.349)	0.261	(0.213)
D5.	Theft	1.109***	(0.349)	0.261	(0.217)
D6.	Robbery	1.109***	(0.349)	-0.009	(0.021)
D7.	Extortion	1.109***	(0.349)	0.020	(0.021)
E.	30th percentile value				
E1.	Violent crime	1.044***	(0.359)	-0.107**	(0.051)
E2.	Assault	1.044***	(0.359)	-0.079*	(0.048)
E3.	Sexual offense	1.044***	(0.359)	-0.027**	(0.013)
E4.	Property crime	1.044***	(0.359)	0.277	(0.236)
E5.	Theft	1.044***	(0.359)	0.277	(0.240)
E6.	Robbery	1.044***	(0.359)	-0.003	(0.022)
E7.	Extortion	1.044***	(0.359)	0.016	(0.022)
F.	35th percentile value				
F1.	Violent crime	1.077***	(0.416)	-0.061*	(0.037)
F2.	Assault	1.077***	(0.416)	-0.031	(0.035)
F3.	Sexual offense	1.077***	(0.416)	-0.024	(0.016)
F4.	Property crime	1.077***	(0.416)	0.118	(0.184)
F5.	Theft	1.077***	(0.416)	0.097	(0.194)
F6.	Robbery	1.077***	(0.416)	0.007	(0.023)
F7.	Extortion	1.077***	(0.416)	0.027	(0.027)

* $p < .10$, ** $p < .05$, and *** $p < .01$. Standard errors are clustered by municipality. First-stage estimates are the estimated coefficients on the treatment dummy 1996. The IV estimates are the estimates of β_1 in equation (1). Refer to Table 3 for controls included in the analysis. Panels A to F use the top and bottom 10th, 15th, 20th, 25th, 30th, and 35th percentile values of damage distribution within the supported group and the not-supported group, respectively, to define the most severely damaged and least damaged municipalities within the supported group and the not-supported group, respectively, that are omitted from the sample. This rule selects 490, 470, 460, 460 450, and 420 municipalities as sample municipalities in panels A to F, respectively.

Table 5 presents a series of robustness checks using alternative definitions for the treatment group and the control group. The main results are also reported for comparison purposes in panel D. The first-stage estimates reported in column (1) are robust to different sample specifications. The coefficients on the treatment dummy 1996 are consistently highly statistically significant and their point estimates are statistically not different from the main results. The IV estimates presented in column (2) also reconfirm the previous findings that high school participation significantly reduces arrest rates for violent crimes but not for property crimes. Inevitably, as the sample sizes decrease, the degrees of freedom become smaller. When the top and bottom 35th percentile values are used as the cut-off values, the estimates become statistically insignificant for assault and sexual offense. However, the point estimates are statistically not different from the main findings.

Secondly, even after retaining in the sample a set of municipalities that suffered similar degrees of earthquake damage, one might be concerned that differential effects of earthquake shocks on the two groups drive the main results. For example, local labour market conditions may have deteriorated more in the treatment group than in the control group, which might have differently affected schooling and the criminal decisions of adolescents in the two groups. Because the sampled municipalities are geographically close to each other and likely to be embedded in the same local labour market, this concern is unlikely to be severe. Nevertheless, to address this type of concern, I regress the arrest rates of cohorts younger and older than those aged 18 - 19, respectively, on high school participation rates. Recall that high school participation rates appear in equation (1) with a three-year-period lag. Because high school is a three-year-course, equation (1) estimates the effect of high school participation on arrest rates for those who have just completed high school (if they succeed in completing). The cohorts younger than those aged 18 - 19 have not yet participated in high school in period t – 3, and therefore the high school participation in period t - 3 should not affect the arrest rates of younger cohorts. Similarly, the high school participation in period t - 3 should not affect the arrest rates of older cohorts because they had made a high school participation decision before period t -3. In contrast, if any earthquake-related shocks drive the main results, they would also affect the arrest rates of age bands other than 18 - 19.

Table 6 regresses the arrest rates of three different age cohorts 14 - 15, 16 - 17, and 20 or above on high school participation rates. The main results are also reported for comparison purposes. Table 6 indicates that the first-stage estimates are highly statistically significant, while there was no significant effect of high school participation on the arrest rates for cohorts younger or older than those aged between 18 - 19. The reduced-form estimates also confirm that the policy interventions had no significant effect on the arrest rates of age cohorts other than those 18 - 19. Thus, the results do not support the possibility that the main findings are driven by earthquake-related shocks that happened to affect schooling and crime in the disaster areas.

Table 6: Alternative regression specifications

Dependent variable:	% High school		Arrest rates by age cohort			
	First-stage		Reduced-form		IV	
Age cohort	Policy	Std. err.	Policy	Std. err.	% High school	Std. err.
A. 14 - 15						
Violent crime	1.109***	(0.349)	0.030	(0.225)	0.027	(0.189)
Property crime	1.109***	(0.349)	0.340	(0.937)	0.306	(0.775)
B. 16 - 17						
Violent crime	1.109***	(0.349)	0.038	(0.097)	0.035	(0.080)
Property crime	1.109***	(0.349)	-0.456	(0.811)	-0.411	(0.688)
C. 18 - 19						
Violent crime	1.109***	(0.349)	-0.127**	(0.061)	-0.114**	(0.047)
Property crime	1.109***	(0.349)	0.290	(0.264)	0.261	(0.213)
D. 20 or above						
Violent crime	1.109***	(0.349)	-0.001	(0.024)	-0.001	(0.020)
Property crime	1.109***	(0.349)	0.029	(0.053)	0.026	(0.044)

* $p < .10$, ** $p < .05$, and *** $p < .01$. Standard errors are clustered by municipality. First-stage and reduced-form estimates are the estimated coefficients on the treatment dummy 1996. The IV estimates are the estimates of β_1 in equation (1). Panels A to D correspond to the regressions for the arrest rates of individuals aged 14-15, 16-17, 18-19, and 20 or above, respectively. Refer to Table 3 for the controls included in Panel B.

7. Social savings through crime reduction

Because crime generates huge social costs, it would be informative to assess the magnitude of the social benefits brought about by reducing crime through schooling. Firstly, this section computes the estimates of social benefits associated with crime reduction following a one-percentage-point increase in the high school participation rate based on the estimates obtained in Section 5. Secondly, this section addresses an important question for policy making: would a policy to increase police resources yield the same impact at a lower cost?

The social savings estimates presented in this section must be interpreted with the following caveats. The social saving estimates presented in this section must be interpreted with the following caveats. Firstly, general equilibrium effects are not taken into account. For example, increasing the number of high school graduates may increase the number of crimes committed by them to the extent that an increase in the number of high school graduates reduces their wage. Equally, a decrease in the supply of individuals without education at a high school level may raise their wage, which in turn may discourage them from committing crimes. In either case, the social saving estimates would be biased. However, Lochner and Moretti (2001) show that the net general equilibrium effect of increasing schooling on crime is negligible. Note also that the general equilibrium effects potentially bias social saving estimates only if education affects crime through a change in labour market opportunities. If education affects crime due to, for example, a change in risk preferences or better knowledge about the consequences of engaging in crime, the general equilibrium effects would not be relevant to the estimation of social savings.

Secondly, the social savings from crime reduction are estimated as a result of a one-percentage-point increase in the high school participation rate from approximately 96 per cent to 97 per cent. To the extent that there is a decreasing return to schooling, the social savings may be understated. Equally, the social savings may be overstated if the adolescents affected by the policies are those who are most responsive to an increase in schooling in terms of criminal activity. Thirdly, the social benefits following an increase in the high school participation rate are evaluated for a year, although raising high school participation would have a longer-lasting effect than one year. In particular, increasing schooling at an early stage of life not only affects current criminal activity but may also affect future criminal activity as adults. Thus, the social savings estimates may be understated in this regard.

Despite the limitations of the exercise, I compute the social savings to provide a rough notion of how much society can benefit by encouraging the high school participation of adolescents. Because Section 5 shows that high school participation reduces violent crimes but not property crimes, this section computes the social savings due to a reduction in violent crimes. The cost of crime included in the computation is (i) cost for crime prevention, (ii) cost in response to crime (e.g., incarceration costs) and (iii) cost as a consequence of crime (e.g., compensation for damage) as detailed in Appendix B.

Panel A of Table 7 presents estimated changes in arrests and actual crimes as a result of a one-percentage-point increase in the high school participation rate. Because 16 per cent of actual crimes are reported to police and 48 per cent of recorded crimes are cleared up, 33 reductions in arrests implies estimated 449 reductions in actual crimes.[14] Panel B presents the benefits of crime reduction through schooling and indicates that the social savings due to a reduction in crimes is ¥875,612 thousand. A total of 123,411 students went on to high schools in the year 2000 in the sample area, and an additional 1,274 students would have raised the high school participation rate by one percentage point. Because attending high schools costs ¥509 thousand per head annually on average (Ministry of Education, Culture, Sports, Science and Technology, 2002), an additional 1,274 students would have cost approximately ¥648,191 thousand, yielding net social savings of ¥227,421 thousand. One important caveat is that the calculation of the cost of raising the high school participation rate does not include foregone earnings. If foregone earnings of ¥1,000 thousand per annum per head is added to the direct cost of schooling, raising the high school participation rate costs approximately ¥1,930,368 thousand.[15] The net effect of raising the schooling level is

[14]Not every recorded crime results in the arrest of criminals due to, for example, the limited ability of the criminal justice system. Likewise, not every actual crime is reported to police due to, for example, the reluctance of people to report crimes. According to an international crime victimisation survey in 2000, 16 per cent of violent crimes were reported to police in Japan. The low report rate for violent crime is partly due to a low report rate for sex offences, i.e., fewer than 10 per cent of sex offences were reported to police in 2000.

[15]The foregone earnings of those participating in high school are measured by the average annual earnings of workers who did not graduate from high school.

Table 7: Social savings through crime reduction

A: Estimated change in arrests/crimes	
Change in arrests	33
Change in actual crimes	449
B: Benefit of crime reduction through schooling	(¥1,000)
Average cost per crime	1,948
Benefit from 1pp increase in schooling	875,612
Cost of 1pp increase in schooling	648,191
Net benefit	227,421
C: Cost of crime reduction using police forces	(¥1,000/officers)
Average cost per police officer	16,771
Required number of police officers	138
Cost to increase police officers	2,311,028

All costs are inflated to represent 2003 real prices using the Consumer Price Index except for the cost of increasing schooling, which is evaluated at 2000 because an increase in education at period t has an effect on crime at period t+3. Strictly speaking, therefore, the social savings reaped in 2003 must be evaluated by the present discounted value in 2000. However, the interest rates during the periods concerned in Japan were extremely low, ranging from 0.15 to 0.25 per cent. The effect of interest rates is thus omitted in the analysis. Costs and benefits are expressed in ¥1,000.

thus a deficit of ¥1,054,756 thousand. However, it is important to note that crime reduction is one of the numerous effects of schooling, such as a contribution to better health and economic growth.

Panel C presents the cost of crime reduction using the police force. Hiring an additional officer costs an estimated ¥16,771 thousand annually, generating a social savings of ¥6,354 thousand due to crime reduction.[16] To yield a level of social savings equivalent to that resulting from a one-percentage-point increase in the high school participation rate (¥875,612 thousand), an additional 138 police officers are required. An additional 138 police officers cost approximately ¥2,311,028 thousand. Thus, it is less expensive to reach a target level of social benefits by improving schooling (¥648,191 thousand without foregone earnings and ¥1,930,368 thousand with foregone earnings) than by strengthening police forces.

[16] Levitt (1997) reports that an additional police officer imposes non-salary overhead costs of a roughly equal magnitude as salary. The average salary of a police officer in Japan is ¥8,386 thousand (Society for the Study of Pay Structures of Local Government Officials, 2004), yielding the estimated average cost of ¥16,771 thousand for hiring additional officer. Levitt (1997) estimates that violent and property crimes decrease by 3.2 and 1.6, respectively, per additional officer in the United States. Given that violent and property crimes cost estimated ¥1,948 thousand and ¥75 thousand on average, respectively, an additional officer in Japan yields social savings of ¥6,354 thousand under the assumption that the decrease in crime per additional officer is equivalent in the United States and Japan.

8. Conclusions

This paper explores the causal effect of schooling on youth crime. To identify the effect of schooling, I use an exogenous variation in schooling caused by policy interventions in specific municipalities after the Kobe earthquake that hit Japan in 1995. Based on a comparison of the municipalities with and without the policy interventions that were exposed to similar degrees of earthquake damage, I find that the policies raised the high school participation rate in the affected municipalities by approximately 1.109 percentage points. Subsequently, the total number of juvenile arrests for violent crime in the affected municipalities was lowered by approximately 0.114 per 1,000 juvenile population on average, corresponding to approximately a 24 per cent reduction relative to its mean. Because not every crime is reported to police and cleared up, the reduction in arrest rates implies a greater reduction in the actual number of crimes. In contrast, I find no significant reduction in the arrest rates for property crime.

The negative impact of schooling on violent crime guides me to narrow down possible operating mechanisms though which schooling reduces crime. Rational choice models predict that higher educational attainment reduces crime through the higher opportunity cost of committing crimes (Ehrlich, 1975; Huang et al., 2004; Lochner, 2004). This operating path may be plausible because violent crime is typically associated with a longer sentence than property crime, meaning that criminals must pay a higher opportunity cost when they commit violent crimes. The results are also consistent with behavioural economic models, such as those of O'Donoghue and Rabin (2001), suggesting that education may reduce crime by affecting the time or risk preferences of individuals or by enabling them to more accurately compute the cost of engaging in crimes. This path is also plausible because violent crime may be perceived as being more risky than property crime. In addition, because violent crime typically entails a higher associated cost, the ability to accurately compute the associated future cost may discourage adolescents from committing serious offenses. Some literature suggests that more schooling reduces crime because adolescents have less time to be involved in criminal activity (Witte and Tauchen, 1994). However, educational incapacitation is unlikely to be the dominant path for a reduction in crime in the current context because the vast majority of individuals are no longer at high school at the time the arrest rates are observed (cf. equation (1)).

To assess the cost-effectiveness of schooling as a means of reducing crime, I provide estimates of how much society can save by reducing crime through schooling. The results indicate that increasing schooling yields positive net social benefits. Moreover, I find that it is less expensive to reach a target level of social benefits by improving schooling than by strengthening the police force. It is also important to note that schooling has other positive effects on society that would not necessarily occur through strengthening police forces, such as improving health and political awareness and enhancing long-run economic growth

(Barro, 2001; Dee, 2004; Grossman, 2006). Thus, a policy that encourages schooling would be an attractive supplement to the traditional methods of combating juvenile crime that increase formal deterrence.

Appendix

Appendix A. Computation of the number of juvenile arrests

The crime indicator is defined as the number of juvenile arrests per 1,000 juvenile population, where juvenile refers to individuals aged between 18 and 19. The number of arrests is recorded by police office but not by age. I therefore compute the number of juvenile arrests in police office o in prefecture p under the following assumption.

Assumption: the juvenile arrest rate relative to the total arrest rate is constant across offices in prefecture p

$$\frac{arrest_o^j / pop_o^j}{(arrest_o^a + arrest_o^j)/(pop_o^a + pop_o^j)} = \frac{\sum_o arrest_o^j / \sum_o pop_o^j}{\sum_o (arrest_o^a + arrest_o^j)/\sum_o (pop_o^a + pop_o^j)} \tag{2}$$

where the superscripts j and a refer to *juvenile* and *adult*, respectively. The left-hand side corresponds to the "number of arrested juveniles over the juvenile population" relative to the "number of total arrests over the total population" in office o in prefecture p. The right-hand side is the same fraction in prefecture p after aggregating up across o. Underscript p is suppressed from equation (2) for brevity. The assumption essentially means that the tendency of juveniles to commit crimes relative to the whole population is constant across police offices in a prefecture. Equation (2) can be re-arranged to obtain the number of arrested juveniles in police office o in prefecture p:

$$arrest_o^y = \sum_o arrest_o^y \times \frac{(arrest_o^a + arrest_o^y)}{\sum_o (arrest_o^a + arrest_o^y)} \times \frac{\sum_o (pop_o^a + pop_o^y)}{(pop_o^a + pop_o^y)} \times \frac{pop_o^y}{\sum_o pop_o^y} \tag{3}$$

which, in words, means "juvenile arrests in police office o in prefecture p = [total juvenile arrests in prefecture p] × [office o share of total arrests in prefecture p] × [inverse of office o population share] × [office o juvenile population share]". Underscript p is suppressed from equation (3) for brevity. Finally, the number of juvenile arrests in police office o in prefecture p is computed using equation (3), and subsequently is divided by juvenile population to obtain arrest rates.

Appendix B. Calculation of costs per crime

Costs per crime consist of (i) costs for crime prevention, (ii) costs in response to crime (e.g., incarceration costs) and (iii) costs as a consequence of crime (e.g., compensation for damage) as reported in Table 8. Costs for crime prevention consist of the expenditure on crime preventative activities by probation officers (Ministry of Justice, 2007) and does not contain the expenditure on private insurances or other fees borne by individuals to protect themselves. Costs in response to crime consist of incarceration costs, probation costs, lawyers' fees, police activities, subsistence and medical fees for those in custody (Cabinet Office, 2006). Costs as a consequence of crime consist of compensation for damage and legal aid for victims (Cabinet Office, 2008).

Table 8: Costs of crime in Japan

		¥million (2003 prices)
Costs for crime prevention:	Prevention	587
Costs in response to crime:	Incarceration, probation	51,536
	Lawyers fees	7,754
	Subsistence, medicine	9,180
	Police activity	19,226
Costs as a consequence of crime:	Compensation for damage	7,808
	Legal aid for victims	1,247
Total		97,338

To make figures comparable, every figure is adjusted to 2003 values using the Consumer Price Index in case figures for 2003 are not available.

Table 8 indicates that the costs of crime correspond to a total of ¥97,338 million. To decompose the total cost into costs by crime type (i.e., violent crime and property crime), the proportion of cost for each crime type over the total cost obtained from Dubourg et al. (2005) is used as a weight. Because approximately 73 per cent of the total cost of crime was incurred due to violent crime in 2003, it is estimated that ¥97,338 million * 0.73 = ¥71,057 million was incurred due to violent crime. Subsequently, the total cost for violent crime is divided by the total number of violent crimes in 2003 to obtain the cost per violent crime (i.e., ¥71,057 million ÷ 68,246 = ¥1,041 thousand).

Unfortunately, information on costs due to physical and emotional impact on victims and lost output are not available despite the fact that these intangible costs account for a significant proportion of costs per crime particularly for violent crime. For example, intangible costs accounted for approximately 65 per cent and 90 per cent of costs per crime for assault in the United Kingdom and the United States, respectively (Dubourg et al., 2005; Miller et al., 1996). As omitting these costs would severely understate costs per crime, the UK values of the costs associated with mental and physical stress and productivity losses *per* *violent crime* obtained from Dubourg et al. (2005) are used as a proxy for the corresponding values in Japan. Thus, the estimates presented in this section should be interpreted as an approximation rather than the accurate measure of costs per crime. Finally, adding the intangible costs associated with violent crime to the calculation yields the cost per violent crime, ¥1,948 thousand = ¥1,041 thousand (tangible costs) + ¥907 thousand (intangible costs).

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