Identification of Causal Effects using the 1995 Earthquake in Japan

Studies of Education and Health

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Declaration

I declare that the thesis is my own work and has not been submitted for a degree at another university.

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Abstract

This thesis aims to identify causal effects using a natural experimental approach. We focus on the Great Hanshin-Awaji Earthquake in midwestern Japan as a source of exogenous variation in the variables of interest.

Chapter 1 explores the causal effect of schooling on juvenile delinquency using variation in schooling caused by policy interventions in specific municipalities after the earthquake. Using the instrumental variable estimator to address endogeneity problems arising from simultaneity and unobserved heterogeneity, we find that schooling reduces juvenile delinquency, although some of our estimates have large standard errors and are imprecisely estimated. The results indicate that a one-percentage-point increase in the high school participation rate reduces the number of juvenile arrests by approximately 1.1 per 1,000 youths. [1] Estimates of social benefits show that it is less expensive to reach a target level of social benefits by improving schooling than by strengthening police forces.

Chapter 2 studies the causal effect of volunteer work on the mortality of the elderly. After the earthquake, levels of volunteering increased considerably in municipalities hit by the earthquake, while other municipalities did not experience such a sharp increase. This exogenous shift in levels of volunteering is exploited to address the endogeneity problem associated with estimating the effects of volunteering. Specifically, unobserved heterogeneity across municipalities that affects both morality and the level of volunteering, such as the quality of local health care services, may bias estimates on the effect of volunteering. The results indicate that volunteering has no significant effect on mortality amongst people in their 50s and 60s, while it significantly reduces mortality amongst people in their 70s and 80s or older. Evaluated at the mean, the estimate implies that the life of approximately one person aged 80 or older (out of 186 persons) is saved in a given year when the number of volunteers increases by 100 (out of 1,911 persons).

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[1]The unconditional mean of the number of juvenile arrests per 1,000 youths is 12.1.

Preface

During the last few decades, an increasingly large number of studies that use natural experiments to identify causal effects have been conducted in many different subject areas, including those of labour, education and health. Revealing causal effects is useful as it enables us to predict the consequences of changes in policies or current circumstances.

Natural experiments are situations in which causal variables of interest are exogenously shifted by, for example, policy changes or natural forces, which allow researchers to estimate causal effects. Natural-experimental studies use observational data, unlike experimental studies such as randomised trials where the treatment is randomly assigned to subjects. Therefore, if the relevant observational data are already available, natural experiments are a more practical and less time-consuming method of estimating causal effects than experimental studies. Moreover, a natural-experimental approach allows the estimation of causal effects that cannot be conducted in other ways due to, for instance, ethical or practical reasons. For example, Scholte et al. (2012) study the effect of in utero exposure to famine on health and labour market outcomes later in life. Due to the food embargo imposed by German forces during 1944 and 1945, individuals in a certain cohort in the western part of the Netherlands were exposed to famine in utero. Using the Dutch Hunger Winter famine, Scholte et al. (2012) find that in utero exposure to famine causes adverse health and labour market outcomes later in life. Without using a natural-experimental approach, it would be impossible to

examine such an effect.

However, natural-experimental approaches also face statistical issues that must be addressed for casual inference to be reliable. Firstly, as researchers have no control over the assignment of a treatment, the assignment of subjects to treatment and control groups is rarely random. For example, those likely to benefit from the treatment may self-select themselves into the treatment group. It could also be the case that the assignment of the treatment is correlated with some unobservable variables that affect the outcomes of interest. Secondly, there may be other events occurring at the same time as an experimental treatment that also affect the outcome variables, and these effects may be falsely attributed to the treatment. Thirdly, policy changes may occur as a response to changes in outcome variables; in other words, policy changes could be endogenous. Lastly, trends in treatment and control groups can be different, which potentially biases the estimated effects of interest. Finding a suitable comparison group is therefore an essential part of drawing a valid causal inference. Furthermore, given that researchers have no control over the assignment of treatments, it is crucial to understand the origin of the variations used to identify key parameters, and the study must be carefully designed in a way that approximates a real experiment.[2]

In this thesis, we exploit several exogenous shocks induced by the Great Hanshin-Awaji Earthquake to draw causal inferences. The Great Hanshin-Awaji Earthquake occurred in Japan on 17th January 1995, with its epicentre in the southern

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[2]Note that the aforementioned problems are major threats to identification among others and by no means constitute an exhaustive list of potential problems with the natural-experimental approach.

part of the Hyogo prefecture. The square in Panel I of Figure 1 indicates the location of the Hyogo prefecture in Japan, while the cross in Panel II indicates the location of the epicentre within the Hyogo prefecture. The earthquake was recorded at a magnitude of 7.3 and reached the maximum possible intensity of 7 on the Japanese intensity scale in the southern part of the Hyogo prefecture.[3] 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 were evacuated. More than 682,182 houses, factories and shops were destroyed or burnt down, and vital utility services such as water, electricity, gas 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 schoolteachers were injured, and many students lost their houses and guardians. As a result, various relief measures were adopted to support students, teachers and schools.

In Chapter 1, we use exogenous variation in schooling caused by policy interventions in specific municipalities after the earthquake to explore the causal effect of schooling on juvenile delinquency. Individual returns to schooling have been studied extensively, but the social returns to schooling have received less attention. If schooling yields not only individual returns but also social returns – such as a reduction in crime – then the rationale for policies that encourage individual investments in schooling is strengthened. Using the Instrumental Variable

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[3]A seismic intensity of 7 is assigned to quakes strong enough to alter land forms or cause landslides.

Figure 1: Hyogo Prefecture and the Epicentre

I. The Hyogo Prefecture
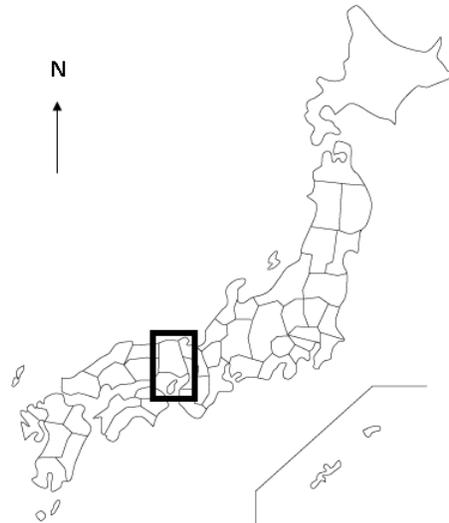
II. The Epicentre in the Hyogo Prefecture

Notes: The square in Panel I indicates the location of the Hyogo prefecture in Japan. The cross in Panel II shows the location of the epicentre within the Hyogo prefecture.

(IV) estimator to address endogeneity problems arising from simultaneity and un-observed heterogeneity, we find that schooling reduces juvenile delinquency, al-though some of our estimates have large standard errors and are imprecisely esti-mated. The results indicate that a one-percentage-point increase in the high school participation rate reduces the number of juvenile arrests by approximately 1.1 per 1,000 youths.[4] Estimates of social benefits show that it is less expensive to reach a target level of social benefits by improving schooling than by strengthening police forces.

Chapter 2 explores the causal effect of volunteer work that provides daily as-sistance to old people on elderly mortality. After the earthquake, the level of volunteering increased considerably in municipalities hit by the earthquake, while other municipalities did not experience such a sharp increase. This exogenous shift in the level of volunteering can be exploited to address the usual endogene-ity problem associated with estimating the effects of volunteering. Specifically, unobserved heterogeneity across municipalities that affects both morality and the level of volunteering, such as the quality of local health care services, may bias es-timates of the effect of volunteering. Additionally, there may be more volunteers in municipalities containing a large number of people with a high risk of mortal-ity, such as the elderly. It is therefore hard to conclude if the level of volunteering affects mortality or if the converse is the case.

The results indicate that levels of volunteering have no significant effect on mortality amongst people in their 50s and 60s, while it significantly reduces mor-

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[4]The unconditional mean of the number of arrested youths is 12.1 per 1,000 youths.

tality amongst people in their 70s and 80s or older. Evaluated at the mean, the estimate implies that the life of approximately one person aged 80 or above (out of 186 persons) is saved in a given year when the number of volunteers increases by 100 (out of 1,911 persons).

Chapter 1

Education and Crime

The welfare of individuals has been traditionally measured by indicators of wealth such as income. During the last decade, however, non-monetary indicators of welfare, such as health and happiness, began to receive more attention (Centre for Educational Research and Innovation, 2010). Although education is likely to contribute to the non-monetary welfare of individuals, research on the effect of education on non-monetary outcomes is relatively scarce. In Chapter 1, among various non-monetary outcomes, we examine 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 1.1 shows that spending on public order and safety corresponds to more than one per cent of gross domestic product (GDP) in several OECD countries

and more than two per cent of GDP in the United States and the United Kingdom in 2004 (UK Cabinet Office, 2006). 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).

A conventional policy to reduce crime is to increase the severity of punishments or police resources. The effectiveness of these conventional policies, however, 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. Reducing crimes by improving education may, however, help to 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, education contributes to better health and political awareness and also improves long-run economic growth (Barro, 2001; Dee, 2004; Grossman, 2005). These effects may in turn indirectly contribute to crime reduction.

Figure 1.1: Spending on Public Order and Safety in OECD Countries
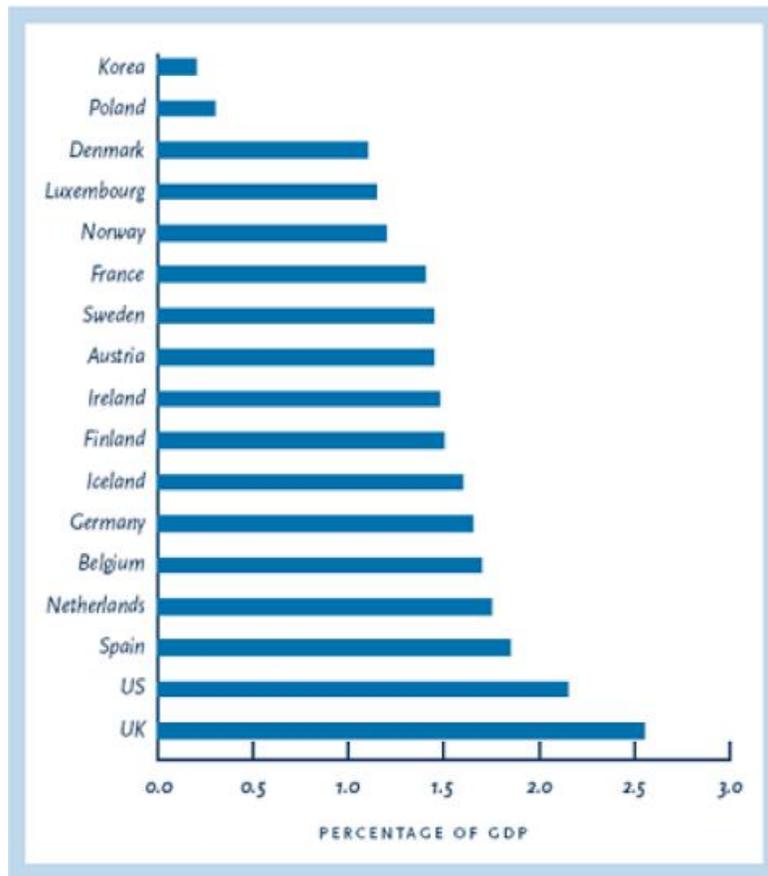
Notes: The graph indicates spending on public order and safety as percentage of GDP in 2004.

Despite the enormous potential of education as a crime-reduction method, empirical studies on the effect of education on crime are scarce. Statistical evidence suggests 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 & 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. In Chapter 1, we aim to establish this causation from a quantitative standpoint. Additionally, we compute social benefits brought about by reducing crime through education.

To identify the causal effect of schooling on crime, we exploit exogenous variation in schooling caused by governmental support granted to students, teachers and schools in specific municipalities after the earthquake.[1] Using exogenous variation is important because the causal variable considered, schooling, is potentially endogenous. Specifically, unobserved heterogeneity across municipalities, such as a local labour market condition, may affect local education and crime lev-

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[1]It is worth noting the work of Cipollone & Rosolia (2007) because their estimation technique is relevant to this chapter. They focus on the effect of the school achievement of boys on that of girls using a policy intervention triggered by an earthquake that occurred in Southern Italy in 1980 as an instrument for the school achievement of boys. The implemented policy allowed certain cohorts of males in specific towns affected by the earthquake to be exempt from military service. Their work is based on town cohort-level analysis, and they find that an increase in male high school graduation rates significantly increases female graduation rates.

els at the same time. People living in a high-crime area may not invest much in education because they obtain a higher return from participating in crime than from working in a formal sector. If this is the case, the crime level can be negatively correlated with the educational level even if there is no causal effect of education on crime. Based on the comparison of the arrest rates between the municipalities with and without the policy intervention, our results indicate that higher school attainment levels reduce the number of crimes committed by youth.

The reminder of the chapter is structured as follows. Section 1.1 surveys the theoretical and empirical literature on the relationship between education and crime, while Section 1.2 explains educational policies and their effects on schooling. The identification strategy is discussed in Section 1.3, and definitions of variables and data sources are presented in Section 1.4. In Section 1.5, we present the econometric specification and discuss empirical findings, and examine their robustness in Section 1.6. As crime generates huge social costs, it is interesting to know how much social benefits are brought about by reducing crime. In Section 1.7, we thus compute estimates of social benefits due to reductions in crime through education. Finally, Section 1.8 concludes the chapter.

1.1   Literature Review

In this section, we first present the theoretical literature exploring the potential mechanisms of the effect of education on crime. Subsequently, we offer a selected survey of the empirical literature on the relationship between education and crime.

1.1.1 Theoretical Literature

Rational Choice Model

Since the pioneering work of Becker (1968), which uses economic theory to model criminal behaviour, a large number of studies have been conducted on the economics of crime (Block & Heineke, 1975; Bourguignon et al., 2003; Ehrlich, 1973). Becker's (1968) work is based on the assumption that individuals are rational, and it predicts that individuals choose to commit a crime when the returns exceed the costs of participating in crime.

Nearly a decade later, Ehrlich (1975) explicitly analysed the role of education in affecting criminal behaviour within a rational choice model framework. Ehrlich (1975) regards education as an efficiency parameter that affects the returns from both legal activity and crime. More education increases the returns from both legal activity and crime. The effect of education on crime thus depends on the relative size of an increase in the returns from legal activity and crime. If education increases the return from legal activity more than that from crime, ceteris paribus, more education would reduce an individual's incentive to engage in crime. Therefore a key element that determines the effect of education on crime is the difference in the extent to which education affects the returns to legal activity and crime.

The ideas of Ehrlich (1975) are formalised by other researchers who added a micro-foundation to the aforementioned intuitions (Huang et al., 2004; Lochner, 2004). For example, Lochner (2004) developed a two-period life-cycle model of

crime to investigate the interaction among crime, formal employment and educational choices. In his model, individuals with more education commit fewer unskilled crimes because they earn higher income in the formal labour market. In other words, their opportunity costs from forgone work are higher, as are their expected costs of incarceration.

Behavioural Economic Model and Others

The criminal behaviour of youths is also analysed from perspectives other than the rational choice framework presented above. Embracing insights from psychology, O'Donoghue & Rabin (2001) cast doubt on the presumption of the rational choice model that an individual always behaves in her own best interest. Their model allows the possibility of an individual to make a mistake and argues that excessive myopia and projection bias play a role in explaining juvenile delinquency. Participation in crime involves a trade-off between short-run benefits (e.g., a monetary gain) and long-run costs (e.g., losing a job opportunity in the future). An excessively myopic individual pays too little attention to the future consequences of her current action and thus commits a crime. Furthermore, an individual may underestimate the cost of future consequences of his current action and behave in a way that does not maximise his true well-being. O'Donoghue & Rabin (2001) argue that education may be a useful tool to reduce juvenile delinquency by providing youth with information about the risks involved in their current behaviours. For example, raising perceptions about the cost of being arrested or jailed may help to reduce youth crime.

In line with O'Donoghue & Rabin (2001), Witte (1997) argues that education may change the time preferences of an individual or provide her with the information about "rules of the game", in other words, what is moral or legal. More education may thus affect criminal behaviour by enabling her to judge what is moral or to compute accurately the cost of engaging in crime. Witte (1997) also points out a possible indirect effect of education on crime: more time spent in school may provide an individual with less crime prone friends, making the individual less crime prone. This channel would be more relevant to crimes committed due to peer pressure.

In summary, every piece of theoretical literature makes the same prediction: education reduces crime. To investigate whether these theoretical prediction is supported by empirical evidence, several attempts have been made to empirically examine the relationship between education and crime.

1.1.2 Empirical Evidence

Descriptive Studies

Witte & Tauchen (1994) focus on young male adults living in Philadelphia and estimate the effect of high school graduation and time spent in school on the probability of arrest. Their results from a probit model suggest that high school graduation status is not significantly associated with the probability of arrest, whereas the proportion of time allocated to school during each year is negatively associated with the probability of arrest.

Using data on males aged between 14 and 21 from the 1980 wave of the National Longitudinal Survey of Youth (NLSY), Grogger (1998) estimates the effect of years of education and high school graduation status on property crimes. The results from a probit model suggest that neither educational measure is significantly associated with property crimes.

Lochner (2004) estimates the effect of high school graduation status on the crimes committed by young male adults using the same data as Grogger (1998). In their specification, the observable variables such as test scores in mathematics and verbal ability, and family background are controlled for. These variables are expected to capture the individual characteristics (e.g., learning ability) that potentially affect criminal behaviour. If these variables do not fully capture the effect of the individual characteristics on crime, the Maximum Likelihood estimator would be biased. Using a probit model, Lochner (2004) finds that high school graduation status is negatively correlated to both property and violent crimes.[2]

Studies identifying Causal Effects

The negative association between education and crime shown in some early literature does not necessarily mean education reduces crime. Firstly, unobserved heterogeneity across individuals such as a variation in learning ability may affect

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[2]It is rather surprising that Grogger (1998) and Lochner (2004) show opposite results using the same data. It could be due to slightly different definitions of a crime indicator for property crimes, i.e., Grogger (1998) uses the income-based crime participation dummy whereas Lochner (2004) uses criminal income and self-reported criminal participation as indicators of property crimes. In addition, Grogger (1998) does not control for cognitive ability and therefore his estimates may be biased.

the decisions on both crime participation and human capital investment. For example, an individual with a high learning ability investing intensively in education may participate in crime less. If it is the case, education and crime show a spurious negative correlation even if education does not "cause" crime to reduce. Secondly, the decisions on the participation in crime and human capital investment may be simultaneously determined. On one hand, education may reduce crime due to, for example, an increased opportunity cost of committing a crime. On the other hand, the more an individual spends time committing a crime, the less the individual may want to spend time to invest in education due to, for example, the increased probability of being arrested before the benefits of education can be reaped. In order to address the potential endogeneity issues, recent literature has started using IVs for education to estimate its causal effect on crime.

Lochner & Moretti (2004) is the first paper that attempts to draw inferences on the causal effect of education on crime. They address the endogeneity of schooling using the time variation in the number of years of compulsory schooling at state level as an instrument for schooling. Based on the US census panel data, the first analysis finds that years of schooling reduce the probability of incarceration of male adults, while high school graduation status does not have a significant effect on the probability of incarceration. A limitation of the analysis using incarceration status is that it does not allow estimating the effect of schooling on the minor offenses unlikely to result in incarceration (e.g., shoplifting). Moreover, it does not allow estimating the effect on different categories of crimes.

In order to overcome these issues, based on the US census and FBI crime

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report state level panel data, Lochner & Moretti (2004) estimate the effect of the average education and the high school graduation rate on the arrest rates of male adults by crime category. Using the time variation in the number of years of compulsory schooling as an instrument, Lochner & Moretti (2004) find that both the average education and the high school graduation rate reduce the arrest rate, though the IV estimates are not always precisely estimated. The effect of schooling varies considerably with crime categories: for instance, another year of education reduces assault by about 29 percent and robbery by about 0.5 percent. The analysis, however, may be overestimating the effect of schooling to the extent that more educated individuals are less likely to be incarcerated or arrested.

In order to address this possibility, based on the NLSY cross-sectional data, they estimate the effect of self-reported years of schooling and high school graduation status on self-reported participation in crime and the experience of being incarcerated for young male adults. Instead of using an instrument to address the possibility of endogeneity, test scores of mathematics and verbal ability, and family backgrounds are included into the analysis to capture the unobserved heterogeneity across individuals. The results indicate that both self-reported years of schooling and high school graduation status significantly reduce the probability of having an experience of being incarcerated. The results for self-reported participation in crime are mixed: years of schooling and high school graduation status are negatively associated with crimes for white males, whereas higher self-reported schooling is associated with higher crime for black males.

Following the work of Lochner & Moretti (2004), other empirical work ex-

ploiting IVs is conducted based on the aggregated data. Endogeneity of education is still a concern since the aggregate level unobserved heterogeneity such as a local labour market condition may simultaneously affect local educational and crime levels. For example, people living in a high crime area may not invest much in education since they get a higher return from drug trafficking than working in a formal sector, which may cause a spurious negative correlation between education and crime.

Buonanno & Leonida (2006) attempt to address a potential endogeneity issue using the system generalised method of moments estimation where the lagged values of a measure of schooling are used as instruments. Based on regional level panel data of Italy from 1980 to 1995, they find that average years of schooling reduce the recorded crime rate for property crimes: a one year increase in average schooling significantly reduces the property crime rate by 0.9 percentage points. The effect of another year of schooling is smaller for the total crime rate (that contains both property and violent crimes) and less precisely estimated. Although the effect on violent crimes is not reported, this would imply that schooling does not have as much effect on violent crimes as property crimes (if any).

Machin et al. (2011) estimate the effect of education on crime convictions using regression discontinuity design. Their analyses are based on data on adults in England and Wales from 1946 to 1970 from the Offenders Index Database. Using an exogenous variation in education caused by a change in compulsory school leaving age law in 1972, they find that an increase in the average age of leaving school reduces the property crime conviction rate for males. Likewise,

a decrease in the proportion of people with no educational qualifications reduces the property crime conviction rate for males. For example, a one percentage point decrease in the proportion of people with no educational qualifications reduces the property crime conviction rate by 0.85 percent. Such a relationship is, however, not observed for the violent crime conviction rate and the conviction rates of every type of crimes for females.

Although previous literature uses different functional form assumptions, different measures of crime, and data on different countries, education is found to reduce property crimes, while its effect on violent crimes is mixed.

1.2 Policy Context

To identify the causal effect of schooling, we exploit exogenous variation in schooling caused by governmental support granted to students, teachers and schools in specific municipalities after the Great Hanshin-Awaji earthquake. This section explains educational policies and their effects on schooling.

1.2.1 Policies in Focus

Shortly after the earthquake, the government defined disaster areas corresponding to ten cities and ten towns in the Hyogo prefecture and five cities in Osaka prefecture shown in Figure 1.2. The government applied the Disaster Relief Act (DRA) to these municipalities and targeted these areas for providing various forms of

Figure 1.2: Disaster Relief Act

Notes: The shaded area indicates ten cities and ten towns in the Hyogo prefecture
and five cities in the Osaka prefecture where the Disaster Relief Act was applied.

support.[3] Firstly, the government provided textbooks and school supplies to junior high school students in need of emergency assistance due to the earthquake. The provision took place from January 1995 to March 1995, and 62,034 students received textbooks and school supplies.[4]

Secondly, schoolteachers and their families were also targeted for relief. Schoolteachers and their families whose (i) residences were more than 20 per cent destroyed in

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

[4]The figure includes the number of junior high school students and primary school pupils.

terms of the total area or (ii) family members were dead or injured were exempted from paying medical bills. As a result, 10,802 exemptions took place between January 1995 and May 1995.[5]

Finally, schools in the disaster area were also financially supported. Junior high schools in the disaster area were subsidised to construct temporary classrooms and reconstruct school buildings. The construction of temporary classrooms started in February 1995, and a total of 394 classrooms and 255 school buildings for junior high schools were (re)constructed. Schools were eligible to receive the subsidies if the total repair or construction costs per school were above certain thresholds.[6]

1.2.2 Potential Effects of the Policy Intervention

To understand the effect of the policies on schooling measured by the high school participation rate, it is necessary to understand the Japanese educational system: after completing primary school at age 12, every pupil must go on to junior high school, which lasts for three years. After graduating from a junior high school, students can choose whether to go on to high school. The policies are, therefore, expected to have raised the high school participation rate (or to have prevented it

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[5]A total of 1,877 exemptions were recorded for schoolteachers working for private school, whereas the number of exemptions for schoolteachers working for public schools is unknown. Hence, we compute the number of exemption for public school teachers assuming that the number of exemptions per school is identical in private schools and in public schools.

[6]Threshold repair/construction costs are ¥400k, ¥600k, ¥800k and ¥1,500k for civic, national prefectural and private schools, respectively. Subsidy rates also vary with types of schools: the rate is 100 per cent for national schools, 67 per cent for prefectural and civic schools and 50 per cent for private schools. Although this policy does not have a clearly defined target area, schools that received subsidies concentrate on municipalities where the DRA was applied.

Figure 1.3: Timing of the Policy Intervention
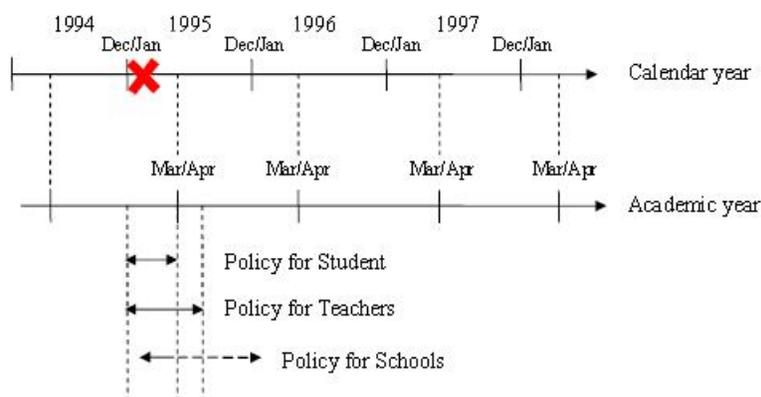
Notes: The top arrow indicates the calender year, while the second top arrow shows the academic year starting from April and ending in March. The cross points the timing at which the earthquake occurred.

from dropping) by mitigating shocks of the earthquake on schooling.

We now discuss the timing of the effect of the policy intervention on schooling. The governmental support for (i) students, (ii) teachers and (iii) schools was respectively provided from (i) January 1995 to March 1995, (ii) January 1995 to May 1995 and (iii) February 1995 until the (re)construction of school buildings was completed. Figure 1.3 summarises the timing of the policy interventions. The top arrow indicates the calendar year, while the second arrow from the top 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 schools at the time of the earthquake were poten-

tially affected by the policy intervention. Recalling that a junior high school is a three-year course, students affected by the policies went on to high schools (if they choose to) in April 1995, 1996 or 1997. The policies therefore potentially affected the high school participation rate in years 1995, 1996 and 1997.

1.3 Identification Strategy

The nature of the policies outlined in the previous section raises several empirical concerns. Namely, the set of municipalities eligible to receive the governmental support (hereafter, "supported group" for brevity) suffered the damage of the earthquake at the same time, implying that not only the policies but also the damage must have affected schooling. To capture the effect of the policies on schooling, it is thus essential to disentangle the effects of the policies and damage on schooling.

For this purpose, we drop a subset of municipalities in the supported group that were heavily damaged and limit our attention to the municipalities least damaged by the earthquake. The damage of the earthquake is measured by the percentage of people who died or were injured and households that had their houses destroyed. A municipality is classified as heavily damaged if one or more damage measures were recorded above the top 25th percentile value of damage distribution within the supported group. Given that the distribution of damage is highly negatively skewed, this rule drops the most damaged municipalities. As a result, 31 municipalities are retained as the treatment group, accounting for 0.001 per

cent of the people died, 0.003 per cent of people seriously injured and 0.12 per cent of households that had their houses fully destroyed. In fact, although the retained municipalities were officially included in the disaster area and targeted by the policies, a large number of the retained municipalities recorded little damage.

To capture the effect of the policy intervention on schooling, school achievement in the treatment group could be compared to the rest of municipalities not eligible to receive any support (hereafter, "non-support group" for brevity). The municipalities in the non-support group were, however, presumably exposed to less damage than the municipalities in the treatment group. The difference in degrees of damage may bias the estimates of the treatment effect for several reasons.

Firstly, different degrees of damage imply different assignments of other support related to the earthquake. To the extent that other support also affected schooling, estimates of the effect of the policy intervention on schooling would be biased. Secondly, it might be the case that higher damage in a municipality led to less labour market opportunity for the youth, which in turn encouraged the students in the highly damaged area to go on to high school. Equally, it could be the case that higher damage created more job opportunities for, for example, construction work, which encouraged the students in the highly damaged area to enter the labour market. Either way, the estimates of the effect of policy intervention would be biased. Thirdly, it is likely that school facilities in the treatment group were more damaged, resulting in underestimation of the effect of the policies.

To address these concerns, we restrict our attention to a subset of municipalities in the non-support group that lie within similar distances from the fault

line to the municipalities in the treatment group. We measure the orthogonal distances from the fault line to each municipality in the treatment group and use the maximum distance as the standard distance. Every municipality lying within the standard distance from the fault line without the policy intervention is selected as a control municipality. This rule ensures that treatment and control municipalities lie within similar distances from the fault line and selects 32 municipalities as a control group. The final sample thus consists of 63 municipalities in the Hyogo and Osaka prefectures as presented in Figure 1.4. The cross indicates the epicentre, and the faults that caused the earthquake are located along the line lying from the southwest to the northeast. Not surprisingly, municipalities lying along the faults that recorded the high level of damage are dropped from the sample. The retained municipalities suffered the similar degrees of damage, which has important implications to the identification of the treatment effect.

Firstly, a change in youth labour market conditions according to the different degrees of damage is taken into account. As municipalities in treatment and control groups suffered similar degrees of damage, the effect of the earthquake on youth labour market conditions (if any) would have been similar. Secondly, similar degrees of damage imply similar assignments of other earthquake-related support. For example, several cash-transfer schemes for victims were implemented in the disaster area. These schemes either gave or loaned money to victims to support the recovery of their lives and distributed money according to the degrees of damage that victims suffered. These cash-transfer schemes would have affected the two groups similarly because they suffered similar degrees of dam-

Figure 1.4: Sampled Municipalities

Notes: The cross is the epicentre and the line lying from the southwest to northeast indicates the faults that caused the earthquake. The black and grey areas correspond to the treatment and control groups, respectively.

	Treatment		Control	
	31 municipalities		32 municipalities	
	Mean	Std. Err.	Mean	Std. Err.
# of Deaths per 1,000 pop	0.01	0.00	0.00	0.00
# of Injury per 1,000 pop	0.20	0.06	0.09	0.04
# of Destroyed hh per 1,000 hh	13.98	6.85	0.06	0.02

Notes: Table reports weighted means. Weights are total population in the first and second rows, and total households in the third row.

age. Lastly, similar degrees of damage imply similar degrees of destruction in schooling, which addresses potential underestimation of the treatment effect on schooling.

Table 1.1 summarises the extent of the damage from the earthquake in the treated and control municipalities. Table 1.1 indicates that more households had their houses damaged in the treatment group. Detailed data on the extent of damage, however, shows that a large number of households in the treatment group experienced minor damage as opposed to the full destruction of houses. Regarding loss of life and injury, very few people died or were injured in the two groups, implying that presumably the direct effects of the earthquake were very limited.

Panel I of Figure 1.5 plots the fraction of junior high school students who went on to high school, while Panel II plots the number of juvenile arrests per 1,000 youths. The solid and dotted lines represent the treatment and control groups, respectively, and the years between two vertical lines correspond to the years in

Figure 1.5: Schooling and Crime

I. High School Participation Rates
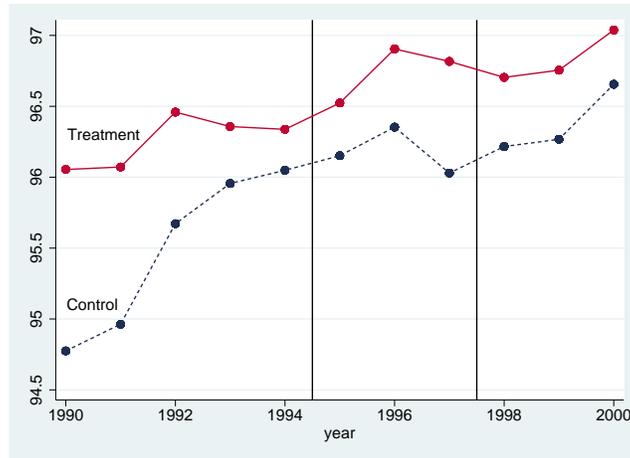
II. Juvenile Arrest Rates (per 1,000 youths)
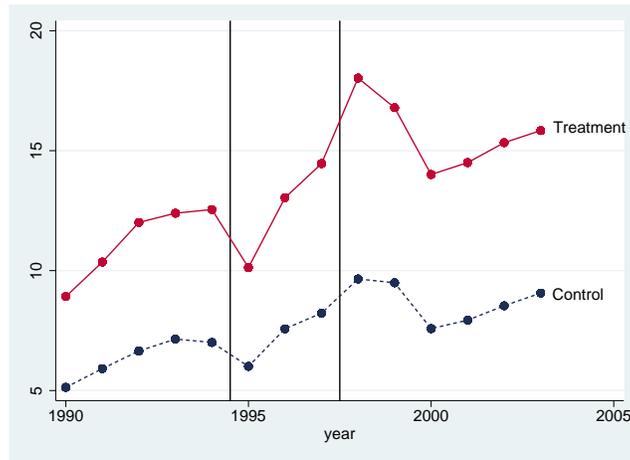
Notes: Panel I plots the weighted average high school participation rates. Panel II plots the weighted average youth arrest rates. Weights are the number of junior high school graduates for Panel I and youth population for Panel II. Solid line: least damaged municipalities eligible to receive educational support (treatment group). Dotted line: municipalities without the educational support lying within the similar distances from the fault line to the treated municipalities (control group).

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which the high school participation rate is potentially affected by the policy intervention. Panel I indicates that the average high school participation rates of both groups show upward trends. Particularly, the upward trend in the control group is steeper prior to the earthquake, leading to underestimation of the effect of the policies on schooling. After the earthquake, the average high school participation rate increased in the treatment group in years 1995 and 1996. Moreover, the high school participation rates fell more in the control group in year 1997. Panel I supports the possibility that the policies raised the high school participate rate or prevented it from dropping in the treatment group.

The graph of the juvenile arrest rates shows slightly steeper upward trends in the treatment group prior to the earthquake and during the period potentially affected by the policy intervention, which may underestimate the effect of schooling on arrests. Note that the fact that the arrest rate is higher in the treatment group does not invalidate the identification because the systematic differences in the average arrest rates are picked up by the municipality fixed effects in the econometric specification.

1.4 Data and Variables

Crime data for 115 police offices in the Hyogo and Osaka prefectures from 1993 to 2003 are taken from the Statistical Crime Report 1993 to 2003 (*Hanzai Tokeisyo*). The crime indicator is defined as the number of arrested youths per 1,000 youths. An issue is that the number of arrests is recorded by police office but not by age.

The number of arrested youths in a police office in a prefecture is thus computed under the assumption that the tendency of the youth to commit a crime relative to the tendency of the whole population (i.e., youths and adults) to commit a crime is constant across offices in a prefecture. Details of the computation are presented in data appendix 1.

Crimes are divided into four categories: total crime, felony, total theft and non-trespass theft. Total crime includes every act in violation of the criminal law. Felony consists of murder, robbery, rape and arson. Total theft consists of three different types of thefts: (i) trespass theft, defined as the unlawful taking of property from the possession of another by entering a structure to commit a theft, and (ii) non-trespass theft, defined as vehicle thefts and any theft except trespass theft (e.g., motorbike theft and luggage lifting).

Data on covariates is available for 163 municipalities in the Hyogo and Osaka prefectures from 1993 to 2003. As the data for crimes and covariates have different units of observations, these data are matched in the manner detailed in data appendix 2. As a result of matching, the number of municipality-police office pairs becomes 94. After retaining in the sample only the municipalities lying within the similar distances from the faults as described in Section 1.3, our sample consists of observations for 63 municipality-police office pairs in the Hyogo and Osaka prefectures from 1993 to 2003.

Data on the high school participation rate, defined as the share of junior high school graduates who go on to high schools, is obtained from the School Basic Survey Report 1990 to 2000 (*Gakko Kihon Chosa Hokokusyo*). The survey

covers every school in Japan and collects information on the number of classes, students and teachers, budget of a school, careers of graduates, etc. The survey has been conducted every year since 1948 and is a good source of community-level information on schooling.

Other data on covariates is 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 conditions of municipalities. The survey has been conducted since 1976 and provides 1,500 social and demographic variables by municipality. As socio-economic indicators, wealth level, job opportunity and welfare generosity are included. Wealth level is measured by income per capita, and job opportunity is measured by the unemployment rate. Poverty and less job opportunity would reduce expected income from legal activities and hence is expected to raise the level of crime. Welfare generosity, measured by welfare expenditure per capita, is expected to reduce the level of crime by providing people with an extra income.

Table 1.2 summarises the pre-quake characteristics of the treatment and control municipalities.[7] The number of arrests per 1,000 youths in Panel I indicates that there are more arrests in treatment municipalities in every crime category. The higher arrest rate per se does not cause bias as long as the pre-quake trends are similar in two groups. The graph of the juvenile arrest rate in Panel II of Figure 1.5 shows the similar trends prior to the earthquake. In the econometric specification, the systematic differences in the average arrest rates are picked up

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[7]The pre-quake period refers to year < 1995.

Table 1.2: Pre-Quake Municipality Characteristics

	Treatment		Control	
	31 municipalities		32 municipalities	
	Mean	Std. Err.	Mean	Std. Err.
I. # Arrests per 1,000 youth population				
Total crime	12.47	1.21	7.08	0.91
Felony	0.44	0.60	0.22	0.23
Total theft	6.84	0.72	4.36	0.54
Non-trespass theft	5.84	0.65	3.64	0.47
II. Covariates				
% High school	96.24	4.86	95.44	6.98
% Unemployed	5.59	0.48	4.05	0.57
Income per cap	1564.00	125.94	1506.32	177.95
Social welfare expend. per cap	24.78	1.59	14.29	1.68

Notes: Panel I reports weighted means using youth population as a weight. Income and social welfare per capita are the average annual values in ¥1,000. Panel II reports weighted means using total population as a weight except the high school participation rate where the number of junior high school graduates are used as a weight.

by the municipality fixed effects. Schooling and socio-economic characteristics shown in Panel II indicate the higher school attainment, the unemployment rate, income per capita and social welfare expenditure per capita in the treatment municipalities. However, the differences in the means are not statistically different from zero for the high school participation rate and income per capita. In contrast, the unemployment rate and social welfare expenditure are statistically different from zero, which potentially biases the results. The differences in unemployment rate and per capita social welfare expenditure are, however, statistically constant across pre- and post-earthquake periods, implying that these differences are absorbed by the municipality fixed effects.

1.5 Specification and results

We now present the specification of the model and discuss the findings. This study investigates the effect of schooling on juvenile delinquency by regressing a measure of juvenile crime on a measure of schooling, controlling for various socio-economic indicators. The following model is set up:

$$Crime_{mt} = \alpha School_{mt-3} + X'_{mt}\beta + v_m + \mu_t + u_{mt} \tag{1.1}$$

where $Crime_{mt}$ represents the number of juvenile arrests per 1,000 youths municipality m in year t (hereafter, "the arrest rate" for brevity), where "youths" refers

to individuals aged between 18 and 19. $School_{mt-3}$ is the fraction of junior high school students who went on to high school in year $t-3$. The high school participation rate enters equation (1.1) with a three-period-lag because an individual who entered high school in year $t-3$ graduates in year t at age 18.[8] β is the vector of parameters for the time varying covariates, X_{mt}. ν_m and μ_t are the municipality and year fixed effects, respectively, and u_{mt} represents the disturbance term.

The main coefficient of interest is α, which measures the effect of schooling on crime. An econometric issue in the estimation of equation (1.1) is that schooling is potentially endogenous. Specifically, unobserved heterogeneity across municipalities, such as a local labour market condition, may simultaneously affect local educational and crime levels . People living in a high crime area may not invest much in education because they get a higher return from participating in crime than working in a formal sector. If this is the case, α can be negative even if there is no causal effect of education on crime.

We begin by estimating equation (1.1) using the WG estimator to deal with a possible correlation between the high school participation rate and the municipality fixed effects, ν_m. Table 1.3 presents the WG estimates of regressions in which the arrest rate is regressed on the high school participation rate and other controls between 1993 and 2003.[9] The WG estimates in Table 1.3 indicate that the

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[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 $x \geq 4$. A crime committed by those aged 20 or above is classified as an adult crime according to Japanese criminal law. Since our interest lies in estimating the effect of schooling on juvenile delinquency, we use equation (1.1) where the measure of schooling enters with a three-period-lag.

[9]Note that the arrest rate reflects not only occurrences of crime but also the effort and resources of police, as not every criminal is arrested. The arrest rate thus does not necessarily accurately reflect the actual level of crime. However, the effort level of police in a municipality is unlikely

Table 1.3: Effects of Schooling on Juvenile Delinquency (WG estimates)

Dependent variable	# of juvenile arrests per 1,000 youths			
	(1)	(2)	(3)	(4)
Crime category	total crime	total theft	felony	non-trespass
% High school	-0.114	-0.036	-0.039*	-0.038
	(0.170)	(0.089)	(0.020)	(0.083)
Controls	yes	yes	yes	yes
Municip. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
# of Obs.	693	693	693	693
# of Municp.	63	63	63	63
R-squared	0.318	0.239	0.318	0.246

Notes: Table reports estimated coefficients on α in equation (1.1). Standard errors in parentheses are clustered by municipality. * denotes $p<0.1$. Labels of the dependent variables indicate the following. Total crime: any act in violation of the criminal law; total theft: any kind of theft; felony: homicide, robbery, arson and rape; non-trespass theft: vehicle theft and the theft without entering a structure. Controls included are the municipality fixed effects, year fixed effects, logged income per capita, unemployment rate and logged welfare expenditure per capita.

high school participation rate is negatively associated with every type of crime, although its effects are statistically insignificant except for felony. Column (3) indicates that a one-percentage-point increase in the high school participation rate is associated with an approximately 0.04 decrease in the number of juvenile felony arrests per 1,000 youths.

The WG estimator is inconsistent if the disturbances, u_{mt}, and the high school participation rate are correlated. A consistent estimator for α can be obtained by using the IV estimator that requires an instrument for schooling. This chapter uses an exogenous variation in schooling caused by the policy intervention outlined in Section 1.3. We begin by estimating the first-stage equation of equation (1.1) where the high school participation rate is instrumented with the educational policies, dummies that equal one if municipality m belongs to the treatment group and year is 1995, 1996 or 1997, and zero otherwise. The treatment dummies reflect the effect of the policy intervention on the high school participation rate and are therefore expected to be positive.

Table 1.4 reports the first-stage estimates of the effect of the policy intervention. Column (1) controls for the municipality and year fixed effects, while Column (2) additionally controls for the socio-economic indicators. The first-stage results in both columns show that the high school participation rate in the treatment group was lower in 1995, although the effect is insignificant. This is not

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to sharply change over time, in which case the effort level is captured by the municipality fixed effects. The amount of police resources in a municipality is more likely to change over time depending on the economic condition of the municipality. We therefore control for income level in the regression equation.

Table 1.4: Effects of Educational Policies on the High School Participation Rate

Dependent variable: High school participation rate			
	(1)	(2)	(3)
Treatment 1995	-0.270	-0.255	-
	(0.162)	(0.165)	-
Treatment 1996	0.086	0.107	-
	(0.134)	(0.139)	-
Treatment 1997	0.371***	0.393***	0.409***
	(0.151)	(0.153)	(0.150)
Controls	no	yes	yes
Municip. FE	yes	yes	yes
Year FE	yes	yes	yes
# of Obs.	693	693	693
# of Municp.	63	63	63
R-squared	0.794	0.796	0.795

Notes: Standard errors in parentheses are clustered by municipality. *** denotes $p<0.01$. Treatment 1995, Treatment 1996 and Treatment 1997 are dummies that equal one in 1995, 1996 and 1997, respectively, in the treated municipalities, and zero otherwise. Controls included are the municipality fixed effects, year fixed effects, logged income per capita, unemployment rate and logged welfare expenditure per capita.

surprising given that the earthquake occurred in mid-January 1995 and the high school participation rate was observed at the end of March, implying that presumably not enough time had passed since the policy was implemented and therefore the disaster area was still in turmoil. Columns (1) and (2) indicate that high school participation rates were higher in the treatment group in 1996 and 1997. The estimated treatment effect in 1997 is statistically significant at one per cent, while the estimated effect in 1996 is insignificant. Since the IV estimator with uninformative instruments is known to be biased toward OLS, we do not use treatment dummies 1995 and 1996 as instruments given that they do not have any predictive power. After omitting treatment dummies 1995 and 1996 from the first-stage equation, the coefficient on treatment dummy 1997 is 0.409 with a t-statistic of 2.73 (F-statistic 7.5). The coefficient estimate implies that the high school participation rate was higher in treated municipalities by approximately 0.409 percentage points in 1997.

Finally, Table 1.5 presents the second-stage estimates of the effect of schooling on the arrest rate using treatment dummy 1997 as an instrument. Columns (1) to (4) report the effect on the arrest rate for total crimes, total theft, felony and non-trespass theft, respectively, controlling for the municipality and year fixed effects. Columns (1) to (4) indicate that the high school participation rate reduces the arrest rate for every type of crime. Columns (1) to (4) are correctly specified if every difference across municipalities is time invariant. If the time varying observable characteristics also affect the arrest rate, however, columns (1) to (4) are misspecified.

Table 1.5: Causal Effects of Schooling on Juvenile Delinquency (IV estimates)

Dependent variable	# of arrested youths per 1,000 youth population							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Crime category	total crime	total theft	felony	non-trespass	total crime	total theft	felony	non-trespass
% High school	-1.064	-1.339	-0.132	-1.290	-1.086	-0.989	-0.216	-1.017
	(1.278)	(1.042)	(0.119)	(1.036)	(1.120)	(0.852)	(0.138)	(0.878)
Controls	no	no	no	no	yes	yes	yes	yes
Municip. FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
# of Obs.	693	693	693	693	693	693	693	693
# of Municp.	63	63	63	63	63	63	63	63

Notes: Table reports estimated coefficients on α in equation (1.1). Standard errors in parentheses are clustered by municipality. Refer to Table 1.3 for labels of crime category. Controls included in the analysis are the municipality fixed effect, year fixed effect, logged income per capita, unemployment rate and logged welfare expenditure per capita.

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Another possible issue with columns (1) to (4) is a selection bias. Namely, the assignment of municipalities to a treatment group might be endogenous. For example, poor municipalities with fragile infrastructures may be severely damaged by the earthquake and, consequently, more likely to be targeted by the policies. The level of crime in those poor municipalities may be high due to few opportunities in the formal labour market, for example. If that is the case, estimates of the effect of schooling on the arrest rate would be biased. It is therefore essential to control for socio-economic characteristics in addition to the municipality fixed effects.

To allow for the possibility that time varying socio-economic characteristics affect the arrest rate and also to account for a possible selection bias, we control for income, unemployment rate and social welfare expenditure in columns (5) to (8). The results are not sensitive to the inclusion of these variables, and the magnitude of the estimates (in absolute terms) is greater for total crime and felony. Column (5) indicates that a one-percentage-point increase in the high school participation rate reduces the number of total arrests per 1,000 youths by approximately 1.1 on average.

Table 1.5 shows that every estimate is not statistically significant at conventional significance levels. A possible econometric issue here is that the estimation may be suffering from a weak instrument problem, meaning that the instrument and the high school participation rates are not highly correlated enough. There may be not much variation left for the policy intervention to affect high school participation rates because the rates are already high in our sample. With a weak

instrument, IV estimates from a just-identified model tend to be too imprecise (Angrist & Pischke, 2009). Stock et al. (2002) compute the critical value for the weak instrument test based on the first-stage F statistic and suggest that the F statistic above approximately 10 makes IV inference reliable. As our first-stage F statistic is 7.5, a weak instrument problem is possibly a cause of the estimates being imprecise in Table 1.5. However, 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.

Another possible econometric reason for the insignificant effects is a small sample size. Due to the nature of the identification strategy detailed in Section 1.3, the results are based on the restricted sample, leading to high standard errors. Furthermore, the estimates in Table 1.5 are identified from variations in schooling in the municipalities intervened by the policies, implying that the identification comes from a much smaller sample than the actual sample. Despite these limitations, however, it is worth emphasising that the signs of our estimated coefficients of interest are robust to regression specifications and consistent with theoretical predictions and existing empirical literature.

1.6 Robustness Checks

In this section, the robustness of the results obtained in the previous section is examined. To address a concern that the results are a product of mere coinci-

dence by retaining a certain set of municipalities in the sample, the results using an alternative definition of the control group are presented. Recall that, in the previous section, we retained a subset of municipalities in the supported group that were least damaged as the treatment group and those in the non-support group lying within the similar distances from the faults to treatment group. As the retained municipalities were exposed to similar degrees of damage, the effect of the earthquake or earthquake-related interventions would have been similar across the treatment and control municipalities.

Another way of defining the control group is to use measures on the earthquake damage instead of using geographical distance measures. Figure 1.6 shows a distinction of municipalities in the Hyogo and Osaka prefectures according to their damage status and eligibility to receive the support. The black area indicates municipalities in the supported group, while the grey area indicates municipalities not targeted by the policies despite being damaged by the earthquake. Comparing those damaged municipalities targeted and not targeted by the policies would allow us to identify the effect of the policies on schooling.

However, a potential issue with this approach is that, even after restricting our attention to the damaged municipalities, there might be a difference in the extent of damage among municipalities with positive levels of damage. Precisely, the municipalities in the supported group were more damaged. The difference in degrees of damage may bias the results, as discussed previously. We thus further restrict our sample and drop a subset of municipalities in the supported group that are heavily damaged and those in the non-support group that are least damaged.

Figure 1.6: Municipalities by Damage Status and Treatment Eligibility

Notes: The black area corresponds to municipalities targeted by the policy intervention, while the grey area indicates municipalities not targeted by the intervention despite being damaged by the earthquake.

A municipality in the supported group is classified as heavily damaged if one or more damage indicators were recorded above the top 25th percentile value of damage distribution within the supported group. Likewise, a municipality in the non-support group is classified as least damaged if one or more damage indicators were recorded below the bottom 25th percentile value of damage distribution within the non-support group.[10] This rule selects 31 municipalities in the supported group as the treatment group and 27 municipalities in the non-support group as the control group (see Figure 1.7). As the distance from the fault line is highly correlated with the degrees of damage, it is not surprising that the sampled municipalities greatly overlap with those in Section 1.3.

Panel I of Table 1.6 reports the first-stage estimates where a treatment dummy is used as an instrument for the high school participation rate. The treatment dummy equals one if a municipality belongs to the treatment group and the year is 1997 and zero otherwise. As in Table 1.5, treatment dummies 1995 and 1996 are not included in the analysis because they do not have any predictive power. The first column controls for the municipality and year fixed effects, while the second column additionally controls for socio-economic characteristics. The coefficient estimates presented in Panel I are larger than the corresponding estimates in Table 1.4. For example, the estimated treatment effect is now 0.483 with a t-statistic of 2.84 (F-statistic 8.1) in the second column, implying that the high school participation rate was higher in treated municipalities by approximately 0.483 percentage points in 1997.

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[10]Our result is not sensitive to this particular cut-off value.

Figure 1.7: Sampled Municipalities using Alternative Selection Rule

Notes: The cross is the epicentre and the line lying from the southwest to northeast indicates the faults that caused the earthquake. The black area indicates the least damaged municipalities among those targeted by the policy intervention (the treatment group), while the grey area corresponds to heavily damaged municipalities among those not targeted by the intervention (the control group).

Table 1.6: Robustness Checks

I. First-stage estimates		
(1) % High school	0.460***	0.483***
	(0.168)	(0.170)
II. IV estimates		
(2) Total crime	-0.660	-0.671
	(1.103)	(0.968)
(3) Total theft	-0.795	-0.551
	(0.845)	(0.714)
(4) Felony	-0.164	-0.219*
	(0.108)	(0.121)
(5) Non-trespass	-0.783	-0.578
	(0.846)	(0.731)
Controls	no	yes
Municip. FE	yes	yes
Year FE	yes	yes
# of Obs.	572	572
# of Municp.	52	52
F-statistics	7.5	8.1

Notes: Standard errors in parentheses are clustered by municipality. *** denotes $p<0.01$ and * refers to $p<0.1$. Dependent variables are the high school participation rate in panel I, and the number of arrests per 1,000 youths in panel II. Refer to Table 1.3 for labels of crime category. Controls included are the municipality fixed effects, year fixed effects, logged income per capita, unemployment rate and logged welfare expenditure per capita. The F-statistics on the excluded instrument are reported.

Panel II of Table 1.6 presents the IV estimates of the effect of the high school participation rate on the arrest rates. The results are qualitatively unaffected by the change in the definition of the control group: the higher high school participation reduces every type of crime, although the effect is not precisely estimated. An exception is the estimate on felony, which has now become significant (p-value 0.07). The estimate on felony in row (4) of Table 1.6 indicates that a one-percentage-point increase in the high school participation rate reduces the number of arrests per 1,000 youths by approximately 0.22.

1.7 Social Savings through Crime Reduction

As crime generates huge social costs, it is interesting to know how much social benefits are brought about by reducing crime through education. In this section, two important questions for policy making are addressed: (i) Do policy benefits exceed costs? (ii) Would a policy to increase police resources yield the same impact at less cost? To tackle these questions, we present the social saving estimates from crime reduction due to a one-percentage-point increase in the high school participation rate in the Hyogo and Osaka prefectures.[11] The social saving estimates presented in this section must be interpreted with the following several caveats.

Firstly, general equilibrium effects are not taken into account. On one hand,

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[11]After retaining in the sample only the municipalities exposed to similar degrees of damage as described in Section 1.3, our sample consists of observations for 63 municipality-police office pairs. The estimates presented thus correspond to those in the retained municipalities (i.e., a subset of municipalities in the Hyogo and Osaka prefectures).

increasing the high school graduation rate, for example, may increase the crimes committed by high school graduates to the extent that an increase in high school graduates reduces the wage of high school graduates. If this were the case, the social saving estimates would be overestimated. On the other hand, a decrease in the supply of high school dropouts may raise the wage of high school dropouts, which in turn reduces the crimes committed by high school dropouts. If this were the case, the social saving estimates would be underestimated. However, Lochner & Moretti (2001) show that the net general equilibrium effect of increasing schooling on crime is negligible. Furthermore, 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 a change in time preferences or an increase in the knowledge about the consequences of 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.[12] On one hand, the social savings may be overestimated to the extent that those affected by the policy are crime-prone individuals with a low level of educational. On the other hand, the social savings may be underestimated to the extent that there is a decreasing return to schooling. Thirdly, the social benefits of increasing the high school participation rate are

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[12]The average high school participation rate in the treatment group before the earthquake is 96.24 per cent.

evaluated for a year. To the extent that raising the high school participation rate has a long-run effect, the social saving estimates may be underestimated.

Lastly, due to data limitations, the estimate of costs per crime does not contain intangible costs such as the costs associated with mental stress and productivity losses. Costs per crime consists 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 detailed in Appendix 3. The social saving estimates would thus be underestimated because the intangible cost is likely to account for a significant part of cost per crime. Despite the limitations of the exercise, we estimate the social savings to provide a rough idea of how much social benefits are generated by increasing the high school participation rate.

Panel I of Table 1.7 presents estimated changes in arrests and actual crimes as a result of an increase in schooling. Given that 67 per cent of actual crimes are reported to police and 16 per cent of reported crimes are cleared up, a 233 reduction in arrests implies an estimated 2,248 reduction in actual crimes.[13] Panel II presents the benefits of crime reduction through schooling and indicates that the social savings due to a reduction in crimes is ¥1,078 million. A total of 87,524 students went on to high schools in the year 2000 in the sample area, and an additional 904 students would have raised the high school participation rate by one

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[13]Not every reported 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 the reluctance of people to report crimes, for example. According to the international crime victimisation survey in 2000, 67 per cent of crimes are reported to police. The survey covers all of Japan and is not focused only on the Hyogo and Osaka prefectures. The estimate is thus computed assuming that the tendency of people to report crimes to the police in the Hyogo and Osaka prefectures is the same as the tendency across Japan.

Table 1.7: Social Savings through Crime Reduction

I: Estimated change in arrests/crimes	
Change in arrests	233
Change in actual crimes	2,248

II: Benefit of crime reduction through schooling	(¥1,000)
Average cost per crime	479
Benefit from 1pp increase in schooling	1,077,561
Cost of 1pp increase in schooling	705,823
Net benefit	371,738

III: Cost of crime reduction using police forces	(¥1,000/officers)
Average cost per police officer	17,616
Required number of police officers	178
Cost to increase police officers	3,135,648

Notes: All costs are inflated to represent 2003 real prices using the Consumer Price Index except the cost of increasing schooling that is evaluated at 2000 as 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 have to 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 percent. The effect of the interest rates is thus omitted in the analysis. Costs and benefits are expressed in ¥1,000.

percentage point. Given that attending high schools costs ¥781 thousand per head annually on average (Ministry of Education, Culture, Sports, Science and Technology, 2002), an additional 904 students would have cost about ¥706 million, yielding the net social savings of ¥372 million.

An important caveat is that the calculation of the cost of raising the high school participation rate does not include foregone earnings. If the foregone earnings of ¥1 million per annum per head is added to the direct cost of schooling, raising the high school participation rate costs about ¥ 1,615 million.[14] The net effect of raising the schooling level is thus in a deficit of ¥538 million. However, it is important to note that crime reduction is one of the numerous effects of schooling. Among others, education also contributes to better health, political awareness, and long-run economic growth (Barro, 2001; Dee, 2004; Grossman, 2005). These by-products may in turn indirectly contribute to crime reduction.

Panel III presents the cost of crime reduction using police forces. Hiring an additional officer costs an estimated ¥17,616 thousand annually, generating a social savings of ¥3 million due to crime reduction.[15] To yield the same amount of social savings resulting from a one-percentage-point increase in the high school

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[14]Foregone earnings of participating in the high school are measured by the average annual earnings of workers who did not graduate from the high school.

[15]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 ¥8,808 thousand (Ministry of Internal Affairs and Communications, 2004), yielding the estimated average cost of ¥17,616 thousand for hiring additional officer. Levitt (1997) estimates that property and violent crimes decrease by 1.6 and 3.2, respectively, per additional officer in the US. Given that a property crime and a violent crime cost an estimated ¥71 thousand and ¥1,853 thousand on average, respectively, an additional officer in Japan yields social savings of ¥6 million under the assumption that the decrease in crime per additional officer is the same in the US and Japan.

participation rate (¥1,078 million), an additional 178 police officers are required. An additional 178 police officers cost about ¥3,136 million, which is far more expensive than the cost of raising the high school participation rate, ¥706 million (without foregone earnings) or ¥1,615 million (with forgone earnings).[16]

1.8   Conclusions

In this chapter, we explore the causal effect of schooling on juvenile delinquency. To identify the effect of schooling, we use exogenous variation in schooling caused by a policy intervention in specific municipalities after the Great Hanshin-Awaji Earthquake. Before summarising the results, we mention several caveats for interpretation.

The estimates have high standard errors; thus, they are often not statistically significant at conventional significance levels. A possible reason for the high standard errors is a weak instrument. The F-statistics on the excluded instrument range between 7.5 and 8. With a weak instrument, IV estimates from a just-identified model tend to be too imprecise (Angrist & Pischke, 2009). However, the IV estimator in a just-identified model is known to be approximately centred around the true parameter.

Another possible econometric reason for the insignificant effects is a small sample size. Due to the nature of the identification strategy, the results are based

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[16]If the non-salary overhead cost of hiring an additional officer is excluded from the calculation, hiring 178 police officers costs roughly ¥1,571 million, still approximately double the cost of raising the high school participation rate.

on the restricted sample. Furthermore, the coefficient of interest is identified from variations in schooling in the municipalities intervened by the policies, implying that the identification comes from a much smaller sample than the actual sample. Despite the limitations, however, it is worth emphasising that the signs of our estimated coefficients of interest are robust throughout the analyses and consistent with theoretical predictions and existing empirical literature.

Based on the comparison of municipalities with and without the policy intervention, we find that the policies raised the high school participation rate in the affected municipalities by approximately 0.41 percentage points. Subsequently, the total number of juvenile arrests in those affected municipalities was lowered by approximately 1.09 per 1,000 youths.[17] The negative impact of schooling on crime is consistent with rational choice models predicting that higher educational attainment reduces crimes through a higher opportunity cost of committing crimes (Ehrlich, 1975; Huang et al., 2004; Lochner, 2004). Likewise, the results are also consistent with a behavioural economic model predicting that education reduces crime by affecting the time preferences of individuals or enabling them to more accurately compute the cost of engaging in crimes (O'Donoghue & Rabin, 2001).

To assess the cost-effectiveness of schooling as a means of reducing crime, we provide the evidence of how much society can save by reducing crime through education. The results indicate that increasing educational attainment yields positive social benefits. Furthermore, we find that it is less expensive to reach a target level

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[17]The unconditional means of the high school participation rate and the number of arrests per 1,000 youths are 96.1 per cent and 12.1, respectively.

of social benefits by improving schooling than by strengthening police forces. It is also important to note that education has other positive effects on society not necessarily brought about by strengthening police forces, such as improving health and political awareness and enhancing long-run economic growth (Barro, 2001; Dee, 2004; Grossman, 2005). A policy that encourages schooling would thus be an attractive supplement to the traditional methods of combating juvenile delinquency that increase formal deterrence.

1.9   Data Appendix

Appendix 1: Computation of the Number of Juvenile Arrests

The crime indicator is defined as the number of juvenile arrests per 1,000 youths. The number of arrests is recorded by police office but not by age. We therefore compute the number of juvenile arrests in police office o in prefecture p under the following assumption.

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

$$\frac{arrest_o^y/pop_o^y}{(arrest_o^a + arrest_o^y)/(pop_o^a + pop_o^y)} = \frac{\sum_o arrest_o^y/\sum_o pop_o^y}{\sum_o (arrest_o^a + arrest_o^y)/\sum_o (pop_o^a + pop_o^y)} \quad (1.2)$$

where the superscripts y and a refer to *youth* and *adult*, respectively. The left-hand side corresponds to "the number of arrested youths over the youth population" relative to "the number of the total arrests over the total population" in office o in prefecture p. The right-hand side is the same fraction in prefecture p after aggregated up across o. Underscript p is suppressed from equation (1.2) for brevity. The assumption essentially means that the tendency of youths to commit a crime relative to the whole population is constant across offices in a prefecture. Equation (1.2) can be re-arranged to obtain the number of arrested youths in office o in prefecture p:

$$arrest_o^y = \sum_o arrest_o^y \times \frac{(arrest_o^a + arrest_o^y)}{\sum_o (arrest_o^a + arrest_o^y)} \times \frac{\sum_o (pop_o^a + pop_o^y)}{(pop_o^a + pop_o^y)} \times \frac{pop_o^y}{\sum_o pop_o^y} \qquad (1.3)$$

which, in words, means "youth arrests in office o in prefecture p = [total youth arrests in prefecture p] $\times$ [office o share of total arrests in prefecture p] $\times$ [inverse of office o population share] $\times$ [office o youth population share]". Underscript p is suppressed from equation (1.3) for brevity. As data on every component of the right-hand side of equation (1.3) is available, the number of youth arrests in office o in prefecture p is computed using equation (1.3).

Appendix 2: Matching of Data

Data on crime is available for 115 police offices in the Hyogo and Osaka prefectures, while data on covariates is available for 163 municipalities in the Hyogo and Osaka prefectures. As the data for crimes and covariates have different units of observations, these data are matched in the following manners.

Case A: One police office covers several municipalities

The data for covariates in levels are summed, and the data for covariates in rates are subsequently computed. For example, assume that police office A covers municipalities B and C. To compute the unemployment rate defined by the share of the unemployed to total labour force, in the first step, the numbers of the unemployed and total labour force, respectively, in municipalities B and C are summed.

In the second step, the total number of the unemployed in municipalities B and C is divided by the total number of labour force in municipalities B and C. In the third step, the unemployment rate obtained is matched with the crime data of police office A.

Case B: One municipality is covered by several police offices

The crime data in levels are summed, and the crime rates are subsequently computed. For example, assume that municipality A is covered by police offices B and C. To compute the crime rate defined by the number of crimes per 1,000 youths, in the first step, the number of crimes reported to police offices B and C are summed. In the second step, the total number of crimes reported in police offices B and C is divided by the total number of youths in city A (and multiplied by 1,000), and the crime rate obtained is subsequently matched with covariates data of city A.

As a result of matching, the number of municipality-police office pairs becomes 94. After retaining in the sample only the municipalities exposed to similar degrees of damage as described in Section 1.3, our sample consists of observations for 63 municipality-police office pairs in the Hyogo and Osaka prefectures from 1993 to 2003.

Appendix 3: 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). Due to data limitations, the estimates of costs per crime do not contain intangible costs such as the costs associated with mental stress and productivity losses.

Costs for crime prevention consist of the expenditure on crime preventative activities by probation officers (Ministry of Justice, 2010) 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 1.8 presents all costs included in the calculation. 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 1.8: Costs of Crime

		¥million
Costs for crime prevention:	Prevention	598
Costs in response to crime:	Incarceration, probation	52,513
	Lawyers fees	7,900
	Subsistence, medicine	9,354
	Police activity	19,591
Costs as a consequence of crime:	Compensation for damage	9,226
Total		99,183

Chapter 2

Volunteer Work and Mortality

Millions of people across the world work for free. More than a quarter of citizens in Japan and the United States and more than 40 per cent of citizens in the United Kingdom participate in volunteer work.[1] Although volunteer work is expected to benefit society, formal research on its potential benefit is scarce. If volunteer work has no significant impact on the well-being of society, allocating time to different activity might be a more efficient allocation of resources. Among various types of volunteer work, this study focuses on the effect of volunteer work providing daily assistance to the elderly on elderly mortality.

Clarifying this effect is important for several reasons. Firstly, longer and healthier lives are an obvious source of an individual's happiness; thus, the poten-

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[1]Japanese, UK and US data are from the Survey on Time Use and Leisure Activities, the Citizenship Survey and the Current Population Survey, respectively. The figures are proportions of people who engaged in volunteer work at least once in the past 12 months.

tially significant impact endorses the importance of volunteer activity. Secondly, given global population ageing, health care expenditures are likely to rise further; hence, clarifying the contribution of volunteering to longevity and better health is important for the overall maintenance of society. Lastly, List & Lucking-Reiley (2002), based on their study of charitable giving, suggest that people donate more money if they believe that their contribution helps achieve the goal of the charity. To the extent that the same mechanism holds for donations of time to a charity, clarifying the outcomes of volunteer work may affect people's decisions to volunteer.

A major challenge to identify the causal effect of volunteering on mortality is endogeneity of volunteering. There may be more volunteers in municipalities with a large number of people with a high risk of mortality such as the elderly. It is thus hard to conclude if the level of volunteering affects mortality or vice versa. Equally, unobserved heterogeneity across municipalities that affects both mortality and the level of volunteering (e.g., the quality of local health care services) may bias estimates of the effect of volunteering.

To identify the causal effect of volunteering on mortality, we use exogenous variation in volunteering caused by the Great Hanshin-Awaji earthquake, which occurred in the midwestern part of Japan in 1995. The level of volunteering considerably increased in municipalities hit by the earthquake, while other municipalities with no damage did not experience such a sharp increase in volunteering. Based on a comparison of mortality between the municipalities with and without damage, our results indicate that a one-percentage-point increase in the number

of volunteers reduces the number of deaths of those aged 80 or above by approximately 0.14 per cent. Evaluated at the mean, the estimate implies that approximately one life of people aged 80 or older (out of 186 persons) is saved in a given year when the number of volunteers increases by 100 (out of 1,911 persons). Although the nature of the data does not allow us to determine if the reduction is driven by a reduction in the mortality of volunteers or care recipients, supporting evidence suggests that the reduction is likely to have occurred among care recipients. A series of robustness checks are conducted to ensure that the results are not driven by direct effects of the earthquake.

To the best of the author's knowledge, this is the first paper to examine the causal effect of volunteering on its outcome with (we argue) a credible identification strategy. Rather surprisingly, the effects of volunteering, especially its effect on the recipients of volunteer service, are overlooked despite its enormous potential effects given the scale and popularity of volunteer activity. Previous research on volunteering conducted by economists primarily focuses on determinants of the supply of volunteer labour, seeking to understand why people work for nothing (Day & Devlin, 1998; Duncan, 1999; Freeman, 1997; Menchik & Weisbrod, 1987). Previous studies most closely related to this topic are those on donations of *money*, as opposed to *time*, to charity that investigate people's motivations to donate and optimal ways of fund-raising (Andreoni, 1989, 1990; Benabou & Tirole, 2006; DellaVigna et al., 2012; Fong & Luttmer, 2009; Grossman, 2010; Landry et al., 2006; Shang & Croson, 2009; Vesterlund, 2003).

The outcomes of volunteer work have been analysed by social scientists in

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other disciplines such as gerontology, sociology and psychology. Their primary focus is on its effect on the well-being and mental health of volunteers themselves such as happiness, life satisfaction, self-reported health and depression (Morrow-Howell et al., 2003; Musick & Wilson, 2003; Piliavin & Siegl, 2007; Thoits & Hewitt, 2001; Van Willigen, 2000; Young & Glasgow, 1998). Among these studies, there are several papers particularly focusing on the effect of volunteer work on mortality (Harris & Thoresen, 2005; Musick et al., 1999; Oman et al., 1999; Shmotkin et al., 2003). Using the Cox proportional hazards model, these papers compute hazard ratios for mortality that indicate the relative risk of mortality compared to a reference group conditional on control variables and find lower mortality risk among volunteers relative to non-volunteers. An issue with these studies is that it is not clear if volunteering is reducing the risk of death or if people with a low risk of mortality are participating in volunteering from the outset.

There is, however, one paper that exploits a difference in timing to address the endogeneity of volunteering. Luoh & Herzog (2002) estimate its effect on the mortality of volunteers and find that elderly Americans who engaged in volunteer work for 100 hours or more are less likely to die relative to those who volunteered for fewer than 100 hours. Their analytical strategy is to regress mortality status in period t on volunteer participation status in period $t-1$ to identify the causal effect of volunteering on mortality, controlling for health status prior to participation in volunteer work (i.e., health status in period $t-2$). A potential issue with their approach is that estimates of the effect of volunteering are biased if time-invariant unobserved heterogeneity that affects the health status of an individual is also

correlated with the volunteer participation status of the individual.[2]

The remainder of the chapter proceeds as follows. Section 2.1 explains the effect of the earthquake on volunteering. In Section 2.2, we show the econometric specification and discuss the identification strategy. Section 2.3 presents data sources, while Section 2.4 discusses the empirical findings. Section 2.5 checks the robustness of the results, and Section 2.6 concludes the paper.

2.1   Effect of the Earthquake on Volunteering

Shortly after the earthquake, a large number of volunteers from different parts of Japan gathered in the disaster area to provide emergency support, such as transporting relief, managing asylums, providing meals and nursing. A total of 1,377 thousand people volunteered over a year after the earthquake, approximately 70 per cent of whom were participating in volunteering for the first time (Hyogo Prefecture, 1996). Their activities attracted the attention of the media and were broadcast across Japan. Since then, volunteering has become a popular activity among the public, as is shown by the fact that 1995 is called "*Volunteer Gannen*", meaning "the starting year of volunteerism".[3]

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[2]Another potential issue is that Luoh & Herzog (2002) group those who did not volunteer and those who volunteered fewer than 100 hours into the same category and use them as a reference group. In other words, the reference group also contains those who participated in volunteer work; thus, it is hard to interpret their estimates. In addition, the previous literature uses different cut-off values for measures of volunteering, e.g., the cut-off value of 40 hours in Musick et al. (1999), while the value is 100 hours in Luoh & Herzog (2002). It is therefore not clear how robust their results are to different cut-off values.

[3]Although volunteer activities had been taking place even before 1995, the activities were mainly conducted by people in, for example, certain religious or political groups and were not necessarily popular activities among the public.

Figure 2.1: Characteristics of Volunteers

I. Volunteers by Age

II. Volunteers by Occupation
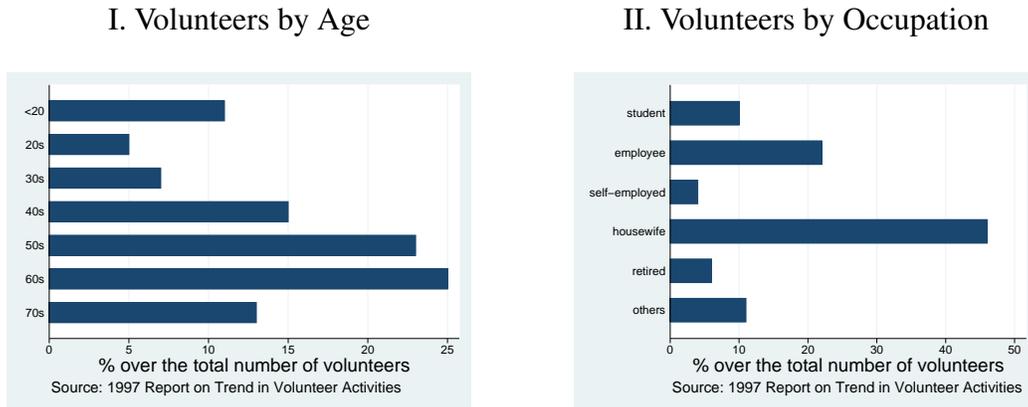
In this paper, we focus on the volunteers registered to the Hyogo Council of Social Welfare. These volunteers are based in the Hyogo prefecture and engage in activities that mainly target the elderly (Hyogo Council of Social Welfare, 1997). As of 2000, approximately 83 per cent of the volunteer activities target the elderly at home, and the rest targets the disabled and people in hospitals and care homes. The main activities of the volunteers are to deliver meals, visit houses, hospitals and care homes for a chat, organise community gatherings and provide assistance for daily tasks (e.g., housework). Figure 2.1 summarises the characteristics of volunteers. Panel I indicates that people in their 60s account for the largest proportion of volunteers, 25.4 per cent. People in their 50s are the second largest, 23.2 per cent, implying that these two age bands account for about half of volunteers. Panel II presents the occupational composition of volunteers. A noticeable feature of the occupational composition is that housewives alone account for nearly half of volunteers. In the municipalities hit by the earthquake, the level

of volunteering jumped up after the earthquake, whereas it evolved smoothly in other municipalities.

Figure 2.2 plots the level of volunteers in the municipalities hit by the earthquake relative to those not hit by the earthquake. For example, the relative level of volunteers plotted on the y-axis equals 0.1 if the number of volunteers in the municipalities hit by the earthquake was 10 per cent higher, and it equals zero if there is no difference in the number of volunteers in the two groups. The graph shows that the level of volunteers was similar in the two groups before the earthquake in 1995. Although the relative level of volunteers in the municipalities hit by the earthquake seems to slightly increase during the pre-quake period, the difference is not statistically different from zero. In contrast, the level of volunteers sharply increased after the earthquake by approximately 20 to 25 per cent and stayed at the higher level, implying that the earthquake caused a persistent rather than a temporary increase in the level of volunteers.

2.2 Identification Strategy

We now turn to a discussion of identification strategy. This study explores the causal effect of volunteering on elderly mortality by regressing mortality on a measure of volunteering, controlling for various socio-economic indicators. The following model is set up:

$$ln(\#deaths)_{it} = \beta_0 + \beta_1 ln(\#volunteers)_{it} + X'_{it}\gamma + v_i + \tau_t + \varepsilon_{it} \qquad (2.1)$$

Figure 2.2: Relative Level of Volunteers in the Treatment Group
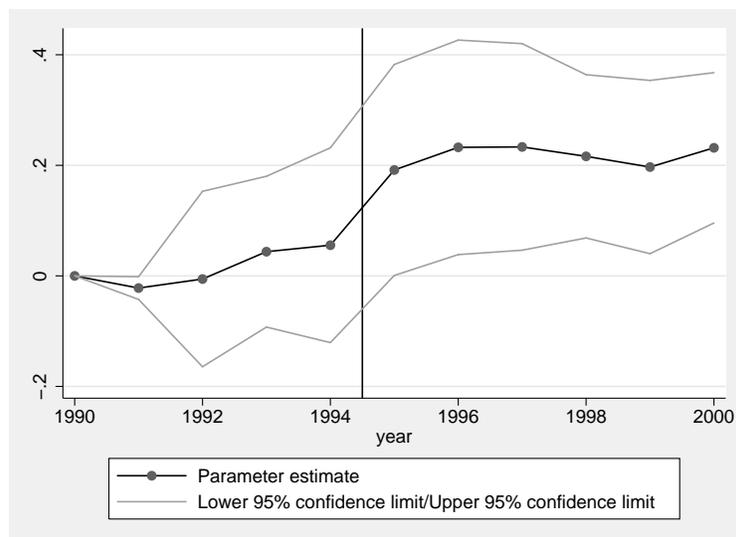
Notes: The figure plots the level of volunteers in the treatment group relative to the control group after controlling for income per capita, unemployment rate, sex ratio, the number of the elderly, the number of divorces, high school participation rate, elderly medical expenditure per capita, and municipality and year fixed effects. A municipality is classified as treated if the municipality recorded a positive level of physical damage due to the earthquake. Two grey lines indicate 95 percent confidence intervals.

where $ln(\#deaths)_{it}$ represents the logged number of deaths in municipality i in year t and $ln(\#volunteers)_{it}$ is the logged number of volunteers. The time varying socio-economic characteristics, X_{it}, and parameter, γ, are $K \times 1$ vectors, where K is the number of socio-economic indicators. ν_i and τ_t are the municipality and year fixed effects, respectively, and ε_{it} is the disturbance term.

The main coefficient of interest is β_1 that measures the effect of volunteering on mortality. An econometric issue in the estimation of equation (2.1) is endogeneity of volunteering. Specifically, unobserved heterogeneity such as the quality of local health care service may affect both mortality and the level of volunteering. It may also be the case that there are more volunteers in the municipalities with a large number of vulnerable people such as the elderly (reverse causality). If it is the case, the OLS estimator for β_1 is biased. An unbiased estimator can be obtained by using the IV estimator that requires an instrument giving exogenous variation in volunteering.

In this chapter, physical damage caused by the earthquake is used as an instrument for volunteering. In order for the physical damage to be a valid instrument, we require an assumption that, conditional on volunteering, the physical damage has no direct effect on mortality. The nature of the instrument, however, raises several concerns about the validity of the assumption. Firstly, 6,434 people were killed by the earthquake and thus the identifying assumption does not hold in the year 1995. Apart from the accidental deaths due to the earthquake, chronic ill-

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nesses may have deteriorated due to unfamiliar lives in asylums or other quake related disruptions in daily life, which may have lead to an increase in mortality.

To address the temporary increase in mortality in the aftermath of the earthquake, we omit the year 1995 from the sample. The earthquake occurred in January 1995 and the peak number of refugees recorded was 316,678 in January 1995. The number of refugees steadily decreased as (re)construction of houses and temporary housing proceeded and 97 per cent of refugees left asylums by August 1995. The earthquake also caused serious damage to infrastructure: water, electricity, gas supply and phone lines were cut off in the disaster area.[4] Repairs of the infrastructure was, however, extremely rapid. Electricity and phone lines were repaired by the end of January 1995, and water and gas supplies resumed by the end of April 1995. Medical facilities were also heavily damaged by the earthquake. As of January 1995, 33 per cent of hospitals and clinics in the Hyogo prefecture were closed due to damage to buildings or disruption in infrastructure. Within a month, however, 70 per cent of hospitals and clinics had resumed operations, and subsequently, 93 per cent resumed operations by the end of 1995. Omitting year 1995 from the sample, therefore, also addresses the possibility of an increase in mortality induced by a temporary disruption in daily life, infrastructure and medical facilities.

Nevertheless, there may still be a concern that the damage of the earthquake had long-term consequences on mortality even after omitting year 1995 from the

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[4]Water, electricity, gas supply and phone lines were cut off in 1,270, 2,600, 845 and 100 thousand households, respectively, immediately after the earthquake.

sample. For instance, the earthquake may have disproportionately killed infirm people such as the elderly, leading to a lower mortality afterwards because relatively fitter people survived. It could also be the case that the mental stress induced by facing the deaths of a large number of people or losing houses, for example, had a persistent effect on mortality even after 1995. To address these concerns, we further restrict the sample and drop a set of municipalities heavily damaged by the earthquake. Figure 2.3 shows municipalities in the Hyogo prefecture according to their damage status. The shaded area in Figure 2.3 corresponds to a set of municipalities that recorded positive levels of physical damage. Among the set of municipalities with damage, the grey area corresponds to a subset of municipalities that recorded moderate damage, while the black area corresponds to those heavily damaged.[5] These are the four municipalities close to the epicentre, which account for a substantial amount of physical damage due to the earthquake: 98, 85 and 88 per cent of the total deaths, serious injury and full destruction of houses, respectively.

Omitting these municipalities has several implications. Firstly, most individuals who died due to the earthquake were living in these heavily damaged municipalities. If the earthquake killed vulnerable people disproportionately, it would thus have happened in these omitted municipalities. Secondly, if the mental stress caused by, for example, experiencing a large number of deaths in a neighbourhood or severe destruction of houses had a long-term effect on mortality, it would

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[5]A municipality recorded above the 75th percentile value of deaths is classified as a heavily damaged municipality. In the section of robustness checks, different definitions of heavily damaged municipalities are used.

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have affected mortality in omitted municipalities. Furthermore, approximately 98 per cent of refugees stayed in these municipalities. Therefore, if a life in asylums had a long-term effect on mortality, it is likely to have happened in these municipalities. Finally, as mentioned earlier, medical facilities were also heavily damaged by the earthquake. Given that most disruptions in medical facilities were restored by the end of 1995, any effect of the disruptions on mortality is likely to be accounted for by omitting the year 1995 from the sample.

Nevertheless, one may still be concerned that the disruption in medical facilities had long-term consequences. For example, a chronic disease might have deteriorated because one could not obtain adequate medical treatment in 1995. Omitting municipalities heavily damaged by the earthquake addresses these types of concerns since medical facilities that experienced heavy damage were concentrated in the omitted municipalities.[6] Block I of Table 2.1 presents the average destruction due to the earthquake in the treatment and control municipalities. Recall that a municipality is categorised as treated if the municipality recorded a positive level of physical damage due to the earthquake. The average destruction in control municipalities is therefore equal to zero by construction. Block I indicates that approximately 0.5 per cent of people were injured and approximately 10 per cent of households had their houses damaged in the treatment municipalities. As expected, the proportion of people who died, 0.006 per cent, is not great given that the most heavily damaged municipalities are omitted from the sample.

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[6]As of 27th February 1995, about 99 per cent of hospitals and clinics closed were located in the omitted municipalities.

Figure 2.3: Municipalities by Damage Status

Notes: The cross indicates the epicentre. The black area is a subset of municipalities heavily damaged by the earthquake and omitted from the sample, while the grey area is a subset of municipalities suffered moderate damage. A municipality is classified as heavily damaged if the municipality was recorded above the 75th percentile value of the distribution of the death measure. Death is measured by the number of deaths due to the earthquake, such as being crushed or killed in a fire.

Table 2.1: Municipality Characteristics

| | Treatment | | Control | | |
| | 28 municipalities | | 56 municipalities | | |
	Mean	Std. Err.	Mean	Std. Err.	p-value
I. Destruction due to the Quake					
Death per 1,000 pop	0.06	0.03	0.00	0.00	-
Injured per 1,000 pop	5.45	3.05	0.00	0.00	-
Destroyed hh per 1,000 hh	114.35	65.28	0.00	0.00	-
II. Demography					
Sex ratio	0.97	0.12	0.93	0.05	0.75
% Pop under 15	18.70	5.14	19.08	2.11	0.94
% Pop over 65	11.14	2.95	16.03	1.55	0.11
% Divorced	2.44	0.86	1.76	0.23	0.33
% Widowed	7.38	2.01	10.24	1.04	0.16
In-migration per 1,000 pop	46.07	5.51	32.25	1.87	0.00
Out-migration per 1,000 pop	44.92	6.02	34.34	1.82	0.03
III. Education					
% Compulsory	22.45	6.15	31.54	2.96	0.14
% High school	33.44	9.73	30.22	3.67	0.71
% University	14.89	4.44	10.15	1.31	0.20
IV. Economy					
% Unemployed	1.68	0.56	1.21	0.17	0.32
% Employed	47.42	13.61	48.21	5.27	0.95
Income per cap	1,400	177	1,128	61	0.07
Medical exp per cap	637	84	516	27	0.09

Notes: Mean refers to weighted means where population is used as weights. Standard errors are reported next to the means. Block I presents figures as of 1995, whereas Blocks II to IV show the average during pre-quake period, i.e., between 1990 and 1994. p-value corresponds to p-values of t-tests under the null hypothesis of equality of means between the treatment and control groups. Each label of variables in Block I indicates the following: number of deaths due to the direct effect of the earthquake, number of people injured, and number of households whose houses are destroyed. Sex ratio corresponds to the number of males relative to females. % divorced and widowed are defined over people aged 15 or above. Per capita taxable income and public medical expenditure for the elderly are measured in ¥1,000.

For the identification of the causal effect of volunteering, it is important that the treatment and control municipalities had similar characteristics prior to the earthquake. Blocks II to IV of Table 2.1 compare the pre-treatment characteristics of the two groups. Block I indicates that there is no important difference in sex and age compositions, divorce rate and widowed rate. However, treated municipalities display higher in-migration and out-migration rates. The different migration patterns bias the results if the pattern is correlated with mortality and different between treatment and control groups. For example, elderly people in the treatment group may have moved out after the earthquake in search of better medical facilities. Given that the data on the characteristics of migrants, such as age, are not available, it is not possible to track who migrated.

If the migration pattern is correlated with mortality (or age), however, the migration would have affected the age compositions of the two groups. To check this possibility, we conduct a t-test under the null hypothesis of constant differences in the percentage of people under 15, between 15 and 65, and over 65 between the two groups before and after the earthquake. The null hypothesis is not rejected for every age band, implying that there is no evidence that different migration rates altered age compositions in the two groups. Block III suggests that the level of educational attainment is higher in the treatment than in the control group but the difference is not statistically significant. With regard to characteristics related to the labour market and the economy, the percentages of unemployment and employment are similar in the two groups as shown in Block IV. Per capita income and public elderly medical expenditure are higher in the treatment group, but the

Figure 2.4: Volunteers per elderly person

differences are not statistically significant.

The identification of the effect of volunteering also requires that trends in the treatment and control groups were the same prior to the earthquake. Figure 2.4 plots the weighted-average number of volunteers per elderly person in the treated and control municipalities.[7] Figure 2.4 shows that pre-quake trends in the number of volunteers were similar between the two groups. The two series, however, diverge after the earthquake, and the treated municipalities exhibit a higher number of volunteers per elderly person. Turning to mortality, Figure 2.5 plots the average

[7]Elderly people correspond to those aged 80 or older. Changing the definition of old people does not significantly alter the graph. The weights are elderly population and quake damage.

number of deaths by age cohort. Mortality is expressed per 1,000 people in the corresponding age cohort. Not surprisingly, Figure 2.5 shows sharp spikes in the year of the earthquake, 1995, regardless of age cohort. The pre-quake trends in mortality were similar across two groups in every age cohort, with slightly steeper downward trends in the control group for mortality of people in their 70s and 80s or older, potentially leading to the underestimation of the effect of volunteering. After the earthquake, the mortality of people in their 80s or older in the treatment group appears to decrease to a greater extent, while such a pattern was not observed in any other age cohort.

2.3   Data and Variables

Our sample consists of observations for 88 municipalities in the Hyogo prefecture from 1990 to 2000. Mortality data are obtained from the Current Population Survey (*Jinko Dotai Chosa*) conducted every year by the Ministry of Health, Labour and Welfare. The survey has been conducted since 1898 and collects data on birth, death, stillbirth, marriage and divorce. Data on age-specific mortality (i.e., the number of people who died in their 50s, 60s, 70s, and 80s or older) are taken from the survey.

Data on volunteering are available from the Report on the Activity of the Hyogo Council of Social Welfare (*Kennai Syakyo Katsudo Genkyo Chosa*). The report provides various figures related to volunteering, such as the number of volunteers and voluntary groups at the municipality level in the Hyogo prefecture.

Figure 2.5: Mortality Rate by Age Cohort

I. Mortality of people in their 50s
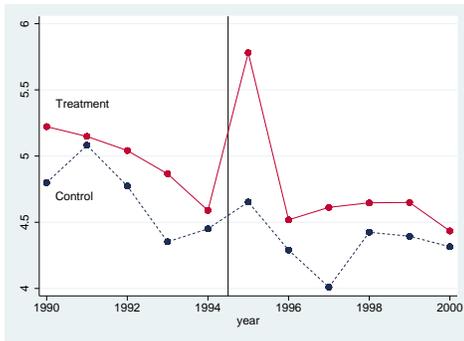
II. Mortality of people in their 60s
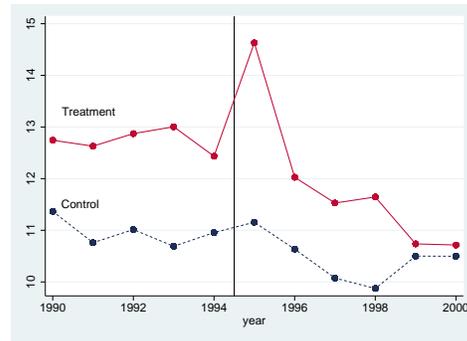
III. Mortality of people in their 70s
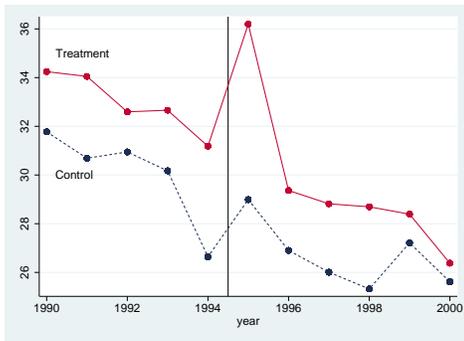
IV. Mortality of people in their 80s+
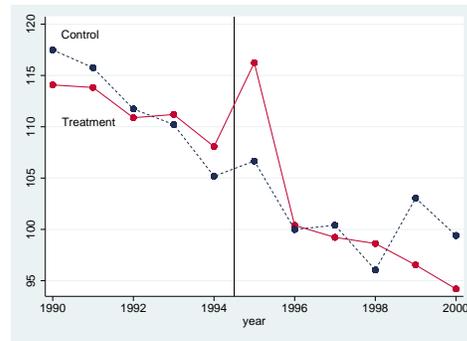
Notes: Solid and dotted lines correspond to the weighted average number of deaths per 1,000 persons by age cohort in the treatment and control municipalities, respectively. Weights are population in the corresponding age cohort.

Characteristics of care receivers and the problems and challenges that volunteer activities in Hyogo face are also described in the report. It is a detailed and useful source of information about volunteering in the Hyogo prefecture and is available from 1990 onwards. The descriptions of characteristics of volunteers and volunteer activities are, however, rather narrative; thus, it is difficult to understand a statistical overview of the characteristics. We therefore also extract data from the Report on the Trend in Volunteer Activity (*Volunteer Katsudo Doko Chosa Hokokusyo*). It is a survey report of a survey on volunteering in the Hyogo prefecture that has been conducted every four years since 1984. The survey collects various information about volunteering, such as sex, age and occupation of volunteers, frequency of volunteer activity and the financial conditions of volunteer groups. Data on the characteristics of volunteers and contents of volunteer work are obtained from the survey report.

Information on the physical damage due to the earthquake is provided by the disaster prevention division of the Hyogo prefecture. The document reports physical damage due to the earthquake officially confirmed by the Fire and Disaster Management Agency of the Ministry of Internal Affairs and Communications. The earthquake damage is categorised into loss of life and damage to buildings, and each category is further divided into sub-categories according to the extent of damage.

Public medical expenditure for the elderly is obtained from the National Health Insurance and Health Insurance for the Aged in Hyogo (*Hyogo no Kokuho Rouken*) from 1990 to 2000. The document summarises public expenditures related to

health at the municipality level in the Hyogo prefecture. Data on covariates other than the public medical expenditure are obtained from a survey report, the System of Social and Demographic Statistics of Japan (*Syakai Jinko Tokei Taikei*). The survey covers every municipality in Japan and collects information on the socio-economic and demographic conditions of municipalities. The survey has been conducted since 1976 and provides 1,500 social and demographic variables by municipality. As socio-economic indicators, we use municipality-level measures of economic activity, demography and educational level: taxable income per capita, unemployment rate, sex ratio, population, number of divorces and high school participation rate. The first lag of population is used because mortality and population in period t are simultaneously determined.

2.4   Results

We begin by presenting the OLS estimates of equation 2.1. Table 2.2 shows the estimates of regressions in which age-specific mortality is regressed on municipality and year fixed effects, and other controls between 1990 and 2000. Dependent variables in columns (1) to (4) correspond to the logged number of people who died in their 50s, 60s, 70s, and 80s or older, respectively. Table 2.2 indicates that every coefficient estimate is not statistically different from zero, implying that volunteering has no effect on mortality at any age. The OLS estimator is, however, biased if (i) unobserved heterogeneity across municipalities that affects mortality is also correlated with the number of volunteers or (ii) levels of mortality and vol-

Table 2.2: Effects of Volunteering on Age Specific Mortality (OLS estimates)

Dependent Var.	# Deaths in 50s	# Deaths in 60s	# Deaths in 70s	# Deaths in 80s+
	(1)	(2)	(3)	(4)
# Volunteers	0.033	0.005	-0.016	0.004
	(0.027)	(0.013)	(0.012)	(0.010)
Controls	yes	yes	yes	yes
Municip. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
# Obs.	824	837	837	837
# Municip.	84	84	84	84

Notes: Standard errors are clustered by municipality. The dependent variables are the logged number of age-specific deaths. Controls included into the analysis are income per capita, unemployment rate, sex ratio, population, number of divorces, high school participation rate, elderly medical expenditure per capita, and municipality and year fixed effects. Columns (1) to (4) use the number of people in their 50s, 60s, 70s and 80s or older, respectively, as a population measure. Every variable in level is logged.

unteers are simultaneously determined. To address the possible endogeneity of volunteering, we estimate equation 2.1 using the IV estimator.

Panel I of Table 2.3 shows estimates of the first-stage equation. The earthquake damage is measured by percentage of people injured.[8] The damage indicator is equal to zero in every municipality before the earthquake and takes a positive value, for example, 10, after the earthquake if 10 per cent of the population is injured in a municipality. Odd-numbered columns correspond to a specification that controls only for municipality and year fixed effects. Estimates indicate positive effects of the earthquake damage on volunteering, and the effects are highly statistically significant.

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[8]Other damage indicators are used in Section 2.5 as a robustness check.

Table 2.3: Effects of Volunteering on Age Specific Mortality (IV estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var.	# volunteers		# volunteers		# volunteers		# volunteers	
I. First stage est.	0.208***	0.205***	0.207***	0.207***	0.207***	0.204***	0.207***	0.220***
	(0.031)	(0.037)	(0.030)	(0.038)	(0.030)	(0.040)	(0.030)	(0.038)
Dependent Var.	# deaths in 50s		# deaths in 60s		# deaths in 70s		# deaths in 80s+	
II. IV estimates	-0.004	-0.030	-0.028	-0.012	-0.124***	-0.076**	-0.179***	-0.140***
	(0.064)	(0.068)	(0.057)	(0.046)	(0.025)	(0.035)	(0.042)	(0.043)
Controls	no	yes	no	yes	no	yes	no	yes
Municip. FE	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes
# Obs.	825	824	838	837	837	837	837	837
# Municp.	84	84	84	84	84	84	84	84
F-statistics	46.4	30.5	47.5	29.3	47.5	26.2	47.5	32.8

Notes: Standard errors are clustered by municipality. *** denotes p<0.01. Controls included into the analysis are income per capita, unemployment rate, sex ratio, population, the number of divorces, high school participation rate, and municipality and year fixed effects. The measure of population differs by column: columns (1) and (2), (3) and (4), (5) and (6), and (7) and (8) controls for the number of people in their 50s, 60s, 70s, and 80s or older, respectively. Every variable in level is logged. The damage indicator corresponds to percentage of people injured. The F-statistics on the excluded instrument are reported.

Odd-numbered columns are, however, not correctly specified if any difference across municipalities that varies over time affects mortality. To account for time varying heterogeneity across municipalities that may affect mortality, socio-economic indicators are added to specifications in the even-numbered columns. The results are robust to the inclusion of additional controls, and the effect of earthquake damage on volunteering remains highly statistically significant. Column (8) of Panel I indicates that a one-percentage-point increase in the percentage of people injured raises the number of volunteers by approximately 22 per cent on average, ceteris paribus.

Note that the F-statistic reported in the last row of Table 2.3 clearly exceeds 10. It is important for the identification that the instrument is not weak, meaning that the instrument and the level of volunteering 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 inference reliable. We therefore conclude that we do not have a weak instrument problem.

Panel II of Table 2.3 summarises the IV estimates of equation 2.1. The estimates presented in odd-numbered columns indicate that volunteering has no effect on mortality of people in their 50s and 60s, while it significantly reduces mortality among people in their 70s and 80s or older. The significance of the estimates remains unchanged after adding socio-economic indicators as controls, and the results reconfirm the previous findings. Column (8) of panel II suggests

that a one-percentage-point increase in the number of volunteers reduces mortality among people in their 80s or older by approximately 0.14 per cent on average, ceteris paribus. Evaluated at the mean, the estimate implies that roughly one life of people aged 80 or older (out of 186 persons) was saved in a given year when volunteers increased by 100 (out of 1,911 persons).

Our estimates capture the net effect of volunteering on mortality and thus do not allow us to determine whether the reduction in mortality is driven by that of volunteers or care recipients. The reduction in mortality of people in their 80s or older is, however, likely to have occurred among recipients given that every volunteer was younger than 80 (Hyogo Council of Social Welfare, 1997). We therefore conclude that the reduction in mortality of people in their 80s or older is caused by that of care receivers. The effect on people in their 70s is inconclusive given that 13 per cent of volunteers in our sample are those in their 70s. It is, however, likely that the reduction in mortality occurred among care recipients because volunteers are presumably fit enough to participate in volunteer activities and are also fitter than care recipients. This inference is further strengthened by the fact that volunteering had no effect on the mortality of people in their 50s and 60s. As roughly half of volunteers were aged between 50 and 69, we would observe a reduction in mortality of people in their 50s and 60s if volunteering reduced mortality of volunteers themselves.[9] It has been established that volunteering reduces mortality

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[9]The estimates in Table 2.3 capture a contemporaneous effect of volunteering on mortality; hence, they reflect a short-run effect of volunteering. Table 2.3 indicates that the short-run mortality of people in their 50s and 60s is not affected by volunteer work. This, however, does not exclude the possibility that volunteering affects mortality of those in their 50s and 60s later in their lives.

of the elderly. The results are, however, not informative about the mechanisms through which volunteering affects mortality. To infer the operating mechanisms, we regress volunteering on mortality decomposed by cause of death. Although it is at best suggestive evidence, examining the types of deaths affected by volunteering would help to narrow down the potential operating mechanisms. Several possible channels are discussed below.

Firstly, it may be the case that volunteering enhances social contact (e.g., Oman et al., 1999) and more social contact subsequently leads to better health and lower risk of mortality (House et al., 1988). Interactions with volunteers may encourage positive emotions, such as happiness and a sense of security, which serves to reduce stressors and thereby improve the health status of an individual. The link between stressors and health status is well documented. For example, Kiecolt-Glaser & Glaser (1995) find evidence that stressors affect the immune system, which in turn influences cancer and infectious illness progression.

Secondly, using volunteer services means that someone visits the elderly regularly, implying that the elderly are found relatively quickly if they, for example, have a heart attack or slip in a bathroom. This is a potentially important channel given that "*kodokushi*", meaning "dying alone", is an important social issue among the elderly in Japan. Since the 1970s, there have been numerous accidents in which the elderly living alone were found dead in their houses some time after they have passed away. The Ministry of Health, Labour and Welfare launched a project to fight against "*kodokushi*", and a project report highlights the importance of social contact as a means to prevent "*kodokushi*" of the elderly. In a similar

vein, regular visits from volunteers also imply that the elderly are provided with much care relative to the absence of volunteers. Recalling that one of the main activities of volunteers is to provide meals, volunteers may help the elderly to have well-balanced meals or remind them to take medicine, which may subsequently improve their general health conditions.

Table 2.4 reports the IV estimates of the effect of volunteering on cause-specific mortality.[10] The results indicate that volunteering significantly reduces mortality caused by cerebral vascular disease, heart disease and decrepitude but has no significant effect on mortality caused by cancer, high blood pressure, pneumonia, diabetes and accidental death. Many diseases categorised as cerebral vascular disease and heart disease are acute diseases such as stroke, intracerebral bleeding and heart attack. Volunteering may thus have reduced mortality caused by these diseases by providing quicker access to medical help when the elderly suffered acute diseases. The inference is further strengthened by the fact that volunteering had no effect on mortality caused by high blood pressure and diabetes that are some of the main causes of cerebral vascular disease and heart disease. If volunteering reduced mortality due to cerebral vascular disease and heart disease by helping the elderly to recover from these illnesses, it would have also affected mortality due to diseases that caused cerebral vascular disease and heart disease. Furthermore, given that the estimates in Table 2.4 are that of the short-

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[10]A caveat is that data on mortality by cause are available by municipality but not by age. Since a type of mortality prevalent among young people, such as mortality related to delivery or suicide, is irrelevant to our analysis, we run regressions only on types of mortality prevalent among people in their 70s or older.

Table 2.4: Effects of Volunteering on Cause Specific Mortality (IV estimates)

Dependent Variable: ln(# of deaths by cause)	
Causes of Death	
(1) Cerebral vascular disease	-0.261***
	(0.079)
(2) Heart disease	-0.182***
	(0.064)
(3) Decrepitude	-0.283**
	(0.123)
(4) Cancer	-0.058
	(0.043)
(5) High blood pressure	-0.056
	(0.084)
(6) Pneumonia	-0.058
	(0.094)
(7) Diabetes	-0.014
	(0.155)
(8) Accidental death	0.089
	(0.069)
Controls	yes
Municip. FE	yes
Year FE	yes
# Obs	837
# Municip.	84

Notes: Standard errors are clustered by municipality. *** denotes $p<0.01$ and ** denotes $p<0.05$. Controls included into the analysis are income per capita, unemployment rate, sex ratio, total population, the number of divorces, high school participation rate, elderly medical expenditure per capita, and municipality and year fixed effects. Every variable in level is logged. Damage indicator corresponds to percentage of people injured.

to medium-term effects of volunteering, it would not be plausible that mortality caused by these serious illnesses is affected by volunteers in the short run.

The significant reduction in mortality caused by decrepitude means that the elderly with no particular known disease who would otherwise have passed away in the absence of volunteers lived longer. A possible interpretation is that volunteers helped to improve general health conditions of the elderly by providing them with well-balanced meals or enabling them to exercise when they might otherwise have been confined to bed.

Mortality caused by illnesses documented to be affected by an improvement in immune system strength, such as cancer, high blood pressure and infectious illnesses (pneumonia), was not affected by volunteering. The results imply that the reduced stress channel is unlikely to be an operating mechanism.[11] Diabetes is also driven by stress, as well as by genes, malnutrition and obesity. Apparently, volunteering does not affect mortality due to diabetes caused by genetic factors. Volunteering could potentially reduce mortality caused by diabetes via improved nutritional status of the elderly, but the process would take a longer period of time than what we are currently looking at.

The insignificant effect of accidental deaths seems plausible. One of the main causes of accidental deaths is traffic accidents. Given that most volunteer activities take place indoors (e.g., providing meals), accidental deaths are likely to be

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[11] An important caveat for the interpretation is that estimates in Table 2.4 are that of short- to medium-term effects. Insignificant effects on mortality caused by cancer, high blood pressure, diabetes and pneumonia in the short run do not exclude the possibility that volunteering affects mortality caused by these diseases in the longer term.

irrelevant to volunteering. Alternatively, our measure of mortality due to accidents may not be sufficiently precise to capture the effects of volunteering on accidental deaths of the elderly, as approximately 40 per cent of people who died due to accidental deaths were younger than 70 years of age.

2.5   Robustness Checks

This section firstly conducts a series of placebo tests to ensure that the observed increase in volunteering is not spurious. Secondly, we examine the robustness of the results to different sample specifications to establish that the results in the previous section are not a product of mere coincidence using a specific sample. We then show that the results are not sensitive to different regression specifications. Finally, some additional results are presented to establish that the results are not driven by some unobserved factors correlated with volunteering that reduce mortality in general.

Firstly, one may be concerned that the results are driven by a difference in the pre-quake characteristics of volunteering. It might be the case that damaged municipalities lie in an area with a high risk of earthquakes and therefore norms of cooperation are well developed, leading to a high level of volunteering even prior to the earthquake in 1995. To address this concern, we falsely assign the earthquake damage to the pre-quake period. Damage indicators are now included separately by year and equal positive values from 1991 to 2000 in the municipalities with positive levels of damage and zero otherwise. The damage indicator in

year 1990 is omitted as the default year.

The estimates reported in Panel A of Table 2.5 indicate a significantly lower level of volunteering (approximately two per cent) in the treatment group in 1991. Although it does not support the hypothesis that the level of volunteering was higher in the treatment group even before the earthquake, the significant difference is potentially worrying if the difference is not constant and persists throughout the pre-quake period.[12] The result, however, shows no statistically significant difference after 1991 prior to the earthquake. In contrast, there is a persistent and statistically significant difference after the earthquake. We therefore conclude that the findings in the previous section are unlikely to be driven by a pre-quake difference in volunteering. An interesting point to note is that the level of volunteering sharply increased by 20 to 25 per cent after the earthquake and stayed at almost the same level, implying that the earthquake caused a persistent rather than a temporary change in volunteering.

In a similar vein, it might also be the case that norms of cooperation are more developed in municipalities closer to the fault because the damaged municipalities lie in an area with a high risk of earthquakes. To address this concern, we omit the treatment group from our sample and falsely assign treatment status to municipalities lying just outside of the original treatment group (fake treatment group). The municipalities retained in the sample (i.e., the fake treatment group and the control group) did not record any damage due to the earthquake and thus were presumably unaffected by the earthquake, while the fake treatment group

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[12]If the difference is systematic, it is absorbed by the municipality fixed effects.

Table 2.5: Falsification Exercises

Dependent Variable: ln(# of volunteers)

Panel A	(1)	(2)	(3)	(4)
% injured 1991	-0.022**	-0.023**	-0.023**	-0.023**
	(0.011)	(0.011)	(0.011)	(0.011)
% injured 1992	-0.004	-0.007	-0.006	-0.006
	(0.080)	(0.080)	(0.080)	(0.080)
% injured 1993	0.044	0.042	0.043	0.041
	(0.069)	(0.069)	(0.069)	(0.069)
% injured 1994	0.057	0.057	0.055	0.074
	(0.089)	(0.089)	(0.089)	(0.091)
% injured 1996	0.228**	0.231**	0.228**	0.245**
	(0.099)	(0.100)	(0.100)	(0.100)
% injured 1997	0.233**	0.234**	0.231**	0.251**
	(0.094)	(0.094)	(0.094)	(0.096)
% injured 1998	0.216***	0.217***	0.214***	0.234***
	(0.073)	(0.074)	(0.074)	(0.074)
% injured 1999	0.195**	0.198**	0.197**	0.218***
	(0.077)	(0.078)	(0.079)	(0.076)
% injured 2000	0.229***	0.233***	0.231***	0.254***
	(0.066)	(0.067)	(0.069)	(0.066)
Panel B	(1)	(2)	(3)	(4)
fake treatment	0.009	-0.003	0.052	-0.002
	(0.198)	(0.175)	(0.172)	(0.164)
Controls	yes	yes	yes	yes
Municip. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes: Standard errors are clustered by municipality. *** denotes $p<0.01$, ** denotes $p<0.05$ and * refers to $p<0.1$. Dependent variable is the logged number of volunteers. Controls included in the analysis are income per capita, unemployment rate, sex ratio, population, the number of divorces, high school participation rate, elderly medical expenditure per capita, and municipality and year fixed effects. Measures of population differ by column: columns (1) to (4) control for the number of people in their 50s, 60s, 70s, and 80s or older, respectively. Every variable in level is logged. Damage indicator corresponds to percentage of people injured. Panel A falsely assigns damage indicator to the pre-quake period, i.e., year<1995. The sampled municipalities are the treatment group, where four most heavily damaged municipalities are omitted, and the control group (84 municipalities) except Panel B (the control and outer control groups, 58 municipalities).

lies closer to the fault. Panel B of Table 2.5 presents the coefficient estimates on a fake treatment dummy that equals one for municipalities in the fake treatment group after the earthquake and zero otherwise. The estimates show no statistically significant difference between the fake treatment and control groups, suggesting that the results in the previous section are unlikely to be driven by a difference in volunteering according to geographical proximity to the fault.[13]

Secondly, a series of robustness checks are conducted using different sample specifications. Recall that we omit heavily damaged municipalities close to the epicentre from the sample to address a concern that the earthquake damage or the earthquake-related disruption had a long-term effect on mortality (cf. Figure 2.3). Given that loss of life was concentrated in the four omitted municipalities, a long-term effect of the earthquake on mortality is likely to be taken care of. One may be concerned, however, that the results are sensitive to the sample specification. To address this concern, we present the results based on the full sample and the samples without the one to five most heavily damaged municipalities. The distribution of mortality due to the earthquake is highly negatively skewed, and the heaviest damaged municipality, Kobe, alone accounts for 71 per cent of the total number of deaths. Following Kobe, the top two to five most heavily damaged municipalities account for 89 per cent, 96 per cent, 98 per cent and 98 per cent of

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[13]There are two municipalities in the treatment group that recorded one person lightly injured and geographically detached from the rest of municipalities. We treat these two municipalities as if there was no one injured and included them in the control group because the objective of this exercise is to check if the level of volunteering differs according to geographical proximity to the fault. Treating the two municipalities as the treatment group and omitting them from the sample yields qualitatively similar results.

the total deaths, respectively.

Table 2.6 presents a series of robustness checks where alternative definitions of heavily damaged municipalities are used. First-stage estimates are robust to different sample specifications and always highly statistically significant. An interesting point to note is that the magnitudes of the coefficients tend to increase as more municipalities are omitted up to four municipalities, implying that the level of volunteering increased most not in the most heavily damaged but in moderately damaged municipalities. A possible interpretation is that people in the most heavily damaged area could not afford to volunteer because they themselves or their houses were heavily damaged. In contrast, people in the moderately damaged area understood the importance of cooperation as they themselves had been affected by the earthquake, but because they did not suffer heavy damage, they could afford to volunteer and help others. Consequently, the notion of volunteering may have developed more in the moderately damaged area. The IV estimates in Table 2.6 also reconfirm the previous findings that volunteering had no statistically significant effect on mortality of people in their 50s and 60s, while it significantly reduced mortality of people in their 70s and 80s or older.

One may be concerned that the results in the previous section are a product of mere coincidence using a particular damage indicator, percentage of people injured, as an instrument. We now investigate if the results are robust to different choices of damage indicators. Panel A-1 of Table 2.7 reports the results where volunteering is instrumented with four different damage indicators: percentage of (i) people seriously injured, (ii) people lightly injured, (iii) households whose

Table 2.6: Alternative Sample Specifications

	First-stage estimates				IV estimates			
Dependent var.	# volunteers	# volunteers	# volunteers	# volunteers	# deaths 50s	# deaths 60s	# deaths 70s	# deaths 80s+
Population measure	people in 50s	people in 60s	people in 70s	people in 80s+	people in 50s	people in 60s	people in 70s	people in 80s+
Sample specifications								
Full sample	0.173***	0.174***	0.171***	0.184***	-0.062	-0.059	-0.105*	-0.150***
	(0.035)	(0.037)	(0.037)	(0.038)	(0.081)	(0.070)	(0.055)	(0.050)
1 omitted municip.	0.173***	0.174***	0.171***	0.184***	-0.0596	-0.0578	-0.104*	-0.150***
	(0.035)	(0.037)	(0.038)	(0.038)	(0.081)	(0.070)	(0.056)	(0.050)
2 omitted municip.	0.172***	0.173***	0.170***	0.183***	-0.056	-0.058	-0.105*	-0.149***
	(0.035)	(0.037)	(0.038)	(0.038)	(0.082)	(0.071)	(0.058)	(0.050)
3 omitted municip.	0.195***	0.197***	0.193***	0.207***	-0.032	-0.013	-0.080**	-0.145***
	(0.031)	(0.033)	(0.035)	(0.033)	(0.071)	(0.048)	(0.038)	(0.044)
4 omitted municip.	0.205***	0.207***	0.204***	0.220***	-0.030	-0.012	-0.076**	-0.140***
	(0.037)	(0.038)	(0.04)	(0.038)	(0.068)	(0.046)	(0.035)	(0.043)
5 omitted municip.	0.203***	0.205***	0.203***	0.218***	-0.031	-0.017	-0.077**	-0.141***
	(0.036)	(0.037)	(0.038)	(0.038)	(0.067)	(0.044)	(0.035)	(0.042)

Notes: Standard errors are clustered by municipality. *** denotes p<0.01, ** denotes p<0.05 and * refers to p<0.1. The first-stage estimates are estimated coefficients on damage indicator where percentage of people injured is used as an instrument for volunteering. The IV estimates are estimated coefficients on β_1 in equation (2.1). Dependent variables are mortality of people in their 50s, 60s, 70s, and 80s or older. Controls included in the analysis are the number of volunteers, income per capita, unemployment rate, sex ratio, population, the number of divorces, high school participation rate, elderly medical expenditure per capita, and municipality and year fixed effects. Every variable in level is logged. The sampled municipalities are full sample, or the control group and the treatment group, where 1, 2, 3, 4 or 5 most heavily damaged municipalities are omitted. The year 1995 is omitted from every sample.

houses are fully destroyed and (iv) households whose houses are half destroyed.

Panel A-1 indicates that earthquake damage had significantly raised the level of volunteering regardless of types of damage indicators, which in turn significantly reduced mortality of people in their 70s and 80s or older. We thus conclude that the results in the previous section are not a product of mere coincidence of using a particular instrument.

To address a concern that the earthquake had damaged medical facilities and subsequently affected mortality, the year 1995 and a subset of municipalities heavily damaged by the earthquake were omitted. Given that most damaged medical facilities resumed operations by the end of 1995 and were located in the omitted municipalities, the concern is likely to be taken care of. Nevertheless, we control for the supply of medical facilities, measured by the number of hospitals and clinics, to investigate if the main findings are not driven by omitting the relevant variable. A limitation of this approach is that the data on the number of medical facilities have a few missing values, particularly in rural areas.

Panel A-2 of Table 2.7 reports coefficient estimates of equation (2.1) with an additional control, the number of medical facilities. The sample size is smaller due to missing values, and standard errors are higher compared to the corresponding estimates in Table 2.4.[14] The effect of volunteering on mortality of people in their 70s seems to be sensitive to the inclusion of this variable: the size of the coefficient estimate is reduced, and the effect is not significant. In contrast, the

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[14]The sample size in Panel A-2 of Table 2.7 is 706 (cf. 837 in Table 2.3), except for the regression of mortality of people in their 50s where the sample size is 694 (cf. 824 in Table 2.3).

Table 2.7: Alternative Regression Specifications

	First-stage estimates				IV estimates			
Dependent var.	# volunteers	# volunteers	# volunteers	# volunteers	# deaths 50s	# deaths 60s	# deaths 70s	# deaths 80s+
Population measure	people in 50s	people in 60s	people in 70s	people in 80s+	people in 50s	people in 60s	people in 70s	people in 80s+
A-1. Alternative damage measures								
% seriously injured	2.690***	2.706***	2.670***	2.867***	-0.067	-0.042	-0.077*	-0.163**
	(0.545)	(0.568)	(0.599)	(0.561)	(0.073)	(0.074)	(0.045)	(0.074)
% lightly injured	0.218***	0.220***	0.217***	0.234***	-0.027	-0.009	-0.076**	-0.138***
	(0.038)	(0.040)	(0.041)	(0.040)	(0.069)	(0.046)	(0.034)	(0.043)
% fully destroyed hh	0.038***	0.039***	0.038***	0.043***	-0.037	0.008	-0.123**	-0.132*
	(0.009)	(0.009)	(0.010)	(0.010)	(0.104)	(0.083)	(0.063)	(0.078)
% half destroyed hh	0.028***	0.028***	0.028***	0.030***	-0.049	0.030	-0.130**	-0.148**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.105)	(0.077)	(0.060)	(0.072)
A-2. Additional controls								
	0.232***	0.229***	0.228***	0.229***	-0.061	-0.158	-0.019	-0.235***
	(0.065)	(0.065)	(0.067)	(0.065)	(0.073)	(0.150)	(0.081)	(0.073)

A-3. Alternative dependent var.

	First-stage estimates				IV estimates			
Dependent var.	# volunteers	# volunteers	# volunteers	# volunteers	# deaths 00s	# deaths 10s	# deaths 20s	
Population measure	people in 00s	people in 10s	people in 20s	people in 80s+	people in 00s	people in 10s	people in 20s	
	0.215***	0.206***	0.208***	-	-0.051	0.040	0.034	-
	(0.039)	(0.039)	(0.039)	-	(0.058)	(0.055)	(0.047)	-

Notes: Standard errors are clustered by municipality. *** denotes p<0.01, ** denotes p<0.05 and * refers to p<0.1. The first-stage estimates are estimated coefficients on damage indicator where percentage of people injured is used as an instrument for volunteering except A-1 (percentage of people seriously injured, lightly injured, and households who had their houses fully destroyed and half destroyed). The IV estimates are estimated coefficients on β_1 in equation (2.1). Dependent variables are mortality of people in their 50s, 60s, 70s, and 80s or older except A-3 (mortality of people in their 00s, 10s and 20s). Controls included in the analysis are the number of volunteers, income per capita, unemployment rate, sex ratio, population, the number of divorces, high school participation rate, elderly medical expenditure per capita, and municipality and year fixed effects. A-2 also includes the number of hospitals and clinics. Every variable in level is logged. The sampled municipalities are the treatment group, where four most heavily damaged municipalities are omitted, and the control group. The year 1995 is omitted from every sample.

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effect on mortality of people in their 80s or older remains significant, and the estimated elasticity of volunteering is now larger in absolute value, -0.235. It might be the case that the omission of this variable biased the estimate towards zero or the effect of volunteering on elderly mortality is larger (in absolute terms) in urban areas.[15]

Finally, we investigate whether the main results are not driven by some unobserved factors correlated with volunteering by regressing volunteering on the mortality of children and young adults. Since the volunteering in focus targets the elderly, it should not affect the mortality of children and young adults if it is indeed the volunteering that drives the main results. Mortality of three different age bands, 0 - 9, 10 - 19 and 20 - 29, is regressed on volunteering and other controls.[16] Panel A-3 of Table 2.7 indicates no effect of volunteering on the mortality of children and young adults. It is thus concluded that the main findings are not driven by some unobserved factors correlated with volunteering that reduce mortality in general.

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[15]Greater supply of medical facilities is likely to reduce mortality. At the same time, there may be more volunteering in a municipality where the supply of medical facility is scarce. If this is the case, the negative effect of volunteering on mortality is positively biased, resulting in a bias towards zero.

[16]As levels of child and young adult mortality are very low in our sample, many observations equal zero; thus, the logged value is not defined. We therefore recode zero to one and take the log of the recoded values to make coefficient estimates comparable to the previous results. Additionally, we also try two different functional form specifications of child and young adult mortality: (i) raw values of mortality and (ii) an indicator variable that equals one if a municipality recorded a positive value of mortality and zero otherwise. The results are not sensitive to the specifications of functional forms.

2.6 Conclusions

This chapter explored the causal effect on age-specific mortality of volunteer work that provides daily assistance to the elderly. To identify the causal effect on mortality, we exploit exogenous variation in volunteering caused by the earthquake that hit the midwestern part of Japan in 1995. The level of volunteering considerably increased in municipalities hit by the earthquake, while other municipalities with no damage did not experience such a sharp increase in volunteering. Based on a comparison of mortality in the municipalities with and without damage, our results suggest that volunteering has no significant effect on mortality among people in their 50s and 60s, while it significantly reduces mortality of people in their 70s and 80s or older. The effect on mortality of people in their 80s or older is almost double its effect on those in their 70s. Evaluated at the mean, the estimate implies that approximately one life of people aged 80 or older (out of 186 persons) is saved in a given year when the number of volunteers increases by 100 (out of 1,911 persons).

Although the nature of the data does not allow us to determine if the reduction in mortality occurred among volunteers or care receivers, that of people in their 80s or older is likely to be driven by care recipients because every volunteer is younger than 80. Our estimates capture a contemporaneous effect of volunteering on mortality; thus, they reflect a short-run effect of volunteering. To the extent that volunteering also reduces mortality of volunteers in the future, effects of volunteering on mortality may be even higher in the long run.

We contribute to the literature on volunteering by revealing the causal effect of volunteering on mortality. Given that population ageing is proceeding at a considerable speed in Japan and burdening public health care expenditures, the contribution of volunteers to support the elderly is important for the maintenance of society. The results also have implications for other countries facing an ageing society. The ratio of the elderly to the working-age population is highest in Japan, 36 per cent, followed by many Western European countries such as Italy and Germany, both at 31 per cent (United Nations, 2011). Coupled with low birth rates, population ageing is projected to proceed further; thus, it is important to build a society in which not only governments but also citizens support the elderly. Finally, most volunteers in our sample are not in the labour force (e.g., housewives). The results suggest that their activities yield social benefits; therefore, policies that encourage or facilitate volunteering may help to mobilise (human) resources available to society to enhance the well-being of the elderly.

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