Doctoral Thesis

Essays on Economics of Disasters and Networks

Yuzuka Kashiwagi

Graduate School of Economics, Waseda University
Supervisor: Professor Yasuyuki Todo

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1. Introduction

Natural threats such as earthquakes, hurricanes, and floods themselves are unavoidable. However, disasters can potentially be mitigated, controlled, and overcome by human efforts because natural threats become disasters only when they cause significant damage to human society, as inferred from the fact that they are not counted as disasters if they happen in non-residential areas. Above all, natural threats can lead to economic development in some cases. For example, the periodical flooding of the Nile River brought nutrient-rich soil to the land, which is said to have contributed to the development of Egyptian civilization. The destruction by natural threats may also bring productivity improvement and thus achieve economic development in the long run through the similar mechanism with Joseph A. Schumpeter's "creative destruction," as suggested by Crespo Cuaresma, Hlouskova, and Obersteiner (2008). The Great Kanto Earthquake followed this pattern (Okazaki, Okubo, and Strobl 2019). Besides, social capital formation driven by natural threats can also be another channel for promoting post-disaster development (Skidmore and Toya 2002). The goal of this dissertation is to contribute to the attempts to pursue the ways to get along with the fierce aspect of nature, exploring the negative impact, the mitigation strategy, and the possible blessing of natural threats.

Recently, the number of disasters has been increasing dramatically worldwide as in Figure 1.1. In contrast, looking at the trend of the number of deaths for the same set of disasters in Figure 1.2, it is not increasing. Rather, today we rarely have unspeakably high fatal events we see in the past such as the mega flood in 1931 in China, reportedly having resulted in 3,700,000 deaths. Thus, Figure 1.2 suggests that human damage has been dramatically mitigated thanks to the development of science and rescue schemes.

However, the amount of total damage to property, crops, and livestock has been increasing drastically as depicted in Figure 1.3. Such a trend can be observed even if we subtract the effect of the increase in the number of events as in Figure 1.4. While it is not a direct translation, it can be safely assumed the positive correlation between such damage on stock and economic damage in devastated areas caused directly by the damage on the stock. Thus, it is most likely that such economic damage due to disasters has also been increasing over years. What is troublesome about economic damage by disasters is that the shocks can spill over to non-devastated areas through supply chains, causing further economic damage not only in but also outside devastated areas. According to Tokui et al. (2012), the damage by supply chain disruption greatly exceeds the economic damage of firms in disaster-hit areas due to stock damage. In total, the damage can be large enough to cause macroeconomic fluctuations (Acemoglu et al. 2012). Therefore, the relative importance to deal with the economic impact of disasters is growing.

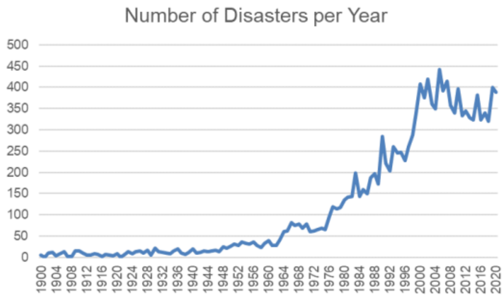
Studies on economics of disasters have begun with those by Hirshleifer (1966), although disasters have long gained attention by economists (Kunreuther and Rose 2004). Many of the quantitative studies have investigated the macroeconomic impact of disasters (Cavallo et al. 2013; Hsiang and Jina 2014; Noy 2009; Noy and Nualsri 2007; Raddatz 2009; Skidmore and Toya 2002; Strobl 2012; Albala-Bertrand 1993; Fomby, Ikeda, and Loayza 2013; Jaramillo 2009; Loayza et al. 2012; Sawada, Bhattacharyay, and Kotera 2011). The pioneer study of empirical micro approach is Kunreuther et al. (1978); (Sawada 2012), but microeconometric research on disasters became relatively popular only quite recently. Although many of them use data without socio-economic networks, anecdotal evidence and qualitative case studies suggest the high possibility that the impact of disasters is exacerbated or mitigated through socio-economic networks (Jones and Faas 2017). More microeconometric research using network information is needed to reveal when and how networks affect the impact of disasters and vice versa. In this essay, we examine the socio-economic impact of disasters and disaster-relief policy, especially focusing on network relationships of economic actors.

Among various networks featured in economics, economics of disasters have discussed the potential importance of network aspects in the context of such as supply chain resilience and social capital formation. Existing literature find that the negative shocks are propagated through input-output linkages (Acemoglu, Ozdaglar, and Tahbaz-Salehi 2015a; Auer, Levchenko, and Sauré 2019; Barrot and Sauvagnat 2016; Boehm, Flaaen, and Pandalai-Nayar 2015; Caliendo et al. 2017; Caliendo and Parro 2014; Carvalho et al. 2016; Cravino and Levchenko 2016; Di Giovanni, Levchenko, and Mejean 2018; Fieler and Harrison 2018; Horvath 1998; Huo, Levchenko, and Pandalai-Nayar 2019; Kikkawa, Magerman, and Dhyne 2017; Long and Plosser 1983; Tintelnot et al. 2018), and cause macroeconomic fluctuations (Acemoglu et al. 2012). Using micro data, studies of the Great East Japan Earthquake and the one assuming homogeneity of disaster impact across disaster types in the United States consistently confirmed negative propagation of natural disaster shocks through supply chains to direct partners outside disaster-hit areas (Barrot and Sauvagnat 2016; Carvalho et al. 2016; Lu et al. 2017). However, the heterogeneity across network characteristics and types of disasters and how the negative propagation effect can be mitigated by post-disaster policy are insufficiently investigated.

As to social capital, a study using country-level data suggests that disasters increase all three dimensions of social capital, i.e., bonding, bridging, and linking social capital, and, through the channel, bring long-run economic growth (Toya 2014). The positive association between social capital and economic recovery seems to be unquestionable (Aldrich 2012). However, at the micro-level studies, both positive and negative impact of disasters on bonding social capital or social preference are found in existing literature. Some existing literature find that the improvement is likely to be associated with social support, but which facets of social support contribute to the change is still not clear. Common facets of social support to be considered are exchange, reciprocity, help seeking, types of support, and types of people giving support (Faas and Jones 2017). Although post-disaster changes and status in support are more or less examined in economics literature, the change in view on help seeking is rarely evaluated due to data constraints. Exceptions are Chantarat et al. (2019) and Chantarat, Lertamphainont, and Samphantharak (2016), which find disaster experience discourages help seeking and significantly reduce trust in neighbors. Another point to be addressed more is the disaster impact on bridging social capital, linkage among different groups, or weak ties. The micro-investigation of it is limited (Andrabi and Das 2017; Fleming, Chong, and Bejarano 2014), although the importance of diversification is often advocated in literature (Beugelsdijk and Smulders 2003; Amiti and Konings 2007; Todo, Matous, and Inoue 2016; Frankel and Romer 1999; Keller 2004; Granovetter 1973; Burt 1992).

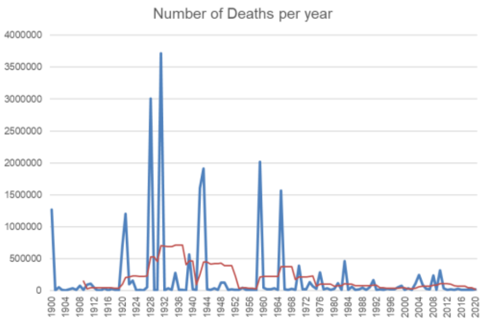
This dissertation consists of five chapters. Chapter 1 is the introduction which describes the motivation and the overview of this study. Chapter 2 examines the negative impact of supply chain disruption by disasters and resilient structures of supply chain networks. Chapter 3 considers the mitigation of supply chain damage by government policy. Chapter 4 explores whether and how disasters facilitate the building of weak ties, which we find essential to supply chain resilience in Chapter 2, using household data. Chapter 5 is concluding remarks.

Figure 1.1 Number of Climatological, Geophysical, Hydrological, and Meteorological Disasters



Data source: EM-DAT (2021)

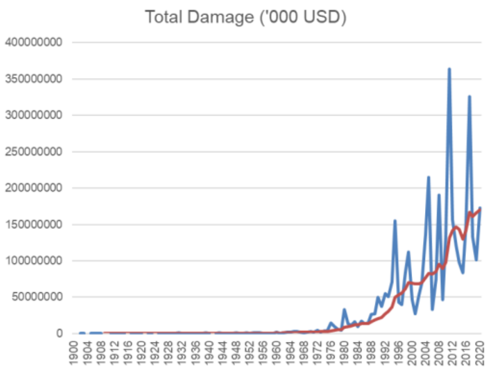
Figure 1.2 Number of Deaths by Climatological, Geophysical, Hydrological, and Meteorological Disasters



Data source: EM-DAT (2021)

Note: The blue line indicates the number of deaths for each year and the red line shows the 10-year moving average.

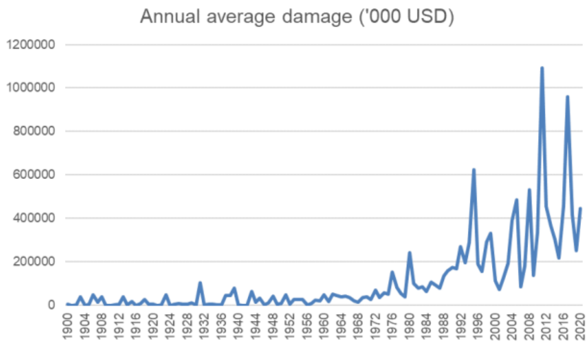
Figure 1.3 Total Damage by Climatological, Geophysical, Hydrological, and Meteorological Disasters



Data Source: EM-DAT (2021)

Note: The blue line indicates the value for each year and the red one shows the 10-year moving average.

Figure 1.4 Annual Average Damage by Climatological, Geophysical, Hydrological, and Meteorological Disasters



Data Source: EM-DAT (2021)

2. Supply Chain Disruption by Disasters

In this chapter, we examine whether and how economic shocks from natural disasters propagate through supply chains to regions not directly hit by disasters. Although such propagation has been studied, less attention has been paid to heterogeneity across network characteristics of firms. Focusing on the impacts of a hurricane and using firm-level data for major firms in the world that include supply-chain ties among them, we find evidence of intra-national propagation of the shock in the US but not inter-national propagation. Our results further suggest that other network characteristics such as network density are important determinants of the level of propagation.

This chapter is based on Kashiwagi, Todo, and Matous (2021) "Propagation of economic shocks through global supply chains – Evidence from Hurricane Sandy" published in *Review of International Economics*.

2.1. Introduction

Negative economic shocks have been found to propagate through input-output linkages to both upstream and downstream firms, leading to a substantial effect on the entire economy (Acemoglu et al. 2012; Bigio and La'O 2016; Caliendo et al. 2014; Di Giovanni and Levchenko 2010). Suppliers of a firm directly affected by a negative shock due to natural disasters may also be indirectly affected because of lack of demand, whereas its customers may be affected because of lack of material, parts, and components. While the literature mostly relies on input-output tables aggregated at the sector level, recent studies have begun utilizing newly available firm-level data with information on supply-chain links to investigate this issue (Barrot and Sauvagnat 2016; Carvalho et al. 2016; Lu et al. 2017). Most of these studies take the Great East Japan Earthquake as a source of exogenous economic shocks because its direct effects are exogenous and extensive (Cavallo, Powell, and Becerra 2010) and because it is one of the disasters that shed lights on supply chain disruptions caused by natural disasters. Another study by Boehm, Flaaen, and Pandalai-Nayar (2015), who examine propagation from parent firms damaged by a disaster affecting their overseas affiliates, also focuses on the Great East Japan Earthquake. These studies confirm that, through domestic supply chains or international shareholding networks, economic shocks by natural disasters can degrade the performance of firms that are located outside the directly affected region.

However, as described in detail in the next paragraph, the case of the Great East Japan Earthquake is unique in several aspects such as types of the supply chains it struck and the scale of it, which are potentially vital to determine the impact. Besides, shareholding networks, which are used to study international propagation, may transmit shocks differently from arms-length transactions. This study extends literature by exploring how different types of networks affect the propagation of shocks through supply chains.

One of the unique aspects of the case of the Great East Japan Earthquake is that Japanese supply chains have special characteristics, often described as *keiretsu* supply chains. Unlike arms-length relationships typical in Western countries and some other worlds, *keiretsu* supply chains are constructed based on close and exclusive vertical relationships among firms (Kosaka et al. 2020; Dyer and Nobeoka 2000). Besides, *keiretsu* firms are often cross-shareholding, assist the development of products one another, and exchange engineers and other personnel to ensure the coordination (Lincoln and Shimotani 2009; Aoki 1988). Accordingly, *keiretsu* supply chains prefer to continue relationships even if problems such that would make firms in arms-length relationships exit the relationship arise (Kosaka et al. 2020). Those strong relationships may affect the resilience of supply chains. Second, Japanese supply chains are also densely connected topologically as featured by two types, "meshed structure" and "diamond structure". "Meshed structure" indicates a complicatedly trundled structure, while "diamond structure" is a structure of supply chains where a certain ultimate supplier provides inputs to all or many intermediate suppliers. Such dense structure of Japanese supply chains may influence the degree of propagation of shocks. Third, the Great East Japan Earthquake is one of the most devastating disasters in the world and is often described as “once-in-a-thousand-year” crisis of Japan. The impact on firms is also extraordinarily as inferred from the fact that governments still provide relief subsidies to firms hit by the Great East Japan Earthquake exceptionally, even though more than 10 years have passed since it struck. Because of those characteristics, there is a room to discuss the external validity of the evidence from the Great East Japan Earthquake case. One exception is the study by Barrot and Sauvagnat (2016), which examines all kinds of disasters in the US, including earthquakes, blizzards, floods, and hurricanes, as a source of exogenous homogeneous shocks, and finds there is propagation of shocks through supply chains. Although the different economic impact by disaster-type, such as more persistent impact of earthquakes on economy, are often observed in literature, it is not clear from their study whether the effect is driven by certain types of disasters. In addition, although they also explore the heterogeneous impact across input specificity, the effects of other types of networks including international links and network structures are not explored.

To address these issues, we take Hurricane Sandy (henceforth, the hurricane) that hit the east coast of the US in October 2012 as a source of negative shocks to investigate how negative shocks from a hurricane propagate both within and across countries through supply chains. The hurricane struck the US as a category 3 (“Devastating damage will occur”) in the Saffir-Simpson Hurricane Wind Scale, which is observed once every a few years at the national level (Blake, Landsea, and Gibney 2011). It reported to have caused an economic loss of US$ 50 billion, the second largest economic loss from a natural disaster worldwide since 2010 (Center for Research on the Epidemiology of Disasters 2017). Importantly, the major hurricane hit a region that had rarely been affected by hurricanes (Kunz et al. 2013; Blake, Landsea, and Gibney 2011). Moreover, even when it was approaching to the US, it was not forecast to hit as a hurricane and thus no official warning for hurricane was issued (Rice 2012). Therefore, the strike of the hurricane was unpredictable.

Specifically, we estimate how much the sales of firms outside the hurricane’s disaster area changed after the hurricane if their direct or indirect customers or suppliers were located in the disaster-hit counties. We use ordinary least squares (OLS) estimation because of the exogenous nature of the hurricane, finding that after the hurricane, the sales growth of US suppliers and customers of firms directly damaged by the hurricane was significantly lower than that of other firms. This finding confirms the upstream and downstream propagation within the country at the firm-level found in Barrot and Sauvagnat (2016) and Carvalho et al. (2016). However, we do not find significant effect on non-US suppliers and customers of firms directly damaged by the hurricane. Our further analysis imply that the difference from Boehm, Flaaen, and Pandalai-Nayar (2015) may be attributed to the vulnerability of shareholding links, special characteristics of Japanese supply chains, and possibly the giganticness of the Great East Japan Earthquake. We also examine the heterogeneity of the propagation effect across network types such as supply chain links in shareholding relationships and those of specific inputs. The results suggest that they can magnify propagation. We further examine how characteristics of supply chains as a network affect propagation, because recent literature find a large role of the structure of interfirm networks in propagation (Acemoglu, Ozdaglar, and Tahbaz-Salehi 2015b; Elliott, Golub, and Jackson 2014; Joya and Rougier 2019), and because Japanese supply chain is characterized by the dense structure as described above. Our analysis reveals that when a firm’s partners are densely connected with each other through supply chains, propagation of negative effects is augmented as the shock can circulate in the dense network.

This study contributes in various aspects to the growing literature on the propagation of economic shocks through supply chains (Acemoglu, Ozdaglar, and Tahbaz-Salehi 2015a; Auer, Levchenko, and Sauré 2019; Barrot and Sauvagnat 2016; Boehm, Flaaen, and Pandalai-Nayar 2015; Caliendo et al. 2017; Caliendo and Parro 2014;

Carvalho et al. 2016; Cravino and Levchenko 2016; Di Giovanni, Levchenko, and Mejean 2018; Fieler and Harrison 2018; Horvath 1998; Huo, Levchenko, and Pandalai-Nayar 2019; Kikkawa, Magerman, and Dhyne 2017; Long and Plosser 1983; Tintelnot et al. 2018). First, we investigate how the network structure, network density in particular, affects propagation rather than simply focusing on direct links with damaged firms. The use of measures of network structure is new in empirical literature on the propagation of disaster shocks through supply chains, although a similar issue has been examined in the context of financial networks (Acemoglu, Ozdaglar, and Tahbaz-Salehi 2015b; Brakman and van Marrewijk 2019; Elliott, Golub, and Jackson 2014). Besides, density of supply chains differs depending on countries and is high in a part of disaster-prone countries such as Japan, and thus it is worthwhile to study whether the difference matters to propagation of shocks through supply chains.

Second, we find the heterogeneous effect by network type such as inter-national links, and supply chain links with shareholding relationships or with research collaboration. The examining the effects of international supply chain links and shareholding links is new to the literature, although it is important in interpreting the findings of the literature as mentioned above and obtain policy implications. A new dataset that includes information on global supply chain links allows us to test such heterogeneity. The last one, the effect of research collaboration, has already examined by Barrot and Sauvagnat (2016) and our results confirm theirs. These findings add to the several strands of literature related to the capability of substituting for damaged firms or production network formation (Huneeus 2018; Lim 2017; Oberfield 2018; Pankratz and Schiller 2019).

Third, our result that the hurricane shocks propagate substantially through supply chains adds to the external validity of the findings from the previous literature that investigate impacts of disasters (Barrot and Sauvagnat 2016; Boehm, Flaaen, and Pandalai-Nayar 2015; Carvalho et al. 2016). Past studies take the Great East Japan Earthquake or occurrence of any types of disasters as a disaster case. The Great East Japan Earthquake is a once-in-a-thousand-year earthquake. We extend the literature by focusing on hurricane shocks of category 3, which are destructive but more frequently occur, often cause different impact on economy from earthquakes, and are increasing in the world possibly due to climate change.

In addition, this study, exploring disaster impact on production activities in and outside the disaster-hit region, relates to the literature on macroeconomic consequences of disasters. In the literature, some studies found a negative macroeconomic effect of natural disasters (Cavallo et al. 2013; Hsiang and Jina 2014; Noy 2009; Noy and Nualsri 2007; Raddatz 2009; Skidmore and Toya 2002; Strobl 2012), while some others found no significant or even positive effect (Albala-Bertrand 1993; Fomby, Ikeda, and Loayza 2013; Jaramillo 2009; Loayza et al. 2012; Skidmore and Toya 2002; Sawada, Bhattacharyay, and Kotera 2019). These mixed results may partly come from heterogeneity of supply-chain characteristics that leads to different degrees of economic impacts, as found in this study. The literature on macroeconomic consequences across disaster types also suggest that climatic disasters are associated with better recovery than geologic disasters, such as earthquakes (Skidmore and Toya 2002; Sawada, Bhattacharyay, and Kotera 2019). Consistently, our study using the hurricane as well as a previous literature using various disasters find less propagation than the one using an earthquake.

Finally, our findings suggest a policy implication that diversifying supply-chain partners can mitigate propagation of shocks through supply chains and thus, lead to economic resilience. Although some studies argue the policy implications of the role of supply chains in recovery from disasters (Todo, Nakajima, and Matous 2015), better supply-chain structures to mitigate propagation of disaster shocks have not yet been proposed in the literature.

2.2. Data

2.2.1. Data Sources

This study uses two datasets, LiveData of FactSet Revere and Osiris of Bureau van Dijk. LiveData is a unique firm-level dataset that covers some 110,000 major firms from around the world, including 17,656 in the United States (US), and contains information on supply-chain ties among them, which is collected from public sources, such as financial reports, firm websites, and news articles. Supply-chain information has become widely available through the Internet. Most importantly, in the US, the Financial Accounting Standards Board requires publicly listed firms to disclose customers who account for at least 10% of total sales, including foreign customers, in their financial reports. After automatically collecting information from the Internet and identifying the identification number of each supplier and customer, trained analysts of FactSet Revere manually verify it. Therefore, the coverage of supply-chain information in LiveData is sufficiently high, at least as high as that in Compustat, which also relies on information from US financial reports and has been used in previous studies, such as Barrot and Sauvagnat (2016), as we will show later. Although LiveData focused on US firms in earlier periods, it has recently expanded its coverage to other regions, mostly Europe and Asia. In this regard, an advantage of LiveData over Compustat is that the former includes more extensive supply-chain information of non-US firms. We utilise LiveData for 2011, one year before Hurricane Sandy, to identify pre-disaster global supply chains, which include 110,316 firms and 71,669 supply chain ties. Among the 110,313 firms, 17,656 are located in the US, 3,908 in Japan, and 2,499 in the United Kingdom (UK).

The other dataset, Osiris, includes firm-level data for mostly publicly listed firms in a number of countries. Because Osiris contains detailed and globally comparable financial information, we extract from Osiris each firm's information such as sales, value of total assets, number of employees, firm age, industry code, and fiscal year end.[1] In this data, subsidiaries of a multinational company are not included.[2]

We merge LiveData and Osiris first using the International Securities Identification Number (ISIN), then using Ticker Symbols, countries, and company names for unmatched firms, and finally using zip codes, countries, and company names. Among 37,698 listed firms in Osiris, we have to omit from the sample for regressions 11,178 firms that cannot be matched with any firm in LiveData, although we construct network-related variables from the LiveData before the merge. We exclude firms in the financial and real estate industries and governments, assuming that those are less likely to be affected by supply chain disruptions caused by natural disasters. Our benchmark regressions restrict our sample to firms that were not directly hit by Hurricane Sandy to examine propagation from damaged firms only to firms that were not directly damaged by the hurricane. Therefore, we exclude 714 firms in areas damaged at least moderately, as defined by the Federal Emergency Management

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[1] They are on consolidated basis unless firms have no subsidiary. Consolidated value of sales is the sum of the sales of parent firms and consolidated subsidiaries, and non-consolidated subsidiaries are treated as a separate firm. The use of consolidated value involves the similar issue with the use of headquarter address to identify firms' location because some consolidated subsidiary may locate in disaster areas, leading to underestimation. However, consolidated value is globally favored when comparing performance among firms because non-consolidated value makes it hard to paint a precise picture as parent firms can strategically manipulate and is also largely influenced by each dependency on its consolidated subsidiaries.

[2] For example, Microsoft Japan Co. Ltd, Microsoft's subsidiary based in Japan, is not included and the country information for Microsoft Corporation is recorded as the United States.

Agency (FEMA) (Federal Emergency Management Agency 2014), and shown as hatched areas in Figure 2.1. In addition, we limit the sample to those whose operation status is active as of 2012, dropping 21 inactive firms. Finally, we exclude firms without sufficient information. Accordingly, the number of observations for our benchmark regression is 11,697, among which 1,984 are in the US, 2,487 in Japan, 2,050 in China, 946 in Taiwan, and 558 in the UK, as shown in Table 2.1. The same table presents the number of publicly listed firms in 2011 in each of the top five countries in our sample in column (4) and the ratio of firms in our sample (for the US, we add the number of listed firms in the disaster areas that are dropped from the sample) to publicly listed firms in column (5), showing that the coverage for most countries is reasonably high.

Because of the coverage of LiveData and Osiris, our dataset mostly focuses on major transactions among publicly listed firms and relatively large firms in the world. For example, the average number of suppliers per firm in our dataset is approximately two, whereas the median number of employees of any firm's suppliers and customers is 2196.5.

Finally, it should be noted that our analysis is conducted at the firm level, not at the establishment level, because the locational information of establishments of firms is limited, and because we do not have any information about supply-chain relationships between establishments or sales of establishments. See Appendix A2.1 for more details.

2.2.2. Variable Construction

Following the previous literature on propagation of disaster shocks, our key independent variables are dummies for the existence of each firm's suppliers and customers that were directly damaged by Hurricane Sandy. To create these variables, we first identify the global supply chains in 2011, one year before Hurricane Sandy, using all firms in LiveData, including observations omitted from our estimation sample.

Next, we define firms directly damaged by Hurricane Sandy as 774 firms whose headquarters are in 'very highly damaged counties', according to the Federal Emergency Management Agency (2014). In these highly affected regions (filled-in areas surrounded by the hatched area, in New York, New Jersey, and Connecticut in Figure 2.1), more than 10,000 people in each county were exposed to storm surge, many buildings were flooded more than one meter in depth, and their exterior walls collapsed (Federal Emergency Management Agency 2013, 2014). It is most likely that the production activities of firms subjected to such conditions were heavily disturbed. We create a dummy variable coded one if any of each firm's suppliers is located in these heavily affected counties and another coded one if any of its two-step suppliers (suppliers of suppliers) is located in these counties. In addition, we define corresponding dummies for customers and two-step customers in the heavily affected counties.

To control for the size of the production network of each firm, we include the log of the number of suppliers, two-step suppliers, customers, two-step customers, suppliers of customers, and those transaction partners in the US plus one in the set of independent variables. We also control for the internationalization of the focal firm, using the logs of the number of suppliers and customers in the country in which the focal firm is located plus one. We also incorporate another measure, Bonacich (1987)'s power centrality to represent each firm's centrality in the global supply chain. Although the number of supply chain partners is also a measure of network centrality, it captures only direct links and ignores indirect links or the network characteristics of partners. Bonacich (1987)'s power centrality of $u, c_u(\alpha, \beta)$ is defined as $c_u(\alpha, \beta) = \sum_v (\alpha + \beta c_v) A_{uv}$, where β and A are an attenuation parameter, and the graph adjacency matrix. α is set such that $\sum_u c_u(\alpha, \beta)^2$ equals the number of vertices. Bonacich (1987)'s power centrality is designed to evaluate the bargaining power, incorporating the aspect that the partner firms with more links have more power in negotiation as they have more options.

In some specifications, we incorporate the density of firms' egocentric networks, measured by the local clustering coefficient. The density of ego network is defined as the ratio of the number of actual supply-chain ties between partners of the focal firm to the number of all possible ties between them (Barabási 2016; Jackson 2010). For example, if a firm has three partners and two among the three are also connected with supply chains, its density is one-third. When a firm is not linked with any other firm or linked with only one firm, we define the density as zero. In the estimations, we also include dummy variables for no link and one link to account for any possible bias due to the arbitrary definitions.

The dependent variable is sales growth from 2011 to 2012. Sales growth is calculated as $SalesGrowth_{i(2011-2012)} = 2 \times \left((netsales_{2012} - netsales_{2011})/(netsales_{2012} + netsales_{2011})\right)$. This is the form recommended by Cravino and Levchenko (2016) and Davis, Haltiwanger, and Schuh (1996). The denominator is the average sales of the two periods, not the pre-disaster sales. Some of the attractive features of this form is that it can be calculated for observations whose values at the start or end period is zero and is bounded between -2 and 2. Control variables include sales growth from 2010 to 2011 to capture the pre-disaster characteristics, sales per worker in 2011 in log form to represent productivity, and the number of workers and value of total assets in 2011 in log form to represent firm size, firm age, interaction terms between industry dummies and country dummies for firms outside the US, and those between industry dummies and state dummies for firms in the US. These interaction terms with industry dummies help us to control price changes at the level of industry-country pairs for non-US firms and industry-state pairs for US firms. The single terms of each interaction term are also included. The dependent variable and these controls are constructed based on Osiris data. We use industry dummies based on the firms' four-digit industry group codes in the Global Industry Classification Standard (GICS). Country dummies are classified by the location of the firm headquarters.

2.2.3. Descriptive Statistics

The upper rows of Table 2.2 show the summary statistics for the variables related to supply chains. The mean and median of the number of suppliers is 1.618 and 0, respectively. On average, the number of domestic suppliers in our data is 0.784, indicating that the number of domestic suppliers and that of foreign suppliers do not differ substantially. Focusing on US firms, we find that their average number of domestic suppliers is 3.917. [3]

Looking at the mean of the dummy variable for damaged suppliers, we find 3.9% of all firms in our global sample are directly connected to suppliers directly damaged by the hurricane. When we disaggregate the dummy for any link with damaged suppliers into a dummy for US firms and non-US firms, 2.7% of US firms are directly linked to suppliers in the damaged area.

The mean of the number of customers is 2.083. The mean of the number of domestic customers for the global sample and the US sample is 0.805 and 3.802, [4] respectively. Including indirect links, firms in the sample have on average 24 two-step customers. Furthermore, 2.9% of US firms have customers in the damaged areas, while the corresponding figure for non-US firms are 1.2%.

The bottom rows of Table 2.2 indicate summary statistics of other network measures and other control variables. The median pre-disaster sales growth is 7.9%, whereas the median number of workers and firm age are 931 and 22 years, respectively. These figures confirm that the sample firms are mostly large, established, and

[3] The corresponding average found by Barrot and Sauvagnat (2016), 1.38, who use the Compustat Segment files to identify suppliers and customers.

[4] The mean for US firms reported in Barrot and Sauvagnat (2016) is 0.711.

growing firms.

Appendix Table 2.1 reports the share of each of the four-digit industries classified by the GICS, as explained in Subsection 2.2.2. The major industries in our sample are the capital goods, materials, and technology hardware and equipment industries. Capital goods industry include, for example, manufacturers of industrial machinery and industrial components, non-residential construction, and electric cables and wires, electrical components or equipment. Many firms in materials industry in our sample produce various chemicals, iron and steel and related products, while some produce other material and related products such as metal, paper, and construction materials. Technology hardware and equipment industry mainly includes manufacturers of electronic component, electronic equipment and instruments, communication equipment and products, technology hardware such as personal computers and cellular phones. These three industry categories account for approximately 40 percent in our sample. Residential building construction is categorized in consumer durables and apparel industry, but its share in the industrial category is small in our sample. Although it is hard to compare with previous studies because exact proportions of each sector in their sample of analysis are not available, our sample may include more manufacturers and less construction business. Carvalho et al. (2020) report that the share of construction is the largest in their data, followed by wholesale and retail, before data preprocessing, and these two account for more than half, while the share of manufacturing industry is less than 15 percent, among 18 industrial categories of Japan Standard Industrial Classification.

2.3. Empirical Strategy

2.3.1. Conceptual Framework

Our conceptual framework is built upon general equilibrium models of production networks developed and extended by Acemoglu et al. (2012), Barrot and Sauvagnat (2016), and Carvalho et al. (2016) as detailed in Appendix A2.2. When disasters such as a hurricane and an earthquake hit a firm, their production capacity is destroyed by them and thus affect the output of disaster-hit firms. When these firms are suppliers of parts and components, their customers outside disaster-hit areas may suffer from the lack of inputs and thus be forced to reduce its production. The propagation of shocks in the opposite direction may also be the case due to lack of demand. Similarly, if there is reduction in output of suppliers and customers outside the disaster-hit area, it may affect their suppliers and customers, i.e., 2-step suppliers and customers of disaster-hit firms. Therefore, our first set of hypotheses is as follows.

Hypothesis 1: The sales growth of customers of firms directly damaged by a natural disaster is on average lower than that of customers of undamaged firms as a result of supply chain disruptions.

Hypothesis 2: The sales growth of two-step customers of firms directly damaged by a natural disaster is on average lower than that of two-step customers of undamaged firms, and the effect on their two-step customers is smaller than the effect on their direct customers.

Hypothesis 3: The sales growth of suppliers of firms directly damaged by a natural disaster is on average lower than that of suppliers of undamaged firms as a result of supply chain disruptions.

Hypothesis 4: The sales growth of two-step suppliers, that is, suppliers of suppliers, of firms damaged directly by a natural disaster is on average lower than that of two-step suppliers of undamaged firms, and the effect on their two-step suppliers is smaller than the effect on direct suppliers.

These hypotheses have already been supported empirically by Barrot and Sauvagnat (2016) and Carvalho et al. (2016) using data for supply chains within a country, although the effect on two-step suppliers and customers is found smaller but not necessarily significant. The present study extends their analysis to incorporate global supply chains. One possible cause of the difference is related to the difficulty of finding substitutes for damaged suppliers and customers. Barrot and Sauvagnat (2016) find, both theoretically and empirically, that shocks propagate more through supply chains when inputs are more specific and substitution is more difficult. Recently, theoretical and empirical analyses by Allen (2014), Antràs, Fort, and Tintelnot (2017), and Bernard, Moxnes, and Saito (2019) incorporate costs of searching for transaction partners and find that the search cost is an important determinant of partners. In our context, their results imply that when a firm's suppliers or customers are affected by a negative shock, the firm can substitute new partners for damaged ones only if the expected benefit from having new partners exceeds the search cost.

Under these circumstances, substitution of suppliers and customers within a country may or might not be more difficult than substitution across countries. On one hand, internationalized firms using a wide variety of inputs, including those from foreign countries, may have greater ability to collect information in the world market than firms with only domestic partners. Therefore, internationalized firms' costs of searching for new partners are lower than those of non-internationalized firms. In addition, as internationalized firms are likely to be more productive, as argued by the heterogeneous-firm trade model of Melitz (2003) and evidenced by many empirical studies, such as Bernard and Jensen (2004), internationalized firms can obtain larger benefits from continuing production by finding new partners and thus, are willing to pay search costs. Therefore, internationalized firms may find it easier to substitute for inputs from firms affected by a shock. If this is the case, international propagation of shocks is smaller in size than intra-national propagation. On the other hand, inputs from a foreign country may be more specific to the exporting country and unavailable domestically. If so, international substitution for inputs is more difficult than intra-national, and thus, shocks propagate more across countries than within a country. Therefore, we obtain the following set of hypotheses.

Hypothesis 5a: The negative effect of damaged firms on their customers (suppliers) in the same country is on average larger than the effect on their customers (suppliers) in different countries.

Hypothesis 5b: The negative effect of damaged firms on their customers (suppliers) in the same country is on average smaller than the effect on their customers (suppliers) in different countries.

Finally, we investigate the role of network density in the propagation of shocks. The egocentric network of a particular firm is considered to be dense when partners linked with the firm are also linked with each other. Theoretically, there are two conflicting views of the effect of network density on propagation. On one hand, in a dense network, shocks are circulated and thus, may be amplified. In the context of diffusion of behaviours, Centola (2010) empirically finds quicker diffusion in dense networks than sparse ones. On the other hand, in a dense network, individuals and firms are more likely to trust each other, creating social capital (Coleman 1988). In this situation, firms in dense supply chains may help each other absorb shocks in the wake of a disaster. Such dense supply chains are typically found in the *keiretsu* relationship among Japanese firms, where each large final assembler and its direct and indirect suppliers form an exclusive group of firms (Ahmadjian and Lincoln 2001; Aoki 1988). After the Great East Japan Earthquake in 2011, damaged firms involved in the *keiretsu* supply chains are found to more quickly recover from the disaster (Todo, Nakajima, and Matous 2015).

In practice, network density's two opposing forces lead to mixed results regarding its effect on propagation both theoretically and empirically. For example, Acemoglu, Ozdaglar, and Tahbaz-Salehi (2015b) use a theory of interbank networks and find that dense networks are resilient to a sufficiently small financial shock because the shock is absorbed. However, they find that when a shock is sufficiently large, dense networks are not resilient because the shock cannot be absorbed but is instead circulated and amplified. Accordingly, our last set of hypotheses is as follows.

Hypothesis 6a: The negative effect of damaged firms on their customers (suppliers) is on average larger in absolute value when the customers' (suppliers') ego network is denser.
Hypothesis 6b: The negative effect of damaged firms on their customers (suppliers) is on average smaller in absolute value when the customers' (suppliers') ego network is denser.

2.3.2. Estimation Methodologies

To test these hypotheses, we consider the following estimation equation and estimate the propagation effect by OLS, following Barrot and Sauvagnat (2016) and Carvalho et al. (2016) :

$$SalesGrowth_{i(2011-2012)} = \beta_0 + \beta_1 ShockXUS_i + \beta_2 ShockXnonUS_i + \beta_3 X_{i2011} + \epsilon_{i2012}. \tag{2.1}$$

The dependent variable, $SalesGrowth_{i(2011\text{-}2012)}$, is the growth rate of sales of firm *i* from 2011 to 2012. Firm *i* can be either in the US but outside the disaster area or in any other country in the world.

Shock, or more precisely *ShockXUS* and *ShockXnonUS*, is a vector of key independent variables that represent ties with suppliers and customers directly hit by Hurricane Sandy. When we examine downstream propagation, that is, propagation from suppliers to customers, we measure ties with directly damaged suppliers using the dummy for the existence of damaged suppliers of firm *i*, following the previous studies (Barrot and Sauvagnat 2016; Carvalho et al. 2016). In addition to firm *i*'s direct ties, *Shock* includes measures of suppliers of firm *i*'s suppliers, or firm *i*'s two-step suppliers, that were directly hit by the hurricane. To distinguish between propagation within the US and beyond the US, we classify *Shock* variables by the location of firm *i*, either in the US or outside the US, using the interaction terms between US or non-US dummy and *Shock* variables. Similarly, when we examine upstream propagation from customers to suppliers, we use the dummy variables for firm *i*'s damaged customers and damaged two-step customers. The vector of the control variables *X* includes firm attributes and network related variables, as described in Subsection 2.2.2.

The end month of the fiscal year reported in Osiris varies across countries and firms, although it is December for the majority, or 64 percent, of firms (See Appendix A2.3 and Appendix Table 2.2 for more details). Because sales of each firm in our sample cover different time periods depending on its fiscal year end, the effect of supply-chain ties with directly affected firms on sales growth should also be affected by the fiscal year end. Therefore, for all the regressions throughout the paper, we include dummies for different fiscal-year end months and interaction terms between these dummies and the key variables for supply-chain ties, setting December as the base month. That is, our actual estimation equation is given by:

$$\begin{aligned} SalesGrowth_{i(2011-2012)} &= \beta_0 + \beta_1 ShockXUS_i + \beta_2 ShockXnonUS_i \\ &+ \sum_{k=January}^{November} \gamma_{1k} D_{ik} \times ShockXUS_i \\ &+ \sum_{k=January}^{November} \gamma_{2k} D_{ik} \times ShockXnonUS_i \\ &+ \sum_{k=January}^{November} \gamma_{3k} D_{ik} + \beta_3 X_{i2011} + \epsilon_{i2012}, \end{aligned} \tag{2.2}$$

where *ShockXUS* and *ShockXnonUS* are the vector of key independent variables as explained in equation (2.1), i.e., ties with suppliers and customers directly hit by Hurricane Sandy for US firms and non-US firms, respectively, and D_{ik} is the dummy variable that takes a value of one if firm *i*'s fiscal year end month is month *k*.

2.3.3. Identification and Checks

Our identification assumption is that our treatment variables, the existence of links with damaged suppliers or customers, are exogenous after controlling the number of suppliers and customers. This assumption is likely to hold, considering the following characteristics of Hurricane Sandy. First, it hit a region that had rarely been affected by hurricanes at least for the past 160 years, according to geoscience papers by Blake, Landsea, and Gibney (2011) and Kunz et al. (2013). In other words, the strike of the hurricane was not anticipated in advance. Second, even when it was approaching to the US, it was not forecast to hit as a hurricane and thus government agencies that are responsible for issuing warnings for hurricanes and tropical storms issued no warnings for the hurricane (Rice 2012). Therefore, the strike of the hurricane was unpredictable. However, there may be systematic difference between firms with links with damaged suppliers and customers and those without, and the unobserved characteristics in error terms may be correlated with the treatment variables. If this is the case, estimation results are biased. Therefore, we need to test whether the above assumption holds.

For this purpose, we run OLS estimations to examine whether a firm's supply chain links to damaged suppliers or customers can predict sales growth before the disaster, including network variables, the interaction terms between country and industry dummies, and those between US-state and industry dummies as control variables. Table 2.3 shows that neither the dummy for the existence of suppliers nor customers in disaster areas is correlated with sales growth before the hurricane. The results suggest that sales growth rate was balanced before the disaster, and hence, our key variables of interest, the dummies for the existence of links with damaged firms, is likely to be uncorrelated with the error term in equation (2.2).

However, we are still concerned about whether our results are generated by the peculiar characteristics of damaged firms because the hurricane hit the industrial areas on the east coast of the US around New York City. To check whether this is the case, we experiment with two sets of placebo tests. First, we estimate the effect of supply-chain links with firms that are not in the damaged areas of the hurricane but are similar in attributes to those in the damaged areas. We select these firms similar to damaged firms from all US firms not directly damaged by the hurricane using a propensity score matching (PSM) technique. That is, we estimate how the dummy variable for damaged firms is correlated with the number of workers, amount of total assets, and industry dummies using logit and match each damaged firm with another with the closest predicted probability. Second, we estimate the effect of links with firms in neighbouring states of the very highly damaged areas, such as Vermont, New Hampshire, Maryland, the District of Colombia, Ohio, Virginia, and West Virginia. In this placebo test, we assume that firms geographically close to damaged firms are similar in attributes to damaged firms.

The results from the first and the second placebo tests are shown in Appendix Table 2.3 and Appendix Table 2.4, respectively. In the tables, some of the effects of the domestic links with firms similar in attributes to or geographically close to damaged firms are positive and significant, while mostly no effect is negative and significant. These results suggest that because firms in the damaged areas around New York City are likely to be more advanced than those in other areas in the world, a firm's links with firms similar to the damaged firms are often positively correlated with the firm's sales growth. Besides, the lack of significant effect of direct links with firms geographically close to damaged ones in Appendix Table 2.4 suggest that our baseline findings are not likely to be driven by macro shocks to east coast of the United States. An exception is negative and significant coefficients of the dummy for any two-step link with customers near the disaster areas (columns [2] and [4] of Appendix Table 2.4). However, because we do not find any significant effect of the dummy for any two-step link with firms in the disaster areas as we will observe later (Table 2.5 and Table 2.6), we do not regard the two sets of evidence as showing overestimation of the propagation effect through two-step links.

2.4. Results

2.4.1. Results for Direct Effects of Disaster Shocks

Before we explore the propagation of disaster shocks, we first test whether or not the sales growth of the firms whose headquarters are located in the counties that were hit by Hurricane Sandy is negatively affected. For this purpose, we use a sample different from the main sample explained above, or a sample of both US firms in and outside the disaster-hit areas taken from the same data sources as the main sample, and regress the sales growth of firms on a dummy variable indicating whether the firm was in the disaster-hit areas. Consistent with the baseline estimation, we set December as the base month of fiscal year end. It is dominant, approximately 70%, also in the estimation sample of Table 2.4. In this estimation, we control for firms' characteristics, such as sales growth in the pre-disaster period, sales per worker in 2011 in log form, the number of workers in 2011 in log form, value of total assets in 2011 in log form, firm age, and interaction terms between industry dummies and state dummies.

The results are presented in Table 2.4. As shown in column (1), the coefficient of the disaster shock dummy is negative and significant. Such a negative effect of the shock dummy is not observed for alternative shock dummy that indicates whether the firm is in the counties with lower damage (column (2)), suggesting that the hurricane had negative impact on the sales growth of the firms in counties that suffered 'very highly' from the hurricane. In Appendix Table 2.5, we further find that the direct disaster shock is not significantly correlated with pre-disaster sales growth. These findings suggest that the hurricane had negative impact on the sales growth of the firms in counties that suffered 'very highly' from the hurricane while these affected firms were not systematically different from others in the pre-disaster period.

2.4.2. Benchmark Results

For the remaining analysis, we use the baseline sample of firms that are not directly damaged by the hurricane as described in Section 2.2.1. The benchmark results of downstream propagation of disaster shocks are presented in Table 2.5. In all estimations, we include the interaction terms between each of the dummies for the end month of the accounting period and the key independent variables for links with damaged firms but do not present the results for the interaction terms for brevity of presentation. Because the base case for the fiscal year end is December, the coefficient of the key independent variable represents the effect on the change rate of sales from calendar year 2011 to calendar year 2012.[5]

Columns (1) and (2) of Table 2.5 suggest that when US firms are linked with any damaged supplier, their sales growth is 1.89 percentage points lower than when they are not directly linked with any damaged supplier. This result suggests that negative shocks of the hurricane propagated downstream along supply chains to US customers and the propagation effect is not only statistically significant but also economically significant, supporting Hypothesis 1 in Subsection 2.3.1. In column (3) of Table 2.5, we consider the potential direct impact from other major disasters that hit the US from 2010 to 2012 by including the dummy variable that indicates whether the firm's headquarter is located in the counties hit by the other disasters during the period. Although Hurricane Sandy is much bigger than any other during the period, the other disasters may have had an influence on firms' sales and biased the results. Following Barrot and Sauvagnat (2016), we obtain the list of disasters and county-level location information of each disaster from Spatial Hazard Events and Losses Database for the United States Version 18.1.

[5] Although the benchmark results focus on sales growth from 2011 to 2012, we repeat the estimation using sales growth from 2011 to 2013 as a dependent variable. The results are in Appendix Table 2.9.

As in column (3), the results are not affected even after considering the effect from other disasters.

Furthermore, in Table 2.5, we observe that the coefficients of the links with damaged suppliers for their non-US customers (*Dummy for any link with damaged suppliers × non-US dummy*) are positive but statistically insignificant, indicating no significant effect on non-US customers. It supports hypothesis 5a. This evidence implies that the negative shock from the hurricane did not significantly propagate downstream beyond the US borders. Boehm, Flaaen, and Pandalai-Nayar (2015) find the international propagation from Japanese parent firms to foreign affiliates in the wake of the Great East Japan Earthquake. The difference from their study probably comes from two reasons. First, because the magnitude of the direct damages by the Great East Japan Earthquake was substantially larger than that by Hurricane Sandy, the economic effect of the former is more likely to propagate widely than that of the latter. Second, the difference in the results may reflect the difference in flexibility of links between intra-firm and arms-length transactions. In intra-firm transactions, inputs are more likely to be firm-specific and unavailable in the market. As a result, propagation between unaffiliated firms has been found to differ from that between affiliated firms according to some anecdotal evidence (Hattori 2011).

In addition, the coefficients of indirect two-step links with damaged suppliers in columns (2) and (3) of Table 2.5 are negative but insignificant for both US and non-US customers. Although the size of the coefficients of two-step links is larger than that of the direct links, it is likely because we do not consider the number of such links. As we explore in the next section or column (1) of Appendix Table 2.6, we observe smaller and insignificant coefficient for 2 step links when we use the number of links instead of the dummy variable. Besides, the sample size of this study is sufficiently large and thus it is scarcely likely that the statistical insignificance is driven by low statistical power. Thus, we interpret the result as it implies that there is no propagation of the negative shock from the hurricane beyond direct customers. This result does not fully support Hypothesis 2, implying that there is no propagation beyond direct customers. However, Hypothesis 2 also states that the effect on indirect customers would be smaller than those on direct customers because negative shocks are absorbed along supply chains due to substitution of partner firms. Our results indicate that negative shocks substantially diminish along supply chains and disappear in two steps. Barrot and Sauvagnat (2016) also find no downstream propagation beyond direct customers based on firm-level panel analysis for the US. Although Carvalho et al. (2016) find propagation beyond direct customers up to customers four steps away from damaged suppliers after the Great East Japan Earthquake, this is possibly because of the extremely large direct effect of the Great East Japan Earthquake, as mentioned in the earlier paragraph of this section and Section 2.1. Another possible reason for the difference is that supply chains of Japanese firms are likely to be more vulnerable to economic shocks, because they are characterized by strong and long-term relationships known as *keiretsu* (Dyer and Nobeoka 2000). In a *keiretsu* relationship, inputs are often developed through the collaboration between the supplier and the customer and thus, firm specific or even product specific (Aoki 1988). Moreover, severing long-term relationships is more difficult (Altomonte and Ottaviano 2009). The input-specific and long-term *keiretsu* relationships may have led to the propagation of the shock to indirectly linked supply-chain partners observed in Carvalho et al. (2016).

The results for upstream propagation from damaged customers to their suppliers in Table 2.6 suggest that upstream propagation is similar to downstream propagation. Table 2.6 shows negative, significant, and large effects of the existence of links with damaged customers on sales growth of US suppliers, which is consistent with Hypothesis 3. If a firm with no link with damaged customers were linked with a damaged customer, its sales growth rate would decline by 5.15 percentage points because of propagation of negative shocks. This finding suggests that domestic suppliers of directly damaged firms are affected by a lack of demand from damaged customers immediately after the hurricane. It is notable that the effect of damaged firms on their supply-chain partners (Table 2.5 and Table 2.6) is smaller than the effect of the hurricane on damaged firms (Table 2.4), confirming that the indirect effect is smaller than the direct effect.

By contrast, consistent with hypothesis 5a, we do not find any significant effect of links with damaged customers on non-US suppliers in Table 2.6, as in the case of the effect of links with damaged suppliers in Table 2.5. This result suggests that negative shocks did not propagate significantly from damaged customers in the US to their suppliers outside the US.

Lastly, two-step links with damaged customers have a smaller and insignificant effect on US and non-US suppliers (columns [2] and [3], respectively, of Table 2.6), partly supporting Hypothesis 4. Therefore, we conclude that damaged customers did not negatively or substantially affect their indirect suppliers.

2.4.3. Robustness Checks

To check the robustness of the benchmark results, we experiment with several alternative specifications. First, the benchmark estimations define firms directly damaged by the hurricane as those in 'very highly damaged areas' identified by FEMA (2014) (Figure 2.1). However, in these 'very highly damaged areas', there must have been a variation in the level of damages across firms and households. If this is the case, some of the firms defined as damaged in our data were not heavily damaged, and accordingly, the effect of links with 'damaged firms' in our data underestimates the effect of links with truly damaged firms.

To identify links with truly damaged firms, we assume that when a firm is extremely damaged and thus, could not recover or needed a very long time to recover, supply-chain links with the firm should have been lost after the hurricane. Therefore, we alternatively define links with damaged firms as those that existed in the pre-disaster supply-chain link data but did not exist anymore in the post-disaster data. Then, we conduct OLS estimations as before, using the alternatively defined dummy variable for links with damaged firms and report the results in Table 2.7. Columns (1) and (2) indicate that the effect of direct suppliers and customers which suffered from the disaster on sales growth of US firms is negative and significant, while their effect on firms outside the US is insignificant. These are consistent with the benchmark results, confirming propagation within the country but no significant propagation beyond the country.

Second, in the benchmark regressions, firms in the disaster-hit areas are defined as those whose headquarter is located in the very highly damaged counties officially defined. Alternatively, we utilize the address information of establishments of firms reported for a subset of firms and define a firm in the disaster-hit areas if any establishment of the firm is located in the very highly damaged counties (See Appendix A2.1 for more details). The results shown in Appendix Table 2.7 are essentially the same as the benchmark results.

Thirdly, although we have used dummy variables for the various types of supply-chain links as the key independent variables in our estimations, the effect of supply chain links with damaged suppliers/customers may be increasing with the number of such suppliers/customers. Therefore, we experiment with the log of the number of suppliers or customers of various types examined in the benchmark estimations. We have obtained similar results with the baseline, as shown in Appendix Table 2.6 for the brevity.

Finally, because our data cover major supply-chain links between major firms in the world, the number of suppliers and customers reported in the data is small and the median is zero (Subsection 2.2.3). To check whether the presence of many firms with only a few reported suppliers or customers biases our results, we add interaction terms between the key variables and the dummy for zero or one supplier or customer. The results in Appendix Table 2.8 indicate that the effect of links with damaged suppliers and customers is significant only for firms with more than one reported links, possibly because their supply-chain links are more adequately captured in the data than those with few links.

2.4.4. Heterogeneous Effects

We further examine the possibility of heterogeneity of effects of damaged partners depending on link characteristics in three ways so as to explore whether there are certain conditions under which the propagation of the negative shocks is alleviated or amplified.

Specificity of Goods

First, we check whether the negative effects are amplified when the damaged suppliers or customers produce specific goods, following Barrot and Sauvagnat (2016). We test with Rauch (1999)'s commodity classifications of differentiated goods. Because our data do not include commodity information for each firm, we assume that a firm produces specific goods if its GICS code at the industry level corresponds to any SITC code at the commodity level defined as differentiated goods according to Rauch (1999).[6]

The results shown in columns (1) and (2) in panel (A) of Table 2.8 indicate that the negative effect of links with damaged specific suppliers on their customers in the US is negative and significant, while that of links with damaged non-specific suppliers is smaller and insignificant. The effect beyond the direct customers when the inputs are specific is negative but insignificant as shown in column (2). As in columns (3) and (4), we obtained similar results when we use alternative definition; a firm is assumed to be a specific goods producer if it has relatively large R&D expenses, that is, their share to its sales is above the 75-percentile value among US firms in the database. Panel (B) of Table 2.8 presents results for upstream propagation. Unlike the downstream propagation, whether or not the specificity of outputs damaged customers produce matters is not very clear. The coefficients of links with damaged non-specific customers and damaged specific two-step customers are negative and significant. However, those of links with damaged specific customers are negative but smaller in absolute terms than those of damaged non-specific customers in columns (1) and (2), whereas they are positive in (3) and (4). The contradictory results between downstream and upstream propagation arise possibly because producers of specific goods do not necessarily utilize specific inputs. Accordingly, when specific-good producers were damaged by the hurricane, their suppliers that did not necessarily produce specific goods may have found alternative customers relatively easily. As a result, the economic shock might not always propagate from producers of specific goods to their suppliers.

Overall, our results in Table 2.8 point to the important role of input specificity in propagation of negative shocks, being consistent with Barrot and Sauvagnat (2016). Accordingly, we conclude that the propagation effect is larger when search barriers are higher.

Network Density

Furthermore, we investigate the effect of network structure on propagation of shocks. In particular, we focus on the density of each firm's egocentric network, as explained in Subsection 2.3.1. Network density is measured by the local clustering coefficient defined by the ratio of the actual number of links between the focal node's partners/neighbours to the number of all possible pairs among the partners, representing how densely the focal node's partners/neighbours are linked with each other (Subsection 2.2.2). Using network density and its interaction term with the dummies for links with directly damaged firms, we test how network density affects the propagation effect.

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[6] GICS codes are more aggregated than SITC. We classify firms as specific if its GICS code covers at least one SITC code that Rauch (1999) classifies as "differentiated goods."

In columns (1) and (2) of Table 2.9, we find that the interaction term between the dummy variables for links with damaged suppliers or customers for US firms and the local clustering coefficient is negative and highly significant. It supports hypothesis 6a. By contrast, the same columns show that the coefficient of the interaction term for two-step links with damaged US firms is positive significantly or insignificantly, consistent with hypothesis 6b. Although it is not easy to interpret these contrasting findings on direct and two-step links, we pay more attention to the findings on direct links, given the insignificant effect of two-step links in the benchmark regressions (Table 2.5 and Table 2.6), and weakly interpret them as showing that the negative effects of damaged partners are circulated within dense networks and intensified.

Multi-layered Networks

Lastly, we check whether the negative effect through supply chains is alleviated or amplified by shareholding links. The literature clearly shows that when firms transact with foreign partners, firms choose either vertical integration with shareholding relationships, that is, intra-firm transactions, or outsourcing without shareholding relationships, that is, arm's-length transactions (Alfaro et al. 2019; Antràs and Chor 2013; Del Prete and Rungi 2017). It is also shown that depending on the organizational mode, changes of production and trade in supply chains in response to an economic shock may vary (Alessandria, Kaboski, and Midrigan 2010; Altomonte and Ottaviano 2009).

When suppliers and customers are in shareholding relationships, parts and components transacted between them are likely to be specific to the firm pairs. Therefore, substituting for parts and components developed from such collaborations between suppliers and customers in exclusive and long-term relationships or selling them to other firms is more difficult than otherwise. Thus, the negative effect of damaged suppliers (customers) on their customers (suppliers) that engage in shareholding relationships with the damaged suppliers (customers) may be larger than on other customers (suppliers) without shareholding relationships. On the other hand, the exclusiveness based on the high priority of the transaction based on the strong and long-term relationships may possibly alleviate the propagation of shocks. When suppliers are major shareholders of their customers, or vice versa, damaged suppliers may try to allocate more from the limited amount of their parts and components to the affiliated customers than to unaffiliated customers to maximize profits of the affiliated firm group. Similarly, when customers are major shareholders of their suppliers, or vice versa, damaged customers under limited operations try to buy their inputs more from the affiliated suppliers. Thus, the negative effect of damaged suppliers (customers) on their affiliated customers (suppliers) through shareholding ties may be smaller than on unaffiliated customers (suppliers). The information about shareholding relationships between firms is taken from Orbis dataset, which is a superset of Osiris. It covers 200 million firms around the world including non-listed small and medium enterprises. We merge it with supply chain data as we did for Osiris. Using the merged data, we test whether propagation from disaster-hit firms to their suppliers and customers through supply chains is different if firms are also linked through a shareholding relationship, forming a multi-layered network.

The result reported in column (1) of Table 2.10 indicates that shareholding links are more likely to amplify negative effects of damaged suppliers as suggested by the bigger negative coefficient, confirming our presumption above. For upstream propagation, column (2) of Table 2.10 shows that the effect of damaged customers with shareholding relationship is positive whereas that of damaged customers without shareholding relationship is negative. Although it is not so clear why the former is positive significant, these results suggest that the shareholding relationship may alleviate upstream propagation. This is probably because damaged customers under limited operations due to the direct disaster damages put priority on their affiliated firms when they buy inputs from suppliers so that they can maximize their affiliated group profits. Since disaster damaged firms are often forced limited operations instead of complete halts, the priority of transaction can differentiate the level of disaster shock propagations.

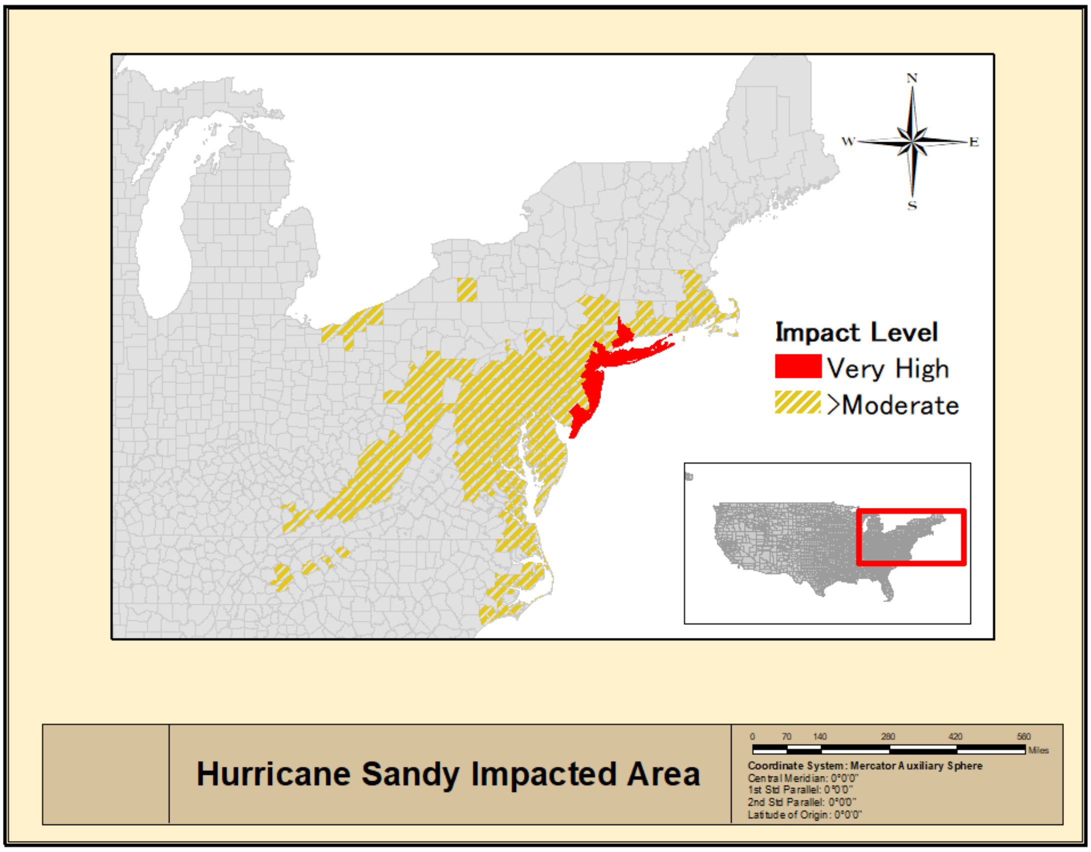
2.5. Conclusions

In this study, we take Hurricane Sandy, which struck the east coast of the US in 2012, as a source of negative economic shocks and examine whether the shock propagated to firms outside the disaster area through global supply chains. Specifically, using firm-level data on global supply chains, we analyse how sales growth of firms is affected by their direct and indirect suppliers and customers directly damaged by the hurricane.

Our results show that direct links with suppliers and customers suffered from a hurricane decreased the sales growth of firms within the US, while the effect on firms outside the US was not significant. Therefore, we conclude that negative economic shocks by a hurricane propagate through domestic supply chains, which is consistent with the evidence from the Great East Japan Earthquake (Carvalho et al. 2016). Although both hurricanes and earthquakes are ‘low-probability-high-consequence events’ (Skidmore and Toya 2002), geologic disasters such as earthquakes are found to be associated with worse macroeconomic recovery than climatic disasters. This study, finding significant propagation of hurricane shocks, suggests that even climatic disasters can give substantial damage on supply chains. Our further analysis shows a larger propagation effect on US firms when inputs are specific, consistent with a previous study. These results suggest that search barriers amplify propagation of shocks through supply chains and thus, source multiplication or preparation for the emergency substitution is needed to suppress the damage from supply chain disruption. In addition, we find that the propagation effect on US firms is larger when their ego network is denser, that is, their partners are connected with each other. All these findings imply that supply-chain links with diverse partners on networks lead to economic resilience to propagation of negative shocks.

Although our study is unique in that we use a hurricane shock, measures of network characteristics, and global supply chain data to investigate the propagation of disaster shocks through supply chains, there are several limitations. First, our data are limited to publicly listed firms and their major supply-chain relationships because the internationally comparable financial data is mostly limited to publicly listed firms and our major data source of supply chain information relies on public information such as financial reports and websites. Although we confirmed that the coverage of the publicly listed firms and their major links in our data is as high as that in the Compustat Segment files used in previous study, ignoring smaller firms and links among them may have underestimated the propagation of shocks. Second, because our data is not at the establishment level but at the firm-level, we identify firms directly damaged by the hurricane by the location of their headquarters. If a major plant of a firm whose headquarter is not located in the disaster area is in the disaster area, we classify this firm as one not directly damaged by the hurricane. This may also have resulted in underestimation of the true effect of the hurricane, although the existing studies, such as Barrot and Sauvagnat (2016) and Carvalho et al. (2016), embody the same data limitation. However, it should be emphasized that regardless of the two caveats we find significant negative propagation of shocks by a hurricane within a country. One of the characteristics of this study is that we find the negative propagation from relatively large firms in urban areas, which may be useful to discuss the supply chain risk by disasters in urban areas. Past studies focusing on a single disaster case find such negative propagation of disaster shocks from rural firms because of the characteristics of the disaster-hit region. Although we provide another evidence of the propagation of disaster shocks under different conditions, more studies are needed to verify the external validity of disaster impacts on supply chains because different disasters may bring different consequences. In addition, our data lacks sufficient information on quarterly-level sales, products’ prices, and quantities. Because of this limitation, we cannot conduct detailed analysis on how the impact changes over time nor how price changes affect the results. For the latter, by utilizing industry information as well as location information, we consider price changes at the level of industry-country pairs for non-US firms and industry-state pairs for US firms, but we could not consider price changes at the firm level. Finally, although our results imply benefits of network diversification and flexibility in terms of economic resilience, we did not conduct any cost-benefit analysis. Thus, the investigation of the optimal level of diversification is left for future study.

Figure 2.1: Area Damaged by Hurricane Sandy



Data Source: FEMA (2014).

Notes: This map is drawn by the author using ArcGIS Desktop. Filled-in area surrounded by the hatched area and the hatched area indicate counties that were impacted by the hurricane at the "very high" and "high"-"moderate" level defined by FEMA (2014), respectively.

Table 2.1: Number of Firms by Country of Location (Top 5 Countries)

	(1)	(2)	(3)	(4)	(5)
Country	Number of firms in the sample	Share in the sample (%)	Number of listed firms in the disaster areas dropped from the sample	Number of publicly listed firms in 2011	(1+3)/(4)
United States	1,984	16.96	714	4,171	0.647
Japan	2,487	21.26		2,280	1.091
China	2,050	17.53		2,342	0.875
Taiwan	946	8.09		790	1.197
United Kingdom	558	4.77		1,987	0.281

Data Source of column (4) : CEIC (2020) for Taiwan and World Bank (2018) for others. In column (5), some are above 1, which is likely due to the difference in the timing of reflecting the information when firms are delisted.

Table 2.2: Summary Statistics

Variable	Mean	S.D.	Min.	Median	Max
Links with suppliers in 2011					
# of suppliers	1.618	7.755	0	0	233
-- in logs	0.363	0.774	0	0	5.455
# of domestic suppliers	0.784	4.993	0	0	189
-- in logs	0.201	0.567	0	0	5.247
# of US suppliers	0.817	5.089	0	0	189
-- in logs	0.202	0.581	0	0	5.247
# of suppliers in 2 steps	19.520	86.432	0	0	1341
-- in logs	0.659	1.523	0	0	7.202
Links with damaged suppliers in 2011					
Dummy for any link with damaged suppliers×US dummy	0.027	0.162	0	0	1
Dummy for any link with damaged suppliers×non-US dummy	0.012	0.108	0	0	1
Dummy for any 2-step link with damaged suppliers×US dummy	0.064	0.244	0	0	1
Dummy for any 2-step link with damaged suppliers×non-US dummy	0.043	0.203	0	0	1
Links with customers in 2011					
# of customers	2.083	7.200	0	0	196
-- in logs	0.424	0.885	0	0	5.283
# of domestic customers	0.805	3.408	0	0	107
-- in logs	0.232	0.608	0	0	4.682
# of US customers	0.812	3.511	0	0	107
-- in logs	0.222	0.612	0	0	4.682
# of customers in 2 steps	23.775	101.468	0	0	2297
-- in logs	0.762	1.644	0	0	7.740
Links with damaged customers in 2011					
Dummy for any link with damaged customers×US dummy	0.029	0.168	0	0	1
Dummy for any link with damaged customers×non-US dummy	0.012	0.109	0	0	1
Dummy for any 2-step link with damaged customers×US dummy	0.075	0.264	0	0	1
Dummy for any 2-step link with damaged customers×non-US dummy	0.045	0.208	0	0	1
Other networks measure in 2011					
Local Clustering Coefficient	0.014	0.071	0	0	1
Firm attributes					
Sales growth from 2010 to 2011	0.104	0.324	-1.969	0.079	2.000
Sales growth from 2011 to 2012	0.013	0.327	-2.000	0.014	1.999
Sales per worker in 2011	647	7317	0	222	496205
-- in logs	5.391	1.221	-3.298	5.401	13.115
# of workers in 2011	4966	27798	1	931	2200000
-- in logs	6.769	1.916	0	6.836	14.604
Value of total assets in 2011	1697118	8202269	4	257425	331052000
-- in logs	12.444	1.976	1.495	12.458	19.618
Firm age	32.787	30.815	2	22	647

Table 2.3 : Pre-disaster Parallel Trend Test

	(1)	(2)	(3)	(4)
	Dependent variable: Sales growth 2010-2011			
Dummy for any link with damaged suppliers ×US dummy	-0.0151			
	(0.0112)			
Dummy for any link with damaged suppliers ×non-US dummy		-0.00327		
		(0.0196)		
Dummy for any link with damaged customers ×US dummy			0.0110	
			(0.00711)	
Dummy for any link with damaged customers ×non-US dummy				-0.0283
				(0.0267)
Network controls	YES	YES	YES	YES
Country dummies × Industry dummies	YES	YES	YES	YES
US-state dummies × Industry dummies	YES	YES	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES	YES	YES
Observations	11,697	11,697	11,697	11,697
R-squared	0.206	0.206	0.206	0.207

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Network controls include the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Table 2.4 : Direct Effect

	(1)	(2)
VARIABLES	Dependent variable: Sales growth 2011-2012	
Dummy for firms in very highly damaged area	-0.0705**	-0.0760**
	(0.0285)	(0.0325)
Dummy for firms in highly damaged area		-0.0240
		(0.0384)
Controls for Firm Characteristics	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with disaster variables	YES	YES
Observations	2,529	2,529
R-squared	0.199	0.201

Notes: Robust standard errors clustered at the state level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. The sample is US firms. Controls for Firm Characteristics includes sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log. Single terms of each interaction term are also included.

Table 2.5: Effects of Damaged Suppliers

	(1)	(2)	(3)
	Dependent variable: Sales growth 2011-2012		
Dummy for any link with damaged suppliers × US dummy	-0.0189**	-0.0132**	-0.0142***
	(0.00820)	(0.00517)	(0.00521)
Dummy for any link with damaged suppliers × non-US dummy	0.00837	0.00681	0.00672
	(0.0151)	(0.0166)	(0.0166)
Dummy for any 2-step link with damaged suppliers × US dummy		-0.0222	-0.0226
		(0.0149)	(0.0148)
Dummy for any 2-step link with damaged suppliers × non-US dummy		-0.00932	-0.00922
		(0.0332)	(0.0332)
Controls	YES	YES	YES
Country dummies × Industry dummies	YES	YES	YES
US state dummies × Industry dummies	YES	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES	YES
Other disaster dummy	NO	NO	YES
Observations	11,697	11,697	11,697
R-squared	0.186	0.187	0.187

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Table 2.6: Effects of Damaged Customers

	(1)	(2)	(3)
	Dependent variable: Sales growth 2011-2012		
Dummy for any link with damaged customers ×US dummy	-0.0515***	-0.0471***	-0.0476***
	(0.0153)	(0.00739)	(0.00731)
Dummy for any link with damaged customers ×non-US dummy	0.0211	0.0194	0.0195
	(0.0355)	(0.0375)	(0.0375)
Dummy for any 2-step link with damaged customers ×US dummy		-0.0144	-0.0143
		(0.0189)	(0.0189)
Dummy for any 2-step link with damaged customers ×non-US dummy		-0.0218	-0.0219
		(0.0229)	(0.0230)
Controls	YES	YES	YES
Country dummies × Industry dummies	YES	YES	YES
US state dummies × Industry dummies	YES	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES	YES
Other disaster dummy	NO	NO	YES
Observations	11,697	11,697	11,697
R-squared	0.186	0.189	0.189

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Table 2.7: Results from Alternative Definition of Damaged Firms

	(1)	(2)
	Dependent variable: Sales growth 2011-2012	
Downstream propagation		
Dummy for any link with damaged suppliers using alternative definition ×US dummy	-0.0303***	
	(0.00578)	
Dummy for any link with damaged suppliers using alternative definition ×non-US dummy	0.0315	
	(0.0269)	
Upstream propagation		
Dummy for any link with damaged customers using alternative definition ×US dummy		-0.0895***
		(0.0137)
Dummy for any link with damaged customers using alternative definition ×non-US dummy		-0.0292
		(0.0222)
Controls	YES	YES
Country dummies × Industry dummies	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES
Other disaster dummy	NO	NO
Observations	11,697	11,697
R-squared	0.186	0.186

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Table 2.8: Effect of Input/Output Specificity

(A) Downstream Propagation

	(1)	(2)	(3)	(4)
	Dependent variable: Sales growth 2011-2012			
	Differentiated goods		R&D	
Dummy for any link with damaged specific suppliers ×US dummy	-0.0146**	-0.0157**	-0.106***	-0.105***
	(0.00664)	(0.00761)	(0.0122)	(0.0148)
Dummy for any link with damaged non-specific suppliers ×US dummy	-0.00372	-0.00861	-0.0182**	-0.0111**
	(0.00721)	(0.00677)	(0.00823)	(0.00556)
Dummy for any link with damaged specific suppliers ×non-US dummy	0.0242	0.0255	0.0657	0.0466
	(0.0286)	(0.0328)	(0.0767)	(0.0770)
Dummy for any link with damaged non-specific suppliers ×non-US dummy	-0.0201	-0.0257	0.00594	0.00742
	(0.0277)	(0.0304)	(0.0160)	(0.0191)
Dummy for any 2-step link with damaged specific suppliers ×US dummy		-0.0213		0.0313***
		(0.0161)		(0.0103)
Dummy for any 2-step link with damaged non-specific suppliers ×US dummy		0.0507***		-0.0135
		(0.0112)		(0.0168)
Dummy for any 2-step link with damaged specific suppliers ×non-US dummy		-0.0186		0.0508*
		(0.0331)		(0.0288)
Dummy for any 2-step link with damaged non-specific suppliers ×non-US dummy		0.0479		-0.00310
		(0.0441)		(0.0389)
Controls	YES	YES	YES	YES
Country dummies × Industry dummies	YES	YES	YES	YES
US state dummies × Industry dummies	YES	YES	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES	YES	YES
Other disaster dummy	NO	NO	NO	NO
Observations	11,697	11,697	11,697	11,697
R-squared	0.187	0.188	0.186	0.188

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

(B) Upstream Propagation

	(1)	(2)	(3)	(4)
	Dependent variable: Sales growth 2011-2012			
	Differentiated goods		R&D	
Dummy for any link with damaged specific customers ×US dummy	-0.0386***	-0.0274***	0.158***	0.234***
	(0.0104)	(0.00562)	(0.0236)	(0.0215)
Dummy for any link with damaged non-specific customers ×US dummy	-0.0457***	-0.0398***	-0.0524***	-0.0489***
	(0.0147)	(0.00625)	(0.0155)	(0.00776)
Dummy for any link with damaged specific customers ×non-US dummy	0.0757	0.0757	0.637***	0.602***
	(0.0712)	(0.0742)	(0.0273)	(0.0513)
Dummy for any link with damaged non-specific customers ×non-US dummy	-0.00293	-0.00448	0.0146	0.0131
	(0.0199)	(0.0225)	(0.0347)	(0.0399)
Dummy for any 2-step link with damaged specific customers ×US dummy		-0.0605***		-0.110***
		(0.0106)		(0.00694)
Dummy for any 2-step link with damaged non-specific customers ×US dummy		0.0180		0.00416
		(0.0158)		(0.0180)
Dummy for any 2-step link with damaged specific customers ×non-US dummy		-0.0117		0.0157
		(0.0402)		(0.0422)
Dummy for any 2-step link with damaged non-specific customers ×non-US dummy		-0.0359		-0.0159
		(0.0350)		(0.0238)
Controls	YES	YES	YES	YES
Country dummies × Industry dummies	YES	YES	YES	YES
US state dummies × Industry dummies	YES	YES	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES	YES	YES
Other disaster dummy	NO	NO	NO	NO
Observations	11,697	11,697	11,697	11,697
R-squared	0.187	0.191	0.187	0.190

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Table 2.9: Effect of Network Density

	(1)	(2)
	Dependent variable: Sales growth 2011-2012	
Dummy for any link with damaged suppliers ×US dummy × local clustering coefficient	-0.265***	
	(0.0266)	
Dummy for any link with damaged customers×US dummy × local clustering coefficient		-1.941***
		(0.0403)
Dummy for any 2-step link with damaged suppliers ×US dummy × local clustering coefficient	0.355***	
	(0.125)	
Dummy for any 2-step link with damaged customers×US dummy × local clustering coefficient		0.00153
		(0.0522)
Dummy for any link with damaged suppliers ×US dummy	0.00704	
	(0.00625)	
Dummy for any link with damaged customers ×US		0.103***
		(0.00884)
Dummy for any 2-step link with damaged suppliers ×US dummy	-0.0462***	
	(0.0167)	
Dummy for any 2-step link with damaged customers ×US dummy		-0.0146
		(0.0199)
Local clustering coefficient	-0.130	0.00430
	(0.142)	(0.0577)
Supply-chain-shock variables for non-US firms	YES	YES
Controls	YES	YES
Country dummies × Industry dummies	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES
Other disaster dummy	NO	NO
Observations	11,697	11,697
R-squared	0.190	0.195

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, isolates dummy, and one link dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Table 2.10: The Effect through Capital Shareholdings Networks

	(1)	(2)
	Dependent variable: Sales growth 2011-2012	
Dummy for any link with damaged suppliers in shareholding relationships ×US dummy	-0.0736***	
	(0.00998)	
Dummy for any link with damaged suppliers not in shareholding relationship ×US dummy	-0.0172**	
	(0.00819)	
Dummy for any link with damaged suppliers ×non-US dummy	0.00937	
	(0.0151)	
Dummy for any link with damaged customers in shareholding relationships ×US dummy		0.0910***
		(0.0312)
Dummy for any link with damaged customers not in shareholding relationships ×US dummy		-0.0586***
		(0.0153)
Dummy for any link with damaged customers ×non-US dummy		0.0213
		(0.0357)
Controls	YES	YES
Country dummies × Industry dummies	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES
Other disaster dummy	NO	NO
Observations	11,697	11,697
R-squared	0.186	0.186

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers, capital shareholdings links, capital shareholdings links×non-US dummy (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Appendix

A2.1. *Locational information of firms*

In the LiveData, besides the address of the headquarter, the address of at least one non-headquarter establishment is available for 83.8 percent of firms located in the US with any reported supplier or customer. However, our dataset does not seem to cover most establishments of included firms, because the average number of non-headquarter establishments is 0.839, a small number for relatively large firms in our dataset. In addition, we do not have any information about supply-chain relationships between establishments or sales of establishments. Therefore, our analysis is conducted at the firm level, rather than at the establishment level. Specifically, benchmark regressions define firms damaged by the hurricane as those whose headquarter is located in the officially-defined very highly damaged areas (Tables 2.5-2.6 and 2.8-2.10). However, as a robustness check, we define damaged firms as those any of whose establishments is located in the damaged areas. Based on this alternative definition of damaged firms, 782 firms are identified as damaged firms. Compared with 774 damaged firms based on the benchmark definition, the number of damaged firms increases only slightly because of the limited information about the location of non-headquarter establishments. Accordingly, the average numbers of damaged suppliers and customers in our sample based on the alternative definition, 0.077 and 0.071, respectively, are quite similar to those based on the benchmark definition, 0.076 and 0.070, respectively. As a result, the regression results for propagation effects using the alternative definition shown in Appendix Table 2.7 are essentially the same as those using the benchmark definition shown in Table 2.5 and Table 2.6.

A2.2. Theoretical Background

We particularly rely on Barrot and Sauvagnat (2016) and Carvalho et al. (2016), who examine propagation of shocks from natural disasters, generating several theoretical predictions, as follows.

In the model of Barrot and Sauvagnat (2016), firms monopolistically produce a variety of goods that can be consumed or used as inputs for other products. A firm's production function is characterized by constant elasticity of substitution among labour and a variety of intermediate inputs and constant returns to scale. Simulation analysis by Barrot and Sauvagnat (2016) shows that destruction of a firm's production capacity by a disaster negatively affects the output of its direct customers through input-output linkages. Using a similar theoretical model, Carvalho et al. (2016) reach the same conclusion. In their model, when a firm's productivity is negatively affected by a shock, the price of the firm's product becomes higher. As a result, its customers utilise a smaller amount of its product as an intermediate good, producing a smaller amount of their products. Indirect customers of the firm directly damaged by a shock, such as customers of their direct customers, or two-step customers, are also negatively affected, although the effect on indirect customers is smaller than the effect on direct customers.

Carvalho et al. (2016) further show that a negative productivity shock on a firm decreases the output of the firm's upstream suppliers when labour and intermediates are substitutes. This effect is due to an increase in the demand for labour relative to intermediates because of a negative productivity shock on the intermediate-goods sector. As in the case of downstream propagation, negative shocks propagate further to more upstream suppliers beyond direct suppliers, although the propagation effect diminishes along supply chains.

A2.3. *Differences in the fiscal year-ends among firms*

Fiscal year-ends vary across firms, although it is December for the majority, or 64 percent, of firms in the sample, as shown in Appendix Table 2.2. The difference in fiscal year-ends should be incorporated into the analysis of the effect of links with damaged firms on sales growth calculated from sales in fiscal years 2011 and 2012. To explain why, suppose that the fiscal year-end of a firm is December. Then, our measure of sales growth indicates the change in sales from the period January–December 2011 to the period January–December 2012 and covers two months after the hurricane that hit the US at the end of October 2012. Now, suppose that the fiscal year end of another firm is March, for example. Then, sales growth for this firm represents the change from the period April 2011–March 2012 to the period April 2012–March 2013 and includes five months after the hurricane (i.e. from November 2012 to March 2013). Therefore, the effect of links with damaged firms on firms whose fiscal year-end is December can be smaller or larger than the effect on firms whose fiscal year-end is March, depending on how long the propagation effect lasts. To incorporate these differences, we include dummies for different fiscal year-end months and interaction terms between these dummies and the key variables for supply-chain ties, setting December as the base month.

In the benchmark regression in Table 2.5, the coefficient of the interaction term between the dummy for any links with damaged suppliers and the US dummy is -0.0189, indicating that firms whose fiscal end-year is December and that are linked with damaged suppliers reduced sales of US firms from January-December 2012 by 1.89% compared with sales from January-December 2011, other things being equal. The coefficients of the triple interaction terms between the dummy for the link with any damaged supplier, the US dummy, and the dummy for each fiscal year-end indicate the additional effect. Considering the magnitude of the direct damages by the hurricane, we focus on the short-term effect for two months and do not show the coefficients of the triple interactions for brevity of presentation.

Appendix Table 2.1: Number of Firms by Industry

Industry Group	Freq.	Percent
Capital Goods	2,019	17
Materials	1,408	12
Technology Hardware & Equipment	1,141	10
Software & Services	822	7
Consumer Durables & Apparel	750	6
Food, Beverage & Tobacco	642	5
Retailing	553	5
Energy	502	4
Pharmaceuticals, Biotechnology & Life Sciences	475	4
Consumer Services	457	4
Commercial & Professional Services	433	4
Health Care Equipment & Services	422	4
Transportation	399	3
Semiconductors & Semiconductor Equipment	343	3
Automobiles & Components	327	3
Media	324	3
Utilities	290	2
Food & Staples Retailing	171	1
Household & Personal Products	111	1
Telecommunication Services	108	1

Appendix Table 2.2: Number of Firms by Fiscal Year-End

Fiscal year-end	Number of firms	Percent
December	7,486	64
March	525	4
June	364	3
September	247	2
February	105	1
April	69	1
July	59	1
October	58	1
August	57	0
May	55	0
January	46	0
November	33	0
Fiscal Year End Info Unavailable	2,593	22

Appendix Table 2.3: Effects of Links with Firms Similar in Attributes to Damaged Firms

	(1)	(2)	(3)	(4)
	Dependent variable: Sales growth 2011-2012			
Downstream propagation				
Dummy for any link with suppliers similar in attributes to damaged firms ×US dummy	0.0216***		0.0215***	
	(0.00595)		(0.00599)	
Dummy for any link with suppliers similar in attributes to damaged firms ×non-US dummy	0.0508		0.0508	
	(0.0420)		(0.0420)	
Dummy for any 2-step link with suppliers similar in attributes to damaged firms ×US dummy	-0.00453		-0.00461	
	(0.0145)		(0.0145)	
Dummy for any 2-step link with suppliers similar in attributes to damaged firms ×non-US dummy	0.00371		0.00398	
	(0.0263)		(0.0264)	
Upstream propagation				
Dummy for any link with customers similar in attributes to damaged firms ×US dummy		0.0245***		0.0249***
		(0.00706)		(0.00706)
Dummy for any link with customers similar in attributes to damaged firms ×non-US dummy		0.0810		0.0812
		(0.0489)		(0.0489)
Dummy for any 2-step link with customers similar in attributes to damaged firms ×US dummy		0.0177		0.0178
		(0.0179)		(0.0179)
# Dummy for any 2-step link with customers similar in attributes to damaged firms ×non-US dummy		0.0379		0.0379
		(0.0261)		(0.0261)
Controls	YES	YES	YES	YES
Country dummies × Industry dummies	YES	YES	YES	YES
US state dummies × Industry dummies	YES	YES	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES	YES	YES
Other disaster dummy	NO	NO	YES	YES
Observations	11,697	11,697	11,697	11,697
R-squared	0.187	0.189	0.187	0.189

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Appendix Table 2.4: Effects of Links with Firms Geographically Close to Damaged Firms

	(1)	(2)	(3)	(4)
	Dependent variable: Sales growth 2011-2012			
Downstream propagation				
Dummy for any link with suppliers geographically close to	-0.0130		-0.0131	
damaged firms ×US dummy	(0.0119)		(0.0119)	
Dummy for any link with suppliers geographically close to	-0.0152		-0.0153	
damaged firms ×non-US dummy	(0.0775)		(0.0775)	
Dummy for any 2-step link with suppliers geographically close	0.00447		0.00441	
to damaged firms ×US dummy	(0.0176)		(0.0177)	
Dummy for any 2-step link with suppliers geographically close	-0.0193		-0.0190	
to damaged firms ×non-US dummy	(0.0414)		(0.0414)	
Upstream propagation				
Dummy for any link with customers geographically close to		0.00555		0.00539
damaged firms ×US dummy		(0.00686)		(0.00685)
Dummy for any link with customers geographically close to		0.0671		0.0673
damaged firms ×non-US dummy		(0.0519)		(0.0519)
Dummy for any 2-step link with customers geographically		-0.0626***		-0.0621***
close to damaged firms ×US dummy		(0.0187)		(0.0188)
Dummy for any 2-step link with customers geographically		-0.0214		-0.0212
close to damaged firms ×non-US dummy		(0.033)		(0.0336)
Controls	YES	YES	YES	YES
Country dummies × Industry dummies	YES	YES	YES	YES
US state dummies × Industry dummies	YES	YES	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES	YES	YES
Other disaster dummy	NO	NO	YES	YES
Observations	11,697	11,697	11,697	11,697
R-squared	0.186	0.188	0.186	0.188

Notes: Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Appendix Table 2.5: Pre-disaster Parallel Trend Test between Firms in and outside the Disaster Areas

	(1)	(2)
	Dependent variable: Sales growth 2010-2011	
	All	US
Dummy for firms in very highly damaged area	0.00275	-0.0103
	(0.00709)	(0.0240)
Network controls	YES	YES
Country dummies × Industry dummies	YES	NO
US-state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with the disaster variable	YES	YES
Observations	16,835	2,560
R-squared	0.143	0.119

Notes: Robust standard errors clustered at the country (column (1))-/state (column (2))- level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. The sample for this table includes both firms in very highly damaged areas and those outside disaster-hit areas. Network controls include the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers, Bonacich power centrality, and isolates dummy. Single terms of each interaction term are also included.

Appendix Table 2.6: Robustness Test-Using Number of Links-

	(1)	(2)
	Dependent variable: Sales growth 2011-2012	
# of links with damaged suppliers +1 (log) ×US dummy	-0.0295***	
	(0.00739)	
# of links with damaged suppliers +1 (log) ×non-US dummy	-0.00367	
	(0.0255)	
# of links with damaged 2-step suppliers +1 (log) ×US dummy	0.0179	
	(0.0113)	
# of links with damaged 2-step suppliers +1 (log) ×non-US dummy	0.0226	
	(0.0231)	
# of links with damaged customers +1 (log) ×US dummy		-0.0501***
		(0.00736)
# of links with damaged customers +1 (log) ×non-US dummy		0.0240
		(0.0424)
# of links with damaged 2-step customers +1 (log) ×US dummy		-0.0218*
		(0.0128)
# of links with damaged 2-step customers +1 (log) ×non-US dummy		-0.0226
		(0.0166)
Controls	YES	YES
Country dummies × Industry dummies	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES
Other disaster dummy	NO	NO
Observations	11,697	11,697
R-squared	0.187	0.189

Notes. This table presents a variant of the baseline regressions in Tables 2.5 and 2.6 where the number of links are used as key independent variables instead of dummy variables. Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Appendix Table 2.7: Robustness Test Using All Address Information

	(1)	(2)
	Dependent variable: Sales growth 2011-2012	
Dummy for any link with damaged suppliers ×US dummy	-0.0211**	
	(0.00807)	
Dummy for any link with damaged suppliers ×non-US dummy	0.00707	
	(0.0154)	
Dummy for any link with damaged customers ×US dummy		-0.0491***
		(0.0154)
Dummy for any link with damaged customers ×non-US dummy		0.0205
		(0.0353)
Controls	YES	YES
Country dummies × Industry dummies	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES
Other disaster dummy	NO	NO
Observations	11,697	11,697
R-squared	0.186	0.186

Notes. This table presents a variant of the baseline regressions in Tables 2.5 and 2.6 where the address for both headquarters and others are used as the location information of firms to identify the firms in disaster-hit areas. Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Appendix Table 2.8: Robustness Test-Separating the Effect on Subsample of Firms with Few Links Reported-

	(1)	(2)
	Dependent variable: Sales growth 2011-2012	
Dummy for any link with damaged suppliers ×US dummy	-0.0206**	
	(0.00830)	
- ×dummy for # of suppliers≦1	0.0180**	
	(0.00752)	
Dummy for any link with damaged suppliers ×non-US dummy	0.0149	
	(0.0173)	
- ×dummy for # of suppliers≦1	-0.0482	
	(0.0532)	
Dummy for any link with damaged customers ×US dummy		-0.0499***
		(0.0149)
- ×dummy for # of customers≦1		0.0360**
		(0.0175)
Dummy for any link with damaged customers ×non-US dummy		0.0198
		(0.0387)
- ×dummy for # of customers≦1		0.0405
		(0.0746)
Controls	YES	YES
Country dummies × Industry dummies	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES
Other disaster dummy	NO	NO
Observations	11,697	11,697
R-squared	0.186	0.187

Notes. This table presents a variant of the baseline regressions in Tables 2.5 and 2.6 where we separate the effects on the subsample of firms with few reported links. We created the dummy variable which is one if the number of suppliers/ customers is zero or one and the interaction terms with supply-chain variables of interests. We included them in the baseline regression model and repeated the estimation. Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Appendix Table 2.9: Longer-Term Effect

	(1)	(2)
	Dependent variable: Sales growth 2011-2013	
Dummy for any link with damaged suppliers ×US dummy	-0.0258***	
	(0.00757)	
Dummy for any link with damaged suppliers ×non-US dummy	0.0127	
	(0.0273)	
Dummy for any link with damaged customers ×US dummy		-0.00354
		(0.00976)
Dummy for any link with damaged customers ×non-US dummy		0.0276
		(0.0320)
Controls	YES	YES
Country dummies × Industry dummies	YES	YES
US state dummies × Industry dummies	YES	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES	YES
Other disaster dummy	NO	NO
Observations	11,697	11,697
R-squared	0.209	0.209

Notes. This table presents a variant of the baseline regressions in Tables 2.5 and 2.6 where the outcome variable is sales growth from 2011 to 2013. Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

Appendix Table 2.10: Horizontal Propagation

	(1)
	Dependent variable: Sales growth 2011-2012
Dummy for any link with damaged customers ×US dummy	-0.0401***
	(0.00862)
Dummy for any link with damaged customers ×non-US dummy	0.0149
	(0.0404)
Dummy for any 2-step horizontal link with damaged suppliers ×US dummy	0.00140
	(0.0177)
Dummy for any 2-step horizontal link with damaged suppliers ×non-US dummy	0.0196
	(0.0228)
Controls	YES
Country dummies × Industry dummies	YES
US state dummies × Industry dummies	YES
Dummies for the end month of the accounting period and their interaction with supply-chain-shock variables	YES
Other disaster dummy	NO
Observations	11,697
R-squared	0.188

Notes. This table presents a variant of the baseline regressions in Table 2.6 where the horizontal propagation, i.e. propagation of shocks from other suppliers of the shared customers, is explored. Robust standard errors clustered at the country level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% level. Controls include sales growth from 2010 to 2011, the amount of total asset in log, the number of employees in log, firm age, and sales per worker in log, the number of suppliers, customers, domestic suppliers, domestic customers, 2-step suppliers, 2-step customers, other suppliers of customers, US suppliers, US customers, US 2-step suppliers, US 2-step customers, other US suppliers of customers (all in logs), Bonacich's power centrality, and isolates dummy. Single terms of each interaction term are also included. When we use triple interactions, we also use single terms and double interactions when necessary.

3. Impact of Relief Policy on Supply Chains

This chapter evaluates the impact of "group subsidies," a policy intervention intended to repair and reinstall damaged capital goods and facilities of small- and medium-sized enterprises after the Great East Japan earthquake. Employing a propensity score matching and difference-in-differences approach, we find a positive indirect effect of group subsidies on firms in disaster-hit prefectures that did not receive any group subsidy but were linked through supply chains with a recipient firm. Our results indicate the propagation of post-disaster policy effects through supply chains. This chapter is based on Kashiwagi and Todo (2021b) "PROPAGATION OF POSITIVE EFFECTS OF POST-DISASTER POLICIES THROUGH SUPPLY CHAINS" published in *Contemporary Economic Policy*.

3.1.Introduction

When a natural disaster hits a region, the economic shock propagates to regions that are not directly hit by the disaster through the disruption of supply chains. Customers of firms directly hit by the disaster may shrink production owing to lack of material, parts, or components, whereas their suppliers may follow suit owing to lack of demand. The recent emerging literature on this issue found econometric evidence of such propagation, using firm-level data and supply chain information on the United States (Barrot and Sauvagnat 2016), Japan (Carvalho et al. 2016), and the world (Kashiwagi, Todo, and Matous 2021). Some other studies, such as Hallegatte (2012), Henriet, Hallegatte, and Tabourier (2012), and Inoue and Todo (2019a), performed simulation analysis on agent-based models to confirm the substantial indirect effect of disasters owing to the propagation of the shock. This issue has become more urgent because the frequency and severity of natural disasters is projected to increase owing to climate change (Milly et al. 2002) and evolving seismic trends (Beroza 2012).

Following a natural disaster, recent policy interventions of governments and other institutions often include subsidies and financial relief to repair or reinstall damaged capital stocks and maintain employment, and alleviate its negative effect at the firm level. A few studies have examined the direct effect of such interventions on the recovery of private firms. Notably, De Mel, McKenzie, and Woodruff (2012) used a randomized experiment to examine the effect of relief aid and access to capital on the post-disaster recovery of microenterprises. They find a positive effect of the intervention, particularly, on the profit and revenue of retailers, but not on those of firms in the manufacturing and other service sectors. Kashiwagi (2020) also find sectoral differences in the effect of public support and provide suggestive evidence that the access to quick support from supply chain partners in distant areas is the cause of little impact of official aid on recovery at least in some sectors, and the exception is the other service sector than retailers, whose firms are relatively large and which has less variation in the size of firms. However, to the best of the authors' knowledge, no study has examined the indirect effect of post-disaster policy interventions on the performance of firms that are not directly hit by a disaster, but are linked to those that are directly affected. Negative impact of disasters on firms is no more a local issue but regional and national concern because the negative effect on firms in disaster-hit areas can propagate to firms not directly hit by disasters and the indirect impact is large enough to cause macroeconomic fluctuations (Acemoglu et al. 2012). This reality partly forms the motivation to start publicly supporting firms in disaster-hit areas (The Small and Medium Enterprise Agency of Japan 2011). In other words, while such policy intervention on firms in disaster-hit areas is often costly, but the large expense is accepted based on the assumption that it would help not only firms receiving public support but those suffered from supply chain disruptions or the broad economy. However, whether this assumption is valid has not been examined yet, although the validity is crucial to the cost-benefit evaluation of the policy and the resulting justification of continuing use of this costly policy in spite of a financial burden it places on government.

To fill the research gap and contribute to post-disaster policy-making, this study estimates the indirect effects of the policy intervention of "group subsidies," which were provided to small- and medium-sized enterprises (SMEs) after the Great East Japan earthquake and the subsequent tsunami in 2011 to repair and reinstall their damaged fixed assets. We utilize comprehensive firm-level data on more than 1 million Japanese firms that contain information on approximately 5 million supply chain links among them. To avoid biases owing to self-selection of firms receiving subsidies and unobservable factors, we employ a propensity score matching (PSM) estimation, combined with a difference in differences (DID) approach.

Our analysis reveals that the subsidies have a direct positive and statistically significant effect on the sales and employment of their recipients, particularly the small-sized ones. Further, we find an indirect positive effect of the subsidies on sales of firms in the earthquake-hit region that did not receive the subsidy but were linked with a recipient through supply chains. Therefore, our results suggest that post-disaster subsidies to reconstruct the damaged production facilities of small enterprises can effectively facilitate their recovery, and that the positive effect propagates to other firms through supply chains. It is further suggested that the evaluation of the effect of a policy on enterprises should take into account its indirect effects because of their propagation through supply chains.

This paper contributes to the existing literature in two ways. First, it contributes to the literature on enterprise resilience against supply chain disruptions. As mentioned above, growing strand of literature explore the issue of supply chain disruptions by various types of economic shocks (Acemoglu, Ozdaglar, and Tahbaz-Salehi 2015a; Auer, Levchenko, and Sauré 2019; Barrot and Sauvagnat 2016; Boehm, Flaaen, and Pandalai-Nayar 2015; Caliendo et al. 2017; Caliendo and Parro 2014; Carvalho et al. 2016; Cravino and Levchenko 2016; Di Giovanni, Levchenko, and Mejean 2018; Fieler and Harrison 2018; Horvath 1998; Huo, Levchenko, and Pandalai-Nayar 2019; Kikkawa, Magerman, and Dhyne 2017; Long and Plosser 1983; Tintelnot et al. 2018). However, they mostly focused on how shocks propagate through supply chains. Thus, little is known about how to mitigate the damage on supply chain partners of disaster-hit firms with post-disaster measures, although the effect of popular pre-disaster mitigation measure Business Continuity Plan (Program) on supply chain disruption and the impact of various mitigation measures on disaster-hit firms themselves have already been evaluated (Azadegan et al. 2020; Poontirakul et al. 2017; Kashiwagi 2020; De Mel, McKenzie, and Woodruff 2012; Todo, Nakajima, and Matous 2015). The present paper addresses this issue by examining the effect of relief subsidies on suppliers and customers of subsidy receivers.

Second, the present paper also relates to literature on positive spillovers. Positive spillovers through economic networks such as knowledge and productivity spillovers by FDI and international trade have been observed (Keller and Yeaple 2009; Haskel, Pereira, and Slaughter 2007; Takii 2005; Javorcik 2004; Todo 2006; Todo and Miyamoto 2006; Todo, Zhang, and Zhou 2011; Todo, Zhang, and Zhou 2009; Alvarez and López 2008). Criscuolo et al. (2019) also find some positive spillover effect of the investment subsidy in terms of employment using ward level data, although they deny its conclusiveness. The present study adds to the literature by presenting another source of positive spillovers through inter-firm networks: a relief subsidy to repair and reinstall damaged facilities.

3.2. Group Subsidies after the Great East Japan Earthquake

This study particularly focuses on a policy intervention by the Japanese government after the Great East Japan earthquake (hereafter, the earthquake) in March 2011: the Subsidies for the Recovery of Facilities of Groups of Small and Medium-sized Enterprises (*Chusho Kigyo tou Gurupu Shisetsu tou Hukkyu Seibi Hojo Jigyo*), known as the “group subsidies” (*Gurupu Hojokin*). The earthquake, which was of magnitude 9.0 on the Richter scale, was the fourth severest earthquake in the world since 1900. The death toll, including missing persons, reached almost 19,000 (Cabinet Office of Japan 2012). The epicenter was off the coast of Northeastern Japan, a less industrially advanced region where many small- and medium-sized suppliers to the automobile and electric machinery industries are located (Ministry of Economy 2011). The direct loss of economic facilities, including buildings, utilities, and social infrastructure was estimated to be 16.9 trillion Japanese yen, or approximately 212 million US dollars using the exchange rate in 2011 (Cabinet Office of Japan 2012).

The government of Japan, through the SME Agency under the Ministry of Economy, Trade and Industry (METI) and prefecture governments, provided the group subsidies (henceforth referred to as the subsidies) to groups of SMEs in the areas damaged by the earthquake, i.e., Hokkaido, Aomori, Iwate, Miyagi, Fukushima, Tochigi, Ibaraki, and Chiba prefectures. The program subsidizes 75% (50% covered by the central government and 25% by the prefecture’s government) of the costs incurred to repair or reinstall capital goods of SMEs destroyed by the earthquake and the subsequent tsunami (SME Agency of Japan 2011). Although the receipt of the subsidy is limited to SMEs in the prefecture that the application form was submitted to, firms outside the prefecture can be a member of the group to help a damaged firm that fails to join groups of firms in the prefecture (Taguchi 2013).

A notable feature of this policy is that the subsidies are provided to SMEs that form a group. In particular, to promote economic recovery of the region, the subsidies target four types of groups: (1) firm groups in major industrial clusters in the region; (2) firm groups important to the regional economy in terms of employment and size; (3) firm groups important to supply chains in Japan; and (4) groups of retail stores essential to regional communities (SME Agency of Japan 2011).

The acceptance rate of applications to the subsidies was high, indicating that the selection of recipient firms was not strict once they successfully formed a group. For example, among the applicant groups in Iwate prefecture, 37% and 83% were provided with subsidies in the first year and second year, respectively. Most applicants belonging to National Conference of the Association of Small and Medium Business Entrepreneurs answered that they successfully received subsidies, as reported by a firm-level survey (National Conference of the Association of Small and Medium Business Entrepreneurs 2013).

The first round of the subsidies was announced in June 2011, three months after the earthquake, and granted in August 2011 (SME Agency of Japan 2011). As of December 2018, more than 7 years after the earthquake, the program continues to provide subsidies to SMEs. The policy outflow is extremely large, with a total of 504 billion yen (approximately 4.5 billion US dollars) granted to 705 groups as group subsidies by 2018.

For illustrative purposes, let us provide two examples from the first round of the subsidy program (SME Agency of Japan 2012). In the first example, 17 firms in the electronics and precision machinery industries in the coastal areas of the Iwate prefecture formed a group and received the subsidies. These firms were linked through supply chains and shared other business relationships prior to the earthquake. One of the recipient firms, which had five of its plants completely destroyed by the tsunami, received 700 million yen to purchase production facilities for relocating the plant to a different location. Although this firm laid off all its 230 employees after the earthquake, it was projected to rehire 70 employees in 2012 because of the subsidies received. The second example is taken from the retail sector. In Iwate prefecture, 30 retail shops in a shopping center were flooded after the tsunami and caught fire owing to the earthquake formed a group to receive the subsidies of 670 million yen to repair buildings and facilities. This subsidy facilitated the reopening of the shopping center in December 2011, 9 months after the earthquake.

Although this policy targeted groups of firms, it was not supposed to promote new links among firms in the disaster areas. Because targets of the subsidies were firms in industrial and retail clusters and those linked through supply chains, groups were usually formed by firms that were already connected prior to the earthquake (SME Agency of Japan 2011). In addition, once approved, a subsidy was provided to each firm in the group, rather than to the representative of the group. Therefore, we would not expect additional interactions among recipient firms in the same group or additional effects of such interactions on firm performance after receiving the subsidies.

3.3. Data

3.3.1. Data Source

This study utilizes the firm-level data collected by the Tokyo Shoko Research (TSR), one of the two largest corporate research companies in Japan. The TSR data contains corporate information, such as each firm's location, sales value, and the number of employees, and information on up to 24 suppliers of intermediates and up to 24 clients of products. Because the TSR data include the identification number of each supplier and client, we could identify the network of firms through supply chains in Japan. Although the upper limit of the number of suppliers and clients, 24, is too small for many large firms, it still captures most of the supply chain networks by considering the supplier-client relationships in each direction. The TSR data are also used in, for example, Carvalho et al. (2016) and Inoue and Todo (2019b, 2019a), who examine the propagation of negative shocks through supply chains after the earthquake.

Specifically, we utilize the TSR data licensed to the Research Institute of Economy, Trade and Industry (RIETI) in 2011 and 2014. Because most corporate information is collected a year before the year of licensing, our data cover detailed corporate information published in the fiscal years 2010 and 2013. Additionally, because the TSR data include information about sales in the previous year, data on sales during the fiscal years 2009 and 2012 are available. Our TSR data for 2010 contain 1,161,096 firms and 4,971,671 supply chain links.

Because the TSR data are at the firm level, supplier-client relationships at the plant level could not be identified. Because of this data limitation, we fail to identify earthquake-hit firms headquartered outside the disaster areas, but operating a production plant in the disaster areas. However, our analysis focuses on SMEs, of which only 19 % had more than one branch (including the headquarter) in 2010 and thus the ratio of those involved in this failure should be even smaller. Also, when we restrict to firms with only one plant or establishment, the results are found to be essentially the same.

3.3.2. Identification of the Disaster Areas and Subsidized Firms

We assume that firms in the disaster areas hit by the earthquake or the subsequent tsunami suffered direct damage from the earthquake. In this study, the disaster areas are defined by the combination of three types of regions. The first type is areas officially determined by the Act on Special Financial Support to Deal with the Designated Disaster of Extreme Severity. Second, we also identify areas that were reached by the tsunami, using the geographical information provided by the Center for Spatial Information Science at the University of Tokyo based on Article 41-2 issued on April 28, 2011 by the Ministry of Land, Infrastructure, Transport and Tourism (MLIT). Finally, we regard as the disaster areas those in which residents were required to evacuate because of the accident of the Fukushima Daiichi Nuclear Power Plant after the tsunami and thus firms were required to shut down. These evacuation areas are defined by the official map provided by the Ministry of Economy, Trade and Industry. We focus on the four prefectures that were severely affected by the disaster, Aomori, Iwate, Miyagi, and

Fukushima, and areas in other prefectures are excluded from the disaster areas. The disaster areas defined in this study are filled in Figure 3.1, whereas the four prefectures are filled or hatched.

Names of firms that received the subsidies in 2011 and 2012 are available in lists of recipient firms provided by the governments of the four prefectures affected by the disaster. Because only the prefecture of each recipient firm, rather than its detailed address, is available in the lists except for that of the Iwate prefecture, we identify recipient firms in each prefecture in the TSR data by using their names and corporate classifications, e.g., two types of limited liability companies specific to Japan, *kabushiki gaisha* and *yugen gaisha*. We find a small percentage of firms in the Fukushima prefecture that are matched with more than one firm in the TSR data on using only names and corporate classifications. In these cases, we chose firms in the TSR data that can be classified as SMEs according to the definition of SME Agency of Japan (2018). Despite these efforts, we fail to accurately identify 27 subsidized firms in Fukushima in the TSR data, or 0.7% of all subsidized firms. The unidentified firms are excluded from our sample. Because the list of recipients in the Iwate prefecture shows the city of each firm, we also used city names when multiple firms were matched using only firm names and corporate classifications.

Following this matching process, we match 50.3% of recipients of the subsidies on the lists with firms in the TSR data. One reason for the relatively low match ratio is that some of the names of the recipients of the subsidies in the lists were of persons, rather than those of companies. Because we presume that these enterprises represented by a person's names are most likely to be microenterprises, the omission of these firms from the sample may not result in substantial biases in estimates. Another reason for the relatively low match ratio is that the TSR data cover around 53% of all firms in Japan (Carvalho, Nirei, and Saito 2014).

Although the subsidies were provided to SMEs in eight prefectures, as presented in Section 3.2, the disaster areas according to the aforementioned official definition of disaster were restricted to only four of the eight prefectures, Aomori, Iwate, Miyagi, and Fukushima. Therefore, when we estimate the direct effect of the subsidies on recipient firms in the disaster areas, we ignore the recipients in the four prefectures, Hokkaido, Tochigi, Ibaraki, and Chiba, that were outside the official disaster areas. When we estimate the indirect effect on non-recipients outside the disaster areas, we treat recipients in the four prefectures outside the disaster areas as non-recipients. Biases arising from this assumption may be minimal as the number of recipients in the four prefectures outside the disaster areas is quite small compared to the total number of non-recipients outside the disaster areas in the 43 prefectures in Japan.

3.3.3. Construction of Variables and Samples

As we mentioned earlier, we use data related to 2010 and 2013. Our primary outcome variables are sales, the number of workers, and sales per worker in the two time periods. As explained in detail later, we employ the PSM estimations. Our covariates in the pre-earthquake period for matching include sales, the number of workers, firm age, the firm president's age, the number of plants, an index of credit evaluation provided by TSR, the number of suppliers, and the number of customers in 2010, as well as the sales growth from 2009 to 2010. These variables are taken directly from the TSR data. Additionally, our covariates contain dummy variables for firms in the official disaster areas (when appropriate), for firms hit by the tsunami in particular, and for firms that were forced to evacuate owing to the accident in the Fukushima Daiichi Nuclear Power Plant after the tsunami, and the number of SMEs in disaster-hit areas located within 1 km from the focal firm. These variables can be constructed from geographic information of the officially identified tsunami-hit areas and the evacuation areas. We also incorporate dummies for different levels of seismic intensity at the city/town level. In Japan, a particular scale called *shindo* ranging from zero to seven is usually used for the seismic intensity. For example, the seismic scale of 6- and 6+

is declared when people can barely stand and some wood-made houses fall down, and when people cannot stand and many wood-made houses fall down, respectively. Because most of the disaster areas experienced the seismic scale of at least 5+, we utilize dummies for 6-, and 6+ of the scale.

In several estimations, we focus either on small- or medium-sized firms. We follow the SME Agency of Japan (2018) to define SMEs, as shown in Table 3.1. Because the subsidies were primarily provided to the secondary and tertiary industries, we drop entities in agriculture, forestry, and fishery and public entities, such as governments, academic institutions, schools, and political and religious institutions.

3.3.4. Descriptive Statistics

Table 3.2 presents summary statistics for the three samples of firms used in the estimations: (1) SMEs in the disaster areas; (2) SMEs in the disaster-hit prefectures that are linked to firms in the disaster areas through supply chains; and (3) SMEs outside the disaster-hit prefectures that are linked to firms in the disaster areas through supply chains. The first sample is used to estimate the direct effect of the subsidies, whereas the second and third are used to estimate the indirect effect of the subsidies, or propagation of their effect, within and beyond the disaster-hit prefectures, respectively. The average sales value in 2013 for the first, second, and third sample is 540, 479, and 2,539 million yen, respectively. The significant difference between the first two and the third reflects the fact that firms in the disaster-hit prefectures are relatively small, particularly compared with those in industrial cities such as Tokyo and Osaka. The average number of suppliers and customers for the first two samples is approximately five, while it is substantially larger for the third. In the first sample, firms that received subsidies in 2011 and 2012 account for 4% and 8%, respectively. In the second sample, 22% and 15% of the firms are linked with subsidized suppliers and customers through supply chains, respectively, while in the third, the corresponding shares are smaller, reflecting fewer distant links than neighboring links in general.

3.4. Empirical Methodologies

3.4.1. Estimation of Direct Effects

We first estimate the direct effect of the subsidies on the recovery and growth of firms after the damage caused by the earthquake. In these estimations, we focus on the SMEs in the officially defined disaster areas (see Section 3.3.2) and examine the possible differences in post-earthquake sales and employment between firms with and without the subsidies.

There are two potential issues that may bias the estimates. First, the subsidies were not provided randomly to SMEs in the disaster areas, but to those that were eligible and applied for the subsidies and were approved by the government of the prefecture concerned. Second, unobservable firm attributes, such as the availability of private support, may be crucial for both firm performance and the receipt of the subsidies. These two econometric issues generate biases because of endogeneity.

To correct for such endogeneity biases, we employ the PSM procedure with a DID estimation developed by Blundell and Costa Dias (2000). The PSM approach can correct biases caused by the endogenous selection of recipients based on observed characteristics, and the DID estimation can correct biases owing to unobservable firm attributes that are constant over times. This approach is often used in policy evaluation using non-experimental data. Using this approach, for example, Görg, Henry, and Strobl (2008) estimate the effect of grant support to firms on their exporting activity, whereas Beņkovskis, Tkačevs, and Yashiro (2019) examine the effect of EU regional support on firm productivity. While this approach corrects biases due to observed and time-invariant unobserved characteristics, time variant unobserved characteristics such as the existence of private support may potentially affect the result. However, because private support was often provided through supply chains to mitigate the disruption of supply chains, it is likely that the probability of receiving private support is alike among firms with similar observed characteristics. Another underlying assumption to satisfy is Stable Unit Treatment Value Assumption (SUTVA). Considering the nature of the subsidy that its use is limited to restore and reinstall facilities damaged by the disaster, the receipt of the subsidy by a firm would not worsen the activity of another firm. Positive spillover by the recovery of subsidized firms may exist, possibly causing under-estimation of the effect of the subsidy.

Specifically, we first estimate a logit model to examine factors that determine the participation of SMEs in the disaster areas in the subsidy program, using the pre-earthquake firm attributes, such as sales, the number of workers, firm age, the age of the firm's president in 2010, the number of suppliers, the number of customers, the number of damaged SMEs within a 1 km radius, and the number of plants (all of the above are log values), and the credit evaluation index provided by the TSR in 2010 and the rate of change of sales from 2009 to 2010. We include the number of suppliers and customers and the number of neighboring SMEs because the subsidy was provided to groups of SMEs, which also comprised SMEs that are linked through supply chains or geographical agglomeration (e.g., retail shops in a shopping center and manufacturing firms in an industrial park). We also incorporate dummies for firms hit by the tsunami, for firms in the evacuation areas, for different levels of seismic intensity, and for prefectures. These disaster- and geography-related dummy variables enable us to control for variation in direct damages due to the earthquake and tsunami that could have affected receipt of the subsidies.

In this first process, we divide the SMEs in the disaster areas into sector-level subsamples to allow the sectoral differences in the tendency of firms which are more likely to receive the subsidy. Although our data include industry classification codes of TSR at the three-digit level, the number of subsidized firms in the sample for measuring the direct effects is too small to divide them into detailed industry classifications even at the two-digit level. Therefore, we classify firms into four sectors: the manufacturing industry, other secondary industries, the wholesale and retail industry, and other service industries.

Next, by using the estimates from the logit model for each sector, we calculate the propensity score, the predicted probability of receipt of the subsidies, given the pre-earthquake attributes. Subsequently, we match each recipient firm with a non-participant having a propensity score closest to that of the recipient. One notable issue in this matching process is that firms' fiscal year-end months vary substantially. If the fiscal year-end month for two particular firms is different, their sales and sales growth in the pre-earthquake period are defined as those in different time periods, and may, thus, capture different economic shocks. To avoid matching two firms with similar sales or sales growth because of different economic shocks, we match firms within the same sector and same fiscal year-end month. We impose common support, dropping firms whose propensity score is outside the overlap of the two distributions of recipients and non-recipients. Additionally, we set the caliper of the difference in the propensity score at 0.05, matching two firms only when the difference between their propensity scores is less than 5%. After matching, we check whether treatment firms (recipients of the subsidies) and matched controls are balanced in terms of pre-earthquake attributes by using *t* tests.

Finally, using the matched sample, we run an ordinary least squares (OLS) estimation on the following standard DID model.

$$\ln Y_{it} = \beta_0 + \beta_1 Post_{it} + \beta_2 Subsidy_i + \beta_3 Subsidy_i \times Post_{it} + D_i \delta + \varepsilon_{it}\,, \tag{3.1}$$

where Y_{it}, $Subsidy_{it}$, and $Post_{it}$ denote an outcome variable, the dummy variable for receipt of the subsidy, and the dummy for the post-subsidy year of firm *i* in time *t*, respectively. The outcome variables are the log of sales, the number of workers, and sales per worker. Time *t* represents either the pre-subsidy year, 2010, or the post-subsidy year, 2013. D_i is a set of dummy variables, such as sector dummies, zip-code dummies, and fiscal-year-end dummies. These dummies can control for unobservable effects of sectors, geography including region-specific damages of the earthquake and tsunami, and time that may remain even after the PSM procedure. To account for possible serial correlation, we use robust standard errors clustered at the firm level.

In Section 3.5, we begin with all SMEs in the disaster areas. Subsequently, we distinguish between medium- and small-sized firms to examine whether firm size influences the direct effect of the subsidies.

3.4.2 Estimation of Indirect Effects through Supply Chains

Next, we examine whether the positive effect of the subsidies propagates through supply chains. When firms had suppliers or customers that were directly affected by the earthquake and tsunami, they could have been subjected to the indirect negative effects of the earthquake because of the disruption of supply chains, particularly in the short run, as found in the literature (Barrot and Sauvagnat 2016; Carvalho et al. 2016; Kashiwagi, Todo, and Matous 2021). However, if their damaged suppliers or customers were supported by the subsidies and, thus, recovered more quickly than otherwise, the propagation effect on the firms may have been smaller than the case when their suppliers or customers did not receive any subsidy. To estimate this indirect effect, we adopt the PSM-DID approach, which is similar to that used in the previous section.

In this estimation, we distinguish between links with recipient suppliers and customers to examine the presence of downstream (from subsidized suppliers to their customers) and upstream (from subsidized customers to their suppliers) propagation of the positive effects of the subsidies. For this purpose, we should examine the effect of links with subsidized suppliers and customers separately, because the matching with subsidized suppliers and with subsidized customers should be conducted separately.

In addition, we deal with the following two samples. The first one comprises SMEs in the four prefectures severely hit by the earthquake, Aomori, Iwate, Miyagi, and Fukushima, to examine the propagation of the effect of the subsidies within the geographically neighboring region. The other comprises SMEs outside the four prefectures to examine distant propagation. In both cases, because the subsidies were provided to only SMEs, we focus on SMEs linked with recipient SMEs. In each case, our PSM-DID approach utilizes firms that did not receive the subsidies but were linked with firms in the officially defined disaster areas through supply chains. Then, we examine whether the performance of firms linked with a subsidized supplier (or customer) is better than those linked with a non-subsidized supplier (customer).

Specifically, we first run a logit estimation to estimate the determinants of the links of firms with any supplier (customer) that received the subsidy, given any possible link with a firm in the disaster areas, for each of the four sectors defined in Section 3.4.1. The matching covariates are basically same with Section 3.4.1 but we replace *shindo* dummies with a disaster area dummy. Second, using the propensity scores from the logit estimations, we match each firm linked with any recipient supplier (customer) with a firm that is linked with any non-recipient supplier (customer) but not with any recipient supplier (customer) within the same sector and the same fiscal-year-end month. Subsequently, we run the following DID regression using OLS on the sample after the PSM:

$$\ln Y_{it} = \beta_0 + \beta_1 Post_{it} + \beta_2 LinkSubsidy_i + \beta_3 LinkSubsidy_i \times Post_{it} + D_i\delta + \varepsilon_{it}\,, \tag{3.2}$$

where *LinkSubsidy* is the dummy variable for any supply-chain link with a recipient supplier (customer). Other variables are the same as in equation (3.1). As in the estimation of the direct effect, robust standard errors clustered at the firm level are used.

3.5. Results on Direct Effects

3.5.1. Logit Estimations and Balancing Tests

We start with the benchmark results for the direct effect of the subsidies on recipient firms by following the procedure explained in Section 3.4.1. First, we run logit estimation for firms in the disaster areas in each of the four sectors. The results shown in Table 3.3 indicate that some of the covariates significantly affect the receipt of the subsidies. Caliendo and Kopeinig (2008) argue that the choice of the covariates affects PSM estimates and suggest the exclusion of covariates that do not affect the treatment significantly. We experiment with various sets of covariates, for example, by dropping insignificant covariates and including squared terms, and confirm that the PSM estimates are robust to alternative choices of covariates. Because we run similar logit estimations using several different samples later, the specifications presented in this paper use the same set of covariates in every logit estimation to avoid arbitrary choices of covariates in different estimations. Each of the covariates used is significantly correlated with the treatment variable in several estimations.

After matching by using the propensity scores obtained from the logit estimations, we check whether the treatment group and the matched control group are balanced. Specifically, we conduct *t* tests to examine whether each of the covariates is systematically different between the two groups. Table 3.4 shows the results from the *t* tests. The mean of the most covariates is significantly different between the treatment and the control group before matching, suggesting that recipients of the subsidies were self-selected. However, after matching, we cannot reject the null hypothesis that the mean is the same between the two groups for all covariates. Therefore, we conclude that the matching is appropriately done.

3.5.2. DID Estimations of Direct Effects

Using the matched sample, we estimate equation (3.1) using no dummies, using zip-code and sector dummies, and using sector, zip-code, and fiscal year-end dummies to control for unobservable effects. Our outcome variables are sales, the number of workers, and sales per worker.

The results are shown in Table 3.5. Using any outcome variable, the different sets of dummy variables in the set of controls result in a very similar value and significance of the coefficients. In the matching process, we control for prefecture dummies in the logit model and match each treated firm with a control firm in the same sector and of the same fiscal year-end month. We further confirm that no fixed effect is omitted in any regression shown in Table 3.5. Therefore, these similar results across specifications imply that the treatment group is adequately matched with the control group so that the treatment variable is not correlated with any characteristic specific to prefectures, sectors, or fiscal year-end months. Moreover, the coefficient on the dummy for the receipt of the subsidies is insignificant in the first row of all specifications, suggesting that the receipt of the subsidies is not correlated with pre-subsidy outcomes in the matched sample. We conclude from these results combined with those in the previous subsection that the matching is successful.

Most importantly, the third row labeled *Subsidies×Post* presents the effect of the subsidies on the post-subsidy outcomes. We find that the receipt of the subsidies had no significant effect on any of the outcome variables at the significance level of 5%, while the effect on sales is positive and significant at the 10% level.

3.5.3. Distinguishing between Small and Medium-Sized Enterprises

We further distinguish between small- and medium-sized enterprises, as defined in Table 3.1, and apply the same PSM-DID procedure as above. We run a set of logit estimations for the sub-sample of small or medium firms in the disaster areas and match each recipient with a non-recipient within the same firm-size category using the propensity scores. Although we do not show the results from the logit estimations or balancing tests to ensure brevity of presentation, we confirm that the treatment and the matched control group are balanced in every PSM estimation. We experiment with various sets of the dummy variables, as in Section 3.5.2, and find that the results are essentially the same. Therefore, we only show the results using the full set of the zip-code, sector, and fiscal year-end month dummies.

The results for small firms shown in columns (1) through (3) of Table 3.6 indicate that the subsidies to small firms had a positive and highly significant effect on sales and employment after the receipt of the subsidies. The effect is large because post-earthquake sales and employment of subsidized small firms are higher by approximately 12% and 7%, respectively, than those of small non-subsidized firms with similar attributes. Because both sales and employment increased, the effect on sales per capita of subsidized small firms is positive but not significantly different from zero (column [3] of Table 3.6).

To further investigate the mechanism behind the positive effect of the subsidies on small firms' sales, we experiment with the rate of profits to sales as an outcome variable. The result shown in column (4) of Table 3.6 indicates no significant effect of the subsidies on the profit rate for small firms. Combined with the insignificant effect on sales per worker of small firms, this result implies that the subsidies increased sales of small firms mostly because the subsidies enabled small firms damaged by the earthquake and tsunami to resume production earlier than otherwise by repairing or reinstalling capital goods and facilities. This earlier recovery of production activities could also protect employment that would have been lost without the subsidies.

There may be some other possible mechanisms of the positive effect on sales. For example, because the subsidies targeted groups of firms, not individual firms, firms could have improved productivity due to spillovers of knowledge and information within the group, or they could have improved markups due to increased market and bargaining power of the group. However, if this would be the case, sales per worker or the profit rate of subsidized firms should have improved. Thus, we reject the hypothesis that group formation had a positive effect on firm performance in addition to the pure recovery effect. This interpretation is in line with the fact that most groups were formed by firms that were already connected through supply chains or in industrial or commercial clusters, and thus that additional interactions among firms in the group after the receipt of the subsidies were minimal (Section 3.2).

In contrast to the positive effect on small firms, the subsidies have an insignificantly negative effect on both sales and employment of medium-sized firms at the 5% level (columns [5]-[8] of Table 3.6). The stark contrast between small- and medium-sized firms may reflect the fact that the latter are more likely to receive other types of support, such as that from their business partners, than small firms that have to rely on public support, such as the group subsidies. Todo, Nakajima, and Matous (2015) show that recovery from the earthquake was faster when firms were linked with more suppliers and customers outside the disaster areas, implying that these firms could receive support from their supply-chain partners. Kashiwagi (2020) further finds that as the number of links with firms outside disaster-hit areas increases, the recovery effect of public support disappears in some sectors, including retails and manufacturing sectors. As small firms are less likely to connect with distant partners, it may cause the difference. In other words, medium-sized firms that did not receive the group subsidy but were linked with other firms may have recovered as quickly as those that received the subsidies. Besides, the difference in the access to financial institutions, which can provide another way to restore facilities and reduces the urgent necessity of the subsidy, may exist between small- and medium-sized firms, and thus the subsidy little differentiate the post-disaster sales and employment of medium-sized firms. Another possible reason for the absence of any positive effect on medium firms is that subsidies were not sufficiently large to cover damages of medium firms. Because data on the amount of subsidies to each firm are not available, we cannot check whether this statement is justified by comparing, for example, the ratio of the amount of subsidies to sales between small and medium firms.

3.6. Results on Indirect Effects

Next, we estimate the indirect effect of the group subsidies on firms linked with subsidized firms through supply chains. We follow the procedure in Section 3.4.2 for the two sub-samples of firms: one is of the firms in the four disaster-hit prefectures, and the other is for those outside the four prefectures.

3.6.1. Indirect Effects within the Disaster-Hit Prefectures

First, we examine the propagation of the effect of the subsidies within the disaster-hit prefectures. In this estimation, we match each non-recipient of the subsidies in the disaster-hit prefectures that was linked to a subsidized supplier (or customer in another set of estimations) with another non-recipient linked with a non-subsidized firm in the disaster areas. It must be noted that the disaster areas are areas officially defined to be severely hit by the earthquake and tsunami or areas in which residents were required to evacuate because of the accident of the Fukushima nuclear plant (the filled-in areas in Figure 3.1), whereas the four disaster-hit prefectures include areas that include those outside the officially defined disaster areas (the hatched areas). We assume that supply chain links within the region are dense and strong and, thus, the indirect effects may be more prevalent within a region than outside it.

The first half of Table 3.7 shows the results from the balancing tests after matching for this estimation of the indirect effect of links with subsidized suppliers. We cannot reject the hypothesis that firm attributes are the same between the two groups after matching. For brevity of presentation, we do not show the results from balancing tests for the estimation of the indirect links with subsidized customers, but the results are quite similar to those in Table 3.7.

The estimated indirect effects of subsidized suppliers and customers are presented in Table 3.8. Column (1) indicates a positive and weakly significant effect of a subsidized supplier of the focal firm on the firm's sales and employment after the earthquake. In columns (4) and (5), we find a positive and significant effect of links with subsidized customers on sales and employment.

Further, we particularly examine the effect of a focal firm's link with subsidized *small* firms on its post-disaster performance, because the direct effect of the subsidies is found to be significant only for *small* firms. Specifically, using a sample of non-recipient firms in the disaster-hit prefectures linked with any small firm in the disaster areas, we match each firm linked with any subsidized small supplier (or customer) with a firm linked with only non-subsidized small supplier (customer). It is noted that although we restrict supply-chain links to those with subsidized small firms, our sample does not necessarily restrict to small firms but includes medium firms as well.

Columns (1) and (4) of Table 3.9 demonstrate a positive and significant indirect effect of the links with subsidized small suppliers and customers on sales. The effect is larger and more significant than the corresponding effect found in Table 3.8, confirming our presumption that links with subsidized *small* firms should have been more effective than links with subsidized *medium* firms. By contrast, column (5) of Table 3.9 shows an insignificant indirect effect on employment, although it is positive and significant when we do not restrict to subsidies to small firms (column [5] of Table 3.8). Thus, we conclude that the indirect effect on employment is not quite robust.

These findings imply that while firms linked with firms damaged by a disaster may be negatively affected by supply-chain disruptions, as found in the literature, the propagation of the negative effect can be mitigated when the damaged firms are subsidized and thus can recover earlier.

3.6.2. Indirect Effects outside the Disaster-Hit Prefectures

Next, we investigate the propagation of the effect of the subsidies outside the region. In this estimation, we utilize the sample of SMEs outside the four disaster-hit prefectures linked with SMEs in the disaster areas through supply chains and match each SME outside the disaster-hit prefectures linked with any subsidized SME with another linked with only a non-subsidized SME in the disaster areas. The right half of Table 3.7 shows that firm attributes are not systematically different between the treatment and matched control groups. Table 3.10 presents the results from the PSM-DID estimation. Further, we restrict to a sample of SMEs outside the disaster-hit prefectures linked with any *small* firm in the disaster areas, as we did in Table 3.9, and show the PSM-DID results in Table 3.11.

Table 3.10 and Table 3.11 indicate no positive and significant indirect effect beyond the region in any estimation. The effect of links with subsidized small customers on post-subsidy sales is even negative and slightly significant. This is possibly because firms outside the disaster-hit prefectures linked with any firm in the disaster areas are larger and connected with more firms than those in the disaster-hit prefectures with such a link. The average number of workers in 2010 for the former type, 46, is substantially larger than that for the latter, 17 (Table 3.2), because only relatively large and productive firms can reach distant partners even though we restrict our sample to SMEs. This is analogous to the logic that only large and productive firms can export, as found in the literature in international economics (Melitz 2003; Bernard and Jensen 2004). Large firms may not need to rely on public support by, for example, finding a substitute for affected partners in the disaster areas. Accordingly, large firms linked with affected partners without any subsidy may have recovered from the earthquake as quickly as those linked with affected and subsidized partners. This is in line with the results of Barrot and Sauvagnat (2016), Kashiwagi, Todo, and Matous (2021), and Inoue and Todo (2019a) who find an important role of substitution of partners in propagation of a shock.

3.7. Robustness Checks of the Spillover Effect

In order to test the robustness of the different spillover effects from the subsidized small firms to firms within and outside disaster-hit prefectures found in Tables 3.9 and 3.11, respectively, we conduct robustness checks. First, we replicate Tables 3.9 and 3.11 based on a simpler PSM-DID setting. Specifically, to calculate propensity scores, we run a single logit model without dividing into subsamples by sector but instead with adding sector dummies and fiscal year end month dummies as matching covariates. Besides, we match two firms that have closest propensity scores regardless of their fiscal year end month and sector. Other settings such as covariates are same with the baseline. The results for the spillover within disaster-hit prefectures are presented in Table 3.12. Similar to Table 3.9, we find a positive and significant indirect effect of the links with subsidized small suppliers and customers on sales and sales per worker and insignificant indirect positive effects of those on employment after the disaster, confirming the positive spillover within the disaster-hit prefectures.

The results for the indirect effect to firms outside disaster-hit area also give consistent ones with the baseline, Table 3.11. They are presented in Table 3.13. We do not find any significant effect of the links with subsidized small suppliers and customers on any of three outcome measures after the disaster, suggesting that no spillover of the effect of the subsidy to firms outside disaster-hit areas through supply chains.

Next, we change our estimation method from PSM to Inverse Propensity Score Weighting (IPSW). The advantage of this method is IPSW utilizes full sample so that this has more statistical power. Besides, it considers the degree of the similarity of the matched pairs. However, it is not so simple to use a propensity score calculated by sector in reweighting approaches like IPSW as in PSM, where unites are simply discarded to improve the balance. Thus, we follow the simple procedures similar to the first set of the robustness checks as in above paragraphs. We also impose the common support restriction as we do in estimations with PSM. As in Tables 3.14 and 3.15, the results for the spillover effect within and outside disaster-hit prefectures are consistent with the baseline shown in Tables 3.9 and 3.11.

Estimation using Entropy Balancing (EB) also gives similar results, as in Tables 3.16 and 3.17. EB is another reweighting method to achieve covariate balance. EB's attractive feature is that covariate balance is ensured in its process unlike IPSW. The basic settings are the same with the other estimations in this section, but we change the preprocessing method to EB and remove common support restriction.

Overall, we find that the effect of the subsidy spillovers through supply chains within a region but not to firms outside a region. Therefore, we conclude that our findings are robust to various preprocessing models.

3.8. Conclusions

This study evaluates the impact of the subsidies to repair and reinstall damaged capital goods and facilities of SMEs affected by the Great East Japan earthquake. Our innovation is that we estimate the indirect effect on firms that did not receive the subsidies but were linked with recipient firms through supply chains. The indirect effect is worth investigating because many recent studies show that negative shocks of natural disasters propagate through supply chains (Barrot and Sauvagnat 2016; Carvalho et al. 2016; Kashiwagi, Todo, and Matous 2021). We employ a PSM-DID approach to minimize possible biases owing to endogeneity and identify the average treatment effect on the treated (ATT).

We start with the analysis of direct effect of the subsidies and find a positive effect of the subsidies on post-disaster sales and employment of small recipient firms, that is, those with 20 employees or less in the manufacturing sector and five or less in the service sector (Table 3.1). Because we also find no significant effect on their sales per worker or profit rate, we conclude that the positive effect of the subsidies stems mostly from earlier recovery of production activities of the small recipients, not from their larger market power or productivity spillovers due to group formation.

By contrast, we find no positive and significant effect on any outcome of medium-sized recipients. This contrast between small and medium firms may be attributed to the fact that the medium-sized firms were more likely to receive other supports from, for example, their business partners, such as suppliers and customers, or to have access to financial institutions than small firms. Alternatively, the contrast may be due to that the amount of the subsidies to medium firms was not sufficient compared with their firm size.

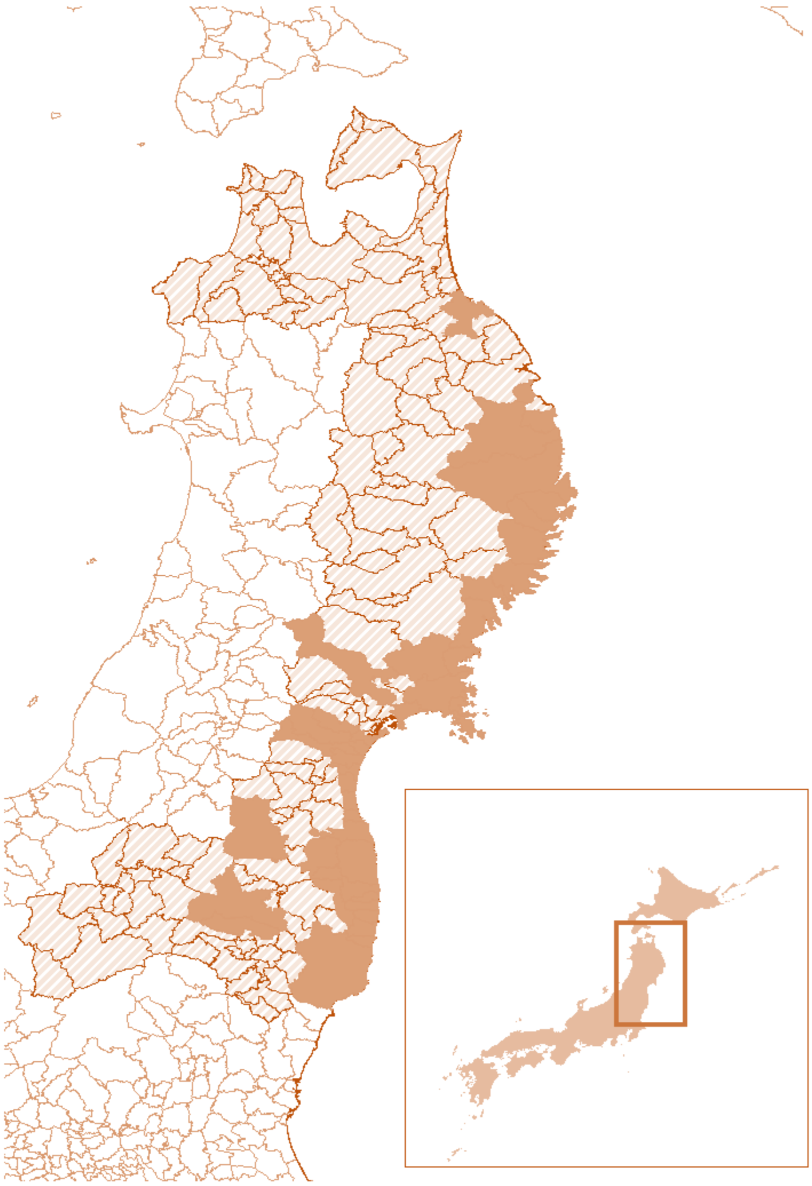
We also find that links with subsidized small firms lead to higher sales of their neighboring supply-chain partners. This evidence clearly shows that the positive effect of the subsidies on small firms propagated through supply chains within the region. However, our results reveal no indirect effect beyond the disaster-hit prefectures, possibly because distant firms linked with subsidized firms were relatively large and were not significantly affected by the links.

Our finding on the positive indirect effect of policies through supply chains would contribute to the literature, showing an important policy implication that such indirect effects should be incorporated when post-disaster policies are evaluated. Although previous studies have found that supply chains can be a channel of propagation of the negative shocks caused by natural disasters, this study shows that they can also be a channel of propagation of positive policy effects, thereby mitigating their negative effects. The finding of the positive role of supply chains after natural disasters is in line with Todo, Nakajima, and Matous (2015), who find their similar positive role in facilitating support from business partners to firms damaged by disasters. These positive roles of supply chains should not be undervalued when we consider policies for recovery from natural disasters.

However, our analysis also reveals that the group subsidies are not always effective. Particularly, we find that medium-sized firms that did not receive the subsidy recovered as quickly as those that received it. This result should be interpreted with caution because we examined relatively long-term effects, i.e., effects 2 to 3 years after the earthquake, and ignored the immediate effects of the subsidies. However, this suggests that larger firms are more likely to receive support from other sources, such as supply-chain partners, than the government. Hence, the government should be careful about providing post-disaster support to eligible SMEs to ensure the efficient use of public resources.

As global value chains (GVCs) have expanded to many countries, including emerging and less developed countries (Baldwin 2016), these policy implications are applicable to other countries that have many SMEs that have been integrated into GVCs and that experience a number of major disasters. Our analysis suggests that subsidies to firms to restore and reinstall capital goods can be quite effective to facilitate the recovery of disaster-hit regions, particularly when the region has a cluster of firms linked through supply chains. Further, the government may have to focus on small firms as the target recipients.

Figure 3.1: Disaster Areas and Disaster-Hit Prefectures



Notes: The disaster areas are filled, whereas the four disaster-hit prefectures (Aomori, Iwate, Miyagi, and Fukushima) are filled or hatched.

Table 3.1: Definition of SMEs

Industry	Small and medium enterprises (either 1 or 2 is satisfied)		Small enterprises
	(1) Paid-in capital	(2) Number of full-time workers	Number of full-time workers
Manufacturing, construction, transport, and others	300 million yen or less	300 or fewer	20 or fewer
Wholesale	100 million yen or less	100 or fewer	5 or fewer
Retail	50 million yen or less	50 or fewer	5 or fewer
Other services	50 million yen or less	100 or fewer	5 or fewer

Source: SME Agency (2018)

Table 3.2: Summary Statistics

Samples	(1) Firms in the disaster areas (N = 9,697)				(2) Firms in the disaster-hit prefectures linked with firms in the disaster areas (N = 12,838)		(3) Firms outside the disaster-hit prefectures linked with firms in the disaster areas (N = 10,740)	
Variable	Mean	S.D.	Min.	Max.	Mean	S. D.	Mean	S.D.
Sales (1000 yen, 2013)	540250	2971060	100	131000000	478677	2162921	2539207	14300000
(in logs)	11.86	1.47	4.61	18.69	11.93	1.39	13.20	1.74
Sales (1000 yen, 2010)	443950	2533274	141	123000000	394676	1733089	2353018	12100000
(in logs)	11.71	1.39	4.95	18.62	11.78	1.34	13.18	1.70
Number of workers (2013)	18.26	66.73	1.00	3741.00	17.20	49.00	47.30	117.46
(in logs)	2.05	1.14	0.00	8.23	2.11	1.09	2.90	1.35
Number of workers (2010)	17.66	58.90	1.00	2469.00	16.66	48.66	46.26	108.97
(in logs)	2.04	1.12	0.00	7.81	2.09	1.07	2.91	1.34
Sales growth (2009-10)	-0.04	0.30	-2.71	3.64	-0.03	0.31	-0.02	0.31
Firm age	28.53	14.18	1.00	101.00	28.41	13.94	36.15	17.15
(in logs)	3.25	0.58	0.69	4.62	3.25	0.56	3.48	0.57
President's age	59.95	10.57	27.00	101.00	59.97	10.49	59.87	10.54
(in logs)	4.08	0.19	3.30	4.62	4.08	0.18	4.08	0.19
Number of plants (log)	0.23	0.57	0.00	11.00	0.23	0.56	0.57	1.25
Credit evaluation index (0-100)	49.31	4.68	22.00	73.00	49.21	4.83	51.19	6.14
Number of suppliers	4.74	6.65	0.00	151.00	5.30	6.92	10.92	15.90
(in logs)	1.42	0.78	0.00	5.02	1.54	0.76	2.06	0.89
Number of customers	4.63	15.81	0.00	781.00	4.87	12.81	16.72	41.02
(in logs)	1.24	0.90	0.00	6.66	1.33	0.87	2.15	1.13
Dummy for small firms	0.69	0.46	0.00	1.00	0.68	0.46	0.39	0.49
Number of SMEs within 1km radius (in logs)	4.24	1.36	0.00	7.27	-	-	-	-
Dummy for disaster areas	1.00	0.00	1.00	1.00	0.58	0.49	0.00	0.00
Dummy for tsunami-hit areas	0.12	0.32	0.00	1.00	0.04	0.20	0.00	0.00
Dummy for evacuation areas	0.01	0.09	0.00	1.00	0.00	0.06	0.00	0.00
Seismic scale in the disaster areas (4-6+)*	6.04	0.34	4.00	6.50	-	-	-	-
Dummy for subsidy recipients	0.12	0.32	0.00	1.00	-	-	-	-
Dummy for subsidy recipient in 2011	0.04	0.20	0.00	1.00	-	-	-	-
Dummy for subsidy recipient in 2012	0.08	0.26	0.00	1.00	-	-	-	-
Dummy for a link with any subsidized supplier	-	-	-	-	0.22	0.41	0.11	0.31
Dummy for a link with any subsidized customer	-	-	-	-	0.15	0.36	0.09	0.29

*: The seismic scale of 5-, 5+, 6-, and 6+ are counted as 5, 5.5, 6, and 6.5, respectively. We take logs after adding one if the minimum value of the variable is zero.

Table 3.3: Logit Estimations

	(1)	(2)	(3)	(4)
Dependent variable:	Receipt of the group subsidies in 2011 or 2012			
Sales (log)	0.509***	0.182	0.0968	0.166
	(0.134)	(0.117)	(0.100)	(0.106)
# of workers (log)	0.0574	0.245*	0.441***	0.240**
	(0.136)	(0.125)	(0.115)	(0.107)
Sales growth (2009-2010)	0.153	0.0658	-0.470	-0.614*
	(0.345)	(0.202)	(0.379)	(0.346)
firm age (log)	0.839***	0.604***	0.632***	0.811***
	(0.192)	(0.158)	(0.154)	(0.161)
President's age (log)	-0.592	-0.207	-0.361	-0.243
	(0.495)	(0.362)	(0.371)	(0.443)
# of plants (log)	0.112	0.977***	0.543***	0.296
	(0.273)	(0.275)	(0.208)	(0.258)
Credit evaluation index	-0.0148	0.0131	-0.0167	-0.0459***
	(0.0208)	(0.0162)	(0.0205)	(0.0178)
# of suppliers (log)	-0.355**	-0.0539	0.120	-0.0787
	(0.177)	(0.114)	(0.145)	(0.141)
# of customers (log)	-0.000233	0.116	-0.0192	0.107
	(0.135)	(0.108)	(0.0731)	(0.0916)
Tsunami dummy	2.559***	2.509***	2.536***	2.361***
	(0.217)	(0.150)	(0.181)	(0.207)
Evacuation dummy	1.946**	3.109***	3.117***	-
	(0.856)	(0.307)	(0.812)	-
# of damaged SMEs within 1km (log)	-0.0521	-0.200***	-0.0669	-0.257***
	(0.0679)	(0.0502)	(0.0666)	(0.0616)
Seismic scale 6-	1.053	15.69	14.75	0.304
	(1.151)	(680.9)	(994.1)	(1.043)
Seismic scale 6+	0.886	15.12	14.32	0.766
	(1.170)	(680.9)	(994.1)	(1.062)
Prefecture dummies	YES	YES	YES	YES
Number of observations	1,126	4,035	2,464	2,048
Pseudo R2	0.250	0.271	0.227	0.215

Notes: Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This table is the results of estimation for SMEs in the disaster areas in each of the four sectors: column (1) manufacturing, column (2) other secondary industries, column (3) the wholesale and retail industry, and column (4) other service industries. We take logs after adding one if the minimum value of the variable is zero.

Table 3.4: Balancing Tests for Direct Effect

Variable	Before matching				After matching			
	Mean		*t* value	*p* value	Mean		*t* value	*p* value
	Treated	Control			Treated	Control		
Sales (log)	12.36	11.61	17.42	0.00	12.25	12.25	0.02	0.99
# of workers (log)	2.59	1.95	18.52	0.00	2.51	2.50	0.09	0.93
Sales growth (2009-2010)	-0.03	-0.04	1.09	0.28	-0.03	-0.02	-0.56	0.57
firm age (log)	3.45	3.22	12.57	0.00	3.41	3.42	-0.46	0.65
President's age (log)	4.08	4.08	1.46	0.15	4.08	4.09	-1.21	0.23
# of plants (log)	0.29	0.13	16.42	0.00	0.25	0.26	-0.25	0.81
Credit evaluation index	50.64	49.11	10.47	0.00	50.48	50.52	-0.15	0.88
# of suppliers (log)	1.65	1.39	10.79	0.00	1.62	1.61	0.19	0.85
# of customers (log)	1.46	1.20	9.27	0.00	1.44	1.37	1.45	0.15
# of damaged SMEs within 1km (log)	3.99	4.28	-6.54	0.00	4.03	3.98	0.77	0.44
Tsunami dummy	0.45	0.07	39.82	0.00	0.34	0.34	0.05	0.96
Evacuation dummy	0.04	0.00	11.26	0.00	0.03	0.03	0.00	1.00
Seismic scale of 6	0.60	0.60	0.03	0.97	0.60	0.61	-0.44	0.66
Seismic scale of 6+	0.30	0.24	4.41	0.00	0.30	0.30	0.10	0.92

Note: We take logs after adding one if the minimum value of the variable is zero.

Table 3.5: Direct Effect of the Subsidies: All SMEs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:		Sales (log)			Employment (log)			Sales per worker (log)	
Subsidies	0.00121	0.0748	0.0761	0.00506	0.0543	0.0568	-0.00361	0.0204	0.0191
	(0.0668)	(0.0670)	(0.0657)	(0.0554)	(0.0547)	(0.0538)	(0.0390)	(0.0379)	(0.0377)
Post	0.104***	0.104***	0.104***	-0.0213	-0.0213	-0.0213	0.145***	0.145***	0.145***
	(0.0213)	(0.0214)	(0.0214)	(0.0130)	(0.0130)	(0.0131)	(0.0196)	(0.0197)	(0.0198)
Subsidies×Post	0.0494*	0.0494*	0.0494*	0.0192	0.0192	0.0192	0.0305	0.0305	0.0305
	(0.0291)	(0.0292)	(0.0293)	(0.0187)	(0.0188)	(0.0188)	(0.0275)	(0.0276)	(0.0277)
ZIP code FE	NO	YES	YES	NO	YES	YES	NO	YES	YES
Industry FE	NO	YES	YES	NO	YES	YES	NO	YES	YES
Fiscal year end FE	NO	NO	YES	NO	NO	YES	NO	NO	YES
Number of firms	1764	1764	1764	1764	1764	1764	1764	1764	1764
Adjusted R2	0.00127	0.0626	0.0878	-0.000771	0.0983	0.122	0.00819	0.127	0.132

Notes: Robust standard errors clustered at the firm level are in parentheses. *Subsidies* indicate the dummy variable for receiving the group subsidy in 2011 or 2013. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6: Direct Effect of the Subsidies: Comparison between Small and Medium Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Small firms				Medium firms			
Dependent variable:	Sales	Employ-ment	Sales per worker	Profit rate	Sales	Employ-ment	Sales per worker	Profit rate
Subsidies	0.00423	-0.00362	0.00771	-0.0122	0.0927	0.0692	0.0236	-0.00279
	(0.0690)	(0.0463)	(0.0562)	(0.00910)	(0.0869)	(0.0637)	(0.0558)	(0.00480)
Post	0.136***	0.0111	0.146***	0.0550***	0.0913***	-0.0610***	0.173***	0.0207***
	(0.0315)	(0.0197)	(0.0323)	(0.0123)	(0.0274)	(0.0230)	(0.0272)	(0.00572)
Subsidies×Post	0.118***	0.0686**	0.0499	0.00594	-0.0604*	-0.0104	-0.0501	0.00280
	(0.0438)	(0.0266)	(0.0452)	(0.0147)	(0.0363)	(0.0301)	(0.0349)	(0.00771)
ZIP code FE	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES	YES	YES
Number of firms	872	872	872	520	776	776	776	513
Adjusted R2	0.0889	0.232	0.0656	0.0541	0.154	0.215	0.216	0.0442

Notes: Robust standard errors clustered at the firm level are in parentheses. *Subsidies* indicate the dummy variable for receiving the group subsidy in 2011 or 2013. *Post* is the dummy variable for the post-treatment year (2013). We include profit rate in 2010 as a matching covariate only for columns (4) and (8) to achieve balance. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.7: Balancing Tests for Indirect Effects

Variable	Sample of firms in the disaster areas after matching				Sample of firms outside the disaster areas after matching			
	Mean		*t* value	*p* value	Mean		*t* value	*p* value
	Treated	Control			Treated	Control		
Sales (log)	11.95	11.96	-0.27	0.79	12.65	12.66	-0.28	0.78
# of workers (log)	2.22	2.24	-0.68	0.50	2.60	2.62	-0.59	0.55
Sales growth (2009-2010)	-0.04	-0.04	0.73	0.47	-0.02	-0.02	-0.16	0.87
firm age (log)	3.32	3.32	-0.37	0.71	3.42	3.43	-0.12	0.91
President's age (log)	4.08	4.08	0.65	0.52	4.08	4.09	-0.30	0.77
# of plants (log)	0.20	0.19	0.41	0.68	0.31	0.31	0.10	0.92
Credit evaluation index	49.52	49.66	-0.97	0.33	50.29	50.42	-0.70	0.49
# of suppliers (log)	1.83	1.83	-0.39	0.70	2.08	2.09	-0.20	0.84
# of customers (log)	1.40	1.41	-0.46	0.64	1.77	1.76	0.23	0.81
Disaster dummy	0.64	0.64	-0.42	0.68	-	-	-	-
Tsunami dummy	0.05	0.06	-0.96	0.34	-	-	-	-
Evacuation dummy	0.00	0.00	-0.94	0.35	-	-	-	-

Note: These results are for the indirect effect of links with subsidized suppliers. Results for the indirect effect of links with subsidized customers are very similar to those in this table and thus omitted for brevity of presentation. We take logs after adding one if the minimum value of the variable is zero.

Table 3.8: Indirect Effect of the Subsidies within the Region

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized suppliers	0.0104	-0.0102	0.0205			
	(0.0375)	(0.0298)	(0.0218)			
Link with subsidized customers				0.0569	0.0319	0.0250
				(0.0454)	(0.0358)	(0.0279)
Post	0.156***	0.00810	0.167***	0.161***	0.00396	0.176***
	(0.0104)	(0.00704)	(0.0105)	(0.0122)	(0.00734)	(0.0117)
Link with subsidized suppliers×Post	0.0261*	0.0148	0.0113			
	(0.0147)	(0.0101)	(0.0147)			
Link with subsidized customers×Post				0.0403**	0.0236**	0.0166
				(0.0176)	(0.0118)	(0.0177)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	4,908	4,908	4,908	3,292	3,292	3,292
Adjusted R2	0.0595	0.0953	0.154	0.116	0.0956	0.210

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.9: Indirect Effect of the Subsidies to Small Firms within the Region

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	-0.0391	-0.0157	-0.0233			
	(0.0955)	(0.0776)	(0.0534)			
Link with subsidized small customers				0.161	0.120	0.0408
				(0.0999)	(0.0740)	(0.0611)
Post	0.223***	0.0407***	0.203***	0.199***	0.0183	0.201***
	(0.0265)	(0.0152)	(0.0259)	(0.0220)	(0.0155)	(0.0234)
Link with subsidized small suppliers× Post	0.0895**	0.000904	0.0883**			
	(0.0376)	(0.0239)	(0.0367)			
Link with subsidized small customers× Post				0.118***	-0.00450	0.123***
				(0.0356)	(0.0233)	(0.0375)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	886	886	886	788	788	788
Adjusted R2	0.114	0.122	0.143	0.210	0.111	0.268

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Indirect Effect of the Subsidies outside the Region

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized suppliers	-0.0112	-0.0256	0.0145			
	(0.0472)	(0.0374)	(0.0250)			
Link with subsidized customers				-0.0283	-0.0283	1.62e-05
				(0.0549)	(0.0456)	(0.0311)
Post	0.0653***	-0.00142	0.0879***	0.0760***	0.00146	0.0968***
	(0.00931)	(0.00740)	(0.00964)	(0.0116)	(0.00876)	(0.0108)
Link with subsidized suppliers×Post	0.000326	-0.00352	0.00374			
	(0.0134)	(0.0104)	(0.0139)			
Link with subsidized customers×Post				-0.0232	-0.0130	-0.0102
				(0.0158)	(0.0144)	(0.0157)
Prefecture FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	3,784	3,784	3,784	2,714	2,714	2,714
Adjusted R2	0.254	0.223	0.278	0.340	0.223	0.371

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1

Table 3.11: Indirect Effect of the Subsidies to Small Firms outside the Region

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	-0.0268	-0.0601	0.0334			
	(0.189)	(0.147)	(0.0893)			
Link with subsidized small customers				-0.136	-0.0902	-0.0460
				(0.127)	(0.102)	(0.0705)
Post	0.0269	-0.0433**	0.0951***	0.0289*	-0.0371***	0.0909***
	(0.0319)	(0.0198)	(0.0319)	(0.0164)	(0.0129)	(0.0164)
Link with subsidized small suppliers ×Post	-0.00823	0.00390	-0.0121			
	(0.0411)	(0.0279)	(0.0412)			
Link with subsidized small customers ×Post				-0.0547*	-0.0313	-0.0234
				(0.0309)	(0.0358)	(0.0281)
Prefecture FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	310	310	310	496	496	496
Adjusted R2	0.235	0.277	0.303	0.246	0.182	0.285

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1

Table 3.12 Indirect Effect of the Subsidies to Small Firms within the Region (with PSM-DID in Different Setting)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	0.0296	0.0267	0.00285			
	(0.0926)	(0.0760)	(0.0533)			
Link with subsidized small customers				0.128	0.0743	0.0534
				(0.0965)	(0.0713)	(0.0565)
Post	0.230***	0.0231	0.226***	0.201***	0.0151	0.206***
	(0.0249)	(0.0177)	(0.0256)	(0.0225)	(0.0157)	(0.0231)
Link with subsidized small suppliers× Post	0.0762**	0.0157	0.0607*			
	(0.0357)	(0.0251)	(0.0357)			
Link with subsidized small customers× Post				0.105***	-0.00534	0.110***
				(0.0357)	(0.0231)	(0.0359)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	988	988	988	888	888	888
Adjusted R2	0.132	0.122	0.153	0.177	0.120	0.258

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1. Although, in the baseline estimation, we run logit estimation by sector and matching observations with those with the same sector and fiscal-year end, the estimation shown in this table run a single logit estimation and matching observations even if the pair does not share their sector or fiscal-year end.

Table 3.13 Indirect Effect of the Subsidies to Small Firms outside the Region (with PSM-DID in Different Settings)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	0.0346	0.0437	-0.00911			
	(0.165)	(0.132)	(0.0795)			
Link with subsidized small customers				0.0104	0.0248	-0.0144
				(0.134)	(0.104)	(0.0706)
Post	0.0635*	0.00766	0.0807**	0.0144	-0.0194	0.0587***
	(0.0334)	(0.0271)	(0.0384)	(0.0177)	(0.0149)	(0.0198)
Link with subsidized small suppliers× Post	-0.0408	-0.0327	-0.00808			
	(0.0405)	(0.0319)	(0.0450)			
Link with subsidized small customers× Post				-0.0339	-0.0450	0.0111
				(0.0302)	(0.0334)	(0.0297)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	390	390	390	562	562	562
Adjusted R2	0.144	0.165	0.270	0.172	0.119	0.269

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1. Although, in the baseline estimation, we run logit estimation by sector and matching observations with those with the same sector and fiscal-year end, the estimation shown in this table run a single logit estimation and matching observations even if the pair does not share their sector or fiscal-year end.

Table 3.14 Indirect Effect of the Subsidies to Small Firms within the Region (with Inverse Propensity Score Weighting)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	0.125	0.118**	0.00651			
	(0.0809)	(0.0584)	(0.0564)			
Link with subsidized small customers				0.113	0.136*	-0.0230
				(0.103)	(0.0798)	(0.0539)
Post	0.212***	0.0252***	0.208***	0.236***	0.0240***	0.233***
	(0.00967)	(0.00645)	(0.00970)	(0.00757)	(0.00499)	(0.00774)
Link with subsidized small suppliers× Post	0.0915***	0.0149	0.0768**			
	(0.0328)	(0.0240)	(0.0323)			
Link with subsidized small customers× Post				0.110**	-0.00998	0.120**
				(0.0546)	(0.0212)	(0.0555)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	3,668	3,668	3,668	4,457	4,457	4,457
Adjusted R2	0.144	0.138	0.154	0.136	0.149	0.284

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.15 Indirect Effect of the Subsidies to Small Firms outside the Region (with Inverse Propensity Score Weighting)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	0.175	0.156	0.0183			
	(0.128)	(0.106)	(0.0547)			
Link with subsidized small customers				-0.108	-0.0527	-0.0553
				(0.113)	(0.0839)	(0.0623)
Post	0.0404***	-0.0165*	0.0818***	0.0215***	-0.00367	0.0500***
	(0.0110)	(0.00936)	(0.0121)	(0.00784)	(0.00571)	(0.00806)
Link with subsidized small suppliers× Post	-0.00205	-0.0137	0.0116			
	(0.0271)	(0.0210)	(0.0284)			
Link with subsidized small customers× Post				-0.0646*	-0.0630	-0.00160
				(0.0377)	(0.0388)	(0.0363)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	1,501	1,501	1,501	2,260	2,260	2,260
Adjusted R2	0.147	0.168	0.240	0.234	0.149	0.294

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.16 Indirect Effect of the Subsidies to Small Firms within the Region (with Entropy Balancing Model)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	0.140*	0.0878	0.0523			
	(0.0812)	(0.0629)	(0.0418)			
Link with subsidized small customers				0.188**	0.141**	0.0473
				(0.0885)	(0.0624)	(0.0498)
Post	0.210***	0.0250**	0.205***	0.217***	0.0248***	0.213***
	(0.0165)	(0.0120)	(0.0130)	(0.00954)	(0.00649)	(0.00949)
Link with subsidized small suppliers× Post	0.0951***	0.0149	0.0802***			
	(0.0293)	(0.0204)	(0.0268)			
Link with subsidized small customers× Post				0.0729***	-0.0165	0.0894***
				(0.0271)	(0.0169)	(0.0265)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	3,749	3,749	2,749	4,491	4,491	4,491
Adjusted R2	0.120	0.126	0.150	0.276	0.180	0.298

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1.

Table 3.17 Indirect Effect of the Subsidies to Small Firms outside the Region (with Entropy Balancing Model)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Sales	Employ-ment	Sales per worker	Sales	Employ-ment	Sales per worker
Link with subsidized small suppliers	-2.71e-05	2.43e-06	-2.95e-05			
	(0.129)	(0.102)	(0.0623)			
Link with subsidized small customers				0.000167	9.74e-05	7.01e-05
				(0.101)	(0.0811)	(0.0530)
Post	0.0313*	-0.0232*	0.0794***	0.0288***	-0.00673	0.0604***
	(0.0164)	(0.0127)	(0.0176)	(0.0107)	(0.00600)	(0.0106)
Link with subsidized small suppliers× Post	-0.00974	-0.00135	-0.00839			
	(0.0274)	(0.0206)	(0.0286)			
Link with subsidized small customers× Post				-0.0473*	-0.0546*	0.00737
				(0.0260)	(0.0287)	(0.0240)
ZIP code FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Fiscal year end FE	YES	YES	YES	YES	YES	YES
Number of firms	1,608	1,608	1,608	2,366	2,366	2,366
Adjusted R2	0.142	0.171	0.259	0.182	0.126	0.266

Notes: Robust standard errors clustered at the firm level are in parentheses. *Post* is the dummy variable for the post-treatment year (2013). *** p<0.01, ** p<0.05, * p<0.1.

4. Building New Network Links

This chapter investigates whether and how a natural disaster affects the expectation for support from different religious groups in future emergencies. Among the various dimensions of social ties, helping network is context-specific connections but so invaluable in emergencies that the improvement in expectations of help can enable stronger relationships among people. We conducted a household survey in Central Sulawesi, Indonesia, which was hit by the 2018 Sulawesi earthquake. In this region, people tended not to expect emergency cooperation from organizations with religious affiliations that differ from their own before the earthquake because of the deeply rooted psychological barriers across religions. Our empirical results suggest that the individuals who suffered from the earthquake exhibit higher expectations for access to emergency support from other religious groups. Furthermore, we find that the receipt of emergency support both at the individual level and the village level contributes to the improvement. These findings imply that disasters expand individuals' subjectively perceived safety nets by providing opportunities for people to realize the potential.

This chapter is based on Kashiwagi and Todo (2021a), "How Do Disasters Change Inter-Group Perceptions? Evidence from the 2018 Sulawesi Earthquake" published in RIETI Discussion Paper Series.

4.1. Introduction

Natural disasters may change individual perceptions toward others. For example, a growing strand of literature finds that social preferences can change after disasters (Cassar, Healy, and von Kessler 2017; Chantarat et al. 2019; Fleming, Chong, and Bejarano 2014; Andrabi and Das 2017; Shoji 2018a; Castillo and Carter 2011; Samphantharak 2014; Voors et al. 2012). While among various aspects of individual perceptions, preferences shape economic decisions and behaviors and thus the post-disaster changes gain growing attention, the similarly essential factor determining these is subjective expectations, as summarized in Delavande, Giné, and McKenzie (2011). Regarding post-disaster changes, Chantarat et al. (2019) and Chantarat, Lertamphainont, and Samphantharak (2016) found that a severe flood in Cambodia and Thailand, respectively, reduced the perception of dependability of intragroup social networks in future floods.

This paper extends the literature, focusing on the expectations of individuals for dependability on others in different social groups from their own group in case of emergency for the following three reasons. First, intergroup social networks are an important part of social capital (Granovetter 1973; Burt 1992), which has been found to affect economic development (Knack and Keefer 1997; Zak and Knack 2001; Toya 2014; Putnam, Robert, and Raffaella 1993) and violence (Lederman, Loayza, and Menéndez 2002; DELLER and DELLER 2010; Messner, Rosenfeld, and Baumer 2004). In addition, economic growth and violence have been observed to be influenced by natural disasters (Sarsons 2015; Trainor, Barsky, and Torres 2006; Goltz 1984; Quarantelli 1994; Rodríguez, Trainor, and Quarantelli 2006; Harper and Frailing 2010; Shoji 2018b; Sawada, Bhattacharyay, and Kotera 2011; Fomby, Ikeda, and Loayza 2013; Loayza et al. 2012; Cavallo et al. 2013; Hsiang and Jina 2014; Noy 2009; Noy and Nualsri 2007; Raddatz 2009; Skidmore and Toya 2002; Strobl 2012). Especially, the impact of disasters on violence is observed to be larger in religiously divided societies (Shoji 2018b). Therefore, to understand post-disaster development and peace, it is worthwhile to explore how disasters change intergroup perceptions.

Second, the literature often focuses on the impacts of natural disasters on social capital, violence, and economic development, and limited studies investigate whether and how disasters revise subjective expectations for emergency assistance from others. An exception is Chantarat et al. (2019) and Chantarat, Lertamphainont, and Samphantharak (2016), who examine the impact of disasters on the subjective expectation for emergency help from the government and people linked through social networks in regions where little variation in ethnicity and religion exists. However, among the three dimensions of social capital that are observed to change after natural disasters, i.e., bonding social capital within the group, bridging social capital between groups, and linking social capital between people and organizations (Andrabi and Das 2017; Toya 2014; Fleming, Chong, and Bejarano 2014), Chantarat et al. (2019) and Chantarat, Lertamphainont, and Samphantharak (2016) analyze the first and third. Therefore, we pay particular attention to the second, social capital that bridges across groups. Because bridging social capital is usually weaker (Etang, Fielding, and Knowles 2011; Binzel and Fehr 2013; Johansson-Stenman, Mahmud, and Martinsson 2009) and works differently (Granovetter 1973; Burt 1992) than bonding social capital, the two types of social capital should be affected by natural disasters differently.

Finally, existing results on the impact of disasters on perceptions toward others are mixed. Chantarat et al. (2019) and Chantarat, Lertamphainont, and Samphantharak (2016) find a negative effect of a severe flood on the expectation for emergency help from people connected through social networks and its negative effect on the number of dependable friends. These results are consistent with the negative effects of natural disasters on social capital found in Chantarat et al. (2019), Castillo and Carter (2011), Fleming, Chong, and Bejarano (2014), and Toya and Skidmore (2014).[7] In contrast, their positive effects on perceptions are found in Cassar, Healy, and von Kessler (2017), Castillo and Carter (2011), Andrabi and Das (2017), and Fleming, Chong, and Bejarano (2014). Moreover, anecdotal evidence following the 2004 Indian Ocean Tsunami shows that a disaster can have contrasting consequences for social unity and ties. The tsunami hit two conflict zones in Aceh of Indonesia and Sri Lanka, where severe military conflicts for independence had lasted for approximately three decades before the tsunami. After the tsunami, the conflict between the Indonesian government and the Free Aceh Movement was resolved, with the tsunami likely playing the primary role in leading to peace (Pandya 2006; International Crisis Group 2005; Sukma 2006). However, the same tsunami escalated the confrontation between the central government and the Tamil Tiger in Sri Lanka (Beardsley and McQuinn 2009). These contrasting results were generated possibly because the perception of others in adversary groups changed positively in Indonesia and negatively in Sri Lanka in the process of their recovery from the tsunami.[8] Therefore, testing whether disasters improve or deteriorate the expectation for emergency assistance from people in other groups and what

[7] Those studies are based on econometric analysis using micro data from developing countries, while Toya and Skidmore (2014) is a country-level analysis using the data from both developed and developing countries.

[8] The way the adversary group involved the recovery process was quite different in these two countries. In Indonesia, the central government and Indonesians outside Aceh provided long-lasting and great support to the devastated areas of the tsunami and played a significant role in their reconstruction and recovery process. By stark contrast, the Tamil Tiger in Sri Lanka obstinately prevented the Sri Lankan government, military, and all the other entities outside the Tamil Tiger from directly providing relief supplies, including drinking water, and ordered all to channel emergency support through Tamil Tiger's aid body (International Crisis Group 2006; Beardsley and McQuinn 2009)

differentiates the direction of the effect would help interpret the mixed results on the concomitant social capital and decision-related and behavioral changes found in the literature.

To answer these questions, we took the case of the 2018 Sulawesi earthquake (hereafter, the earthquake) in Central Sulawesi Province of Indonesia on September 28, 2018, and conducted a household survey of 4,154 cacao farmers in two affected regencies (local administrative units under provinces) in 2019. Using the survey data, we investigate the impact of the earthquake on the respondents' expectations for emergency support from different religious groups. Our target areas are suitable for this research for the following five reasons. First, the earthquake was of Mw. 7.5 and associated with a tsunami of 3 meters high and liquefaction in some areas, killing 4,340 people and causing a direct economic loss of 1.45 billion USD. Because of the substantial impact of the earthquake, we would expect possible changes in the perception toward people in other groups. Second, approximately half of the targeted areas were heavily hit by the earthquake, while the other half were not. By surveying all cacao farmers in the targeted subdistricts, our sample consists of two groups, one affected by the earthquake and the other not, that were exogenously divided by the earthquake. This natural experimental setting allows us to estimate the effect of disasters on the perceptions of different groups. Third, there were armed conflicts between Christians and Muslims in Central Sulawesi from 1998 to 2001. Even after the cease fire in 2001, open and hidden hostility existed among religious groups throughout the province (Beech and Suhartono 2018; Diprose and Ukiwo 2008; Ploughshares 2005). Therefore, it is interesting to see how perceptions of different religious groups measured by expectation of support from other religious groups in case of emergency changed after the earthquake. Fourth, because the central government of Indonesia generally restricted the activities of international nongovernmental organizations (NGOs) in the affected areas after the earthquake, providers of relief aid were mainly domestic organizations, except for official international organizations. In addition, severe damage to the local airport prevented aid groups from flying to the area in the early stage. Religious organizations located on Sulawesi Island, in addition to local volunteers and other local organizations, provided support from the very early stage.[9] Therefore, support from Muslim Indonesians to Christian Indonesians and vice versa is predicted to a large extent. Finally, our target region, Sigi and Donggala in Central Sulawesi Province, are among the three regencies hit by the earthquake and were a single regency until 2008. Therefore, these two regencies are likely to be institutionally analogous.

Our estimation results show that the earthquake enhances the expectation of farmers hit by the disaster to receive emergency support from villagers of other religions. In addition, we find that a possible mechanism of this effect is farmers' experiences of receiving or observing support activities by organizations of other religions after the earthquake. However, this effect of the earthquake on farmers’ perception is not observed when the atmosphere in their village is aggressive or when they used to live in the conflict zone as a member of one of the primary religious groups involved in the conflict. Furthermore, the heterogeneity in the effect depending on the types of disasters and damage is observed.

This paper contributes to the existing literature in four ways. First, a growing strand of literature shows the impact of disasters on the perception or preference of sufferers. Disasters or negative economic shocks have been

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[9] For example, Caritas Makassar, a Christian organization, was reported on the next day to be already en route to the devastated areas from the place approximately 10 hours away by car.

found to change disaster victims' risk preferences (Cameron and Shah 2015; Page, Savage, and Torgler 2014; Ingwersen 2014; Hanaoka, Shigeoka, and Watanabe 2018; Chantarat et al. 2019; Reynaud and Aubert 2014; Cassar, Healy, and von Kessler 2017; Eckel, El-Gamal, and Wilson 2009; Van Den Berg, Fort, and Burger 2009; Bchir and Willinger 2013; Willinger, Bchir, and Heitz 2013), time preferences (Cassar, Healy, and von Kessler 2017; Voors et al. 2012; Callen 2015), trust (Cassar, Healy, and von Kessler 2017; Chantarat et al. 2019; Fleming, Chong, and Bejarano 2014; Andrabi and Das 2017; Shoji 2018a), altruism (Chantarat et al. 2019; Castillo and Carter 2011; Cassar, Healy, and von Kessler 2017; Samphantharak 2014; Voors et al. 2012), and subjective expectations of future disasters (Cameron and Shah 2015; Chantarat et al. 2019; Chantarat, Lertamphainont, and Samphantharak 2016) and intragroup social networks (Chantarat et al. 2019; Chantarat, Lertamphainont, and Samphantharak 2016). Although most studies on social preferences and perceptions focus on the change among homogeneous groups or neighborhoods without exploring the heterogeneity across the closeness in the relationship, there are some exceptions. For example, Fleming, Chong, and Bejarano (2014) and Andrabi and Das (2017) find a positive impact of disasters on trust in strangers and foreigners, respectively. In contrast, the city-level analysis by Drago, Belloc, and Galbiati (2016) indicates that the power of political-religious leaders was substantially enhanced by an earthquake. Our results add to the literature by providing evidence on improving interreligious perceptions in a divided society after a severe disaster.

Second, our outcome measure, the expectation for access to help from other groups in case of emergency, is closely related to risk sharing behaviors. When people in various groups expect support from each other in case of emergency, they count on each other as their safety net and thus can share their risks. In particular, in rural areas of developing countries, because formal insurance mechanisms and official safety nets by the government are insufficient, informal mechanisms, such as support from relatives or friends, play an essential role in overcoming negative shocks due to disasters and other events (Besley, 1995; Rosenzweig, 1988b; Rosenzweig and Stark, 1989; Fafchamps and Lund, 2003; Fafchamps and Gubert, 2007; Angelucci et al., 2018). Among various informal mechanisms, risk sharing across groups with different attributes and preferences is found to be more effective because different groups face different idiosyncratic shocks. However, such risk sharing relationships across heterogeneous groups are rarely constructed in practice because of the costs of network formation (Fafchamps and Lund, 2003; Fafchamps and Gubert, 2007). We empirically find that the earthquake led to both actual emergency support beyond religious differences and higher expectations for intergroup support in the case of emergencies, while keeping the expectation for intragroup support at the same level with those who did not suffer from the earthquake. These findings imply that disasters can help people expand their risk-sharing networks.

Third, this paper also relates to the literature on the macroeconomic impact of disasters (Cavallo et al. 2013; Hsiang and Jina 2014; Noy 2009; Noy and Nualsri 2007; Raddatz 2009; Strobl 2012). Some of the studies find a positive impact of disasters (Skidmore and Toya 2002; Sawada, Bhattacharyay, and Kotera 2011; Fomby, Ikeda, and Loayza 2013; Loayza et al. 2012). In particular, Toya (2014) points out that disasters improve all three dimensions of social capital, i.e., bonding, bridging, and linking social capital, leading to economic growth after disasters. Our findings provide micro evidence that supports the positive impact of disasters on bridging social capital and thus one channel of post-disaster growth. Our finding is also in line with the argument in economics that ties with physically or topologically distant agents provide large benefits (Beugelsdijk and Smulders 2003;

Amiti and Konings 2007; Todo, Matous, and Inoue 2016; Frankel and Romer 1999; Keller 2004).

Last, this study contributes to a series of studies that try to understand post-disaster changes in rivalry relationships and conflicts in divided societies. How disasters change conflicts in and between divided societies has long been discussed (Sukma 2006; Pandya 2006; International Crisis Group 2005; Beardsley and McQuinn 2009). Similarly, deterioration and improvement of relationships among rivalry countries after disasters have been observed, as summarized in Kelman (2011). However, those findings mostly rely on country- or subnational-level data or qualitative case studies, and empirical studies using micro data on post-disaster changes in rivalry are lacking. Thus, it is difficult to interpret the opposing effect of disasters observed in previous literature. This study examines interreligious relationships within a rural divided community, adds new evidence, and provides practical implications.

4.2. Background

4.2.1. Conflicts between Muslims and Christians

In Central Sulawesi, Muslims (77.72% in the 2010 census) and Christians (Protestant 16.98% and Catholic 0.82% in the 2010 census) are the two major religious groups. Although gulfs between religious groups did not exist in the precolonial period or pretwenties century, religious rivalries were developed by the 1980s through policies by the Dutch Indies government and the Suharto administration. During the period called *zaman gerombolan* in the 1950s and 60s, Muslim rebels tortured or killed Christians, and some Christians retaliated. It remains a sensitive memory to both Muslims and Christians in the region (Aragon 2014). From 1998 to 2001, religious conflicts between Muslims and Christians occurred in Poso, Central Sulawesi. Because of conflicts, approximately 1,000 people were killed, and tens of thousands of people expelled from Poso (British Broadcasting Corporation 2004). Although a declaration of peace was signed by both sides in December 2001, people throughout Central Sulawesi still suffer from sporadic violence. For example, in 2005, various incidents of bombing, shooting dead, or beheading Christians occurred in the province, and on December 31, 2005, a bombing attack at a market in Palu that was popular among Christians killed seven people and made approximately 45 people wounded allegedly by Islamic militants. In September 2006, three local Catholics were executed to incite violence against Muslims. On November 27, 2020, in Sigi Regency, a terrorist attack by Muslims killed four Christians and burned down eight houses in a Christian community. More incidents exist, and such interreligious violence can also be observed for other religions, such as bombing of a Hindus temple in Poso in 2006. Because of such historical backgrounds, people are so sensitive to religious differences that they rarely expect interreligious cooperation, even in the case of emergency.

4.2.2. The 2018 Sulawesi Earthquake

This region was hit by the 2018 Sulawesi earthquake that occurred on September 28, 2018. The earthquake was reported to have a moment magnitude scale of 7.5 (Mw7.5) at a depth of 20.0 km, and shaking reached IX (VIOLENT) on the 12-point modified Mercalli intensity (MMI) scale (United States Geological Survey 2018). Earthquakes equivalently categorized in the MMI include the 2004 Indian Ocean earthquakes and tsunami and the Great East Japan earthquake in 2011. It was the second most devastative disaster in Indonesia since the 2004

Indian Ocean earthquakes and tsunami, following the 2006 Yogyakarta earthquake on Java Island. The 2018 Sulawesi earthquake also triggered tsunamis, liquefaction, and landslides. There were several foreshocks of the earthquake of Mw7.5, of which the largest one was an earthquake of Mw6.1 and hit just south of the epicenter of the Mw7.5 earthquake three hours earlier. Other foreshocks were weak. In addition, a series of aftershocks followed, but even the largest aftershock was Mw5.8 and did not result in additional large damage. The disaster caused 4,340 deaths, 1.5 million affected individuals, and economic damage of 1.45 billion USD (EM-DAT 2021; OCHA 2018).[10] The damage was concentrated along the Palu-Koro fault line, as explained in detail later.

Indonesia is prone to natural disasters such as earthquakes because it is located around the collision point of three tectonic plates. However, where and when a major earthquake strikes are unpredictable by today's science and technology (Asim et al. 2018).

4.3. Data

4.3.1. Survey

We conducted a post-disaster survey of households in two of the three worst-affected districts, the Sigi and Donggala regencies, Central Sulawesi Province, Indonesia, from July to August 2019. As mentioned above, these two regencies belonged to be the same regency until 2008, only a decade ago. In addition, cocoa farmers in the 13 targeted subdistricts (out of 31 subdistricts) in these regencies were universally surveyed by an agricultural NGO one year before the disaster based on the census of cocoa farmers, although it only collects agricultural information. Our survey targeted all 4,154 cocoa farming households surveyed by the organization. Among them, 90% joined our survey. In this study, we use only the post-disaster survey data we collected because of the lack of information required for our analysis of the pre-disaster data.

In 91% of the surveyed households, we interviewed the household head. If the household head was unavailable, another family member, mostly the spouse of the head, responded to the survey. The survey was implemented by local enumerators who speak the local language, Bahasa Indonesia, using a questionnaire translated from English to Bahasa Indonesia.

In the survey, in addition to standard questions about household characteristics, questions about expectations of possible emergency support from various sources were asked. Specifically, we asked the respondents to choose all actors they can ask for help when they are in trouble, such as when facing food or water shortages after natural disasters. The possible choices to the question include Christian organizations, Muslim organizations, Christians living in the same village, Muslims living in the same village, politicians, government officials, the village leader, and others. Thus, we can capture whether each respondent expects to obtain help from different religious groups in emergencies.

4.3.2. Variable Construction

Figure 4.1 depicts the geographic distribution of sample households, colored red, yellow, or green depending on the level of damage by the earthquake, as explained later. The two regencies stretch along the north-south

[10] Casualties and economic damage are taken from EM-DAT, while the number of affected is from OCHA.

direction and cover a considerably wide range of land areas, totaling 10,471.71 km^2, which is mostly mountainous. Accordingly, the households in our sample are clustered in several habitable regions located vertically on the map. Figure 4.1 also shows the epicenter of the mainshock, which is located close to the center of the sample households; dotted circles that show the distance from the epicenter; and all fault lines in this region provided by the GEM Global Active Faults Database by Styron and Pagani (2020). A long fault line in the center of the map, the Palu-Koro fault line, is quite close to some of the sample households, while it is not so close to others. The Sulawesi earthquake was a lateral-fault type earthquake that often concentrates physical damage along earthquake faults. The Palu-Koro fault line is the one that caused massive physical damage in the case of the 2018 Sulawesi earthquake (Geospatial Information Authority of Japan 2019). Accordingly, we observe large variations in the level of damage experienced by our sample households by the earthquake. In Figure 4.1, red squares representing fully destroyed houses are concentrated along the fault line, whereas green squares representing houses without any damage are often located far from the fault.

Based on this observation, we measure the intensity of the earthquake on each household by the minus log of the shortest distance from the Palu-Koro fault line to the household. We take minuses so that this measure is increasing in the degree of shocks to each household by the earthquake. In our data, although enumerators were supposed to record the longitude and latitude of each household surveyed, geolocation information is missing for 111 households, i.e., 2.98% of those who responded to the survey. In these cases, we measure the distance from the fault line by the mean value of the distance among households in the same village. Excluding these observations does not affect the baseline results, as we will explain later.

In some alternative specifications, we measure the intensity of earthquake shocks by the degree of damage to houses. In our survey, we asked about the degree of damage to houses on a 5-point scale: no damage at all, some cracks on the wall, partly destroyed, half-destroyed, and fully destroyed. Using this item, we create a dummy variable that is coded one if the house was destroyed partly or more by the earthquake and zero otherwise.

To control for the potential effect of distance from the epicenter, we include the minus log of the distances from the epicenters of the main shock and the largest foreshock that occurred earlier on the same day. The epicenter information was taken from United States Geological Survey (2018).

In the questionnaire, we also have a multiple-choice question that asks "When you are in trouble such as food or water shortage after a natural disaster, who can you ask for help?" The answer options include Christian organizations, Muslim organizations, Christians living in the same village, and Muslims living in the same village, as well as options not related to religions such as village leaders. Combining the response to this question with the information about their religion, we create dummy variables that measure whether they expect intergroup emergency support from organizations in other religions and from villagers in other religious groups.[11] In addition, it should be noted that this question is hypothetical, and the respondents who received support from other religious

[11] For example, if a Muslim interviewee has selected Christian organizations and Christians living in the same village, then both dummy variables for the expectation for intergroup cooperation are coded one. If an interviewee is Hindu and select Muslim organizations, then only the first one, a dummy for such expectation toward other religious organization, is coded one. If a Muslim selects only Muslim Organizations, both dummy variables are coded zero.

organizations in the recovery process from the 2018 Sulawesi earthquake do not necessarily expect such intergroup emergency support in the future and vice versa.

In some specifications, we use an alternative outcome measure that captures whether the respondent counts the community members as a source of help in need, even if religious differences exist. Our data contain each household's helping network information. Specifically, we asked respondents to list all the people from whom they can borrow Rp. 500,000 (approximately 35 US dollars as of September 2021) when they face any difficulty due to, for example, illness, accidents, crimes, and disasters, and need money. Then, we further asked the respondents about attributes of the listed people, such as their relationships with the respondents, location, and religion. From the information, we create a dummy variable that is coded one if the respondent has named neighbors in different religious groups as a possible source of such support and zero otherwise.

4.3.3. Descriptive Statistics

Table 4.1 shows summary statistics. 20.1 percent and 12.5 percent of respondents expect that they can rely on villagers and organizations in other religious groups, respectively, to obtain emergency support. The minimum, average, and maximum distances from the Palu-Koro fault line are 0.03, 11.72, and 79.4 km, respectively. This implies that some of the households experienced quite strong tremors that caused significant damage, while others were far away enough to have no damage.

Among the respondents to our survey, only 14.1 percent are female, possibly because we mainly surveyed household heads. More than half of them did not complete middle school. The average age among those who provided birth-year information is 45. Most of them (90.2%) never moved out of the regency in the past 20 years. A total of 1.2 percent of the respondents, dominantly Christians, used to live in Poso within the past 20 years. A total of 3.3 percent had lived outside Sulawesi Island within the past 20 years. Looking at their ethnicity, the shares of Kaili and Bugis, two major ethnicities, are 39.6 and 17.9 percent, respectively. The shares of Muslims and Christians are 50.2 and 31.3 percent, respectively. The average amount of annual cacao production is 636.9 kg, and less than 10 percent earn their income mainly from off-farm activities. According to PT Koltiva, which has a large database of Indonesian cocoa farmers, CocoaTrace, the demographics from our sample generally match the overall demographics of more than 160,000 cocoa farmers registered in CocoaTrace, although the female farmer ratio is slightly lower in our data.

4.4. Empirical Strategy

4.4.1 Conceptual Framework

In a society segregated into conflicting groups, mutual help between groups is not commonly expected. However, when a severe natural disaster impacts society, some people are in a critical situation where they lack essential goods and services, such as clean water, food, houses, and medical services. Then, they may have to ask people in another group for emergency help because people in the same group are similarly affected by the disaster. If they can successfully obtain support from the conflicting group,[12] they may positively revise their perception

[12] While we conducted the survey, we heard such stories in the devastated area of the earthquake. For example,

toward the group and expect possible assistance from the group in case of emergency in the future. As summarized in Delavande, Giné, and McKenzie (2011), past experience by individuals often predicts their expectations about future outcomes. Besides, experience of or information from others close to them is sometimes associated with their expectations. Even if they themselves have not received support from the conflicting group, but if their friends, relatives, or neighbors have received support from the group, they may also revise their perception toward the group positively. Therefore, people who suffered from a severe disaster may have higher expectations for emergency support from other groups than those who did not.

However, the mechanism of updates of individuals' perception may be more complex under uncertainty, as Kahneman et al. (1982) suggest. For instance, people tend to overvalue new information that is consistent with prior beliefs and satisfies their own current preferences and undervalue it otherwise (Nickerson 1998; Rabin and Schrag 1999; Kahneman et al. 1982). If this overvaluation is sufficiently large, people who received emergency support from other religious groups may stick to their prior belief that other religious groups would not help them and perceive the new positive signal as a coincidence. A new negative signal such as other groups' refusal to help may reinforce people's prior belief. In these cases, people who suffered from a severe disaster may have equal or lower expectations for emergency support from other groups than those who did not.

4.4.2. Estimation Methodologies

We estimate the impact of the earthquake on the expectations for possible emergency support beyond religious borders by ordinary least squares (OLS) using the following linear probability model with subdistrict and religion fixed effects, δ_s, δ_r, respectively:

$$ExpSupport_{i,2019} = \alpha + \beta_1 Earthquake_{i,2018} + X_{i,2019}\,\gamma + \delta_s + \delta_r + \epsilon_{i,s,r,2019}. \tag{4.1}$$

The dependent variable, $ExpSupport_{i,2019}$, is a dummy variable that measures whether individual i expects emergency support from organizations or villagers in different religious groups. *Earthquake* is a variable indicating the intensity of earthquake shocks measured by the minus log of the distance from the Palu-Koro fault line. The distance from the fault line is also used in previous literature (Andrabi and Das 2017). X is a vector of controls. We control the two major ethnicity dummies (Kaili, Bugis), female dummy, and religion dummies. The age of the respondent is also controlled. For people who do not know their age or whose age information is missing, we code age as 999. The dummy variable indicating the missing value of age is also included. We also consider the education level using the set of dummies that indicate the respondents' highest education status: elementary school incomplete, elementary school graduate, middle school graduate, high school graduate, and college graduate, setting no education as the baseline category. For some observations, education information is unavailable. In those cases, we created a dummy indicating that the education level was missing. Dummy variables for moving status in the last 20 years are also included. In particular, we considered whether each respondent had

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a Muslim woman told us that she sought for water shortly after the 2018 Sulawesi earthquake and lined up for emergency water assistance, but she failed to obtain it from her fellow communities due to excessive demand. Therefore, she asked for help to an unfamiliar Christian organization distributing emergency aid supplies nearby, although she expected to be refused because of the historical background between Muslims and Christians. Then, she successfully obtained water from the Christian organization.

never moved out, lived outside Sulawesi Island for more than six months, or lived in Poso Regency in Central Sulawesi Province for more than six months. Poso regency is where the latest brutal conflict took place. In addition, to control the income level, total cacao production (kg/year), a set of dummies for the share of nonagricultural income (positive but less than half, half, more than half), and the interaction terms between the amount of production and each dummy for the share are included. All of the variables above are directly taken from the survey data unless otherwise mentioned. Distances from the epicenters of the 2018 Sulawesi earthquake and the largest foreshock that occurred earlier on the same day are also included as controls in the same form as the distance from the Palu-Koro fault. In estimation, we rely on robust standard errors clustered at the subdistrict level.

4.4.3. Identification Strategies

Our identification assumption is that the treatment variable *Earthquake*, or the distance from the Palu-Koro fault to each household, is exogenous. This assumption is likely to hold because earthquakes along the Palu-Koro fault line have been rare and unpredictable, as their recurrence interval is approximately 700 years over the past 2000 years (Bellier et al. 2001). However, there may be systematic differences between households living near active faults and others in their attributes. If unobserved characteristics of each household in error terms are correlated with the treatment variable, its effect on perception estimated by OLS is biased.

To check whether the above assumption holds, we conduct two types of tests. First, we conduct balance tests to confirm that our treatment variable is uncorrelated with observed household characteristics, regressing several variables on our treatment variable with a set of fixed effects. The results shown in Table 4.2 indicate that the treatment variable is not significantly correlated with cacao production of the household or the gender or education level of the respondent. However, there remains a possibility that the treatment variable is still correlated with unobserved characteristics, such as pre-disaster social preferences, but we cannot test the correlation due to lack of pre-disaster data. Therefore, we further conduct a placebo test, regressing the variable for post-disaster perception on the distance from an active dextral fault line in the same regency, other than the Palu-Koro fault line, as shown in Figure 4.1.[13] The results presented in Table 4.3 indicate that the effect of the placebo variable is statistically insignificant, suggesting that the distribution of individual perceptions toward other groups is generally unrelated to the distance from an active dextral fault. Therefore, we assume that prior to the 2018 Sulawesi earthquake, unobserved household characteristics that determine perceptions toward other groups are not correlated with the distance from the fault line that caused the 2018 Sulawesi earthquake.

[13] Placebo tests are originally referred to medical experiments to test the effectiveness of medicine by giving one group of patients an ineffective medicine or placebo and the other group possibly effective medicine. Accordingly, one basic way to conduct a placebo test in econometric analysis is replicating analysis using a fake treatment as the key independent variable, as explained in World Bank (2016). Although analysis using a fake outcome, such as pretreatment value of the outcome, as the dependent variable is sometimes called a placebo test in the literature. In this chapter, we use the term "placebo test" when we use a fake treatment variable as the key independent variable, because our data do not include pre-disaster information that can be used as a fake outcome variable.

4.5. Results

4.5.1. Benchmark Results

Table 4.4 shows the benchmark results from OLS estimations of equation (4.1). We first estimate the impact of the level of physical shocks by the earthquake, *Earthquake*, defined by the minus log of the km distance from the Palu-Koro fault to each household. We find in columns (1)-(3) of Table 4.4 that the effect of earthquake shocks on expectations for emergency support from villagers in other religious groups is statistically significant.[14] The size of the coefficient in column (3) using the full set of control variables and fixed effects indicates that doubling the distance from the fault lowers the probability of positive expectation by 4.66 percentage points. Because its average probability is 20 percent (Table 4.1), this effect is economically significant. As shown in Appendix Table 4.1, such a significant change has not been observed for the expectation for village leaders or for those in the same religion.[15]

In columns (4)-(6) of Table 4.4, the effect of earthquake shocks on expectations for emergency support from organizations, rather than villagers, in other religious groups is estimated. Although the effect is positive and significant in column (4), it is not in columns (5) and (6) where fixed effects are included. These results imply that the earthquake may not have changed the expectation for support from organizations of other religious groups.

4.5.2. Robustness Checks

To check the robustness of the baseline results, we experimented with several alternative methods and specifications. First, our benchmark results are generated from OLS estimations of a linear probability model, following previous studies such as Drago, Belloc, and Galbiati (2016) to estimate the impact of earthquakes on socioeconomic outcomes. The linear probability model has an advantage over binary response models, such as logit and probit models. For example, with logit or probit models, all the observations for which the independent variables perfectly predict the outcome are dropped from the analysis. In our case, if the expectation dummy of all households in a particular subdistrict is zero or one, these households are dropped from the estimation. In addition, unlike estimators from linear probability models, fixed-effects logit estimators may be biased due to the incidental-parameters problem because the sample size for some religions or subdistricts is not large (Wooldridge 2002). However, we conduct logit estimations that correspond to columns (1) and (4) in Table 4.4. In the logit estimations, we do not use religion and subdistrict fixed effects to avoid dropping households with the same

[14] In an alternative specification, we replace log of distance measures with quadratic form of distances. For 98% of observations, the effect of the distance on the expectation for villagers of other religions uniformly decreases in the distance, although the coefficient of the single term is only weakly significant.

[15] The reason why we do not observe any negative effect of the earthquake on the intragroup expectation, unlike Chantarat et al. (2019), is possibly because of a relatively high ratio of recipients of informal support, 39.5%, and a low ratio of those suffering from misallocation of support, only 12.4%, although 42.3% complain that aid allocation was not fair. Chantarat et al. (2019) observes 6% for the former and similarly limited support from other sources in a sample where 65% are flooded households. Similarly, Chantarat, Lertamphainont, and Samphantharak (2016) observe low ratios of receipt.

outcome within the religion or subdistrict as described above. The marginal effects evaluated at the means of all independent variables shown in columns (1) and (2) of Table 4.5 confirm the baseline results in columns (1) and (4) in Table 4.4 from the linear probability model.

Second, as explained in Section 4.3.2, when households lack information on their longitude and latitude, we replace the missing value of the distance from the fault line with its average within each village. In this robustness check, we drop these households without locational information, or 2.98 percent of all households, and repeat the baseline analysis. Columns (3) and (4) of Table 4.5 provide quite similar results to those of the baseline in columns (3) and (6) in Table 4.4.

Third, we experiment with an alternative treatment variable that measures the level of shocks of the earthquake. Our baseline treatment variable, the minus log of the distance from the fault line, may not adequately measure the level of damage to each household because geographic situations, such as the hardness of the ground, vary. Therefore, we alternatively use a dummy variable that takes a value of one if the house of a household was damaged partly, by half, or completely by the earthquake. This variable can measure the level of damage to each household more directly. However, because the quality of a house is endogenously determined by the household, including its unobserved characteristics that may also be correlated with its perception toward other groups, it should be emphasized that using the level of damage to houses may cause larger bias due to endogeneity than using the distance from the fault line. The results from the use of the alternative treatment variable are shown in columns (5) and (6) of Table 4.5. We find a positive and significant effect not only on the expectation for emergency support from villagers in other religious groups but also on that from organizations of other religious groups. The cause of the difference from the baseline result cannot be pinned down as mentioned above.

Finally, we use an alternative outcome measure based on each household's helping network. As explained in Section 4.3.2, this measure is a dummy variable that is coded one if the respondent named neighbors in different religious groups as a possible source of monetary support when she faces difficulties and zero otherwise. The results presented in Table 4.6 indicate a positive effect of the earthquake on reliance on neighbors of other religions, showing consistency with the results from the baseline outcome measure.

Overall, our results on the effect of the earthquake on the expectation for help from villagers of other religions are quite robust to various specifications, as shown in columns (1)-(3) of Table 4.4, columns (1), (3), and (5) of Table 4.5, and Table 4.6. In contrast, the effect on the expectation for help from organizations of other religions is positive and significant in column (4) of Table 4.4 and columns (2) and (6) of Table 4.5 but insignificant in columns (5)-(6) of Table 4.4 and column (4) of Table 4.5. One possible reason for the lack of robustness regarding the expectation for help from organizations of other religions may be insufficient variations in the distance from the fault line to each household once we control for subdistrict dummies. However, the difference between the longest and shortest distances within the subdistrict is on average 16 km. The mean, median, 75th percentile and 99th percentile of the distance in the subsample of damaged households are 5, 2, 6, and 24 km, respectively. Therefore, the distance from the fault line to households within each subdistrict is likely to vary sufficiently so that estimations of the effect of distance are possible. However, we are still concerned about the lack of robustness of the results using the expectation for help from other religious organizations and hence will hereafter rely on the more robust results using the expectation for help from villagers of other religions.

4.5.3. Heterogeneity

In this subsection, we will explore heterogeneity in the effect of the disaster on perception toward other groups, depending on the types of damage and disasters at the household level and characteristics at the household and village level.

Effect of Various Types of Damage and Disasters

First, we disaggregate the earthquake shock into specific damage types and explore the difference across types of damage. Specifically, we consider five types of damage to each household by the earthquake reported in the survey: damage to its house measured by a dummy variable to show if the house was destroyed partly or more; damage to its business assets measured by a dummy to show if the business assets were damaged so much that the damage negatively affected the production; loss of any household member, relative, or close friend; injury of the respondent; and injury of any household member, relative or close friend.

The results in Table 4.7 suggest that damage to business assets has a similarly positive effect to that of housing damage on the expectation for support from villagers in other religious groups, while loss of any household member, relative, or close friend has a negative and significant impact. This finding is consistent with Whitt and Wilson (2007), who find that stress due to the loss of any family member discourages cooperation among evacuees in the United States because such stress makes people turn inward-looking. In addition, the effect of own injury of the respondent or injury of any household member, relative or close friend is not significant at the 5-percent level. Overall, the results of Table 4.7 imply that although damage to physical assets by the earthquake promoted the expectation for help from people of other religions, loss of any close person led to the opposite effect, deteriorating perception toward other groups.

Second, we explore whether the effect differs by disaster type. The earthquake triggered landslides, tsunamis, fissures, liquefaction, and tremors. In our sample, all of the households reporting liquefaction also suffered from fissures. Therefore, we create three dummy variables indicating landslides, tsunamis, and fissures or liquefaction, including them as independent variables in addition to the distance from the fault line in the baseline equation. The results are presented in column (4) of Table 4.7. The effect of the distance from the fault line is similar to that in the baseline result in column (3) of Table 4.4. Landslide shows a significantly positive effect on the expectation for intergroup emergency support. However, the effect of the dummy for fissures or liquefaction is negative and significant. A total of 68.8% of households affected by a fissure or liquefaction in our sample lost people close to them, and the share is higher than that in households affected by any other disaster type. Therefore, we interpret that the negative coefficient of the dummy for fissures or liquefaction in this heterogeneity analysis is driven by the higher possibility of loss of close persons in the areas affected by a fissure or liquefaction. Castillo and Carter (2011) consistently point out that a shock that is too large might negatively affect cooperation. Tsunami does not show a significant effect.

Share of the Same Religious Group Members in Their Neighborhood

Third, religious homogeneity within the village may affect the change in the perception toward other religious groups. If most households in a village believe in one particular religion so that they helped each other after the earthquake but did not necessarily receive support from other religious groups, their view against intergroup cooperation may not have been improved. Moreover, if all households in a village share a religion, the expectation for support from people in the same village but in the other religions should be zero. In our sample, 11.15% of the sample households are located in villages where all respondents share a religion. Because our sample includes only cocoa farmers and excludes households with other professions, we cannot perfectly capture the share of each religion in each village. However, assuming that the share of each religion for cocoa farmers is similar to its share for all households, we create a dummy variable that is coded one if all the cocoa farmers in our sample belong to the same religion and zero otherwise. In other words, we assume that the dummy indicates whether the religious homogeneity of the village is quite high. Then, we include the dummy and its interaction term with the treatment variable, *Earthquake*, as independent variables and estimate equation (4.1) to examine how the effect of the earthquake varies depending on the religious homogeneity.

In column (5) of Table 4.7, we find that the coefficient of the interaction term is negative and significant, and the size is similar to that of the distance from the fault line. These findings imply that in villages with religious homogeneity, the effect of the earthquake on perception toward other religious groups is insignificant, as hypothesized above.

Past Experiences

Finally, we explore the impact of past experiences, such as those in previous severe natural disasters, and the religious conflict between Christians and Muslims in Poso, Central Sulawesi from 1998 to 2001. In our sample, some, mostly Christians, have lived in Poso in the last 20 years, i.e., during or after the conflict. They may have stronger hostility against their opponent group and may react differently to the earthquake. We create a dummy variable that indicates experiences in previous more severe natural disasters and another that indicates previous living experience in Poso and the interaction terms with the earthquake variable to estimate the effect of past experiences.

Using the whole sample, no interaction term in column (1) of Table 4.8 shows a significant effect, suggesting that on average, no heterogeneity in the effect of the earthquake was caused by experiences of living in a conflict area or past severe disasters. We further focus on the subsample of Christians for two reasons. First, in our sample, respondents who have lived in Poso are mostly Christians. Second, their experience during devastation in past disaster events may differ between Muslims, which make up the majority of the population, and comparatively fewer Christians. Even though the damage could be more severe at the individual level, the regional impact of disasters that struck during past decades was much smaller and gained less attention from other areas. Thus, available support from Christians may have been limited in the past events, and thus Christians might have had to rely on others, even when majority Muslims could deal with the disaster only among support from their fellows. The results in column (2) of Table 4.8 indicate negative and significant coefficients of the dummy for the experience of living in Poso and its interaction with the earthquake measure, suggesting that Christians who lived in Poso were less likely to improve in perception toward other religious groups after the earthquake. We interpret this as showing that people with considerable hostility toward conflicting groups based on their lively experiences find it more difficult to overcome it even when they are provided an opportunity to receive from conflicting groups. In contrast, the coefficients of the dummy for experiencing any previous severe disaster and its interaction with the earthquake measure are positive and significant. These findings imply that Christians who experienced more severe disasters in the past were likely to have higher expectations for interreligious support than those without.

In other words, the effect of disasters on perception toward other groups can be persistent, and thus, accumulating experiences of natural disasters can bring better perception toward others.

Atmosphere

Furthermore, we investigate whether the effect of the earthquake on the respondent's expectation for intergroup emergency support is affected by the atmosphere of her neighborhood. In villages where neighbors tend to be irritated and aggressive, people may be more pessimistic about the possibility of obtaining help from their neighbors. Because asking other religious groups for help is riskier in villages in a bad atmosphere than in calm and peaceful villages, people may be more hesitant to do so in the former type of village. To test this hypothesis, we measure the level of bad atmospheres in the neighborhood of each respondent by the share of villagers who answered that they often or quite often found people in their village and the surrounding areas getting easily irritated or having got into frequent arguments recently. Adding this variable and its interaction term with the earthquake variable into the baseline equation, we run the estimation. The result is presented in column (3) of Table 4.8, showing a negative and significant coefficient of the interaction term. This finding implies that a bad atmosphere in a village obstructs intergroup cooperation among people in the village and thus obstructs improvement in the perception toward others.

4.5.4. Mechanism

Finally, we explore the possible mechanism behind the better perception of the victims of the earthquake toward other groups, following the method of testing for mediation by Baron and Kenny (1986). We consider the receipt of intergroup support in the wake of the 2018 Sulawesi earthquake and the presence of such support in the respondents' villages as mediator variables. In our data, we can identify those who have received support from Christian organizations or from Muslim organizations at the household level. Using the information, we create a dummy variable that is coded one if the respondent received any assistance from organizations of different religious groups after the earthquake and zero otherwise. Furthermore, we calculate the share of recipients of such intergroup support in each village and create a dummy variable that is coded one if the share is 50% or higher. Then, we examine the correlation of each of the mediator variables with our treatment variable representing the damage by the earthquake, *Earthquake*, and with our outcome variable representing post-disaster perception toward other religious groups, *ExpSupport*. In mediation analysis, if the correlation coefficient of Earthquake on *ExpSupport* is economically zero, it is considered a complete mediation. If the size of the coefficient is reduced but still different from zero, it is a partial mediation, and potentially additional paths can exist. Because identifying the conclusive mechanism is beyond this study, we aim to capture partial mediation. To test the joint significance of paths, we use bootstrap standard errors and bias-corrected confidence intervals with 5,000 replications that are recommended by Preacher and Hayes (2008).

The results are presented in Table 4.9. In column (1), as a reference, the relationship between the minus log of kilometer distance from the fault, *Earthquake*, and the expectation for emergency support from villagers in different religious groups, *ExpSupport*, is presented. The coefficient of *Earthquake* suggests that if the distance from a fault is doubled, *ExpSupport* decreases by 12 percentage points. This correlation is statistically significant. In columns (2) and (3), we confirm that *Earthquake* is positively correlated with the respondent's receipt and her neighbors' receipt of emergency support from organizations of other religious groups. In columns (4)-(6), we add the two support-related mediator variables into the estimation model used in column (1) and repeat the OLS estimation. The positive and significant coefficients for both of the support-related variables in columns (4)-(6) suggest that both their own receipt of support from organizations in other religious groups and the receipt of such support by many neighbors are associated with a better perception toward other religious groups. These findings imply that through receiving support from organizations of different religious groups, observing such support to neighbors, or hearing positive rumors of such support, people improve their impressions of religious groups different from theirs and positively update their expectations against villagers in those religious groups.

Similar positive effects are observed even if we limit our sample to households with housing damage caused by the earthquake, as shown in Appendix Table 4.2. Some may be concerned about the endogeneity of the mediator variables for the receipt of interreligion support. Unobserved characteristics in error terms such as the personality of respondents or their previous interactions with those in different groups may be correlated with the receipt of support. If this is the case, the estimated coefficient will be biased. To address this concern, we repeat the analysis using instrumental variable (IV) estimations. Specifically, as a set of instruments, we utilize a dummy variable indicating whether the majority of people in each village observed any organization of different religious groups (either Muslim or Christian organizations) after the earthquake and a dummy indicating whether the majority did not see any government or nongovernment organization after the earthquake. These dummy variables indicate the presence of support organizations in the respondent's neighborhood, rather than receipt of support from these organizations, and thus are determined by external factors, such as the level of damage at the village level and accessibility to the village, not by characteristics of each household. Moreover, the dummies for the presence of support organizations are prerequisites for the receipt of support from them and thus are suitable instruments. The results from the IV method using these instruments shown in Appendix Table 4.3 are essentially the same as those presented in Table 4.9.

Moreover, the coefficient of the distance from the fault line in column (6) of Table 4.9 is not zero but much smaller than that in column (1) and statistically significant only at the 10-percent level compared with the significance level of 5 percent in column (1). These results suggest that the effect of damage by the earthquake on the expectation of emergency support from other religious groups is partially mediated by the direct and indirect experience of receiving support from other religious groups. This is further confirmed by the statistical significance of the indirect effects or the effects given to the outcome by the earthquake via the mediator variables presented at the bottom of Table 4.9. Therefore, we conclude that the earthquake provided its victims an opportunity to cooperate beyond religious differences, and their receipt of support from organizations of other religious groups or observation of such support to their neighbors led to higher expectations for future emergency support from other religious groups.

4.6. Discussion and Conclusion

Using household-level data collected in Central Sulawesi hit by the 2018 Sulawesi earthquake, this paper examines how experiencing a severe natural disaster changes perception toward others. This region is suitable for this analysis because psychological barriers to other religious groups are high due to the cruel interreligious conflict in this province that lasted until the early 2000s followed by sporadic attacks until recently. We find that people affected by the earthquake more severely tend to expect support from other religious groups in the future more. We interpret this result as showing that experiencing natural disasters can improve the perception of affected people toward other groups. Furthermore, our mediation analysis reveals that the better perception of the earthquake victims toward conflicting groups is partially driven by receiving support from the conflicting groups after the earthquake and observing such interreligion support to neighbors.

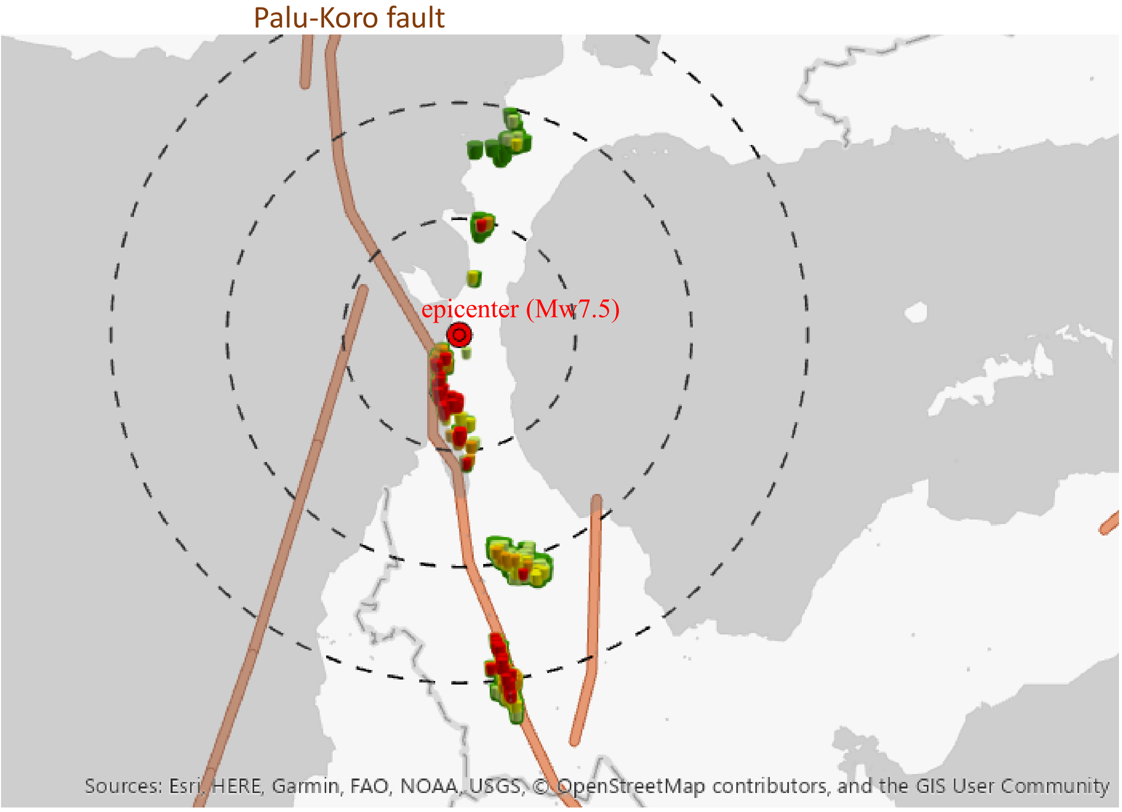
Our findings are consistent with the previous findings of positive effects of natural disasters on perception toward others outside their neighborhood by, for example, Fleming, Chong, and Bejarano (2014), and Andrabi and Das (2017). Our study extends the literature by finding a positive effect of disasters on perceptions toward conflicting groups within a village. Our conclusion is also in line with another experience in Indonesia in which the ethnic conflict in Aceh was resolved after the 2004 Indian Ocean Tsunami. In addition, our result implies that experiencing a severe disaster can expand social capital, including risk-sharing networks with groups with different attributes. Because positive effects of social capital on economic development are often found in the literature, our finding further supports existing studies that found a positive effect of natural disasters on economic growth at the country level (Skidmore and Toya 2002).

However, our results should be viewed with caution because we also find substantial heterogeneity in the effect of the earthquake on perception toward other religious groups, depending on characteristics of damage by the earthquake, households, and villages. For example, while the effect of damage to houses and production assets by the earthquake on perception toward other religious groups is generally positive, the effect of loss of close persons is negative. When people were affected by fissures or liquefaction that caused major casualties by the earthquake, their perception toward other religious groups tended to deteriorate. Therefore, the possibility that a disaster can worsen perception toward others within a divided society should not be undervalued, especially when applying the baseline findings of this paper to disasters or devastated areas with more casualties; the 2018 Sulawesi earthquake reportedly caused 4,340 deaths to the 1.5 million affected people out of the total population of 2.9 million (OCHA 2018; Neilan 2018). In addition, we find that the victims of the earthquake who have lived in a region of major fatal conflicts were less likely to be different from people with little damage by the earthquake in their perception toward conflicting groups, possibly because of their substantial hostility against conflicting groups based on their lively experiences. Furthermore, we also find that the atmosphere of neighborhoods matters to the change in their perception. The heterogeneity in the effect of the earthquake on perception toward others found in this study may explain mixed results in the empirical literature on the effect of disasters on social relationships (Chantarat et al. 2019; Castillo and Carter 2011; Fleming, Chong, and Bejarano 2014; Toya and Skidmore 2014; Cassar, Healy, and von Kessler 2017; Andrabi and Das 2017).

This study provides a policy implication for emergency support after natural disasters. Emergency support by a particular religious, ethnic, cultural, or regional group should be provided not only to victims who are in or closely related to the group but also to victims in various groups. Such attempts will help expand risk-sharing networks, improve intergroup perception, and may lead to peace. However, to maximize the positive effect of intergroup support, additional care may be necessary for those losing their close persons.

Finally, we should mention several caveats of this paper. First, we assume that our treatment measure, the minus log of the distance from the fault line to each household, is not correlated with the error term of the estimation equation. Although we tried to justify our assumption by the balance tests (Table 4.2) and the placebo tests (Table 4.3), we cannot directly control pre-disaster perception due to a lack of data. Second, although our mediation analysis suggests that receiving or observing support from other groups is a possible channel of the effect of experiencing the earthquake on perception toward other groups, we do not show the complete mechanism. We leave more precise analysis of the mechanism to future research.

Figure 4.1: Distribution of Housing Damage



Note: Each pin indicates a household's location and is colored by housing damage rank, with red for fully destroyed houses, green for no damage, and colors close to green having less damage. Damage rank categories include fully destroyed, half destroyed, partly destroyed, cracks on the wall, and no damage at all. Lines in brown are active faults identified by the GEM Global Active Faults Database by Styron, Richard, and Marco Pagani (2020). The epicenter of the mainshock is drawn in the center of the map with its concentric circles.

Table 4.1: Summary Statistics

Variable	Mean	S.D.	Min	Max
Expectation for support from villagers in different religious groups	0.201	0.401	0	1
Expectation for support from organizations in different religious groups	0.125	0.331	0	1
km from fault	11.717	13.170	0.025	79.443
-ln (km from fault)	-1.867	1.165	-4.375	3.672
-ln (km from epicenter)	-4.459	0.692	-5.104	-2.116
-ln (km from epicenter of the largest foreshock)	-4.190	1.050	-5.013	0.867
Female	0.141	0.349	0	1
Muslim	0.502	0.500	0	1
Christian	0.313	0.464	0	1
Kaili	0.396	0.489	0	1
Bugis	0.179	0.384	0	1
College graduate	0.0190	0.136	0	1
High school graduate	0.187	0.390	0	1
Middle school graduate	0.262	0.440	0	1
Primary school graduate	0.383	0.486	0	1
Primary school incomplete	0.063	0.242	0	1
Age	45.391	11.179	17	114
total cacao production (kg/year)	636.855	530.080	0	8595
off farm income share (=half)	0.223	0.417	0	1
off farm income share (<half, >0)	0.206	0.404	0	1
off farm income share (>half)	0.098	0.298	0	1
never moved out in past 20 years	0.902	0.300	0	1
once lived outside Sulawesi for more than 6 months in past 20 years	0.033	0.179	0	1
once lived in Poso for more than 6 months in past 20 years	0.012	0.110	0	1

Note: Statistics for age are those for the subsample consisting of observations with age information.

Table 4.2: Balance Test

	(1)	(2)	(3)
Sample:	All	All	All
Method:	LPM	LPM	LPM
Dependent variable:	Total cacao production (kg/year)	Female	Middle school graduates or higher
- ln (km from fault)	-5.990	0.000	-0.000
	(48.641)	(0.015)	(0.030)
N	3,641	3,641	3,641
R-squared	0.132	0.057	0.050
Controls	No	No	No
Religion FE	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. As religion FE, we include the Muslim dummy and Christian dummy. While these data are basically cross-sectional, we have some panel structures for some agriculture-related items. Even when we use total cacao production from pre-disaster data, we do not observe any significant effect. Because the pre-disaster data have many missing values, 1053 out of 3641 observations, we prefer to present the result using the value for the year that includes the month of the earthquake in between, which was collected after the earthquake.

Table 4.3: Placebo Test

	(1)	(2)
Sample:	All	All
Method:	LPM	LPM
Dependent variable:	Expectation for emergency support from other religious groups	
Support from:	villagers	organizations
- ln (km from another fault)	-0.0261	-0.202
	(0.229)	(0.132)
N	3,641	3,641
R-squared	0.403	0.276
Mean Dep.	0.201	0.125
Controls	Yes	Yes
Religion FE	Yes	Yes
Subdistrict FE	Yes	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. "- ln (km from another fault)" is measured by minus log of the distance from an active fault other than the Palu-Koro fault. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. Mean Dep.=mean of the dependent variable.

Table 4.4: Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	All	All	All	All	All	All
Method:	LPM	LPM	LPM	LPM	LPM	LPM
Dependent variable:	Expectation for emergency support from other religious groups					
Support from:	villagers	villagers	villagers	organizations	organizations	organizations
- ln (km from fault)	0.120***	0.0484**	0.0466***	0.0944***	-0.0288	-0.0209
	(0.0320)	(0.0160)	(0.0137)	(0.0198)	(0.0254)	(0.0281)
Observations	3,641	3,641	3,641	3,641	3,641	3,641
R-squared	0.121	0.379	0.406	0.111	0.252	0.273
Mean Dep.	0.201	0.201	0.201	0.125	0.125	0.125
Controls	No	No	Yes	No	No	Yes
Religion FE	No	Yes	Yes	No	Yes	Yes
Subdistrict FE	No	Yes	Yes	No	Yes	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. “Mean Dep.” indicates the mean of the dependent variable.

Table 4.5: Robustness Checks

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	Subsample with full information	Subsample with full information	All	All
Method	Logit	Logit	LPM	LPM	LPM	LPM
Dependent variable	Expectation for emergency support from other religious groups					
Support from	villagers	organizations	villagers	organizations	villagers	organizations
- ln (km from fault)	0.116***	0.0774***	0.0428***	-0.0228		
	(0.0321)	(0.0212)	(0.0130)	(0.0285)		
housing damage (≧partly damaged)					0.136***	0.0533**
					(0.0361)	(0.0230)
Observations	3,641	3,641	3,547	3,547	3,641	3,641
R-squared			0.420	0.277	0.418	0.276
Wald chi2	11.69	7.60				
Pseudo R-squared	0.127	0.154				
Log pseudo likelihood	-1594.451	-1160.074				
Mean Dep.	0.201	0.125	0.202	0.127	0.201	0.125
Controls	No	No	Yes	Yes	Yes	Yes
Religion FE	No	No	Yes	Yes	Yes	Yes
Subdistrict FE	No	No	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. Mean Dep.=Mean of the dependent variable. Columns (1) and (2) present marginal effects.

Table 4.6: Alternative Dependent Variable

	(1)	(2)
Sample	All	All
Method	LPM	LPM
Dependent variable	Any neighbor in other religious groups in helping networks	
- ln (km from fault)	0.0128**	0.0146**
	(0.00580)	(0.00535)
Observations	3,641	3,641
R-squared	0.013	0.046
Mean Dep.	0.017	0.017
Controls	No	Yes
Religion FE	No	Yes
Subdistrict FE	No	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. Mean Dep.=Mean of the dependent variable

Table 4.7: Heterogeneous Effect (1)

	(1)	(2)	(3)	(4)	(5)
Sample	All	All	All	All	All
Method	LPM	LPM	LPM	LPM	LPM
Dependent variable	Expectation for emergency support from villagers in other religious groups				
housing damage (≧partly damaged)	0.0977***	0.132***	0.0977***		
	(0.0273)	(0.0343)	(0.0278)		
big damage on business assets	0.0717***		0.0741***		
	(0.0191)		(0.0189)		
loss of family, relatives/close friends	-0.0924***	-0.0835***	-0.0705***		
	(0.0186)	(0.0168)	(0.0222)		
Injured	0.0958	0.103*	0.120*		
	(0.0575)	(0.0553)	(0.0643)		
family/relatives/close friends injured			-0.0396*		
			(0.0202)		
- ln (km from fault)				0.0583***	0.0510***
				(0.0126)	(0.0135)
landslides				0.267***	
				(0.0730)	
tsunamis				-0.0240	
				(0.181)	
Fissures/liquefaction				-0.0901**	
				(0.0326)	
dummy for homogeneous village					-0.109**
					(0.0419)
- ln (km from fault) × dummy for homogeneous village					-0.0524***
					(0.00984)
Observations	3,641	3,641	3,641	3,641	3,641
R-squared	0.427	0.424	0.428	0.446	0.407
Mean Dep.	0.201	0.201	0.201	0.201	0.201
Controls	Yes	Yes	Yes	Yes	Yes
Religion FE	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. Mean Dep.=Mean of the dependent variable

Table 4.8: Heterogeneous Effect (2)

	(1)	(2)	(3)
Sample	All	Christian	All
Method	LPM	LPM	LPM
- ln (km from fault)	0.0472**	0.108**	0.0544***
	(0.0170)	(0.0416)	(0.0158)
dummy for living experience in Poso	-0.0784**	-0.293***	
	(0.0348)	(0.0197)	
- ln (km from fault) × dummy for living experience in Poso	0.0161	-0.121***	
	(0.0675)	(0.0175)	
dummy for severer disaster experience	-0.0685	0.364***	
	(0.135)	(0.0923)	
- ln (km from fault) × dummy for severer disaster experience	-0.00764	0.244***	
	(0.0376)	(0.0427)	
bad-mood in village			-0.334***
			(0.0641)
-ln (km from fault) ×bad-mood in village			-0.112***
			(0.0191)
Observations	3,641	1,141	3,641
R-squared	0.406	0.273	0.411
Mean Dep.	0.201	0.172	0.201
Controls	Yes	Yes	Yes
Religion FE	Yes	No	Yes
Subdistrict FE	Yes	Yes	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. Mean Dep.=Mean of the dependent variable

Table 4.9: Mediation Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	All	All	All	All	All	All
Method	LPM	LPM	LPM	LPM	LPM	LPM
Dependent variable	Expectation for emergency support from villagers in other religious groups	Receipt of interreligious support	Receipt of interreligious support by majority in village	Expectation for emergency support from villagers in other religious groups		
-ln (km from fault)	0.120***	0.136***	0.127***	0.0623**	0.0731**	0.0541*
	(0.0320)	(0.0303)	(0.0269)	(0.0265)	(0.0322)	(0.0282)
dummy for support from organizations of different religious groups				0.423***		0.316***
				(0.0963)		(0.0920)
dummy for share of those receive support from organizations of different religious groups≧50% in village					0.367***	0.178**
					(0.0864)	(0.0716)
Observations	3,641	3,641	3,641	3,641	3,641	3,641
R-squared	0.121	0.163	0.145	0.266	0.230	0.282
Mean Dep.	0.201	0.191	0.186	0.201	0.201	0.201
Controls	No	No	No	No	No	No
Religion FE	No	No	No	No	No	No
Subdistrict FE	No	No	No	No	No	No
Effect	Coef.	Bootstrap SE		95% Bias-Corrected Conf. Interval		
indirect effect via own experience of receipt	0.043	0.004		[0.036, 0.051]		
indirect effect via neighbors' experience	0.023	0.003		[0.016,0.030]		
total indirect effect	0.066	0.004		[0.058,0.074]		

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. Mean Dep.=Mean of the dependent variable. Indirect effect via own experience of receipt indicates that via a mediator variable dummy for support from organizations of different religious groups, while indirect effect via neighbors' experience is that via a mediator variable dummy for share of those receive support from organizations of different religious groups ≧50% in village. They are calculated by multiplying the coefficients of each path involved. The total indirect effect is the sum of these calculated indirect effects.

Appendix

Appendix Table 4.1: Impact on the Expectation for Other Entities

	(1)	(2)	(3)	(4)
Sample	all	all	all	all
Method	LPM	LPM	LPM	LPM
Dependent variable	Expectation for emergency support from			
Belief about	villagers in the same religion		village leader	
-ln (km from fault)	0.195***	0.0368	0.0821	-0.0313
	(0.0672)	(0.0338)	(0.0480)	(0.0477)
Observations	2,969	2,969	3,641	3,641
R-squared	0.206	0.546	0.000	0.233
Mean Dep.	0.353	0.353	0.475	0.475
Controls	No	Yes	No	Yes
Religion FE	No	Yes	No	Yes
Subdistrict FE	No	Yes	No	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. Columns (1) and (2) use a subsample that includes Christians and Muslims. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. Mean Dep.=Mean of the dependent variable.

Appendix Table 4.2: Estimation of the Impact of Support using the Sample of Households with Damage

	(1)	(2)	(3)
Sample	suffered	suffered	suffered
Method	LPM	LPM	LPM
Dependent variable		Expectation for emergency support from different religious groups	
Belief about	villagers	villagers	villagers
dummy for support from organizations of different religious groups	0.283***		0.206***
	(0.0468)		(0.0380)
dummy for share of those receive support from organizations of different religious groups≧50% in village		0.278***	0.158***
		(0.0261)	(0.0258)
Observations	2,338	2,338	2,338
R-squared	0.515	0.505	0.524
Mean Dep.	0.243	0.243	0.243
Controls	Yes	Yes	Yes
Religion FE	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes

Note: Robust standard errors clustered at the subdistrict level are in parentheses. *, **, and *** signify statistical significance at the 10, 5, and 1% levels, respectively. As religion FE, we include the Muslim dummy and Christian dummy. Controls include age, dummy indicating if no information on age is available, ethnicity dummies (Kaili, Bugis), female dummy, dummies for the highest education achieved (college graduates, high school graduates, middle school graduates, primary school graduates, primary school incomplete, missing education information), total cacao production (kg/year), interaction terms between total cacao production (kg/year) and off-farm income share dummies, off-farm income share dummies (positive but less than half, half, more than half), residential history (never moved out, once lived outside Sulawesi Island, once lived in Poso), minus of log of the distance from the epicenter, and minus of log of the distance from the epicenter of the largest foreshock. Mean Dep.=Mean of the dependent variable.

Appendix Table 4.3: Estimation of the Impact of Support with the IV Method

	(1)	(2)	(3)	(4)
Sample	all	All	all	all
Method	IV	IV	IV	IV
Dependent variable		Expectation for emergency support from different religious groups		
Belief about	villagers	Villagers	villagers	villagers
-ln (km from fault)	0.0845***	0.0775***	0.0910***	0.0841***
	(0.0243)	(0.0249)	(0.0280)	(0.0286)
dummy for support from organizations of different religious groups	0.445***	0.450***		
	(0.108)	(0.121)		
dummy for share of those received support from organizations of different religious groups≧50% in village			0.289***	0.289***
			(0.0619)	(0.0696)
Observations	3,641	3,641	3,641	3,641
R-squared	0.195	0.206	0.219	0.229
Mean Dep.	0.201	0.201	0.201	0.201
Controls	No	Some	No	some
Religion FE	Yes	Yes	Yes	Yes
Subdistrict FE	Yes	Yes	Yes	Yes
Cragg-Donald Wald F statistic (weak identification test)	529.63	491.24	2934.31	2853.71
Wu-Hausman test P-value (endogeneity test)	0.001	0.004	0.906	0.873
Hansen J statistic P-value (Over identification test)	0.188	0.176	0.190	0.185

Note: Controls for this table are female dummy, Kaili dummy, Bugis dummy, total cacao production (kg/year), education level dummy (1 if middle school graduates or higher, 0 otherwise), age, and dummy for those without information on age. Subdistrict dummies are partialled out in estimating. The exogeneity of the dummy for the share of those who received support from organizations of different religious groups ≥50% in villages cannot be rejected. Mean Dep.=mean of the dependent variable

5. Summary and Conclusions

Recent disaster events such as extensive damage by earthquakes in Kumamoto prefecture, which had been said to be a region being free from earthquake risk, and record-breaking rainfall by Typhoon Hagibis, which swept East and Central Japan, are other indications of the harsh reality that today anywhere in Japan can be subject to almost all kinds of disasters but some topography-specific disasters like volcanic disasters. In addition, the possibility that two major earthquakes are predicted to hit economic agglomerations, the Tokai region and Tokyo, respectively, in not distant future. Therefore, it is with urgent necessity to establish a way to tackle disasters.

This is as true in other countries as it is in Japan. As extreme weather is becoming more frequent in Japan, so the world with less experience of disaster events faces increasing catastrophes. Now, disaster risks are a shared issue around the world.

Japan and other disaster-prone countries have developed methodologies to deal with physical damage by disasters and rescue victims. Indeed, it is not yet perfect but great progress can be observed by the significant decline in casualties by disasters. However, what with less scientific research to cushion socio-economic blow, what with increasingly networked society and economy, the socio-economic impact of disasters becomes larger and larger as in Chapter 1.

Accordingly, this dissertation investigates the socio-economic impact of disasters, shedding light on socio-economic networks. Chapters 2 and 3 look into the relationship between disaster processes and supply chain networks, while Chapter 4 explores the building of new social networks after disasters.

In Chapter 2, we examine the propagation of disaster shocks through global supply chains. One of the striking points in our analysis is that we pay more attention to the differences in network characteristics and explore how the impact of supply chain disruption is differentiated by them. Our findings suggest that domestic networks, dense networks, strong relations based on shareholding relationships, and networks of specific goods are less resilient, leading to the conclusion that the diversification of networks in terms of both geographically and topologically is the key to establish resilient supply chains.

In Chapter 3, we investigate whether the effect of the current policy scheme to support disaster-hit firms spillovers to those suffering from supply chain disruption. For medium-sized firms, which are more likely to be linked with distant firms, the impact of relief subsidy to restore or repair capitals seems to be limited likely because the provision of subsidies takes time, and thus they achieve recovery with quick remedies provided by, for example, supply chain partners outside devastated areas. Thus, we do not observe any positive spillover to their partners, either. In contrast, small firms are greatly benefitted from government support, and so do their supply chain partners within a region.

In Chapter 4, we explore how people change the attitude or view to psychologically distant groups. Specifically, utilizing the religious variations among villagers in Indonesia and the history-rooted psychological barriers across religions, we investigate how shocks by disasters change their expectation for emergency support from different religions. We find that the expectation is positively updated through the interaction and cooperation during disasters. As such expectations and risk-sharing incentive are said to promote building social relationships in previous literature, our results imply that the experience during reconstruction processes encourages people to build distant relationships or weak ties.

This dissertation provides the following policy implication about disasters and networks. To establish resilient supply chains, network diversification in terms of both geographically and topologically is essential.[16] Such diversification is also found to be profitable in normal times as it improves firms' performance, innovative capacity, productivity, and national income (Beugelsdijk and Smulders 2003; Amiti and Konings 2007; Todo, Matous, and Inoue 2016; Frankel and Romer 1999; Keller 2004; Granovetter 1973; Burt 1992). Besides, the negotiation power of each firm is considered to increase as they have wider options of transaction of a certain product (Bonacich 1987; The Small and Medium Enterprise Agency of Japan 2016). Financial institutions also evaluate firms highly if they form resilient supply chain structures and thus the risk of a downturn in business is low. Therefore, network diversification is beneficial both in normal times and in the devastation of natural disasters and thus it should be attempted unless the extra cost by small lot orders or the long distance transportation surely exceeds the expected great benefits of diversification.

Some of the major barriers that hinder the diversification is the lack of capability of finding distant partners and psychological resistance to connect with them. According to the findings in this dissertation, disasters and relief activity appear to increase the individuals' willingness to involve themselves with socially distant entities. Thus, by establishing an effective system to support them to connect with distant entities as a part of a reconstruction policy, the diversification of networks may be promoted successfully and a virtuous cycle may be created.

The dissertation also provides encouraging evidence to a current policy intervention, suggesting it help the recovery of dense networks or the network among neighborhoods. A current policy scheme which provides the public assistance for firms directly hit by disasters to repair and reinstall damaged facilities is found to be helpful not only for the targeted firms but also for firms suffering from supply chain disruptions, especially when the network links are those between firms within the four disaster-hit prefectures.

To conclude, network diversification or the existence of weak ties is the key for self-sustained resilience against disasters, and catastrophes provide a good opportunity to overcome psychological resistance to build such resilient networks. Public policy should be the one that support the private efforts for improving the resilience and supplement the limitation of self-help efforts.

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[16] Building links with a distant partner is also expected to mitigate the risk that a firm causes supply chain disruption when disasters strike the firm, and some institutions have tried to support such efforts. For example, Niigata prefecture introduces Otagaisama BC as an application of firms' Business Continuity Plan (BCP or BC plan). It aims for firms not to cause serious supply chain disruptions even in the case of disasters by being interconnected with peer firms in geographically distant areas. The interconnected firms outside the devastated area are to provide series of emergency support including substitution of the business of the sufferer in case of major catastrophes. The impact analysis is beyond this thesis since they are so new and limited in number that it is unsuitable to test empirically.

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