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## Peer Effects in Employment Status: Evidence from Housing Lotteries for Forced Evacuees in Fukushima

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**Peer Effects in Employment Status: Evidence from Housing Lotteries for  
Forced Evacuees in Fukushima\***

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**Abstract**

Does a high peer employment rate increase individual employment probability? We exploit the random assignment of temporary housing to evacuees from the Fukushima Daiichi nuclear power plant accident to identify the effect of neighbors' employment rates on an individual's probability of finding a job post-evacuation. Using unique survey data, we find that a one standard deviation increase in the initial employment rate of an individual's peers makes the hazard of restarting work 1.41 times larger during the six months after move-in. We also show suggestive evidence for social norm to work as an underlying mechanism for the observed peer effect.

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## I. Introduction

Does a high employment rate among an individual's neighbors increase that individual's employment probability? Previous studies have shown that neighborhood quality is strongly correlated with an individual's labor market outcomes (Borjas, 1995; Cutler and Glaeser, 1997; Weinberg, Reagan, and Yankow, 2004). However, well-known problems such as self-sorting of peers, common shocks, and the reflection problem (Manski 1993, 2000) make it difficult to identify a causal effect of the peer employment rate. Although a few recent studies attempt to solve these problems using an instrumental variable for peer employment status (Maurin and Moschion 2009) or controls for various fixed effects to absorb self-sorting of neighbors (Bayer, Ross and Topa 2008), experimental evidence from randomly assigned peers is still scarce in the context of peer effects in employment, unlike peer effects in student outcomes or workplace productivity.<sup>1</sup>

We exploit the random assignment of temporary housing to evacuees from the Fukushima Daiichi nuclear power plant accident to identify the effect of the neighbors' employment rate on the probability of finding a job after evacuation. After the accident caused by the Great East Japan Earthquake and the subsequent tsunami, people living within a 30-km radius of the plant were forced to evacuate to other municipalities. Several months later, many of these evacuees were moved from emergency shelters to publicly provided temporary housing units allocated by government lottery. This situation provides a rare opportunity to examine the causal effect of the peer employment rate using randomly assigned neighbors.

We use unique survey data collected from 14 temporary housing clusters 2.5 years after the

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<sup>1</sup> Random assignment of classmates and college roommates is widely used in the literature of peer effects on student behaviors; Sacerdote (2014) provides a comprehensive survey. Moreover, there is a growing body of literature to utilize random assignment of peers to examine peer effects on other outcomes such as worker productivity (Mas and Moretti 2009, Bandiera, Brankay, and Rasul 2010) and crime (Bayer, Hjalmarsson, and Pozen 2009).

accident. Among the 587 sample individuals, aged 20–69 years, 479 were not employed as of the end of March 2011, the month when the power plant accident occurred. We use a sample of these evacuees to estimate the effect of the ratio of neighbors who had already been working before moving to temporary housing on the length of time until the unemployed evacuees restarted work. We find a significantly positive peer effect that is robust to various controls for individual characteristics and housing cluster fixed effects. The effect is not only statistically significant but also economically substantial: a one standard deviation (16 percentage points) increase in the initial employment rate of an individual's peers makes the hazard of restarting work 1.41 times larger during the first six months after housing move-in.

This paper relates to the growing literature on neighborhood effects for people who are exogenously assigned to a new neighborhood. Previous studies suggest that the significance of peer effects may depend on the degree to which the randomly assigned residents can maintain their sense of community in the new neighborhood. Social experiments that relocate randomly picked residents in poor neighborhoods to neighborhoods with lower poverty rates find significant effects on various socioeconomic outcomes of children who have long been exposed to the new neighborhood (Chetty et al. 2016), but no improvement on the outcomes of adults (Kling, Liebman, and Katz, 2007; Barnhardt et al., 2015).<sup>2</sup> The adult participants rather feel isolated from the new neighbors (Barnhardt et al. 2015). Damm (2014) finds that the labor market outcomes of refugee immigrants to Denmark, whose residential location was quasi-randomly assigned, are not affected by the overall employment rate and the overall average skill level in the neighborhood. However, their employment probability is affected by the average skill level of non-Western immigrants living in the neighborhood. Studies on ethnic enclaves for refugee immigrants (Edin et al., 2003; Damm, 2009)

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<sup>2</sup> In the setting of refugee immigrants, Gould et al. (2011) also study the random assignment of residences to Yemenite immigrants in Israel and find a significant long-term impact of childhood living environment on many socioeconomic outcomes in adulthood, such as educational attainment.

also suggest that neighbors who share the same ethnic background affect the immigrants' labor market performances. Our findings of positive peer effects among evacuees strengthen this argument since all residents of a temporary housing cluster are from the same municipality.

An important difference between our study and these existing studies is that the latter examine how local neighbors affect the new migrants, whereas we focus on the peer effect between new migrants.<sup>3</sup> It is frequently observed that new communities are formed exclusively by new migrants, e.g., refugee camps, temporary housing for natural disasters, and school dormitories. Nonetheless, empirical evidence regarding the peer effect in employment among such new migrants is scarce, compared to studies on the influence of local neighbors on new migrants.

This distinction in the setting could also affect the mechanism of peer effects. In the context of the effect of local neighbors on new migrants, the social norm to work, one of the potential drivers of peer effects, may not be important because the neighbors know that it is difficult for the new migrants, who are unfamiliar with the local labor market, to find a job. Rather, they may feel emotional sympathy for the migrants.<sup>4</sup> In contrast, all the evacuees in our setting are on an equal ground in the sense that they were forced to move to Iwaki in March 2011. Thus, the unemployed evacuees may feel pressure from their peers who have already found a job. Consistent to this prediction, we show suggestive evidence supporting the social norm channel, although we do not

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<sup>3</sup> Considering that we examine the peer effects among people in the same community, this study also relates to the literature of peer effects in employment among previous co-workers (Laschever, 2009; Brown and Laschever, 2012; Cingano and Rosolia, 2012) and friends (Cappellari and Tatssiramos, 2015). Laschever (2009), whose study is the closest to ours, also examines the peer effect in employment among randomly assigned peers. He shows a significant peer effect in employment among those who were in the same company during World War I.

<sup>4</sup> Damm (2009) finds evidence that is consistent with the peer effects through the information sharing channel but contrary to the social norm channel.

rule out the information-sharing channel.

This study also contributes to the post-disaster rehabilitation policy literature. Disasters increase the unemployment rate (Groen and Polivka 2008), and this is a critical concern among policymakers (US Bureau of Labor Statistics 2006). However, few empirical studies suggest how employment can be restored. Our findings suggest that social interactions with employed individuals could significantly facilitate reemployment.

The remainder of this paper is organized as follows: Section II introduces the detailed process of housing lotteries and explains how we define the peer group. Section III describes the data and summary statistics. The empirical strategy to estimate peer effects is presented in Section IV, and the results in Section V. Section VI further discusses the underlying mechanisms for the observed peer effect and shows suggestive evidence for the social norm to work. Section VII provides our concluding remarks.

## **II. Background**

### **A. Housing lotteries for the forced evacuees in Fukushima**

The Fukushima Daiichi nuclear power plant accident—caused on March 11, 2011, by the Great East Japan Earthquake and a subsequent tsunami—forced over 100,000 citizens to evacuate. Citizens of the municipalities within a 30-km radius of the power plant were ordered to evacuate to outside areas; most of them headed to large cities in Fukushima, such as Iwaki, Aizu, and Koriyama.

Our study site is Iwaki city, which is 30–60 km from the Fukushima Daiichi nuclear power plant. Iwaki is one of the municipalities that accepted a large number of forced evacuees. While the city hosts 36 clusters of publicly provided temporary housing for 3,500 households, housing for the tsunami-affected Iwaki citizens amounts to only around 180 units. The rest are for forced evacuees from six radiation-affected municipalities: Futaba, Okuma, Tomioka, Naraha, Hirono, and

Kawauchi.<sup>5</sup>

In Iwaki city, the provision of temporary housing was slow and gradual due to the vast amount of required housing and limited land availability. Although the occupancy of the first cluster was completed in May 2011, some clusters were still under construction even in the summer of 2012. While awaiting the construction of temporary housing, the evacuees had to stay at emergency shelters such as schools and public halls. Given this delay in housing provision, some of the evacuees started to work before moving into the temporary housing.

The allocation procedure for temporary housing for the forced evacuees has two important features that are relevant to the identification of peer effects in employment. First, each municipality held the right-to-use of a few clusters in Iwaki. Hence, all residents of a housing cluster were from the same municipality. Second, the municipal governments used a lottery to allocate housing to its citizens. When the construction of a housing cluster was completed, evacuees from the municipality that held the right-to-use could apply for that housing. The location of housing within the cluster was also randomly allocated through the lottery. Those selected through the lottery could move in, and most of them did so within a few months. Those not selected had to wait for the next lottery opportunity.

This set of circumstances provides us with a natural experimental setting to identify the peer effect in employment status. Since residents of clusters are randomly selected, the employment status of neighbors at the time of move-in to their cluster is exogenously given. Furthermore, the neighbors are originally from the same municipality. This helps ensure their sense of community, and therefore

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<sup>5</sup> While the evacuation orders for Kawauchi and Hirono were lifted in January and March 2012, respectively, most evacuees were still in their temporary homes as of September 2013, when our data were collected. Only around 20 percent of citizens had returned to Hirono, and most of these were elderly. Regarding Kawauchi, no official statistics on the rate of return until June 2014 were available.

promotes active social interactions among the residents. By leveraging these circumstances, we can estimate the impact of the employment rate among peers at the time of move-in to the cluster on the probability that the initially unemployed evacuees restart work.

Official statistics show that it was relatively easy for the evacuees to find a job in Iwaki city. Although the devastating earthquake and tsunami destroyed many jobs, reconstruction generated large labor demand. Figure 1 compares the active job openings to applicants ratio reported by public job-placement agencies in the Iwaki area with the national average. The difference between the national average and Iwaki before the earthquake and power plant accident was negligible. After the quake, the ratio for Iwaki started to rise faster than the national average. It peaked at the end of 2012, and stayed significantly higher than the national average in subsequent years. While the number of job openings increased in most industries, the construction industry experienced by far the largest increase in labor demand. The number of new job openings in construction rose 2.5-fold from 2010 to 2011,<sup>6</sup> and remained unchanged in 2012. In particular, a large number of workers were needed to clean up the radiation-affected areas, which does not require a high level of skill. Among other industries, the number of new job openings rose 1.8-fold in manufacturing and 1.6-fold in the medical and welfare industries. Furthermore, the government subsidized the firms that employed those from the disaster-affected areas.

[Insert Figure 1 about here]

## **B. Definition and measurement of peers' employment**

We define each individual's "peer group" as his or her neighbors living in the same block (subcluster), except his or her own family members living in the same house. On average, each housing cluster is divided into 3.8 blocks, with around 34 households per block. As an example,

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<sup>6</sup> Number of job openings posted to Hello Work Taira, the largest public job-placement agency in Iwaki city. Retrieved from Fukushima Labour Bureau (2011, 2012, 2013).



Figure 2 shows a map of the Rinjo cluster, one of the clusters in our sample, with 106 housing units divided into 4 blocks (A-D). To give a specific image of the peer group, let us use a person living in housing unit A1-1. The peer group for this person is defined as those living in the 40 housing units in block A, except his or her own family members living in A1-1.

[Insert Figure 2 about here]

We define the peer group at the block level, because neighbors in the same block seem to be a good approximation of the range of people with whom an individual communicates regularly. First, the residents of the same block often have to work together for the maintenance of public space and facilities in their cluster. Each cleaning task is assigned to a group of residents from a certain block; e.g., Block A is in charge of the main entrance of the cluster, and Block B the meeting room. Second, the randomized allocation of housing completely disassociated local communities that existed before the power plant accident, and thus the evacuees knew few people in the cluster when they moved in (Shoji and Akaike 2014). Therefore, it is reasonable to presume that they initially communicated with only a few adjacent households. Third, some blocks are geographically isolated from the others even within the same cluster. For example, Block A in Figure 2 is separated from the other blocks by a ditch running through the cluster. Fourth, the opportunities to get to know the evacuees in the other blocks are limited for working-age adults because they seldom participate in social events held by the municipality and non-governmental organizations to encourage communication among the evacuees in the same cluster beyond blocks. Finally, there is also anecdotal evidence based on the authors' field interviews; some evacuees were unaware for a long period of time that their friends had also moved into the same cluster, because they were allocated to different blocks.

Furthermore, by defining peer groups at the block level, we can control for cluster characteristics, such as time of construction, geographic features, and which municipality has a right-to-use. These factors might cause spurious correlation between the peer employment rate and the individual's own probability of taking a job after moving into the housing. For example, the

residents of a cluster constructed later would be more likely to restart work *before* moving in or take a job soon after. By examining the block-level employment rate, controlling for cluster fixed effects, we can identify the causal effect of the neighbors' employment rate on the probability of restarting work, given the cluster characteristics are constant.

One might be still concerned about the use of neighbors at the block level. First, individuals living near the border of a block may not as frequently communicate with neighbors in their own block as those living inside the block. Therefore, the peer effect may be smaller for them than for other individuals. Nevertheless, we will show, in Appendix A1, that the magnitude of peer effects from block members do not vary with the location of the housing unit in the block, supporting the use of block as the unit of peer group. Second, in theory, a peer group could also be defined as a group of individuals with similar demographic characteristics. However, the employment rate of peers defined in this way is not independent of an individual's own characteristics, because demographic characteristics are correlated with both the individual's own and the peers' employment rates. Furthermore, it is not feasible to limit the range of peers to the same demographic groups and run a separate regression for each group, because the sample size is too small.

Given these arguments, we define the initial employment rate of individual  $i$ 's peers,  $peer_i$ , as the ratio of peers who had restarted work by the month they moved into the temporary housing among all peers living in the same block at the time of the survey. Specifically, for each respondent aged 20–69 years, we generate a dummy indicator that takes the value of 1 if he or she had already restarted work before moving into temporary housing (i.e., the month of move-in is later than the month of restarting work). Then, we take the average of this dummy indicator for all respondents in the same block except members of the respondent's own household.

Note that the employment status of each member of the peer group is measured at the time this particular member, and not when individual  $i$ , moved in. We define  $peer_i$  in this way for two reasons.

First, it is independent of the individual's own characteristics. Strictly speaking, the people who should actually matter to an individual are the neighbors who were around at the time the individual moved in, not everyone who lived in the same block as of September 2013. Additionally, the peers' employment status at the time this individual moved in would be more relevant than the status at the time each person in the peer group moved in. However, if the time each individual moved in were used, the peers' employment rate would correlate with an individual's own characteristics, as the peers' employment rate increases over time. That is, people who moved in later tend to face a higher peer employment rate. Therefore, we use the exogenous measure of peers' initial employment status.

Second, as shown later, about three quarters of our sample moved into temporary housing within two months after completion of construction. Therefore, in practice, the difference between these two measures of peer employment is very small. As discussed in detail in Appendix A2, we limit our sample to those who moved in within two months after the completion of construction, as a robustness check, and find that results do not change qualitatively.

### **III. Data**

#### **A. Survey design and sample selection**

In September 2013, we conducted a unique household survey of residents of the temporary housing clusters in Iwaki city.<sup>7</sup> The survey households were selected using the stratified random sampling method. In the first stage, we non-randomly selected 14 of 36 clusters based on cluster size and location and the municipality holding a right-to-use. We excluded clusters of evacuees from tsunami-affected areas in Iwaki from our sample because their assignment to temporary housing was

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<sup>7</sup> As of September 2013, the occupancy rate of temporary housing in Iwaki was as high as 95.8 percent (a press release by Fukushima prefecture

[http://www.pref.fukushima.lg.jp/uploaded/life/174887\\_383752\\_misc.pdf](http://www.pref.fukushima.lg.jp/uploaded/life/174887_383752_misc.pdf)). Thus, the potential bias caused by endogenous attrition from temporary housing is expected to be minimal, if at all.

not based on the lottery. In the second step, approximately 50 percent of the housing was randomly selected in each cluster (a total of 784 of 1,733 housing units). In the third step, vacant housing units were replaced with a neighbor, if available. Ultimately, we visited 701 households and completed a survey with 518 of them.<sup>8</sup> Table 1 presents a breakdown of the municipalities of the sample households (Column (3)) among the total evacuee households in Iwaki (Column (4)); as one can see, our sample is not biased to any particular municipalities.

[Insert Table 1 about here]

Table 2 lists the housing clusters we surveyed and the number of blocks in each. The Onigoe cluster is shared among three municipalities: Kawauchi, Hirono, and Iwaki. Since the housing lottery was conducted within each municipality that shares in the housing, we treated the block occupied by Kawauchi and the three blocks occupied by Hirono as separate clusters.

[Insert Table 2 about here]

Although we interviewed one person per household, we collected basic information, such as gender, age in the 10-year category, and employment status, for all individuals living in the same household. The number of all individuals included in the data is 1,117. Among them, we limited our sample to 587 individuals aged 20–69 years. Table 3 compares the demographic composition of these 587 individuals in our data with the population of the six municipalities where the evacuees lived before the power plant accident, based on the Population Census 2010. Our sample is older than the baseline population, mainly because households with young children were more likely to move out of the region permanently or choose other options such as leased housing financed by the government. Further, the individuals in our sample are less likely to have university degrees because older cohorts are on average less educated.

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<sup>8</sup> Correlation between the non-response rate at the block level and the ratio of individuals who already started to work at the time of move-in is very weak and statistically insignificant (correlation coefficient = -0.03, p-value= 0.85).

[Insert Table 3 about here]

Among the 587 evacuees aged 20–69 years, 108 were working as of the end of March 2011, the month when the accident occurred. We assume they continued working at the same job and exclude them from the analyses sample, although they are included in the computation of the peer employment rate at the time of move-in.

## **B. Variables and summary statistics**

Our main outcome variable is the hazard to restart work after the power plant accident. This variable is constructed from the month when the individual restarted working.<sup>9</sup> Figure 3 summarizes the Kaplan–Meier survival function for the sample of those who had not yet restarted work as of the end of March 2011 ( $N = 479$ ). As shown in the graph, men tend to restart work sooner than women do. About 51.6 percent of men and 24.3 percent of women restarted work by the time the survey was conducted in September 2013. We take into account this gender-based difference in the hazard function.

[Insert Figure 3 about here]

Table 4 shows the summary statistics by employment status as of the end of March 2011. The first column includes all individuals aged 20–69 years; this is the sample used to calculate the peer employment rate. The second column includes those who had not yet restarted work as of the end of March 2011; i.e., the sample used in the main analysis. For comparison, the third column includes those who continued to work.

[Insert Table 4 about here]

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<sup>9</sup> Unfortunately, the survey did not ask for much detail on the current economic situation, such as earnings, occupation, and other job information as of the survey date, because such questions can offend the evacuees who were forced to leave their homes and jobs. We do not know to what extent an evacuee’s current job is related to the job held before the accident, either.

Panel A of Table 4 shows that those who continued working are more likely to be men, aged 40–59 years, more educated, in larger households, and engage in construction and utility industries.<sup>10</sup> On the other hand, damage to the house and loss of family members is uncorrelated with employment right after the accident. Nonetheless, we control for these variables because the level of damage from the earthquake and tsunami may affect labor supply by way of the amount of financial support provided by the government. Additionally, it may have some psychological effect. Respondents' subjective well-being is used in the analyses of underlying mechanisms in Section VI, Subsection C.

Panel B summarizes the evacuation process. On average, evacuees moved into temporary housing 9.5 months after the accident, with no difference between those who continued working and those who did not. Further, 74 percent of our sample moved into temporary housing within two months after completion of construction; thus, the average employment rate when each person in the peer group moved in should serve as a good proxy for the average employment status of the neighbors when the individual moved in.

Panel C summarizes the individual's own employment status and the peers' employment rate. At the time of move in, about 33 percent of all evacuees aged 20–69 years had already restarted work. Among those who did not continue working right after the accident, 17 percent restarted work. As already shown in Figure 3, men are more likely to restart work than women. By the time of the survey in September 2013, 48 percent of all evacuees and 36 percent of those who did not work at the end of March 2011 had restarted work.

As explained in Section I, Subsection B, our key explanatory variable is  $peer_i$ , the fraction of peers who had already started to work before they moved in. The variable calculation is based on all

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<sup>10</sup> We combined utility with the construction industry because most of the workers in the utility industry in our sample are blue-collar workers doing maintenance and construction work. Both industries faced similar labor shortage for reconstruction work after the earthquake.

587 individuals. The average number of peers in our data is 15.6,<sup>11</sup> and the peers' average employment rate is 32 percent. As expected from the random assignment of peers, these variables are uncorrelated with the individual's own employment status. The standard deviation is 16 percent, and Figure 4 shows the kernel density.

[Insert Figure 4 about here]

#### IV. Empirical model to estimate the peer effects

To identify the causal effect of  $peer_i$ —the employment rate of neighbors at the time of move-in, as defined above—on the hazard of restarting work, we estimate the following Cox proportional hazard model:

$$(1)h(t|peer_i, sex_i, x_i) = \lambda(t; sex_i)\exp(X_i\beta + peer_iD_{it}^{bef}\gamma^{bef} + peer_iD_{it}^{0-5}\gamma^{0-5} + peer_iD_{it}^{6-11}\gamma^{6-11} + peer_iD_{it}^{1yr}\gamma^{1yr})$$

The hazard of restarting work,  $h(t|peer_i, sex_i, x_i)$ , is the probability density associated with individual  $i$  restarting work in the  $t$ -th month after March 2011, when the power plant accident occurred, conditional on not having resumed working since March 2011.<sup>12</sup>

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<sup>11</sup> Note that our data are based on a 50 percent random sample of the housing cluster residents, not a complete survey, and 32 percent of the survey households do not include any individuals aged 20–69 years. Thus, although the average number of housing units per block is 34, the average number of peers in our data is 15.6.

<sup>12</sup> Previous studies estimate the impact of peers' labor market outcome on the outcome of respondents at a certain period, such as the period of the household survey (Damm, 2009; 2014; Edin et al., 2003). Nonetheless, we conduct the hazard model to exploit the information on when the respondents restarted to work. Furthermore, since the duration between time of move-in and survey period shows large variation among respondents, regressing survey date employment status on  $peer_i$  would yield imprecise and hard-to-interpret estimation results.

We allow the effect of  $peer_i$  to vary with the number of months since the individual moved into the temporary housing cluster. Specifically, we interact with four dummy variables,  $D_{it}^{bef}$ ,  $D_{it}^{0-5}$ ,  $D_{it}^{6-11}$ , and  $D_{it}^{1yr}$ , which take 1 if month  $t$  falls before move-in for individual  $i$ , within 0–5 months after move-in, within 6–11 months after move-in, and more than 1 year after move-in, respectively. We expect that  $\gamma^{bef} = 0$  because the individual has not yet even met his or her prospective neighbors.<sup>13</sup> Therefore, we use this as a placebo test. If the peer effect exists,  $\gamma^{0-5}$  is expected to be positive.  $\gamma^{6-11}$  and  $\gamma^{1yr}$  can also be positive, but are expected to be smaller than  $\gamma^{0-5}$  for several reasons. First, if the peer effect is heterogeneous across individuals, those who are affected by peers find a job quickly and exit from the sample, especially when  $peer_i$  is high. Thus, those who remain in the sample after six months are likely to be irresponsive to the peer employment rate. Second, since  $peer_i$  is measured around move-in, the difference between  $peer_i$  and employment rate among peers in month  $t$  increases over time. Hence,  $peer_i$  becomes less relevant.

We also allow the baseline hazard,  $\lambda(t; sex_i)$ , to vary between the genders, because the survival functions shown in Figure 3 are quite different between men and women. The vector of control variables  $X_i$  in the exponential part includes dummies for 10-year age categories, dummies for educational background, household size, dummies for the level of housing loss, an indicator for any loss of household members due to the tsunami or earthquake, dummies for 0–5 months after move-in, 6–11 months after move-in, and more than 1 year after move in, and housing cluster fixed effects. Standard errors are clustered at the housing cluster level to allow any unobservable, time-variant common shock to apply to all households in the same housing cluster.

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<sup>13</sup> For the 85 individuals who already started working before moving into temporary housing,  $peer_i$  should have no effect. Nonetheless, we included them in the sample to avoid potential biases arising from left censoring of the sample. That is, if we limited our sample to those who did not start work before move-in and set the month of move-in as the starting point, the remaining sample would be non-randomly selected. We thank Daniel Hamermesh for pointing out this issue.



The variables included in X also serve as controls for financial compensation from TEPCO. While there are many categories of compensation, the two most relevant are the compensations for mental distress and forgone income, both introduced in August 2011.<sup>14</sup> The monthly compensation for mental distress is 100,000 yen per person, and it is supposed to be paid until the evacuation order is lifted. Thus, the expected time when this compensation is canceled depends on the resident municipality before the accident, which is controlled for through cluster fixed effects. The compensation for forgone income is determined by the income from the job lost due to the power plant accident.<sup>15</sup> Although we do not have a direct measure for the pre-accident income, we control for human capital variables such as age, education, gender, and the job before the accident. It is worth emphasizing that the amount of compensation would be uncorrelated with the peer employment rate even without these controls, while the amount of compensation might have directly affected the individual's labor supply.

As Manski (1993, 2000) points out, in general settings, it is difficult to estimate the causal effect of the behaviors of an individual's peers, because self-sorting or common shocks could generate spurious correlation between an individual's and his or her peers' behaviors. That is,

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<sup>14</sup> Before the introduction of the formal compensation scheme in August 2011, all forced evacuees received provisional compensations, which were lump sums of 1,000,000 yen per household (April 2011) and 300,000 yen per person (July 2011).

<sup>15</sup> Strictly speaking, the compensation for forgone income was initially defined as the difference between current income and income before the accident. However, this scheme was criticized for discouraging labor supply because it works as a 100 percent income tax. Given this criticism, the compensation scheme was revised in March 2012 so that income from jobs held after the accident is deducted from "current income." Thus, the compensation for forgone income might have had a negative substitution effect during the period August 2011–February 2012. However, such a substitution effect does not exist for other periods.

individuals in the same group tend to behave in the same way because they have similar individual characteristics or face similar conditions. We solve the problem of self-sorting by exploiting the random assignment of housing locations by lottery. Moreover, our estimates are not affected by common shocks because the employment status of peers at the time of move-in should be uncorrelated with any block-level shocks that would occur after move-in. Furthermore, thanks to the lottery, individuals currently living in the same block are unlikely to have experienced any common shocks before move-in because they were randomly chosen from different temporary shelters and did not know each other until moving into the housing. Finally, by using the employment status of peers determined before they moved in as the main explanatory variable, we can also avoid the problem of simultaneity, or the reflection problem.

The key assumption for our identification strategy is that the assignment of temporary housing is actually random. Random assignment of housing, with the characteristics of the housing clusters being constant, predicts that the peers' employment status should be independent of individual characteristics. To confirm this, we use the sample of those who were not working as of the end of March 2011 to regress the neighbors' employment rate on the individual characteristics and the housing cluster fixed effects. Table 5 shows the results. Columns (1) and (2) present estimates both without and with control for the job before the power plant accident and the gap between the completion of construction and move-in, respectively. None of the coefficients is statistically significant, and the joint tests of significance for age dummies, education dummies, housing loss dummies, and dummies for the job before the accident are insignificant. The insignificance of the timing of move-in implies that the random assignment was applied not only for those who moved in immediately after the completion of construction but also for those who came in later for some reason. These results reassure us of the randomness of the assignment of peers within housing clusters.

[Insert Table 5 about here]

## V. Results

### A. Baseline Result

Table 6 shows the estimated coefficients of the hazard model (1). Column (1) controls only for basic demographic background characteristics such as age, educational background, household size, dummies for housing loss and an indicator for the loss of any household members. In Column (2), we add the job before the power plant accident. The results are almost the same. As expected,  $peer_i$  does not have any effect on the hazard before move-in to the temporary housing. In contrast, there is a statistically significant positive peer effect in the first six months after move-in. After the first six months, the effect fades away.

[Insert Table 6 about here]

To interpret the estimated  $\gamma$ , recall that, when  $peer_i$  increases from  $a$  to  $a + b$ , the right-hand side of equation (1) becomes  $\exp(\gamma b)$  times larger. Given that the standard deviation of  $peer_i$  is 0.16, the estimated coefficient in Column (2) implies that a one standard deviation increase in  $peer_i$  would make the hazard of restarting work  $\exp(2.164 \times 0.16) = 1.41$  times larger during the first six months.

For the sake of comparison with existing studies, we convert this estimate into the effect of a one percentage point increase in peer employment rate on an individual's own employment rate as follows. The average hazard (probability of finding a job in each period) in the first six months after move-in is 1.9 percent, and a one percentage point increase in  $peer_i$  raises this to 1.9 percent  $\times \exp(2.164 \times 0.01) = 1.94$  percent. Thus, the change in the employment rate after the first six months since move-in should be  $(1-0.019)^6 - (1-0.0194)^6 = 0.0022$ . This means a one percentage point increase in the peer employment rate leads to 0.22 percentage point increase in an individual's own employment probability. This is smaller than the effect estimated by Maurin and Moschion (2009), who found that a one percentage point increase in a neighbor's labor market participation increased one's own labor market participation by about 0.6 percentage points, among French women aged 21–

35 years.

Although only a few of the other variables are statistically significant, the signs of the estimated coefficients are reasonable. Individuals older than 60 years of age are less likely to start working, probably reflecting the lack of employment opportunities available to them. The difference across educational backgrounds is negligible. Household size, the level of housing loss, and loss of household members seem to have negligible effects. Those who had no job before the accident (the reference group for job before the accident) are significantly less likely to restart working.

#### **B. “Endogenous” peer effect vs. “exogenous effect” from peer’s characteristics**

Manski (1993) decomposed the correlation between individuals and peer group into the following three effects: a correlated effect (a spurious correlation caused by self-sorting or common shocks), an endogenous effect (the causal effect of peer behavior on an individual’s behavior) and an exogenous effect, meaning that the individual’s behavior is influenced by pre-determined characteristics of peer group members, which also influence other members’ behaviors.

In our setting, random assignment of housing units rules out the correlated effect. However, it is possible that some pre-determined characteristics of peer group members can affect both the individual’s and the peer’s employment probability. In particular, work experience prior to the power plant accident may affect not only the person’s own employment probability after evacuation but also the neighbors’ employment probability through his or her networks outside the community.

Hence, we control for the peer’s work experience prior to the accident. Specifically, we focus on two variables: the proportion of peers whose job prior to the accident was in the construction or utility industry and the proportion of peers who were not employed even before the accident. First, individuals who used to work in the construction and utility sector were much more likely to continue working right after the accident, because of the high demand for reconstruction work. Therefore, they are likely to know better than others where evacuees can find jobs. In contrast, individuals who were not employed even before the accident are unlikely to start working, nor do

they have any useful job search information for neighbors.

Furthermore, we also control for peers' average years of schooling. Generally, more educated people tend to have broader networks. Thus, having neighbors with higher educational attainment may have a positive impact.

Table 7 shows the estimated effects of peers' employment rates and the effect of the three pre-determined variables of peers' characteristics, both interacted with the four period dummies. The effect of the peer employment rate is significantly positive in the first six months after move-in, which is qualitatively the same as the benchmark results shown in Table 6. Furthermore, the effects of added variables are mostly insignificant. These results imply that the observed effect of peer employment rate on individual's hazard to start working is the causal effect of peer behavior on an individual's behavior.

[Insert Table 7 about here]

### **C. Heterogeneous Effect**

Finally, we examine whether the peer effect is heterogeneous across age and educational background. In particular, people older than 60 years may behave differently, given that many people start to consider retirement after age 60. It is also policy relevant since about half of the adult evacuees living in temporary housing are older than age 60. Regarding educational background, as surveyed by Ioannides and Loury (2004), existing studies on network and referral in job searches show that high school graduates rely on networks in a neighborhood more than college graduates do.

Table 8 shows the estimated coefficients of the interaction terms between  $peer_i$  and dummy variables for being older than age 60 and for being vocational or university graduates. First, Columns (1) and (2) show that the peer effect has a statistically significant positive impact for those with a high school or lower education level. This result is consistent with previous studies on network and referral effects. However, note that the point estimate of the effect in the first six months for the more

educated group is also positive and half as large as that of the less-educated group. Given the smaller sample size of the more-educated group, we cannot conclude whether the peer effect exists for individuals with vocational or college education.

[Insert Table 8 about here]

Second, Columns (3) and (4) of Table 8 show that the peer effect has a statistically significant positive impact only for those younger than age 60. Although the interaction term between  $peer_i$  and dummy variables for being older than 60 years is not statistically significant, the point estimate indicates that  $peer_i$  has a negative effect on the job-restarting hazard even for those older than age 60.

## **VI. Social norm to work as a potential underlying mechanism**

There are three potential channels driving the peer effect in employment: the social norm to work, information sharing, and the referral effect. First, the social norm to work makes unemployed evacuees feel uncomfortable when others in the same block have a job. Such pressure may make more evacuees resume work. The second channel is information sharing. Since most of the forced evacuees were unfamiliar with job opportunities in Iwaki city when they moved into temporary housing, information from other evacuees who had already found jobs may play an important role in their job search. Third, given the high vacancy ratio, employers may search for workers through referrals of incumbents. That is, an evacuee who already restarted work may inform his or her neighbors of job vacancies.

It is difficult to disentangle all of the three mechanisms rigorously, given the lack of information about evacuees' current jobs.<sup>16</sup> However, as suggestive evidence, we can test whether

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<sup>16</sup> Information sharing is expected to improve match quality, whereas social pressure can lower the reservation wages. Thus, if we were able to estimate the effect of  $peer_i$  on the current earnings, we could test information sharing. Moreover, if we knew whether the respondents tend to start working

social norms play an important role, by examining the impact of the peer employment rate on the subjective well-being of the initially unemployed evacuees. The idea is as follows: If the social norm to work drives the peer effects, the employment rate among an individual's peers will have a negative effect on their subjective well-being for two reasons. First, those who have not yet started to work feel more uncomfortable not working when more neighbors are working. Second, even those who could find a job after the move-in may also feel unhappier if social pressure has made them take a job with unfavorable conditions. On the contrary, information sharing or the referral effect will not produce such a negative effect of the peer employment rate on subjective well-being. If there is any effect, the peer's employment rate should have a positive effect because more information gained through the employed peers enables the unemployed to find a better job.

We estimate an ordered probit model in which the dependent variable is subjective well-being compared to other evacuees and the explanatory variables are  $peer_i$  and other control variables included in the hazard model. Respondents' subjective well-being is elicited by asking the question "Do you think happier than the other evacuees in the same cluster?"<sup>17</sup> The possible answers range from 1 (strongly disagree) to 7 (strongly agree). Note that this variable is available only for respondents within the survey household. Columns (1) and (2) of Table 9 show that  $peer_i$  has a

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in the same occupation as their peers, we could test the referral effect. Unfortunately, as explained in footnote 8, we were not able to ask these questions.

<sup>17</sup> Unlike the standard questionnaire design used in the literature, we employ relative happiness compared with other evacuees in the same cluster for three reasons. First, we believe our question clearly captures the effects of disparity in employment status within a cluster. Second, if we employ the standard question, we can easily expect that most, if not all, respondents would select the lowest score, given their current socioeconomic and emotional situation. Therefore, we cannot observe enough variation. Finally, we believe that it is against research ethics to ask the respondents—who have obviously experienced one of the worst hardships in their lives—to what extent they feel happy.

statistically significant negative coefficient, implying that a higher peer employment rate makes the initially unemployed evacuees unhappier, as expected, if the social norm to work drives the peer effect.

However, the negative effect on happiness can also be explained by loneliness or isolation; the unemployed evacuee finds stronger group identity with other unemployed evacuees (as compared to other employed evacuees), and he or she feels lonelier and more isolated when surrounded by more employed evacuees in the same neighborhood. This sense of loneliness and isolation may well make him or her more likely to express relative unhappiness. To test this alternative interpretation, we examine the impact of peers' employment rate on two measures of loneliness and isolation: number of evacuees to have conversation in the cluster and the indicator of trust toward other evacuees in the cluster.<sup>18</sup> The results are presented at Columns (3) to (6) of the table. The estimated effects on the measures are statistically insignificant for all columns, contrary to the supposed effects of loneliness/isolation.

[Insert Table 9 about here]

Our finding that the social norm to work is an important driver of the peer effect is consistent with field interviews conducted by the authors with evacuees, who accused other evacuees of not working. It is also in line with Stutzer and Lalive's (2004) findings that the social norm to work in a geographically defined local community shortens the duration of unemployment. In addition, evacuees can find a job relatively easily, so long as they want, thanks to high labor demand, as explained in Subsection IIA. Indeed, most evacuees did not have prolonged job searches: among 174

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<sup>18</sup> The number of neighbors to have conversation is measured as (1) none, (2) 1–3 people, (3) 4–5, (4) 6–10, (5) 11–15, (6) 16–20, and (7) 21–. Trust is based on the subjective information elicited by the following question: Generally, would you say that (1) most people in the cluster can be trusted, (2) you cannot be too careful, or (3) no idea. The indicator of trust takes unity if the answer is (1), and zero otherwise.



individuals who restarted work between April 2011 and September 2013, 149 did so within three months after they started to search for a job. These circumstances could potentially strengthen the peer effect driven by peer pressure against those not working.

## **VII. Conclusion**

In this study, we exploit the random assignment of temporary housing for evacuees from the Fukushima Daiichi nuclear power plant accident to identify the effect of their neighbors' employment rate on their probability of finding a job after the evacuation. While controlling for housing cluster fixed effects, the assignment of blocks within each housing cluster is found to be completely random, and this enables us to identify the causal effect of neighbors' employment status on each resident's probability of finding a job for him or herself. We find a significantly positive peer effect that is robust to controls for various individual-level characteristics and different sample restrictions. Not only is the effect statistically significant, but its size is substantial: a one standard deviation increase in the peer employment rate increases the hazard of restarting work by 1.41 times in the first six months after move-in. In addition, we find evidence consistent with the hypothesis that the observed peer effect is caused by the social norm that everyone should work.

Natural disasters and civil conflicts often force people in the affected area to migrate and form new communities. Our results imply that the members of the new community to which the new migrant was assigned can substantially affect his or her economic outcomes. Evacuees and refugees are influenced more by their peer evacuees or refugees than local neighbors, who are often economically more advantaged.

Our findings also provide some useful insights into post-disaster rehabilitation policy. First, the significantly positive peer effect suggests that social interactions with employed individuals can significantly facilitate reemployment. In particular, segregating unemployed evacuees could impede recovery. Second, ignoring the spillover effect might lead to under-evaluation of the effectiveness of

policies to promote employment of evacuees. Third, even if each cluster of evacuees is ex ante homogenous, peer effects may generate persistent disparities across clusters by amplifying random shocks to each cluster. Fourth, the fact that social norms play an important role implies that fostering social capital may enhance resilience against natural disasters.

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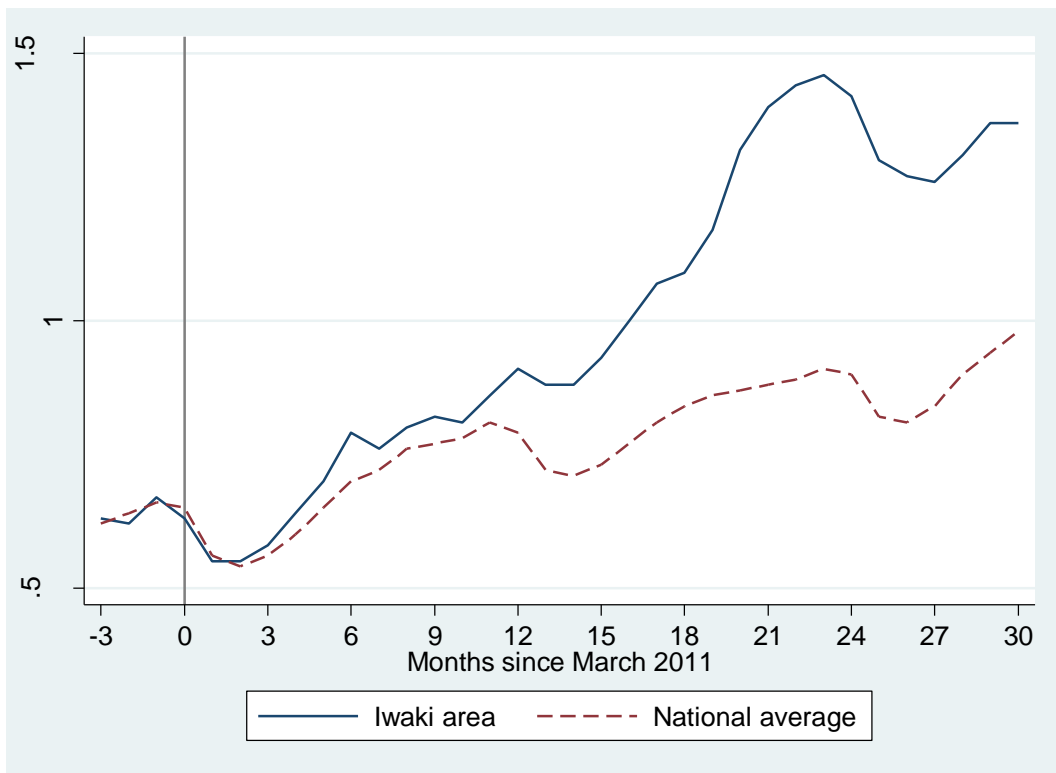


Figure 1 Active job openings to applicants ratio, December 2010–September 2013

#### Sources

Iwaki area: Monthly press releases by Fukushima Labour Bureau available at [http://fukushima-roudoukyoku.jsite.mhlw.go.jp/jirei\\_toukei/koyou\\_toukei/koyou\\_situgyou.html](http://fukushima-roudoukyoku.jsite.mhlw.go.jp/jirei_toukei/koyou_toukei/koyou_situgyou.html) (accessed 8/28/2015). Sum of three job-placement offices in Iwaki city.

National average: Monthly reports of the Employment Service Agency (*shokugyo antei gyomu tokei*), Ministry of Health, Labour and Welfare.



Figure 2 Example of a housing cluster map (Rinjo cluster)

Note: Photo taken by the author

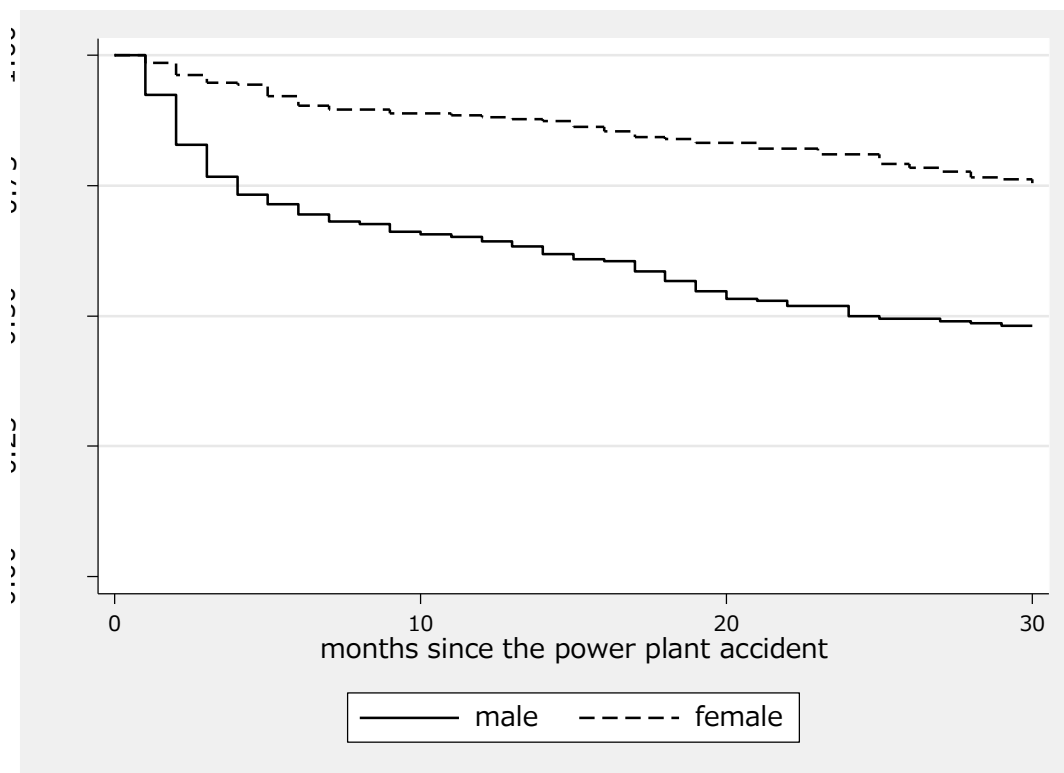


Figure 3 Kaplan–Meier survival estimates

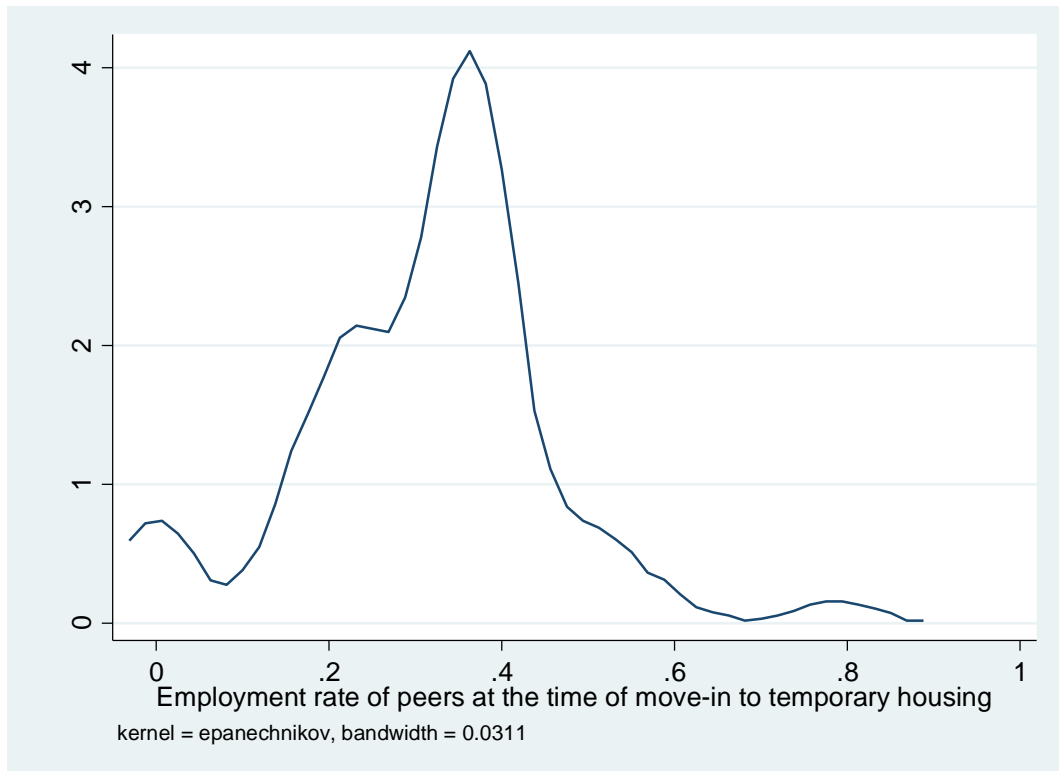


Figure 4 Kernel density of peer's employment rate at the time of move-in



Table 1: Breakdown of survey households

Municipality	Sample size	Fraction in the sample (percentage)	Fraction in the total number of evacuees in Iwaki city (percentage)
Futaba	30	5.8	7.4
Okuma	70	13.5	18.7
Tomioka	100	19.3	12.9
Naraha	185	35.8	33.8
Kawauchi	15	2.9	1.5
Hirono	115	22.2	21.3
Iwaki*	--	--	4.4
Others	2	0.4	
Missing	1	0.2	
Total	518		

\* We exclude evacuees from tsunami-affected areas in Iwaki from our sample because their assignment to temporary housing was not based on the lottery.

Table 2: Surveyed housing clusters and number of blocks in each cluster

Name of housing cluster	Completion	Municipality of origin	Number of blocks
Minamidai	8/2011	Futaba	8
Shimoyada	11/2011	Okuma	4
Kamikajiro	5/2012	Okuma	3
Izumitamatsuyu	9/2011	Tomioka	4
Kamiyoshima	6/2011	Tomioka	1
Rinjo	7/2012	Naraha	4
Uchigoshiramizu	10/2011	Naraha	1
Takaku10	7/2011	Naraha	9
Takaku5	6/2011	Naraha	1
Takaku 9	7/2011	Naraha	10
Onigoe*	10/2011	Kawauchi	1
		Hirono	3
		(Iwaki)	(1)
Takaku 2	6/2011	Hirono	1
Takaku 3	6/2011	Hirono	1
Takaku 4	6/2011	Hirono	1

Note: The Kawauchi, Hirono, and Iwaki municipalities share the Onigoe cluster. Since the lottery was conducted within each municipality, in the analysis we treated blocks occupied by different municipalities as different clusters. The block occupied by Iwaki citizens is excluded from our analysis sample.

Table 3: Comparison between our sample and the Population Census before the accident

	Our sample: all individuals aged 20–69 years as of September 2013 (N= 587) (shown in percentages)	Population Census: all individuals aged 20–69 years living in the six municipalities prior to the Great Japan Earthquake (shown in percentages)
Age		
20–29 years	10	16
30–39 years	13	20
40–49 years	16	19
50–59 years	22	25
60–69 years	39	21
Education		
Junior high school	19	14
High school	60	61
Vocational/Jr college	14	12
University	6	12

Note: Population Census was conducted by the Statistic Bureau of Japan in October 2010. The numbers shown in the table are the averages for the six municipalities where the respondents of our original survey resided before the Great Japan Earthquake.

Table 4: Summary statistics

	All individuals aged from 20– 69 years (N= 587)		Employment status at the end of March 2011				
	Mean (1)	S.D. (2)	Mean (3)	S.D. (4)	Mean (5)	S.D. (6)	
Panel A: Individual characteristics							
1 if male	0.51		0.44		0.83		***
Age (years) as of September 2013							***
20–29 (reference)	0.10		0.10		0.08		
30–39	0.13		0.12		0.16		
40–49	0.16		0.14		0.25		
50–59	0.22		0.20		0.31		
60–69	0.39		0.43		0.19		
Education							***
Junior high school (reference)	0.19		0.20		0.13		
High school	0.60		0.62		0.55		
Vocational/Jr college	0.14		0.14		0.18		
University	0.06		0.05		0.15		
Household size	2.84	1.27	2.76	0.61	3.20	0.98	**
Housing loss							
None (reference)	0.34		0.34		0.32		
Partial	0.40		0.40		0.40		
Half	0.19		0.19		0.17		
Full	0.08		0.07		0.11		
1 if lost household member(s) by the tsunami	0.06		0.06		0.06		
Job before the accident							***
Not working	0.23		0.29		0.01		
Agriculture and fishery	0.05		0.05		0.02		
Construction/utility	0.23		0.18		0.44		
Manufacturing	0.11		0.11		0.11		
Sales and service	0.09		0.10		0.05		
Medical and nursing	0.07		0.07		0.08		
Others	0.22		0.20		0.29		
Respondents' subjective well-being <sup>#</sup>							
Unhappier than other evacuees (1–3)	0.15		0.15		0.10		

Neutral (4)	0.39		0.39		0.41	
Happier than other evacuees (5–7)	0.46		0.45		0.48	
Panel B: Evacuation process						
Period of move-in to temporary housing (months since accident)	9.45	5.69	9.41	5.60	9.67	6.09
The gap between construction completion and move-in						
0–2 months	0.74		0.75		0.69	
3–5 months	0.10		0.09		0.12	
6–9 month	0.06		0.05		0.08	
10–14 months	0.08		0.09		0.06	
15–29 months	0.02		0.02		0.04	
Panel C: Own and peers' employment status						
Already working at the time of moving into the temporary housing	0.33		0.17		1	***
Males	0.51		0.30		1	***
Females	0.14		0.08		1	***
Restarted work by September 2013	0.48		0.36		1	
Size of peer group in the same block	15.6	8.4	15.7	8.4	15.5	8.6
Percentage peers already started to work before they moved in ( $peer_i$ )	0.32	0.16	0.31	0.15	0.33	0.20

\* Sum of all individuals aged 20–69 living in the six municipalities (Futaba, Okuma, Tomioka, Naraha, Hirono, and Kawauchi) as of October 2010.

# Data from only survey respondents. Sample size of age from 20 to 69: 295, not working: 227, working: 68

Table 5: Test for exogeneity of neighbors' employment status

Dependent variable: Percentage of neighbors who had started working before moving into temporary housing

	(1)	(2)
Male	-0.009 [0.010]	-0.011 [0.007]
Age 30–39 years	-0.020 [0.034]	-0.024 [0.037]
Age 40–49 years	0.040 [0.032]	0.035 [0.032]
Age 50–59 years	-0.004 [0.027]	-0.009 [0.030]
Age 60–69 years	0.005 [0.027]	0.002 [0.028]
High school	-0.001 [0.015]	0.001 [0.015]
Vocational/Jr college	-0.011 [0.033]	-0.014 [0.034]
University	0.036 [0.048]	0.029 [0.043]
Household size	0.003 [0.004]	0.003 [0.004]
Housing loss: partial	-0.016 [0.012]	-0.016 [0.014]
Housing loss: half	0.001 [0.034]	0.000 [0.032]
Housing loss: full	-0.007 [0.035]	-0.008 [0.035]
Dummy for having lost household member(s) by the tsunami	-0.009 [0.031]	-0.012 [0.032]
Job before the accident: agriculture and fishery		0.008 [0.031]
Job before the accident: construction/utility		0.020 [0.029]
Job before the accident: manufacturing		0.024 [0.042]
Job before the accident: sales and service		-0.006 [0.018]

Job before the accident: medical		0.036
		[0.023]
Job before the accident: others		0.001
		[0.021]
Gap between construction completion and move-in		-0.001
		[0.002]
<hr/>		
P-values of F-test for joint significance		
Age dummies	0.28	0.30
Education dummies	0.46	0.21
Housing loss dummies	0.50	0.60
Job before the accident	--	0.11
<hr/>		
Observations	479	477
R-squared	0.231	0.236
<hr/>		

\*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Housing cluster fixed effects are included. Standard errors clustered at the housing cluster are in brackets.

Table 6: Effects on the hazard of restarting work

	(1)	(2)
Percentage of neighbors who had restarted working before moving into the temporary housing ( $peer_i$ )		
×before move-in	0.265 [1.608]	-0.308 [1.513]
×0–5 months after move-in	2.665** [1.041]	2.164** [0.870]
×6–11 months after move-in	0.799 [0.938]	0.648 [0.944]
×more than 1 year after move-in	0.458 [1.210]	0.485 [1.259]
Age 30–39 years	0.156 [0.325]	-0.179 [0.383]
Age 40–49 years	0.305 [0.364]	-0.064 [0.417]
Age 50–59 years	0.196 [0.318]	-0.154 [0.350]
Age 60–69 years	-0.832* [0.456]	-0.956* [0.488]
High school	-0.211 [0.147]	-0.143 [0.181]
Vocational/Jr college	-0.279 [0.280]	-0.242 [0.230]
University	0.047 [0.284]	0.343 [0.240]
Household size	0.076 [0.076]	0.067 [0.075]
Housing loss: partial	0.024 [0.191]	0.010 [0.176]
Housing loss: half	-0.369 [0.247]	-0.391* [0.223]
Housing loss: full	-0.004 [0.280]	0.070 [0.279]
Dummy for having lost household member(s) by the tsunami	-0.350 [0.402]	-0.330 [0.391]
Job before the accident: agriculture and fishery		1.973***



		[0.491]
Job before the accident: construction/utility		2.148***
		[0.384]
Job before the accident: manufacturing		2.139***
		[0.323]
Job before the accident: sales and service		1.952***
		[0.418]
Job before the accident: medical		1.848***
		[0.423]
Job before the accident: others		1.883***
		[0.353]
<hr/>		
Number of observations	10,942	10,906
Number of individuals	479	477
<hr/>		

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered at the housing cluster are in brackets. Both specifications include controls for housing cluster fixed effects and dummy for 0–5 months after move-in, 6–11 months after move-in, and more than 1 year after move-in.

Table 7 “Endogenous” peer effect vs. “exogenous effect” from peers’ characteristics

	(1)	(2)
Percentage of neighbors who had restarted working before moving into the temporary housing ( $peer_i$ )		
×before move-in	0.457 [1.548]	-0.782 [1.274]
×0–5 months after move-in	3.152*** [1.133]	1.805* [0.979]
×6–11 months after move-in	0.037 [1.671]	-0.736 [1.563]
×more than 1 year after move-in	0.407 [1.337]	-0.447 [1.393]
Neighbors’ average years of schooling		
×before move-in	0.293 [0.179]	0.350** [0.173]
×0–5 months after move-in	0.032 [0.233]	0.046 [0.231]
×6–11 months after move-in	-0.022 [0.347]	-0.040 [0.312]
×more than 1 year after move-in	0.161 [0.236]	0.126 [0.211]
Percentage of neighbors who were not employed before the accident		
×before move-in	-0.791 [1.531]	1.137 [1.447]
×0–5 months after move-in	-2.018* [1.042]	0.160 [1.089]
×6–11 months after move-in	1.893 [2.847]	3.509 [2.841]
×more than 1 year after move-in	-0.388 [1.430]	2.006 [1.724]
Percentage of neighbors whose job prior to the accident was construction/utility		
×before move-in	-0.070 [0.942]	0.051 [0.820]
×0–5 months after move-in	1.956 [2.018]	1.148 [1.759]

×6–11 months after move-in	1.027	0.530
	[1.698]	[1.545]
×more than 1 year after move-in	-0.160	-0.901
	[2.695]	[2.778]
Control for job before the accident	no	yes
Observations	10,942	10,906

Table 8: Heterogeneous effects

Coefficient of percentage of neighbors who had restarted working before moving into the temporary housing (peer <sub>i</sub> ) interacted with:	(1)	(2)	(3)	(4)
High school or less	0.980	0.563		
×before move-in	[1.435]	[1.254]		
High school or less	3.047***	2.778***		
×0–5 months after move-in	[0.989]	[0.878]		
High school or less	0.878	0.919		
×6–11 months after move-in	[1.382]	[1.325]		
High school or less	0.704	0.906		
×more than 1 year after move-in	[1.128]	[1.258]		
Vocational/Jr college/University	-1.516	-2.566		
×before move-in	[1.949]	[2.012]		
Vocational/Jr college/University	1.278	0.173		
×0–5 months after move-in	[1.705]	[1.481]		
Vocational/Jr college/University	0.258	-0.099		
×6–11 months after move-in	[1.279]	[1.451]		
Vocational/Jr college/University	-1.055	-1.410		
×more than 1 year after move-in	[2.151]	[2.126]		
Younger than age 60			1.185	0.640
×before move-in			[1.209]	[1.149]
Younger than age 60			3.759***	3.172***
×0–5 months after move-in			[0.907]	[0.726]
Younger than age 60			1.900**	1.628
×6–11 months after move-in			[0.958]	[1.098]
Younger than age 60			1.752	1.695
×more than 1 year after move-in			[1.336]	[1.427]
60 years or older			-1.604	-2.276
×before move-in			[2.314]	[2.327]
60 years or older			-1.160	-1.792
×0–5 months after move-in			[1.988]	[2.074]
60 years or older			-2.114	-2.367
×6–11 months after move-in			[1.899]	[1.965]
60 years or older			-1.809	-2.013
×more than 1 year after move-in			[1.946]	[2.029]
Control for job before the accident	no	yes	no	yes

Number of observations	10,942	10,906	10,942	10,906
Number of individuals	479	477	479	477

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\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered at the housing cluster are in brackets. Both specifications include the same explanatory variables as in Table 6.

Table 9: Test for social norms as mechanisms of peer effects

	Happiness compared to other evacuees		The number of evacuees to have conversation		Trust in the other evacuees	
	Ordered Probit		Ordered Probit		Probit	
	(1)	(2)	(3)	(4)	(5)	(6)
Percentage of neighbors who had restarted working before moving into the temporary housing ( $peer_i$ )	-0.727**	-0.785**	-0.095	-0.176	-0.295	-0.438
	[0.358]	[0.320]	[0.620]	[0.634]	[0.655]	[0.673]
Control for job before the accident	no	yes	no	yes	no	yes
Number of individuals	227	226	227	226	226	225

Note: Happiness is measured by a 1–7 scale. The number of evacuees to have conversation is measured by a 1–7 scale (1: none, 2: 1–3 people, 3: 4–5, 4: 6–10, 5: 11–15, 6: 16–20, 7: 21–). Trust measure is self-reported binary variable. The coefficients are reported for Columns (1) to (4) and marginal effect at the mean for (5) and (6). \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Standard errors clustered at the housing cluster are in brackets. Other explanatory variables are the same as those in Table 6 and female dummy.

## Appendix

### A1. Robustness checks for alternative definition of peer groups

As mentioned in the main text, we define an individual's peer group as neighbors living in the same blocks mainly because it is a good proxy for people with whom an individual actually communicates. Admittedly, however, individuals living near the border of a block may be less sensitive to the employment rate of block members than individuals living in the center of a block. Thus, in this appendix section, we test whether the magnitude of peer effects from block members is weaker for residents of housing adjacent to the border with the next block. Specifically, we add two control variables to Equation (1): an indicator of housing adjacent to the border with the next block and its interaction with  $peer_i$ .

Results are shown in Table A1. Columns (1) and (2) replicate our benchmark results presented in Table 6, and Columns (3) and (4) present the result of modified specification. The coefficient of  $peer_i \times 0-5$  months after move-in remains positive and statistically significant, while the coefficients of the interaction terms are insignificant, implying that the magnitude of peer effect does not vary with the location of housing in the block.

Note that it is difficult to define peer groups without using blocks. One important merit of using the block, as the definition of a peer group, is that it is free from arbitrariness because zoning of each block is predetermined. Since the housing clusters vary in their shape and size, and our survey is not a complete survey of each cluster, it is difficult to apply unified standards such as "20 households on the same street."

### A2. Robustness checks for different sample restrictions

Table A2 presents the results of robustness checks for different sample selections.

First, Columns (1) and (2) limit the sample to those who moved in within two months following the completion of the housing cluster. This reduces the sample size to about 74 percent of its original size and boosts the standard error, but the size of the estimated effect in the first six months becomes slightly larger. These findings reassure us that our results are not driven by people who moved in later.

Second, Columns (3) and (4) limit the sample to respondents. The motivation for this robustness check is that information on individuals other than respondents may contain more errors. Since the sample size is reduced to half, the standard errors are boosted and the estimated effect becomes statistically insignificant with controls for job before the accident, but the overall pattern remains the same.

Among the 480 individuals in the sample for our main analysis, 126 were not employed before the accident, and 94 percent of them were not employed as of the survey in September 2013. Thus, most of the people who were not employed before the accident would not work regardless of the peer employment rate. To check this, Columns (5) and (6) exclude individuals who were not employed before the power plant accident. The results do not change much. Note that we cannot run a separate regression for those who were not employed, since the transition to employment occurred only seven times among those who were not employed before the accident.



Table A1: Robustness checks for different definitions of peer group

(1) (2) People living in the same block, excluding own family members (replication of Table 6)

(3) (4) Heterogeneous peer effect from the block members is allowed between the residents of housing adjacent to the border with the next block and the other residents.

	(1)	(2)	(3)	(4)
Peer <sub>i</sub> ×before move-in	0.265 [1.608]	-0.308 [1.513]	-0.091 [1.512]	-0.385 [1.485]
Peer <sub>i</sub> ×0–5 months after move-in	2.665** [1.041]	2.164** [0.870]	2.378** [1.096]	2.176* [1.186]
Peer <sub>i</sub> ×6–11 months after move-in	0.799 [0.938]	0.648 [0.944]	-0.364 [1.203]	-0.272 [1.128]
Peer <sub>i</sub> ×more than 1 year after move-in	0.458 [1.210]	0.485 [1.259]	-0.314 [1.539]	0.045 [1.687]
Peer <sub>i</sub> ×before move-in × Adjacent to the block border			0.767 [1.078]	0.234 [1.098]
Peer <sub>i</sub> ×0–5 months after move-in × Adjacent to the block border			0.221 [0.626]	-0.408 [0.673]
Peer <sub>i</sub> ×6–11 months after move-in × Adjacent to the block border			1.830 [1.182]	1.049 [1.083]
Peer <sub>i</sub> ×more than 1 year after move in × Adjacent to the block border			1.099 [1.541]	0.060 [1.771]
Adjacent to the block border			-0.091 [1.512]	-0.385 [1.485]
Controls for job bef. Acc.	No	Yes	No	Yes
Number of observations	10,942	10,906	10,942	10,906
Number of individuals	479	477	479	477

\*\*\* p < 0.01, \*\* p < 0.05, \* p < 0.1. Standard errors clustered at the housing cluster are in brackets.

Other explanatory variables are the same as those in Table 6.

Table A2: Robustness checks for different sample restrictions

- (1) (2) Individuals who moved in within two months after completion of the housing cluster
- (3)(4) Respondents only
- (5) (6) Excluding individuals who were not employed before the power plant accident

	(1)	(2)	(3)	(4)	(5)	(6)
Peer <sub>i</sub> ×before move-in	0.099 [1.847]	-0.609 [1.716]	0.142 [2.069]	-1.314 [2.143]	0.044 [1.392]	-0.377 [1.393]
Peer <sub>i</sub> ×0–5 months after move-in	3.300*** [1.239]	2.513* [1.373]	2.610** [1.327]	1.567 [1.337]	2.412** [1.034]	2.003** [1.000]
Peer <sub>i</sub> ×6–11 months after move-in	0.816 [1.431]	0.415 [1.497]	-0.689 [2.455]	-1.43 [2.344]	0.811 [0.982]	0.613 [0.989]
Peer <sub>i</sub> ×more than 1 year after move-in	-0.512 [1.324]	-0.94 [1.307]	1.372 [2.008]	0.76 [1.907]	-0.126 [1.756]	-0.198 [1.674]
Controls for job bef. Acc.	No	Yes	No	Yes	No	Yes
Number of observations	8,395	8,359	6,403	6,373	7,214	7,178
Number of individuals	359	357	264	263	353	351

\*\*\* p < 0.01, \*\* p < 0.05, \* p < 0.1. Standard errors clustered at the housing cluster are in brackets.

Other explanatory variables are the same as those in Table 6.