Tsukuba Economics Working Papers
No. 2013-002

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April 2013

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April 28, 2013

Acknowledgements
I wish to thank my field team – Jonati Torocake, Viliame Manavure, Viliame Lomaloma, and 16 enumerators – for their advice, enthusiasm, and exceptional efforts on behalf of this project. Special thanks are owed to the Fijians of the region who so willingly participated in the survey. The Cakaudrove Provincial Office in Fiji offered valuable institutional support. This research has been made possible through support provided by the Sumitomo Foundation, the Japan Society for the Promotion of Science, and the Ministry of Education, Culture, Sports, Science and Technology in Japan. Any errors of interpretation are solely the author's responsibility.

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Abstract

Although international remittances are important insurance against natural disasters in developing countries, fraud is a pitfall of international labor migration. This paper addresses an unexplored question about the disaster-fraud nexus: Do natural disasters beget fraud victimization among the poor as they seek labor migration for coping? I exploit a natural experiment: Two years after a cyclone, a huge number of Fijian males were defrauded of application fees for labor migration to the Middle East in 2005. My household survey data, which by chance I collected before the fraudulence was noticed, are free from underreporting/misreporting out of embarrassment. Controlling for the endogeneity of household housing damage reveals that housing damage strongly increases individual member's job application that later turned out to be fraud victimization. Households resort to high-risk, high-return labor migration because their domestic coping options are constrained by their labor endowment.

Keywords: labor migration; fraud victimization; natural disaster; risk coping; poverty; Fiji

JEL classification: K42; O15; Q54

1. Introduction

International labor migration and remittances play important roles in many developing countries (e.g., Lucas and Stark 1985; Taylor 1999; Adams and John 2003; Hanson 2010). In particular, international remittances can play a significant insurance role as both an ex ante strategy (e.g., Rosenzweig and Stark 1989; Paulson 1994; Amuedo-Dorantes and Pozo 2006) and an ex post strategy (e.g., de la Briere et al. 2002; Gubert 2002; Halliday 2006; Yang and Choi 2007). This is especially so for major covariate shocks, such as natural disasters, because local risk sharing is weak or even nonexistent, because disaster shocks greatly reduce pooled resources that can be shared. Using country panel data, Yang (2008) shows that migrants' remittances strongly respond to hurricane damages in developing countries. Poor populations in rural developing areas are especially vulnerable to increasingly frequent and extreme weather events, as they rely heavily on weather-dependent livelihood activities, such as agriculture, livestock, and fishery (e.g., Adger et al. 2003; Pelling 2003; Eriksen and O'Brien 2007; Sawada 2007;

Strömberg 2007), and international remittances are expected to play an increasing role as insurance among them (Marchiori and Schumacher 2011 theoretically show that climate change increases international migration).

A pitfall in international labor migration, especially from developing countries, is fraud: Many labor migrants rely on recruitment agencies having information about and market power in foreign job markets, and their potential for fraud and abuse has been noted (World Bank 2006b). All that is known about recruitment agencies' fraud, however, is based on anecdotal evidence and limited case studies.[1] As far as I know, no survey on victimization of recruitment fraud – the scamming of people seeking, not performing, employment – exists.[2] More broadly, most extant crime and victim studies in developing areas (including those discussed below) focus on violent and property crimes, and the literature virtually ignores fraud and fraud victimization among poor populations (some Living Standard Measurement Surveys of the World Bank, for example, cover crime victimization, but not fraud victimization). This paper addresses an important question about the disaster-fraud nexus that has not yet explored in the literature: Do natural disasters beget fraud victimization among the poor as they seek labor migration to cope with disaster shocks? If so, private risk coping is a channel for fraud victimization among the poor, and through this channel, natural disasters exacerbate the pitfalls of international labor migration.

A major challenge in fraud studies is that victim data are potentially underreported and/or misreported out of embarrassment. Victims of white-collar crimes, as well as some other crimes such as sexual assault, are reluctant to report the incidents (Ennis 1967): Many victims "often are viewed with a mixture of skepticism, suspicion, and disbelief, and viewed as unworthy of

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[1] In their study of Sri Lankan labor migrants to the Middle East in the mid-1980s, Eelens and Speckmann (1990, p319) mention the prevalence of fraudulent recruitment agencies: "Almost weekly, serious cases of fraud by bogus agents are reported in the local newspapers. Frequently, these types of agents promise the prospective migrants foreign employment, collect the fees and they disappear. Cases are known in which a single fraudulent agent cheated several hundreds would-be migrants. Although the Sri Lanka Fraud Bureau has taken action against such malpractices, many poor people are victims of these unscrupulous individuals." Spaan (1994) studies the role of recruitment agencies in Indonesian international migration.

[2] The International Crime Victim Survey of the United Nations Center for International Crime Prevention, for example, covers only consumer fraud. In developed countries, recruitment fraud in out-migration has received little attention; for example, recruitment fraud is not listed as a common type of fraud in Levi and Burrows's (2008) extensive study of available fraud data in the United Kingdom.

society's protection" (Walsh and Schram 1980, pp. 46-47).[3] Distinct from conventional fraud-victim data gathered after the fraudulence is noticed by victims or the public, by chance I conducted a household survey before fraud victims and non-victims became aware of the fraudulence of a recruitment agency (discussed shortly), but after they decided whether or not they would apply for labor migration. In my survey, respondents never manipulate records to cover up (or exaggerate) victimization experiences before they know about them. Thus, I can directly examine people's job applications without relying on information about their victimization experiences, which their decisions later turned out to be.

Recruitment fraud is especially relevant in Pacific island states with small economies, which are often dependent on international labor migrations (Bertram 1986; World Bank 2006a).[4] In 2005, a private recruitment agency defrauded more than 20,000 Fijian males of application fees for labor migration to the Middle East. This large-scale recruitment fraud occurred two years after a tropical cyclone hit part of the country in January 2003, when the region was still in the reconstruction phase. The fraud per se was not necessarily related to the cyclone, because the fraud was prevalent across most of the country, including regions not hit by the cyclone. Exploiting this unique sequence as a natural experiment, I show that household disaster victimization increases individual members' job applications, i.e., ex post fraud victimization. Even though village-level covariate disaster shocks are exogenous to individual households, household-level disaster damage can be correlated with unobserved individual/household heterogeneity affecting job applications (as discussed below). Controlling for the potential endogeneity of household housing damage by using exogenous flood shock as an instrument reveals even stronger coping responses than those otherwise. I then explore why households seek coping through high-risk, high-return labor migration by examining how their coping responses are differentiated by their coping capacity. Only when damage endogeneity is controlled for, results indicate that households resort to labor migration because their domestic coping options are constrained by their labor endowment. Thus, the poor's limited coping capacity underlies the risk-coping channel for their post-disaster fraud victimization.

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[3] In a U.S. national survey conducted in 1991, Titus et al. (1995) find that only one third of self-reported consumer-fraud victims report their experiences to the authorities.

[4] In contrast with anthropologists' in-depth ethnographic studies, systematic economic works using micro-level survey data are scant in the Pacific region.

The disaster-fraud nexus is related to two lines of literature other than that addressing migration and remittances, as discussed above. First, economists extensively study how poverty and inequality can potentially cause crime, and a growing number of related works have been recently conducted in developing countries (e.g., Fajnzylber et al. 1998; Bourguignon 2000; Gaviria and Pages 2002). Identifying causality is a central challenge – poverty and crime are jointly determined (Ehrlich 1973) and related with unobserved factors. A possible identification strategy is to use shocks as exogenous factors. Miguel (2005) examines how the number of murders (of old women) in Tanzanian villages corresponds to rainfall shocks (which determine income) in his reduced-form analysis; Fafchamps and Minten (2006) examine how the number of crimes in communes in Madagascar responds to transitory poverty (head count ratio), using the fuel crisis as a natural experiment. Distinct from these studies based on village-/commune-level data, my analysis focuses on individual-/household-level factors and thus can capture heterogeneity according to micro-level coping capacity.[5]

Second, although criminologists have paid increasing attention to abuses and offences in the aftermath of natural disasters (e.g., Davila et al. 2005; Zahran et al. 2009; Harper and Frailing 2010; Brown 2012; White 2012), especially in the chaotic relief phase (the worst behaviors of humankind occur during the crisis, as discussed, for example, by Teh 2008 about the Indian Ocean tsunami), economic studies on post-disaster crime are very scarce. A growing number of economists examine various impacts of natural disasters, such as education (e.g., Yamauchi et al. 2009; Sacerdote 2012; Deuchert and Felfe 2013), labor (e.g., Sarmiento 2007; Belasen and Polachek 2009), and migration (e.g., Boustan et al. 2012), but not crime; in a related study, Leeson and Sobel (2008) show that disaster aid increases public corruption across U.S. states (Cavallo and Noy 2009; Kellenberg and Mobarak 2011 review macro impacts of natural disasters). As far as I know, this paper is the first economic study on post-disaster fraud. Fijians' fraud victimization through their coping responses in the reconstruction phase intimates that natural disasters can adversely affect crime and crime victimization in a broader and more persistent way than current criminology research suggests.

[5] Some economic works examine crime and conflicts as determinants of migration; using household-level data in Colombia, Rodrigues and Villa (2012), for example, show that exposure to kidnap risk increases international labor migrations (see also Engel and Ibáñez 2007).

The rest of the paper is organized as follows. Section 2 describes the data, the recruitment fraud, and the disaster. Section 3 presents the econometric specification to identify the effects of disaster damage on job applications that later turned out to be fraud victimization, which is followed by the estimation results in Section 4. Section 5 examines the heterogeneous damage effects by individual/household coping capacity. The last section concludes.

2. Data, fraud, and disaster

2.1. Fijian kinship and data

I start with a description of Fijian kinship, which shapes my survey's sampling design and underlies my empirical strategy. Each native Fijian belongs to a lineage of the *vanua-yavusa-mataqali-tokatoka* hierarchy: Vanua consists of several yavusa; yavusa consists of several mataqali; and mataqali consists of several tokatoka (Ravuvu 1983). Roughly matching an old district (*tikina*) in the administrative unit, vanua ranges over several villages (*koro*); a village consists of one or a few yavusa, which includes several lower-order units, mataqali, and then tokatoka. These kin groups are of central importance for village governance, ritual, and livelihoods (Turner 1992); in particular, rural land is communally owned by mataqali and is privately used; by law it cannot be sold (communal land consists of about 83% of the country's total land).

In June-September 2005, I conducted a household survey among 906 households in 43 native Fijian villages in Cakaudrove Province in the northern region of the country.[6] In each of 16 districts in the province, villages were intentionally chosen to cover distinct environmental and economic conditions. In each village, households were stratified by tokatoka and a combination of individual leadership status (e.g., kin leader) and major asset holdings (e.g., shops) (all tokatoka were sampled); in each stratum, households were randomly sampled (50% of the population in each stratum, on average). The 43 villages in the sample cover 20 vanua, 53 yavusa, 146 mataqali, and 234 tokatoka in total (an average village consists of 3.4 mataqali and

[6] The province, consisting of part of Vanua Levu, all of Taveuni, and other small islands, significantly lags behind the main island, Viti Levu, where the state capital, two international airports, and most tourism businesses are situated. Fiji is divided almost evenly between native Fijians and Indo-Fijians. My study focuses on native Fijians. The fraudulent recruitment agency discussed below is staffed by only native Fijians, and its recruitment drive in rural areas covered only native Fijian villages.

5.6 tokatoka, and on average, each mataqali and tokatoka consists of 14 and 8 households, respectively, in the population).[7]

At the time of interviews, the mean monthly total income earned by sample households is F$1,582, or F$288 per capita (F$1 = US$.60). Virtually all households employ traditional farming practices, using no mechanized equipment or animal traction to produce taro, cassava, coconut, and kava plants. Many households also engage in artisanal fishing and handicraft making. Farming, fishing, and handicraft making, respectively, account for 66%, 11%, and 10% of income earned.

2.2. Recruitment fraud

Since the invasion of Iraq in 2003, U.K.- and U.S.-based private security companies have been seeking personnel for their operations in the Middle East (e.g., delivering supplies to U.S. armed forces in Iraq) (MacLellan 2006). Pay is good, but the jobs are dangerous (some casualties have been reported). With a large pool of former army personnel, Fiji has been a major labor supplier. The Fijian government welcomed this movement as a solution for its unemployment (the government was not directly involved). In late 2004 and early 2005, a private Fijian recruitment agency conducted the largest recruitment drive in the country. All males between the ages of 18 and 60 were eligible to apply by paying a fee in advance (applicants were not necessarily promised a job). According to news media, the agency collected US$2 million fees from at least 20,000 applicants.

The agency's recruitment drive extensively covered Cakaudrove Province. In my survey, village selection was made before I became aware of the recruitment drive, and the recruitment drive did not affect the household sampling design, as discussed above. While I was pretesting the questionnaires in May 2005, I first noticed the unusual recruitment drive and quickly added questions about job applications. I was not very suspicious about potential fraudulence, and during interviews several months after people's application decisions, respondents were still unaware of the fraudulence. Right after the survey was completed, news media started to report its fraudulence, as a huge number of applicants got neither a job nor a refund. At that time, the agency's director had already left the country.

2.3. Fraud victimization

[7] Marriage across different kin groups in the village or different villages is common, and this paper focuses on the kin groups to which households currently belong.

The empirical analysis covers 1,239 males aged between 18 and 60, who were eligible for the jobs offered by the recruitment agency; this sample includes 783 households (87% of the whole sample) – with complete data on the damage caused by the cyclone in January 2003, discussed shortly – in all 43 sample villages. Although the agency's recruitment drive did not cover all villages in the region, all eligible individuals are effectively attempted victims, because people in villages not covered by the recruitment drive were privately informed of the job opportunity.[8] No respondents had difficulty in answering questions about their job applications; 19% of eligible individuals (27% of households with eligible members, or 23% of the whole sample) applied. Most application decisions were made in early 2005, i.e., at least two years after the cyclone. Although 36% of households with eligible members have more than one eligible individual, only one application was made by over 90% of households with an applicant(s); that is, in most cases, each household selected one migrant laborer, indicating that migration is a collective family decision.

To apply, an average applicant spent F$230, and an average victimized household spent F$242 (close to the mean monthly per-capita income). This amount includes application fees and all other related expenses, especially for transportation; to file an application, applicants travelled to the recruitment agency's office in the biggest town in the province, and many revisited to check their status.

2.4. Disaster and aid

On 13 January 2003, Cyclone Ami swept over the northern and eastern regions of the country; Ami was the only cyclone in the northern region from 1991 through 2005 (McKenzie et al. 2005). According to respondents' subjective assessments, about 64% of residents' dwellings – a main house and/or free-standing units, such as the kitchen, shower, and toilet (not all households have such units, as these facilities are often located inside the main house) – were damaged (henceforth *disaster victims*), and the mean value of total housing damage in the whole sample was about F$1,100 (the descriptive statistics of housing damage and aid among all households in the sample, including those with no members eligible for the job, are very similar

[8] Since which villages in the sample were directly covered is unknown, I cannot examine the targeting of fraud; the month when fraud attempt was made – directly or indirectly – is not known, either.

to those discussed here). The cyclone caused no casualties, and permanent migration was virtually nonexistent in its aftermath.

The provision of emergency relief (see Takasaki 2011b for details) was followed by housing reconstruction programs. About one quarter of households received construction materials (henceforth *aid recipients*), and the mean amount received in the whole sample was almost F$700. Although almost all aid recipients were disaster victims (i.e., virtually no leakage), about 40% of disaster victims were aid recipients (i.e., large under-coverage), and aid was strongly targeted on the value of housing damage (Takasaki 2011a). Although aid was most commonly received in 2004, i.e., before job application decisions were made, a significant proportion of full construction materials was provisioned in 2005, i.e., during or after job application decisions were made; in particular, although about 23% of recipients received aid in 2005, 42% of the amount was provisioned then. Although there was at least one disaster victim in each village, a few villages had no aid recipients. Post-disaster antisocial or conflict behavior was not reported in the region.

2.5. Disaster vs. fraud victimization

This sub-section provides descriptive evidence for the damage effects on job applications, or equivalently ex post fraud victimization, and the potential role of disaster aid in mitigating such damage effects. First, if housing damage increases fraud victimization, we would expect that job applications are more common among disaster victims than non-victims and housing damage is more common and larger among job applicants than non-applicants. Supporting results are found for job application (9% difference) and the incidence of housing damage (14% difference), though there is no significant difference in the value of housing damage by job application (see panels A and C of Table 1). Next, if housing aid decreases fraud victimization, we would expect that job applications are less common among aid recipients than non-recipients among disaster victims, and housing aid is less common and smaller among job applicants than non-applicants. No supportive results are found regardless of the timing of aid: None of these differences are significantly different from 0 at conventional levels (see panels B and C, the former of which reports results for aid in 2003-04; those for aid in 2003-05 are very similar).

3. Econometric specification

I start with the following reduced-form specification:

$$y_1 = \alpha + \beta y_2 + \boldsymbol{\gamma x} + \boldsymbol{V} + u \,. \qquad (1)$$

where y_1 is a dummy variable for individual job application (i.e., ex post fraud victimization); y_2 is a dummy variable for housing damage; $\boldsymbol{x}$ is a vector of exogenous individual and household controls (defined below); $\boldsymbol{V}$ is a vector of village dummies; and u is an error term. I consider disaster aid as an additional determinant in the next section. Village dummies fully control for village-level factors affecting job applications, such as distance to the recruitment agency's office and local labor-market conditions, as well as village-level covariate disaster shocks and aid supply. Equation (1) is estimated by Ordinary Least Squares (OLS) (linear probability model). As disaster damage is measured at the household level, standard errors are clustered by household. If disaster victimization increases job applications, $\beta > 0$. The assumption of a constant damage effect in equation (1) is relaxed in Section 5.

The identification assumption in equation (1) that household-level housing damage is uncorrelated with unobserved individual/household heterogeneity determining job application decisions, such as ability and preference, may not hold, as follows. With village-level covariate shocks controlled for, whether or not housing is damaged in the village depends on its unobserved quality prior to the disaster, in particular, its durability against cyclone shocks (e.g., heavy wind, rain, flood), which is a function of cumulative investments in housing that the household had made, as well as its location in the village. If housing quality is positively (negatively) correlated with say, preference for labor migration, i.e., households with housing less (more) vulnerable to cyclones tend to apply for the job, the estimated damage effect is biased downward (upward).

My empirical strategy is two-fold. First, since housing location is fixed with mataqali (a village subgroup owing land), it is effectively captured by mataqali fixed effects. Second, I endogenize household housing damage by using flood shock as an excluded instrumental variable (IV). The household survey asked about the magnitude of flood, not damage caused by the flood, that the household had experienced on its land, based on a five-point scale (0: none, 1: small, 2: some, 3: large, 4: very large); 45% of households had experienced a flood. The identification assumption is that household flood shock affects its housing damage within mataqali and is uncorrelated with unobserved individual/household heterogeneity in job applications. In particular, household housing investments correlated with housing vulnerability to flood on its micro location, if any, are assumed to be uncorrelated with household unobserved heterogeneity. If flood shock augments housing damage and the damage effect on job

applications is positive, we would expect that flooding is more common and larger not only among disaster victims than non-victims, but also among job applicants than non-applicants. These expectations are strongly supported by the results reported in panels A and C, respectively, of Table 1, which indicate a 30% and 16% difference in flood incidence and almost a 1 and .5 difference in flood magnitude.

The two-equation system is

$$y_1 = \alpha_1 + \beta_1 y_2 + \boldsymbol{\delta}_1 \boldsymbol{x} + \boldsymbol{G} + u_1, \quad (2)$$

$$y_2 = \alpha_2 + \gamma_2 w + \boldsymbol{\delta}_2 \boldsymbol{x} + \boldsymbol{G} + u_2, \quad (3)$$

where w is flood shock; $\boldsymbol{G}$ is a vector of mataqali dummies; and u_1 and u_2 are error terms. The endogeneity of housing damage y_2 is controlled for by estimating equation (2) using a two-stage least squares (2SLS) estimator with the first-stage equation (3), where w is an excluded IV.

The selection of individual and household controls follows the literature on both crime victimization and migration.[9] Individual factors capture demographics (household headship and age [minus 18]; recall that age 18 is the youngest in the sample), education (primary-school completion, see below), and employment (permanent wage labor). Permanent employment, the opportunities for which are limited and exist mostly in the public sector, is considered exogenous, because individuals cannot flexibly adjust it (its status did not change after application decisions were made); dropping the employment variable significantly alters none of the remaining results. Household factors capture demographics (the size of male/female working adults [ages 18-60], children, and elderly) and assets (agricultural land),[10] but not income. Though income is often considered as an important determinant of crime victimization, it is endogenous as a determinant

[9] Sociologists categorize determinants of crime victimization into exposure, attractiveness of potential targets, guardianship, and proximity to potential offenders (Cohen et al. 1981). The first three are often captured by demographic factors, asset/income, and police access, respectively (e.g., Barslund et al. 2007). Determinants of migration often include demographic factors, education, asset/income, home location, and migrant network, as well as shocks (e.g., Borjas 1989; Stark 1991). Police access, proximity to potential offenders (e.g., direct coverage by the recruitment drive), and home location are fully controlled for by village (mataqali) dummies in our models. Migrant networks in the Middle East are very limited – acquaintance with someone who already had gotten the same job is uncommon in the sample. In another words, the recruitment agency is the only network available.

[10] Household land holdings (under the customary tenure) at the time of the fraud attempt should be almost the same as those at the time of interviews used here. The present data lack information about non-land asset holdings prior to the fraud attempt.

of job application, because in anticipation of labor migration and remittances, the household may adjust its earning efforts, and any unobserved factors that determine income, such as skills, may also influence migration decisions (even income measured before the fraud attempt, which the present data lack, would be endogenous). Still, household permanent income is controlled for by employment and assets, as well as demographic factors. The descriptive statistics of all controls reported in panel C of Table 1 show that compared to non-applicants, job applicants are younger, more educated, and in households with fewer male working adults.[11]

4. Estimation results

OLS and 2SLS estimation results of equation (2) are reported in panel A of Table 2 (in all regression analyses, mataqali with only one household in the sample are dropped). The OLS estimate of the damage effect without individual/household controls is about .08 and statistically significant at a 1% significance level (column 1). The comparable 2SLS estimate is about .25, i.e., about three times the OLS estimate, and also significant at a 1% significance level (column 4). Flood shock – on a normalized five scale (with 1 = very large flood) – strongly determines housing damage in the first-stage equation (3) (F value for this excluded IV is 69), and the reduced-form result – equation (2) with housing damage replaced with flood shock – is also statistically significant (the table reports estimated coefficients only for flood shock); in particular, experiencing a very large flood increases the probability that housing is damaged by .44 and that the individual applies for the job by .11. When individual and household controls are added, the estimated damage effects do not significantly change in either OLS or 2SLS (columns 2 and 4).[12]

Thus, housing damage is endogenous in equation (2); that assuming its exogeneity leads to strong downward bias in its estimated effects suggests a significant positive correlation of unobserved housing quality and individual/household heterogeneity. Disaster victimization increases the probability that the individual is victimized by fraud by .25; then, if there had been no cyclone, the fraud-victimization rate among disaster victims would have been 17.9% (=.224/1.25) and that among disaster victims and non-victims combined would have been 16.2%

[11] Although the present data cannot capture military experience, anecdotal evidence suggests that individuals with such experience are not very common in the sample; indeed, households with a member currently working in the military are rare.

[12] The OLS estimates of equation (1) with village dummies – either without or with individual/household controls – are very similar to those of equation (2).

(= (787×.179 + 452×.133)/1239) (Table 1), and thus, out of the actual overall fraud victimization (19%), about 3% was brought by disaster victimization.

Estimation results for controls are consistent with the descriptive findings above (the results are similar when disaster damage is excluded in equation 2). On one hand, young and educated household heads of households with fewer male working adults are more likely to apply.[13] I interpret these results when I consider the potentially heterogeneous impacts of these controls by disaster victimization below. On the other hand, neither employment nor assets significantly alter application. These results suggest that the opportunity cost of migration – losing the current employment – is low, because migrant jobs are better than domestic ones, and application is largely neutral to household welfare; that is, most households are so poor that they would potentially be attracted by this job opportunity.

For a robustness check, I estimate the damage effect at the margin by redefining y_2 in equations (2) and (3) as housing damage value (log). Distinct from damage incidence, in which recall errors are minor, measurement errors in damage value can be significant, causing attenuation bias that also can be controlled for in the 2SLS estimation. Estimation results reported in panel B of Table 2 (controls are not shown for brevity) are qualitatively the same as those for the damage dummy. In particular, the 2SLS estimates of the damage effect are about three times the OLS estimates; according to the former, a 10% increase in damage value augments the probability of job application by about .003. Flood shock is a strong instrument (a very large flood increases damage value by over 300%) and the corresponding reduced-form results are consistent with the 2SLS results.

So far, I have not considered disaster aid as a potential determinant of job applications. As housing aid is positively correlated with housing damage (targeting), whether this omitted variable causes bias in the estimated damage effects depends on whether it influences individual job applications. With a lack of valid instruments for housing aid, I estimate models with housing aid as an additional exogenous control to see whether the remaining estimation results change significantly. Adding the dummy for aid receipt in 2003-04 to the models with the damage dummy does not significantly alter the remaining results, nor does adding the amount of

[13] When three dummies for secondary-school completion or above (18% of eligible individuals), secondary-school incompletion (37%), and primary-school completion (28%) are used (with primary-school incompletion or below as a base), none of the three estimated coefficients are significantly different from each other (results not shown).

aid received (log) to the models with the damage value significantly change the remaining results (columns 3 and 6); using housing aid in 2003-05 yields very similar results (results not shown). Hence, the potential bias caused by omitted disaster aid is unlikely to be significant.[14]

5. Coping capacity

How distinctly do disaster victims employ high-risk, high-return labor migration as a coping strategy, depending on their coping capacity? I focus on individual- and household-level labor capacities, which are shown to directly affect job applications above: individual labor productivity (quality) captured by headship, age, and education (primary-school completion) and household labor endowment (quantity) captured by the number of male working adults.[15] Specifically, I augment equation (2) by adding an interaction term of y_2 and one of these four factors separately. In the 2SLS, housing damage and its interaction term are two endogenous variables to be controlled for by using flood shock and its interaction with the corresponding control as two excluded IVs.

The OLS and 2SLS estimation results for the dummy for housing damage without aid receipt as a control are reported in Table 3, where columns (1) and (6), respectively, replicate those with no interaction term reported in columns (2) and (5) of panel A of Table 2. For clarity, each of the remaining columns reports only a selected control interacted (the results of controls not reported are very similar to those reported in Table 2). The results with aid receipt – in 2003-04 or 2003-05 – as an additional control are very similar. Across specifications, flood shock and its interaction term are strong instruments (almost all the F-values for the excluded IVs in the first stage are greater than 30), and the corresponding reduced-form results are consistent with the 2SLS results discussed next and statistically significant (results not shown).

The estimated potentially heterogeneous damage effects by coping capacity are quite distinct between OLS and 2SLS.

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[14] Although consistent with the descriptive findings above, the estimated aid effects in OLS – for either aid dummy or amount – are negative and very small in magnitude with no statistical significance, those in the 2SLS are considerable in magnitude with statistical significance and still much smaller in magnitude than the corresponding damage effects; interpreting these results with potential bias requires strong caution, however.

[15] Analyzing assets (land) in the same way shows that assets do not significantly differentiate the damage effects; doing so for employment is infeasible because it is relatively uncommon (only 9% of male working adults are employed).

1) Headship: Although the estimated damage effect is statistically significant only for male adults other than heads and headship augments job applications only among disaster non-victims in the OLS (column 2), the converse holds true in the 2SLS – the damage effect is statistically significant only for heads and headship augments job applications only among disaster victims (the joint significance tests are significant at a 1% significance level) (column 7).
2) Age: Although the estimated damage effects are smaller among older adults than younger ones in the OLS (column 3), in the 2SLS the estimates are not differentiated by age and the negative impact of age becomes statistically weak (column 8).
3) Education: Although the OLS estimates of the damage effects are not differentiated by education (column 4), in the 2SLS the damage effect is statistically significant only for the educated, and education significantly augments job applications only among disaster victims (the joint significance tests are significant at a 1% significance level) (column 9).
4) Male labor endowment: Although the OLS estimates of the damage effects are not differentiated by labor endowment (column 5), in the 2SLS it significantly decreases the damage effects – they are statistically significant for households only with one or two eligible members (mean size is 2.1). It also decreases and increases job applications among disaster victims and non-victims, respectively, with almost the same magnitude at the margin (about .09) (these results are significant at least at a 5% significance level) (column 10).

These 2SLS results suggest that household/individual labor capacities strongly differentiate coping responses among disaster victims. The household with small labor endowments employs labor migration for coping by selecting a member with high labor productivity – an educated head, i.e., a main bread earner, regardless of his age – as a migrant laborer; in contrast, the household with relatively large labor endowments can better cope with disaster damage through domestic labor supply without relying on risky migration. Thus, households resort to coping through labor migration when their domestic coping options are constrained by their labor endowment. Among disaster non-victims, the household with large labor endowments can better seek labor migration for risky investment (not coping), and a selected migrant laborer is not necessarily a member with high labor productivity. This positive labor-endowment effect on migration does not appear among disaster victims, because it first determines their domestic labor supply for coping; disaster victims with limited domestic options

rely more heavily on risky migration by utilizing a productive migrant laborer than disaster non-victims. The biased OLS estimations fail to capture the differentiating role of coping capacity; according to the OLS results, regardless of labor endowments, a young male adult but a head (regardless of education) would tend to be a migrant laborer among disaster victims.

For a robustness check, I repeat all analyses for housing damage value (log) and its interaction terms; in the 2SLS estimation, the same set of excluded IVs as above are used. The OLS and 2SLS results without the amount of aid received as a control are reported in Table 4, the format of which is the same as that of Table 3 (the results with the log amount of aid received – in 2003-04 or 2003-05 – are very similar). Almost all estimation results are qualitatively the same as those for the damage dummy; as the only exception, the estimated coefficient for housing damage value interacted with age is statistically nonsignificant also in the OLS (column 3).[16]

6. Conclusion

Although international remittances are important insurance against natural disasters in developing countries, fraud is a pitfall of international labor migration. This paper addressed an unexplored question about the disaster-fraud nexus: Do natural disasters beget fraud victimization among the poor as they seek labor migration for coping? I exploited a natural experiment: Two years after a cyclone, a huge number of Fijian males were defrauded of application fees for labor migration to the Middle East in 2005. My household survey data, which by chance I collected before the fraudulence is noticed, are free from underreporting/misreporting out of embarrassment. Controlling for the endogeneity of household housing damage by using exogenous flood shock as an instrument reveals that housing damage strongly increases individual member's job application that later turned out to be fraud victimization. Households resort to high-risk-high-return labor migration because their domestic coping options are constrained by their labor endowment.

Hence, the significant potential of international labor migration as insurance underlies recruitment fraud as its pitfall, and this pitfall is exacerbated by natural disasters because the

[16] In the 2SLS, the estimated coefficient for housing damage value interacted with household head is statistically significant at almost a 10% significance level and the estimated damage effect for household head (.050) is significant at a 1% significance level; the estimated effects of male labor endowment among disaster non-victims and at the mean housing damage value (.059 and -.024, respectively) are significant at a 15% and 10% significance level, respectively.

poor with limited coping capacity resort to risky migration. The risk-coping channel makes the poor who are vulnerable to disasters also vulnerable to potential post-disaster fraud. Even if the post-disaster antisocial or conflict behavior highlighted by criminologists is uncommon or even nonexistent, the risk-coping channel can bring fraud victimization throughout the relief and reconstruction phases, especially among the poor. Researchers and policy makers should pay greater attention to the risk of post-disaster fraud, which may be greater, be more persistent, and lead to more adverse consequences than normally thought. Methodologically, controlling for the endogeneity of household-level disaster shocks is crucial to identify their impacts. Failing to do so could give rise to strong bias.

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Table 1. Job application (ex post fraud victimization), disaster damage/aid, and individual/household characteristics.

A. Job application by housing damage

	All	Disaster victims	Disaster non-victims	Difference	Standard error of difference
Job application (0/1)	0.19 (0.39)	0.22 (0.42)	0.13 (0.34)	0.09 ***	0.02
Flood (0/1)	0.46 (0.50)	0.57 (0.50)	0.27 (0.44)	0.30 ***	0.04
Flood magnitude (0-4)[a]	1.29 (1.57)	1.65 (1.64)	0.66 (1.22)	0.98 ***	0.12
No. observations	1239	787	452		

B. Job application by aid receipt among disaster victims

	All	Aid receipients in 2003-04	Aid non-receipients in 2003-04	Difference	Standard error of difference
Job applicantion (0/1)	0.22 (0.42)	0.22 (0.42)	0.22 (0.42)	0.00	0.03
No. observations	787	250	534		

C. Disaster damage/aid and individual/household characteristics by job application

	All	Job applicants	Job non-applicants	Difference	Standard error of difference
Housing damage (0/1)	0.64 (0.48)	0.75 (0.44)	0.61 (0.49)	0.14 ***	0.04
Housing damage value (F$)	1067 (2144)	1197 (2331)	1036 (2098)	161	175
Aid receipt in 2003-04 (0/1)	0.21 (0.41)	0.24 (0.43)	0.20 (0.40)	0.04	0.03
Aid receipt in 2003-05 (0/1)	0.27 (0.44)	0.30 (0.46)	0.26 (0.44)	0.04	0.04
Aid received in 2003-04 (F$)	409 (1503)	276 (1232)	439 (1558)	-163	109
Aid received in 2003-05 (F$)	659 (1910)	571 (1763)	680 (1943)	-110	142
Flood (0/1)	0.46 (0.50)	0.58 (0.49)	0.43 (0.49)	0.16 ***	0.04
Flood magnitude (0-4)[a]	1.29 (1.57)	1.67 (1.62)	1.20 (1.55)	0.47 ***	0.13
Household head (0/1)	0.50 (0.50)	0.52 (0.50)	0.49 (0.50)	0.03	0.04
Age	36.0 (12.1)	34.8 (10.1)	36.3 (12.5)	-1.53 *	0.80
Primary-school completion (0/1)	0.83 (0.37)	0.94 (0.25)	0.81 (0.39)	0.13 ***	0.02
Employment (0/1)	0.09 (0.29)	0.08 (0.28)	0.09 (0.29)	-0.01	0.02
Agricultural land (acre)	3.24 (4.96)	3.18 (5.64)	3.26 (4.79)	-0.08	0.54
No. males 18-60 years old	2.14 (1.25)	1.89 (1.13)	2.20 (1.27)	-0.30 ***	0.10
No. females 18-60 years old	1.53 (0.98)	1.50 (0.96)	1.53 (0.98)	-0.04	0.08
No. < 18 years old	2.47 (2.07)	2.60 (2.08)	2.44 (2.07)	0.16	0.17
No. > 60 years old	0.42 (0.68)	0.46 (0.69)	0.41 (0.68)	0.05	0.05
No. observations	1239	236	1003		

Notes: Standard deviations are shown in parentheses. Standard errors are clustered by household. [a]0 = none, 1 = small, 2 = some, 3 = large, 4 = very large. *10% significance, **5% significance, ***1% significance.

Table 2. Damage effects on job application (ex post fraud victimization).

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Housing damage						
Housing damage (0/1)[a]	0.077 ***	0.087 ***	0.091 ***	0.244 ***	0.254 ***	0.271 ***
	(0.026)	(0.025)	(0.027)	(0.090)	(0.087)	(0.094)
Household head (0/1)		0.092 **	0.092 **		0.090 **	0.091 **
		(0.037)	(0.037)		(0.036)	(0.036)
Age - 18		-0.004 ***	-0.004 ***		-0.004 ***	-0.004 ***
		(0.001)	(0.001)		(0.001)	(0.001)
Primary-school completion (0/1)		0.126 ***	0.127 ***		0.141 ***	0.144 ***
		(0.022)	(0.023)		(0.024)	(0.024)
Employment (0/1)		0.027	0.028		0.039	0.043
		(0.044)	(0.044)		(0.043)	(0.043)
Log of agricultural land (m^2)		-0.002	-0.002		-0.004	-0.004
		(0.008)	(0.008)		(0.007)	(0.007)
No. males 18-60 years old		-0.025 **	-0.025 **		-0.030 **	-0.029 **
		(0.012)	(0.012)		(0.012)	(0.012)
No. females 18-60 years old		0.011	0.011		0.016	0.016
		(0.015)	(0.016)		(0.015)	(0.015)
No. < 18 years old		0.001	0.001		-0.001	-0.001
		(0.007)	(0.007)		(0.006)	(0.006)
No. > 60 years old		0.018	0.017		0.018	0.017
		(0.021)	(0.021)		(0.021)	(0.021)
Aid recipt (0/1)			-0.018			-0.086 *
			(0.034)			(0.049)
R-squared	0.270	0.301	0.302			
No. observations	1201	1189	1189	1201	1189	1189
First stage: Flood shock (0-1)				0.438 ***	0.448 ***	0.417 ***
				(0.053)	(0.053)	(0.051)
Reduced-form: Flood shock (0-1)				0.107 ***	0.114 ***	0.113 ***
				(0.041)	(0.040)	(0.041)
B. Housing damage value						
Log of housing damage value (F$)[a]	0.008 **	0.009 **	0.011 ***	0.031 **	0.032 ***	0.036 ***
	(0.004)	(0.004)	(0.004)	(0.013)	(0.012)	(0.013)
Log of aid received (F$)			-0.008			-0.020 **
			(0.005)			(0.008)
Individual and household controls	No	Yes	Yes	No	Yes	Yes
R-squared	0.265	0.294	0.296	0.239	0.268	0.271
No. observations	1185	1173	1173	1185	1173	1173
First stage: Flood shock (0-1)				3.129 ***	3.203 ***	2.952 ***
				(0.369)	(0.371)	(0.335)
Reduced-form: Flood shock (0-1)				0.096 **	0.103 **	0.105 ***
				(0.041)	(0.040)	(0.040)

Notes: Dependent variables are a dummy variable for job application. Standard errors clustered by household are in parentheses. Other controls not shown here are mataqali dummies and constant; individual and household controls shown in panel A are not shown in panel B. [a]Endogenous variable in 2SLS. *10% significance, **5% significance, ***1% significance.

Table 3. Heterogeneous effects of housing damage on job application (ex post fraud victimization).

	OLS					2SLS				
(n=1189)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Housing damage (0/1)[a]	0.087 ***	0.123 ***	0.154 ***	0.079 **	0.056	0.254 ***	0.093	0.255 *	0.110	0.614 ***
	(0.025)	(0.034)	(0.041)	(0.040)	(0.047)	(0.087)	(0.117)	(0.143)	(0.173)	(0.187)
Housing damage × Household head[a]		-0.069					0.315 *			
		(0.044)					(0.171)			
Housing damage × (Age - 18)[a]			-0.004 **					0.000		
			(0.002)					(0.007)		
Housing damage × Primary-school completion[a]				0.009					0.169	
				(0.045)					(0.182)	
Housing damage × No. males 18-60 years old[a]					0.016					-0.185 **
					(0.019)					(0.078)
Household head (0/1)	0.092 **	0.134 ***				0.090 **	-0.103			
	(0.037)	(0.043)				(0.036)	(0.106)			
Age - 18	-0.004 ***		-0.002			-0.004 ***		-0.004		
	(0.001)		(0.002)			(0.001)		(0.004)		
Primary-school completion (0/1)	0.126 ***			0.120 ***		0.141 ***			0.022	
	(0.022)			(0.034)		(0.024)			(0.126)	
No. males 18-60 years old	-0.025 **				-0.036 **	-0.030 **				0.094 *
	(0.012)				(0.017)	(0.012)				(0.054)
R-squared	0.301	0.303	0.304	0.301	0.302					
F-value for excluded IVs in the first stage										
Housing damage						71.3	36.0	35.9	35.7	37.1
Housing damage interacted with selected control							33.9	31.9	35.6	19.8

Notes: Dependent variables are a dummy variable for job application. Standard errors clustered by household are in parentheses. Only controls interacted are shown in columns (2)-(5) and (7)-(10). Other controls not shown here are employment (0/1), log of land (m^2), no. females 18-60 years old, no. < 18 years old, no. > 60 years old, mataqali dummies, and constant. [a] Endogenous variables in 2SLS. Excluded IVs are flood shock (0-1) and its interaction with the corresponding control. *10% significance, **5% significance, ***1% significance.

Table 4. Heterogeneous effects of housing damage value on job application (ex post fraud victimization).

	OLS					2SLS				
(n=1173)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log of housing damage value (F$)[a]	0.009 **	0.014 ***	0.016 ***	0.007	0.004	0.032 ***	0.013	0.033 *	0.021	0.071 ***
	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)	(0.012)	(0.016)	(0.019)	(0.024)	(0.023)
Log of housing damage value × Household head[a]		-0.010					0.037			
		(0.006)					(0.023)			
Log of housing damage value × (Age - 18)[a]			0.000					0.000		
			(0.000)					(0.001)		
Log of housing damage value × Primary-school completion[a]				0.002					0.013	
				(0.006)					(0.025)	
Log of housing damage value × No. males 18-60 years old[a]					0.002					-0.020 **
					(0.003)					(0.009)
Household head (0/1)	0.093 **	0.132 ***				0.089 **	-0.052			
	(0.038)	(0.043)				(0.037)	(0.089)			
Age - 18	-0.004 ***		-0.002			-0.004 ***		-0.004		
	(0.001)		(0.002)			(0.001)		(0.004)		
Primary-school completion (0/1)	0.120 ***			0.111 ***		0.133 ***			0.076	
	(0.023)			(0.033)		(0.024)			(0.110)	
No. males 18-60 years old	-0.023 *				-0.033 **	-0.027 **				0.059
	(0.012)				(0.017)	(0.012)				(0.040)
R-squared	0.294	0.296	0.296	0.294	0.295					
F-value for excluded IVs in the first stage										
Log of Housing damage value						77.9	39.0	38.2	37.4	39.1
Log of Housing damage value interacted with selected control							35.4	35.8	38.0	19.2

Notes: Dependent variables are a dummy variable for job application. Standard errors clustered by household are in parentheses. Only controls interacted are shown in columns (2)-(5) and (7)-(10). Other controls not shown here are employment (0/1), log of land (m^2), no. females 18-60 years old, no. < 18 years old, no. > 60 years old, mataqali dummies, and constant. [a] Endogenous variables in 2SLS. Excluded IVs are flood shock (0-1) and its interaction with the corresponding control. *10% significance, **5% significance, ***1% significance.