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Fraud and Poverty: Exploring Ex Ante Victim Data

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Abstract

Fraud studies rely on potentially underreported/misreported victim data in developed countries, virtually ignoring developing countries. This paper proposes using *ex ante victim data*, to be collected *before* attempted victims become aware of the fraudulence, and examines *recruitment fraud*, which is tightly linked with poverty. In rural Fiji, almost one quarter of households were defrauded of application fees for labor migration. The bigger problem is indirect costs: Controlling for victim endogeneity reveals that households' false expectations about international remittances led to a significant reduction in the domestic private transfers victims received. The analysis sharply identifies who was victimized and why/why not.

Key Words: Recruitment fraud; Poverty; Ex ante victim data; Labor migration; Private transfers; Fiji

JEL Codes: K42, O12.

I. Introduction

Fraud – "deceit or intentional misrepresentation of fact with the intent of unlawfully depriving a person of his or her property rights" (Rush 1986, p. 103) – is a major crime in human society. This paper addresses two fundamental lacunae in fraud studies. The first lacuna, which is empirical, is that the literature virtually ignores developing countries, especially poor populations. Fraud in developed countries is an enormous problem. Anderson (1999) estimates the total annual fraud losses (excluding costs for preventing and responding to fraud) in the United States to be about $440 billion. In a U.S. national survey in 1991, Titus, Heinzelmann, and Boyle (1995) find that 31% of respondents had experienced victimization or attempted victimization of consumer fraud in the preceding year, and 15% of the total sample suffered successful victimizations, with an average loss of $216, suggesting over $40 billion annual losses among Americans (cf. the total cost of street crime in 1992 is estimated at about $17.6 billion, Klaus 1994).

In contrast, fraud in developing countries has received very limited attention and most extant crime and victim studies focus on violent and property crimes.[1] Some Living Standard Measurement Surveys (LSMS) of the World Bank, for example, cover crime victimization, but not fraud victimization.[2]

What underlies the virtual ignorance of fraud among the poor is a common perception that fraud is a problem of the wealthy: Those with limited incomes and assets are unattractive to fraudsters and even if they are defrauded, their loss cannot be significant.[3] I argue that crime and development studies miss a potentially huge problem of fraud, which is directly linked with poverty. First, the poor in developing countries are more vulnerable to fraud in productive activities than consumer fraud, because they are willing to pay for better earning opportunities. Second, victims can suffer from much more than defrauded money (*direct costs*), because with false expectations about earnings induced by the fraud, people alter their behaviors in adverse ways (*indirect costs*, or externalities). Fraud in productive activities may significantly undermine peoples' risk attitudes, investments, trust, and informal institutions; fraud may be a hidden cause of poverty traps, as theorized by Mehlum, Moene, and Torvik (2005) for crime in general (see also Lloyd-Ellis and Marceau 2003).

This paper examines *recruitment fraud* in developing countries.[4] Although international labor migration and remittances play important roles in many developing

[1] Since Becker's (1968) pioneering work, there has been a surge of economic studies on crime, victimization, and punishment. Although most extant studies focus on urban crime in both developed and developing countries (e.g., Glaeser and Sacerdote 1999), some empirical works highlight crime in rural developing areas (e.g., Fafchamps and Minten 2006; Miguel 2005). This paper adds to the growing literature on poverty and crime in rural developing areas.

[2] In contrast, significant attention has been given to corruption – "acts in which the power of public office is used for personal gain in a manner that contravenes the rules of the game" (Jain 2001, p.73). Seligson (2006) examines household-level corruption victimization.

[3] In developed countries, policing agencies, news media, and survey firms also give much less attention to frauds against poor consumers than those against businesses and public bodies (e.g., Levi and Burrows 2008).

[4] Recruitment fraud is part of employment fraud, involving the scamming of people seeking, not performing, employment. Although in-house recruitment can be a fraud, recruitment fraud targeting the poor is mostly recruitment agencies' fraud. In this paper, I use recruitment fraud and recruitment agencies' fraud interchangeably.

areas, labor migrants often rely on recruitment agencies with information about and market power in foreign job markets and their potential fraud and abuse have been frequently noted (World Bank 2006b). All that is known about recruitment agencies' fraud, however, is based on anecdotal evidence and limited case studies.[5] As far as I know, there exists no victim survey on recruitment fraud. The International Crime Victim Survey (ICVS) of the United Nations Center for International Crime Prevention covers only consumer fraud. In developed countries, recruitment fraud in out-migration has received little attention.[6]

The second lacuna in the literature, which is methodological, is that it relies on incomplete victim data. A fundamental difference exists between fraud and other crimes: When responding to fraud attempts, victims are unaware of the offense. The decisions that attempted victims had thought were good later turned out to be bad. This gap between *expectation* and *reality* not only brings fraud into existence, but also strongly shapes attempted victims' responses to survey questions. It is well known that victims of white-collar crimes are reluctant to report the incidents because they feel embarrassed (Ennis 1967): Many victims "often are viewed with a mixture of skepticism, suspicion, and disbelief, and viewed as unworthy of society's protection" (Walsh and Schram 1980, pp.46-47). Titus, Heinzelmann, and Boyle (1995) find that only one third of self-reported consumer-fraud victims report their experiences to the authorities; Mason and Benson (1996) show an even smaller rate of reporting (23%). Underreporting of victimization per se is also very likely in fraud victim surveys, though this critical problem has not yet been systematically addressed. Other information related to fraud victimization also can be misreported by respondents who hide the incidents, for coherence. Such

[5] 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.

[6] 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.

underreporting/misreporting problems can be significant in any fraud victim surveys that collect information about the victimization (reality) *after* victims and/or the public become aware of the fraudulence. Now, imagine a survey for attempted victims conducted *before* they become aware of the fraudulence. As respondents never manipulate records to cover up (or exaggerate) the victimization experience before they know about it, the survey is free from fraud-induced measurement errors. The distinction of *ex ante victim data* on expectation from *ex post victim data* on reality is critical for fraud (but not for other crimes). This distinction has been ignored in the literature, however;[7] all available fraud victim data are ex post victim data.

The gap between expectation and reality in fraud victimization also strongly differentiates its consequences before the fraudulence is noticed (*ex ante impacts*) from those afterward (*ex post impacts*). Researchers have not yet explored ex ante impacts, although they are indispensable to measure indirect costs of fraud victimization. As fraud victimization is not an exogenous shock, but a bad decision made by victims, controlling for its endogeneity is crucial to identify its true impacts – either ex ante or ex post; victim endogeneity, however, has not yet been addressed in the fraud literature.[8] Ex ante victim surveys can collect not only reliable information about outcomes, but also instrumental variables (IV) needed for identification.

This paper conducts a first ex ante victim data analysis. Section II offers a conceptual framework of fraud victimization, victim data, and victim endogeneity. At first glance, collecting ex ante victim data seems infeasible, because researchers and professionals cannot tell the fraudulence until victims or the public notice it; moreover, such data collection can be morally wrong, because even if such early detection of fraud is made, it should be used to prevent the fraud per se. I propose feasible data-collection designs. Section III describes my own ex ante victim data on an organized recruitment fraud in Fiji. In 2005, a recruitment agency defrauded more than 20,000 individuals of

[7] Criminologists consider the time gap between the commission of the offence and awareness, reporting, and recording as a data problem in fraud statistics (e.g., Levi and Burrows 2008).

[8] Some economic works examine broad consequences of crime – other than fraud – and victimization by addressing their endogeneity. Crime can have a long-run effect, such as child abuse as a source of future crime (Currie and Tekin 2006), and can involve negative externalities, such as the effect on property value (Linden and Rockoff 2008).

application fees for labor migration to the Middle East.[9] I conducted a survey about people's job applications, before they had become aware of the offense. Sections IV and V, respectively, investigate who applied for the jobs and why/why not. In Section VI, as an example of ex ante impacts of fraud victimization, I examine household domestic private transfers, which play a central role in kin-based Fijian society (Hann 2006). With strong expectations and beliefs about labor migration among potential donors, the *substitution effect* – newly available international remittances reduce domestic private transfers – may already be at work.[10] Because the expected substitution is not realized, all reduced private transfers, if any, are indirect costs borne by victims.

The major findings can be summarized as follows. First, recruitment fraud victimization is highly prevalent in rural Fiji. Almost one quarter of households in the region were defrauded, with direct costs in a range of monthly per-capita earned income. Second, victims suffer much more. The prospective labor migration greatly lowered the domestic private transfers victims received. Third, preventing fraud victimization is very difficult. Only better domestic employment is shown to be a solution. The last section concludes by synthesizing the major findings and discussing promising data collection and questions for new fraud study.

II. Conceptual framework and fraud-victim data

A. Fraud victimization

Consider one fraudster and one individual. Fraud victimization consists of four stages, as depicted in Figure 1 (recruitment fraud is used as an example here, but the framework applies to other types of fraud, such as consumer fraud). Stage 1 occurs prior to the fraudster's fraud attempt. Stage 2 begins once the fraudster targets the individual; he/she becomes an attempted victim before he/she knows. The individual's decision

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[9] Recruitment fraud as a pitfall in international labor migrations is particularly relevant in Pacific island states, which are often dependent on them (Bertram 1986; World Bank 2006a). In contrast with extensive ethnographic studies by anthropologists, systematic economic works using micro survey data are very scant in the region.

[10] Following seminal works by Becker (1974), Barro (1974), and Cox (1987), economists have extensively studied household private transfers. Numerous studies examine motives for migrants' remittances and their consequences (e.g., Lucas and Stark 1985) (see Rapoport and Docquier 2006 for a review). My inquiry is closely related to the crowding-out of private transfers (see Cox and Fafchamps 2008 for a review).

involves a tradeoff – taking an attractive job offer requires an advance payment. Stage 2, during which the attempted victim can make a decision, is usually short, because the longer the wait, the higher the risk of the fraudulence being detected. Stage 3 begins once the individual's decision determines his/her *expected* status; only if he/she takes the fraudster's offer, the status changes to an attractive one (such as employment) and the fraudster's status changes correspondingly – successful or unsuccessful attempt. Stage 4 begins once the fraudulence is detected; [11] the individual becomes aware of his/her *realized* status – victim or non-victim. Fraud is sharply distinguished from other crimes by this gap between expected and realized status; in other crimes, stages 2 and 3 are nonexistent and ex ante impacts/victim data are irrelevant. This gap is also irrelevant for the fraudster (the shift to stage 4 alters risk of being captured though).

Ex ante and ex post impacts of fraud victimization are realized in stages 3 and 4, respectively. Whether ex ante impacts matter and ex ante victim data at least theoretically can be collected strongly depend on stage 3's duration, which can significantly vary. On the one hand, it can be virtually instant (e.g., online consumer fraud); then, ex ante impacts are nonexistent and there is virtually no chance to collect ex ante victim data. On the other hand, if the fraudulence remains unknown, stage 3's duration is infinite and thus only ex ante impacts matter; however, victimization per se is never noticed (stage 4 does not emerge). In practice many frauds are in between these two extremes.[12]

B. Ex post vs. ex ante victim data

Surveys that collect crime-victim data are either victim surveys or household surveys with a victimization module. They have distinct potential advantages in collecting ex post fraud-victim data in stage 4; although a victim survey on one specific fraud (i.e., no fraud heterogeneity) enables a sharp identification of determinants of fraud victimization,[13] a household survey can capture its broad impacts.

[11] In practice, the transition from stage 3 to stage 4 may be gradual as the attempted victim's information and/or suspicion grows. The timing of transition also can vary among attempted victims and between them and others (e.g., news media).
[12] Although ex ante impacts may be persistent in stage 4, it is very difficult to distinguish them from ex post impacts, because both are caused by the same decision.
[13] Although a small number of victims of a specific crime often make statistical analysis infeasible, a particular fraud may involve a large number of victims; however, criminologists' studies often cover only victims, lacking a non-victim sample (e.g.,

The ex ante victim survey to be conducted in stage 3 is totally different from the ex post survey; standard questions about victimization do not work, simply because respondents have not yet become aware of their realized status. The key is whether any of the household survey's modules other than victimization capture respondents' expected status. The following three designs are possible:

1) *Passive design*: A household survey is accidentally conducted in stage 3, and this is noticed only after the survey was completed.
2) *Proactive design*: Researchers or professionals conduct a household survey on potential fraud, hoping that it covers stage 3.
3) *Reactive design*: Researchers or professionals who conduct a household survey get suspicious about fraudulence by chance, *before* attempted victims do so (stage 3), and they add questions about the potential but concrete fraud.

Although all these three designs rely heavily on chance (as any natural experiment does), they have distinct strengths and weaknesses as follows.

The passive design in principle can be applied to any standard household survey. It depends on pure luck not only to encounter the fraud at the "right" time, but also to accidentally capture fraud-related data, along with fraud information that can be matched with the survey data (e.g., identification of victims). Although the odds that a particular survey satisfies these conditions are negligible, there may exist such surveys somewhere in the world. All that is needed is to search accidental data collection among available household surveys by using specific widespread frauds as a marker and to pay attention to such a possibility in future surveys.

The proactive survey captures household behaviors corresponding to potential frauds, about which only incomplete information is available (e.g., the authorities'). For example, in the case where recruitment agencies' fraud is known to be prevalent, the survey can ask questions not only about realized migration (e.g., migrant's attributes, remittances), but also about household efforts to send a migrant laborer, including non-

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Trahan, Marquart, and Mullings 2005). Large-scale general victim surveys (e.g., ICVS) potentially capture not only fraud victims and non-victims, but also individuals on whom fraud attempts were not made (not shown in Figure 1) (e.g., Titus, Heinzelmann, and Boyle 1995); this is also the case for large-scale household surveys (e.g., LSMS) if a fraud-victimization module is added.

materialized ones.[14] The proactive design is effective only in fraud-prone locales. With a risk of missing stage 3 and fraud-related data, a cross-cutting design for other purposes (e.g., realized migration) is recommended.

Compared to the proactive design, the reactive survey using richer prior information about potential fraud has significant advantages: The probability of covering stage 3 is much higher, survey questions can be more concrete and focused, and it can be applied in any locale (it is a cross-cutting design by definition). The possibility of using a reactive design is more restrictive, however, for the following reasons. First, it strongly relies on researchers' and practitioners' flexibility. It probably fits best in researchers' small-scale surveys with limited institutional constraints. Second, it depends on researchers' strong sensitivity and luck to notice a weak sign of the potential fraud earlier than attempted victims do. Third, researchers must be truly unsure of their suspicion; otherwise, they instead should report it to the authorities.

C. Victim endogeneity

To identify ex ante and ex post impacts of endogenous fraud victimization, researchers can use IVs that determine the attempted victim's decision in stage 2, but do not directly influence the outcome of interest in stages 3 and 4, respectively.[15] Good candidates for IVs include factors that alter the attempted victim's perception about the fraud attempt and/or the benefit-cost calculation in his/her decision making. This is especially so for an ex ante impact analysis, because the attempted victim has not yet considered such factors as those determining his/her realized status.

In practice, some information about frauds and/or fraudsters needs to be collected, and tradeoffs exist between ex post and ex ante surveys. On the one hand, in the ex post survey, researchers who can identify realized frauds (and maybe also fraudsters) can collect detailed, specific data; they need to rely on respondents' retrospection about stage 2, however, which might involve considerable misreporting, as discussed above. On the

[14] It is even possible to design a panel survey with many waves with short intervals – to be set depending on stage 3's duration – hopefully covering different victimization stages of the same household. Such detailed data are extremely useful to dissect the mechanism of fraud victimization.

[15] Alternatively, a natural experiment such that attempted victims' decisions are considered to be almost random with respect to the outcome of interest can be looked for, especially in the passive design.

other hand, although such misreporting is not a major concern in the ex ante survey, researchers employing the proactive design, with limited information about specific frauds, necessarily rely on less focused questions; this problem is smaller in the reactive design using richer prior information.

III. Fijian recruitment fraud and ex ante victim data

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. With a large pool of former army personnel, Fiji has been a major labor supplier. Even though some casualties had been reported, the Fijian government welcomed this movement as a solution for its unemployment. In late 2004 and early 2005, a Fijian recruitment agency conducted the largest recruitment drive in the country. All healthy males between the ages of 18 and 60 were eligible to apply. According to news media, the agency collected US$2 million fees from at least 20,000 applicants.

In June-September 2005, I conducted a survey among 906 randomly selected households in 43 native Fijian villages in Cakaudrove Province, which was extensively covered by the agency's recruitment drive.[16] The sampling design was not affected by job applications at all.[17] The survey was designed to collect information about demographics, assets, production, income, and transfers (but not consumption). While I was pretesting the questionnaires in May 2005, I became aware of the unusual recruitment drive and quickly added questions about job applications. I was not very suspicious about potential

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[16] Fiji is divided almost evenly between native Fijians and Indo-Fijians. My study focuses on native Fijians. The fraudulent recruitment agency is staffed by only native Fijians and its recruitment drive in rural areas covered native Fijian villages only on the three largest islands in the country – Viti Levu, Vanua Levu, and Taveuni. Cakaudrove 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.

[17] There are 5,117 households in 134 native Fijian villages in the province (in the population). In each of 16 districts, villages were intentionally chosen to cover distinct environmental and economic conditions, before I became aware of the recruitment drive. In each village, households were stratified by a smallest kin-group unit (sub-linage or extended family, locally known as tokatoka, Ravuvu 1983) and the combination of leadership status (e.g., kin leaders) and major asset holdings (e.g., shops); in each stratum, households were randomly sampled.

fraudulence. During interviews several months after people's application decisions, respondents were still unaware of the fraudulence (stage 3). In this way, I accidentally implemented the reactive design. This household survey with a module on one specific potential fraud combines advantages of the different types of surveys discussed above. In particular, as victims were defrauded of similar amounts of money, heterogeneity in the severity of victimization is very small, further sharpening the identification of determinants and impacts of victimization.

As I became more aware of this recruitment agency (especially through an interview with its employees), my suspicion about potential fraudulence grew during the survey. Right after the survey was completed, news media started to report its fraudulence, as most applicants got neither a job nor a refund (stage 4 began). At that time, the agency's director, who originated from the study region, had already left the country.[18] Unfortunately, the Fijian coup in December 2006 precluded me from conducting the ex post victim survey I had planned. As such, quite distinct from extant fraud studies on ex post victim data, I collected only ex ante victim data.

IV. Determinants of fraud victimization

A. Descriptive statistics

The empirical analysis covers 1,247 males aged between 18 and 60, who were eligible for the jobs offered by the recruitment agency; this sample includes 787 households (87% of the whole sample) in all 43 sample villages (see panel A of Table 1).[19] All these eligible individuals/households are attempted victims; the fraud attempt was at least indirectly made on all individuals in the region, because individuals in villages uncovered by the recruitment drive were privately informed of the job opportunity (which villages in the sample were directly covered is unknown). No respondents had difficulty answering questions about their job applications; 238 individuals (19% of the eligible) of 212 households (27% of households with eligible

[18] According to news media, the director returned to Fiji in late 2009, and a police investigation is still in process at the time of this writing. Thus, fraud victims examined in this paper are not legally "fraud victims."

[19] The mean monthly household pre-transfer income – including a very small amount of public transfers (mostly pension) – is F$1,703, or F$290 per capita. Farming accounts for two thirds of total income, followed by fishing, handicrafts, and permanent wage labor.

members, or 23% of the whole sample) applied (panel B). Although 36% of households 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.

In order to apply, an average applicant spent F$230 (F$1 = US$.60), and an average victimized household spent F$242 (close to the mean monthly per-capita income).[20] 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 (with a population of about 3,000) in the province, and many revisited to check their status (most sample villages lack phone access).[21]

B. Econometric specification

Who was vulnerable to this recruitment fraud? I estimate determinants of job application among eligible individuals and households, using the following models:

$$d_{ihv} = \alpha_0^1 + \boldsymbol{\alpha}_{11}^1 \boldsymbol{x}_{ihv}^1 + \boldsymbol{\alpha}_{12}^1 \boldsymbol{x}_{ihv}^2 + \boldsymbol{\alpha}_2^1 \boldsymbol{x}_{hv} + \boldsymbol{\alpha}_{31}^1 \boldsymbol{x}_v^1 + \boldsymbol{\alpha}_{32}^1 \boldsymbol{x}_v^2 + \phi_v + e_{ihv}, \tag{1.1}$$

$$d_{hv} = \alpha_0^2 + \boldsymbol{\alpha}_{11}^2 \boldsymbol{x}_{hv}^1 + \boldsymbol{\alpha}_{12}^2 \boldsymbol{x}_{hv}^2 + \boldsymbol{\alpha}_2^2 \boldsymbol{x}_{hv} + \boldsymbol{\alpha}_{31}^2 \boldsymbol{x}_v^1 + \boldsymbol{\alpha}_{32}^2 \boldsymbol{x}_v^2 + \phi_v + e_{hv}, \tag{1.2}$$

where i, h, and v stand for individual, household, and village, respectively; d_{ihv} and d_{hv}, respectively, are a dummy variable for job application for eligible individual i and any eligible member of household h; $\boldsymbol{x}_{ihv}^1$ and $\boldsymbol{x}_{ihv}^2$, respectively, are a vector of individual factors for individual i and household members other than individual i; $\boldsymbol{x}_{hv}^1$ and $\boldsymbol{x}_{hv}^2$, respectively, are a vector of eligible and non-eligible members of household h; $\boldsymbol{x}_{hv}$ is a vector of household factors; $\boldsymbol{x}_v^1$, and $\boldsymbol{x}_v^2$, respectively, are a vector of village factors unrelated and related to the fraudster; ϕ_v is unobservable village factors that determine job application; and e_{ihv} and e_{hv} are residuals. Following the migration literature, potential migrants and other members staying home are differentiated. Equations (1.1) and (1.2) are estimated by a linear probability model (LPM) and probit.

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[20] Simple extrapolation suggests that approximately 1,540 individuals and 1,300 households were victimized across the whole province, and the total loss reached over F$350,000, which is comparable to media reports.

[21] At the time of interviews, 80% of applicants were still waiting for the result, 10% were accepted, 8% were rejected, and 2% withdrew their application. None of the accepted applicants got the job. I repeated all regression analyses for applicants whose status was still unknown, finding results very similar to those presented below.

The selection of potential determinants follows the literature on both migration and crime victimization.[22] All explanatory variables are listed in Table 2, along with their descriptive statistics. First, individual factors capture demographics (headship and age),[23] health (disability), education (adults' secondary education), and employment (permanent wage labor).[24] Good health is an eligibility criterion for the job other than sex-age. Permanent employment, the opportunities for which are very limited and mostly in the public and tourism sectors, is considered exogenous, because individuals cannot flexibly adjust it (its status did not change at all after application decisions were made); dropping the employment variables significantly alters none of the remaining results (this is also true for the models discussed in Sections V and VI).

Second, household factors capture demographics (female headship, age of household head, and the size of four cohorts) and assets (agricultural land), but not

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[22] Sociologists categorize determinants of victimization into exposure, guardianship, attractiveness of potential targets, and proximity to potential offenders (Cohen, Kluegel, and Land 1981). The first three are often captured by demographic factors, police access, and asset/income, respectively; clear categorization is not always possible – for example, some measures, such as age, capture both exposure and attractiveness (e.g., Barslund et al. 2007). Guardianship matters relatively little for fraud victimization; as police stations in the study area are located with close proximity to local markets, police access is captured by market access anyway. Proximity to potential offenders is directly captured by the proximity measures defined below. Extant studies of consumer fraud victimization also highlight the role of cognitive deficiency and social interaction/status as determinants of victimization (Lee and Soberon-Ferrer 1997). The former is captured by education, age, and health. As kin-based hierarchy plays a central role in villagers' lives in Fiji (Turner 1992), I added kin-status variables as covariates, finding no significant results (household private transfers discussed later also are neutral to kin status, Takasaki forthcoming). Determinants of migration include demographic factors, assets, location, and migrant network (e.g., Munshi 2003). Although the Fijian data lack information about a migrant network in the Middle East, the next section gives evidence that it plays a limited role.

[23] Some criminologists find that younger people are more likely than the elderly to be victimized by consumer frauds, challenging the common belief among practitioners and professionals that the elderly are the main victims (Titus, Heinzelmann, and Boyle 1995). Van Wyk and Mason (2001) attribute this to distinct socialization and risk-taking among different cohorts. The analysis below shows that job application is neutral to age.

[24] Some variables defined for eligible individuals (eligible household members) are not used for other household members (non-eligible household members), because they are redundant with similar household factors defined next. A potentially important individual factor that the present data cannot capture is military experience; however, anecdotal evidence suggests that such individuals are not so common in the sample. Indeed, households with a member currently working in the military are rare.

income. Though income is often considered as an important determinant of crime victimization, it is endogenous as a determinant of job application for the following reasons: 1) in anticipation of labor migration and remittances, the household may adjust its earning efforts, and 2) any unobservable 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 permanent wage labor (of any household members) and land holdings, as well as household demographic factors.[25] Third, village factors unrelated to the fraudster capture market access (travel time to the closest local market), village size (total number of households in the village, in the population), welfare, and inequality (village mean and standard deviation of agricultural land in the whole sample).[26]

I conjecture that two eligibility factors – sex-age and health – are significant determinants. Specifically, households with more eligible male adults are more likely to apply (*endowment effect*); individuals in households with more eligible members are less likely to apply, because others can apply (multiple applications from the same household are rare, as discussed above). Disabled individuals are less likely to apply. I also conjecture that own and other family members' permanent employment discourages job application, because of the high opportunity cost of migration and its relatively low benefit with secure employment in the family, respectively. If victimization is associated with low welfare, not only permanent employment – both own and others' – but also land holdings should discourage job applications. Nonsignificant results suggest that welfare difference *within the sample* does not differentiate victimization; that is, most households are so poor that they were potentially attracted by this job opportunity.

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[25] Land is communally owned by a within-village clan (locally known as *mataqali*, which consists of several *tokatoka*), is privately used, and by law cannot be sold (about 83% of the country's total land is communal). Land holdings at the time of the fraud attempt should be almost the same as those at the time of interviews used here. Information about non-land asset holdings and shocks such as sickness (other than chronic illness captured by disability status) prior to the fraud attempt is lacking, however (only information about sickness in the past one year is available). Thus, the model cannot tell whether job applications responded to transitory shocks.

[26] Many extant studies highlight inequality as a cause of crime (e.g., Bourguignon 2000).

Last, village factors related to the fraudster capture the physical and social distance between the fraudster and attempted victims. The physical distance – measured by villagers' travel time to the recruitment agency's office – determines the travel cost of lodging an application, as well as the agency's recruitment drive (indeed, there were no applicants in six remote villages in the sample). The social distance is captured by the dummy for two villages in the home district of the agency's director.[27] The director had built a good reputation there by offering casual labor employment and making school donations (he was viewed as a hero).[28] I conjecture that as attempted victims become physically and socially closer to the fraudster, they are more likely to apply, because applying is less costly and they are more positive about the job (as confirmed in Section V). These proximity measures serve as excluded IVs in Section VI.

In an alternative specification, all village factors, including ϕ_v, are fully controlled for by village dummies. Similar results of the individual/household factors with village dummies and observable village factors, as reported next, give evidence that unobservable village factors are unlikely to cause bias in the latter specification.

C. Estimation results

LPM results are reported in Table 3, where robust standard errors are shown in the models with village dummies and standard errors are clustered by village (43 clusters) in the models with observable village factors (probit estimates are very similar). First, eligibility – sex-age and health – and proximity to the fraudster are strong determinants.

[27] It is assumed that these two proximity measures, as well as other covariates, are uncorrelated with unobservable village factors ϕ_v. Although the recruitment drive can be related with such factors (the fraudster may have prior information about good targets), the physical distance and the director's birthplace are not. An exception for the latter may be found in large cities with strong, crime-driven segregation, where future offenders commonly originate from certain areas. Rural Fiji is far from such a case.

[28] Two pieces of evidence strongly indicate that discounted fees were charged in the director's home district. First, the mean cost for application per victimized individual – including transportation – in the director's home-district villages was about half of that elsewhere (F$125 vs. F$263). Second, regressing the application cost on the two proximity variables among victimized individuals shows that it is lower in the fraudster's home district by F$125 and becomes higher with greater distance (F$31 per 100% increase); both results are significant at least at a 1% significance level and the R squared is .24. The next section gives evidence that the home-district dummy captures the social distance more strongly than discounted fees.

In particular, the probability of applying for a job in the fraudster's home district is higher by about 34% and a 100% increase in the travel time to the fraudster's office lowers the probability by .08-.10 (no other village factors have significant effects). Second, although individuals in the household with a permanent wage laborer are less likely to apply (the results in the household-level analysis are statistically weak), neither eligible individual's own employment status nor household land holdings are significant (all other factors are also nonsignificant). Thus, what matters is not one's own opportunity cost of migration, but secure employment in the family, and households at any welfare level are vulnerable.

V. Reasons for fraud victimization

A. Descriptive statistics

The survey asked each applicant why he applied (up to three reasons) and each household with no applicants why none of its eligible members applied (up to three reasons) (panels B and C, respectively, of Table 1).[29,30] While almost 80% of applicants were attracted by high salary, many households did not apply because of a concern about qualifications and high fees. While security mattered for 14% of non-applicant households, almost 20% of them questioned the contract's authenticity; that is, about 12% of households in the whole eligible sample took precautions to avoid victimization. Job type and location (abroad) were also relatively common reasons for both application and non-application; that is, individual job preference mattered in the opposite way. In contrast, alternative activities and acquaintance with someone who already had gotten the same job were uncommon reasons for both application and non-application, reflecting the small opportunity cost of migration, as found above, and a limited migrant network in the Middle East (in another words, the recruitment agency was the only network available).

B. Econometric specification

To examine which reasons mattered for whom, I estimate determinants of reasons for application among applicant individuals and reasons for non-application among non-applicant households, using the following models:

[29] Directly asking reasons in this manner is effective only in the ex ante victim survey; misreporting is most likely in the ex post victim survey. It also much better works for a victim survey on a specific fraud than one covering various frauds.
[30] Panel B also shows reasons for applying at the household level. Not surprisingly, the results are almost the same as the individual-level results.

$$p_{ihv}^{j} = \alpha_0^{1j} + \boldsymbol{\alpha_{11}^{1j} x_{ihv}^{1}} + \boldsymbol{\alpha_{12}^{1j} x_{ihv}^{2}} + \boldsymbol{\alpha_{2}^{1j} x_{hv}} + \boldsymbol{\alpha_{31}^{1j} x_{v}^{1}} + \alpha_{32}^{1j} x_v^2 + \phi_v^j + e_{ihv}^j \text{ if } d_{ihv} = 1, \quad (2.1)$$

$$p_{hv}^{j} = \alpha_0^{2j} + \boldsymbol{\alpha_{11}^{2j} x_{hv}^{1}} + \boldsymbol{\alpha_{12}^{2j} x_{hv}^{2}} + \boldsymbol{\alpha_{2}^{2j} x_{hv}} + \boldsymbol{\alpha_{31}^{2j} x_{v}^{1}} + \alpha_{32}^{2j} x_v^2 + \phi_v^j + e_{hv}^j \text{ if } d_{hv} = 0, \quad (2.2)$$

where p_{ihv}^{j} and p_{hv}^{j}, respectively, are a dummy for the reason j for individual i's application and household h's non-application (three and six most common reasons discussed above are considered).[31] Equations (2.1) and (2.2) are estimated by LPM and probit. All explanatory variables in equations (2.1) and (2.2), respectively, are the same as those in (1.1) and (1.2), with an exception that the fraudster's physical distance is not included in x_v^2 (which is a scalar). The analyses including the fraudster's physical distance as a control show that it has no significant effects; that is, although it strongly determines individuals'/households' application decisions, as found above, it does not affect the reasons for their decisions.[32] Excluding this variable does not significantly alter results of the remaining variables, either. These subsample analyses involve potential selection bias, because application decisions are endogenous. I estimate a selection probit model (Greene 2000, p.857), where the sample-selection equations for equations (2.1) and (2.2), respectively, are (1.1) and (1.2) with the fraudster's physical distance as an excluded variable for identification. Results are very similar to the probit estimates.[33]

C. Estimation results

LPM estimates with observed village factors are reported in Table 4, where standard errors are clustered by village (probit estimates are very similar).[34] The following results buttress earlier findings on determinants of job application. First, as making a job application is neutral to welfare level, high salary is attractive at every

[31] The regression analysis of reasons for applying at the household level – equation (2.2) for $d_{hv} = 1$ – yields very similar results to the individual-level results.
[32] The nonsignificant result for high fees as a reason for non-application may seem counterintuitive. However, what the survey asked about is application fees unrelated to the physical distance, not application costs including transportation (application fees are separately controlled for by the fraudster's home district dummy, as discussed above).
[33] Estimated covariance of error terms in the selection probit is not significantly different from zero in most cases; in the few cases where it is significant, the estimated coefficients are very similar to the probit results. Thus, selection bias is unlikely to be a major concern.
[34] In equation (2.1), the dummy for eligible individual's disability and the dummy for female head are dropped, because their variations in the subsample are limited and they perfectly predict some reasons.

welfare level (measured by permanent wage labor and land holdings); at the same time, the poor do not apply because of high fees. Second, applicants in the fraudster's home district are more likely to have been attracted by the job type and location than others were; non-applicants there are less likely to have been concerned about qualification, job type, and security. Thus, the fraudster's reputation altered peoples' perceptions about the prospective job, as hypothesized above.[35] Third, households with more eligible males are less likely to be cautious (precaution is neutral to other factors). That is, the endowment effect shapes victimization, partly because it alters people's taking precautions.[36]

VI. Ex ante impacts of fraud victimization on household private transfers

A. Descriptive statistics

Most households in the eligible sample had participated in private transfers in the past one month (i.e., several months after their job application decision) (see Table 5).[37] Household private transfers consist of cash, inkind, and labor time; in the calculation of total transfers, labor time is monetized using the mean daily wage of casual labor in each village (it ranges from F$10 to F$20, with a median of F$15). The sample means of

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[35] The dummy for the fraudster's home district is significant for neither high salary nor high fees, suggesting that it is likely to capture the social distance more strongly than discounted fees.

[36] The following findings about people's preferences are obtained. First, security concerns are stronger in a village with smaller land endowment and larger inequality. This indicates that land distribution shapes risk preference (when village dummies are used as controls, disability status, permanent employment, and the number of eligible males, which show significant results in Table 4, lose their statistical significance). Second, the job type is attractive to applicants (with small land holdings) in a small village, it is not attractive to non-applicants in a large village with large land holdings, and the job location is attractive for applicants in a small village with small land holdings. This suggests that village economic opportunities shape preferences for job type and location. Other significant results are as follows: non-applicant households with an old head are more likely to be concerned about qualifications; households with a disabled non-eligible member are more concerned about security; and high salary strongly attracts households with a disabled member.

[37] Respondents were asked about each major transfer received from and given to other households in the past month and then in the past year. Although the latter annual transfer data contain transfers made in part of stage 1, all of stage 2, and part of stage 3, these three cannot be distinguished from each other (the month when transfers were made is unknown). The ex ante impact analysis in this section focuses on transfers in the past month (stage 3). Informal loans were much less common and much smaller than gift transfers examined here.

monthly gross private transfers received and given are F$141 and F$104, respectively, the difference of which mainly comes from across-village cash and inkind transfers. Specifically, although most households participated in within-village transfers and the amounts received and given were almost balanced, across-village transfers received were more common and larger than those given; most across-village transfers were cash and inkind (exchanged through kin network Takasaki forthcoming), while labor-time transfers consisted of one quarter of within-village transfers. Almost all across-village transfers were domestic ones; distinct from smaller island states, such as Tonga, where overseas migrations and remittances are common (Bertram 1986), international transfers were negligible among Fijians in the sample.[38]

B. Econometric specification

To identify how job application d_{hv} affects household private transfers (ex ante impacts), I employ the following endogenous dummy variable model:

$$y_{hv} = \beta_0 + \beta_1 d_{hv} + \boldsymbol{\beta_1 x_{hv}^{12}} + \boldsymbol{\beta_2 x_{hv}} + \boldsymbol{\beta_{31} x_v^1} + \rho_v + \varepsilon_{hv}, \tag{3}$$

$$d_{hv} = \alpha_0 + \boldsymbol{\alpha_1 x_{hv}^{12}} + \boldsymbol{\alpha_2 x_{hv}} + \boldsymbol{\alpha_{31} x_v^1} + \boldsymbol{\alpha_{32} x_v^2} + \sigma_v + \omega_{hv}, \tag{4}$$

where y_{hv} is the amount of net transfers received (gross transfers received minus gross transfers given) by household h in the past month; $\boldsymbol{x_{hv}^{12}}$ is a vector of individual factors for any household members summarized in Table 2; ρ_v and σ_v are unobservable village factors that determine private transfers and job application, respectively; and ε_{hv} and ω_{hv} are residuals.[39] For the same reasons given above, observed earned income is not

[38] In contrast, according to the household survey conducted in five major towns and nine villages in the main island Viti Levu in 2005 by the World Bank (2006a), 26% of 211 native Fijian households had overseas migrants and 34% received overseas remittances. This indicates a potentially significant difference in Fijians' transfer patterns between the main island and other islands and between urban and rural areas. The World Bank (2006a) also attributes the survey result to growing international labor migration, especially to the Middle East, emphasizing its importance. This paper's finding on recruitment fraud highlights a significant pitfall in this development path and corresponding policy making.

[39] This model can also estimate ex post impacts of endogenous fraud victimization. With a lack of ex post victim data, I cannot examine how households – both victims and non-victims – made readjustments in their private transfers after becoming aware of the fraudulence (in stage 4).

included as a household factor.[40] Note that $\boldsymbol{x}_{\boldsymbol{hv}}^{\mathbf{12}}$ is the only difference in equation (4) from (1.2).

The substitution effect indicates that domestic private transfers are reduced by prospective remittances, i.e., negative β_1 (constant effect is assumed). The endogeneity of household application decision d_{hv} is apparent: It is influenced by unobservable factors such as preference and skills in ε_{hv} that determine private transfers. Since households that tend to send a migrant laborer are likely to be those that rely more on private transfers, the OLS estimates for β_1 are expected to be biased *upward*. The identifying assumption for the IV estimation is that proximities to the fraudster $\boldsymbol{x}_{\boldsymbol{v}}^{\mathbf{2}}$ affect private transfers only through job application; that is, it is uncorrelated with any unobservable village factors ρ_v, as well as unobservable household factors ε_{hv}. Moffitt (1996) clarifies the assumptions necessary for the two-stage least squares (2SLS) estimator to represent a causal effect of an endogenous dummy variable at the individual level with an aggregate-level IV (the same is illustrated by Wooldridge 2002, pp. 133-134).

First, preferential treatment in the fraudster's home district was made, not because of village characteristics that could be related with unobservable village factors affecting private transfers, but because of the place of his birth. Still, if ρ_v is different between the fraudster's home district and other districts in the absence of the fraud attempt, the exclusion restriction does not hold. With a lack of transfer data prior to the fraud attempt (stage 1), showing direct counterevidence against this possibility is infeasible. A piece of indirect counterevidence is that self-reported measures of village social problems, which serve as proxies for unobservable social ties determining private transfers, are not significantly different across the districts.[41]

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[40] With a lack of valid IVs, controlling for the income endogeneity is infeasible (cf. Jensen 2004; Juarez 2009; Kazianga 2006). In equation (3), permanent income is captured by a set of controls, but transitory income is not, as discussed above.

[41] Villagers' social ties not only directly determine within-village transfers, but also indirectly influence across-village transfers; for example, weak social ties in the village may be associated with weak or strong household transfer networks out of the village. The social-problem measures are according to village leaders' subjective assessments about low attendance at communal activities/ceremonies, decreasing sharing practices, and conflicts among households/villages (incidence was reported by 53%, 37%, and 26%, respectively, of the sample villages).

Next, the identifying assumption for the fraudster's physical distance does not hold if the agency's location coincides with other facilities that influence private transfers or other households with whom distinct across-village transfers are made. The former include postal offices where villagers can receive/send cash and inkind from/to cities. Most postal offices are located closely to local markets, the access to which is separately controlled for. The latter transfers exchanged with households in the town are not significantly different from those in other locations. The social-problem measures discussed above are also uncorrelated with the fraudster's physical distance. An assumption that x_v^2 is uncorrelated with ε_{hv} is supported analogously.

I employ three specifications of 2SLS: an overidentified model with the fraudster's social and physical distance as excluded IVs and just-identified models 1 and 2 with the former and the latter, respectively, as a unique excluded IV. Although the over-identified model is not unbiased in small samples, the just-identified models are approximately median-unbiased. I also estimate the limited information maximum likelihood (LIML) estimator for the over-identified model, which is less precise than 2SLS, but less biased (under constant effects) (e.g., Davidson and MacKinnon 1993; Mariano 2001). Standard errors are clustered by village (Shore-Sheppard 1996 highlights the importance of this cluster adjustment with an aggregate-level IV).

Two extensions are made to further test the substitution effect. First, I compare gross transfers received and given. If potential donors respond to prospective remittances, households with an applicant should receive less gross transfers than others do.[42] Second, I compare within- and across-village transfers received. Since international remittances are the more "direct" substitute for the latter cash and inkind transfers, job application should reduce them more than the former.[43]

[42] If households received more transfers to finance their applications several months ago, they are likely to have received smaller transfers in the past month, even in the absence of the substitution effect, and may offer larger transfers for reciprocation. Almost no respondents, however, reported job application as a reason for the transfers they received in the past year, which include those received in stage 2. Consumption and ritual (funeral, wedding, and other ritual events) were the most common reasons for transfers made in both the past month and the past year (Takasaki forthcoming).

[43] While an average household in the sample is a net recipient of across-village transfers, within-village transfers are balanced between receiving and giving. Then, reducing

Distinct from gross within-village transfers received, many households are not recipients of across-village transfers (Table 5). I first employ 2SLS for amounts with many zeros. This model captures the combination of impacts on receipt and amount received conditional on receipt, without making any distributional assumptions (Angrist 2001).[44] Next, to identify the impact on receipt, I employ 2SLS with a dummy for receipt of transfers as a dependent variable in equation (3).

C. Estimation results

The estimated coefficients (marginal effects) of job application (β_1) are reported in Table 6. Column (1) shows results for net transfers received, with no exogenous covariates – $\boldsymbol{x_{hv}^{12}}$, $\boldsymbol{x_{hv}}$, and $\boldsymbol{x_v^1}$. In all cases, the excluded IVs strongly determine job application in the first-stage equation (4) (as in equation 1.2 above). The fraudster's social distance is a much stronger determinant than the physical distance (53.1 vs. 13.9 of F value); when these two are jointly used, the F value is 26.6 and the overidentification test (Hansen J statistic) does not reject a null hypothesis that they are uncorrelated with the error term in equation (3). Although the OLS estimates of β_1 are negative and small in magnitude with no statistical significance, the 2SLS estimates are much larger in magnitude with strong statistical significance in all specifications, and their point estimates across specifications are in a similar range; in particular, the LIML estimates are almost the same as the 2SLS estimates.[45] When the exogenous covariates are added (column 2), the estimation results for β_1 do not significantly change (those of the exogenous covariates – for the OLS and the over-identified 2SLS – are reported in the

transfers to households in the village is probably less costly for potential donors outside the village, who rely less on transfers received from them, than those in the same village.

[44] Although a two-part model is commonly used in empirical works on private transfers, the identifying assumption does not hold in 2SLS conditional on positive (receipt), because excluded IVs affect participation (Angrist 2001). A tobit model relies on the assumption of the normally distributed error term in equation (3), against which counterevidence is found below. For the robustness check, I analyze not only the level of gross transfers received, the results of which are shown below, but also their natural log, finding qualitatively the same results.

[45] Table 6 also reports results of a reduced-form analysis of equation (3), where the endogenous variable d_{hv} is replaced with the excluded IVs $\boldsymbol{x_v^2}$. Their estimated coefficients take the expected signs and are statistically significant (the fraudster's physical distance in the overidentified model is significant at almost a 10% significance level).

Appendix).[46] These results buttress the robustness of the estimated marginal effects of the IV regressions. Households with a job applicant receive smaller net transfers by F$139-171 – a range comparable to the mean gross transfers received – than others do; in contrast, the OLS estimates ignoring victim endogeneity are strongly biased upward.

The estimation results of gross transfers received and given strongly confirm my conjecture (columns 3-6). According to the 2SLS and LIML estimates, the estimated marginal effects of job application on gross transfers received are very close to those on net transfers received, and those on gross transfers given are very small and statistically nonsignificant;[47] in contrast, only the latter results are significant in the OLS estimates.

The estimated marginal effects of job application on gross within- and across-village transfers received are reported in columns (2)-(4) of Table 7.[48] As I conjectured, prospective remittances more strongly reduce across-village transfers than within-village transfers. In particular, while results of within-village transfers lose their statistical significance with the exogenous covariates added (panel B), those of across-village transfers do not significantly change; the latter results are very similar to each other across specifications. The probability of receiving across-village transfers for households with a job applicant is almost .5 lower than others',[49] and they receive smaller amounts by about F$100. These results suggest that the significant reduction in total gross transfers received (column 1, which replicates the results in columns 3 and 4 of Table 6)

[46] The estimated coefficients of the exogenous covariates are very similar to each other between the OLS and 2SLS estimates, buttressing their exogeneity (Appendix). Although households with a disabled member are big recipients, those with permanent employment and large land holdings (i.e., high permanent income) are big donors.

[47] The corresponding reduced-form results are statistically significant only for gross transfers received. Results of the just-identified model 2 are weak: The estimated β_1 becomes much smaller with the exogenous covariates added, losing its statistical significance at a conventional level, and the corresponding reduced-form results are also statistically nonsignificant; in contrast, results of the other IV regressions are very similar to each other. Since these results suggest that the fraudster's physical distance is a relatively weak IV, the remaining analyses exclude the just-identified model 2.

[48] In all specifications, the (joint) significance tests for excluded IV(s) show strong results and overidentification tests are satisfactory.

[49] The bivariate probit estimate is considerably smaller (about .3 marginal effect), questioning the assumption of the bivariate normal distribution of the error terms.

comes mainly from the termination of receipt of across-village transfers.[50] Overall, these results suggest that a reduction in the private transfers that victims received during all of stage 3 (several months) was greater than the money they lost to apply for the job.

VII. Conclusion

Fraud studies rely on potentially underreported/misreported victim data in developed countries, virtually ignoring developing countries. This paper proposed collecting victim data before attempted victims become aware of the fraudulence (ex ante victim data), and examined recruitment fraud in rural Fiji. Almost one quarter of households were defrauded of application fees for labor migration. Although all households at any welfare level are vulnerable to victimization (all are poor), households without secure domestic employment, with more eligible members (male healthy adults), and with "close" proximity to the fraudster (which shapes their perceptions and benefit-cost calculation regarding the job opportunity) are more vulnerable.

Although defrauded money among the poor is not a large amount, the much bigger problem is that people's behaviors are adversely altered by their false expectations induced by the fraud (ex ante impacts). In particular, recruitment-fraud victims lost domestic private transfers they could have received from other households outside the village, as a result of the substitution for prospective international remittances. This is one example of the potentially broad adverse effects of fraud victimization. Fraud in productive activities may further persistent poverty and underdevelopment (i.e., fraud-induced poverty traps). Then, poverty alleviation as a long-run solution for victimization – especially through better domestic employment – is stuck. Controlling for victim endogeneity by using instruments – such as proximity to potential fraudsters – is crucial to identify fraud impacts; failing to do so gives rise to strong bias.

Is fraud prevalent in other developing areas? As a very basic understanding is lacking, conventional fraud-victim data first need to be collected in developing countries.

[50] Consider a counterfactual with no job application (victimization). The mean amount of gross transfers received in the whole sample would be F$48 (actual means) + 27% (proportion of victimized households) * F$100 = F$75. The mean amount in the case where only the proportion of recipients changes, with no change in the mean amount received among recipients, would be (F$48 / 30% (actual proportion of recipients)) * (30% + 27% * 50%) = F$70.

The recent expansion of data collection about consumer fraud (e.g., ICVS) is important, but misses bigger problems. Fortunately, it is straightforward to expand available victim surveys and household surveys with a victimization module (e.g., LSMS) to cover a broader range of frauds, especially in productive activities. Such ex post victim data, however, are most likely to incompletely or even mistakenly capture the prevalence and seriousness of frauds and cannot be used to identify determinants and ex ante impacts of fraud victimization. Ex ante victim data can be a breakthrough in fraud study and policy. Data collection requires a totally different approach; the passive, proactive, and reactive designs are all demanding and rely heavily on chance, but none of them are infeasible.

The paper's ex ante victim data analysis opens up new, promising avenues for research. The following three sets of questions about methodology, the victimization mechanism, and its impacts are of great importance:

1) What difference can ex ante victim data really make? How different would the paper's empirical findings have been if ex post victim data had been analyzed?
2) How can the mechanism of fraud victimization, in particular, attempted victims' decisions, be dissected? What policies can effectively prevent victimization? Structural modeling combined with ex ante victim data seems very promising.
3) How broadly, seriously, and persistently do fraud and fraud victimization affect peoples' behaviors and welfare, before and after they become aware of the fraudulence? For example, does informal risk sharing work against fraud victimization (victims' bad decision)? How does victimization influence subsequent production and investments? Does fraud cause poverty traps?

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Figure 1. Conceptual framework of fraud victimization.

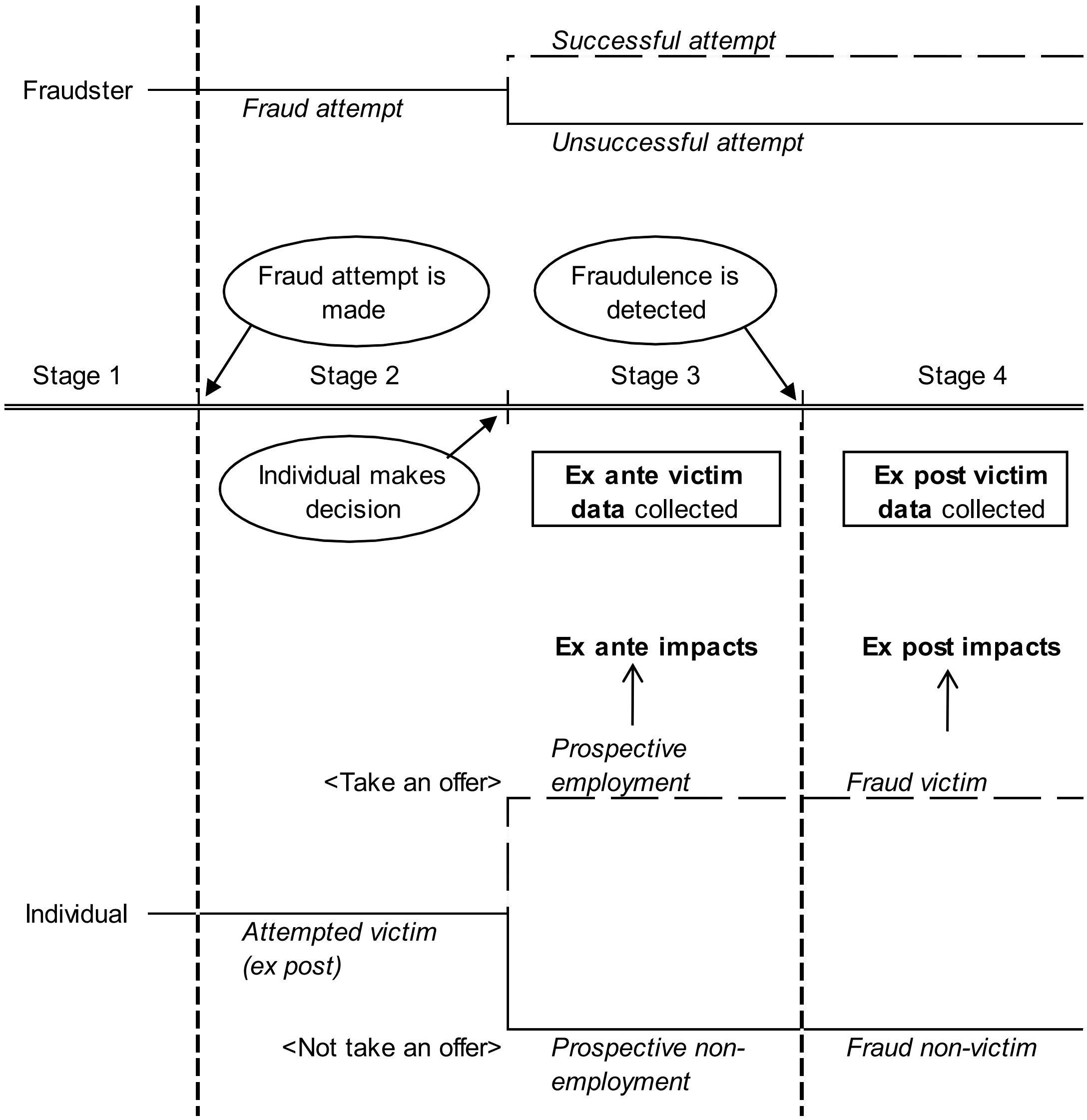

Table 1. Recruitment fraud victimization in rural Fiji.

	Individual	Household
A. Whole sample		
No. eligible male adults in the household:		
One		64%
Two		22%
Three		10%
Four		3%
Five		1.0%
Six		0.6%
Victimization (job application)	19%	27%
Mean monthly household pre-transfer income (F$)		1703
		(2092)
No. observations	1247	787
B. Victim sample		
No. victims		
One		91%
Two		7%
Three		2%
Four		0.5%
Mean application costs (F$)	230	242
	(127)	(125)
Reasons for application:		
Salary	79%	79%
Job type	24%	24%
Abroad	16%	16%
No alternative	6%	7%
Know someone who already works	9%	8%
No. observations	238	212
C. Non-victim sample		
Reasons for non-application:		
Unqualified		38%
Fees		26%
Job type		31%
Abroad		33%
Security		14%
Precaution		19%
Alternative		10%
Do not know anyone who already works		9%
No. observations	1009	575

Notes: Individual and household sample means are shown; all percentages are sample proportions. Standard deviations are in parentheses.

Table 2. Means of explanatory variables.

	Individual	Household
Individual factors:		
	Eligible individual (x^1_{ihv})	*Eligible household members (x^1_{hv})*
Household head dummy	0.50 (0.50)	
Age	36.0 (12.1)	
Mean age		37.6 (8.8)
Disability dummy	0.07 (0.26)	0.11 (0.31)
Adults' secondary education dummy	0.57 (0.50)	0.65 (0.48)
Permanent wage labor dummy	0.09 (0.29)	0.14 (0.35)
	Other household members (x^2_{ihv})	*Non-eligible household members (x^2_{hv})*
Disability dummy	0.30 (0.46)	0.22 (0.42)
Adults' secondary education dummy	0.78 (0.42)	0.65 (0.48)
Permanent wage labor dummy	0.10 (0.31)	0.04 (0.20)
		Any household members (x^{12}_{hv})
Disability dummy		0.29 (0.45)
Adults' secondary education dummy		0.83 (0.38)
Permanent wage labor dummy		0.17 (0.38)
Household factors (x_{hv}):		
Female head dummy	0.04 (0.21)	0.05 (0.21)
Age of household head[a]	50.8 (12.9)	49.2 (13.6)
No. <18 years old	2.47 (2.07)	2.50 (1.97)
No. males 18-60 years old	2.14 (1.25)	1.58 (0.94)
No. females 18-60 years old	1.53 (0.97)	1.35 (0.85)
No. >60 years old	0.42 (0.68)	0.40 (0.68)
Agricultural land (acres)	3.21 (4.95)	2.95 (4.85)
Village factors unrelated to the fraudster (x^1_v):		
Market access (min)	79.5 (66.2)	81.0 (67.3)
No. households in village	53.3 (21.6)	53.5 (21.5)
Village mean agricultural land (acres)[b]	3.09 (1.81)	3.06 (1.80)
Village standard deviation of agricultural land (acres)[b]	3.57 (3.98)	3.51 (3.94)
Village factors related to the fraudster (x^2_v):		
Fraudster's home district dummy	0.08 (0.28)	0.09 (0.28)
Access to fraudster (min)	194.9 (125.8)	195.9 (127.3)
No. observations	1247	787

Notes: Standard deviations are in parentheses.

[a] No. of observations are 1244 individuals and 785 households.

[b] Means/standard deviations including households with no eligible members.

Table 3. Determinants of recruitment fraud victimization - linear probability model.

	Individual		Household	
	(1)	(2)	(3)	(4)
Individual factors:	*Eligible individual*		*Eligible household members*	
Household head dummy	0.051	0.053		
	(0.039)	(0.033)		
Log of age	-0.084	-0.084		
	(0.047)	(0.057)		
Log of mean age			-0.025	-0.025
			(0.089)	(0.077)
Disability dummy	-0.076 *	-0.083 *	-0.134 **	-0.135 **
	(0.035)	(0.031)	(0.049)	(0.047)
Adults' secondary education dummy	0.030	0.029	0.054	0.054
	(0.023)	(0.025)	(0.033)	(0.027)
Permanent wage labor dummy	-0.023	-0.048	-0.068	-0.100
	(0.046)	(0.042)	(0.053)	(0.054)
	Other household members		*Non-eligible household members*	
Disability dummy	-0.016	-0.023	0.005	-0.004
	(0.026)	(0.025)	(0.042)	(0.041)
Adults' secondary education dummy	0.032	0.030	0.028	0.019
	(0.029)	(0.033)	(0.036)	(0.044)
Permanent wage labor dummy	-0.071 *	-0.104 **	-0.060	-0.146
	(0.035)	(0.034)	(0.074)	(0.083)
Household factors:				
Female head dummy	-0.009	0.003	-0.035	-0.022
	(0.054)	(0.055)	(0.066)	(0.082)
Log of age of household head	-0.057	-0.024	-0.110	-0.066
	(0.057)	(0.051)	(0.077)	(0.079)
No. <18 years old	0.001	0.002	-0.003	-0.001
	(0.006)	(0.006)	(0.013)	(0.012)
No. males 18-60 years old	-0.020	-0.023	0.057 **	0.056 *
	(0.011)	(0.012)	(0.020)	(0.024)
No. females 18-60 years old	0.003	0.004	0.020	0.018
	(0.014)	(0.017)	(0.023)	(0.027)
No. >60 years old	0.023	0.026	0.030	0.030
	(0.019)	(0.022)	(0.028)	(0.030)
Log of agricultural land (acres)	-0.005	-0.004	-0.015	-0.023
	(0.017)	(0.018)	(0.025)	(0.023)
Village factors unrelated to the fraudster:				
Log of market access (min)		-0.007		-0.009
		(0.012)		(0.017)
Log of no. households in the village		0.028		0.035
		(0.034)		(0.048)
Log of village mean agricultural land (acres)		-0.011		-0.029
		(0.123)		(0.167)
Log of village standard deviation of agricultural land (acres)		0.015		0.015
		(0.060)		(0.087)
Village factors related to the fraudster:				
Fraudster's home district dummy		0.340 ***		0.348 ***
		(0.054)		(0.081)
Log of access to fraudster (min)		-0.081 **		-0.102 *
		(0.029)		(0.039)
Village dummies	Yes	No	Yes	No
F	6.4 ***	12.8 ***	7.1 ***	8.1 ***
R squared	0.207	0.131	0.252	0.141
No. observations	1244	1244	785	785

Notes : Robust standard errors are shown in columns (1) and (3); standard errors are clustered by village in columns (2) and (4). Other control not shown here is constant.

*** Significant at 0.1 percent level.

** Significant at 1 percent level.

* Significant at 5 percent level.

Table 4. Reasons for recruitment fraud victimization and non-victimization - linear probability model.

	Reasons for application among victimized individuals			Reasons for non-application among non-victimized households					
	Salary	Job type	Abroad	Un-qualified	Fees	Job type	Abroad	Security	Pre-caution
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Individual factors:	*Eligible individual*			*Eligible household members*					
Household head dummy	0.037	-0.193	-0.029						
	(0.105)	(0.127)	(0.112)						
Log of age	-0.129	0.046	0.107						
	(0.103)	(0.139)	(0.124)						
Log of mean age				0.046	-0.149	0.173	0.012	-0.146	0.123
				(0.103)	(0.105)	(0.106)	(0.117)	(0.096)	(0.090)
Disability dummy				0.077	0.035	-0.041	-0.031	0.149 *	-0.058
				(0.068)	(0.078)	(0.049)	(0.052)	(0.059)	(0.041)
Adults' secondary education dummy	-0.047	-0.020	-0.008	-0.084	-0.039	-0.022	-0.042	0.046	0.001
	(0.056)	(0.044)	(0.088)	(0.048)	(0.034)	(0.035)	(0.043)	(0.026)	(0.037)
Permanent wage labor dummy	0.014	-0.019	-0.101	-0.047	-0.123 **	0.020	0.120	-0.078 *	0.052
	(0.098)	(0.083)	(0.063)	(0.064)	(0.044)	(0.056)	(0.072)	(0.034)	(0.048)
	Other household members			*Non-eligible household members*					
Disability dummy	0.250 **	0.006	0.170	-0.070	0.000	-0.064	-0.018	0.109 *	0.015
	(0.080)	(0.102)	(0.089)	(0.054)	(0.053)	(0.051)	(0.056)	(0.047)	(0.042)
Adults' secondary education dummy	-0.006	0.005	0.027	-0.062	0.015	0.029	0.073	-0.028	-0.008
	(0.049)	(0.080)	(0.038)	(0.045)	(0.048)	(0.035)	(0.048)	(0.034)	(0.036)
Permanent wage labor dummy	0.053	0.032	-0.020	-0.017	-0.014	0.052	0.054	0.005	-0.058
	(0.097)	(0.194)	(0.091)	(0.072)	(0.073)	(0.091)	(0.104)	(0.055)	(0.068)
Household factors:									
Female head dummy				0.037	-0.067	0.034	-0.029	-0.019	-0.071
				(0.116)	(0.079)	(0.108)	(0.086)	(0.056)	(0.047)
Log of age of household head	-0.113	-0.029	-0.076	0.287 ***	0.082	-0.169	0.079	-0.034	-0.098
	(0.149)	(0.227)	(0.182)	(0.080)	(0.108)	(0.109)	(0.075)	(0.082)	(0.076)

(continued)

No. <18 years old	0.001	-0.007	-0.018	0.004	-0.014	0.026 *	0.006	-0.017	0.007
	(0.011)	(0.014)	(0.013)	(0.016)	(0.016)	(0.011)	(0.016)	(0.012)	(0.012)
No. males 18-60 years old	-0.015	0.011	-0.015	-0.027	-0.020	0.002	-0.005	-0.026 *	-0.051 *
	(0.024)	(0.035)	(0.031)	(0.024)	(0.021)	(0.021)	(0.026)	(0.011)	(0.019)
No. females 18-60 years old	0.019	0.014	0.002	-0.031	0.026	0.014	-0.023	0.012	0.019
	(0.017)	(0.035)	(0.024)	(0.029)	(0.025)	(0.024)	(0.030)	(0.025)	(0.030)
No. >60 years old	0.010	-0.048	-0.045	0.043	0.028	-0.004	-0.008	-0.008	-0.011
	(0.063)	(0.065)	(0.052)	(0.034)	(0.034)	(0.040)	(0.034)	(0.030)	(0.028)
Log of agricultural land (acres)	-0.023	-0.111 *	0.018	-0.049	-0.099 **	0.007	0.028	-0.019	0.033
	(0.049)	(0.054)	(0.033)	(0.030)	(0.031)	(0.027)	(0.041)	(0.024)	(0.033)
Village factors unrelated to the fraudster:									
Log of market access (min)	-0.031	-0.040	0.014	-0.004	-0.019	-0.001	0.002	0.012	0.000
	(0.030)	(0.032)	(0.010)	(0.014)	(0.023)	(0.014)	(0.021)	(0.013)	(0.013)
Log of no. households in the	0.014	-0.191 **	-0.133 *	0.006	-0.018	0.140 *	-0.017	-0.056	-0.025
village	(0.089)	(0.069)	(0.061)	(0.048)	(0.057)	(0.065)	(0.047)	(0.047)	(0.040)
Log of village mean agricultural	0.011	-0.223	-0.519 ***	0.167	0.168	0.472 *	0.022	-0.288	-0.140
land (acres)	(0.415)	(0.282)	(0.139)	(0.150)	(0.147)	(0.214)	(0.128)	(0.158)	(0.119)
Log of village standard	0.191	0.072	0.193 *	-0.079	-0.073	-0.247	0.000	0.221 *	-0.009
deviation of agricultural land	(0.278)	(0.188)	(0.091)	(0.093)	(0.100)	(0.143)	(0.079)	(0.101)	(0.072)
Village factors related to the fraudster:									
Fraudster's home district	-0.181	0.168 **	0.093 **	-0.200 **	-0.056	-0.122 *	-0.050	-0.120 *	-0.023
dummy	(0.137)	(0.055)	(0.026)	(0.057)	(0.049)	(0.057)	(0.035)	(0.045)	(0.069)
F	7.9 ***	6.6 ***	10.1 ***	6.9 ***	2.4 **	2.7 **	1.3	4.6 ***	2.3 *
R squared	0.187	0.117	0.121	0.084	0.045	0.059	0.018	0.087	0.051
No. observations	238	238	238	621	621	621	621	621	621

Notes: Standard errors are clustered by village. Other control not shown here is constant.

*** Significant at 0.1 percent level.

** Significant at 1 percent level.

* Significant at 5 percent level.

Table 5. Household private transfers.

	Gross transfers received		Gross transfers given	
	Partici-pation	Mean amounts (F$)	Partici-pation	Mean amounts (F$)
All (F$)	92%	141 (183)	88%	104 (141)
Same village	89%	93 (126)	87%	89 (117)
Other village or city	30%	48 (121)	15%	15 (58)
Cash and inkind (F$)	92%	114 (160)	88%	78 (118)
Same village	89%	70 (105)	87%	66 (94)
Other village or city	30%	44 (112)	15%	12 (52)
Labor (man-day)	25%	1.9 (4.8)	26%	1.7 (4.3)
Same village	24%	1.6 (4.2)	25%	1.5 (4.0)
Other village or city	4%	0.3 (2.1)	4%	0.2 (1.2)

Notes : These are transfers in the past one month. Standard deviations are in parentheses. No. of observations vary across variables (771-785).

Table 6. Estimated marginal effects of recruitment fraud victimization on household private transfers.

	Net transfers received		Gross transfers received		Gross transfers given	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraud victimization dummy:						
OLS	-19.4	-14.7	14.1	24.6	33.9 *	39.4 **
	(15.9)	(16.4)	(19.6)	(20.8)	(14.9)	(12.7)
2SLS - overidentified model	-152.7 **	-158.1 ***	-180.1 ***	-141.4 ***	-26.4	17.6
	(59.1)	(36.0)	(45.0)	(37.1)	(65.0)	(34.5)
2SLS - just-identified model 1	-162.8 **	-171.6 ***	-145.6 ***	-144.6 ***	17.6	27.4
	(74.5)	(39.9)	(26.1)	(29.1)	(86.1)	(36.3)
2SLS - just-identified model 2	-136.9 *	-138.5 *	-233.9 *	-136.9	-95.5	3.3
	(64.5)	(58.3)	(99.2)	(87.6)	(77.9)	(60.3)
LIML - overidentified model	-152.9 **	-158.5 ***	-183.1 ***	-141.5 ***	-29.6	17.5
	(59.2)	(36.1)	(46.3)	(37.1)	(68.2)	(34.6)
2SLS - over-identified model:						
First stage: Fraudster's home district dummy[a]	0.37 ***	0.34 ***				
	(0.07)	(0.08)				
First stage: Log of access to fraudster (min)[a]	-0.10 **	-0.10 *				
	(0.03)	(0.04)				
Excluded IVs: F[a]	26.6 ***	28.6 ***				
Overidentification test: Hansen J	0.12	0.24	0.94	0.01	1.75	0.15
Reduced-form: Fraudster's home district dummy	-62.0 **	-62.3 ***	-44.5 *	-50.0 **	17.6	12.3
	(22.9)	(13.1)	(19.8)	(18.1)	(39.2)	(17.2)
Reduced-form: Log of access to fraudster (min)	12.2	12.4	26.3 *	13.6	13.9	1.0
	(7.1)	(7.5)	(10.8)	(11.9)	(8.5)	(9.4)
2SLS - just-identified model 1:						
First stage: Fraudster's home district dummy[a]	0.43 ***	0.42 ***				
	(0.06)	(0.05)				
Excluded IV: F[a]	53.1 ***	69.1 ***				
Reduced-form: Fraudster's home district dummy	-70.7 **	-72.1 ***	-63.3 ***	-60.8 ***	7.6	11.5
	(24.1)	(11.7)	(16.1)	(11.5)	(36.9)	(14.9)
2SLS - just-identified model 2:						
First stage: Log of access to fraudster (min)[a]	-0.13 ***	-0.14 ***				
	(0.03)	(0.04)				
Excluded IV: F[a]	13.9 ***	12.4 ***				
Reduced-form: Log of access to fraudster (min)	17.7 *	19.6 *	30.3 **	19.4	12.3	-0.5
	(8.7)	(8.3)	(10.8)	(10.9)	(8.9)	(8.8)
Exogenous covariates	No	Yes	No	Yes	No	Yes
No. observations	771	769	772	770	771	769

Notes: Fraudster's home district dummy and log of access to fraudster are excluded IVs in over-identified model; fraudster's home district dummy is an excluded IV in just-identified model 1; and log of access to fraudster is an excluded IV in just-identified model 2. Standard errors clustered by village are in parentheses. Exogenous covariates in columns (2), (4), and (6) are those shown in appendix.

*** Significant at 0.1 percent level.

** Significant at 1 percent level.

* Significant at 5 percent level.

[a] Results in columns (3) and (5), respectively, are almost and exactly the same as those in (1); results in (4) and (6), respectively, are almost and exactly the same as those in (2).

Table 7. Estimated marginal effects of recruitment fraud victimization on household private gross transfers received by location.

	All	Within-village	Across-village	
	Amounts	Amounts	Amounts	Receipt
	(1)	(2)	(3)	(4)
A. Models with no exogenous covariates				
OLS	14.1	7.7	5.8	-0.01
	(19.6)	(12.6)	(11.2)	(0.04)
2SLS - over-identified model	-180.1 ***	-57.3 *	-117.3 ***	-0.48 ***
	(45.0)	(22.8)	(29.3)	(0.10)
2SLS - just-identified model 1	-145.6 ***	-49.7 **	-94.0 ***	-0.49 ***
	(26.1)	(18.2)	(13.5)	(0.07)
LIML - over-identified model	-183.1 ***	-57.4 *	-119.3 ***	-0.48 ***
	(46.3)	(22.8)	(30.2)	(0.10)
Bivariate probit (at means)				-0.33 ***
				(0.05)
No. observations	772	777	784	784
B. Models with exogenous covariates				
OLS	24.6	13.3	10.4	0.00
	(20.8)	(13.2)	(12.0)	(0.04)
2SLS - over-identified model	-141.4 ***	-31.3	-103.6 ***	-0.45 ***
	(37.1)	(22.7)	(23.7)	(0.10)
2SLS - just-identified model 1	-144.6 ***	-41.1 *	-97.7 ***	-0.52 ***
	(29.1)	(20.5)	(18.1)	(0.09)
LIML - over-identified model	-141.5 ***	-31.4	-103.8 ***	-0.45 ***
	(37.1)	(22.8)	(23.8)	(0.10)
Bivariate probit (at means)				-0.31 ***
				(0.07)
No. observations	770	775	782	782

Notes: Fraudster's home district dummy and log of access to fraudster are excluded IVs in over-identified model and excluded variables in the transfer-receipt equation in the bivariate probit; and fraudster's home district dummy is an excluded IV in just-identified model 1. Standard errors clustered by village are in parentheses. Exogenous covariates are those shwon in appendix. Results in column (1) is the same as those in columns (3) and (4) of Table 6.

*** Significant at 0.1 percent level.
** Significant at 1 percent level.
* Significant at 5 percent level.

Appendix. Determinants of household private transfers.

	OLS			2SLS		
	Net transfers received	Gross transfers received	Gross transfers given	Net transfers received	Gross transfers received	Gross transfers given
	(1)	(2)	(3)	(4)	(5)	(6)
Fraud victimization dummy[a]	-14.7	24.6	39.4 **	-158.1 **	-141.4 ***	17.6
	(16.4)	(20.8)	(12.7)	(36.0)	(37.1)	(34.5)
Individual factors (any household members):						
Disability dummy	48.3 **	43.2 *	-5.1	36.2 *	29.2	-6.9
	(14.3)	(16.7)	(13.5)	(14.1)	(18.2)	(13.5)
Adults' secondary education dummy	2.7	13.1	10.1	12.8	24.7	11.6
	(11.4)	(15.6)	(11.6)	(11.2)	(17.9)	(12.4)
Permanent wage labor dummy	-3.4	40.4	42.7 **	-23.0	17.5	39.7 **
	(14.8)	(22.3)	(13.8)	(16.7)	(23.6)	(14.5)
Household factors:						
Female head dummy	38.2	28.9	-9.5	35.2	25.3	-9.9
	(36.3)	(32.8)	(26.0)	(43.5)	(39.3)	(24.6)
Log of age of household head	12.0	6.7	-3.6	4.3	-1.9	-4.8
	(27.3)	(28.1)	(29.9)	(29.9)	(32.9)	(30.0)
No. <18 years old	-0.3	1.7	2.1	-0.2	1.8	2.1
	(2.7)	(2.7)	(2.0)	(2.7)	(2.7)	(1.9)
No. males 18-60 years old	-9.1	-5.2	3.8	-1.5	3.6	5.0
	(7.7)	(7.7)	(7.1)	(8.3)	(9.2)	(7.4)
No. females 18-60 years old	-6.0	-7.1	-0.8	-3.3	-4.0	-0.4
	(7.4)	(5.6)	(6.3)	(8.7)	(7.5)	(6.0)
No. >60 years old	-12.1	3.6	15.5	-2.6	14.5	16.9
	(13.8)	(9.7)	(9.9)	(14.6)	(11.1)	(9.3)
Log of agricultural land (acres)	-13.1	10.0	23.3 *	-15.7	7.0	22.9 *
	(11.8)	(11.7)	(9.2)	(10.9)	(12.4)	(9.6)
Village factors:						
Log of market access (min)	1.3	1.3	-0.1	1.3	1.2	-0.1
	(2.9)	(6.4)	(4.5)	(4.4)	(8.2)	(4.6)
Log of no. households in the village	19.8	14.6	-5.7	25.2	20.8	-4.8
	(18.2)	(24.7)	(12.4)	(15.5)	(21.5)	(12.2)
Log of village mean agricultural land (acres)	63.3	182.5 *	118.7 *	18.1	130.0	111.8 *
	(54.2)	(82.2)	(46.7)	(52.3)	(82.0)	(49.7)
Log of village standard deviation of agricultural land (acres)	-49.9	-96.7 *	-46.6	-25.7	-68.7	-42.9
	(32.9)	(47.8)	(27.1)	(30.7)	(46.7)	(28.5)
F	3.4 ***	2.1 *	4.8 ***	4.1 ***	3.0 *	6.4 ***
R squared	0.032	0.044	0.076			
No. observations	769	770	769	769	770	769

Notes : Standard errors clustered by village are in parentheses. Other control not shown here is constant.

*** Significant at 0.1 percent level.

** Significant at 1 percent level.

* Significant at 5 percent level.

[a] The endogenous variable in 2SLS; fraudster's district dummy and log of fraudster's access are excluded IVs. The results in columns (1) and (4), (2) and (5), and (3) and (6) are the same as those in columns (2), (4), and (5), respectively, of Table 6.