Split Decisions: Household Finance When a Policy Discontinuity Allocates Overseas Work

Michael Clemens and Erwin Tiongson

Abstract

Overseas work can raise a family's income but also split the household geographically, with theoretically ambiguous net effects on decisions about work, investment, and education. We study a policy discontinuity in the Philippines that resulted in quasi-random assignment of temporary, partial-household migration to high-wage jobs in Korea. This allows quasi-experimental estimates of reduced-form effects on migrant households' spending, saving, borrowing, business income, and investment in children. A purpose-built survey allows nonexperimental tests of different mechanisms for the effect. These suggest that an important channel — beyond remittance income — involves changes in household decision-making power when migration separates married couples. We find no evidence that migration affects labor force participation by non-migrant family members.

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Michael Clemens
Center for Global Development

Erwin Tiongson
World Bank, AIM, and IZA

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Center for Global Development
2055 L Street NW
Washington, DC 20036

202.416.4000
(f) 202.416.4050

www.cgdev.org

1 Introduction

Real wages can differ between countries by hundreds of percent, even for equivalent workers (McKenzie et al. 2010; Ashenfelter 2012; Clemens et al. 2009). This suggests that households in developing countries can acquire a major source of finance by sending a family member to work temporarily abroad. But although temporary migration programs "are widely used around the world ... there is little evidence as to their development impacts" (Gibson and McKenzie 2014).

Measuring the household impacts of migration is difficult and complex. It is difficult because households that self-select into migration can differ unobservably from other households. And it is complex because migration affects households by various mechanisms beyond simply remittances. First, migration can reduce non-remittance income at home—both mechanically, because migrant breadwinners are absent, and by altering the labor supply of non-migrant family members. Second, migration often splits households geographically, changing household behavior by giving non-migrants greater control over household decisions.

In this paper, we estimate the reduced-form effects of temporary overseas work in a quasi-experiment, and conduct a non-experimental decomposition to test different theoretical mechanisms for the effect. We study a sample of the households of 899 workers in the Philippines from a universe of 25,320 who applied to high-wage temporary jobs in Korea between 2005 and 2007. Each applicant was required to achieve a fixed cutoff score on a test of basic Korean language skill. In 2010 we surveyed the households of applicants who had scored close to the cutoff and had thus been subjected to a natural policy discontinuity in their ability to migrate.

Much of the literature on household impacts of migration relies on instrumental variables in cross-sectional data (surveyed by McKenzie and Sasin 2007, and recently including Antman 2013 and Cortés 2014). A handful of recent studies use natural and designed experiments to study the household effects of international migration (Gibson et al. 2011) and rural-urban migration (Bryan et al. 2014). Growing awareness of selection bias has led to greater use of household panel data (Gibson and McKenzie 2014) and impact evaluation methods to establish the counterfactual (surveyed by McKenzie and Yang 2012).

This paper makes three contributions. First, it uses a natural policy discontinuity to minimize selection bias. It does this with novel data from a developing country that is a hub for international migration: the Philippines has the ninth-largest stock of emigrants, 5.2 million (UN 2013). Second, it presents evidence that migration affects these households through three independent mechanisms: changing remittance income, non-remittance income, and household decision-making responsibilities. The mechanism of changing decisions by splitting households is underexplored. Third, it offers evidence that substantial bias could arise from using observational estimators to measure the impact of migration on the same households.

We find that migration has important reduced-form effects on these households' financial behavior—including tripling expenditure on education and health, reducing borrowing, and raising savings. Migration reduces non-remittance income, mechanically through the absence of breadwinners who migrate, but does not affect non-migrants' labor supply. Migration causes families to send children to private schools and private clinics, but not to start businesses. For migrants who are initially married, migration causes large shifts in household decision-making power. This change in decision-making accounts for a substantial part of the impact on behavior. For example, part of the effect of migration on education and health spending appears to arise independently of remittances, through shifting responsibility for household decisions from husbands to wives. Finally, non-experimental estimates with the same data contradict the quasi-experimental estimates by finding a significant negative relationship between migration and non-migrant labor supply. This relationship may therefore reflect self-selection on unobservable traits, rather than an effect of migration.

We begin by discussing the different theoretical mechanisms by which migration can affect household finance, and prior literature on each mechanism. The following sections discuss the natural experiment that created the policy discontinuity, and describe our novel survey data. We then present evidence on reduced-form impacts of migration, and offer a nonexperimental exploration of impact mechanisms. We conclude by discussing the external validity of these estimates and comparing our quasi-experimental results to those obtained with more standard observational estimators.

2 Mechanisms for the effect of overseas work

Labor migration by one household member bundles different effects on the household. It often brings substantial new income to the household that can in theory alleviate capital constraints; the same income can in theory change labor supply decisions and home production by other household members; and migration can change the migrant's participation in household decisions.

To sort out these mechanisms formally, we model migration as a family decision (Mincer 1978) by a collective household engaged in dynamic optimization, with and without borrowing constraints.[1] The two household members $(1, 2)$ get utility from consumption (c_1, c_2) and leisure (ℓ_1, ℓ_2) over lifetime T, and have an available investment that requires no labor input (Bardhan and Udry 1999, pp. 7–18).

Member 1 can work in the home country at wage w. Member 2 can migrate, to spend a fraction $0 \leqslant m \leqslant 1$ of work time[2] earning the overseas wage $w_o > w$, thus earning overall wage

$$w^* = mw_o + (1-m)w. \tag{1}$$

There is pure disutility of having a family member overseas, captured by $0 \leqslant \phi \leqslant 1$, analogous in modeling terms to Moffitt's (1983) "welfare stigma". Suppose each household member has an additively separable egoistic utility term. For given m the household solves

$$\max_{c^1,c^2,\ell^1,\ell^2} \int_0^T e^{-\rho t}\Big(u(c^1,\ell^1) + (1-\phi m)u(c^2,\ell^2)\Big)\,\mathrm{d}t. \tag{2}$$

The term $-\phi m$ captures not just the disutility of partial-household migration, but also the corresponding change in the "balance of power" between household members (as in Udry

[1] The unitary household model has been criticized both theoretically (Chiappori 1988, 1992) and empirically (Alderman et al. 1995; Fortin and Lacroix 1997). A unitary model seems especially inappropriate for households split by great distance, and recent evidence suggests non-cooperative behavior among spouses in migrant households (Chen 2013).

[2] Here m is an exogenous parameter, not a choice variable. The reason is that the sampling universe of our survey comprises exclusively households that took serious steps to acquire overseas work; for all of these households, the perceived optimal m is 1. From the household's point of view, either $m = 1$ as they desire or it is exogenously set to zero by forces beyond their control.

1996; Basu 2006; Bobonis 2009) when one member is absent.

With borrowing constraints. Suppose, for the moment, that the household cannot borrow or lend. Capital evolves subject to

$$\dot{k} = \theta(m)f(k) + w(1-\ell^1) + w^*(1-\ell^2) - c. \tag{3}$$

where $\theta(m)$ reflects the productivity of some home production process—a family business, a farm, or (more abstractly) the production of high-quality children (Becker 1991; Baland and Robinson 2000; Caucutt and Lochner 2012) or even investment in migration by other family members as a form of human capital (Schultz 1961; Sjaastad 1962; Connell et al. 1976).[3] Note that $\theta \equiv \theta(m)$, that is, migration can change home productivity: it can raise home productivity by bringing in new ideas or inspiration, or lower home productivity by taking away individuals that determine home production.

We solve (2) and (3) as an autonomous program of optimal control, for which the Pontryagin conditions on the current-value Hamiltonian are $u_{\ell^1} = \mu w$; $(1-\phi m)u_{\ell^2} = \mu w^*$; $u_{c^1} = \mu$; $(1-\phi m)u_{c^2} = \mu$; and $\dot{\mu} = \mu\big(\rho - \phi(m)f'(k)\big)$, where the subscript denotes the partial derivative. The first two of these give

$$\ell^1_m > 0, \quad \ell^1_{m\phi} > 0 \quad \Longleftrightarrow \quad \ell^2_m \lessapprox 0. \tag{4}$$

That is, non-migrants supply less labor (consume more leisure) due to migration by one household member, provided that migration does not cause the migrant to consume much more leisure. But the magnitude of this effect declines as migration causes a smaller shift in the balance of power (ϕ is smaller).

Migration affects investment through different channels. Letting labor income net of consumption $\Psi_{w^*} \equiv w(1-\ell_1) + w^*(1-\ell_2) - c$, then (3) gives

$$\dot{k}_m = f'k_m + f\,\theta_m + (w_o - w)\,\Psi_{w^*} + \Psi_m. \tag{5}$$

[3] We have the standard boundary conditions $k_t \geqslant 0$, the shadow value of capital $\mu(T) > 0$, and $\mu(T)k(T) = 0$. The subscript t is suppressed for clarity, and a superscript dot indicates the derivative with respect to t. We assume u and f are concave, continuous, and twice differentiable.

The first term on the right side captures increased investment as the borrowing constrained household uses migration earnings to raise home production. In the second term, the migrant's absence can directly change the home production technology by altering θ. In the third term, migration affects labor income net of consumption by altering the wage w^* and non-migrants' labor supply. In the fourth term, migration decreases the influence of the migrant in day-to-day household decisions, changing the balance of decision-making power between migrant and non-migrants.

The entire timepath of non-migrant labor supply can fall due to foreseen migration, but non-migrant labor supply is unchanged at the moment that migration raises w^*. Intuitively, non-migrants in this circumstance choose consumption of leisure according to the household's permanent income. This implies that shorter spells of migration, with a foreseeable end, will have a smaller effect on non-migrant labor supply.

Without borrowing constraints. If we allow the household to borrow,[4] $f' = r$ in (5). Migration raises investment by more in borrowing-constrained households than in borrowing-unconstrained households (provided $k < \bar{k}$). It can be shown that the labor supply effect (4) only arises when there are borrowing constraints or capital market imperfections.

This simple model predicts that migration can affect household financial decisions through three channels. First, migration can alleviate borrowing constraints with remitted income. This channel would unambiguously raise investment (if $k < \bar{k}$). Second, migration can reduce non-remittance income by changing the home production technology (for example if the migrant is better at running a family farm than other household members), or by reducing labor supply by non-migrants. The magnitude of the labor supply effect is smaller to the degree that migration causes a smaller change in household decision-making power, and to the degree that migration duration is shorter. Third, migration can change financial decisions by changing who makes them (for example if non-migrants have different preferences than migrants). The net effect of migration on consumption, savings, and investment is ambiguous.

[4]The equation of motion (3) becomes $\dot{k} = \theta f(\bar{k}) - r(\bar{k} - k) + w(1 - \ell_1) + w^*(1 - \ell_2) - c$, where $\bar{k} = f'^{-1}(r/\theta)$ is the unconstrained optimal investment. If the capital market is efficient and frictionless ($r \approx \rho$) and the home wage is constant ($\dot{w} = 0$), then non-migrant labor supply does not change over time (because $\dot{\ell}^1 = 0$).

3 Prior research on three mechanisms

Each of these three impact mechansims has been the subject of a sizeable literature. These discussions are disconnected and usually occur in contexts outside migration.

First, economists have long investigated how credit constraints shape household decisions on savings and investment. Recent influential studies trace the effects of increases in capital on investments in home production and new business.[5] Others stress the effects of credit constraints on investments in human capital (surveyed by Lochner and Monge-Naranjo 2012) and on consumption decisions.[6] Yang (2008) finds that shocks to remittances cause human capital accumulation and entrepreneurship in households in the Philippines, suggesting that they face capital constraints.

Second, a large literature investigates whether transfers (usually public transfers) affect household labor supply (surveyed by Moffitt 2002 and recently including Garthwaite et al. 2014, Baicker et al. 2014, and De Hoop and Rosati 2014). This research typically finds substantial effects, and it is a small leap to think that households receiving remittance transfers would exhibit similar effects. A negative association between remittance receipts and labor force participation, particularly by migrants' spouses, has been noted in numerous countries by a long literature (surveyed by Adams 2011 and Antman 2013, and recently De and Ratha 2012; Gunatilaka 2013; Abdulloev et al. 2014).

Third, economists have studied the effect of household composition and decision-making structure on finance and investment choices (Becker 1991; Bergstrom 1997). An important strand of literature studies the effect of shifts in decision-making power on household investments (broadly considered), especially testing whether household savings and investment decisions change when direct control over income shifts from one member to another.[7] In-

[5] These studies include Hubbard (1998); Udry and Anagol (2006); De Mel et al. (2008); Banerjee et al. (2010); Gertler et al. (2012); Wang (2012).

[6] The effects of credit constraints on consumption choices is investigated in early work by Hanushek (1986); Altonji and Siow (1987) and recently by Karlan and Zinman (2010); Dupas and Robinson (2012); Kaboski and Townsend (2012).

[7] Research on the effects of intrahousehold shifts in power over income includes Duflo (2000); Qian (2008); Agnew et al. (2008); Ashraf (2009); Ashraf et al. (2010).

trahousehold changes in decision-making power have been shown to have important effects on family financial decisions, including in the Philippines (Ashraf 2009). Numerous observational studies correlate the migration of a household member to investment in children and care for the elderly.[8]

Migration bundles these effects.[9] The combined effect has been the subject of a recent and growing literature on the micro effects of migration, surveyed by Hanson (2009) and Antman (2013). Though much of the early work on migration stresses the effects of remittances alone,[10] a new literature seeks to measure the reduced-form effects of migration itself.[11]

Existing research is just beginning to sort out the different mechanisms of migration impact. The latest work suggests that, beyond remittances, also important are the ways that migration shapes household decision-making (Ashraf et al. 2014) and perceived returns to investment (Kandel and Kao 2001; McKenzie and Rapoport 2011).

4 A policy discontinuity in the Philippines

We study the effects of international migration on households in the Philippines in a natural quasi-experiment. A large group of Filipino applicants to jobs in Korea were required to pass a Korean language test. Large numbers of applicants either barely passed or barely failed the exam, and those who failed could not migrate. This setting is well-suited for the regression discontinuity design (RDD), which can approximate the causal identification offered by

[8] E.g. Antman (2011, 2012); De and Ratha (2012); Alcaraz et al. (2012); Gerber and Torosyan (2013); Sarma and Parinduri (2013); Bargain and Boutin (2014); Dinkelman and Mariotti (2014); Botezat and Pfeiffer (2014); Cortés (2014); De Arcangelis et al. (2014)

[9] Migration certainly can affect households by other channels as well. In particular, economists have studied the effect of social networks and information flows on household decisions about finance and investment (e.g Jensen 2010; Alatas et al. 2012). This literature suggests that information on investment returns often passes through social networks and can substantially influence household savings and investment decisions. We abstract from these effects for this study because the overseas jobs are short-term, involve little social integration with people at the destination, and involve low-skill work unlikely to bring migrants important changes in transferable skills, cultural attitudes, or technical knowledge.

[10] Rapoport and Docquier (2006) and Yang (2011) survey this literature, which includes Cox Edwards and Ureta (2003); Yang (2008); Alcaraz et al. (2012); De and Ratha (2012); López-Videla and Machuca (2014).

[11] Studies of the effects of migration on source households, including but not principally focusing on remittances, include Hanson and Woodruff (2003); Macours and Vakis (2010); Taylor and López-Feldman (2010); Quisumbing and McNiven (2010); Mergo (2012); Cortés (2014).

randomized experiments in real settings (Cook and Wong 2008).

4.1 A language test for high-wage jobs in Korea

In 2004, the government of the Philippines signed a bilateral agreement with the Republic of Korea allowing participation of Filipino workers in Korea's Employment Permit System (EPS). EPS issues temporary visas to work in Korea, visas today accessible to workers from 16 developing countries across Asia. In the Philippines, EPS jobs are advertised and recruitment takes place exclusively through the Philippine Oveseas Employment Administration (POEA) of the national government. EPS job contracts are initially for three years, and are renewable up to five years, but workers may not settle in Korea.

EPS jobs are only accessible to people 18–39 years old, with either a high-school or vocational degree and two years of work experience, or a tertiary degree and one year of work experince. In Korea, most of the workers perform low-skill labor in small enterprises (fewer than 300 employees), almost all of which are manufacturing plants. During 2008–2011 the average wage was about PHP35,000–38,000 per month (about US$820–800). Employers pay for workers' lodging and for some meals (usually daytime meals only, but occasionally dinner as well). The typical one-time, all-inclusive cost of starting an EPS-Korea job is approximately PHP25,500–32,500 (US$550–700), that is, less than one month's earnings.[12]

Starting with the second wave of workers, in 2005, all Filipino applicants to EPS jobs were required to pass a Korean Language Test (KLT). This 90-minute, 200-question examination tested basic listening and reading in Korean. The test was administered at three locations in the Philippines and graded in Korea. The maximum score was 200 points, and a score of 120

[12]This includes a one-time cost of PHP19,000–25,000 (US$410–540) for application fees and travel; this comprises a PHP729 training fee, PHP1,500 medical examination fee, US$50 POEA processing fee, US$25 Overseas Workers Welfare Administration membership fee, PHP900 Philhealth/Medicare fee, PHP100 Home Development Mutual Fund membership (known as "*Pag-Ibig*"), PHP2,500 visa fee, and PHP10,000 for one-way airfare to Korea (PHP16,000 for chartered flight). Beyond these POEA application fees are the cost of the Korean language exam: This comprises a one-time KLT test fee of US$30. Many applicants also take a preparatory course in Korean language, offered by numerous private teachers, which costs around PHP5,000–7,000. The application fees, travel cost, test fee, and Korean language course costs sum to about PHP25,500–32,500. In the years relevant to this study, the KLT was administered by the International Korean Language Foundation, at five test centers across the Philippines (Manila, Pampanga, Baguio, Cebu, and Davao). The KLT has since been superseded by the Test of Proficiency in Korean (TOPIK), administered by the Human Resources Development Service of Korea.

points or greater was required to secure a work permit.

For the purposes of this study, there were five EPS recruitment rounds in the Philippines, each of which administered one KLT.[13] Table 1 shows the number of people who sat for each round of the KLT, and the numbers whose scores fell within five points of the 120-point cut-off. The large number of test-takers provides substantial density near the cutoff, suggesting a regression discontinuity design to evaluate the effects of migration on EPS job-applicants' households. Migration by test-passers typically occurred within a few months of the KLT, and our household survey occurred in early 2010. Households are therefore surveyed about 3–5 years after potential migration began.

4.2 The regression discontinuity design

We estimate the effects of migration with the regression discontinuity design or RDD. Because RDD results can be sensitive to functional form assumptions (Gelman and Imbens 2014), we show all main results with parametric and nonparametric estimation. The parametric sharp (intent-to-treat, ITT) estimation is

$$y = \alpha + \beta_{\text{ITT}} d + \eta s + \boldsymbol{\rho}' X + \varepsilon \tag{6}$$

where y is an outcome of interest, $d = 1$ if the applicant passed the test and 0 otherwise, s is the applicant's score minus the cutoff score (thus $s = 0$ barely passes), X is a vector of predetermined covariates,[14] α is a constant, ε is an error term, and β_{ITT}, η, and vector $\boldsymbol{\rho}$ are to be estimated. The parametric fuzzy (treatment-on-treated, TOT) estimation is the two-stage least squares estimation of

$$\begin{aligned} y &= \alpha + \beta_{\text{TOT}} \tau + \eta s + \boldsymbol{\rho}' X + \varepsilon \\ \tau &= \zeta + \mu d + \nu \end{aligned} \tag{7}$$

[13] An additional EPS recruitment round occurred in 2004, before the KLT became a requirement, and further EPS recruitment continued starting in mid-2010, after we conducted our survey.

[14] These are the characteristics of the applicant at the time of job applicant, prior to the test, as listed on the application form: age, whether or not employed, months of work experience, gender, whether or not married, whether or not a college graduate, a set of dummies for the five rounds of the test, and a set of dummies for the major regions of the country (National Capital Region [NCR], Luzon except NCR, Visayas, and Mindanao).

where 'treatment' $\tau = 1$ if the applicant is a migrant and 0 otherwise, α and ζ are constant terms, ν is an error term, and β_{TOT}, η, μ, and vector $\boldsymbol{\rho}$ are to be estimated.

We check the robustness of these results to fully nonparametric sharp and fuzzy RDD (Hahn et al. 2001; Porter 2003; Nichols 2007; Imbens and Lemieux 2008).[15] The sharp nonparametric estimator is[16]

$$\gamma_{\text{ITT}} = y_{s\downarrow 0} - y_{s\uparrow 0}. \tag{8}$$

This is the effect of barely passing the test. We then estimate the treatment-on-treated (TOT) effect as fuzzy nonparametric RDD:

$$\gamma_{\text{TOT}} = \frac{y_{s\downarrow 0} - y_{s\uparrow 0}}{\tau_{s\downarrow 0} - \tau_{s\uparrow 0}}. \tag{9}$$

This is the effect of migration by a household member. The ITT and TOT effects are "local" estimates in that they show the effect on households that 1) were "compliers", i.e. whose migration decisions were changed by the test result (Angrist et al. 1996, assuming no defiers) and 2) whose member scored near the passing threshold $s = 0$. The ITT effect is the effect of having a member barely pass the language test; the TOT is the effect of migration.

Each empirical method has drawbacks. The parametric regressions use the whole dataset, while the nonparametric regressions use only datapoints within the optimal bandwidth from the discontinuity—somewhere between 1.5–3.5 test-score points. The parametric regressions impose the assumption of local linearity in the relationship between the outcome and the running variable—plausible given the small sampling window, but nevertheless an assumption—while the nonparametric regressions do not. Thus the parametric tests have more power but possibly more bias, while the nonparametric tests are less powerful but possibly less biased. In interpreting the tables to come, we focus on results where the magnitude of the coefficient estimate is similar between the two specifications, and the estimate is statistically precise in the high-power parametric tests or in both tests.

[15] We use the asymptotically optimal bandwidth proposed by Imbens and Kalyanaraman (2012), and the triangular kernel shown optimal by Fan and Gijbels (1996) and Cheng et al. (1997) for its boundary properties; Lee and Lemieux (2010) argue that the choice of kernel "typically has little impact in practice."

[16] For individual i, treatment status is $\tau^i \in \{0, 1\}$ where treatment is migration, and $s \equiv$ [raw score] $- 120$. Then $y_{s\downarrow 0} \equiv \lim_{s\to 0+} E_i[y^i]$; $y_{s\uparrow 0} \equiv \lim_{s\to 0-} E_i[y^i]$; $\tau_{s\downarrow 0} \equiv \lim_{s\to 0+} E_i[\tau^i]$; $\tau_{s\uparrow 0} \equiv \lim_{s\to 0-} E_i[\tau^i]$.

4.3 Checking the discontinuity: Sampling universe

Three testable conditions are necessary (not sufficient) for the test-passing threshold to be useful in identifying the effect of migration. First, at the threshold, there must be a large discontinuity in the treatment variable: deployment of the worker to Korea. Second, there must be no discontinuity in baseline traits of the job applicants. Such a discontinuity would suggest self-selection across the threshold. Third, there must be no bunching of test-score density above or below the threshold. This would suggest that test-takers are able to manipulate their scores—either through legitimate means (putting extra effort when they know they're about to barely fail) or illegitimate (such as paying bribes for extra points).[17]

In the sampling universe there is a very large discontinuity at the cutoff score in the probability of deployment—departing for a job in Korea. But there is little evidence of a significant discontinuity in the baseline characteristics of the job applicants. Table 2 shows that there is a jump of 69–70 percentage points in the probability of deployment at the cutoff score.

The rest of the table tests for discontinuities in all known baseline characteristics of the job applicants: age, sex, education, work experience, employment, marital status, and test batch. Almost all of the variables exhibit no discontinuity; the dummy for college graduate shows a small positive discontinuity that is significant at the 5% level in the parametric but not the nonparametric specification. Figure 1 shows some of these results in graphical form, displaying both an unsmoothed average at each test score (gray circle) and a local linear regression. The upper-left panel shows the jump in deployment probability. The upper-right and lower-left panels show the lack of discontinuity in baseline education and employment of the applicant.

Were test-takers able to self-select across the discontinuity? First, there is no statistically significant jump in test-score density at the passing threshold. Figure 2 shows the McCrary (2008) nonparametric test for manipulation of the test score variable. While there is an increase in the density at the passing threshold, it is small in magnitude, not statistically significant, and

[17] Clean identification with RDD requires other assumptions that we cannot test but that appear plausible in this case. It requires there to be no "bitterness" effect of barely failing the exam, so that some outcome could be attributable to having come very close to passing without passing. In this case we consider it unrealistic for families' financial decisions years later to depend substantially on such an effect.

within the observed variance in score density at nearby levels. This is reassuring but does not *per se* rule out self-selection. Second, in all of the analysis to follow, the test score we use is exclusively the test score from each worker's *first* attempt to pass the KLT. A small number of failers re-took the test in later rounds, and if we were to use scores from subsequent attempts, this would raise the possibility of workers self-selecting across the passing-score cutoff.[18] Third, the test was administered and scored by a Korean institution and we are not aware of any substantial reports of corruption or other irregularities in scoring or record-keeping.

5 New survey data

We conducted a new, purpose-built survey of the households of EPS-Korea job applicants who has scored near the passing threshold. Survey teams visited the households in February and March of 2010. Any knowledgeable respondent present at the time of the survey team's visit was allowed to complete the survey.[19]

The first five rows of Table 3 show that passing the language test had large effects on migration behavior by surveyed households. In the survey sample, barely passing the test causes a large increase in the probability that the household had an applicant who initially departed for Korea (68 percentage points); had a member in Korea at the time of the 2010 survey (40 percentage points); ever had a member in Korea, at the time of the survey or earlier (48–49 percentage points); has someone anywhere abroad at the time of the survey (26 percentage points); and ever had someone abroad (20–22 percentage points). A graphical representation is shown in the upper-left panel of Figure 3.

[18] Because the test-score we use is only the first-attempt score, a small number of those with failing scores are deployed to Korea. These are people who failed the first attempt but passed on subsequent attempts. This is of concern to external validity, but not internal validity.

[19] Cull and Scott (2010) find that survey responses on the financial life of Ghanaian households provided by knowledgeable respondents are as good as full household enumeration and better than responses from randomly selected respondents.

5.1 Sampling strategy and nonresponse

The potential sampling universe was all 3,201 households whose applicant had scored within five points of the cutoff ($-5 \leqslant s < 5$). From these we provided target addresses to the field enumerators in stages, to sample as close to the discontinuity as possible. First, we gave only the addresses of households whose applicant was within one point of the cutoff score. After the firm had exhausted those addresses we provided a second set of target households two points of the cutoff, and so on. Power calculations before the survey suggested a target sample size of roughly 900 households.[20]

The firm chose which households in each group were feasible to interview and attempted to reach and interview the feasible households. Thus it is important that the survey was "blind" in two senses. First, no one at the survey firm knew which households were those whose member passed and which were those whose member failed. The firm was only given the name, permanent address, and phone number of the applicant. The firm did not base its decision to attempt to reach any particular household on the treatment status of the household. Second, the survey enumerators and respondents were told only that the study was a "follow-up survey on families of POEA job applicants". Neither enumerators nor respondents were told that a goal of the study was to identify the effect of migration by a household member on the household. Survey respondents were not asked anything about the language test or that household's applicant's score.

In the end, enumerators attempted to locate the permanent addresses of 2,029 EPS applicants and obtained 899 completed surveys. There were two broad classes of reasons why one of the targeted households did not yield a completed survey.

The first class of reasons for noncompletion ($N = 508$) was that the survey team could not physically locate the address on the application form. This was often because the address was either partially illegible, incomplete, or erroneously digitized. In limited cases it was because

[20] Following Duflo et al. (2007) we estimated that 900 households would allow a 92% chance of detecting a 10 percentage-point difference in household-level school enrollment fraction, a 94% chance of detecting a 10 percentage-point difference in the fraction of household engaged in entrepreneurial activity, and a 93% chance of detecting a change of 0.2 in household-level ln remittances (all at the 5% significance level).

available maps, street signs, house numbers, questioning bystanders could not lead survey teams to the desired location. The second class of reasons ($N = 622$) was that the survey team, after locating the residence, could not acquire a completed survey. Including both classes of noncompletion, 44% of the targeted households resulted in a completed survey.

There is little reason to expect systematic nonresponse bias from the first class of noncompletion, the failure to locate a given physical address. Any information provided on the job application form—including the fact that the applicant wrote an address that was incomplete or difficult to physically locate—is predetermined and cannot have been affected by the results of the language exam. Only after reaching the physical address the enumerators sought could they have acquired facts about the applicant that could have been affected by treatment (all surveys were conducted in person). Considering only those households whose addresses could be physically located, 59% of the households where a survey was attempted resulted in a completed survey.

There is more potential for nonresponse bias from the second class of reasons for noncompletion of the survey. Once the target address is located, whether or not a survey is obtained is not predetermined and could be affected by the test outcome. For example, if passing the test results in migration, and the family uses the resulting remittances to buy a new home and move to another part of the city, this would cause nonresponse at a located address.

The recorded reasons for nonresponse at located addresses were that the current residents or neighbors indicated that the target family had moved away ($N = 256$); no one was home at two visits or the residents were not the job applicant's family and did not know about the applicant's famliy ($N = 215$); the applicant's family was present but declined to complete the survey ($N = 149$); or the applicant had died ($N = 2$).

5.2 Checking the discontinuity: Survey sample

We test for nonresponse bias in two ways: by checking for a discontinuity in the survey completion rate for households in the sampling universe, and checking for a discontinuity in baseline predetermined applicant characteristics among the applicants whose households completed

the survey.

The last row of Table 2 tests for a discontinuity in the survey completion rate among all targeted households. The completion rate does not significantly change at the cutoff score. The lower-right panel of Figure 1 shows the same result graphically. This result is incomptabile with any large effect of working in Korea on relocation by the applicant's family prior to the survey.

Even if barely passing the test did not change the overall response rate, it could still in principle change the composition of households completing the survey. We thus test for a discontinuity, in the survey sample, in the predetermined baseline traits of the applicants from each household.

The remaining tests in Table 3 show no evidence of nonresponse bias in the survey sample. There is no statisitically significant discontinuity, at the cutoff score, in any of the predetermined observed traits of applicants from these households. A representative row of the table is shown graphically in the upper-right panel of Figure 3: There is no discontinuity in the responding households' applicants' baseline education levels. The rows of Table 3 on geographic location are shown graphically in the maps of Figure 4 (nationwide) and Figure 5 (zoomed in to the National Capital Region). These maps show that the locations of the 899 households in the survey sample are similar on both sides of the discontinuity.

Collectively, this evidence suggests that having a household member barely pass the test is a strong source of exogenous variation in household exposure to having that member work in Korea.

6 Quasi-experimental reduced-form effects

We now report parametric and nonparametric RDD regressions, sharp (ITT) and fuzzy (TOT), using the job applicant's Korean Language Test score as the running variable. We consider first the effects of test-passing and migration on households, then the effects on individual adults, and finally the effects on individual children. In most tables, the first four columns

of estimates show the parametric results, $\hat{\beta}_{\text{ITT}}$ and $\hat{\beta}_{\text{TOT}}$ from equations (6, 7); the last four columns show the nonparametric results, $\hat{\gamma}_{\text{ITT}}$ and $\hat{\gamma}_{\text{TOT}}$ from equations (8, 9).

"Treatment" is defined as a household in which any member *ever* worked in Korea—at the time of the survey or earlier. Defining treatment in this way, rather than as current presence of a household member in Korea, prevents self-selected return migration from being a source of endogenous treatment. An alternate definition of "treatment" is explored in section 8. Descriptive statistics are reported in Appendix Tables 1 and 2.

6.1 Effects on households

Having the applicant pass the test and migrate to Korea has important effects on household composition (Table 4a).[21] The most robust finding across specifications is the effect on gender balance in the household. This is unsurprising since 80% of applicants in the sample are male. Passing the test causes an 8–10 percentage-point increase in the fraction of working-age adults in the household who are female. Ever having a member in Korea causes a 16–21 percentage point increase in this fraction. Considering only households in which the applicant was married at the time of application, these effects are larger: 9–15 and 19–29 percentage-points, respectively. Among households where the applicant was initially unmarried, passing the test and migration also cause substantial rises in the fraction of household members who are children.[22]

Passing the test and migration have important effects on household income (Table 4b). To estimate these effects, we transform Philippine peso quantities with the inverse hyperbolic sine (*asinh*), which dominates other transformations in a setting where the outcome frequently takes zero values.[23] In the first row, there is no statistically significant effect on total household

[21] The composition of the household refers to the composition of household members who are *not* in Korea at the time of the survey.

[22] This does not appear to be because passing the test and migration cause marriage and childbearing among initially unmarried applicants, because we observe no significant ITT or TOT effect on marriage of the applicant (results not shown). It may be the case that young members of the extended family join the household when a young adult migrates; our survey instrument did not capture the exact familial relationship between all members of each household, so this cannot be definitively demonstrated.

[23] The alternatives have many drawbacks. Using linear pesos has the disadvantage of sensitivity to single observations of large quantities; using a log transformation has the disadvantage of throwing away information by

income—where again the household includes only those members not currently in Korea. The following rows reveal why: passing the test and migration cause large increases in remittance income that are offset by decreases in non-remittance income. The decrease in non-remittance income appears to arise from a decrease in wage income (statistically significant only in the parametric specifications, for all households) and a decrease in business income from agricultural activity (significant in all specifications at the 10% level, only for the households of initially-married applicants).

What is the mechanism for these effects on non-remittance income? Recall the mechanisms implied by equation (5). Mechanically, migration by the applicant removes the applicant's own home-country wage or business earnings from household income. Migration could also change non-migrants' labor supply decisions, either through the income effect of remittances or by changing household decision-making on matters affecting income. Finally, remittances could alleviate capital constraints on income-generating activity. Analysis in the sections to come will seek to shed some light on these mechanisms. For now, suffice it to say that there is little evidence that migration and remittances—bundled together—cause new entrepreneurial income by these households in this timeframe (3–5 years). It may be the case the remittances cause investment in entrepreneurial activity among households *conditional on the remitter having migrated* (as in Yang 2008) but here the fact that migration removes a potential participant in entrepreneurial activity appears to offset any such effect.

Finally, passing the test and migrating cause important changes in household expenditure, savings, and borrowing (Table 4c). They cause important increases in particular categories of household expenditure, large rises in savings (for households with initially unmarried applicants), and decreases in borrowing from other family members (for households with initially married applicants). Migration by the applicant causes households to spend hundreds of percent more on education and health care, and approximately 30–37 percent more on "quality of life" purchases like transportation, clothing, and ceremonies. There are no significant ef-

truncating zeros—which are common here, such as for business income and remittance income. Regression coefficients on variables transformed with the inverse hyperbolic sine can be interpreted identically to those using the traditional log transformation (as approximating percent changes) for any peso quantity encountered in practice—since $\frac{d}{dx} asinh x = \frac{1}{\sqrt{1+x^2}} \approx 1/x = \frac{d}{dx} \ln x, \forall x \gtrsim 2$. But unlike the log transformation, the inverse hyperbolic sine has desirable properties near zero and is defined at zero ($asinh\, 0 = 0$). More in Burbidge et al. (1988) and MacKinnon and Magee (1990). The magnitudes of coefficient estimates in this paper are robust to the use of the log transformation, though results with *asinh* use all available data and show greater statistical precision.

fects on spending on food, drink, and tobacco, or on purchases of durable goods.[24] Migration causes the household to be seven percentage points less likely to have borrowed from family members, in the preceding six months, for non-business purposes. The effects on savings, "quality of life" spending, and borrowing appear concentrated in households where the applicant was initially unmarried. Migration may also reduce borrowing from non-family for non-business purposes by 9–23 percentage points among households with initially-married applicants, but these estimates are only significant at the 10% level.

Again, equation (5) suggests various mechanisms for these effects. For example, if migration causes more spending on health and education, this could be because remittances alleviate capital constraints on health and education spending, or because migration changes who makes household decisions. The same could be true of borrowing. The sections to come will approach these questions.

We note important differences between the effects of migration on reported income and reported expenditure. Migration causes remittances at a level that roughly replaces the cash income that migrants would have brought to the household if they had not migrated. This includes, if the survey question is correctly answered, in-kind remittances such as purchased gifts. But migration causes increases in reported expenditure without increases in reported income, without appearing to cause increases in borrowing.

This disparity might arise from underreporting of specific types of income due to difficulties in eliciting information from survey respondents. For example, if a migrant used overseas earnings to directly pay for a child's school fees, the survey respondent might not think of this as a remittance—it was not sent from Korea into the hands of the survey respondent—but is likely to report this expense when asked how much the household spends on education. For many related reasons the literature broadly considers the economic well-being of the poor to be more accurately reflected by expenditure and consumption measures rather than income measures, in both developed and developing countries (e.g. Chen and Ravallion 2007; Meyer

[24] "Food" = food, beverages, and tobacco. "Health & educ." = school, medicine, and medical care. "Quality of life" = fuel, transportation, household & personal care, clothing, recreation, family occasions, gifts. "Durables" = durable goods, taxes, home improvement. "Savings" includes deposits in banks, paying off loans, making loans to others.

and Sullivan 2008).

6.2 Effects on adults

The next table explores the effects on labor market outcomes for individual working-age adults in the household: the applicant, all non-applicant adults, and the applicant's spouse (Table 5).

Passing the test and migration have very large effects on labor market outcomes for the applicant. Wage income rises by hundreds of percent.[25] This is primarily because passing the test causes the applicant to be 28–31 percentage points more likely to be abroad, and 40–43 percentage points more likely to be in Korea at the time of the survey.

We do not observe statistically significant effects on labor market outcomes for non-applicant working-age adults in the household, or for applicants' spouses. In the parametric specfication there are negative, statistically insignificant coefficients for spouses' having worked in the previous six months and on spouses' wage income. The same coefficients are positive and statistically insignificant in the nonparametric specification, suggesting that the estimates are sensitive to an assumption of a locally linear relationship between the outcome and the running variable. The coefficients on spouses' days of work in the previous month are positive and statistically insignificant in both specifications.

Finally, we test whether the applicant's passing the test or migrating has an effect on migration by other adults in the household. We might expect a positive effect if having one member migrate encourages other members to migrate, such as through alleviating constraints on credit or information that prevent migration. Peer effects of program participation can cause its effects to snowball to a magnitude larger than the individual effect (e.g. Dahl et al. 2014). We might expect a negative effect if migrants regret the experience and pass information to other household members that reduces their migration. We observe no statistically signficant effects one way or the other.

[25] The simple average of applicants' monthly wage income is 4,698 pesos per month if they failed the test, and 26,986 pesos per month if they passed.

6.3 Effects on children

Table 6 proceed to test for effects on children in the applicant's household. The most robust finding is that passing the test and migration cause large increases in the fraction of children whose school is private rather than public, and who are taken to private clinics rather than public clinics when they are sick.

Migration by the applicant causes a 29–34 percentage-point increase in the probability that a child in the household attends a private school, conditional on attending any school. Migration also causes a 37–58 percentage-point increase in the probability that a child in the household is taken to a private health facility, conditional on being taken to any health facility in the previous month. (In the parametric specification this latter effect is only significant at the 10% level.) These are unlikely to reflect quantity-quality tradeoffs in child-rearing because we do not observe significant overall effects on the fraction of household members who are children (Table 4a).

Restricting the sample to only children who are children of the applicant and/or the applicant's spouse results in very similar coefficient estimates for these effects on private schooling and private health care, though larger standard errors (the sample is cut by roughly half). There is suggestive evidence that in this restricted sample children are caused to earn a greater number of awards at school, though these estimates are only statistically significant at the 10% level and only in the nonparametric regressions. This is not an objective measure of performance at school, since it is possible that private schools give awards more frequently.

There is no statistically signficant effect of the applicant's migration on whether or not anyone reads to children in the evening, the respondent's desired years of education for children, child labor, or child school enrollment. 91% of school-age children are already in school in this sample.

7 Mechanisms of the reduced-form effects

Equation (5) suggests that reduced-form effects on household financial decisions could arise not only from changes in household income but also from shifts in household decision-making power when married workers migrate. We first conduct quasi-experimental tests of whether migration by the applicant affects who makes household decisions. We then conduct non-experimental tests of the degree to which changes in decision-making can explain changes in household finance.

Table 7 tests the effect of passing the test and migration on the role of the applicant in household decisions, as subjectively assessed by the survey respondent. Respondents were asked who bears the principal responsibility for household decisions in five areas. Their response options for each area were: 1) themselves, 2) another identified member, or 3) shared decision-making by themselves and another identified member. They were not prompted to consider the applicant in particular. The outcome variable in this table is an indicator for whether or not the job applicant is a principal or shared decision-maker in each area.

There are large and significant effects in households where the applicant was married at the time of application; there are no signifcant effects in households where the applicant was initially unmarried. For example, migration causes initially married applicants to be 60–64 percentage points less likely to the primary or shared decisionmaker for childcare, 58–67 percentage points less likely for major purchases, and 43–67 percentage-points less likely for entrepreneurship. (This last coefficient is statistically signficant only in the parametric specification.)[26]

Given that changes in household decision-making power can affect financial decisions in Fil-

[26] It is possible that treatment could change reported decision-making power only by changing who is responding to the survey, because different respondents report different opinions of decision-making power. For example, it has been observed in other contexts that wives report different patterns of household decision-making than husbands. The magnitudes of these differences are not typically large (e.g. Becker et al. 2006). Nevertheless, for this reason we check the robustness of Table 7 by controlling, in the parametric regressions, for the gender of the respondent. Appendix Table 3 reports these results. The results change little. The coefficients in the regressions for initially married applicants fall slightly in magnitude—by 5–10 percentage points—but remain strongly statistically significant for all categories except home repairs. The coefficients for initially unmarried applicants remain insignificant.

ipino households (Ashraf 2009), it is plausible that these large effects of migration on decision-making could affect financial decisions independently of their effect on household income. This policy discontinuity does not allow an experimental test to unbundle these effects, since treatment means that changes in income and decision-making are always bundled. We can offer suggestive, observational evidence on the relative importance of the income and decision-making channels.

Table 8 conducts these non-experimental tests with the Gelbach (2014) decomposition. This decomposition estimates

$$\hat{\delta} = \hat{\beta}_{\text{ITT}}^{\text{base}} - \hat{\beta}_{\text{ITT}}^{\text{full}} \tag{10}$$

for each of three sets of possible mechanisms, where $\hat{\beta}_{\text{ITT}}^{\text{base}}$ is the estimate from equation (6). In each case, $\hat{\beta}_{\text{ITT}}^{\text{full}}$ is the estimate from the same regression when the vector of controls X is augmented to include controls for channels through which the reduced-form effect might act. For the 'remittance income' channel, the additional control is *asinh* total remittance receipts by the household. In the 'non-remittance income' channel, the additional control is *asinh* all other income. In the 'decision-making' channel, the additional controls are a vector of five household-level dummies, each of which equals 1 if the applicant is primary or joint decision-maker in that area for the housedhold, in each of the five areas examined in Table 7. The estimates $\hat{\delta}$ are an observational measure of the portion of the reduced-form effect that passes through each channel (analogous to Baron and Kenny 1986; Imai et al. 2011).[27]

Recall the three channels of effect on financial decisions posited by equation (5). First, remittances can alleviate capital constraints. Second, migration can reduce non-remittance household income by reducing labor supply. In theory this can occur both mechanically (through the absence of the migrant) and by reducing labor supply by non-migrant household members (with an income effect), though we see no evidence for the latter possibility in Table 5. Third, the absence of the migrant can change who makes household decisions.

[27]The standard errors rest on the conditional sphericality assumption: that the added controls in the full specification (the potential mechanisms) are orthogonal to the controls in the base specification. This assumption is reasonable if we omit the controls X for baseline characteristics (applicant's age at the time of application, and so on), because passing the test is orthogonal to these traits (Table 3). For this reason the regressions in Table 8 omit the baseline controls X from equation (6). Regardless, the results do not substantially change when baseline controls are included (results not shown).

The decomposition in Table 8 suggests that all three mechanisms shape household financial decisions in households where the applicant was initially married, but that in households where the applicant was initially unmarried, the remittance income mechanism dominates.

In the sample of all households, we see the rise in remittance income tending to cause a rise in expenditures while the fall in non-remittance income (due to the absence of the migrant) tending to cause a fall in expenditures. The net effect, as in Table 4c, is that there is no robust and significant evidence of an overall increase in household expenditures. In some categories of spending—notably education and health—the mechanism of the rise in remittance income dominates the fall in non-remittance income. In the sample of all households, the decision-making channel only contributes significantly to the reduced-form effect for "quality of life" expenses.

This is quite different when we restrict the sample to households where the applicant was initially married. Here, substantial portions of the causal relationship between passing the test and household financial decisions appear to occur because migration alters intrahousehold decision-making power. Recall from Table 4c that for initially-married applicants passing the test causes health and education spending by the household to rise by about two thirds ($\ln(1+0.91)=0.65$), some of which appears to be for children's private schooling and private health care (Table 6). Migrants' remittances are not the only mechanism for that reduced-form effect, according to the decomposition for initially-married applicants in Table 8. Eight percentage-points of the effect are caused by the fact that passing the test causes a large decrease in applicants' responsibility for household decisions, independently of remittance income.

Because 80% of applicants are male and this is the sample of initially-married applicants, in most cases this means that migration causes decision-making power to shift from husband to wife. This is compatible with different preferences for health and education spending between husbands and wives. It also suggests that the effects on spending for children's private schooling and health care in Table 6 may not reflect only the alleviation of capital constraints on investment in children, but a more nuanced bundle of mechanisms.

Likewise, among initially-married applicants, an important part of the effect of passing the

test on borrowing from family appears to arise through changes in household decision-making, independently of remittance income. This could arise from effects on both supply and demand for family loans: for example, applicant's wives might be less interested in borrowing from family than husbands are, or less able to borrow from family than husbands are.

At the bottom of the table, it is clear that the remittance channel dominates in households where the applicant is initially unmarried. This is intuitive: unmarried applicants are more likely to be living with their parents. They are thus less likely to be a major breadwinner for the household (thus effects on non-remittance income are less important) and they are less likely to play a central role in household decisions (thus effects on decision-making are less important).

The coefficients in Table 8 strongly suggest that in the households of initially-married applicants, the effects of migration on household decision-making are important and separate from its effects on income. But the relative sizes of the coefficients cannot reliably establish the relative importance of the different mechanisms, for at least two reasons. First, the decision-making dummies are only a rough proxy for the degree to which household decision-making changes. Variance in those proxies can only approximate variance in unobserved determinants of relative decision-making power. Second, we cannot be sure that all money earned by the applicant in Korea but spent in the Philippines is reported as remittances by the respondent. As discussed above, if the applicant pays private school fees directly, it may not have occurred to the survey respondent to report this as money sent to the household. Both of these possibilities are suggested by the fact that the sum of the component effects in the decomposition often explains only the minority of the reduced-form effect.

8 External validity

The external validity of these results is circumscribed by a number of considerations.

First, households are observed 3–5 years after the language test result allocated migration opportunities (Table 1). Thus any effects in the long term or the very short term are not

observed. This has certain advantages: for example it is possible that applicants who went to the time and expense of studying the Korean language but barely failed the test could be financially harmed—such as if they paid for a Korean class and lost the investment. This would tend to inflate the measured financial benefits of treatment. But because households are interviewed at least three years after the test result, the harm would need to persist for years in order to affect these impact estimates. We consider this unlikely. The time window has disadvantages as well: It is possible, for example, that effects on entrepreneurial activity show up years after migration. This research design cannot detect them.

Second, the analysis thus far has defined 'treatment' as a household having *ever* had a member work in Korea—which includes households with a current migrant in Korea and households with a return migrant from Korea. Another population with a different propensity for return migration could thus exhibit different treatment effects. Table 9 repeats part of the analysis in Tables 4b, 4c, and 5 with 'treatment' redefined as the applicant's *current* presence in Korea. The results are broadly similar. This is to be expected, since about four fifths of the applicants who have ever been in Korea are still in Korea at the time of the survey (Table 3, rows 2–3). Batch 1 applicants are more likely to have finished their time in Korea by the time of the survey than batch 5 applicants; this is another reason why the parametric regressions above contain a set of dummies for batch number.

Third, this is a temporary migration program, and these results need not apply to permanent migration. Under the EPS agreement workers do not have an option to migrate permanently to Korea. Theory suggests that households counting on longer-term migration opportunities might adjust expenditure to a greater degree (equation (3)). Theory further suggests that the effects depend on the degree to which migration alters patterns of decision-making power in the household (equation (4)). Thus the effects could be different—for example—in households where pre-migration responsibility for decisions is more shared than in households where it is more exclusive to members who migrate.

Fourth, workers that self-select for overseas work, and moreover are willing to learn basic Korean, could be different from average households. While the effects measured here are policy-relevant in the sense that they plausibly apply to marginal new participants in the

program—say, if the cutoff score were changed—they need not apply to average Filipinos. Table 10 explores how the households of test-failers in the survey sample differ from the same outcomes in a nationally-representative survey conducted by the Philippine government.[28] We leave out test-passers so as to remove the effects of EPS-Korea migration.

Households in the survey sample are much more likely to already have a member abroad than typical households in the Philippines. Sample households have somewhat more income (about 35% more) than typical households, a difference entirely accounted for by the fact that they have more remittance income. Sample households are less likely to have monthly savings (and when they save, save less), are much less likely to have businesses, and live in somewhat better-quality houses. They are more likely to be in Luzon. Their heads of household are younger and have 3.5 years more education, and their children are 12 percentage points more likely to be in school.

In short, relative to the country as a whole, the survey sample captures households that have similar incomes in the absence of remittances, have more experience with migration and thus somewhat higher incomes due to remittances, are more likely to invest in human capital and work for wages than to run a business, and save less. The broad pattern is that households in the survey sample emphasize investments in human capital (education, migration) over entrepreneurship.

This research design cannot answer several questions about the effects of migration. It cannot measure how the effect depends purely on the gender of the migrant for theoretical reasons (women who self-select to apply for an overseas job could be quite different from men who do so) and empirical reasons (the applicant is female in only 20% of sampled households). The design also cannot measure any external effects, positive or negative, on other households—households from which no member applied to an EPS-Korea job. It cannot measure the effect of strategic decisions made prior to migration caused by foresight of the future option to migrate (Batista et al. 2012). And it cannot reliably measure the effects of migration experience

[28] We use a household-matched nationally representative sample from the 2006 Family Income and Expenditure Survey (FIES) and Labor Force Survey (LFS). 2006 is the most recent matched FIES-LFS microdata publicly-available from the National Statistical Office at the time of writing. We inflate all peso figures from 2006 to 2010 using the Consumer Price Index.

on return migrants (Reinhold and Thom 2011), because return migrants are self-selected from current migrants and, in this sample, small in number.

9 Non-experimental estimators of reduced-form effects

Could the preceding effects have been well-identified without a quasi-experiment? It is plausible that the households who self-select to apply migrate in general, and through this program in particular, could differ in many ways from other households. If all such differences were observable, such as the differences seen in Table 10, quasi-experimental methods like RDD would have less value.

We follow LaLonde (1986), Smith and Todd (2005), McKenzie et al. (2010), and others in constructing analogous nonexperimental tests of migration treatment effects by using the nationally representative data in Table 10 to construct a synthetic control group. That is, we create a new dataset that retains only treated households from the survey sample and stacks them with *all* households in the nationally representative sample.[29] We then estimate treatement effects with ordinary least squares (OLS) and propensity score matching (PSM), for comparison to the RDD estimates.

Table 11 shows OLS and PSM estimates for several of the outcomes in Tables 4b, 4c, 5, and 6. The OLS controls and PSM matching variables describe the age and education of the household head, the location of the household, and the materials used to build the residence.[30] This is certainly not an exhaustive catalog of controls that could be used, but collectively these observable traits might be thought to proxy for a substantial part of the variance in unobserved determinants of income and expenditure.

The ITT effects estimated by these observational methods are markedly different. In the first two rows of Table 11, OLS and PSM find that having an applicant pass the test causes a

[29] This plausibly assumes that the fraction of Filipino households that have had a member in Korea is very small. The FIES-LFS data contain an indicator of whether or not household members are currently abroad, but do not contain information on specific destinations nor on past migration experience.

[30] These controls are: household size, HoH (Head of Household) age, HoH years educ., plus dummies for HoH female, HoH married, standalone house, family owns residence, strong wall materials, strong roof materials, and three regions (one region omitted).

large and statistically significant decline in labor supply by other working-age adults in the household, as measured by having worked in the past six months or by days of work in the previous month. These effects are not seen in Table 5. This suggests that the households whose members self-select into this the EPS Korea migration opportunity have other, unobserved traits that make non-applicant members mildly less likely to work. This unobserved difference could be mistaken, in any observational study, for an effect of program participation.

In the next row, OLS and PSM likewise find that having an applicant pass the test causes a statistically significant increase in the probability that children in the applicant's household attend school. No such effect is seen in the quasi-experimental results of Table 6. It could be that families that self-select into this program have unobserved traits—such as aspirations for their children's economic advancement—that cause greater school enrollment for children independently of the program, and that are not controlled away by observable traits like the education level of the household head. Non-experimental studies could then falsely attribute a positive effect on children's overall school enrollment to a program of this kind.

The remaining rows of Table 11 show various other conclusions that might be reached with OLS and PSM: passing the test causes total household income to decline, as remittance income fails to compensate for lost non-remittance income. Income from entrepreneurial activity—unaffected in the quasi-experimental estimates—appears to greatly fall. Both education and health expenditures and savings—which are positively affected in the quasi-experimental results—appear to *fall* by large amounts.

Some OLS and PSM specifications would surely perform better than others, and we test only one. It was set in advance and never altered—to rule out cherry-picking—and different specifications would yield different conclusions. But these results, analogously to LaLonde's and others since, do suggest the care that must be taken in giving strictly causal interpretation to coefficients in observational studies of migration. Families that self-select into migration, for example, might have traits that make them less apt for entrepreneurship at home. Unless aptness for entrepreneurship can be fully observed or accurately proxied for, this difference would be mistaken in observational studies for a negative effect of migration on entrepreneurship.

10 Conclusion

We find that the opportunity to have a member temporarily work in Korea has important effects on households in the Philippines within 3–5 years. These conclusions arise from the natural quasi-experiment of a rather sharp policy discontinuity, with thick data near the cutoff and little evidence of score manipulation or nonresponse bias.

The overseas work opportunity causes applicants' earnings to rise by hundreds of percent, without on average causing some of the offsetting harms to their households' finance and investment often discussed in theory and in observational studies. There are no significant effects on labor force participation by other household members, including applicants' spouses. But if the migrant households in the same data had been compared to observably similar Filipino households using non-experimental estimators, the analysis could have found significant negative effects on labor force participation. This suggests that households' self-selection on unobserved traits could be a first-order concern in observational studies of the effects of migration on households.

We furthermore find that migration of a household member to Korea causes important changes in expenditures by migrants' families. It causes them to spend much more on health and education, somewhat more on "quality of life" expenditures like transportation and ceremonies, and neither more nor less on food, drink, tobacco, and durables. Migration also reduces borrowing (in households of initially-married applicants) and raises saving (in households of initially unmarried applicants). Migration causes children in the household to be much more likely to be sent to private schools and private clinics than their public counterparts. A non-experimental decomposition of these reduced-form effects suggest that important portions of these effects arise not through changes in income but through changes in household decision-making power that occur when married couples are geographically split, even temporarily. This corroborates previous evidence that husbands and wives have different preferences, in the Philippines (Ashraf 2009) and in migrant households elsewhere (Ashraf et al. 2014).

We do not find migration to cause increases in entrepreneurial activity. Migration by initially-married applicants causes a fall in income from household-owned farming, livestock, forestry,

and fishing. This appears to be a mechanical consequence of the absence of an applicant who is a breadwinner in those areas. Migration does not affect agricultural income for the households of initially-unmarried applicants, and does not significantly affect non-agricultural business income for all households on average. This is not necessarily incompatible with the finding of Yang (2008) that remittances to the Philippines encourage some types of entrepreneurial activity. Yang tests the effects of remittances conditional on the household already having a migrant; the present work tests the effects of migration, which can both bring remittances into the household and take potential entrepreneurs out of the household.

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Table 1: The Korean Language Test

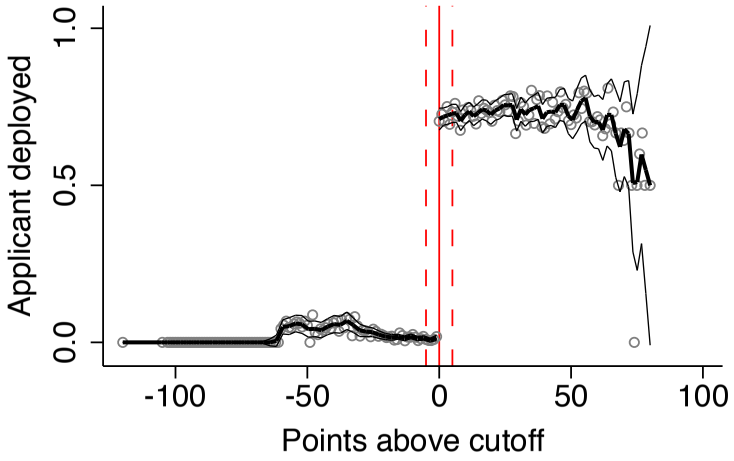
Batch	Date	Total #	$-5 \leqslant s < 5$
1	Sep 2005	411	56
2	Nov 2005	2,811	435
3	Jun 2006	6,110	1,045
4	Oct 2006	7,586	1,291
5	May 2007	8,402	589
Total		25,320	3,416

Table 2: Check discontinuity for 3,201 households in sampling universe with $-5 \leqslant s < 5$

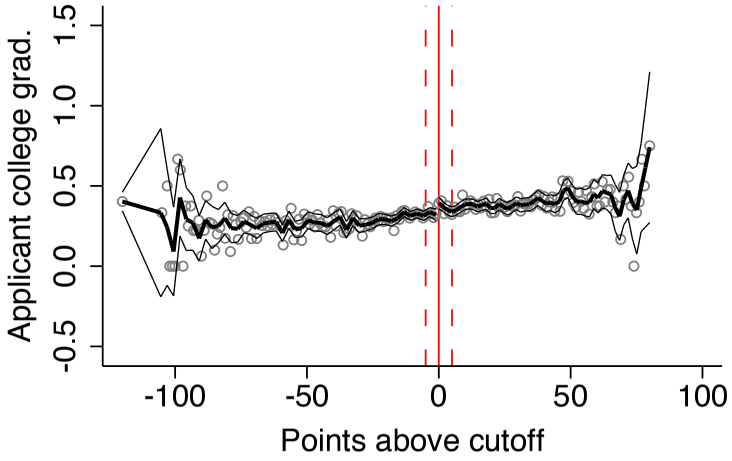
	Parametric		Nonparametric	
Outcome	$\hat{\beta}_{\text{ITT}}$	s.e.	$\hat{\gamma}_{\text{ITT}}$	s.e.
Migration behavior after application				
Applicant deployed?	0.699***	(0.024)	0.686***	(0.025)
Traits of applicant at the time of application				
Age	−0.320	(0.319)	−0.309	(0.438)
Female?	−0.003	(0.029)	0.007	(0.031)
College grad.?	0.083**	(0.034)	0.062*	(0.037)
Months experience	−1.813	(3.929)	2.245	(7.521)
Employed?	−0.025	(0.032)	0.008	(0.061)
Married?	−0.006	(0.035)	−0.017	(0.038)
Test batch 1	−0.017*	(0.009)	−0.013	(0.010)
Test batch 2	0.006	(0.025)	−0.006	(0.028)
Test batch 3	0.002	(0.032)	0.065	(0.060)
Test batch 4	0.028	(0.034)	0.005	(0.065)
Test batch 5	−0.018	(0.026)	−0.027	(0.028)
Survey response				
Completed survey?	−0.019	0.050	0.120	0.081

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Figure 1: Discontinuities in sampling universe



N = 23448, bandwith = 2.113



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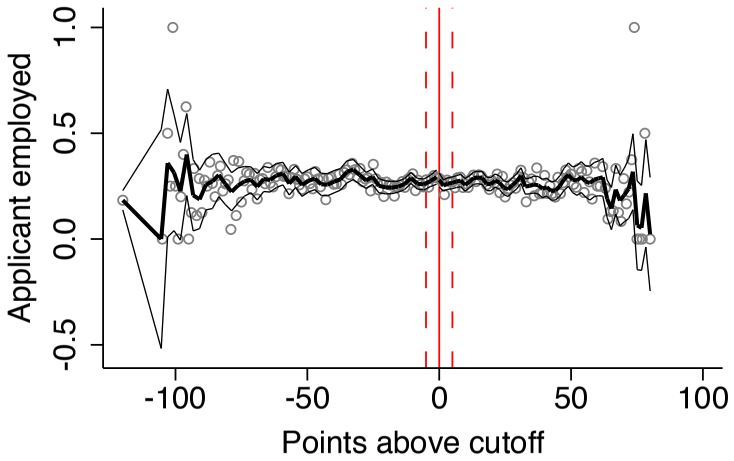
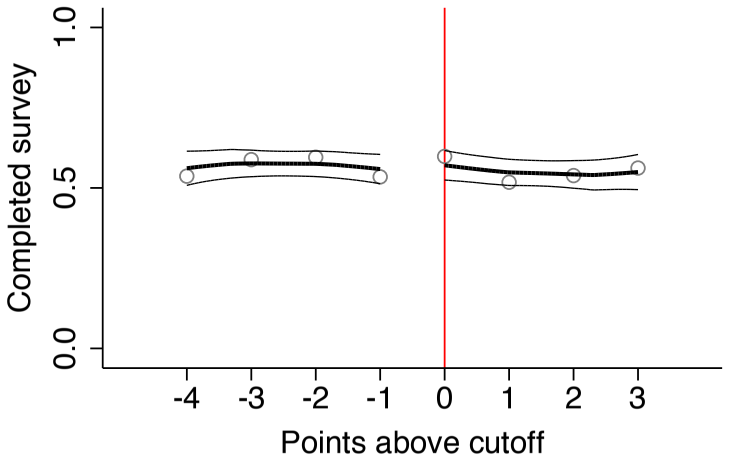


N = 23448, bandwith = 2.211



Good addresses only. N = 1532, bandwith = 2.360

Figure 2: McCrary nonparametric test for score manipulation

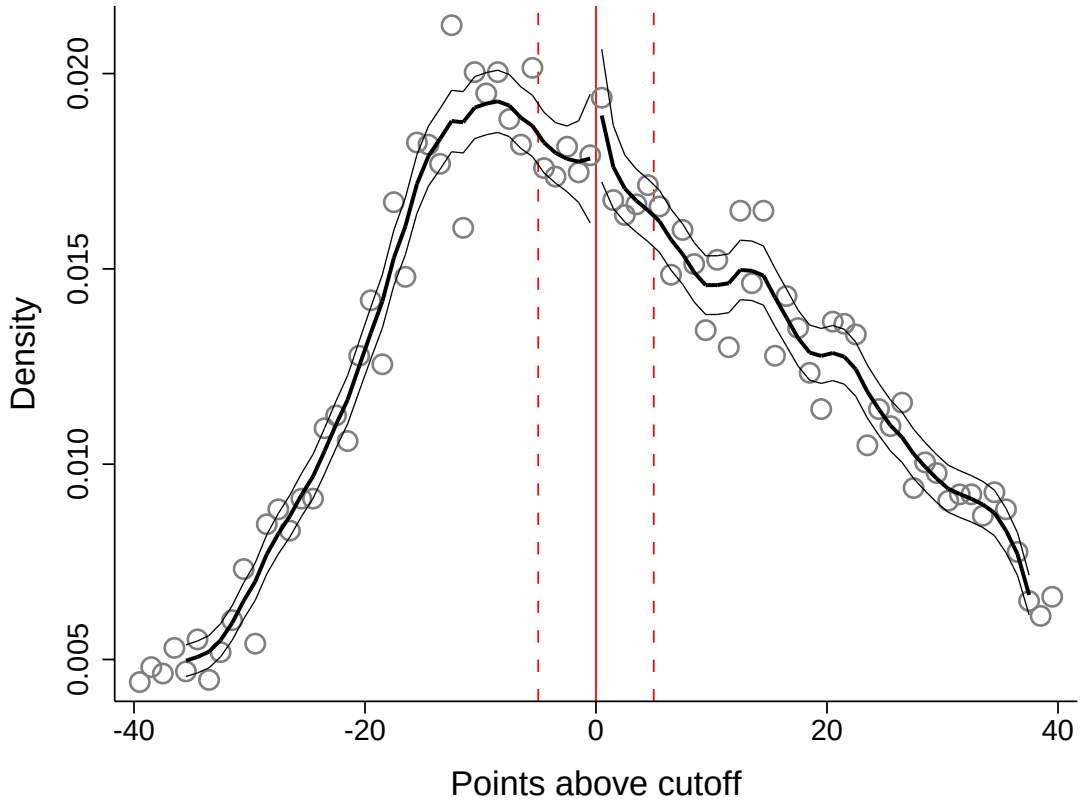


Table 3: Check discontinuity for 899 households in survey sample

	Parametric		Nonparametric	
Outcome	$\hat{\beta}_{\text{ITT}}$	s.e.	$\hat{\gamma}_{\text{ITT}}$	s.e.
Migration behavior after application				
Applicant deployed?	0.677***	(0.042)	0.683***	(0.041)
Anyone now in Korea?	0.404***	(0.053)	0.402***	(0.054)
Anyone ever in Korea?	0.492***	(0.053)	0.480***	(0.055)
Anyone now abroad?	0.262***	(0.058)	0.256***	(0.061)
Anyone ever abroad?	0.222***	(0.051)	0.199***	(0.052)
Traits of applicant at the time of application				
Age	0.615	(0.544)	0.450	(0.556)
Female?	−0.050	(0.050)	0.006	(0.052)
College grad.?	0.035	(0.058)	−0.031	(0.059)
Months experience	2.438	(6.573)	10.933	(6.808)
Employed?	−0.005	(0.054)	0.042	(0.053)
Married?	−0.003	(0.061)	0.025	(0.110)
Region: NCR	−0.003	(0.052)	−0.034	(0.049)
Region: Luzon	−0.017	(0.059)	0.012	(0.058)
Region: Visayas	0.010	(0.034)	0.014	(0.037)
Region: Mindanao	0.011	(0.021)	0.008	(0.024)
Test batch 1	−0.022	(0.016)	−0.019	(0.016)
Test batch 2	0.000	(0.042)	−0.076	(0.077)
Test batch 3	−0.038	(0.055)	0.071	(0.099)
Test batch 4	0.078	(0.060)	0.009	(0.107)
Test batch 5	−0.018	(0.047)	−0.010	(0.048)

Data for households in survey sample. $N_{\text{fail}(s<0)} = 460$, $N_{\text{pass}(s\geqslant 0)} = 439$.
NCR = National Capital Region. "Luzon" omits NCR. "Anyone" means any household member.
Parametric ITT estimate of β_{pass} from OLS regression of outcome on dummy for passing test, without covariates.

Figure 3: Discontinuities in survey sample

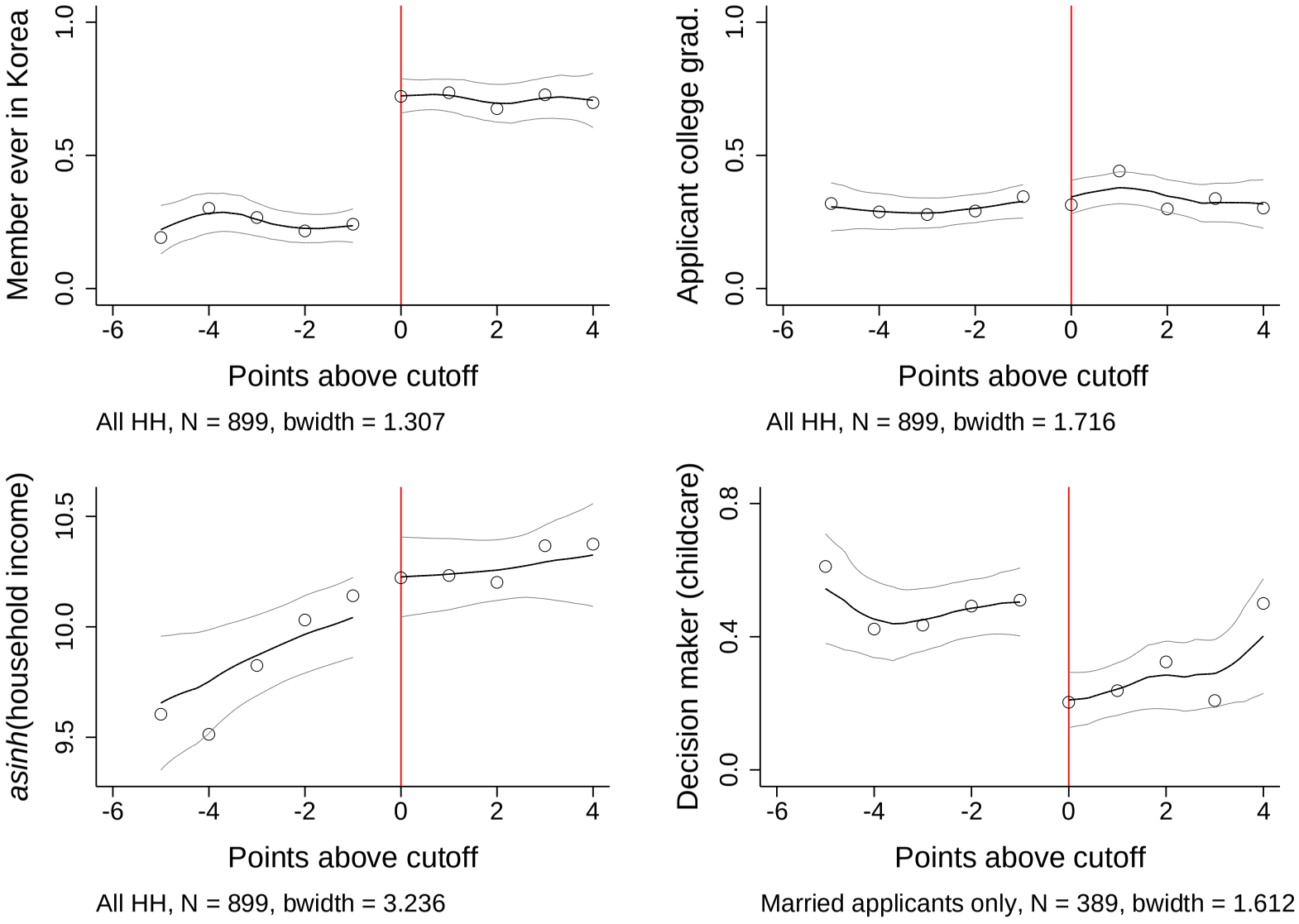
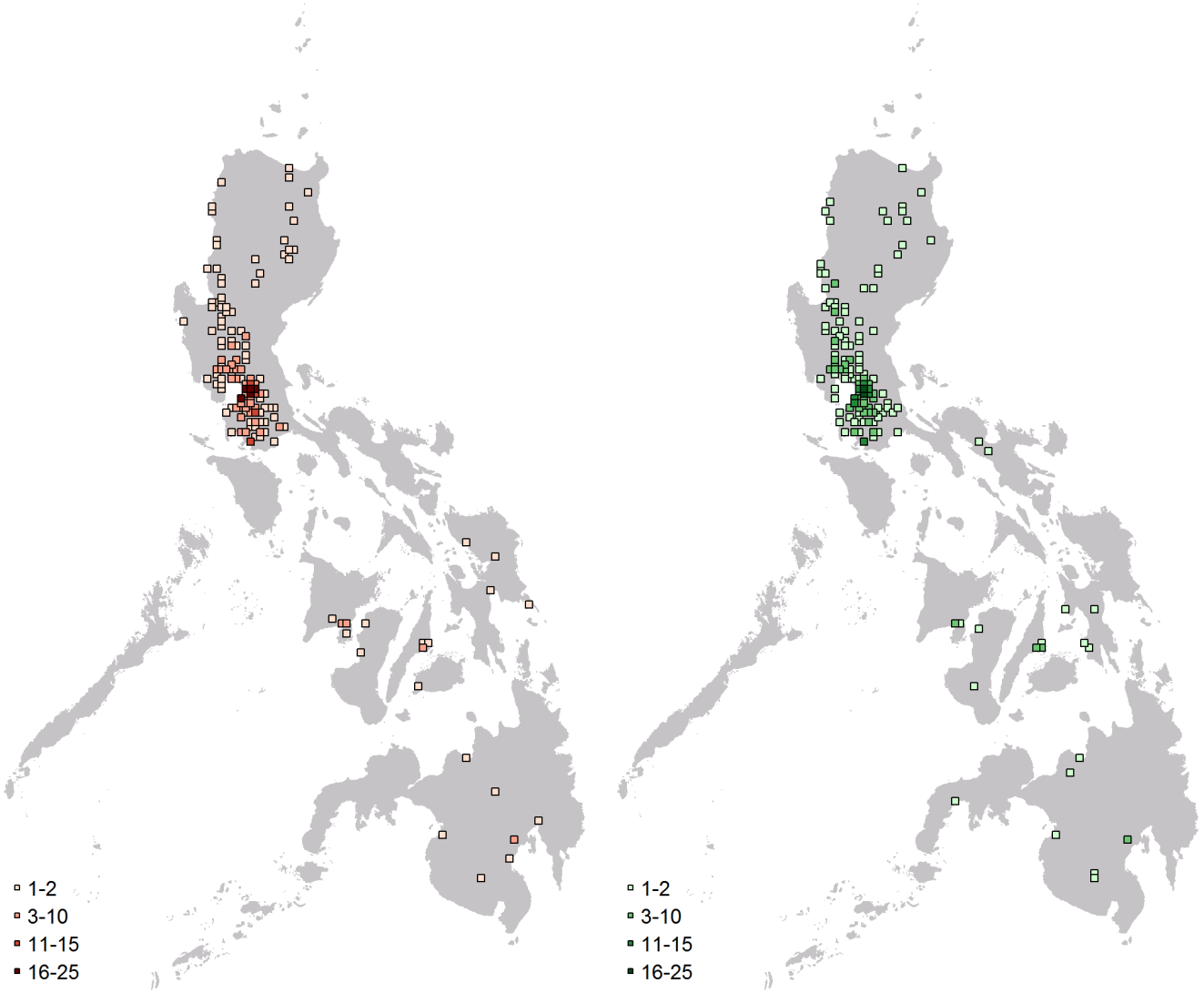


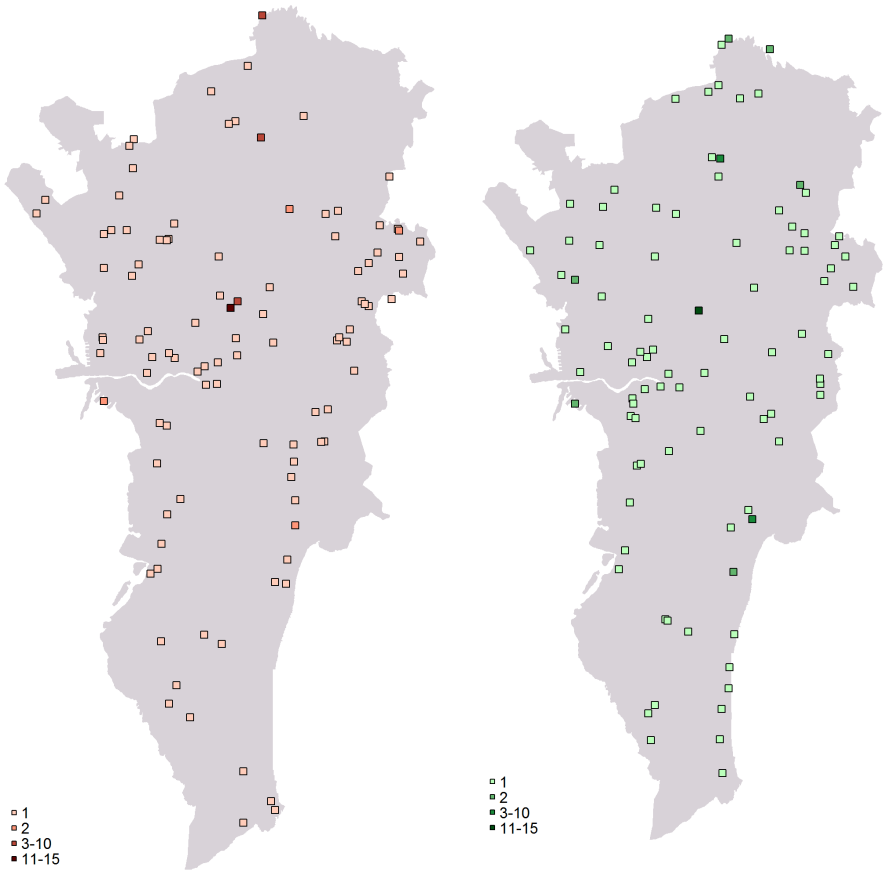
Figure 4: Locations of sample households: nationwide



(a) Barely failing

(b) Barely passing

Figure 5: Locations of sample households: National Capital Region only



(a) Barely failing (b) Barely passing

Table 4a: Impacts on households: Composition of the household (omitting members in Korea)

	Parametric				Nonparametric			
Outcome	$\hat{\beta}_{\text{ITT}}$	s.e.	$\hat{\beta}_{\text{TOT}}$	s.e.	$\hat{\gamma}_{\text{ITT}}$	s.e.	$\hat{\gamma}_{\text{TOT}}$	s.e.
All households								
Frac. adult, 18 ⩽ age <65	−0.050	(0.026)	−0.102	(0.053)	−0.062**	(0.030)	−0.129**	(0.062)
Frac. child, age < 18	0.044	(0.024)	0.089	(0.049)	0.051	(0.029)	0.106	(0.059)
Frac. elderly, age ⩾ 65	0.007	(0.018)	0.013	(0.035)	0.011	(0.019)	0.023	(0.041)
Frac. female, 18 ⩽ age < 65	0.080***	(0.030)	0.163***	(0.058)	0.100***	(0.030)	0.209***	(0.061)
Households with applicant initially married, only								
Frac. adult, 18 ⩽ age <65	−0.021	(0.039)	−0.044	(0.080)	−0.058	(0.066)	−0.129	(0.143)
Frac. child, age < 18	0.007	(0.038)	0.016	(0.079)	0.029	(0.068)	0.064	(0.147)
Frac. elderly, age ⩾ 65	0.014	(0.017)	0.029	(0.037)	0.019	(0.018)	0.039	(0.036)
Frac. female, 18 ⩽ age <65	0.090**	(0.044)	0.193**	(0.084)	0.147***	(0.046)	0.294***	(0.082)
Households with applicant initially unmarried, only								
Frac. adult, 18 ⩽ age < 65	−0.068	(0.035)	−0.134	(0.070)	−0.074	(0.038)	−0.162	(0.086)
Frac. child, age < 18	0.067**	(0.031)	0.132**	(0.062)	0.069**	(0.029)	0.151**	(0.068)
Frac. elderly, age ⩾ 65	0.001	(0.028)	0.002	(0.055)	−0.014	(0.055)	−0.030	(0.120)
Frac. female, 18 ⩽ age < 65	0.048	(0.040)	0.096	(0.075)	0.013	(0.068)	0.029	(0.148)

All fractions include only household members not in Korea at the time of the survey.
$Nfail = 460$, $Npass = 439$ Of 899 total households, in 400 the applicant was married at the time of application.
Of married applicants, 52 of 400 are female. Of unmarried applicants, 127 of 499 are female
'Treatment' = household ever had a member in Korea. ITT = intent-to-treat effect, TOT = treatment-on-treated effect.
All variables at household level. Working age means 18 ⩽ Age < 65. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 4b: Impacts on households: Income

	Parametric				Nonparametric			
Outcome	$\hat{\beta}_{\text{ITT}}$	s.e.	$\hat{\beta}_{\text{TOT}}$	s.e.	$\hat{\gamma}_{\text{ITT}}$	s.e.	$\hat{\gamma}_{\text{TOT}}$	s.e.
All households								
asinh All income	−0.081	(0.208)	−0.163	(0.419)	−0.056	(0.295)	−0.116	(0.616)
Remittance inc. only	2.179***	(0.594)	4.423***	(1.124)	2.376***	(0.616)	4.949***	(1.209)
Non-remittance inc. only	−0.786**	(0.317)	−1.595**	(0.645)	−0.705**	(0.331)	−1.469**	(0.692)
Wage inc. only	−1.520**	(0.617)	−3.086**	(1.234)	−0.637	(1.128)	−1.398	(2.423)
Business inc. only, agr.	−0.323	(0.294)	−0.655	(0.594)	−0.526	(0.340)	−1.096	(0.715)
Business inc. only, non-agr.	−0.145	(0.334)	−0.295	(0.672)	−0.751	(0.636)	−1.640	(1.415)
Households with applicant initially married, only								
asinh All income	−0.250	(0.348)	−0.531	(0.734)	0.411	(0.639)	0.915	(1.419)
Remittance inc. only	2.043**	(0.902)	4.333**	(1.762)	2.968***	(0.916)	5.923***	(1.757)
Non-remittance inc. only	−1.427***	(0.519)	−3.027***	(1.118)	−1.014	(0.857)	−2.246	(1.918)
Wage inc. only	−1.597*	(0.918)	−3.388*	(1.865)	0.581	(1.650)	1.289	(3.750)
Business inc. only, agr.	−0.703*	(0.405)	−1.492*	(0.871)	−0.930**	(0.473)	−1.857*	(0.981)
Business inc. only, non-agr.	−0.262	(0.521)	−0.556	(1.084)	−0.614	(0.968)	−1.371	(2.189)
Households with applicant initially unmarried, only								
asinh All income	0.037	(0.250)	0.074	(0.486)	−0.398	(0.373)	−0.865	(0.836)
Remittance inc. only	2.218***	(0.808)	4.379***	(1.482)	1.114	(1.447)	2.433	(2.990)
Non-remittance inc. only	−0.199	(0.391)	−0.393	(0.760)	−0.723	(0.664)	−1.580	(1.477)
Wage inc. only	−1.621*	(0.838)	−3.200*	(1.634)	−1.623	(1.530)	−3.559	(3.276)
Business inc. only, agr.	0.048	(0.431)	0.094	(0.838)	−0.615	(0.849)	−1.330	(1.877)
Business inc. only, non-agr.	−0.054	(0.443)	−0.108	(0.861)	−0.864	(0.845)	−1.881	(1.865)

asinh is inverse hyperbolic sine. $Nfail = 460$, $Npass = 439$ Of 899 total households, in 400 the applicant was married at the time of application.
Of married applicants, 52 of 400 are female. Of unmarried applicants, 127 of 499 are female
'Treatment' = household ever had a member in Korea. ITT = intent-to-treat effect, TOT = treatment-on-treated effect.
All variables at household level. Wage income includes only those working in Philippines.
Agr. = agriculture: farming, livestock, forestry, fishing. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 4c: Impacts on households: Spending, saving, and borrowing

Outcome	Parametric				Nonparametric			
	$\hat{\beta}_{ITT}$	s.e.	$\hat{\beta}_{TOT}$	s.e.	$\hat{\gamma}_{ITT}$	s.e.	$\hat{\gamma}_{TOT}$	s.e.
All households								
asinh Expenditures: Total	0.179**	(0.073)	0.364**	(0.147)	0.129	(0.124)	0.280	(0.271)
Food	0.026	(0.090)	0.053	(0.181)	0.004	(0.079)	0.009	(0.165)
Quality of life	0.181**	(0.078)	0.368**	(0.158)	0.146*	(0.078)	0.304*	(0.162)
Educ. & health	1.182***	(0.317)	2.399***	(0.661)	0.977***	(0.322)	2.035***	(0.679)
Durables	0.382	(0.445)	0.776	(0.893)	0.571	(0.794)	1.250	(1.738)
asinh Savings	0.887*	(0.471)	1.800*	(0.963)	0.801	(0.490)	1.668	(1.030)
Borrowed from family for business?	−0.053	(0.045)	−0.107	(0.091)	−0.081*	(0.044)	−0.169*	(0.094)
Borrowed from non-family for business?	−0.027	(0.038)	−0.055	(0.076)	0.086	(0.071)	0.188	(0.159)
Borrowed from family for non-bus.?	−0.034**	(0.015)	−0.068**	(0.032)	−0.034**	(0.017)	−0.072**	(0.036)
Borrowed from non-family for non-business?	−0.050	(0.041)	−0.101	(0.081)	−0.047	(0.042)	−0.097	(0.087)
Households with applicant initially married, only								
asinh Expenditures: Total	0.117	(0.106)	0.247	(0.221)	0.161	(0.105)	0.321	(0.209)
Food	0.008	(0.154)	0.018	(0.321)	−0.044	(0.206)	−0.097	(0.459)
Quality of life	0.088	(0.105)	0.186	(0.218)	0.144	(0.108)	0.287	(0.214)
Educ. & health	0.912**	(0.430)	1.935**	(0.940)	0.711*	(0.423)	1.418	(0.870)
Durables	−0.300	(0.663)	−0.637	(1.393)	0.815	(1.173)	1.810	(2.602)
asinh Savings	−0.234	(0.728)	−0.496	(1.516)	0.544	(1.349)	1.205	(3.042)
Borrowed from family for business?	−0.108	(0.073)	−0.229	(0.154)	−0.136**	(0.068)	−0.270*	(0.140)
Borrowed from non-family for business?	−0.073	(0.062)	−0.154	(0.133)	0.107	(0.116)	0.237	(0.256)
Borrowed from family for non-bus.?	−0.055**	(0.023)	−0.116**	(0.053)	−0.028	(0.059)	−0.061	(0.132)
Borrowed from non-family for non-business?	−0.107*	(0.063)	−0.226*	(0.132)	−0.042	(0.121)	−0.092	(0.267)
Households with applicant initially unmarried, only								
asinh Expenditures: Total	0.231**	(0.100)	0.457**	(0.198)	0.086	(0.169)	0.186	(0.359)
Food	0.045	(0.111)	0.088	(0.216)	−0.057	(0.109)	−0.125	(0.237)
Quality of life	0.247**	(0.111)	0.487**	(0.222)	0.169	(0.194)	0.365	(0.417)
Educ. & health	1.425***	(0.461)	2.813***	(0.921)	1.192**	(0.472)	2.603**	(1.029)
Durables	0.951	(0.608)	1.879	(1.194)	0.406	(1.076)	0.878	(2.320)
asinh Savings	1.738***	(0.599)	3.432***	(1.245)	1.247**	(0.616)	2.722**	(1.387)
Borrowed from family for business?	−0.003	(0.059)	−0.006	(0.114)	−0.022	(0.105)	−0.047	(0.226)
Borrowed from non-family for business?	−0.003	(0.049)	−0.006	(0.094)	0.060	(0.086)	0.130	(0.192)
Borrowed from family for non-bus.?	−0.015	(0.021)	−0.030	(0.041)	−0.049	(0.047)	−0.109	(0.104)
Borrowed from non-family for non-business?	−0.009	(0.053)	−0.018	(0.103)	−0.003	(0.101)	−0.007	(0.221)

asinh is inverse hyperbolic sine. $Nfail = 460$, $Npass = 439$ Of 899 total households, in 400 the applicant was married at the time of application.
Of married applicants, 52 of 400 are female. Of unmarried applicants, 127 of 499 are female. All variables at household level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
'Treatment' = household ever had a member in Korea. ITT = intent-to-treat effect, TOT = treatment-on-treated effect.

Table 5: Impacts on individual adults

Outcome	Parametric				Nonparametric			
	$\hat{\beta}_{\text{ITT}}$	s.e.	$\hat{\beta}_{\text{TOT}}$	s.e.	$\hat{\gamma}_{\text{ITT}}$	s.e.	$\hat{\gamma}_{\text{TOT}}$	s.e.
Individual adults: Applicants only								
Worked in past 6 months?	0.024	(0.044)	0.048	(0.088)	−0.034	(0.083)	−0.074	(0.182)
Days worked, previous mo.	0.662	(0.446)	1.143	(0.757)	0.682	(1.072)	1.534	(2.379)
Any wage income?	0.070	(0.057)	0.142	(0.113)	0.063	(0.107)	0.137	(0.233)
asinh wage income	2.483***	(0.620)	5.014***	(1.155)	2.571***	(0.664)	5.355***	(1.247)
Now in Korea?	0.429***	(0.051)	0.866***	(0.082)	0.403***	(0.054)	0.838***	(0.085)
Now abroad?	0.310***	(0.058)	0.626***	(0.111)	0.281***	(0.061)	0.586***	(0.120)
Individual adults: Non-applicants only								
Worked in past 6 months?	−0.051	(0.039)	−0.093	(0.072)	0.045	(0.070)	0.084	(0.133)
Days worked, previous mo.	−0.158	(0.710)	−0.273	(1.212)	0.407	(1.216)	0.602	(1.797)
Any wage income?	−0.031	(0.036)	−0.056	(0.066)	0.030	(0.064)	0.056	(0.120)
asinh wage income	−0.451	(0.328)	−0.826	(0.597)	−0.098	(0.578)	−0.183	(1.075)
Now in Korea?	0.008	(0.009)	0.014	(0.017)	0.022	(0.014)	0.041	(0.026)
Now abroad?	0.013	(0.021)	0.024	(0.038)	0.039	(0.036)	0.072	(0.068)
Individual adults: Spouses of applicants only								
Worked in past 6 months?	−0.121	(0.085)	−0.247	(0.172)	0.003	(0.157)	0.007	(0.337)
Days worked, previous mo.	0.876	(1.980)	1.114	(2.338)	0.867	(3.099)	0.974	(3.392)
Any wage income?	−0.104	(0.081)	−0.213	(0.167)	0.118	(0.140)	0.256	(0.312)
asinh wage income	−1.185	(0.744)	−2.416	(1.531)	0.464	(1.293)	1.007	(2.828)
Now in Korea?	−0.014	(0.024)	−0.029	(0.049)	0.037	(0.047)	0.080	(0.098)
Now abroad?	0.009	(0.039)	0.018	(0.078)	0.076	(0.065)	0.165	(0.143)

N: applicants = 875, non-applicant adults = 2,142, applicants' spouses = 421.
'Treatment' = household ever had a member in Korea. ITT = intent-to-treat effect, TOT = treatment-on-treated effect.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 6: Impacts on children

Outcome	Parametric				Nonparametric			
	$\hat{\beta}_{ITT}$	s.e.	$\hat{\beta}_{TOT}$	s.e.	$\hat{\gamma}_{ITT}$	s.e.	$\hat{\gamma}_{TOT}$	s.e.
All children								
School-age								
In school?	0.007	(0.034)	0.016	(0.080)	−0.014	(0.062)	−0.037	(0.164)
if so, private facility?	0.120**	(0.056)	0.294**	(0.143)	0.144**	(0.056)	0.340**	(0.143)
Awards at school	0.322	(0.440)	0.791	(1.076)	0.549	(0.409)	1.294	(0.974)
All ages								
Visited health facility past mo.?	−0.027	(0.034)	−0.061	(0.075)	0.028	(0.060)	0.069	(0.149)
if so, private facility?	0.225*	(0.126)	0.372*	(0.205)	0.373***	(0.130)	0.578**	(0.225)
Working?	−0.003	(0.007)	−0.007	(0.014)	−0.001	(0.007)	−0.003	(0.016)
Does anyone read to child?	0.014	(0.048)	0.031	(0.106)	0.025	(0.049)	0.055	(0.109)
Desired years of education	0.687	(0.531)	1.524	(1.167)	0.534	(0.522)	1.182	(1.149)
Children of applicant and/or applicant's spouse only								
School-age								
In school?	0.052	(0.045)	0.108	(0.093)	0.010	(0.079)	0.028	(0.221)
if so, private facility?	0.108	(0.077)	0.243	(0.171)	0.171**	(0.076)	0.387**	(0.185)
Awards at school	0.956	(0.703)	2.144	(1.559)	1.247*	(0.640)	2.821*	(1.467)
All ages								
Visited health facility past mo.?	−0.021	(0.048)	−0.041	(0.094)	−0.065	(0.085)	−0.193	(0.257)
if so, private facility?	0.306	(0.197)	0.586	(0.382)	0.578*	(0.301)	0.953	(0.604)
Working?	−0.010	(0.007)	−0.020	(0.014)	−0.010	(0.010)	−0.021	(0.021)
Does anyone read to child?	−0.095	(0.066)	−0.188	(0.131)	−0.068	(0.068)	−0.141	(0.141)
Desired years of education	1.027	(0.704)	2.031	(1.376)	0.303	(0.881)	0.669	(1.939)

***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. School-age: age $\geqslant 4$ and age < 18. N: All children = 1,323, children of applicant and/or spouse = 729.
'Treatment' = household ever had a member in Korea. ITT = intent-to-treat effect, TOT = treatment-on-treated effect.

Table 7: Impacts on applicants' role in household decisions

Outcome	Parametric				Nonparametric			
	$\hat{\beta}_{ITT}$	s.e.	$\hat{\beta}_{TOT}$	s.e.	$\hat{\gamma}_{ITT}$	s.e.	$\hat{\gamma}_{TOT}$	s.e.
Individual working-age adults: All applicants								
Decisions: *childcare*	−0.120**	(0.057)	−0.242**	(0.114)	−0.075	(0.107)	−0.165	(0.236)
Decisions: *home repairs*	−0.057	(0.058)	−0.115	(0.116)	−0.089	(0.108)	−0.195	(0.239)
Decisions: *major purchases*	−0.124**	(0.059)	−0.251**	(0.117)	−0.135**	(0.062)	−0.281**	(0.129)
Decisions: *entrepreneurship*	−0.133**	(0.060)	−0.269**	(0.120)	−0.075	(0.111)	−0.164	(0.245)
Decisions: *weekend activities*	−0.162***	(0.058)	−0.327***	(0.117)	−0.151**	(0.060)	−0.315**	(0.127)
Individual working-age adults: Applicants initially married only								
Decisions: *childcare*	−0.280***	(0.087)	−0.596***	(0.203)	−0.307***	(0.087)	−0.635***	(0.212)
Decisions: *home repairs*	−0.192**	(0.090)	−0.409**	(0.196)	−0.246	(0.165)	−0.582	(0.446)
Decisions: *major purchases*	−0.316***	(0.089)	−0.674***	(0.209)	−0.280***	(0.091)	−0.579***	(0.214)
Decisions: *entrepreneurship*	−0.313***	(0.090)	−0.668***	(0.209)	−0.183	(0.164)	−0.432	(0.406)
Decisions: *weekend activities*	−0.323***	(0.089)	−0.688***	(0.205)	−0.307***	(0.090)	−0.635***	(0.208)
Individual working-age adults: Applicants initially unmarried only								
Decisions: *childcare*	0.025	(0.075)	0.049	(0.145)	0.125	(0.134)	0.260	(0.293)
Decisions: *home repairs*	0.052	(0.077)	0.101	(0.149)	0.029	(0.137)	0.060	(0.285)
Decisions: *major purchases*	0.036	(0.079)	0.070	(0.151)	0.009	(0.143)	0.020	(0.297)
Decisions: *entrepreneurship*	0.008	(0.080)	0.016	(0.153)	0.005	(0.144)	0.010	(0.297)
Decisions: *weekend activities*	−0.013	(0.077)	−0.024	(0.147)	0.065	(0.138)	0.138	(0.297)

'Decisions' is an indicator variable for whether the applicant was the primary or joint decision-maker on each subject.
N: applicants = 875, applicants initially married = 389, applicants initially unmarried = 486.
'Treatment' = household ever had a member in Korea. ITT = intent-to-treat effect, TOT = treatment-on-treated effect.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 8: Non-experimental Gelbach decomposition of household effects on spending, saving, and borrowing

Outcome	Remittance income $\hat{\delta}_{\text{ITT}}$	s.e.	Non-remittance income $\hat{\delta}_{\text{ITT}}$	s.e.	Decision-making $\hat{\delta}_{\text{ITT}}$	s.e.
All households						
asinh Expenditures: Total	0.035***	(0.009)	−0.034**	(0.014)	0.009	(0.005)
Food	0.016***	(0.004)	−0.026**	(0.011)	−0.006	(0.011)
Quality of life	0.042***	(0.011)	−0.035**	(0.014)	0.015**	(0.006)
Educ. & health	0.082***	(0.021)	−0.033**	(0.014)	0.031	(0.019)
Durables	0.120***	(0.031)	−0.100**	(0.041)	0.008	(0.020)
asinh Savings	0.181***	(0.047)	−0.133**	(0.054)	0.031	(0.033)
Borrowed from family for business?	−0.015***	(0.004)	0.001**	(0.000)	−0.014	(0.007)
Borrowed from non-family for business?	−0.007***	(0.002)	0.000**	(0.000)	0.001	(0.003)
Borrowed from family for non-bus.?	−0.002***	(0.001)	0.003**	(0.001)	−0.003	(0.002)
Borrowed from non-family for non-business?	−0.007***	(0.002)	−0.012**	(0.005)	0.001	(0.001)
Households with applicant initially married, only						
asinh Expenditures: Total	0.051**	(0.020)	−0.059***	(0.021)	0.038***	(0.011)
Food	0.049**	(0.020)	−0.051***	(0.018)	0.019	(0.021)
Quality of life	0.067**	(0.027)	−0.064***	(0.023)	0.025	(0.013)
Educ. & health	0.059**	(0.024)	−0.114***	(0.041)	0.079***	(0.029)
Durables	0.163**	(0.066)	−0.225***	(0.080)	0.079	(0.059)
asinh Savings	0.185**	(0.075)	−0.251***	(0.089)	−0.025	(0.082)
Borrowed from family for business?	−0.019**	(0.008)	0.001***	(0.001)	−0.046***	(0.013)
Borrowed from non-family for business?	−0.002**	(0.001)	−0.001***	(0.000)	−0.005	(0.003)
Borrowed from family for non-bus.?	−0.004**	(0.002)	0.004***	(0.001)	−0.005	(0.003)
Borrowed from non-family for non-business?	−0.008**	(0.003)	−0.018***	(0.007)	−0.012***	(0.004)
Households with applicant initially unmarried, only						
asinh Expenditures: Total	0.019***	(0.007)	−0.010	(0.019)	0.007	(0.008)
Food	−0.008***	(0.003)	−0.008	(0.014)	−0.005	(0.021)
Quality of life	0.017***	(0.006)	−0.010	(0.018)	0.003	(0.012)
Educ. & health	0.066***	(0.024)	−0.004	(0.008)	−0.011	(0.038)
Durables	0.051***	(0.018)	−0.022	(0.039)	0.004	(0.049)
asinh Savings	0.140***	(0.050)	−0.037	(0.065)	−0.045	(0.071)
Borrowed from family for business?	−0.012***	(0.004)	0.001	(0.001)	0.004	(0.008)
Borrowed from non-family for business?	−0.011***	(0.004)	0.000	(0.000)	0.002	(0.007)
Borrowed from family for non-bus.?	0.000***	(0.000)	0.001	(0.002)	−0.001	(0.003)
Borrowed from non-family for non-business?	−0.007***	(0.003)	−0.003	(0.006)	−0.000	(0.006)

asinh is inverse hyperbolic sine. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Robust standard errors under assumption of conditional sphericality.
'Remittance income' = *asinh* total remittances, 'Non-remittance income' = *asinh* all other household income.
'Decision-making' = is a vector of 5 household-level dummies for whether applicant is decision maker in each area of Table 7

Table 9: Treatment redefined as 'household member currently in Korea'

Outcome	Parametric $\hat{\beta}_{\text{TOT}}$	s.e.
Household effects: income		
asinh All income	−0.201	(0.526)
Remittance inc. only	5.448***	(1.170)
Non-remittance inc. only	−1.965**	(0.774)
Wage inc. only	−3.800**	(1.517)
Business inc. only, agr.	−0.806	(0.674)
Business inc. only, non-agr.	−0.363	(0.731)
Household effects: expenditure		
asinh Expenditures: Total	0.448**	(0.185)
Food	0.065	(0.256)
Quality of life	0.454**	(0.200)
Educ. & health	2.955***	(0.871)
Durables	0.956	(1.078)
asinh Savings	2.217	(1.134)
Borrowed from family for business?	−0.132	(0.113)
Borrowed from non-family for business?	−0.067	(0.093)
Borrowed from family for non-bus.?	−0.084	(0.044)
Borrowed from non-family for non-business?	−0.124	(0.099)
Individual adults: Applicants only		
Worked in past 6 months?	0.059	(0.107)
Days worked, previous mo.	1.147	(0.763)
Any wage income?	0.176	(0.133)
asinh wage income	6.228***	(1.202)
Individual adults: Non-applicants only		
Worked in past 6 months?	−0.108	(0.083)
Days worked, previous mo.	−0.340	(1.511)
Any wage income?	−0.065	(0.076)
asinh wage income	−0.952	(0.688)

Of 899 households, 473 ever had member in Korea and 321 currently have member in Korea. 'Treatment' = household currently has member in Korea. TOT = treatment-on-treated effect. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 10: Compare barely-failing sampled households to whole country

	Sample, $s < 0$	Whole country		
Outcome	Mean (μ_1)	Mean (μ_2)	s.d.	$p(\mu_1 = \mu_2)$
Households				
No. members	5.128	4.952	[2.238]	0.0663
Member overseas?	0.380	0.0695	[0.253]	< 0.001
Total income	18668.1	17143.8	[20601.0]	0.120
Wage income	9577.7	12749.5	[21779.8]	< 0.001
Remittance income	5059.9	1945.9	[8073.1]	< 0.001
Expenditures: total	17403.9	14639.0	[14789.6]	< 0.001
Food	8980.4	7083.4	[4524.4]	< 0.001
Quality of life	6603.3	3112.3	[4601.0]	< 0.001
Educ. & med.	1095.2	1057.6	[3002.0]	0.738
Durables	725.0	717.7	[3184.6]	0.945
Any savings? (flow)	0.263	0.496	[0.498]	< 0.001
Savings (flow)	1088.1	1811.0	[8208.3]	0.00140
Business (agr.)?	0.0870	0.397	[0.487]	< 0.001
Business (non-agr.)?	0.113	0.395	[0.487]	< 0.001
Own residence?	0.796	0.705	[0.454]	< 0.001
Strong wall material	0.822	0.598	[0.488]	< 0.001
Region: NCR	0.265	0.131	[0.335]	< 0.001
Region: Luzon (not NCR)	0.654	0.220	[0.412]	< 0.001
Region: Visayas	0.0543	0.419	[0.491]	< 0.001
Region: Mindanao	0.0261	0.231	[0.420]	< 0.001
Head of household				
Age	40.84	47.34	[13.91]	< 0.001
Female?	0.184	0.174	[0.377]	0.560
Years education	11.42	7.864	[3.774]	< 0.001
Married?	0.779	0.814	[0.387]	0.0699
School-age children				
In school?	0.913	0.764	[0.423]	< 0.001

Sample households restricted to those whose applicant barely failed exam. "Agr." = agriculture.
Money in 2010 PHP/mo. Nationally representative data from 2006, inflated with CPI.
Households: $N_{\text{samp},s<0} = 460$, $N_{\text{ctry}} = 38,453$. Children: $N_{\text{samp},s<0} = 433$, $N_{\text{ctry}} = 55,642$.
Nationwide data weighted with frequency weights. Expenditures defined in **??**.

Table 11: Common non-experimental estimates of household and individual effects

			Matching					
	OLS		2 nearest neighbors		10 nearest neighbors		Mahalanobis	
Outcome	ITT	s.e.	ITT	s.e.	ITT	s.e.	ITT	s.e.
Individual working-age adults: Non-applicants only								
Worked in past 6 months?	−0.141***	(0.0164)	−0.161***	(0.0281)	−0.157***	(0.0196)	−0.160***	(0.0333)
Days worked, previous mo.	−1.004***	(0.365)	−0.196	(0.615)	−0.455	(0.427)	0.656	(0.673)
Individual children (*age*< 18): All								
In school?	0.0433***	(0.0101)	0.0365**	(0.0187)	0.0368***	(0.0123)	0.0252	(0.0233)
Households: All								
asinh All income	−0.304***	(0.0747)	−0.311***	(0.0813)	−0.330***	(0.0777)	−0.300***	(0.0853)
Remittance inc. only	3.826***	(0.240)	3.447***	(0.294)	3.449***	(0.257)	3.337***	(0.346)
Non-remittance inc. only	−1.598***	(0.133)	−1.596***	(0.140)	−1.622***	(0.137)	−1.590***	(0.141)
Wage inc. only	−3.952***	(0.248)	−3.672***	(0.302)	−3.615***	(0.272)	−3.999***	(0.342)
Business inc. only, agr.	−1.169***	(0.123)	−1.581***	(0.188)	−1.666***	(0.146)	−1.405***	(0.240)
Business inc. only, non-agr.	−2.932***	(0.125)	−3.089***	(0.220)	−3.052***	(0.165)	−2.960***	(0.286)
asinh Expenditures: Total	−0.0190	(0.0306)	−0.00909	(0.0422)	−0.0388	(0.0359)	0.00917	(0.0481)
Food	−0.0590*	(0.0308)	−0.0174	(0.0381)	−0.0480	(0.0346)	−0.0359	(0.0419)
Quality of life	0.628***	(0.0334)	0.642***	(0.0501)	0.630***	(0.0407)	0.640***	(0.0592)
Educ. & health	−0.274**	(0.127)	−0.346**	(0.154)	−0.388***	(0.139)	−0.169	(0.170)
Durables	−0.275	(0.183)	−0.261	(0.229)	−0.440**	(0.200)	−0.132	(0.264)
asinh Savings	−2.272***	(0.196)	−2.280***	(0.255)	−2.296***	(0.219)	−2.315***	(0.303)

asinh is inverse hyperbolic sine. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.
Treatment = household *ever* had a member in Korea. Control variables in OLS and matching variables in PSM:
Household size, HoH (Head of Household) age, HoH years educ., plus dummies for HoH female, HoH married, standalone house, family owns residence, strong wall materials, strong roof materials, four regions.

Online Appendix

"Split Decisions: Household finance when a policy discontinuity allocates overseas work"

A1. Descriptive statistics

Appendix Table 1 presents descriptive statistics for the household-level regressions in the main text. Appendix Table 2 presents descriptive statistics for the regressions at the individual level—applicants, non-applicant adults, and children.

A2. Robustness

Appendix Table 3 shows the parametric regressions of Table 7 when one additional control variable is added to all regressions: a dummy that equals one when the survey respondent is female, zero otherwise.

Appendix Table 1: **Descriptive statistics for survey sample: Household level**

	Mean	Std. Dev.	Min	Max	*N*
Traits of household's applicant at time of application					
Age	29.998	(4.465)	18.92	38.77	899
Female?	0.199	(0.400)	0	1	899
College grad.?	0.324	(0.468)	0	1	899
Months experience	70.112	(52.428)	0	301	899
Employed?	0.275	(0.447)	0	1	899
Married?	0.445	(0.497)	0	1	899
Region: NCR	0.268	(0.443)	0	1	899
Region: Luzon	0.647	(0.478)	0	1	899
Region: Visayas	0.057	(0.231)	0	1	899
Region: Mindanao	0.028	(0.165)	0	1	899
Test batch 1	0.021	(0.144)	0	1	899
Test batch 2	0.129	(0.335)	0	1	899
Test batch 3	0.281	(0.450)	0	1	899
Test batch 4	0.377	(0.485)	0	1	899
Test batch 5	0.191	(0.394)	0	1	899
Traits of household at time of survey					
Number of household members	5.128	(2.010)	1	13	899
Working age	3.321	(1.613)	1	10	899
excl. Korea	2.948	(1.645)	0	10	899
% female	0.575	(0.256)	0	1	893
Age < 18	1.472	(1.247)	0	6	899
Age $\geqslant 65$	0.335	(0.651)	0	3	899
asinh Income, total	10.073	(1.735)	0	13.11	899
asinh Remittance income	4.662	(5.042)	0	13.00	899
asinh Non-remittance income	9.160	(2.531)	0	12.22	899
asinh Wage income	5.524	(5.101)	0	11.83	899
asinh Business income, agr.	0.641	(2.213)	0	11.00	899
asinh Business income, non-agr.	0.826	(2.437)	0	11.98	899
asinh Expenditures: Total	10.357	(0.606)	8.313	13.28	899
Food	9.621	(0.843)	0	13.01	899
Quality of life	9.326	(0.663)	6.757	12.39	899
Educ. & health	6.185	(2.740)	0	11.79	899
Durables	3.802	(3.547)	0	11.80	899
asinh Savings	2.340	(3.747)	0	12.02	899
Borrowed from family for bus.?	0.172	(0.378)	0	1	899
...non-family for bus.?	0.107	(0.309)	0	1	899
...family for non-bus.?	0.0200	(0.140)	0	1	899
...non-family for non-bus.?	0.118	(0.323)	0	1	899

asinh is inverse hyperbolic sine. "Agr." = agriculture. "Bus." = business

Appendix Table 2: **Descriptive statistics for survey sample: Individual level**

	Mean	Std. Dev.	Min	Max	*N*
Individual data: Applicants					
Worked in past 6 months?	0.855	(0.352)	0	1	875
Days worked, previous mo.	23.743	(2.590)	2	31	611
Any wage income?	0.706	(0.456)	0	1	875
asinh wage income	6.227	(5.290)	0	11.63	875
Now in Korea?	0.339	(0.474)	0	1	875
Now abroad?	0.462	(0.499)	0	1	875
Decisions: *childcare*	0.329	(0.470)	0	1	875
Decisions: *home repairs*	0.341	(0.474)	0	1	875
Decisions: *major purchases*	0.387	(0.487)	0	1	875
Decisions: *entrepreneurship*	0.431	(0.495)	0	1	875
Decisions: *weekend activities*	0.367	(0.482)	0	1	875
Individual data: Non-applicant adults					
Worked in past 6 months?	0.497	(0.500)	0	1	2142
Days worked, previous mo.	22.992	(4.616)	2	31	644
Any wage income?	0.308	(0.462)	0	1	2142
asinh wage income	2.328	(4.175)	0	11.41	2142
Now in Korea?	0.018	(0.134)	0	1	2142
Now abroad?	0.069	(0.254)	0	1	2142
Individual data: Children					
School-age children					
In school?	0.906	(0.292)	0	1	1031
if so, private facility?	0.354	(0.479)	0	1	934
Awards at school	2.070	(3.755)	0	35	934
All ages					
Visited health facility past mo.?	0.138	(0.345)	0	1	1381
if so, private facility?	0.679	(0.468)	0	1	190
Working?	0.004	(0.066)	0	1	1381
Does anyone read to child?	0.373	(0.484)	0	1	1381
Desired years of education	11.270	(5.607)	0	16	1381

asinh is inverse hyperbolic sine. "Agr." = agriculture. "Bus." = business

Appendix Table 3: **Impacts on applicants' role in household decisions, controlling for gender of respondent**

	Parametric			
Outcome	$\hat{\beta}_{\text{ITT}}$	s.e.	$\hat{\beta}_{\text{TOT}}$	s.e.
Individual working-age adults: All applicants				
Decisions: *childcare*	−0.080	(0.055)	−0.166	(0.113)
Decisions: *home repairs*	−0.023	(0.057)	−0.048	(0.116)
Decisions: *major purchases*	−0.088	(0.056)	−0.181	(0.115)
Decisions: *entrepreneurship*	−0.102	(0.058)	−0.211	(0.120)
Decisions: *weekend activities*	−0.128**	(0.056)	−0.264**	(0.116)
Individual working-age adults: Applicants initially married only				
Decisions: *childcare*	−0.210**	(0.084)	−0.467**	(0.203)
Decisions: *home repairs*	−0.132	(0.087)	−0.295	(0.197)
Decisions: *major purchases*	−0.256***	(0.086)	−0.571***	(0.211)
Decisions: *entrepreneurship*	−0.264***	(0.088)	−0.589***	(0.215)
Decisions: *weekend activities*	−0.265***	(0.086)	−0.591***	(0.208)
Individual working-age adults: Applicants initially unmarried only				
Decisions: *childcare*	0.042	(0.074)	0.081	(0.142)
Decisions: *home repairs*	0.067	(0.075)	0.130	(0.146)
Decisions: *major purchases*	0.053	(0.075)	0.102	(0.147)
Decisions: *entrepreneurship*	0.024	(0.077)	0.046	(0.148)
Decisions: *weekend activities*	0.003	(0.074)	0.006	(0.142)

Controls in parametric regressions include dummy = 1 if survey respondent female, 0 otherwise.
'Decisions' is an indicator variable for whether applicant was primary or joint decision-maker.
'Treatment' = household ever had a member in Korea. ITT = intent-to-treat effect,
TOT = treatment-on-treated effect. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.