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

Michael Clemens and Erwin Tiongson

Abstract

Labor markets are increasingly global. Overseas work can enrich households but also split them geographically, with ambiguous net effects on decisions about work, investment, and education. These net effects, and their mechanisms, are poorly understood. 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 unusually reliable measurement of the reduced-form effect of these overseas jobs on migrant households. A purpose-built survey allows nonexperimental tests of different theoretical mechanisms for the reduced-form effect. We also explore how reliably the reduced-form effect could be measured with standard observational estimators. We find large effects on spending, borrowing, and human capital investment, but no effects on saving or entrepreneurship. Remittances appear to overwhelm household splitting as a causal mechanism.

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1. Introduction

Overseas workers are expected to send home over US$600 billion per year by 2014, double the amount just 8 years before (Ratha and Silwal 2012). This has raised interest in how international migration might finance investment in developing areas.

But migration affects more than just incomes. Another important effect is that overseas jobs, unlike many local jobs, often disrupt and split households for extended periods. Substantial literatures have separately studied the effects of new income and changes in household composition. These two effects are bundled in labor migration, but research has only begun to accurately measure the net effects or gauge the relative importance of each mechanism. Existing efforts face major empirical challenges in accounting for migrant self-selection.

We present estimates of the effects of temporary overseas work in a setting that allows unusually reliable causal identification. We study a sample of the households of 25,320 workers in the Philippines who applied to high-wage temporary jobs in Korea between 2005 and 2007. Each applicant was required to pass a basic test in the Korean language, and all those who failed were unable to take the job. In 2010 we surveyed the households of those who barely passed for comparison to the households of those who barely failed. This regression discontinuity design allows uncommonly reliable attribution of differences in those households to the effects of overseas work. Our purpose-built survey allows nonexperimental tests of impact mechanisms.

We contribute to the literature in three principal ways. First, we offer well-identified estimates of the reduced-form effect of large increases in income for one member of a low-income household bundled with the change in household composition that accompanies overseas work. Each of these bundled effects, separately, is the subject of an active literature. Second, we explore theoretically and empirically the relative importance of these mechanisms for the effect of overseas work. Third, we offer evidence on how the subjects of this natural experiment differ from representative Filipinos, in both observed and unobserved traits. We explore unobserved differences by comparing our

quasi-experimental estimates with those we would have obtained with more common observational estimates.

The results show that migration by one household member causes large increases in remaining household members' spending on health and education, quality of life, and durables, but no increases in savings. Migration causes substantial reductions in borrowing from family members outside the household. We find no significant effect on starting or investing in entrepreneurial activity or on labor suppy by other family members. Migration by the applicant causes children to be much more likely to attend private school and receive awards at school. Migration causes large changes in decision-making power between household members, but nonexperimental evidence suggests that most of the migration effect arises through the remittance mechanism rather than through changing household decision-making. We do find some evidence that migration affects home production technology, particularly in agriculture, by altering household composition. We find that widely-used nonexperimental estimators of migration effects can, in the same data, spuriously attribute causation to self-selection.

We begin by discussing the separate literatures on microeconomic effects of credit constraints and on household composition and decision-making, as well as the literature on the bundle of these effects that arises from migration. The next section derives a simple model of the migration effect and its mechanisms. The following sections discuss the natural experiment that created the policy discontinuity, and describe our new survey data. We then present evidence on reduced-form impacts of migration, and compare our quasi-experimental results to those obtained with more standard observational estimators. We conclude with a nonexperimental exploration of impact mechanisms, and a discussion of external validity.

2. Financial decisions in families split by work

Labor migration by one household member bundles different effects on the household. Of these, two of the most important have each been the subject of a separate literature:

There is often an important change in household income that could alleviate credit constraints, and there is a change to the migrant's participation in household decisions and in home production.

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.[1] Others stress the effects of credit constraints on investments in human capital,[2] and on consumption decisions.[3] A common theme is that access to capital substantially alters all of these decisions in many settings.

Second, and separately, 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.[4] A different strand traces the effects of parental absence—through one parent's labor supply, departure, or death—on investments in children's human capital.[5] These effects, too, are typically substantial.

Migration bundles these two effects.[6] The combined effect has been the subject of a

[1]These studies include Hubbard (1998); Hurst and Lusardi (2004); Udry and Anagol (2006); De Mel et al. (2008); McKenzie and Woodruff (2008); Banerjee et al. (2010); Gertler et al. (2012); Wang (2012).

[2]See for example Kane (1994); Blau (1999); Keane and Wolpin (2001); Carneiro and Heckman (2002); Cameron and Taber (2004); Akee et al. (2010); Lochner and Monge-Naranjo (2011); Dahl and Lochner (2012); Macours et al. (2012); Caucutt and Lochner (2012).

[3]The effects of credits constraints on consumption choices is investigated by Hanushek (1986); Altonji and Siow (1987); Paxson (1992); Morduch (1995); Case and Deaton (2001); Karlan and Zinman (2010); Dupas and Robinson (2012); Kaboski and Townsend (2012).

[4]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).

[5]Work on the effects of parental absence and household composition includes Blau and Grossberg (1992); Angrist and Johnson (2000); Lang and Zagorsky (2001); Corak (2001); Gertler et al. (2004); Page and Stevens (2004); Ruhm (2004); Gennetian (2005); Lyle (2006); Booth and Tamura (2009).

[6]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

recent and growing literature on the micro effects of migration, surveyed by Hanson (2009) and Antman (2012). Though much of the early work on migration stresses the effects of remittances alone,[7] a new literature seeks the effects of migration itself, comprising both channels above.[8]

In this paper we contribute to these literatures by offering unusually well-identified estimates of the reduced-form net effects of migration, and by theoretically-grounded tests of the relative importance of different impact mechanisms. Much of the previous research on reduced-form effects faces important challenges of causal identification that are difficult to address with the observational methods most commonly used. In rare cases, researchers have been able to compare observational estimates of migration effects to more reliably identified estimates, and have found large differences (McKenzie et al. 2010; Gibson et al. 2011). Moreover, existing research has only begun 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. 2011) and perceived returns to investment (Kandel and Kao 2001; McKenzie and Rapoport 2011).

3. Mechanisms of impact

We posit three principal channels by which labor migration by one household member can affect household finance and investment decisions. First, new income can alleviate constraints on borrowing and consumption smoothing. Second, changes in the location of family members can affect their power to influence decisions. Third, changes in the composition of the household back home can affect home production technology. Each emerges from a simple dynamic optimization model of household savings and in-

migrants important changes in transferable skills, cultural attitudes, or technical knowledge.

[7] Rapoport and Docquier (2006) and Yang (2011) survey this literature, which includes Cox Edwards and Ureta (2003); Yang (2008); Alcaraz et al. (2012). A focus of this work has been the effect of remittances and similar transfers on labor force participation by recipients (Rodriguez and Tiongson 2001; Görlich et al. 2007; Ardington et al. 2009; Edmonds and Schady 2012).

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

vestment. We consider unitary and collective households, with and without borrowing constraints.

3.1. Unitary household model

We begin modeling the migrant's household as unitary (Blundell and MaCurdy 1999) with an available investment that requires no labor input (Bardhan and Udry 1999, pp. 7–18). The two household members $(1, 2)$ get utility from consuming a fixed Hicksian composite good $c \equiv c_1 + c_2$ and from leisure (ℓ_1, ℓ_2) over lifetime T. Member 1 can work in the home country at wage w. Member 2 can migrate,[9] to spend a fraction $0 \leqslant m \leqslant 1$ of work time 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". For given m it solves

$$\max_{c,\ell_1,\ell_2} \int_0^T e^{-\rho t}\Big(u(c, \ell^1, \ell^2) - \phi m\Big)\,\mathrm{d}t. \tag{2}$$

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

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

where θ 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).[10] We solve (2) and (3) as an autonomous program of optimal control,

[9] 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.

[10] 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

for which the Pontryagin conditions on the current-value Hamiltonian are $u_{\ell^1} = \mu w$; $u_{\ell^2} = \mu w^*$; $u_c = \mu$; and $\dot{\mu} = \mu\left(\rho - \theta f'(k)\right)$, where the subscript denotes the partial derivative. The first two conditions imply

$$\ell_m^1 > 0 \quad \iff \quad \ell_m^2 \lessapprox 0. \tag{4}$$

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. The equations of motion for c and ℓ^1 come from differentiating the fourth Pontryagin condition and substituting for μ:

$$\dot{c} = -\frac{u_c}{u''}\left(\theta f'(k) - \rho\right) \qquad ; \qquad \dot{\ell^1} = -\frac{u_{\ell^1}}{u''}\left(\theta f'(k) - \rho - \frac{\dot{w}}{w}\right). \tag{5}$$

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

$$\dot{k}_m = \theta f' k_m + \left(w_o - w\right)\Psi_{w^*}, \tag{6}$$

The first term on the right side captures increased investment as the borrowing constrained household uses migration earnings to raise home production. The second term captures the effect of migration earnings on wage income net of consumption, which acts exclusively through raising wage w^*. Each is a different aspect of a single channel of the effect of migration on investment: via higher wages. Barring large declines in labor supply caused by the new earnings, home production rises and all new income is (necessarily) reinvested in it.

Without borrowing constraints. If we allow the household to borrow,[11] the effect of

with respect to t. We assume u and f are concave, continuous, and twice differentiable.

[11] 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. Optimal consumption and non-migrant leisure (5) now follow $\dot{c} = -\frac{u_c}{u''}(r - \rho)$ and $\dot{\ell^1} = -\frac{u_{\ell^1}}{u''}\left(r - \rho - \frac{\dot{w}}{w}\right)$. In contrast to (5), 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 $\dot{\ell^1} = 0$). 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.

migration on investment is

$$\dot{k}_m = rk_m + (w_o - w)\,\Psi_{w^*}, \tag{7}$$

where the first term on the right is the return to saving the new income in the bank. Home production is fixed at $f(\bar{k})$—where $\bar{k}$ is unconstrained optimal investment—and is unaffected by migration. Comparing (6) and (7) shows that the effect of migration on investment is greater for borrowing-constrained households than for borrowing-unconstrained households whenever home production is less than optimal, i.e. $k < \bar{k}$.

The unitary household model makes two key predictions: When one household member migrates for work, 1) non-migrants reduce labor supply, to a degree that increases with borrowing constraints and capital market imperfections; and 2) the household raises investment, to a degree that increases as the household faces greater borrowing contraints, solely because earnings rise.

3.2. Collective household model

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 between two countries. An alternative optimization program allows each household member an additively separable egoistic utility term. For given m the household solves

$$\max_{c^1,c^2,\ell^1,\ell^2} \int_0^T e^{-\rho t}\Big(u(c^1,\ell^1) + (1-\phi m)u(c^2,\ell^2)\Big)\,\mathrm{d}t. \tag{8}$$

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

With borrowing constraints. The equation of motion for capital (3) is unchanged, but now $\theta \equiv \theta(m)$. 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. The Pontryagin conditions for this collective household 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 $\dot{\mu} = \mu\left(\rho - \phi(m)f'(k)\right)$. The first two of these give

$$\ell^1_m > 0, \quad \ell^1_{m\phi} > 0 \quad \Longleftrightarrow \quad \ell^2_m \lessapprox 0. \tag{9}$$

Thus non-migrants still respond to migration by consuming more leisure, but now to a degree that gets smaller when migration causes a smaller shift in the balance of power (ϕ is smaller).

In the collective household, migration affects investment through the earnings channel in (7), plus two additional channels:

$$\dot{k}_m = f'k_m + f\theta_m + (w_o - w)\,\Psi_{w^*} + \Psi_m. \tag{10}$$

The first term of (10) is identical to (6), but in the second term an additional effect arises: the migrant's absence can directly change the home production technology by altering household θ. In the third term migration affects labor income net of consumption by altering the wage w^*, as in (6), but in the fourth term there is a new and independent effect: 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.

Without borrowing constraints, $f' = r$ in (10). As in (7), migration raises investment by more in borrowing-constrained households than in borrowing-unconstrained households (provided $k < \bar{k}$). For the collective household, too, it can be shown in equations of motion analogous to (5) that the labor supply effect (9) only arises when there are borrowing constraints or capital market imperfections.

The collective household enriches the models' predictions:

First, migration by one member can still reduce labor supply by other members, but this effect varies. We saw in the unitary household that the labor supply effect will be

smaller when borrowing constraints are smaller and capital markets more frictionless. In the collective household, the labor supply effect also depends on the degree to which migration causes household decision-making power to shift.

Second, migration can affect household saving and investment through three channels in the collective household, and the net effect need not be positive. In the unitary household, only the migrant's earnings affect saving and investment. In the collective household, migration can separately affect investment in two other ways. Migration can alter the balance of power to make saving and investment decisions (e.g. the remaining spouse has different consumption preferences). Migration can separately alter the technology of home production (e.g. the remaining spouse is better or worse at some home production activity such as farming).

4. A policy discontinuity in the Philippines

We identify these effects in a single setting with an unusual natural experiment. A large group of Filipino applicants to high-wage jobs overseas—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. Comparing applicant households in the passing and failing groups years later allows uncommonly reliable estimation of the pure effect of migration on these households. This setting is well-suited for the regression discontinuity design (RDD), which can approximate the causal identification offered by 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 Em-

ployment 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 tests basic listening and reading in Korean. The test is administered at three locations in the Philippines and graded in Korea. The maximum score is 200 points, and a score of 120 points or greater is 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 cutoff. The large number of test-takers provides substantial density

[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.

[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.

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, we use fully nonparametric sharp and fuzzy RDD (Hahn et al. 2001; Porter 2003; Nichols 2007; Imbens and Lemieux 2008). Because RDD results are also notoriously sensitive to bandwidth selection, we tie our hands with the asymptotically optimal bandwidth recently proposed by Imbens and Kalyanaraman (2012).[14]

For each outcome μ we first estimate the intent-to-treat (ITT) effect (where treatment τ is migration) with sharp nonparametric RDD. This is the effect of barely passing the language test on "compliers" (Angrist et al. 1996)—those whose migration decisions were changed by the test result—whose families were willing to complete the survey, and who had scores near the passing threshold $s = 0$:[15]

$$\text{ITT} = \mu_{s\downarrow 0} - \mu_{s\uparrow 0}. \tag{11}$$

We then estimate the treatment-on-treated (TOT) effect as fuzzy nonparametric RDD. This is the effect of migration by a household member:

$$\text{TOT} = \frac{\mu_{s\downarrow 0} - \mu_{s\uparrow 0}}{\tau_{s\downarrow 0} - \tau_{s\uparrow 0}}. \tag{12}$$

This again is the local average treatment effect on "compliers", whose households com-

[14] We use 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."

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

pleted the survey, and whose scores were near the passing threshold.

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).[16]

In the sampling universe there is a very large jump in deployment probability at the cutoff score, but little evidence of a significant discontinuity in the baseline characteristics of the job applicants. Table 2 shows that there is a jump of 68 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, civil status, and test batch. None exhibit statistically significant jumps at the cutoff score. 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

[16] 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.

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 well 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.[17] 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.[18]

5.1. Sampling strategy

In order to ensure that the sample was as representative as possible of the sampling universe, we provided target addresses to the survey firm in stages. First, we gave only the addresses of households whose applicant was within one point of the cutoff score. Only after the firm had attempted to contact all of those households did we provide a second set of target households whose applicant was within two points of the cutoff. We proceeded in this fashion, one point at a time, until our resources for conducting

[17] 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.

[18] Cull and Scott (2010) show 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.

the survey were exhausted (which occurred at 5 points from the cutoff).[19]

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. 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.

Rough power calculations before the survey suggested a target sample size of roughly 900 households.[20] In the end, enumerators attempted to locate the permanent addresses of 2,053 EPS applicants, of which they successfully located and visited 1,532. Of these visits, 899 (59%) resulted in a completed survey. The rest were nonresponders—either because the residents declined to complete the survey (9%), no one was home or the residents were not the applicant's family (15%), or neighbors indicated that the applicants' family had once lived there but had moved away (17%) or died (0.1%). All 899 completed surveys represent the households of applicants who scored within 5 points of the cutoff.

5.2. Checking the discontinuity: Survey sample

Because only 59% of located addresses produced a completed survey, we must check for bias due to nonresponse. For example, if an important use of remittances were to purchase a new residence and move away, passing the test itself may affect the response rate. This could produce differences across the passing threshold not attributable to passing the test or migration.

[19] An alternative method might have led to a less representative sample. For instance, if we had provided all of the target addresses at once, the survey firm might have visited households that were easier to reach but further from the discontinuity before attempting to contact all households near the discontinuity.

[20] Following Duflo et al. (2007) we estimated that 900 households would give us 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).

The lower-right panel of Figure 1 checks for a discontinuity in this response among households in the sampling universe near the passing threshold. Barely passing the test did not cause an applicant's household to be statistically significantly more or less likely to complete the survey.

It is nevertheless possible that passing the test altered the *composition* of households completing the survey. For this reason we repeat the analysis of Table 2 on the survey sample in Table 3. The first rows show that there is a very large discontinuity in household-level migration exposure at the test-score discontinuity. A graphical representation is shown in the upper-left panel of Figure 3. The rest of Table 3 tests whether there is a discontinuity at the threshold score in applicants' baseline traits, among households in the survey sample.

There is no statistically significant difference in any of the baseline traits at the threshold in the survey sample. 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 evidence on reduced-form impacts

We now report sharp (ITT) and fuzzy (TOT) nonparametric RDD regressions 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. The first column of each table

shows the average outcome for test-failers approaching the cutoff score. "Treatment" is defined as a household in which any member *ever* worked in Korea. 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.

6.1. Effects on households

Table 4a shows household-level effects on family composition and income. Unless otherwise stated, household members are considered members even when abroad. There are no statistically significant effects on the number of total household members, or on the number who are working age, less than working age, or more than working age. When household members in Korea are not included in household size, we cannot reject the hypothesis that the TOT effect of migration is –1. Migration by the job applicant causes a 21 percentage point rise in the fraction of working-age household members (excluding those in Korea) who are female.

In the middle rows of the table, there is no statistically significant effect on three separate measures of the total income of the household (excluding members in Korea): a dummy for nonzero income, the value of income (in pesos per month), and the natural log of income. A graphical representation of the *ln* income regression is in the lower-left panel of Figure 3. Migration by the job applicant causes a substantial rise in remittance income that is mostly or fully offset by causing a decline in non-remittance income. Remitters appear to send roughly the same amount that they would be contributing to household income if they were working in the Philippines.

At the bottom of the table, we see no evidence of effects on entrepreneurial activity—whether measured as a dummy for having any income from entrepreneurial activities, or the natural log of that income—in the agricultural or non-agricultural sectors. There is suggestive evidence that migration causes a decline in the fraction of households engaged in farming, but this is statistically imprecise ($p = 0.14$).

Table 4b shows household-level effects on spending, saving, investing, and borrow-

ing by the household members who remain in the Philippines. Migration by the job-applicant causes monthly expenditures to rise substantially. The effect on overall peso-value expenditures is on the order of 60% and significantly different from zero at the 3% level; the effect in ln pesos is about 30% but not statistically precise. We cannot reject the hypothesis that none of this increase comes from increased spending on food. Rather, the increase principally comprises a 30–60% rise in "quality of life" spending, a 68–150% rise in "health and education" spending, and a 91–146% rise in "durable goods" spending.[21] Migration does not affect average monthly savings when households with zero savings are included, but among households who have nonzero monthly savings, there is statistically imprecise evidence of a 20% rise in savings ($p = 0.11$).[22]

The remaining rows of the table show that migration by the job applicant causes households to borrow less from family members in the 6 months preceding the survey. There is no statistically significant effect on borrowing from sources outside the family. There is no effect on whether or not the family owns its residence, nor on the number of bedrooms in the residence.

We note important differences between the effects of migration on reported income and reported expenditure. Migration causes remittances at a level that roughly replace 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 expenditure well beyond these reported increases in income, without appearing to cause increases in borrowing.

We believe this disparity is caused by underreporting of specific types of income due to difficulties in eliciting information from survey respondents. For example, if a migrant used overseas earnings to purchase a motorcycle for the household on a home visit, the

[21] These ranges represent the linear peso and ln peso results, respectively. The linear peso results are statistically precise at the 3% level or below for "quality of life" and "education & health", and the ln peso results are significant at the 12% level or below for all three categories.

[22] "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, extending loans.

survey respondent might not think of this as an in-kind remittance—it was not brought from Korea—but is likely to report it when asked about major purchases the household made recently. 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 and Sullivan 2008).

6.2. Effects on individual adults

Table 5 presents impacts of migration on individual adults: applicants, applicants' spouses only, and all non-applicants (including spouses).

Migration does not cause a significant change in the fraction of applicants who are employed at the time of the survey. Treatment (the applicant *ever* migrated to Korea) causes an 84 percentage-point increase in the probability that the applicant is in Korea at the time of the survey (a small portion had migrated and then returned). Having ever migrated to Korea causes a 59 percentage point increase in the probability that the applicant is anywhere outside the Philiippines at the time of the survey.

Migration by the applicant has no discernible effects on labor force participation by the applicant's spouse—whether measured as a dummy for working, the number of days worked, a dummy for any wage earnings, or the natural log of wage earnings.

The bottom portion of the table considers all non-applicant adults, including applicants' spouses. There is likewise no effect of the applicant's migration on their labor force participation, by any measure. There is suggestive evidence that migration by the applicant causes a 4 percentage point increase in the probability that a non-applicant adult in the same household is in Korea, but this is not statistically precise ($p = 0.12$). Migration by the applicant does not cause any changes in adults' years of education, visits to health facilities, or visits to private health facilities in particular.

Table 6 shows the impacts on household decision-making by applicants. Household

survey respondents were asked who bears the principal responsibility for household decisions in five areas. They could answer themselves, another identified member, or shared decision-making by themselves and another identified member. 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.

Migration by the applicant causes important changes in how household decisions are made. It causes decision-making by the applicant to decline in all areas, though these declines are only statistically significant in the case of "major purchases" and "weekend activities". The lower half of the table considers only applicants who were married at baseline. For them, the magnitude of the declines is much larger, and they are statistically significant for "childcare", "major purchases", and "weekend activities". A graphical representation of the "childcare" row is shown in the lower-right panel of Figure 3.

6.3. Effects on individual children

Table 7 shows the effects on children. The table considers household members under age 18 who are the children of the applicant or the applicant's spouse.

Considering only those of schooling age ($\geqslant 6$), migration does not affect school enrollment. This is to be expected given that over 98% of children in this age group are already enrolled. Migration does cause a 41 percentage point increase in the probability that a child is in private school (from a base of 28 percent). Migration also causes a 30 percentage point increase in the fraction of children who are receiving awards at school. This could be due to better performance in school, or due to a different propensity for private schools to give awards.

Considering children of all ages, migration by the applicant does not cause changes in the probability that these children visited a health facility in the previous month. There is suggestive evidence that it causes more of those visits to be at private health facilities, but this is imprecisely measured (ITT $p = 0.16$, TOT $p = 0.18$). Migration by the applicant does not affect the probability that children are working, the probability that

anyone in the household reads to the child in the evenings, or the years of education that the respondent wishes the child to someday attain.

We explore heterogeneous reduced-form impacts by pre-treatment subgroups in Appendix section A1.

6.4. Selection bias and nonexperimental reduced-form results

Could the preceding effects have been well-identified without the quasi-experiment we use? It is plausible that households with migrants differ in many ways from households without migrants, ways that might not be the result of migration. If all such differences were observable, quasi-experimental methods like RDD would have less value. Here we test for differences in unobservable determinants of outcomes between the survey sample and the national population.

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.[23] We then estimate treatement effects with ordinary least squares (OLS) and propensity score matching (PSM), for comparison to the RDD estimates.

Table 8 shows this exercise for selected RDD regressions. The first column simply reproduces the ITT result from RDD analysis in an earlier table. The second column shows the coefficient on the treatment variable in an OLS regression including numerous controls. If we could not carry out a quasi-experimental analysis, it might be tempting to think that these controls capture much of the important economic difference between households that self-select into this form of migration and those that do not.[24] The

[23] 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.

[24] 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

next three columns use PSM estimators with nearest-neighbor matching on 2, 5, and 10 neighbors. The matching variables are identical to the control variables in the OLS column. The final column uses PSM with Mahalanobis-distance matching.

The observational estimators can lead to estimates of the "effects" of migration that are spurious. The first row shows that these observational estimators only capture about half of the effect of migration on health and education spending. This is reasonable since Table 10 shows that households that self-select into applying for these jobs have a greater propensity to invest in adults' and childen's human capital than typical households. The next row shows that the observational estimators exaggerate the magnitude of the effect on running a business—again reasonable since households whose members apply to jobs in Korea are much less likely to run a business, for reasons that can include unobservable traits. The last two rows show that the observational estimates spuriously find positive effects of migration on children's school enrollment and adults' education levels. Again this is likely because the households in the sample place a greater emphasis on education than other households, for reasons that are in part unobservable.

The success of these nonexperimental estimators obviously depends on the control variables chosen, but whether any given set of controls is adequate is in most settings untestable. Here we show that a plausible set of controls is inadequate to control away large amounts of unobserved difference between the true control group and the synthetic control group.

7. Nonexperimental evidence on mechanism of impact

By what mechanism do these effects arise? Above we offered theoretical support for three candidates in a dynamically-optimizing, credit-constrained, collective household: Migrants' earnings can alleviate credit constraints on consumption and investment, migrants' absence can alter their relative power over financial decisions, and migrants'

materials, and three regions (one region omitted).

absence can change the skill and technology of home production.

We offer suggestive, nonexperimental tests of these hypotheses following the logic of Baron and Kenny (1986), extended theoretically by Imai et al. (2011) and extended empirically to the case of nonparametric RDD by Frölich (2007). These tests are not well-identified tests of the different causal mechanisms because, in the terminology of Imai et al., they fail the sequential ignorability assumption: even if the migration treatment is plausibly exogenous, the degree of exposure to different treatment mechanisms within the treatment or control groups may not be exogenous. It is nevertheless informative to explore correlations between the degree of treatment effect and observables that signify treatment via single mechanisms.

Table 9 conducts these mechanism tests. The first three columns simply reproduce selected TOT regressions from Tables 4a, 4b, 5, and 7. The center trio of columns include as a covariate the household's ln monthly remittance income. The rightmost trio of columns include as covariates five indicator variables for whether the applicants is a principal or joint decision-maker in the five areas of Table 6.

The patterns in Table 9 suggest that remittances are far and away the most important mechanism of the migration effect. In the first three rows, almost none of each effect on spending is accounted for by controlling for changes in decision-making power, but all of each effect is accounted for by controlling for remittances. In the following three rows, the same is true for the effect on borrowing from family members: controlling for changes in decision-making patterns does not substantially alter the result, while controlling for remittances eliminates the statistical significance of the result.

Continuing down the table, controlling for changes in decision-making does little to alter the treatment effect while controlling for remittances greatly alters the effect. In some cases, remittances appear to be the reason for the migration treatment effect: Migration causes more children to be in private school, but the result vanishes after controlling for remittances. In other cases, remittances appear to be the reason why migration does *not* affect the outcome: Migration has no statistically significant effect

on whether the household receives income from farming, but after controlling for remittances, migration has a significant negative effect on farming activity. The effect is intuitive: If the household member who would do most of the farming is abroad, farming can only continue if remittances pay for someone else to do it. Likewise, the applicant's migration has no effect on days worked by non-applicants, but after controlling for remittances, the effect is significant and positive. This also is to be expected: If a breadwinner is away but does not send remittances, other household members must work more to supplement income.

The fact that migration affects agricultural activity—controlling for remittances—is evidence that migration substantially alters investment in home production by altering the technology of home production. It is not definitive evidence, in part because the change in coefficients is not statistically precise. The opposite pattern is seen in the effects of migration on non-agricultural business: the magnitude of the negative coefficient on migration greatly diminishes when remittances are controlled for (though the change in coefficients is also not statistically precise). This pattern could arise because remittances decrease the propensity for remaining family members to start non-agricultural businesses. But this pattern is not compatible with important effects of migrant absence *per se* on non-agricultural entrepreneurial activity. This form of migration may alter home production by removing from the household the person who would otherwise be engaged in farming, but does not appear to remove the person who would otherwise be starting or operating a non-farm business.

8. External validity

The survey sample is far from representative of all Filipinos. The treatment effects we measure are estimates of the effect on 1) households of people who applied for this type of job in Korea, 2) who scored near the cutoff, 3) whose migration decisions were altered by their test score, and 4) whose households yielded a completed survey.

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.[25] 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 physical capital (entrepreneurship, savings).

9. Conclusion

We find that migration from the Philippines to temporary jobs in Korea has important effects on migrants' households. These are theoretically and empirically different from the effects of remittances, as remittances are just one portion of the bundled treatment that is migration. For example, Yang (2008) finds that remittances to the Philippines encourage some types of entrepreneurial activity, conditional on the household already

[25] 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.

having a migrant. This is compatible with our finding that migration has no significant effect on household entrepreneurial activity, since migration is a different treatment: It both puts remittances into the household and takes potential entrepreneurs out of the household.

The model predicts that in unitary households, migration affects investment behavior solely by raising earnings—increasing self-finance and alleviating any borrowing constraints. In collective households, there are two additional channels: migration can alter the balance of power in household decisionmaking and can alter the technology of home production. We find that the most important of these are far and away the financial effects, suggesting that the simplicity of the unitary household model is adequate to explain the most important economic impacts of migration in this setting. While migration causes large changes in how household decisions are made, these changes explain almost none of the important impacts of migration on spending, borrowing, or investment. There is suggestive but statistically imprecise evidence that the collective household is relevant: migration does appear to alter the technology of home production, perhaps by drawing breadwinners out of farming, though this evidence is statistically imprecise.

We find no evidence to support any effect of migration on labor force participation by the spouses or other family members of migrants. The model provides potential explanations for this result: the predicted effect of migration on others' labor force participation is smaller when borrowing constraints are smaller. Households in the sample borrow extensively and the increase in income accompanying migration causes them to borrow less, not more. They may therefore face small borrowing constraints, though we do not have direct evidence of this.

The above findings are compatible with the households surveyed being credit-constrained human capital investors: Migration causes greater private schooling for children, more awards at school, and greater household expenditure on health and education. The findings are not broadly compatible with these households being credit-constrained physical capital investors: Migration has no significant effects on entrepreneurial activity—

except perhaps drawing some families' breadwinners out of farming. It does not raise savings, but causes borrowing to markedly decrease.

Our 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 179 [20%] of our sampled households). Our 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; Jensen and Miller 2012). And it cannot reliably measure the effects of migration experience on return migrants (Reinhold and Thom 2011), because return migrants are self-selected from current migrants.

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Table 1: **The Korean Language Test**

Batch	Date	Total #	⩽5 pts from cutoff score
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: **Checking discontinuity for 23,448 households in sampling universe**

Outcome	$\mu_{s\uparrow 0}$	$\mu_{s\downarrow 0} - \mu_{s\uparrow 0}$	s.e.	p-val.	band-width
Migration behavior after application					
Applicant deployed?	0.0175	0.678	0.0284	< 0.001	2.113
Traits of applicant at the time of application					
Age	30.21	−0.270	0.387	0.486	4.428
Female	0.210	0.000834	0.0548	0.988	2.073
College grad.	0.326	0.0397	0.0632	0.529	2.149
Months experience	70.56	−2.536	3.456	0.463	8.112
Employed	0.291	0.0103	0.0609	0.865	2.211
Married	0.447	−0.0491	0.0666	0.460	2.203
Test batch 1	0.0244	−0.0131	0.0102	0.199	1.610
Test batch 2	0.155	−0.0317	0.0483	0.511	2.151
Test batch 3	0.279	0.0647	0.0602	0.282	2.381
Test batch 4	0.368	0.00516	0.0647	0.936	2.413
Test batch 5	0.174	−0.0247	0.0504	0.625	2.165

Data for households in survey sample. $N_{\text{fail}(s<0)} = 12,577$, $N_{\text{pass}(s\geqslant 0)} = 10,871$.
$\mu_{s\uparrow 0}$ is the mean of the local regression using data for test-failers only, evaluated at $s = 0$.
Optimal bandwidth selected by the method of Imbens and Kalyanaraman (2012).
Triangular kernel.

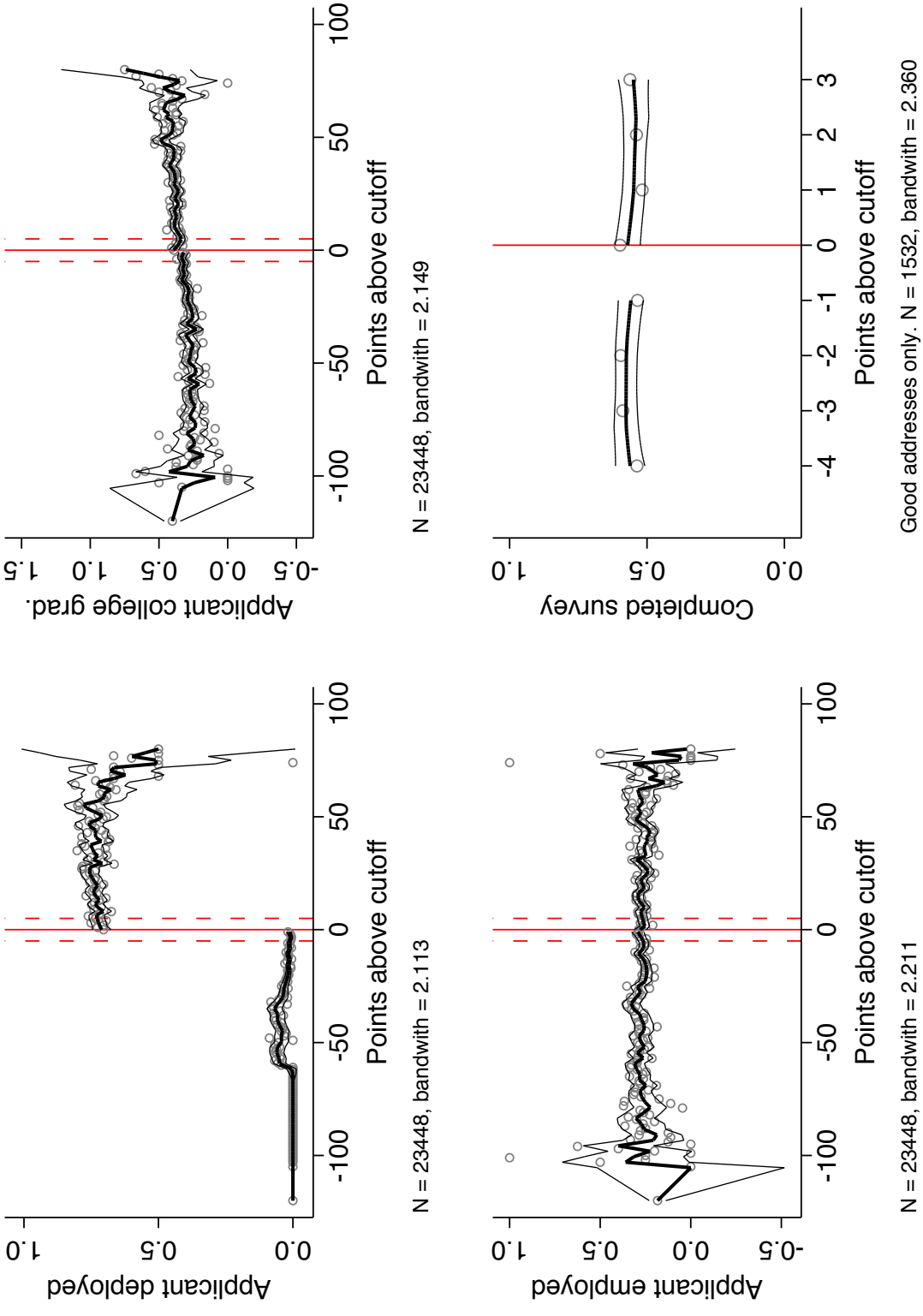


Figure 1: **Discontinuities in sampling universe**

Figure 2: **McCrary (2008) nonparametric test for score manipulation**

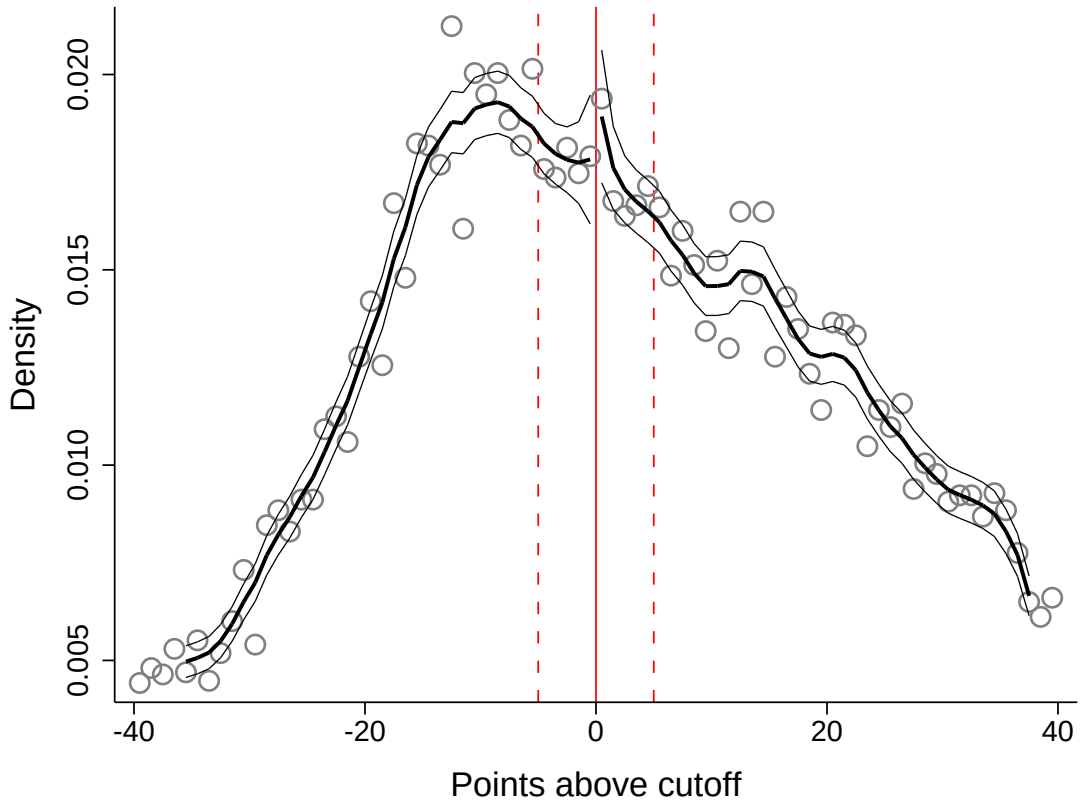


Table 3: **Checking discontinuity for 899 households in survey sample**

Outcome	$\mu_{s\uparrow 0}$	$\mu_{s\downarrow 0} - \mu_{s\uparrow 0}$	s.e.	p-val.	band-width
Migration behavior after application					
Applicant deployed?	0.0172	0.683	0.0407	< 0.001	1.157
Anyone now in Korea?	0.155	0.402	0.0540	< 0.001	1.379
Anyone ever in Korea?	0.241	0.480	0.0551	< 0.001	1.307
Anyone now abroad?	0.422	0.256	0.0608	< 0.001	1.560
Anyone ever abroad?	0.672	0.199	0.0522	< 0.001	1.801
Traits of applicant at the time of application					
Age	30.16	0.450	0.556	0.418	1.768
Female	0.216	0.00591	0.0521	0.910	1.941
College grad.	0.345	−0.0305	0.0593	0.607	1.716
Months experience	64.27	10.93	6.808	0.108	1.472
Employed	0.216	0.0416	0.0533	0.435	1.956
Married	0.460	0.0246	0.110	0.823	2.373
Region: NCR	0.198	−0.0340	0.0487	0.485	1.834
Region: Luzon	0.681	0.0118	0.0585	0.840	1.857
Region: Visayas	0.0862	0.0138	0.0365	0.706	1.844
Region: Mindanao	0.0345	0.00837	0.0242	0.729	1.825
Test batch 1	0.0259	−0.0187	0.0164	0.255	1.819
Test batch 2	0.144	−0.0755	0.0775	0.329	2.268
Test batch 3	0.276	0.0710	0.0991	0.474	2.245
Test batch 4	0.366	0.00947	0.107	0.929	2.413
Test batch 5	0.181	−0.00961	0.0481	0.842	1.911

Data for households in survey sample. $N_{\text{fail}(s<0)} = 460$, $N_{\text{pass}(s\geqslant 0)} = 439$.
$\mu_{s\uparrow 0}$ is the mean of the local regression using data for test-failers only, evaluated at $s = 0$.
NCR = National Capital Region. "Luzon" omits NCR. "Anyone" means any household member.
Optimal bandwidth selected following Imbens and Kalyanaraman (2012). Triangular kernel.

Figure 3: **Discontinuities in survey sample**

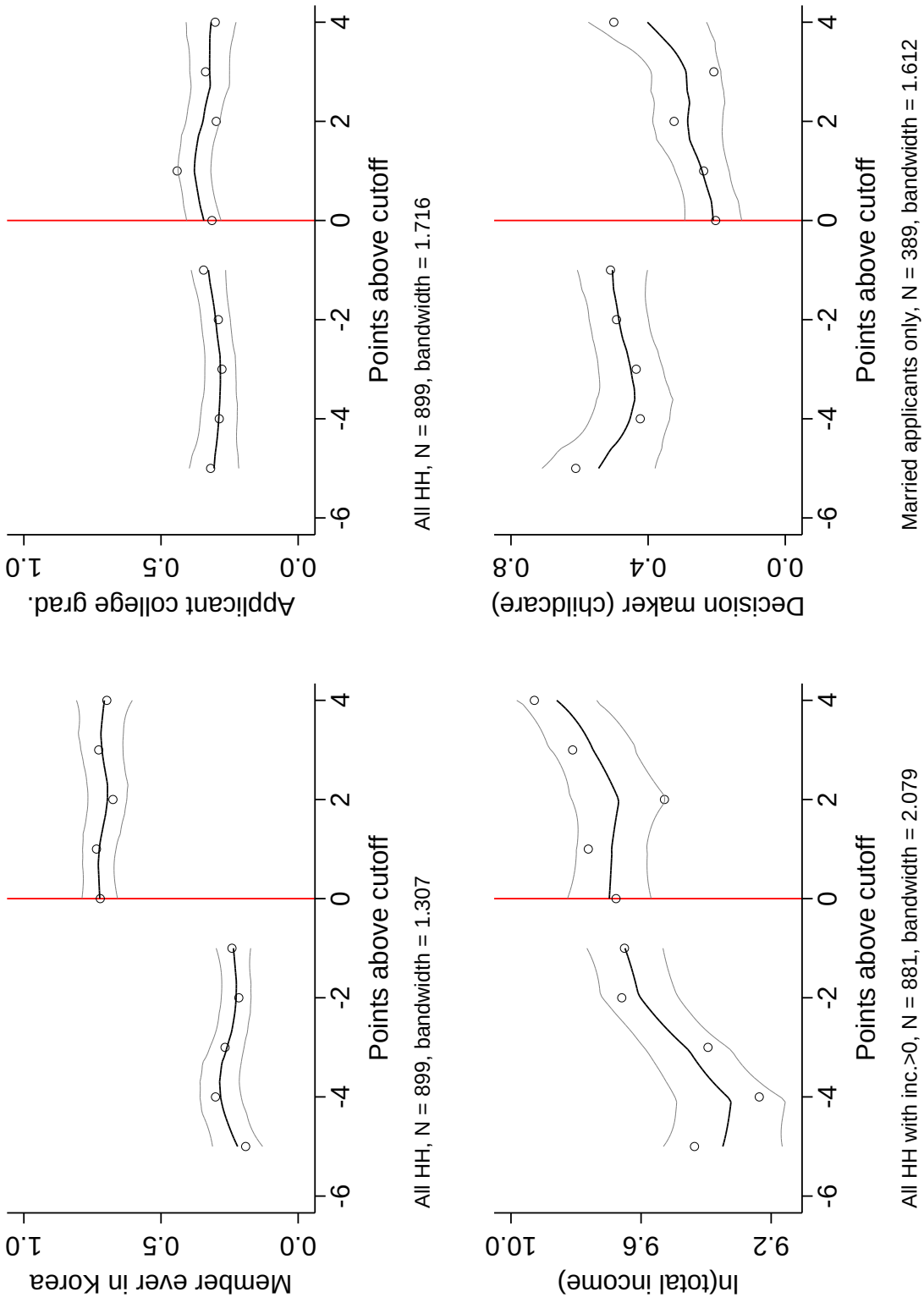
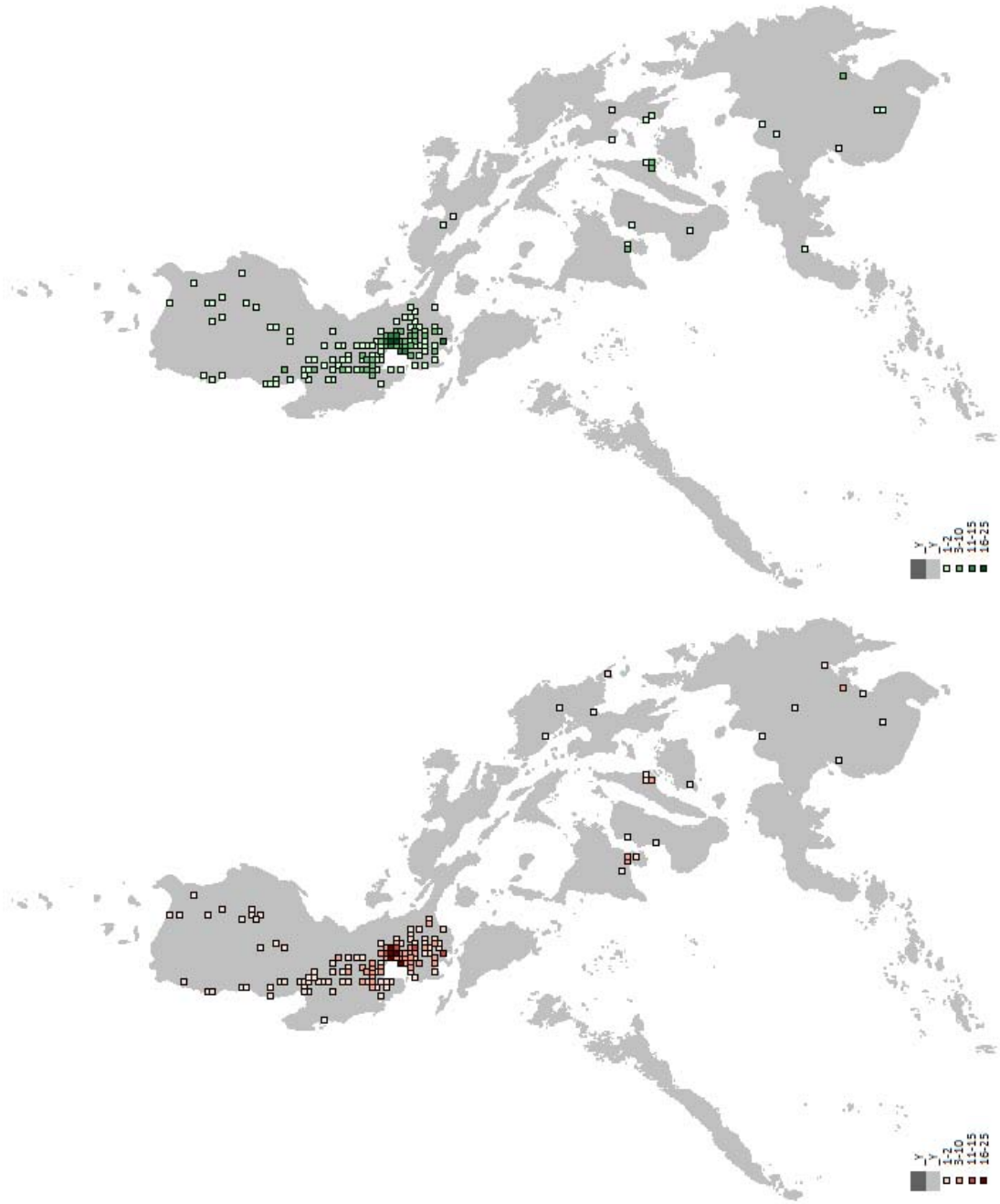
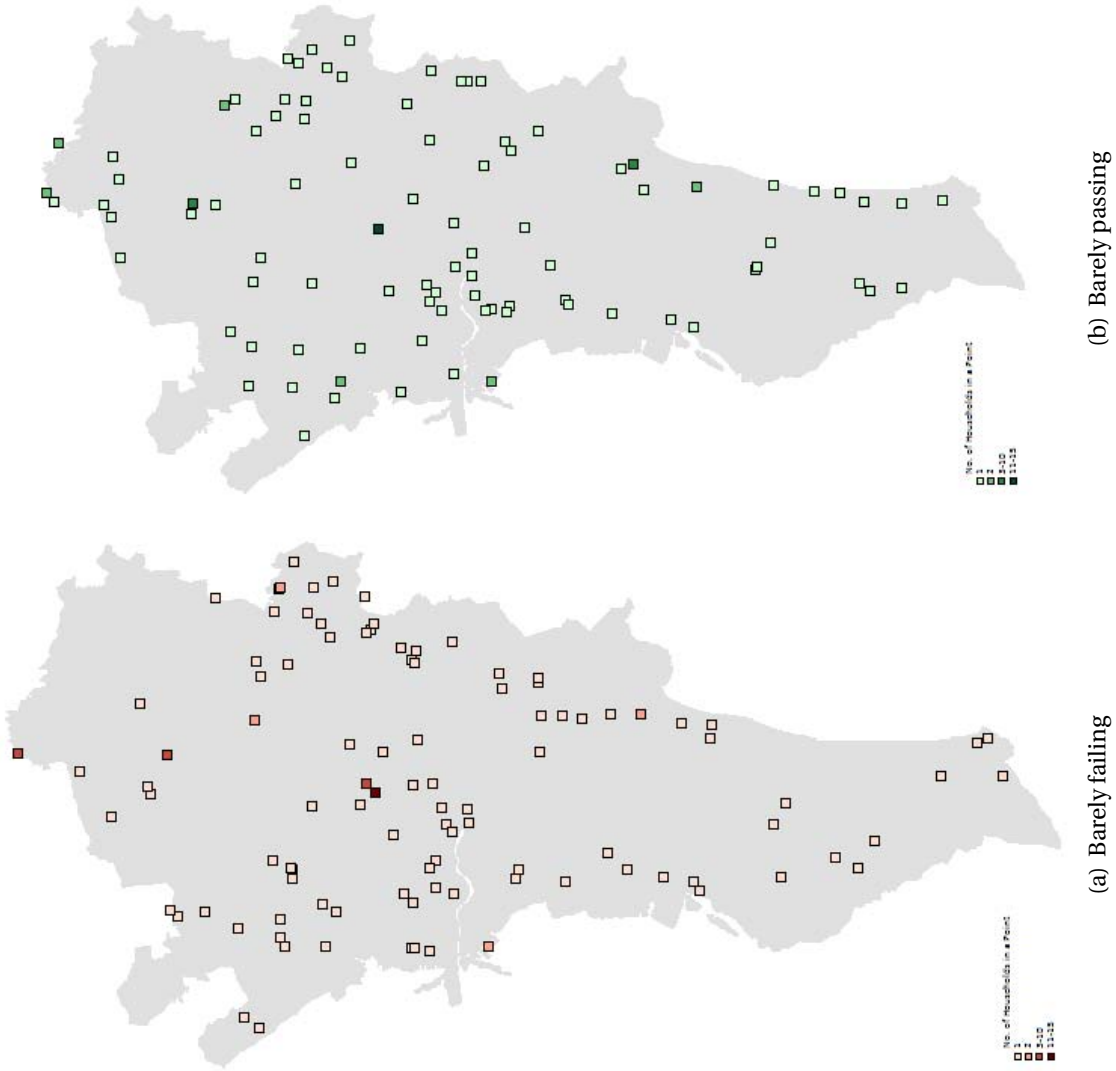


Figure 4: **Locations of surveyed households: nationwide**



(a) Barely failing (b) Barely passing



(a) Barely failing

(b) Barely passing

Figure 5: **Locations of surveyed households: National Capital Region only**

Table 4a: **Impacts on households: Composition and income**

Outcome	$\mu_{s\uparrow 0}$	Intent-to-treat effect			Treatment-on-treated effect			band-
		$\mu_{s\downarrow 0} - \mu_{s\uparrow 0}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0}-\mu_{s\uparrow 0}}{\tau_{s\downarrow 0}-\tau_{s\uparrow 0}}$	s.e.	p-val.	width
Number of household members								
Total	5.194	0.118	0.464	0.799	0.260	1.013	0.798	2.056
Working age	3.369	−0.145	0.387	0.709	−0.315	0.841	0.708	2.505
excl. Korea	3.276	−0.326	0.215	0.129	−0.679	0.437	0.120	1.943
% female	0.537	0.0995	0.0304	0.00108	0.209	0.0608	< 0.001	1.754
Age $\geqslant 65$	1.456	0.182	0.284	0.523	0.397	0.617	0.520	2.134
Age < 18	0.322	0.0671	0.152	0.660	0.146	0.332	0.660	2.543
Any income?: Total	0.983	0.00296	0.0158	0.851	0.00616	0.0327	0.851	1.785
From remittances	0.388	0.226	0.0614	< 0.001	0.472	0.121	< 0.001	1.836
Non-remittances	0.957	−0.0498	0.0311	0.109	−0.104	0.0655	0.114	1.997
Income: Total	21405.8	1764.3	2739.7	0.520	3675.2	5711.1	0.520	1.858
From remittances	4734.6	5690.4	1371.5	< 0.001	11853.9	2764.7	< 0.001	1.467
Non-remittances	16671.2	−3926.2	2533.9	0.121	−8178.7	5183.9	0.115	1.897
ln (*Income*): Total	9.652	0.0397	0.203	0.845	0.0901	0.459	0.844	2.079
From remittances	9.141	0.244	0.156	0.117	0.543	0.342	0.113	1.877
Non-remittances	9.240	−0.260	0.145	0.0730	−0.550	0.298	0.0648	1.944
Business? (agr.)	0.155	−0.0623	0.0418	0.136	−0.130	0.0878	0.139	1.819
ln (*bus. inc.* agr.)	7.396	−0.483	0.834	0.562	5.083	16.03	0.751	2.236
Business? (non-agr.)	0.162	−0.105	0.0803	0.193	−0.229	0.180	0.204	2.184
ln (*bus. inc.* non-agr.)	6.972	0.384	1.038	0.711	0.648	1.760	0.713	2.577

All variables at household level. Working age means $18 \leqslant$ Age < 65. Money amounts in Philippine pesos per month, average over 6 previous months. 'Income' means income going to household members *in the Philippines*. Treatment = household *ever* had a member in Korea. "Bus. inc." = business income. Optimal bandwidth selected by the method of Imbens and Kalyanaraman (2012). Triangular kernel. "Agr." = agriculture (farming, livestock, forestry, fishing).

Table 4b: **Impacts on households: Spending, saving, investing, borrowing**

Outcome	$\mu_{s\uparrow 0}$	Intent-to-treat effect			Treatment-on-treated effect			band-
		$\mu_{s\downarrow 0} - \mu_{s\uparrow 0}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0} - \mu_{s\uparrow 0}}{\tau_{s\downarrow 0} - \tau_{s\uparrow 0}}$	s.e.	p-val.	width
Expenditures: Total	18374.8	5436.0	2417.7	0.0245	11554.2	5379.8	0.0317	3.091
Food	9451.2	487.1	1577.2	0.757	1061.3	3435.4	0.757	2.518
Quality of life	6898.2	1941.2	892.5	0.0296	3981.7	1878.7	0.0341	3.551
Educ. & health	1054.3	1285.9	472.5	0.00650	2678.6	1005.5	0.00772	1.875
Durables	893.4	596.5	683.6	0.383	1303.8	1470.7	0.375	2.204
Savings	1422.1	−278.9	1332.7	0.834	−581.4	2773.6	0.834	3.251
ln (*expenditures*): Total	9.657	0.129	0.124	0.301	0.280	0.271	0.301	2.724
Food	9.001	0.00446	0.0795	0.955	0.00930	0.165	0.955	1.468
Quality of life	8.609	0.146	0.0775	0.0595	0.304	0.162	0.0601	1.523
Educ. & health	6.349	0.312	0.195	0.110	0.684	0.435	0.115	1.875
Durables	5.989	0.475	0.285	0.0958	0.913	0.525	0.0818	1.960
ln (*Savings*)	7.399	−0.0363	0.500	0.942	−0.0648	0.888	0.942	3.458
Any savings?	0.276	0.0956	0.0585	0.102	0.199	0.123	0.106	1.684
Borrowed for business reasons?								
from family	0.181	−0.0810	0.0440	0.0656	−0.169	0.0938	0.0718	1.887
from other	0.106	0.0859	0.0710	0.227	0.188	0.159	0.236	2.030
Borrowed for non-business reasons?								
from family	0.0345	−0.0345	0.0170	0.0427	−0.0718	0.0363	0.0479	1.626
from other	0.147	−0.0466	0.0417	0.264	−0.0970	0.0867	0.263	1.746
Family owns residence?	0.794	0.000421	0.0897	0.996	0.000919	0.195	0.996	2.228
Number of bedrooms	2.253	−0.00121	0.220	0.996	−0.00263	0.478	0.996	2.318

All variables at household level. Working age means $18 \leqslant \text{Age} < 65$. Money in PHP/mo., average over 6 mos. Savings is flow, not stock.
"Food" = food, beverages, and tobacco. "Health & educ." = school, medicine, and medical care. "Savings" includes deposits in banks, paying off loans, extending loans. "Quality of life" = fuel, transportation, household & personal care, clothing, recreation, family occasions, gifts.
"Durables" = durable goods, taxes, home improvement. Bandwidth selection following Imbens and Kalyanaraman (2012), triangular kernel.
Treatment = household *ever* had a member in Korea.

Table 5: **Impacts on individual adults**

Outcome	$\mu_{s\uparrow 0}$	Intent-to-treat effect			Treatment-on-treated effect			band-
		$\mu_{s\downarrow 0} - \mu_{s\uparrow 0}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0} - \mu_{s\uparrow 0}}{\tau_{s\downarrow 0} - \tau_{s\uparrow 0}}$	s.e.	p-val.	width
Applicants only ($N = 875$)								
Worked in past 6 months?	0.833	−0.0339	0.0830	0.683	−0.0744	0.182	0.682	2.227
Now in Korea?	0.142	0.403	0.0541	< 0.001	0.838	0.0854	< 0.001	1.406
Now abroad?	0.336	0.281	0.0612	< 0.001	0.586	0.120	< 0.001	1.603
Applicant's spouses only ($N = 421$)								
Worked in past 6 months?	0.492	0.00331	0.157	0.983	0.00716	0.337	0.983	2.147
Days worked, previous mo.	0.471	0.202	0.814	0.804	0.409	1.627	0.802	3.276
Any wage income?	0.251	0.00755	0.134	0.955	0.0163	0.289	0.955	2.138
ln wage income	9.262	−0.225	0.505	0.656	−0.270	0.615	0.661	3.390
All non-applicant adults only ($N = 2142$)								
Worked in past 6 months?	0.493	0.0446	0.0703	0.526	0.0840	0.133	0.528	2.103
Days worked, previous mo.	0.262	0.161	0.226	0.477	0.304	0.426	0.476	3.192
Any wage income?	0.232	−0.0120	0.0591	0.839	−0.0225	0.110	0.838	2.490
ln wage income	9.171	−0.0910	0.133	0.493	−0.163	0.235	0.489	1.873
Now in Korea?	0.00730	0.0218	0.0141	0.122	0.0412	0.0263	0.117	2.041
Now abroad?	0.0695	0.0387	0.0361	0.284	0.0723	0.0677	0.285	2.402
Years of education	11.03	0.00221	0.269	0.993	0.00413	0.503	0.993	4.436
Visited health facility past mo.	0.141	−0.00140	0.0493	0.977	−0.00262	0.0921	0.977	2.356
if so, private facility?	0.697	−0.00662	0.173	0.969	−0.0110	0.285	0.969	2.904

All variables at individual level. 'Adult' means $18 \leqslant$ Age < 65. Money amounts in Philippine pesos per month, average over 6 previous months.
'Decisions' is an indicator variable for whether the applicant was the primary or joint decision-maker on each subject.
For applicants N=875, for non-applicant adults N=2142. Bandwidth selection following Imbens and Kalyanaraman (2012), triangular kernel.
Treatment = household *ever* had a member in Korea.

Table 6: **Impacts on decision-making by applicant**

Outcome	$\mu_{s\uparrow 0}$	Intent-to-treat effect			Treatment-on-treated effect			band-
		$\mu_{s\downarrow 0} - \mu_{s\uparrow 0}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0} - \mu_{s\uparrow 0}}{\tau_{s\downarrow 0} - \tau_{s\uparrow 0}}$	s.e.	p-val.	width
Applicants ($N = 875$)								
Decisions: *childcare*	0.367	−0.0748	0.107	0.484	−0.165	0.236	0.486	2.087
Decisions: *home repairs*	0.377	−0.0889	0.108	0.410	−0.195	0.239	0.415	2.275
Decisions: *major purchases*	0.451	−0.135	0.0618	0.0286	−0.281	0.129	0.0297	1.967
Decisions: *entrepreneurship*	0.456	−0.0747	0.111	0.501	−0.164	0.245	0.502	2.151
Decisions: *weekend activities*	0.416	−0.151	0.0601	0.0119	−0.315	0.127	0.0130	1.955
Married applicants only ($N = 389$)								
Decisions: *childcare*	0.510	−0.307	0.0870	< 0.001	−0.635	0.212	0.00278	1.612
Decisions: *home repairs*	0.508	−0.246	0.165	0.136	−0.582	0.446	0.192	2.033
Decisions: *major purchases*	0.608	−0.280	0.0910	0.00211	−0.579	0.214	0.00671	1.895
Decisions: *entrepreneurship*	0.589	−0.183	0.164	0.264	−0.432	0.406	0.286	2.010
Decisions: *weekend activities*	0.588	−0.307	0.0898	< 0.001	−0.635	0.208	0.00223	1.782

Observations are individuals. Treatment = household *ever* had a member in Korea. Bandwidth selection follows Imbens and Kalyanaraman (2012), triangular kernel. 'Decisions' is an indicator variable for whether the applicant was the primary or joint decision-maker on each subject.

Table 7: **Impacts on individual children of applicant or applicant's spouse**

Outcome	$\mu_{s\uparrow 0}$	Intent-to-treat effect			Treatment-on-treated effect			band-
		$\mu_{s\downarrow 0} - \mu_{s\uparrow 0}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0} - \mu_{s\uparrow 0}}{\tau_{s\downarrow 0} - \tau_{s\uparrow 0}}$	s.e.	p-val.	width
In school? (if age $\geqslant 6$)	0.984	−0.00862	0.0402	0.830	−0.0309	0.145	0.832	2.108
if so, private facility?	0.275	0.175	0.0754	0.0202	0.405	0.190	0.0328	1.599
Any awards at school?	0.340	0.147	0.0676	0.0300	0.302	0.145	0.0370	1.732
Visited health facility past mo.?	0.144	−0.0496	0.0840	0.555	−0.139	0.237	0.556	2.156
if so, private facility?	0.525	0.453	0.322	0.159	0.683	0.512	0.182	2.109
Working?	0.0105	−0.0105	0.0105	0.317	−0.0217	0.0217	0.316	1.396
Does anyone read to child?	0.537	−0.0747	0.0690	0.279	−0.154	0.144	0.283	1.930
Desired years of education	11.51	−0.707	1.007	0.483	−1.799	2.602	0.489	3.163

$N = 729$. All variables at individual level. 'Child' means age < 18. With age $\geqslant 12$, $N = 117$. Treatment = household *ever* had a member in Korea. Bandwidth selection following Imbens and Kalyanaraman (2012), triangular kernel.

Table 8: **Compare policy discontinuity estimates with observational estimates**

Outcome	RDD*	OLS	Matching, nearest neighbors			Matching
			2	5	10	Mahala-nobis
Households						
ln Expenditures: Educ. & med.	0.684	0.406	0.387	0.355	0.298	0.435
	(0.435)	(0.0779)	(0.112)	(0.0985)	(0.0922)	(0.130)
Business (non-agr.)?	−0.229	−0.297	−0.320	−0.323	−0.317	−0.313
	(0.180)	(0.0155)	(0.0250)	(0.0212)	(0.0195)	(0.0318)
Children (6 ⩽ age < 18)						
In school?	−0.00862	0.0372	0.0164	0.0348	0.0360	0.0202
	(0.0402)	(0.01000)	(0.0175)	(0.0138)	(0.0123)	(0.0227)
Adults (age ⩾ 18)						
Years of educ.	0.00221	0.212	0.278	0.298	0.370	0.616
	(0.269)	(0.102)	(0.175)	(0.144)	(0.129)	(0.213)

*Regression Discontinuity Design estimates from Tables 4a, 4b, 5, and 7. Standard errors in parentheses.
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.

Table 9: **Nonexperimental evidence on effect mechanisms**

Covariates:	None			*ln* Remittances			Decision-making		
Outcome	$\frac{\mu_{s\downarrow 0}-\mu_{s\uparrow 0}}{\tau_{s\downarrow 0}-\tau_{s\uparrow 0}}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0}-\mu_{s\uparrow 0}}{\tau_{s\downarrow 0}-\tau_{s\uparrow 0}}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0}-\mu_{s\uparrow 0}}{\tau_{s\downarrow 0}-\tau_{s\uparrow 0}}$	s.e.	p-val.
Households									
ln Exp.: Quality of life	0.304	0.162	0.0601	0.292	0.267	0.274	0.306	0.172	0.0762
ln Exp.: Educ. & med.	0.684	0.435	0.115	−0.00899	0.650	0.989	0.778	0.460	0.0908
ln Exp.: Durables	0.913	0.525	0.0818	0.544	1.031	0.598	0.943	0.543	0.0824
Borrowed (family)?	−0.0718	0.0363	0.0479	−0.107	0.0766	0.163	−0.0741	0.0384	0.0538
Business? (agr.)	−0.130	0.0878	0.139	−0.322	0.175	0.0654	−0.143	0.0952	0.134
Business? (non-agr.)	−0.228	0.180	0.205	−0.0741	0.277	0.789	−0.231	0.189	0.222
Non-applicant adults									
Worked (6 mos.)?	0.0842	0.133	0.526	0.210	0.209	0.314	0.0744	0.135	0.581
Days worked (past mo.)	0.295	0.442	0.504	1.145	0.338	< 0.001	0.309	0.434	0.477
Any wage income?	−0.0229	0.110	0.835	0.124	0.155	0.422	−0.0189	0.113	0.867
ln wage income	−0.163	0.235	0.489	−0.930	0.602	0.122	−0.168	0.253	0.506
Children (of applicant or spouse), $6 \leqslant age < 18$									
In school?	−0.0309	0.145	0.832	−0.0364	0.649	0.955	−0.165	0.295	0.578
Private?	0.405	0.190	0.0328	2.877	2.548	0.259	0.495	0.272	0.0686
Anyone read to child?	−0.154	0.144	0.283	0.497	0.425	0.242	−0.183	0.185	0.323

Family borrowing is non-business only. Covariates included following Frölich (2007)
Treatment = household *ever* had a member in Korea. Bandwidth selection follows Imbens and Kalyanaraman (2012), triangular kernel.
'Decision-making' variables are five dummies for whether applicant is primary or joint decision-maker in all five areas of Table 6.

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	23015.9	17143.8	[20601.0]	0.00425
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
ln(bus. income, agr.)	7.305	7.580	1.199	0.177
Business (non-agr.)?	0.113	0.395	0.487	< 0.001
ln(bus. income, non-agr.)	6.972	7.926	1.376	< 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
Children ($6 \leqslant$ age < 18)				
In school?	0.968	0.842	[0.363]	< 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 4b.

Appendix

A1. Heterogeneous reduced-form impacts by pre-treatment subgroups

Table A1 explores the heterogeneity of selected reduced-form impacts by pre-treatment subgroups. The first three columns repeat results from the full sample, for reference only (896 households). The second trio of columns are restricted to the subsample in which the applicant was married at the time of application (398 households). The third trio of columns are restricted to the subsample in which the applicant was unemployed at the time of application (649 households).

We do not find strong patterns of heterogeneity in the results by these two subgroups. Standard errors are much larger in the subgroups; the samples are substantially smaller. The positive effect on education/health expenditures and the negative effect on whether the family engages in farming may both be larger among already-married applicants, but these changes are not statistically precise. The effect of the applicant's migration on OFW status of other adults in the household is significant at the 12% level in households whether the applicant is already married.

Among households where the applicant was initially unemployed, the positive effect on durable goods expenditures greatly decreases and becomes statistically insignificant. The effect on private schooling of children of the applicant or applicant's spouse may be larger in households where the applicant was initially unemployed, but this change is not statistically precise.

Appendix Table A1: **Heterogeneous reduced-form effects by pre-treatment subgroups**

Sample:	Full			Applicant already married			Applicant not already employed		
Outcome	$\frac{\mu_{s\downarrow 0}-\mu_{s\uparrow 0}}{\tau_{s\downarrow 0}-\tau_{s\uparrow 0}}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0}-\mu_{s\uparrow 0}}{\tau_{s\downarrow 0}-\tau_{s\uparrow 0}}$	s.e.	p-val.	$\frac{\mu_{s\downarrow 0}-\mu_{s\uparrow 0}}{\tau_{s\downarrow 0}-\tau_{s\uparrow 0}}$	s.e.	p-val.
Households									
ln Exp.: Quality of life	0.304	0.162	0.0601	0.287	0.214	0.180	0.429	0.195	0.0281
ln Exp.: Educ. & med.	0.684	0.435	0.115	1.260	1.237	0.308	0.660	0.512	0.197
ln Exp.: Durables	0.913	0.525	0.0818	1.102	1.344	0.412	0.143	1.029	0.890
Borrowed (family)?	−0.0718	0.0363	0.0479	−0.0615	0.132	0.643	−0.0471	0.0340	0.166
Business? (agr.)	−0.130	0.0878	0.139	−0.210	0.115	0.0675	−0.150	0.109	0.168
Business? (non-agr.)	−0.228	0.180	0.205	−0.155	0.269	0.564	−0.205	0.207	0.323
Non-applicant adults									
Worked (6 mos.)?	0.0842	0.133	0.526	−0.0668	0.141	0.635	−0.115	0.0950	0.224
Days worked (past mo.)	0.295	0.442	0.504	−0.801	1.312	0.542	0.294	0.766	0.701
Any wage income?	−0.0229	0.110	0.835	−0.0131	0.210	0.950	−0.0987	0.0792	0.213
ln wage income	−0.163	0.235	0.489	0.102	0.498	0.838	−0.241	0.288	0.402
Currently in Korea?	0.0412	0.0263	0.118	0.0381	0.0367	0.300	0.0352	0.0242	0.146
Currently OFW?	0.0723	0.0676	0.285	0.115	0.0742	0.120	0.0714	0.0490	0.145
Children (of applicant or spouse), $6 \leqslant age < 18$									
In school?	−0.0309	0.145	0.832	−0.0442	0.135	0.744	0.0190	0.149	0.898
Private?	0.405	0.190	0.0328	0.388	0.175	0.0265	0.667	0.251	0.00798
Anyone read to child?	−0.154	0.144	0.283	−0.268	0.370	0.468	−0.121	0.170	0.479

Family borrowing is non-business only. Sample sizes (HHs): Full 896; applicant married 398; applicant unemployed 649.
Treatment = household *ever* had a member in Korea. Bandwidth selection follows Imbens and Kalyanaraman (2012), triangular kernel.
'Already married' and 'Not already employed' refer to the time at which the applicant applied to the overseas job (pre-treatment).