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INFORMATION, INTERMEDIARIES, AND INTERNATIONAL MIGRATION

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ABSTRACT

Job seekers face substantial information frictions, especially in international labor markets where intermediaries match prospective migrants with overseas employers. We conducted a randomized trial in Indonesia to explore how information about intermediary quality shapes migration outcomes. Holding access to information about the return to choosing a high-quality intermediary constant, intermediary-specific quality disclosure reduces the migration rate, cutting use of low-quality providers. Workers who do migrate receive better pre-departure preparation and have improved experiences abroad, despite no change in occupation or destination. These results are not driven by changes in beliefs about average provider quality or the return to migration. Nor does selection explain improved outcomes for those who migrate with quality disclosure. Together, our findings are consistent with an increase in the option value of search: with better ability to differentiate offer quality, workers search longer, select higher-quality intermediaries, and ultimately have better migration experiences.

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1 Introduction

Information frictions abound in labor markets: employers often struggle to evaluate applicant skill, while job seekers face challenges assessing the relative risks and returns to different employment opportunities. The latter frictions are especially salient in international labor markets where workers must match with employers abroad and navigate complex migration processes. In such contexts, intermediaries have a clear role to play in matching aspiring migrants to jobs. Yet, job seekers often have limited information on intermediary quality and little is known about how these entities affect migration choices and outcomes. This stands in contrast to well-established work on intermediaries in goods markets.[1] In this paper, we offer a new perspective on labor intermediaries and how information frictions shape international job search and migration outcomes.

We study how information about intermediary quality affects migration choices and experiences. Using original survey data from roughly 7,000 female former migrants in Indonesia, we construct quality ratings for agencies responsible for recruiting, training, and placing migrant workers in overseas labor markets. We show, through a randomized experiment, that access to these ratings slowed migration as women waited for offers from higher-quality agencies. Once matched, these women received better pre-departure training and had better experiences with their overseas employers. We interpret these results through a search model, which clarifies how access to new signals of intermediary quality can increase the option value of search, slow the migration rate, and ultimately improve migration outcomes.

The international labor migration process in Indonesia is similar to other low- and middle-income countries across Asia and elsewhere. Every year, hundreds of thousands of low-skill workers go abroad on temporary contracts lasting 2-3 years. Mirroring global trends, women comprise a sizeable share of all Indonesian migrants.[2] They specialize in domestic work and elderly care, and their incomes far exceed those prevailing in labor markets back home. While migrant wages are contractually fixed through bilateral legislation, the quality of pre-departure placement and training, as well as on-the-job amenities tend to vary substantially across workers. Our own formative work and complementary research in Sri Lanka (Fernando and Lodermeier, 2021) suggest that intermediaries, also known as placement agencies, loom large in explaining these varied migration experiences.

Placement agencies facilitate the migration process from start to finish and are especially important for female migrants, who face risks so challenging that sending-country governments sometimes ban their female labor force from working in certain destinations and occupations (Shivakoti et al., 2021). Such blunt policy measures deprive households of high-return work and can push determined workers to migrate via risky, unsanctioned pathways. This raises the question of whether alternative interventions might direct potential migrants to high-quality intermediaries, alleviating some of the downside risks of migration in weak regulatory environments.

Quality disclosure interventions could be particularly effective in settings like ours where intermedi-

[1] See, among others, Ahn et al. (2011), Allen (2014), Antras and Costinot (2011), Mitra et al. (2018), Startz (2021). There is a small literature on migrant smugglers, but this work is focused more on illegal border crossings than on labor market intermediation (Auriol and Mesnard, 2016; Friebel and Guriev, 2006; Gathmann, 2008; Tamura, 2010).

[2] Globally, women comprised nearly half of the 281 million international migrants in 2020 (UN DESA, 2020). In Indonesia, women account for roughly 70 percent of all migrants utilizing formal channels.

aries are numerous and the market is fragmented.[3] While a large number of intermediaries should, in theory, imply considerable choice, it also makes it difficult to learn about agency quality: social networks are limited, and migrations last 2-3 years, which together limit the number of (noisy) agency-specific signals available to aspiring migrants.

Our experiment aimed to identify the value of quality disclosure while holding beliefs about the return to agency quality constant. To do this, we designed three information products. First, a "report card" featured quality rankings of locally-relevant agencies, computed using experiences of female migrants who had returned to Indonesia in the two years preceding our study. Second, to illustrate the "why" and "how-to" of choosing a good agency, we developed an edutainment-style comic book that tells the story of a woman navigating the migration process. Finally, to fix baseline knowledge of the returns to both migration and agency quality, we designed an infographic that illustrated average migration outcomes for women who migrated with high- and low-quality agencies.

We conducted our study in 400 of Indonesia's largest migrant-sending villages. Among these, 101 received the report card and infographic, 98 received the comic and infographic, 101 received all three information products, and 100 control group villages received no information. In late 2015, we disseminated the information materials in interactive community meetings, which reached nearly 30,000 migration-age-eligible women in treated villages. Over the next four years, we tracked 4,805 women who, at baseline, expressed an interest in migrating in the future. We collected detailed survey data capturing migration choices, plans, experiences, and beliefs. To isolate the impact of quality disclosure, we pay particular attention to the effect of the report card conditional on access to the comic and infographic.[4] This nets out the impact of other information products and the facilitation sessions, which, by bringing prospective migrants together, could have affected migration behavior.

To guide our analysis, we develop a theoretical model of migration choice as sequential job search under uncertainty. This mirrors the empirical reality: most migrants live in rural areas and receive sporadic offers from middlemen linked to distant, urban agencies with overseas job openings. In this setup, the report card enables potential migrants to better differentiate high- and low-quality offers. Access to information increases the expected utility of search and raises the quality threshold for offer acceptance. The overall impact on the migration rate is ambiguous. If the threshold effect dominates, the migration rate will decrease, and migration experiences will improve, but differentiation could also help choosy women locate high-quality offers faster, increasing the migration rate. Use of ungraded agencies, however, should unambiguously decrease because the quality threshold increases but beliefs about ungraded agencies do not change. Our model also highlights how changing beliefs about the returns to migration or the distribution of agency quality can affect migration behavior, offering an alternative channel for impact.

Conditional on access to general information about the return to choosing a high-quality agency (via the comic and infographic), the report card significantly reduced the migration rate by 4.8 percentage

[3] In Indonesia, there were roughly 450 agencies operating at the time of our study, alongside many other unregistered providers. This large number is typical of international migration markets; for example, at the time of writing there were 750, 834, and 1,088 legal agencies operating in Sri Lanka, Nepal, and the Philippines respectively.

[4] We alleviate concerns about multiple testing by focusing on outcomes specified in our AEA pre-registration and by reporting sharpened q-values that control the false discovery rate (FDR) (Benjamini et al., 2006), pooling across tests within families of outcomes.

points (p.p.), or 13 percent relative to the control group mean ($p = 0.02$ using conventional standard errors, FDR-adjusted q-value= 0.12). Consistent with our theory of search, the bulk of this decline reflects reduced use of ungraded agencies. For those who migrate, the report card increased the quality of pre-departure preparation (e.g., skills and rights training) by 0.17 standard deviation (s.d.) units ($p = 0.005$, $q = 0.06$) and non-pecuniary aspects of job quality (e.g., employment and living conditions) by 0.10 s.d. units ($p = 0.02$, $q = 0.08$). These improvements are in those parts of the migration process where agencies have the most scope for influence. We see little effect on net compensation, which remains largely outside the control of individual agencies or employers.

Further results support an "increased value of search" interpretation of these core findings. First, the report card had no impact on intentions or steps taken to migrate among those that had not yet done so. Second, the report card did not shift beliefs about average agency quality or experiences while abroad. All these findings are consistent with the hypothesis that the report card increased the reservation threshold to migrate, resulting in higher-quality matches. Indeed, holding access to other information products constant, the report card reduced the likelihood of migrating with a provider in the bottom third of the agency quality distribution by 13 p.p. ($p = 0.005$, $q = 0.04$), while increasing the likelihood of using a provider in the top third by 11.3 p.p. ($p = 0.02$, $q = 0.11$).

This raises the question of whether the report card improved outcomes for "always taker" migrants or simply worked by dissuading marginal women who would have counterfactually chosen low-quality agencies and/or taken low-quality jobs. We find no evidence of such selection effects. Nor do we find spillovers or market-level effects on agencies, which is consistent with Indonesian villages being large, our interventions only reaching a subset of potential migrants, and placement agency catchment areas being large and diffuse. Thus, our results are not driven by changes in migrant selectivity, by broad improvements in agency quality, or by a crowding out of low-quality agencies from treated markets.

The implied value of avoiding bad migration outcomes appears to be high. By delaying migration, women postpone access to large financial returns: the median female migrant earns nearly five times more than the median Indonesia-based worker.[5] By comparing discounted earnings flows at migration rates observed with and without the report card, we infer that women who received the report card should expect to earn USD 197 less than those without the card. This amounts to roughly half of one month's wages abroad and suggests the information in the report card has significant value to women.

Migration has long been conceived as an investment strategy (Sjaastad, 1962), and Burda (1995) was one of the first to formulate migration choice under uncertainty as an option value problem.[6] However, this framework has not informed recent debates in the migration literature. One reason why may be that the option value mechanism is difficult to isolate empirically given the many confounding explanations for low migration rates in observational settings.[7] This is a key innovation of our study. We effectively

[5] The report card had no significant impact on household or individual income, but we are only powered to detect very large effects given the report card's 4.8 p.p. effect on the migration rate.

[6] See also McCall and McCall (1987), who formulate migration as a sequential search problem but focus more on the decision of where rather than when to migrate.

[7] For example, recent work on internal migration takes a broader view of migration costs, including the non-monetary disutility of migration, which implies much higher reservation values for potential migrants (Lagakos et al., 2020). Such an explanation would be consistent with lower migration rates and also indistinguishable from an option value to waiting, especially without rich data on beliefs and intentions to migrate. The same observational equivalence with respect to reservation values would apply to any unobservable improvement in actual or expected employment options at home.

vary the option value to waiting by randomizing access to a technology that allows potential migrants to better parse good offers from bad.

Our study thus offers new insights on the foundations of migration choice in settings with sizable wage differentials. A large literature, on both domestic and international migration, has explored barriers related to information, liquidity, risk, and utility costs (e.g., Angelucci, 2015; Baseler, 2020; Bazzi, 2017; Bryan et al., 2014; Lagakos et al., 2020; Shrestha, 2019). Yet, even when provided with intensive facilitation that acts on several of these margins, aspiring migrants from the Philippines could not be induced to work abroad (Beam et al., 2016). In settings where offers arrive sequentially, it may be that workers perceive an "option value to waiting": they are willing and able to migrate but rationally hold out from doing so in the hopes of drawing a better offer in the future.

Our findings also contribute to a growing literature on information frictions in job search. Much recent research focuses on gaps in employers' knowledge of worker skills (Abebe et al., 2021; Abel et al., 2020; Bassi and Nansamba, 2021; Carranza et al., 2020; McCasland and Hardy, 2020; Pallais, 2014). On the worker side, recent work has focused on misalignment in job seeker beliefs about search success and average prospective job quality (Bandiera et al., 2020; Chakravorty et al., 2021; Conlon et al., 2018), and inefficiencies in job seeker search processes (Belot et al., 2018). We focus on a novel friction: workers may not be able to perfectly observe the returns to a given job offer, and this may affect both how long they search and the quality of the eventual match. While understudied, this mechanism is widely relevant, as evidenced by popular websites that feature information on non-pecuniary aspects of firm quality (e.g., `glassdoor.com` and `indeed.com`). Limited information on job quality—especially among rural-to-urban migrants—might also contribute to the high job turnover rates documented by Donovan et al. (2021) across many low-income countries.

Finally, we contribute to the literature on quality disclosure (see, e.g. Andrabi et al., 2017; Dafny and Dranove, 2008; Hastings and Weinstein, 2008; Jin and Leslie, 2003). Much of this work explores consumer behavior in settings with considerable choice over goods at a moment in time. Thus, the focus is on how quality disclosure affects *which* good is chosen. Yet, many complex markets, including those for labor, real estate, and marriage, are characterized by intensive search over time. We provide theoretical and empirical evidence on how disclosure affects *when* to consume a good (or take a job), and how disclosure can reduce the number of transactions in markets where search is sequential. Our worker-side study is complemented by Fernando and Singh (2021), who examine firm-side effects of quality disclosure in an international migration setting. They find that Sri Lankan placement agencies invest in improving ratings criteria when told they will be part of a forthcoming government-run quality disclosure program. Taken together with our findings, this suggests that improved quality disclosure could have important, wide-ranging welfare effects in migration markets.

The remainder of the paper proceeds as follows. Section 2 provides background on the migration process and role of intermediaries. Section 3 describes the experimental design. Section 4 presents the search model. Section 5 describes the survey data and empirical strategy. Section 6 presents the main results, and Section 7 concludes.

2 Background: Labor Migration with Intermediaries

This section provides background on international labor migration. While some features are unique to the Indonesian context, many are shared by other low- and middle-income sending countries.

2.1 Temporary Female Labor Migration

In 2016, an estimated 9 million Indonesians were working abroad (World Bank, 2017). These migrant workers, who remitted nearly USD 11.2 billion in 2018, play an important role in reducing poverty (Adams and Cuecuecha, 2010; Canuto and Rafha, 2011; Makovec et al., 2018). Many engage in short-term, low-skilled work, on contracts lasting 2-3 years. Because these migrants are disproportionately female, sometimes have low levels of education, and often work as domestic helpers, they are vulnerable to abuse and exploitation (International Organization for Migration, 2010). Since 2010, the Government of Indonesia has focused considerable attention on ensuring the safety of these workers. Key policy developments include moratoria banning work in high-risk destinations, as well as new regulations governing the placement and protection of migrant workers.[8]

As a result, the number of Indonesians migrating through official channels for temporary overseas work has steadily declined. In 2010, official government statistics recorded 575,805 placements, 78 percent of which were women; by 2018, placements had declined to 283,640, with 70 percent women (BNP2TKI, 2013, 2018). At the same time, growing restrictions may have increased the rate of risky, "non-procedural" migration—essentially, working abroad without use of licensed agencies, and/or proper documentation, approvals, and contracts. A recent World Bank (2017) report estimates, for example, that 39 percent of female migrants and 58 percent of male migrants were not fully documented.

Our project focuses exclusively on female migrants for reasons informed by extensive qualitative work we conducted prior to designing the intervention.[9] Although women account for the majority of temporary formal migrants, we found they were less informed than men about different migration service providers and their quality. We therefore hypothesized that there was greater scope for information about service providers to improve female migrants' outcomes. Moreover, female migrants are more likely to use placement agencies compared to men, and the welfare of female migrants is a policy priority, as they are more vulnerable to exploitation and very poor job outcomes.

2.2 Intermediaries and the Migration Process

Mapping the Migration Process. Conditional on taking a documented path, most migrants use placement agencies.[10] Under the status quo, a potential migrant has three ways to connect to an

[8] A moratorium on sending migrants to Malaysia was imposed in 2009 following high-profile abuse cases. The ban was lifted in 2011 after an MOU was signed between Indonesia and Malaysia. In 2011, the execution of Rubiyati binti Sapubi, a maid who was convicted of killing her employer after years of abuse and being refused permission to return home, prompted the Indonesian government to impose a ban on female migrants working in domestic jobs in Saudi Arabia. The moratorium was expanded and formalized in 2015 with a complete ban on such migrants going to 21 countries in the Middle East and North Africa (MENA). This ban was still in place at the time of writing.

[9] This includes conversations with over 160 former and potential migrant workers, several migration service providers, and government officials at the national and local levels (Bazzi et al., 2014).

[10] According to government data, in 2019 just 17 percent of migrants used a formal channel that did not require an agency (Badan Pelindungan Pekerja Migran Indonesia, 2019). In addition to higher-skilled migration through specialized

agency: (i) she can contact the agency directly, (ii) she can connect via local government, or (iii) she can work with a "sponsor". Sponsors are middlemen who partner with agencies that have job openings. Sponsors travel to villages to identify interested prospective migrants and often assist with administrative requirements, thereby offering "doorstep" assistance connecting with an agency. While there is no requirement for migrant workers to use a sponsor, most do, because the migration process is complex, and locating an agency with job openings is often very difficult for migrants to do on their own. Thus, the most common female migrant search strategy is to wait until a sponsor approaches her with an offer to connect to an agency, and decide whether to accept.

Once a migrant has signed with an agency, the agency is responsible for her pre-departure training and preparation. Once all requirements are met, the agency facilitates departure from Indonesia. When migrants arrive in the destination country, they are usually met by a representative of a destination-based placement agency. These agencies partner with Indonesian agencies and serve as a link between the migrant worker and her employer. While the worker is abroad, the Indonesian agency is responsible for providing assistance if problems arise and facilitating repatriation once a contract ends.

Some women also migrate outside the formal system. These "non-procedural" migrants include those who go abroad on a tourist visa and search for work in the destination country, and cases where individuals work with sponsors or placement agencies that are not sanctioned by the government. Such non-procedural migration is common, especially among men and those migrating to Malaysia, and is thought to be particularly risky (World Bank, 2017). While our baseline focused exclusively on migrants who chose government-sanctioned agencies, our endline tracked migration regardless of chosen provider. At endline, we see that 18 percent of women migrated with an agency that they could either not name (a likely sign of informality) or that was not on the government's list of sanctioned agencies. Another 4 percent of women migrated with a sponsor but no agency, and 11 percent migrated without any intermediary (likely a mix of return to a previous employer and undocumented migration).

Migration Quality and the Role of Agencies. Migration quality is determined by both monetary and non-monetary factors. Migrants' wages are typically set through bilateral memorandum of understanding (MOU) legislation and vary by occupation and destination country. The MOUs often result in binding minimum wages for migrant labor (see McKenzie et al., 2014, for evidence from similar labor migrants from the Philippines). Moreover, the Indonesian government regulates placement and preparation costs levied by agencies. Thus, we expect agency-specific variation in net compensation (conditional on occupation and destination) to be driven by non-compliance with official guidelines.

Variation in non-monetary aspects of quality, including on-the-job amenities like working and living conditions, is driven by two factors. First, the quality of pre-departure training affects migrants' experiences with their employers. A housemaid, for example, may struggle to meet employer expectations if she is not trained in basic language skills, or in how to use household appliances. Second, the quality of the employers themselves plays a significant role in migrants' experiences. During our qualitative research, agencies told us they could, to some extent, influence the quality of employer placement by partnering with more reputable counterparts in the destination country.[11]

programs, migrants may go abroad without an agency if they return to a previous employer.

[11] Fernando and Singh (2021) find evidence of the same among Sri Lankan migrant worker placement agencies.

Appendix Table D.1 investigates the extent to which agencies might shape migration quality. We use baseline survey data collected in 2015 from recently-returned female migrants living in 400 major migrant-sending villages (see Section 5.1 for details). We consider agencies' contribution to three critical aspects of migration quality, which are summarized by standardized indices: pre-departure preparation, job quality abroad (excluding compensation), and compensation net of fees.[12] The table reports R^2 measures from regressions of these three outcome variables on increasingly rich fixed effects, with agency fixed effects added last. Consistent with the institutional constraints described above, migrant compensation is relatively fixed within departure year×destination×occupation cells. This set of fixed effects explains 41 percent of the variation in the pay index; adding agency fixed effects only raises the R^2 by 0.05 (column 3). Agency fixed effects explain relatively more of the variation in the pre-departure training and job-quality indices, raising the respective R^2 from 0.07 to 0.21 (pre-departure preparation, column 1) and from 0.18 to 0.26 (job quality, column 2).

Market Structure, Scope for Choice, and Migrant Information. Indonesia's migration market is fragmented at both the national and local level. At the start of our study, there were 451 placement agencies legally registered with the government. Appendix Figure D.1 uses government administrative data from 2016-2019 to assess the scope for agency choice during our intervention period. It plots a histogram of the number of agencies per village placing workers during this period and the village's Herfindahl index, a measure of market concentration, which captures the likelihood that any two migrants are matched to the same agency. The median village had 61 active agencies with a Herfindahl of 0.05, pointing to considerable competition. While some villages exhibit greater agency concentration (see the long tail in Panel B), there was ample scope for migrants in most villages to choose among different sanctioned agencies at the time of our intervention.

2.3 What do Migrations Look Like?

We now draw on our survey data to give a brief overview of migration experiences. We use a "supplementary" sample of women interviewed at endline that were not explicitly targeted by our treatments (we describe this data in detail in Section 5.1). Table 1 explores differences in outcomes among those

[12] The *pre-departure preparation index* includes: a dummy indicating whether the agency provided training, time spent on training, the share of government-mandated training topics covered by the agency (equipment/tools required for job, job skills, destination information, how to remit money, migration insurance policy, how to behave on the job, destination country culture, how to get help when abroad, the repatriation process, migrant worker rights, the migration contract), the migrant's subjective grade (0-10) of the agency training, whether the migrant signed a contract (in Indonesian, that she understood) while at the agency, whether the agency allowed the migrant to leave the training facility and residence, whether the agency held the migrant's identity documents, whether the agency followed legal procedures (per the migrant's assessment), and the migrant's subjective overall rating of the agency on a 0-10 scale. The *job quality index* includes: whether the migrant was given a weekly day off, the job matched the contract, the employer allowed the migrant to retain her identity documents, the migrant had her own private living quarters, the migrant received proof of payment, the migrant was allowed to leave the employer's residence, and the migrant's overall subjective rating of the migration experience. The *pay index* includes: total wages net of salary deductions, total earnings (wages plus other income from the agency, sponsor, and employer) net of costs (salary deductions plus other migration costs paid to the agency, sponsor, employer, or other entities), whether the migrant received the full contracted salary, whether the migrant received salary payments on time, and whether the migrant received additional pay for overtime work.

who migrated with sanctioned agencies and those who used unsanctioned placement providers.[13] This split helps illustrate some of the margins along which intermediaries may shape migration experiences.

Panel A reports demographic characteristics. The average migrant taking a sanctioned path is 33, and over three quarters are married. Thirty eight percent report primary education or less. Seventy six percent had housemaid duties, 28 percent nanny duties, and 46 percent elder care duties; 96 percent reported at least one of these. Twenty-one percent of women migrating with sanctioned agencies worked in a Middle Eastern or North African (MENA) country, despite an ongoing moratorium banning most types of female migrant labor in the MENA region. Women migrating with unsanctioned service providers are older, 26 p.p. more likely to have primary or less education, more likely to perform housemaid and nanny duties, and 45 p.p. more likely to work in a MENA country.

Panel B shows that agencies do not always fully prepare women to migrate. Migrants using sanctioned agencies received information on 61 percent of "priority" training topics and signed a contract they understood 62 percent of the time. On average, migrants rate their agency as a 7.6 on a scale of 0-10. Migrants using unsanctioned providers fare significantly worse: they get trained on half as many topics and are 18 p.p. less likely to sign a contract they understood, despite assigning similar ratings to their agencies. Overall, women using unsanctioned agencies score 0.33 standard deviation (s.d.) units worse on our pre-departure preparation summary index.

Panel C shows that migrants faced difficult working conditions. Among migrants who used sanctioned agencies, 70 percent had no weekly day off, 49 percent could not leave the worksite, 60 percent did not retain their identity documents, 25 percent reported verbal abuse from employers, and 8 percent physical or sexual abuse.[14] Women who migrated with unsanctioned providers reported consistently worse experiences, scoring on average 0.15 s.d. units lower on our job quality summary index.

Panel D shows that while overtime pay is not guaranteed, most women choosing a sanctioned agency were paid the amount stipulated in their contract (just 13 percent reported salary cuts). Average monthly earnings net of migration costs were IDR 4.8 million (USD 347 at the 2019 exchange rate of IDR 13,900 per USD). By comparison, women choosing an unsanctioned agency were 27 p.p. less likely to report overtime pay, 6 p.p. more likely to report salary cuts, and earned IDR 912,000 less. As a result they score 0.26 s.d. units worse on our pay summary index.

3 Experimental Design

The main goal of our experiment is to assess how information about the quality of migration service providers impacts migration outcomes. Our interventions aim to remedy information failures in two ways: first, by providing new information on agency quality based on the experience of thousands of women across hundreds of migrant-sending communities; second, by encouraging migrants to act on already-available information and to search locally for information. To do this, we developed original measures of agency quality, as well as three information products described here.

[13] We drop women who migrated on their own, as this group is likely a mix of those returning to a former employer and those taking an undocumented route.

[14] To minimize reporting bias, migrants placed slips of paper in envelopes to report experiences of abuse; these envelopes were opened at the office after the interview was complete.

Measuring Agency Quality. We construct quality "grades" using data from nearly 7,000 recently-returned migrants who were able to identify their agency. For each migrant, we developed an overall "migration quality" index, which included indicators related to pre-departure training and experience at the agency in Indonesia, experience at the partner agency in the destination, net pay, and job quality (time off, harassment by the employer, etc.). We constructed empirical Bayes estimates of agency-specific components of this index, first residualizing out age, education, year of migration, and district of residence. To further limit sampling variation, we use grades for the 59 agencies who had at least 30 migrant ratings. These graded agencies account for 75 percent of placements at baseline. Our ratings perform well in out-of-sample checks: the grades are significantly correlated with government data on problematic migration episodes in non-study districts, as well as cases of human trafficking recorded by the International Organization for Migration. The online data appendix B provides additional detail on the methodology, indicators included in the ratings, and these out-of-sample validation tests.

Experimental Information Products. We designed an *infographic* to facilitate a common understanding of the returns to migration with high- versus low-quality placement agencies. It included information that our qualitative research indicated was important for migrants' well-being and of interest to potential migrants; this included training quality, quality of life abroad, pay, and job quality. Using our baseline survey, we calculated average outcomes for women migrating with agencies in both the top and bottom 20 percent of the quality distribution. The infographic used simple illustrations to represent these inter-quintile differences (see Panel A of Appendix Figure D.2 for an example).

We communicated agency-specific ratings through a *report card*. We re-scaled our empirical Bayes estimates to run from 50 (worst) to 95 (best) to mimic the grading scale used in the Indonesian school system and mapped scores to pain-scale-style smiley faces that could be readily understood (see Panel B of Appendix Figure D.2). Each of the 101 subdistricts in our study had a unique report card featuring agencies that operated locally. The cards also included the name and contact information of each agency, as well as the number of reviews used to construct the agency rating.

In order to encourage women to place weight on agency quality and spur local information discovery, we developed an edutainment-style *comic book* (Panel C of Appendix Figure D.2 shows an example page). The comic tells the story of a woman deciding to migrate and explores how to navigate agency choice when working with a sponsor. The woman resists an offer to migrate with a low-quality agency and instead waits for an offer from a higher-quality agency. The story also explains how agencies are expected to prepare migrants, including the provision of training, insurance, and emergency assistance.

Site Selection and Randomization. We used 2012-2013 Indonesian government administrative placements data to identify study sites. We first limited the data to the island of Java, which is home to roughly 56 percent of Indonesia's population and 73 percent of its registered female migrants. We then identified the eight largest migrant-sending districts on the island. Within these districts, we selected 400 of the largest female migrant-sending villages.

We randomly divided our 400 study villages into four experimental arms, stratifying on above/below median (i) share of migrants placed in the MENA region, (ii) agency Herfindahl index, and (iii) number of female migrants per capita. All placement information came from government administrative data in

2012-2013. We distributed the infographic and the report card in 101 "report card only" villages, the infographic and the comic in 98 "comic book only" villages, and the infographic, the comic, and the report card in 101 "comic and report card" villages. In the latter, the woman in the comic uses the report card to select a high-quality agency.The 100 control group villages did not receive any materials.

Intervention Implementation and Materials Distribution. We distributed intervention materials through interactive community meetings. Professional facilitators led the meetings along with trained "local facilitators" from the villages. Each village hosted three meetings that targeted former migrant workers and women aged 18-40 who were interested in migrating. Facilitators promoted meetings through print materials posted in the villages, public announcements (e.g., over mosque loudspeakers), and word-of-mouth. Facilitators specifically invited women who we enrolled in a follow-up survey sample while conducting the baseline. Women did not know which intervention materials would be shared, and pre-session "marketing" was the same across all arms. In total, 28,170 women aged 18-40 participated in the community meetings across the 300 treatment villages, for an average attendance of 94 women per village. For comparison, the average village has roughly 2,000 adult women.

In order to maximize treatment exposure and ensure the information remained salient, we re-distributed intervention materials to women who had not yet migrated by the time of a "midline" follow up survey during the fourth quarter of 2017, two years after the intervention. This distribution only targeted women participating in this midline, and no group events were organized in the community.

4 Theoretical Framework

In this section, we develop a search model to highlight key channels through which our intervention might impact migration outcomes. We focus on the report card, since it specifically addresses information frictions related to agency quality, while the other materials address multiple frictions that could have differing impacts.[15] Conditional on community meetings and receipt of the infographic, the report card helps women discern which agencies are "good" and which are "bad", thereby facilitating *quality differentiation* among agencies. In so doing, the card also communicates additional information about the distribution of migration outcomes, which could shift *beliefs about the return to migration.*

4.1 Model Setup

We model a woman's migration decision as a search problem, following canonical models of sequential job search in partial equilibrium (Burdett and Ondrich, 1985; McCall, 1970; Mortensen, 1986).[16] This framework is well suited to our setting for two reasons. First, migrants typically wait for an offer from a sponsor to migrate; thus, job offers arrive sequentially. Second, our intervention reached a small share

[15] For example, the comic both emphasized the importance of agency quality and provided information on migrant worker rights, while the facilitation sessions and infographic covered information on the returns to migration while stimulating broad-based discussion and local information discovery amongst attendees.

[16] For an alternative to search, see Burda (1995), who develops a model of migration in which the option value to waiting is driven by changes in the return to migration.

(5 percent) of the adult female population; we therefore expect general equilibrium effects to be limited (see Section 6.1 for supporting evidence).

We write the model in discrete time. Assume the woman is infinitely lived, risk neutral, and applies discount factor β to future utility. Each period she remains in Indonesia, she receives utility h. Migration offers arrive with per-period probability λ. If a woman accepts an offer at time t, her expected discounted future utility is $u_t \in [\underline{U}, \overline{U}]$.[17] Consistent with the substantial ex-ante search frictions in our setting, a woman cannot directly observe u_t, but (i) knows that each offer is drawn from stationary distribution $F_u(\cdot)$ with expected value $\mathbb{E}[u_t] = \mu$, and (ii) observes a signal $q_t \in [\underline{Q}, \overline{Q}]$ that is informative, in that the woman's (rational) forecast based on her signal, $Q_t = \mathbb{E}[u_t \mid q_t]$, is strictly increasing in q_t, and hence the mapping $q_t \to Q_t$ is 1:1. Denote the distribution of Q_t as $F_Q(\cdot)$.

Then, the expected lifetime utility of search in the present period is:

$$V = h + \beta \left[\lambda \int_{\underline{U}}^{\overline{U}} \max\{Q_t, V\} dF_Q + (1-\lambda)V \right]. \tag{1}$$

This implies a familiar reservation policy: a woman will migrate when $Q_t \geq Q^* = V$ and otherwise stay home to continue search. After setting $V = Q^*$, some algebra, and integration by parts, we obtain the following implicit equation for reservation expected utility:

$$Q^*[1 - \beta(1-\lambda)] = h + \beta\lambda \left[\mu + \int_{\underline{U}}^{Q^*} F_Q(Q) dQ \right]. \tag{2}$$

Now consider improving the migrant's screening technology, embodied in our experiment by the agency report card. We model this as a second signal, $r_t \in [\underline{R}, \overline{R}]$, which is only available for graded agencies. Let $r_t = \emptyset$ for all ungraded agencies. Further assume (i) women do not update utility forecasts for ungraded agencies, and (ii) r_t is non-degenerate for at least some q_t. Denote $\tilde{Q}_t = \mathbb{E}[u_t \mid q_t, r_t]$. A key insight is that this signal *increases differentiation*, in that it allows migrants to differentiate the quality of firms with the same initial quality signal q_t. This implies the distribution of $\tilde{Q}_t$ is riskier than the distribution of Q_t in the Rothschild and Stiglitz (1970) sense. To see this, note that:

$$\mathbb{E}[\tilde{Q}_t \mid q_t] = \mathbb{E}[\mathbb{E}[u_t \mid q_t, r_t] \mid q_t] = \mathbb{E}[u_t \mid q_t] = Q_t. \tag{3}$$

Now define $\zeta = \tilde{Q}_t - Q_t$. Clearly $\tilde{Q}_t \overset{d}{=} Q_t + \zeta$. Using (3), and noting that each q_t maps to a single value of Q_t, we have $\mathbb{E}[\zeta \mid q_t] = \mathbb{E}[\zeta \mid Q_t] = 0$. Then we can apply Theorem 2 in Rothschild and Stiglitz (1970) to Q_t and $\tilde{Q}_t$. Intuitively, the distribution of $\tilde{Q}_t$ is more "spread out": for each value of Q_t where r_t is non-degenerate, the migrant can further differentiate between firms by referring to r_t.

4.2 Impacts of Increased Differentiation

We now show how increased differentiation impacts the per-period migration rate, $\lambda(1 - F_Q(Q^*))$. Holding Q^* constant, a shift from F_Q to $F_{\tilde{Q}}$ will increase the right hand side of equation 2 by Theorem 2 in Rothschild and Stiglitz (1970). Thus, the reservation expected utility must increase to $\tilde{Q}^* > Q^*$. Intuitively, when women are better able to differentiate between good and bad quality firms, they raise their standards as they face a better chance of finding a high-quality placement. Note that the

[17] We think of u_t as comprising both monetary and non-monetary costs and benefits of a given migration.

reservation utility increases to the same $\tilde{Q}^*$ regardless of whether a given offer is graded or not. Because women are risk neutral, the precision of their forecast does not matter; all that matters is whether an offer's associated expected utility is sufficiently high.

Figure 1 illustrates implications for the migration rate. First, Panel (a) considers the effect on the overall migration rate. This is ex-ante ambiguous. Point A illustrates the case of an "eager" migrant who accepts most offers even without an additional signal. At this point, $F_Q(Q^*) < F_{\tilde{Q}}(Q^*)$, and an increase in the reservation expected utility, coupled with a shift to $F_{\tilde{Q}}$, will always decrease the per-period migration rate. Intuitively, better screening technology increases the returns to continued search. At point B, where $F_Q(Q^*) > F_{\tilde{Q}}(Q^*)$, an increase in the reservation utility could either increase or decrease the migration rate; the figure illustrates a case where the rate increases. This point highlights the case of a "choosy" migrant, who only accepts high-quality offers. Increased differentiation helps her secure a high-quality offer faster, hastening migration. By contrast, Panel (b) shows that use of ungraded agencies will unambiguously decrease. This happens because reservation expected utility increases, but forecasts for ungraded agencies are unchanged, meaning $F_{Q|r_t=\emptyset} = F_{\tilde{Q}|r_t=\emptyset}$. Note that this prediction would be unchanged if women instead inferred that being left off the report card was a bad signal.

In Appendix A, we extend our analysis of the overall migration rate, assuming that there exists a parameter σ, which indexes the riskiness of $F_Q(\cdot)$. We show that if the new, "riskier" CDF lies above the original CDF at the original Q^* (i.e., $\partial F_Q(Q^*;\sigma)/\partial\sigma \geq 0$), then an increase in differentiation (i) reduces the probability of migrating and (ii) increases expected utility conditional on migrating. More generally, when differentiation decreases the migration rate, utility conditional on migrating will increase. By contrast, when the migration rate increases, the impact on expected utility for migrants is ambiguous.[18]

4.3 Impacts of Shifting Beliefs

So far, we have assumed that women know the joint distribution of u_t, q_t, and r_t and make rational forecasts. We now relax that assumption and study the impact of shifting beliefs about the return to migrating. In this case, beliefs about the distribution of returns, $F_u(\cdot)$, may diverge from reality. Consider a shift in the average *perceived* return to migration by factor Δ, captured by a translation of the distribution $F_u(\cdot)$ to $G_u(\cdot)$ such that $F_u(x) = G_u(x + \Delta)\ \forall x \in [\underline{U}, \overline{U}]$. While beliefs about $F_u(\cdot)$ shift, the distribution of signals q_t does not. In line with this, assume women update priors such that Δ is only partially passed through to the woman's posterior, Q_t: $F_Q(x) = G_Q(x + \eta\Delta)$, $\eta \in (0, 1)$.

In Appendix A, we show that shifting priors by $\Delta > 0$ will increase reservation expected utility by a factor less than $\eta\Delta$ and increase the migration rate.[19] Intuitively, this means making women more optimistic will increase migration, while making them more pessimistic will reduce migration. Importantly, shifting beliefs alone has no impact on women's ability to differentiate, nor does it affect the actual quality of offers. It follows that shifts that increase the migration rate, which must correspond to a reduction in the lowest q_t that triggers migration, will *reduce* utility conditional on migrating, while shifts that decrease the migration rate will have the opposite effect.[20]

[18] In both cases, a revealed preference argument implies that expected utility *unconditional* on migration must increase.
[19] A shift of $\Delta < 0$ will reduce reservation expected utility and reduce the migration rate.
[20] Impacts on overall welfare are less clear and depend on biases in beliefs. If baseline priors are inaccurate, bringing priors in line with the truth will increase welfare, while distorting priors away from the truth will decrease welfare.

We can distinguish learning from differentiation by assessing impacts on beliefs: differentiation alone has no effect on women's priors, while learning implies that priors should shift towards the information shock. We use rich survey data on beliefs to test this prediction, exploiting the fact that beliefs are heterogeneous within communities, while information delivered to communities is not.

5 Empirical Strategy

This section describes our survey and administrative data as well as the core regression specification. Further details on data sources and variable construction is available in Appendix B.

5.1 Data

We conducted four rounds of surveys (Appendix B includes a timeline). The baseline took place in early/mid 2015. In selecting the baseline sample, we first conducted a village-wide listing to identify former migrants and women interested in migrating regardless of past migration status. We enrolled two samples of women. The first group, a "tracking" sample of 4,805 women, stated they were interested in migrating in the near future. We follow these women in all later rounds, and they comprise our core analysis sample. These women were also explicitly invited to the information sessions in all treatment villages. We stratified the tracking sample by migration history: 2,403 women had migrated before while 2,402 had not. The second group, a "former migrant" sample of 5,607 women, had previously migrated with an agency and returned home in 2014 or 2015. We used data from these women to construct agency quality measures but did not target them for future follow up.

We collected data on demographic and behavioral characteristics as well as expectations and beliefs about migration from all women. We also collected detailed information from former migrants on their experiences during their last migration including compensation, costs, use of sponsors, experiences with the agency in Indonesia, and experiences with the employer abroad.

After the intervention in late 2015, the tracking sample participated in three follow up surveys: (i) a first "midline" August-December 2016, (ii) a second midline October-December 2017, and (iii) the final "endline" May-October 2019. All three waves collected data on new migrations. We also collected data on expectations and beliefs about migration. For women working abroad, we conducted a phone survey that covered the same set of topics, but was shorter, as they had less time for an interview. If a woman could not be reached, then we conducted a short-form "informant survey" with a household member knowledgeable about the woman's work status. Informants could be respondents' spouses, parents, children over 18 years of age, other extended family, or neighbors.

During the endline survey, we also interviewed 2,418 recently-returned migrants who were not part of the tracking sample. This "supplementary" sample included women who had departed for overseas work after the intervention began but who were not explicitly invited to attend community information-sharing meetings. We use this sample (i) to test for the reach of our interventions and (ii) to calculate updated agency grades reflecting endline experiences.

Our project also uses administrative data from the Indonesian government. We select study sites using placement records from 2011-2013. We evaluate village-wide treatment effects using a separate

batch of placement data from 2015-2019. One important limitation of these data is that they only record migrant departures facilitated by sanctioned agencies. Finally, we used administrative data on migrant returns from 2010-2013 to validate our agency grades (see Appendix B for additional detail).

5.2 Estimating Equation and Adjustments for Multiple Testing

We estimate the following regression equation:

$$y_{iv} = \beta_0 + \beta_1 comic\&report_v + \beta_2 comic_v + \beta_3 report_v + \gamma_s + \varepsilon_{iv} \tag{4}$$

where y_{iv} is the outcome of interest for woman i in village v; $comic\&report_v$ identifies villages randomly selected to receive the report card, the comic book, and community facilitation (which included the infographic); $comic_v$ identifies villages randomly selected to receive the comic book and community facilitation; and $report_v$ identifies villages that received the report card and community facilitation. We also report estimates of the impact of the report card conditional on access to other information products, $\beta_1 - \beta_2$, as we are especially interested in isolating the effect of the report card; and, for symmetry, the impact of the comic conditional on access to other information products, $\beta_1 - \beta_3$.

One concern with our analysis relates to multiple testing—not just with respect to outcomes, but also with respect to comparisons between experimental arms. To address the former, our main analysis focuses on outcomes listed in our pre-registration: the migration rate, the market share of placement agencies, use of middlemen, migrant beliefs about agency quality, migrant satisfaction and experiences abroad, and migrant compensation and fees paid to intermediaries.[21] Since our survey included multiple measures of beliefs, satisfaction/experiences abroad, and compensation/fees, we aggregate these into standardized indices following Kling et al. (2007). To address concerns related to multiple comparisons between experimental arms, we report sharpened q-values that control the false discovery rate (or FDR, the share of all rejections that are false) following Benjamini et al. (2006). Setting a q-value discovery threshold of x means that $100 \times x$ percent of all rejections at this threshold would be false.[22]

Table 2 describes the tracking sample control group and verifies that baseline outcomes of women in the tracking sample are balanced across treatment arms. Around two thirds of the control group planned to migrate to an Asian country and planned to use a sponsor. Women understood that migrants have difficult jobs: on average, they expected to earn IDR 5.8 million/month in wages, work 12 hours a day, and reported a fifty percent chance of having a day off. Twenty-two percent expected to experience physical abuse. Compared to summary statistics for former migrants in Table 1, tracking sample women had reasonable expectations regarding pay, were somewhat optimistic regarding time off, but were pessimistic regarding risk of abuse. Overall, characteristics are well balanced across arms with just one of 22 joint tests that $\beta_1 = \beta_2 = \beta_3 = 0$ rejected at the 10 percent level or higher.

[21] Our pre-registration highlights use of administrative data to measure the migration rate and agency market share. While we report such results, our main analysis focuses on these outcomes as measured in our survey data. This is both because our intervention had limited reach outside the tracking sample, who were specifically invited to the facilitation sessions, and because the administrative data cannot capture use of unsanctioned agencies and other forms of undocumented migration, which is common in our sample and important for understanding impacts on overall migrant experiences.

[22] We form FDR families by pooling p-values associated with all reported tests in a given table, with each table focusing on a given family of outcomes. The only exception is Table 4, which includes results for two conceptually distinct families: the migration rate and use of intermediaries (agencies and sponsors). We calculate q-values separately for those two.

Appendix Table D.2 reports survey follow-up rates. Despite high rates of migration, we interviewed around two thirds of women in person in the three follow-up rounds and 74-83 percent of women either in person or by phone (we term these "direct interviews"). We verified the migration status of 97-99 percent of the sample using either direct or informant interviews. While there are no significant differences across treatment in "any interview" rates, the report card (conditional on the comic) was associated with a 4-6 p.p. higher rate of in-person interview at midlines 1 and 2. There were no differential interview rates at endline. We therefore rely on direct interviews from this final round for outcomes other than the migration rate and use of agencies/sponsors, which are easily reported by informants.

6 Main Results

This section presents our core findings on migration outcomes. After demonstrating the successful diffusion of information among the tracking sample, we show how the treatments reshaped migration choices. We then examine impacts on migration experiences and show that the improvements associated with the report card cannot be explained by selection into migration. We close with a discussion of downstream effects on well-being and a revealed-preference estimate of the value of quality disclosure.

6.1 First Stage: Exposure to Intervention Materials

In Table 3, we examine self-reported exposure to information materials among the tracking sample. Panel A reports exposure during the second midline, which took place two years after the intervention. We limit attention to in-person interviews, since we used examples of the intervention materials to prompt responses. In treatment areas, 18-19 percent of women recalled attending a community meeting on migration in 2015 (column 1). Recall of the infographic is similar, ranging from 11 percent in the comic-only arm to 14 percent in the report card plus comic arm. Women in villages where the report card was distributed were 10-13 p.p. more likely to recall the report card than those in control villages (column 3). Likewise, women in villages where the comic was distributed were 17-19 p.p. more likely to recall the comic (column 4). In control villages, roughly 4 percent of women recognized all materials, which could be due to incorrect recall or exposure to other interventions by NGOs.

Panel B reports exposure during the endline survey, which took place 3.5-4 years after the intervention. While self-reported meeting attendance is roughly unchanged, women are significantly more likely to report having seen the information sharing materials, reflecting the supplementary distribution of materials that took place at the end of the second midline.[23]

Finally, Appendix Table D.4 uses the supplementary sample to understand the reach of our intervention. Unlike the tracking sample, these women were not encouraged to attend the community meetings in 2015 and did not receive supplemental materials in 2017. They rarely recognized materials assigned to their community: less than 5 percent recalled attending a community meeting, and we find no evidence of increased recognition of the report card and infographic in treatment villages. Women in comic villages are 6 p.p. more likely to recognize the comic, however. Overall, these results suggest

[23] Relative to the second midline, at endline more women in control villages report having seen the comic (14.9 percent versus 4 percent). This may reflect the rollout of a government-run program, *Desmigratif*, which aimed at empowering women in migrant-sending communities across Indonesia and which, in some cases, provided comic vignettes to villagers.

that our intervention's reach was concentrated among the tracking sample, making village-level effects unlikely. Given this, we focus on the tracking sample for the main analysis.

6.2 Migration Choice, Intentions, and Beliefs

Table 4 uses the tracking sample at endline to study impacts on the migration rate and use of intermediaries. We focus on departures since October 2015, i.e., after community meetings in treatment villages. We study impacts on the migration rate using the full sample (column 1) and the subset of women for whom we have direct interviews (columns 2), as informants were not able to reliably report agency names. Since our theory only delivers an unambiguous prediction for migration without a graded agency, we report impacts on this outcome in column 3.[24] We do, however, use informant reports of whether or not a migrant used an agency or sponsor in columns 4 and 5, as informants were typically knowledgeable about these outcomes.

Relative to the control group, none of the treatments significantly shifted the overall migration rate, though women in villages that received the report card and comic were 3.7 p.p. less likely to migrate with an ungraded provider ($p = 0.055$, $q = 0.15$). Our most intensive treatment also shifted use of intermediaries among migrants, with women in this arm 3.8 p.p. more likely to use an agency ($p = 0.053$, $q = 0.19$) and 8.2 p.p. more likely to use a sponsor ($p = 0.001$, $q = 0.02$). We also see a marginally significant shift towards sponsors in the comic and report card only arms.[25]

To isolate the effect of quality disclosure, we study the impact of the report card conditional on receipt of the comic and facilitation ($\beta_1 - \beta_2$). The report card reduced the migration rate by 4.8 p.p. (column 1; $p = 0.02, q = 0.12$), or a 13 percent reduction relative to control villages, where 37 percent had migrated. Reassuringly, results are similar when limiting attention to directly interviewed migrants in column 2 (a 5.9 p.p. reduction, $p = 0.01$, $q = 0.12$). In line with our theory of search, the reduction in migration is driven by offers from ungraded providers: women are 4.9 p.p less likely to migrate with such a provider (column 3; $p = 0.02$, $q = 0.12$), a 22 percent reduction relative to the control mean. Among migrants, the report card had no effect on use of agencies (column 4) and sponsors (column 5). Finally, the comic, conditional on access to the other materials, only had a significant impact on use of ungraded providers (a 3.9 p.p. decline, $p = 0.06$, $q = 0.15$).

We use reports of migration month and year to explore the timing of conditional report card treatment effects. Figure 2 reports dynamic treatment effects based on the panel analogue of equation (4) with a full set of monthly interaction terms. The report card significantly reduced migration by around 3 p.p. by January 2016, 3-4 months after community meetings. This timing is plausible given the typical lag between initial migration intent and eventual departure. The negative effects stabilized around 3.5 p.p. in mid-to-late 2016, only to start falling again around mid-2018. This occurred roughly six months after the second midline survey (see gray shading), when we reintroduced the information materials to tracking sample respondents. This suggests two possibilities: either the report card had a

[24] We classify migrations in which a woman reports not knowing the name of her agency as ungraded, as this is a likely signal of an unsanctioned agency. Consistent with this hypothesis, women using "unknown" agencies generally have significantly worse experiences compared to women migrating with known, sanctioned agencies.

[25] With point estimates similar across treatment arms, this shift towards sponsors may reflect a generalized effect of the facilitation sessions, which brought migrants together to discuss the migration process, including use of intermediaries.

significant effect on migration behavior even absent the in-depth facilitation during community meetings, or the follow up visit kept the report card top of mind.

The long-term persistence of the report card's effects raise the question of whether this reflects reduced search effort, due to either discouragement or more pessimistic beliefs about migration outcomes. Table 5 evaluates impacts on search effort among non-migrants, which we proxy using reported migration plans and steps taken to migrate, including securing relevant permissions, choosing a sponsor, and contacting and choosing an agency. Point estimates are small, rarely significant, and almost always positive. Notably, the report card, conditional on access to other intervention materials, more than doubled the share of non-migrants who had already contacted an agency, which is consistent with our option-value-to-search mechanism. However, this result is not robust to FDR adjustments ($q = 1$), reflecting the large number of null effects in the table. Taken together, we find no evidence that the intervention materials meaningfully deterred potential migrants.

Even if the report card did not deter potential migrants, it could have increased pessimism, making women choosier when considering offers and thereby reducing the migration rate. Table 6 tests this hypothesis by studying impacts on beliefs, again restricting the endline sample to directly interviewed non-migrants. We consider three indices capturing beliefs about the migration experience. The *agency* index comprises seven questions about the pre-departure experience with the agency (e.g., whether the agency provides information on migrant worker rights). The *job quality* index comprises nine questions about the employment experience abroad (e.g., being required to work more than 12 hours a day). Finally, the *infographic* index comprises eight questions about the agency and job experiences that are included in the infographic.[26] For each question, women assessed the likelihood of a given binary outcome on a 0-10 scale (0 for no chance, 10 for certainty), first for themselves and then for other women like them in their community. We focus on beliefs about others, which are less likely to be influenced by, for example, a woman's hopes for using the report card to secure a better placement. For each outcome, we follow Kling et al. (2007) in constructing standardized indices across the component measures, first recoding each component to go from 0, the worst, to 10, the best possible expected outcome.

We see little evidence of systematic effects on beliefs in Table 6. The estimated null effects are quite precise, with standard errors rarely being larger than 0.03 standard deviation units. In short, we find no indication that any of the interventions moved average beliefs about the migration process. This could be because our sample villages are among the largest migrant-sending communities in Indonesia, meaning women had ample time to learn about average migration outcomes. Moreover, the treatments did not change the variance in beliefs. Our results are unchanged when considering women with above- and below-median baseline beliefs separately (see Appendix Table D.5), and the distribution of beliefs is nearly identical across treatment arms (see Appendix Figure D.3).

Despite conveying novel information about potential risks and uncertainties, neither the report

[26]The agency index includes beliefs about whether the agency will allow the migrant to leave the training facility, hold the migrant's documentation*, provide information on legal rights*, follow legal procedures, provide accurate information, provide quality food and water during training, and have respectful staff. The job quality index includes beliefs about whether the migrant will work more than 12 hours per day*, have a job that matches the contract signed pre-departure*, have a day off each week*, retain their salary as scheduled*, have no wage deductions beyond the contracted amounts*, retain control of their documentation*, receive gifts from the employer, complete their full contracted employment, and not experience physical abuse. The infographic index includes all foregoing measures with an asterisk.

card nor the comic changed beliefs about migration experiences or intentions to migrate. Rather, the combined evidence thus far suggests that with the report card, women may have opted to delay migration, waiting longer for a higher-quality match. This is the mechanism highlighted by the model in Section 4, and the remainder of the paper aims to substantiate such an interpretation. We begin with evidence of why women may be waiting longer to migrate, namely that the report card helps them secure offers from better agencies that result in better migration experiences.

6.3 Migration Experiences

The model in Section 4 predicts that when the migration rate decreases, those who do migrate should have better experiences. Here, we provide evidence consistent with this prediction, using the three experience summary indices featured in Table 1: *pre-departure preparation* (the most direct indicator of improved agency quality), *job quality* (non-pecuniary employment amenities), and *pay*. Appendix Tables D.6-D.8 report impacts on individual index components.

Table 7 presents results for directly interviewed migrants. Again, there are no significant treatment effects relative to the control arm. However, conditional on the comic and facilitation, the report card significantly improved pre-departure preparation within Indonesia and job amenities abroad. The report card increased the pre-departure preparation index by 0.17 s.d. units ($p = 0.005$, $q = 0.061$), including a 2 week increase in time spent on training and a 10 p.p. increase in the likelihood of signing a contract. These quality improvements extend to the migrant's time abroad: the report card increased job quality by 0.10 s.d. units ($p = 0.017$, $q = 0.081$), including an 8 p.p. increase in the likelihood of getting a weekly day off. Together, these results suggest that quality disclosure improved women's experiences throughout the migration process.

We also find that the comic, conditional on receipt of the other materials, had a similar positive impact on the job quality index (a 0.11 s.d. unit increase $p = 0.008$, $q = 0.061$) and a smaller, though still marginally significant, impact on pre-departure preparation (0.11 s.d. units, $p = 0.066$, $q = 0.250$). This suggests a complementarity between the report card and the comic. For example, the report card may have had a more powerful impact when women were given greater context on why agency choice matters, or encouragement to choose a good agency may have had little impact absent a screening tool. Given our design, however, we cannot rule out the possibility that other aspects of our treatments (e.g., the group meetings themselves) induced migrants to make choices that led to worse experiences, dampening the standalone effects of the report card and comic. Finally, we note that gains associated with the report card and comic do not extend to pay, which was unmoved by our treatments.

These improved experiences line up with the parts of the migration process that agencies have the most scope to influence. Recall from the discussion of Appendix Table D.1 that there is little cross-agency variation in migrant pay for women going to a given destination to work in a given occupation. There is, however, much more variation in non-pecuniary dimensions of the migration process, especially during pre-departure preparation. Still, some job quality improvements may come from choosing destinations or occupations that require more preparation or offer better employment conditions. In practice, we find little evidence of treatment-induced sorting towards destinations or occupations associated with higher returns (i.e., Asian countries and formal, non-household-bound jobs, see Appendix Table D.9).

This suggests that much of the variation in realized migration experiences may come from the agency itself rather than the types of jobs or destinations that the agency specializes in. Later, we address a confounding explanation based on selection into migration.

6.4 Migration Provider Market Share

Another way to assess impacts on migration quality is to look directly at the quality of agencies used for migration. Doing so requires addressing three challenges. First, many agencies used at endline were not graded at baseline: only 18 percent of endline migrants used an agency featured on the report cards. Second, agency quality could have shifted over time. Third, baseline grades do not cover providers operating outside the formal system, including unsanctioned agencies or sponsors who send migrants abroad through informal channels. We address this final challenge by focusing on the "market share" of placement providers within our tracking sample. To address the first two challenges, we construct an updated set of provider grades reflecting endline experiences of the 2,418 women in the supplementary sample, who, per Table D.4, were largely unexposed to our interventions.[27] We include separate provider categories for (i) sponsor-only migrations, (ii) "self migrations" where the respondent used neither agency nor sponsor, and (iii) probable unsanctioned agencies. For this last category, we compute separate grades for cases where the respondent could not offer a name and cases where the name could not be found on official government lists. This approach yields a grade for 96 percent of tracking sample migrants at endline, with no differences in coverage across treatment arms.

Table 8 reports significant positive treatment effects on placement provider quality. We classify providers as either low, average, or high quality based on terciles of the endline grade distribution in the control group. Relative to the control group, we find that the report card arm and the report card plus comic arm impacted provider choice. Respondents in both treatment arms were less likely to migrate with a low quality provider, with larger point estimates in the combined treatment arm (13 p.p., $p = 0.003$, $q = 0.035$; relative to 7.7 p.p., $p = 0.095$; $q = 0.22$). The average provider grade also increased significantly in the combined treatment arm ($p = 0.034$; $q = 0.13$).

We find strong evidence that the report card, conditional on the comic and facilitation, shifted migrants towards higher-quality providers, reducing use of low-quality providers by 13 p.p. ($p = 0.003$, $q = 0.035$), while increasing use of high-quality providers by 11.3 p.p. ($p = 0.023$, $q = 0.110$). Column 4 shows a 2.3 unit increase in the average grade ($p = 0.015$, $q = 0.098$). Figure 3 illustrates distributional impacts of the report card conditional on other intervention materials: a Kolmogorov-Smirnov test rejects equality of grade distributions between the comic only and comic + report card arms with a p-value of 0.011. Table 9 shows that these impacts do not reflect a shift towards agencies on the report card, or a use of better graded agencies. Rather, the report card increased use of ungraded, but sanctioned agencies by 11 p.p. ($p = 0.031$, $q = 0.102$), while reducing use of providers outside the formal legal system (unsanctioned agencies or no agency) by 11 p.p. ($p = 0.010$, $q = 0.075$).

[27] We follow the same approach used to construct baseline grades for the report card: we (i) take an index summarizing overall migrant experiences (here, a simple average of the pre-departure preparation, job quality, and pay indices), (iii) residualize out education, age, district of residence and departure year, (iii) construct empirical Bayes estimates of provider-specific means, and (iv) re-normalize these to run from 50-95. In Data Appendix B, we document persistence in agency grades: a one unit increase in the baseline grade is associated with a 0.19-0.26 unit increase in the endline grade.

These patterns are consistent with our theoretical framework. Sponsor-only migrations receive an average endline grade of 50, self migrations a 59, and unsanctioned agencies a 72, while sanctioned agencies (both on and off the report card) receive an 82. If the report card shifted potential migrants' reservation quality threshold up, this would dissuade use of lower-quality ungraded providers, while potentially having mixed effects on takeup of offers from firms on the report card (recall Figure 1).

6.5 Does Selection Drive Quality Improvements?

Together, the evidence in Tables 7 and 8 suggests that information frictions constrain migrants to hastier departures with agencies offering less pre-departure preparation and jobs with lower non-pecuniary benefits. An important question, then, is whether this reflects selection versus an improvement in outcomes for "always taker" migrants. For example, the report card could have dissuaded low human capital migrants, who would have counterfactually chosen poorer-quality agencies and gone on to worse jobs. A related concern is selection into direct (in person or phone) interview. While our treatments had no impact on the share of migrants interviewed directly (Appendix Table D.3), it is also important to test for differential selection into direct interview by treatment arm.

A Machine-Learning Approach to Counterfactual Migration Experiences. First, we develop a measure of predicted migration experience based solely on pre-treatment survey data. Concretely, we train a random forest on a broad set of individual-specific covariates measured during the baseline survey (see Appendix B for detail on covariates, which include beliefs, locus of control, cognitive ability, time and risk preferences). To train, we focus on the subset of the tracking sample that migrated at baseline, using pre-intervention migration experience as the outcome of interest.[28] We then use this model to predict future migration experience for women in the tracking sample. This delivers a measure of potential migration experience that is (i) highly predictive of actual migration experiences at endline (see details in Appendix B), and (ii) balanced across treatment arms at baseline (Table 2).

Table 10 tests for differential selection into migration by regressing an indicator for migration on predicted migration experience, the three treatment dummies, and interactions between predicted experience and treatment. In column 1, women predicted to have better experiences based on their demographic characteristics are more likely to migrate, but there is no evidence of differential selection across treatment arms; we cannot reject the null that all interaction terms are jointly equal to zero, $p = 0.849$. Column 2 adds controls for demographics, cognitive ability, personality/preferences, planned search behavior, and beliefs, as well as their interactions with treatment indicators. While less parsimonious, this specification addresses differential selection on observables that may not correlate with predicted experience. Again we find no evidence of differential selection ($p = 0.849$).[29]

The last two columns of Table 10 repeat the exercise, this time limiting the sample to migrants and testing for differential selection into direct interview. Columns 3 and 4 show that there is no evidence of differential selection into direct interview.

[28] To simplify the exercise, we train the forest to predict an "aggregate experience" index, which is a simple average of the pre-departure preparation, job quality, and pay indices. Insights are similar when looking at each separately.

[29] See the notes to Table 10 for a full list of covariates. To avoid over-rejection in the presence of many regressors, we use a wild bootstrap F test, which delivers accurate inference (see Richard, 2019).

A Model-Driven Selection Correction Procedure. In a second approach, presented in Appendix C, we use the model in Section 4 to inform a selection-correction procedure. In short, we model migration experience as a function of a woman's observable characteristics and placement provider choice (captured by either provider fixed effects or the provider endline grade) and perform a Heckman correction for selection into migration. Treatment indicators are included in the first stage, but not the second. The exclusion restriction is intuitive: the treatments affect the likelihood of migrating (e.g., by shifting a woman's reservation agency quality threshold) but should have no impact on experience conditional on agency choice. This lets us decompose a migrant's experience into four parts: (i) selection on observable characteristics, (ii) agency choice, (iii) selection on unobservables, and (iv) a residual. We can then test whether the average value of these components differs significantly across treatment arms.

Appendix Table D.10 reports the decomposition for $\beta_1 - \beta_2$. The results suggest that agency choice is the most important driver of treatment effects on pre-departure preparation, explaining roughly two thirds of the improvement in pre-departure preparation attributable to the report card. Agency choice also accounts for as much as half of the report card's impact on the job quality index. In contrast, we find little role for selection into migration, either in terms of observables or unobservables.

In sum, both of the above approaches suggest that the report card did not substantively change the average characteristics of women migrating. This is in line with the report card having no impact on destination, occupation, or pay. Rather, the report card helped women have better migration experiences by avoiding low quality agencies.

6.6 Impacts on Downstream Outcomes and Robustness

While we did not list such outcomes in our pre-registration, the interventions may have had broader impacts on women and their households, by, for example, changing income sources and shifting women's attachment to the labor market. We explore some of these possibilities here.

In Appendix Table D.11, we find no significant effects of the report card or comic on household income or expenditure, housing quality, assets, or use of social protection programs. Relative to the control group, households in our most intensive treatment do report slightly less household income while faring better on the housing quality index, but q-values are equal to 1, so we refrain from over-interpreting these estimates. The overall lack of impacts on downstream outcomes is unsurprising given that the report card did not shift migrants' earnings and only reduced the migration rate by 4.8 p.p. We are therefore only powered to detect very large impacts on income.

Appendix Table D.12 reports impacts on occupation and earnings at endline. Relative to the control group, women who received both the comic and the report card are 4.1 p.p. ($p = 0.018$, $q = 0.24$) more likely to be self employed; conditional on access to other intervention materials, the report card is associated with a 5.6 p.p. ($p = 0.001$, $q = 0.022$) increase. The comic, conditional on access to other intervention materials, had a similar effect. These impacts are substantial relative to the control mean of 17 percent. There are negative, but insignificant effects in the 1-2 p.p. range on the likelihood of being unemployed, working for a wage, being a casual worker, and being abroad at the time of the survey. Together, these results suggest women substituted towards self-employment from various alternatives. Self-employment may have been an attractive way to earn income while affording the flexibility to

pursue migration plans. Column 7 reports treatment effects on monthly earnings for the sub-sample of directly interviewed respondents. As with household income, estimates are small and insignificant.

While Appendix C discusses robustness in detail, we briefly summarize two key checks here. First, we use administrative placements data from 2015-2019 and our supplementary endline sample to confirm a lack of village-wide effects on the (sanctioned) migration market. This is consistent with the fact that our intervention ultimately had limited reach within large study villages. Second, we address concerns related to low direct interview rates among migrants (roughly 60 percent, on average) by estimating impacts on migration experience and provider choice among sub-samples of women who migrated earlier in the post-intervention period and generally had higher direct interview rates (approximately 80 percent). Overall, results are less precisely estimated, but strikingly similar.

6.7 Estimating the Value of Information

Evidence so far suggests that the report card effectively made women choosier and, in so doing, slowed down the migration rate as women avoided offers from low-quality agencies. This slowdown is striking given the large financial returns to migration. In the control group at endline, only 53 percent of non-migrant women worked for pay in the past year. Among those who did work for pay, the median woman had an average monthly income of IDR 1,009,000 (USD 78 at the 2019 exchange rate of IDR 13,900 per USD). By way of comparison, the median migrant in the control group reported average monthly earnings (wages plus other income less deductions and other migration costs) of IDR 5,262,540 (USD 379). These numbers suggest a raw return to migration of USD 301 per month.[30]

Given that the report card did not change occupation or earnings abroad, women who delayed migrating put off the opportunity to earn significantly higher wages. This implicit willingness to pay for continued search motivates a revealed preference estimate of the value of information. We use migration rates in the comic only (C) and comic plus report card (CR) arms to estimate the monthly hazard of receiving and accepting an offer, $\xi_C = 0.0096$, $\xi_{CR} = 0.0113$. We take the median monthly earnings in Indonesia (h) and abroad (w) referenced above, and assume women discount cash flows at monthly rate r in line with real deposit interest rates.[31] Anchoring earnings at the median is reasonable given the limited evidence of selection effects in Section 6.5.

For simplicity, we assume women are infinitely lived and only migrate once for 24 months, the sample median. Then at any given time, expected discounted earnings for a woman who has not yet migrated as a function of the monthly migration rate ξ is given by:

$$V(\xi) = h + \xi \left[\sum_{t=1}^{24} \left(\frac{1}{1+r} \right)^t w + \left(\frac{1}{1+r} \right) \sum_{t=25}^{\infty} h \right] + (1-\xi) \left(\frac{1}{1+r} \right) V(\xi) \tag{5}$$

Solving for $V(\xi_C)$ and $V(\xi_{CR})$ and taking the difference implies that the report card induced a discounted earnings loss of USD 197 to prolong search. This amounts to roughly half a month's earnings abroad.

[30] This is in line with the (noisy) estimate of the USD 292 return to migration, obtained by dividing the effect of the report card on individual monthly earnings (an insignificant USD 5 reduction) by the effect on the likelihood a woman is abroad at endline (an insignificant 1.7 p.p. reduction, see Appendix Table D.12).

[31] Specifically, we use World Bank/IMF data on the deposit interest rate and inflation for 2019 (6.688 percent and 3.031 percent, respectively) to calculate a monthly real rate of $r = 0.30$ percent assuming monthly compounding.

7 Conclusion

We use a randomized controlled trial, spanning 400 villages in Indonesia, to study how information frictions in global labor markets shape the choices and experiences of prospective female migrant workers. Conditional on access to other intervention materials, novel information on intermediary agency quality—in the form of an easy-to-read agency "report card"—significantly reduced the migration rate while leaving beliefs about the return to migration and intentions to migrate unchanged. Women who migrated with this information worked with better-quality placement providers, received better pre -departure preparation, and enjoyed jobs with better non-pecuniary benefits. Revealed preference calibrations suggest that information has substantial value, as the foregone income associated with delaying migration is equal to roughly half a month's wages abroad.

Overall, our results are consistent with a model of search in which the report card significantly increased migrants' ability to *differentiate high- and low-quality agencies*. Intuitively, when information about agency quality is limited, all migration offers look similar, meaning an eager prospective migrant will take the first offer available. When there is more variation in observable offer quality, women may turn down low-quality offers in the hopes of securing a more attractive offer in the future. In line with this theory, we see that the report card reduced the share of women migrating with service providers in the bottom tercile, while increasing use of top-tercile providers. Even so, the *individuals* who defer migrating are not differentially selected, as we show using multiple approaches.

We also rule out the possibility that the report card shifted beliefs about the return to migration, dissuading marginal migrations that would have yielded low returns. The lack of treatment effects on beliefs—overall, when splitting by baseline beliefs, and when considering both location and scale—suggests this mechanism was not relevant for our sample. Another possibility is that the report card shifted outcomes through market-wide effects, for example by providing incentives for placement agencies to invest in quality or by driving low-quality agencies out of the market. We find little indication of such village-level treatment effects, either in administrative data on the rate of migration with sanctioned agencies or in a supplementary sample of women who were rarely directly exposed to our intervention.

Taken as a whole, our results suggest that prospective international migrants in Indonesia face substantial information frictions when selecting placement services. We provide fresh evidence that remedying information frictions on the job-seeker side can induce a slower, more deliberative search process that ultimately connects workers to higher-quality service providers. This can have important welfare consequences, especially in settings like ours, where job seekers are often vulnerable to mistreatment by employers. Our results also highlight that policymakers interested in facilitating safe, high-return migration must address both information frictions *and* the rate at which women get access to offers. The spread of mobile internet and online platforms for job search could facilitate this process.

Finally, while our experiment was not well-positioned to generate or identify general equilibrium effects, our findings do suggest that policies designed to alleviate information frictions at the market level could amplify benefits by reducing demand for placement services offered by low-quality firms. Understanding how firms respond to efforts to improve transparency and promote quality disclosure is an important area for future research.

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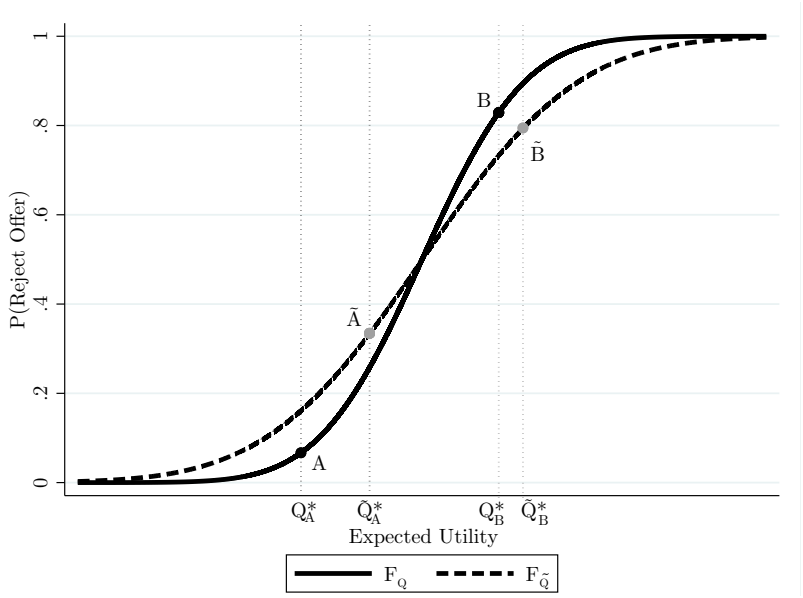
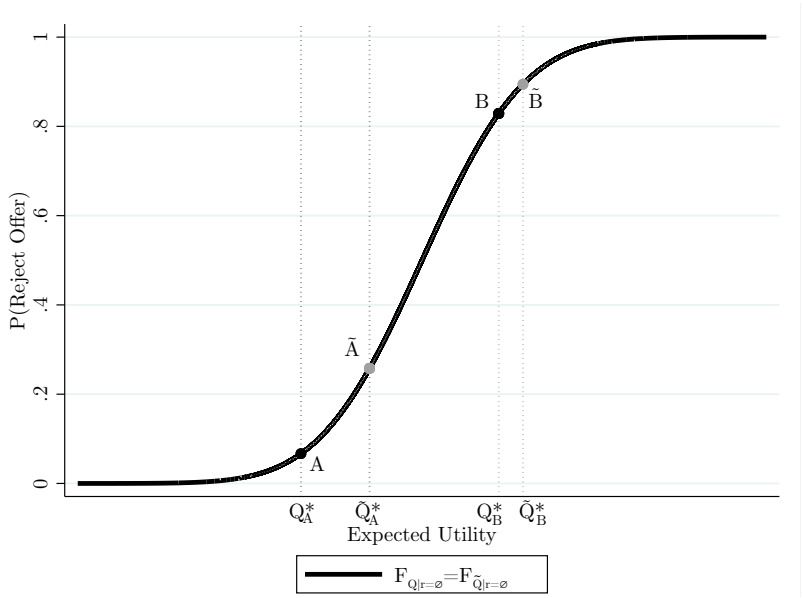
Tables and Figures

Figure 1: Theoretical Example: Impact of the Report Card on Migration Rates

(a) Impact on Overall Probability of Migration

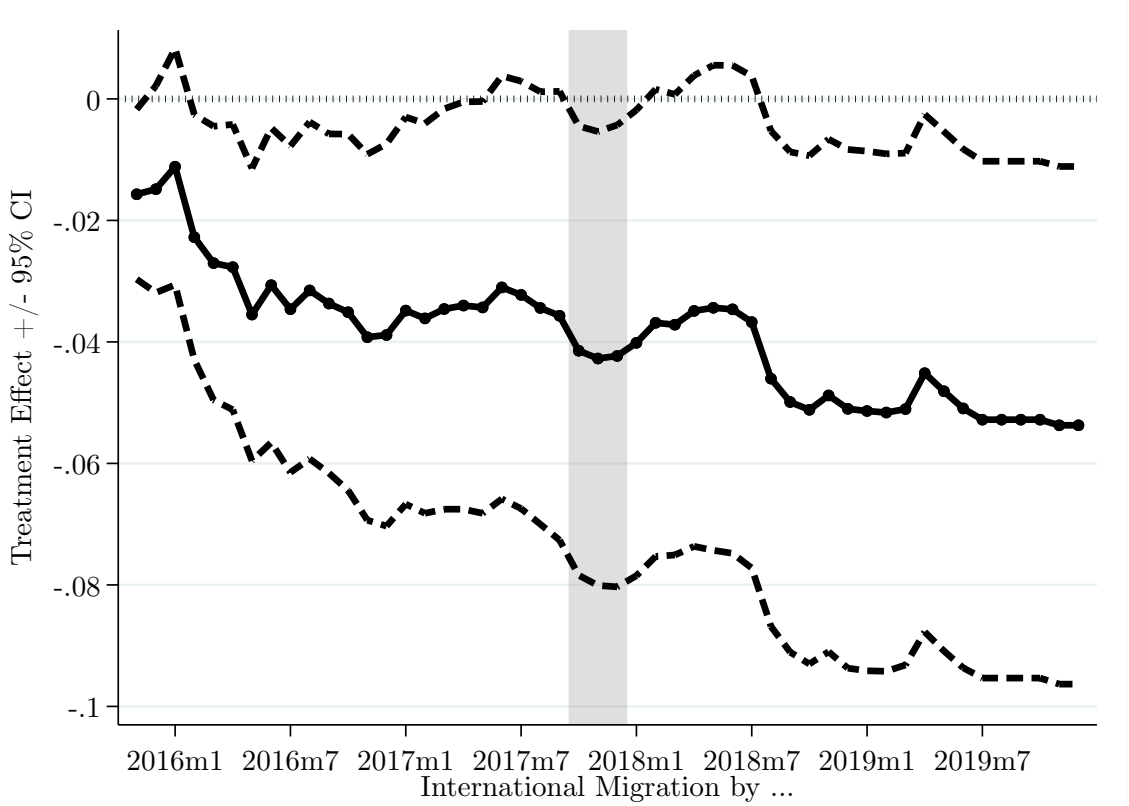


(b) Impact on Use of Ungraded Agencies



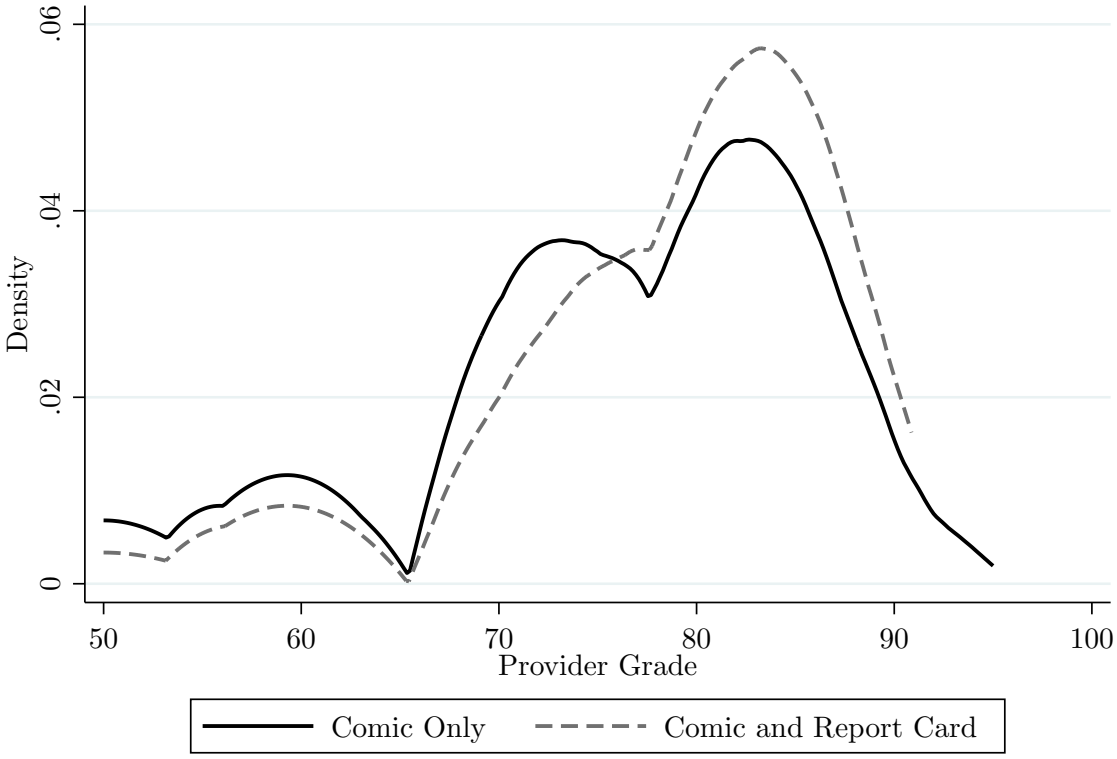
Notes: This figure graphs the CDF of the offer distribution, in terms of expected utility from the migrant's perspective. Panel (a) considers the likelihood of any migration, and panel (b) the likelihood of migrating with ungraded agencies. F_Q is the distribution without quality disclosure, $F_{\tilde{Q}}$ is the distribution after quality disclosure. The graph also shows how quality disclosure affects reservation utility—Q^*, $\tilde{Q}^*$—and the migration rate for two hypothetical migrants, A and B. Absent quality disclosure migrant A accepts most offers. Quality disclosure increases her reservation expected utility and decreases the likelihood she migrates in any given period. Migrant B, on the other hand, rejects most offers prior to quality disclosure. Disclosure also increases her reservation utility, but the likelihood she migrates increases, because disclosure increases her ability to identify sufficiently attractive graded offers. Panel (b) shows that both types of migrants are less likely to use ungraded agencies, since they do not update their forecasts for such agencies, meaning $F_{Q|r_t=\emptyset} = F_{\tilde{Q}|r_t=\emptyset}$.

Figure 2: Impact of the Report Card on the Migration Rate Over Time



Notes: This figure reports time-varying estimates of $\beta_1 - \beta_2$ based on equation (4), augmented so that all treatment variables are interacted with a full set of month fixed effects. The regression also includes month fixed effects and strata dummies. Sample includes all women in the tracking sample with a direct or informant interview at follow up three. The grey shaded bar denotes the second follow up survey, which included a re-distribution of intervention materials. Dashed lines indicate 95 percent confidence intervals based on robust standard errors clustered at the village level.

Figure 3: Distributional Impacts of the Report Card on Migration Provider Quality



Kolmogorov-Smirnov p-value for test of equality of distributions is 0.009.

Notes: This figure reports the distribution of updated provider grades among migrants in the tracking sample at endline. The grades are calculated at endline using experiences of women in the separate, supplementary endline sample who had negligible exposure to the intervention. We construct empirical Bayes estimates of average overall experience, defined as a simple average of the pre-departure preparation, job quality, and pay indices. When estimating these grades, women who migrated with an agency that was not on the Indonesian government's list of approved placement agencies are treated as a single group, as are women who reported using an agency but do not report a name for the agency, women who migrate with a sponsor and no agency, and women who migrate with no sponsor and no agency. See Section 6.4 for further details.

Table 1: Average Migration Outcomes by Provider Type

	(1) Sanctioned Agency Mean	(2) Unsanctioned Difference	(3) N
Panel A: Characteristics of Women and Jobs			
Age	32.737	3.098***	2053
	[6.895]	(0.341)	
Married	0.765	0.021	2053
	[0.424]	(0.020)	
Primary or Less	0.378	0.259***	2053
	[0.485]	(0.025)	
Worked as Housemaid	0.763	0.117***	2050
	[0.426]	(0.018)	
Worked as Nanny	0.282	0.091***	2050
	[0.450]	(0.023)	
Worked as Elder Caretaker	0.456	-0.272***	2050
	[0.498]	(0.021)	
Worked in MENA Country	0.214	0.446***	2050
	[0.410]	(0.024)	
Panel B: Experiences with Agency^{+}			
Fraction Priority Topics Trained On	0.608	-0.332***	1897
	[0.410]	(0.021)	
Signed and Understood Contract in Indonesian	0.617	-0.176***	1924
	[0.486]	(0.024)	
Allowed to Leave Agency Premises	0.251	0.007	1823
	[0.434]	(0.022)	
Subjective Agency Rating (0-10)	7.616	-0.103	1895
	[1.847]	(0.112)	
Pre-Departure Preparation Index	0.155	-0.331***	1924
	[0.383]	(0.021)	
Panel C: Experiences with Employer			
No Weekly Day Off	0.696	0.104***	2046
	[0.460]	(0.020)	
Received Proof of Payment	0.755	-0.239***	2045
	[0.430]	(0.023)	
Allowed to Leave Worksite	0.506	-0.113***	2043
	[0.500]	(0.023)	
Employer Retained Identity Documents	0.602	0.105***	2046
	[0.490]	(0.023)	
Experienced Physical or Sexual Abuse	0.078	0.016	2040
	[0.269]	(0.014)	
Experienced Verbal Abuse	0.252	0.040*	2038
	[0.434]	(0.022)	
Subjective Experience Grade (0-10)	7.363	-0.107	2024
	[2.073]	(0.116)	
Job Quality Index	-0.073	-0.151***	2048
	[0.483]	(0.024)	
Panel D: Compensation			
Received Overtime	0.439	-0.273***	1997
	[0.497]	(0.020)	
Experienced Salary Cuts	0.132	0.056***	2032
	[0.338]	(0.019)	
Average Monthly Wage, Millions IDR	6.068	-1.502***	2049
	[2.136]	(0.101)	
Net Earnings, Millions IDR	4.824	-0.912***	1981
	[1.911]	(0.083)	
Pay Index	0.032	-0.264***	2051
	[0.526]	(0.023)	

Notes: $^{+}$ Excludes women migrating with a sponsor and no agency. Sample limited to women in the endline supplementary sample who migrated with either a sponsor or an agency. Robust standard errors clustered at the village level in parentheses, standard deviations in brackets. "Unsanctioned" indicates the woman migrated with an unsanctioned migration service provider (either a sponsor without an agency, or an agency that the woman could either not name or was not on the government's list of officially sanctioned agencies). The first column shows the mean of the outcome variable for the sample migrating with a sanctioned agency. The second column shows the regression coefficient of the outcome variable on an indicator variable for an unsantioned migration. Pay and migration costs are winsorized at the 99th percentile before constructing the net pay variable.

Table 2: Balance Test: Baseline Differences in Demographic Characteristics Among Tracking Sample

	(1) Control Mean	(2) β_1: Report + Comic + Fac.	(3) β_2: Comic + Fac.	(4) β_3: Report + Fac.	(5) P-value: $\beta_1 = \beta_2 = \beta_3 = 0$	(6) N
Age	28.74	-0.0761	-0.385	-0.653*	0.290	4805
Single	0.148	0.0305	0.0232	0.0254	0.366	4805
Married	0.740	-0.00310	-0.0159	0.00707	0.764	4805
Divorced, Separated or Widowed	0.112	-0.0274**	-0.00731	-0.0324***	0.0178**	4805
Did Not Graduate Primary School	0.123	0.0170	0.00420	0.0157	0.635	4804
Primary School Graduate	0.370	-0.0221	-0.0307	-0.0145	0.491	4804
Secondary School Graduate	0.508	0.00510	0.0265	-0.00115	0.649	4804
Fraction Correct: Raven's Test	0.684	-0.0105	-0.0144	-0.0180*	0.401	4795
Fraction Correct: Math Problems	0.427	-0.0105	-0.0166	-0.0291	0.456	4795
High (Above-Median) Locus of Control	0.458	-0.0172	-0.0276	-0.0252	0.510	4804
Plans to Migrate to MENA Country	0.308	-0.00668	-0.0150	-0.0297	0.760	4698
Plans to Migrate to Asian Country	0.684	0.00718	0.0166	0.0317	0.719	4698
Plans to Use a Sponsor	0.663	0.0465*	0.0148	0.0384	0.226	4721
Plans to Go Directly to an Agency	0.249	-0.0387*	-0.0226	-0.0234	0.365	4721
Probability Not Allowed Outside Agency Premises	0.583	-0.0106	-0.00276	0.00146	0.852	4802
Expected Salary (Millions IDR)	5.825	-0.0471	0.0700	0.00939	0.918	4566
Expected Hours of Work Per Day	12.00	0.0208	0.0598	-0.0329	0.984	4530
Probability Paid Less Than Contract	0.330	0.0284*	0.0199	0.00854	0.268	4803
Probability Gets Day Off	0.506	0.00124	-0.00415	0.0298*	0.129	4803
Probability Experience Physical Abuse	0.216	0.0135	0.00541	0.00200	0.768	4803
Predicted Migration Experience	0.00362	-0.00504	0.00112	-0.00265	0.336	4805
Has Migrated Before	0.494	-0.00873	0.0225*	0.00855	0.179	4805

Notes: Estimates are based on regressions that control for strata fixed effects. Standard errors are heteroskedasticity robust, and clustered at the village level. Predicted migration experience is based on a regression forest trained on tracking sample migrants, which aims to predict a simple average of the baseline pre-departure preparation, job quality, and pay indices – see Appendix C for detail. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10

Table 3: First Stage: Impact of Treatment on Self-Reported Exposure to Intervention Material

	Attended Community Meeting on Migration	Recognizes Infographic	Recognizes Report Card	Recognizes Comic
Panel A. Tracking Sample, Midline 2				
β_1: Report Card + Comic + Facilitation	0.12***	0.10***	0.13***	0.19***
	(0.018)	(0.015)	(0.018)	(0.019)
	[0.001]	[0.001]	[0.001]	[0.001]
β_2: Comic + Facilitation	0.13***	0.070***	0.026**	0.17***
	(0.018)	(0.013)	(0.011)	(0.018)
	[0.001]	[0.001]	[0.010]	[0.001]
β_3: Report + Facilitation	0.12***	0.077***	0.100***	0.043***
	(0.019)	(0.015)	(0.016)	(0.014)
	[0.001]	[0.001]	[0.001]	[0.002]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.004	0.032*	0.109***	0.020
	(0.020)	(0.016)	(0.018)	(0.024)
	[0.246]	[0.024]	[0.001]	[0.129]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.001	0.025	0.035	0.148***
	(0.022)	(0.018)	(0.022)	(0.021)
	[0.274]	[0.063]	[0.042]	[0.001]
Control Mean	0.062	0.043	0.038	0.041
N	3398	3409	3409	3398
Panel B. Tracking Sample, Endline				
β_1: Report Card + Comic + Facilitation	0.14***	0.23***	0.25***	0.48***
	(0.016)	(0.019)	(0.018)	(0.023)
	[0.001]	[0.001]	[0.001]	[0.001]
β_2: Comic + Facilitation	0.14***	0.21***	0.047***	0.48***
	(0.018)	(0.019)	(0.012)	(0.027)
	[0.001]	[0.001]	[0.001]	[0.001]
β_3: Report + Facilitation	0.11***	0.18***	0.22***	0.097***
	(0.016)	(0.017)	(0.019)	(0.025)
	[0.001]	[0.001]	[0.001]	[0.001]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.001	0.024	0.204***	0.006
	(0.021)	(0.026)	(0.020)	(0.028)
	[0.274]	[0.108]	[0.001]	[0.246]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.026	0.052**	0.028	0.385***
	(0.020)	(0.024)	(0.024)	(0.026)
	[0.073]	[0.014]	[0.079]	[0.001]
Control Mean	0.023	0.035	0.024	0.149
N	3078	3067	3070	3078

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. In person interviews only. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table 4: Impact on Migration and Use of Intermediaries at Final Follow Up

		Among Directly Interviewed:		Among Migrants:	
	(1)	(2)	(3)	(4)	(5)
	Migrated Post Intervention	Migrated Post Intervention	Migrated with Ungraded Provider	Used Agency	Used Sponsor
β_1: Report Card + Comic + Facilitation	-0.018	-0.030	-0.037*	0.038*	0.082***
	(0.020)	(0.021)	(0.019)	(0.019)	(0.026)
	[0.40]	[0.23]	[0.15]	[0.19]	[0.021]
β_2: Comic + Facilitation	0.030	0.029	0.012	0.018	0.049*
	(0.020)	(0.021)	(0.020)	(0.021)	(0.025)
	[0.23]	[0.24]	[0.40]	[0.30]	[0.19]
β_3: Report + Facilitation	0.0053	0.0023	0.0018	0.029	0.047*
	(0.020)	(0.020)	(0.019)	(0.020)	(0.027)
	[0.58]	[0.59]	[0.59]	[0.22]	[0.21]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.048**	-0.059**	-0.049**	0.020	0.033
	(0.021)	(0.023)	(0.021)	(0.019)	(0.023)
	[0.124]	[0.124]	[0.124]	[0.288]	[0.221]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.024	-0.032	-0.039*	0.009	0.034
	(0.021)	(0.022)	(0.020)	(0.018)	(0.024)
	[0.359]	[0.233]	[0.154]	[0.300]	[0.221]
Control Mean	0.370	0.271	0.227	0.889	0.788
N	4664	3762	3757	1726	1724

Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. Outcomes related to the migration rate (columns 1-3) and outcomes related to use of intermediaries (columns 4-5) treated as two separate FDR families. All regressions control for strata fixed effects. The outcome in the first column is collected from directly interviewed migrants and, when women were not available, informants. The second and third column limit the sample to directly interviewed women (migrants and non-migrants), since informants could not reliable report agency names. The final two columns limit the sample to migrants, and use both direct interviews and informant reports to assess use of agencies and sponsors. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table 5: Impact on Migration Plans and Preparation: Non-Migrants at Final Follow Up

	(1) Plans to Migrate Next Year	(2) Has Family Permission	(3) Has Village Permission	(4) Chosen Sponsor	(5) Contacted Agency	(6) Chosen Agency
β_1: Report Card + Comic + Facilitation	0.010	0.0021	0.0079	0.021*	0.0061	0.0010
	(0.014)	(0.011)	(0.0088)	(0.012)	(0.0055)	(0.0097)
	[1]	[1]	[1]	[1]	[1]	[1]
β_2: Comic + Facilitation	0.0049	0.0012	-0.0028	0.013	-0.0051	0.0045
	(0.015)	(0.013)	(0.0085)	(0.012)	(0.0042)	(0.011)
	[1]	[1]	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	0.011	-0.00085	-0.0018	0.013	-0.0019	-0.0011
	(0.014)	(0.011)	(0.0085)	(0.012)	(0.0047)	(0.0093)
	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.005	0.001	0.011	0.008	0.011**	-0.003
	(0.015)	(0.012)	(0.008)	(0.013)	(0.005)	(0.011)
	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.001	0.003	0.010	0.008	0.008	0.002
	(0.014)	(0.011)	(0.008)	(0.013)	(0.005)	(0.010)
	[1]	[1]	[1]	[1]	[1]	[1]
Control Mean	0.072	0.043	0.024	0.040	0.009	0.031
N	2708	2689	2689	2689	2689	2689

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table 6: Impact on Beliefs Among Non-Migrants

	(1) Agency Index	(2) Job Quality Index	(3) Infographic Index
β_1: Report Card + Comic + Facilitation	0.010	-0.038	-0.035
	(0.028)	(0.028)	(0.026)
	[1]	[1]	[1]
β_2: Comic + Facilitation	-0.016	-0.019	-0.021
	(0.031)	(0.028)	(0.026)
	[1]	[1]	[1]
β_3: Report + Facilitation	-0.013	-0.043	-0.047*
	(0.028)	(0.028)	(0.026)
	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.026	-0.019	-0.013
	(0.032)	(0.028)	(0.026)
	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.023	0.005	0.013
	(0.029)	(0.029)	(0.027)
	[1]	[1]	[1]
Control Mean	0.000	0.000	0.000
N	2597	2597	2597

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to non-migrants directly interviewed at final follow-up. The agency index includes questions about the following: will the agency take identity documents, will the agency give information on migrant worker rights, will the agency provide clean food and water, will the agency staff treat migrant workers with respect, will the agency staff let the migrant workers leave the dorms/training center, will the agency follow legal procedures, will the agency give accurate information. The job quality index includes information about the following: will the migrant have to work 12+ hours, will the job match the contract, will the migrant get a day off, will the migrant's salary be retained, will the migrant be paid less than their contract, will the employer hold the migrant's identity documents, will the employer provide presents, will the migrant have to return early, will the migrant experience physical abuse. The infographic index includes the first two components of the agency index and the first 6 components of the job quality index. Index components are coded so that a higher value indicates a better outcome. The baseline analogues of the indices exclude beliefs about the agency/employer taking identity documents, the job matching the contract, and salary retention because these questions were not included in the baseline survey. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table 7: Impact on Pre-Departure Preparation and Experience Abroad

	Standardized Experience Indices		
	(1) Pre-Departure Preparation	(2) Migration Experience: Job Quality	(3) Migration Experience: Pay
β_1: Report Card + Comic + Facilitation	0.084	0.065	-0.015
	(0.059)	(0.041)	(0.049)
	[0.33]	[0.33]	[0.68]
β_2: Comic + Facilitation	-0.088	-0.038	-0.013
	(0.060)	(0.040)	(0.044)
	[0.33]	[0.50]	[0.68]
β_3: Report + Facilitation	-0.023	-0.049	-0.029
	(0.059)	(0.039)	(0.046)
	[0.68]	[0.42]	[0.68]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.172***	0.103**	-0.002
	(0.059)	(0.043)	(0.045)
	[0.061]	[0.081]	[0.815]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.107*	0.114***	0.014
	(0.058)	(0.043)	(0.047)
	[0.250]	[0.061]	[0.682]
Control Mean	0.000	-0.000	0.000
N	1016	991	996

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to directly interviewed migrants in the tracking sample. The *pre-departure preparation index* includes: use of an agency, whether the agency provided training, time spent on training, the share of government-mandated training topics covered by the agency, the migrant's subjective grade (0-10) of the agency training, whether the migrant signed a contract (in Indonesian, that she understood) while at the agency, whether the agency allowed the migrant to leave the training facility and residence, whether the agency held the migrant's identity documents, whether the agency followed legal procedures (per the migrant's assessment), and the migrant's subjective overall rating of the agency on a 0-10 scale. All outcomes are coded to zero if the woman did not use an agency to migrate. The *job quality index* includes: whether the migrant was given a weekly day off, the job matched the contract, the employer allowed the migrant to retain her identity documents, the migrant had her own private living quarters, the migrant received proof of payment, the migrant was allowed to leave the employer's residence, and the migrant's overall subjective rating of the migration experience. The *pay index* includes: total wages net of salary deductions, total earnings (wages plus other income from the agency, sponsor, and employer) net of costs (salary deductions plus other migration costs paid to the agency, sponsor, employer, or other entities), whether the migrant received the full contracted salary, whether the migrant received salary payments on time, and whether the migrant received additional pay for overtime work. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table 8: Impact on Market Share of Placement Providers

	(1) Low Quality Provider	(2) Average Quality Provider	(3) High Quality Provider	(4) Provider Grade
β_1: Report Card + Comic + Facilitation	-0.13***	0.055	0.079	2.02**
	(0.044)	(0.046)	(0.049)	(0.94)
	[0.027]	[0.30]	[0.22]	[0.12]
β_2: Comic + Facilitation	-0.0012	0.037	-0.035	-0.35
	(0.046)	(0.044)	(0.049)	(0.98)
	[0.64]	[0.48]	[0.49]	[0.56]
β_3: Report + Facilitation	-0.077*	0.080*	-0.0029	0.75
	(0.046)	(0.044)	(0.047)	(0.92)
	[0.21]	[0.21]	[0.64]	[0.48]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.133***	0.019	0.114**	2.371**
	(0.044)	(0.046)	(0.050)	(0.958)
	[0.027]	[0.562]	[0.102]	[0.090]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.057	-0.025	0.082*	1.266
	(0.043)	(0.046)	(0.048)	(0.910)
	[0.304]	[0.509]	[0.212]	[0.304]
Control Mean	0.371	0.294	0.335	76.235
N	968	968	968	968

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. We calculate updated grades by using experience reports in the extra experience sample. We construct empirical Bayes estimates of average overall experience, defined as a simple average of the pre-departure preparation, job quality, and pay indices. We construct dummy variables to identify high (top third of grade distribution), average (middle third), and low-quality (bottom third) providers. Women who migrated with an agency that was not on the Indonesian government's list of approved placement agencies are treated as a single group, as are: (i) women who reported using an agency but do not report a name for the agency, (ii) women who migrate with a sponsor and no agency, and (iii) women who migrate with no sponsor and no agency. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table 9: Impact on Types of Migration Providers Used

	(1) Used Agency on Report Card	(2) Report Card Grade (if Used Graded Agency)	(3) Used Sanctioned Agency Not on Report Card	(4) No Agency or Unsanctioned Agency
β_1: Report Card + Comic + Facilitation	0.048	-1.43	0.070	-0.12***
	(0.036)	(2.60)	(0.046)	(0.044)
	[0.61]	[1]	[0.61]	[0.073]
β_2: Comic + Facilitation	0.047	-0.61	-0.043	-0.0040
	(0.033)	(2.54)	(0.045)	(0.043)
	[0.61]	[1]	[0.80]	[1]
β_3: Report + Facilitation	0.0083	-1.00	0.063	-0.071
	(0.031)	(2.64)	(0.044)	(0.044)
	[1]	[1]	[0.61]	[0.61]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.002	-0.825	0.113**	-0.115***
	(0.037)	(2.206)	(0.046)	(0.041)
	[1]	[1]	[0.102]	[0.073]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.040	-0.429	0.008	-0.048
	(0.036)	(2.337)	(0.045)	(0.042)
	[0.799]	[1]	[1]	[0.799]
Control Mean	0.158	78.738	0.462	0.379
N	1015	185	1015	1015

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. We calculate updated grades by using experience reports in the extra experience sample. We construct empirical Bayes estimates of average overall experience, defined as a simple average of the pre-departure preparation, job quality, and pay indices. We construct dummy variables to identify high (top third of grade distribution), average (middle third), and low-quality (bottom third) providers. Women who migrated with an agency that was not on the Indonesian government's list of approved placement agencies are treated as a single group, as are: (i) women who reported using an agency but do not report a name for the agency, (ii) women who migrate with a sponsor and no agency, and (iii) women who migrate with no sponsor and no agency. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table 10: Testing for Differential Selection into Migration and Direct Interview Conditional on Migration

	Migration		Direct Interview | Migrated	
	(1)	(2)	(3)	(4)
	Migrated	Migrated	Direct Interview	Direct Interview
Report Card + Comic + Facilitation	-0.015	0.51*	-0.046	-0.32
	(0.020)	(0.29)	(0.037)	(0.60)
Comic + Facilitation	0.030	0.10	0.0059	-0.25
	(0.020)	(0.30)	(0.036)	(0.58)
Report + Facilitation	0.0067	0.24	-0.0054	-0.16
	(0.020)	(0.28)	(0.035)	(0.58)
Predicted Migration Experience	0.69***	0.84**	-0.14	0.70
	(0.25)	(0.42)	(0.39)	(0.70)
Predicted Experience × Report Card + Comic + Fac.	-0.38	-0.61	0.093	-0.71
	(0.32)	(0.59)	(0.55)	(1.02)
Predicted Experience × Comic + Fac.	-0.27	-0.57	-0.30	-0.37
	(0.34)	(0.68)	(0.54)	(1.02)
Predicted Experience × Report Card + Fac.	0.013	-0.66	-0.21	-1.48
	(0.35)	(0.59)	(0.55)	(0.98)
P-value: All interactions equal to zero	0.724	0.954	0.478	0.752
Control Mean	0.370	0.370	0.596	0.596
N	4664	4664	1745	1745
Additional Controls + Interactions?	No	Yes	No	Yes

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. Additional controls include demographic characteristics (age, education, marital status, past migration experience), fraction correct on a series of Ravens' matrices and math questions, locus of control, big 5 personality characteristics, an unincentivized measure of time preference, an incentivized measure of risk, baseline plans to not rely on a sponsor for agency choice, and measures of beliefs about the return to migration. Missing values are dummied out and recoded to zero. All controls are controlled for directly and interacted with each of the three treatment indicators. F-tests of joint significance are Wild cluster bootstrapped with 9,999 replications. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

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A Theory Appendix

A.1 Detailed Derivations: Impact of Increased Spread

To better see how a mean preserving increase in risk impacts Q^*, assume the distribution of Q_t has risk parameter σ, where higher levels of σ indicate greater levels of risk in line with Theorem 2 in Rothschild and Stiglitz (1970); thus we write $F_Q(Q;\sigma)$. More formally, we first assume $F_Q(\cdot;\sigma)$ satisfies one of the standard Rothschild and Stiglitz (1970) definitions of increasing risk:

$$\int_{\underline{U}}^{x} \left[F_Q(Q;\sigma+\delta) - F_Q(Q;\sigma)\right] dQ \geq 0 \ \forall x \in [\underline{U},\overline{U}), \text{ and } \int_{\underline{U}}^{\overline{U}} \left[F_Q(Q;\sigma+\delta) - F_Q(Q;\sigma)\right] dQ = 0$$

for all $\delta > 0$. Next assume $F_Q(Q;\sigma)$ is continuously differentiable in Q and σ and that $\frac{\partial F_Q(Q;\sigma)}{\partial \sigma}$ is bounded. Then we can apply the bounded convergence theorem to conclude:

$$\int_{\underline{U}}^{x} \frac{\partial F_Q(Q;\sigma)}{\partial \sigma} dQ \ = \int_{\underline{U}}^{x} F_{Q\sigma}(Q;\sigma) dQ \geq 0 \ \forall x \in [\underline{U},\overline{U}) \text{ and } \int_{\underline{U}}^{\overline{U}} \left[F_{Q\sigma}(Q;\sigma)\right] dQ = 0. \tag{A.1}$$

Beginning with equation (1) in the main text, we set $V = Q^*$ and after some algebra obtain

$$Q^*(1-\beta) = h + \beta\lambda \left[\int_{Q^*}^{\overline{U}} (Q - Q^*) dF_Q(Q)\right] \tag{A.2}$$

Now we use equation (A.2) to implictly differentiate to solve $\partial Q^*/\partial\sigma$:

$$\begin{aligned}(1-\beta)\partial Q^* &= \beta\lambda \left[-(Q-Q^*)|_{Q^*}\partial Q^* - \int_{Q^*}^{\overline{U}} dF_Q(Q;\sigma)\partial Q^* + \int_{Q^*}^{\overline{U}} (Q-Q^*) dF_{Q\sigma}(Q;\sigma)\partial\sigma\right] \\ &= \beta\lambda \left[-(1-F_Q(Q^*;\sigma))\partial Q^* + \int_{Q^*}^{\overline{U}} (Q-Q^*) dF_{Q\sigma}(Q;\sigma)\partial\sigma\right],\end{aligned}$$

and rearranging gives

$$(1-\beta+\beta\lambda(1-F_Q(Q^*;\sigma)))\partial Q^* = \beta\lambda \left[\int_{Q^*}^{\overline{U}} (Q-Q^*) dF_{Q\sigma}(Q;\sigma)\partial\sigma\right].$$

Next, we integrate the right hand side of equation (A.3) by parts:

$$\int_{Q^*}^{\overline{U}} (Q-Q^*) dF_{Q\sigma}(Q;\sigma)\partial\sigma = (Q-Q^*)F_{Q\sigma}(Q;\sigma)\ |_{Q^*}^{\overline{U}} - \int_{Q^*}^{\overline{U}} F_{Q\sigma}(Q;\sigma) d(Q-Q^*) \tag{A.3}$$

Since $F_Q(\overline{U};\sigma) = 1 \ \forall\sigma$, the first term is zero; further note that $d(Q-Q^*) = dQ$. Thus,

$$(1-\beta+\beta\lambda(1-F_Q(Q^*;\sigma)))\partial Q^* = \beta\lambda \left[-\int_{Q^*}^{\overline{U}} F_{Q\sigma}(Q;\sigma) dQ\right]\partial\sigma,$$

which then gives us

$$\frac{\partial Q^*}{\partial\sigma} = \frac{-\int_{Q^*}^{\overline{U}} F_{Q\sigma}(Q;\sigma) dQ}{(1-\beta+\beta\lambda(1-F_Q(Q^*;\sigma)))} \geq 0 \tag{A.4}$$

because equation (A.1) implies the numerator is positive.

The next question is what happens to the migration rate, $\lambda\left[1-F_Q(Q^*;\sigma)\right]$, as σ increases. Here,

we simply differentiate

$$\frac{\partial}{\partial\sigma}\lambda\left[1-F_Q(Q^*;\sigma)\right]=-\lambda\left[dF_Q(Q^*;\sigma)\frac{\partial Q^*}{\partial\sigma}+F_{Q\sigma}(Q^*;\sigma)\right]. \tag{A.5}$$

The first term on the right hand side is always positive, while $F_{Q\sigma}(Q^*;\sigma)$ is of indeterminate sign. The derivative will be unambiguously negative whenever $F_{Q\sigma}(Q^*;\sigma)\geq 0$. It follows from this that the likelihood of rejecting an offer will increase, as will search time. To show that welfare conditional on migrating, $\mathbb{E}[Q_t \mid Q_t \geq Q^*]$, increases, we need to do additional work. First, note that:

$$\begin{aligned}\mathbb{E}[Q_t \mid Q_t \geq Q^*]=&\frac{\int_{Q^*}^{\overline{U}} Q dF_Q(Q;\sigma)}{1-F_Q(Q^*;\sigma)}=\frac{\mu-\int_{\underline{U}}^{Q^*} Q dF_Q(Q;\sigma)}{1-F_Q(Q^*;\sigma)}\\ =&\frac{\mu-Q^*F_Q(Q^*;\sigma)+\int_{\underline{U}}^{Q^*} F_Q(Q;\sigma)dQ}{1-F_Q(Q^*;\sigma)}\end{aligned} \tag{A.6}$$

where we obtain the last equality by integration by parts. After this, we differentiate:

$$\begin{aligned}\frac{\partial\mathbb{E}[Q_t \mid Q_t \geq Q^*]}{\partial\sigma}=&\frac{\left[1-F_Q(Q^*;\sigma)\right]\int_{\underline{U}}^{Q^*} F_{Q\sigma}(Q;\sigma)dQ}{\left[1-F_Q(Q^*;\sigma)\right]^2}\\ &+\frac{\left[\mu-Q^*+\int_{\underline{U}}^{Q^*} F_Q(Q;\sigma)dQ\right]\left[dF_Q(Q^*)\frac{\partial Q^*}{\partial\sigma}+F_{Q\sigma}(Q^*;\sigma)\right]}{\left[1-F_Q(Q^*;\sigma)\right]^2}\end{aligned} \tag{A.7}$$

Then note that

$$\begin{aligned}\mu-Q^*+\int_{\underline{U}}^{Q^*} F_Q(Q;\sigma)dQ=&\int_{Q^*}^{\overline{U}} Q dF_Q(Q;\sigma)-Q^*(1-F_Q(Q^*;\sigma))\\ =&\int_{Q^*}^{\overline{U}}(Q-Q^*)dF_Q(Q;\sigma)\geq 0\end{aligned} \tag{A.8}$$

Plugging this in we have:

$$\begin{aligned}\frac{\partial\mathbb{E}[Q_t \mid Q_t \geq Q^*]}{\partial\sigma}=&\frac{\left[1-F_Q(Q^*;\sigma)\right]\int_{\underline{U}}^{Q^*} F_{Q\sigma}(Q;\sigma)dQ}{\left[1-F_Q(Q^*;\sigma)\right]^2}+\\ &\frac{\left[\int_{Q^*}^{\overline{U}}(Q-Q^*)dF_Q(Q;\sigma)\right]\left[dF_Q(Q^*;\sigma)\frac{\partial Q^*}{\partial\sigma}+F_{Q\sigma}(Q^*;\sigma)\right]}{\left[1-F_Q(Q^*;\sigma)\right]^2}\end{aligned} \tag{A.9}$$

The first term in the sum is positive because $\int_{\underline{U}}^{Q^*} F_{Q\sigma}(Q;\sigma)dQ\geq 0$ by equation (A.1). Thus, whenever $F_{Q\sigma}(Q^*;\sigma)\geq 0$ the entire derivative is unambiguously (weakly) positive. Even if this condition does not hold, inspecting equation (A.9) shows that the sum will also be positive whenever $\left[dF_Q(Q^*;\sigma)\frac{\partial Q^*}{\partial\sigma}+F_{Q\sigma}(Q^*;\sigma)\right]>0$. Recall the change in the migration rate is $\partial/\partial\sigma\lambda(1-F_Q(Q^*;\sigma))=-\lambda\left[dF_Q(Q^*;\sigma)\frac{\partial Q^*}{\partial\sigma}+F_{Q\sigma}(Q^*;\sigma)\right]$. Thus, whenever this term is negative, we have $\frac{\partial\mathbb{E}[Q_t|Q_t\geq Q^*]}{\partial\sigma}>0$.

A.2 Detailed Derivations: Impact of Shifting Beliefs

To assess the impact of change in the location of beliefs, we rewrite equation (A.2) to capture a translation of Q_t by $\eta\Delta$ (here, we use the fact that $dF_Q(x) = dG_Q(x + \eta\Delta)$):

$$Q^*(1-\beta) = h + \beta\lambda \left[\int_{Q^*-\eta\Delta}^{\overline{U}} (Q + \eta\Delta - Q^*) dF_Q(Q) \right] \tag{A.10}$$

Differentiating implicitly we see that

$$\frac{\partial Q^*}{\partial \Delta} = \frac{\beta\lambda\eta(1 - F_Q(Q^* - \eta\Delta)}{1 - \beta + \beta\lambda(1 - F_Q(Q^* - \eta\Delta))} \geq 0 \tag{A.11}$$

To evaluate the effect on the migration rate, we start by noting that $G_Q(Q^*) = F_Q(Q^* - \eta\Delta)$. Then we differentiate:

$$\frac{\partial}{\partial \Delta}\lambda\left[1 - F_Q(Q^* - \eta\Delta)\right] = \lambda\left[-dF_Q(Q^* - \eta\Delta)\right]\left[\frac{\partial Q^*}{\partial \Delta} - \eta\right] \geq 0 \tag{A.12}$$

because $^{\partial Q^*}/_{\partial \Delta} < \eta$.

B Data Appendix

A supplementary document with extensive detail on our data, how we constructed outcomes of interest, and how we constructed agency grades is available at https://drive.google.com/file/d/12gCihwwMKiim9QTBpHcn1zu5vS6asLG9/view?usp=sharing.

C Robustness

In this appendix we discuss a few key points of robustness briefly summarized in the main text.

C.1 Confirming a Lack of Village-Wide Effects

Recall from Table 3 that most women in the supplementary sample did not report exposure to our interventions. Given our limited reach within rather large study villages (with populations of 2,000 migration-age women on average), we do not expect to find village-wide impacts of the information treatments. We confirm this in Appendix Table D.13, which uses administrative placements data from October 2015 to December 2019 to estimate treatment effects on total formal migration and use of agencies by quality. We omit treatment effects for bottom-tercile providers, because those in the bottom tercile of the endline grade distribution are almost entirely either non-agency migrations or migrations with unsanctioned agencies, neither of which can be detected by the administrative data. Overall, we find no substantive impacts on village-level migration flows or use of agencies in different quality bins.

Another way to test for village-wide effects is to examine impacts within the supplementary sample, which was notably less exposed to treatment compared to the tracking sample. If the intervention had village-wide effects, we expect women in the supplementary sample to benefit. However, if low-quality offers turned down by tracking sample women were taken up by women in the supplementary sample, women in the supplementary sample could be negatively impacted. Appendix Tables D.15 and D.14 look at treatment effects on agency choice and migration experience in the supplementary sample. Overall, impacts are minimal, though the report card is associated with a marginally significant improvement in pre-departure preparation, alongside a 7.2 percentage point reduction in use of bottom-tercile providers. Both of these effects have large q-values however, so we prefer not to read too much into them. Taken

as a whole, this suggests the spillover effects were muted, relative to the direct effects of information provision.

C.2 Accounting for Non-Response

While we find no evidence of differential selection into direct interview by treatment, our direct interview rate among migrants is still somewhat low, at 60 percent. As an additional check to understand whether and how low direct interview rates might influence our results, we exploit the fact that women who migrated shortly after treatment are more likely to be directly interviewed. More specifically, we observe a significant effect of the report card on the migration rate 6 months after intervention (recall Figure 2) and we successfully interviewed 80 percent of these early migrants at endline. Tables D.16 and D.17 reproduce our analyses of impacts on migration experiences and placement provider market share limiting the sample to woman who migrated within 6 months of the interventions and 12 months of the interventions (where we attain a 67 percent direct interview rate). Overall, results are remarkably similar, though less precise. This is in line with our other findings suggesting selection is not a major driver of the report card's effects among migrants.

C.3 A Model-Driven Selection Correction Procedure.

In the second approach to examining the role of selection, we use the model in Section 4 to inform a selection-correction procedure. Recall that a woman will migrate if the expected utility associated with a migration offer exceeds her reservation utility. Now, we separate a woman's forecast into three parts: expected returns based on her observable characteristics, $\mathbf{x}_i'\boldsymbol{\zeta}$, expected agency-specific value add, $\gamma_a = \mathbb{E}[u_t \mid q_t] - \mu_i$, and the effect of other individual characteristics unobservable to the econometrician, ε_i, where $\mu_i = \mathbf{x}_i'\boldsymbol{\zeta} + \varepsilon_i$ is the overall expected utility of migration (integrated across the distribution of offers). We also allow for individual variation in outside options, $Q_i^* = Q^* + \eta_i$. Putting this together, a woman migrates with agency a if $Q_{ia} = \mathbf{x}_i'\boldsymbol{\zeta} + \gamma_a + \varepsilon_i \geq Q^* + \eta_i$. Conditional on migrating, realized utility is

$$y_{ia} = \mathbf{x}_i'\boldsymbol{\zeta} + \gamma_a + \iota_a + \varepsilon_i + \nu_{ia} \tag{A.13}$$

where ι_a is the woman's forecast error in terms of agency value add, and $\mathbb{E}[\nu_{ia} \mid \mathbf{x}_i, a, \varepsilon_i] = 0$. To reduce notational clutter, denote total agency value by $\delta_a = \gamma_a + \iota_a$. Then, expected utility among women who migrate is a function of observable characteristics, agency choice, and selection on unobservables:

$$\mathbb{E}[y_{ia} \mid Q_{ia} \geq Q_i^*; \mathbf{x}_i, a] = \mathbf{x}_i'\boldsymbol{\zeta} + \delta_a + \mathbb{E}[\varepsilon_i \mid Q_{ia} \geq Q_i^*; \mathbf{x}_i, a]. \tag{A.14}$$

The above equation refers to the migration process when the report card is not available.

With the report card, women have a more precise signal of agency quality, and their reservation utility increases. This will change selection into migration (as the threshold is higher conditional on expected agency value add) and expected experience conditional on migrating (through the agency effect). Then, the difference in average migrant outcomes is:

$$\Delta\mathbb{E}[y \mid \text{migrate}] = \Delta\mathbb{E}[x]'\boldsymbol{\zeta} + \Delta\mathbb{E}[\delta] + \Delta\mathbb{E}[\varepsilon \mid \text{migrate}] \tag{A.15}$$

where Δ indicates the change in expected outcome between individuals with and without the report card. Thus, we can decompose the treatment effect on migration experience into the components due to changes in observable characteristics of migrants ($\Delta\mathbb{E}[x]'\boldsymbol{\zeta}$), changes in the quality of selected agencies ($\Delta\mathbb{E}[\delta]$), and changes in selection on unobservables due to a shift in reservation utility ($\Delta\mathbb{E}[\varepsilon \mid \text{migrate}]$).

In order to perform this decomposition, we need to derive consistent estimates of $\boldsymbol{\zeta}$, δ_a, and $\mathbb{E}[\varepsilon \mid Q \geq Q^*]$. To do this, we put more structure on the problem and assume the unobservable terms are jointly normally distributed, which lets us implement a Heckman (1976) two-step selection model.

In step one, we estimate the probability of migration as

$$\mathbb{P}(\text{migrate}_i) = \mathbb{P}(\eta_i - \gamma_a - \varepsilon_i \leq \mathbf{z}_i'\boldsymbol{\zeta}) = \Phi(\mathbf{z}_i'\boldsymbol{\zeta}) \tag{A.16}$$

where $\Phi(\cdot)$ is a standard normal CDF, and we have normalized $Q^* = 0$ without loss of generality. In step two, we recover

$$\mathbb{E}[y_{ia} \mid \mathbf{x}_i, a] = \mathbf{x}_i'\boldsymbol{\zeta} + \delta_a + \rho\sigma\lambda(\mathbf{z}_i'\boldsymbol{\zeta}) \tag{A.17}$$

where $\lambda(\cdot)$ is the inverse Mills ratio, $\rho = \text{corr}(\eta_i - \gamma_a - \varepsilon_i, \varepsilon_i + \nu_{ia})$ and $\sigma = \text{sd}(\varepsilon_i + \nu_{ia})$. To avoid having identification solely off functional form, we introduce an exclusion restriction motivated by the search model. Note that the report card should impact reservation utility, Q_i, but have no effect on migration outcomes conditional on agency choice. Thus, we include the treatment indicators in the first, but not the second, stage of the selection model, meaning the vector $\mathbf{z}_i$ includes all variables in $\mathbf{x}_i$ as well as a vector of treatment dummies.[32]

With unbiased estimates of $\boldsymbol{\zeta}$, δ_a, and $\rho\sigma$ we can perform the decomposition by first taking the estimated components from the second step of the correction model (as well as the residual, since its average value can vary by treatment) and then regressing each on the three treatment indicators. The resulting coefficients—and differences between the comic + report card + facilitation and comic + facilitation coefficients—identify the relative contribution of each component mechanism in explaining the treatment effects reported in Table 7.[33]

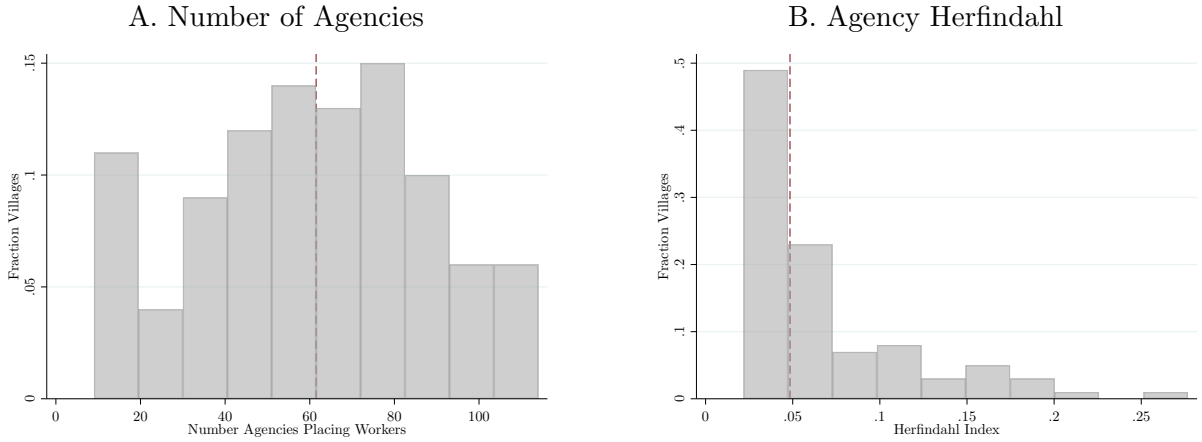
Table D.10 reports decomposition results for the three migration experience indices, using agency fixed effects to estimate $\Delta\mathbb{E}[\delta]$ in columns 1-3 and endline agency grades in columns 4-6. We only present decomposition results for $\beta_1 - \beta_2$ (the effect of the report card conditional on access to other intervention materials), since other treatment effects in Table 7 are not significant. The $\mathbf{x}$ vector comprises a large set of individual characteristics: age, marital status, education, cognitive ability (measured by fraction correct on a series of Raven's Matrices and basic math questions), non-cognitive traits (e.g., locus of control, big 5 personality traits), risk and time preferences, prior search experience, and beliefs about migration experiences. Columns 1 and 4 of Table D.10 suggest that agency choice is the most important driver of treatment effects on pre-departure preparation, explaining roughly two thirds of the improvement in pre-departure preparation attributable to the report card. Agency choice also accounts for roughly half of the report card's impact on the job quality index (column 2, significant at the 10 percent level), although estimates using provider endline grades instead of provider fixed effects are smaller (though significant at the 5 percent level). In contrast, we find little role for selection into migration, either in terms of observables or unobservables. Rather, the remainder tends to load on residual differences, which could indicate that we have not fully accounted for key migrant characteristics, a concern our next approach attempts to address.

[32] The exclusion restriction would be violated if, for example, the report card motivated women to acquire more skills prior to migration, or gave women the ability to negotiate with their agency to secure better training. We consider this unlikely given the nature of agency placement and training, as well as the explicit focus of the report card on agency quality.

[33] We calculate standard errors by generating 500 bootstrap samples clustered by village; for each sample we re-calculate endline provider grades, then run the two-step procedure and perform the decomposition. Standard errors are based on the distribution of estimated treatment coefficients in the final decomposition regressions. Sums of components in this analysis differ slightly from the estimates in Table 7 because we do not partial out the randomization strata in the decomposition exercise.

D Appendix Figures and Tables

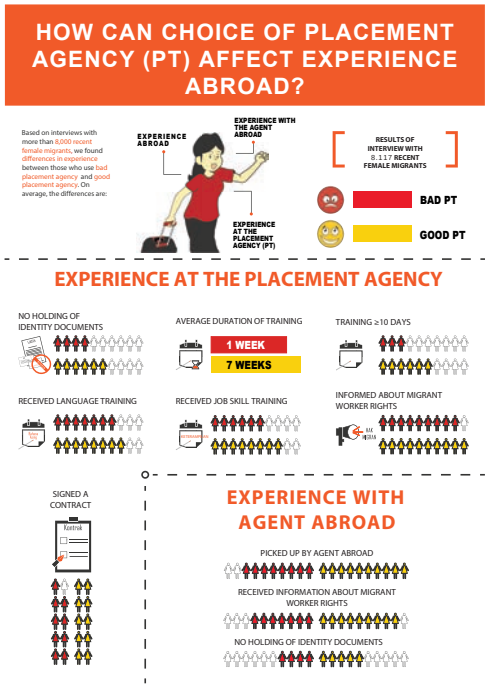
Figure D.1: Agency Concentration within Villages During Intervention Period



Notes: Panel A reports the distribution of the number of agencies placing migrants between 2016 and 2019 in control group villages. Panel B reports the distributions of agency concentration in control group villages, measured by the Herfindahl index, over the same time period. The Herfindahl is the sum of the squared firm market shares (here the share of placed female migrants) within a village. A Herfindahl of 1 corresponds to one agency placing all female migrants; the index goes to zero as competition increases.

Figure D.2: Examples of Intervention Materials with English Translations

A. Infographic



B. Report Card


PLACEMENT AGENCY (PPTKIS) REPORT CARD

(PERUSAHAAN PENGERAH TENAGA KERJA INDONESIA SWASTA)

BASED ON EXPERIENCE OF FORMER MIGRANTS

XXXXXXXXXXXX SUBDISTRICT - DISCTRICT OF CILACAP

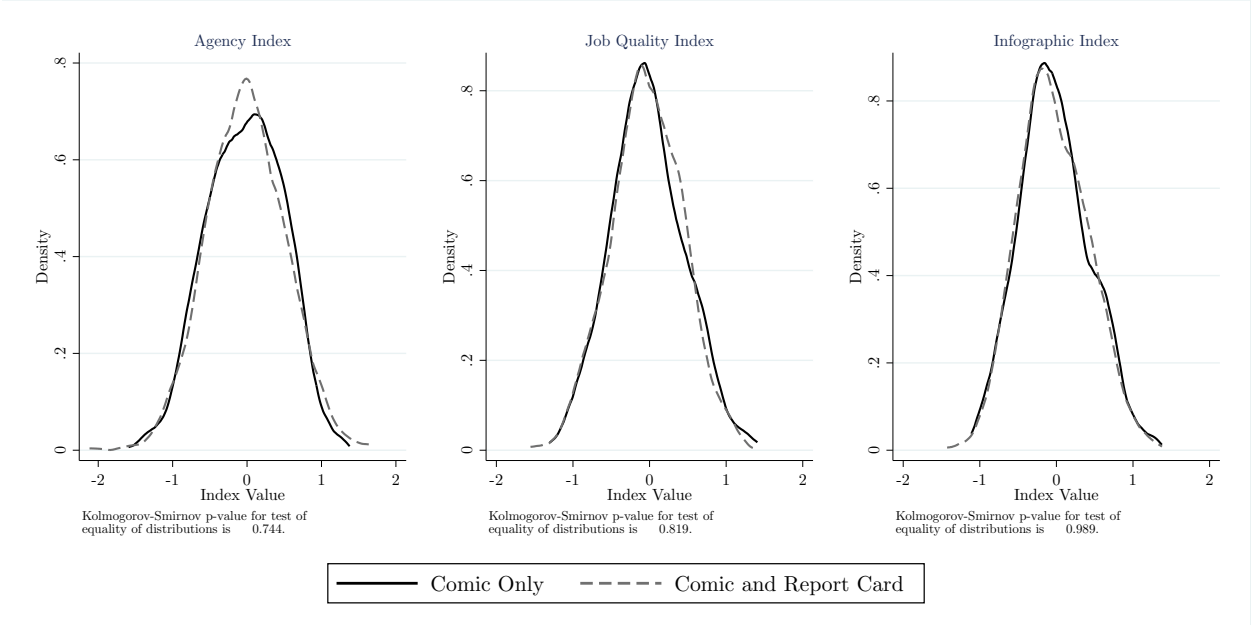
RANKING No.	NAME OF PLACEMENT AGENCY (PPTKIS)	Number Reviews	GRADE OF PPTKIS
1	DWIPA HARITAMA	59	95
2	SENTOSAKARYA ADITAMA	87	94
3	WAHANA KARYA SUPLAINDO	88	93
4	SRITI RUKMA LESTARI	36	92
5	FORWARD GLOBAL	41	90
6	SUMBER DHARMA BHAKTI	58	89
7	ANTOBINTAN PERMAI	38	89
8	SUMA JAYA	31	88
9	NURWIRA CAHAYA	37	87
10	PUTRA JABUNG PERKASA	66	86
11	PUTRA PARA UTAMA KARYA	32	83
12	FORTUNATAMA INSANI	55	83
13	PUTRA BRAGAS MANDIRI	36	82
14	MUTIARA PUTRA UTAMA	39	80
15	EKASANTI JAYAMULYA	48	79
16	ARNI FAMILY	92	78
17	ABUL PRATAMAJAYA *	110	72
18	MITRA SOLUSI INTEGRITAS	45	72
19	ELSHAFAH ADI WIGUNA MANDIRI *	185	64
20	QAFCO	45	60

Information Center:
0877.6513.5020 / 0813.3731.2623

C. Comic Book

Notes: Interested readers can view complete translations of the report card, infographic, and comic book online via embedded links in this note.

Figure D.3: Impact of Report Card on Distribution of Beliefs



Notes: Sample Limited to Non-Migrants interviewed directly at final follow up. Panel A is based on questions about the likelihood of a given outcome for others like the respondent in her village while Panel B is based on the same questions as applied to the respondent herself. The "Agency Index" includes questions about the following: will the agency take identify documents, will the agency give information on migrant worker rights, will the agency provide clean food and water, will the agency staff treat migrant workers with respect, will the agency staff let the migrant workers leave the dorms/training center, will the agency follow legal procedures, will the agency give accurate information. The "Job Quality Index" includes information about the following: will the migrant have to work 12+ hours, will the job match the contract, will the migrant get a day off, will the migrant's salary be retained, will the migrant be paid less than their contract, will the employer hold the migrant's identity documents, will the employer provide presents, will the migrant have to return early, will the migrant experience physical abuse. The "Infographic Index" includes the first two components of the agency index and the first 6 components of the job quality index. Index components are coded so that a higher value indicates a better outcome. The baseline analogues of the indices exclude beliefs about the agency/employer taking identity documents, the job matching the contract, and salary retention because these questions were not included in the baseline survey.

Table D.1: How Much Variation in Baseline Outcomes Can Agency Fixed Effects Explain?

	(1) Pre-Departure Preparation Index	(2) Job Quality Index	(3) Pay Index
Departure Year FE	0.000	0.005	0.004
+Destination FE	0.022	0.137	0.376
+Occupation FE	0.037	0.156	0.382
Departure × Destination × Occupation FE	0.074	0.181	0.409
+Agency FE	0.206	0.255	0.459
N	4055	4055	4055

Notes: This table displays R^2s from regressions of the specified outcomes on specified sets of fixed effects. The *pre-departure preparation index* includes: a dummy indicating whether the agency provided training, time spent on training, the share of government-mandated training topics covered by the agency (equipment/tools required for job, job skills, destination information, how to remit money, migration insurance policy, how to behave on the job, destination country culture, how to get help when abroad, the repatriation process, migrant worker rights, the migration contract), the migrant's subjective grade (0-10) of the agency training, whether the migrant signed a contract (in Indonesian, that she understood) while at the agency, whether the agency allowed the migrant to leave the training facility and residence, whether the agency held the migrant's identity documents, whether the agency followed legal procedures (per the migrant's assessment), and the migrant's subjective overall rating of the agency on a 0-10 scale. The *job quality index* includes: whether the migrant was given a weekly day off, the job matched the contract, the employer allowed the migrant to retain her identity documents, the migrant had her own private living quarters, the migrant received proof of payment, the migrant was allowed to leave the employer's residence, and the migrant's overall subjective rating of the migration experience. The *pay index* includes: total wages net of salary deductions, total earnings (wages plus other income from the agency, sponsor, and employer) net of costs (salary deductions plus other migration costs paid to the agency, sponsor, employer, or other entities), whether the migrant received the full contracted salary, whether the migrant received salary payments on time, and whether the migrant received additional pay for overtime work. Sample limited to women interviewed at baseline who migrated in 2011 or 2012. We drop singleton cells formed by our departure year × destination × occuption fixed effects and agency fixed effects.

Table D.2: Testing for Differential Attrition Across Treatment Arms

	Follow Up 1			Follow Up 2			Follow Up 3		
	(1) In Person Interview	(2) Direct Interview	(3) Any Interview	(4) In Person Interview	(5) Direct Interview	(6) Any Interview	(7) In Person Interview	(8) Direct Interview	(9) Any Interview
β_1: Report Card + Comic + Facilitation	0.029	0.00036	-0.0022	0.027	0.0070	-0.015**	0.00091	-0.013	-0.0071
	(0.021)	(0.017)	(0.0034)	(0.021)	(0.020)	(0.0063)	(0.021)	(0.018)	(0.0067)
β_2: Comic + Facilitation	-0.015	-0.017	0.00064	-0.029	-0.026	-0.018***	-0.022	-0.011	0.00085
	(0.022)	(0.017)	(0.0029)	(0.020)	(0.020)	(0.0063)	(0.021)	(0.018)	(0.0064)
β_3: Report + Facilitation	0.028	0.018	-0.0024	0.026	0.033*	-0.0094*	0.0041	-0.0045	0.0011
	(0.021)	(0.016)	(0.0032)	(0.022)	(0.019)	(0.0056)	(0.020)	(0.018)	(0.0065)
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.044**	0.017	-0.003	0.056***	0.033	0.003	0.023	-0.002	-0.008
	(0.021)	(0.015)	(0.003)	(0.020)	(0.020)	(0.007)	(0.020)	(0.016)	(0.007)
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.001	-0.017	0.000	0.000	-0.026	-0.005	-0.003	-0.009	-0.008
	(0.020)	(0.014)	(0.004)	(0.022)	(0.019)	(0.007)	(0.020)	(0.016)	(0.007)
Control Mean	0.654	0.834	0.994	0.634	0.735	0.988	0.648	0.791	0.975
N	4805	4805	4805	4805	4805	4805	4805	4805	4805

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table D.3: Differences in In-Person and Direct Interview Rates At Endline Among Migrants

	(1) In Person Interview	(2) Direct Interview
β_1: Report Card + Comic + Facilitation	-0.032	-0.044
	(0.030)	(0.036)
β_2: Comic + Facilitation	0.0066	0.0026
	(0.030)	(0.036)
β_3: Report + Facilitation	0.014	-0.0064
	(0.030)	(0.035)
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.039	-0.047
	(0.031)	(0.036)
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.046	-0.038
	(0.031)	(0.035)
Control Mean	0.285	0.596
N	1745	1745

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table D.4: First Stage: Impact of Treatment on Self-Reported Exposure to Intervention Material, Extra Experience Sample

	(1)	(2)	(3)	(4)
	Attended Meeting	Recognizes Infographic	Recognizes Report Card	Recognizes Comic
β_1: Report Card + Comic + Facilitation	0.035***	0.018	0.0093	0.066***
	(0.0097)	(0.015)	(0.0095)	(0.023)
	[0.0090]	[0.39]	[0.42]	[0.024]
β_2: Comic + Facilitation	0.034***	0.023	0.00067	0.060**
	(0.010)	(0.015)	(0.0093)	(0.026)
	[0.0090]	[0.23]	[0.94]	[0.052]
β_3: Report + Facilitation	0.027***	0.023	0.021**	0.0073
	(0.0094)	(0.014)	(0.010)	(0.022)
	[0.024]	[0.20]	[0.10]	[0.89]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.000	-0.005	0.009	0.007
	(0.013)	(0.016)	(0.010)	(0.026)
	[0.940]	[0.889]	[0.499]	[0.889]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.008	-0.005	-0.012	0.059***
	(0.012)	(0.015)	(0.011)	(0.023)
	[0.643]	[0.889]	[0.423]	[0.032]
Control Mean	0.012	0.047	0.022	0.130
N	2395	2397	2399	2401

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. In person interviews only. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table D.5: Impact on Beliefs Among Non-Migrants, Heterogeneity by Baseline Beliefs

	Agency Index	Job Quality Index	Infographic Index
Panel A. Women with Above-Median Baseline Beliefs			
β_1: Report Card + Comic + Facilitation	0.0078	-0.037	-0.034
	(0.039)	(0.037)	(0.037)
	[1]	[1]	[1]
β_2: Comic + Facilitation	-0.046	-0.010	-0.011
	(0.041)	(0.034)	(0.036)
	[1]	[1]	[1]
β_3: Report + Facilitation	-0.018	-0.084**	-0.070**
	(0.038)	(0.036)	(0.034)
	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.054	-0.027	-0.023
	(0.043)	(0.038)	(0.039)
	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.026	0.047	0.036
	(0.041)	(0.040)	(0.038)
	[1]	[1]	[1]
Control Mean	0.101	0.095	0.061
N	1327	1322	1315
Panel B. Women with Below-Median Baseline Beliefs			
β_1: Report Card + Comic + Facilitation	0.032	-0.041	-0.028
	(0.040)	(0.038)	(0.034)
	[1]	[1]	[1]
β_2: Comic + Facilitation	0.034	-0.025	-0.017
	(0.044)	(0.039)	(0.035)
	[1]	[1]	[1]
β_3: Report + Facilitation	0.0099	-0.00068	-0.017
	(0.041)	(0.040)	(0.036)
	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.002	-0.017	-0.010
	(0.042)	(0.036)	(0.034)
	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.022	-0.041	-0.011
	(0.039)	(0.038)	(0.036)
	[1]	[1]	[1]
Control Mean	-0.118	-0.098	-0.069
N	1270	1275	1282

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to non-migrants directly interviewed at final follow-up. See notes to Table 6 for additional detail on index components. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table D.6: Impact on Aspects of Migrant Pre-Departure Preparation

	(1) Fraction Topics Trained	(2) Received Training	(3) Weeks in Training^{+}	(4) Subjective Training Rating	(5) Used Agency	(6) Signed and Understood Contract	(7) Allowed to Leave Agency Grounds	(8) Allowed to Retain ID Documents	(9) Agency Followed Legal Procedures	(10) Subjective Agency Rating
β_1: Report Card + Comic + Facilitation	0.051	0.027	1.04	0.42	0.030	0.057	0.020	0.0045	0.011	0.37
	(0.044)	(0.045)	(0.83)	(0.38)	(0.028)	(0.046)	(0.042)	(0.043)	(0.038)	(0.28)
	[0.75]	[1]	[0.72]	[0.75]	[0.75]	[0.72]	[1]	[1]	[1]	[0.72]
β_2: Comic + Facilitation	-0.042	-0.054	-1.16*	-0.44	-0.016	-0.040	-0.027	-0.056	-0.017	-0.13
	(0.043)	(0.044)	(0.67)	(0.35)	(0.031)	(0.046)	(0.041)	(0.041)	(0.038)	(0.29)
	[0.86]	[0.72]	[0.60]	[0.72]	[1]	[0.87]	[1]	[0.72]	[1]	[1]
β_3: Report + Facilitation	-0.030	-0.051	-0.25	-0.41	0.011	-0.025	0.0092	0.0069	0.017	-0.022
	(0.044)	(0.048)	(0.71)	(0.38)	(0.029)	(0.045)	(0.042)	(0.042)	(0.037)	(0.27)
	[1]	[0.75]	[1]	[0.75]	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.093**	0.082*	2.197***	0.855**	0.046	0.097**	0.048	0.061	0.028	0.501*
	(0.045)	(0.045)	(0.816)	(0.370)	(0.028)	(0.045)	(0.039)	(0.042)	(0.038)	(0.275)
	[0.589]	[0.603]	[0.589]	[0.589]	[0.666]	[0.589]	[0.718]	[0.714]	[1]	[0.603]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.081*	0.078	1.290	0.825**	0.020	0.082*	0.011	-0.002	-0.006	0.392
	(0.045)	(0.049)	(0.849)	(0.395)	(0.026)	(0.044)	(0.040)	(0.043)	(0.036)	(0.252)
	[0.603]	[0.666]	[0.666]	[0.589]	[1]	[0.603]	[1]	[1]	[1]	[0.666]
Control Mean	0.563	0.690	5.417	5.082	0.877	0.592	0.292	0.336	0.798	6.649
N	977	977	976	974	1015	1001	955	984	975	979

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to directly interviewed migrants in the tracking sample. Outcomes for women migrating without agencies are recoded to zero. $^{+}$Top-coded at 99th percentile. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table D.7: Impact on Non-Pecuniary Aspects of Migrant Job Quality

	(1) Got Weekly Day Off	(2) Job Matched Contract	(3) Migrant Kept Documents	(4) Had Own Room	(5) Subjective Experience Grade	(6) Received Proof Payment	(7) Allowed to Leave Work Site
β_1: Report Card + Comic + Facilitation	0.018	0.078**	0.040	0.023	0.0010	0.057	-0.012
	(0.048)	(0.034)	(0.045)	(0.045)	(0.18)	(0.040)	(0.046)
	[1]	[0.38]	[0.91]	[1]	[1]	[0.77]	[1]
β_2: Comic + Facilitation	-0.060	0.026	-0.029	-0.0091	-0.19	-0.013	-0.00064
	(0.045)	(0.036)	(0.045)	(0.045)	(0.16)	(0.044)	(0.042)
	[0.81]	[0.97]	[0.97]	[1]	[0.91]	[1]	[1]
β_3: Report + Facilitation	-0.012	-0.0064	-0.063	-0.044	-0.26	0.048	-0.033
	(0.044)	(0.039)	(0.044)	(0.047)	(0.16)	(0.041)	(0.044)
	[1]	[1]	[0.77]	[0.91]	[0.77]	[0.91]	[0.97]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.078*	0.051	0.069	0.032	0.188	0.070	-0.011
	(0.046)	(0.033)	(0.044)	(0.046)	(0.185)	(0.043)	(0.046)
	[0.768]	[0.768]	[0.768]	[0.973]	[0.906]	[0.768]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.030	0.084**	0.103**	0.067	0.262	0.009	0.021
	(0.045)	(0.036)	(0.043)	(0.048)	(0.183)	(0.040)	(0.048)
	[0.973]	[0.382]	[0.382]	[0.768]	[0.768]	[1]	[1]
Control Mean	0.357	0.774	0.406	0.502	7.553	0.709	0.586
N	982	976	982	974	982	980	971

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to directly interviewed migrants in the tracking sample. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table D.8: Impact on Aspects of Migrant Job Compensation

	(1) Wages Net Costs^{+}	(2) Total Earnings^{+}	(3) No Salary Cuts	(4) No Retained Salary	(5) Received Overtime
β_1: Report Card + Comic + Facilitation	-942.5	-436.0	0.030	0.0013	0.010
	(713.4)	(712.5)	(0.031)	(0.031)	(0.047)
	[1]	[1]	[1]	[1]	[1]
β_2: Comic + Facilitation	-353.3	-378.5	0.042	-0.0044	-0.029
	(673.8)	(657.9)	(0.031)	(0.027)	(0.044)
	[1]	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	-710.6	-327.6	0.039	-0.012	-0.040
	(692.0)	(649.1)	(0.028)	(0.027)	(0.048)
	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-589.178	-57.494	-0.012	0.006	0.039
	(639.891)	(648.339)	(0.030)	(0.031)	(0.045)
	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-231.890	-108.311	-0.009	0.013	0.051
	(648.608)	(641.135)	(0.027)	(0.030)	(0.049)
	[1]	[1]	[1]	[1]	[1]
Control Mean	11274.578	10539.925	0.856	0.119	0.477
N	978	858	979	981	968

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to directly interviewed migrants in the tracking sample. $^{+}$IDR 10,000s, top-coded at 99th percentile. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table D.9: Impact on Destination and Occupation Abroad (Migrants Only)

	Destination			Occupation Abroad			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Asia	MENA	Other	Domestic Worker	Elderly Caregiver	Babysitter/ Nanny	Formal Sector Work
β_1: Report Card + Comic + Facilitation	-0.010	0.014	-0.0039	0.026	-0.0014	0.024	-0.048**
	(0.040)	(0.040)	(0.0040)	(0.036)	(0.047)	(0.036)	(0.021)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
β_2: Comic + Facilitation	-0.043	0.040	0.0032	0.015	-0.041	0.013	-0.012
	(0.044)	(0.043)	(0.0065)	(0.037)	(0.043)	(0.034)	(0.023)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	0.010	-0.0069	-0.0035	-0.0063	-0.017	0.025	-0.022
	(0.041)	(0.041)	(0.0038)	(0.038)	(0.044)	(0.034)	(0.023)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.033	-0.026	-0.007	0.011	0.040	0.012	-0.037*
	(0.045)	(0.045)	(0.005)	(0.036)	(0.047)	(0.040)	(0.019)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.021	0.021	-0.000	0.032	0.015	-0.000	-0.026
	(0.042)	(0.042)	(0.001)	(0.037)	(0.047)	(0.040)	(0.019)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
Control Mean	0.736	0.260	0.004	0.787	0.441	0.228	0.083
N	1018	1018	1018	1018	1018	1018	1018

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to directly interviewed migrants in the tracking sample. Occuption categories are not mutually exclusive. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table D.10: Decomposition of the Report Card's Impacts on Migration Experiences

	Agency Fixed Effects			Provider Endline Grade		
	Pre-Departure Preparation Index	Job Quality Index	Pay Index	Pre-Departure Preparation Index	Job Quality Index	Pay Index
	(1)	(2)	(3)	(4)	(5)	(6)
Agency Effect	0.114**	0.043	0.031	0.124**	0.013**	0.012*
	(0.053)	(0.029)	(0.033)	(0.049)	(0.006)	(0.006)
Selection on Observables	0.002	0.014	0.008	0.005	0.016	0.016
	(0.015)	(0.022)	(0.023)	(0.019)	(0.023)	(0.025)
Selection on Unobservables	0.004	0.009	-0.000	0.002	0.008	0.001
	(0.004)	(0.009)	(0.004)	(0.004)	(0.009)	(0.005)
Residual Differences	0.069***	0.044	-0.030	0.057	0.071*	-0.020
	(0.024)	(0.031)	(0.029)	(0.040)	(0.037)	(0.037)

Notes: Robust standard errors clustered at the village level in parentheses. Following a procedure detailed in Section C.3, each column uses a two-step Heckman procedure to decompose the treatment effect on migration experience into four components: an agency effect, observable baseline characteristics of the migrant, selection on unobservables, and residual differences that cannot be explained by the other three factors. The first three columns use agency fixed effects to account for the agency effect, the final three columns use the agency endline grade (with ungraded agencies separately dummied out). The randomized information treatments are included in the first stage migration equation, but not the second stage returns equation. Observable characteristics include age, marital status dummies, a dummy to identify former migrants, strata dummies, the fraction of correct answers on a series of Raven's Matrices, the fraction of correct answers on a series of math questions, a locus of control score, big 5 personality characteristics (agreeableness, conscientiousness, extraversion, openness, and stability) scores, a mental health score, a dummy identifying less-trusting individuals, an estimate of the discount factor based on hypothetical multiple price list money choices, dummies to identify different choices in an incentivized risk game, a dummy to identify women who had sought advice on agency choice at baseline, a dummy identifying women planning to migrate to the Middle East, a dummy to identify women planning to migrate directly with an agency or some other way (versus with a sponsor), the woman's expected net salary, expected hours per day of work, and standardized belief indices captureing baseline beliefs about the quality of agencies, sponsor, and on-the-job experiences for both the woman herself and others. Additional details available in the Data Appendix. Standard errors are based on 500 bootstrap replications, clustered at the village level. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table D.11: Impact on Household Economic Welfare

	(1) Household Monthly Income	(2) Household Total Expend.	(3) Household Food Expend.	(4) Housing Quality Index	(5) Asset Index	(6) Dependence on Social Protection Index
β_1: Report Card + Comic + Facilitation	-0.30**	-0.065	0.024	0.17**	-0.025	0.048
	(0.15)	(0.10)	(0.044)	(0.073)	(0.048)	(0.053)
	[1]	[1]	[1]	[1]	[1]	[1]
β_2: Comic + Facilitation	-0.18	0.026	0.014	0.10	-0.049	0.058
	(0.16)	(0.11)	(0.047)	(0.078)	(0.050)	(0.056)
	[1]	[1]	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	-0.069	-0.036	0.013	0.12	-0.061	0.025
	(0.16)	(0.11)	(0.045)	(0.077)	(0.050)	(0.055)
	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.118	-0.091	0.010	0.066	0.024	-0.009
	(0.154)	(0.098)	(0.045)	(0.071)	(0.050)	(0.049)
	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.227	-0.029	0.012	0.051	0.035	0.023
	(0.158)	(0.095)	(0.043)	(0.070)	(0.050)	(0.048)
	[1]	[1]	[1]	[1]	[1]	[1]
Control Mean	4.155	2.686	1.368	0.000	0.000	-0.000
N	4550	4456	4465	4618	4532	4672

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. All income and expenditure measures are topcoded at the 99th percentile and reported in millions IDR. Monthly household income includes wages, business profits, the value of agricultural production less costs, remittances, and other income. The housing index includes indicators of roof material, wall material, floor material, water source, and toilet facilities. The asset index includes indicators for ownership of a bicycle, motorcycle, boat, TV, air conditioner, heater, gat stove, refridgerator, motorboat, car, house, and land. The dependence on social protection index includes indicators for receipt of 7 common social protection programs in Indonesia. We construct the housing index using factor analysis, extracting the first factor and signing the index so higher values correspond to better outcomes. The asset and social protection indices are GLS-weighted averages of their standardized components. We normalize all indices relative to the control group mean and standard deviation.

Table D.12: Impact on Occuptional Status and Earnings

	(1) Unemployed	(2) Unpaid Family Worker	(3) Casual Worker	(4) Wage Employee	(5) Self Employed	(6) On Migration	(7) Monthly Earnings
β_1: Report Card + Comic + Facilitation	-0.023	-0.0037	0.00028	-0.020	0.041**	0.0042	-0.091
	(0.021)	(0.0054)	(0.011)	(0.017)	(0.017)	(0.018)	(0.11)
	[1]	[1]	[1]	[1]	[0.24]	[1]	[1]
β_2: Comic + Facilitation	-0.0090	-0.0056	0.0086	0.00078	-0.016	0.021	-0.022
	(0.022)	(0.0055)	(0.011)	(0.017)	(0.017)	(0.018)	(0.11)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	-0.0065	-0.0052	0.011	0.0079	-0.013	0.0026	-0.039
	(0.022)	(0.0054)	(0.012)	(0.017)	(0.015)	(0.018)	(0.11)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.014	0.002	-0.008	-0.021	0.056***	-0.017	-0.069
	(0.022)	(0.005)	(0.013)	(0.017)	(0.017)	(0.018)	(0.113)
	[1]	[1]	[1]	[1]	[0.022]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.016	0.002	-0.011	-0.028*	0.053***	0.002	-0.053
	(0.021)	(0.004)	(0.014)	(0.016)	(0.016)	(0.019)	(0.113)
	[1]	[1]	[1]	[1]	[0.022]	[1]	[1]
Control Mean	0.352	0.018	0.061	0.148	0.170	0.248	1.458
N	4659	4646	4659	4659	4659	4672	3695

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Monthly income is only available for directly interviewed migrants and is measured in millions IDR, top-coded at the 99th percentile. For women currently abroad, we calculate total earnings less deductions to date, divided by the number of months abroad. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table D.13: Impact on Village-Level Agency Market Shares of Sanctioned Agencies: Administrative Data

	Levels				Fraction Female Migrants		
	(1) Total	(2) Average Graded Placements	(3) High Graded Placements	(4) Placements With Agencies Lacking Endline Grade	(5) Average Graded Placements	(6) High Graded Placements	(7) Placements With Agencies Lacking Endline Grade
β_1: Report Card + Comic + Facilitation	-1.45	-1.49	2.97	-2.93	0.0027	0.016	-0.019
	(15.8)	(4.10)	(7.83)	(5.83)	(0.011)	(0.016)	(0.014)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
β_2: Comic + Facilitation	-2.89	-0.54	-0.99	-1.36	0.0098	-0.0100	0.00016
	(14.7)	(3.93)	(7.50)	(5.41)	(0.013)	(0.016)	(0.015)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	8.39	-0.25	10.7	-2.03	-0.0016	0.034**	-0.032**
	(15.2)	(4.07)	(7.67)	(5.77)	(0.011)	(0.015)	(0.014)
	[1]	[1]	[1]	[1]	[1]	[0.65]	[0.65]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	1.436	-0.959	3.958	-1.563	-0.007	0.026*	-0.019
	(15.324)	(3.495)	(8.041)	(5.625)	(0.013)	(0.016)	(0.014)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-9.842	-1.248	-7.693	-0.901	0.004	-0.018	0.013
	(15.882)	(3.677)	(8.217)	(5.983)	(0.011)	(0.015)	(0.012)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]
Control Mean	221.220	43.410	91.870	85.940	0.194	0.399	0.406
N	400	400	400	400	400	400	400

Notes: Heteroskedasticity robust standard errors in parentheses. Data are at village level, and cover October 2015 to December 2019. All regressions control for strata fixed effects. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10.

Table D.14: Impact on Pre-Departure Preparation and Experience Abroad: Results from Supplementary Sample

	Standardized Experience Indices		
	(1) Pre-Departure Preparation	(2) Migration Experience: Job Quality	(3) Migration Experience: Pay
β_1: Report Card + Comic + Facilitation	0.051	-0.034	0.012
	(0.043)	(0.031)	(0.033)
	[1]	[1]	[1]
β_2: Comic + Facilitation	-0.035	-0.026	0.013
	(0.043)	(0.032)	(0.033)
	[1]	[1]	[1]
β_3: Report + Facilitation	0.0047	0.034	0.0079
	(0.041)	(0.029)	(0.030)
	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.086*	-0.008	-0.001
	(0.045)	(0.031)	(0.034)
	[0.688]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.047	-0.068**	0.004
	(0.044)	(0.028)	(0.031)
	[1]	[0.342]	[1]
Control Mean	-0.244	-0.079	-0.032
N	2415	2411	2416

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to directly interviewed migrants in the supplementary sample. The *pre-departure preparation index* includes: use of an agency, whether the agency provided training, time spent on training, the share of government-mandated training topics covered by the agency, the migrant's subjective grade (0-10) of the agency training, whether the migrant signed a contract (in Indonesian, that she understood) while at the agency, whether the agency allowed the migrant to leave the training facility and residence, whether the agency held the migrant's identity documents, whether the agency followed legal procedures (per the migrant's assessment), and the migrant's subjective overall rating of the agency on a 0-10 scale. All outcomes are coded to zero if the woman did not use an agency to migrate. The *job quality index* includes: whether the migrant was given a weekly day off, the job matched the contract, the employer allowed the migrant to retain her identity documents, the migrant had her own private living quarters, the migrant received proof of payment, the migrant was allowed to leave the employer's residence, and the migrant's overall subjective rating of the migration experience. The *pay index* includes: total wages net of salary deductions, total earnings (wages plus other income from the agency, sponsor, and employer) net of costs (salary deductions plus other migration costs paid to the agency, sponsor, employer, or other entities), whether the migrant received the full contracted salary, whether the migrant received salary payments on time, and whether the migrant received additional pay for overtime work. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table D.15: Impact on Market Share of Placement Providers – Results from the Supplementary Sample

	(1) Low Quality Provider	(2) Average Quality Provider	(3) High Quality Provider	(4) Provider Grade
β_1: Report Card + Comic + Facilitation	-0.064**	0.033	0.031	0.72
	(0.031)	(0.031)	(0.029)	(0.66)
	[0.60]	[1]	[1]	[1]
β_2: Comic + Facilitation	0.0082	-0.011	0.0027	-0.36
	(0.033)	(0.030)	(0.029)	(0.69)
	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	-0.019	-0.015	0.034	0.34
	(0.032)	(0.030)	(0.030)	(0.67)
	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.072**	0.044	0.028	1.075
	(0.032)	(0.031)	(0.029)	(0.688)
	[0.600]	[0.890]	[1]	[0.890]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.045	0.048	-0.004	0.379
	(0.031)	(0.031)	(0.030)	(0.670)
	[0.890]	[0.890]	[1]	[1]
Control Mean	0.402	0.324	0.274	74.648
N	2410	2410	2410	2410

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. We calculate updated grades by using experience reports in the extra experience sample. We construct empirical Bayes estimates of average overall experience, defined as a simple average of the pre-departure preparation, job quality, and pay indices. We construct dummy variables to identify high (top third of grade distribution), average (middle third), and low-quality (bottom third) providers. Women who migrated with an agency that was not on the Indonesian government's list of approved placement agencies are treated as a single group, as are: (i) women who reported using an agency but do not report a name for the agency, (ii) women who migrate with a sponsor and no agency, and (iii) women who migrate with no sponsor and no agency. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.

Table D.16: Impact on Pre-Departure Preparation and Experience Abroad – Early Migrants

	Migrated within 6 Months Int.			Migrated within 12 Months Int.		
	(1) Pre-Departure Preparation	(2) Migration Experience: Job Quality	(3) Migration Experience: Pay	(4) Pre-Departure Preparation	(5) Migration Experience: Job Quality	(6) Migration Experience: Pay
β_1: Report Card + Comic + Facilitation	0.094	0.12	-0.13	0.10	0.075	-0.11
	(0.091)	(0.076)	(0.087)	(0.079)	(0.058)	(0.070)
	[0.61]	[0.61]	[0.61]	[0.61]	[0.61]	[0.61]
β_2: Comic + Facilitation	-0.083	0.032	-0.14*	-0.036	-0.028	-0.060
	(0.082)	(0.067)	(0.083)	(0.077)	(0.052)	(0.066)
	[0.61]	[0.69]	[0.61]	[0.69]	[0.69]	[0.61]
β_3: Report + Facilitation	-0.029	-0.020	-0.12	-0.016	-0.055	-0.069
	(0.087)	(0.078)	(0.093)	(0.078)	(0.057)	(0.072)
	[0.75]	[0.80]	[0.61]	[0.81]	[0.61]	[0.61]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	0.177**	0.089	0.006	0.136*	0.102*	-0.048
	(0.085)	(0.068)	(0.080)	(0.071)	(0.057)	(0.066)
	[0.610]	[0.610]	[0.891]	[0.610]	[0.610]	[0.692]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	0.122	0.142*	-0.014	0.117	0.129**	-0.040
	(0.091)	(0.079)	(0.092)	(0.073)	(0.061)	(0.071)
	[0.610]	[0.610]	[0.832]	[0.610]	[0.610]	[0.692]
Control Mean	0.063	-0.018	0.208	0.015	-0.011	0.149
N	304	299	300	490	476	477

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Sample limited to directly interviewed migrants in the tracking sample (80% of 6 month migrants, 67% of 12 month migrants). The *pre-departure preparation index* includes: use of an agency, whether the agency provided training, time spent on training, the share of government-mandated training topics covered by the agency, the migrant's subjective grade (0-10) of the agency training, whether the migrant signed a contract (in Indonesian, that she understood) while at the agency, whether the agency allowed the migrant to leave the training facility and residence, whether the agency held the migrant's identity documents, whether the agency followed legal procedures (per the migrant's assessment), and the migrant's subjective overall rating of the agency on a 0-10 scale. All outcomes are coded to zero if the woman did not use an agency to migrate. The *job quality index* includes: whether the migrant was given a weekly day off, the job matched the contract, the employer allowed the migrant to retain her identity documents, the migrant had her own private living quarters, the migrant received proof of payment, the migrant was allowed to leave the employer's residence, and the migrant's overall subjective rating of the migration experience. The *pay index* includes: total wages net of salary deductions, total earnings (wages plus other income from the agency, sponsor, and employer) net of costs (salary deductions plus other migration costs paid to the agency, sponsor, employer, or other entities), whether the migrant received the full contracted salary, whether the migrant received salary payments on time, and whether the migrant received additional pay for overtime work. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.01.

Table D.17: Impact on Market Share of Placement Providers – Early Migrants

	Migrated within 6 Months Int.				Migrated within 12 Months Int.			
	(1) Low Quality Provider	(2) Average Quality Provider	(3) High Quality Provider	(4) Provider Grade	(5) Low Quality Provider	(6) Average Quality Provider	(7) High Quality Provider	(8) Provider Grade
β_1: Report Card + Comic + Facilitation	-0.16**	0.044	0.11	2.72*	-0.15**	0.086	0.060	2.39*
	(0.078)	(0.078)	(0.085)	(1.39)	(0.064)	(0.064)	(0.067)	(1.23)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
β_2: Comic + Facilitation	-0.100	0.12	-0.017	0.77	-0.062	0.061	0.00076	1.32
	(0.077)	(0.079)	(0.073)	(1.48)	(0.065)	(0.061)	(0.064)	(1.31)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
β_3: Report + Facilitation	-0.11	0.11	-0.0013	1.21	-0.090	0.10*	-0.013	1.17
	(0.081)	(0.082)	(0.083)	(1.50)	(0.066)	(0.060)	(0.065)	(1.32)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_2$: Report Card | Comic + Fac.	-0.059	-0.073	0.132	1.953	-0.085	0.026	0.059	1.068
	(0.071)	(0.077)	(0.084)	(1.396)	(0.061)	(0.065)	(0.068)	(1.169)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
$\beta_1 - \beta_3$: Comic | Report Card + Fac.	-0.048	-0.068	0.116	1.509	-0.057	-0.016	0.073	1.213
	(0.075)	(0.082)	(0.092)	(1.412)	(0.062)	(0.064)	(0.069)	(1.199)
	[1]	[1]	[1]	[1]	[1]	[1]	[1]	[1]
Control Mean	0.405	0.270	0.324	76.398	0.402	0.260	0.339	76.063
N	285	285	285	285	466	466	466	466

Notes: Robust standard errors clustered at the village level in parentheses. Sharpened q-values that control the false discovery rate (FDR) in square brackets. The FDR family pools across all outcomes and tests in the table. All regressions control for strata fixed effects. Direct interviews only (80% of 6 month, 67% of 12 month migrants). We calculate updated grades by using experience reports in the extra experience sample. We construct empirical Bayes estimates of average overall experience, defined as a simple average of the pre-departure preparation, job quality, and pay indices. We construct dummy variables to identify high (top third of grade distribution), average (middle third), and low-quality (bottom third) providers. Women who migrated with an agency that was not on the Indonesian government's list of approved placement agencies are treated as a single group, as are: (i) women who reported using an agency but do not report a name for the agency, (ii) women who migrate with a sponsor and no agency, and (iii) women who migrate with no sponsor and no agency. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.01.