Information, Intermediaries, and International Migration[*]

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Abstract

Job seekers often face substantial information frictions related to potential job quality. This is especially true in international labor markets, where intermediaries match prospective migrants with employers abroad. We ran a randomized trial in Indonesia to explore how information about intermediary quality shapes migration choices and outcomes. Information reduces the migration rate. However, workers who do migrate receive better pre-departure preparation and have better experiences abroad, despite no change in occupation or destination. We find no evidence that information changes intentions to migrate or beliefs about the return to migration or intermediary quality. Nor does selection explain the improved outcomes for workers who choose to migrate with the information. Together, our findings are consistent with an increase in the option value of search. This offers a new perspective on the importance of information and matching frictions in global labor markets.

Keywords: International Migration, Information, Middlemen, Quality Disclosure, Search

JEL Codes: F22, O15, D83, L15

1   Introduction

Information frictions are a prominent feature of many labor markets. Employers often struggle to evaluate applicant skill while job seekers face constraints assessing the relative risks and returns to different employment opportunities. The latter set of frictions are especially salient in international labor markets where workers must match with employers overseas and navigate complex migration processes. Some may be dissuaded by these difficulties and ultimately forego otherwise high-return employment opportunities. In such contexts, intermediaries have a clear role to play in matching aspiring migrants to jobs abroad. While intermediaries are central to the functioning of global labor markets, prospective workers may face a new set of information frictions regarding intermediary quality, and little is known about how these entities shape migration choices and welfare. This stands in contrast to well-established work on intermediaries in international trade in goods (e.g., Ahn et al., 2011; Antras and Costinot, 2011). We help fill this knowledge gap and, in so doing, offer a new perspective on how information frictions shape overseas job search and migrant welfare.

Our contribution focuses on how information about intermediary quality affects migration choices and experiences. Using original survey data from roughly 7,000 female former migrants in Indonesia, we constructed quality ratings for intermediary agencies responsible for recruiting, training, and placing migrant workers in overseas labor markets. Women who received access to these ratings were significantly less likely to migrate. However, those who did migrate received better pre-departure preparation and experienced better on-the-job amenities—two margins where agencies have scope for considerable influence. Our intervention did not change beliefs about the returns to migration or intermediary quality; nor did it change intentions to migrate in the future. Rather, we argue the new information led women to prolong search in an effort to secure an offer from a higher-quality agency, thereby reducing the share of migrants utilizing lower-quality, riskier providers. We formalize these arguments in a search model, which clarifies how access to new signals of intermediary quality can increase the option value of search and thereby slow the migration rate.

The international labor migration process in Indonesia is similar to other low- and middle-income countries across Asia and beyond. Every year, hundreds of thousands of low-skill workers go abroad on temporary contracts lasting 2–3 years. Mirroring global trends in international migration, women comprise a sizeable share of all Indonesian migrants.[1] They specialize in domestic work and elderly care, and their incomes far exceed those prevailing in labor markets back home. While migrant wages are typically 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. From our own formative work (see Section 2.2) and complementary findings for Sri Lanka (Fernando and Lodermeier, 2021), it is clear that intermediaries often loom large in explaining these varied migration experiences. Indonesian placement agencies facilitate the migration process from start to finish and are especially important for female migrants, who face distinct risks and challenges. The risks often prove so challenging for sending-country governments that they frequently opt to ban their female labor force from working

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[1]Women comprise nearly half of the 281 million international migrants in 2020, and they are increasingly represented among labor migrants. See the International Organization for Migration: https://migrationdataportal.org/themes/gender-and-migration. In Indonesia, women account for roughly 70 percent of all migrants utilizing formal channels.

in certain destinations and occupations (see Section 2.1 on Indonesia and McKenzie et al., 2014, on the Philippines). Such blunt policy measures deprive households of high-return work opportunities, raising 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.

This is the point of departure for our study, which leverages the collective experiences of former migrants to help future ones select higher-quality intermediaries. We provided new information about intermediary quality to potential migrants across several hundred of Indonesia's largest migrant-sending villages. In the median control group village, aspiring migrants face a fragmented market with nearly 40 agencies placing workers during our intervention period. While this should in theory translate to considerable choice, it also makes it difficult to learn about agency quality: social networks are limited and migrations take 2-3 years to complete, making the number of (noisy) signals per agency low. Our experiment randomized access to an agency "report card" that ranked locally-relevant intermediaries on quality. A separate, cross-cut treatment called attention to the importance of intermediary choice.

To populate the report card, we constructed an index of agency quality based on the migration experiences of female migrants who had returned to Indonesia in the two years preceding our study.[2] We also developed an edutainment-style comic book that told the story of a woman navigating the migration process with particular emphasis on the importance of choosing a high-quality agency. Half of our 400 study villages received the comic while half received the report card, with 100 villages receiving both. To ensure comparable baseline knowledge of the returns to both migration and agency quality, all 300 treatment villages received an infographic that illustrated average migration outcomes for women who migrated with high- and low-quality agencies. In late 2015, we disseminated the information materials in community meetings that reached nearly 30,000 migration-age-eligible women. Over the next four years, we tracked 4,805 women who, at baseline, expressed an interest in migrating in the future.

We inform our empirical analysis with a model of migration choice as sequential job search under uncertainty. This mirrors the empirical reality that most migrants live in rural areas, and receive sporadic offers from middlemen who connect them to distant agencies (typically based in Jakarta, Indonesia's capital) that currently have job openings. In this setup, the report card enables potential migrants to better differentiate between high- and low-quality offers. Meanwhile, the comic book may strengthen the effect of the report card by encouraging migrants to pay attention to quality and by encouraging local information discovery. Access to information increases the expected utility of search and raises the quality threshold for offer acceptance. The resulting effect on the migration rate is ambiguous: if the threshold effect dominates, the migration rate will decrease, but differentiation could help women locate high-quality offers faster, increasing the migration rate. While we designed the infographic to anchor priors, it is theoretically possible for both interventions to shift beliefs about the return to migration, which could in turn impact the migration rate.

Motivated by these insights, we use detailed survey data capturing migration choices, plans, experiences, and beliefs to understand mechanisms of influence. To ensure our comparisons do not confound information products, we focus on the effect of the report card conditional on access to the comic and infographic, as well as the effect of the comic conditional on access to the report card and

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[2]This index spanned three domains: pre-departure preparation (e.g., skills and rights training), non-pecuniary job quality abroad (e.g., access to identity documents, employer abuse), and pay amount and structure.

infographic. Our first core finding is that the report card significantly reduced the migration rate by 4.4 percentage points (12 percent relative to the control group mean); the impact of the comic is also negative but smaller and statistically insignificant. Several results support an "increased value of search" interpretation of these results. First, neither the report card nor the comic changed intentions or steps taken to migrate among those that had not yet done so. Second, we find no evidence that the report card or any other treatment shifted beliefs about average agency quality and experiences while abroad.

In line with the model's predictions, the treatment-induced decline in the migration rate coincided with an improvement in the quality of migration experiences. Using a summary index of pre-departure preparation, we find a 0.17 standard deviation (s.d.) increase in quality due to the report card and a 0.11 s.d. unit increase due to the comic. These interventions also improved migrants' on-the-job amenities (i.e., measures of employment and living conditions) by 0.10-0.11 s.d. units. These improvements are in those parts of the migration process where agencies have the most scope for influence. We see little effect on monetary returns or pay structure, which remain largely outside the control of individual agencies or employers. These improved migrant outcomes have important welfare implications and are not the result of shifts in destination or occupation choices, which are effectively unchanged by the treatments. Rather, these improvements appear to stem from helping migrants avoid riskier offers. The report card not only reduced migration without an agency, but also helped potential migrants avoid agencies that were not likely to be officially sanctioned by the government; in our survey data, women migrating with such agencies consistently reported the worst outcomes across a range of migration experience measures.

This raises the question of whether the intervention simply worked by dissuading "marginal" women who would have counterfacutally chosen low-quality agencies and/or taken low-quality jobs. Using two distinct approaches to investigate selection, we find no evidence of such effects. And we find no substantive evidence of 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 diffuse. Thus, our results are not driven by broad improvements in agency quality, or by a crowding out of low-quality agencies from treated markets. Taken as a whole, our findings suggest that information frictions related to intermediary quality constrain migration choice, leading to hastier matches with lower-quality agencies that then provide an inferior migration experience.

Our paper is novel in its focus on intermediary quality and search in international labor markets. Despite growing work on intermediaries in developing country trade (see, e.g., Allen, 2014; Mitra et al., 2018; Startz, 2021), we know relatively little about intermediaries in labor markets outside of a small literature on the smuggling industry for illegal migration, which is focused more on border crossings than on labor market intermediation (Auriol and Mesnard, 2016; Friebel and Guriev, 2006; Gathmann, 2008; Tamura, 2010). A notable exception is Fernando and Singh (2021), who document that Sri Lankan placement agencies invest in improving ratings criteria when told they will be part of a forthcoming government-run quality disclosure program. This paper, however, has limited ability to speak to worker-side responses to such information. Our paper therefore fills an important gap, as intermediaries are central to a thriving international labor migration industry (Agunias, 2009), and understanding how information affects use of intermediaries is key to formulating effective migration policy. Moreover, we offer broader insights into job search mechanisms in environments where outcomes

are uncertain and where information about how expected outcomes vary across offers is limited.

Our study offers a new lens on a persistent puzzle in the migration literature, namely why, despite such large wage differentials, migration rates appear to be relatively low. A large literature, on both domestic and international migration, has explored barriers related to information, liquidity, risk, and utility costs (e.g., Angelucci, 2015; Bazzi, 2017; Baseler, 2020; 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 international migrants from the Philippines could not be induced to work abroad (Beam et al., 2016). In settings like ours where offers arrive sequentially, one theoretically appealing explanation is that workers perceive an "option value to waiting" whereby 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.

Migration has long been conceived as an investment strategy (Sjaastad, 1962), but Burda (1995) appears to have been one of the first to formulate migration choice under uncertainty as an option value problem.[3] However, to our reading, 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.[4] This is a key innovation of our study. We effectively vary the option value to waiting by randomizing access to a technology that allows potential migrants to better parse good offers from bad. This reduced the migration rate without changing underlying beliefs about the returns distribution or intentions to migrate, consistent with a search-based theory of migration choice. Our results generate some novel insights: for example, migration rates may be *lower* in markets where information frictions are smaller, provided offers arrive sequentially. Thus, to foster high-return migration, policy should focus on both information and ensuring migrants can easily access multiple offers over a short period of time.

Our findings also contribute to a fast-growing labor literature on information frictions in job search. Much recent work focuses on gaps in employers' knowledge of worker skills (e.g., 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, Bandiera et al. (2020) study how job seeker beliefs about search success impact search effort and employment outcomes; Conlon et al. (2018) model and estimate how job seeker beliefs about future offers evolve in response to prior offers; Chakravorty et al. (2021) show that giving vocational trainees information about prospective job quality improves placement outcomes; and Belot et al. (2018) show that giving job seekers guidance on how to broaden search increases interview requests. 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-urban migrants—could contribute to the

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

[4]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.

high turnover rates observed in many low-income countries (Donovan et al., 2021).

Finally, we contribute new insights to a rich literature on quality disclosure, spanning markets in education (Andrabi et al., 2017; Hastings and Weinstein, 2008), healthcare (Dafny and Dranove, 2008), and food services (Jin and Leslie, 2003). Much of this work explores consumer behavior in settings where there is considerable choice over goods at any given moment in time. Thus, the focus is typically 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/matches in markets where search is sequential.

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, including the selection model, 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 of these migrants engage in short-term, low-skilled work, on contracts lasting 2-3 years. However, 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 certain classes of informal workers from traveling to high-risk destinations, as well as updated regulations governing the placement and protection of migrant workers.[5]

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). Nevertheless, migration remains an important source of income for many Indonesian communities, especially since international migration is geographically concentrated (see

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[5]A moratorium on sending migrants to Malaysia was imposed in 2009 following a series of high-profile abuse cases. The ban was lifted in 2011 after an MOU was signed between the governments of 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 temporary ban on sending female migrants working in domestic jobs in Saudi Arabia. The moratorium was expanded and formalized with a complete ban on such migrants going to 21 countries in the Middle East and North Africa (MENA) region through Ministry of Manpower Regulation No. 260 of 2015. This ban was still in place at the time of writing.

Appendix Figure B.1). Moreover, growing restrictions may have increased the rate of risky, "non-procedural" migration—essentially, travelling abroad to work without proper documentation, approvals, and contracts in place. A recent World Bank (2017) report, for example, estimated that 39 percent of female migrants and 58 percent of male migrants were not fully documented.

Our project focuses exclusively on female migrants for several reasons that were informed by extensive qualitative work we conducted before designing the intervention, including interviews 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). 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 in Indonesia, as they are more vulnerable to exploitation and very poor job outcomes.

2.2 Intermediaries and the Migration Process

Mapping the Migration Process. Appendix Figure B.2 provides a high-level overview of the typical migration process for workers using placement agencies at the time of our experiment (2015-2019).[6] Conditional on taking a documented path, most migrants use placement agencies: 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).

Under the status quo, a potential migrant has three ways to connect to an 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 help individuals navigate the migration process, from securing necessary legal documentation to identifying an agency with open job postings. 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 can be difficult. 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. The amount of required training varies by job and destination country, but should be no less than 10 days unless the migrant is returning to a former employer. The migrant must also finalize her documentation during this time and pass a series of tests, including a job competency test and a health checkup. 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 migrant worker placement agency. These agencies partner with Indonesian agencies to identify and place migrant workers, with the destination country agency serving as a link between the migrant worker and her employer. While the worker is abroad, the Indonesian agency is responsible for providing assistance should problems arise and facilitating repatriation once a worker's contract comes to an end.

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[6]This workflow does not apply to migrants seeking employment through unofficial/non-procedural channels. Nor does it apply to higher-skilled migrants seeking employment through specialized programs, like government-to-government arrangements with South Korea and Japan. Non-procedural migration is common, especially among men and those migrating to Malaysia (World Bank, 2017).

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 given types of migrant labor (see McKenzie et al., 2014, for evidence from similar types of labor migrants from the Philippines). Moreover, the Indonesian government regulates placement and preparation costs levied by agencies (International Organization for Migration, 2010). 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.[7]

Figure 1 demonstrates the extent to which agencies might shape migration quality along monetary and non-monetary dimensions. We use baseline survey data that we collected in 2015 from 8,010 recently-returned female migrants living in 400 high-migration villages on the island of Java (see Section 5.2 for details). Panel A of Figure 1 focuses on the distribution of migrant earnings, captured by average monthly wages net of migration costs, Panel B on an index that averages 14 standardized measures of pre-departure preparation, and Panel C on an index that averages 16 standardized indicators of experiences abroad.[8] Higher values always indicate better outcomes.

Consistent with the institutional constraints described above, Panel A shows that monthly wages net of migration costs are relatively fixed within a given departure year and destination×occupation. This set of fixed effects explains 73 percent of the variation in net pay); adding agency fixed effects only raises the R^2 by 0.02. Meanwhile, the other, non-pecuniary aspects of quality follow a notably different pattern consistent with greater scope for agency influence. Agency fixed effects explain relatively more of the variation in pre-departure training (raising the R^2 from 0.05 to 0.19, panel B) and work experiences abroad (raising the R^2 from 0.12 to 0.20, panel C), even looking within narrowly defined destination×occupation cells.

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 B.3 uses government administrative

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[7](Fernando and Singh, 2021) find evidence of the same in Sri Lanka.

[8]The pre-departure preparation index includes a dummy variable indicating 10 days of training or more; dummies for whether the woman was trained on equipment needed to do the job, the required skills, information about the destination country, how to send money home, how to use insurance, how to behave on the job, destination country culture, how to get help if there is a problem, repatriation processes, migrant worker rights, and the migration contract; a dummy for whether the woman stated her job matched her contract; and a dummy for whether the woman signed a contract in the agency. The experience abroad index includes dummy variables indicating that the woman experienced: a day off, less than 12 hours/day of work, no on-the-job injuries, allowed to contact family, allowed to pray, was not fired, allowed to retain identity documents, no physical abuse, no sexual abuse, no verbal abuse, and no "other" abuse.

data from 2016-2019 to assess the number of agencies placing workers in control group villages and their market share during our intervention period (Section 5.1 details the data). It plots a histogram of the number of agencies per village 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.[9] The median village had 38 active agencies with a Herfindahl of 0.2, pointing to considerable competition. While some villages exhibit greater agency concentration (see the long tail in Panels A and B), there was ample scope for migrants in most villages to choose among different agencies at the time of our intervention.

2.3   What do Migrations Look Like?

We now draw on our baseline data to give a brief overview of migration experiences. Panel A of Table 1 reports demographic characteristics of sampled migrants. Given that Indonesia's Middle East and North Africa (MENA) moratorium substantially restricted migration to these destinations during our study period (see footnote 5), we report overall means, the mean for migrants returning from non-MENA destinations, and the difference vis-à-vis MENA migrants. The average migrant is 32, and over three quarters are married. Over half report primary education or less, and virtually none have post-secondary education. In line with this, 78 percent reported having housemaid duties, 29 percent reported nanny duties, and 32 percent reported elder care duties; 97 percent of women reported at least one of these. Fifty-six percent of migrants surveyed at baseline worked in the Middle East.

Panel B shows that agencies do not always fully prepare women to migrate. Just 50 percent reported getting at least 10 days of training, a conservative lower bound of the government mandate for most workers. Migrants reported receiving information on 62 percent of "priority" training topics. Panel C shows that migrants faced difficult working conditions: three-quarters of women reported no weekly day off, 55 percent reported more than 12 hour work days, and 20 percent reported an on-the-job injury. Seventy-eight percent reported their employer kept their identity documents, restricting freedom of movement, while 31 percent reported verbal abuse from employers, and 9 percent reported physical or sexual abuse.[10] Women who migrated to MENA have consistently worse experiences.

Panel D of Table 1 shows that while overtime pay is rare and salaries are often late, most women were paid the amount stipulated in their contract (just 14 percent reported salary cuts). Average monthly earnings net of migration costs were IDR 2.88 million (USD 221 at a 2015 exchange rate of IDR 13,000 per USD); for comparison, Indonesia's official monthly minimum wage (inflation adjusted to June 2015) was IDR 2.02 million (USD 155) in 2012, the median migration departure year.

Finally, Panel E shows that status quo knowledge of agencies was limited. Eighty two percent of women could not name an agency operating in the community (apart from the one they used for migration), only 34 percent reported getting advice on agency choice from the community, and 80 percent relied on their sponsor to select the agency. Further, just 29 percent of women interviewed at baseline believed that there is a relationship between agency and employer quality. In our formative qualitative research, migrants routinely told us that agency choice was not important, rather citing

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[9]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.

[10]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.

"luck" or "fate" as the primary determinant of a good migration experience. While there is certainly a large idiosyncratic component to experience, largely driven by employer quality, agency quality also plays an important role and was being overlooked at baseline.

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 core interventions aim to remedy information failures in two different ways: first, by explicitly providing new information on agency quality based on the experience of thousands of women spread across hundreds of migrant-sending communities; second, by encouraging migrants to act on already-available information and to engage in local search for information. To do this, we developed original measures of agency quality, as well as three information products (see Appendix Figure B.4 for examples).

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 recently-returned migrant, we developed an overall "migration quality" index, which included indicators related to her pre-departure training and experience at the agency, her experience (if any) at the partner agency in the destination country, net pay, and non-monetary aspects of job quality (time off, harassment from the employer, ability to contact family, etc.). We constructed empirical Bayes estimates of agency-specific components of this index, first residualizing out basic demographics including age, education, year of migration, and area of residence. To further minimize the influence of sampling variation, we only use grades for the 59 agencies who had at least 30 migrant experience ratings at baseline. These graded agencies account for 75 percent of placements in our baseline data.[11] Our agency grades do well in terms of out of sample prediction: the grades are significantly correlated with administrative data on problematic migration episodes in non-study districts, as well as cases of human trafficking recorded by the International Organization for Migration. Appendix D provides additional detail on the methodology, indicators included in the quality measures, and these out-of-sample validation tests.

Infographic.   We designed an "infographic" to facilitate a common understanding of the returns to migration and differences in returns across high- and low-quality placement agencies (Panel A of Appendix Figure B.4 shows the first of two pages). It included information on topics that our qualitative research indicated was important for migrants' well-being and of interest to potential migrants; this included training quality, quality of life, 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 agency quality distribution. The infographic used simple illustrations to represent differences in outcomes to ensure the information was easily accessible to those with limited education.

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[11]The number of female migrant placements among rated agencies from 2012-2013 was 1,243 compared to 422 placements for the 322 agencies with fewer than 30 observations in our baseline data.

Report Card. 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 to ensure the aggregate scores were well understood (see Appendix Figure B.4, Panel B). Each of the 101 subdistricts in our study had a unique report card, with each report card featuring agencies that operated in the local area. The report cards also included the name and contact information of each agency, as well as the number of reviews used to construct the agency rating.

Comic Book. 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 B.4 shows an example of one page). The comic tells the story of a woman deciding to migrate and explores how to navigate the process of choosing an agency when working with a sponsor. The woman in the story resists an offer from a sponsor offering placement with a low-quality agency and instead waits for an offer from a higher-quality agency. The story also explains what agencies are expected to do to prepare migrants, including the provision of training, insurance, and emergency assistance.

Randomization. We randomly divided our 400 study villages into four experimental arms, stratifying on (i) share of migrants placed in the MENA region (above/below median), (ii) agency Herfindahl index (above/below median), and (iii) number of female migrants per capita (above/below median). All placement information came from government administrative data in 2012-2013.[12] The 100 villages in the control group did not receive any of the intervention materials; we distributed both the infographic and the report card in 101 "report card only" villages; we distributed the infographic and the comic in 98 "comic book only" villages; and we distributed the infographic, the comic book, and the report card in 101 "comic and report card" villages. In these cross-randomized villages, the woman in the comic makes use of the report card to select a high-quality agency.

Intervention Implementation and Materials Distribution. Intervention materials were distributed over the course of interactive community meetings in treatment villages. The meetings were led by professional facilitators employed by a facilitation firm that worked with trained "local facilitators" who lived in the villages. Local facilitators were typically women identified by the village head. Most of these women had local leadership roles and were linked to the local migrant workers' union or an NGO working on migration. Community meetings were held three times in different places in the village. The meetings targeted former migrant workers and women aged 18-40 who were interested in migrating in the future. The meetings were promoted through print materials posted in the villages, public announcements (e.g., over mosque speakers), and word-of-mouth by facilitators. Facilitators made a special effort to invite women who were enrolled in our follow-up survey sample. Importantly, women did not know which types of intervention materials would be shared, and pre-session "marketing" was the same across all experimental 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.

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[12] We used Stata software to randomly assign villages to each experimental arm at a 1:1:1:1 ratio within each stratum.

In order to maximize treatment exposure and ensure the information remained salient, we re-distributed intervention materials to women in our follow-up survey who had not yet migrated. This took place during the fourth quarter of 2017, two years after the interventions. This distribution only targeted women participating in the follow up survey, and no group events were organized in the community (see Section 5 for more details on this survey and sample).

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 and the comic, which both aimed to address information frictions migrants face when selecting an agency. While community-based migration events and the infographic may have been important, these elements and the frictions they address are not the central focus of our experiment.

Conditional on group meetings and receipt of the infographic, the report card helped migrants discern which agencies were "good" and which were "bad", thereby facilitating *quality differentiation* among agencies. In so doing, the card also communicated additional information about the distribution of migration outcomes, which could have shifted migrant *beliefs about the return to migration*. While the comic book did not directly provide information about agency quality or migration outcomes, it did encourage migrants to seek information about agency quality, which could have increased effort to extract information from other community members. Thus, there is scope for both interventions to improve differentiation and shift beliefs. The following discussion focuses on these two channels and their implications for migration rates and experiences abroad.

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).[13] 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 (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 $u_t \in [\underline{U}, \overline{U}]$.[14] 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 $F_Q(\cdot)$.

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[13]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.

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

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 the effect of improving the migrant's screening technology, embodied in our experiment by either the agency report card or enhanced information flows within the community following the comic book discussion. We model this as a second signal available to the migrant, $r_t \in [\underline{R}, \overline{R}]$, which 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.[15]

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.

Even so, per Figure 2, the effect on the migration rate is ex-ante ambiguous, depending on how $F_{\tilde{Q}}(Q^*)$ and $F_Q(Q^*)$ relate to one another. 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 (increase the offer rejection rate). Intuitively, better screening technology increases the returns to search, reducing the per-period migration rate. 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.

In Appendix A.2, we extend these arguments, assuming that there exists a parameter σ, which indexes the riskiness of $F_Q(\cdot)$.[16] We then show that whenever 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 has the

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[15]Intuitively, this is because the distribution of $\tilde{Q}_t$ is more "spread out": for each value of Q_t, the migrant can further differentiate between firms by referring to r_t. See Appendix A.1 for details.

[16]We assume $F_Q(Q_t; \sigma)$ is continuously differentiable in both Q_t and σ.

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following effects: (i) the probability of migrating declines, (ii) the duration of search/time spent in Indonesia increases, and (iii) expected utility conditional on migrating increases. 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.[17]

In the report card treatment, only a subset of offers will generate an additional signal, r_t. Our model can accommodate this by assuming that $r_t = \emptyset$ and $\mathbb{E}[u_t \mid q_t, \emptyset] = \tilde{Q}_t = \mathbb{E}[u_t \mid q_t] = Q_t$ for some share of offers. A natural question is how this treatment impacts migration with graded agencies. The answer depends on the joint distribution of u_t, q_t, and r_t. If, for example, unrated agencies tend to be better than rated agencies and the report card usually leads to negative updating ($\tilde{Q}(q_t, r_t) < Q(q_t)$), use of top-rated agencies could even decrease. Conversely, the report card could increase use of poorly-rated agencies if migrants are not very choosy and the report card usually leads to positive updating.

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: $F_Q(x) = G_Q(x + \eta\Delta)$, $\eta \in (0,1)$.[18]

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]

Our most direct way to parse learning effects from differentiation is to assess impacts on beliefs: differentiation alone has no effect on women's priors, while a learning channel implies that priors should shift towards the information shock. In our empirical work, 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.

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[17]In both cases, a revealed preference argument implies that expected utility *unconditional* on migration must increase.

[18]This assumption captures Bayesian-style updating, where only part of the shift in the prior is passed to the posterior.

[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 were inaccurate, bringing priors in line with the truth will increase welfare, while distorting priors away from the truth will decrease welfare.

5   Empirical Strategy

This section describes our survey and administrative data as well as the core estimating equation. Additional detail on data sources and variable construction is available in Appendix C.

5.1   Administrative Data and Site Selection

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, dropping very large villages.[21] Appendix Figure B.5 presents a map of our study villages and their respective treatment assignments. We also use administrative data from 2015-2019 to assess whether treatments had aggregate impacts on village-level migration outcomes.

5.2   Survey Data

We conducted four rounds of surveys over the course of the project (see Appendix Figure B.6 for a project 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.[22] We enrolled two samples of women at baseline. First, a "tracking" sample of 4,805 women who stated they were interested in migrating in the near future. These women were targeted for survey in all future follow up rounds and comprise our core analysis sample. We stratified the tracking sample by migration history; thus 2,402 women had never migrated before while 2,403 had prior migration experience. Second, we collected data from an additional "former migrant" sample of 5,607 women who had previously migrated with an agency. We used data from these women to construct agency quality measures but did not target them for future follow up.

After intervention implementation in late 2015, the tracking sample participated in three follow up surveys. The first follow up was conducted between August and December 2016, the second between October and December 2017, and the final "endline" between May and October 2019. All three waves collected data on new migrations and women's experiences during those migrations. We also collected data on expectations and beliefs about migration. For women who were working abroad at the time of a follow-up survey, we conducted a phone survey that covered the same set of topics, but was shorter, as these women typically had less time for an interview. If a woman was not available to be interviewed (e.g.,

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[21]We dropped large villages with a population density of more than 4,000 persons per square kilometer or a total population of more than 15,000 to limit logistical complexity during intervention implementation. We also dropped one village initially sampled for inclusion in the study because the village head insisted on being involved in site and sample selection. This village was replaced by the next-largest village on our list.

[22]Among former migrants, we prioritized women who migrated with an agency and returned home in 2014 or 2015.

because she had migrated abroad and 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 above 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 were in our study villages at the time the endline was conducted and had departed for overseas work after the intervention began.[23] We use this sample to test for the reach of our intervention (since these women were not explicitly invited to attend community information-sharing meetings) and to test for spillover effects.

5.3 Estimating Equation

In order to identify the effect of easing information frictions (and permitting more differentiation between agencies), we need to estimate the impact of the report card holding all other experimental stimuli constant. Similarly, we would like to isolate the effect of the comic book conditional on other stimuli. We anticipate that both the infographic and the act of organizing the community meetings could affect migration behavior in multiple ways: the infographic could shift beliefs about the returns to migrating, for example, while the community meetings could make migration more salient, promote information sharing, or help women forge social connections that facilitated migration.

In order to difference out the effects of the meeting and the infographic, we adopt the following regression specification:

$$y_{iv} = \beta_0 + \beta_1 report_v + \beta_2 comic_v + \beta_3 infographic_v + \gamma_s + \varepsilon_{iv} \qquad (3)$$

where y_{iv} is the outcome of interest for woman i living in village v, $report_v$ identifies all villages randomly selected to receive a report card (both report card only and comic and report card villages), $comic_v$ identifies all villages randomly selected to receive the comic book, and $infographic_v$ identifies all villages that received an infographic/community meeting (i.e., all intervention villages). This fully saturated regression specification lets us focus on the treatment effects that map back to our theoretical framework: β_1 is the effect of the report card *conditional on distributing the comic book*, while β_2 is the effect of the comic book *conditional on distributing the report card*. The coefficient on the $infographic_v$ dummy is the negative of the comic book × report card interaction effect we would estimate in a more traditional fully saturated model. If we assume no complementarity or substitutability between the comic book and the report card, β_3 can be interpreted as the combined effect of the infographic and community meeting. We do not, however, believe this assumption is reasonable, especially since one goal of the comic book was to communicate the importance of paying attention to agency quality. We therefore refrain from extensive interpretation of β_3, though we report coefficients throughout the paper for completeness.

Appendix Table B.1 presents descriptive statistics for the tracking sample control group and uses specification (3) to verify 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

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[23]These respondents were identified through a listing exercise using key informants and snowball sampling.

expected to work 12 hours a day and report a fifty percent chance of having a day off. Twenty-two percent expect to experience physical abuse. Compared to summary statistics for former migrants in Table 1, tracking sample women believe they will get more time off and earn more, but are at greater risk of abuse. Overall, characteristics are well balanced across arms with just one of 19 joint tests that $\beta_1 = \beta_2 = \beta_3 = 0$ rejected at the 10 percent level or better.

Appendix Table B.2 reports follow-up rates for the survey rounds. High rates of migration meant we were only able to interview 64-67 percent of women in person in the three follow-up rounds of data collection. However, we successfully contacted 74-83 percent of women either in person or by phone (we term these "direct interviews") and 97-99 percent of respondents once informant interviews are included. While there are no significant differences across treatment in "any interview" rates, the report card is associated with a 4-5 percentage point (p.p.) higher rate of in-person interview at follow up 1 and 2. There were no differences in interview rates across the treatment arms in follow-up 3. Given this, whenever possible we rely on data from the final follow up for outcomes other than the migration rate, which is easily reported by informants.

6   Main Results

This section presents our core findings on migration outcomes. After demonstrating the successful diffusion of information within treated communities, we show how the report card, and to a lesser extent the comic, reshaped migration choices. We then examine impacts on migration experiences and show that the improvements observed cannot be explained by changes in migrant selectivity. We close with a discussion of robustness and alternative explanations.

6.1   First Stage: Exposure to Intervention Materials

On average, roughly 94 women per village attended community meetings in treatment villages in late 2015. While many of these meetings attracted large numbers of women interested in migrating, their scale was small relative to the size of most villages, which, on average, were home to around 2,000 women aged 18+. In Table 2, we examine self-reported exposure to information materials among tracking sample participants up to four years after the community-based intervention in late 2015. These results are important not only for interpreting the treatment effects on migration choice and experiences but also in clarifying the scope for general equilibrium effects.

Panel A reports exposure among the tracking sample during the second follow up, which took place two years after the intervention. We limit attention to women interviewed in person or over the phone. Column 1 shows that 19 percent of women recalled attending a community meeting on migration in 2015 (control mean+β_3), with no significant difference across treatment arms. Recall of the infographic is similar, ranging from 11 percent in the comic only arm (control mean+$\beta_3 + \beta_2$) to 15 percent in the report card plus comic arm (control mean+$\beta_3 + \beta_2 + \beta_1$). Women in villages where the report card was distributed were 11 p.p. more likely to recall the report card than those in other villages (column 3). Likewise, women in villages where the comic was distributed were 15 p.p. more likely to recall the comic (column 4). In control villages, roughly 4 percent of women reported recognizing all types of intervention

materials, which could be due to incorrect recall or exposure to initiatives by other actors like NGOs.

Panel B reports exposure during our third follow up, or 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 follow up 2.[24]

Finally, Panel C of Table 2 uses the supplementary sample to understand the reach of our intervention materials in treated communities. Unlike the tracking sample, these women did not receive encouragement to attend the community meetings in 2015 and did not receive supplemental distribution of materials in 2017. Women in the supplementary sample rarely recognized intervention materials assigned to their community: less than 5 percent of these women recalled attending a community meeting on migration, and we find no significant 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, the results in Panel C suggest that our intervention's reach was somewhat limited and concentrated among the tracking sample, making village-level market-wide effects unlikely. Given this, we focus on the tracking sample for the rest of our main analysis.

6.2 Migration Choice, Intentions, and Beliefs

Table 3 uses the tracking sample at follow up 3—including both direct and informant interviews—to establish our core findings on the migration rate. We focus on departures that took place from October 2015, corresponding to migration following the community meetings in treatment villages. Column 1 shows that nearly four years after these meetings, the report card (conditional on access to the comic and infographic) reduced the international migration rate by 4.4 p.p. This represents a 12 percent reduction in the likelihood of migration relative to women in the control group, where 36.6 percent had migrated by endline.[25] This effect is more than twice as large as the effect of the comic (2.3 p.p., not statistically different from zero). The report card mostly reduced migration with, rather than without, an agency as seen in columns 2 and 3, respectively. The most intensive intervention (comic + report card) did, however, reduce migration without an agency by 1.5 p.p., a 37 percent reduction in potentially risky, undocumented migration relative to the control group mean of 4.1 percent.

We use reports of migration month and year to explore the timing of treatment effects in Figure 3. The graphs report dynamic treatment effects based on the panel analogue of equation (3) with a full set of monthly interaction terms. The report card significantly reduced migration by around 2 p.p. by January 2016, 3-4 months after the rollout of community meetings. This timing is plausible given the several month lag between initial migration intent and eventual departure. The negative effects stabilized around 2.5 p.p. in mid-to-late 2016, only to start falling again around mid-2018. This significant jump down occurred roughly six months after the second follow up survey (see shaded bar on graphs), when we reintroduced the information materials to tracking sample respondents. This suggests

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[24]Relative to the second followup, 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.

[25]These high rates of migration among the tracking sample stand in contrast to another recent study of international migration in a similar population of low-skill women in the Philippines (Beam et al., 2016).

two possibilities: either the report card had a significant effect on migration behavior even absent the in-depth facilitation during community meetings, or the followup visit was important for keeping the report card top of mind. Panel B reports similar dynamics for the comic, but, like the endline estimate in Table 3, these treatment effects are smaller and never significantly different from zero.

While these findings are consistent with women revising upwards their reservation expected utility for accepting an offer, they are also consistent with a downward shift in beliefs about the return to migrating. We present a first test of this hypothesis in column 4 of Table 3. This specification limits the sample to directly interviewed non-migrants and reports treatment effects on intention to migrate. If the report card made migration less attractive, more non-migrants exposed to this intervention should report having no interest in migrating in the future. The point estimates on both the report card and the comic are very small (0.2 p.p. or less), although the standard errors are non-trivial (0.027 and 0.024, respectively). Appendix Table B.3 takes a deeper look at this hypothesis by considering plans to migrate in the next year (arguably a better proxy of "serious" migration intentions) and concrete steps taken to migrate, including securing family and village head permission, choosing a sponsor, and choosing an agency. Point estimates are very small, never significant, and almost always positive. Taken together, there is no evidence that the intervention materials deterred potential migrants.

Even if the report card did not deter potential migrants, it could have made them more pessimistic, making them choosier when considering offers and thereby reducing the migration rate. Table 4 tests this hypothesis by studying treatment effects on beliefs, again restricting the final follow up sample to directly interviewed non-migrants.[26] We consider three indices capturing beliefs about various aspects of the migration experience pre- and post-departure. The *agency* index comprises beliefs about seven aspects of the pre-departure experience with the agency (e.g., whether the agency provides information on migrant worker rights, provides quality food). 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 provided to all treatment villages.[27] For each underlying question, women were asked at endline to assess the likelihood of a given binary outcome based on a 0-10 scale (0 indicating no chance, 10 indicating certainty), first for themselves and then for other women like them in their community. For each outcome, we follow Kling et al. (2007) in using the mean and standard deviation of the control group to construct standardized indices across the component measures, first recoding each component so 0 represents the worst possible expected outcome and 10 the best possible expected outcome.

We see little evidence of any systematic treatment effects on beliefs in Table 4, either about one's own potential migration experience (columns 1-3) or those of others (columns 4-6). The estimated null

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[26]We restrict the sample to non-migrants because the experience of migrating could dramatically shift beliefs. Results are not substantively different when including migrants in the sample, however.

[27]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 residence, 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 full 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 the aforementioned measures that have an asterisk.

effects are quite precise, with standard errors rarely being larger than 0.03 standard deviations. In short, neither the report card, comic, nor infographic moved average beliefs about the migration process. This could be because our sample villages are some of the biggest migrant-sending villages in Indonesia, meaning women have had ample time to learn about average migration outcomes.

Another possibility is that the treatments reduced the variance in beliefs (e.g., making optimists less optimistic and pessimists less pessimistic). However, Appendix Table B.4 shows that our results are unchanged when considering women with above-median (Panel A) and below-median (Panel B) baseline beliefs separately.[28] Similarly, Appendix Figure B.7 shows that the distribution of beliefs are very similar across treatment arms; here, we focus on comparing women in comic only villages to those in report card plus comic villages to illustrate the report card's lack of effects.

Despite conveying novel information about potential risks and uncertainties, neither the report card nor the other materials changed beliefs about migration experiences or intentions to migrate in the future. Rather, the combined evidence in Table 3, Figure 3, and Table 4 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 by providing evidence of why individuals may be waiting longer to migrate, namely that the intervention materials help women secure higher-quality offers that translate into a better migration experience.

6.3   Migration Experiences

The model in Section 4 predicts that when the migration rate decreases, those who do migrate should have better migration experiences. We test this prediction in Table 5, which restricts attention to women who migrated by the time of the final follow up.

To measure migration quality, we construct three indices capturing distinct families of outcomes: pre-departure preparation (the most direct indicator of improved agency quality), job quality (which captures non-pecuniary job amenities), and pay. Again, we follow Kling et al. (2007) in standardizing outcomes relative to the control mean and standard deviation.[29] Appendix Tables B.5-B.7 report impacts on individual index components.

Table 5 shows that the report card and the comic improved migration experiences both in terms of pre-departure preparation within Indonesia and job amenities. Conditional on access to the comic, the

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[28]We calculate medians by sub-district, since each of the 100 sub-districts in our sample received a unique report card.

[29]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.

report card increased the pre-departure preparation index by 0.17 standard deviations (s.d.), including a 2 week increase in time spent on training and a 10 p.p. increase in signing a contract, while the comic increased quality by 0.11 s.d., including an 8 p.p. increase in signing a contract. These quality improvements extend to the migrant's time abroad: the report card and the comic led to a 0.10-0.11 s.d. increase in job quality/on-the-job amenities, including an 8 p.p. increase in the likelihood of getting a weekly day off (report card) and a 10 p.p. increase in the likelihood the migrant was able to retain her identity documents (comic book). Together, these results suggest that the reduction in information frictions led to marked improvements in women's experiences throughout the migration process.[30] Report-card- and comic-associated gains do not extend to pay, however, which was largely unmoved by the information intervention.

These improvements line up with the parts of the migration process that agencies have the most scope to influence. Recall from the discussion of Figure 1 that there is little variation in migrant pay across agencies for women going to a given destination to work in a given occupation. However, there is much more variation in non-pecuniary dimensions of the migration process, especially at the pre-departure preparation phase. It is therefore reassuring to see that the report card and comic affected margins of migration quality that are most easily influenced by agencies. That said, some of the improvement in job quality may come from working in destinations or occupations that require more pre-departure preparation or offer better employment conditions (recall from Table 1, e.g., that women working in MENA experience notably worse outcomes). In practice, we find little evidence of sorting towards higher-quality migration pathways (see Appendix Table B.9). Neither the comic nor the report card led to shifts in destinations or occupations associated with higher returns (i.e., Asian countries and formal, non-household-bound jobs). This suggests that much of the observed variation in realized migration experiences may come from the agency itself rather than the types of jobs or destinations that the agency may specialize in. Later, we address a confounding explanation based on migrant selectivity.

Another way to assess impacts on migration quality is to look directly at the quality of agencies used for realized migrations. Since not all agencies used at endline were rated at baseline, we group agency choice into six classes: migration without an agency, migration with a "high-grade" agency (top third of grade distribution), migration with an "average" (middle third) agency, migration with a "low-grade" (bottom third) agency, migration with an agency that was not graded but was on the Indonesian government's list of approved placement agencies (an "ungraded agency"), and migration with an "unknown agency". This last category includes all women who either (i) reported that they used an agency but did not know what its name was, or (ii) provided an agency name that was not on the list of government-approved agencies. We anticipate that unknown agencies are more likely to be unsanctioned, low-quality operations that operate outside the government's purview.

To validate these classifications, Appendix Table B.10 shows how migration outcomes vary by agency choice. For this analysis, it is important to focus on directly interviewed women, as informants (family members of migrants) had limited information on agency names; only 24 percent of informants could recall the agency name, and informant-migrant cross reports, when available, suggest informant reports are low quality. To avoid confounding due to treatment, we limit the analysis to tracking sample women

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[30]These differences persist when we limit attention to women who migrated with an agency (see Appendix Table B.8).

in the control group and the entire supplementary sample.[31] The omitted group is always high-grade agencies. The first three columns consider differences in outcomes absent other controls. Women who migrated with unknown and low-grade agencies fare significantly worse than women who migrated with high-grade agencies across all dimensions of experience, while women migrating with ungraded agencies and no agency fare worse in terms of pre-departure preparation and pay. Average agencies appear to be slightly worse in terms of pre-departure preparation, but otherwise not very different from high-grade agencies. These patterns are virtually unchanged when we add controls for migrant age, marital status, and education (columns 4-6), suggesting differences do not simply reflect sorting by human capital. These patterns remain after adding controls for occupation and destination (columns 7-9), but point estimates attenuate slightly. Overall, this suggests that our classifications capture meaningful differences in agency quality, though some of this may be driven by the type of jobs offered by different agencies. Unknown agencies appear to be as bad, if not worse than, low-grade agencies, while ungraded agencies appear to be worse than high-grade agencies but better than low-grade agencies.

Table 6 reports treatment effects on agency choice. To begin, we reproduce the baseline finding that the report card reduced the migration rate among the sub-sample of directly interviewed women (Panel A, column 1). The core extensive margin result is essentially unchanged (a 5.4 p.p. reduction in migration due to the report card when limiting to direct interviews, compared to 4.4 p.p. when including informant reports). In contrast to the results in Table 3, women are less likely to migrate without an agency (Panel A, column 7). The treatment effect (-1.9 p.p.) is larger than the estimate we obtain with the full sample (-1.1 p.p.), possibly because informants may have been more prone to misreport the given migrant's use of an agency. This type of measurement error can lead to attenuation bias when the outcome is binary (Meyer and Mittag, 2017).

Table 6 also shows that women are 3.0 and 1.1 p.p. less likely to migrate with unknown and average agencies, respectively. Use of high- and low-grade agencies, as well as ungraded agencies is unchanged.[32] Panel B of Table 6 reports results conditional on migrating: the report card shifts the composition of migrations towards ungraded agencies (an increase of 11 p.p.) and away from unknown agencies. The fact that the report card does not affect use of high-grade agencies suggests offers from these agencies are rarely close to women's reservation quality thresholds. Put another way, if these offers were almost always accepted even without the report card, treatment would leave their incidence unchanged.

More generally, it is striking how few women migrate with graded agencies. Among migrants, just 15 percent migrated with a graded agency, compared to 46 percent with ungraded agencies and 26 percent with unknown agencies. Since the majority of women interviewed at baseline migrated with graded agencies, this likely reflects a shift in market structure, possibly spurred by Indonesia's moratorium on sending migrants to MENA countries. Yet despite this, migrants still managed to secure better quality placements. This is a novel implication of the reservation utility mechanism in our model: by making women universally choosier, the report card induced women to turn down low-quality offers, *regardless of whether they were on the report card.* Put differently, provided the report card shifted reservation

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[31]We include the supplementary sample for power purposes, given that very few were exposed to intervention materials. Results are essentially unchanged when we just use the control group.

[32]To provide more granularity, Appendix Figure B.8 limits the sample to the 103 women in the comic only and comic plus report card arms who migrated with a graded agency and graphs the cumulative distribution function (CDF) of agency grades: overall, the distributions are very similar and not significantly different.

utility, there is scope for it to improve placements even when offers from graded agencies are uncommon.

6.4 Does Selection Drive Quality Improvements?

Together, the evidence in Tables 5 and 6 suggests that information frictions may constrain migrants to hastier departures with agencies offering less pre-departure preparation and jobs with lower (expected) non-pecuniary benefits. An important question, then, is whether this result reflects selection versus an improvement in outcomes for "always taker" migrants. For example, the report card could have crowded out low human capital migrants, who would have counterfactually received less training and worse jobs. Recall that neither the comic nor the report card led to shifts in occupational or destination choices that might otherwise be associated with selection based on migrant "quality" (see Appendix Table B.9). While this provides some initial evidence against a purely selection-based interpretation, it is not definitive. In the remainder of this section, we use two complementary approaches to provide more direct evidence that selection plays a negligible role in explaining the positive effects of the information materials on migrant experiences pre- and post-departure. We also provide evidence that directly supports a shift to higher quality agencies conditional on migrating.

A Model-Driven Selection Correction Procedure. Our first approach uses 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 woman's 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:

$$Q_{ia} = \mathbf{x}_i'\boldsymbol{\zeta} + \gamma_a + \varepsilon_i \geq Q^* + \eta_i \tag{4}$$

Conditional on migrating, realized utility is:

$$y_{ia} = \mathbf{x}_i'\boldsymbol{\zeta} + \gamma_a + \lambda_a + \varepsilon_i + \nu_{ia} \tag{5}$$

where λ_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 $\delta_a = \gamma_a + \lambda_a$. Then, expected utility among women who migrate is a function of observable characteristics, agency choice, and selection on unobservables:

$$\mathbb{E}[y_{ia} \mid Q_{ia} \geq Q_i^*; \mathbf{x}_i, a] = \mathbf{x}_i'\boldsymbol{\zeta} + \delta_a + \mathbb{E}[\varepsilon_i \mid Q_{ia} \geq Q_i^*; \mathbf{x}_i, a] \tag{6}$$

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

When the report card *is* available, two things change: women have a more precise signal of agency quality and their reservation utility increases. This will change selection in terms of who migrates (as the bar is higher conditional on expected agency value add), and will also change expected experience

conditional on migrating (through the agency effect). Then the difference in average outcomes between women migrating with and without the report card is:

$$\Delta\mathbb{E}[y \mid \text{migrate}] = \Delta\mathbb{E}[x]'\boldsymbol{\zeta} + \Delta\mathbb{E}[\delta] + \Delta\mathbb{E}[\varepsilon \mid \text{migrate}] \tag{7}$$

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 a component that is due to changing observable characteristics of migrants ($\Delta\mathbb{E}[x]'\boldsymbol{\zeta}$), another due to changes in the quality of selected agencies ($\Delta\mathbb{E}[\delta]$), and a term that reflects 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

$$\mathbb{P}(\text{migrate}_i) = \mathbb{P}(\eta_i - \gamma_a - \varepsilon_i \leq \mathbf{x}_i'\boldsymbol{\zeta}) = \Phi(\mathbf{x}_i'\boldsymbol{\zeta}) \tag{8}$$

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

$$\mathbb{E}[y_{ia} \mid \mathbf{x}_i, a] = \mathbf{x}_i'\boldsymbol{\zeta} + \delta_a + \rho\sigma\lambda(\mathbf{x}_i'\boldsymbol{\zeta}) \tag{9}$$

where $\lambda(\cdot)$ is the inverse Mills ratio, $\rho = \text{corr}(\eta_i - \gamma_a - \varepsilon_i, \varepsilon_i + \nu_{ia})$ and $\sigma = \text{sd}(\varepsilon_i + \nu_{ia})$. Joint normality is a strong assumption, and, as written, is doubly unattractive because the second stage is identified off functional form alone. To address this, 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.[33]

Once we have unbiased estimates of $\boldsymbol{\zeta}$, δ_a, and $\rho\sigma$ we can perform the decomposition by 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 identify the relative contribution of each component mechanism in explaining the treatment effects reported in Table 5.[34]

Table 7 reports decomposition results for the three migration experience indices, using agency fixed effects to estimate $\Delta\mathbb{E}[\delta]$.[35] The $\mathbf{x}$ vector comprises a large set of individual characteristics, including

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[33]This assumption 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. Given the nature of agency placement and training, as well as the explicit focus of the report card on agency quality, we consider this restriction reasonable and also more appealing than using individual or village characteristics in the first step.

[34]We calculate standard errors by generating 500 bootstrap samples (we resample entire villages to account for clustering, within each randomization stratum); for each sample we 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 5 because we do not partial out the randomization strata in the decomposition exercise.

[35]In Appendix Table B.14 we report results that instead using dummies for no agency, ungraded agency, low-grade agency, average agency, and high-grade agency (the omitted group is unknown agency). This approach avoids over-attributing agency contributions based on a large number of sparse fixed effects, but runs the risk of under-attributing contributions

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 migration search experience, and beliefs about migration experiences.

From column 1 of Table 7, it is clear 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 (Panel A, column 1) and half of the changes attributed to the comic book and infographic (although the latter effects are statistically insignificant). Agency choice also accounts for roughly half of the report card's impact on the job quality index (column 2, not significant). In contrast, we find little role for selection into migration, either in terms of observables or unobservables. Rather, the remainder of the decompositions tend to load on residual differences, which could indicate that we have not fully accounted for either key migrant characteristics, a concern our next approach attempts to address.

A Machine-Learning Approach to Counterfactual Migration Experiences. In a second, less parametric, approach to quantifying selection, we use machine learning techniques to develop a measure of predicted migration experience based solely on pre-treatment survey data. Concretely, we predict experiences by training a random forest on a broad set of individual-specific covariates measured during the baseline survey (see Appendix C 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.[36] We then use this model to predict future migration experience for women in the tracking sample. This procedure delivers a measure of potential migration experience that is (i) highly predictive of actual migration experiences at endline (see Appendix Figure B.9), and (ii) balanced across treatment arms at baseline (columns 1 of Table 8).

Looking across the remaining columns of Table 8, we see little evidence that the report card or comic induced selective migration of women with better ex-ante potential. It is also reassuring that there are not substantive differences in predicted experience when comparing the full sample of migrants to those interviewed directly. The estimated effects (measured in standard deviation units) are quite small with tight confidence intervals (standard errors less than 0.01), especially compared to the estimated treatment effects on actual migration experiences in Table 5.

In sum, both the more structured decomposition and the machine learning approach suggest that the report card did not substantively alter 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 similar women have better migration experiences by avoiding low quality agencies.[37] It is therefore not surprising that conditioning on predicted experience in a given domain leaves the treatment effects on actual experiences in that domain unchanged (compare Appendix Table B.11 to Table 5).

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since the agency categories are coarse.

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

[37]The lack of selection effects is not entirely surprising: although our model predicts the report card should discourage women from taking lower-quality offers, higher human capital women may also have higher outside options in Indonesia. Thus, those selected out could be drawn from across the skill distribution.

6.5 Impacts on Downstream Outcomes

Evidence so far points to the possibility that the report card effectively made migrants "choosier" and, in so doing, induced migrants to avoid offers from low-quality agencies. It is striking that this happened despite the diminished market share of graded agencies in the post-baseline period. One concern is that this could lead to welfare losses, rather than gains, e.g., if potential migrants spend time waiting for high-quality offers that never arrive, forgoing the significantly higher wages associated with migration. In order to address this concern, we collected detailed data on household-level measures of income, assets, and vulnerability at endline.[38] We also collected data on individual employment status, to understand what women do instead of migrating.

Appendix Table B.12 reports impacts on a household's economic condition. Column 1 reports treatment effects on average monthly household income, which includes individual wages and business profits, the value of agricultural production less costs, remittances, and other income (government transfers, rental income, etc.). Treatment effects are small and never significant. In the control group, a monthly income of IDR 4.2 million translates to roughly US\$ 301 . The coefficient on the report card translates to a \$8.55 reduction, or just 3 percent of the control group mean. We see similarly small treatment effects on household expenditures and food expenditures (columns 2 and 3, respectively). We also see no significant effects on standardized indices measuring housing quality, asset ownership, and dependence on social protection.[39] The lack of effects on assets and housing quality are especially notable, since many migrant-sending households use remittances for home improvement.

Appendix Table B.13 studies impacts on occupation and earnings of women in our sample at endline. Women who received both the comic and the report card are 6 p.p. more likely to be self employed, a substantial shift 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 on a migration at the time of the surevy, suggesting women substituted towards self-employment from various alternatives. Column 7 reports treatment effects on monthly income for the sub-sample of directly interviewed respondents. As with household income, treatment effects are small (less than 5 percent of the control group mean for the report card and comic) and not significantly different from zero. Overall, there is no evidence that foregone migrations led to significantly worse economic outcomes for sampled women and their households. While this analysis is subject to power limitations,[40] our model points to a second reason why this might be: the marginal women who delayed migration due to the report card may have had relatively less to gain from migration in the first place.

6.6 Robustness

We discuss here a few key points of robustness with results available in the Appendix.

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[38]These modules were administered to informants if the migrant was not available for direct interview.

[39]Indices are constructed using factor analysis, with outcomes defined so higher values indicate better outcomes. We standardize each principal component relative to the control group mean. See table footnotes for details on components.

[40]Recall that the report card reduced the migration rate by just 4.4 p.p., meaning migration would have to have extremely large impacts on outcomes to be detectable.

Baseline Imbalance on Prior Migration Experience. Appendix Table B.1 shows that there is moderate imbalance of prior migration experience across different intervention arms, with women in the report card arm (conditional on comic access) less likely to be former/experienced migrants. This is despite the fact that our original sampling strategy specified enrolling six novice and experienced migrants in each village.[41] This runs the risk of overstating the report card's negative effect on migration because experienced migrants are more likely than novices to migrate after the baseline. To assess robustness, we re-weight the sample to match our original sampling plan (6 novice and 6 experienced migrants in each village). Appendix Tables B.15-B.18 report main results. Overall, our findings are unchanged: treatment effects on the migration rate increase in magnitude, if anything.

Confirming a Lack of Village-Wide Effects. Recall from Table 2 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 Tables B.19 and B.20, which use administrative placements data from October 2015 to December 2019 to estimate treatment effects on total formal migration and use of graded agencies by quality. Overall, we find no substantive impacts on village-level migration flows or use of high/medium/low-grade agencies.

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 B.21 and B.22 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 increase in use of ungraded agencies and pre-departure preparation, while the comic is associated with more use of low-grade agencies and a reduction in the job quality index. Taken as a whole, this suggests the spillover effects were muted, relative to the direct effects of information provision.

Accounting for Non-Response. Despite our very high rates of follow up when including informant reports, we were only able to directly interview 79 percent of the tracking sample at endline. This does not affect our top-line findings regarding impacts on the migration rate, but it does pose issues for interpreting our treatment effects on agency choice and migration experience. Our biggest concern is differential selection into direct interview by treatment. Fortunately, direct interview rates are similar and not significantly different by treatment (see Appendix Table B.2). The same holds when restricting to migrants (see Appendix Table B.23). Appendix Table B.24 tests whether there is any evidence of differential selection on observables by treatment arm. Here, we focus on all international migrants, and regress baseline outcomes on an informant interview dummy, treatment dummies, and their interaction with the informant interview dummy. We do find some evidence of differential selection into informant

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[41]The imbalance stems from replacement sampling: in some villages the field team struggled to find 6 migrants of the relevant type. To address this, additional women were enrolled in villages with surplus survey candidates. We instructed the field team to draw replacements from the same treatment group, but this protocol was not always followed.

interview by treatment arm. Most notably, women in the report card arm (conditional on access to the comic) who were not directly interviewed score marginally worse on the "basic covariates" random forest experience prediction, are less likely to be single, more likely to have been planning a migration to MENA at baseline, have lower salary expectations, and expect fewer days off and less risk of abuse.

This raises concerns that our treatment effects on migration experience could be biased, as directly interviewed migrants are not representative of the full population of migrants. To address this, we construct inverse probability weights, using the full set of characteristics in Table B.24, with which we re-weight both direct- and informant-interview women to match the mean of the overall sample of migrants. We do this by treatment arm, in an effort to recover means representative of all migrants in a given arm. Appendix Table B.25 reproduces Appendix Table B.24 with these weights, verifying that the weights address differential selection on observables by interview status. Appendix Table B.26 reproduces our core experience results using these weights. The results are virtually unchanged, suggesting that non-response does not pose a major issue for estimated treatment effects on experience.

7 Conclusion

We use a large-scale randomized controlled trial, spanning 400 villages in Indonesia, to study how information frictions in the international migration market shape the choices and experiences of prospective female migrant workers. Access to supplementary 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 in the future unchanged. Moreover, women who did migrate received better pre-departure preparation and enjoyed jobs with better non-pecuniary benefits. We also find that exposure to a comic book, which articulated the importance of choosing a high-quality agency and encouraged information acquisition, improved pre-departure experience and job quality. There is no evidence that the reduced migration rate led to long-term economic losses for sampled women or their households.

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 very limited, all migration offers look more or less the same, meaning a 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 both without an agency and with poorly documented "unknown" agencies, consistent with marginal migrations being riskier and more reliant on low-quality intermediaries. Even so, the *individuals* who defer their migrations are not substantially different from other migrants: we take multiple approaches to studying selection and find that the report card had little impact on the types of women who migrate.

We also rule out the possibility that the report card shifted women's 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 driving low-quality agencies out of the market. This is unlikely, given that (i) we do not observe any village-level treatment effects on the migration rate (consistent with the idea that firms turned down by directly treated women were easily able to find a "replacement" worker in the same village), and (ii) we find limited impacts on the migration outcomes of women in a supplementary sample, who were rarely directly exposed to our interventions.

One lingering question is why the comic book improved migration outcomes, despite its limited impact on the migration rate and agency choice. While we do not observe statistically significant effects on these outcomes, point estimates are generally in line with the effect of the report card, just smaller in magnitude. It is therefore possible that relatively modest shifts in our coarse measures of agency choice still translated into meaningful impacts on migration experience. In terms of mechanisms, the comic may have encouraged information acquisition through village networks, improving agency choice in a way our relatively coarse agency classifications failed to detect.

Taken as a whole, our results suggest that prospective international migrants in Indonesia face substantial information frictions when selecting placement services. We provide novel, well-identified evidence that remedying information frictions on the job seeker side can induce prospective migrants to engage in a slower, more deliberative search process that ultimately connects them to higher-quality service providers. This can have important welfare consequences, especially in settings like ours, where job seekers are often vulnerable to mistreatment at the hands of their employers. While our experiment was not well-positioned to generate or identify general equilibrium effects, our results 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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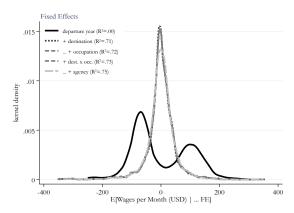
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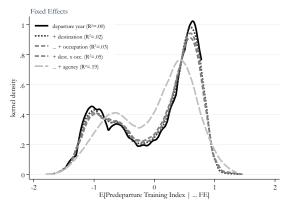
Tables and Figures

Figure 1: Variation in Pecuniary and Non-Pecuniary Dimensions of Migration Quality
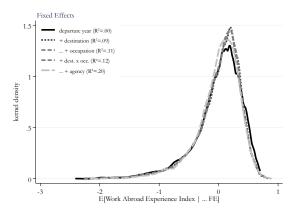
A. Average Monthly Wages
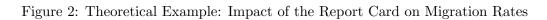
B. Predeparture/Training

C. Work Abroad

Notes: Sample limited to women interviewed at baseline who migrated in 2011 or 2012, in agency and destination×occupation cells of 2 or more.

Figure 2: Theoretical Example: Impact of the Report Card on Migration Rates
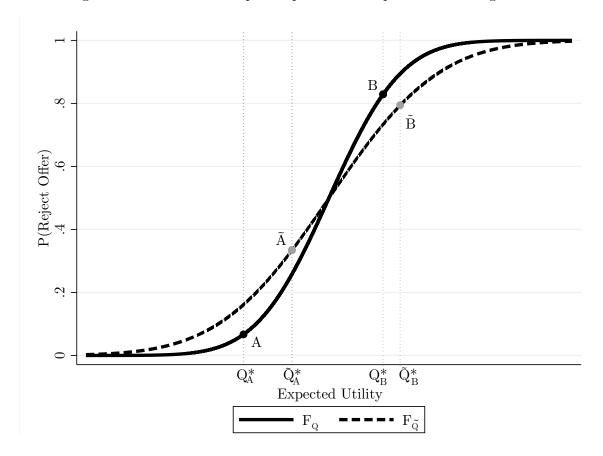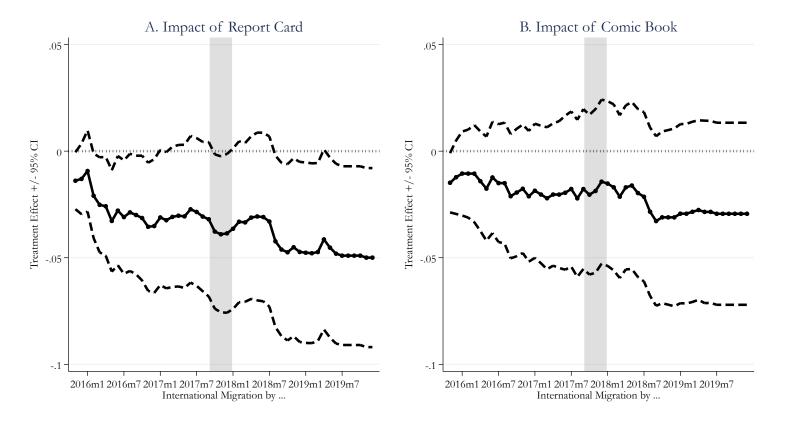
Figure 3: Impact of Report Card and Comic Book on Migration Rate Over Time

A. Impact of Report Card

B. Impact of Comic Book

Notes: 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. Estimates use regression equation 3, 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.

Table 1: Baseline Sample: Average Migration Outcomes

	(1) Overall Mean	(2) Non-MENA Mean	(3) MENA Difference	(4) N
Panel A: Characteristics of Women and Jobs				
Age	31.932	29.580	4.231***	7002
	[6.848]	[6.096]	(0.173)	
Married	0.778	0.719	0.106***	7006
	[0.416]	[0.450]	(0.011)	
Primary or Less	0.553	0.290	0.474***	7005
	[0.497]	[0.454]	(0.012)	
Any Secondary	0.443	0.704	-0.469***	7005
	[0.497]	[0.457]	(0.012)	
Post-Secondary	0.003	0.006	-0.005***	7005
	[0.056]	[0.078]	(0.001)	
Worked as Housemaid	0.782	0.612	0.306***	7006
	[0.413]	[0.487]	(0.012)	
Worked as Nanny	0.293	0.194	0.178***	7006
	[0.455]	[0.396]	(0.012)	
Worked as Elder Caretaker	0.316	0.585	-0.484***	7006
	[0.465]	[0.493]	(0.012)	
Worked in MENA Country	0.556	0.000	(1.000)	7004
	[0.497]	[0.000]	(0.000)	
Panel B: Experiences with Agency				
10+ Days Training	0.495	0.631	-0.245***	6996
	[0.500]	[0.483]	(0.014)	
Fraction Priority Topics Trained On	0.622	0.646	-0.043***	7003
	[0.389]	[0.376]	(0.013)	
Job Matched Contract	0.782	0.830	-0.088***	6964
	[0.413]	[0.375]	(0.011)	
Dealt with Agency in Destination	0.861	0.978	-0.211***	6998
	[0.346]	[0.147]	(0.009)	
Panel C: Experiences with Employer				
No Weekly Day Off	0.746	0.632	0.206***	7000
	[0.435]	[0.482]	(0.012)	
Work >12 Hours/Day	0.551	0.460	0.164***	6999
	[0.497]	[0.498]	(0.013)	
Injured on the Job	0.199	0.177	0.038***	7002
	[0.399]	[0.382]	(0.010)	
Not Allowed to Contact Family	0.107	0.107	0.000	7002
	[0.309]	[0.309]	(0.008)	
Employer Retained Identity Documents	0.778	0.669	0.198***	7000
	[0.415]	[0.471]	(0.014)	
Experienced Physical or Sexual Abuse	0.094	0.061	0.059***	6983
	[0.292]	[0.239]	(0.007)	
Experienced Verbal Abuse	0.307	0.265	0.074***	6975
	[0.461]	[0.442]	(0.012)	
Panel D: Compensation				
Paid Overtime	0.321	0.591	-0.486***	6996
	[0.467]	[0.492]	(0.010)	
Salary Paid Late	0.395	0.399	-0.007	6989
	[0.489]	[0.490]	(0.013)	
Salary Cut	0.142	0.069	0.132***	6987
	[0.349]	[0.254]	(0.008)	
Net Earnings, Millions IDR	2.878	3.482	-1.086***	6735
	[1.358]	[1.459]	(0.038)	
Panel E: Knowledge and Beliefs				
Cannot Name Agency (Apart from Own)	0.817	0.795	0.038***	6983
	[0.387]	[0.403]	(0.012)	
Received Advice on Agencies From Community	0.336	0.408	-0.128***	7001
	[0.473]	[0.491]	(0.012)	
Sponsor Selected Agency	0.799	0.715	0.153***	7004
	[0.400]	[0.452]	(0.011)	
Believes Agency and Employer Quality Related	0.292	0.348	-0.100***	6960
	[0.455]	[0.476]	(0.012)	

Notes: Sample limited to migration departures in 2011 or later. Robust standard errors clustered at the village level in parentheses, standard deviations in brackets. "MENA" indicates the woman migrated to the Middle East or North Africa on her last migration. The share of women migrating to these destinations is . The first two columns show the means of the outcome variable (leftmost column) for the full sample (column 1) and for the non-MENA sample (column 2). The third column shows the regression coefficient of the outcome variable on an indicator variable for a MENA migration. Net pay measured in millions June 2015 IDR winsorized at the top and bottom 1%.

Table 2: First Stage: Impact of Treatment on Self-Reported Exposure to Intervention Material

	(1) Attended Community Meeting on Migration	(2) Recognizes Infographic	(3) Recognizes Report Card	(4) Recognizes Comic
Panel A. Tracking Sample, Follow Up 2				
β_1: Report Card	-0.004	0.032*	0.109***	0.020
	(0.020)	(0.016)	(0.018)	(0.024)
β_2: Comic	-0.001	0.025	0.035	0.148***
	(0.022)	(0.018)	(0.022)	(0.021)
β_3: Infographic	0.128***	0.046**	-0.009	0.023
	(0.028)	(0.023)	(0.024)	(0.028)
P-value: Comic + Report Card vs Control:				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.000***	0.000***	0.000***	0.000***
Control Mean	0.062	0.043	0.038	0.041
N	3398	3409	3409	3398
Panel B. Tracking Sample, Follow Up 3				
β_1: Report Card	0.005	0.024	0.204***	0.006
	(0.020)	(0.026)	(0.020)	(0.028)
β_2: Comic	0.016	0.052**	0.028	0.385***
	(0.019)	(0.024)	(0.024)	(0.026)
β_3: Infographic	0.120***	0.157***	0.019	0.091**
	(0.025)	(0.030)	(0.027)	(0.037)
P-value: Comic + Report Card vs Control:				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.000***	0.000***	0.000***	0.000***
Control Mean	0.027	0.035	0.024	0.149
N	3725	3067	3070	3078
Panel C. Supplementary Sample, Follow Up 3				
β_1: Report Card	0.000	-0.005	0.009	0.007
	(0.013)	(0.016)	(0.010)	(0.026)
β_2: Comic	0.008	-0.005	-0.012	0.059***
	(0.012)	(0.015)	(0.011)	(0.023)
β_3: Infographic	0.026*	0.028	0.012	0.001
	(0.016)	(0.022)	(0.015)	(0.034)
P-value: Comic + Report Card vs Control:				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.000***	0.235	0.325	0.004***
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. All regressions control for strata fixed effects. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table 3: Impact on Migration Status at Final Follow Up: Tracking Sample

	(1) Migrated Post Intervention	(2) Migrated with Agency	(3) Migrated without Agency	(4) Plans to Migrate[+]
β_1: Report Card	-0.044**	-0.036*	-0.019	-0.002
	(0.021)	(0.020)	(0.019)	(0.027)
β_2: Comic	-0.023	-0.021	-0.010	0.000
	(0.021)	(0.020)	(0.018)	(0.024)
β_3: Infographic	0.050*	0.053*	-0.010	0.021
	(0.029)	(0.028)	(0.028)	(0.037)
P-value: Comic + Report Card vs Control:				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.404	0.838	0.049**	0.490
Control Mean	0.366	0.324	0.111	0.216
N	4664	4646	1730	2720

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≤ 0.10. [+]Sample limited to directly interviewed non-migrants at final follow up.

Table 4: Impact on Beliefs Among Non-Migrants

	Beliefs about Self			Beliefs about Others		
	(1) Agency Index	(2) Job Quality Index	(3) Infographic Index	(4) Agency Index	(5) Job Quality Index	(6) Infographic Index
β_1: Report Card	0.012	-0.010	-0.012	0.026	-0.019	-0.013
	(0.030)	(0.027)	(0.025)	(0.032)	(0.028)	(0.026)
β_2: Comic	0.009	0.005	0.014	0.023	0.005	0.013
	(0.029)	(0.027)	(0.025)	(0.029)	(0.029)	(0.027)
β_3: Infographic	-0.036	-0.032	-0.042	-0.039	-0.024	-0.034
	(0.041)	(0.038)	(0.035)	(0.043)	(0.040)	(0.037)
P-value: Comic + Report Card vs Control:						
$\beta_1 + \beta_2 + \beta_3 = 0$	0.590	0.155	0.106	0.714	0.181	0.188
Control Mean	-0.000	0.000	0.000	0.000	0.000	0.000
N	2597	2597	2597	2597	2597	2597

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$. Sample limited to non-migrants directly interviewed at final follow-up. 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 analogs 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 5: 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	0.168***	0.103**	-0.002
	(0.060)	(0.043)	(0.045)
β_2: Comic	0.107*	0.114***	0.014
	(0.058)	(0.043)	(0.047)
β_3: Infographic	-0.191**	-0.152***	-0.027
	(0.084)	(0.058)	(0.065)
P-value: Comic + Report Card vs Control:			
$\beta_1 + \beta_2 + \beta_3 = 0$	0.153	0.116	0.758
Control Mean	0.000	0.000	0.000
N	1005	990	996

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≤ 0.10. 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.

Table 6: Impact on Migration: by Type of Placement Agency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		High-Grade	Average	Low-Grade	Ungraded	Unknown	Migrated without
	Migrated	Agency	Agency	Agency	Agency	Agency	Agency
Panel A. Extensive Margin, Including Non-Migrants							
β_1: Report Card	-0.054**	0.001	-0.011**	-0.001	0.004	-0.030***	-0.019**
	(0.022)	(0.006)	(0.005)	(0.008)	(0.017)	(0.010)	(0.008)
β_2: Comic	-0.033	0.001	-0.003	0.005	-0.020	-0.012	-0.008
	(0.022)	(0.006)	(0.005)	(0.007)	(0.017)	(0.009)	(0.007)
β_3: Infographic	0.058*	0.003	0.011	0.002	0.017	0.012	0.016
	(0.030)	(0.008)	(0.008)	(0.010)	(0.023)	(0.015)	(0.012)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	0.163	0.421	0.544	0.379	0.957	0.003***	0.170
Control Mean	0.268	0.013	0.013	0.015	0.122	0.069	0.033
N	3762	3762	3762	3762	3762	3762	3762
Panel B. Agency Choice Conditional on Migrating							
β_1: Report Card	0.000	0.014	-0.029	0.015	0.098**	-0.067*	-0.045
	(.)	(0.022)	(0.020)	(0.028)	(0.047)	(0.035)	(0.028)
β_2: Comic	0.000	0.011	-0.004	0.026	-0.005	-0.026	-0.020
	(.)	(0.022)	(0.018)	(0.025)	(0.046)	(0.034)	(0.026)
β_3: Infographic	0.000	-0.000	0.028	-0.010	-0.041	0.003	0.034
	(.)	(0.030)	(0.029)	(0.035)	(0.064)	(0.050)	(0.041)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	.	0.280	0.783	0.224	0.271	0.014**	0.281
Control Mean	1.000	0.048	0.048	0.056	0.456	0.258	0.123
N	3880	1008	1008	1008	1008	1008	1008

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$. We create six agency classes: migration without an agency, migration with a high-grade agency (top third of grade distribution), migration with an average (middle third) agency, migration with a low-grade (bottom third) agency, migration with an agency that was not graded but was on the Indonesian government's list of approved placement agencies (an ungraded agency), and migration with an unknown agency. This last category includes all women who either (i) reported that they used an agency but did not know what its name was, or (ii) provided an agency name that was not on the list of government-approved agencies.

Table 7: Decomposition of Impacts on Migration Experiences

	Pre-Departure Preparation Index	Job Quality Index	Pay Index
	(1)	(2)	(3)
Panel A. Decomposing the Effect of the Report Card			
Agency Effect	0.118**	0.045	0.028
	(0.055)	(0.028)	(0.033)
Selection on Observables	-0.002	0.015	0.008
	(0.015)	(0.023)	(0.023)
Selection on Unobservables	0.001	0.006	-0.000
	(0.003)	(0.007)	(0.003)
Residual Differences	0.067***	0.043	-0.027
	(0.024)	(0.031)	(0.030)
Panel B. Decomposing the Effect of the Comic Book			
Agency Effect	0.053	0.016	-0.020
	(0.053)	(0.030)	(0.035)
Selection on Observables	-0.006	0.018	0.013
	(0.016)	(0.021)	(0.019)
Selection on Unobservables	0.001	0.003	-0.000
	(0.002)	(0.007)	(0.002)
Residual Differences	0.057**	0.073**	0.015
	(0.023)	(0.030)	(0.030)
Panel C. Decomposing the Effect of the Infographic			
Agency Effect	-0.096	-0.046	-0.008
	(0.077)	(0.040)	(0.044)
Selection on Observables	0.004	-0.029	-0.012
	(0.020)	(0.032)	(0.031)
Selection on Unobservables	-0.001	-0.005	0.000
	(0.004)	(0.009)	(0.003)
Residual Differences	-0.105***	-0.069	-0.003
	(0.033)	(0.043)	(0.042)

 Notes: Robust standard errors clustered at the village level in parentheses. Each column in each panel 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 randomized information treatments are included in the first stage migration equation, but not the second stage returns equation. See section 6.4 for more detail. Standard errors are based on 500 bootstrap replications. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table 8: Selection into Migration by Treatment Arm Based on Predicted Migration Experience

	(1) Full Sample	(2) All Migrants	(3) Direct Interview Migrants
β_1: Report Card	-0.006	-0.009*	-0.006
	(0.004)	(0.005)	(0.006)
β_2: Comic	-0.002	-0.004	-0.002
	(0.004)	(0.005)	(0.006)
β_3: Infographic	0.004	0.005	-0.001
	(0.005)	(0.006)	(0.008)
P-value: Comic + Report Card vs Control:			
$\beta_1 + \beta_2 + \beta_3 = 0$	0.201	0.067*	0.112
Control Mean	0.002	0.009	0.008
N	4789	1730	1008

Notes: Robust standard errors clustered at the village level in parentheses. Regressions include strata fixed effects. Outcome is predicted migration experience. Migration experience is predicted using random forest models trained on baseline data from tracking sample women who had migrated at baseline. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$.

A Theory Appendix

A.1 Relative Riskiness of Q_t and $\tilde{Q}_t$

The following arguments below draw on widely-used equivalence properties established and discussed in seminal work by Rothschild and Stiglitz (1970, 1971, 1972). To establish the riskiness ordering, we focus on showing that $\tilde{Q}_t$ can be constructed by taking Q_t and adding "zero conditional mean" noise ζ, where $\mathbb{E}[\zeta \mid Q_t] = 0$. First, note that by the Law of Iterated Expectations:

$$\mathbb{E}[\tilde{Q}_t \mid q_t] = \mathbb{E}[\mathbb{E}[u_t \mid q_t, r_t] \mid q_t] = \mathbb{E}[u_t \mid q_t] = Q_t \tag{A.1}$$

Now define $\zeta = \tilde{Q}_t - Q_t$. Clearly $\tilde{Q}_t \overset{d}{=} Q_t + \zeta$. Using (A.1), 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 $\tilde{Q}_t$.

A.2 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:

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

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:

$$\int_{\underline{U}}^{x} \frac{\partial F_Q(Q; \sigma)}{\partial \sigma}\, dQ = \int_{\underline{U}}^{x} F_{Q\sigma}(Q; \sigma)\, dQ \geq 0 \ \forall x \in [\underline{U}, \overline{U}) \text{ and } \int_{\underline{U}}^{\overline{U}} [F_{Q\sigma}(Q; \sigma)]\, dQ = 0. \tag{A.2}$$

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

$$Q^*(1 - \beta) = h + \beta\lambda \left[\int_{Q^*}^{\overline{U}} (Q - Q^*)\, dF_Q(Q) \right] \tag{A.3}$$

Now we use equation A.3 to implictly differentiate to solve $\partial Q^*/\partial \sigma$.

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

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

Rearranging:

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

Next, we integrate the right hand side of equation A.4 by parts:

$$\int_{Q^*}^{\overline{U}}(Q - Q^*)dF_{Q\sigma}(Q; \sigma)\partial\sigma = (Q - Q^*)F_{Q\sigma}(Q; \sigma) \mid_{Q^*}^{\overline{U}} - \int_{Q^*}^{\overline{U}}F_{Q\sigma}(Q; \sigma)d(Q - Q^*) \tag{A.4}$$

Since $F_Q(\overline{U}; \sigma) = 1 \ \forall\sigma$ then the first term is zero; further note that $d(Q - Q^*) = dQ$. Putting all of this together we have:

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

Giving us

$$\frac{\partial Q^*}{\partial\sigma} = \frac{-\int_{Q^*}^{\overline{U}}F_{Q\sigma}(Q; \sigma)dQ}{(1 - \beta + \beta\lambda(1 - F_Q(Q^*; \sigma)))} \geq 0 \tag{A.5}$$

because equation A.2 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:

$$\frac{\partial}{\partial\sigma}\lambda\left[1 - F_Q(Q^*; \sigma)\right] = -\lambda\left[dF_Q(Q^*; \sigma)\frac{\partial Q^*}{\partial\sigma} + F_{Q\sigma}(Q^*; \sigma)\right] \tag{A.6}$$

The first term on the right hand side of the equation is always positive, while $F_{Q\sigma}(Q^*; \sigma)$ is of indeterminate sign. Note 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:

$$\mathbb{E}[Q_t \mid Q_t \geq Q^*] = \frac{\int_{Q^*}^{\overline{U}}QdF_Q(Q; \sigma)}{1 - F_Q(Q^*; \sigma)} = \frac{\mu - \int_{\underline{U}}^{Q^*}QdF_Q(Q; \sigma)}{1 - F_Q(Q^*; \sigma)}$$

$$= \frac{\mu - Q^*F_Q(Q^*; \sigma) + \int_{\underline{U}}^{Q^*}F_Q(Q; \sigma)dQ}{1 - F_Q(Q^*; \sigma)} \tag{A.7}$$

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

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$$\frac{\partial \mathbb{E}[Q_t \mid Q_t \geq Q^*]}{\partial \sigma} = \frac{[1 - F_Q(Q^*; \sigma)] \int_{\underline{U}}^{Q^*} F_{Q\sigma}(Q; \sigma) dQ}{[1 - F_Q(Q^*; \sigma)]^2} +$$

$$\frac{\left[\mu - Q^* + \int_{\underline{U}}^{Q^*} F_Q(Q; \sigma) dQ \right] \left[dF_Q(Q^*) \frac{\partial Q^*}{\partial \sigma} + F_{Q\sigma}(Q^*; \sigma) \right]}{[1 - F_Q(Q^*; \sigma)]^2} \tag{A.8}$$

Then note that

$$\mu - Q^* + \int_{\underline{U}}^{Q^*} F_Q(Q; \sigma) dQ = \int_{Q^*}^{\overline{U}} Q dF_Q(Q; \sigma) - Q^*(1 - F_Q(Q^*; \sigma))$$

$$= \int_{Q^*}^{\overline{U}} (Q - Q^*) dF_Q(Q; \sigma) \geq 0 \tag{A.9}$$

Plugging this in we have:

$$\frac{\partial \mathbb{E}[Q_t \mid Q_t \geq Q^*]}{\partial \sigma} = \frac{[1 - F_Q(Q^*; \sigma)] \int_{\underline{U}}^{Q^*} F_{Q\sigma}(Q; \sigma) dQ}{[1 - F_Q(Q^*; \sigma)]^2} +$$

$$\frac{\left[\int_{Q^*}^{\overline{U}} (Q - Q^*) dF_Q(Q; \sigma) \right] \left[dF_Q(Q^*; \sigma) \frac{\partial Q^*}{\partial \sigma} + F_{Q\sigma}(Q^*; \sigma) \right]}{[1 - F_Q(Q^*; \sigma)]^2} \tag{A.10}$$

The first term in the sum is positive because $\int_{\underline{U}}^{Q^*} F_{Q\sigma}(Q; \sigma) dQ \geq 0$ by equation A.2. 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.10 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 $\frac{\partial \mathbb{E}[Q_t \mid Q_t \geq Q^*]}{\partial \sigma} > 0$.

A.3   Detailed Derivations – Impact of Shifting Beliefs

To assess the impact of change in the location of beliefs, we rewrite equation A.3 to capture a translation of Q_t by $\eta\Delta$ (here, we use the fact that $dF_Q(x) = dG_Q(x + \eta\Delta)$):

$$Q^*(1 - \beta) = h + \beta\lambda \left[\int_{Q^* - \eta\Delta}^{\overline{U}} (Q + \eta\Delta - Q^*) dF_Q(Q) \right] \tag{A.11}$$

Differentiating implicitly we see that

$$\frac{\partial Q^*}{\partial \Delta} = \frac{\beta\lambda\eta(1 - F_Q(Q^* - \eta\Delta))}{1 - \beta + \beta\lambda(1 - F_Q(Q^* - \eta\Delta))} \geq 0 \tag{A.12}$$

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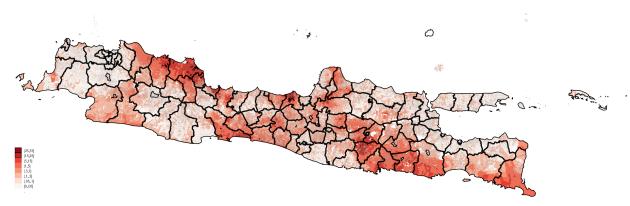
we differentiate:

$$\frac{\partial}{\partial \Delta} \lambda \left[1 - F_Q(Q^* - \eta \Delta) \right] = \lambda \left[-dF_Q(Q^* - \eta \Delta) \right] \left[\frac{\partial Q^*}{\partial \Delta} - \eta \right] \geq 0 \qquad \text{(A.13)}$$

because $\partial Q^* / \partial \Delta < \eta$.
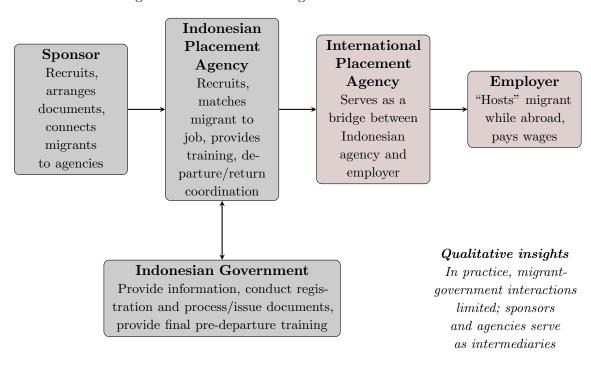
B Appendix Figures and Tables

Figure B.1: Female International Labor Migrants on the Island of Java
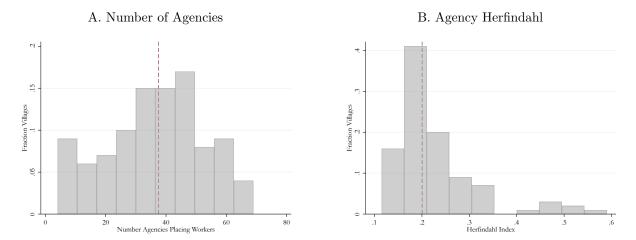
Notes: This figure graphs the number of female migrants placed during the intervention period (October 2015-December 2019) per 100 female residents. The numerator is measured in 2016 from official government placements data graciously shared with us by the Indonesian government (see Section 5), and the denominator comes from the Village Potential (*Podes*) triennial administrative census from 2011.

Figure B.2: Overview of Migration Process in Indonesia

Notes: The figure reflects the most common process used by formal migrants at the time of the experiment (2015-2019). In 2017, Indonesia passed a law (UU 18/2017), designed to create a more active role for the government in providing potential migrants with information, connecting workers to placement agencies and job opportunities (ideally obviating the need for sponsors), and providing training. Law implementation lagged passage; we therefore focus on the older framework, which was relevant for migrants in our sample.

Figure B.3: Agency Concentration within Villages During Intervention Period

A. Number of Agencies

B. Agency Herfindahl

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.

Figure B.4: Examples of Intervention Materials

A. Infographic

B. Report Card

C. Comic Book

Figure B.5: Study Villages with Treatment Assignment

non-study
■ control
■ report card only
■ comic only
■ comic and report card
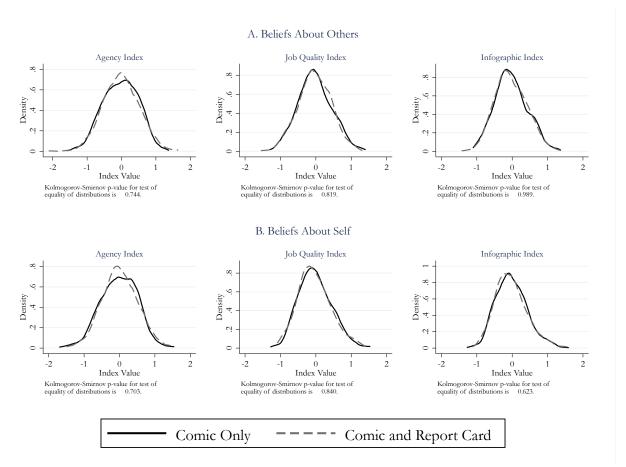
Figure B.6: Study Timeline

Activity	2015									...	2016					...	2017			...	2019					
	4	5	6	7	8	9	10	11	12		8	9	10	11	12		10	11	12		5	6	7	8	9	10
Baseline Survey																										
Computing Agency Rankings																										
Intervention Implementation																										
Follow Up Survey 1																										
Follow Up Survey 2 + Second Materials Distribution																										
Follow Up Survey 3																										
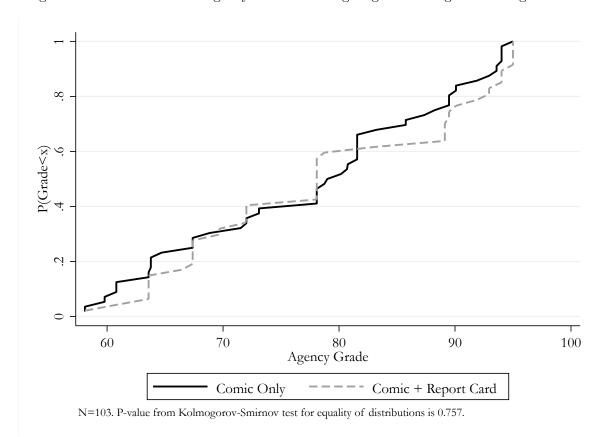
Notes: Numbers in boxes below years indicate months. The second materials distribution involved distributing report card, comic, and infographic materials to women in the relevant treatment groups participating in an in-person survey for follow up 2.

Figure B.7: Impact of Report Card on Distribution of Beliefs

A. Beliefs About Others

Kolmogorov-Smirnov p-value for test of equality of distributions is 0.744.

Kolmogorov-Smirnov p-value for test of equality of distributions is 0.819.

Kolmogorov-Smirnov p-value for test of equality of distributions is 0.989.

B. Beliefs About Self

Kolmogorov-Smirnov p-value for test of equality of distributions is 0.703.

Kolmogorov-Smirnov p-value for test of equality of distributions is 0.840.

Kolmogorov-Smirnov p-value for test of equality of distributions is 0.623.

——— Comic Only ----- Comic and Report Card

Notes: Sample Limited to Non-Migrants interviewed directly at final follow up. 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 analogs 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.

Figure B.8: Distribution of Agency Grades Among Migrants Using Graded Agencies

N=103. P-value from Kolmogorov-Smirnov test for equality of distributions is 0.757.

Notes: Sample is limited to women who migrated with graded agencies after October 2015 in the comic only and comic and report card treatment arms. Direct interviews only.

Figure B.9: Validating Predicted Migration Experience

i. Pre-Departure Preparation

$\beta_{OLS}=$ 1.143, $se_{OLS}=$ 0.230

ii. Non-Monetary Experience

$\beta_{OLS}=$ 3.184, $se_{OLS}=$ 0.130

iii. Monetary Compensation

$\beta_{OLS}=$ 5.869, $se_{OLS}=$ 0.195

Local Linear Fit   ——   Regression Fit

Notes: The x-axis is predicted migration experience (an average of the pre-departure preparation, job quality, and pay indices) based on a random forest model fit on pre-intervention migration experiences of tracking sample women who had already migrated at baseline. The y-axis is endline migration experience (an average of the pre-departure preparation, job quality, and pay indices) among all tracking sample women. Graphs report the results of local linear fit with 95 percent confidence intervals in grey, as well as linear regression fit. Standard errors for linear regressions are clustered at the village level.

Table B.1: Balance Test: Baseline Differences in Demographic Characteristics Among Tracking Sample

	(1) Control Mean	(2) β_1: Report Card	(3) β_2: Comic	(4) β_3: Infographic	(5) P-value: $\beta_1 = \beta_2 = \beta_3 = 0$	(6) N
Age	28.74	0.309	0.577	-0.963*	0.290	4805
Single	0.148	0.00737	0.00517	0.0180	0.366	4805
Married	0.740	0.0128	-0.0102	-0.00568	0.764	4805
Divorced, Separated or Widowed	0.112	-0.0201	0.00501	-0.0123	0.0178**	4805
Did Not Graduate Primary School	0.123	0.0128	0.00128	0.00292	0.635	4804
Primary School Graduate	0.370	0.00869	-0.00752	-0.0232	0.491	4804
Secondary School Graduate	0.508	-0.0214	0.00624	0.0203	0.649	4804
Fraction Correct: Raven's Test	0.684	0.00397	0.00757	-0.0220	0.401	4795
Fraction Correct: Math Problems	0.427	0.00614	0.0187	-0.0353	0.456	4795
High (Above-Median) Locus of Control	0.458	0.0105	0.00802	-0.0357	0.510	4804
Plans to Migrate to MENA Country	0.308	0.00836	0.0230	-0.0380	0.760	4698
Plans to Migrate to Asian Country	0.684	-0.00938	-0.0245	0.0410	0.719	4698
Plans to Use a Sponsor	0.663	0.0316	0.00810	0.00671	0.226	4721
Plans to Go Directly to an Agency	0.249	-0.0161	-0.0153	-0.00729	0.365	4721
Expected Salary (Millions IDR)	5.825	-0.117	-0.0565	0.126	0.918	4566
Expected Hours of Work Per Day	12.00	-0.0390	0.0537	0.00612	0.984	4530
Probability Gets Day Off	0.506	0.00539	-0.0286*	0.0244	0.129	4803
Probability Experience Physical Abuse	0.216	0.00810	0.0115	-0.00610	0.768	4803
Has Migrated Before	0.494	-0.0312**	-0.0173	0.0397**	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. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10

Table B.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	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)
β_2: Comic	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)
β_3: Infographic	-0.016	0.001	0.000	-0.029	0.001	-0.012	-0.019	-0.002	0.009
	(0.030)	(0.022)	(0.005)	(0.030)	(0.028)	(0.009)	(0.029)	(0.024)	(0.010)
P-value: Comic + Report Card vs Control:									
$\beta_1 + \beta_2 + \beta_3 = 0$	0.175	0.983	0.511	0.204	0.724	0.021**	0.965	0.459	0.286
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$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.10.

Table B.3: 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) Chosen Agency
β_1: Report Card	0.004	0.001	0.011	0.008	-0.003
	(0.015)	(0.012)	(0.008)	(0.013)	(0.011)
β_2: Comic	0.001	0.003	0.010	0.008	0.002
	(0.014)	(0.011)	(0.008)	(0.013)	(0.010)
β_3: Infographic	0.006	-0.002	-0.013	0.005	0.002
	(0.021)	(0.017)	(0.012)	(0.017)	(0.015)
P-value: Comic + Report Card vs Control:					
$\beta_1 + \beta_2 + \beta_3 = 0$	0.461	0.846	0.368	0.081*	0.912
Control Mean	0.075	0.043	0.024	0.040	0.031
N	2720	2692	2692	2692	2692

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$\leq$ 0.10.

Table B.4: Impact on Beliefs Among Non-Migrants – Heterogeneity by Baseline Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)
		Beliefs about Self			Beliefs about Others	
	Agency Index	Job Quality Index	Infographic Index	Agency Index	Job Quality Index	Infographic Index
Panel A. Women with Above-Median Baseline Beliefs						
β_1: Report Card	0.019	-0.022	-0.007	0.050	-0.026	-0.025
	(0.040)	(0.036)	(0.035)	(0.044)	(0.038)	(0.039)
β_2: Comic	0.005	-0.003	0.006	0.026	0.046	0.035
	(0.040)	(0.036)	(0.037)	(0.041)	(0.040)	(0.038)
β_3: Infographic	-0.037	0.032	-0.024	-0.066	-0.058	-0.045
	(0.054)	(0.052)	(0.051)	(0.058)	(0.052)	(0.052)
P-value: Comic + Report Card vs Control:						
$\beta_1 + \beta_2 + \beta_3 = 0$	0.743	0.848	0.487	0.801	0.302	0.345
Control Mean	0.092	0.075	0.049	0.100	0.095	0.061
N	1331	1322	1319	1325	1319	1313
Women with Below-Median Baseline Beliefs						
β_1: Report Card	0.008	-0.010	-0.022	0.001	-0.019	-0.009
	(0.040)	(0.033)	(0.033)	(0.041)	(0.036)	(0.034)
β_2: Comic	0.015	0.006	0.030	0.022	-0.040	-0.010
	(0.041)	(0.035)	(0.034)	(0.039)	(0.038)	(0.036)
β_3: Infographic	-0.020	-0.095**	-0.067	0.007	0.016	-0.008
	(0.056)	(0.048)	(0.047)	(0.058)	(0.054)	(0.050)
P-value: Comic + Report Card vs Control:						
$\beta_1 + \beta_2 + \beta_3 = 0$	0.926	0.002***	0.058*	0.446	0.260	0.434
Control Mean	-0.111	-0.073	-0.049	-0.117	-0.097	-0.069
N	1266	1275	1278	1272	1278	1284

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$. Sample limited to non-migrants directly interviewed at final follow-up. 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 analogs 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 B.5: 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	0.093**	0.081*	2.216***	0.851**	0.044	0.095**	0.050	0.059	0.026	0.484*
	(0.045)	(0.045)	(0.815)	(0.370)	(0.028)	(0.045)	(0.040)	(0.042)	(0.038)	(0.277)
β_2: Comic	0.082*	0.078	1.293	0.826**	0.019	0.082*	0.011	-0.003	-0.006	0.391
	(0.045)	(0.049)	(0.849)	(0.395)	(0.026)	(0.044)	(0.040)	(0.043)	(0.036)	(0.252)
β_3: Infographic	-0.124**	-0.130**	-2.488**	-1.243**	-0.033	-0.120*	-0.042	-0.053	-0.008	-0.498
	(0.062)	(0.066)	(1.097)	(0.528)	(0.041)	(0.063)	(0.058)	(0.060)	(0.053)	(0.389)
P-value: Comic + Report Card vs Control:										
$\beta_1 + \beta_2 + \beta_3 = 0$	0.249	0.533	0.221	0.251	0.292	0.214	0.658	0.944	0.767	0.185
Control Mean	0.563	0.689	5.427	5.066	0.876	0.592	0.294	0.337	0.798	6.639
N	974	974	973	971	1004	1000	952	981	972	976

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≤ 0.10. 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.

Table B.6: 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	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)
β_2: Comic	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)
β_3: Infographic	-0.090	-0.058	-0.132**	-0.076	-0.444*	-0.022	-0.022
	(0.064)	(0.051)	(0.062)	(0.066)	(0.245)	(0.060)	(0.064)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	0.704	0.024**	0.373	0.609	0.971	0.151	0.801
Control Mean	0.357	0.774	0.406	0.502	7.547	0.709	0.586
N	982	976	982	974	981	980	971

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≤ 0.10. Sample limited to directly interviewed migrants in the tracking sample.

Table B.7: 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	-589.178	-57.494	-0.012	0.006	0.039
	(639.891)	(648.339)	(0.030)	(0.031)	(0.045)
β_2: Comic	-231.890	-108.311	-0.009	0.013	0.051
	(648.608)	(641.135)	(0.027)	(0.030)	(0.049)
β_3: Infographic	-121.402	-270.154	0.051	-0.017	-0.079
	(937.810)	(919.556)	(0.041)	(0.041)	(0.066)
P-value: Comic + Report Card vs Control:					
$\beta_1 + \beta_2 + \beta_3 = 0$	0.187	0.541	0.331	0.967	0.824
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. All regressions control for strata fixed effects. * p$\leq$ 0.10, ** p$\leq$ 0.05, *** p$\leq$ 0.10. Sample limited to directly interviewed migrants in the tracking sample. $^+$IDR 10,000s, top-coded at 99th percentile.

Table B.8: Impact on Pre-Departure Preparation and Experience Abroad – Migrants who Used Agencies

	Standardized Experience Indices		
	(1)	(2)	(3)
		Migration	Migration
	Pre-Departure	Experience:	Experience:
	Preparation	Job Quality	Pay
β_1: Report Card	0.111***	0.094**	-0.015
	(0.040)	(0.043)	(0.048)
β_2: Comic	0.086**	0.137***	0.015
	(0.040)	(0.045)	(0.050)
β_3: Infographic	-0.155***	-0.154**	-0.015
	(0.056)	(0.061)	(0.070)
P-value: Comic + Report Card vs Control:			
$\beta_1 + \beta_2 + \beta_3 = 0$	0.309	0.084*	0.766
Control Mean	0.200	0.002	-0.003
N	886	873	878

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≤ 0.10. Sample limited to directly interviewed migrants in the tracking sample who used agencies. 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.

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

	Destination			Occupation Abroad			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Domestic Worker	Elderly Caregiver	Babysitter/ Nanny	Formal Sector Work
	Asia	MENA	Other				
β_1: Report Card	0.032	-0.025	-0.007	0.010	0.037	0.013	-0.038*
	(0.045)	(0.044)	(0.005)	(0.036)	(0.046)	(0.040)	(0.020)
β_2: Comic	-0.022	0.022	-0.000	0.035	0.013	0.005	-0.026
	(0.042)	(0.042)	(0.001)	(0.037)	(0.047)	(0.040)	(0.019)
β_3: Infographic	-0.019	0.016	0.004	-0.014	-0.053	0.006	0.015
	(0.061)	(0.061)	(0.007)	(0.053)	(0.063)	(0.052)	(0.030)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	0.813	0.737	0.329	0.394	0.943	0.517	0.018**
Control Mean	0.737	0.259	0.004	0.785	0.442	0.231	0.084
N	1006	1006	1006	1006	1006	1006	1006

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≤ 0.10. Sample limited to directly interviewed migrants in the tracking sample. Occuption categories are not mutually exclusive.

Table B.10: Differences in Migration Experience by Agency Type

	(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	(7) Pre-Departure Preparation	(8) Migration Experience: Job Quality	(9) Migration Experience: Pay
Unknown Agency	-0.401***	-0.179***	-0.398***	-0.365***	-0.151***	-0.346***	-0.243***	-0.073*	-0.197***
	(0.040)	(0.042)	(0.055)	(0.040)	(0.041)	(0.056)	(0.038)	(0.043)	(0.055)
Low-Grade Agency	-0.317***	-0.214***	-0.454***	-0.273***	-0.166***	-0.384***	-0.128***	-0.067	-0.219***
	(0.045)	(0.055)	(0.063)	(0.046)	(0.054)	(0.064)	(0.044)	(0.055)	(0.063)
Average Agency	-0.015	-0.036	-0.023	-0.016	-0.036	-0.020	-0.015	-0.033	-0.004
	(0.050)	(0.057)	(0.074)	(0.049)	(0.056)	(0.074)	(0.046)	(0.056)	(0.071)
Ungraded Agency	-0.078**	-0.040	-0.137**	-0.075**	-0.040	-0.136**	-0.055	-0.034	-0.104*
	(0.037)	(0.041)	(0.056)	(0.036)	(0.040)	(0.056)	(0.034)	(0.039)	(0.054)
No Agency	-1.673***	0.018	-0.147**	-1.641***	0.022	-0.118*	-1.568***	0.057	-0.018
	(0.036)	(0.046)	(0.060)	(0.036)	(0.044)	(0.061)	(0.035)	(0.044)	(0.058)
Age				-0.004***	0.008***	0.002	-0.002	0.010***	0.004***
				(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)
Married				-0.003	-0.136***	-0.188***	-0.001	-0.115***	-0.183***
				(0.028)	(0.035)	(0.041)	(0.028)	(0.035)	(0.040)
Divorced/Separated				-0.002	-0.189***	-0.144***	-0.002	-0.168***	-0.144***
				(0.031)	(0.044)	(0.047)	(0.031)	(0.043)	(0.045)
Widowed				0.011	-0.219***	-0.093	0.041	-0.181***	-0.056
				(0.051)	(0.069)	(0.071)	(0.051)	(0.069)	(0.072)
Some Primary				-0.011	0.004	0.095	0.037	0.031	0.142*
				(0.095)	(0.090)	(0.082)	(0.081)	(0.083)	(0.086)
Primary Graduate				0.094	0.089	0.210***	0.124	0.106	0.242***
				(0.098)	(0.086)	(0.079)	(0.083)	(0.079)	(0.084)
Some Jr. Secondary				0.083	0.106	0.265***	0.082	0.105	0.277***
				(0.100)	(0.089)	(0.086)	(0.085)	(0.083)	(0.091)
Jr. Secondary Graduate				0.143	0.224***	0.335***	0.109	0.204**	0.305***
				(0.098)	(0.086)	(0.082)	(0.083)	(0.079)	(0.086)
Some Sr. Secondary				0.052	0.132	0.272***	0.008	0.103	0.224**
				(0.108)	(0.103)	(0.100)	(0.094)	(0.097)	(0.104)
Sr. Secondary Graduate				0.152	0.230***	0.394***	0.113	0.190**	0.359***
				(0.099)	(0.088)	(0.085)	(0.085)	(0.081)	(0.090)
Post-Secondary				0.119	0.412***	0.662***	0.060	0.326**	0.572***
				(0.108)	(0.135)	(0.134)	(0.095)	(0.132)	(0.130)
Job in MENA							-0.275***	-0.157	-0.105
							(0.078)	(0.148)	(0.409)
Job in Asia							-0.026	0.020	0.057
							(0.077)	(0.149)	(0.408)
Eldercare							0.045***	-0.038*	0.200***
							(0.016)	(0.023)	(0.024)
Housemaid							0.029*	-0.117***	-0.108***
							(0.016)	(0.023)	(0.026)
Nanny/Babysitter							-0.004	-0.021	0.049**
							(0.015)	(0.021)	(0.021)
Dep. Var. Mean	-.213	-.075	-.019	-.213	-.075	-.019	-.213	-.075	-.019
N	2666	2658	2664	2666	2658	2664	2666	2658	2664
Control Set	None	None	None	+Demographics	+Demographics	+Demographics	+Job Chars.	+Job Chars.	+Job Chars.

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≤ 0.10. We create six agency classes: migration without an agency, migration with a high-grade agency (top third of grade distribution), migration with an average (middle third) agency, migration with a low-grade (bottom third) agency, migration with an agency that was not graded but was on the Indonesian government's list of approved placement agencies (an ungraded agency), and migration with an unknown agency. This last category includes all women who either (i) reported that they used an agency but did not know what its name was, or (ii) provided an agency name that was not on the list of government-approved agencies. Sample limited to tracking sample migrants in the control group and migrants in the supplementary sample in all treatment arms. Missing values of covariates are recoded to zero and dummied out. Low-grade agencies have below-median grades (less than 78).

Table B.11: Impact on Pre-Departure Preparation and Experience Abroad – Controlling for Predicted Experience

	Standardized Experience Indices		
	(1)	(2) Migration Experience: Job Quality	(3) Migration Experience: Pay
	Pre-Departure Preparation		
β_1: Report Card	0.171***	0.115***	0.009
	(0.058)	(0.041)	(0.042)
β_2: Comic	0.109*	0.119***	0.019
	(0.057)	(0.041)	(0.043)
β_3: Infographic	-0.189**	-0.149***	-0.023
	(0.082)	(0.055)	(0.060)
P-value: Comic + Report Card vs Control:			
$\beta_1 + \beta_2 + \beta_3 = 0$	0.110	0.041**	0.913
Control Mean	0.000	0.000	0.000
N	1005	990	996

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≤ 0.10. 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. All regressions additionally control for two measures of predicted migration experience, based on a regression forest fit to (a) non-tracking sample women and (b) tracking sample women at baseline.

Table B.12: 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	-0.118	-0.091	0.010	0.066	0.024	-0.009
	(0.154)	(0.098)	(0.045)	(0.071)	(0.050)	(0.049)
β_2: Comic	-0.227	-0.029	0.012	0.051	0.035	0.023
	(0.158)	(0.095)	(0.043)	(0.070)	(0.050)	(0.048)
β_3: Infographic	0.049	0.055	0.003	0.052	-0.085	0.035
	(0.225)	(0.146)	(0.064)	(0.105)	(0.071)	(0.073)
P-value: Comic + Report Card vs Control:						
$\beta_1 + \beta_2 + \beta_3 = 0$	0.047**	0.524	0.586	0.022**	0.598	0.364
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. All regressions control for strata fixed effects. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. 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 indices using factor analysis, extracting the first factor and signing the index so higher values correspond to better outcomes. We then normalize each index relative to the control group mean and standard deviation.

Table B.13: Impact on Occuptional Status and Earnings

	(1)	(2) Unpaid Family Worker	(3) Casual Worker	(4) Wage Employee	(5) Self Employed	(6) On Migration	(7) Monthly Earnings
	Unemployed						
β_1: Report Card	-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)
β_2: Comic	-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)
β_3: Infographic	0.007	-0.007	0.020	0.029	-0.069***	0.019	0.030
	(0.031)	(0.007)	(0.018)	(0.023)	(0.023)	(0.026)	(0.158)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	0.268	0.494	0.980	0.218	0.018**	0.817	0.424
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. All regressions control for strata fixed effects. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10. 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.

Table B.14: Decomposition of Impacts on Migration Experiences – Using Agency Class Dummies Instead of Agency Fixed Effects

	Pre-Departure Preparation Index	Job Quality Index	Pay Index
	(1)	(2)	(3)
Panel A. Decomposing the Effect of the Report Card			
Agency Effect	0.093*	0.010	0.014
	(0.050)	(0.008)	(0.013)
Selection on Observables	-0.001	0.023	0.013
	(0.014)	(0.021)	(0.022)
Selection on Unobservables	-0.000	0.006	0.000
	(0.002)	(0.007)	(0.003)
Residual Differences	0.093***	0.071**	-0.019
	(0.030)	(0.035)	(0.039)
Panel B. Decomposing the Effect of the Comic Book			
Agency Effect	0.028	-0.001	-0.003
	(0.049)	(0.007)	(0.012)
Selection on Observables	-0.009	0.016	0.006
	(0.014)	(0.019)	(0.019)
Selection on Unobservables	-0.000	0.003	0.000
	(0.002)	(0.006)	(0.002)
Residual Differences	0.086***	0.093**	0.005
	(0.028)	(0.039)	(0.041)
Panel C. Decomposing the Effect of the Infographic			
Agency Effect	-0.056	-0.004	0.000
	(0.070)	(0.010)	(0.016)
Selection on Observables	0.003	-0.028	-0.010
	(0.019)	(0.028)	(0.030)
Selection on Unobservables	0.000	-0.005	-0.000
	(0.003)	(0.008)	(0.003)
Residual Differences	-0.146***	-0.113**	-0.013
	(0.042)	(0.052)	(0.054)

 Notes: Robust standard errors clustered at the village level in parentheses. Each column in each panel 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 randomized information treatments are included in the first stage migration equation, but not the second stage returns equation. See section 6.4 for more detail. Standard errors are based on 500 bootstrap replications. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table B.15: Impact on Migration Status at Final Follow Up: Tracking Sample – Reweighted

	(1) Migrated Post Intervention	(2) Migrated with Agency	(3) Migrated without Agency	(4) Plans to Migrate[+]
β_1: Report Card	-0.055***	-0.048**	-0.016	-0.005
	(0.021)	(0.021)	(0.020)	(0.028)
β_2: Comic	-0.029	-0.022	-0.024	0.012
	(0.020)	(0.019)	(0.021)	(0.024)
β_3: Infographic	0.062**	0.059**	0.006	0.021
	(0.031)	(0.029)	(0.031)	(0.039)
P-value: Comic + Report Card vs Control:				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.277	0.582	0.086*	0.311
Control Mean	0.373	0.331	0.109	0.213
N	4664	4646	1730	2720

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$. [+]Sample limited to directly interviewed non-migrants at final follow up. Observations are re-weighted to equalize the number of former and novice migrants within each village.

Table B.16: Impact on Beliefs Among Non-Migrants – Reweighted

	Beliefs about Self			Beliefs about Others		
	(1) Agency Index	(2) Job Quality Index	(3) Infographic Index	(4) Agency Index	(5) Job Quality Index	(6) Infographic Index
β_1: Report Card	0.036	-0.016	-0.019	0.044	-0.013	-0.011
	(0.033)	(0.029)	(0.027)	(0.035)	(0.030)	(0.028)
β_2: Comic	0.027	-0.005	0.001	0.044	0.004	0.006
	(0.030)	(0.028)	(0.026)	(0.032)	(0.030)	(0.028)
β_3: Infographic	-0.063	-0.017	-0.027	-0.061	-0.014	-0.025
	(0.044)	(0.040)	(0.038)	(0.046)	(0.041)	(0.039)
P-value: Comic + Report Card vs Control:						
$\beta_1 + \beta_2 + \beta_3 = 0$	0.974	0.160	0.077*	0.357	0.428	0.266
Control Mean	0.005	0.002	0.005	0.005	-0.003	0.001
N	2597	2597	2597	2597	2597	2597

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≤ 0.10. Observations are re-weighted to equalize the number of former and novice migrants within each village. Sample limited to non-migrants directly interviewed at final follow-up. 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 analogs 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 B.17: Impact on Migration: by Type of Placement Agency – Reweighted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		High-Grade	Average	Low-Grade	Ungraded	Unknown	Migrated without
	Migrated	Agency	Agency	Agency	Agency	Agency	Agency
Panel A. Extensive Margin, Including Non-Migrants							
β_1: Report Card	-0.064***	0.002	-0.014**	-0.003	-0.005	-0.027***	-0.019**
	(0.024)	(0.006)	(0.006)	(0.010)	(0.019)	(0.010)	(0.009)
β_2: Comic	-0.037*	0.001	-0.002	0.006	-0.023	-0.009	-0.014
	(0.022)	(0.006)	(0.005)	(0.007)	(0.017)	(0.009)	(0.010)
β_3: Infographic	0.067**	0.003	0.013	-0.001	0.023	0.009	0.023*
	(0.032)	(0.008)	(0.008)	(0.013)	(0.025)	(0.015)	(0.013)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	0.104	0.352	0.532	0.862	0.740	0.007***	0.200
Control Mean	0.276	0.012	0.012	0.020	0.129	0.067	0.032
N	3762	3762	3762	3762	3762	3762	3762
Panel B. Agency Choice Conditional on Migrating							
β_1: Report Card	0.000	0.018	-0.038*	0.007	0.087*	-0.050	-0.042
	(0.000)	(0.021)	(0.022)	(0.034)	(0.051)	(0.035)	(0.029)
β_2: Comic	0.000	0.011	-0.002	0.024	0.001	-0.015	-0.038
	(0.000)	(0.022)	(0.017)	(0.025)	(0.047)	(0.034)	(0.032)
β_3: Infographic	0.000	-0.003	0.035	-0.019	-0.043	-0.012	0.056
	(0.000)	(0.029)	(0.030)	(0.045)	(0.069)	(0.050)	(0.045)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	.	0.219	0.778	0.698	0.356	0.039**	0.388
Control Mean	1.000	0.043	0.045	0.071	0.467	0.245	0.118
N	4789	1008	1008	1008	1008	1008	1008

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≤ 0.10. We create six agency classes: migration without an agency, migration with a high-grade agency (top third of grade distribution), migration with an average (middle third) agency, migration with a low-grade (bottom third) agency, migration with an agency that was not graded but was on the Indonesian government's list of approved placement agencies (an ungraded agency), and migration with an unknown agency. This last category includes all women who either (i) reported that they used an agency but did not know what its name was, or (ii) provided an agency name that was not on the list of government-approved agencies. Sample limited to directly interviewed migrants in the tracking sample. Observations are re-weighted to equalize the number of former and novice migrants within each village.

Table B.18: Impact on Pre-Departure Preparation and Experience Abroad – Reweighted

	Standardized Experience Indices		
	(1)	(2) Migration Experience:	(3) Migration Experience:
	Pre-Departure Preparation	Job Quality	Pay
β_1: Report Card	0.161***	0.126***	0.025
	(0.061)	(0.045)	(0.048)
β_2: Comic	0.150**	0.119***	0.017
	(0.067)	(0.043)	(0.048)
β_3: Infographic	-0.225**	-0.187***	-0.053
	(0.092)	(0.062)	(0.067)
P-value: Comic + Report Card vs Control:			
$\beta_1 + \beta_2 + \beta_3 = 0$	0.162	0.177	0.823
Control Mean	0.001	0.013	0.010
N	1005	990	996

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$. 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. Observations are re-weighted to equalize the number of former and novice migrants within each village.

Table B.19: Impact on Village-Level Migration Flows: Administrative Data

	Levels				Fraction Female Migrants		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total	Asia	MENA	Other	Asia	MENA	Other
β_1: Report Card	1.436	1.618	-0.170	-0.012	0.006	-0.006	-0.000
	(15.324)	(15.118)	(0.763)	(0.027)	(0.013)	(0.013)	(0.000)
β_2: Comic	-9.842	-9.475	-0.366	0.000	-0.003	0.003	0.000
	(15.882)	(15.595)	(0.747)	(0.024)	(0.010)	(0.010)	(0.000)
β_3: Infographic	6.952	7.416	-0.465	0.001	0.014	-0.014	0.000
	(21.626)	(21.296)	(1.083)	(0.037)	(0.018)	(0.018)	(0.000)
P-value: Comic + Report Card vs Control:							
$\beta_1 + \beta_2 + \beta_3 = 0$	0.927	0.978	0.176	0.682	0.214	0.217	0.418
Control Mean	221.220	212.360	8.820	0.040	0.919	0.080	0.000
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. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table B.20: Impact on Village-Level Agency Market Share: Administrative Data

	Levels				Fraction Female Migrants			
	(1) All Graded Placements	(2) High Graded Placements	(3) Average Graded Placements	(4) Low Graded Placements	(5) All Graded Placements	(6) High Graded Placements	(7) Average Graded Placements	(8) Low Graded Placements
β_1: Report Card	-0.715	-1.123	-0.997	1.405	0.003	-0.006	0.001	0.008
	(3.076)	(1.876)	(2.110)	(0.950)	(0.013)	(0.006)	(0.008)	(0.010)
β_2: Comic	2.040	1.119	-0.366	1.287	0.019*	0.005	-0.001	0.015*
	(2.995)	(1.569)	(1.996)	(0.854)	(0.012)	(0.005)	(0.008)	(0.008)
β_3: Infographic	1.405	0.184	2.266	-1.045	-0.001	0.001	0.007	-0.009
	(4.282)	(2.412)	(3.047)	(1.175)	(0.017)	(0.008)	(0.011)	(0.012)
P-value: Comic + Report Card vs Control:								
$\beta_1 + \beta_2 + \beta_3 = 0$	0.377	0.911	0.674	0.049**	0.059*	0.951	0.328	0.101
Control Mean	26.760	10.490	12.680	3.590	0.124	0.041	0.056	0.027
N	400	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. * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.10$.

Table B.21: Impact on Migration: by Type of Placement Agency - Results from the Supplementary Sample

	(1)	(2)	(3)	(4)	(5)	(6)
						Migrated
	High-Grade	Average	Low-Grade	Ungraded	Unknown	without
	Agency	Agency	Agency	Agency	Agency	Agency
β_1: Report Card	-0.000	-0.019	0.029	0.054*	-0.023	-0.043
	(0.012)	(0.015)	(0.018)	(0.033)	(0.026)	(0.027)
β_2: Comic	-0.005	-0.011	0.042**	0.009	-0.015	-0.020
	(0.013)	(0.011)	(0.018)	(0.032)	(0.023)	(0.026)
β_3: Infographic	-0.006	0.026	-0.021	-0.059	0.025	0.032
	(0.019)	(0.019)	(0.022)	(0.045)	(0.036)	(0.036)
P-value: Comic + Report Card vs Control:						
$\beta_1 + \beta_2 + \beta_3 = 0$	0.443	0.722	0.005***	0.879	0.608	0.212
Control Mean	0.048	0.040	0.045	0.439	0.214	0.211
N	2418	2418	2418	2418	2418	2418

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≤ 0.10. We create six agency classes: migration without an agency, migration with a high-grade agency (top third of grade distribution), migration with an average (middle third) agency, migration with a low-grade (bottom third) agency, migration with an agency that was not graded but was on the Indonesian government's list of approved placement agencies (an ungraded agency), and migration with an unknown agency. This last category includes all women who either (i) reported that they used an agency but did not know what its name was, or (ii) provided an agency name that was not on the list of government-approved agencies.

Table B.22: 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	0.086*	-0.008	-0.001
	(0.045)	(0.031)	(0.034)
β_2: Comic	0.046	-0.067**	0.004
	(0.044)	(0.028)	(0.031)
β_3: Infographic	-0.081	0.042	0.009
	(0.061)	(0.043)	(0.045)
P-value: Comic + Report Card vs Control:			
$\beta_1 + \beta_2 + \beta_3 = 0$	0.237	0.274	0.718
Control Mean	-0.243	-0.078	-0.032
N	2415	2411	2416

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

Table B.23: Differences in In-Person and Direct Interview Rates At Endline Among Migrants

	(1) In Person Interview	(2) Direct Interview
β_1: Report Card	-0.041	-0.043
	(0.031)	(0.036)
β_2: Comic	-0.047	-0.042
	(0.032)	(0.035)
β_3: Infographic	0.055	0.040
	(0.043)	(0.049)
P-value: Comic + Report Card vs Control:		
$\beta_1 + \beta_2 + \beta_3 = 0$	0.272	0.218
Control Mean	0.288	0.594
N	1730	1730

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≤ 0.10.

Table B.24: Baseline Differences Between Directly Interviewed and Unreachable Migrants by Treatment Arm

	(1) Control Mean	(2) γ_1: Report Card	(3) γ_2: Comic	(4) γ_3: Infographic	(5) γ_4: Report × Informant	(6) γ_5: Comic × Informant	(7) γ_6: Infographic × Informant	(8) γ_7: Informant Interview	(9) P-value: $\gamma_1 = \gamma_2 = ...=\gamma_7 = 0$	(10) N
Predicted Experience: Extended Covariates	0.00892	-0.00527	-0.00249	-0.000922	-0.00916	-0.00425	0.0147	-0.00155	0.307	1730
Age	28.33	-0.660	0.603	-0.588	0.998	0.229	-0.875	-0.461	0.146	1730
Single	0.167	0.0678*	0.0261	-0.0257	-0.119**	-0.00998	0.0664	0.0435	0.163	1730
Married	0.672	-0.0313	-0.0283	0.0402	0.0992	0.00257	-0.0406	-0.0487	0.536	1730
Divorced, Separated or Widowed	0.160	-0.0366	0.00222	-0.0144	0.0194	0.00741	-0.0257	0.00520	0.398	1730
Did Not Graduate Primary School	0.0802	-0.00954	0.0125	0.0172	0.0277	-0.00740	0.0117	-0.0218	0.555	1730
Primary School Graduate	0.377	0.0163	-0.00528	-0.0252	0.0225	0.0835	-0.147	0.0299	0.167	1730
Secondary School Graduate	0.542	-0.00674	-0.00722	0.00806	-0.0502	-0.0761	0.135	-0.00814	0.547	1730
Fraction Correct: Raven's Test	0.685	-0.0209	0.00230	0.0121	0.0224	-0.0166	-0.00362	-0.0138	0.758	1730
Fraction Correct: Math Problems	0.412	0.00585	0.00111	-0.0151	-0.0153	-0.0137	0.0430	-0.0161	0.998	1730
High (Above-Median) Locus of Control	0.491	0.00111	-0.0289	-0.0716	-0.0553	-0.00896	0.144	-0.145***	0.0104**	1730
Plans to Migrate to MENA Country	0.260	-0.0192	0.0122	0.0153	0.134**	0.0671	-0.193**	0.0291	0.212	1709
Plans to Migrate to Asian Country	0.733	0.0185	-0.00920	-0.0140	-0.134**	-0.0650	0.193**	-0.0269	0.208	1709
Plans to Use a Sponsor	0.661	0.0379	0.0795*	-0.0715	-0.0468	-0.141**	0.160*	-0.0528	0.376	1703
Plans to Go Directly to an Agency	0.238	-0.0326	-0.0805**	0.0831	0.0269	0.158***	-0.148*	0.0475	0.137	1703
Expected Salary (Millions IDR)	5.856	0.223	0.100	-0.126	-0.809*	-0.670	1.055*	0.361	0.0886*	1668
Expected Hours of Work Per Day	12.17	-0.109	-0.0789	0.0296	0.987	0.754	-0.779	-0.442	0.722	1644
Probability Gets Day Off	0.497	0.0397	-0.0462	-0.00902	-0.103**	-0.0524	0.154**	-0.0353	0.0259**	1730
Probability Experience Physical Abuse	0.182	0.0543**	0.0225	-0.0494	-0.0782**	-0.0340	0.118**	-0.0326	0.116	1730
Has Migrated Before	0.698	-0.0285	0.0417	0.00590	0.00434	-0.0602	0.0528	-0.0436	0.456	1730

Notes: Estimates are based on regressions that control for strata fixed effects. Standard errors are heteroskedasticity robust, and clustered at the village level. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10

Table B.25: Baseline Differences Between Directly Interviewed and Unreachable Migrants by Treatment Arm: Inverse Probability Weighted

| | (1) | (2) | (3) | (4) | (5) γ_4: | (6) γ_5: | (7) γ_6: | (8) γ_7: | (9) P-value: | (10) |
	Control Mean	γ_1: Report Card	γ_2: Comic	γ_3: Infographic	Report × Informant	Comic × Informant	Infographic × Informant	Informant Interview	$\gamma1 = \gamma_2 =$ ...$=\gamma_7 = 0$	N
Predicted Experience: Extended Covariates	0.00884	-0.00588	-0.00431	0.000853	-0.00384	0.000499	0.00633	-0.00469	0.625	1730
Age	28.32	-0.0712	0.813	-1.193	-0.279	-0.132	0.428	-0.0968	0.358	1730
Single	0.171	0.0209	0.0221	-0.00283	-0.00102	0.00268	-0.00126	0.000948	0.958	1730
Married	0.670	0.00622	-0.0284	0.0282	0.00314	-0.00137	-0.00295	-0.0000437	0.982	1730
Divorced, Separated or Widowed	0.159	-0.0271	0.00631	-0.0254	-0.00212	-0.00131	0.00420	-0.000904	0.489	1730
Did Not Graduate Primary School	0.0819	-0.00162	0.00745	0.0273	0.0125	0.0000187	-0.0167	0.00844	0.817	1730
Primary School Graduate	0.378	0.0207	0.0291	-0.0781	-0.00519	-0.000116	-0.00863	0.0148	0.735	1730
Secondary School Graduate	0.540	-0.0190	-0.0366	0.0509	-0.00726	0.0000977	0.0253	-0.0232	0.966	1730
Fraction Correct: Raven's Test	0.684	-0.00661	-0.00436	0.00548	-0.00472	-0.00221	0.00838	-0.00365	1.000	1730
Fraction Correct: Math Problems	0.409	0.00913	-0.00501	-0.00732	-0.00846	0.000686	0.0213	-0.0152	0.999	1730
High (Above-Median) Locus of Control	0.485	-0.0130	-0.0346	-0.0184	-0.000164	-0.00384	0.0165	-0.0125	0.802	1730
Plans to Migrate to MENA Country	0.259	0.0288	0.0428	-0.0494	0.00466	-0.0134	-0.0129	0.0251	0.960	1709
Plans to Migrate to Asian Country	0.734	-0.0290	-0.0383	0.0498	-0.00456	0.0154	0.0128	-0.0248	0.971	1709
Plans to Use a Sponsor	0.655	-0.000204	0.0234	0.0230	0.0272	0.00197	-0.0402	0.0190	0.965	1703
Plans to Go Directly to an Agency	0.238	-0.00979	-0.0154	0.00434	-0.0193	-0.000830	0.0312	-0.0178	0.998	1703
Expected Salary (Millions IDR)	5.831	-0.107	-0.166	0.302	-0.0279	0.0182	0.0510	-0.0412	0.991	1668
Expected Hours of Work Per Day	12.13	0.364	0.292	-0.419	-0.134	-0.176	0.438	-0.202	0.988	1644
Probability Gets Day Off	0.496	-0.000795	-0.0665**	0.0520	-0.00235	0.00225	-0.00230	-0.00201	0.238	1730
Probability Experience Physical Abuse	0.181	0.0215	0.0107	-0.00312	-0.00912	-0.00574	0.0129	0.00105	0.917	1730
Has Migrated Before	0.699	-0.0166	0.0230	0.00843	-0.00636	-0.00324	0.0171	-0.00914	0.911	1730

Notes: Estimates are based on regressions that control for strata fixed effects. Standard errors are heteroskedasticity robust, and clustered at the village level. Observations are re-weighted so that baseline means in each treatment arm among directly-interviewed and informant-interviewed migrants match overall treatment arm mean. * p≤ 0.10, ** p≤ 0.05, *** p≤ 0.10

Table B.26: Impact on Pre-Departure Preparation and Experience Abroad: Inverse Probability Weighted

	Standardized Experience Indices		
	(1) Pre-Departure Preparation	(2) Migration Experience: Job Quality	(3) Migration Experience: Pay
β_1: Report Card	0.168***	0.107**	0.002
	(0.063)	(0.044)	(0.046)
β_2: Comic	0.103*	0.100**	-0.003
	(0.058)	(0.045)	(0.047)
β_3: Infographic	-0.186**	-0.144**	-0.022
	(0.086)	(0.060)	(0.066)
P-value: Comic + Report Card vs Control:			
$\beta_1 + \beta_2 + \beta_3 = 0$	0.152	0.142	0.648
Control Mean	-0.001	0.002	0.006
N	1005	990	996

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≤ 0.10. Sample limited to directly interviewed migrants. Observations are re-weighted so that baseline means in each treatment arm among directly-interviewed migrants match overall treatment arm mean.

C Data Appendix

This appendix provides additional detail on the various data sets we use and how we constructed outcomes variables. We also provide additional detail on how we trained random forests to predict migrant experience.

C.1 Original Survey Data

We collected four rounds of survey data, beginning with a baseline survey, which took place between April and June 2015. The "tracking sample" (4,805 women interested in migrating in the future; half with prior migration experience and half without) took a long-form survey, which collected information on personal characteristics, including education, cognitive ability, risk attitudes, beliefs about migration, and (for former migrants) details of the woman's most recent migration experience. An additional sample of 5,607 women who had migrated before received a short survey, which focused on past migration experience. We use baseline data for the the following purposes:

- Constructing agency quality rankings, which populate the report cards

- Calculating average migration outcomes by agency quality quintile, which informs the infographic

- Verifying randomization balance

- Constructing measures of predicted migration experience used in the selection analysis in Section 6.4

We targeted the tracking sample for three more survey rounds. A first short-run follow up (midline 1) took place between August and December 2016. The second follow-up (midline 2) spanned October to December 2017, and the final endline survey took place between May and October of 2019. During the endline we also interviewed an additional, 2,418 women who were not part of the tracking sample but had migrated during the post-intervention period. The midlines were very short, collecting data on basic migration outcomes (e.g. whether migrated, what destination, which agency), plans for future migrations, and beliefs about migration outcomes. The endline was longer, collecting additional detail on the migration process, costs and payments associated with migration, and women's experiences at the agency and abroad. The endline also collected data on beliefs about migration outcomes, current economic activity, and household income and assets. Since we observe differential direct interview rates during both midlines but not the endline, we use the endline to construct primary outcomes whenever possible.

The remainder of this section provides additional detail on how key variables used in the analysis were constructed. Unless otherwise noted, we do not use informant responses when constructing variables.

- **Exposure to interventions.** Community meeting attendance is based on women's self-reports of whether they attended a community meeting about migration in the village in 2015. We did not specify the content of the meeting beyond this. For women interviewed in person, enumerators displayed samples of the infographic, report card, and comic and asked women if they recognized

them. Women interviewed over the phone were asked to report exposure based on a verbal description of the intervention material.

- **Migration.** *Migrated post intervention* is based on a woman's report of whether she migrated abroad in October 2015 or later. *Migrated with agency* indicates a woman migrated abroad in October 2015 or later *and* used an agency to migrate. *Migrated without agency* indicates a woman migrated abroad in October 2015 or later and *did not* use an agency to migrate. We use informant reports whenever a woman was not directly interviewed.

- **Migration destination and occupation.** We record the destination country for each migration, and classify countries according to whether they are in Asia, the Middle East and North Africa (MENA), or other parts of the world. To assess occupation, we ask migrants about the type of work they perform, allowing them to specify multiple options. Formal sector work includes non-domestic helper and non-caregiver jobs typically done outside the home for a wage (factory worker, cruise ship attendant, cashier, etc.).

- **Migration plans.** To capture general interest in migration, we ask women whether they plan to migrate (again) in the future. To measure firmness of plans we also construct a dummy variable indicating that the woman plans to migrate within the next year. We also asked women if they had completed the following pre-migration tasks: secured family permission, got permission from the village head, chosen a sponsor, and chosen an agency.

- **Beliefs.** In order to elicit beliefs about migration outcomes, we first prompted the respondent with a migration outcome, such as "gets at least one day off per week". We then asked the respondent to report how many female migrants in their village (out of 10) experience this outcome, using – when interviewing in person – a card with a line of 10 women to assist with visualization. For a subset of scenarios, we also asked the respondent to report her likelihood of experiencing the same, again on a 0 to 10 scale. When constructing endline beliefs indices we limit attention to beliefs questions included in both the phone and in person survey instrument. We follow Kling et al. (2007) when constructing standardized indices, first imputing missing index components with the treatment group mean, then standardizing each component relative to the control group mean and standard deviation, and finally averaging all components into an overall index. 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 more than 12 hours per day, 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 analogs 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.

- **Migration experience.** Our analysis focuses on the subset of experience measures collected as part of both the phone and in-person survey instruments. We follow Kling et al. (2007) when constructing standardized indices, first imputing missing index components with the treatment group mean, then standardizing each component relative to the control group mean and standard deviation, and finally averaging all components into an overall index. 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 (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. All outcomes in this index 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.

- **Agency choices.** We use migrants' reports of their placement agency's name to construct agency quality classifications. During each survey round we first mapped the migrant reports to the list of placement agencies sanctioned by the Indonesian government at the time of the survey. (For post-baseline survey rounds we updated our original baseline list, keeping the names of any agencies that lost certification over the ensuing years.) If a woman did not know the name of her agency, or the agency was not on the list of sanctioned agencies, we classify this migration as one with an "unknown agency". Migrations with sanctioned agencies that were not listed on the report card are classified as "ungraded agency" migrations. Finally, we split migrations with graded agencies into "high grade" (top third of grade distribution among realized endline migrations), "average grade" (middle third), and "low-grade" (bottom third) migrations.

- **Household income and expenditure.** The endline survey asked either the tracking sample

woman (in the case of in-person interviews) or the informant (when the focal woman was either unavailable or interviewed over the phone) to report on household economic wellbeing. To calculate *household income*, we aggregate reports of wage earnings and business profits earned by residents in the past month, average monthly remittances from non-resident household members, and average monthly agricultural and "other" income (including rents, social protection payments etc.). Agricultural income is calculated as the market value of output (consumed and sold) less the cost of inputs, including labor. We value family labor at market rates when calculating costs. Total expenditures reflect the respondent's report on all spending in the past month. We separately ask respondents to report food expenditure in the past month.

- **Household assets and dependence on social protection.** The endline survey asked either the tracking sample woman (in the case of in-person interviews) or the informant (when the focal woman was either unavailable or interviewed over the phone) to provide details on household assets and receipt of social protection. Enumerators also recorded the quality of the home based on observation. The *housing quality index* is constructed using factor analysis. We first create a series of dummy variables identifying the home's type of roof, wall, floor, source of drinking water, and toilet facility. The first factor assigned opposite-signed weights to higher vs lower quality construction (e.g. a tile versus a palm roof), so we extract that factor, rotate it so higher values indicate better outcomes, and standardize it relative to the control group mean and standard deviation. To construct the *asset index* we run factor analysis on a series of dummy variables indicating ownership of a bicycle, motorcycle, boat, television, air conditioner, heater, gas stove, refrigerator, motor boat, car, house, and land. We extract the first factor, which weights assets positively, with larger weights on "higher status" assets like a motorcycle, television, and house, and standardize it relative to the control group. Finally, we create a *dependence on social protection index*, which uses factor analysis to aggregate dummy variables for receipt of seven of Indonesia's biggest social assistance programs/social assistance cards (Program Keluarga Sejahtera, Bantuan Siswa Miskin, Kartu Keluarga Sejahtera, Kartu Indonesia Pintar, Kartu Indonesia Sehat, Jaminan Kesehatan Nasional, and Raskin/Bantuan Pangan Non Tunai) – again we extract the first factor, rotate it so higher values correspond to better outcomes, and standardize it relative to the control mean.

- **Occupational status.** We asked all tracking sample women (or informants, in cases where a direct interview was not possible) to report on the woman's current occupation, regardless of migration status. Women currently abroad are classified as "on migration" regardless of what they do abroad. Women in Indonesia as classified as either unemployed (this includes women who are not actively seeking work), an unpaid family worker, a casual worker (low skill, irregular work often paid in daily or weekly installments), a wage worker, or self employed.

- **Monthly earnings.** Women in Indonesia were asked to report their total earnings in the past month. Non-workers are coded as earning zero income. For women on migration, we calculate total earnings less deductions to date, divided by the number of months abroad.

C.2 Administrative Data

We use administrative data on migrant departures and returns in our analysis. Here we describe the different datasets we received and how they were processed to facilitate analysis.

- **Placements Data: 2011-2013.** Before starting the project, we obtained administrative placements data for all of 2011, 2012, and part of 2013, which we used to create a list of sample villages and create some of our randomization strata. These data include information on the migrant's gender, agency, destination, home district, and address, the latter of which was written in a non-standard format. We limited the sample to our eight study districts and extracted sub-district and village names from the full addresses using a combination of code and hand inspection. We were able to match 94 percent of 2012-2013 female placements in our study districts to village codes. We use these data to calculate the number of migrants per village, as well as the Herfindahl index based on 2012 and 2013 placements.

- **Repatriation Data: 2010-2013.** We received data on migrant returns processed by a dedicated terminal for return migrants at Soekarno-Hatta airport in Jakarta between 2010-2013. While not all return migrants passed through this (now defunct) terminal, a large number did, with numbers processed ranging from 135,289 in 2013 to 357,854 in 2010. These data include information on the migrant's gender, home address, country of work, and agency. The data also record the reason why the migrant returned, including end of contract, visit to Indonesia, or "troubles", a catch-all term meant to identify migrants returning early due to issues abroad. We combine agency-specific counts of troubled workers in 2013 with the total number of agency departures in 2011 (since most contracts are two years long) to construct a proxy of agency quality, which we used to validate our own agency grades.

- **Placements Data: 2015-2019.** After the end of field activities we received a final transfer of placements data spanning 2015-2019. These data include information on the month of departure, destination, placement agency, migrant gender, and migrant address. The government coded province, district, sub-district, and village names in these data and did not provide string addresses. The percent of female placements in our eight study districts with a valid village name is 50.4, 68.6, 83.8, 91,8, and 91.3 percent in 2015, 2016, 2017, 2018, and 2019 respectively. Overall, 83 percent of records in the post-intervention period have a valid village name. We use these data to construct measures of total departures by village, departures by destination, and to construct measures of agency market share. We also use these data to visualize the geographic distribution of migration rates in Appendix Figure B.1.

C.3 Secondary Data

In order to calculate the number of female migrants per capita, which we used to stratify our randomization, we require village-level population estimates. We use the Village Potential (*Podes*) triennial administrative census from 2011, available from the Indonesian Central Statistical Agency (*Badan Pusat Statistik*, BPS) . This survey provides information on key characteristics of all Indonesian villages typically reported by the village head.

C.4 Random Forest Predictions of Migration Experience

We train a random forest to predict migration experience associated with migrations recorded at baseline. To do this, we first construct baseline analogues of the pre-departure preparation, job quality, and pay indices. The components of the indices are the same, except we eliminate a dummy variable indicating that the migrant used an agency from the pre-departure preparation index, since – by design – all migrants interviewed at baseline had used an agency. To simplify the analysis we then average the three indices to create a single overall measure of experience.

We train the algorithm on tracking sample women with past migration experience at baseline. When training the forest we enable honest splitting, meaning the sub-samples used to determine a tree's splits differs from that used to populate the leaf nodes (Wager and Athey, 2018); all available parameters (the fraction of the sample used to build each tree, the number of variables tried for each split, the minimum number of observations in each tree leaf, the fraction of data used for determining splits, whether to prune estimation sample trees so no leaves are empty, the maximum imbalance of a split, and the imbalance penalty) are tuned via cross validation.

When preparing variables for the forest, we convert all categorical variables into dummy variables, with missing values assigned their own category. We construct separate dummy variables to identify missing values of continuous variables, and then recode missing continuous variables to the mean. The following list details all variables included in the forest.

- Age
- Marital status
- Education
- Randomization strata
- Ethnic group
- Can read
- Can write
- Dummies identifying preferred gamble in an incentivized risk task
- Discount factor implied by a series of hypothetical monetary choices
- Beliefs for self, and others in village (out of 10): will receive good pocket money from sponsor, sponsor will take person to a good agency, sponsor will not extort, agency will provide clean food and water, agency staff will treat migrant with respect, migrant will be allowed outside agency, agency will follow legal procedures, agency will provide accurate information, agency will provide information about migrant worker rights, migrant will get weekly day off, migrant will receive presents, migrant will not be paid less than contract, migrant will not have to work more than 12 hours/day, migrant will not return early, migrant will not suffer physical abuse
- Expected number days off: more than once a month, approximately once a month, at least twice a year, approximately once a year, never
- Expected salary net of fees
- Expected hours of work per day
- Year (as of baseline) hopes to migrate
- Plans to migrate to Asia
- Plans to migrate to MENA

- Plans to skip sponsor, go directly to agency
- Dummies for planned occupation abroad (elder care, babysitter, nurse, domestic worker, driver, store/restaurant/hotel staff, agricultural worker, mining worker, construction worker, factory worker, cruise ship crew, other)
- Dummies for who received advice on sponsors from (family, friends/neighbors, village head, agency, another sponsor, other)
- Dummies for who received advice on agencies from (family, friends/neighbors, village head, agency, another sponsor, other)
- Has chosen a sponsor
- Number of sponsors known from village
- Number of sponsors known from outside village
- Number of sponsors talked to from village
- Number of sponsors talked to from outside village
- Dummies identifying woman's reported qualities of a good, bad sponsor
- Dummies identifying woman's reported qualities of a good, bad agency
- Trust questions: most people can be trusted/need to be careful; would a lost purse with IDR 200,000 and an ID card be returned by someone outside your village: very likely, somewhat likely, somewhat unlikely, very unlikely
- Mental health inventory, dummies identifying often/sometimes/never to the following scenarios in the past 4 weeks: had trouble sleeping, been bothered by things that don't usually bother, felt lonely, experienced sadness, experienced anxiety or fear, had difficulty concentrating, normal tasks felt like an effort, had difficulty remembering/recalling something
- Fraction correct: Raven's Matrix questions
- Fraction correct: Math questions
- Locus of control score
- Big 5 scores: extraversion, agreeableness, conscientiousness, neuroticism, openness

D   Constructing Measures of Agency Quality

D.1   Indicators of Quality of Migration Experience

An inherent challenge to measuring agency quality is that there are many potential indicators spanning both inputs (e.g. time and quality of pre-departure training, effort made to ensure the migrant is well informed of her contract and rights, quality of partner agency in the destination country) and outputs (compensation, working conditions, employer quality). We estimated quality by first creating an aggregate "migration experience" measure, which combines measures across the following domains:

- **Experience with the Indonesian Placement Agency:** This domain includes indicators for whether the woman received at least 10 days/2 weeks training (the lowest legal minimum at the time of the study), the number (out of 11) of key topics a migrant was trained on, an indicator for whether the woman signed a contract while at the PT, and an indicator for whether the woman's job was in accordance with her contract.

- **Experience with the Destination Placement Agency:** This domain included indicators for whether a woman worked with an agency in the destination country, whether the agency picked her up at the airport (versus the employer), whether the agency allowed the migrant to retain her own personal documents, whether the agency gave the woman information on migrant worker rights and whether the agency gave the migrant information on how to seek help in the destination country.

- **Compensation:** This domain consists of average monthly pay (both regular salary and bonuses) net of migration costs.

- **Experience with the Employer:** This domain included indicators for whether or not the migrant experienced salary cuts, received at least one day off per week, was not required to work more than 12 hours per day, was paid for overtime, was given proof of payment, did not sustain injuries on the job, was allowed to contact her family, was allowed to pray, was not paid late, experienced retained salary payments, was fired, was allowed to retain her personal documents, experienced physical abuse, experienced sexual abuse, experienced verbal abuse and experienced other forms of abuse. All the indicators were constructed so that a value of 1 signaled a positive experience (e.g. no salary cuts, allowed to pray) and 0 signaled a negative experience.

We created an aggregate index by standardizing each of the above index components, calculating the average within each domain, and then taking a simple average across the four domains. We chose these inputs for two reasons. First, qualitative research with former and potential migrants and interviews with NGO and government stakeholders suggested that these inputs are important determinants of migrants' experiences. Second, we found that this combination of inputs was particularly successful at identifying high-performing agencies in our out-of-sample validation tests.

D.2 Estimating Agency Quality

We now use our index of migration experience quality to obtain estimates of agency quality. Migrant experience is assumed to be determined by three main factors: First is the migrant's qualifications and skill as a worker – for example, more qualified migrants will likely be paid higher salaries and secure jobs with better amenities. Second is the input of her agency – the agency can improve a woman's experience in several ways, e.g. by providing comprehensive training, providing information on migrant workers' rights and by partnering with more reputable counterpart agencies in the destination country. Third are all other factors – such as whether a migrant is "lucky" and gets a kind, considerate employer. We can represent this in equation form:

$$experience_{ia} = \mathbf{qual}'_{ia}\boldsymbol{\eta} + \gamma_a + \varepsilon_{ia} \tag{A.14}$$

where $experience_{ia}$ is the experience of migrant i migrating with agency a, $\mathbf{qual}_{ia}$ is a vector of characteristics capturing migrant quality/experience, γ_a is an agency fixed effect, and ε_{ia} is an error term capturing idiosyncratic factors. Given this structure, the agency effect will also capture systematic variation across agencies in terms of job characteristics including destination country and job type. We decided to include this in the agency effect because, conditional on migrant qualifications, this can be an important driver of migrant welfare.

We employ empirical Bayes techniques, commonly used in the economics literature measuring teacher quality (Chetty et al., 2014; Herrmann et al., 2016; Kane and Staiger, 2008; McCaffrey et al., 2004), to obtain our measure of agency quality. A key advantage of this technique (as compared to estimating agency fixed effects via OLS) is that it "shrinks" agency effects towards the mean in a way that is proportional to sample size. Thus, when fewer migrant ratings are available, there is more mean reversion. To operationalize this, we first use OLS to residualize out measures of migrant quality, including dummy variables for highest education, age, age squared, migration year dummies, and district of residence dummies. We then construct empirical Bayes estimates for the agency effects and discard estimates for agencies with fewer than 30 migrants in our survey. We re-scale the remaining estimates to run from a minimum of 50 to a maximum of 95, in order to mimic the distribution of grades in the Indonesian school system.

After constructing the ratings, we ran out-of-sample validation tests to ensure that our measures of quality could predict the experiences of migrants outside our estimation sample. Here, we correlated our rankings with rates of problems as recorded in government repatriation records (see Appendix C for detail), as well as with the International Organization for Migration's (IOM's) records on verified victims of trafficking. We drop all women in the government data who live in our study districts to ensure that there is no overlap between the women in the government repatriation records and our survey data. Appendix Figure D.1 shows the average rate of problems in government data, per 2013 arrivals records, and the rate of victims of trafficking for three categories of agencies: those with grades less than 65 (roughly the bottom third of agencies), those with grades 65-85 (roughly the middle third of agencies) and those with grades above 85 (roughly the top third of agencies). Note that there is no bar in Panel B for the "grade above 85" group, because no agencies in this group had recorded victims of trafficking.

It is clear that migrants who choose agencies with higher grades experience fewer problems. We have also verified that these correlations hold up when we limit our analysis to agencies that are legally certified to place women outside the Middle East. This is particularly important given the government moratorium on sending informal sector workers to this region of the world.
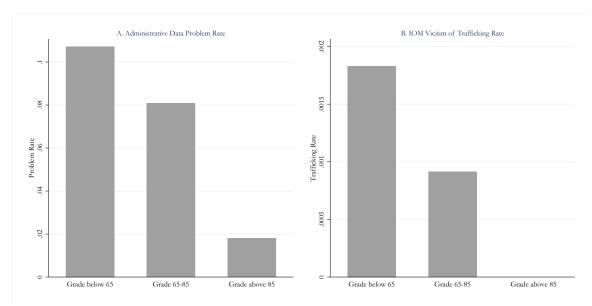
Figure D.1: Out-of-Sample Validation of Agency Quality Index

Notes: Panel A reports the mean rate of problematic repatriations in 2013 for agencies that we score below 65, 65-85 and above 85. Panel B reports the corresponding means in trafficking reported by the International Organization for Migration.