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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 conducted a randomized trial in Indonesia to explore how information about intermediary quality shapes migration choices and outcomes. Information reduces the migration rate, lowering use of low-quality intermediaries. However, workers who migrate receive better pre-departure preparation and have higher-quality job experiences abroad, despite no change in occupation or destination. Information does not change 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: with better ability to differentiate offer quality, workers become choosier and ultimately have better migration experiences. This offers a new perspective on the importance of information and matching frictions in global labor markets.

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A data appendix is available at <http://www.nber.org/data-appendix/w29588>

# 1 Introduction

Information frictions are a prominent feature of many labor markets. Employers often struggle to evaluate applicant skill while job seekers face challenges in assessing the relative risks and returns to different employment opportunities. The latter frictions are especially salient in international labor markets where workers must match with employers abroad and navigate complex migration processes. 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 face additional information frictions regarding intermediary quality, and little is known about how these entities shape migration choices and outcomes. 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](#)). In this paper, we offer a new perspective on labor intermediaries and 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 randomly 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, 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, slow the migration rate, and ultimately improve migration outcomes.

The international labor migration process in Indonesia is similar to other low- and middle-income countries across Asia and beyond. Every year, hundreds of thousands of low-skill workers go abroad on temporary contracts lasting 2–3 years. Mirroring global trends, women comprise a sizeable share of all Indonesian migrants.<sup>1</sup> They specialize in domestic work and elderly care, and their incomes far exceed those prevailing in labor markets back home. While migrant wages are contractually fixed through bilateral legislation, the quality of pre-departure placement and training, as well as on-the-job amenities tend to vary substantially across workers. 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 in certain destinations and

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<sup>1</sup>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.

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, 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 over 60 agencies placing workers during our study period. While this should, in theory, imply 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.

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.<sup>2</sup> We also developed an edutainment-style comic book that told the story of a woman navigating the migration process with 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: 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) with job openings. In this setup, the report card enables potential migrants to better differentiate high- and low-quality offers. Meanwhile, the comic may strengthen the effect of the report card by encouraging attention to quality and 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 identify 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 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

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<sup>2</sup>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, hours worked, employer abuse), and pay amount and structure.

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 comic shifted beliefs about average agency quality and experiences while abroad; this may reflect the infographic’s role in anchoring beliefs across treatment arms.

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., employment and living conditions) by roughly 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. Rather, such improvements appear to stem from migrants avoiding riskier offers. The report card not only reduced migration without an agency but also helped potential migrants avoid agencies that were not 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 counterfactually chosen low-quality agencies and/or taken low-quality jobs. Using two distinct approaches to investigate selection, we find no evidence of such effects. Nor do we find spillovers or market-level effects on agencies, which is consistent with Indonesian villages being large, our interventions only reaching a subset of potential migrants, and placement agency catchment areas being large and diffuse. Thus, our results are not driven by 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.

The financial returns to migration in our setting are high, with the median migrant earning over six times more than the median Indonesia-based worker. By delaying migration, women postpone access to these returns.<sup>3</sup> This motivates a revealed-preference estimate of the value of information contained in the report card. By comparing discounted earnings flows at migration rates observed with and without the report card, we infer that women who received the report card should expect to earn USD 304 less than those without the card. This amounts to roughly half of one month’s wages abroad. This suggests information has significant value to women, an important finding given widespread concern that many migrants across the globe work in difficult conditions abroad to provide for their families back home.

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 migration intermediaries outside of a small literature on the smuggling industry, which is focused more on illegal border crossings than on labor

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<sup>3</sup>We find no significant treatment effects on household or individual income, but we are only powered to detect very large effects given the report card’s 4.4 percentage point effect on the migration rate.

market intermediation (Auriol and Mesnard, 2016; Friebel and Guriev, 2006; Gathmann, 2008; Tamura, 2010). A notable exception is Fernando and Singh (2021), who show 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 address worker-side responses to such information. We therefore fill an important gap, as intermediaries are central to a thriving international labor migration industry (Agunias, 2009), and understanding how information affects use of their services 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 provides a new perspective on the foundations of migration choice in settings with large wage differentials. A large literature, on both domestic and international migration, has explored barriers related to information, liquidity, risk, and utility costs (e.g., Angelucci, 2015; 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. Our results further suggest that the option value channel could be more important in settings where migrants have *better* information on the quality of different offers.

Migration has long been conceived as an investment strategy (Sjaastad, 1962), but Burda (1995) was one of the first to formulate migration choice under uncertainty as an option value problem.<sup>4</sup> 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.<sup>5</sup> 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 fresh insights: for example, migration rates may be *lower* in markets where information frictions are smaller, provided offers arrive sequentially. Thus, to foster high-quality 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 (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

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<sup>4</sup>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.

<sup>5</sup>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.

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](https://www.glassdoor.com) and [indeed.com](https://www.indeed.com)). Limited information on job quality—especially among rural-urban migrants—might also contribute to the high turnover rates documented by Donovan et al. (2021) across many low-income countries.

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 with 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 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 types of workers from traveling to high-risk destinations, as well as new regulations governing the placement and protection of migrant workers.<sup>6</sup>

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<sup>6</sup>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



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 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 informed by extensive qualitative work we conducted before designing the intervention.<sup>7</sup> 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).<sup>8</sup> 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

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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.

<sup>7</sup>This includes 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).

<sup>8</sup>This does not apply to migrants seeking employment through irregular channels. Nor does it apply to higher-skilled migration 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).



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 placement agency. These agencies partner with Indonesian agencies to identify and place migrant workers, with the destination 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 if problems arise and facilitating repatriation once a contract ends.

**Migration Quality and the Role of Agencies.** Migration quality is determined by both monetary and non-monetary factors. Migrants’ wages are typically set through bilateral memorandum of understanding (MOU) legislation and vary by occupation and destination country. The MOUs often result in binding minimum wages for migrant labor (see McKenzie et al., 2014, for evidence from similar 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.<sup>9</sup>

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.<sup>10</sup> 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 $\times$ 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, non-pecuniary aspects of quality follow a distinct pattern consistent with greater scope for agency influence. Agency fixed effects explain relatively more of the variation in

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<sup>9</sup>Fernando and Singh (2021) find evidence of the same among Sri Lankan migrant worker placement agencies.

<sup>10</sup>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.

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  $\times$  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 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.<sup>11</sup> The median village had 61 active agencies with a Herfindahl of 0.05, pointing to considerable competition. While some villages exhibit greater agency concentration (see the long tail in 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 6), 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.

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.<sup>12</sup> 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.

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<sup>11</sup>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.

<sup>12</sup>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.

Finally, Panel E shows limited status quo knowledge of agencies. 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 “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 interventions aim to remedy information failures in two ways: first, by providing new information on agency quality based on the experience of thousands of women across hundreds of migrant-sending communities; second, by encouraging migrants to act on already-available information and to search locally for information. To do this, we developed original measures of agency quality, as well as three information products described here.

**Measuring Agency Quality.** We construct quality “grades” using data from nearly 7,000 recently returned migrants who were able to identify their agency. For each migrant, we developed an overall “migration quality” index, which included indicators related to pre-departure training and experience at the agency, experience (if any) at the partner agency in the destination, net pay, and non-monetary aspects of job quality (time off, harassment by 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. These graded agencies account for 75 percent of placements in our baseline data. Our ratings do well out of sample prediction: the grades are significantly correlated with government data on problematic migration episodes in non-study districts, as well as cases of human trafficking recorded by the International Organization for Migration. Appendix D provides additional detail on the methodology, indicators included in the ratings, 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.

**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 that could be readily understood (see Panel B of Appendix Figure B.4). Each of the 101 subdistricts in our study had a unique report card featuring agencies that operated locally. The cards also included the name and contact information of each agency, as well as the number of reviews used to construct the agency rating.

**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 above/below median (i) share of migrants placed in the MENA region, (ii) agency Herfindahl index, and (iii) number of female migrants per capita. All placement information came from government administrative data in 2012-2013.<sup>13</sup> The 100 villages in the control group did not receive any intervention materials. We distributed both the infographic and the report card in 101 “report card only” villages, the infographic and the comic in 98 “comic book only” villages, and the infographic, the comic book, and the report card in 101 “comic and report card” villages. In these latter, 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.

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

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<sup>13</sup>We used Stata software to randomly assign villages to each experimental arm at a 1:1:1:1 ratio within each stratum.

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 address information frictions migrants face when selecting an agency. Conditional on community meetings and receipt of the infographic, the report card helped women 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 *beliefs about the return to migration*. While the comic 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).<sup>14</sup> 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  $\beta$  to future utility. Each period she remains in Indonesia, she receives utility  $h$ . Migration offers arrive with per-period probability  $\lambda$ . If a woman accepts an offer at time  $t$ , her expected discounted future utility is  $u_t \in [\underline{U}, \bar{U}]$ .<sup>15</sup> 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  $q_t \in [\underline{Q}, \bar{Q}]$  that is informative, in that the woman’s (rational) forecast based on her signal,  $Q_t = \mathbb{E}[u_t | q_t]$ , is strictly increasing in  $q_t$ , and hence the mapping  $q_t \rightarrow Q_t$  is 1:1. Denote the distribution of  $Q_t$  as  $F_Q(\cdot)$ .

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

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

<sup>14</sup>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.

<sup>15</sup>We think of  $u_t$  as comprising both monetary and non-monetary costs and benefits of a given migration.

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:

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

Now consider 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,  $r_t \in [\underline{R}, \overline{R}]$ , which is non-degenerate for at least some  $q_t$ . Denote  $\tilde{Q}_t = \mathbb{E}[u_t | q_t, r_t]$ . 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.<sup>16</sup>

## 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 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  $\sigma$ , which indexes the riskiness of  $F_Q(\cdot)$ .<sup>17</sup> 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 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.<sup>18</sup>

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  $\mathbb{E}[u_t | q_t, \emptyset] = \tilde{Q}_t = \mathbb{E}[u_t | q_t] = Q_t$  for some share of

<sup>16</sup>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.

<sup>17</sup>We assume  $F_Q(Q_t; \sigma)$  is continuously differentiable in both  $Q_t$  and  $\sigma$ .

<sup>18</sup>In both cases, a revealed preference argument implies that expected utility *unconditional* on migration must increase.



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  $\Delta$ , captured by a translation of the distribution  $F_u(\cdot)$  to  $G_u(\cdot)$  such that  $F_u(x) = G_u(x + \Delta) \forall x \in [\underline{U}, \bar{U}]$ . 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  $\Delta$  is only partially passed through to the woman’s posterior,  $Q_t$ :  $F_Q(x) = G_Q(x + \eta\Delta)$ ,  $\eta \in (0, 1)$ .

In Appendix A, we show that shifting priors by  $\Delta > 0$  will increase reservation expected utility by a factor less than  $\eta\Delta$  and increase the migration rate.<sup>19</sup> 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.<sup>20</sup>

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

## 5 Empirical Strategy

This section describes our survey and administrative data as well as the core 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

<sup>19</sup>A shift of  $\Delta < 0$  will reduce reservation expected utility and reduce the migration rate.

<sup>20</sup>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.



villages, dropping very large villages.<sup>21</sup> 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 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.<sup>22</sup> We enrolled two samples of women. The first group, a “tracking” sample of 4,805 women, stated they were interested in migrating in the near future. We follow these women in all follow up rounds, and they comprise our core analysis sample. We stratified the tracking sample by migration history: 2,403 women had migrated before while 2,402 had not. The second group, a “former migrant” sample of 5,607 women, had previously migrated with an agency. We used data from these women to construct agency quality measures but did not target them for future follow up.

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

After intervention implementation in late 2015, the tracking sample participated in three follow up surveys: (i) from August to December 2016, (ii) from October to December 2017, and (iii) the final “endline” from May to October 2019. All three waves collected data on new migrations. We also collected data on expectations and beliefs about migration. For women 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., 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 had departed for overseas work after the intervention began but who were not explicitly invited to attend community information-sharing meetings. Thus, we use this sample to test for the reach of our intervention.

## 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

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<sup>21</sup>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.

<sup>22</sup>Among former migrants, we prioritized women who migrated with an agency and returned home in 2014 or 2015.

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.

We estimate the following regression to difference out the effects of the meeting and infographic:

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

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 treatment villages). This fully saturated regression specification lets us focus on the treatment effects that map back to our theoretical framework:  $\beta_1$  is the effect of the report card *conditional on distributing the comic book*, while  $\beta_2$  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  $\times$  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,  $\beta_3$  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  $\beta_3$ , 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 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 higher.

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 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 treatment arms in follow-up 3. Whenever possible, we rely on this final rounds 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 among the tracking sample, 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 cannot be explained by changes in migrant selectivity. We also address robustness and alternative explanations. We close with a discussion of downstream effects on welfare and a revealed-preference estimate of the value of information.

### 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 village-wide 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 +  $\beta_3$ ), 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 recognized 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 the endline survey, which took place 3.5-4 years after the intervention. While self-reported meeting attendance is roughly unchanged, women are significantly more likely to report having seen the information sharing materials, reflecting the supplementary distribution of materials that took place at the end of follow up 2.<sup>23</sup>

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 materials assigned to their community: less than 5 percent recalled attending a community meeting, and we find no evidence of increased recognition of the report card and infographic in treatment villages. Women in comic villages are 6 p.p. more likely to recognize the comic, however. Overall, these results suggest that our intervention's

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<sup>23</sup>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.

reach was somewhat limited and concentrated among the tracking sample, making village-level, let alone market-wide, effects unlikely. Given this, we focus on the tracking sample for the main analysis.

## 6.2 Migration Choice, Intentions, and Beliefs

Table 3 uses the tracking sample at endline—including both direct and informant interviews—to establish our core findings on the migration rate. We focus on departures since October 2015, corresponding to migration following the community meetings in treatment villages. 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. (column 1). This represents a 12 percent reduction in the likelihood of migration relative to control villages, where 36.6 percent had migrated. 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 (columns 2 and 3, respectively). However, the most intensive intervention (comic + report card) reduced migration without an agency by 1.5 p.p., a 37 percent reduction in non-procedural 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 typical 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 occurred roughly six months after the second follow up survey (see gray shading), when we reintroduced the information materials to tracking sample respondents. This suggests 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 cross-sectional 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, rarely 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.<sup>24</sup> We consider three indices capturing beliefs about the migration experience. The *agency* index comprises seven questions about the pre-departure experience with the agency (e.g., whether the agency provides information on migrant worker rights, 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.<sup>25</sup> 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 systematic treatment effects on beliefs in Table 4, either about one’s own potential migration experience (columns 1-3) or that of others (columns 4-6). The estimated null 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. 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 with evidence of why women may be waiting longer to migrate, namely that the intervention materials help secure offers that result in better migration experiences.

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<sup>24</sup>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.

<sup>25</sup>The agency index includes beliefs about whether the agency will allow the migrant to leave the training facility, hold the migrant’s documentation\*, provide information on legal rights\*, follow legal procedures, provide accurate information, provide quality food and water during training, and have respectful staff. The job quality index includes beliefs about whether the migrant will work more than 12 hours per day\*, have a job that matches the contract signed pre-departure\*, have a day off each week\*, retain their salary as scheduled\*, have no wage deductions beyond the contracted amounts\*, retain control of their documentation\*, receive gifts from the employer, complete their full contracted employment, and not experience physical abuse. The infographic index includes all prior measures with an asterisk.

### 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 the quality of migration experiences, 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.<sup>26</sup> Appendix Tables B.5-B.7 report impacts on individual index components.

Table 5 shows that the report card and the comic improved pre-departure preparation within Indonesia and job amenities abroad. Conditional on access to the comic, the 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 of retaining 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.<sup>27</sup> Report-card- and comic-associated gains do not extend to pay, however, which was largely unmoved by the information intervention.

These improvements in migration quality 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 during pre-departure preparation. That said, some of the improvement in job quality may come from working in destinations or occupations that require more preparation or offer better employment conditions (recall from Table 1, e.g., that women working in MENA experience 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

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<sup>26</sup>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 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 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.

<sup>27</sup>These differences persist when we limit attention to women who migrated with an agency (see Appendix Table B.8).



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 migration. Since not all agencies used at endline were rated at baseline, we group endline agencies into six classes: migration without an agency, with a “high-grade” agency (top third of grade distribution), with an “average” (middle third) agency, with a “low-grade” (bottom third) agency, with an agency that was not graded but was on the Indonesian government’s list of approved agencies (an “ungraded agency”), and with an “unknown agency”. This last category includes all women who either (i) reported that they used an agency but did not know its name, 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.

We validate these classifications in Appendix Table B.10, which shows how migration outcomes vary by agency choice. To avoid confounding due to treatment, we limit the analysis to tracking sample women in the control group and the entire supplementary sample.<sup>28</sup> 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.<sup>29</sup>

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<sup>28</sup>We 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. 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.

<sup>29</sup>To provide more granularity, Appendix Figure B.8 limits the sample to the 103 women in the comic only and comic plus



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 migration to MENA countries. Yet, 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 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.

**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,  $\gamma_a = \mathbb{E}[u_t | q_t] - \mu_i$ , and the effect of other individual characteristics unobservable to the econometrician,  $\varepsilon_i$ , where  $\mu_i = \mathbf{x}'_i\boldsymbol{\zeta} + \varepsilon_i$  is the overall expected utility of migration (integrated across the distribution of offers). We also allow for individual variation in outside options,  $Q_i^* = Q^* + \eta_i$ . Putting this together, a woman migrates with agency  $a$  if  $Q_{ia} = \mathbf{x}'_i\boldsymbol{\zeta} + \gamma_a + \varepsilon_i \geq Q^* + \eta_i$ . Conditional on migrating, realized utility is

$$y_{ia} = \mathbf{x}'_i\boldsymbol{\zeta} + \gamma_a + \nu_a + \varepsilon_i + \nu_{ia} \tag{4}$$

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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.

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

$$\mathbb{E}[y_{ia} \mid Q_{ia} \geq Q_i^*; \mathbf{x}_i, a] = \mathbf{x}_i' \boldsymbol{\zeta} + \delta_a + \mathbb{E}[\varepsilon_i \mid Q_{ia} \geq Q_i^*; \mathbf{x}_i, a]. \quad (5)$$

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

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

$$\Delta \mathbb{E}[y \mid \text{migrate}] = \Delta \mathbb{E}[x]' \boldsymbol{\zeta} + \Delta \mathbb{E}[\delta] + \Delta \mathbb{E}[\varepsilon \mid \text{migrate}] \quad (6)$$

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

In order to perform this decomposition, we need to derive consistent estimates of  $\boldsymbol{\zeta}$ ,  $\delta_a$ , 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

$$\mathbb{P}(\text{migrate}_i) = \mathbb{P}(\eta_i - \gamma_a - \varepsilon_i \leq \mathbf{x}_i' \boldsymbol{\zeta}) = \Phi(\mathbf{x}_i' \boldsymbol{\zeta}) \quad (7)$$

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

$$\mathbb{E}[y_{ia} \mid \mathbf{x}_i, a] = \mathbf{x}_i' \boldsymbol{\zeta} + \delta_a + \rho \sigma \lambda(\mathbf{x}_i' \boldsymbol{\zeta}) \quad (8)$$

where  $\lambda(\cdot)$  is the inverse Mills ratio,  $\rho = \text{corr}(\eta_i - \gamma_a - \varepsilon_i, \varepsilon_i + \nu_{ia})$  and  $\sigma = \text{sd}(\varepsilon_i + \nu_{ia})$ . To avoid having identification solely off functional form, we introduce an exclusion restriction motivated by the search model. Note that the report card should impact reservation utility,  $Q_i$ , but have no effect on migration outcomes conditional on agency choice. Thus, we include the treatment indicators in the first, but not the second, stage of the selection model.<sup>30</sup>

With unbiased estimates of  $\boldsymbol{\zeta}$ ,  $\delta_a$ , and  $\rho \sigma$  we can perform the decomposition by first taking the estimated components from the second step of the correction model (as well as the residual, since its average value can vary by treatment) and then regressing each on the three treatment indicators. The resulting coefficients identify the relative contribution of each component mechanism in explaining the treatment effects reported in Table 5.<sup>31</sup>

<sup>30</sup>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. We consider this unlikely given the nature of agency placement and training, as well as the explicit focus of the report card on agency quality.

<sup>31</sup>We calculate standard errors by generating 500 bootstrap samples clustered by village; for each sample we run the

Table 7 reports decomposition results for the three migration experience indices, using agency fixed effects to estimate  $\Delta\mathbb{E}[\delta]$ .<sup>32</sup> The  $\mathbf{x}$  vector comprises a large set of individual characteristics, including age, marital status, education, cognitive ability (measured by fraction correct on a series of Raven’s Matrices and basic math questions), non-cognitive traits (e.g., locus of control, big 5 personality traits), risk and time preferences, prior search experience, and beliefs about migration experiences.

Column 1 of Table 7 suggests 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 (although these 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 tends to load on residual differences, which could indicate that we have not fully accounted for key migrant characteristics, a concern our next approach attempts to address.

**A Machine-Learning Approach to Counterfactual Migration Experiences.** In a second, less parametric, approach to quantifying selection, we develop a measure of predicted migration experience based solely on pre-treatment survey data. Concretely, we train a random forest on a broad set of individual-specific covariates measured during the baseline survey (see Appendix 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.<sup>33</sup> 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 (column 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 compared to the 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. 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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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.

<sup>32</sup>In Appendix Table B.12 we report results that instead use the six agency type dummies from Table 6. This approach avoids over-attributing agency contributions based on a large number of sparse fixed effects, but it runs the risk of under-attributing contributions since the agency categories are coarse.

<sup>33</sup>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.

## 6.5 Impacts on Downstream Outcomes and Robustness

Our interventions may have had broader impacts on women and their households, by, for example, changing income sources and shifting women’s attachment to the labor market. In Appendix Table B.13, we find no significant effects on household income or expenditure, housing quality, assets, or use of social protection programs. This is perhaps unsurprising given that the report card did not shift migrants’ earnings and only reduced the migration rate by 4.4 p.p.

Appendix Table B.14 explores 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 at the time of the survey, suggesting women substituted towards self-employment from various alternatives. Self-employment may have been an especially attractive way to earn income while affording the flexibility to pursue migration plans. Column 7 reports treatment effects on monthly income for the sub-sample of directly interviewed respondents. As with household income, treatment effects are small and not significantly different from zero. Overall, there is little evidence that the report card or the comic led to significantly worse economic outcomes for women and their households.

While Appendix E discusses robustness checks in detail, we briefly summarize these results here. First, we assess the implications of baseline imbalance on prior migration experience, finding that treatment effects are, if anything, larger in magnitude when re-weighting to equalize the share of former migrants across treatment arms. Second, we use administrative placements data and our supplementary sample to confirm a lack of village-wide effects on the migration market. This is consistent with the fact that our intervention ultimately had limited reach within large study villages. Finally, we address possible non-response bias by reweighting the sample of directly interviewed migrant women to match the overall sample of migrant women. Our results are unchanged, suggesting that lower direct interview rates among migrants are not biasing our treatment effects on migration experiences.

## 6.6 Estimating the Value of Information

Evidence so far suggests that the report card effectively made women “choosier” and, in so doing, induced migrants to avoid offers from low-quality agencies. This is striking given the enormous financial returns to migration in our setting. In the control group at endline, only 53 percent of non-migrant women reported working for pay in the past year. Among those who did work for pay, the median woman had an average monthly income of IDR 1,009,000 (USD 78 at the 2019 exchange rate of IDR 13,900 per USD). By way of comparison, the median migrant in the control group reported average monthly earnings (wages plus other income less deductions and other migration costs) of IDR 7,992,230 (USD 575). These numbers suggest a raw return to migration of USD 497. This is close to the (noisy) Wald estimate of the USD 499 return to migration, obtained by dividing the effect of the report card on household income (an insignificant USD 8.5 reduction, see Appendix Table B.13) by the effect on the likelihood a woman is abroad at endline (an insignificant 1.7 p.p. reduction, see Appendix Table B.14).

Given that the report card did not change occupation or earnings abroad, women who delayed migrating put off the opportunity to earn significantly higher wages. This implicit willingness to pay for

continued search motivates a revealed preference estimate of the value of information. We use migration rates in the comic only ( $C$ ) and comic and report card ( $CR$ ) arms to estimate the monthly hazard of receiving and accepting an offer,  $\xi_C = 0.0095$ ,  $\xi_{CR} = 0.011$ . We take the median monthly earnings in Indonesia ( $h$ ) and abroad ( $w$ ) referenced above, and assume women discount cash flows at monthly rate  $r$  in line with real deposit interest rates.<sup>34</sup> Anchoring earnings at the median is reasonable given the limited evidence of selection effects in Section 6.4.

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

$$V(\xi) = h + \xi \left[ \sum_{t=1}^{24} \left( \frac{1}{1+r} \right)^t w + \left( \frac{1}{1+r} \right) \sum_{t=25}^{\infty} h \right] + (1 - \xi) \left( \frac{1}{1+r} \right) V(\xi) \quad (9)$$

Solving for  $V(\xi_C)$  and  $V(\xi_{CR})$  implies that women who receive the report card are willing to incur a discounted earnings loss of IDR 4,227,422 (USD 304) to prolong search. This amounts to roughly half a month’s earnings abroad. Appendix Figure B.10 shows how this estimated willingness to pay varies with the real interest rate, earnings abroad, and earnings in Indonesia. Overall, these sensitivity tests suggest that our main calibration offers a plausible upper bound on the value of information, provided forecasted earnings abroad are close to the sample median.

## 7 Conclusion

We use a large-scale randomized controlled trial, spanning 400 villages in Indonesia, to study how information frictions in global labor markets shape the choices and experiences of prospective female migrant workers. Access to novel information on intermediary agency quality—in the form of an easy-to-read agency “report card”—significantly reduced the migration rate while leaving beliefs about the return to migration and intentions to migrate in the future unchanged. Women who migrated with this information 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. Revealed preference calibrations suggest that information has substantial value, as the foregone income associated with delaying migration is equal to roughly half a month’s wages abroad.

Overall, our results are consistent with a model of search in which the report card significantly increased migrants’ ability to *differentiate high- and low-quality agencies*. Intuitively, when information about agency quality is 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

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<sup>34</sup>Specifically we use World Bank/IMF data on the deposit interest rate and inflation for 2019 (6.688 percent and 3.031 percent, respectively) to calculate a monthly real rate of  $r = 0.30$  percent assuming monthly compounding.

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 by employers. Our results also highlight that policymakers interested in facilitating safe, high-return migration must address both information frictions *and* the rate at which women get access to offers. The spread of mobile internet and online platforms for job search could facilitate this process.

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

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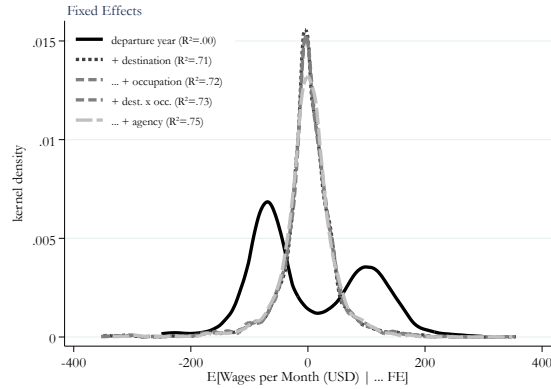
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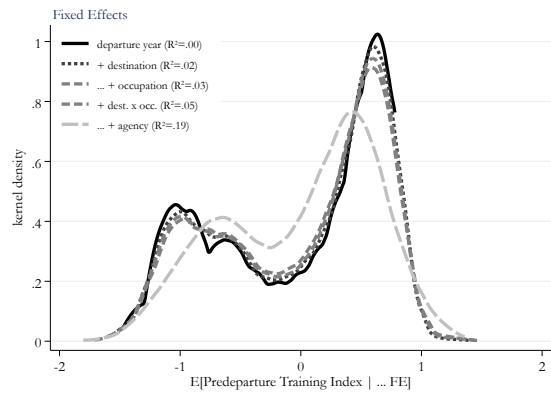
# Tables and Figures

Figure 1: Variation in Pecuniary and Non-Pecuniary Dimensions of Migration Quality

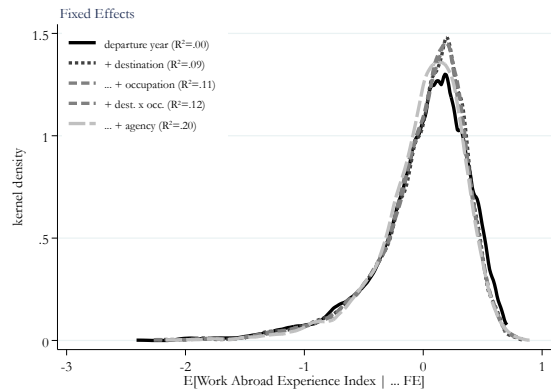
## A. Average Monthly Wages



## B. Predeparture/Training

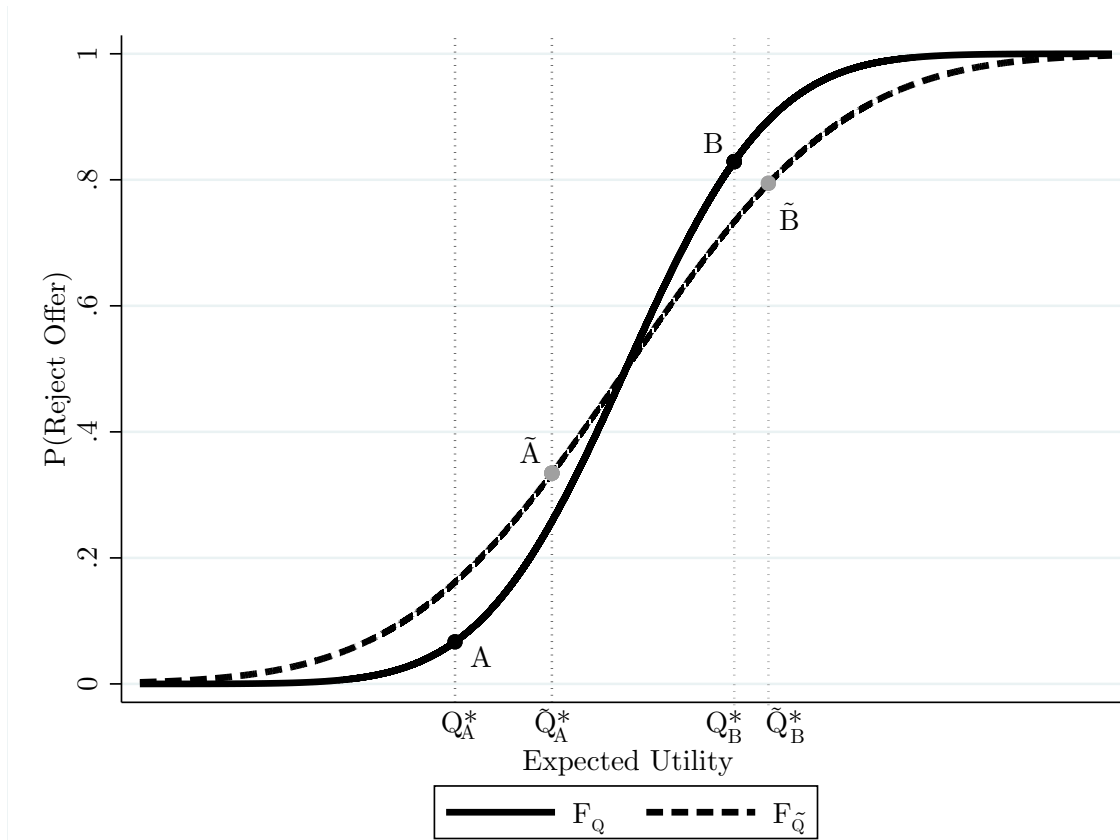


## C. Work Abroad



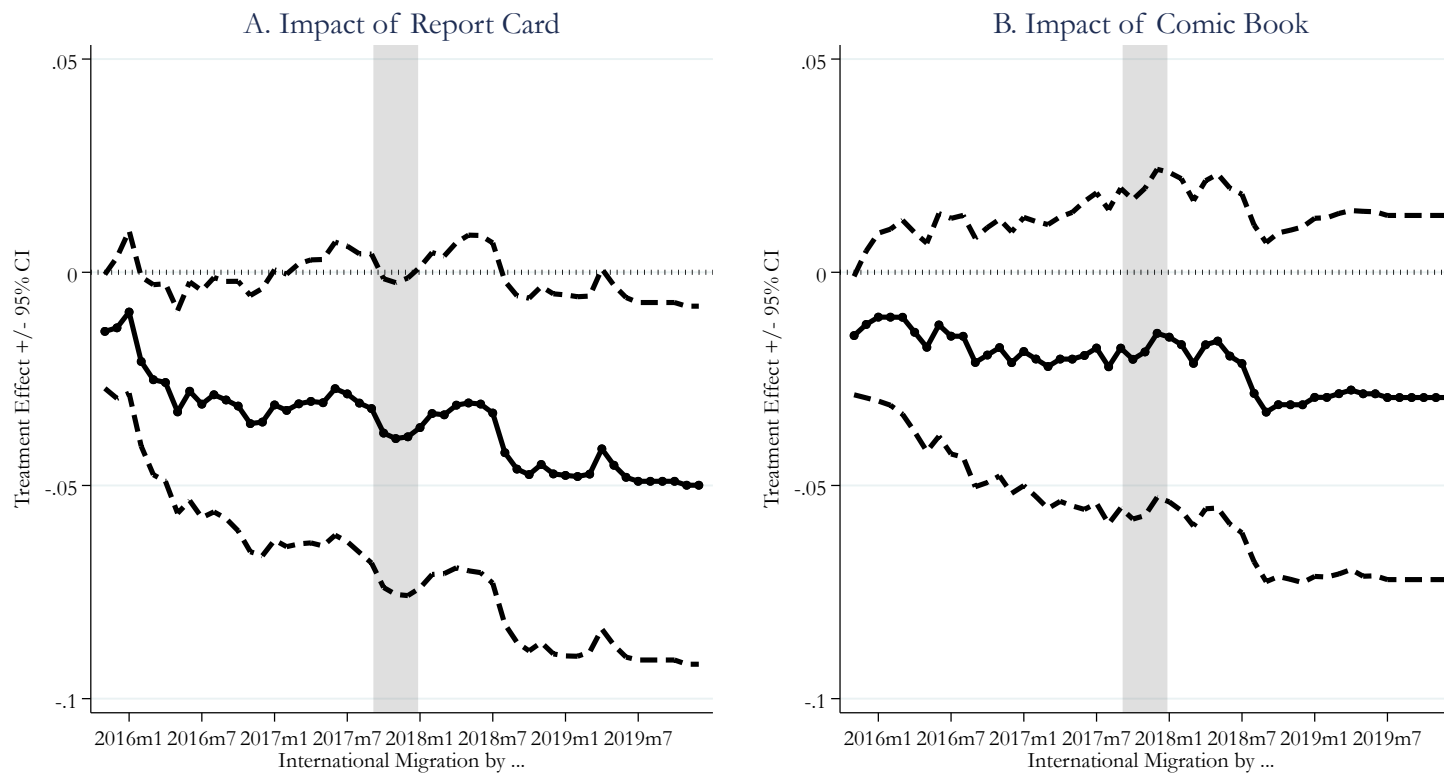
*Notes:* Each panel shows (i) the kernel density of the given outcome after residualizing out the given set of fixed effects indicated in the upper left quadrant, and (ii) the  $R^2$  from the regression generating those residuals. The outcomes include average monthly wages (Panel A), an index spanning predeparture preparation and training outcomes (Panel B), and job quality/on-the-job amenities abroad (Panel C). See footnote 10 for details on the components of these indices in the latter panels. Sample limited to women interviewed at baseline who migrated in 2011 or 2012, in agency and destination $\times$ occupation cells of 2 or more.

Figure 2: Theoretical Example: Impact of the Report Card on Migration Rates



*Notes:* This figure graphs the CDF of the offer distribution, in terms of expected utility from the migrant's perspective.  $F_Q$  is the distribution without quality disclosure,  $F_{\tilde{Q}}$  is the distribution after quality disclosure. The graph also shows how quality disclosure affects reservation utility –  $Q^*$ ,  $\tilde{Q}^*$  – and the migration rate for two hypothetical migrants,  $A$  and  $B$ . Absent quality disclosure migrant  $A$  accepts most offers. Quality disclosure increases her reservation expected utility and decreases the likelihood she migrates in any given period. Migrant  $B$ , on the other hand, rejects most offers prior to quality disclosure. Disclosure also increases her reservation utility, but the likelihood she migrates increases, because disclosure increases her ability to identify sufficiently attractive offers.

Figure 3: Impact of Report Card and Comic Book on Migration Rate Over Time



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

*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
<i>Panel A. Tracking Sample, Follow Up 2</i>				
$\beta_1$ : Report Card	-0.004 (0.020)	0.032* (0.016)	0.109*** (0.018)	0.020 (0.024)
$\beta_2$ : Comic	-0.001 (0.022)	0.025 (0.018)	0.035 (0.022)	0.148*** (0.021)
$\beta_3$ : Infographic	0.128*** (0.028)	0.046** (0.023)	-0.009 (0.024)	0.023 (0.028)
<i>P-value: Comic + Report Card vs Control:</i>				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.000***	0.000***	0.000***	0.000***
Control Mean	0.062	0.043	0.038	0.041
N	3398	3409	3409	3398
<i>Panel B. Tracking Sample, Follow Up 3</i>				
$\beta_1$ : Report Card	0.005 (0.020)	0.024 (0.026)	0.204*** (0.020)	0.006 (0.028)
$\beta_2$ : Comic	0.016 (0.019)	0.052** (0.024)	0.028 (0.024)	0.385*** (0.026)
$\beta_3$ : Infographic	0.120*** (0.025)	0.157*** (0.030)	0.019 (0.027)	0.091** (0.037)
<i>P-value: Comic + Report Card vs Control:</i>				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.000***	0.000***	0.000***	0.000***
Control Mean	0.027	0.035	0.024	0.149
N	3725	3067	3070	3078
<i>Panel C. Supplementary Sample, Follow Up 3</i>				
$\beta_1$ : Report Card	0.000 (0.013)	-0.005 (0.016)	0.009 (0.010)	0.007 (0.026)
$\beta_2$ : Comic	0.008 (0.012)	-0.005 (0.015)	-0.012 (0.011)	0.059*** (0.023)
$\beta_3$ : Infographic	0.026* (0.016)	0.028 (0.022)	0.012 (0.015)	0.001 (0.034)
<i>P-value: Comic + Report Card vs Control:</i>				
$\beta_1 + \beta_2 + \beta_3 = 0$	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. \*  $p \leq 0.10$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .



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 <sup>+</sup>
$\beta_1$ : Report Card	-0.044** (0.021)	-0.036* (0.020)	-0.011 (0.007)	-0.002 (0.027)
$\beta_2$ : Comic	-0.023 (0.021)	-0.021 (0.020)	-0.005 (0.007)	0.000 (0.024)
$\beta_3$ : Infographic	0.050* (0.029)	0.053* (0.028)	0.000 (0.011)	0.021 (0.037)
<i>P-value: Comic + Report Card vs Control:</i>				
$\beta_1 + \beta_2 + \beta_3 = 0$	0.404	0.838	0.047**	0.490
Control Mean	0.366	0.324	0.041	0.216
N	4664	4646	4664	2720

Notes: Robust standard errors clustered at the village level in parentheses. All regressions control for strata fixed effects. \*  $p \leq 0.10$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .  
<sup>+</sup>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
$\beta_1$ : Report Card	0.012 (0.030)	-0.010 (0.027)	-0.012 (0.025)	0.026 (0.032)	-0.019 (0.028)	-0.013 (0.026)
$\beta_2$ : Comic	0.009 (0.029)	0.005 (0.027)	0.014 (0.025)	0.023 (0.029)	0.005 (0.029)	0.013 (0.027)
$\beta_3$ : Infographic	-0.036 (0.041)	-0.032 (0.038)	-0.042 (0.035)	-0.039 (0.043)	-0.024 (0.040)	-0.034 (0.037)
<i>P-value: Comic + Report Card vs Control:</i> $\beta_1 + \beta_2 + \beta_3 = 0$	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)	(2)	(3)
	Pre-Departure Preparation	Migration Experience: Job Quality	Migration Experience: Pay
$\beta_1$ : Report Card	0.168*** (0.060)	0.103** (0.043)	-0.002 (0.045)
$\beta_2$ : Comic	0.107* (0.058)	0.114*** (0.043)	0.014 (0.047)
$\beta_3$ : Infographic	-0.191** (0.084)	-0.152*** (0.058)	-0.027 (0.065)
<i>P-value: Comic + Report Card vs Control:</i>			
$\beta_1 + \beta_2 + \beta_3 = 0$	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. \*  $p \leq 0.10$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ . 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)
	Migrated	High-Grade Agency	Average Agency	Low-Grade Agency	Ungraded Agency	Unknown Agency	Migrated without Agency
<i>Panel A. Extensive Margin, Including Non-Migrants</i>							
$\beta_1$ : Report Card	-0.054** (0.022)	0.001 (0.006)	-0.011** (0.005)	-0.001 (0.008)	0.004 (0.017)	-0.030*** (0.010)	-0.019** (0.008)
$\beta_2$ : Comic	-0.033 (0.022)	0.001 (0.006)	-0.003 (0.005)	0.005 (0.007)	-0.020 (0.017)	-0.012 (0.009)	-0.008 (0.007)
$\beta_3$ : Infographic	0.058* (0.030)	0.003 (0.008)	0.011 (0.008)	0.002 (0.010)	0.017 (0.023)	0.012 (0.015)	0.016 (0.012)
<i>P-value: Comic + Report Card vs Control:</i>							
$\beta_1 + \beta_2 + \beta_3 = 0$	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
<i>Panel B. Agency Choice Conditional on Migrating</i>							
$\beta_1$ : Report Card	0.000 (.)	0.014 (0.022)	-0.029 (0.020)	0.015 (0.028)	0.098** (0.047)	-0.067* (0.035)	-0.045 (0.028)
$\beta_2$ : Comic	0.000 (.)	0.011 (0.022)	-0.004 (0.018)	0.026 (0.025)	-0.005 (0.046)	-0.026 (0.034)	-0.020 (0.026)
$\beta_3$ : Infographic	0.000 (.)	-0.000 (0.030)	0.028 (0.029)	-0.010 (0.035)	-0.041 (0.064)	0.003 (0.050)	0.034 (0.041)
<i>P-value: Comic + Report Card vs Control:</i>							
$\beta_1 + \beta_2 + \beta_3 = 0$	.	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. \*  $p \leq 0.10$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ . 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 (1)	Job Quality Index (2)	Pay Index (3)
<i>Panel A. Decomposing the Effect of the Report Card</i>			
Agency Effect	0.118** (0.055)	0.045 (0.028)	0.028 (0.033)
Selection on Observables	-0.002 (0.015)	0.015 (0.023)	0.008 (0.023)
Selection on Unobservables	0.001 (0.003)	0.006 (0.007)	-0.000 (0.003)
Residual Differences	0.067*** (0.024)	0.043 (0.031)	-0.027 (0.030)
<i>Panel B. Decomposing the Effect of the Comic Book</i>			
Agency Effect	0.053 (0.053)	0.016 (0.030)	-0.020 (0.035)
Selection on Observables	-0.006 (0.016)	0.018 (0.021)	0.013 (0.019)
Selection on Unobservables	0.001 (0.002)	0.003 (0.007)	-0.000 (0.002)
Residual Differences	0.057** (0.023)	0.073** (0.030)	0.015 (0.030)
<i>Panel C. Decomposing the Effect of the Infographic</i>			
Agency Effect	-0.096 (0.077)	-0.046 (0.040)	-0.008 (0.044)
Selection on Observables	0.004 (0.020)	-0.029 (0.032)	-0.012 (0.031)
Selection on Unobservables	-0.001 (0.004)	-0.005 (0.009)	0.000 (0.003)
Residual Differences	-0.105*** (0.033)	-0.069 (0.043)	-0.003 (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. \*  $p \leq 0.10$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

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

	(1)	(2)	(3)
	Full Sample	All Migrants	Direct Interview Migrants
$\beta_1$ : Report Card	-0.006 (0.004)	-0.009* (0.005)	-0.006 (0.006)
$\beta_2$ : Comic	-0.002 (0.004)	-0.004 (0.005)	-0.002 (0.006)
$\beta_3$ : Infographic	0.004 (0.005)	0.005 (0.006)	-0.001 (0.008)
<i>P-value: Comic + Report Card vs Control:</i>			
$\beta_1 + \beta_2 + \beta_3 = 0$	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. \*  $p \leq 0.10$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .