Banned from the Band: The Effect of Migration Barriers on Origin-Country Labor Market Decisions

Caroline Theoharides*
Amherst College

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Abstract

International migration flows are often constrained by destination country immigration policies with little understanding of how these policies affect the welfare of origin countries. To estimate a causal effect of migration barriers on labor market behavior in the migrant-sending country, this paper exploits a policy change that led to the halt of the largest migration channel for Filipinos. In 2005, Japan dramatically changed the requirements for Filipinos migrating as overseas performing artists (OPAs), resulting in a decline from 71,108 to 925 new workers migrating per year. Certain areas of the Philippines historically sent a larger share of OPAs, and I employ a difference-in-differences estimation strategy that uses historical OPA migration to define the treatment dosage. International migration falls in response to the policy change by a larger amount than the policy itself would impose, indicating the importance of spillovers across migrant occupations. Domestically, more children are employed, and adults are more likely to be unemployed, look for additional hours, and engage in short term work. These results suggest that migration barriers and the elimination of controversial migration channels can have important repercussions for labor market behavior in migrant-sending countries.

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*Converse Hall, Amherst, MA 01002. Email: ctheoharides@amherst.edu. I thank the Overseas Worker Welfare Administration (OWWA), Philippine Overseas Employment Administration (POEA), and the National Statistics Office (NSO) for access to the data; Dunhill Alcantara, Helen Barayuga, Nimfa de Guzman and Nerissa Jimena of POEA and Lex Pineda, and Rosanna Siray of OWWA were invaluable in compiling these databases. I thank Manuela Angelucci, Raj Arunuchalam, Susan Dynarski, Susan Godlonton, Joshua Hyman, Jeffrey Smith, and Dean Yang, as well as seminar participants at Dartmouth College, Smith College, the University of Michigan, NEUDC, and the 2015 Migration and Development Conference for valuable comments. I gratefully acknowledge support from the Rackham Merit Fellowship and the National Science Foundation Graduate Research Fellowship.

1 Introduction

Global labor mobility is far from free. Immigration policies in destination countries serve as a major determinant of emigration flows (Clemens, 2011; Ortega and Peri, 2014), and policy debates around the globe currently focus on how to control the flow of migrants, be it through quotas, point systems, or border fences. Yet, international migration provides substantial benefits to poor countries, leading to increases in schooling (Cox-Edwards and Ureta, 2003; Dinkelman and Mariotti, 2014; Theoharides, 2015) and household investment (Woodruff and Zenteno, 2007; Yang, 2008) as well as reductions in risk (Yang and Choi, 2007). Further, Clemens, Montenegro and Pritchett (2008) show that the same worker earns substantially different wages depending on the country of employment.

Despite the benefits of migration for migrant-sending countries, the migration literature has focused primarily on the effect of immigration policies on native workers (Borjas, 2003; Card, 2009; Ottaviano and Peri, 2012), and very few studies examine the implications of such policies on the migrant-sending country. Clemens (2011) asserts that gains from reducing barriers to international migration are much larger than gains from reducing barriers to trade or capital flows. The majority of evidence is restricted to the effects on world GDP; studies estimate that eliminating migration barriers could lead to gains in world GDP of 50 to 150 percent (Klein and Ventura, 2007; Moses and Letnes, 2004; Hamilton and Whalley, 1984; Iregui, 2001). The evidence on the microeconomic effects is even more limited. Dinkelman and Mariotti (2014) find reduced investment in education in response to a migration ban imposed by the Malawian government that halted migration of Malawians to South Africa. Feigenberg (2015) finds that increased migration costs due to the construction of the border fence between Mexico and the U.S. substantially reduced migration.

In this paper, I provide empirical evidence on the effects of migration barriers imposed by the host country on migrant-sending countries. Specifically, I answer the causal question: What is the effect of the closure of a major migration channel on the labor market behavior of households in the country of origin? To answer this question, I exploit a policy change in Japan that imposed significant barriers on the migration of Filipino Overseas Performing Artists (OPAs), the largest occupation for Filipino migrants. OPAs are primarily women working as hostesses in nightclubs and gentlemen's clubs, and the nature of OPA migration is

historically controversial. In 2005, in response to claims from the United States that OPAs were victims of trafficking, Japan dramatically raised the education and experience requirements for Filipinos migrating to Japan as OPAs. This effectively closed this migration channel, with Filipino OPA migration to Japan falling from 71,108 in 2004 to 6,696 in 2006 and to 925 by 2011.

Not all geographic regions of the Philippines were affected equally by this policy change. Migrant networks matter in terms of where individuals migrate and what they do there (Munshi, 2003), and the Philippines is no exception. Certain areas of the Philippines historically send migrants to certain destinations and in certain occupations (Theoharides, 2015). As a result, provinces that specialize in OPA migration receive a larger treatment dosage from the policy change than those that do not. Exploiting this natural experiment, I employ a difference-in-differences estimation strategy using the percent of OPA employment in the province population in a base year as a continuous policy variable to define the treatment dosage.

I first examine the policy's impact on migration. Since many quotas or points systems refer specifically to migrants in certain occupations and are destination specific, it is important to quantify spillover effects and switching behavior across migration channels in order to estimate implications of migration policies for migrant sending countries. The elimination of a migration channel for a particular occupation (destination) could lead to little or no change in total migration if migrants can easily switch to a new occupation (destination). Alternatively, such a barrier could lead to greater decreases in migration than would be predicted due to spillovers to other occupations (eg. from closing recruitment centers).

I then examine the effects on domestic labor market activity. At a microeconomic level, migration barriers can have a number of effects on migrant-sending countries. As barriers are imposed, remittances from the affected destinations will halt. This reduction in income may lead to more binding credit constraints for households. As a result, households may change their labor market choices both domestically and internationally. For instance, household members that were not previously working may now seek employment, potentially causing employment or unemployment rates to rise. If households are able to adjust their employment decisions to compensate for lost income from migrants, then there should not be effects on things like consumption or education. If, on the other hand, unemployment rises, this suggests

that households are not able to perfectly adjust and there will likely be adverse implications on other outcomes as well. Thus, understanding labor market responses provides a window into the overall disruptiveness of the policy change.

I find that in response to the policy change, migration decreases more for provinces with a higher baseline OPA share. Specifically, moving from the 25th to 75th percentile of the baseline OPA share is associated with a 10.1% greater decrease in migration after the policy change. This effect is larger than the policy change would predict, suggesting substantial negative spillover effects. Domestic helpers, plumbers, carpenters, and production workers all are hired at a lower rate than prior to the policy change in high OPA share provinces, and I cannot rule out that the negative spillovers adversely affect both male and female migrants. Domestically, labor force participation increases by 0.27% more in high than in low OPA share provinces after the policy change. Child labor increases by 2.1%, and 2.1% more individuals are now engaged in short-term work. The domestic labor market results suggest that when migration is reduced and remittances are no longer available, domestic labor market choices are substantially different in order to cope with these changes, but that households cannot fully compensate for lost migration opportunities and would like to work more domestically. A number of robustness checks corroborate the main results.

This paper provides the first microeconomic estimates of the effects of a migration barrier on labor market decisions in the country of origin. It also provides the first estimates of spillovers on other types of migration due to the imposition of barriers. This will help policy makers in migrant-sending countries predict the resiliency of their overseas workforce to changes in the migration policy of destination countries. Finally, not only does this policy result in the closure of a major migration channel for Filipinos, but it also halts migration in a controversial migration channel. Restrictions on labor mobility are perhaps greatest when an occupation is deemed exploitive. The economics literature on trafficking is limited, and in particular the literature is silent on the implications of policies that regulate this type of employment for sending countries. My paper is the first to provide empirical estimates of the economic effects on sending countries of banning migration opportunities in occupations often perceived as vulnerable or exploitive. Such estimates are important when considering the necessary social safety nets for households when individuals are removed from controversial employment environments.

The remainder of the paper is organized as follows. Section 2 provides background on migration from the Philippines with a focus on overseas performing artists in Japan and the subsequent anti-trafficking campaign that led to their decline. Section 3 discusses the data used in the analysis. The methodology is discussed in Section 4. Section 5 presents the results, and Section 6 concludes.

2 Background

2.1 Filipino Migration

International migration is a common labor market option in the Philippines. The Philippine Overseas Employment Program, established in 1974, promotes contract migration of its citizens, and approximately 2% of the population migrates annually in a variety of occupations. This is legal and temporary migration through licensed recruitment agencies, and contract duration is about two years on average. Workers are classified as either new hires who are working abroad on a new labor contract or as rehires renewing an existing contract. Family members of migrants typically remain at home in the Philippines. Contract migration is an increasingly common global phenomenon, particularly in Asia and the Middle East. While the Philippines was the first country to establish temporary contract migration as a labor market alternative, Indonesia, Sri Lanka, India, Bangladesh, and Tajikistan, among others, have all adopted or are in the process of adopting similar programs (Asis and Agunias, 2012; Rajan and Misha, 2007; Ray, Sinha and Chaudhuri, 2007; World Bank, 2011).

Table 1 shows the top 10 occupations for new overseas Filipino workers in 2004, the year prior to the Japanese policy change. Overseas Performing Artists (OPAs) and domestic helpers are overwhelmingly the largest occupations and predominantly employ women. Migrants also go to a wide range of destination countries. For women, the largest destination in 2004 was Japan, though destinations throughout Asia and the Middle East are common. Approximately 50% of men work in Saudi Arabia. Likely due to migrant networks, location of origin in the Philippines is an important determinant of where and in what occupations migrants work while abroad. Stories of success abroad circulate in communities, and prospective migrants trust the experiences of those in their neighborhoods and choose to follow similar migration trajectories in terms of chosen recruitment agency, destination, and occupation (Barayuga, 2014). Theo-

harides (2015) shows that province-level historic destination and occupation shares are a strong predictor of variation in contemporaneous province-level migration rates. This emphasizes the importance of migrant networks, whether through social networks or agglomeration effects, such as the prevalence of middlemen to facilitate the migration process to certain destinations or in certain occupations. In Section 4, I explore the strength of OPA migrant networks in particular.

2.2 Overseas Performing Artist Migration

As shown in Table 1, OPAs compose 25.5% of new migrants from the Philippines in 2004. Approximately 96% of OPAs are female, and 98.8% of OPAs work in Japan. In Table 2, I compare the characteristics of OPAs to the characteristics of all other new contract migrants in 2004. OPAs are more likely to be female than the average non-OPA contract migrant. They are also younger, with an average age of 25 years compared to 32 years. This is primarily because the maximum age for OPAs hired by Japan is 35 years of age (Parrenas, 2008). OPA wages are high. Average monthly wages are $1,857 compared to $417 for other contract migrants (and $276 for the average domestic helper). The contract durations are also much shorter, with an average duration of 4.6 months compared to 20.5 months. Migrants from the Philippines on average are quite well educated when compared with the Philippine population as a whole (Theoharides, 2015), yet OPAs are an exception. With a 10-year primary and secondary education system in the Philippines, the average contract worker has 13.3 years of education, or almost a college degree. OPAs, on the other hand, have 9.4 years of education on average, meaning that the average OPA is not a high school graduate.

The term "Overseas Performing Artist" is an umbrella term encompassing women employed as choreographers, dancers, composers, musicians, and singers. The nature of the employment of these women is as hostesses in gentlemen's clubs in Japan, where the dress code is "high heels and 'sexy' dresses" (Parrenas, 2008). Prior to 2005, recruiting agencies typically sent a photograph to prospective Japanese employers to aid their selection of OPAs. While POEA conducted an audition prior to deployment, recruiting agencies would ensure selected OPAs passed the audition, often through impersonation (Barayuga, 2014).[1]

[1]Selected OPAs without performance talent would send someone else to engage in the audition for them. Prior to the policy change in Japan, POEA was in the process of implementing a fingerprint scanning system in order to combat issues of impersonation in the interview process. The system was scrapped once OPA migration

The actual work of OPAs in Japan is largely debated. Media reports and a number of studies assert that OPA employment is exploitive and essentially forced prostitution (Douglass, 2003; Ministry of Foreign Affairs of Japan, 2004). Alternatively, Parrenas (2011) contends that while a certain level of intimacy is expected of OPAs, forced prostitution is uncommon. In addition to the controversial nature of employment, many OPAs become attached to the Yakuza (Japanese organized crime) and are often victims of debt bondage through fees incurred during training or the confiscation of passports. OPAs typically do not receive their salaries until the end of the contract in order to ensure they do not leave prior to the completion of the contract (Parrenas, 2008).

Starting in 2000 with the passage of the Victims of Trafficking and Violence Protection Act, the U.S. began a campaign to crack down on human trafficking worldwide. In the 2004 and 2005 U.S. Trafficking in Persons Reports, Filipino OPAs in Japan were identified as victims trafficked into forced prostitution. In response, Japan adopted the Action Plan of Measures to Combat Trafficking in Persons (Ministry of Foreign Affairs of Japan, 2004). This dramatically altered the requirements for hiring OPAs bound for Japan. Before 2005 applicants were eligible for OPA employment as long as they met the requirements of a government agency in their country of origin (Parrenas, 2008).[2] Through a bilateral agreement with Japan, the Philippines only required OPAs to complete a training certificate of 6 months or less in duration and pass an audition. In response to the trafficking accusations, Japan revised their policy to require all OPAs to have 2 years of education or training in performance, and the Philippine government was no longer eligible to evaluate performers (Parrenas, 2008).[3] Because the population of OPAs from the Philippines is historically poorly educated, these policy changes imposed huge barriers to migration for traditional OPAs. Most experienced OPAs were not able to return to Japan for employment, and with limited economic opportunities at home, took part in migrant reintegration programs sponsored by the Philippine government (Parrenas, 2008).

The changes in outmigration of OPAs in response to the policy change can easily be seen

fell in response to the policy change (Barayuga, 2014).

[2]Applicants were also eligible for OPA employment in Japan if they had 2 years of either training or work experience as a performing artist.

[3]While higher education standards for migrants may cause long run increases in education through aspirational effects of a higher expected wage premium (Shrestha, 2012), in the case of OPAs, poverty is believed to be the major impetus for migration for OPAs, and stigmas attached to OPA migration limit the aspirational effects. Further, Theoharides (2015) finds that most effects on human capital are due to remittances rather than aspirations.

from the plot of new OPA contracts over time shown in Figure 1. In response to the ban, annual OPA migration to Japan plummeted from 71,108 in 2004 to 6,698 workers in 2006. Overall OPA migration fell from 25.5% of all Filipino migration annually to 2.4%. It should be noted that concern over the work of Filipino OPAs in Japan was not a new phenomenon. The dip in deployment between 1994 and 1995 was in response to more stringent requirements imposed by the Philippine Labor Secretary to combat perceived exploitation of Filipinas. Upon her resignation, OPA migration returned to and surpassed its previous levels.[4]

OPA migration is not distributed evenly across the Philippines. Figure 2 plots the province-level OPA migration rates in 1993 and shows that there is substantial variation in which provinces send OPAs. OPA migration was concentrated in the provinces surrounding Manila as well as a few provinces in the Visayas and in southern Mindanao. Figure 3 plots the OPA migration rates in 2004 and highlights the importance of geographic migrant networks for OPAs. Provinces that have high rates of OPA migration in 1993 continue to in 2004, whereas provinces that had low rates of OPA migration in 1993 still have very few OPA migrants as a portion of the population in 2004. Anecdotally, migrant networks are particularly important for OPAs. As for most contract migrants, word of mouth and trust play significant roles in where individuals migrate. As noted above, contract duration of OPAs is much shorter than for other contract migrants. As a result, OPAs return to the Philippines much more frequently (4 times as often) compared to other migrants, and the monetary benefits of OPA migration are thus much more visible to those still in the Philippines (Barayuga, 2014). Further, since OPAs are required to attend a training center prior to deployment, Filipinas from one province will typically enroll in a training center together, often one that is recommended by a person related to the trainee (Barayuga, 2014).

2.3 Spillover Effects

While the OPA policy change only directly affected the migration of OPAs, the ban may affect migration in other occupations and destinations, causing effects on migration that are

[4]The late 1980's and early 1990's included multiple attempts by the Philippine government to ban the hire of female workers (OPAs and other occupation groups), but these bans were short-lived. For instance, in 1991 President Corazon Aquino ordered a total ban of OPA deployment, but it was rescinded almost immediately when recruiters promised to support a new system of deployment (Kapunan, 1996). The deployment of domestic helpers was banned in January 1988, but by May, new agreements had been reached with most destination countries (Mydans, 1988).

larger or smaller than the magnitude of the ban itself. This natural experiment provides a unique opportunity to test for these types of spillover effects. Since many quotas or points systems refer specifically to migrants in certain occupations and are destination specific, it is important to quantify spillover effects and switching behavior across migration channels in order to estimate implications of migration policies for migrant sending countries.

Spillover effects may occur for a number of reasons. First, spillover effects may occur that reduce migration in occupations other than the OPAs directly affected by the policy change. When opportunities are reduced due to migration barriers, this will lead to the elimination of remittances from that channel. If households are credit constrained, they may no longer be able to afford the migration fees for other household members to migrate to other destinations or in other occupations. This would cause migration to decline by more than the magnitude of the migration barrier. Further, the multiplier may also be larger due to changes in the presence of recruitment agencies or off-site recruiting. Recruiting agencies typically recruit for possible OPAs as well as several other occupations. After the policy change, recruiting agencies may choose to close or no longer hold off-site recruitment in the towns where they typically recruited OPAs. As a result, OPA recruitment will decline, but employment in other occupations will fall as the workers are recruited from other locations in the Philippines.

On the other hand, we might expect effects smaller than the magnitude of the ban if potential OPA migrants can easily switch between occupation categories and destinations. For instance, a prospective OPA migrant may instead move abroad as a domestic helper. In the case of migration from the Philippines, switching behavior seems less likely for two reasons. First, the importance of migrant networks results in rigidity in the local labor market that makes it more difficult for "OPA provinces" to easily become "domestic helper provinces." Second, there is an excess supply of migrants from the Philippines (McKenzie, Theoharides and Yang, 2014; Theoharides, 2015), and so it seems unlikely that OPAs can easily switch when their employment opportunities are no longer available since a surplus of potential and more highly educated migrants already exists. Further, most migrant occupations require considerably more education than the average 9 years of education obtained by OPAs (Theoharides, 2015).

3 Data

To calculate each province's baseline share of OPAs, I use an original dataset of all new migrant departures from the Philippines between 1992 and 2009. I use probabilistic matching to combine two government administrative datasets from the Philippine Overseas Employment Agency (POEA) and the Overseas Worker Welfare Administration (OWWA). POEA records all new migration episodes from the Philippines in order to verify that workers are paid the wages stipulated by their contract. The data include name and demographics, as well as destination, occupation, employer, recruiting agency, and wages. OWWA, on the other hand, is concerned with the welfare of the workers and their families. While recording similar identifying information and demographics, OWWA's key variable of interest is the home address of the migrant so that in the event of natural disasters or other turmoil in the destination country, they can contact the migrant's family. Combining the POEA and OWWA data creates a unique dataset that includes both the occupation and destination of the migrant as well as their home address in the Philippines.[5,6] I then aggregate individual records annually by occupation and province to determine the number of new OPAs in each province in the base year. I divide by the working population at baseline as calculated from the Philippine Census of Population in order to calculate the baseline share of OPAs. I also use this original dataset to calculate both the overall number of new migrants and the number of new migrants by occupation and gender at the province level.

Figure 4 plots the baseline shares for each province as circles. There is substantial variation in the OPA shares at baseline, indicating that provinces will experience different dosages of treatment in response to the OPA policy change. To be clear, the shares are low, and the average OPA share at baseline is 0.05% of the population. Yet, compared to an average province-level migration rate at baseline of 0.39%, OPA migration clearly represents a significant portion of all overseas migration episodes.[7]

I use the 1992-2011 Labor Force Surveys (LFS) from the National Statistics Office (NSO) to

[5] I match the data using first name, middle name, last name, date of birth, destination country, gender, and year of departure using probabilistic or fuzzy matching techniques as discussed by Winkler (2004). The match rate is approximately 90% for 1993, the year in which the baseline values are calculated. See Theoharides (2015) for further details.

[6] Unfortunately, home address of the migrant was not recorded by OWWA between 1999 and 2003.

[7] This includes only new outflow of temporary migrants, while the 2% migration rate mentioned in Section 2.1 includes both temporary and permanent migrants.

calculate domestic labor market outcomes and a number of covariates. The LFS is a quarterly household survey conducted on a rotating panel of households. The survey asks about the recent employment status and work history of all members of the household of twelve or more years of age, including overseas members of the household. I use these data to construct labor force participation and unemployment rates, the fraction of working aged individuals looking for additional work, and the rate of child labor.[8]

Table 3 shows summary statistics for both datasets. The total migration rate from the POEA data is 0.39%. Using the POEA data, I can also calculate occupation-specific migration rates. For OPAs, the average migration rate is 0.06% in 2004. In 2006, the year after the policy change, the OPA migration rate is less than 0.01% of the population. 5.71% of the labor force is unemployed over the sample period from 1998 to 2011. 3.27% of children between the ages of 5 and 12 worked at least one hour in the past week. 4.84% of the working-age population reports looking for additional work to supplement their current employment. Approximately 12% of the working-age population report that their jobs are not permanent.

4 Empirical Strategy

To obtain a causal estimate of the effect of the OPA policy change on migration and employment outcomes, I exploit the fact that, due to historic migration networks, provinces with a larger share of OPAs as a portion of their population will experience a larger reduction in migration as a result of the ban compared to provinces with a smaller share of OPAs. This can be seen in Figure 4, which plots the OPA migration rates in 1993 (baseline) and in 2009. The dosage that each province receives in response to the policy change is the vertical distance between the circle and the triangle for each province. Provinces are sorted by 1993 share such that the further right a province is in the figure, the larger the effect of the policy change in the province tends to be (province 63 has a lower dosage than province 64).

Formally, I implement a difference-in-differences style analysis with a continuous treatment variable. I estimate the following equation:

$$Y_{pt} = \beta_0 + \beta_1 Post_t * ShareOPA_{p0} + \alpha_p + \gamma_t + \epsilon_{pt} \quad (1)$$

[8]Child labor is defined as children between the ages of 5 and 12 working for at least 1 hour per week. I also examine the rate of children between the ages of 13 and 15 working at least one hour per week.

where Y_{pt} is some outcome variable for province p in year t. $Post_t$ is a dummy variable equal to 1 for the years 2006 to 2011 and equal to 0 for 1998 to 2004.[9,10] I exclude 2005 from the analysis since the ban occurred halfway through 2005. $ShareOPA_{p0}$ is the number of OPAs in province p in some base period divided by the total working population in the base period. I define the base period as 1993, though the results are also robust to using 1992 as the base year.[11] α_p are province fixed effects, γ_t are year fixed effects, and ϵ_{pt} is the error term, which I cluster at the province level. 77 provinces are used in the analysis, and all regressions are weighted by the province population in 1993.[12] β_1 estimates the differential effect of the policy change for an OPA province with a 1 percentage point higher baseline OPA share on the province-level outcome Y.

The identifying assumption for β_1 to be a valid estimate of the causal effect of the OPA ban is that in the absence of the policy change, the outcome variable of interest in provinces with different baseline shares would have been parallel. I test this assumption for every outcome that I examine in Section 5 and find no evidence of differential pre-trends.

In the ideal experiment, OPA migration rates would be randomly assigned at baseline across provinces. In the case of the continuous difference-in-differences identification strategy, the province fixed effects remove concern about time-invariant differences in provinces with varying baseline shares. However, a lingering question is why certain provinces historically sent a high share of OPAs while others did not. If these differences result in differential trending of variables related to the outcome variable, this may lead to biased estimates. To determine what explains the high or low base share OPA migration rates in certain provinces, I regress the OPA share in 1993 on a vector of covariates.

The results are shown in Table 4, Column 1. Most of the point estimates are quite small in magnitude, and the covariates do not have a statistically significant relationship with the share OPA, suggesting that there are not systematic differences in demographics across high and low OPA share provinces. However, the percent of the population with some high school and the percent urban have precisely estimated correlations with the share OPA at baseline.

[9] When examining the outcomes calculated with POEA/OWWA data, because of the lack of municipality data in 1999 to 2003, I use a pre-period from 1996-1998 & 2004.

[10] The results are robust to different definitions of the pre-period and post-period and are shown in Table 6.

[11] I use 1993 as the base year because province of origin is non-missing 86% of the time compared to 84% of the time in 1992.

[12] In order to maintain consistent geographic definitions of province over time, I assign all provinces to their 1991 geographic boundaries.

High OPA share provinces are less likely to have a higher portion of the population with some high school education and are more likely to live in urban areas. While these two covariates are correlated with the OPA shares, the number of statistically significant characteristics is similar to what would be found due to chance. To alleviate concern that differences in provinces at baseline may lead to differential trending in omitted variables related to the outcome variable, I include controls that capture differential changes across provinces. I specifically include pre-policy controls in a base year (1993) interacted with the post dummy to pick up trends correlated with the controls. Since I use multiple periods in my estimation, my preferred specification includes pre-policy controls interacted with a linear time trend variable rather than just a post dummy.

Assigning baseline OPA shares 10 years before the policy change occurred reduces concern that these shares are formed endogenously. For the baseline OPA share to make sense as a measure of treatment dosage, high OPA provinces at baseline must remain high OPA sending provinces in later years prior to the policy change. In Section 2, I discussed the importance of both destination and occupation-specific migrant networks in explaining outmigration rates across the Philippines. I formalize this with respect to OPA migration in Table 4. Specifically, I regress the province-level share of OPAs in 1997, 2004, and 2009 on the share of OPAs at baseline in 1993 and a vector of covariates. In Row 1, Columns 3 and 5, it is clear that baseline OPA shares are a strong predictor of later OPA migration rates. The magnitude in absolute value of the 1993 baseline share point estimate is over 250 times greater in 1997 (over 70 times greater in 2004) than the next largest point estimate, and is extremely precisely estimated. In 2009, five years after the policy change, the baseline OPA migration rate is no longer predictive of the remaining OPA migration rate.

5 Results

5.1 Effects on Migration

In Table 5, I present estimates of the effect of the OPA policy change on the total province-level contract migration rate. Column 1 shows the specification in equation 1 for just 2004 and 2006, the years immediately before and after the policy change. Column 2 adds baseline controls interacted with a post dummy. Column 3 extends the sample period to include

multiple years before and after the policy change. Column 4 is analogous to Column 2, but with the longer sample period, and Column 5 adds baseline controls interacted with a linear time trend. Column 5 is my preferred specification. The interpretation of the coefficient is that the policy reduced the total migration rate by 0.883 percentage points more for a province with a 1 percentage point higher baseline OPA share.

Recall from Table 3 that the average fraction of OPAs out of the province population at baseline is 0.06% of the province population. Thus, interpreting the effects in terms of a one-percentage point increase is unrealistic given the magnitude of the OPA migration rate. Instead, I scale the results by the magnitude of the interquartile range of the fraction OPA, which is 0.03. As a result, the effect of moving from the 25th percentile of OPA shares at baseline to the 75th percentile leads to a 0.03*-0.883=0.026 percentage point decrease in the total migration rate. Off a mean total migration rate of 0.26% in the pre-period, this leads to a 10.10% decline in the total migration rate in the 75th percentile of OPA provinces compared to the 25th percentile. The average province sends 1,654 migrants in the pre-period, which implies that migration falls by 167.1 more migrants in the 75th percentile of baseline OPA share than in the 25th percentile, moving from pre to post.

As stated earlier, one concern with this estimation strategy is potential differential trending of the migration rate by baseline OPA share. I formally test for differential trends in the migration rate by estimating the relationship between the baseline OPA share and the change in the migration rate in the pre-period. I estimate the following equation:

$$\Delta(Y_{pt}) = \beta_0 + \beta_1 ShareOPA_{p0} + \beta_2 X_{p0} + \gamma_t + \epsilon_{pt} \tag{2}$$

where t is the pre-period, $\Delta(Y_{pt})$ is the change in the province-level outcome variable in province p from year $t-1$ to year t, and X_{p0} is a vector of covariates at baseline. The results are shown in Table 5, Column 5. I find that a 1 percentage point increase in the OPA migration rate at baseline leads to a tiny change in the total migration rate that is statistically indistinguishable from zero. This suggests that the migration rate in provinces with higher OPA shares was not changing differentially compared to lower share provinces, and the trends in the pre-period are in fact parallel.

In addition to checking for pre-trends, I also test the robustness of my preferred specification

(Table 5, Column 5) in Table 6. First, one might be concerned that the results are driven by provinces with the highest OPA base shares. Turning to Figure 4, there appears to be four extreme outliers in OPA base shares. These outliers are each of the four districts of Metro Manila. I drop each of these districts in Columns 1 through 4 of Table 6. The results are quite robust to their exclusion, suggesting that they are not driving the results. In Column 6, I drop all four districts of Metro Manila. While the point estimate drops and is imprecisely estimated, the negative sign indicates a similar pattern to the main results, with the OPA ban leading to less migration overall. Figure 4 might also lead to concern about non-linearities in the effects. One option would be to assign a binary treatment variable. However, the choice of treatment and control is somewhat arbitrary so I prefer the continuous treatment variable. Instead, I restrict the sample to just those provinces above the mean and median OPA migration rate. The shares shown in Figure 4 are much more linear in this region. The results in Columns 5 and 6 of Panel B are robust to this sample restriction.[13]

In Panel B, Columns 1-3, I test the robustness of the results to alternative specifications of the sample period. The results are again quite robust to truncating the pre and post period. In Column 4, I also provide an additional check of the validity of the estimates by conducting a false experiment. I define 1998 as the pre-period and 2004 as the post period. There is no differential effect on the migration rate moving from this false pre to post period across high and low OPA provinces. The results are also robust to the exclusion of 1990 population weights (Panel A, Column 7).

5.2 Spillover Effects

The magnitude of the point estimates on migration provides an estimate of the spillovers from migration as discussed in Section 2.3. The effects on outmigration may be exactly equal to the effects of the ban itself, indicating that for each OPA affected by the ban, there is one fewer migrant. Intuitively, if migrant networks are perfectly predictive such that the assigned treatment dosage from the base share is exactly the treatment dosage realized, a one percentage point increase in the baseline OPA share should lead to a one percentage point decline in the total migration rate if the effect of the ban is realized without spillover effects.

[13] In Appendix A, I examine a binary treatment variable where the control group is determined based on propensity scores. All of my results are robust to this alternative specification.

However, while historically high OPA provinces remain high OPA provinces over time, base shares are an imperfect predictor of the future migration rate. Turning back to Table 4, Column 5, we see that a one-percentage point higher OPA share at baseline leads to a 0.44 percentage point higher OPA migration rate in 2004, the year prior to the policy change. Thus, while high OPA provinces still have higher OPA migration rates right before the policy change occurred, the treatment dosage actually experienced by these provinces will be less in reality than the baseline share would suggest. Comparing the point estimates to 1 in order to determine the spillover effect is thus incorrect given that base shares are not perfectly predictive. Specifically, a one-percentage point increase in the baseline OPA migration rate implies a 0.44 percentage point higher OPA migration rate in 2004. Thus, for the effect of the ban to be fully realized, the total migration rate should decline by -0.44 percentage points.

While positive or negative spillover effects may exist in the total migration rate, they should not be present in the OPA migration rate itself. Thus, to first examine the accuracy of this type of test for spillover effects, I first look at the effect of the policy change on the OPA migration rate. Shown in Table 7, Column 1, a one percentage point increase in the baseline OPA share causes a 0.47 percentage point decline in the OPA migration rate when moving from the pre to post period, indicating that the effect of the ban is fully realized as expected.

Turning back to Table 5, I can compare the point estimates to -0.44 in order to determine if there are spillover effects. I can reject that -0.883 is equal to -0.44 (with a p-value equal to 0.07). This implies that there are negative spillovers to migration from the policy change, and prospective new migrants besides OPAs are more adversely affected by the policy change in high OPA provinces compared to low OPA provinces. In terms of magnitudes, OPA migration decreases by 126.9 more migrants in the 75th percentile of OPA provinces compared to the 25th percentile, while the total number of migrants declines by 167.1 migrants. This suggests a spillover effect of 40.2 fewer migrants in high compared to low OPA provinces.

5.3 By Occupation

In Section 5.2, I showed that there are large and statistically significant spillover effects of the policy change on new hire migration. This means that migration in occupations other than OPAs must be affected by the policy change. In Table 7, I estimate the effect of the ban on

occupation-specific migration rates for the top 38 occupations for Filipino contract migrants.[14] Over half of other occupations appear to experience a decline as a result of the OPA ban, and the negative point estimates are larger in magnitude and more precisely estimated than the positive coefficients. This suggests that there are negative spillovers from the policy change on other occupations. The magnitude of the effects varies substantially across occupations. Other than OPAs, domestic helpers experience by far the largest decline in response to the policy change. The effect, however, may be biased due to a violation of the parallel trends assumption in the pre-period. Given that the magnitude of the coefficient of interest is substantially larger than the coefficient on the pre-trend, this suggests that there is a differential decline in domestic helper migration in high compared to low OPA provinces after the policy change, but the magnitude is ambiguous. Production workers, caregivers, carpenters, and plumbers and welders also experience quite large declines in new hire migration after the policy change in high OPA provinces relative to low OPA provinces. Differential trending does not appear to be a problem for these occupations.[15] These declines help shed light on which migrant occupations tend to be more sensitive to this migration barrier.

5.4 By Gender

While OPA migration is historically an overwhelmingly female occupation, the occupations that decline in response to the OPA policy change are a combination of occupations that are predominantly female (in the case of domestic helpers), mixed gender (such as production workers), and predominantly male (carpenters). Because OPA migration is largely female, the direct effect of the ban should be felt exclusively by females, yet the occupation results suggest that there may be spillover effects onto male migration as well. I examine this explicitly by looking at the response of the male and female migration rates to the OPA policy change. If I find a non-zero point estimate for males, this suggests that there are spillover effects for men from the ban on female OPA migration.

After the policy change, both male and female migration rates decline in high OPA provinces compared to low OPA provinces, though the results are not precisely estimated for men (see Table 8). For females, the magnitude of the ban would be fully realized if the

[14] These 38 occupations make up 96% of all new contract migration.

[15] While there appears to be a pre-trend for caregivers, it is in the opposite direction, and thus biases against finding this negative effect when moving from pre to post.

point estimate is equal to -0.44. I can reject that the point estimate is equal to -0.44, suggesting negative spillovers for females. The total magnitude of the spillovers is the difference between the main effect in Table 5 (-0.883) and -0.44. This yields spillover effects of a magnitude of approximate -0.44. While the male results are imprecisely estimated, it appears this spillover effect contributes evenly for men (0.193) and women (0.69-0.44=0.25). Thus, the policy change led to approximately equal negative spillovers for men and women.

5.5 Domestic Employment

I next turn to examining the domestic employment choices of individuals in the Philippines in response to the OPA policy change. Due to the high wages of OPAs compared to domestic employment, when OPA migration is no longer an option, households may have to reallocate labor market choices within the household. For instance, individuals who were not previously part of the labor force may seek employment or currently employed household members may try to work more hours. In Table 9, I examine the effect of the OPA policy change on province-level employment variables.

First, in Columns 1-3 examine the effect of the migration barrier on the unemployment rate.[16] Positive point estimates in columns 1-3 suggest that increased barriers to migration lead to increases in the unemployment rate as more individuals try to find employment to cope with reduced remittances from abroad. However, only the point estimate on female employment is statistically different from zero. Moving from pre to post period and the 25th to 75th percentile of OPA employment, unemployment increases by 1.97%, or 191 women. This means for each OPA migrant that no longer goes abroad, there are 1.5 more women who are now unemployed (191/126.9). Taking into account spillovers to other migrant groups, there are 1.14 more unemployed women for each migrant that no longer goes abroad (191/167.1).

In Columns 4-6, I estimate the effect of the ban on labor force participation.[17] Total labor force participation increases by 0.27% from the pre to post period when comparing the 75th percentile of baseline OPA migration to the 25th percentile. This suggests that as

[16]The unemployment rate is defined as the number of unemployed individuals out of the total labor force. Individuals are defined as unemployed if they are out of work, actively seeking work, and available for work.

[17]Labor force participation is defined as the number of unemployed and employed individuals out of the total working aged population in the province. Migrant workers are not counted in either the numerator or denominator in the estimates presented here. However, the results are robust to counting migrants as employed and as part of the working population. They are available upon request.

job opportunities abroad become more scarce, an increasing number of individuals seek paid employment. The effects on overall labor force participation appear to be driven by larger increases among females, though the difference is not statistically precise. Examining the interquartile range, female labor force participation increases by 0.41% from pre to post. Male labor force participation also increases by 0.15%.

In terms of magnitudes, a 0.27% increase in labor force participation yields 3,985 more individuals participating in the labor force in the average province. With approximately 167 fewer migrants in high OPA provinces, this implies that for each OPA migrant, 23.9 more individuals are in the labor force. This seems like a large increase until one considers the salaries earned by these workers. OPAs earn an average of $1,857 USD per month. The average worker in the Philippines earns $87 per month. With 23.9 more workers in the labor force, this would lead to $2079 USD per month if all workers were employed at the average wage. With an unemployment rate of 5.5% in the pre-period, this suggests that 1 of these workers will be unemployed, so wages likely increase by at most $1914. Using these conservative bounds on employment, labor force participation would just offset the lost wages from OPAs.[18]

Table 10 shows additional domestic employment results. Column 1 shows that after the policy change, 5.56% more of those currently employed in the 75th percentile of OPA provinces say they are looking for additional work when compared to those in the 25th percentile of provinces. This is not surprising since households now need to compensate for lost remittances from high salaries abroad with lower domestic wages. Individuals are also more likely to be engaged in short-term employment rather than permanent employment, as shown in Column 2. Short-term employment is defined as seasonal, casual, or temporary work. 2.1% more individuals are working in short term contracts in high OPA provinces compared to low OPA provinces after the policy change. This is a less desirable form of employment due both to lower wages and the temporary nature of employment, and so the increased likelihood of this type of work suggests that households are more desperate for paid employment when migration opportunities become more limited. Hours worked appear to decrease, though the point estimate is not statistically significant.

Child labor is a pervasive issue throughout the Philippines, and Table 11 examines the

[18]Using data from the LFS and FIES, (Theoharides, 2015) calculates that for every migrant that goes abroad, approximately 4 households receive remittances. While this statistic is not specific for OPA migrants, it suggests that substantially more households benefit from migration than directly send migrants abroad.

effects of the policy change on child labor.[19] Moving from the pre to post period, provinces in the 75th percentile of baseline OPA migration have a 2.1% higher rate of child labor for 5 to 12 year olds than those provinces in the 25th percentile. Given there were on average 5,052 children aged 5 to 12 working in a province in the pre-period, this means 105 more children are engaged in at least one hour of work per week in high OPA provinces compared to low OPA provinces. With 126 fewer OPAs in high OPA share provinces compared to low, for each OPA who can no longer go abroad 0.83 more children are now engaged in paid work. Considering the total decline in migration, for each migrant who no longer goes abroad, 0.63 more children are employed. To be clear, one hour of work is not synonymous with school dropout, but it is indicative of adjustment of domestic labor market choices in response to the policy change. Rates of child labor increase in high OPA provinces relative to low OPA provinces for 13 to 15 year olds as well. Both male and female rates of child labor also increase. Checks for pre-trends indicate that there is not differential trending in the pre-period, and thus the identifying assumption is not violated.

The effects of the policy change on domestic labor market variables suggest that when migration opportunities are more limited, households alter their domestic labor market decisions by engaging in less desirable labor market opportunities. Thus, the results suggest that while the policy change sought to eliminate a potentially exploitive migration channel, there is a tradeoff in terms of the consequences in the domestic labor market.

5.6 Dynamics

The previous results all estimate the average effect of the OPA policy change on a variety of migration and domestic labor market outcomes. In actuality, the response may be dynamic and the effects of the policy change may vary for years immediately following the policy change versus years further in the future. To examine these dynamic responses and how fast adjustments take place, I estimate the following equation:

[19] Child labor is split into two age brackets: children aged 5 to 12 and children aged 13 to 15. It is defined as the number of children in a given age bracket working at least 1 hour per week out of the same-aged population. Hours worked are not collected for children under age 16, so I am limited to this definition. The LFS also does not differentiate between hazardous work and non-hazardous work.

$$Outcome_{pt} = \beta_0 + \beta_1 ShareOPA_{p0} * \mathbb{1}_{2006} + \cdots + \beta_6 ShareOPA_{p0} * \mathbb{1}_{2011} + \beta_7 X_{p0} * TimeTrend + \alpha_p + \gamma_t + \epsilon_{pt} \quad (3)$$

where $ShareOPA_{p0} * \mathbb{1}_{2006}$ is the share OPA at baseline in province p interact with a dummy variable equal to 1 in the year 2006. I interacted the share OPA with dummy variables for each year in the post period. Examining the point estimates on each of these variables allows me to determine the dynamic labor market response to the policy change.

Table 12 presents the results. Column 1 shows the effects on the total migration rate. The effect of the policy change persists for high baseline OPA provinces compared to low OPA provinces as more time elapses since the policy change. The effects appear to persist for both male and female migrants, though the male results are not precisely estimated. The effects on the OPA migration rate provide a good check: the OPA migration rate declines by approximately 0.4 percentage points more in high compared to low OPA provinces, which is exactly what one would predict given the strength of migrant networks.

In terms of the labor results, both the labor force participation rate and the share of individuals seeking additional work increase the further out from the policy change. This suggests that initially households are able to cope with lower rates of OPA migration, but as time passes, they seek alternative domestic labor market options due to the loss of income from abroad. Child labor rates increase by a larger amount in high OPA provinces compared to low OPA provinces a few years after the policy change. Unemployment, however, increases in the year immediately after the policy change, but the estimates in future years are quite noisy. Overall, it appears that the policy change had a lasting effect both on migration opportunities and on domestic labor market variables.

6 Conclusion

Migration policies imposed by destination countries substantially limit global labor mobility. While numerous papers have addressed the effects of such policies on native workers in destination countries, the literature is largely silent on the effects on migrant-sending countries. Using a policy change in Japan that imposed significant barriers to the migration of

Overseas Performing Artists (OPAs) from the Philippines as a natural experiment, this paper provides the first estimates of the causal effects of migration barriers on labor market choices in migrant-sending countries. I employ a difference-in-differences estimation strategy using the percent of OPA employment in the province population in a base year as a continuous policy variable to define the treatment dosage. Because the policy change occurred in response to accusations of trafficking, the results also provide some of the first estimates of the effects of limiting migration in occupations deemed exploitive or controversial.

I find that in response to the policy change, total migration decreases more for provinces with a higher baseline OPA share. Specifically, moving from the 25th to 75th percentile of the baseline OPA share is associated with a 10.1% greater decrease in total migration. I also find substantial negative spillover effects, with migration declining by more than the amount of the policy change. Domestic helpers, plumbers, carpenters, and production workers, among others, all are hired at a lower rate than prior to the policy change in high OPA share provinces, and spillover effects appear to effect both male and female migrants. Domestically, the labor force participation rate and unemployment rate increase more in high OPA share provinces after the policy change than in low OPA share provinces. Child labor increases differentially by 2.1%, and 2.14% more individuals are now engaged in short-term work. Dynamically, many of these adverse effects become more pronounced over time. A number of robustness checks corroborate the main results.

The results suggest that immigration policies imposed by destination countries have substantial implications for migrant-sending countries. While migration is a lucrative employment option, relying on these opportunities makes migrant-sending countries vulnerable to destination country policy shocks. As more quotas are imposed and anti-trafficking campaigns increase, such policies will continue to have important implications for poor, migrant-sending countries. Policymakers in these countries would do well to use their limited social safety nets to help households in the presence of such policy changes.

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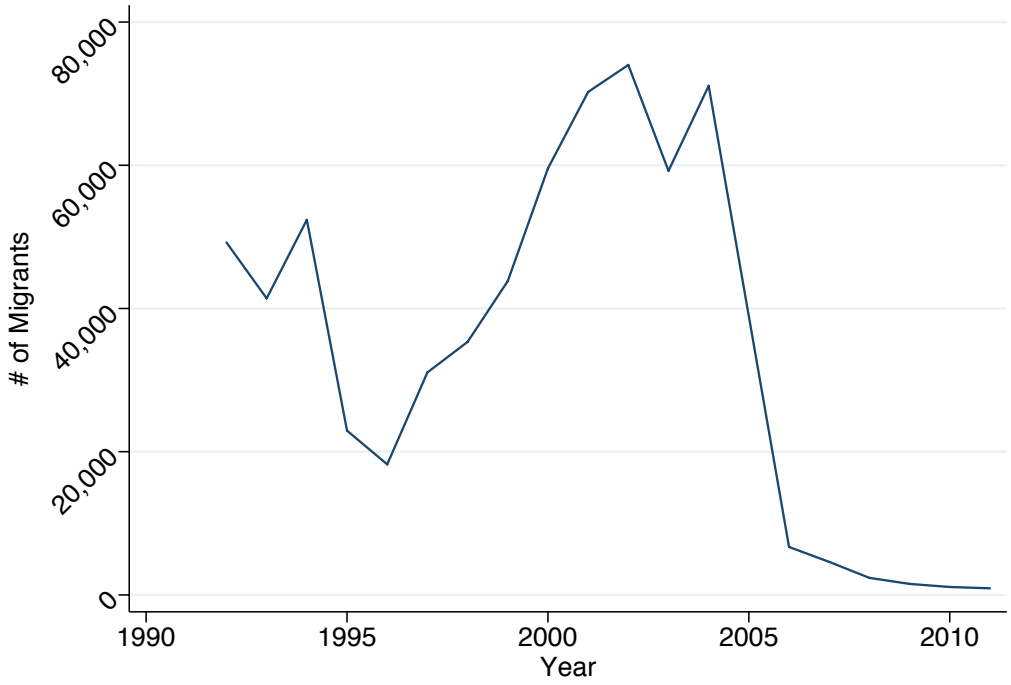
Figure 1: OPA Migration Over Time



Source: POEA.

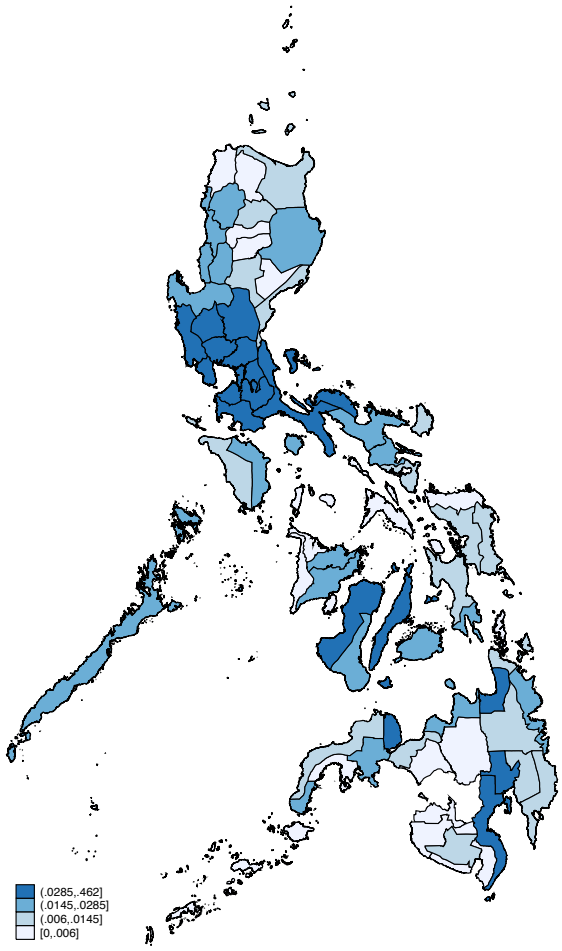


Figure 2: 1993 OPA Migration Rates by Province

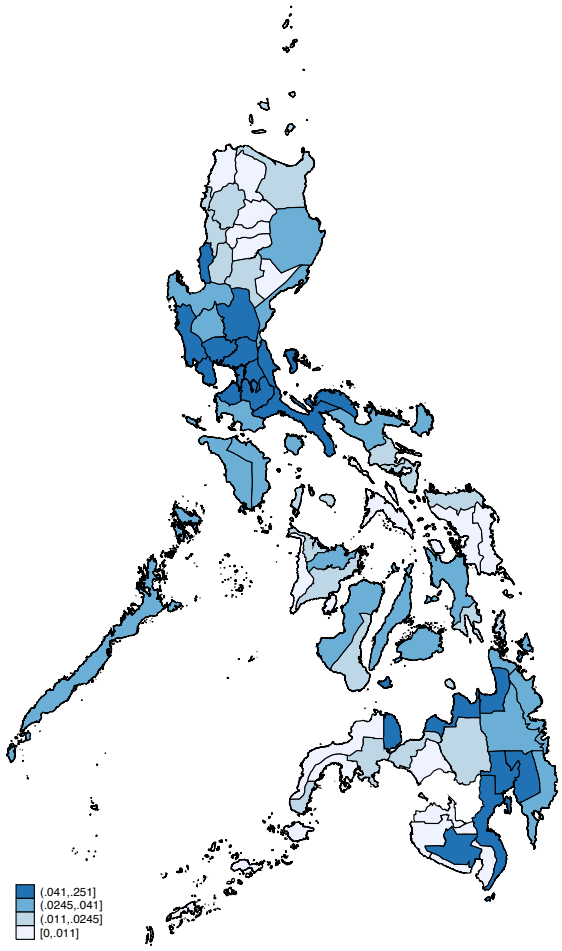
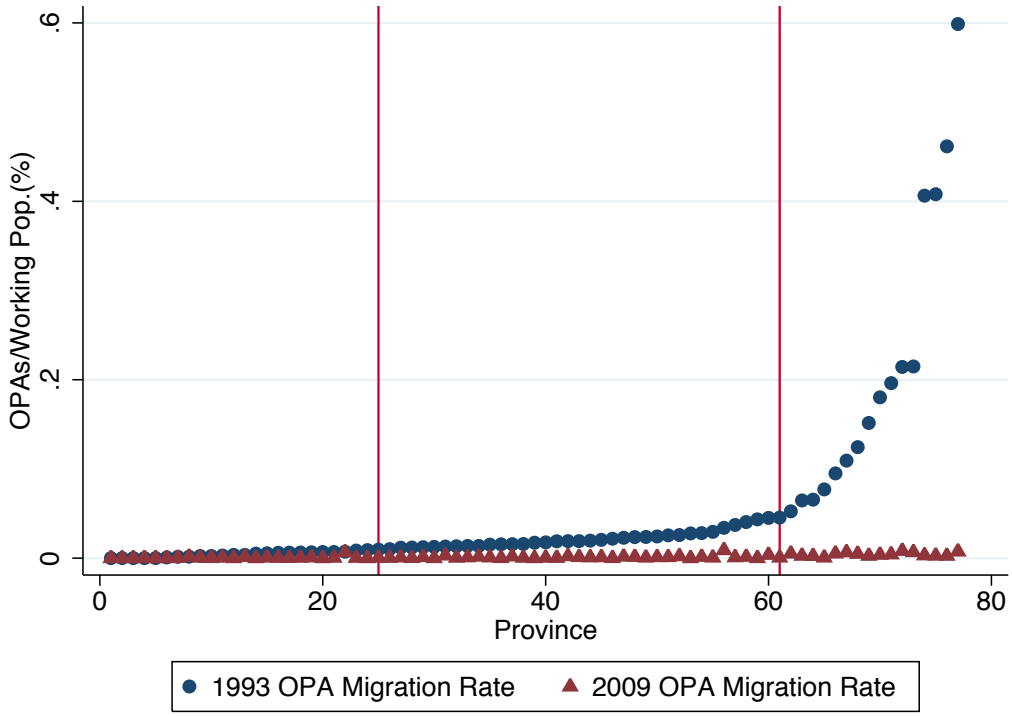


Figure 3: 2004 OPA Migration Rates by Province

Figure 4: Treatment Dosage: OPA Migration Rates in 1993 and 2009



Note: The left vertical line marks the province with the OPA base share at the median (0.01) while the right vertical line marks the province with the OPA base share at the mean (0.05)

Table 1. Top 10 Occupations for Contract Migrants

Occupation	Total	% of Total	% Female
Overseas Performing Artists	71,982	25.5	95.5
Domestic Helpers	63,591	22.6	98.2
Caregivers	20,349	7.2	95.8
Production NEC	18,225	6.5	52.3
Medical Workers	12,418	4.4	85.0
Building Caretakers	10,232	3.6	84.6
Cooks and Waiters	9,482	3.4	59.5
Laborers	7,874	2.8	14.9
Tailors	7,519	2.7	92.0
Engineers	7,409	2.6	5.3
Total	229,081	81	74.3

Notes: The summary statistics are for 2004 and are based on 80 occupation categories.
Source: POEA and author's calcuations.

Table 2. OPA and Non-OPA Characteristics in 2004

	OPA Migrants	Non-OPA Migrants
Female (%)	95.7	67.0
Age (Years)	25.2	32.2
Monthly Salary (USD)	1857.3	417.3
Years of Education	9.4	13.3
Contract Duration (Months)	4.6	20.5

Source: POEA, SOF, and authors' calculations.

Table 3. Summary Statistics

	N	Mean	Std. Dev.	Min	Max
Migration Variables					
OPA Migration Rate (1993-Base share)	77	0.06	0.11	0.00	0.60
Total Migration Rate	616	0.39	0.28	0.01	1.59
OPA Migration Rate (2004)	77	0.05	0.07	0.00	0.28
OPA Migration Rate (2006)	77	0.01	0.01	0.00	0.03
Domestic Labor Market Variables					
Labor Force Participation	1,001	70.59	6.44	51.06	90.17
Unemployment Rate	1,001	5.71	3.16	0.00	16.46
Child Employment Rate	1,001	3.27	4.36	0.00	33.00
Looking for Additional Work	1,001	4.84	4.17	0.00	24.28
Short Term Job	1,001	12.03	5.78	1.81	59.90
Working Population	1,001	527,456	454,109	7,523	2,417,697

Notes: Summary statistics are not population weighted. There are 77 provinces per year. Summary statistics for the total migration rate are calculate for 1996-1998, 2004, and 2006-2009. Summary statistics for the domestic labor market variables are calculated for 1998-2004 and 2006-2011.
Source: POEA, OWWA, LFS, and Census of Population.

Table 4. Effect of Covariates and Migrant Networks on OPA Migration Rates

	1993	1997	1997	2004	2004	2009	2009
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share OPA (1993)			0.5372***		0.4404***		0.0054
			(0.0267)		(0.0541)		(0.0037)
Female	0.0038	0.0036	0.0022*	-0.0052	-0.0050	0.0003	0.0003
	(0.0073)	(0.0057)	(0.0013)	(0.0085)	(0.0039)	(0.0003)	(0.0003)
Age	-0.0008	-0.0053	-0.0025**	-0.0188*	-0.0098	-0.0003	-0.0001
	(0.0086)	(0.0047)	(0.0011)	(0.0111)	(0.0081)	(0.0005)	(0.0004)
Married	0.0012	0.0037*	-0.0002	0.0036	0.0029*	0.0001	0.0000
	(0.0023)	(0.0021)	(0.0005)	(0.0037)	(0.0018)	(0.0001)	(0.0001)
Elementary Graduate	0.0009	-0.0002	-0.0004	0.0000	0.0010	0.0002*	0.0002*
	(0.0015)	(0.0011)	(0.0003)	(0.0021)	(0.0014)	(0.0001)	(0.0001)
Some High School	-0.0078**	-0.0019	-0.0002	0.0048	0.0049*	0.0002*	0.0002**
	(0.0039)	(0.0030)	(0.0006)	(0.0043)	(0.0026)	(0.0001)	(0.0001)
High School Graduate	-0.0018	0.0004	0.0005**	0.0029	0.0019**	0.0001**	0.0001**
	(0.0024)	(0.0012)	(0.0002)	(0.0018)	(0.0008)	(0.0001)	(0.0000)
Some College	0.0065	0.0002	-0.0011***	-0.0067**	-0.0027	-0.0002*	-0.0002*
	(0.0043)	(0.0024)	(0.0004)	(0.0030)	(0.0019)	(0.0001)	(0.0001)
College Graduate	0.0040	0.0080**	0.0005	0.0118**	0.0030	0.0002	0.0001
	(0.0038)	(0.0034)	(0.0006)	(0.0050)	(0.0027)	(0.0001)	(0.0001)
Employment Rate	0.0012	-0.0001	0.0010**	0.0026	0.0011	0.0001*	0.0001
	(0.0020)	(0.0017)	(0.0005)	(0.0027)	(0.0014)	(0.0001)	(0.0001)
Urban	0.0035***	0.0015***	0.0000	0.0017***	0.0005	0.0000**	0.0000
	(0.0009)	(0.0004)	(0.0001)	(0.0005)	(0.0004)	(0.0000)	(0.0000)
Child Unemployment Rate	-0.0006	0.0003	-0.0005**	-0.0024*	-0.0011	-0.0001**	-0.0001*
	(0.0013)	(0.0009)	(0.0002)	(0.0014)	(0.0007)	(0.0001)	(0.0001)
Unemployment Rate	-0.0082	-0.0068	0.0004	-0.0028	-0.0012	0.0001	-0.0000
	(0.0066)	(0.0064)	(0.0013)	(0.0056)	(0.0030)	(0.0003)	(0.0003)
Looking for Additional Work	0.0319*	0.0172	-0.0017	0.0020	-0.0008	-0.0002**	-0.0002**
	(0.0167)	(0.0117)	(0.0030)	(0.0022)	(0.0014)	(0.0001)	(0.0001)
Short Term Job	-0.0002	-0.0006	-0.0001	-0.0021*	0.0002	0.0001	0.0001
	(0.0012)	(0.0009)	(0.0002)	(0.0013)	(0.0007)	(0.0001)	(0.0000)
Obs	77	77	77	77	77	77	77
R2	0.865	0.863	0.994	0.741	0.887	0.553	0.588
Mean Dep. Var	0.06	0.03	0.03	0.05	0.05	0.00	0.00

Notes: Robust standard errors clustered at the province level. All regressions weighted by 1990 working population.
Source: POEA, OWWA, LFS, Census of Population.

Table 5. Effect of OPA Ban on Total Migration Rate

	One Year Pre/Post		Multiple Years Pre/Post		
	(1)	(2)	(3)	(4)	(5)
Post*OPA Share	-0.867***	-1.164***	-1.246***	-1.333***	-0.883***
	(0.187)	(0.419)	(0.205)	(0.374)	(0.247)
Scaled by IQR (%)	(-0.867*.03)/.26=-9.93%	-13.30%	-14.27%	-15.26%	-10.10%
Check for Pre-Trends					-0.040
					(0.060)
Obs	154	154	616	616	616
R2	0.897	0.927	0.878	0.886	0.889
Controls*Post	N	Y	N	Y	N
Controls*Trend	N	N	N	N	Y

Notes: The pre-period is 1996-1998 & 2004. The post-period is 2006-2009. All regressions include province and year fixed effects. Baseline controls for fraction female, average age, fraction married, average education levels, fraction employed, fraction unemployed, fraction urban, fraction looking for additional work, and the fraction working in short term work are included where indicated. Robust standard errors are clustered at the province level. All regressions weighted by 1990 working population. The check for pre-trends examines the effect of the OPA share at baseline on the change in the migration rate between 1996 and 1998. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.
Source: POEA, OWWA, LFS, Census of Population.

Table 6. Robustness Checks: Effect of OPA Share on Total Migration Rates

	Main Specification	No 1st District	No 2nd District	No 3rd District	No 4th District	No Manila	Without Population Weights
Panel A: Geographic Restrictions	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post*Share OPA	-0.883***	-1.173***	-0.901***	-0.752***	-0.848***	-0.355	-0.841***
	(0.247)	(0.194)	(0.307)	(0.221)	(0.243)	(0.306)	(0.246)
Obs	616	608	608	608	608	584	616
R2	0.889	0.890	0.886	0.893	0.892	0.898	0.874

	Pre-period: 1998 & 2004	Pre-period: 1996-1998	Post Period: 2006 Only	Falsification Exercise: 1998 & 2004	Above Average OPA Share Only	Above Median OPA Share Only
Panel B. Sample Period and OPA Share Restrictions	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share OPA	-0.915***	-1.404***	-0.761***	-0.069	-0.953**	-0.890***
	(0.238)	(0.256)	(0.153)	(0.255)	(0.351)	(0.274)
Obs	462	539	385	154	128	312
R2	0.888	0.911	0.926	0.973	0.918	0.903

Notes: The pre-period is 1996-1998 & 2004, and the post-period is 2006-2009 (unless otherwise specified). All regressions include province and year fixed effects. Baseline controls for fraction female, average age, fraction married, average education levels, fraction employed, fraction unemployed, fraction urban, fraction looking for additional work, and the fraction working in short term work are included where indicated. Robust standard errors are clustered at the province level. All regressions weighted by 1990 working population. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.

Source: POEA, OWWA, LFS, Census of Population.

Table 7. Effect of OPA Ban on Occupation-Specific Migration Rates

	Post*Share OPA	Pre-Trends		Post*Share OPA	Pre-Trends
OPAs	-0.465***	0.002	Manufacturing	0.001	-0.003
	(0.029)	(0.007)		(0.003)	(0.002)
Agriculture	-0.001	0.000	Material-Handling	-0.013	0.002
	(0.001)	(0.001)		(0.008)	(0.001)
Engineers	-0.014	0.002	Medical	-0.020	-0.008
	(0.016)	(0.003)		(0.013)	(0.007)
Machine-Tool Operators	-0.005	0.001	Painters	-0.001	-0.001
	(0.003)	(0.002)		(0.002)	(0.001)
Cashiers	0.001	-0.001**	Plumbers, Welders	-0.051*	0.009
	(0.003)	(0.000)		(0.026)	(0.006)
Carpenters	-0.023**	0.005	Processors	0.003**	-0.000
	(0.011)	(0.004)		(0.001)	(0.002)
Building Caretakers	-0.010	-0.001	Production NEC	-0.044***	0.013
	(0.018)	(0.002)		(0.014)	(0.009)
Caregivers	-0.023*	0.015***	Production Supervisors	-0.002	-0.000
	(0.011)	(0.006)		(0.004)	(0.001)
Clerical	0.000	-0.001	Professional NEC	0.000	-0.001
	(0.002)	(0.000)		(0.003)	(0.001)
Clerical NEC	0.001	-0.001	Protective Services	-0.003**	0.000
	(0.008)	(0.001)		(0.002)	(0.000)
Construction	0.002	0.001**	Sales	0.002	-0.000
	(0.001)	(0.001)		(0.002)	(0.000)
Cooks, Waiters	0.009	-0.004**	Sales Workers NEC	0.003**	-0.000
	(0.020)	(0.002)		(0.001)	(0.001)
Domestic Helpers	-0.178***	-0.061**	Salesmen	0.001	-0.002***
	(0.040)	(0.027)		(0.005)	(0.001)
Electrical	-0.009	0.004	Scientists	-0.001	-0.000
	(0.010)	(0.003)		(0.001)	(0.001)
Food Processors	0.002	-0.001*	Service NEC	-0.012	-0.003**
	(0.003)	(0.001)		(0.010)	(0.001)
Hairdressers	0.001	-0.000*	Spinners, Weavers	0.001	0.001
	(0.002)	(0.000)		(0.001)	(0.002)
Laborers	-0.016	0.005	Typists	0.000	-0.002***
	(0.015)	(0.004)		(0.002)	(0.001)
Machine Fitters	-0.006	0.004*	Tailors	-0.012*	-0.006***
	(0.006)	(0.002)		(0.007)	(0.002)
Managers NEC	0.002	-0.001**	Transport Operators	-0.004	-0.007**
	(0.001)	(0.000)		(0.003)	(0.003)

Notes: N=616. The pre period is from 1996 to 1998, and 2004, and the post period is from 2006 to 2009. All regressions include province and year fixed effects, as well as baseline controls interacted with a time trend as in Table 5. Robust standard errors are clustered at the province level. The unit of observation is the province-year. All specifications are waited by the province population in 1990. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.

Source: POEA, OWWA, LFS, and Census of Population.

Table 8. Effect of OPA Ban on Migration Rates, By Gender

	Total	Female	Male
	(1)	(2)	(3)
Post*OPA Share	-0.883***	-0.690***	-0.193
	(0.247)	(0.142)	(0.126)
Scaled by IQR (%)	-10.11%	-12.47%	-6.00%
Check for Pre-Trends	-0.040	-0.041	0.001
	(0.060)	(0.043)	(0.028)
Obs	616	616	616
R2	0.889	0.864	0.884

Notes: The pre-period is 1996-1998 & 2004. The post-period is 2006-2009. All regressions include province and year fixed effects, as well as baseline controls interacted with a time trend as listed in Table 5. Robust standard errors clustered at the province level. All regressions weighted by 1990 working population. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.
Source: POEA, OWWA, LFS, Census of Population.

Table 9. Effect of OPA Ban on Domestic Unemployment and Labor Force Participation

	Unemployment Rate (Total)	Unemployment Rate (Female)	Unemployment Rate (Male)	Labor Force Partcipation (Total)	Labor Force Partcipation (Female)	Labor Force Partcipation (Male)
	(1)	(2)	(3)	(4)	(5)	(6)
Post*OPA Share	2.381	3.998*	1.190	6.425***	7.683***	4.369**
	(2.153)	(2.230)	(2.341)	(1.849)	(2.467)	(1.814)
Scaled by IQR (%)	1.31%	1.97%	0.69%	0.27%	0.43%	0.15%
Check for Pre-Trends	-0.190	0.277	-0.552	1.419	1.442	0.970
	(0.804)	(1.048)	(0.813)	(0.997)	(1.712)	(0.667)
Obs	1001	1001	1001	1001	1001	1001
R2	0.886	0.716	0.909	0.880	0.876	0.916
Mean Dep. Var	5.45	6.09	5.17	70.75	54.11	87.09

Notes: The pre-period is from 1998 to 2004. All regressions include province and year fixed effects, as well as baseline controls interacted with a time trend listed in Table 5. Robust standard errors clustered at the province level. All regressions weighted by 1990 working population. The rate of labor force participation is the number of individuals either employed or unemployed out of the total population. The unemployment rate is defined as the total number of unemployed individuals out of the total labor force (employed plus unemployed), where an individual is classified as unemployment if they are out of work, actively seeking work, and available to work. The mean dependent variable is the mean in the pre-period. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.

Source: POEA, OWWA, LFS, Census of Population.

Table 10. Effect of OPA Ban on Domestic Labor Market Outcomes

	Looking for Additional Work	Short-Term Job	Total Hours Worked
	(1)	(2)	(3)
Post*OPA Share	5.992***	8.433***	-0.568
	(1.506)	(2.559)	(1.224)
Scaled by IQR (%)	5.56%	2.05%	-0.04%
Check for Pre-Trends	0.592	-0.951	-0.721
	(0.654)	(1.052)	(0.578)
Obs	1001	1001	1001
R2	0.754	0.785	0.941
Mean Dep. Var	3.23	12.32	39.3

Notes: The pre-period is from 1998 to 2004. All regressions include province and year fixed effects, as well as baseline controls interacted with a time trend listed in Table 5. Robust standard errors clustered at the province level. All regressions weighted by 1990 working population. Those looking for additional work are employed, but seeking additional hours or a different job. The mean dependent variable is the mean in the pre-period. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.
Source: POEA, OWWA, LFS, Census of Population.

Table 11. Effect of OPA Ban on Domestic Child Labor

	Total		Female		Male	
	Ages 5-12	**Ages 13-15**	**Ages 5-12**	**Ages 13-15**	**Ages 5-12**	**Ages 13-15**
	(1)	(2)	(3)	(4)	(5)	(6)
Post*Share OPA	2.264***	3.893*	1.824**	3.267*	2.716***	4.817
	(0.818)	(2.125)	(0.782)	(1.819)	(0.920)	(2.921)
Scaled by IQR (%)	2.08%	0.66%	2.26%	0.84%	2.01%	0.62%
Check for Pre-Trends	0.177	-0.334	0.671	0.399	0.470	0.651
	(0.753)	(0.599)	(0.938)	(1.676)	(1.857)	(1.866)
Obs	1001	1001	1001	1001	1001	1001
R2	0.825	0.875	0.787	0.810	0.825	0.879
Mean Dep. Var	3.26	17.70	2.42	11.70	4.05	23.40

Notes: The pre-period is from 1998 to 2004. All regressions include province and year fixed effects, as well as baseline controls interacted with a time trend listed in Table 5. Robust standard errors clustered at the province level. All regressions weighted by 1990 working population. The rate of child labor is defined as the number of children in each age range working at least one hour in the past week out of the total number of children in that age range. The mean dependent variable is the mean in the pre-period. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.
Source: POEA, OWWA, LFS, Census of Population.

Table 12. Dynamic Effects of OPA Policy Change

	Total Migration Rate	Total Migration Rate (F)	Total Migration Rate (M)	OPA Migraton Rate	Labor Force Participation	Unemployment Rate	Looking for Additional Work	Child Labor Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share OPA*2006	-0.950***	-0.753***	-0.196*	-0.444***	6.418***	4.696**	5.340***	1.283*
	(0.195)	(0.117)	(0.100)	(0.026)	(1.686)	(2.113)	(1.488)	(0.743)
Share OPA*2007	-0.937***	-0.704***	-0.232	-0.473***	5.934***	-0.215	5.662***	2.161**
	(0.254)	(0.133)	(0.140)	(0.028)	(1.965)	(2.265)	(1.990)	(0.843)
Share OPA*2008	-0.732**	-0.581***	-0.151	-0.487***	7.626***	1.528	7.758***	3.811***
	(0.342)	(0.200)	(0.164)	(0.031)	(2.221)	(2.254)	(2.000)	(1.111)
Share OPA*2009	-0.660	-0.545**	-0.115	-0.493***	7.536***	1.245	9.869***	4.183***
	(0.400)	(0.254)	(0.173)	(0.033)	(2.439)	(2.204)	(2.549)	(1.303)
Share OPA*2010					10.018***	-2.244	10.632***	3.737***
					(3.312)	(2.732)	(3.069)	(1.269)
Share OPA*2011					11.573***	-2.487	15.344***	5.000***
					(3.281)	(2.897)	(3.004)	(1.887)
Obs	616	616	616	616	1001	1001	1001	1001
R2	0.891	0.868	0.885	0.936	0.881	0.892	0.763	0.827

Notes: All regressions include province and year fixed effects. Baseline controls for fraction female, average age, fraction married, average education levels, fraction employed, fraction unemployed, fraction urban, fraction looking for additional work, and the fraction working in short term work are included where indicated. Robust standard errors are clustered at the province level. All regressions weighted by 1990 working population.*** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.

Source: POEA, OWWA, LFS, Census of Population.

Appendix A: Binary Treatment Results

In this paper, I define a continuous treatment variable that is the share OPA in the year 1993. I prefer using this continuous treatment variable because the choice of binary treatment and control groups is somewhat arbitrary. However, in this appendix, I conduct a more traditional difference-in-differences analysis where I define a binary treatment variable equal to 1 for high OPA share provinces. I then regress the treatment variable on a number of covariates in 1993.[20] I predict the propensity scores, and define my control group as those provinces with propensity scores about a specified cutoff. Panels A through D of Appendix Table 1 vary the cutoffs for both the treatment and control group.

The results are quite robust to selecting a control group of provinces using propensity score matching instead of the continuous treatment variable. I find that both the total migration rate and the OPA migration rate decline in treatment province compared to control provinces moving from the pre to post period. Labor force participation, the unemployment rate, the child labor rate, the share of the population looking for additional work, and the share working in short-term jobs all increase more in the treatment than control after the policy change, though not all of these point estimates are statistically different from zero. The results are robust to various definitions of the treatment and control group, as shown by comparing Panels A through D.

[20] Covariates include province-level variables for share female, average age, average education levels, share urban, unemployment rate, employment rate, child labor rate, share of individuals looking for additional work, and share of individuals working in short-term jobs.

Appendix Table 1. Results Using Propensity Score Matching

	Total Migration Rate	OPA Migration Rate	Labor Force Participation	Unemployment Rate	Child Labor Rage	Short Term Job	Looking for Additional Work
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. Treatment Group: Share OPA>0.15, Control Group: P-Score>0.2						
Post*Treatment	-0.300***	-0.152***	0.941	2.916**	0.454	3.783***	0.942
	(0.092)	(0.026)	(0.917)	(1.059)	(0.627)	(0.998)	(0.754)
Obs	136	136	221	221	221	221	221
R2	0.929	0.951	0.896	0.778	0.886	0.719	0.836
	Panel B. Treatment Group: Share OPA>0.15, Control Group: P-Score>0.2 & Share OPA<0.05						
Post*Treatment	-0.244**	-0.163***	0.633	2.633**	0.695	4.400**	1.297*
	(0.110)	(0.023)	(0.823)	(0.979)	(0.550)	(1.524)	(0.675)
Obs	136	136	221	221	221	221	221
R2	0.936	0.947	0.894	0.793	0.868	0.860	0.838
	Panel C. Treatment Group: Share OPA>0.15, Control Group: P-Score>0.1 & Share OPA<0.05						
Post*Treatment	-0.227**	-0.141***	0.742	2.333**	0.172	2.643**	0.794
	(0.090)	(0.025)	(0.696)	(0.927)	(0.449)	(1.170)	(1.065)
Obs	176	176	286	286	286	286	286
R2	0.921	0.928	0.934	0.791	0.943	0.818	0.740
	Panel D. Treatment Group: Share OPA>0.2, Control Group: P-Score>0.1 & Share OPA<0.05						
Post*Treatment	-0.305**	-0.190***	0.775	3.294***	0.537	4.052**	1.462*
	(0.118)	(0.017)	(1.045)	(0.996)	(0.640)	(1.847)	(0.756)
Obs	128	128	208	208	208	208	208
R2	0.942	0.962	0.888	0.808	0.834	0.874	0.815

Notes: The pre-period in columns 1 and 2 is 1996-1998 and 2004. The pre-period in columns 3 through 7 is from 1998 to 2004. All regressions include province and year fixed effects, as well as baseline controls interacted with a time trend listed in Table 5. Robust standard errors clustered at the province level. All regressions weighted by 1990 working The propensity score is calculated by regressing the share OPA in 1993 on a set of baseline covariates. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.
Source: POEA, OWWA, LFS, Census of Population.