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

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May 22, 2014

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Abstract

International migration provides substantial benefits to poor countries, yet emigration flows are often constrained through destination country immigration policies. Despite the prevalence of restrictions on migration flows, the literature is largely silent on the implications of these barriers on migrant-sending countries. To estimate a causal effect of migration barriers on labor market choices of individuals 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, in response to accusations from the United States of human trafficking, Japan dramatically changed the requirements for Filipinos migrating as overseas performing artists (OPAs), resulting in a decline from 71,108 to 6,696 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 1.2%. The effect on international migration is larger than the policy change itself would suggest, indicating the importance of spillovers across migrant occupations. Domestically, more children are employed, and adults are more likely to be unemployed, looking for additional hours, or engaged in short term work. These results suggest that migration barriers and the elimination of controversial migration channels such as sex work can have major repercussions for labor market choices in migrant-sending countries.

JEL: O12, F22

*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 the University of Michigan, 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, 2014) 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 primarily has focused 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.

At a microeconomic level, migration barriers can have a number of effects on migrant-sending countries. Understanding the consequences of such policies is of particular importance for policymakers in 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. Child labor may also rise in response. Internal and international migration may change, though the direction is ambiguous depending on whether credit constraints prevent

migration or whether fewer labor market opportunities at home encourage migration. While previous studies have examined changes in demand for migrants or changes in remittances on a variety of sending-country outcomes, migration barriers imposed by immigration policy differ from changes in host-country demand for migrants in that they typically represent a more permanent change. If households are able to adjust their employment decisions to perfectly 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.

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 decisions of households in the country of origin? To answer this question, I use a policy change in Japan that imposed significant barriers to the migration of Filipino Overseas Performing Artists (OPAs) as a natural experiment. OPAs are primarily women working as hostesses in nightclubs and gentlemen's clubs. 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.

Yet, 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, 2014). 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. The magnitude of the point estimates sheds light on if there is a multiplier effect for migration. If I find a reduction in migrants from treated provinces that is larger than

the reduction imposed by the ban itself, this suggests that there are spillover effects from the policy change. If, on the other hand, I find that the reduction in migrants is smaller than what would occur as a result of the ban, this suggests that OPAs are able to easily switch into new occupations overseas.

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 1.2% greater decrease in migration after the policy change. The effects are larger for new hires, suggesting that new migrant contracts are more vulnerable to policy shocks. I also find a substantial multiplier effect, with migration declining by more than the amount of the policy change. The multiplier effects are larger for new migrants, and current migrants appear to crowd out new migrants by renewing contracts at a higher rate. The spillover effects of this policy lead to reductions in both female and male migration. Domestic helpers, plumbers, laborers, and production workers all are hired at a lower rate than prior to the policy change in high OPA share provinces. Domestically, the unemployment rate for women rises by 1.7% more in high OPA share provinces after the policy change than in low OPA share provinces. Child labor increases differentially by 1.3%, and 1.8% 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, the 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. Robustness checks that define a treated group of provinces and construct a synthetic control group of provinces corroborate the main results.

This paper provides the first microeconomic estimates of the effects of a migration barrier on labor market decisions. It is also the first paper to examine the effects of a migration barrier imposed by the receiving country rather than the sending country. This is an important distinction because, while the effects on households may be similar, the government engagement may be vastly different. I also provide the first estimates of the migration multiplier. 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 incentives for individuals to undertake employment 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, followed by robustness checks in Section 6. Section 7 concludes.

2 Background

2.1 Filipino Migration

International migration is a common labor market option in the Philippines. Started in 1974, the Philippine Overseas Employment Program 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). Theoharides (2014) 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 will 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 hire migration 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 overwhelmingly more 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. 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, 2014), yet OPAs remain the 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 work as hostesses in gentlemens clubs in Japan, where the dress code is "high heels and 'sexy' dresses" (Parrenas, 2008). Prior to 2005, before OPAs were hired recruiting agencies typically sent a photograph to prospective Japanese employers to aid their selection of OPAs. While POEA conducted an audition prior to deployment, this process was often tainted because recruitment agencies would find a way for selected OPAs to pass the audition, often through impersonation (Barayuga, 2014).[1]

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

[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 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 aspira-

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 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.

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).

tional 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.

2.3 Multiplier Effects

While the OPA policy change only directly affected the migration of OPAs, in theory the ban may affect migration in other occupations and destinations. This would result in effects on migration that are larger or smaller than the magnitude of the ban itself. Contextualizing the magnitude of the effects of the OPA ban provides the first estimates of a "migration multiplier." Since many quotas or points systems refer specifically to migrants in certain occupations and are of course destination specific, this multiplier quantifies the importance of spillover effects and switching behavior across migration channels in determining migration outcomes.

Multiplier effects may matter 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 as suggested by the policy change, 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, 2014), and so it seems unlikely that OPAs can easily switch when their employment opportunities are no longer available since a surplus of potential migrants

already exists.

3 Data

To calculate the 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 being paid wages as stipulated by their contract. The data include name and demographics, as well as destination, occupation, employer, 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, OWWAs key variables of interest are the home address of the migrant so that in the event of natural disasters or other turmoil, they can contact the migrants family. Combining these two datasets creates a dataset that includes both the occupation and destination of the migrant as well as their home address in the Philippines.[4,5] 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 0.44%, OPA migration clearly represents a significant portion of all overseas migration episodes.

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

[4]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 (2014) for further details.

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

calculate both total province level migration rates as well as 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 employment and unemployment rates, the fraction of working aged individuals looking for work or looking for additional work, and the fraction of child aged 10 to 14 engaged in at least one or more hours of work per week.

Table 3 shows summary statistics for all three datasets. The new hire migration rate from the POEA data (0.44%) is much lower than the overall migration rate from the LFS (2.44%).[6] Using the POEA data, I can also calculate occupation-specific migration rates. For OPAs, the rate is on average 0.05% in 2004, the year before the policy change, and 0.01% of the population in 2006, the year after the policy change. 8.6% of the working population is currently unemployed.[7] 9.8% of children between the ages of 10 and 14 worked at least one hour in the past week. 1.5% of the working population is actively looking for work, while 4.9% of the population reports looking for additional work to supplement their current employment. Almost 12% of people 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. The further right a province is in the figure, the largest the effect of the policy change in the province.

Formally, I implement a difference-in-differences style analysis with a continuous treatment

[6] Most new hires go abroad on two-year contracts. The new hire migration rate as calculate here only includes the outflow in the current year. Thus, the LFS migration rate will include both these new hires, as well as new hires who are in the second year of their contract. As such, rehires as not simply the residual of the total LFS migration rate and the reported new hire migration rate, but will rather be less than this residual.

[7] The LFS defines unemployment as either those looking for work or discouraged workers.

variable. I estimate the following equation:

$$MigrationRate_{pt} = \beta_0 + \beta_1 Post_t * ShareOPA_{p0} + \beta_2 X_{pt} + \alpha_p + \gamma_t + \epsilon_{pt} \quad (1)$$

where $MigrationRate_{pt}$ is the migration rate 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. 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. 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. There 80 provinces used in the analysis.[8] β_1 estimates the effect of the policy change for OPA provinces with different baseline shares on the province-level migration rate, among other outcomes.

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 migration rates in provinces with different baseline shares are parallel. I test this assumption by plotting the average total province-level migration rates before and after the policy change by quartile of the baseline OPA shares. Figure 5 shows the results. The trends appear parallel in the pre-period, though there does not appear to be much of an effect of the policy in the post-period. I will formally test the parallel trends assumption while controlling for covariates in Section 5.

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 migration rate, this may lead to potential bias. Turning to the data, in order 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

[8] I drop four provinces that were not yet established in 1998 in order to have a balanced panel.

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. 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. Thus, while some covariates are correlated with the OPA shares, the number of statistically significant characteristics is similar to what would be found due to chance. However, to alleviate concern that differences in provinces at baseline may lead to differential trending in omitted variables related to the outcome variable, I will control for these covariates in all specifications.

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 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. 2009 is five years after the policy change, and we can see that following the policy change, a high OPA migration rate at baseline is no longer predictive of the remaining OPA migration rate.

Figure 6 shows the importance of these networks graphically by plotting the province-level OPA migration rate in 1993 against the province-level OPA migration rate in 2004 along with a 45-degree line, shown as a solid line. While a best fit line (dashed) does not lie directly on the 45-degree line, suggesting that there is some movement in OPA shares over time, this figure further supports the importance of historic OPA migration rates in determining OPA migration rates over time. The main differences are due to four outliers, the four districts of Manila. In 1993, these districts composed much larger shares of OPA migration, but over time some of the migration opportunities spread across the provinces.

5 Results

5.1 Effects on Migration

In Table 5, Column 1, I estimate the effect of the OPA policy change on the total province-level contract migration rate, which includes both new hires and rehires and is calculated using the LFS. For a one percentage point increase in the fraction of OPAs in a province at baseline, the total migration rate decreased by -1.07 percentage points. Recall from Table 3 that the average fraction of OPAs out of the province population at baseline is 0.05% 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*1.07=0.03 percentage point decrease in the total migration rate. Off a migration rate with a sample mean of 2.44%, this leads to a 1.2% decline in the total migration rate in the 75th percentile of OPA provinces compared to the 25th percentile.

In Column 2, I estimate the same equation, instead calculating the total overseas migration rate using the Philippines Census of Population. Because the Census is conducted every ten years, I estimate a basic two period difference-in-differences analysis using 2000 as the pre period and 2010 as the post period. Since all Filipino households are asked in the Census about the number of international migrants in the household, using census data provides a check on the estimates using the LFS in Column 1, though the sample period is limited. When moving from the 25th percentile of baseline OPA migration to the 75th percentile, total international migration falls by 0.04 percentage points, or 1.7%, though it is not precisely estimated due to limited power from the small sample size. Though imprecisely estimated, the magnitudes of the estimated effects across the two data sources are similar.

One concern with this estimation strategy is potential differential trending of the total migration rate by baseline OPA share. While Figure 5 suggests that the pre-trends between high and low OPA provinces are parallel, 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:

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

where t is the pre-period, $\Delta(MigrationRate_{pt})$ is the change in the province-level migration rate in province p from year $t-1$ to year t, and $\Delta(X_{pt})$ is the change in province-year level covariates from year $t-1$ to t. The results are shown in row 2 of Table 5. For the LFS results, I find that a 1 percentage point increase in the OPA migration rate at baseline leads to a 0.001 percentage point increase in the change in the total migration rate, and the coefficient is not statistically different from zero. The magnitude of the point estimate is quite small, suggesting that the migration rate in provinces with higher OPA shares is not changing differentially compared to lower share provinces, and the trends in the pre-period are in fact parallel. Using Census data, the coefficient is larger than when using the LFS, but it is statistically imprecise and small relative to the estimated effect in row 1. Further, the positive trending in the pre-period would bias against the estimated effect, suggesting that the results are actually an underestimate.

Second, I also check for differential trends in the pre period using a falsification exercise. Using the LFS data, I estimate equation 1, instead using 1998 and 1999 as pre-periods, and 2000 to 2004 as the post period.[9] I find that when moving from the pre-period to the "post" period, a one percentage point increase in the OPA migration rate at baseline leads to a 0.24 percentage point increase in the total migration rate, though the coefficient is imprecisely estimated.[10]

Next, in Table 5, Column 3, I turn to the effect of the policy change on new contract hires only. Examining the results by just new hires rather than the aggregation of new hires and rehires allows me to examine if new employment is more responsive to policy changes than renewed employment.[11] Shifting from the 25th to 75th percentile of the OPA baseline

[9] Because in the Census data I only have two periods in the pre-period (1990 and 2000), the results of the falsification exercise will be the same as estimating equation (2) above.

[10] One might also be concerned that the negative effects of the policy change on the migration rate are simply a result of mean reversion. For instance, high OPA provinces at baseline could be low OPA provinces in later years, causing the negative effects to be driven by this. However, the high correlation of baseline shares and later OPA migration rates shown in Table 4 indicates that this is unlikely to be the case. As discussed in Section 4, provinces that were high OPA provinces at baseline continue to be in later years as well, and thus mean reversion is not the cause of the effects.

[11] Due to lack of data, I cannot examine the effect of the policy change on rehires only. Data are only available on total migration from the LFS (rehires plus new hires) and new hires from POEA.

share results in a 9.6% decline in the province-level new hire migration rate after the ban. Comparing the point estimate for total migration to the point estimate for new hire migration, magnitudes differ substantially. This indicates that new hires are more adversely affected by the policy change than total migration, which suggests that rehires are less vulnerable to policy shocks than those potential migrants seeking a new contract. I again check for parallel trends, estimating equation 1 on the pre-period of 1996 to 1997. I find that for a one-percentage point increase in the OPA migration rate, the change in the migration rate is 0.27 percentage points and statistically significant. This positive result, while concerning, implies that the effect I find is a lower bound of the true estimate, and its magnitude is much smaller than the overall result shown in Row 1.

5.2 Multiplier Effect

The magnitude of the point estimates provides an estimate of the migration multiplier 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, with perfectly predictive migrant networks 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 a multiplier effect.

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 0.4 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 multiplier 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 0.4 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 -0.4 percentage points.

While multiplier 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 a multiplier effect, I first look at the effect of the policy change on the OPA migration rate. Shown in Table 6, Column 1, a one percentage point increase in the baseline OPA share causes a 0.4 percentage point decline in the OPA migration rate when moving from the pre to post period. Given that a one-percentage point increase in the baseline OPA share implies a 0.4 percentage point higher OPA migration rate in the year prior to the policy change, this indicates that the effect of the ban is fully realized. Higher OPA provinces have lower OPA migration rates by exactly the differential amount of OPAs in the province.

Turning back to Table 5, I can compare the point estimates to 0.4 in order to determine if there is a multiplier effect. In Column 1, while I cannot reject that 1.07 is equal to 0.4, the magnitudes are clearly quite different. This suggests that there is a substantial migration multiplier, and prospective migrants besides OPAs are differentially affected by the policy change in high OPA provinces compared to low OPA provinces. While OPAs clearly can no longer migrate, these multiplier effects may be due to recruitment agencies closing, binding credit constraints as OPA remittances are no longer received, or weakening of migrant networks, among other explanations.

An even larger multiplier can be seen for new hire migration. For the total migration rate to fall by less than the new hire migration rate, rehires must be declining at a slower rate than new hires. This suggests that households may be able to at least partially adjust to the loss of employment opportunities from OPA migration by renewing existing contracts. As a result, given demand is rigid and determined outside the Philippines, fewer potential new migrants in the province are able to go abroad on new contracts relative to lower OPA provinces.

5.3 By Occupation

Above, I showed that there is a large and statistically significant multiplier effect of the policy change on new hire migration. This means that migration in occupations other than OPAs must be affected by the policy change. Using the new hire micro data from POEA, I can further explore this to see which occupations are affected. In Table 6, I estimate the effect of the ban on occupation-specific new hire migration rates for the top 38 occupations for Filipino contract migrants.[12] A number of other occupations appear to experience a decline

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

as a result of the OPA ban. These declines could occur for two reaSons: first because of multiplier effects leading to spillovers from the policy change on other occupations or second because rehires increase in certain occupations in response to the policy change, thus crowding out new hires. Essentially all of the 38 occupations experience a decrease in higher OPA provinces when moving from the pre to post period, though the magnitude of the effects varies substantially across occupations. Domestic helpers experience by far the largest decline in response to the policy change.[13] Production workers, laborers, and plumbers and welders also experience quite large declines in new hire migration in high OPA provinces relative to low OPA provinces. These declines help shed light on who is affected by the multiplier effect.

5.4 By Gender

While OPA migration is a historically overwhelmingly female occupation, the occupations that decline in response to the OPA policy change are both predominantly female (in the case of domestic helpers), mixed gendered (such as production workers), and predominantly male (laborers). 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 are some 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.

As shown in Table 7, this is in fact what I find in both the case of total migration and new hire migration. After the policy change, male migration declines in high OPA provinces compared to low OPA provinces. Scaling the effects by the interquartile range of the OPA base share, total male migration decreases by 0.9%. This suggests that despite the fact that the policy change only affected women, there were spillover effects on the migration of men as well, and after the policy change, in high OPA provinces compared to low OPA provinces, fewer males are migrating. For females, on the other hand, the magnitude of the ban would again be fully realized if the point estimate is equal to -0.4. In the case of total female migration, while the point estimate is larger in absolute value than the 0.4, statistically I cannot reject that

[13]This result for domestic helpers, however, should be discounted as the parallel trends assumption is violated in the pre-period as determined by the two checks shown in Table 5, making it difficult to ascertain the true casual effect. For all other occupations, the data do not reject the parallel trends assumption.

they are the same. Total female migration declines by the anticipated amount from the policy change. For female new hire migration, I can reject that the point estimate, -0.85, is equal to -0.4. Fewer females are able to migrate from high OPA provinces after the policy change compared to low OPA provinces than just the OPA policy would suggest. Thus, to reconcile the total female and new hire female results, females in high OPA provinces in occupations other than OPAs must be renewing their contracts at a higher rate than in low OPA provinces. Overall, these results by gender suggest that this barrier to migration for females had spillovers to both male and female migration.

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.[14] 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 8, I examine the effect of the OPA policy change on the province-level unemployment rate. Unemployment is defined in the LFS as persons who are either currently looking for work or who would like to work, but have given up searching. I calculate this rate of unemployment out of the total working-aged population in the province. When moving from the pre-period to the post-period, the unemployment rate increases by 0.4% more in the 75th percentile of OPA share provinces compared to the 25th percentile. While the point estimate has the anticipated sign, it is not statistically different than zero.

Examining the unemployment rate by gender, the female unemployment rate rises by 1.7% over the interquartile range of OPA provinces from the pre to post period. Given a province-level average of 57,754 women who report being unemployed in the LFS, this means there are 982 more women who are looking for work or are discouraged workers after the policy

[14] I also examine the effects of the OPA ban on internal migration. Internal migration may either increase or decrease in response to the ban. It may increase if, for instance, households now need to seek domestic employment somewhere else in the Philippines as international opportunities are no longer available, or it may decrease if households are now more credit constrained and cannot afford internal migration. I use the 1990, 2000, and 2010 Census of Population to determine the rates of out-migration and in-migration for each province. Unfortunately, the pre-trends for high and low OPA share provinces are far from parallel and so it is impossible to discern what effect is due to the policy and what effect is due to differential trending. Thus, I omit the results from the paper.

change in high OPA provinces compared to low OPA provinces. The average province has 429 OPAs prior to the policy change, and these displaced workers likely account for part of the increase. The other 553 are likely migrants displaced from other occupations due to the migration multiplier or women who were not previously working that now must enter the labor force as a result of the lost OPA income.

Next, I examine a number of other domestic labor market outcomes in Table 9. First, I look at the response of child labor. The child labor rate of employment is defined as the number of children aged 10 to 14 working at least 1 hour per week out of the total population aged 10 to 14.[15] Moving from the pre to post period, provinces in the 75th percentile of baseline OPA migration have a 1.3% higher rate of child labor than those provinces in the 25th percentile. Given there were 10,430 children aged 10 to 14 working in a province in 2004, this means 135.6 more children are engaged in at least one hour of work per week in high OPA provinces compared to low OPA provinces. For each OPA who can no longer go abroad, 0.3 more children are now engaged in paid work. 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.

Column 2 shows that 6.3% 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. Column 3 shows that 3.6% fewer people are looking for work in high OPA provinces compared to low OPA provinces after the policy change. While at first glance this seems like a positive sign of the health of labor market, in reality, this may suggest that the number of discouraged workers has risen and fewer individuals are actually looking for work. Finally, individuals are more likely to be engaged in short-term employment rather than permanent employment. 1.8% more individuals are working in short term contracts in high OPA provinces compared to low OPA provinces after the policy change.

[15] On-time graduation is at age 15 or 16.

6 Robustness Checks

To further alleviate concern that the results are due to differential trending across high and low base share provinces, one might still be concerned that this drives the results, I create a new counterfactual group following Abadie and Gardeazabal (2003) and Abadie, Diamond and Hainmueller (2010). This synthetic control group is made up of the convex combination of the provinces that most closely resemble the highest OPA share provinces prior to the policy change.

The aforementioned papers consider the case of a binary treatment variable. However, in the case of the OPA policy change, provinces were not treated or untreated, but rather received different treatment dosages depending on their historic migrant networks. To conduct the analysis using the synthetic control group, I assign the 9 provinces with the highest OPA base share to the treatment group. This can be seen in Figure 7. These provinces all have a baseline OPA migration rate of 0.15% or greater, and I refer to them as OPA provinces. Also shown in Figure 7, 64 provinces with a base share less than 0.07% are designated as possible controls. I exclude the provinces that are not circled since their treatment dosage is somewhat similar to those designated as the treatment group. Following Abadie and Gardeazabal (2003), I then assign weights to minimize the pre-treatment difference in total migration between the OPA and synthetic OPA groups.

Table 10 shows summary statistics for OPA provinces, all potential control provinces, and synthetic OPA provinces. Compared to OPA provinces, the provinces in the potential control group are married at a higher rate, much less educated, more likely to be employed, less likely to live in urban areas, and have a much higher incidence of child labor. The working age population in these provinces is also much smaller, which is not surprising since the 4 districts of Metro Manila are included as OPA provinces. Turning to the synthetic OPA provinces, while the balance is not perfect, particularly with regards to the size of the working population, the balance is substantially improved in terms of education levels, employment rates, and child labor rates, and most covariates are balanced between the OPA provinces and the synthetic OPA provinces. While the synthetic OPA provinces are still less urban, the balance is much better than when compared to the overall potential control group.

The assigned weights are shown in Table 11. Four provinces make up the synthetic control

with the majority of the weight placed on the province of Bataan. The rest of the weight is split between Batangas, Cebu, and South Cotabato provinces. Figure 8 shows the results. While Bataan province has much of the weight in order to balance the covariates, the total migration rate from Bataan is higher than in the OPA provinces. Thus, in the pre-period, the trends appear similar, but the level of migration differs across the OPA provinces and the synthetic control. Following the OPA policy change in 2005, the gap between the migration rates grows substantially. Migration in OPA provinces increases at a much slower rate than in the synthetic control group. Specifically, the total migration rate in OPA provinces decreases by -1.17 percentage points compared to the synthetic control group. With a standard error of 0.24, this is statistically significant at the 1% level. I cannot reject that this point estimate is statistically the same as -1.07, the main result shown in Table 5. Thus, the synthetic cohort analysis corroborates the robustness of the main results and further assuages concerns that differential trending over time may drive the results.

7 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. 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. Because the policy change occurred in response to accusations of trafficking, the results also provide 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 1.2% greater decrease in total migration. The effects are larger for new hires, suggesting that new migrant contracts are more vulnerable to policy

shocks. I also find a substantial multiplier effect, with migration declining by more than the amount of the policy change. The multiplier effects are larger for new migrants, and current migrants appear to crowd out new migrants by renewing contracts at a higher rate. The spillover effects of this policy lead to reductions in both female and male migration. Domestic helpers, plumbers, laborers, and production workers all are hired at a lower rate than prior to the policy change in high OPA share provinces. Domestically, the unemployment rate for women rises by 1.7% more in high OPA share provinces after the policy change than in low OPA share provinces. Child labor increases differentially by 1.3%, and 1.8% more individuals are now engaged in short-term work. Robustness checks that define a treated group of provinces and construct a synthetic control group of provinces corroborate the main results.

The results suggest that immigration policies imposed by destination countries have substantial implications for migrant-sending countries. Not only are OPA opportunities reduced, but there are also large spillover effects that reduce other migration opportunities. The domestic labor market results suggest that when migration is reduced and remittances are no longer available, the domestic labor market choices are substantially different in order to cope with these changes. However, higher rates of unemployment and more people looking for additional work suggest that households are not able to fully compensate for lost migration opportunities and remittances through domestic employment. Thus, 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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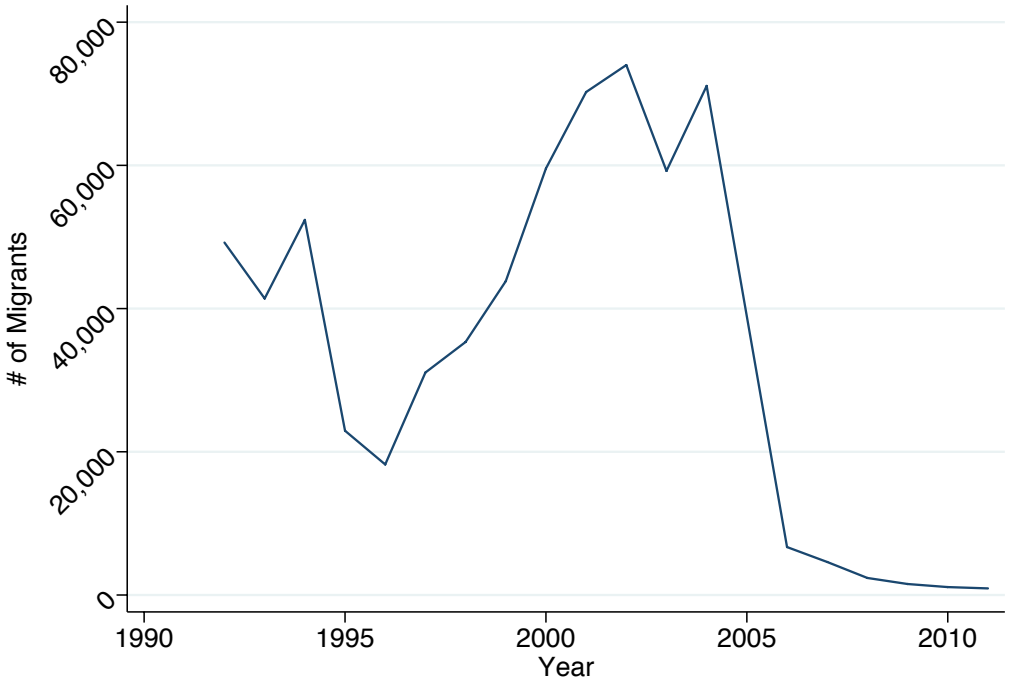
Figure 1: OPA Migration Over Time



Source: POEA.

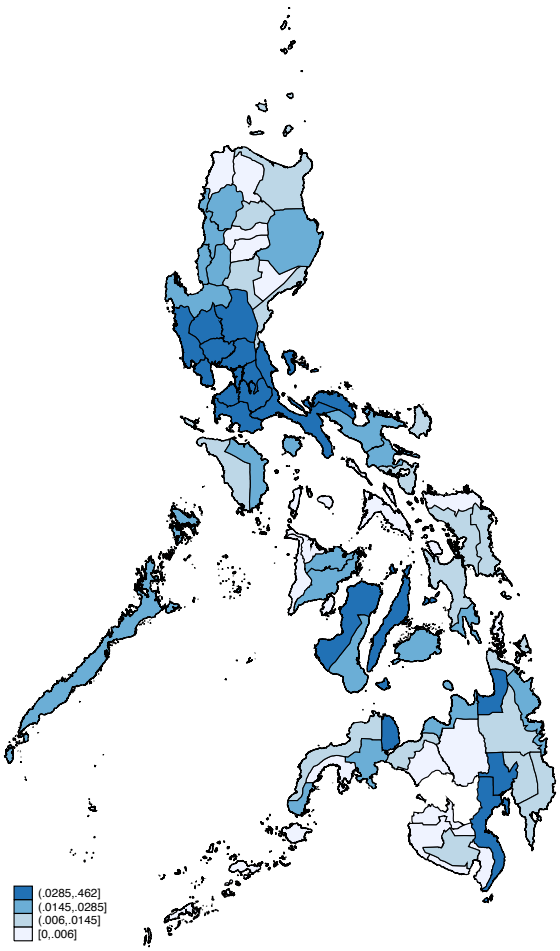


Figure 2: 1993 OPA Migration Rates by Province

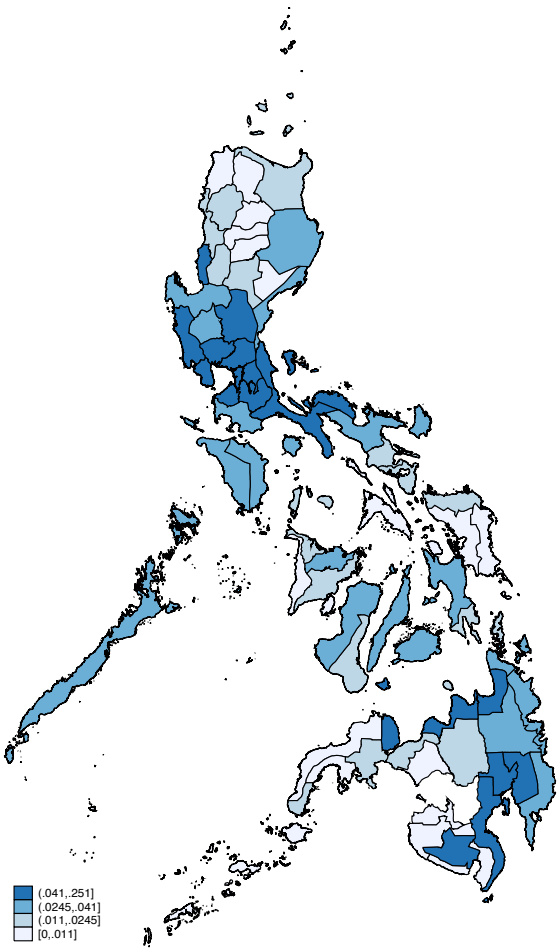


Figure 3: 2004 OPA Migration Rates by Province

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

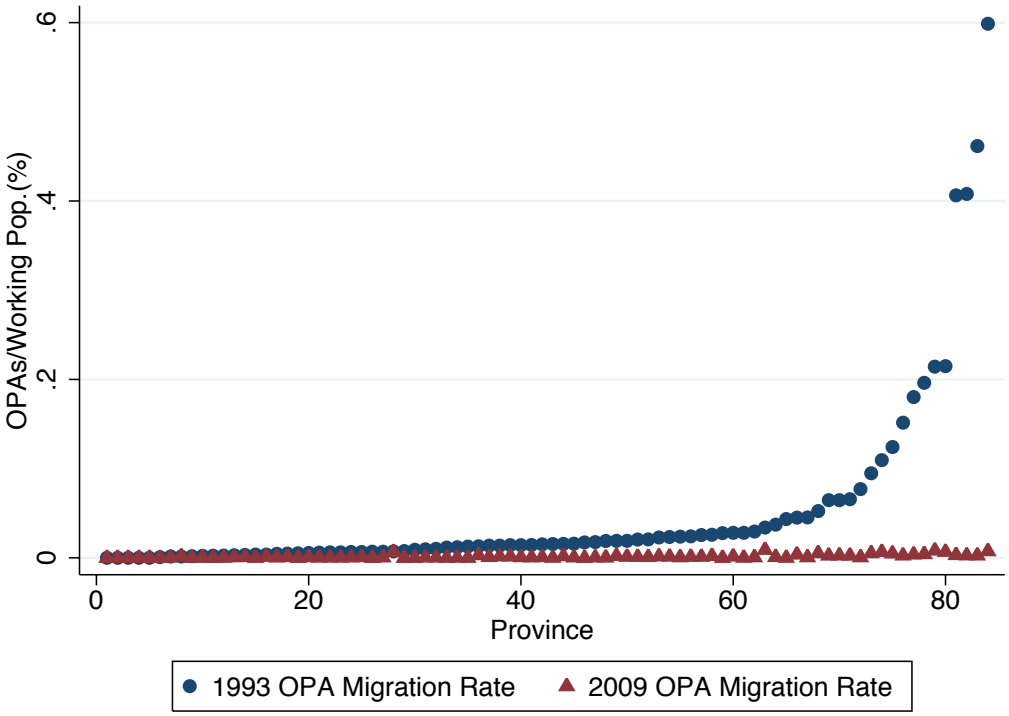
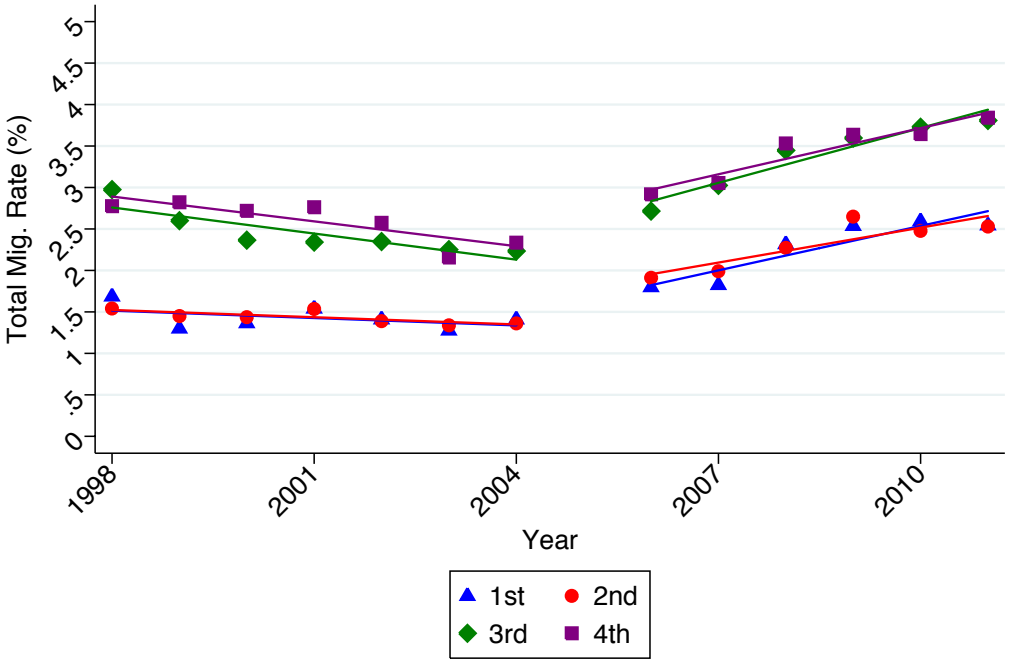


Figure 5: Parallel Trends across OPA Provinces by Base Share Quartile



Notes: The migration rate is the average province–level migration rate for each quartile.
Source: LFS.

Figure 6: Migrant Networks: 1993 and 2004 OPA Shares

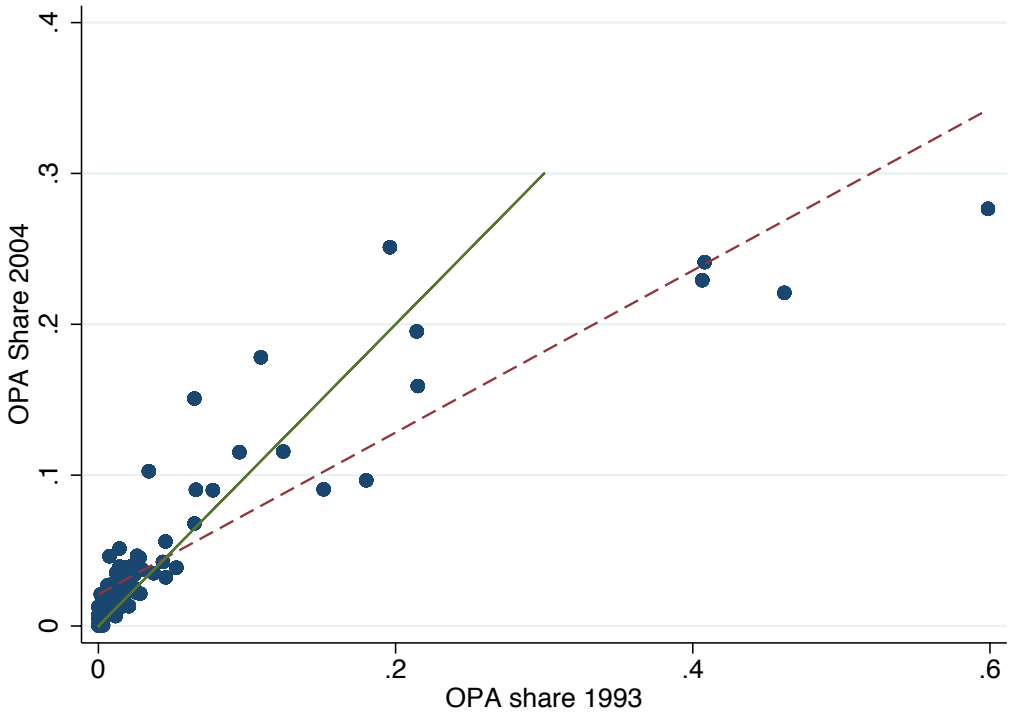


Figure 7: Assignment of "Treatment" and Potential Control Provinces

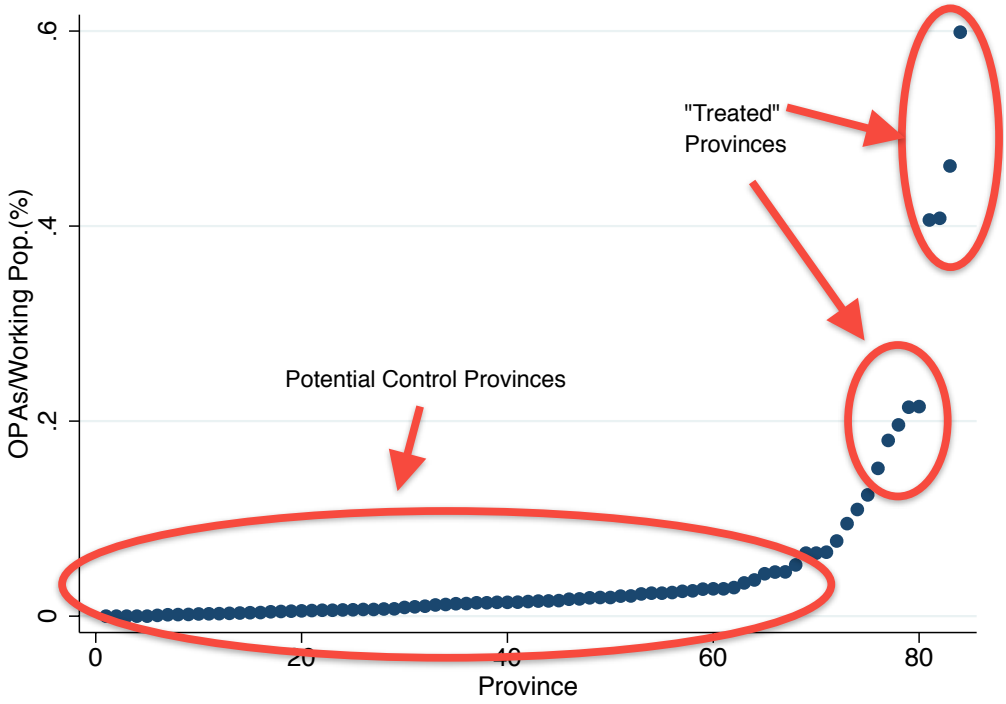


Figure 8: Synthetic Control Results

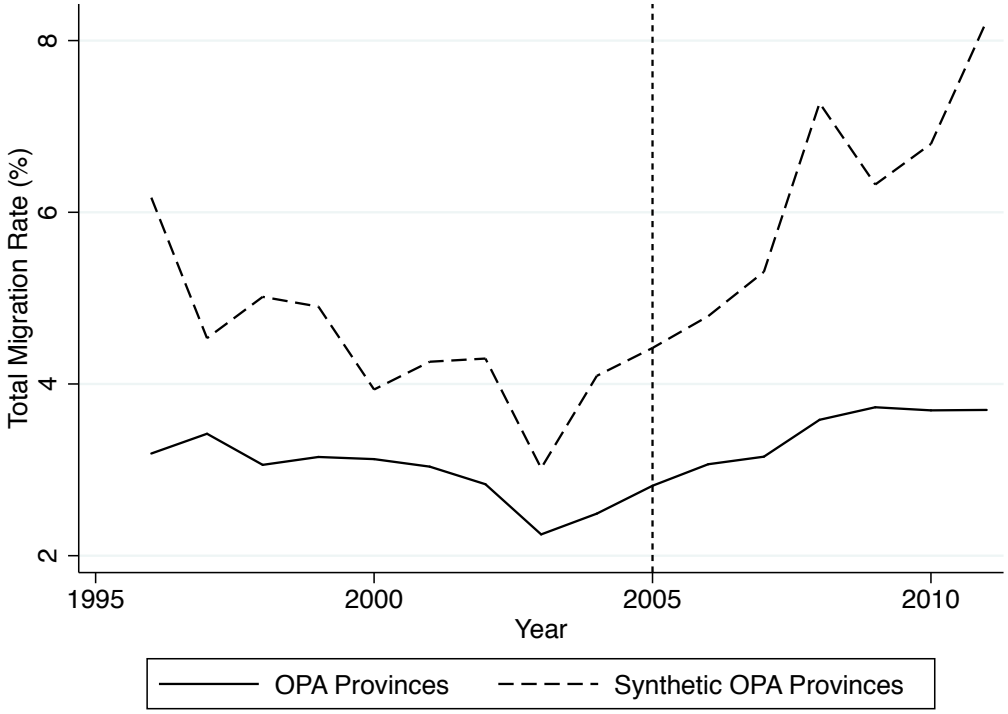


Table 1. Top 10 Occupations for Contract Migrants

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
Contract Duration (Months)	4.6	20.5
Years of Education	9.4	13.3

Source: POEA, SOF, and authors' calculations.

Table 3. Summary Statistics

	N	Mean	Std. Dev.	Min	Max
Migration Variables					
OPA Migration Rate (1993-Base share)	80	0.05	0.11	0.00	0.60
Total Migration Rate (LFS)	1040	2.44	1.61	0.00	8.84
Total Migration Rate (Census)	160	2.53	1.25	0.50	6.19
New Hires Migration Rate	480	0.44	0.28	0.01	1.59
OPA Migration Rate (2004)	80	0.05	0.07	0.00	0.28
OPA Migration Rate (2006)	80	0.01	0.01	0.00	0.03
Domestic Labor Market Variables					
Unemployment Rate	1040	8.58	3.79	0.00	20.02
Child Employment Rate	1040	9.75	10.18	0.00	77.30
Looking for Work	1040	1.52	1.42	0.00	7.13
Looking for Additional Work	1040	4.88	4.23	0.00	24.28
Short Term Job	1040	11.97	5.75	1.81	59.90
Internal Migration Variables					
Out Migration Rate	160	2.85	1.49	0.68	11.44
In Migration Rate	160	2.67	1.80	0.06	11.12
Net Migration Rate	160	-0.17	1.74	-6.60	8.69
Working Population	1027	505,450.8	448,410.9	7,523.7	2,417,697.0

Notes: Summary statistics are not population weighted.
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)			0.5340***		0.4382***		0.0056
			(0.0264)		(0.0519)		(0.0037)
Female	0.0027	0.0046	0.0021*	-0.0050	-0.0077*	0.0002	0.0002
	(0.0071)	(0.0056)	(0.0013)	(0.0091)	(0.0046)	(0.0003)	(0.0003)
Age	-0.0013	-0.0041	-0.0024**	-0.0193*	-0.0083	-0.0003	-0.0001
	(0.0090)	(0.0047)	(0.0011)	(0.0110)	(0.0081)	(0.0004)	(0.0004)
Married	0.0016	0.0037*	-0.0001	0.0040	0.0034*	0.0001	0.0000
	(0.0023)	(0.0020)	(0.0005)	(0.0039)	(0.0018)	(0.0001)	(0.0001)
Elementary Graduate	0.0010	-0.0005	-0.0004	-0.0004	0.0005	0.0002*	0.0002*
	(0.0015)	(0.0010)	(0.0003)	(0.0021)	(0.0013)	(0.0001)	(0.0001)
Some High School	-0.0078**	-0.0021	-0.0002	0.0047	0.0048*	0.0002*	0.0002**
	(0.0039)	(0.0027)	(0.0006)	(0.0045)	(0.0028)	(0.0001)	(0.0001)
High School Graduate	-0.0021	0.0002	0.0005**	0.0029	0.0021**	0.0001**	0.0001**
	(0.0024)	(0.0011)	(0.0002)	(0.0018)	(0.0008)	(0.0001)	(0.0000)
Some College	0.0071	0.0002	-0.0011***	-0.0066**	-0.0022	-0.0002*	-0.0002
	(0.0043)	(0.0026)	(0.0004)	(0.0030)	(0.0018)	(0.0001)	(0.0001)
College Graduate	0.0040	0.0073**	0.0006	0.0117**	0.0039	0.0002	0.0001
	(0.0039)	(0.0031)	(0.0006)	(0.0051)	(0.0029)	(0.0001)	(0.0001)
Employment Rate	0.0011	-0.0002	0.0010**	0.0026	0.0003	0.0001	0.0001
	(0.0020)	(0.0017)	(0.0005)	(0.0026)	(0.0013)	(0.0001)	(0.0001)
Urban	0.0036***	0.0015***	0.0000	0.0017***	0.0007**	0.0001**	0.0000
	(0.0009)	(0.0004)	(0.0001)	(0.0005)	(0.0003)	(0.0000)	(0.0000)
Child Unemployment Rate	-0.0004	0.0004	-0.0004**	-0.0024*	-0.0013	-0.0001**	-0.0001**
	(0.0012)	(0.0009)	(0.0002)	(0.0014)	(0.0008)	(0.0001)	(0.0001)
Unemployment Rate	-0.0035	-0.0067	0.0000	-0.0027	-0.0052*	-0.0000	-0.0001
	(0.0064)	(0.0046)	(0.0011)	(0.0047)	(0.0027)	(0.0002)	(0.0002)
Looking for Additional Work	0.0265	0.0193*	-0.0012	0.0020	0.0000	-0.0001	-0.0001**
	(0.0170)	(0.0102)	(0.0029)	(0.0022)	(0.0012)	(0.0001)	(0.0001)
Short Term Job	-0.0003	-0.0009	-0.0001	-0.0022*	0.0004	0.0001	0.0001
	(0.0012)	(0.0008)	(0.0002)	(0.0012)	(0.0007)	(0.0001)	(0.0000)
Obs	77	80	80	80	80	80	80
R2	0.865	0.860	0.994	0.740	0.888	0.539	0.579
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 and New Hire Migration Rates

	Total Migration Rate (LFS)	Total Migration Rate (Census)	New Hire Migration Rate
Main Results	(1)	(2)	(3)
Post*OPA Share	-1.073***	-1.376	-1.443***
	(0.380)	(1.126)	(0.179)
Scaled by IQR (%)	(-1.073*0.03)/2.44=-1.2%	-1.7%	-9.6%
Check for Pre-Trends			
OPA Share	0.001	0.163	0.270***
	(0.154)	(0.743)	(0.058)
Falsification Test			
"Post"*OPA Share	0.235		0.277***
	(0.413)		(0.059)
N	1040	160	477
R2	0.902	0.971	0.920
Mean Dep. Var (%)	2.44	2.53	0.45

Notes: The pre-period in Column 1 is from 1998 to 2004; in Column 2, it is 2000; in Column 3, it is 1996-1997. The post-period in Column 1 is 2006 to 2011; in Column 2, it is 2010; in Column 3 it is 2006-2009. Observations and R2 are reported for Row 1. For the falsification test in Column 1, the "pre" period is defined as 1998-1999. All regressions include province and year fixed effects, as well as 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. 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 6. Effect of OPA Ban on Occupation-Specific Migration Rates

	Post*ShareOPA		Post*ShareOPA
OPAs	**-0.404*** **	Manufacturing	-0.013***
	(0.014)		(0.002)
Agriculture	-0.004***	Material-Handling	-0.013**
	(0.001)		(0.006)
Engineers	-0.056***	Medical	-0.056***
	(0.013)		(0.012)
Machine-Tool Operators	-0.013***	Painters	-0.011***
	(0.003)		(0.002)
Cashiers	-0.001	Plumbers, Welders	-0.072***
	(0.002)		(0.019)
Carpenters	-0.043***	Processors	-0.003**
	(0.010)		(0.001)
Building Caretakers	-0.011	Production NEC	-0.120***
	(0.013)		(0.013)
Caregivers	-0.059***	Production Supervisors	-0.012***
	(0.016)		(0.004)
Clerical	-0.005***	Professional NEC	-0.006**
	(0.002)		(0.003)
Clerical NEC	-0.003	Protective Services	-0.003**
	(0.007)		(0.001)
Construction	-0.002*	Sales	-0.003*
	(0.001)		(0.002)
Cooks, Waiters	-0.013	Sales Workers NEC	0.000
	(0.014)		(0.001)
Domestic Helpers	-0.284***	Salesmen	-0.001
	(0.045)		(0.004)
Electrical	-0.052***	Scientists	-0.015***
	(0.009)		(0.002)
Food Processors	-0.000	Service NEC	-0.007
	(0.002)		(0.009)
Hairdressers	0.000	Spinners, Weavers	-0.007***
	(0.002)		(0.002)
Laborers	-0.093***	Typists	-0.004***
	(0.011)		(0.001)
Machine Fitters	-0.030***	Tailors	-0.017**
	(0.006)		(0.008)
Managers NEC	-0.003**	Transport Operators	-0.005***
	(0.001)		(0.002)
Obs	477		477

Notes: The pre period is from 1995 to 1997, and the post period is from 2006 to 2009. All regressions include province and year fixed effects, as well as a number of province-year level covariates listed in Table 5. Robust standard errors are clustered at the province level. The unit of observation is the province-year. *** indicates significance at the 1% level. ** indicates significance at the 5% level * indicates significance at the 10% level.
Source: POEA, OWWA, and LFS.

Table 7. Effect of OPA Ban on Migration Rates, By Gender

	Total Female Migration Rate	Total Male Migration Rate	Female New Hire Migration Rate	Male New Hire Migration Rate
	(1)	(2)	(3)	(4)
Post*OPA Share	-0.603**	-0.470**	-0.853***	-0.589***
	(0.268)	(0.194)	(0.099)	(0.096)
Scaled by IQR (%)	-1.4%	-1.25%	-9.8%	-8.9%
N	1040	1040	477	477
R2	0.872	0.919	0.879	0.946
Mean Dep. Var	1.32	1.12	0.26	0.19

Notes: The pre-period in Columns 1 and 2 is from 1998 to 2004; in Column 2, it is 1996 to 1997. The post-period in Columns 1 and 2 is 2006 to 2011; in Column 2, it is 2006 to 2009. Observations and R2 are reported for Row 1. For the falsification test in Column 1, the "pre" period is defined as 1998-1999. All regressions include province and year fixed effects, as well as a number of province-year covariates listed in Table 5. Robust standard errors clustered at the province level. All regressions weighted by 1990 working population.

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

Table 8. Effect on Domestic Unemployment Rate

	Total Unemployment	Female Unemployment	Male Unemployment
	(1)	(2)	(3)
Post*OPA Share	0.893	1.485***	-0.592
	(0.609)	(0.301)	(0.405)
Scaled by IQR (%)	0.4%	1.7%	-0.6%
N	1040	1040	1040
R2	0.900	0.783	0.911
Mean Dep. Var	6.19	2.59	3.59

Notes: The pre-period is from 1998 to 2004. All regressions include province and year fixed effects, as well as a number of province-year covariates listed in Table 5 but excluding all employment controls. Robust standard errors clustered at the province level. All regressions weighted by 1990 working population. The dependent variables is defined as the total number of unemployed or discouraged workers out of the total working-aged population (age 18-65).
Source: POEA, OWWA, LFS, Census of Population.

Table 9. Effect of OPA Ban on Domestic Labor Market Outcomes

	Child Labor (Aged 10-14)	Looking for Additional Work	Looking for Work	Short-Term Employment
	(1)	(2)	(3)	(4)
Post*OPA Share	4.384**	3.186***	-5.810***	7.145***
	(2.023)	(0.384)	(1.972)	(1.776)
Scaled by IQR (%)	1.40%	6.30%	-3.56%	1.80%
N	1040	1040	1040	1040
R2	0.868	0.872	0.696	0.760
Mean Dep. Var	9.75	1.52	4.88	11.97

Notes: The pre-period is from 1998 to 2004. All regressions include province and year fixed effects, as well as a number of province-year covariates listed in Table 5 but excluding all employment controls. Robust standard errors clustered at the province level. All regressions weighted by 1990 working population. The rate of child labor is the number of children aged 10 to 14 working at least one hour in the past week out ofthe total population aged 10 to 14. Those looking for additional work are employed, but seeking additional hours.
Source: POEA, OWWA, LFS, Census of Population.

Table 10. Summary Statistics for OPA, Synthetic OPA, and Control Provinces

	OPA Provinces	All Potential Control Provinces	Synthetic OPA Provinces
	(1)	(2)	(3)
Female (%)	51.49	49.36	49.83
Age (%)	35.30	36.92	35.53
Married (%)	60.73	66.97	63.68
Education			
Elementary graduate (%)	12.78	20.77	17.58
Some high school (%)	11.25	13.61	10.59
High school graduate (%)	29.96	18.70	29.20
Some college (%)	21.63	14.54	20.49
College graduate (%)	17.30	10.11	11.52
Employed (%)	59.93	68.33	59.18
Urban (%)	88.49	30.96	65.18
Working Population	1,272,158	368,864	404,376
Unemployed (%)	9.28	5.26	9.14
Child Labor (%)	2.31	12.70	3.05
Looking for Work (%)	2.02	0.72	1.56
Looking for Additional Work (%)	3.15	2.83	3.07
Short-Term Employment (%)	8.50	13.32	12.67
Number of Provinces	9	64	4

Notes: Summary statistics are calculated for the pre-period, 1996 to 2004.
Source: LFS and author's calculations.

Table 11. Weights for Synthetic Control Group

Province	Weight
Batangas	0.036
Bataan	0.881
Cebu	0.067
South Cotabato	0.017

Source: LFS and author's calculations.