Mattia Makovec, Ririn S. Purnamasari, Matteo Sandi and Astrid R. Savitri

Intended versus unintended consequences of migration restriction policies: evidence from a natural experiment in Indonesia

Article (Accepted version) (Refereed)

Original citation:
Makovec, Mattia and Purnamasari, Ririn S and Sandi, Matteo and Savitri, Astrid R. (2018) *Intended versus unintended consequences of migration restriction policies: evidence from a natural experiment in Indonesia.* Journal of Economic Geography, 18 (4). pp. 915-950. ISSN 1468-2702
DOI: 10.1093/jeg/lby029

Intended vs. Unintended Consequences of Migration Restriction Policies: Evidence from a Natural Experiment in Indonesia

Mattia Makovec
The World Bank
mmakovec@worldbank.org

Ririn S. Purnamasari
The World Bank
rpurnamasari@worldbank.org

Matteo Sandi
CEP, London School of Economics
m.sandi@lse.ac.uk

Astrid R. Savitri
The World Bank
asavitri@worldbank.org

This version: May 3rd, 2018

Abstract

This paper studies the consequences of restrictions to migration at the origin on labor market outcomes and school enrolment in origin communities. Our difference-in-differences specification exploits the differential impact across districts in Indonesia of a reform that restricted the migration of Indonesian female domestic workers towards Saudi Arabia in 2011. Our results suggest that this reform did not lead to higher unemployment in Indonesia, but it increased the proportion of workers employed in informal jobs and in agriculture. No detectable change in the consumption patterns of Indonesian households appears from our analysis, suggesting that rural areas in Indonesia could absorb the sudden increase in the availability of workforce. Our findings also show an increase in junior secondary school enrolment of both males and females, arguably reflecting the importance of the maternal presence in the household for the investment in human capital of children.

Acknowledgements and disclaimer: We are grateful to Vivi Alatas, Çağlar Özden, Paolo Masella, Alan Winters and three anonymous referees for valuable comments on a preliminary version of the results included in this paper. We would like to thank the Guest Editor, Hillel Rapoport, and the organizers of the 9th Migration and Development Conference held at the European University Institute in June 2016. In particular, we are grateful to Francesco Fasani, and Samuel Bazzi for their extremely helpful suggestions on the empirical strategy adopted in the paper. We also thank BNP2TKI (National Agency for the Placement and Protection of Indonesian Migrant Workers) for providing administrative data on documented Indonesian international migrants. Finally, we are indebted towards Hamidah Alatas, Talitha Chairunissa and Daim Syukriyah for their superb research assistance. All errors and mistakes remain our own responsibility. The views and opinions expressed in this paper do not necessarily reflect the position of the World Bank and cannot be attributed to the Government of Indonesia.

1. Introduction and Motivation

The number of international migrants has increased by 41 percent worldwide over the past 15 years, reaching 244 million in 2015, compared with 173 million in 2000. This increase represents a net expansion in the proportion of migrant population, given that over the same period the world population grew at 19 percent (UN International Migration Report, 2015). Migration policies, however, still seem to remain in the domain of national governments in receiving countries, and bilateral agreements between sending and destination countries still represent the exception. This is not surprising, given that for decades migration policies have experienced less cross-country coordination and liberalization compared to trade policies (Hatton, 2007).

Although for decades international migration has remained off the international development agenda (Pritchett, 2006), currently three of the seventeen new Sustainable Development Goals include migration related targets. At least in part, these reflect the recognition that the failure to design appropriate immigration and integration policies in both sending and receiving countries could significantly hinder the large potential benefits of international migration for the world economy (see, e.g., Walmsley and Winters, 2005). The international cooperation in the design of migration policies appears even more relevant in the case of many low- and middle-income countries, that display high emigration rates but also issues of weak implementation capacity, corruption, difficult coordination among government agencies, and poorly regulated labor markets.

This paper studies the effect of a restrictive emigration policy at the origin on labor market outcomes and school enrolment decisions in origin communities. Our empirical analysis presents evidence from Indonesia, which constitutes one of the major origin countries of international migrants. We assess the consequences of a reform in Indonesia that restricted

the international mobility of female domestic workers towards Saudi Arabia. Since Saudi Arabia traditionally represented the primary destination country of female Indonesian migrants, this reform directly affected hundreds of thousands of women. This migration restriction for female domestic workers (henceforth, *moratorium*) was introduced in response to increasingly frequent cases of abuses and harassment suffered by Indonesian domestic workers in Saudi Arabia and other Middle-Eastern countries during the mid- and late 2000s. Since hundreds of thousands of women used to migrate to the Middle East from Indonesia every year, this reform resulted in a unique large-scale natural experiment. Also, the reform is quite unique and unusual in the international landscape, where normally migration restrictions are imposed by destination countries to regulate migration inflows, and resembles somehow the restrictions to emigrate towards Western Europe or the US in place in former Soviet Union Republics until the late 1980s.

The spatial variation in the destination countries of international migrants across Indonesian districts allows us to define a difference-in-differences specification, whereby the outcomes of interest in origin districts of migrants towards Saudi Arabia are compared with those in control districts, i.e., all other districts in Indonesia. Thus, control districts include origin regions of migrants towards all other international destinations. We exploit this spatial heterogeneity in the intensity of the 'intention-to-treat' (ITT) exposure to the 2011 reform to study the labor market and school enrolment effects of (e)migration restrictions in origin communities. Our results are robust to the use of alternative definitions of treatment. Further, we validate our results by running a set of falsification exercises, showing that we cannot find any significant effects on the outcomes of interest in districts sending migrants to destinations not affected by the Saudi Arabia moratorium.

Our results suggest that the migration moratorium to Saudi Arabia did not result in increased unemployment in affected areas. This conclusion applies to both men and women. However, our analysis also suggests that the migration moratorium induced considerable churning in origin local labor markets, as we find detectable increases in employment in the informal sector and in agriculture in the treatment group districts. Our event-study estimates show there being no differential pre-treatment trends in these labor market outcomes between treatment and control districts. This allows us to interpret the observed patterns as the result of the migration moratorium in 2011. For both men and women, the 2011 migration moratorium led to an increase in the fraction of workers employed in the informal sector and in the fraction of workers in agriculture. In the subsequent part of our empirical analysis, we also investigate the effect of the 2011 moratorium on the consumption patterns of Indonesian households. This analysis reveals no detectable discrepancy between the consumption patterns of treatment and control group households. We interpret these results as suggestive that the informal and the agricultural sectors were able to absorb the sudden increased availability of labor in Indonesia. The final part of our analysis documents a significant increase in enrolment in junior secondary school of both males and females in treatment group regions. Since we find a similar effect for both males and females, we interpret this as reflective of the importance of the maternal presence in the household for the investment in education of children. These results contribute to the growing economics literature on the effects of migration policies, as they provide novel evidence on the economic consequences of restrictions to migration at the origin.

The paper is organized as follows. Section 2 discusses the contribution of this study to the existing literature. Section 3 illustrates the Indonesian policy context and the introduction of the 2011 migration moratorium. Section 4 presents our empirical strategy, and Section 5 presents our results. Section 6 concludes.

2. Relevant Literature

Although the population of international migrants increased markedly over the past 15 years, actual international migration flows are still relatively small in size compared with the total world population: the foreign-born population constituted only 3.38 percent of the world population in 2015 (UN International Migration Report, 2015), and 10 percent of the population in OECD countries in 2010 (Ortega and Peri, 2015). Restrictive immigration policies in receiving countries are often indicated as the main reason for low international mobility (see, e.g., Pritchett 2006). Since restrictions to migration are generally imposed by recipient countries, it is not surprising that the majority of studies has focused on restrictions to (im)migration at destination. Focusing on immigration restrictions, Ortega and Peri (2012) document that, on average, migration restrictions decrease immigration by 6 percent among rich countries. Theoharides (2016) documents the effects of the restriction pursued by the Japanese government in 2005 to the immigration of Overseas Performing Artists (OPAs) from the Philippines. She finds that the reform reduced migration flows over and above its intended purpose for the restricted entertainers, as negative spillovers extended to other types of potential migrants in the Philippines. She also finds that this restriction to migration resulted in increased labor force participation rates, lower levels of income, and greater incidence of child labor in mostly affected communities in the Philippines. Some recent studies have also attempted to measure the unrealized economic gains due to excessively restrictive immigration policies, generally concluding that these gains may be very large. Klein and Ventura (2009), Clemens (2011), and Di Giovanni et al. (2015), are some of the studies that document large potential gains from liberalizing international migration. Facchini and Mayda (2009), Boeri (2010), and Facchini et al. (2011), complement this literature by providing evidence on the

economic and political forces that determine the formation of the immigration policies of the host countries.

This study contributes to a number of strands of the migration literature. First, we contribute to the recent literature that examines the consequences of migration policies by focusing on an unusual restriction to emigration imposed at the origin by a major net exporter of international migrants. Due to the rarity of this reform, the migration moratorium implemented in Indonesia since 2011 provides the rare opportunity to assess the consequences of a large-scale restrictive migration policy at the origin. In this respect, our work differs from Theoharides (2016) and is more closely aligned with Dinkelman and Mariotti (2016), who also analyse the consequences of restrictions to migration at the origin. In particular, these authors exploit two exogenous policy shocks which generated first an expansion, and then a sudden and permanent drop in the flows of Malawian workers recruited by mining companies in South Africa: the removal of an existing quota on Malawian workers in South Africa in 1967, and a permanent labor ban dictated by the Malawian President, after a plane crash killed 70 returning migrants in 1974. Their analysis shows the long-run positive consequences of emigration on human capital accumulation in sending communities.

This study also contributes to the literature on the effects of emigration on the origin labor market, by focusing on a context where migration is predominantly a female phenomenon. Until recently, very few papers have looked at the effects on local labor markets of emigration in the countries of origin (Hatton, 2014), and this literature has mostly focused on the effect of emigration on non-migrants' wages. The general conclusion that emerges from this literature is that emigration is likely to exercise upward pressure on wages due to the reduced supply of labor in the origin market. Nonetheless, in most of the contexts examined in the literature, migrants are often *males*. Taylor and Dyer (2009), in their simulation from rural

Mexico, conclude that emigration impacts positively on wages in origin communities where transactions are frequent among households. Mishra (2007) finds that emigration from Mexico in the 1990s caused the relative wage of high-school graduates to increase by 4 percent, and the wages of those who completed college by 3 percent. Evidence from Mexico of a positive effect of emigration on local wages is also presented by Aydemir and Borjas (2007) and by Hanson (2007b). Borjas (2008) finds similar effects for Puerto Rico, and Bouton et al. (2009) for Moldova. Other studies exploring the effect of emigration on wages of non-migrating nationals are Dustmann et al. (2012), and Elsner (2013a, 2013b). Airola (2008), Amuedo-Dorantes and Pozo (2006), and Hanson (2007a and 2007b), present evidence that labor supply decreases in Mexico as a result of migration. Acosta (2006 and 2007) finds similar conclusions from El Salvador, while Damon (2009) shows additional evidence from rural El Salvador that the effect of foregone labor induced by emigration may actually increase on-farm labor hours for all family members and substantially reduce hours of off-farm labor for males.

We contribute to this literature by assessing the consequences of the 2011 migration moratorium for female domestic workers traveling to Saudi Arabia on origin labor markets in Indonesia. International migration is a large scale phenomenon in Indonesia which involves hundreds of thousands of women every year, it is predominantly a female phenomenon, and until 2011 Saudi Arabia constituted the primary destination country for female migrants from Indonesia. Thus, the 2011 migration moratorium to Saudi Arabia provides the rare opportunity to study the effect of female migration on origin labor markets. Given that important gendered patterns may emerge in the labor market effects of emigration, we view this as an important element of contribution of our work.

Finally, we also contribute to the literature that studies the effect of migration on the so-called ‘care-drain’ and family disintegration. Parental absence is often found to generate

significant effects on health, education, social relations and family cohesion in the origin household (for a review of the literature, see Antman, 2013). Ehrenreich and Hochschild (2003), D'Cunha (2005), Oishi (2005), and Fudge (2010), are some of the studies that describe the socioeconomic and political factors pushing women from developing countries to emigrate and work as carers for children or the elderly in richer countries. In the case of Indonesia, Nguyen and Purnamasari (2011) find that female migration does not lower labor-force participation of remaining household members as they do in the case of male migration. Moreover, they find that international migration reduces child labor supply in households with female migrants. Using different sources of microdata on local labor markets in Indonesia, as well as a novel identification strategy, we present new evidence on the effects of female migration on labor market outcomes, per capita consumption, and the school enrolment of children in origin communities in Indonesia.

3. Policy Context: Restrictions to International (E)migration in Indonesia

Before the introduction of recent migration restrictions, Indonesian migration was largely driven by female migration. Figure 1 uses administrative data on annual documented flows to show that, in 2006, female migrants accounted for 80% of total documented migrants (BNP2TKI). In the same year, female migrants to Saudi Arabia and Malaysia, the two top destinations of Indonesian migrant workers, represented 55% of total (documented) flows, and 70% of total female migrants. These figures reflect the result of a rapid "feminization" of migration flows in Indonesia during the previous decade, most likely spurred by an increasing demand for domestic workers from the Middle East and from neighboring countries. In less

than ten years, the female share on total documented migrants rose from 56% in 1996, to 68% in 2000, reaching 78% in 2004 (IOM, 2010).

The statistics from BNP2TKI, unfortunately, do not permit a more detailed breakdown of documented flows by migrants' characteristics. However, data from the recent World Bank Survey on Indonesia International Migration and Remittances (WB-IMR Survey, World Bank, 2014a) show that over 72 percent of Indonesian migrants come from rural areas of the country. Further, the same source confirms that Indonesian women overseas are predominantly employed as domestic workers (representing over 80% percent of total female migrants), as opposed to men, mostly employed in agriculture and construction (around 70% percent of total male migrants). The WB-IMR Survey also shows that female migrants are mostly low-skilled, with 50% of them having only primary education, and 30% of them having junior secondary education.

Emigration in Indonesia is a complex and lengthy process, on paper highly regulated, involving a number of intermediaries and several administrative steps (in Appendix A, we identify at least 22 procedures required before departure). In order to be able to work overseas, Indonesian workers need to apply for a KTKLN card, a special ID card for migrant workers obtainable only with a job offer from a foreign employer. Migrants can apply at local Manpower Offices (Disnaker), or at BNP3TKI offices (local branches of BNP2TKI). However, in rural areas, where these facilities are not always available, prospective migrants are more likely to apply through local agencies or informal sponsors. Migrants frequently lack complete information about the job offered and their prospective employer, since the intermediation is usually carried out between migration agencies in Indonesia and their counterparts at destination; at the same time, the heterogeneity in the quality of agencies is high (IOM, 2010). Further, even in the presence of a written job offer, substantial contractual rights might be

excluded or under-enforced. Despite the complex *de jure* regulation in place on paper, *de facto* migrants are therefore exposed to risks at all stages of the migration process. Further, claims of abuse and exploitation appear common, ranging from unpaid wages to inadequate rest and physical abuse (Farbenblum et. al., 2013, and IOM, 2010).

In an attempt to respond to these challenges, and following a series of tragic cases involving tortures, murders and death sentences experienced in Saudi Arabia by Indonesian female domestic workers, the Indonesian government imposed a moratorium on migration of female domestic workers from Indonesia to Saudi Arabia in June 2011. As Saudi Arabia was not the only placement country where international female migrants reported similar experiences, placement countries on which recent moratoria to migration by the Indonesian government also applied include Malaysia from June 2009 (although this moratorium was lifted in 2011), Kuwait from September 2009, Jordan from July 2010, Syria from August 2011, the United Arab Emirates from October 2013, and Qatar from November 2013. In 2015, the migration ban was extended to 21 countries across the Middle East, North- and East-Africa, and Pakistan (Ministry of Manpower, and BNP2TKI).

Although the large number of restrictions to emigration recently imposed by the Indonesian government reflects the importance of ensuring the security of migrants overseas in the political discussion in Indonesia, this paper focuses on the migration moratorium to Saudi Arabia in 2011. This was the largest ban to migration among those recently implemented in Indonesia, as Saudi Arabia, until 2011, constituted the placement country for the largest number of female migrant workers from Indonesia. The announcement of the emigration ban to Saudi Arabia in June 2011 was triggered by the execution of Ruyati Binti Sapubi, an Indonesian maid who allegedly killed her employer's wife after suffering repeated abuses. After the announcement of the ban, the Saudi government in turn announced the suspension of

work permits to Indonesian domestic workers (The Economist, 2011). It is therefore likely that this moratorium, which was enforced in Indonesia since August 2011, had been binding for the intended target group. The circumstances were similar to those triggering the 2009 emigration ban to Malaysia. However, in December 2011, the moratorium to Malaysia was lifted, after the two governments signed a Protocol aimed at improving the living and working conditions of migrant workers (Hickey et al., 2013, and ILO, 2016).

Coordination problems and duplications of functions between the Ministry of Manpower and BNP2KI might have hindered the full enforcement of migration restrictions in a way which remains difficult to quantify empirically (see Farbenblum et. al., 2013). Furthermore, the extent to which *undocumented* migration flows might have been affected by the moratoria appears unclear *a priori*. Undocumented migration, in fact, is an important phenomenon in Indonesia that is difficult to quantify. Undocumented flows are not included in BNP2TKI administrative data, and in areas of low compliance with government regulations, undocumented flows may have increased as a result of the 2011 reform. For instance, the Strait of Malacca between Sumatra and the Malay Peninsula is known to be a channel through which undocumented Indonesian migrants, typically facilitated by a local *taikong*[1], reach Malaysia to work informally without contract or protection (see Human Rights Watch, 2004, Kaur, 2004, Wong, 2005, and IOM, 2010). These type of flows might be less likely to be discouraged by the restrictions introduced. A recent report by the World Bank (World Bank, 2016), argues that in fact in some cases undocumented migration might have even increased, as a response to the moratoria. On the other hand, in areas characterized by better compliance with migration regulations (e.g. because of a larger presence of formal recruiting channels), undocumented flows might have been further discouraged. Given the challenges associated to the

[1] Term used in Indonesia to identify a smuggler, or an informal/illegal sponsor (Kaur, 2004, and Wong, 2005).

measurement of undocumented migration, our preferred reduced form estimates of the first-order effect of the 2011 moratorium on migration flows are based on documented flows. The next section discusses in detail how this information is relevant to our empirical strategy.

4. Identification Strategy and First Order Effects of Migration Restrictions

Figures 2, 3, 4 and 5 provide descriptive evidence of the effect of the moratoria to Saudi Arabia and other countries on documented migration flows. Figure 2 shows that between 2010 and 2011, when the moratorium to Saudi Arabia was enacted, the yearly flows of female migrants dropped from 203.625 to 110.641 individuals, and by 2012 this number plunged further to only 18.356 individuals. In the case of Malaysia, given the relative importance of male migration to this destination, the drop in the yearly flows of female documented migrants appears less substantial in absolute terms. Weaker enforcement capacity in regions predominantly sending migrants to Malaysia might explain why, after the introduction of the moratorium, the flows of documented female migrants did not fall more dramatically. Furthermore, the removal of the moratorium after 2011 might explain the slight recovery of female migration to Malaysia. Figure 3 shows that, given the overall importance of female migration in Indonesia, also the trends in total documented migration flows to Saudi Arabia resulted heavily affected by the 2011 moratorium.

Figures 2 and 3 also suggest that alternative migration destinations did not act as strong substitutes for female domestic workers, a finding that appears consistent with the lack of fungibility in migration opportunities documented in Theoharides (2016). After 2011, the foregone migrants to Saudi Arabia were seemingly unable to switch to alternative overseas destinations in the short run. In part this is plausibly explained by the fact that other Middle East countries were facing similar challenges of abuse and harassment, and were themselves

subject, with different timings, to migration restrictions in recent years. Migration flows towards alternative destinations (such as Singapore, Taiwan and Hong Kong) gradually increased over time, but only partially compensated for the dramatic drop in flows to Saudi Arabia and Malaysia. Also, due to the more demanding educational requirements in place to migrate to Singapore, Taiwan and Hong Kong, these alternative destinations may not have been viable options for domestic workers affected by the 2011 moratorium. As shown in Figure 4, also male migration did relatively little to substitute for the drop in female migrants in recent years.

Since it is not possible to identify exactly in the administrative data the number of domestic workers by gender and destination country, in Figure 5 we use information on placements by job type at destination. According to the classification of BNP2TKI, "informal jobs" at destination would correspond mostly to those of maids and domestic workers. In line with the previous charts, Figure 5 shows that the share of informal placements halved following the 2011 moratorium, dropping from over 83% in 2009 to 42% in 2014. In sum, this evidence suggests that the migration restriction implied by the 2011 moratorium was indeed binding for most of the prospective female migrants to Saudi Arabia in the short run.

In the first part of our empirical analysis, we quantify the first-order, intended effects of the 2011 moratorium on international migration flows from Indonesia. To this end, we use data from the *Podes* village census, which includes information about village geographic characteristics, infrastructure, political participation, main sources of economic activity, and number of village residents working abroad as documented migrants (TKI) during the survey year. Moreover, in 2005, *Podes* also collected information on the main destination country for people emigrating from each village. Further, in the same year, the gender breakdown of the total number of migrants was also collected. The information is reported by the Head of the

village, and is based on administrative records of international migrants. The fact that the National Statistics Office (BPS) fully validates the statistical information included in each wave of the village census further mitigates concerns on the reliability of the statistics on documented migrants in *Podes*. In fact, *Podes* data are known to match well the aggregate number of documented migrants reported by BNP2TKI, as well as those obtained from national household surveys (Bazzi, 2012).

In our empirical analysis we defined a treatment group and a control group of regions by exploiting heterogeneity across districts in the pre-existing female migration corridors to Saudi Arabia. With "district", here we mean kabupaten/kota, known in Indonesia as "regencies"/"cities", the main administrative subdivision of provinces. These were at the core of the process of decentralization and democratization of Indonesia that followed the fall of Soeharto in May 1998. Starting from 1999, kabupaten/kota were granted considerable authority and independence over the elections of local governments and the administration of local finances. From the administrative standpoint, they include various kecamatan ("sub-districts"), which in turn are sub-divided in villages (kelurahan/desa). In modern Indonesia, districts constitute a relevant spatial unit from an economic as well as political point of view.

Although, in principle, every region was affected by the 2011 moratorium to migration, the intensity of the intention-to-treat (ITT) implied by the reform may have varied across regions due to the importance of migration networks overseas (Munshi, 2003; Beaman, 2012; and Theoharides, 2016). Since information on migration by destination country and by gender prior to the 2011 reform is only available from *Podes 2005*, we used this dataset to identify the villages that, prior to the 2011 moratorium, used to send female migrants mostly to Saudi Arabia, as well as those that used to send female migrants to other destinations and those that were not origin villages for female migrants. Since all other socioeconomic outcomes of

interest in this paper are collected in separate surveys, namely the National Labor Force Survey (Sakernas) and the National Household Consumption Survey (Susenas) of Indonesia, and the most detailed level of spatial disaggregation common to all datasets used was the district, we aggregated up the information collected from *Podes* 2005 at the district level. To be precise, districts where the majority of female migrants in 2005 travelled to Saudi Arabia were grouped in our treatment group, whereas all remaining districts formed our control group[2]. In our econometric analysis, we exploit this spatial variation to retrieve the ITT causal impact of the 2011 moratorium on labor market outcomes and school enrolment in origin communities. Since the Sakernas and the Susenas datasets provide us with an array of information on the socio-economic circumstances of individuals, they are collected every year on the entire national territory and they are representative at the district level (as well as nationally), we can use the variation in space and time in our data to estimate a difference-in-differences specification and retrieve our causal parameters of interest.

Table 1 compares average characteristics of villages in treatment and control districts in 2005. Villages in treatment districts appear to be more populated, they display a greater fraction of Muslim population and a higher number of mosques and primary schools on average. They are also less likely to be coastal villages, and they rely more frequently on industrial production as their main source of income. In contrast, treatment and control regions appear similarly distributed between urban and rural areas, they rely similarly on agriculture and mining, they have similar numbers of secondary schools and their village heads feature comparable characteristics.

Figure 6 shows that female migrants on average constituted roughly 0.4 percent of population in treatment group districts. In 95 percent of treatment group districts, female

[2] In our analysis we test the robustness of our results to alternative plausible definitions of treated and untreated districts.

migrants did not exceed 1.5 percent of population, and they never exceeded roughly 2 percent of population. This suggests that our results are unlikely to be driven by a few villages with unusually high rates of outmigration, and they are rather likely to be uniformly distributed across observations in our treatment group. Figure 7 shows the distribution of treatment districts across provinces in Indonesia. Treatment districts do not appear concentrated in any particular part of the archipelago, but rather they appear uniformly distributed across islands. No treated districts are found in 16 provinces[3]. In four provinces, namely East Java, Riau, South Sulawesi and West Sumatera, between 4 and 8 percent of individuals are in treated districts. In seven provinces, between 8 and 33 percent of individuals are in treated districts: these are, namely, Central Java, Gorontalo, Jambi, Maluku, West Kalimantan, West Nusa Tenggara and West Sulawesi. Finally, more than 33 percent of individuals appear in treated districts in the remaining six provinces of Indonesia, namely Banten, Central Kalimantan, Central Sulawesi, DKI Jakarta, South Kalimantan, and West Java. The fact that observations in treatment and control districts are distributed uniformly across regions in Indonesia, in turn, mitigates concerns that our reduced form estimates may be contaminated by unobserved, region-specific and time-varying shocks, e.g., such as the slowdown in the commodity boom in the early 2010s, which may have affected our labor market outcomes of interest differently across regions.

In the regression analysis, we estimate a series of difference-in-differences specifications at the individual level to assess the effect of the 2011 moratorium on our socio-economic outcomes of interest. We use the yearly Sakernas of Indonesia from 2005-2014 to measure whether individual *i* in year *t* was unemployed, whether s/he was employed in the informal sector, and whether s/he was employed in agriculture. Using the yearly Susenas of Indonesia from 2004-2014, we measure the level of real per capita consumption of Indonesian

[3] These are, namely, Aceh Nanggroe Darussalam, Bali, Bangka Belitung, Bengkulu, DI Yogyakarta, East Kalimantan, East Nusa Tenggara, Kep Riau, Lampung, Maluku Utara, North Sulawesi, North Sumatera, Papua, Papua Barat, South Sumatera, Southeast Sulawesi.

households and the school enrolment status of the children in the household. For both datasets, we used all the survey years for which the data was available. In its functional form, our basic difference-in-differences specification can be expressed as follows:

$$Y_{it} = \beta' D_{SaudiDistrict_i} + \gamma' D_{year_t} + \delta' \left(D_{SaudiDistrict_i} * D_{year_t} \right) + X_{it}'\theta + \mu_i + \varepsilon_{it}, \qquad (1)$$

where Y_{it} represents individual *i*'s outcomes of interest in year *t* (i.e., unemployment status, employment in the informal sector, employment in agriculture, log real per capita consumption and school enrolment of children). $D_{SaudiDistrict_i}$ is a binary indicator that takes up value one if individual *i* resides in a treatment district, and it takes up value 0 otherwise. D_{year_t} is a vector of year fixed effects, $X_{it}^{'}$ is a parsimonious vector of covariates to control for individual *i*'s gender, a quadratic of age and whether individual *i* resides in a rural or urban area. μ_i is a vector of district fixed effects. The coefficient δ' on the interaction term $\left(D_{SaudiDistrict_i} * D_{year_t} \right)$ represents therefore the difference-in-differences estimate of the effect of the 2011 migration moratorium to Saudi Arabia on the dependent variable of interest.

5. Results

a. Effects of the moratorium on migration outflows

The first question we address in our empirical analysis is whether the 2011 migration moratorium to Saudi Arabia achieved its intended objective of eliminating migration of female informal workers from Indonesia towards Saudi Arabia. Unfortunately information at this level of detail is not available for our analysis. This is because the Podes census of villages, which is the only source of information on migration outflows on the entire national territory, did not record information in every village and every census on the gender and destination of migrants. However, as information was collected on the aggregate number of migrants from each village

and in each census, using this data we were able to estimate the effect that the 2011 moratorium to migration to Saudi Arabia had on overall migration flows from villages in treatment and control districts respectively. To this end, we used data from the Podes census of villages from 2005 to 2014 and we estimated the difference-in-differences specification in (1) to quantify the impact of the migration moratorium in 2011 on aggregate village-level migration outflows. Importantly, since the Podes census is conducted every three years, and in 2011 it was conducted in April, i.e., prior to the announcement of the migration moratorium in June 2011, the Podes census collected in 2014 provides our only post-period data available, whereas data collected in 2005, 2008 and 2011 provide us with information prior to the migration moratorium.

Table 2 shows the results of the estimation of the first order effects of the moratoria to Saudi Arabia in 2011 on documented migration flows by 2014. The estimated difference-in-differences coefficient provides evidence of a strong first-order negative effect of the 2011 reform. While aggregate migration flows increased in Indonesia by 2014, villages in treatment districts experienced a significant decline in the number of out-migrants. This conclusion is robust to the inclusion in the estimated equation of time-varying characteristics at the village level and district fixed effects. As columns [2], [3], [5] and [6] show, weighting our estimates by population at baseline (i.e., in 2005), did not alter this conclusion. Overall, the results in Table 2 suggest that aggregate migration flows decreased by roughly 20 percent in treatment villages as a result of the 2011 migration moratorium. As pointed out in Theoharides (2016), whether this is likely to be a lower-bound or an upper-bound estimate of the effect of the reform on the migration flows of informal female workers it intended to affect depends on the nature of the spillover effects of this foregone migration on the migration of others. If the migration of informal female workers was complementary to the migration of others, the results in Table 2 would overestimate the direct effect of the 2011 reform on informal female workers.

If, on the other hand, migration of informal female workers was a substitute of the migration of others, the direct effect of the 2011 reform on informal female workers would be greater than what is suggested by the results in Table 2. Unfortunately, due to data limitations, we are unable to test this in our analysis. However, these results confirm the initial graphic impression that migration restrictions reached their intended outcomes of reducing migration flows in exposed villages. This result is important also because it shows that different geographic areas were exposed differently to the moratoria. This, in turn, justifies our use of a difference-in-differences reduced-form specification in the rest of paper.

b. Effects of the moratorium on labor market outcomes

Having documented the spatially-heterogeneous negative impact of the 2011 migration moratorium on the international migration outflows of Indonesians, in this section we present the results from the estimation of equation (1) on our 'unintended' labor market outcomes of interest. The sudden impediment for female domestic workers to migrate overseas may have not only prevented the migration overseas of these workers, but it may have also had unintended repercussions on the labor markets and household living arrangements in their sending communities. In the remainder of this paper, we investigate these unintended outcomes of the 2011 migration restriction.

For the analysis of the impact of the 2011 reform on labor market outcomes, we combined the information on migration flows from Podes with yearly data from the 2005-2014 Sakernas survey years. The results of this effort are shown in Table 3. For all outcome variables, to test for the existence of differential pre-treatment trends between treatment and control districts, we defined an event study setting where we allowed for separate treatment effects in each pre- and post-moratorium year. In all estimates, individual-level covariates include dummies for whether the individuals are male, whether they reside in a urban region

and a quadratic of age. Time fixed effects and district fixed effects were included in all our estimates. Since the treatment status was defined at the district level, robust standard errors were clustered at the district level in all cases.

In these estimates, since the Sakernas data that we use are collected in August every year, we treat 2011 as the first year post-moratorium to migration to Saudi Arabia. In this regard, it is also noticeable that the announcement of the moratorium to Saudi Arabia in June 2011 followed an escalation of protests that occurred in the previous months against cases of mistreatment of Indonesian domestic workers. For instance, in April 2011, the initial three-year sentence against a Saudi employer accused of torturing an Indonesian maid, was overturned by the appeals court, generating public outcry in Indonesia, as reported by local and international media (BBC, 2011a and 2011b). It is plausible that this mounting tension between Indonesian and Saudi authorities may have led to the immediate enforcement of the provisions of the 2011 migration moratorium. Thus, the 2011 reform may have produced detectable effects already by August 2011. Also for this reason, we treated 2011 as the first 'post-treatment' year in this part of our analysis.

Column [1] shows the results for unemployment for the full sample, while columns [2] and [3] show the unemployment effects of the 2011 moratorium separately for men and women. In all cases, we could not detect any significant impact of the 2011 migration moratorium on unemployment. Figure 8 shows event-study estimates and 95 percent confidence intervals of the causal effect of the moratorium to Saudi Arabia on unemployment for the full sample. These event-study estimates show there to be no differential pre-treatment trends in unemployment rates between treatment and control districts. The joint test of significance of the pre-treatment coefficients shows them to be indistinguishable from zero at all conventional levels (i.e., $P > F = 0.216$). However, Figure 8 also confirms that no increase

in unemployment could be detected in our data as a result of the 2011 migration moratorium, since the post-treatment coefficients are not jointly statistically significant (i.e., $P > F = 0.764$).

Columns [4], [5] and [6] in Table 3 show the results of our difference-in-differences analysis where we estimate a linear probability model for being employed in the informal sector using the specification in equation (1). We hereby consider a worker as employed in the informal sector according to the Labor Force Survey (Sakernas) if one of the following conditions is met: i) the worker is a self-employed in the agriculture sector; ii) the worker is self-employed with temporary or unpaid workers; iii) the worker is a casual worker in either agriculture or non-agriculture sector; iv) the worker is an unpaid family member[4]. Column [4] shows that the 2011 migration moratorium led to a significant increase in the likelihood of individuals in treatment districts to be employed in informal jobs. Columns [5] and [6] show that significant increases in the likelihood to engage in informal jobs were observed for both men and women. Figure 9 shows point estimates and 95 percent confidence intervals of our event study estimates of informality. Similarly to Figure 8, Figure 9 also displays tests of the joint significance of the pre-treatment and post-treatment coefficients in the equation. Figure 9 shows the pre-treatment coefficients to be indistinguishable from zero (i.e., $P > F = 0.164$): the lack of differential pre-treatment trends between treatment and control districts, therefore, allows us to interpret the effects for informality in Table 3 as causal. Post-treatment coefficients

[4] According to this definition, 50.1 percent of total workers result employed in the informal sector in the year prior to the moratorium, 2010. This figure is not far from the informal employment rate released by the National Statistical Office for the same year (57 percent), which is based on a combination of workers' occupation and sector characteristics. The measurement of informality is challenging in Indonesia for the period considered in our analysis, since neither the Sakernas nor the Susenas surveys include questions that enable to extract standard measures of informal employment, such as share of workers with a written contract, or share of workers contributing to compulsory social insurance and social security. Only in the 2016 Sakernas Labor Force Survey, a specific question for employees and casual workers was introduced, to assess whether they worked with written contracts. According to these most recent data, only 20 percent of total workers reported to have a written contract in Indonesia, so our measure might underestimate the true size of informal employment.

in Figure 9 appear jointly statistically significant, and larger effects for informality can be detected in 2012 and persist until the end of our study period.

The estimated coefficient in column [6] of Table 3 indicates a 1.3 percentage points, or 4.2 percent, increase in treatment districts in the female informality rate that is due to the 2011 moratorium. This corresponds to an increase in the number of female informal workers by approximately 132.000 units. We could therefore speculate that 66 percent of the approximately 200.000 female migrants to Saudi Arabia who were not allowed to emigrate after 2010 (as shown by the drop in documented migrants to Saudi Arabia in Figure 2), could have been absorbed in informal jobs in treatment districts.

The results in columns [7], [8] and [9] in Table 3 show that the 2011 migration reform also led individuals in treatment districts to be more likely to engage in agriculture. The estimated positive effect is significant at all conventional levels for the full sample, as well as for both males and females separately. Figure 10 shows that, also in this case, no differential pre-treatment trends were observed between individuals in our treatment and control groups. Unlike the pre-treatment coefficients, the post-treatment coefficients appear jointly statistically significant. The positive effect in Table 3 appears driven also in this case by the positive effects from the years 2012, 2013 and 2014. Again, the effects should be interpreted as an increase by 1.6 (2.3) percentage points in the share of women (men) employed in agriculture in treatment districts after the moratorium, corresponding to an increase by over 200.000 (400.000) female (male) workers in agriculture in treatment districts over the period following the moratorium.

In sum, the lack of effects of the 2011 migration moratorium on unemployment in origin communities, seems to hide the considerable churning that occurred in the Indonesian labor markets as a result of this reform. The sudden impediment to migrate overseas for hundreds of thousands of women led these women and others in their communities to seek employment in

informality and agriculture. The analysis also documents detectable effects of the migration restriction on the labor market outcomes of males. Insofar as the moratorium led to foregone remittances, and thus resulted in a negative income shock, it is not surprising that this reform also had an effect on men, either directly (in the case of men connected to female migrants via family ties) or indirectly (via general equilibrium and/or spillover effects). The comparison of the estimated coefficients in Table 3 for men and women with their respective pre-moratorium rates of informality and employment in agriculture in control districts, show larger effects for women than for men. In particular, the 2011 moratorium led to a 3.8 percent increase in informality for males, and to a 4.2 percent increase for women. The 2011 moratorium also led to a 5.8 percent increase in employment in agriculture for men, and to a 6.8 percent increase for women. Indeed, this appears sensible, as women were the direct target of the 2011 migration moratorium.

These findings reflect the ability of rural areas in Indonesia to absorb large increases in the supply of labor (see Bazzi, 2016). However, the shift towards informality arguably also signals a worsening in the average quality of jobs in treatment districts, which may have been accompanied by a decrease in the living standards of Indonesian households in these regions. The labor market patterns documented in this section may have resulted in an impoverishment of the local population, due to the simultaneous loss of remittances from migrants and to the suboptimal alternatives available in local labor markets to absorb the excess supply of labor in the short run. This is in fact what we test in the following section, where we investigate the impact of the 2011 moratorium at the household level on household-level outcomes, i.e., on real per capita consumption and on school enrolment of children in the relevant ages.

c. Effects of the moratorium on per capita consumption and school enrolment

The results in the previous section provided evidence that the migration restriction enacted by the Indonesian government did not affect substantially the unemployment rates in the local communities of origin, but rather induced a switch towards informal employment and employment in agriculture. In this section of the paper we complement this analysis using individual level data from the consumption household survey *Susenas* (Socio-Economic Survey of Indonesia). Using the Susenas 2004-2014 survey data, we examine the effects of the 2011 migration moratorium on additional dimensions of households' welfare, namely per capita consumption and school enrolment of children in schooling age (in primary and junior secondary school, for both males and females separately).

The 2011 migration moratorium may have affected per capita consumption in treatment communities via a simple income effect: the migration ban might have resulted in a fall in migrant women's labor income and in a drop in remittances, which in turn may have induced a reduction in household consumption. By preventing access to financing from remittances from overseas, the migration ban may have also affected the consumption levels of migrants' households: this is an outcome of primary interest, given that international migration represents a traditional strategy for poor rural households to escape poverty and sustain their origin families. This negative income effect may have been only partially offset by the engagement of the foregone migrants into informal and agricultural jobs.

The 2011 migration moratorium may have also had unintended consequences for school enrolment of children in origin households. Theoharides (2016), for example, finds that the Japanese migration ban in 2005 for OPAs from the Philippines resulted in an increase in child labor in the Philippines. Since informal female workers from Indonesia are generally middle-aged women that would normally leave their children behind when they migrate, this natural experiment allows us to examine the effect of the maternal presence in the household on

children's schooling outcomes. Most of the related literature has explored the effect of parental absence on educational outcomes of children left behind in contexts in which migrants are men; thus this literature has examined, *de facto*, the consequences of the absence of the father when he becomes a migrant. Cox-Edwards and Ureta (2003), Yang (2008) and Alcaraz et al. (2012) are some recent studies that document a positive impact of emigration on the educational achievement of children left behind, mostly through remittances. Antman (2011) provides evidence from Mexico that spousal control over the intra-household allocation of resources is a major mechanism through which parental migration may affect children: while women have a greater control over the decision-making process of the household while the father is abroad, resources shift back to boys once the father has returned. Finally, a number of studies find evidence of detrimental effects of parental absence on the education of children, including Zoller Booth (1995) from Swaziland, Lahaie et al. (2009) from Mexico and Giannelli and Mangiavacchi (2010) from Albania.

For this analysis, we used all the available survey years of the *Susenas* from 2004 until 2014 inclusive. The *Susenas* is a survey regularly conducted by BPS-Statistics Indonesia to collect information on consumption, housing conditions, social benefits, demographics, employment, education and other socioeconomic characteristics of households. It is representative at the *kota/kabupaten* (district) level, and is the main source of information for the calculation of official poverty and inequality statistics by BPS-Statistics Indonesia.

To estimate the effects of the 2011 migration moratorium on our household-level outcomes of interest, we estimated again the difference-in-differences specification in equation (1). Table 4 presents our results. Also in Table 4, all estimated equations include year and district fixed effects. The set of additional covariates is also identical to the set of covariates used for the analysis of labor market outcomes in the previous section. Columns [1] and [2] of

Table 4 show that the migration moratorium in 2011 had no detectable effect on the (log) real per capita expenditure of households in treatment districts. The results in Figure 11 show no significant difference in our estimated pre-moratorium trends until 2010, which validates our interpretation of these results as causal. Also in this case, the test of joint significance of the pre-treatment coefficients shows them to be not statistically different from zero. Figure 11 also shows that the post-treatment coefficients are jointly significantly different from zero at the 10% significance level. This could suggest that, due to the migration restriction and to the suboptimal alternatives immediately available to foregone migrants, living standards and welfare might have actually deteriorated in treatment regions after 2011. However, since the main difference-in-differences estimates in columns [1] and [2] of Table 4 are not distinguishable from zero, we do not find supportive evidence of a negative causal effect of the moratorium on log-consumption in treatment versus control districts.

Columns [3] to [6] in Table 4 report the estimated effect of the 2011 moratorium to Saudi Arabia on enrolment rates in primary and junior secondary education, separately for males and females. Results in columns [3] and [4] suggest that the moratorium to Saudi Arabia had no significant effect on the rate of enrolment in primary school, which is consistent with the fact that Indonesia reached almost everywhere full enrolment in primary education. On the contrary, we find a significant increase in both male and female enrolment in junior secondary school following the introduction of the 2011 migration restriction. Since no differential pre-moratorium trends appear in Figures 12 and 13 for males and females respectively, we interpret these as the effects of the moratorium to migration implemented by the Indonesian government in 2011. The estimates in columns 5 and 6 of Table 4 indicate that the 2011 moratorium led to an increase in male (female) enrolment in junior secondary school by 3.1 (3.8) percentage points in treatment districts, corresponding to a 4.8 (5.8) percent increase in school enrolment rates.

Three potential channels could explain the increase in enrolment for pupils in junior secondary school following the moratorium. First, these results may reflect the importance of the maternal presence for children's human capital investment: although the departure of female domestic workers to Saudi Arabia generates economic benefits for the origin households and communities, the departure of the mother can result in a loss of control over her children's schooling trajectories. This is consistent with the importance of spousal control over the intra-household allocation of resources documented in Antman (2011), as it suggests that mothers have bargaining power within the household. In the absence of any bargaining power, mothers' contribution to the household's welfare would work only through remittances, and the negative income shock induced by the moratorium would be expected to result in a decrease in children's enrolment rate (e.g., children could be involved in labor activities rather than sent to school, as to compensate for the drop in remittances). Until recently, very few studies have focused on this issue (Antman 2013), mostly because female migration is less common in the countries the literature has focused on. Jampaklay (2006), instead, finds that, unlike paternal absence, the absence of the mother in the long run may impact negatively on children's education in Thailand. Cortes (2014) also concludes that the absence of the mother has an overall negative effect on children's education, further arguing that maternal absence is more harmful than the absence of the father. Our findings appear consistent with the evidence in these studies.

A second interpretation of the increase in female enrolment in secondary education in treatment districts resulting from the moratorium is that, while women are working abroad, the labor of daughters substitutes the labor of mothers within the household or in other informal economic activities. Since women are forced not to migrate by the moratorium, they resume domestic activities and free daughters' time for investing in higher education. A third plausible explanation for the increase in enrolment rates in junior secondary education, and consistent

with the fact that results are more pronounced for girls than for boys, might relate to aspirations. While authorities in Saudi Arabia require completion of primary school to be eligible to migrate as a domestic worker, alternative destination countries require completion of junior secondary school. These include Hong Kong, Singapore, Korea and Taiwan. Therefore, the positive effect of the moratorium on school enrolment for pupils in junior secondary school may be explained by the rational decision of the households to invest further in their daughters' education, in order to enable them to migrate as domestic workers towards wealthy households in Hong Kong or Singapore.

d. Robustness checks and placebo effects of the moratorium

The last section of the paper corroborates the analysis in the previous sections, by showing the robustness of our results to a variety of plausible definitions of treatment group districts. This section also shows and discusses the results of a falsification exercise, whereby we impose that the recent migration moratoria affected other destinations, which in reality remained unaffected by the recent migration policies of Indonesia. Both these exercises provide evidence in support of our earlier conclusions, as they suggest that the significance in our difference-in-differences estimates in Tables 3 and 4 is indeed the result of the 2011 moratorium to Saudi Arabia.

In our main analysis, districts where the majority (i.e., 50 percent or more) of female migrants in 2005 travelled to Saudi Arabia were grouped in the treatment group, with all the remaining districts forming the control group. Although this appears as a natural cutoff to distinguish between districts that were more or less exposed to the implementation of the 2011 migration moratorium, in principle all districts may have been affected by this nationwide reform. Thus, alternative cutoffs may seem equally plausible, and indeed useful, to analyze the consequences of the moratorium to Saudi Arabia on origin communities. Tables 5 and 6 show the results of our difference-in-differences analyses on all our outcomes of interest for varying

definitions of treatment group districts. In particular, in the first row of Tables 5 and 6, districts where 40 percent or more of female migrants in 2005 emigrated to Saudi Arabia were grouped in our treatment group; in the second row of Tables 5 and 6, only districts where 60 percent or more of female migrants in 2005 departed to Saudi Arabia were grouped in our treatment group; finally, in the third row of Tables 5 and 6, only districts where 70 percent or more of female migrants in 2005 travelled to Saudi Arabia were grouped in our treatment group.

Columns in Tables 5 and 6 are organized in the same way as in Tables 3 and 4. The results in Tables 5 and 6 appear very similar to those in Tables 3 and 4. Looking at Table 5, no statistically significant effect of the moratorium appears on unemployment, regardless of the cutoff used to define treatment and control districts. In contrast, positive and significant effects of the 2011 migration moratorium appear for both informality and employment in agriculture, for men as well as for women. For both outcomes, a greater effect appears when the strictest definition of treatment group is used (i.e., in the third row, where treatment districts are only districts where at least 70 percent of female migrants in 2005 travelled to Saudi Arabia). This appears sensible, as these are the districts that were expected to be mostly exposed to the implementation of the 2011 migration moratorium. Table 6 shows consistent results with those in Table 4, as it shows no detectable changes in (log) per capita expenditure, but also a positive and significant increase in enrolment rates in junior secondary school due to the 2011 reform. For both men and women, a greater effect appears also in this case when the strictest definition of treatment group is used in the third row. The overall conclusion from the estimates in Tables 5 and 6 is that our results are robust to alternative, plausible, definitions of treatment and control groups.

To further check that our results are not spurious, and that they can be safely attributed to the 2011 migration moratorium, this section also shows the results of a placebo test. In this

exercise, placebo treatment districts are defined as districts that sent the majority of migrants to destinations not affected by the 2011 moratorium to Saudi Arabia. These destinations are: Singapore, Taiwan, Hong Kong, South Korea, Japan, UAE, Kuwait, Jordan, USA, and others. We then compare all our outcomes of interest between the placebo treatment districts and all other Indonesian districts that did not send any migrants overseas in 2005. Districts that sent the majority of migrants to Saudi Arabia and those that sent the majority of migrants to Malaysia in 2005 were excluded from this falsification test.

The results in Tables 7 and 8 show that the 2011 moratorium to Saudi Arabia did not have any significant effect on local labor markets, expenditure patterns and school enrolment rates in districts where the main destinations of female migrants were other countries. This falsification exercise corroborates the validity of our identification strategy, and also supports the causal interpretation of the effects of the 2011 moratorium on the outcomes of interest in the affected communities. Futher, since the alternative destinations considered in the falsification exercise typically attract more skilled female migrants (e.g. knowledge of English is required for migrants applying for working permit as a domestic worker in Singapore), we can infer that our estimated effects for low-skilled domestic workers affected by the moratorium to a traditional destination, are larger than for migrants with higher skills. Finally, the falsification exercise also demonstrates that female migrants to Saudi Arabia and female migrants to other more recent destinations (e.g. Singapore, Taiwan, Hong Kong, South Korea, Japan, UAE, Kuwait, Jordan, USA) are not perfect substitute. This result is also in line with the findings on "sticky corridors" by Theoharides (2016), which show that switching between destinations (or from traditional destinations to new ones) is typically very hard for migrants.

6. Conclusion

This paper studies the consequences of emigration of women on origin communities in Indonesia, currently one of the largest origin countries of international migrants in the world. It analyses the consequences of a policy introduced by the Indonesian government in 2011 that banned the migration of female domestic workers to Saudi Arabia, traditionally the most important destination country for Indonesian female migrants. This reform was sudden, as it was spurred by the unexpected beheading of an Indonesian migrant in Saudi Arabia and by the resulting political turmoil, and it affected hundreds of thousands of women in recent years. At the time of writing, the moratorium is still active. This is an unusual natural experiment, which provides a rare opportunity to assess the effect of a migration restriction at the origin on the origin communities. In addition, since female domestic workers represented around 70% of all documented migrants before the introduction of moratoria, this natural experiment is likely to have important external validity.

Our general conclusion is that the migration moratorium implemented by the Indonesian government in 2011 led to a deterioration in the local labor markets at the origin, by inducing large flows of both male and female workers into informal employment, and employment in agriculture. Although local labor markets appeared capable to absorb the excess labor supply generated by the migration restriction with no significant consequences on unemployment, the restriction to migration ultimately resulted in an increase in informal activities such as agriculture work, which might have worked as a "cushion of last resort" in the absence of good jobs opportunities in local labor markets. We find larger flows into informality and agriculture for women than for men as a consequence of the moratorium. Finally, we also find a positive effect of the moratorium to Saudi Arabia on enrolment in junior secondary school, arguably reflecting the importance of the maternal presence in the household

for the schooling trajectory of her children, while we do not find a statistically significant effect of the moratorium on households' consumption patterns.

From the standpoint of migration policies, our results suggest that alternative policy options ought to be considered in order to make migrants more aware of, and better prepared for, the migration experience and the associated gains and losses. These policies could include more structured pre-departure trainings and information campaigns to raise migrants' awareness of overseas employment and living conditions, enforcement of compulsory insurance and protection schemes, and stricter monitoring of intermediaries' behavior. Further, specific policies and trainings are also required to make migrants more adaptable to changes in circumstances, aiming to reduce their switching costs to better destinations and working opportunities in the presence of sudden external shocks. Finally, our results also stress the importance of elevating the policy debate on international migration beyond national borders, by encouraging cross-country bilateral agreements between sending and receiving countries that can maximize the options for safe and documented work for migrants overseas.

References

Acosta, P., 2006. 'Labor Supply, School Attendance, and Remittances from International Migration: The Case of El Salvador,' *Policy Research Working Paper Series 3903, The World Bank.*

Acosta, P., 2007. 'Entrepreneurship, Labor Markets and International Remittances: Evidence from El Salvador,' *International Migration Policy and Economic Development: Studies Across the Globe", World Bank, Washington, DC.*

Adams, R. H., Jr. and Cuecuecha, A., 2010. 'The economic impact of international remittances on poverty and household consumption and investment in Indonesia,' *Policy Research Working Paper Series 5433, The World Bank.*

Airola, J., 2008. 'Labor Supply in Response to Remittances Income: The Case of Mexico,' *The Journal of Developing Areas,* Vol. 41, No. 2, pp. 69-78.

Alcaraz, C., Chiquiar, D. and Salcedo, A. 2012. 'Remittances, schooling, and child labor in Mexico', *Journal of Development Economics,* 97(1): 156-65.

Amuedo-Dorantes, C., Pozo, A., 2006. 'Migration, Remittances and Male and Female Employment Patterns,' *American Economic Review (Papers and Proceedings)*, 96 (2), 222–226.

Antman, F. M. 2011. "International migration and gender discrimination among children left behind", *American Economic Review*, 101(3): 645-49.

Antman, F. M. 2013. 'The Impact of Migration on Family Left Behind'. In: A.F. Constant and K.F. Zimmermann (Eds.), International Handbook on the Economics of Migration. Edward Elgar Publishing, forthcoming.

Aydemir, A. and G. J. Borjas. 2007. 'A Comparative Analysis of the Labor Market Impact of International Migration: Canada, Mexico, and the United States,' *Journal of the European Economic Association*, 5(4): 663-708.

Bazzi, S. 2012. 'International Migration from Indonesia: Stylized Facts', Mimeo.

Bazzi, S., Gaduh, A., Rothenberg, A., and Wong, M. 2016. 'Skill Transferability, Migration, and Development: Evidence from Population Resettlement in Indonesia', *American Economic Review*, vol. 106, no. 9, (pp. 2658-98), September.

BBC Asia Pacific (2011a) Indonesia condemns Saudi maid abuse acquittal, http://www.bbc.co.uk/news/world-asia-pacific-13878418, April 4th.

BBC Asia Pacific (2011b) Saudi execution prompts Indonesia maid travel ban, http://www.bbc.co.uk/news/world-asia-pacific-13878418, June 22nd.

Beaman, L. 2012. 'Social Networks and the Dynamics of Labor Market Outcomes: Evidence from Refugees Resettled in the U.S.,' *Review of Economic Studies,* vol. 79 (1): pp. 128-161, January.

BNP2TKI National Agency for the Placement and Protection of Indonesian Migrant Workers (Badan Nasional Penempatan dan Perlindungan Tenaga Kerja Indonesia), http://www.bnp2tki.go.id/

Boeri, T. 2010. 'Immigration to the land of redistribution', *Economica,* 77 (651-487).

Borjas, George J. 2008. 'Labor outflows and labor inflows in Puerto Rico,' *Journal of Human Capital,* 2(1), 32-68.

Bouton L., Saumik P. and Tiongson E. R. 2009. 'The impact of emigration on source country wages: Evidence from Republic of Moldova,' mimeo, presented at: *3rd Migration and Development Conference, Paris School of Economics,* September 10-11, 2010.

Clemens, M. A. 2011. 'Economics and Emigration: Trillion-Dollar Bills on the Sidewalk?', *Journal of Economic Perspectives,* 25, 83–106.

Cortes, P. 2014. 'The feminization of international migration and its effects on the children left behind: Evidence from the Philippines', *World Development, forthcoming.*

Cox-Edwards, A. and Ureta, M. 2003. 'International migration, remittances and schooling: Evidence from El Salvador', *Journal of Development Economics,* 72(2): 429-61.

D'Cunha, J. 2005. 'Claim and Celebrate Women Migrants' Human Rights through CEDAW,' *UNIFEM Briefing Paper, www.unifem.org*

Damon, A. 2009. 'Household labor allocation in remittance-receiving households: the case of El Salvador', *mimeo.*

Di Giovanni, J., Levchenko, A. and Ortega F. 2015. 'A Global View of Cross-border Migration', *Journal of the European Economic Association,* 13, 168–202.

Dinkelman, T., and Mariotti, M. (2016) 'The long run effects of labor migration on human capital accumulation in sending communities'**,** *American Economic Journal: Applied Economics,* vol. 8, no. 4, October, pp. 1-35.

Docquier, F., Özden, Çağlar, Peri, G., 2011. 'The wage effects of immigration and emigration.' *NBER Working Paper 16646.*

Dustmann, C., Frattini, T., Rosso, A., 2012. 'The effect of emigration from Poland on Polish wages.' *CReAM Discussion Paper 29.*

Ehrenreich, B. and Hochschild, A., 2003. 'Global Woman: Nannies, Maids and Sex Workers in the New Economy,' *Metropolitan Press, New York, NY.*

Elsner, B., 2013a. 'Does emigration benefit the stayers? Evidence from EU enlargement.' *Journal of Population Economics* 26, 531–553.

Elsner, B., 2013b. 'Emigration and Wages: The EU Enlargement Experiment.' *Journal of International Economics,* 2013, 91(1), 154-163.

Facchini, G., and Mayda, A. M. 2009. 'Does the welfare state affect individual attitudes toward immigrants?' *Review of Economics and Statistics*, 91, 295–314.

Facchini, G., Mayda, A. M., and Mishra, P. 2011. 'Do interest groups affect US immigration policy?' *Journal of International Economics,* 85, 114–128.

Farbenblum, B.,Taylor-Nicholson, E. and Paoletti, S. 2013. *Migrants Workers' Access to Justice at Home: Indonesia*. Open Society Foundations.

Fudge, J. 2010. 'Global care chains: Transnational migrant care workers.' *Paper prepared for the International Association of Law Schools Conference on Labor Law and Labor Market in the New World Economy,* Milan, May 20-22, 2010.

Giannelli, G. C., and Mangiavacchi, L. 2010. 'Children's schooling and parental migration: Empirical evidence on the 'left-behind' generation in Albania', *Labor ,* 24: 76-92.

Hanson, G. H., 2007a. 'Emigration, Remittances and Labor Force Participation in Mexico,' *IDB Publications (Working Papers) 9371, Inter-American Development Bank.*

Hanson, G. H., 2007b. 'Emigration, Labor Supply, and Earnings in Mexico,' *NBER Chapters, in: Mexican Immigration to the United States, pages 289-328 National Bureau of Economic Research, Inc.*

Hatton, T. J. 2007. 'Should we have a WTO for international migration?', *Economic Policy,* 50, 339-383.

Hatton, T.J., 2014. 'The economics of international migration: a short history of the debate.' *Labor Economics*, 30, pp 43-50.

Hickey M., Narendra, P. and Rainwater, K.(2013). 'A Review of Internal and Regional Migration Policy in Southeast Asia', Migrating Out of Poverty, Research Program Consortium, Working Paper 8, September.

Human Rights Watch (2004) *Help wanted: Abuses against migrant female domestic workers in Indonesia and Malaysia*. Human Rights Watch Report 16(9), available online at: http://www.hrw.org/sites/default/files/reports/indonesia0704full.pdf.

International Labor Organization (2016). 'Review of labor migration policy in Malaysia', ILO Regional Office for Asia and the Pacific.

International Organization for Migration (2010). *Labor Migration from Indonesia. An Overview of Indonesian Migration to Selected Destination in Asia and the Middle East.*

Jampaklay, A. 2006. 'Parental absence and children's school enrolment: Evidence from a longitudinal study in Kanchanaburi, Thailand', *Asian Population Studies, 2(1): 93-110.*

Kaur, A. (2004). *Wage Labor in Southeast Asia Since 1840. Globalization, the International Division of Labor and Labor Transformations,* Palgrave, Macmillan.

Klein, P., and Ventura G. 2009. 'Productivity Differences and the Dynamic Effects of Labor Movements', *Journal of Monetary Economics, 56, 1059–1073.*

Lahaie, C., Hayes J. A., Piper T.M., and Heymann, J. 2009. 'Work and family divided across borders: The impact of parental migration on Mexican children in transnational families', *Community, Work and Family, 12(3): 299-312.*

Ministry of Manpower of the Republic of Indonesia (Kementerian Ketenagakerjaan Republik Indonesia) http://naker.go.id/

Mishra, P. 2007. 'Emigration and wages in source countries.' *Journal of Development Economics 82, 180–199.*

Munshi, K. 2003. "Networks in the Modern Economy: Mexican Migrants in the U.S. Labor Market," *Quarterly Journal of Economics, 118(2): 549–599.*

Nguyen, T., and Purnamasari R. 2011. 'Impacts of international migration and remittances on child outcomes and labor supply in Indonesia: How does gender matter?', *Policy Research Working Paper 5591, The World Bank.*

Oishi, N. 2005. 'Women in Motion: Globalization, State Policies, and Labor Migration in Asia.' *Stanford: Stanford University Press.*

Ortega, F. and Peri, G. 2012. 'The effect of income and immigration policies on international migration.' *Boston NBER Working Paper. NBER, p. 18322.*

Ortega, F. and Peri, G. 2015. 'Migration Policies: Recent Advances on Measurement, Determinants and Outcomes', *CESifo Economic Studies, 2015, 521–526.*

Pritchett, L. 2006. "Let Their People Come: Breaking the Gridlock on Global Labor Mobility", Center for Global Development.

Taylor, J. E., and Dyer, G. 2009. 'Migration and the Sending Economy: A Disaggregated Rural Economy wide Analysis,' *Journal of Development Studies, 45 (6), 966-989.*

The Economist (2011), 'Beheading the golden goose', http://www.economist.com/blogs/banyan/2011/07/migrant-workers-saudi-arabia.

Theoharides, C. 2016. "Banned from the Band: The Unintended Consequences of Migration Policy on Origin-Country Labor Market Decisions", Mimeo.

United Nations International Migration Report 2015. New York.

Walmsley, T. L., and Winters, L.A. 2005. 'Relaxing the Restrictions on the Temporary Movement of Natural Persons: A Simulation Analysis', *Journal of Economic Integration*, Center for Economic Integration, Sejong University, vol. 20, pages 688-726.

Wong, D. (2005). 'The Rumor of Trafficking: Border Controls, Illegal Migration, and the Sovereignty of the Nation-State' in Van Schendel, W. and Abraham (Eds.), *Illicit Flows and Criminal Things, States, Borders, and the Other Side of Globalization*, Indiana University Press.

World Bank 2014a. World Bank International Migration and Remittances Survey 2013/14, World Bank Jakarta Office, Poverty Global Practice.

World Bank 2014b. A guide to Working with Indonesian Survey Data: the National Socio-Economic Survey SUSENAS, World Bank Jakarta Office, Poverty Global Practice**.**

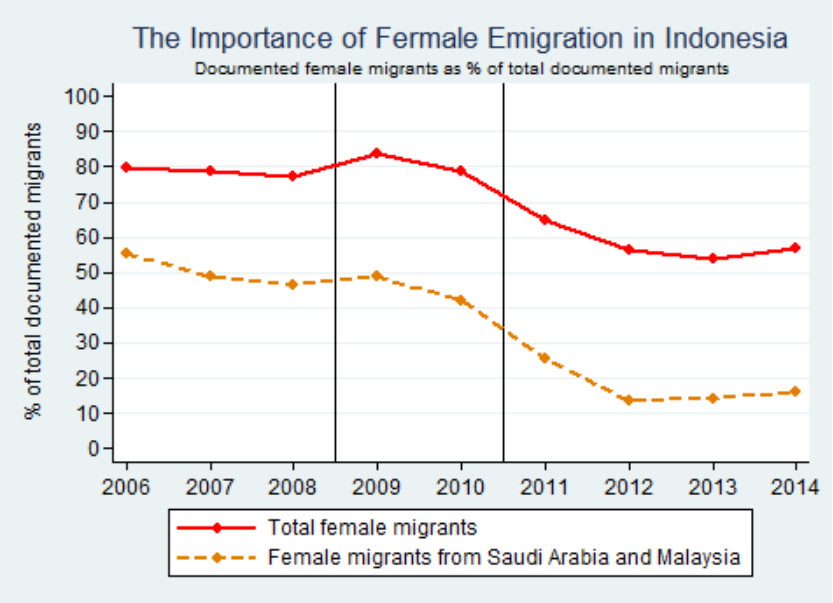
World Bank 2016. Indonesian Global Workers, World Bank Jakarta Office, Poverty Global Practice, mimeo.

Yang, D. 2008. 'International migration, remittances and household investment: Evidence from Philippine migrants' exchange rate shocks', *The Economic Journal 118: 591-630.*

Zoller Booth, M. 1995. 'Children of migrant fathers: The effects of father absence on Swazi children's preparedness for school', *Comparative Education Review, 39(2): 195-210.*

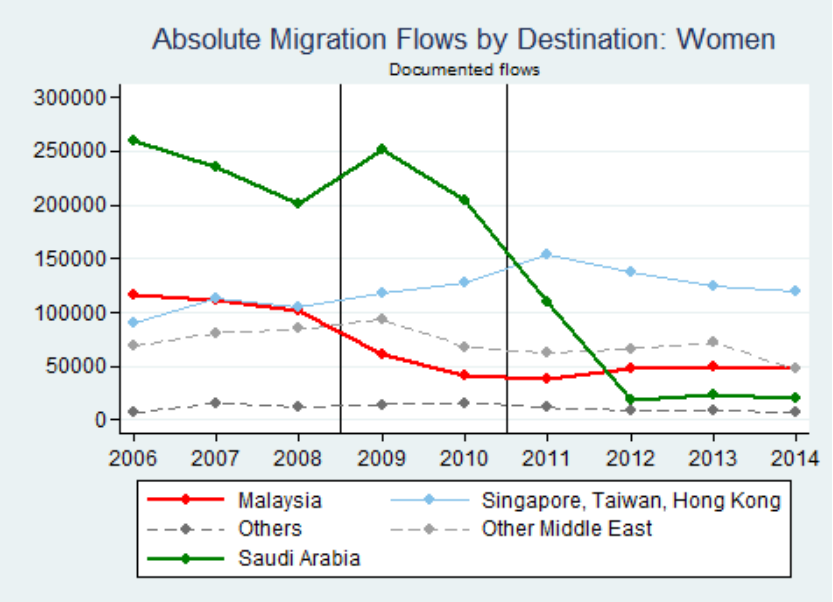
List of Figures and Tables

Figure 1. Female Emigration in Indonesia as % of total documented migrants



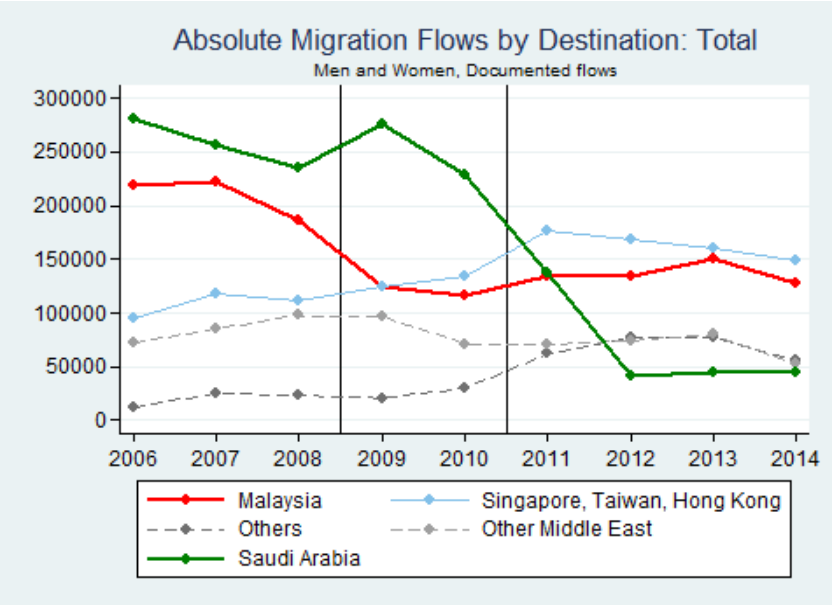
Source: BNP2TKI (http://www.bnp2tki.go.id/stat_penempatan/indeks). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011).

Figure 2. Absolute flows of documented female migrants by destination country



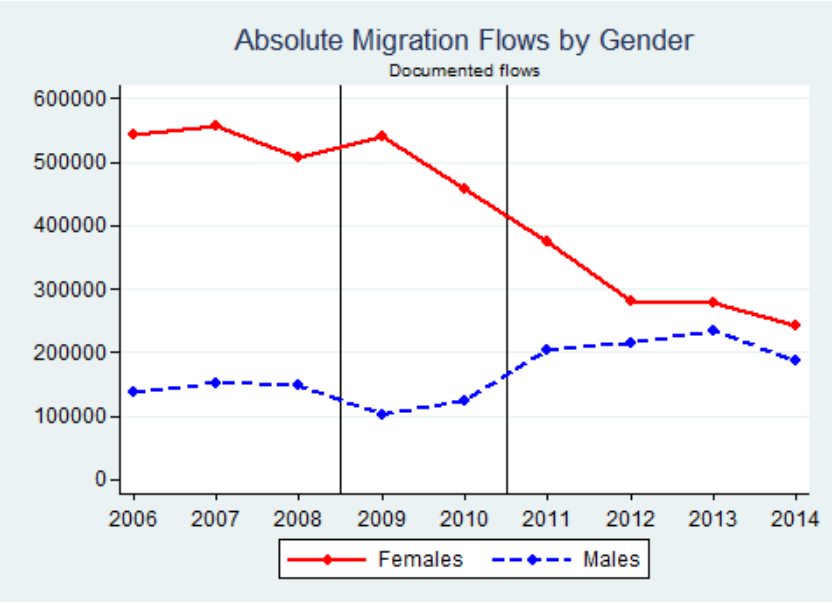
Source: BNP2TKI (http://www.bnp2tki.go.id/stat_penempatan/indeks). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

Figure 3. Absolute flows of total documented migrants by destination country



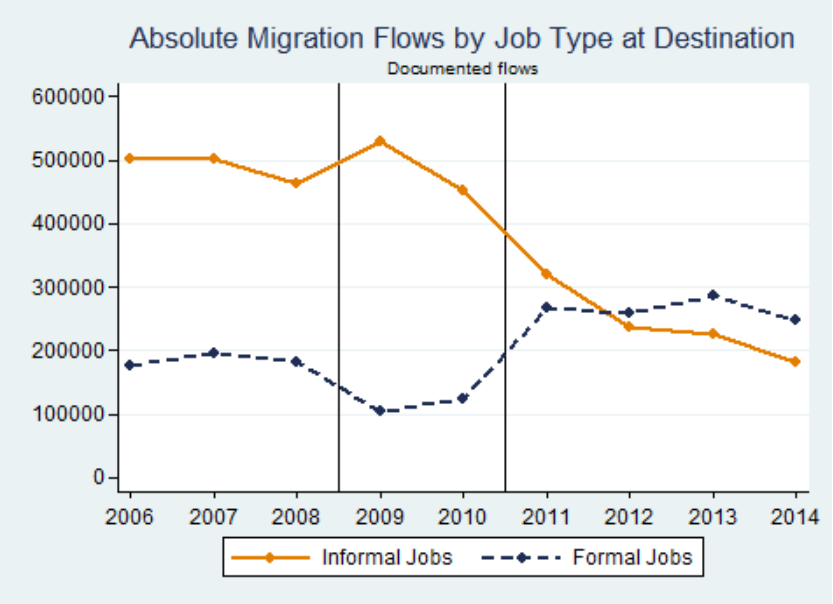
Source: BNP2TKI (http://www.bnp2tki.go.id/stat_penempatan/indeks). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

Figure 4. Absolute flows of total documented migrants by gender



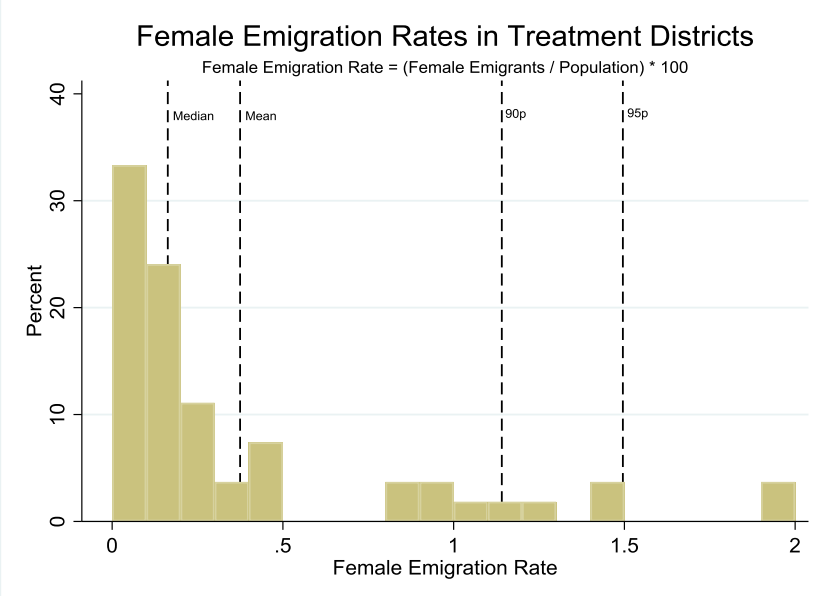
Source: BNP2TKI (http://www.bnp2tki.go.id/stat_penempatan/indeks). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

Figure 5. Absolute flows of total documented migrants by type of job placement in destination countries



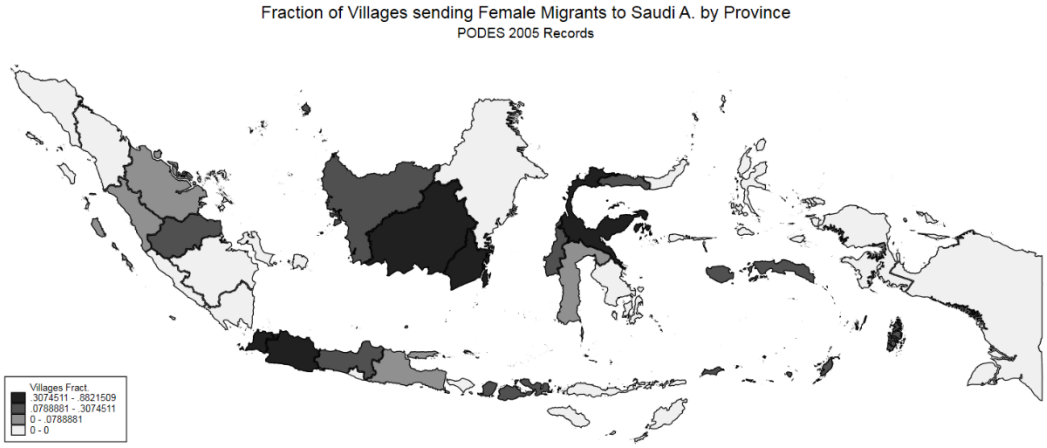
Source: BNP2TKI (http://www.bnp2tki.go.id/stat_penempatan/indeks). Vertical line indicates the moratoria imposed to Malaysia (June 2009) and Saudi Arabia (June 2011). Other Middle East Countries are: United Arab Emirates, Kuwait, Qatar, Oman, Bahrein, Jordan. BNP2TKI data in this figure are end of the year data.

Figure 6. Distribution of female emigration rates in Treatment Group villages



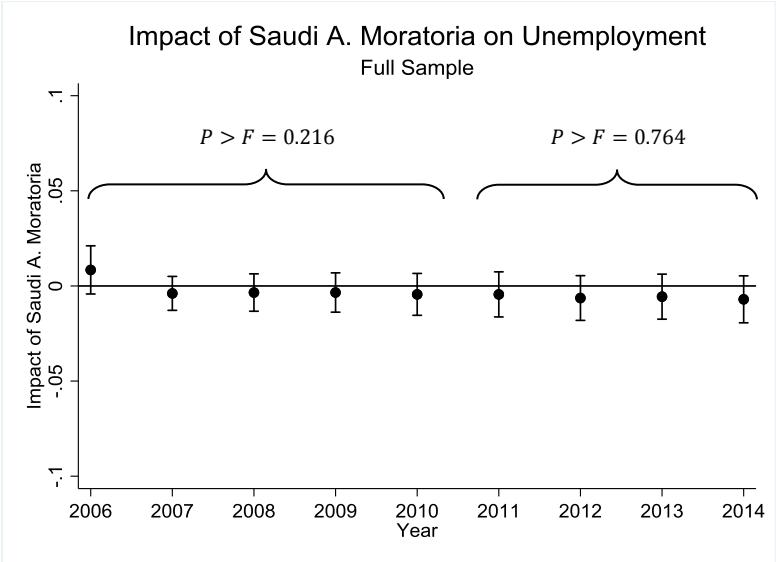
Notes: Figure 6 shows female emigration rates for the treatment group districts in our analysis. The calculations are based on Podes 2005 data.

Figure 7. Distribution of Treated Districts across Provinces in Indonesia.



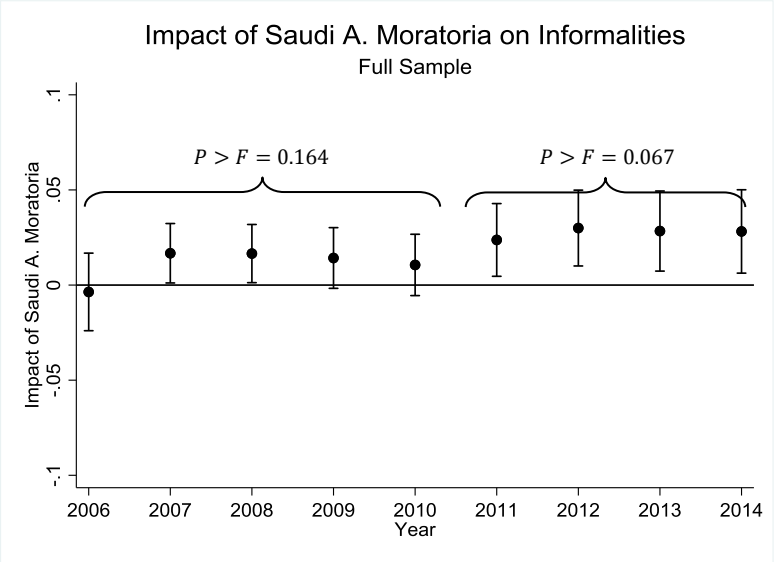
Notes: Figure 7 shows the distribution of treated districts across provinces in Indonesia. No treated districts are found in 16 provinces: these are, namely, Aceh Nanggroe Darussalam, Bali, Bangka Belitung, Bengkulu, DI Yogyakarta, East Kalimantan, East Nusa Tenggara, Kep Riau, Lampung, Maluku Utara, North Sulawesi, North Sumatera, Papua, Papua Barat, South Sumatera, Southeast Sulawesi. In 4 provinces, between 4 and 8 percent of individuals are in treated districts: these are, namely, East Java, Riau, South Sulawesi and West Sumatera. In 7 provinces, between 8 and 33 percent of individuals are in treated districts: these are, namely, Central Java, Gorontalo, Jambi, Maluku, West Kalimantan, West Nusa Tenggara and West Sulawesi. In 6 provinces, more than 33 percent of individuals are in treated districts: these are, namely, Banten, Central Kalimantan, Central Sulawesi, DKI Jakarta, South Kalimantan, and West Java.

Figure 8. Event Study Estimates of Impact of moratorium to Saudi Arabia on unemployment.



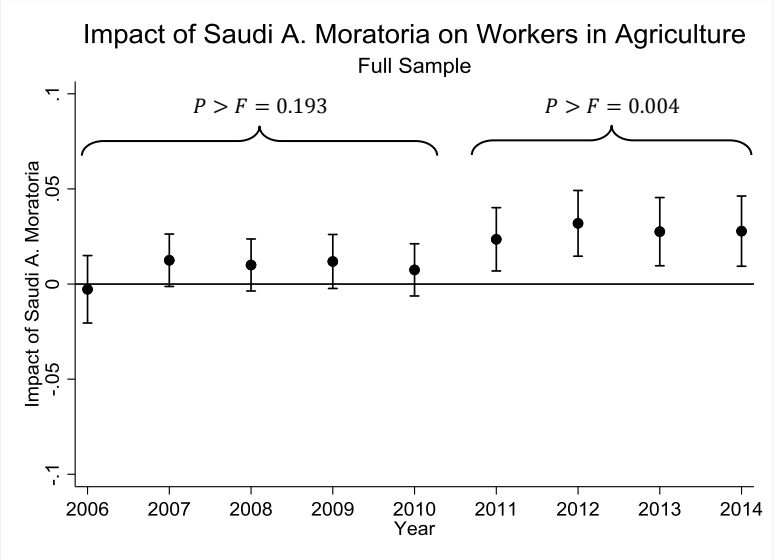
Notes: Figure 8 shows event-study estimates and 95 percent confidence intervals of the causal effect of the moratorium to Saudi Arabia on unemployment. Control variables included are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. The omitted year is 2005. Confidence intervals are based on robust standard errors clustered at the district level.

Figure 9. Event Study Estimates of Impact of moratorium to Saudi Arabia on employment in informal sector.



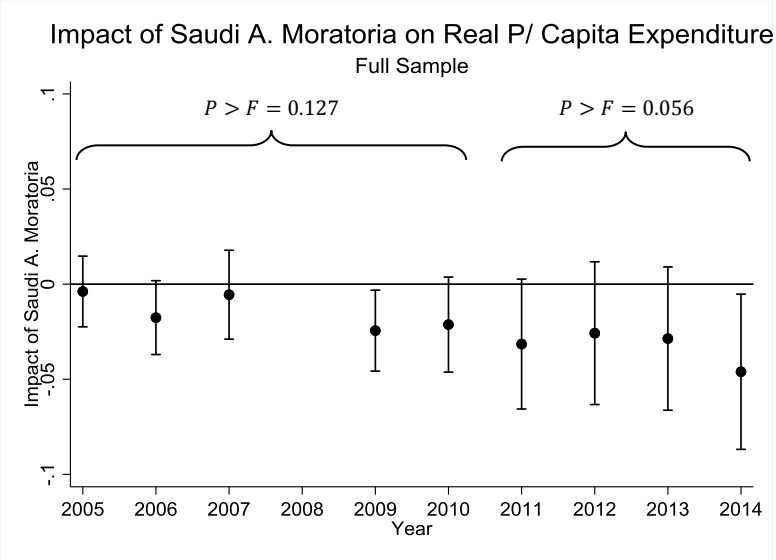
Notes: Figure 9 shows event-study estimates and 95 percent confidence intervals of the causal effect of the moratorium to Saudi Arabia on the likelihood to be employed in the informal sector. Control variables included are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. The omitted year is 2005. Confidence intervals are based on robust standard errors clustered at the district level.

Figure 10. Event Study Estimates of Impact of moratorium to Saudi Arabia on employment in agriculture.



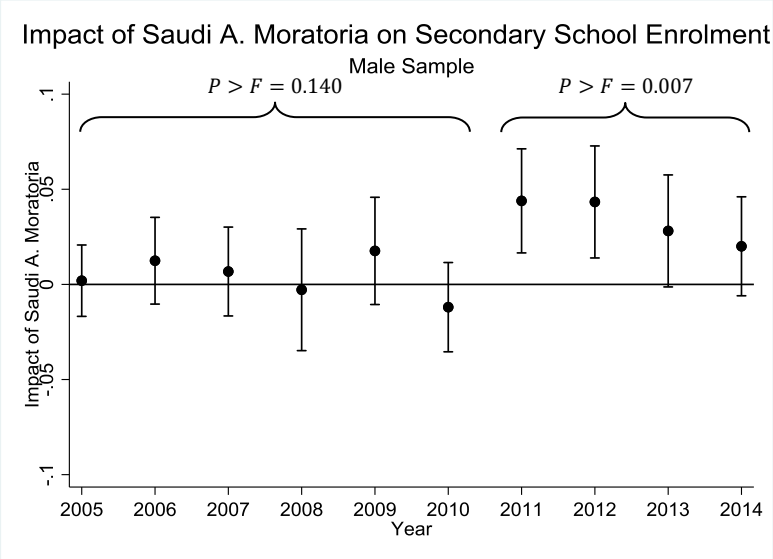
Notes: Figure 10 shows event-study estimates and 95 percent confidence intervals of the causal effect of the moratorium to Saudi Arabia on the likelihood to be employed in the agricultural sector. Control variables included are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. The omitted year is 2005. Confidence intervals are based on robust standard errors clustered at the district level.

Figure 11. Event Study Estimates of Impact of moratorium to Saudi Arabia on (log) per capita consumption.



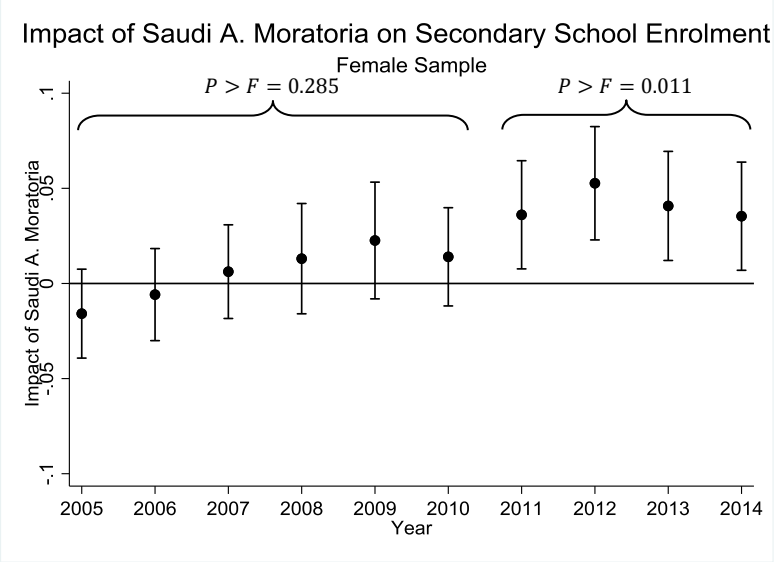
Notes: Figure 11 shows event-study estimates and 95 percent confidence intervals of the causal effect of the moratorium to Saudi Arabia on (log) real per capita consumption. Control variables included are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. The omitted year is 2004. Confidence intervals are based on robust standard errors clustered at the district level.

Figure 12. Event Study Estimates of Impact of moratorium to Saudi Arabia on male enrolment in junior secondary school.



Notes: Figure 12 shows event-study estimates and 95 percent confidence intervals of the causal effect of the moratorium to Saudi Arabia on male enrolment in junior secondary school. Control variables included are dummies for whether the individuals reside in a urban region and a quadratic of age. The omitted year is 2004. Confidence intervals are based on robust standard errors clustered at the district level.

Figure 13. Event Study Estimates of Impact of moratorium to Saudi Arabia on female enrolment in junior secondary school.



Notes: Figure 13 shows event-study estimates and 95 percent confidence intervals of the causal effect of the moratorium to Saudi Arabia on female enrolment in junior secondary school. Control variables included are dummies for whether the individuals reside in a urban region and a quadratic of age. The omitted year is 2004. Confidence intervals are based on robust standard errors clustered at the district level.

Table 1. Districts Characteristics and Balancing Tests: Treatment vs Control Group

	Treatment Districts	Control Districts	Treatment – Control Difference
	[1]	[2]	[1]-[2]
Fraction of *Kelurahan* Villages	0.209	0.291	-0.081
			(0.057)
Fraction of Urban Villages	0.314	0.312	0.002
			(0.051)
Fraction of Coastal Villages	0.087	0.167	-0.080**
			(0.031)
Fraction of Muslim Villages	0.946	0.786	0.160***
			(0.049)
Number of Mosques per Village	5.196	3.704	1.492***
			(0.524)
Number of Churches per Village	0.510	0.928	-0.418**
			(0.173)
Number of Hindu Temples per Village	0.056	0.627	-0.571
			(0.430)
Number of Buddhist Temples per Village	0.061	0.083	-0.021
			(0.028)
Main Source of Income: Agriculture	0.756	0.764	-0.008
			(0.048)
Main Source of Income: Mining	0.003	0.005	-0.002
			(0.004)
Main Source of Income: Industry	0.037	0.018	0.019***
			(0.007)
Main Source of Income: Retail	0.098	0.091	0.007
			(0.022)
Number of Kindergartens	1.633	1.373	0.260
			(0.209)
Number of Primary Schools	3.840	2.967	0.873***
			(0.305)
Number of High Schools	1.467	1.140	0.328
			(0.198)
Number of Vocational Schools	0.660	0.630	0.030
			(0.140)
Number of Hospitals	0.060	0.055	0.005
			(0.014)
Age of Head Village	44.920	44.813	0.106
			(0.323)
Fraction of Male Headed Villages	0.971	0.965	0.006
			(0.005)
Average District Population	1,196,416	623,521.1	572,894.8***
			(90,191.4)
Average No. Male migrant workers (TKI)	893.8	1693.4	-799.6
			(578.7)
Average No. Female migrant workers (TKI)	5187.4	1555.3	3632.2***
			(667.6)
Total Number of Districts	54	243	

Notes: Table 1 shows socio-economic descriptive statistics (averages) calculated in 2005 separately for the treatment and control districts used in our analysis. Standard errors are reported in parentheses. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 2. OLS Estimates of the Impact of 2011 Moratorium to Saudi Arabia on the Number of Migrants

	Dependent Variable: Number of migrants					
	OLS	OLS	OLS	OLS	OLS	OLS
	[1]	[2]	[3]	[4]	[5]	[6]
Saudi A. District	14.014*	15.587	16.894*	-	-	-
	(7.294)	(9.538)	(9.254)			
Post Moratorium	2.115*	2.613*	2.931**	2.250*	2.855**	3.328**
	(1.132)	(1.365)	(1.431)	(1.147)	(1.377)	(1.409)
Saudi A. District *	-4.923***	-5.180***	-4.574**	-5.079***	-5.286***	-4.853***
Post Moratorium	(1.514)	(1.920)	(1.865)	(1.520)	(1.918)	(1.873)
Mean Dep. Var. Pre-Moratorium	20.227	24.005	24.005	20.227	24.005	24.005
Village Characteristics	No	No	Yes	No	No	Yes
District Fixed Effects	No	No	No	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Population-weighted	No	Yes	Yes	No	Yes	Yes
Number of Observations	194946	194946	194946	194946	194946	194946
Number of Villages	49258	49258	49258	49258	49258	49258
Number of Districts	286	286	286	286	286	286

Notes: All the reported difference-in-differences estimates are from OLS regressions of the number of migrants from each village that could be identified in Podes continuously from 1999 to 2014. Robust standard errors (clustered at the district level) are reported in parentheses. Village control variables included are whether the village is located in a rural or urban area, a linear control for the age of the village head and the gender of the village head. Population weights were calculated in 2005. Information on migration at the village level was available from 2005 to 2014. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 3. Impact of moratorium to Saudi Arabia on Local Labor Market Outcomes.

	Unemployed			Employed in Informal Sector			Employed in Agriculture		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Saudi district * Moratorium	-0.003	-0.002	-0.006	0.015**	0.017**	0.013**	0.019***	0.023***	0.016***
	(0.003)	(0.002)	(0.005)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Pre-2011 Mean Dep. Var., Control Group	0.070	0.059	0.088	0.380	0.451	0.309	0.316	0.395	0.237
Sample Used	Full	Male	Female	Full	Male	Female	Full	Male	Female
Background Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.094	0.090	0.103	0.203	0.214	0.178	0.262	0.280	0.226
N	3407695	2090758	1316937	4865110	2425547	2439563	4865110	2425547	2439563

Notes: Table 3 shows estimates of the causal effect of the moratorium to Saudi Arabia on local labor market outcomes. Robust standard errors (clustered at the district level) are reported in parentheses. Control variables included in columns [1], [4] and[7] are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. Control variables included in columns [2], [3], [5], [6], [8] and [9] are dummies for whether the individuals reside in a urban region and a quadratic of age. A worker is defined as employed in the informal sector according to the Labor Force Surveys (Sakernas), if one of the following conditions is met: i) the worker is a self-employed in the agriculture sector; ii) the worker is self-employed with temporary or unpaid workers; iii) the worker is a casual worker in either agriculture or non-agriculture sector; iv) the worker is an unpaid family member. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 4. Impact of moratorium to Saudi Arabia on log per capita consumption and school enrolment rates.

	Log Per capita expenditure		Primary School Enrolment		Junior Secondary School Enrolment	
	[1]	[2]	[3]	[4]	[5]	[6]
Saudi district*Moratorium	-0.024	-0.016	0.003	0.000	0.031***	0.038***
	(0.016)	(0.017)	(0.003)	(0.003)	(0.010)	(0.010)
Pre-2011 Mean Dep. Var., Control Group	12.325	12.275	0.930	0.929	0.640	0.660
Sample Used	Full	Rural	Male	Female	Male	Female
Background Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.534	0.552	0.022	0.022	0.049	0.046
N	12951358	10158214	3935961	3715562	2119195	1994885

Notes: Table 4 shows estimates of the causal effect of the moratorium to Saudi Arabia on (log) real per capita consumption and school enrolment outcomes. Robust standard errors (clustered at the district level) are reported in parentheses. Control variables included in columns [1] and [2] are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. Control variables included in columns [3], [4], [5] and [6] are dummies for whether the individuals reside in a urban region and a quadratic of age. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 5. Impact of moratorium to Saudi Arabia on Local Labor Market Outcomes with Alternative Definitions of Treatment.

	Unemployed			Employed in Informal Sector			Employed in Agriculture		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Saudi district 40 percent * Moratorium	-0.004 (0.003)	-0.003 (0.002)	-0.007 (0.004)	0.015*** (0.006)	0.018*** (0.007)	0.013** (0.006)	0.016*** (0.005)	0.019*** (0.006)	0.014*** (0.005)
Saudi district 60 percent * Moratorium	-0.003 (0.003)	-0.002 (0.002)	-0.005 (0.006)	0.016** (0.007)	0.021*** (0.008)	0.011* (0.007)	0.019*** (0.006)	0.025*** (0.007)	0.013*** (0.006)
Saudi district 70 percent * Moratorium	-0.003 (0.003)	-0.002 (0.002)	-0.006 (0.006)	0.018*** (0.006)	0.023*** (0.007)	0.014** (0.006)	0.021*** (0.005)	0.026*** (0.006)	0.016*** (0.005)
Sample Used	Full	Male	Female	Full	Male	Female	Full	Male	Female
Background Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3407695	2090758	1316937	4865110	2425547	2439563	4865110	2425547	2439563

Notes: Table 5 shows estimates of the causal effect of the moratorium to Saudi Arabia on local labor market outcomes with alternative definitions of treatment. Robust standard errors (clustered at the district level) are reported in parentheses. Control variables included in columns [1], [4] and[7] are dummies for whether the individuals are male, whether they reside in a urban region, and a quadratic of age. Control variables included in columns [2], [3], [5], [6], [8] and [9] are dummies for whether the individuals reside in a urban region, and a quadratic of age. A worker is defined as employed in the informal sector according to the Labor Force Surveys (Sakernas), if one of the following conditions is met: i) the worker is a self-employed in the agriculture sector; ii) the worker is self-employed with temporary or unpaid workers; iii) the worker is a casual worker in either agriculture or non-agriculture sector; iv) the worker is an unpaid family member. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 6. Impact of moratorium to Saudi Arabia on per capita consumption and school enrolment rates with Alternative Definitions of Treatment.

	Log Per capita expenditure		Primary School Enrolment		Junior Secondary School Enrolment	
	[1]	[2]	[3]	[4]	[5]	[6]
Saudi district 40 percent * Moratorium	-0.019	-0.012	0.003	-0.001	0.031***	0.035***
	(0.015)	(0.016)	(0.003)	(0.003)	(0.010)	(0.010)
Saudi district 60 percent * Moratorium	-0.023	-0.019	0.005*	-0.001	0.034***	0.044***
	(0.018)	(0.018)	(0.003)	(0.003)	(0.011)	(0.012)
Saudi district 70 percent * Moratorium	-0.019	-0.014	0.005	-0.002	0.038***	0.044***
	(0.018)	(0.019)	(0.003)	(0.004)	(0.012)	(0.013)
Sample Used	Full	Rural	Male	Female	Male	Female
Background Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	12951358	10158214	3935961	3715562	2119195	1994885

Notes: Table 6 shows estimates of the causal effect of the moratorium to Saudi Arabia on real per capita consumption and school enrolment outcomes with alternative definitions of treatment. Robust standard errors (clustered at the district level) are reported in parentheses. Control variables included in columns [1] and [2] are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. Control variables included in columns [3], [4], [5] and [6] are dummies for whether the individuals reside in a urban region and a quadratic of age. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 7. Falsification Test: Impact of moratorium to Saudi Arabia on Local Labor Market Outcomes in Placebo Districts.

	Unemployed			Employed in Informal Sector			Employed in Agriculture		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Placebo district * Moratorium	0.001	0.004	-0.002	-0.007	-0.012	-0.003	-0.002	-0.008	0.003
	(0.004)	(0.003)	(0.007)	(0.010)	(0.011)	(0.011)	(0.010)	(0.012)	(0.011)
Pre-2011 Mean Dep. Var., Control Group	0.077	0.064	0.097	0.365	0.433	0.294	0.315	0.392	0.236
Sample Used	Full	Male	Female	Full	Male	Female	Full	Male	Female
Background Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.093	0.084	0.109	0.230	0.238	0.215	0.286	0.296	0.267
N	691554	428594	262960	977613	495860	481753	977613	495860	481753

Notes: Table 7 shows placebo estimates of the causal effect of the moratorium to Saudi Arabia on local labor market outcomes. It shows estimates of the causal effect of the moratorium on "Placebo districts", namely districts that sent the majority of migrants to different destinations (such as Singapore, Taiwan, Hong Kong, South Korea, Japan, UAE, Kuwait, Jordan, USA and other destinations), with respect to districts that did not send any migrants overseas in 2005. Districts that sent the majority of migrants to Saudi Arabia and those that sent the majority of migrants to Malaysia were excluded from this falsification test. Robust standard errors (clustered at the district level) are reported in parentheses. Control variables included in columns [1], [4] and[7] are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. Control variables included in columns [2], [3], [5], [6], [8] and [9] are dummies for whether the individuals reside in a urban region and a quadratic of age. A worker is defined as employed in the informal sector according to the Labor Force Surveys (Sakernas), if one of the following conditions is met: i) the worker is a self-employed in the agriculture sector; ii) the worker is self-employed with temporary or unpaid workers; iii) the worker is a casual worker in either agriculture or non-agriculture sector; iv) the worker is an unpaid family member. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Table 8. Falsification Test: Impact of moratorium to Saudi Arabia on per capita consumption and school enrolment rates in Placebo Districts.

	Log Per capita expenditure		Primary School Enrolment		Junior Secondary School Enrolment	
	[1]	[2]	[3]	[4]	[5]	[6]
Placebo district*Moratorium	0.027	0.027	0.012*	0.001	0.016	0.024
	(0.023)	(0.026)	(0.007)	(0.007)	(0.017)	(0.020)
Pre-2011 Mean Dep. Var., Control Group	12.352	12.293	0.916	0.910	0.627	0.639
Sample Used	Full	Rural	Male	Female	Male	Female
Background Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Survey fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.522	0.218	0.054	0.022	0.053	0.051
N	2503668	1947667	755575	704584	388821	365740

Notes: Table 8 shows placebo estimates of the causal effect of the moratorium to Saudi Arabia on (log) per capita expenditure and school enrolment. As Table 7, it shows estimates of the causal effect of the moratorium on "Placebo districts", namely districts that sent the majority of migrants to different destinations (such as Singapore, Taiwan, Hong Kong, South Korea, Japan, UAE, Kuwait, Jordan, USA and other destinations), with respect to districts that did not send any migrants overseas in 2005. Districts that sent the majority of migrants to Saudi Arabia and those that sent the majority of migrants to Malaysia were excluded from this falsification test. Robust standard errors (clustered at the district level) are reported in parentheses. Control variables included in columns [1] and [2] are dummies for whether the individuals are male, whether they reside in a urban region and a quadratic of age. Control variables included in columns [3], [4], [5] and [6] are dummies for whether the individuals reside in a urban region and a quadratic of age. * indicates significance at 10 percent, ** indicates significance at 5 percent, *** indicates significance at 1 percent.

Appendix A. KTKLN Card Application: required steps for perspective migrants

1. Obtain information about the documentation necessary to emigrate for work from the local Office of Manpower (Disnaker) or the local Office of Placement Services and Protection of Indonesian Migrant Workers (BP3TKI)
2. Prepare ID card, birth certificate, and school diploma.
3. Prepare a permission letter from spouse/parent/ guardian, verified by the village head.
4. Fill in a Job Seeker Registration Card issued by Disnaker.
5. Register as a prospective migrant worker at Disnaker office.
6. Attend the socialization meeting conducted by Disnaker to inform perspective migrants on available job vacancies abroad.
7. Attend interests and skills selection tests conducted by Disnaker and PPTKIS (in case the profile of the prospective migrant fits the criteria of the job vacancy).
8. Sign the Placement Agreement with PPTKIS (verified by Disnaker) if the selection test is passed.
9. Reside in temporary accomodations/shelters owned by PPTKIS before departure (for prospective migrant workers selected for informal jobs).
10. Attend trainings, and receive a certificate of attendance.
11. Attend competency test conducted by Professional Certifying Agency, and obtain a skill certificate.
12. Undertake health test
13. Undertake a psychological test
14. Apply for passport
15. Apply for work permit
16. Apply for visa
17. Apply for employment insurance, and obtain an insurance card
18. Contribute to the Labor Development Fund.
19. Attend Pre-Departure Briefing (PAP)
20. Sign a job contract with perspective employer or agency.
21. The migrant worker ID card (KTKLN) is issued by BNP2TKI
22. Depart to destination country

Source: Pocket Book for Prospective Migrant Worker: Working Abroad Legally and Safely, 2011, developed by IOM, United States Government Office to Monitor and Combat Trafficking in Persons (G/TIP), BNP2TKI, and the Ministry of Manpower.