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Andrea Cinque, Lennart Reiners

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Confined to Stay: Natural Disasters and Indonesia's Migration Ban

Abstract

This paper investigates the effects of international migration restrictions on communities' capacity to absorb income shocks after natural catastrophes. We adopt the implementation of an emigration ban on female Indonesians as a natural experiment. After an array of violent assaults against female servants in Saudi Arabia, the Indonesian government issued a moratorium in 2011, preventing millions of female workers to migrate there as domestic workers. Exploiting the exogenous timing of the ban and that of natural disasters allows us to estimate the causal effect of the absence of international migration as an adaptive strategy. Relying on a panel of the universe of Indonesian villages, we use a triple difference strategy to compare poverty levels in the aftermath of natural disasters for villages whose main destination is Saudi Arabia against others, before and after the policy shock. We find that in villages with strong ex-ante propensity to migrate to Saudi Arabia, poverty increases by 13% in face of natural disasters after the ban, further aggravating the already severe consequences induced by those events.

JEL-Codes: F220, J610, Q540.

Keywords: migration, natural disasters, Indonesia, migration ban.

Andrea Cinque
Sciences Po, Department of Economics
France - 75007 Paris
andrea.cinque@sciencespo.fr

Lennart Reiners
University of Göttingen / Germany
lennart.reiners@gmx.de

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1 Introduction

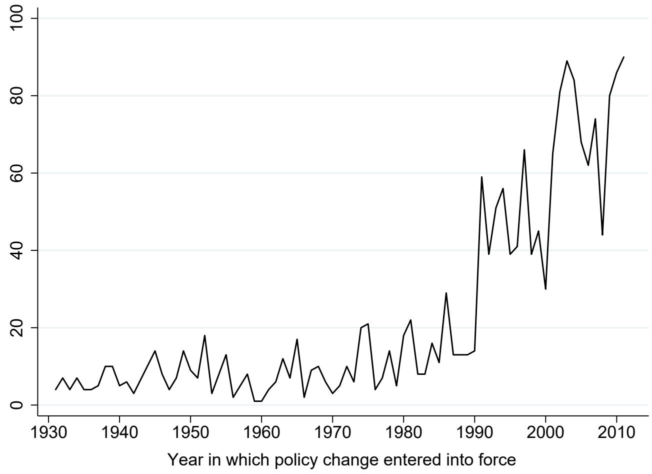
In the coming decades, climate change will exacerbate the frequency of extreme weather events such as floods, droughts and heat waves, affecting livelihoods in manifold ways (FAO, 2018; IPCC, 2021; Jones and O'Neill, 2016). The role of international migration as coping strategy is becoming increasingly important: by 2050, the predicted number of *climate refugees* is estimated to reach hundreds of millions (Rigaud et al., 2018). These developments pose major challenges for governments in both sending and receiving countries around the world. Historically, climate-induced migration has been little restricted as it prevailed mainly as a within-border phenomenon (Cattaneo et al., 2019). Internationally, it occurred in an era of "mass migration", where legal hurdles to move across countries were loose (Spitzer et al., 2020). However, recently countries have been putting more emphasis on migrant selection, resulting in more complex and restrictive regulations (Beine et al., 2016; de Haas et al., 2018). As shown in Figure 1, this trend suggests future scenarios where international migration will be further constrained, potentially undermining its role as major coping strategy to climate change.

This paper examines how natural disasters affect poverty in a scenario where international emigration is heavily restricted. We exploit a unique natural experiment in the form of the sudden implementation of an emigration ban in a country where 7% of the workforce was employed abroad (World Bank, 2017). After repeated cases of abuse and a death sentence of female Indonesian domestic workers in Saudi Arabia, the government decided to emit an emigration ban in 2011 on all women wishing to migrate there as housemaids. In light of heterogeneous migration ties to destination countries across in Indonesia, the ban affected villages to very different degrees. We investigate whether this restrictive emigration policy deprived villages capacity to absorb income shocks induced by natural disasters, a widespread phenomenon in the country. As the moratorium suppressed migration to the second top destination country of Indonesians, we show that this inhibited an important adaptation strategy and therefore had detrimental effects on income for villages with established migration links to Saudi Arabia.

To our knowledge, this paper is among the first to provide causal evidence on the effect of migration restrictions in the context of extreme climatic events [1]. We conduct our analysis in a highly localized setting for the universe of around 70,000 Indonesian villages for the period 2005 - 2011. Our disaggregated data allow us to exploit: (i) spatial variation in the main destination country of international

[1] McLeman (2019) provides descriptive implications of tighter borders on international migration as an adaptation strategy to climatic shocks. Benveniste et al. (2020) and Burzyski et al. (2021) include rising international migration barriers into their climate change models.

Figure (1) Number of new restrictive immigration policies implemented worldwide



Notes Source: Own computation based on DEMIG Policy Database (de Haas et al., 2015).

migrants across Indonesian villages, (ii) the exogeneity of natural disaster events, conditional on village fixed effects and (iii) the implementation of an unexpected national emigration ban which affected one migratory top destination country but not others. Combining data on all three aspects allows us to causally estimate the effect of the moratorium in a triple difference (DDD) setting. We thereby compare poverty levels of villages with migration links to Saudi Arabia - whose majority of migrants went there in 2005 - against the remaining villages, depending on whether they were hit by natural disasters, before and after the moratorium was introduced in 2011.

The DDD allows to overcome potential endogeneity deriving from shocks common to specific groups of villages due to the full saturation of the model with all possible interaction terms. For example, we absorb time-varying labour market response and possible anticipation effects of the moratorium. Two-way interactions, village and year fixed effects leave the only source of variation at the triple interaction. This way, we isolate the causal effect of natural disasters on villages more vulnerable to the introduction of the ban.

In our findings, we first demonstrate that villages with strong migration ties to Saudi Arabia experienced a drastic reduction in international emigration. After the moratorium, the stock of overseas workers decreased by more than 30% compared to villages whose migratory networks were not curtailed. After the ban was implemented, these villages experienced a 13% increase in poverty once hit by disasters. Restrictions to emigration therefore further aggravated the already significant implications for livelihoods induced by these catastrophic events. Estimates presented uphold in manifold robustness checks, including the use of alternative poverty as well as disaster measurements, sub-sample adjustments and placebo regressions.

As an important income shock response (Gröger and Zylberberg, 2016), we find that while internal migration increases in affected villages, it cannot entirely compensate for the lack of opportunities abroad. This also holds for other substitution effects such as male emigration or choosing alternative destination countries. One potential explanation lies in villages' heterogeneous degree of dependency on international emigration: we find that villages with ex-ante higher population shares working abroad are more heavily affected by disasters once the ban is in place.

We discuss two mechanisms underlying our results. Distinguishing between disaster types, the effect is driven by events of heavy rainfall-induced floods. Villages dependent on rainfed agriculture are particularly susceptible, a sector that has been shown to absorb many workers who could no longer migrate to Saudi Arabia due to the ban (Makovec et al., 2018). Exploring this labor market adjustment channel, we show that poverty increases most drastically in villages with economies geared towards rainfed rice production, Indonesia's most important staple and major source of employment. Furthermore, we identify remittances as a key mechanism for two reasons: these financial flows accounted for a significant share of GDP and historically, migrants in Saudi Arabia remitted on average more than those living in other countries.

The results point towards the importance of migration and remittances in reducing disaster-induced income shocks. In a scenario where international migration is regulated but not completely restricted, affected households can decide to move abroad to cope with natural disasters. At the same time, individuals can diversify their climate-induced income shock risks through ex-ante migration decisions (Kleemans, 2015), expecting to smooth consumption by means of remittances (Blumenstock et al., 2016; Gröger and Zylberberg, 2016; Yang and Choi, 2007). Related to our paper, Mbaye and Drabo (2017) show that migration and remittances reduce poverty rates particularly in disaster affected countries. Our study differs from previous publications in that international migration is heavily restricted. However, we reach similarly consequential conclusions: the drastic reduction of emigration opportunities and corresponding remittances makes communities more vulnerable to natural calamities.

This paper expands the empirical evidence on the nexus between climatic events and international migration. Contrary to internal migration, where the majority of studies find it to be a common reaction to natural disasters, evidence of the impact of climatic shocks on international migration is mixed: some studies show a positive link (Backhaus et al., 2015; Coniglio and Pesce, 2015; Drabo and Mbaye, 2015; Gray and Mueller, 2012; Mahajan and Yang, 2020) while others find no association (Beine and Parsons, 2015; Gröschl and Steinwachs, 2017) or heterogeneous links with respect to income (Bertoli et al., 2020; Cai et al., 2016; Cattaneo and Peri, 2016; Martínez Flores et al., 2021). One important difficulty in addressing this question is to empirically establish a causal link: although extreme climatic events are exogenous, omitted biased responses could be correlated with both migration and natural disasters. We overcome these concerns by exploiting a national policy shock affecting only international migration, introduced with the purpose of protecting Indonesian domestic workers abroad and thus plausibly uncorrelated with local village characteristics. In addition, the ban unilaterally affects one important destination country but leaves others unaffected, creating a natural control group.

Finally, this paper relates to the scant studies on the effect of restrictive migration policies on development outcomes at origin. Theoharides (2020) exploits a migration ban from Japan on Filipino migrants, finding that the policy decreased income in sending communities. More closely to our paper, Makovec et al. (2018) study the effect of the Indonesian ban to Saudi Arabia on labour market outcomes at origin. The authors find no effect of the ban on unemployment, but rather a shift towards the agricultural and informal sector. While we exploit the same policy shock, our study focuses on a notably different impact using more granular data: it investigates if this restrictive emigration policy deprives villages' capacity to absorb income shocks induced by natural disasters using a triple difference approach. In doing so, we reconcile and identify their result as one of our mechanisms. Poverty increases in villages that can no longer rely on migration to Saudi Arabia *and* lose the capacity to absorb workers in the agricultural sector due to natural disasters, particularly extreme floods.

The paper is organized in the following order: Section 2 outlines the context of our study, followed by a description of data used and empirical strategy implemented in Section 3 and 4. Results and extensive robustness checks are discussed in Section 5. Lastly, Section 6 concludes.

2 The Indonesian context

2.1 Natural disasters

Due to its unique location as an archipelago located around the equator, in Indonesia climate change-induced disasters prevail along nature-borne risks induced by earthquakes and volcanic activity. Consequently, the World Risk Report ranked Indonesia 38th out of 181 countries worldwide (Aleksandrova et al., 2021). According to global disaster database EM-DAT, the most common mass disasters ever since 1999 have been floods, earthquakes, landslides and volcanic activity (Guha-Sapir et al., 2021). Whereas nature-borne events are difficult to predict and tend to be constant, climate change-induced disaster such as prolonged periods of drought or rain-induced inundations recorded by the Indonesian government are on the rise since 2000 (BNBP, 2020). Due to the scope of disaster types, this leaves virtually all regions of the country affected. Village-census *Podes* in 2005 indicates that more than 40% of villages have been affected by at least one disaster event over the previous three years, illustrated in Figure A1.

Among other impacts, climate change-related calamities such as floods, droughts and heat waves can adversely affect crop yields. In an recent overview, the IPCC outlines that on the current climate trajectory, large parts of crop lands may become barren over the next decades (Shukla et al., 2019). Indonesia is no exception, where studies found that costs induced by climate change amounted to 1.4% of GDP in 2016 already, the majority of which induced by agricultural productivity losses (Hecht, 2016). As one of the world's largest producers, in Indonesia this particularly holds for water-intensive rice, the country's major staple (Connor et al., 2021; Naylor et al., 2007). Sufficient rainfall is hence a key determinant of contemporaneous productivity as shown in Levine and Yang (2014), but increasing events of torrential downpours in the country also pose the risks of inundations, floods and landslides.

With smallholder subsistence farmers constituting the lion's share of production, the increasing susceptibility of rice to climatic changes significantly increases the risk of poverty. For Indonesia, Maccini and Yang (2009) show how birth-year precipitation positively affects adults' socioeconomic outcomes and Caruso et al. (2016) provide evidence that decreasing rice productivity induces violence. In consequence, a common coping strategy involves migrating away from rural areas. Studies show that one of the major migratory push factors can be climate change, either directly or indirectly through the loss of livelihoods [2]. For example, it is estimated that the 2004 Indian ocean tsunami alone left 500 thousand Indonesians internally displaced Gray et al. (2014).

2.2 International migration

Migration both within and outside Indonesia's borders has always played a vital role in shaping the country's development. According to estimates of the World Bank (2017), around 9 million individuals, corresponding to almost 7% of the country's

[2] See for example Flavell et al. (2020) for a recent literature review or Thiede and Gray (2017) and Bohra-Mishra et al. (2014) for an analysis in the Indonesian context.

labor force, were employed abroad in 2016. Most legal migrants leave through formal migration intermediaries and stay abroad for around 2 to 3 years (Bazzi et al., 2021). Historically, the main destination countries of Indonesian migrants have been Malaysia, Saudi Arabia, Singapore and Hong Kong as depicted in Figure A2.

Among the defining characteristics of the country's international migration patterns is considerable heterogeneity in villages' main destination countries. Bazzi (2012) argues that villages' strong migration ties with certain countries like Saudi Arabia are deeply rooted in their ethnic composition and hence tend to be sticky over time. For example, overseas workers from villages with a higher share of households of ethnic Arab origin have a higher propensity to emigrate to Arab countries as compared to typical destinations in South-East Asia. This highlights the importance of networks in the choice of migratory destination countries (McKenzie and Rapoport, 2010).

Another distinctive aspect are pronounced gender patterns as displayed in Figure A2. Indonesia is one of the few countries in the world that exhibits a higher international migration rate of women vs. men. The share of documented female emigrants increased from 56% in 1996 to 78% in 2004, a phenomenon generally attributed to a rapid increase in the demand for domestic workers in the Middle East (IOM, 2010). These countries mainly attract female unskilled workers due to lower educational requirements as compared to other destinations such as Singapore and Hong Kong, presupposing at least completed secondary schooling (World Bank, 2017).

Around 72% of Indonesian emigrants came from rural areas (World Bank, 2017). Individuals from these areas are also more vulnerable to agriculture-related income shocks that affect migration decisions. Since Indonesia experienced highly unequal economic growth across rural and urban areas, many low-skilled and informal workers see international migration as an essential element of their livelihood strategy and an entry point to formal work. Indonesian women working abroad earn on average five times more than those who stay (Bazzi et al., 2021). In addition, migration increases their probability of having a formal work contract upon return (World Bank, 2017).

The migration spell can also positively affect the income of household members at home through remittances. Cuecuecha and Adams Jr. (2016) find that Indonesian households receiving remittances exhibit lower levels of poverty compared to those without. According to a survey on migration and remittances in Indonesia, the amount sent significantly differs by destination country: migrants living in Saudi Arabia tended to remit more in 2005, despite earning on average less than workers to other destinations (Bank Indonesia, 2009).

2.3 The moratorium: Indonesia's migration ban

With the number of domestic workers in the Middle East increasing, reported abuses and harassment of Indonesian women faced by their employers rose as well. This triggered political unrest in Indonesia, peaking in June 2011 when after repeated abuses Ruyati Binti Sapubi, an Indonesian domestic worker in Saudi Arabia, murdered her employer in an attempt of self-defence. For this reason, she was sentenced to death by beheading (The Washington Post, 2011).The event caused a public outcry, provoking the Indonesian government to step in and abruptly emit a moratorium, banning all women to emigrate to Saudi Arabia as domestic workers until today. Other Middle Eastern countries have gradually been affected by similar bans to protect Indonesian female workers later on [3].

Figure A3 shows how the ban reflected in Indonesia's emigration flows of different destinations. It indicates that Saudi Arabia was one of the main destination countries in 2006, accounting for 43% of migrants. However, after the ban the share had decreased to 11% in 2014. This sharp drop is driven by differentials in migrants' gender which are particularly pronounced for Saudi Arabia. In Figure A2 the stock of migrants for each main destination country by gender is shown. Saudi Arabia is predominantly a destination for women (84%) whose majority was employed as domestic workers.

While female migrants across the entire country were affected by the ban, heterogeneities in ethnic compositions and hence migration ties meant that villages were affected to highly varying degrees. At the same time, these structural relationships implied that immediate substitution to other countries as a response was unlikely, particularly given that the ban was gradually extended to similar destination countries. This also reflects in Figure A3, showing virtually no emigration increase to other countries in the aftermath of the ban. More practical considerations also hindered substituting towards illegal emigration to Saudi Arabia: the mere distance between the countries prevented the establishment of informal seaway routes common to undocumented emigration to Malaysia (World Bank, 2017), in line with the negative effect of distance to destination on illegal routes on migration intentions (Friebel et al., 2018).

[3] Only Kuwait and Jordan already saw a comparable policy in 2009/10, but both countries only play a negligible role in Indonesia's emigration as shown in Figure A2.

3 Data

3.1 Village census Podes

We compile a highly granular dataset of Indonesian villages from four waves of the administrative census *Podes* (*Potensi Desa*). It is collected every 3 to 4 years and includes information on village characteristics for the entire country as stated by their heads and administrators [4]. Note that the year a given round is published includes data corresponding to the previous year, that is for example *Podes* 2011 hence contains data prior to the moratorium introduced in 2011.

Podes contains, among others, detailed information on the stock of international out-migrants disaggregated by gender, natural disaster events and socio-economic variables. Across all waves, 2005 was the first census-year that collected information about the stocks of international emigrants per village. Furthermore, it is the only one to provide information on the main migratory destination country by village, which we use to identify those with strong migration links to Saudi Arabia [5]. We further use information from *Podes* on the occurrence of natural disasters, categorized by disaster type and exact timing over the course of the three previous years [6]. Further variables taken from *Podes* include village population, information on the incidence of social conflict, rural status and agricultural activities, all listed in Table A1.

Our main outcome of interest is poverty.We employ the information about the number of issued poverty letters (*SKTM*) in the previous year, a measure used by literature in the Indonesian context (Krishna and Kubitza, 2021; Morgans et al., 2018). *SKTM* are letters provided by the village leader (*kelurahan)*, stating that the individual is poor and therefore eligible for public assistance including free access to medical treatment, preference in scholarship requests and basic food assistance, among others (Fiarni et al., 2013). Eligibility criteria are based on the absolute poverty definition of individuals falling behind the poverty line as established by the Indonesian Statistical Office (BPS). The 14 criteria constituting the poverty line are listed in Appendix A and mainly based on household and their dwelling characteristics. Given that cards are issued by the village administrators,

[4] Administrative units from most to least aggregated in Indonesia are: province (*provinsi*), district (*kabupaten and kota*), sub-district (*kecamatan*) and village (*desa*).

[5] A limitation is that *Podes* does not provide a clear threshold to define a "main" destination, such as 50% of movers to a specific country or above. Our empirical specification will target any potential endogenous misreporting and reduce it to random measurement error: we always include village fixed and the group of villages that indicate Saudi Arabia as a main destination × year fixed effect.

[6] Events recorded are of relevance for the village by definition of the question: "Natural disaster in the last three years that caused important damages and losses".

the criteria can be porous due to different interpretations (Fiarni et al., 2013). We will address this issue using multiple fixed effects and two alternative poverty measurements derived from the census rounds: the number of households living in slums as well as the number of people receiving assistance for public health services.

3.2 Additional sources

Beyond *Podes*, we employ several other data sources that complement our analysis and allow for extensive robustness checks. For the variable on disaster, as an alternative to self-reported events in *Podes* we also use weather station data from the Indonesian Meteorological, Climatological and Geophysical agency (BMKG). It provides information on stations' precise coordinates and the date of extreme weather events in terms of temperatures, precipitation and wind speeds recorded.

Nationwide, time-variant data on village-level poverty is hardly attainable beyond *Podes* for lack of representative survey information that common measurements are based on. Records provided by Smeru Research Institute are the exception, with poverty maps based on small area poverty estimation methods available for the universe of Indonesian villages in 2010 and 2015. Combining governmental statistics data from different realms and administrative levels, their approach incorporates both household consumption data and village characteristics to calculate village-level estimates. We can thereby verify our main results with further measurements, described in more detail in Suhayo et al. (2005).

3.3 Resulting dataset

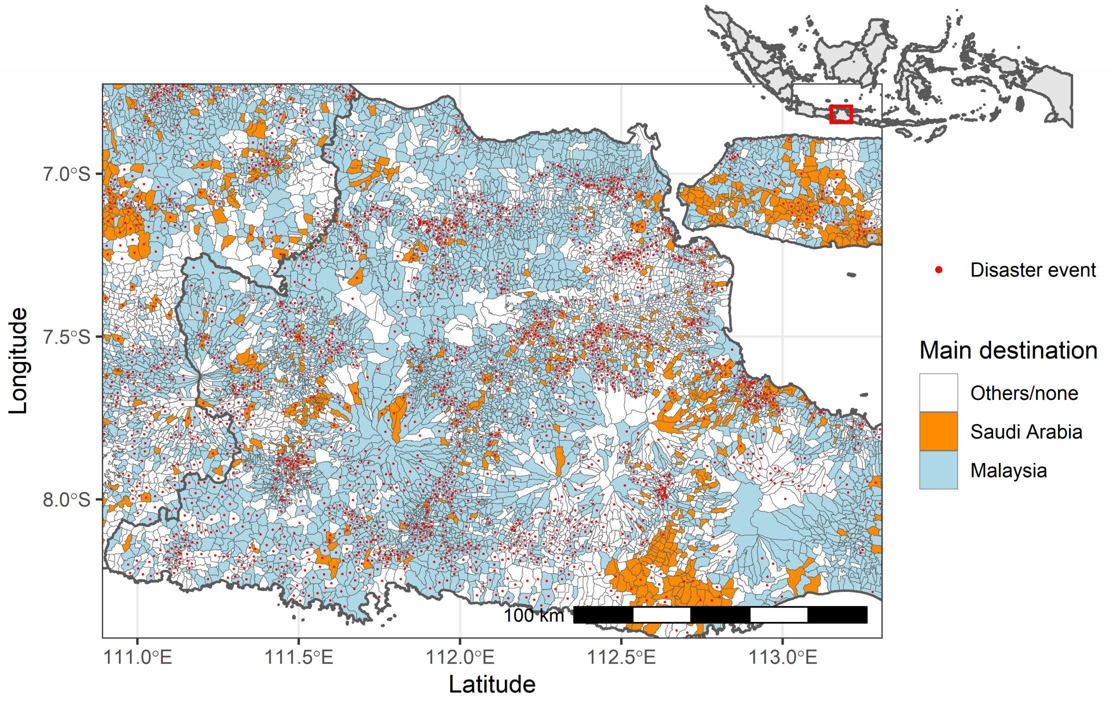
We combine all data sources at the village level, Indonesia's lowest administrative unit. Official administrative IDs provided by BPS available throughout all our sources facilitate this process for same-year data matching. To account for changing IDs across time caused by administrative splits, we link different years by means of a village crosswalk [7]. This allows us to aggregate villages to their 2005 boundaries, our unit of observation. The resulting dataset hence contains $N = 67,987$ villages over the years 2005, 2008, 2011 and 2014 for a total of $N = 268,194$ observations. Table A1 describes basic summary statistics for all variables used in our analyses.

Figure 2 zooms into the province of West Java for illustrative purposes. It shows that the majority of villages in this region had migration links to Malaysia and, to a lower degree, to Saudi Arabia. At the same time, with Java being extremely prone to natural calamities, many villages experienced at least one natural disasters in the three previous years. We will explore the spatial variation of natural disasters

[7] For a detailed explanation, see Appendix B

and heterogeneous migration links to different destination countries throughout our paper.

Figure (2) Main destination country and natural disasters in villages in East Java



Notes: Red dots represent centroids of villages that experienced at least one natural disaster between 2003 and 2005. Main destination refers to the country where most emigrants worked as of 2005. Source: Own computation based on *Podes* 2005.

4 Empirical strategy

4.1 Baseline

To investigate whether international migration can mitigate disaster-induced income shocks, we first establish the disaster-migration nexus in our data by relying on the village panel described above, estimating Equation 1:

$$Poverty_{vt} = \beta_1 D_{vt} + \beta_2 Migrants_{vt} + \beta_3 D_{vt} \times Migrants_{vt} + \lambda X_{vt} + \delta_t + \gamma_v + \eta_{p*t} + \varepsilon_{vt} \tag{1}$$

$Poverty_{vt}$ stands for the number of new poverty cards issued in village v in year t = 2005, 2008, 2011 and 2014. $Migrants_{vt}$ measures the stock of emigrants from village v in year t [8]. D_{vt} is a binary variable indicating whether village v has experienced at least one natural hazard-induced disaster in the previous three years. X_{vt} controls for time-variant variables: log of population and a dummy for social conflict in the previous year. We include several fixed effects: time (δ_t) capture macroeconomic shocks common to the entire country, village (γ_v) control for time-invariant observable and unobservable characteristics such as soil suitability, propensity to be subject to natural disasters, cultural proximity with a specific destination country and established migration networks. Lastly, province-year time (η_{p*t}) capture province-specific time trends. Standard errors are clustered at the village level.

The coefficient of interest β_3 captures the differential effect of a natural disaster event on villages with varying levels of migration. A negative sign would suggest that an increase in migration mitigates the effect of natural disasters on poverty, and vice versa. Although the timing of natural calamities can be assumed to be exogenous, omitted responses that are both correlated with the probability to migrate and households and hence villages' incomes in the aftermath of these events might prevail. Furthermore, Equation 1 is only suggestive of potential smoothing effects of migration, but does not provide any evidence on the effects of increasing barriers to emigrate.

Exploiting the migration ban to Saudi Arabia as a natural experiment allows us to address these concerns. Once introduced in June 2011, it prevented all Indonesian women to migrate to Saudi Arabia as domestic workers. In absence of destination records in *Podes* 2008, 2011 and 2014, we estimate an intention-to-treat (ITT) effect according to initial migration networks. Villages with strong migration links to Saudi Arabia in 2005 are more likely to experience a reduction in the number of out-migrants due to the moratorium. To verify the impact of the ban we therefore first estimate the following event-study model:

$$Migrants_{vt} = \beta_1 SA_v + \beta_2 Year_t + \beta_3 (SA_v \times Year_t) + \lambda X_{vt} + \gamma_v + \eta_{p*t} + \epsilon_{vt} \quad (2)$$

Where SA_v is a binary variable indicating whether Saudi Arabia is a given village's main migratory destination country in 2005. β_3 hence captures the yearly percentage change of migration stocks in villages with Saudi Arabia as the main destination country in 2005 against all other villages. Fixing the base year to 2011, we expect a decrease in migration stocks after 2011 in the treated villages relative

[8] Both variables are transformed by the inverse hyperbolic sine to account for villages with zero migrants or zero issued poverty cards.

to the control group. We include the same set of control variables and fixed effects described for Equation 1.

4.2 Identification

4.2.1 Triple difference

An effective migration restriction policy would imply that villages with migration networks to Saudi Arabia experienced a larger reduction of out-migrants. Therefore, it is conceivable that these villages are more vulnerable to natural disaster-induced income shocks. To test this hypothesis we thus run the following triple difference regression, our main specification of interest:

$$\begin{aligned} Poverty_{vt} = {} & \beta_1 D_{vt} + \beta_2 SA_v + \beta_3 Post2011_t + \\ & \beta_4 (D_{vt} \times SA_v) + \beta_5 (D_{vt} \times Post2011_t) + \beta_6 (SA_v \times Post2011_t) + \\ & \beta_7 (D_{vt} \times SA_v \times Post2011_t) + \lambda X_{vt} + \delta_t + \gamma_v + \eta_{p*t} + \epsilon_{vt} \end{aligned} \tag{3}$$

Where the variables follow the same denominations described above. $Post2011_t$ takes the value one if t is 2014 and zero otherwise. 2011 falls within the pre-treatment period as it covers previous years. As controls, X_{vt} includes the inverse hyperbolic sine of the stock of male emigrants, log of population and a binary variable for conflict events. Again, we add fixed effects for year (δ_t), village (γ_v) and province-year time trends (η_{p*t}). Robust standard errors are clustered at the village level.

As in standard double difference models, the interactions of all three binary variables are included to partial out confounding trends. $D_{vt} \times SA_v$ eliminates time-invariant heterogeneous responses to disasters in villages with Saudi Arabia as the main destination country, and $D_{vt} \times Post2011_t$ captures natural disaster trends that could spuriously affect the dependent variable after the ban. The interaction $SA_v \times Post2011_t$ is essential to control for all observable and unobservable aspects that are affected by the moratorium and could influence poverty, independent of being exposed to disasters. For example, these include direct wealth shocks due to foregone remittances and expected income from migrating as well as common changes in the population compositions due to altered migration patterns. It also captures differential labour market responses to the ban as identified by Makovec et al. (2018). In line with literature finding that emigration exerts upward pressure on local wages at origin (Amuedo-Dorantes and Pozo, 2006; Aydemir and Borjas, 2007; Elsner, 2013; Hanson, 2007; Mishra, 2007), the increase in unskilled female labor supply (those not able to emigrate) conversely could drive wages down, thus potentially increasing poverty.

Finally, the inclusion of the stock of international male migrants as control variable is crucial: some villages could substitute the outflow of female domestic workers to Saudi Arabia with emigration to countries and sectors dominated by men such as construction work in Malaysia.

4.2.2 Causal Interpretation

The identification derives from the triple interaction $D_{vt} \times SA_v \times Post2011_t$. This term allows us to causally identify the effect of natural calamities in villages that could no longer rely on international migration to their main destination country as adaptation strategy.

One potential threat to the identification stems from any potential anticipation effects of the ban. Prior information about its enactment for would-be migrants could alter their migration decisions: they could either anticipate the departure or directly refrain from migrating. At the same time, village heads could issue more poverty letters in anticipation to cope with the foregone income from remittances. These scenarios assume that village heads and individuals possessed prior information on the national government's move to implement a ban. Even if this was true, the interaction term $SA_v \times Post2011_t$ would control for this type of bias common to all villages with ties to Saudi Arabia. The only residual variation derives from natural disasters, quasi-random events once geographic disaster propensity is controlled for by village fixed effects.

Nevertheless, village heads could still over-report disaster events and issue more poverty cards to receive higher government transfers. To upwards bias our results, this would have to systematically happen in villages with links to Saudi Arabia hit by disasters after 2011. To rule out these hypothetical scenarios,we adopt multiple strategies. First of all, we show that the main effect is robust to controlling for the inflow of different transfer types: from local governments (province and district), the central government, foreign and private citizen aid. Secondly, we use two alternative *Podes*-based variable to proxy poverty: the number of social health insurance cards (*Askeskin*) issued in year t-1, which Sparrow et al. (2013) find to be well targeted to the poorest and most vulnerable individuals; and the number of households living in slums [9]. Lastly, we use poverty data external to *Podes*, estimated at the village level by SMERU. We define poverty rate as the share of individuals falling below the poverty line according to the international convention of individuals who gain

[9]In 2016, about 29 millions Indonesians lived in slums with poor basic services, many of them lacking access to sanitation and to safe water (World Bank, 2016).

less than 2$ per day, adjusted to the purchasing power parity.

Self-reporting can analogously affect our measurement of natural disasters, hence we also use extreme weather events from weather stations in Indonesia, provided by BMKG. Results for all alternative data sources discussed above are shown in Section 5.5.

5 Results

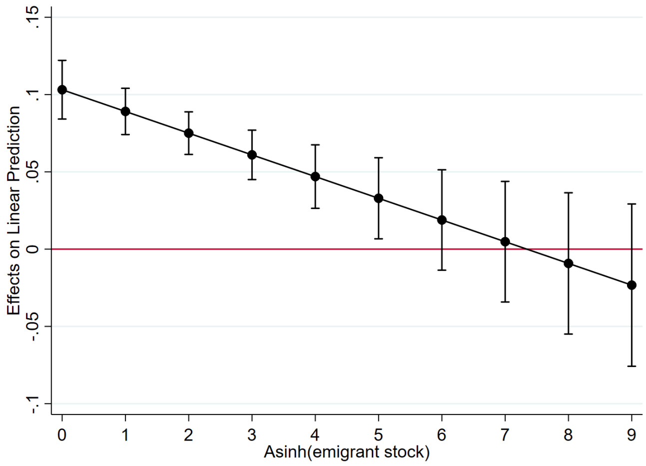
5.1 International migration as an adaptation strategy

We start our analysis by examining the well-established nexus between poverty and natural disasters for our setting as outlined in Equation 1. Table A2 reports the estimated coefficients on the effect of natural disaster, migration and its interaction on poverty, i.e. the extent to which international migration influences the relationship between natural disasters and poverty. In columns (1) and (2) we use the entire stock of migrants, columns (3) and (4) are subset to female migrants only. For either specification, the estimate on natural disasters is positive, thereby replicating established results in the literature (IPCC, 2018). Nevertheless, this effect is mediated with an increase in migration. Marginal effects of the estimation in column (4) are visualised in Figure 3. At zero levels of stock of migrants, natural disasters led to a hike in the number of issued poverty cards by 10%. With an increase in migration, this effect is reduced until turning insignificantly different from 0 in villages with a stock of migrants larger than asinh(6) ($\approx$ 200 migrants, corresponding to around 6% of average village population).

To causally estimate the mitigating effect of migration on disaster-induced income shocks, we exploit the moratorium on female emigrants to Saudi Arabia in 2011. Here, our analysis rests on the assumption that the ban was effective, therefore we first want to quantify its effect with our data. Figure 4 plots the coefficients of the interaction $SA_v \times Year_t$ from Equation 2 in an event-study setting. Using 2011 as the reference year, it shows that the impact of the ban on mobility is considerable. With respect to 2011, the stock of migrants dropped by 30% in 2014 for villages that had Saudi Arabia as main migratory destination country in 2005.

Figure 4 also shows that the gap in the stock of female migrants had been reducing over time. In a counterfactual scenario where the stock of female migrants to Saudi Arabia would have grown at the same linear rate as in the period 2005-2011, in 2014 we could have expected a larger stock of female migrants than the comparison group. However, estimates show a sharp u-turn in this trend: with respect to this hypothetical scenario, the drop in female stocks of migrants even

Figure (3) Average marginal effects of natural disaster events on poverty cards



Notes: Coefficients capture the marginal effects of disasters on poverty cards with an increase in the stock of emigrants with 95% confidence intervals. Control variables include log(population) and a conflict event dummy. Village fixed effects, time fixed effects and province time trends are included. Standard errors are clustered at the village level. Number of observations: 268,194.

amounts to 38% as indicated by the grey x in 2014. These results prove that our approach using Saudi Arabian migration ties in 2005 successfully singles out villages most affected by the moratorium [10].

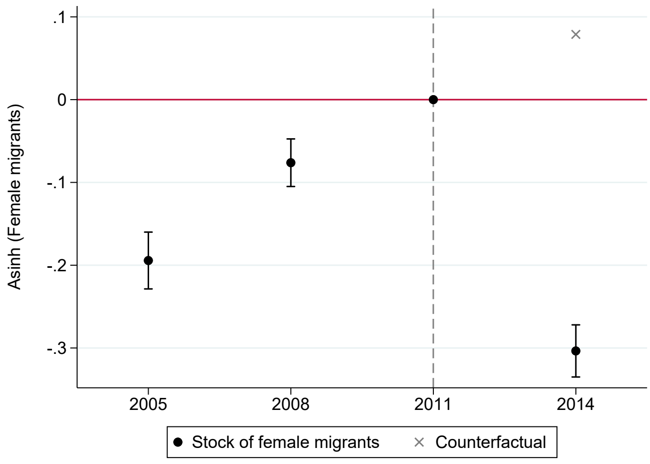
5.2 Disasters and migration under the ban - triple difference

A stronger impact of the moratorium on emigration in villages with strong ties to Saudi Arabia suggests that these locations could be more vulnerable to climatic shocks. Here, our main specification comes in, with Table 1 presenting the result of the DDD model testing this hypothesis. Columns (1) to (3) display the estimations of simple difference-in-difference (DD) models, where each double interaction is presented in a separate regression. Across all interactions in the first three columns, only the double difference coefficient $SA_v \times Post2011_t$ in column (1) is statistically significant. A deterioration in labour markets or potential overall decrease in remittances might underlie these results. Across all columns, the coefficient on disasters is statistically significant at 1%, implying that the number of poverty cards increased by 8.5% to 9.3% in villages hit by natural disasters.

In our main specification shown in column (4), the interaction term $SA_v \times Disaster_{vt}$

[10] Significant and negative estimates for the years leading up to 2011 reflect the growing importance of Saudi Arabia as destination country, which we argue would have further increased had the ban not been implemented. This does not affect the causal interpretation of our results, as in section 5.5 we show the that the parallel trends assumption holds for our baseline model.

Figure (4) The impact of the moratorium: Change in female migrant stocks in villages with Saudi Arabia as main destination country against others



Notes: Coefficients capture β_3 of the event study in Equation 2, i.e. the relative decrease in the inverse hyperbolic sine of female migrants' stocks for villages with Saudi Arabia as main destination country in 2005 vs. others, with 95% confidence intervals. The vertical dotted line indicates the implementation of the ban in 2011, which is also the baseline period. "x" indicates the value of female stocks in the counterfactual scenario where the it would have followed the linear trend of the period 2005-2011. The sample is restricted to villages that indicate they have at least one Indonesian domestic worker abroad in 2005. Control variables include log(population), a conflict event dummy, Village and time fixed effects and province time trends are included. Standard errors are clustered at the village level. Number of observations: 141,107.

is significant with a negative sign. It implies that historically, villages with migration networks to Saudi Arabia tended to cope better with natural disasters. A potential explanation is that migrants in Saudi Arabia on average remitted more than Indonesians working in other destination countries Bank Indonesia (2009). To a lesser degree, this also holds for the interaction on $Post2011_t \times Disaster_{vt}$, suggesting that on average all villages tend to cope better with disasters in 2014 than before. A conceivable explanation is that disaster prevention systems have improved over time. Lastly, the interaction $SA_v \times Post2011_t$ is no longer significant, hence the triple interaction almost entirely explains its coefficient shown in column (1). The triple interaction is positive and significant at 1%, with a coefficient of 11.8%. Results are also qualitatively similar in case we consider the number of natural disasters experienced in village as reported in Table A3 [11]. To interpret the effect of the triple interaction in column (4), we compute marginal effects: villages with migration links to Saudi Arabia hit by natural disasters experienced a poverty increase by 13% after 2011.

[11] In this analysis, we restrict the sample to 2008, 2011 and 2014 for lack of disaster data. Results for the extensive disaster margin are robust to using this subsample as shown in Table A3.

Table (1) Average effect of being subject to disasters on poverty

Dependent	Poverty cards			
	DD			DDD
	(1)	(2)	(3)	(4)
Disaster=1		0.085***	0.086***	0.093***
		(0.008)	(0.008)	(0.009)
SA=1 × Post2011=1	0.062***			0.015
	(0.020)			(0.027)
Post2011=1 × Disaster=1		-0.011		-0.028*
		(0.014)		(0.015)
SA=1 × Disaster=1			-0.025	-0.055**
			(0.020)	(0.023)
SA=1 × Post2011=1 × Disaster=1				0.118***
				(0.039)
Village FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	268,194	268,194	268,194	268,194

Notes. Poverty cards is transformed by the inverse asymptotic sine (asinh). Control variables include asinh(male migrants), log(population) and a conflict event dummy. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

5.3 Substitution effects

5.3.1 Internal migration

With migration restrictions in place, the most conceivable coping strategy of affected individuals is substitution to internal migration: they could decide to move within Indonesia instead of Saudi Arabia as a reaction to natural disasters. In case this new internal migrants' composition was comparable to those who would have migrated had the ban not been implemented, the substitution would only lead to a downwards bias. More specifically, our main results in the DDD model imply that natural disasters increase poverty by 13% in villages with propensity to send workers to Saudi Arabia. If these villagers had now migrated nationally instead, it would reduce the effect of disasters on poverty, implying that the effect of 13% is a lower bound estimate.

However, a possible threat to our identification would arise if the ban affected the selection of migrant profiles into internal migration. If the majority of those who could afford emigrating to a different village do so in the aftermath of natural disasters, the composition of stayers would be more skewed towards poorer individuals, biasing the coefficient upwards. It is difficult to imagine a scenario in which this bias arises only after 2011 and specifically for villages with Saudi Arabia as main destination country. In addition, our measure of issued poverty cards captures the change in the absolute number of poverty cards emitted, or the "new poor" households, partially overcoming changes in composition. Nevertheless, in Table A8 we still test this possibility. In absence of information about internal migration from *Podes*, we proxy it as the change in population. Column (1) shows that that the overall population of villages concerned by the triple interaction drops by 1.3%. The coefficient hints to a potential substitution from international to internal migration. However, once we further interact the DDD coefficient with the change in population, we do not find differential effects on poverty as shown in column (2). It implies that villages in the treated group with higher levels of domestic outmigration, where population growth is negative, do not show differential poverty rates as compared to the control group [12].

5.3.2 Male migration and other countries

The migration ban to Saudi Arabia in 2011 only affected would-be female domestic workers while male migrants were not directly targeted by the moratorium. Therefore male individuals could still decide to move to Saudi Arabia or any other country to overcome income losses from natural disasters and from the lack of female migration to the Gulf country. Moreover, females seeking to work abroad as domestic workers could also opt for any other destination.

To tackle the potential bias from the substitution to male migration, we include the stock of male migrants in all our regressions. Nonetheless, we directly test the potential substitution to male movers in Figure A6. It shows that the stock of male migrants in villages with links to Saudi Arabia follows a similar trend as female migration: the gap to villages with other main destination reduces until 2011, then it slightly widens by 8.3% in 2014. If anything, male migration appears complementary to rather than a substitute for female migration in these villages. A potential explanation could be the fact that the Saudi Arabian government extended a visa ban to all Indonesian workers in June 2011 as a retaliation against the moratorium (BBC, 2011).

Due to data limitations, we are unable to test substitution to other destination countries. However, we believe the magnitude of this readjustment to be limited in the short-run given different educational requirements for Indonesian domestic workers across countries. Saudi Arabia required foreign maids to only have primary

[12]The DDD coefficients in column (2) is insignificant. However, the overall marginal effect of natural disaster for villages with Saudi Arabia as main destination, after 2011, is 12.76% and significant at 1% (not shown).

education, whereas other important destinations such as Taiwan, Hong Kong and South Korea required a minimum of completed secondary education. In any case, all other adaption strategies would reduce income losses from natural disasters and thus imply that our results are a lower bound estimate of the effect.

5.4 Dependency on international and internal migration

We further investigate heterogeneous substitution dynamics from international to national migration by splitting the sample into terciles of initial international emigration rates to measure communities' historical propensity to rely on work overseas [13]. We first check if there is a heterogeneous effect of natural disasters after the ban by each tercile of initial international emigration rate. Secondly, we investigate the potential substitution to internal migration for each sub-sample. Coefficients in Table 2 point towards heterogeneous effects of the triple difference: villages that historically relied more on international migration, i.e. with a relatively higher pre-ban international emigration rate, are those most affected by natural disasters after 2011. The decrease in the population of stayers (i.e. an increase in out-migration to other villages) appears to be driven by the second ex-ante emigration rate tercile.

Our results indicate that communities more reliant on international migration might have over-invested in a riskier adaptation strategy to income shocks from natural disasters. Being reliant on international migration can make it more difficult to switch to alternatives such as moving elsewhere in Indonesia, potentially explaining the results in columns (5) and (6). Furthermore, nine out of ten villages in the sample are rural and therefore more dependent on agriculture, likely to send international migrants (Bazzi, 2017) and vulnerable to disaster-induced income shocks.

Villages ex-ante not strongly reliant on international migration neither show significantly different levels of poverty once hit by natural disasters (column (1)) not exhibit significantly different levels of internal migration (column (2)). As more urban villages (33%, well above the mean of 18% in the entire sample), they are more resilient to climatic shocks and thereby less reliant on internal and international migration as a coping strategy [14].

Finally, villages in the middle tercile are those that experience the largest rise in internal out-migration and no different level in poverty (columns (3) and (4)). The latter hints towards households still being relatively more mobile than those in the

[13] Initial international emigration rate is defined as the the stock of international emigrants divided by the population, averaged for the years leading up to the ban in 2011 (2005 and 2008).

[14] We estimate that urban villages issue 4.68% fewer poverty cards than rural ones if hit by natural disasters.

first tercile: they could more easily switch to internal migration to cope with the income shocks from disasters. At the same time, they potentially did not overshoot investment into international migration compared to households living in villages in the the third tercile.

Table (2) Average effect of disasters on poverty or internal migration by terciles of initial international emigration rate (ER)

	Low initial ER		Middle initial ER		High initial ER	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent:	Poverty cards	Internal migrants	Poverty cards	Internal migrants	Poverty cards	Internal migrants
SA × Post2011 × Disaster	0.091	0.000	0.106	-0.023**	0.113*	-0.003
	(0.072)	(0.011)	(0.076)	(0.009)	(0.066)	(0.009)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	56,884	56,884	56,884	56,884	56,884	56,884
Share of rural villages	0.67	0.67	0.81	0.81	0.89	0.89

Notes. Initial emigration rate (ER) is defined as the the stock of international emigrants divided by the population, averaged for 2005 and 2008. We exclude villages with zero stock of emigrants in years 2005 and 2008. The dependent variable is the inverse hyperbolic sine (asinh) in in columns (1), (3) and (5); log(population-international stock of migrants) in columns (2), (4) and (6). Control variables include asinh(male migrants), log(population) and a conflict event dummy in column (1), (3) and (5); and only conflict in columns (2), (4) and (6). All further interactions are included in the estimation but not displayed here. Robust standard errors are clustered at the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

5.5 Robustness checks

5.5.1 Alternative measurements

We run a series of robustness checks to verify our results, starting with alternative poverty measurements in Table A4. Columns (1) and (2) show the result for our main model, using the number of issued social health cards and households living in slums as dependent variables. Albeit qualitatively different to poverty cards, both measurements reflect dimensions of poverty experienced in the village. Coefficients of the triple interaction are positive and significant for both, indicating that our results are not merely a function of measurement.

This deliberation is further confirmed by results presented in column (3), where poverty is proxied as the number of individuals below the per capita poverty line as defined by the international convention of 2$ PPP [15]. Estimates suggest that villages with migration links to Saudi Arabia once hit by natural disasters experienced a 1.19 percentage point poverty increase after the migration ban. In terms of magnitude, the smaller estimates as compared to the main specification can be explained by the fact that poverty cards measure increments, whereas the figures using poverty rates refer to overall poverty rate [16].

5.5.2 Subsamples and placebos

Table A5 includes local and central government transfers as well as foreign and private citizen aid as controls. In absence of information about these variables in *Podes* 2005, the sample is restricted to the years 2008, 2011 and 2014. Nonetheless, the triple difference coefficient remains unaltered across all columns, suggesting that the larger number of issued poverty cards for treated villages hit by natural disasters after 2011 is not driven by differential financial inflows.

We further adjust our sample in numerous ways to show that results are not driven by the sample choice or outliers in Table A6. First of all, we exclude Java island from the sample in column (1), being the most heavily disaster-prone region. . At the same time, Java hosts half of the Indonesian population and is its economic and political centre. More importantly, it is the island where the majority of Indonesian emigrants originate from. In column (2), we only restrict the analysis to Java. For either analysis, the magnitude of the triple interaction is larger than our main effect and significant on the 5%-level.

In column (3) of Table A6 we exclude villages without migrants in 2005 so that the binary variable SA_v compares villages with Saudi Arabia as main destination only to villages with emigrants in other main destination countries. Lastly, in column (4) we investigate the possibility that although the moratorium was only progressively extended to other important Middle Eastern destination countries, they were already informally affected. For example, this could be due to a more general negative sentiment towards Arab states induced by the 2011 ban or the events leading up to it. Hence we exclude other Middle Eastern countries in this specification, with columns (3) and (4) showing that the results are virtually identical to our baseline regression. Finally, results hold when we trim the sample at the 1^{st} and the 99^{th} percentile of the population (column (5)).

As a placebo test, we replace the binary variable SA_v in Equation 3 by a categorical variable for all destination countries. Figure A4 displays the triple interaction

[15] Note that for data availability reasons described in Section 3, the sample is restricted to the years 2011 and 2014.

[16] *Podes* does not contain information on village poverty card stocks.

coefficients, focusing on the number of issued poverty cards as outcome. The only destination country displaying a positive and significant coefficient compared to the baseline category of no emigrants in 2005 is Saudi Arabia. It is reassuring that the moratorium to Saudi Arabia is the main treatment and only villages with migration links to Saudi Arabia are those that experience higher poverty if hit by natural disasters after the migration ban [17].

5.5.3 Parallel trends

Our identification relies on the exogeneity of natural disasters and of the date of the migration ban. The former are quasi-random events, conditional on village fixed effects. We confirm this by performing a falsification test, regressing the lagged natural disaster dummy taken from previous period on poverty in Table A7. Natural disasters occurred three to six years earlier reassuringly do not have a significant effect on poverty in this period.

Although we argue that the moratorium date is unexpected, villages with migration links to Saudi Arabia could experience different pre-trends in poverty rates. While in theory this should not pose an issue for villages hit by exogenous disasters, it could potentially violate the parallel trend assumptions for villages vulnerable to the ban. Therefore we provide full evidence of the presence of pre-treatment parallel trends, both unconditional and conditional on observable characteristics. As recently highlighted by Olden and Møen (2022), the triple difference estimator does not require two but only one parallel trend assumption, as common biases are partialled out by a first difference. In our context, this means that relative poverty of villages with disasters vs. those without in the treatment group (villages with migration ties to Saudi Arabia) trends similar to relative poverty of villages with disasters vs. those without in the control group (villages with migration ties to other countries) in the absence of treatment (the ban in 2011).

Figure A5, panel A shows that before the ban was introduced in 2011, villages in the treatment and control group had similar trends in the time leading up to the ban. After 2011, both groups experienced an increase in the average number of issued poverty cards. However, this rise is slightly larger for the treated group, in line with our baseline results reported in Table 1. Furthermore, panel B in Figure A5 shows our baseline estimations in an event study, highlighting the absence of pre-treatment trends before the ban was implemented in 2011.

[17] These coefficients are qualitatively similar when excluding villages with no emigrants in 2005 from the sample and using any destination country as a base category.

5.5.4 Spillovers

While villages are stable across space in time, the effects we find are unlikely to be locally bound as disasters and migration restrictions do not halt at borders. Spillovers therefore potentially prevail and could therefore question our identification. For example, our estimates could be biased upwards in case of job seeking displacement in space, leading to decreasing poverty in villages without migratory ties to Saudi Arabia nearby. We approach these deliberations by two robustness checks: the inclusion of additional controls and the adjustment of standard errors.

As proposed by Clarke (2017), we augment our main model with binary variables indicating whether villages were within a certain radius of those with Saudi Arabia as main destination. Table A9 shows that neither our main effect changes nor are the interactions significant for villages of up to 30km distance to our treatment villages (SA=1), pointing to the absence of spillovers within this radius.

We also account for Conley-type spatial correlations of the error term (Conley, 1999) in Table A10 where results stay significant at 5%-level across different distance cut-offs.

5.5.5 Rainfall, floods and Indonesian weather stations

We investigate which type of natural disasters drives our results. Figure A7 displays coefficients of the triple interaction with variable D_{vt} now representing different natural disaster categories experienced by villages. Floods in particular appear to cause significantly higher number of issued poverty cards as compared to not experiencing any natural disaster. This is in line with research showing that floods are also one of the most devastating types in terms of losses and harvest failure (FAO, 2018).

Having established that our baseline results are driven by heavy rain-caused events, we rely on alternative disaster definitions from Indonesian weather stations data to verify our results. At the same time, this allows us to address potential concerns related to reporting bias in *Podes*-coded events. For each of the 170 geocoded weather stations from BMKG operating uninterruptedly between 1990 and 2015, we define extreme rainfall events as the day in which each station records the largest precipitation over the ten previous years. The value one is assigned if extreme rainfall events occurred either between 2003-2005, 2006-2008, 2009-2011 and/or 2012-2014, and zero otherwise [18].

[18] For example, if a given weather station records the day with the largest rainfall between 1995 and 2005 in any day between 2003 and 2005, then the dummy takes the value one. This is repeated for the period 1998-2008, where the variable is one again if the extreme rainfall was recorded for any day in 2006-2008.

Given that we have precise coordinates of the stations, we use different bandwidths of either 10, 15, 20 or 30 km distance to assign villages to their corresponding precipitation records. Figure A8 shows the widespread location of the weather stations used in our analysis and respective buffer zones. For each radius, only villages within the respective buffer are selected. However, as shown in Table A11 our results are stable to the choice of different radii. Across all specifications, the effect of the triple interaction on poverty cards ranges between 25.2% and 63.4%, always significant at 1%-level. Compared to our main results, the larger effect sizes presented here could be explained by the fact that this approach allows us to identify extreme events in a subset of villages.

5.6 Mechanisms

In our main results, we established that curtailed migratory coping strategies in the wake of natural disasters have a diametric effect on poverty. While this in itself already has broad implications, understanding the mechanisms and heterogeneities inherent to this nexus is vital. We approach this analysis by focusing on the immediate income adjustments induced by international migration established in the literature: labour market responses and remittances.

5.6.1 Labor market adjustments

For the same ban analyzed in our study, Makovec et al. (2018) find that while the moratorium did not affect formal unemployment and consumption, it led to an increase in women's employment in the mostly informal agricultural sector. This is in line with the fact that 55% of migrants were employed in that sector prior to moving abroad (Bank Indonesia, 2009).

In Indonesia, rice is the most cultivated and consumed staple, yet it is particularly vulnerable to climate change fueled events. Rice production is located in the mostly agrarian economy in rural areas of the country, where irrigation of fields relies either on rain (*dryland/rainfed*) or man-made schemes (*wetland method*) (Khairulbahri, 2021). Abundant rainfall is positively correlated with rice productivity Levine and Yang (2014), but increasing events of extreme rainfall can cause flash floods and landslides. There appears to be a tipping point from which on additional precipitation decreases not only absolute production by means of diminishing available field size, but also productivity per hectare (Hartono et al., 2020). In fact, Indonesian rice farmers consider floods to pose the greatest threat to their production (Rondhi et al., 2019). Furthermore, the vulnerability to extreme rainfall and floods is larger in lowland/rainfed areas as compared to irrigated fields in topland cultivation (Panda and Barik, 2021).

We find that flood is the type of natural disaster behind the observed poverty increases. Taken together, labor market adjustments towards rice-production in the aftermath of the ban might leave rice-producing villages even more prone to poverty when disasters struck. If floods damage the local crop production, they can limit the capacity of local labour market to absorb the excessive workforce through jobs in agriculture, as it would have occurred otherwise.

In absence of information about direct crop damage estimation across all *Podes* waves, we explore the heterogeneity by type of irrigation in rice production. As outlined, studies show that rainfed rice areas are more vulnerable to extreme climatic events like floods. Thus we estimate a quadruple difference model by interacting all binary variables in Equation 3 with a variable taking the value one if village v mainly cultivates rainfed paddy (15% of villages in our sample) and zero if the it has an irrigation system (85%).

Table 3 displays the results of this regression, where the sample is subset to rural villages that cultivate rice, amounting to 68% of the entire sample. The binary variables on disasters in column (1) include all types of disasters, whereas columns (2) and (3) are coded to capture either floods or any other disasters. Across all columns, the control group is the same and it consists of villages that did not experience any disasters in year t. The impact of any natural calamity on the treated group after 2011 is significant at 5%, indicating 26% more poverty cards emitted in villages with rainfed lowlands as compared to those with irrigated areas (column (1)). Once we restrict the analysis to flood shocks however, we find that for villages with curtailed migratory coping strategies, individuals living in rainfed areas receive 42.2% more poverty cards than those living in villages with irrigated fields, significant at 1%. On the contrary, column (3) shows that this heterogeneity does not prevail if these villages are hit by any another type of natural disaster. We can thereby reconcile and extend Makovec et al. (2018) results: migrants confined to stay in agriculture can no longer adjust their labor market decisions if floods reduces crop yields.

5.6.2 Remittances

Beyond shaping the labor market of sending communities, international migration also influences their available income flows. In fact, sending money to support families at home is one of the key motivation driving people abroad. Restricted emigration therefore affects the amount of remittances received, which in turn influences how receiving communities can smooth consumption when facing natural disasters. There are two reasons why we believe this holds particularly for the set-

Table (3) Average effect of disasters on poverty by type of rice production

Dependent	Poverty cards		
	All disasters	Floods	Other disasters
	(1)	(2)	(3)
Disaster	0.102***		
	(0.011)		
SA × Post2011 × Disaster	0.081		
	(0.061)		
SA × Post2011 × Disaster × Lowlands	0.260**		
	(0.125)		
Flood		0.142***	
		(0.017)	
SA × Post2011 × Flood		0.061	
		(0.078)	
SA × Post2011 × Flood × Lowlands		0.423***	
		(0.149)	
Other disaster			0.077***
			(0.014)
SA × Post2011 × Other disaster			0.099
			(0.081)
SA × Post2011 × Other disaster × Lowlands			0.056
			(0.170)
Village FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	183,337	145,256	147,415

Notes. Poverty cards is transformed by the inverse asymptotic sine (asinh). Control variables include log(population), asinh(male migrants) and a conflict event dummy. All two-way interaction terms are included in the estimation but omitted here. "Flood" and "Other disasters" take the value one in case of a flood or any other disaster than flood occurred within the three previous years, and zero with no disasters. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

ting of our study in Indonesia: first of all, annual remittance inflows amounted to 9 billion USD in 2016, corresponding to 1% of national GDP (World Bank, 2017) and strongly affecting local development (Bal and Palmer, 2020). With Saudi Arabia being one of the main destination countries, the ban and resulting 30% decrease in migratory stocks in affected villages strongly reduced available payment opportunities. Secondly, a 2009 survey showed that Indonesian workers living in Saudi Arabia tended to remit more on average than those in any other main destination (Bank Indonesia, 2009). This points to an even more impactful effect of the moratorium for villages relying on these payments.

Data on remittances is scarce, and to the best of our knowledge no nationally representative survey on migration and remittances collected before and after the moratorium in Indonesia exists. Given this data constraint, we provide a simple back-of-the-envelope calculation combining our results with additional sources. First of all, in Figure 4 we showed that the stock of female migrants in villages with migration link to Saudi Arabia drops by 30.4% in 2014 compared to 2011, or 37.9% in the counterfactual scenario without the migration ban. Secondly, Bank Indonesia (2009) calculated that 95% of Indonesians working abroad transfer money home at least once within the first year of departure. Lastly, Cuecuecha and Adams Jr. (2016) estimate that Indonesian households receiving remittances from abroad have a 27.8% lower probability of being poor than those not receiving any. Assuming that the share of migrants remitting stays constant at 95%, it implies that after 2011 potentially $0.95 \times 0.304 \times 0.278 = 8.02\%$ more households are in poverty because of the lack of remittances. With respect to the counterfactual scenario, the estimate is $0.95 \times 0.379 \times 0.278 = 10\%$. Although one should interpret these simple calculations with caution, it is interesting to notice they are in close range to our main effect of a 13% poverty card increase in affected villages.

6 Conclusion

We investigate whether international migration restrictions affect the capacity of villages to absorb income shocks induced by natural disasters. In Indonesia, a country with long emigration history prone to climatic changes, a ban preventing all women to emigrate to Saudi Arabia as domestic workers was abruptly implemented in 2011. Exploiting this large-scale natural experiment in a triple difference analysis, we show that villages whose migratory opportunities were curtailed experienced a 13% increase in poverty when hit by disasters.

We are amongst the first to causally quantify the unintended consequences of migratory restrictions in the context of climatic shocks. Our results suggest that the aim of the Indonesian government to protect citizens overseas by inhibiting emigration came at a cost for Indonesian communities confined to stay. The burden of this policy was particularly high for areas relying on rainfed irrigation for rice production, a sector that absorbed many would-be international emigrants after the ban. We identify floods as the most consequential disaster type, particularly when hitting these agriculture-intensive villages. These findings hint towards important heterogeneities in how villages can cope with the ban due to their economic structure and shed light on an important mechanism beyond remittances.

Our results are particularly relevant in light of two of the most pressing developments worldwide: the increasing frequency of climate-induced disasters and current political debates to increase barriers to migration. In this respect, we extend findings in literature highlighting the vast gains from reducing barriers to emigration (Bryan and Morten, 2019; Clemens, 2011), yet from the opposite perspective. We show that suppressing international migration curbs one major adaptation strategy to natural disasters common to many developing countries. With a rise in restrictive migration policies against the backdrop of sharpening climatic changes, this scenario has the potential to further severely stress livelihoods in affected communities around the world.

While the setting of our study examines the less frequent case of restrictions implemented by the country of origin, we believe the effects on households relying on migration would be similar for policies enacted by destination countries. More specifically, implications for policy makers in both countries of origin and destination can be derived from this setting: curtailing any coping strategy to climatic changes will have immediate effects, particularly when the opportunities to substitute are limited. In light of current projections on climate-induced migrants going into hundred of millions (Cattaneo et al., 2019), decision makers need to carefully take climatic changes into account when designing migration-related policies. With most destination countries for work-seeking migrants located in economically better off regions, foregone income could further aggravate poverty differentials in those places already economically worse off..

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Appendix

A Criteria for the eligibility of poverty letters (*SKTM*)

1. The floor area of the building where he/she lives is less than 8 square meters per person.
2. The floor of the household's residence is made of earth/bamboo/cheap wood.
3. The walls of the household's residence are made of bamboo/thatch/low quality wood or walls without plastering.
4. Do not have a a sanitation facility/other households use one public latrine.
5. Household lighting sources do not use electricity.
6. Lack of access to clean water (for drinking and cooking).
7. Fuel for daily cooking is firewood/charcoal/kerosene.
8. Consumption of meat/dairy/chicken less than once a week.
9. Can only purchase one new set of clothes once a year.
10. Frequency of eating for each member of the household is maximum twice er day.
11. Inability to pay for treatment to public health centre ("puskesmas"/polyclinics).
12. The source of income for the head of the household is: farmers land area of 0.5 hectares, farm laborers, fishermen, construction workers, plantation workers, or other occupations with an income below IDR 500,000 per month.
13. The educational attainment of the head of the household is less than primary schooling.
14. Do not have savings/assets with the minumum values of IDR 500,000 such as motorbikes, cars, gold, livestock, or other capital goods.

B Constructing a village-crosswalk

Administrative units. As a consequence of Indonesia's decentralization after the fall of the authoritarian Suharto regime in 1998, a splitting process of administrative units ensued at virtually all administrative levels. Indonesia's official administrative levels consist of provinces (*propinsi*), districts (regencies *kabupaten* and cities *kota*), sub-districts (*kecamatan*) and villages (*desa*). In the aftermath of fiscal and administrative decentralization reforms of 2000, districts (the second regional tier) started to receive substantial budgetary spending (but rather minor revenue generating) capacities. In a step-wise process of district proliferation (*pemekaran*), their number increased from 314 in 2000 to 511 in 2014 as recorded in the respective years' census data.

When using any of these administrative levels as the unit of observation, the proliferation of administrative units has to be taken into account to track units over time consistently. It is fairly common that an existing unit (mother) splits into two new units (children) in one year, and then one of the children again into three children some years later. Units therefore can be children in one year, and also mothers in another year.

Beyond names, all administrative units are numbered following a simple and coherent coding methodology based on numbers for identification purposes. The process of splitting administrative units results in shifting administrative codes that are propagated through all administrative levels. Official crosswalks of administrative units allow to track individual codes over the years, allowing to distinguish mother-children relationships.

Village crosswalk. Our approach to tracing villages across time takes the administrative codes provided in all village census-rounds from 2000 through 2014 as its basis (relying on census rounds from 2000, 2003, 2006, 2008, 2011 and 2014). A straightforward id-merge across rounds performs poorly due to the splitting process, the renaming of villages, and re-coding of village ids. To connect all rounds, we rely on three main data sources: (1) the individual census rounds' administrative codes, (2) available crosswalks on the district as well as sub-district level, and (3) village shapefiles.

We proceed in four steps: First, we merge the sub-district crosswalk provided by the Indonesian Family Life Survey (IFLS) and to individual census rounds using corresponding administrative codes. This cross-walk reflects all code changes except for those on the village-level. Second, we employ fuzzy string matching by village names within the same sub-district based on the reconstructed codes. We match the remaining unmatched villages based on the district-level crosswalk. Lastly, for the remainder of unmatched villages, we combine village shapefiles across time to match them with their corresponding pair/mother.Out of the existing 82,190 villages in the 2014 census, we can trace more than 78 thousand back to 2000, representing more than 95% of all villages, thereby covering all sub-districts and identifying mother-child relationships, and pertaining to all census rounds in between. This allows us to create a panel of Indonesian villages and urban precincts for all existing census-rounds from 2000 through 2014. We restrict our sample to *Podes* 2005, 2008, 2011 and 2014 as previous waves do not provide information on international migration.

Table (A1) Summary statistics

	Mean	SD	Min	Max	Obs
Podes (2005, 2008, 2011 and 2014)					
Saudi Arabia as main destination	0.12	0.33	0	1	268,194
Stock of emigrants	18.39	67.40	0	5,912	268,194
Stock of female emigrants	10.78	39.76	0	3,022	268,194
Stock of male emigrants	7.61	38.54	0	4,670	268,194
Disaster in the last threee years	0.40	0.49	0	1	268,194
Number of disasters in the last three years	1.36	2.71	0	69	200,206
Poverty cards	66.58	210.62	0	41,448	268,194
Social health cards	431.93	939.95	0	55,307	268,194
Households living in slums	7.93	95.07	0	22,358	268,194
Population	3,346.63	4,731.86	4	199,996	268,194
Conflict in village	0.03	0.17	0	1	268,194
Rural village	0.82	0.38	0	1	267,724
Lowlands	0.19	0.39	0	1	183,337
Flood in the last three years	0.25	0.43	0	1	214,588
Landslide in the last three years	0.14	0.34	0	1	186,833
Forest fire in the last three years	0.05	0.22	0	1	169,542
Earthquake in the last three years	0.10	0.29	0	1	178,128
Tsunami in the last three years	0.01	0.08	0	1	162,051
Typhoon in the last three years	0.14	0.34	0	1	138,613
Tide in the last three years	0.03	0.18	0	1	123,888
Other disasters in the last three years	0.07	0.26	0	1	44,454
Bank or ATM in the village	0.08	0.27	0	1	268,194
Smeru (2010 and 2015)					
Poverty Rate (below 2$ PPP)	19.05	22.13	0	99.50	131,915

Notes. Information on type of natural disasters and number of natural disasters is restricted to the years 2008, 2011 and 2014. Type of natural disasters are categorical variables assuming the value 1 if that type of natural disaster has occurred in the last three years and 0 in absence of any natural disasters recorded.

Table (A2) Average effect of disasters on poverty

Dependent	Poverty cards			
	All migrants		Female migrants	
	(1)	(2)	(3)	(4)
Disaster=1	0.097***	0.103***	0.095***	0.101***
	(0.010)	(0.010)	(0.009)	(0.009)
Emigrants	0.073***	0.065***		
	(0.004)	(0.004)		
Disaster=1 × Emigrants	-0.013***	-0.014***		
	(0.004)	(0.004)		
Female emigrants			0.081***	0.074***
			(0.004)	(0.004)
Disaster=1 × Female emigrants			-0.014***	-0.015***
			(0.004)	(0.004)
Village FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Province-time trend	No	Yes	No	Yes
Controls	No	Yes	No	Yes
Observations	268,194	268,194	268,194	268,194

Notes. Poverty cards, out-migrants stock and female migrants stock are transformed by the inverse asymptotic sine (asinh). Control variables include log(population) and a conflict event dummy. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A3) Average effect of number of disasters on poverty

Dependent	Poverty cards			
	DD			DDD
	(1)	(2)	(3)	(4)
Number of disasters		0.026***	0.019***	0.027***
		(0.002)	(0.002)	(0.002)
SA=1 × Post2011=1	0.039*			0.015
	(0.021)			(0.024)
Post2011=1 × Number of disasters		-0.010***		-0.013***
		(0.003)		(0.003)
SA=1 × Number of disasters			0.001	-0.011
			(0.005)	(0.007)
SA=1 × Post2011=1 × Number of disasters				0.018**
				(0.008)
Village FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	200,173	200,173	200,173	200,173

Notes. Poverty cards is transformed by the inverse asymptotic sine (asinh). Control variables include asinh(male migrants), log(population) and a conflict dummy. Binary variables SA_v and $Post2011_t$ are omitted because they are absorbed by the fixed effects. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A4) Average effect of disasters on poverty using alternative definitions of poverty

Dependent	Health cards	Households living in slums	Poverty rate 2$ PPP
	(1)	(2)	(3)
SA=1 × Post2011=1 × Disaster=1	0.154*	0.279***	1.186***
	(0.081)	(0.042)	(0.382)
Village FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	268,194	268,194	128,772

Notes. The dependent variables in columns (1) and (2) are transformed by inverse asymptotic sine (asinh). The dependent variable in column (3) is the number of poor people below the poverty line of 2 USD PPP - divided by the total population. Control variables include asinh(male migrants), log(population) and a conflict dummy. All further interactions are included in the estimation but not displayed here. The sample consists of census years 2005, 2008, 2011 and 2014 in columns (1) and (2); and 2011 and 2014 in column (3). Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A5) Average effect of being subject to disasters on poverty by type of financial transfer

Dependent	Poverty cards					
	(1)	(2)	(3)	(4)	(5)	(6)
SA × Post2011 × Disaster	0.097**	0.096**	0.096**	0.096**	0.096**	0.096**
	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)	(0.043)
Asinh(District transfers)		0.005**	0.004**	0.004*	0.004*	0.004*
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Asinh(Province transfers)			0.007***	0.007***	0.007***	0.007***
			(0.002)	(0.002)	(0.002)	(0.002)
Asinh(Central govn't transfers)				-0.002	-0.002	-0.002
				(0.002)	(0.002)	(0.002)
Asinh(Foreign aid)					0.021***	0.021***
					(0.006)	(0.006)
Asinh(Private aid)						-0.002
						(0.005)
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	200,173	200,173	200,173	200,173	200,173	200,173

Notes. Poverty cards is transformed by the inverse asymptotic sine (asinh). District transfer, province transfer and foreign and private aid are in milion IDR. Control variables include asinh(male migrants), log(population) and a conflict event dummy. All further interactions are included in the estimation but not displayed here. Sample years: 2008, 2011, 2014. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A6) Average effect of disasters on poverty: further robustness checks

Dependent	Poverty cards				
	Java excluded	Java only	Only villages with migrants	Exclude Middle East	Trim population
	(1)	(2)	(3)	(4)	(5)
SA × Post2011 × Disaster	0.177**	0.140***	0.118***	0.118***	0.121***
	(0.076)	(0.048)	(0.042)	(0.042)	(0.040)
Village FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	166,496	101,698	140,399	141,111	262,546

Notes. Column (1) excludes villages in the island of Java from the sample. Column (2) is restricted only to villages in Java. Column (3) restricts the sample to villages that had at least an Indonesian worker overseas in 2005. Column (4) excludes UAE, Jordan and Qatar as main destinations from the sample. Column (5) trims the population at the 99th and 1st percentile. Poverty cards is transformed with the inverse asymptotic sine (asinh). Control variables include asinh(male migrants), log(population) and a conflict event dummy. All further interactions are included in the estimation but not displayed here. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A7) Average effect of being subject to disasters on poverty in t-1

Dependent	Poverty cards		
	DD		DDD
	(1)	(2)	(3)
Disaster_{t-1}	-0.018*	-0.010	-0.020*
	(0.010)	(0.009)	(0.010)
Post2011=1 × Disaster_{t-1}	0.034**		0.032**
	(0.015)		(0.016)
SA=1 × Disaster_{t-1}		0.026	0.020
		(0.025)	(0.029)
SA=1 × Post2011=1			0.031
			(0.029)
SA=1 × Post2011=1 × Disaster_{t-1}			0.013
			(0.043)
Village FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	199,899	199,899	199,899

Notes. *t-1* corresponds to a period of 3 to 6 years before *t*. Poverty cards is transformed by the inverse asymptotic sine (asinh). Control variables include asinh(male migrants), log(population) and a conflict event dummy. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A8) Average effect of disasters on population growth and poverty

Dependent	(1) Population	(2) Poverty cards
SA × Post2011 × Disaster	-0.013**	-0.524
	(0.005)	(0.416)
SA × Post2011 × Disaster × log(population)		0.082
		(0.050)
Village FE	Yes	Yes
Time FE	Yes	Yes
Province-time trend	Yes	Yes
Controls	Yes	Yes
Observations	268,194	268,194

Notes. The dependent variable is log(population) in column (1) and asinh(poverty cards) in columns (2). Control variables include a conflict event dummy. All further interactions are included in the estimation but not displayed here. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A9) Average effects in the presence of spillovers

Dependent	Poverty cards			
	DDD			
	(1)	(2)	(3)	(4)
SA=1 × Post2011=1 × Disaster=1	0.119***	0.113***	0.0130***	0.131***
	(0.039)	(0.041)	(0.043)	(0.045)
Distance to village with SA=1:				
0-10km × Post2011=1 × Disaster=1		-0.009	0.007	0.008
		(0.029)	(0.031)	(0.034)
10-20km × Post2011=1 × Disaster=1			0.067	0.067
			(0.044)	(0.046)
20-30km × Post2011=1 × Disaster=1				0.005
				(0.055)
Village FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	268,194	268,194	268,194	268,194

Notes. Poverty cards is transformed by the inverse asymptotic sine (asinh). Control variables include asinh(male migrants), log(population) and a conflict event dummy. The additional distance controls indicate whether a village is within xx kilometers of a village with Saudi Arabia as main migratory destination (centroid based), and set to zero in case the village itself has Saudi Arabia as main. Interactions between all variables included are omitted for presentability reasons. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A10) Average effect of disaster events on poverty: conley standard errors

Dependent	Poverty cards			
Distance cut-off	5 km	10 km	20 km	30 km
	(1)	(2)	(3)	(4)
SA=1 × Post2011=1 × Disaster=1	0.119**	0.119**	0.119**	0.119**
	(0.054)	(0.061)	(0.057)	(0.049)
Village FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	265,804	265,804	265,804	265,804

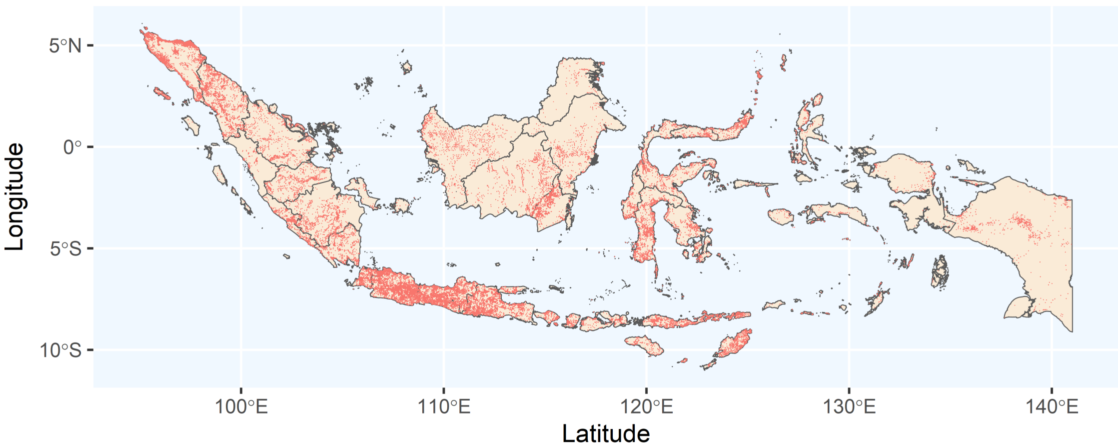
Notes. Poverty cards is transformed by the inverse asymptotic sine (asinh). Control variables include asinh(male migrants), log(population) and a conflict event dummy. All further interactions are included in the estimation but not displayed here. 604 villages are excluded from the sample because of the absence of coordinates. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Table (A11) Average effect of extreme rainfall events on poverty

Dependent	Poverty cards			
Buffer	10 km	15 km	20 km	30 km
	(1)	(2)	(3)	(4)
SA × Post2011 × Extreme rainfall	0.576***	0.629***	0.562***	0.253***
	(0.196)	(0.144)	(0.119)	(0.091)
Village FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Province-time trend	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	15,453	23,735	32,360	51,657
Villages	5,112	7,781	10,524	16,656

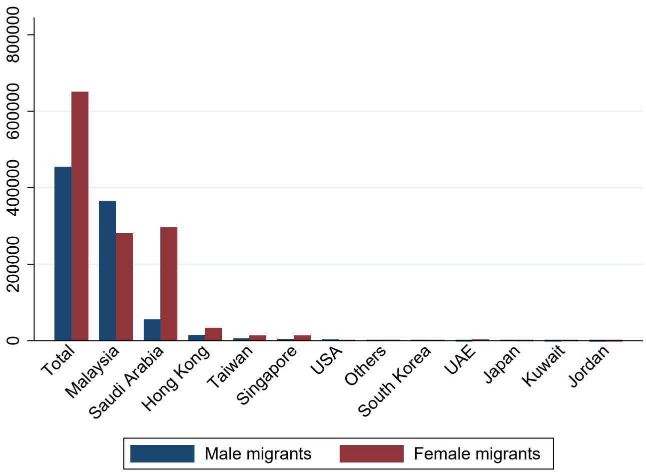
Notes. Poverty cards is transformed with the inverse asymptotic sine (asinh). Extreme rainfall events are defined as days of year t with the largest rainfall in the 10 previous years. Control variables include asinh(male migrants), log(population) and a conflict event dummy. All further interactions are included in the estimation but not displayed here. Robust standard errors are clustered on the village level and reported in parentheses. Significance at or below 1% (***), 5% (**) and 10 percent (*).

Figure (A1) Disaster events in the period 2003-2005



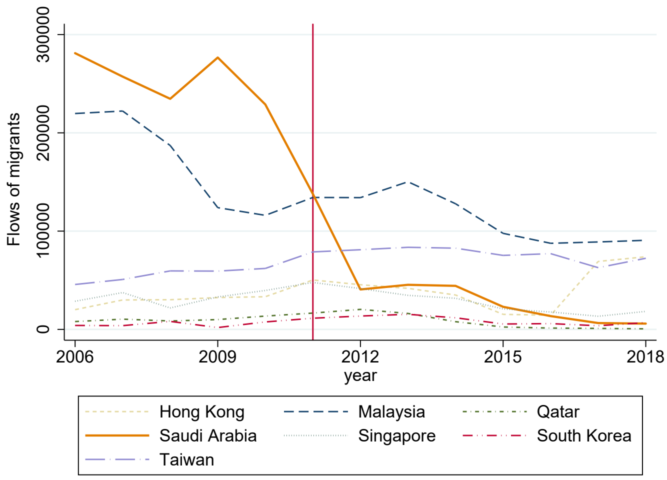
Notes: Red dots represent centroids of villages that experienced at least one natural disaster between 2003 and 2005. Source: Own computation based on *Podes* 2005.

Figure (A2) Stock of emigrants by gender and destination in 2005



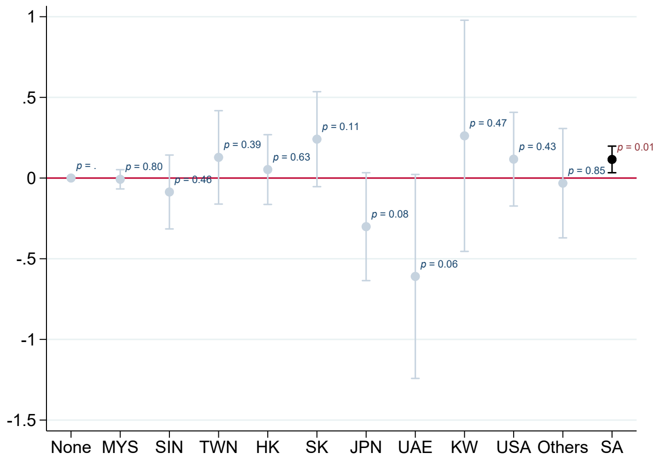
Notes: Source: Own computation based on *Podes* 2005.

Figure (A3) Annual flows of documented migrants per destination



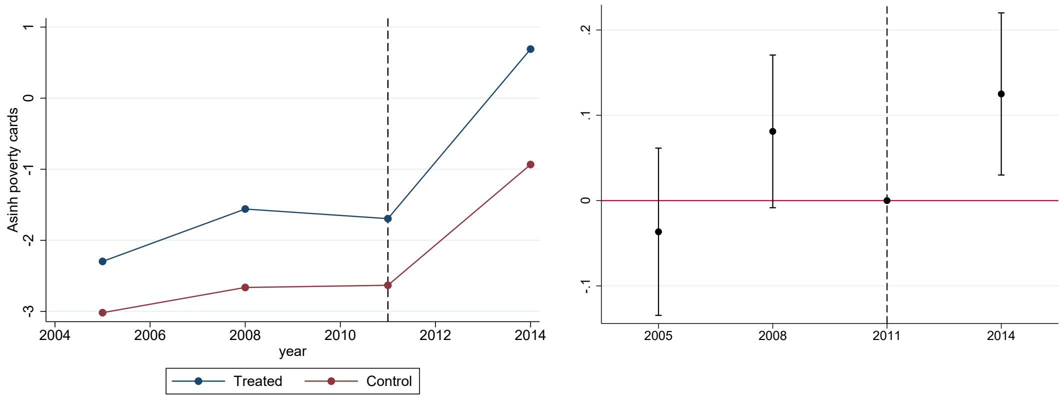
Notes: The vertical line indicates the implementation of the ban in 2011. Source: Own computation based on data from the national placement agency for Indonesian workers abroad (BNP2TKI).

Figure (A4) Placebo: average effects of natural disaster by villages' top destination countries



Notes: Coefficients are bound at 95% confidence intervals. The displayed coefficients capture the effect of natural disasters on poverty cards transformed by the inverse hyperbolic sine. The baseline category is villages with no migrants ("none"). Countries from the left to the right are: Malaysia, Singapore, Taiwan, South Korea, Japan, United Arab Emirates, Kuwait, United States, other countries, Saudi Arabia. Control variables include asinh(male migrants), log(population) and a conflict event dummy. Village fixed effects, time fixed effects and province time trends are included. Standard errors are clustered at the village level. Number of observations: 268,194.

Figure (A5) Triple difference: parallel trends

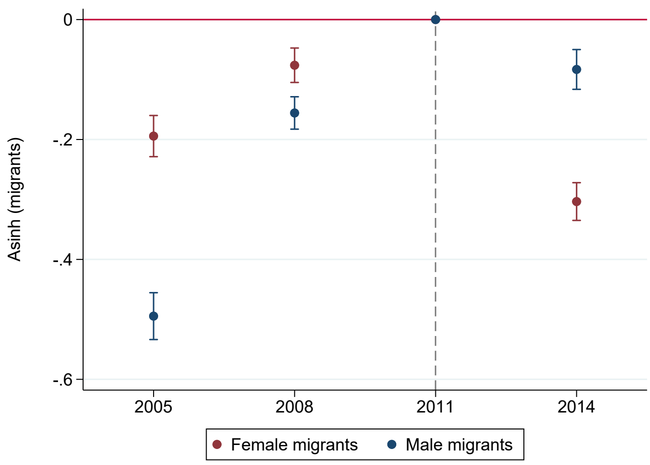


(a) Unconditional parallel trends

(b) Conditional parallel trends: event study

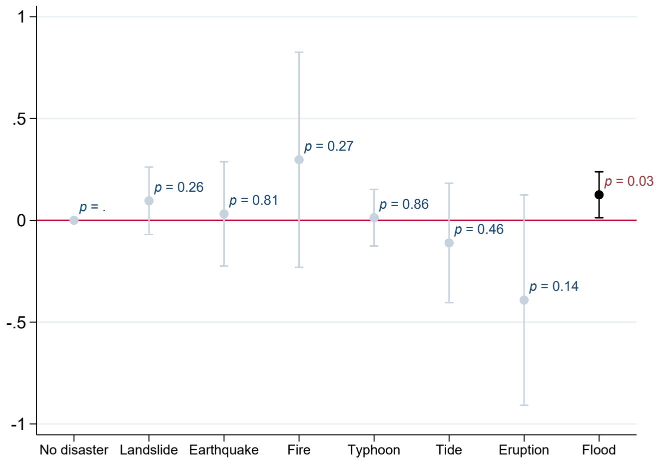
Notes: Panel (a) displays the unconditional parallel trend assumption. The treated group contains villages with Saudi Arabia as main destination country, subtracted of the effect of being hit by natural disaster. The control consists of villages that do not declare Saudi Arabia as the main destination, subtracted of the effect of being hit by natural disaster. Values interpolated between 2005 and 2008, 2008 and 2011 and 2011 and 2014.
Panel (b) shows and event study, plotting the triple difference coefficient of equation 3 with time dummies and with 2011 as the baseline year at 95% confidence interval. Control variables include asinh(male migrants), log(population) and a conflict event dummy. Village fixed effects, time fixed effects and province time trends are included. Standard errors are clustered at the village level. Number of observations: 268,194.

Figure (A6) Female to male substitution: change in female and male migrants stocks in villages with Saudi Arabia as main destination country against others



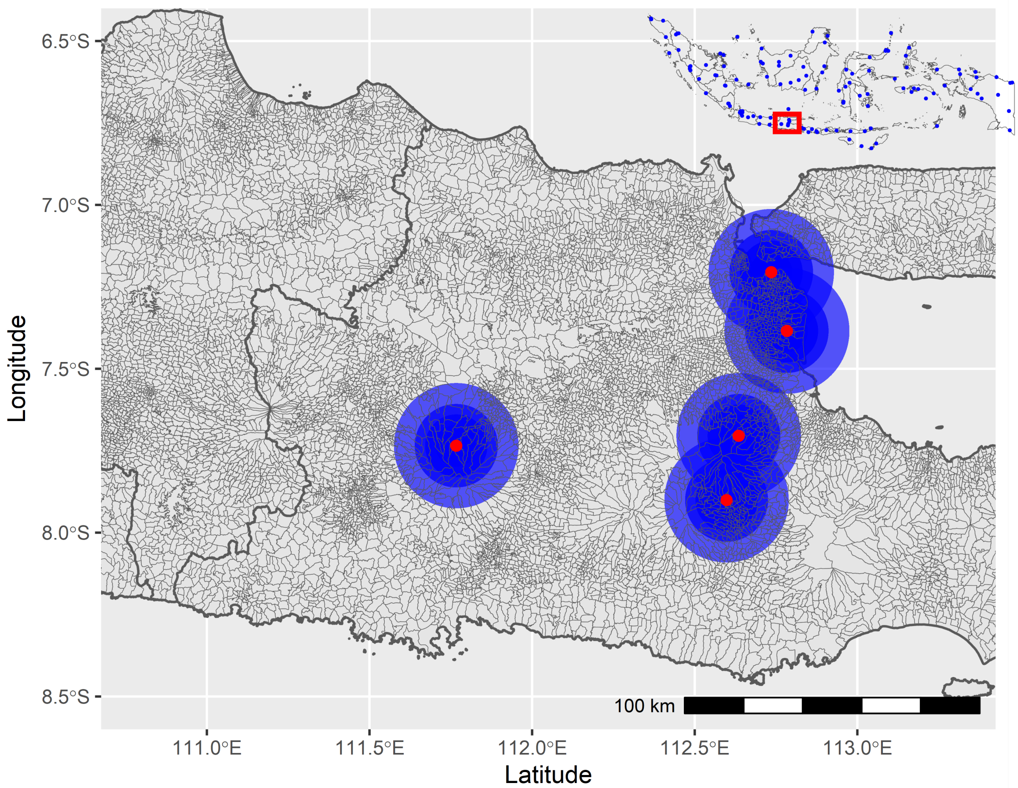
Notes: Coefficients are bound at 95% confidence intervals. The vertical line indicates the implementation of the ban in 2011. The displayed coefficients capture β_3 of the event study from Equation 2, i.e. the relative decrease in the inverse hyperbolic sine of female or male migrants' stocks for villages that indicate Saudi Arabia as the main destination country in 2005 against villages that indicate other countries or have no migrants. The baseline period is 2011. Control variables include log(population) and a conflict event dummy. Village fixed effects, time fixed effects and province time trends are included. Standard errors are clustered at the village level. Number of observations: 141,111.

Figure (A7) Average effects of natural disasters on poverty by type of disaster



Notes: Coefficients are bound at 95% confidence intervals. The displayed coefficients capture the effect of natural disasters on the inverse hyperbolic sine (asinh) of emitted poverty cards after the migration ban in 2011 by type of disaster. The baseline type is "no disaster". We exclude the category "Tsunami" since we only have one observation in the control group after 2011. Control variables include asinh(male migrants), log(population) and a conflict event dummy. Village fixed effects, time fixed effects and province time trends are included. Standard errors are clustered at the village level. The sample includes PODES censuses of 2008, 2011 and 2014. Number of observations: 197,742.

Figure (A8) Geocoded weather stations



Notes: The larger map plots the zoomed in area delimited by the red box in the top right corner. Red dots represent the coordinates of each weather station. Different shades of blue indicate buffer zones of 10, 15, 20 and 30 km respectively. Source: Own computation based on *SKTM* and *Podes*.