Environmental Migration and Labor Markets in Nepal

Jean-François Maystadt, Valerie Mueller, Ashwini Sebastian

Abstract: While an emerging literature cites weather shocks as migration determinants, scant evidence exists on how such migration affects the markets of receiving communities in developing countries. We address this knowledge gap by investigating the impact of weather-driven internal migration on labor markets in Nepal. An increase of 1 percentage point in net migration reduces wages in the formal sector by 5.7%. A similar change in migration augments unemployment by 1 percentage point. The unskilled bear greater consequences. Understanding entrepreneurial constraints and drivers of labor market exits will inform pathways to resilience.

JEL Codes: J21, J61, O15

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A GROWING LITERATURE identifies migration as a response to weather variations in developing countries. Rural workers search for employment elsewhere to mitigate income losses temporarily or move permanently if the damages are severe

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(Halliday 2006; Feng, Krueger, and Oppenheimer 2010; Dillion, Mueller, and Salau 2011; Gray and Mueller 2012a, 2012b; Marchiori, Maystadt, and Schumacher 2012; Gray and Bilsborrow 2013; Bohra-Mishra, Oppenheimer, and Hsiang 2014; Mueller, Gray, and Kosec 2014). An emerging challenge in the climate change debate is to reconcile whether such adaptation bears additional consequences for human security and livelihoods (IPCC 2014a). Some view migration as a key mode of adaptation to extreme climatic events (Desmet and Rossi-Hansberg 2015). Others have warned against the detrimental impacts of environmental migrants on receiving countries (e.g., IPCC 2014a, chap. 19; or Rajundra Pachauri, IPCC chairman, at press conference, March 31, 2014), as "climate change can indirectly increase risks of violent conflicts in the form of civil wars and intergroup violence by amplifying well-documented drivers of these conflicts such as poverty and economic shocks" (IPCC 2014b, 40).

Such ominous rhetoric is reinforced by a lack of empirical evidence on the consequences of economic migration. Measurement of immigration impacts on industrialized countries is ubiquitous (Card 1990, 2005; Borjas 2005, 2006; Boustan, Fishback, and Kantor 2010; Pugatch and Yang 2011; Ottaviano and Peri 2012; Dustmann, Frattini, and Preston 2013). The emerging consensus is that migrants have at most modest effects on hosts' average wages and employment (Blau and Kahn 2015). The literature points to overall gains for nonmigrants due to skill complementarity, with modest negative effects for the unskilled workers or those at the lower end of the wage distribution (Ottaviano and Peri 2012; Dustmann et al. 2013). In developing countries, scant evidence exists on how migration affects receiving communities, let alone the implications of disaster-driven migration (El Badaoui, Strobl, and Walsh 2014; Kleemans and Magruder 2014; Strobl and Valfort 2015). We address this knowledge gap by investigating the impact of weather-driven, internal migration on labor markets in Nepal.

Nepal is an ideal setting to examine the implications of environmental migration. First, the country faces repeated exposure to natural disasters (Dartmouth Flood Observatory 2014) and has a strong tradition of migration during periods of low agricultural productivity (Massey, Axinn, and Ghimire 2010).[1] Second, the positive selection of migrants (Massey et al. 2010; Fafchamps and Shilpi 2013) and the importance of the informal sector (ILO 2010) render the theoretical mechanism underlying migration impacts complex. The inelasticity of labor demand in the formal sector can push workers with a high degree of substitutability to compete with nonmigrants with other skills (Kleemans and Magruder 2014). Increasing the supply of workers can depress wages, yet introducing skilled workers to the less productive informal sector can enhance productivity (Kerr 2013). Empirical estimates of the

1. For example, in 2008, one of the largest floods occurred in eastern Nepal (and the neighboring areas in India) displacing 10 million people (Dartmouth Flood Observatory 2014).

impacts of migration in both sectors reflect the relative strengths of the labor demand and supply effects. We therefore estimate the consequences of environmental migration on both sectors.

In order to address the endogeneity of migration to local employment conditions, we employ a methodology based on Boustan et al. (2010). The intuition of the approach is to address the self-selection of migrants by constructing separate instruments for in- and out-migration based on weather shocks at origin that push migrants from one district and the proximity to particular districts at destination acting as a pull factor. The in- and out-migration rates are computed using predictions of the probability of moving from one district to another as well as bilateral (in- and out-) migration flows. The constructed variable, the predicted in-migration rate subtracted from the predicted out-migration rate, is used as an instrument for the actual net migration rate in the first stage. In the second-stage estimates, native displacement is accounted for by focusing on the relationship between net migration rates and local wages and employment. Our methodology varies from the original approach in exploiting the panel structure of our data. By observing labor market outcomes over two periods, we can further include district and time fixed effects to reduce the potential for omitted variable bias in the second stage.

We contrast the results from our main specification to those using other instrumental variables (IV) approaches in the literature. The first incorporates weighted immigration flows directly as an instrument for the net migration rate, where the weights are based on the historical shares of migrants from a particular origin at the destination (Card 2001, 2009). The second approach exploits rainfall variability at the source location as an exogenous determinant of labor supply (Munshi 2003; Pugatch and Yang 2011; Kleemans and Magruder 2014). Alternative methods provide qualitatively similar results, but at the cost of imposing additional identifying assumptions. Our final conclusions are based on the Boustan et al. (2010) methodology because it is less likely to violate the exclusion restriction given the focus of internal migration and the inability to access data on historical migration patterns. Yet, the fact that we find only slightly larger effects under more common methods is reassuring for future studies lacking the means to perform all three methods.

We find that exposure to flooding pushes a positively selected group of individuals to migrate. Flood-induced migration causes a decline in the employment of natives in the informal sector and an increase in the unemployment of low-skilled natives. Wage effects are concentrated in the formal sector: an increase of 1 percentage point in net migration reduces wages in the formal sector by 5.7%. Data limitations prevent us from isolating the primary driver of the absence of wage effects in the informal sector. Robust findings on employment and formal sector wages show that vulnerability to weather extremes is not limited to those at the source of exposure. Flooding in areas populated by rivers displaces people and can engender spillover effects on migrant hubs.

1. IMMIGRATION EFFECTS ON LABOR MARKETS

Standard models predict that immigration is detrimental to workers who show a high degree of substitutability with migrants (Johnson 1980a, 1980b; Altonji and Card 1991; Card and Lemieux 2001; Borjas 2003; Borjas and Katz 2007; Ottaviano and Peri 2012). Immigration lowers the wages of native workers by creating an excess supply of typically low-skilled labor (Borjas 2003). Native workers will also reduce their supply given diminished returns to employment, the "displacement effect."

One potential concern is that the effect will not be restricted to same-skilled native workers if skilled and unskilled labor are substitute factors of production (D'Amuri and Peri 2014). We analyze the potential impacts of environmental migration in Nepal, where migrants tend to be high skilled. The formal and informal sectors consist of skilled and unskilled workers, with the informal sector employing over 90% of the Nepali labor force (ILO 2010).

To illustrate, consider a representative profit function, $\pi(p, w^H, w^U)$, for the formal sector good y with price p, and two inputs, skilled labor L^H and unskilled labor L^U with wages w^H and w^U:

$$\pi(p, w^H, w^U) = \max_{y, L^H, L^U} \{py - w^H L^H - w^U L^U\colon (y, L^H, L^U) \in T\}. \tag{1}$$

Here T is a convex technology set. By Hotelling's Lemma,

$$-\frac{\partial \pi}{\partial w^U} = L^U(p, w^H, w^U). \tag{2}$$

Taking the partial derivative of (2) with respect to w^H yields the relationship between the formal sector wages w^H and the demand for unskilled labor L^U:

$$-\frac{\partial^2 \pi}{\partial w^U \partial w^H} = \frac{\partial L^U}{\partial w^H}. \tag{3}$$

If skilled and unskilled labor are complementary inputs in the production of the formal sector good, then (3) will be negative. If skilled and unskilled labor are substitutes, then (3) is positive.

Given this framework, the influx of migrants shifts the skilled worker supply curve outward from L^H to $L^{H'}$, causing skilled wages to decrease (fig. 1*a*). The labor supply of natives in the high-skilled market would diminish from L^H to $L^{H'}_{Native}$. The effects of skilled immigration on unskilled wages and employment is ambiguous, depending on the production technology. If $\partial^2\pi/(\partial w^U \partial w^H) < 0$, then the demand for unskilled labor will shift downward. This shift in labor demand causes wages and labor supply to fall in the unskilled labor market (from w^U to $w^{U'}$ and L^U to $L^{U'}$ respectively in fig. 1*b*). The opposite is true when skilled and unskilled labor are substitutes. The key point is that high-skilled immigration can affect the native low-skilled labor market as firms adjust their employment mix in response to changes in relative wages (Lewis 2011; Borjas 2014).

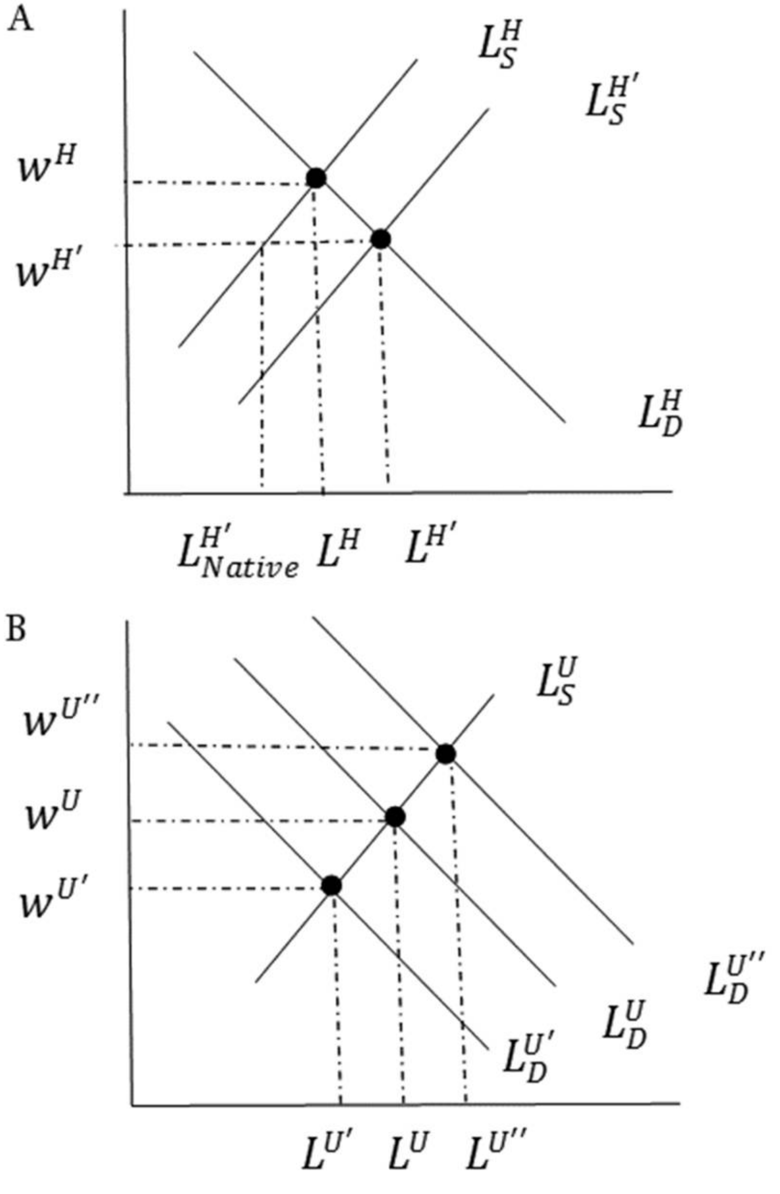

Figure 1. *A*, Skilled labor market; *B*, unskilled labor market

In Nepal, adjustments to an influx of displaced workers will be limited by the size of its firms, a lack of financial capital, and limited aspirations for scaling up enterprises. The informal sector in Nepal consists largely of small firms (Afram and Pero 2012). The demand for hired labor is low: only 13% and 17% of enterprises reported hiring anyone in 2003 and 2010 (table 1). Furthermore, there is limited access to financial

Table 1. Nonmigrant Household Financial and Capacity Constraints of Enterprises (If Owned Enterprise), Weighted Means (and Standard Deviations)

	2003	2010	2003		2010	
	All	All	Low Skill	High Skill	Low Skill	High Skill
	(n = 865)	(n = 1,854)	(n = 695)	(n = 170)	(n = 1,469)	(n = 385)
Is the enterprise registered with the government?						
Yes	.20	.21	.15	.54	.15	.48
	(.40)	(.41)	(.35)	(.50)	(.36)	(.50)
What was the main source of money for setting up the enterprise?						
Didn't need any money	.30	.33	.31	.21	.35	.20
	(.46)	(.47)	(.46)	(.41)	(.48)	(.40)
Own savings	.41	.37	.39	.53	.37	.41
	(.49)	(.48)	(.49)	(.50)	(.48)	(.49)
Relatives or friends	.14	.13	.15	.10	.13	.16
	(.35)	(.34)	(.36)	(.30)	(.34)	(.37)
Bank (agricultural, commercial, Grameen type)	.07	.06	.07	.07	.05	.11
	(.26)	(.25)	(.26)	(.26)	(.23)	(.31)
Other financial institution	.01	.04	.01	.01	.03	.08
	(.12)	(.20)	(.12)	(.12)	(.18)	(.27)
Other	.07	.06	.07	.07	.07	.05
	(.25)	(.25)	(.25)	(.25)	(.25)	(.21)

Have you tried to borrow money to operate or expand your business in the past 12 months? (relative to no)?						
Yes, successfully	.20	.23	.20	.18	.22	.31
	(.40)	(.42)	(.40)	(.39)	(.41)	(.47)
Yes, unsuccessfully	.04	.03	.04	.03	.03	.04
	(.19)	(.17)	(.19)	(.17)	(.17)	(.20)
Did you hire anyone over the last 12 months?						
Yes	.13	.17	.11	.30	.14	.35
	(.34)	(.38)	(.31)	(.46)	(.34)	(.49)
How many workers do you normally hire during a month when the enterprise is operating? (if hired in last 12 months)?						
Number of workers	8.88	9.98	4.99	17.80	11.00	7.84
	(32.10)	(38.60)	(20.60)	(48.20)	(42.80)	(28.40)
What problems, if any, do you have in running your business?						
No major problem	.35	.49	.36	.30	.51	.38
	(.48)	(.50)	(.48)	(.46)	(.50)	(.49)
Capital or credit problem	.15	.13	.15	.22	.13	.16
	(.36)	(.34)	(.35)	(.41)	(.33)	(.36)
Lack of customers	.31	.14	.32	.24	.13	.17
	(.46)	(.34)	(.47)	(.43)	(.34)	(.37)
Other	.18	.25	.17	.25	.23	.30
	(.39)	(.43)	(.38)	(.44)	(.42)	(.46)

capital (Afram and Pero 2012). The majority of small and medium enterprises are financed through households' own savings (approximately 40%; table 1). Only 23% in 2010 tried to obtain a loan to operate or expand their business (table 1).

If skilled and unskilled labor are substitutes, equation (3) suggests that both skilled and unskilled wages in Nepal will decline.[2] Although we expect a decline in both skilled and unskilled wages, the effect of immigration on average wages across sectors is ambiguous since the skill mix may shift in favor of the higher waged skilled workers. We therefore quantify the explicit effects of environmentally induced immigration on wages and employment by worker type and sector empirically.

2. DATA

Our analysis draws from several data sources. First, migration and employment data are taken from two waves of the nationally representative Nepal Living Standards Survey (NLSS): 2003 and 2010. Second, to create weather anomaly variables, we use 0.5 × 0.5 degree gridded satellite-based weather data provided by the POWER (Predicted of Worldwide Energy Resource) project of the National Aeronautics and Space Administration (NASA) of the United States for the years 1981–2013 (NASA 2014). Third, gridded population data are extrapolated from the Center for International Earth Science Information Network at Columbia University. Fourth, river networks and geographic characteristics (such as distance) are extracted from the United States Geological Survey HydroSHEDS (Hydrological Data and Maps Based on Shuttle Elevation Derivatives at Multiple Scales data set).[3] Below we elaborate on how our outcomes and explanatory variables are constructed from the aforementioned data sets.

2.1. Definition of Variables

2.1.1. Migration

We create migration flows using the migration information of 7,000 and 14,000 individuals (residing in 3,954 and 5,556 households in 69 districts)[4] in 2003 and 2010, respectively. Inflows are based on individuals who reported moving to district

2. According to the Nepal Labor Force Survey 2008, 21.3% of the Nepali labor force is considered underutilized because of receiving inadequate earnings in their current job or a mismatch of skills (Central Bureau of Statistics 2009). Furthermore, the ILO (2010) suggests a deficit of productive jobs in the economy. These factors imply that given the current production technologies in Nepal, it is not entirely unreasonable to suggest that low and high-skilled labor may be substituable.

3. The data source is http://hydrosheds.cr.usgs.gov/index.php.

4. In total, six districts are excluded from our panel because they were omitted from the 2003 and 2010 surveys. In 2003, Accham, Mustang, and Rasuwa districts were unreachable due to conflict. Dolpa, Ilam, and Manang districts were omitted in 2010.

k from district *j* in year *t* using NLSS sampling weights for population-based inferences. Bilateral migration outflows are similarly defined. We restrict our focus to inflows and outflows for 4 years preceding the 2003 and 2010 surveys to minimize the impact of recall bias and ensure sufficient coverage of weather events in the period observed.[5] Population figures derived from 1995 (CIESIN) are then used to further convert the migration flows into shares of migrants moving into and out of each district *k* from each district *j* for each year. This procedure creates two 69×69 matrices of bilateral in- and out-migration rates at the district level, which are used to predict net migration rates, the key variable for the identification of the impact of migration in the labor regressions.

2.1.2. Weather Anomalies

We create seasonal flood and drought indicator variables over the same period covering migration flows, for each 0.5×0.5 degree grid that overlaps a district in a given year. Rainfall data vary over 78 grids spanning the country.[6] Heavy monsoon is from June to September. Regular monsoon is from November in the previous year through February of the current year. A flood shock indicator, for each grid in a given year, is set to 1 if cumulative rainfall over the heavy monsoon season exceeds the 90th percentile of the time-series distribution. Similarly, a drought shock indicator, for each grid in a given year, is set to 1 if cumulative rainfall over the regular monsoon season falls below the 10th percentile of the distribution.

Annual district-level flood and drought indicators are set to 1 if a flood or drought occurs in any grid overlapping the district. Figure 2 corroborates that our analysis covers periods of unprecedented increases in the frequency and severity of floods. Panel A depicts the widespread exposure of floods over 1999 to 2002 (UNOCHA 2002). Panel B covers the period of 2006–9, which includes a large scale 2008 flood in eastern Nepal. The breach in an embankment at the Indo-Nepali border in 2008 affected hundreds of thousands of people in Nepal (UNICEF 2008). The flood and drought variables are also interacted with river density data in the regression analysis to capture an additional dimension of district exposure to the weather anomalies. River density is calculated as the length of the river segments in kilometers divided by each district area.

2.1.3. Labor Market Outcomes

We first focus on the employment status of the individual. An individual is considered employed if he reported working in the last 12 months prior to the survey interview. Otherwise, the individual is categorized as unemployed (did not work

5. We later show that predicted migration rates are not sensitive to modifying the number of years over which migration is observed (sec. 4.4).

6. See figure A.1 in the appendix.

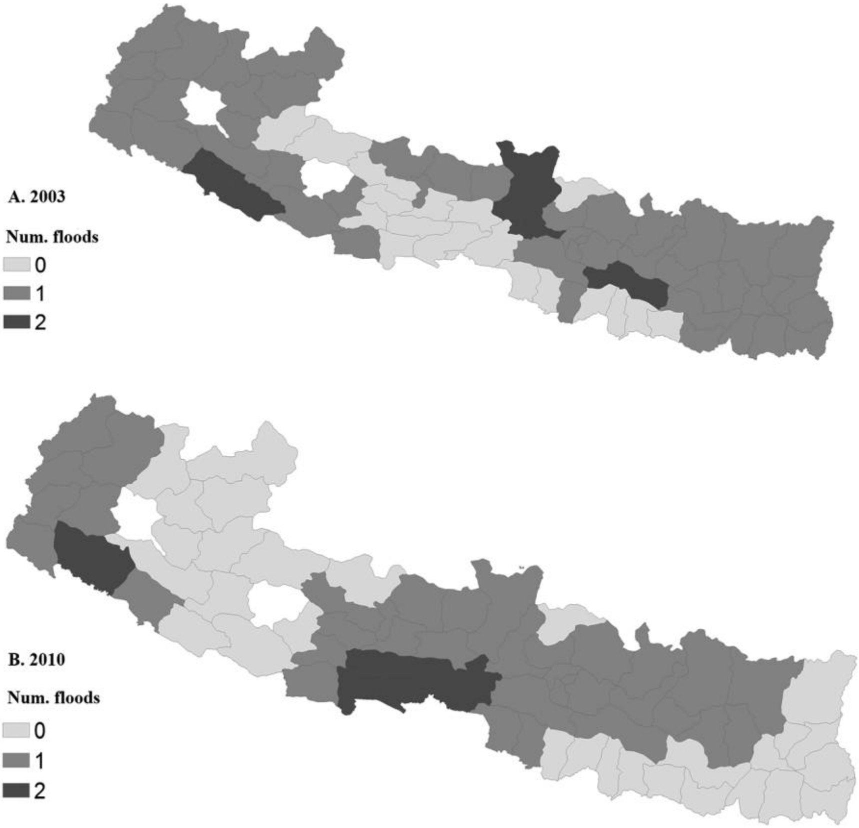

Figure 2. Floods in Nepal, cumulative over previous 4 years, 2003 (*A*) versus 2010 (*B*). Source: Authors' representation based on data from NASA (2014). Districts not used in analysis are omitted from maps. A color version of this figure is available online.

nor engage in domestic activities in the last 12 months) or inactive (did engage in domestic activities in the last 12 months).

Two stratifications are made in the analysis to facilitate the interpretation of results. The first stratification is based on the sector of employment, which relies on the NLSS definition. We also stratify the sample by skill, whereby individuals having more than 10 years of schooling (completed at least a secondary education) are characterized as highly skilled and others are considered low skilled. Sensitivity analysis is performed varying the definition of skilled labor in section 4.4.

Individual and household earnings over a 12-month period are used to construct monthly formal- and informal-sector wages, respectively. We use the national consumer price index to convert 2003 wages into 2010 real terms. Monthly wages for formal-sector workers are taken directly from the survey. For the majority of workers employed in the informal sector, we proxy for earnings with revenues from own farms and enterprises. To construct individual monthly earnings, we divide monthly revenues by the number of members in the household reported to be employed in the enterprise.

Our proxy for informal earnings may under- or overestimate true individual earnings in the informal sector. We might systematically overestimate revenues per capita by omitting hired employees from the denominator (because they were missing from the agricultural module). On the other hand, we may underestimate individual earnings because we are unable to clarify which household members were employed by the enterprise on a permanent basis.

Because household enterprises are more the rule than the exception, we restrict the analysis of migration impacts to the sample of household heads. Particularly for the informal sector, adding members from larger households may attenuate the effect of immigration inasmuch as their employment status may depend on their relative position in the household and other joint household decisions. Since restricting the focus to household heads sufficiently reduces the initial sample size, we detail how heads differ from the rest of the natives in the Summary Statistics section. We additionally assess the implications of relaxing the restriction in section 4.4.

2.2. Summary Statistics

Table 2 compares the characteristics of migrants, nonmigrants, and household heads in our sample. Migrants tend to be younger and more educated than nonmigrants, and a greater percentage are women. The proportion of migrants that completed 10 or more years of schooling is 29%, compared with 14% of nonmigrants in 2003. These differences widen by 2010, when 46% of migrants are considered skilled according to our definition, compared with 18% of nonmigrants.[7] Given the skill differentials, it is not surprising that a greater percentage of migrants work in the formal sector.

When disaggregating employment by industry, we observe that a greater proportion of migrants engage in service-sector employment; 39% of migrants compared to 17% of nonmigrants in 2003. Nonmigrants are disproportionately employed in agriculture. While the agricultural sector remains an important contributor to Nepal's economy, from 1965 to 2010, the share of gross domestic product (GDP) accounted for by agriculture fell from 70% to 30% (ILO 2010). The share of GDP accounted for by services increased from 20% to more than 50% (ILO 2010). These trends suggest that immigration is likely to affect services, the sector that employs the greatest share of migrants.

7. Positive selection in terms of skills can also be shown with regression analysis. The years of education significantly increase the probability to migrate in 2003 and 2010, using an ordinary least squares or a probit estimation and controlling for age, gender, urban, and district fixed effects (not reported here).

Table 2. Summary Statistics: Means of Individual Characteristics of Migrants and Natives Aged 18–65, Weighted Means (and Standard Deviations)

	2003			2010			2003	2010
	Nonmigrant	Migrant	Diff. (*p*-value)	Nonmigrant	Migrant	Diff. (*p*-value)	Nonmigrant HH head	Nonmigrant HH head
Individual variables	(n = 7,303)	(n = 241)		(n = 14,367)	(n = 401)		(n = 2,742)	(n = 5,230)
Age	36.70	28.50	.000	37.80	25.70	.000	43.40	43.70
	(13.60)	(11.60)		(13.60)	(10.10)		(11.60)	(11.50)
Male	.53	.43	.000	.43	.24	.000	.85	.72
	(.50)	(.50)		(.50)	(.43)		(.36)	(.45)
Schooling	3.69	6.52	.000	4.25	8.24	.000	3.36	3.98
	(4.57)	(4.71)		(4.81)	(4.58)		(4.36)	(4.51)
Highly skilled	.14	.29	.174	.18	.46	.000	.12	.14
	(.34)	(.46)		(.39)	(.50)		(.32)	(.35)
Labor variables	(n = 7,303)	(n = 241)		(n = 14,367)	(n = 401)		(n = 2,742)	(n = 5,230)
Employed (last 12 months)	.90	.75	.358	.84	.58	.152	.97	.94
	(.30)	(.43)		(.37)	(.50)		(.17)	(.24)
Unemployed (last 12 months)	.03	.07	.000	.13	.26	.000	.01	.06
	(.18)	(.25)		(.34)	(.44)		(.12)	(.23)
Inactive (last 12 months)	.07	.18	.000	.03	.16	.375	.02	.004
	(.25)	(.39)		(.17)	(.37)		(.13)	(.06)
Work primary job:								
If employed in formal	(n = 6,572)	(n = 180)		(n = 12,068)	(n = 233)		(n = 2,660)	(n = 4,707)
	.26	.32	.084	.20	.27	.027	.31	.23
	(.44)	(.47)		(.40)	(.44)		(.46)	(.42)

Real wage:								
If employed in formal	(n = 1708)	(n = 57)		(n = 2,413)	(n = 63)		(n = 798)	(n = 1,080)
	10,276	10,221	.996	13,445	8,653	.569	14,765	17,582
	(80,981)	(18,267)		(63,605)	(8,107)		(114,300)	(89,454)
If employed in informal[1]	(n = 2,713)	(n = 84)		(n = 5,700)	(n = 75)		(n = 1,323)	(n = 2,034)
	1,566	1,584	.912	3,245	4,049	.783	1,890	3,676
	(5,561)	(2,919)		(24,501)	(10,973)		(7,301)	(27,204)
Share of migrants by industry	(n = 5,960)	(n = 151)		(n = 9,901)	(n = 173)		(n = 2,484)	(n = 4,264)
Agriculture, forestry, and fishery	.70	.52		.71	.53		.70	.67
	(.46)	(.50)		(.46)	(.50)		(.46)	(.47)
Services	.17	.39		.20	.35		.18	.22
	(.38)	(.49)		(.40)	(.48)		(.38)	(.41)
Manufacturing	.08	.08		.05	.06		.06	.05
	(.26)	(.27)		(.22)	(.25)		(.24)	(.23)
Construction	.05	.02		.04	.05		.07	.06
	(.21)	(.13)		(.21)	(.23)		(.25)	(.24)

Note.—Real wages expressed at the monthly level in 2010 rupees. *Highly skilled* is defined as having 10 or more years of schooling. HH = household.

[1] Real monthly wage for individuals in the informal sector constructed using agricultural or enterprise revenues per worker.

Restricting the nonmigrant sample to household heads changes the distribution of gender and age characteristics with negligible effects on educational endowment. Focusing on the heads produces a sample closer to full employment. As expected, household heads obtain greater formal- and informal-sector wages on average (than the complete sample of nonmigrants), and the difference is persistent over time.

3. METHODOLOGY

We use the following empirical model to account for changes in native labor market outcomes attributable to immigration:

$$Y_{ijt} = \alpha_1 + \beta M_{jt} + \lambda X_{ijt} + \gamma Q_{jt} + \delta_j + \delta_t + \epsilon_{ijt},\ t = [2003, 2010]. \tag{4}$$

The dependent variable Y represents the nonmigrant labor outcomes (employed, unemployed, and log monthly wages) for individual level i, living in district j at time t. Employment and wage variables are a function of several factors: the net labor migration rates M to area j over the last 4 years, a vector of demographic controls X that reflect one's earning potential (age, gender, education, occupation), a location variable Q (urban destination), a location fixed effect δ_j to reflect labor market differences at the district level, and a time fixed effect δ_t to account for time effects, common to all districts.

To deal with the endogeneity of the net migration rate M, predicted in- and out-migration rates are used to construct a net migration rate instrument for the observed net migration rates (Boustan et al. 2010). Our main results are drawn from a just-identified equation, known to be median unbiased and unlikely to be subject to weak instrumentation (Angrist and Pischke 2009). In section 4.4, we show the robustness of our results to the introduction of two separate instruments for the net migration rate, that is, the predicted in- and out-migration rates.

We follow Boustan et al. (2010) in how we compute the standard errors in the first- and second-stage regressions. The first-stage regressions use block-bootstrapped standard errors (clustering at the district level) to account for the fact that the predicted in- and out-migration rates are generated regressors. Errors are clustered at the district level in the second stage to allow for correlation between individuals within district-level labor markets.

3.1. Predicted In-migration Rate Component of Instrument

We first delineate how the predicted in-migration rate is computed using equations (5)–(7). The predicted in-migration rate IM for district j is the sum of the products of the predicted number of migrants leaving district k ($\widehat{O_{kt}} \times pop_{k1995}$) and the probability that these migrants move from district k to district j ($\widehat{P_{kjt}}$) for all k locations excluding own district j.

$$IM_{jt} = \sum_{k \neq j} \left(\widehat{O_{kt}} \times pop_{k1995}\right) \times \widehat{P_{kjt}},\ \text{with } t = [2003, 2010], \tag{5}$$

$$O_{kt} = \alpha_2 + \theta_1 Z_{kt-1} + \delta_k + \delta_t + \epsilon_{kt},$$
$$\text{with } t = [2000, 2001, 2002, 2003, 2007, 2008, 2009, 2010], \tag{6}$$

$$P_{kjt} = \alpha_3 + \phi f(d_{kj}) + \delta_t + \epsilon_{kt}, \text{ with } t = [2003, 2010]. \tag{7}$$

The predicted in-migration rates in (5) depend on the predicted out-migration rates ($\widehat{O_{kt}}$) estimated in (6). A linear probability model is used that includes lagged origin district weather shocks (Z_{kt-1}) (floods, droughts, and their interaction with river density) and district and time fixed effects.[8] Including the interactions of weather shocks with river density is motivated by the vulnerability of Nepali households to floods. Although lagged own district variables are used to predict out-migration rates, the final calculation of (5) excludes own district migration, which allows us to avoid endogeneity issues that might arise from using weather variables directly as instruments.

To deal with the risk of spatial dependency inherent when using weather-based data (Aufhammer et al. 2013) in the construction of the predicted out-migration rates, we correct the standard errors for time and spatial correlation (Conley 1999). We assume that spatial dependency disappears beyond a cutoff point of 64 kilometers, which corresponds to the maximum distance between the centroids of any pair of closest neighboring districts. Such a cutoff point is also larger than the average size of a grid (approximately 55 kilometers squared).[9] We also allow for time dependency of up to 2 years, which is larger than the minimum time lag (T powered 0.25) recommended by Green (2003) and Hsiang (2010).

The predicted in-migration rates in (5) also depend on the probability of moving from location k to location j ($\widehat{P_{kjt}}$). We estimate these probabilities denoted in (7) using a dyadic model. The probability of moving between districts depends on their proximity, or the Euclidean distance d_{jk}. We allow for a nonmonotonic relationship between the probability of moving between two locations and their proximity with the introduction of a quadratic term. Equation (7) is estimated using a linear prob-

8. Weather variables are not used directly as instruments, only to construct predicted in- and out-migration rates, which are the excluded instruments used in the analysis. A linear model is preferred since it allows us to correct for spatial dependency using the Conley (1999) correction of standard errors. We discuss the results from alternative specifications, such as the pseudo-maximum-likelihood estimator for fractional data that deals with the existence of abundant zeros, the addition of past migration rates using a dynamic panel model, and allowing for a conflict shock to affect migration flows in section 4.4. We also compare our results to those obtained using common IV approaches in the literature in section 4.4.

9. Our results are consistent when the following alternative cutoff points for spatial dependency are adopted: 34 kilometers, 128 kilometers (doubling our first cutoff), and the average value of the distance between any pair of districts.

ability model with time fixed effects δ_t to account for unobserved time-specific variables that influence migration. Standard errors are clustered at the origin district level.

3.2. Predicted Out-migration Rates Component of Instrument

Thus far, we have explained how we predict in-migration rates (IM_{jt}) using the predicted number of migrants leaving location k ($\widehat{O_{kt}} \times pop_{k1995}$) and the predicted probability of these migrants to move from k to j ($\widehat{P_{kjt}}$). We must also predict out-migration rates to have the complete set of variables necessary to construct our excluded instrument (predicted net migration) in equation (4). Out-migration rates are computed in a similar fashion from equations (8)–(10) below:

$$OM_{jt} = \sum_{k \neq j} \left(\widehat{I_{kt}} \times pop_{k1995}\right) \times \widehat{P_{jkt}}, \text{ with } t = [2003, 2010], \tag{8}$$

$$\begin{gathered} I_{kt} = \alpha_2 + \theta_1 Z_{kt-1} + \delta_k + \delta_t + \epsilon_{kt}, \\ \text{with } t = [2000, 2001, 2002, 2003, 2007, 2008, 2009, 2010], \end{gathered} \tag{9}$$

$$P_{jkt} = \alpha_3 + \phi f(d_{jk}) + \delta_t + \epsilon_{kt}, \text{ with } t = [2003, 2010]. \tag{10}$$

Equation (8) denotes the predicted out-migration rate OM_{jt} of migrants from location j. The predicted out-migration rate from j is estimated as the sum over all destination districts k ($k \neq j$) of the number of migrants settling in destination district k who are estimated to come from source district j. Equation (8) provides the predicted in-migration rate for districts estimated in a similar form to equation (6). From (10), a function of distance across districts is used to estimate the likelihood of individuals leaving source region j to move to region k. Predicted district-level observations of P_{jkt} and I_{kt} from equations (9) and (10) are used to create predicted out-migration flows in (8).

Our identification strategy hinges on the assumption that the predicted net migration rate affects individual labor market outcomes at the destination only through its effect on net migration.[10] By focusing on district-level migration rates, we essentially reduce the potential for the exclusion restriction to be violated due to the spatial correlation of shocks across cities and villages within the same district. Furthermore, by including district fixed effects, we control for unobserved factors at the destination that might be correlated with net migration and affect labor market outcomes.

10. The average net migration rate (table 3) is slightly lower than rates observed in the US literature but within the realm for internal migration in developing countries (Strobl and Valfort 2015).

The main threat to identification would come from spatial correlation between the weather-based variables used to predict net migration rates from sending districts and unobserved local labor market conditions at the district of destination (Boustan et al. 2010; Pugatch and Yang 2011). The exclusion restriction is unlikely to hold if lagged weather shocks affect labor markets in neighboring districts. This is certainly one rationale for lagging these variables when predicting in- and out-migration. Yet we cannot rule out that (lagged) environmental shocks are correlated across districts and feature enough persistency to threaten the validity of the exclusion restriction. We will therefore test the robustness of our analysis in section 4.3 by augmenting the regressions in equation (4) to include spatially lagged environmental shocks that explicitly control for spatial correlation across districts. Another concern is related to the presence of area-specific trends in weather shocks and other variables of interest. Table 3 indicates that based on the panel unit root test introduced by Maddala and Wu (1999), we can reject the null hypothesis that our main variables are nonstationary. The potential confounding role of omitted time variant variables is evaluated in section 4.3.

4. RESULTS

4.1. Predicting Net Migration Rates as Instruments and First-Stage Analysis

We first present the parameter and standard error estimates from the OLS version of (6) (col. 2, table 4). An increase of 1 standard deviation in flood incidence during the heavy monsoon (0.387) reduces the out-migration rate by 0.0006 (at mean river density). Given the mean value of the out-migration rate (0.005), the impact corresponds to a reduction of 12%. However, flood exposure, particularly in areas with dense river networks, can push individuals out of their locations of origin. For example,

Table 3. Descriptive Statistics for District-Level Variables, Periods 2000–2003 and 2007—10 (Districts = 69, n = 552)

	Mean	SD	Fisher's Test
Flood during heavy monsoon (unweighted)	.183	(.387)	329***
Drought during heavy monsoon (unweighted)	.308	(.462)	443***
Total conflicts per square km	.002	(.009)	120
River density (length of river per square km)	.171	(.023)	343***
Actual migration outflow rate from district	.005	(.007)	358***
Actual migration inflow rate to district	.003	(.005)	329***
Aggregate actual net migration rate (cumulative 4-year) (weighted by sample size in each district)	.005	(.031)	919***

*** Significant at 1%.

Table 4. Construction of Instrument and First-Stage Regression

Regression:	IV Construction				First-Stage
Dependent Variable:	Out-migration Rate OLS		In-migration Rate OLS		Actual Net Migration Rate OLS
	(1)	(2)	(3)	(4)	(5)
Predicted net migration rate (cumulative 4-year)					2.606***
					[.925]
Flood in heavy monsoon at $t-1$	–.002***	–.013***	–.000	.003	
	(.001)	(.005)	(.000)	(.004)	
Drought in regular monsoon at $t-1$	–.001	–.000	–.001	–.001	
	(.001)	(.004)	(.000)	(.003)	
Flood in HM at $t-1$ × River density		.067***		–.019	
		(.025)		(.023)	
Drought in RM at $t-1$ × River density		–.004		.000	
		(.021)		(.015)	
Individual controls					Y
Occupational dummies					Y
Observations	552	552	552	552	7,965
R-squared	.013	.019	.004	.005	.639
F-statistic					65.32***
F-statistic on excluded IV[a]					22.53
Stock-Yogo critical values 10% maximal IV size					16.380
Dependent variable means	.005	.005	.003	.003	.005
SD	(.007)	(.007)	(.005)	(.005)	(.031)

Note.—Time and district-origin fixed effects for specifications (1)–(2) and destination fixed effects for specifications (3)–(4) are included. Based on Conley (1999), robust standard errors in parentheses are corrected for spatial dependency with a cutoff point of 64 kilometers. Standard errors in brackets (specification [5]) are bootstrapped and clustered at the district level. HM = heavy monsoon; RM = regular monsoon.

[a] The *F*-test on excluded IV is provided by the Kleibergen-Paap rk Wald *F*-statistic.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

consider individuals living in areas where the river density is 2 standard deviations above the mean. An increase of 1 standard deviation in flood incidence elevates their chance of out-migration by 12%.

The estimates for the in-migration rate counterpart regression, equation (8), are featured in column 4 (table 4). Here, flood and drought incidence do not seem to affect in-migration rates. The lack of significance may be attributable to the uncertainty of weather events at places of destination. An alternative explanation is that migrants may diversify out of agriculture at destinations whereby economic livelihoods are less reliant on weather events. We later compare results from our preferred specification to those imputed from a dynamic model that improves the predictive power of in-migration rates in section 4.4. Such comparisons insure that our final conclusions in the second stage are robust to auxiliary specifications.

We next turn to the models used to predict the probabilities of moving from district k to j and vice versa ([7] and [10]). Both specifications (results not shown here) suggest a convex relationship between the probability of moving and distance: the probability is almost always negatively correlated with the linear term (for 124 and 127 of the 138 estimated pairs in P_{kj} and P_{jk}, respectively) and positively correlated with the squared term (for 132 and 136 of the 138 estimated pairs in the same two specifications). The small sample of district pairs, however, influences the precision of our estimates. About 25% of the coefficients on the linear and squared distance variables are statistically significant at the 10% critical level in both probability specifications.

The last column in table 4 presents the results from the first-stage regression. The first-stage Kleibergen-Paap F-statistic for the excluded instrument is 22.5 (well above the Stock-Yogo critical value). Thus, it is very unlikely that our estimates are biased by having a weak instrument. We will discuss further the validity and the robustness of our first stage in sections 4.3 and 4.4, respectively.

Figure 3 maps the predicted and observed net migration rates. Although strongly correlated in areas with major cities, the two maps substantially differ in that the predicted figures capture a subsample of the observed net migration rates. The striking differences across predicted and observed net migration rates emphasize that the interpretation of our results is not generalizable to any type of migrants in Nepal. In other words, our instruments produce internally valid estimates of the causal effect of environmental migration on labor outcomes. However, given the potential differences in the patterns and selection of environmental migrants versus migrants more broadly, we estimate a local average treatment effect specific to environmental migration. In essence, our estimates are not externally valid for other forms of migration in Nepal.

4.2. Impact of Migration on Hosting Labor Markets

Estimates of the impacts of net migration rates on wages are displayed in table 5. The two-stage least-squares results indicate a strong negative impact in the formal

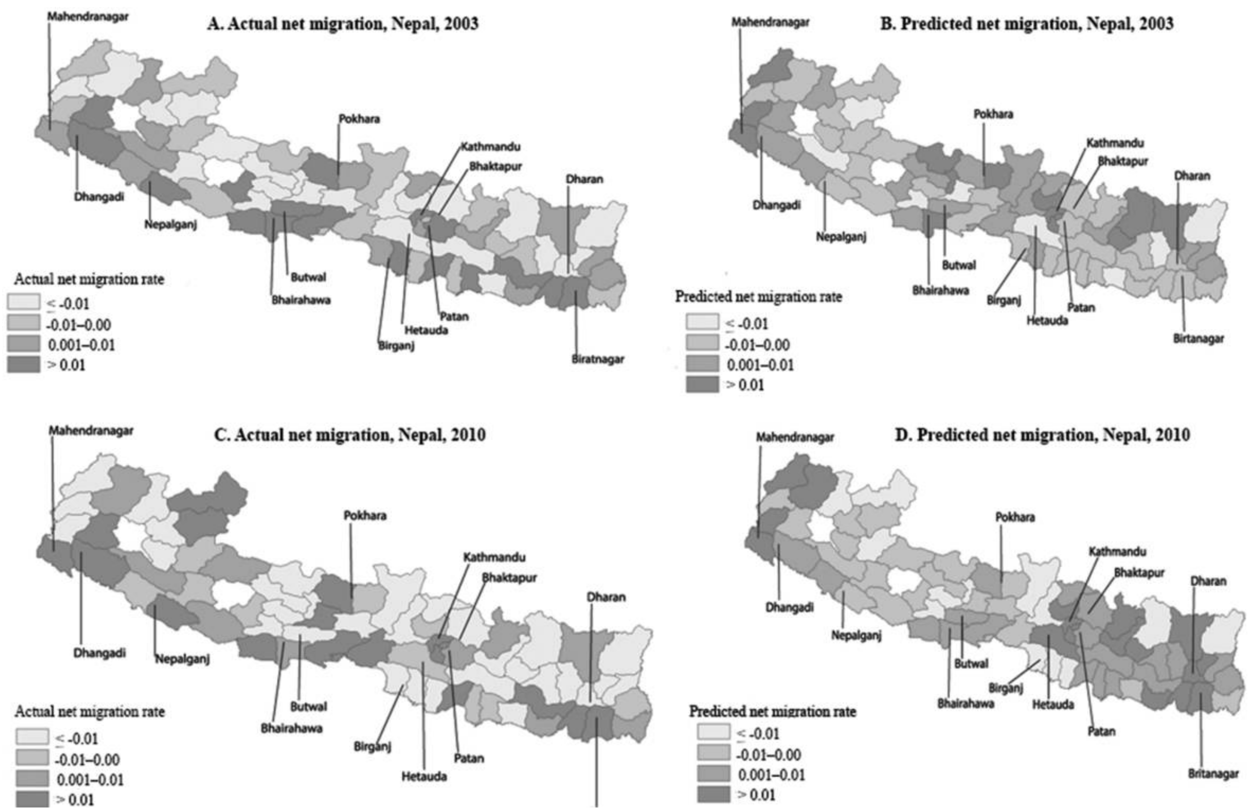

Figure 3. Actual and predicted net migration, Nepal, 2003 and 2010. Source: Authors' representation based on own calculations. Districts not used in analysis are omitted from maps. A color version of this figure is available online.

Table 5. Effect of Net Migration Rate on Wages for Nonmigrant Household Heads Aged 18–65 (Second Stage)

	Dependent Variable: Log Monthly Real Wages (2010 Nepal Rupees)					
	All		Formal Sector		Informal Sector	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Net migration rate (cumulative 4 year)	−1.601	−.885	−5.073***	−5.707***	1.162	2.659
	(.962)	(1.571)	(.560)	(.650)	(1.554)	(2.633)
Individual control	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y
Observations	5,234	5,234	2,119	2,119	3,113	3,113
R-squared	.51	.51	.285	.285	.365	.365
Districts	69	69	67	67	69	69

Note.—Time and district fixed effects included. Standard errors clustered at the district level in parentheses. In all subsequent specifications, 2SLS refers to the second-stage of the two-stage least squares estimates using net migration rates to instrument actual net migration rates. Individual controls include age, gender, and education (years of schooling). Occupation dummies are controls for participation in agriculture, manufacturing, and construction.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

sector (col. 4, table 5). An increase by 1 percentage point in net migration rates would translate into a fall in real wages by 5.7%.[11] The spatial distribution of the

11. One concern is that the estimates may be driven by approximately 642 migrants scattered over few districts. We find that the NLSS net migration rates are relatively similar to those obtained from the census. Computing the net migration rates over the last 5 years for both the NLSS and the census generates similar distributions across districts with coefficients of correlation of 0.74 between the NLSS 2003 and the census 2001 and of 0.66 between the NLSS 2010 and the census 2011. Unfortunately, the census data could not be used as a substitute in the pre-first-stage analysis, since the census does not provide precise information about the year of departure for the migrants and the differences in the timing of interviews across data sources (NLSS vs. census) obfuscates comparisons. As an additional robustness check, we reproduce the regression results excluding each district from the sample and find similar wage effects. The only exception is when Kathmandu is omitted; the coefficient increases to −7.1 but is imprecisely estimated (with a coefficient of 1.06 significant at the 5% critical level in the corresponding first-stage regression). The first-stage relationship remains strong (the Kleibergen-Paap F-statistics for the excluded instrument ranges between 10.4 and 48.5) upon excluding each district from the sample.

formal-sector wage effects are presented in figure 4. Wage effects are not necessarily concentrated in areas with a higher exposure to flooding (fig. 1). An increase in net migration rates from increased frequency of droughts and floods in this part of the world is expected to have profound effects on the economic geography of Nepal. There is quite a bit of variation in the wage effects across space that corresponds to district migration hot spots depicted in figure 3 suffering the most negative consequences.

Although our conceptual framework suggests that changes in the skill composition of the work force could explain the absence of wage effects in the informal sector,

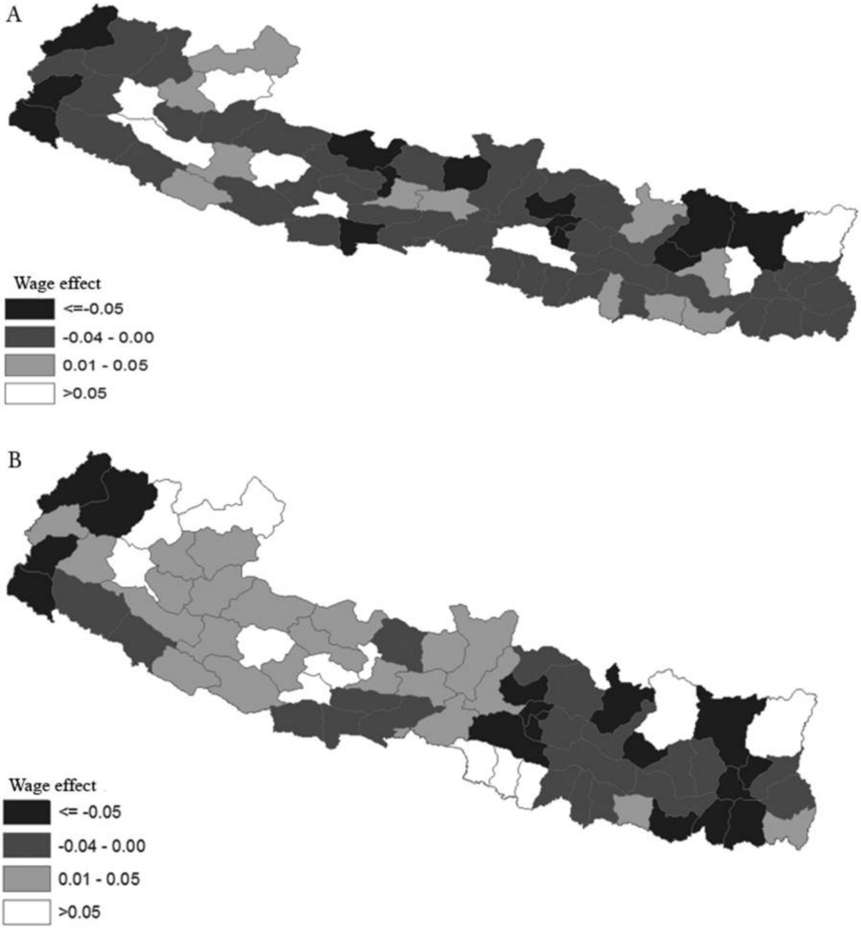

Figure 4. *A*, Estimated effect on formal-sector wages of a 1% increase in within-district predicted net migration rate, 2003. *B*, Estimated effect on formal-sector wages of a 1% increase in within-district predicted net migration rate, 2010.

we cannot rule out the sensitivity of our findings to the measurement of wages. We therefore focus on wage effects in the formal sector. To facilitate economic interpretation of results, we draw on comparisons in formal sector wage effects across worker types. We find that net migration negatively affects the real wages of high-skilled nonmigrants (cols. 1–2, panel A, table 6), especially in the formal sector where most

Table 6. Effect of Net Migration Rate on Wages for Nonmigrant Household Heads Aged 18–65, by Skill (Second Stage)

	Dependent Variable: Log Monthly Real Wages (2010 Nepal Rupees)			
	High Skill		Low Skill	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)
	A. All Sectors			
Net migration rate (cumulative 4-year)	−1.9396*	−1.253	−.6378	.510
	(1.068)	(1.453)	(1.133)	(2.026)
Individual controls	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y
Observations	1,075	1,075	4,154	4,154
R-squared	.464	.464	.480	.480
Districts	60	60	69	69
	B. Formal Sector			
	High Skill		Low Skill	
	OLS (5)	2SLS (6)	OLS (7)	2SLS (8)
Net migration rate (cumulative 4-year)	−1.6747**	−1.8479**	−5.3968***	−6.6017***
	(.705)	(.866)	(.745)	(.938)
Individual controls	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y
Observations	573	573	1,530	1,530
R-squared	.171	.171	.250	.250
Districts	45	45	66	66

Note.—Time and district fixed effects included. Standard errors clustered at the district level in parentheses. High skill refers to those individuals with at least 10 years of education.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

(relatively) high-skilled migrants are competing (cols. 5–6, panel B, table 6). The negative impact found in the formal sector for the low-skilled workers (cols. 7–8, panel B, table 6) confirms that effects transcend beyond workers of a similar profile to migrants due to the substitution between skilled and unskilled labor in the production of formal sector goods.

Tables 7 and 8 point to another source of vulnerability for low-skilled workers. Low-skilled workers face a lower probability of employment (cols. 9 and 10, table 7) and a higher probability of unemployment (cols. 5 and 6, table 8).[12] Raising net migration by 1 percentage point increases the unemployment of unskilled workers by 1.8 percentage points (by 1 percentage point for all workers). A slightly lower (reverse) impact is found for employment probability. Employment and unemployment probabilities have the expected sign for skilled workers, although statistically significant for the probability to be unemployed (cols. 3 and 4, table 8). Such contrasting results are consistent with a greater displacement of low-skilled workers out of the labor market.

While the employment effect mirrors those estimated elsewhere,[13] our wage estimates are larger than those identified in other studies. Specifically, our 5.7% decline in formal sector wages exceeds the 1.2% (formal and informal sectors pooled) and 2.45% (informal sector only) changes reported in Indonesia (Kleemans and Magruder 2014) and the 3.15% decline (all sectors) estimated in Thailand (El Badaoui et al. 2014).[14] Focusing on the negative wage impacts on low-skilled workers, our estimates in Nepal (−6.6%) are closer in magnitude to the low-skilled wage effects found in Thailand (−5.3%) and greater than those measured in Indonesia (−1.18% in the formal sector vs. −3.23% in the informal sector). One possible explanation is that financial constraints pose greater barriers to adjustment in Nepal than in Indonesia and Thailand. The World Bank's Doing Business ranking for accessing credit is 116 in Nepal, compared to 28 in Thailand and 71 in Indonesia (World Bank 2015). The differential effects observed in Nepal are likely driven by the country's inability to absorb the excess labor supply from migration and increase aggregate productivity in the same manner witnessed elsewhere (Olney 2013).

12. Since we do not observe the sector of unemployment for workers who are not currently employed, we estimate the effect of environmental migration on the unemployment of workers by skill.

13. Strobl and Valfort (2015) find a decline of 0.78 (1.03 for regions with low road density) in Uganda associated with an increase in 1 percentage point of in-migration. Employment effects are not reported in El Badaoui et al. (2014), but Kleemans and Magruder (2014) detect a 0.26 percentage point change in the individual probability of employment.

14. El Badaoui et al. (2014) do not distinguish by sectors, while comparisons with the estimates produced in Kleemans and Magruder (2014) are imperfect since we use net migration rates to correct for possible nonmigrant displacements.

Table 7. Effect of Net Migration Rate on Employment for Nonmigrant Household Heads Aged 18–65 (Second Stage)

	Dependent Variable: Employment Probability (Worked in Last 12 Months)					
	Panel A					
	All		Formal Sector		Informal Sector	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Net migration rate (cumulative 4-year)	−.721***	−1.022***	.459*	.827	−1.132***	−1.762**
	(.110)	(.177)	(.241)	(.590)	(.209)	(.690)
Individual controls	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y
Observations	7,965	7,965	7,965	7,965	7,965	7,965
R-squared	.115	.114	.055	.055	.040	.040
Districts	69	69	69	69	69	69
	Panel B					
	High Skill		Low Skill			
	OLS (7)	2SLS (8)	OLS (9)	2SLS (10)		
Net migration rate (cumulative 4-year)	−.113	−.274	−.710***	−1.083***		
	(.170)	(.221)	(.163)	(.209)		
Individual controls	Y	Y	Y	Y		
Occupation dummies	Y	Y	Y	Y		
Observations	1,358	1,358	6,604	6,604		
R-squared	.182	.181	.111	.111		
Districts	64	64	69	69		

Note.—Time and district fixed effects included. Standard errors clustered at the district level in parentheses.

* Significant at 10%.

** Significant at 5%.

*** Significant at 1%.

Table 8. Effect of Net Migration Rate on Unemployment for Nonmigrant Household Heads Aged 18–65 (Second Stage)

	Dependent Variable: Unemployment Probability (Worked in Last 12 Months)					
	All		High Skill		Low Skill	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)	OLS (5)	2SLS (6)
Net migration rate (cumulative 4-year)	1.011***	1.476***	.552***	.684***	1.147***	1.813***
	(.211)	(.190)	(.163)	(.184)	(.329)	(.235)
Individual controls	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y
Observations	7,965	7,965	1,358	1,358	6,604	6,604
R-squared	.100	.099	.153	.153	.095	.092
Districts	69	69	64	64	69	69

Note.—Time and district fixed effects included. Standard errors clustered at the district level in parentheses.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

4.3. Validity of the Instruments

The identification strategy hinges on two main assumptions: the strength and the exogenous nature of the predicted net migration rates used as instruments. First, the *F*-tests, assuming weak instruments, indicate that the instruments are strong predictors of the actual net migration rate (table 4). The Kleibergen Paap rk Wald *F* statistic stands at 22.53 for our preferred linear specification, which exceeds the Stock and Yogo (2005) critical values with 10% absolute bias. We also note that the predicted net migration rates positively affect observed net migration rates, which is reassuring given that just-identified estimates are median unbiased.

Second, it is plausible that the predicted migration rates affect labor market outcomes only through observed migration rates. In section 3, we rationalize the focus of the analysis at the district level and the use of lagged environmental shocks in predicting migration rates to satisfy the exclusion restriction. One possible violation of the exclusion restriction would nonetheless result if weather shocks in neighboring districts have direct impacts on labor market outcomes. We therefore test the stability of our coefficients of interest in the second-stage regressions to the inclusion of spatially lagged variables. The spatially lagged variables are obtained by multiplying the variables used to predict migration in equations (3) and (6) with a distance-based spatial matrix that weighs the value of each variable for one district by the inverse of the Euclidean distances to the geographical centers of all other districts (Anselin 2002). The inclusion of these spatially lagged variables does not alter substantially the magnitude of the impact of migration on labor market outcomes.[15] We can therefore rule out the possible threat to our identification strategy that would result from spatial spillovers from environmental shocks.

The presence of area-specific trends in weather shocks, migration flows, and labor outcomes poses a final concern that regression estimates are based on spurious correlations. As stated, simple descriptive tests reject the null hypothesis of nonstationary variables. The addition of region-specific time fixed effects improves the efficiency of our results and slightly increases the magnitude of the migration impacts on labor outcomes.[16]

4.4. Robustness

We compare the results from our preferred model to those from alternative model specifications. We modify (*a*) the pre-first-stage analysis implemented to construct our instruments, (*b*) the first-stage specifications to allow for multiple instruments, and (*c*) the second-stage regressions allowing for alternative definitions of samples and variables.

4.4.1. Pre-First-Stage Analysis

To predict migration rates, a linear model is preferred since it facilitates the application of the Conley (1999) procedure to deal with potential spatial dependency in the error terms. Nonetheless, concerns may be raised about that choice. First, the abundance of zero observations in the migration rates (in our case, 33%) may affect the consistency of our estimates (Beine, Docquier, and Ozden 2011). Given the fractional nature of our migration rates, we implement the pseudo-maximum-likelihood estimator proposed by Santos Silva, Tenreyro, and Wei (2014) and based on Papke and Wooldridge (1996). Following Santos Silva et al. (2014), we directly report the partial effects of the regressors. Estimates in table A.3 (cols. 3 and 4) indicate that the results

15. Results are provided in table A.1. (Tables A.1–A.11 are available online.) Note that the spatially lagged variables of floods and their related interaction terms are far from significant. Slight evidence of spatial spillovers is found for droughts but fortunately, that is less of a problem since our identification relies on the flood shocks.

16. Due to multicollinearity, district-specific time trends cannot be added. As an alternative, we introduce trends specific to the five Nepalese regions, that is, Eastern, Central, Western, Mid Western, and Far Western. Results are provided in table A.2.

are very close to those provided in our linear model with standard errors corrected for spatial dependency.[17]

Second, we may overestimate the effects of environmental migration by capturing omitted variables such as conflict or historical migration networks in the pre-first-stage regressions.[18] Excluding conflict incidence may lead to an overestimation of the environmental factors of migration if weather shocks are somehow related to violence, as has been found in other contexts (Hsiang, Burke, and Miguel 2013; Burke, Hsiang, and Miguel 2015). Civil war has been a major factor driving migration in Nepal from 1999 to 2006 (Bohra-Mishra 2011). Violent outbreaks led to the movement of political refugees away from conflict-prone areas. The predicted probability of migration decreased for moderate levels of violence and increased as violence became more intense (Bohra-Mishra 2011).[19] Adding a lagged measure of conflict hardly changes the point estimates in the out-migration rate model (eq. [6]) but improves the predictive power of the in-migration model (eq. [9]).[20] One motivation for excluding conflict from the underlying empirical models is that it violates the stationarity assumption. In particular, when we apply the Fisher test for panel unit root using an augmented Dickey-Fuller test, we cannot reject the null hypothesis of nonstationarity (Maddala and Wu 1999).

To examine the robustness of our results to the inclusion of measures of historical networks, we consider a dynamic model that controls for the lagged out-migration rate in equation (6) and the in-migration rate in equation (9). A standard system of generalized method of moments (GMM) dynamic model (Blundell and Bond 1998) is applied with robust standard errors.[21] Columns 6–8 and 17–19 of table A.3 show

17. Log-linearizing the main regressions and applying the Poison regression model proposed by Santos Silva and Tenreyro (2006) yield similar results (not reported here).

18. In addition to the omission of conflict and historical network variables, ignoring spatial spillovers could affect predictions in the pre-first-stage analysis. Spatial autoregressive Lee and Yu (2010) and Durbin models provide similar results (table A.3).

19. Urbanization and labor markets have been affected by conflict in other settings (Kondylis 2010; Alix-Garcia, Bartlett, and Saah 2013; Maystadt and Verwimp 2014; Alix-Garcia and Bartlett 2015).

20. Results are presented in table A.3. A conflict event is defined as a single altercation in which one or more groups use force for a political end (Raleigh et al. 2010). Following this definition, the number of conflict events per square kilometer is defined by district year for the 4 years prior to 2003 and 2010. Between 1996 and 2006, the end of the civil war, about 3,030 conflict events were reported in the ACLED data set for Nepal. Figure A.3 maps the conflict variable in the districts of Nepal over time.

21. The method provides more efficient estimates than difference GMM estimation (Arellano and Bond 1991) but requires an additional assumption with respect to stationarity. We apply Fisher's test for panel unit root using an augmented Dickey-Fuller test (Maddala and

that our results are largely robust. To give perspective on the relative importance of flooding on out-migration rates, auxiliary factors, as proxied through the lagged out-migration rate, influence out-migration rates by a similar order of magnitude. Given the mean value of the out-migration rate (0.005), an increase of 1 standard deviation in historical out-migration rate augments out-migration rates by 32% compared with about a 18%–20% reduction from an equivalent increase in flooding exposure (cols. 7–8, table A.3). While the number of conflicts also has a consistently positive effect on out-migration rates, the effects are smaller with an increase of 1 standard deviation, leading to a 6% increase in out-migration rates (col. 8, table A.3). We are unable to rule out, however, that the small magnitude of the effect of conflict on out-migration rates is partly explained by the likely correlation between weather shocks and conflict (Hsiang et al. 2013; Burke et al. 2015).

We briefly remark on the dynamic in-migration rate model (cols. 17–19, table A.3). Lagged migration is the only statistically significant determinant, reflecting strong network effects. Despite the fact that lagged migration is a strong predictor of migration and conflict appears to significantly hinder in-migration, we defer to our preferred specification. Adding conflict or using a dynamic model requires the imposition of additional assumptions in order to satisfy the exclusion restriction.

A final concern related to our pre-first-stage regression estimates is that inferences may vary with the recall period used to construct the migration flows. Adjusting the number of years over which migration is observed renders no impact on the predicted migration rates (table A.4). We later confirm that the use of these predicted migration rates produces consistent results in the second-stage regressions.

4.4.2. First-Stage Specifications

Our preferred model is based on a just-identified first-stage equation since it has been proven to be median unbiased and unlikely to be subject to weak instrumentation (Angrist and Pischke 2009). We consider how treating the predicted in- and out-migration migration rates as two separate instruments for the observed net migration rates might influence our results. Column 2 of table A.5 replicates our first-stage regressions with these two instruments. As expected, we obtain a positive and negative

Wu 1999). For our main variables reported in table 3, we can reject the null hypothesis of nonstationarity in all variables at any reasonable confidence level. One exception is the number of conflicts per square kilometer, but our results do not depend on the inclusion of the conflict variables. To validate the consistency of the GMM estimator, the test for the first-order serial correlation rejects the null hypothesis of no correlation, while the hypothesis for second-order serial correlation cannot be rejected. The Sargan test for overidentification does not reject the null hypothesis of zero correlation between the instrumental variables and the error term.

coefficient for the predicted out-migration and in-migration rates, respectively. Both instruments continue to be strong predictors of actual net migration rates as indicated by the *t*-tests and *F*-tests. The Kleibergen Paap rk Wald *F* statistic exceeds the Stock and Yogo (2005) critical value with 15% absolute bias.[22] Reviewing the first-stage estimates from multiple empirical models only enhances confidence about the strength of our instruments (table A.5).[23]

4.4.3. *Variants of the Second Stage*

We also assess the robustness of our results to alternative second-stage regressions, allowing for alternative definition of samples and variables. The data constraints to measure individual income generated from family businesses (a large portion of the informal sector) led us to focus on household heads. We first verify our formal sector results hold when including the entire sample of formal sector workers in table A.6. As expected, most coefficients at the individual level are slightly less precisely estimated but overall confirm the results using our preferred sample.

We second challenge the definition of skilled employment (currently proxied by those with 10 years or more of education), instead using the median value of years of education (about 3 years) as the cutoff. Redefining skilled employment produces less precise estimates (table A.7). Generally, our main conclusions are unadulterated (table A.7).

We lastly explore sources of heterogeneity, by differentiating effects by location and gender of the household head. Table A.8 splits the sample across those dimensions. The urban sample is defined as districts with a share of urban population above the sample median. Both dimensions do not seem crucial to understand the impact of environmental migrants on the labor markets in Nepal. The focus on household heads reduces sharply the efficiency on our estimates from the sample of women. Like El Badaoui et al. (2014), we find lower wage elasticity for women compared to men yet a higher employment elasticity. Similarly, the wage response to migration appears to be stronger in urban areas, but labor supply seems more elastic in rural areas. Despite these slight differences, the results are fairly consistent across the gender or urban dimensions.

22. The *F*-statistics on the excluded IVs are above the rule of thumb of 10 provided by Stock and Yogo (2005). When using the predicted out-migration and in-migration rates as separate instruments, the Hansen J test features a *p*-value above .100. It should be noted that the two instruments are similar in nature and the test assumes that at least one instrument is valid.

23. Results from the corresponding second-stage regressions are largely unaltered, if not more efficiently estimated. These are not reported here for ease of presentation but are available upon request from the authors.

4.4.4. Comparison to Other IV Approaches

We compare estimates from the preferred Boustan et al. (2010) method to those from common IV approaches used in the literature. Table A.9 displays the results assuming net migration rate as the endogenous outcome in the first stage. For brevity, the presentation of second-stage estimates is limited to the wage equation in the formal sector (equivalent to the second-stage results presented in column 4 of table 5).

The first approach utilizes weighted lagged rainfall variables as instruments for migration (Pugatch and Yang 2011; Kleemans and Magruder 2014). Pugatch and Yang (2011) construct their migration instrument by taking the sum of weighted lagged rainfall at Mexican state *m* over all Mexican states. The weight is based on the historical share of migrants in the labor force: the migrant stock from origin state *m* in destination state *u* (calculated using data from 1925 and 1930) divided by the number of working males in state *u* in 1970. Here, the exclusion restriction is satisfied as long as unobserved factors driving migration selection bias are not persistent over time. To emulate the approach in our setting, we construct a similar time invariant weight based on the average of the migrant share moving from one district to the other between 1992 and 1999 (cols. 2 and 3, table A.9). The weight is then interacted with time-varying weather variables, in the spirit of Pugatch and Yang (2011). The second-stage results in columns 2 and 3 give a coefficient between −6.6 and −6.3, which is slightly higher in magnitude than the coefficient of −5.7 reported in column 4 of table 5.[24]

The second approach in the literature is to exploit the durability of the migration network without taking into account shocks at the migrant's origin (see Pugatch and Yang [2011] for a discussion). Specifically, weighted immigration flows are used as direct instruments for the net migration rate, where the weights are based on the historical shares of migrants from a particular origin at the destination (Card 2001, 2009). We compute the historical share of migrants in a destination district (averaged between 1992 and 1999), multiplied by the total number of migrants in the destination at time *t* and expressed as a proportion to the population in the destination at time *t*. The second-stage coefficient in the wage equation in the formal sector lies between −7.3 and −6.3.

A major limitation to standard approaches in our setting is the greater potential for the exclusion restriction to be violated. One practical issue in developing countries is

24. Applying a similar weight over the migration sample period (2000–2010) (Kleemans and Magruder 2014) produces estimates even closer in magnitude to those generated from our preferred specification, −5.8 and −6.0, respectively. Columns 2–5 of table A.9, A.10 have also been reproduced using alternative aggregation periods in the construction of the related weight, namely 1992–95, 1992–96, 1992–97, and 1992–98 (instead of 1992–99). Results remain largely unaltered.

information on historical migration patterns is unavailable. We instead are relegated to construct weights based on migration patterns as soon as 5 years prior to the study period. These weights are subject to more scrutiny in satisfying the exclusion restriction. Even in an ideal situation where the weights rely on migration information from a distant past, unobserved factors that drive immigration to a particular destination may persist over time biasing the estimate in the second stage (Pugatch and Yang 2011).

To examine the validity of the exclusion restriction in all approaches, we set up a falsification test replacing the first-stage outcome of actual net migration rates with past migration. Past migration rates are constructed in a similar way, that is, the cumulative migration rates 5 years prior to 2003 and 2010. The results in the appendix (table A.10; appendix available online) demonstrate alternative instrumental variables appear to be strongly correlated with past migration rates in contrast to the predicted migration rates inspired by Boustan et al. (2010). Overall, we observe similar wage effects as those generated from the preferred approach, but at a cost of raising additional endogeneity concerns in our particular setting.[25]

5. CONCLUSION

We employ the Boustan et al. (2010) multistage procedure to identify the effects of environmental migration on the labor markets of hosting communities. Our results indicate wage losses as a result of environmental migration. An increase of 1 percentage point in net migration reduces wages in the formal sector by 5.7%. Employment decreases by 1 percentage point following a similar increase in net migration rate.

Our identification strategy produces the local average treatment effect of environmental migration on wages and employment. For this reason, it is no surprise that the magnitude of the decline in wages exceeds the effect sizes estimated for economic migration in the US literature. For example, Altonji and Card (1991), Pugatch and Yang (2011), and Ottaviano and Peri (2012) find 1%–2% declines in wages among low-skilled workers. Environmental migrants in Asia tend to be positively selected and originate from distinct areas than traditional migrants. Internal migrants in Asia are attracted to cities (Deshingkar 2006). Selectivity in environmental migrant worker attributes and limited destination choices offer explanations for the larger impacts than typically measured in the US immigration literature.

The wage effects estimated in Nepal slightly exceed those measured in other Asian countries (El Badaoui et al. 2014; Kleemans and Magruder 2014). Many countries in this region of the world experience frequent natural disasters. Indonesia and Thailand

25. Since some pieces of the previous literature ignore the effect of local displacement by evaluating the impact of migration inflows on wages rather than net migration rates on wages, we compare the overall losses from this oversight. We find in general larger wage effects in a range between −9.1 and −10.4 but weaker first-stage results (table A.11).

arguably obtain more resilient business climates, with greater access to capital to facilitate market adjustments.

Although migrants are positively selected, we find that employment effects are not concentrated among workers with high substitutability, as in Indonesia (Kleemans and Magruder 2014). The decline in the employment of low-skilled workers in communities hosting environmental migrants raises questions with respect to viable social protection policies. While migration can be a welfare-enhancing adaptation strategy (Bryan, Chowdhury, and Mobarak 2014), our results suggest that additional reforms may be necessary to foster resilience in neighboring markets. The provision of grants to support enterprises following periods of disasters may be a feasible alternative (De Mel, McKenzie, and Woodruff 2012). Access to financial capital could support new enterprises or encourage older enterprises to grow achieving long-term productivity gains.

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