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Emigration and democracy☆

Frédéric Docquier [a,*], Elisabetta Lodigiani [b], Hillel Rapoport [c], Maurice Schiff [d]

[a] FNRS and IRES, Université Catholique de Louvain, Belgium
[b] Ca' Foscari University of Venice, Centro Studi Luca d'Agliano, Italy
[c] Paris School of Economics, University Paris 1 Pantheon-Sorbonne, France, and Bar-Ilan University, Israel
[d] IZA-Bonn, Institute for the Study of Labor, Germany

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ABSTRACT

International migration is an important determinant of institutions, not considered so far in the development literature. Using cross-sectional and panel estimation for a large sample of developing countries, we find that openness to emigration has a positive effect on home-country institutional development (as measured by standard democracy indices). The results are robust to a wide range of specifications and identification methods. Remarkably, the cross-sectional estimates are fully in line with the implied long-run relationship from dynamic panel regressions.

1. Introduction

Recent research has emphasized the importance of institutions for comparative development (e.g., Acemoglu et al., 2005a; Rodrik, 2007) and explored the determinants of institutions. This paper emphasizes the role of emigration in determining institutions, building on cross-country comparisons for a large set of developing countries over the last thirty years.

Fig. 1 shows a close association between emigration and standard indicators of democracy over the period 1980–2010 (for the full set of developing countries): the Freedom House's indices of political rights (PR) and civil liberties (CL) increased by 53 and 60%, respectively; the Simon Fraser Institute's index of Economic Freedom of the World (EF) and the Polity IV Project's index of democracy (P2) increased by 35 and 116%. During the same period, the average emigration rate of developing countries was multiplied by 2.13. This paper investigates whether the positive relationship between openness to emigration and institutional development holds once we control for a number of important variables (such as human capital, income per capita, ethnic fractionalization, trade openness, as well as geographic characteristics) that have been shown to determine institutions, and also survives the introduction of regional fixed-effects. Moreover, we investigate whether the relationship between emigration and democracy can receive a causal interpretation.

We first assess the effect of emigration on institutional quality in OLS regressions, relying on both cross-sectional and dynamic panel regressions. Obviously, there are a number of identification issues that need to be addressed when looking at the effect of emigration on institutions. First among them is reverse causality; the direction of the bias, however, is theoretically uncertain: more democratic countries can "let their people go" more easily, while lack of democracy constitutes a strong push factor for emigration. Second, there may be omitted factors in our regressions that drive the joint patterns of emigration and institutions. It could be argued, for example, that trade and migration are complements while trade can also affect institutional quality. Here again, the direction of the bias would seem uncertain, at least if one follows Rigobon and Rodrik (2005) who found a weak effect of trade on institutions (with heterogeneous effects across institutional indicators).

We address these endogeneity issues using an instrumental variable approach. We rely on three complementary IV strategies: i) a gravity model predicting a country's emigration rate out of a set of reasonably exogenous dyadic variables (these are interacted with time dummies in the dynamic panel regressions); we supplement this approach with ii) weather-based instruments (associated with an indicator of country size), and iii) internal instruments using SYS-GMM estimation. These three complementary IV strategies yield consistent results, reveal a

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* Corresponding author at: IRES-UC Louvain, Belgium.
E-mail address: frederic.docquier@uclouvain.be (F. Docquier).

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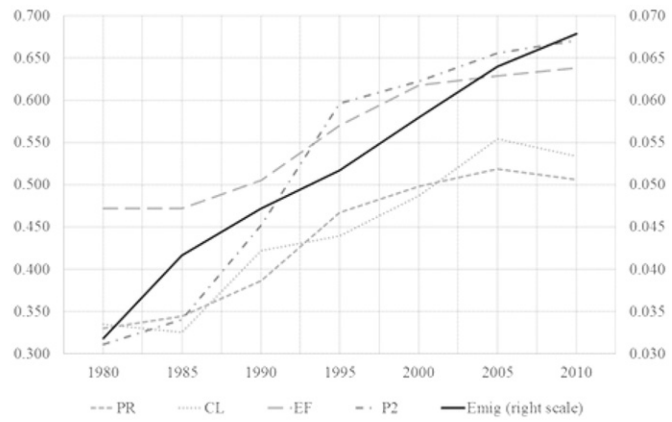

Fig. 1. Democracy and emigration rates over time (1980–2010). Notes. Four democracy indices are normalized between 0 and 1 and measured on the left scale: PR = Freedom House's index of Political Rights; Democracy indices; CL = Freedom House's index of Civil Liberties; EF = Simon Fraser Institute's index of Economic Freedom of the World; and P2 = Polity IV Project's index of democracy (Polity 2). Emigration rate: Emig = stock of emigrants divided by the native population (Brücker et al., 2013). For each indicator, we compute the mean levels of all developing countries in a balanced sample (World Bank classification).

downward bias of OLS estimates (i.e., once reverse causality from bad institutions to low emigration is accounted for, the magnitude of the emigration coefficient is increased), and support a careful causal interpretation of the results.

Our main result is that openness to emigration promotes democratization at home: we uncover a positive and significant effect of emigration on various measures of institutional quality in a large sample of developing countries. This effect is robust across specifications and estimation methods (OLS and IV), with consistent estimates in the cross-section and dynamic panel frameworks. Two second-order results are also worth mentioning. First, there are heterogeneous results across democracy indicators. More precisely, the main result holds mostly for our three de facto indicators of institutional quality: the Freedom House's “Political Rights” and “Civil Liberties” indicators, and the Simon Fraser Institute's “Economic Freedom of the World” indicator, but not for the “Polity 2” indicator of the Polity IV Project, an indicator of *de jure* institutional quality. And second, the effect is fully driven by emigration to rich, highly democratic countries, suggesting that the effect of emigration on home-country institutional outcomes is destination-specific. Indeed, when we use alternative migration data sources allowing to disentangle the effect of emigration to OECD versus non-OECD destinations, the effect of emigration to the latter is virtually zero.

The paper most closely related to ours is Spilimbergo (2009), who also adopts a cross-country approach and shows that foreign-trained students promote democracy at home if foreign education was acquired in democratic countries. While he does not identify the mechanisms that drive his results, he suggests a number of possible channels (e.g., access to foreign media, acquisition of norms and values while abroad that diffuse at home upon return, willingness to preserve the quality of one's network abroad) that can be generalized to other migration experiences as well. Our paper is similar in spirit and execution, with important conceptual differences. First, we estimate the effect of emigration on home-country institutions for all migrants, not just foreign students, meaning that we proceed to a larger scale exercise. Second, Spilimbergo's data contains information on the number of people with foreign training living either abroad or in the home country, making it impossible to know whether the effect is due to those staying abroad or to those who returned. In contrast, our emigration variable consists of the lagged accumulated stock of individuals (aged 25+) born in the home country and living abroad, suggesting that the effect of emigration on democracy needs not be driven by return migration. Third, identification in Spilimbergo's paper fully relies on heterogeneous effects for democratic versus non-democratic destinations. We supplement this identification strategy using different IV approaches, as mentioned above and explained in detail in Section 2 below. Fourth, Spilimbergo finds consistent results only for his “democratic norm at destination” variable, a weighted average of democratic scores at destination which captures whether emigration is directed toward more or less democratic countries. In all his specifications but one, the interaction term between the number of students abroad and the “democratic norm” is not significant. In contrast, our main results are for the emigration rate, suggesting that how open a country is to emigration makes a difference, not just whether its emigration is directed toward destinations with higher or lower democracy scores. Incidentally but quite importantly, this also allows us to interpret the magnitude of the estimated effects.

Other related literature includes mostly political-economy models of the interaction between migration and rent-seeking[1] as well as country case-studies trying to identify specific channels through which migration affects home-country institutions: the “exit effect” à la Hirschman (1970), whereby emigration options (and related expected remittances) reduce the incentives to “voice”[2]; the role of diasporas and their attempts to affect home-country politics, for good or bad[3]; and the diffusion of democratic values and norms acquired by the migrants while abroad and transferred to the home country, be it directly, through return migration and contacts with relatives abroad or indirectly, through the broader scope of social networks.

In particular, two recent micro studies found supportive evidence of a democracy-diffusion effect of emigration. In the context of Cape Verde, Batista and Vicente (2011) took advantage of a survey on perceived corruption in public services to set up the following experiment: survey respondents were asked to mail a pre-stamped postcard if they wanted the results of the survey to be made publicly available in the national and international media. Controlling for individual, household and locality characteristics, Batista and Vicente (2011) regressed response rates – which they interpret as demand for accountability – on migration prevalence at the locality level. They show that current as well as return migrants significantly increase participation rates, and more so for the latter. Interestingly, they find that only migrants to the US seem to make an impact, while migrants to Portugal, the other main destination, do not. The other context we report on is Moldova, a former Soviet Republic with virtually no emigration before the Russian crisis of 1998 and that, since then, has seen a surge in migration outflows, estimated at half-a-million for a population of 3.6 million in 2008. Barsbai et al. (2016) take advantage of the quasi-experimental context of Moldova, of the possibility of controlling for pre-migration political preferences, and of the fact that Moldovan emigration was directed both to the more democratic European Union and to less democratic Russia to identify destination-specific effects. They find that past emigration to the West translates into significantly lower share of votes for the communist party at the community level and provide suggestive evidence for an interpretation in terms of information and cultural transmission channels.[4]

As in Spilimbergo (2009), however, we are unable to disentangle the different channels through which emigration affects democracy at home; rather, our methods allow, and indeed force us to examine the overall impact of emigration on home-country institutions. This is composed of the various direct and indirect channels outlined above.

[1] The idea of migration as a personal response to political and economic repression has a long tradition in economics and political science (see Vaubel, 2008). Recent political economy models of the interaction between emigration, institutions and development include Epstein et al. (1999), Docquier and Rapoport (2003), Mariani (2007) and Wilson (2011).

[2] For example, it is commonly argued that emigration to the United States contributed to delay political change in countries such as Mexico (e.g., Hansen, 1988) or Haiti (e.g., Ferguson, 2003).

[3] See for example Haney and Vanderbush (1999) on Cuba, Ragazzi (2009) on Croatia and Wilson (1995) on Northern-Ireland.

[4] See also Chauvet and Mercier (2014) on migration and political participation in Mali.

The rest of this paper is organized as follows. Section 2 presents the empirical model, discusses the main challenges for the empirical analysis, and describes the data. Section 3 presents the results. Section 4 concludes.

2. Empirical strategy

Our goal is to empirically investigate the effect of emigration on the quality of institutions in the sending country. We will use several indicators of institutional quality, $I_{i,t}$, and measures of emigration, $m_{i,t}$, available for origin country $i = 1, \ldots, N$ and year $t = 1, \ldots, T$. In our benchmark regressions, the emigration rate is computed as the sum of emigrants from country i to OECD destination countries j at time t, $\sum_j M_{ij,t}$, divided by the native population of country i, $N_{i,t}$ (proxied by the sum of the resident and emigrant populations). In this section we present our empirical model (Section 2.1), discuss how we deal with endogeneity issues (Section 2.2), and describe the data sources used for the empirical analysis (Section 2.3).

2.1. Model

Our empirical model features the quality of institutions as the dependent variable. We augment the linear dynamic specification used in previous studies (e.g., Acemoglu et al., 2005b; Bobba and Coviello, 2007; Castello-Climent, 2008; Spilimbergo, 2009) by adding the emigration rate to the set of explanatory variables:

$$I_{i,t} = \alpha + \beta I_{i,t-1} + \gamma m_{i,t-1} + \sum_k \delta_k X^k_{i,t-1} + \varepsilon_{i,t} \quad (1)$$

where α is a constant. The lagged dependent variable enters the set of explanatory variables with coefficient β to account for persistence in institutional quality. Our coefficient of interest, γ, captures the short-run effect of the emigration rate on institutional quality at home. $X^k_{i,t-1}$ is a vector of K additional control variables ($k = 1, \ldots, K$). The vector δ includes parameters associated with the set of controls, and captures their short-run effect on institutional quality. All explanatory variables are lagged by one period (one period represents five years).

Our set of controls X covers the major determinants of democracy identified in the existing empirical literature:

- *Human capital* (labeled as 'HumCap' in the regression tables). Controlling for human capital is important because changes in human capital can jointly affect the quality of institutions and emigration rates. This is because high-skilled individuals have a greater propensity to emigrate than the low-skilled, particularly in developing countries. The literature on institutions and education provides mixed results. Acemoglu et al. (2005b) found no effect of the average years of schooling on education when country fixed effects are factored in. Accounting for persistency in institutions and human capital, Castello-Climent (2008) found a positive effect of the average level of education (for those below the 60th percentile of the education distribution) over the period 1960–2000. Bobba and Coviello (2007) found a positive effect of the average years of schooling over the period 1960–2000. Finally, Murtin and Wacziarg (2014) found a positive effect of primary schooling on democracy over the period 1870–2000. As our sample covers the period 1980–2010, we will use the share of residents aged 25 and over with tertiary (or college) education. This share is a good correlate/determinant of democracy in most of our regressions.[5]
- *Ethnic fractionalization* (labeled as Ethnic), measured as the probability that two randomly selected people from a given country belong to different ethnic groups. Alesina et al. (2003) found that ethnic fractionalization is negatively correlated with indicators of governance quality.
- *Gross Domestic Product per capita* (labeled as 'GDPpc') is used as a control variable, as in most studies on institutions. We express it in logs.
- *Trade openness* (labeled as 'Trade'), measured as the sum of imports and exports as share of GDP. Trade is usually seen as the main indicator of openness. We control for exports and imports to make sure that our emigration rates do not capture other dimensions of openness. In addition, the existing literature has revealed that good institutions are correlated with openness to trade (e.g., Rodrik et al., 2004).
- *Net Official Development Assistance* as share of GNI (labeled as 'ODA'). Using data in the period 1960 to 1999, Djankov et al. (2008) recently argued that foreign aid spurs rent-seeking behavior and has a negative impact on democracy.
- *Legal Origin* dummies (labeled as 'Legal'). These variables identify the legal origin of the Company Law and Commercial Code of each country. We use two dummy variables, one for the English Common Law and one for the French Commercial Code.[6] We will also exclude socialist legal origin countries from our sample in a robustness analysis.
- *Geographic characteristics*. The role of geography in explaining the choice of institutions has been identified in several studies (e.g., Rodrik et al., 2004). Here we use Sachs' set of geographic indicators (Sachs, 2003): country latitude, a dummy for landlocked countries, land area (logs), the percentage of a country's land area in the tropics, and the prevalence of malaria in 1994.
- *Region* dummies, added here to capture unobserved heterogeneity at the regional level.

As is well known, a key issue when adding explanatory variables is that they exhibit collinearity. For example, GDP per capita and human capital are highly correlated, latitude is correlated with legal origin, and the emigration rate itself is highly correlated with trade, human capital and many geographic variables. For this reason, we will add one control (or set of controls) at a time, and show that our results are robust to this procedure.

The dynamic specification (1) has been extensively used to explain the dynamics of persistent variables such as the stock of human/physical capital or GDP per capita. If the explanatory variables are persistent (e.g., $m_{i,t} = m_{i,ss}$ and $X_{i,t} = X_{i,ss}$ $\forall$ t, where subscript ss stands for steady state) and if the coefficient of the lagged dependent is comprised between 0 and 1 (i.e., $\beta \in [0; 1[$), then the level of the dependent variable converges toward a long-run or steady state level,

$$I_{i,ss} = \frac{\alpha + \gamma m_{i,ss} + \delta X_{i,ss}}{1-\beta}, \quad (2)$$

which characterizes the long-run relationship between institutions and the right-hand-side variables. In that case, $\gamma/(1-\beta)$ captures the long-run effect of emigration on democracy.

Estimating Eq. (1) requires panel data while estimating Eq. (2) can be done in a cross-sectional setting with one observation per country. Cross sectional and panel data techniques have their pros and cons. In a cross-section framework, the underlying steady-state assumption, albeit questionable, allows circumventing the difficulties inherent to the endogeneity of the lagged dependent; however, in such framework the omitted variable issue is likely to be severe. In a panel framework on the other hand, we can characterize the transitional dynamics of institutional quality and better deal with unobserved heterogeneity. However, we need to find exogenous instruments that are both country- and time-specific.

2.2. Identification strategy

We will first estimate Eqs. (2) and (1) using OLS or pooled OLS regressions, being aware of the fact that such regressions raise a number

[5] Other measures of human capital are used in the Online Appendix (see Table A.10).

[6] In our sample, there is no country identified as of the German or Scandinavian legal-origin type. Hence, socialist countries form our reference group.

of econometric issues that might generate inconsistent estimates. The key issue when using cross-sectional or pooled OLS regressions is the endogeneity of our main variable of interest, the emigration rate. Endogeneity is due to a number of reasons. First, the quality of institutions is likely to affect both the desire to emigrate (as most people prefer to live in countries with good institutions) and the possibility to emigrate (as bad institutions, or low government effectiveness, can be responsible for large administrative costs).[7] This means that a positive or negative correlation between emigration and institutional quality can be driven by reverse causality. Second, unobserved country characteristics can jointly affect the emigration rate and the quality of institutions.

Causation is hard to establish with aggregate data. Our identification strategy consists in comparing results obtained under three alternative sets of instruments and to show that our IV results are robust to the instrumentation strategy. We first use a two-stage least squares (2SLS) estimation strategy. This requires finding suitable instruments for migration in the first stage. We consider two sets of external instruments which have been commonly used in the migration literature, one based on a "zero-stage" pseudo-gravity model, and one exploiting climatic factors. Then, following Castello-Climent (2008), Bobba and Coviello (2007), and Murtin and Wacziarg (2014), we will compare our results with those obtained with the system-GMM estimator with internal instruments. The use of SYS-GMM enables us to better account for unobservable heterogeneity and persistence in the lagged dependent and other regressors.

2.2.1. Gravity-based 2SLS strategy

Our main 2SLS identification strategy relies on Frankel and Romer (1999) and Feyrer (2009). In the cross-sectional setting, we focus on the year 2000 and construct a gravity-based prediction of bilateral migration stocks, $\hat{M}_{ij,00}$ from origin country i to destination j. In this "zero-stage" gravity model, our set of determinants only includes exogenous variables which are unlikely to directly impact democracy, i.e. variables referred to as "relative geography" variables. We then obtain a predicted emigration rate, $\hat{m}_{i,00}$, by aggregating bilateral migration stocks over destinations, $\sum_j \hat{M}_{ij,00}$, and by dividing that sum by the native population size in 2000, $N_{i,00}$. We use this gravity-based predicted emigration rate to instrument $m_{i,00}$ in our first stage regression (which includes $X^k_{i,00}$, the set of controls of the second stage):

$$m_{i,00} = a_0^{gr} + a_1^{gr}\hat{m}_{i,00} + \sum\nolimits_k a_k^{gr} X^k_{i,00} + \epsilon^{gr}_{i,00}. \tag{3}$$

This method is now standard in the migration literature (e.g., Alesina et al., 2016; Beine and Parsons, 2015; Docquier et al., 2014; Ortega and Peri, 2014) and follows a long tradition of predicting trade openness out of bilateral trade flows.

The gravity-based predictions of bilateral migration stocks are obtained from the following pseudo-gravity model:

$$\ln M_{ij,00} = a_0 + a_j + b_1 Ling_{ij} + b_2 Guest_{ij} + b_3 \ln Dist_{ij} + b_4 \ln \ P_{i,00} + \epsilon_{ij,00} \tag{4}$$

where $Ling_{ij}$ is a dummy variable equal to 1 if the same language is spoken by at least 9% of the population in both countries, $Guest_{ij}$ is a dummy variable equal to 1 if a guest-worker program after 1945 and before the 1980s was observed, $\ln Dist_{ij}$ is the log of the weighted distance that is equal to the distance between i and j based on bilateral distances between the biggest cities of the two countries (with those inter-city distances being weighted by the share of the city in the total population of the country, see Head and Mayer (2002)), $\ln P_{i,00}$ represents the (log) of the total population at origin in 2000, and a_j is a destination-country fixed effect. Our gravity model does not include origin-country fixed effects because the latter are likely to capture the effect of institutions on emigration decisions.[8]

The presence of a large number of zeroes in bilateral migration stocks gives rise to econometric concerns about possible inconsistent OLS estimates. The most appropriate method to estimate the above model is the Poisson regression by pseudo-maximum likelihood (PPML). We will use the PPML command in Stata which builds on the method of Santos Silva and Tenreyro (2011) to identify and drop regressors that may cause the non-existence of the (pseudo-) maximum likelihood estimates. Standard errors are robust and clustered by country pairs.

A limitation of this instrumentation strategy is that most of our determinants of bilateral migration stocks are time-invariant. In the panel setting, therefore, we follow Feyrer (2009) and add time fixed-effects and interactions between geographic distance and time dummies into the "zero-stage" regression (4). Identification comes from the time-varying effect of geographic distance on migration, reflecting gradual changes in transportation and communication costs. Interactions between time dummies and distance account for common shocks in communication and transportation technologies (e.g. improvements in aircraft technology have induced more people to move and have reduced long-distance migration costs). As long as changes in technologies are common to all countries, these time series changes will be exogenous with respect to any particular country, but they will have different effects across country pairs, depending on the relative geographic position.

Table A.2.a in the Online Appendix gives the results of the "zero-stage" regression. Column 1 gives the results of the panel estimation while column 2 gives the cross-sectional results for the year 2000. Overall, geographic characteristics are strong determinants of bilateral migration stocks. As proxies of migration costs, linguistic links favor migration while geographical distance is negatively correlated with bilateral migration stocks. Past guest-worker programs have a positive effect on bilateral migration stocks, as does the population size at origin (in absolute terms, bigger countries send more migrants abroad). In Table A.2.b, it is shown that $\hat{m}_{i,t}$ is an excellent predictor of $m_{i,t}$ and the R^2 of this first-stage regression varies between 0.40 (for the P2 indicator) and 0.56 (for the EF indicator). As in Feyrer (2009), the gravity-based instrumentation strategy performs quite well in the second stage.

2.2.2. Weather-based 2SLS strategy

Relative geography variables can affect institutions through other channels than migration, and first and foremost through trade. While we control for trade flows (as well as for other important variables) in our second-stage regressions, we cannot exclude that our gravity variables also affect institutions through additional channels (e.g., cultural proximity, or technology diffusion). We therefore consider an alternative IV strategy based on weather shocks in the panel setting.[9] While our gravity-based strategy builds on interactions between distance and time dummies, climatic variables truly vary over time. In the first stage, we use the lagged population size in logs ($\ln Pop_{i,t-1}$), lagged number of natural disasters ($Natd_{i,t-1}$), and lagged deviations in temperature from the country-specific mean values ($Temp_{i,t-1}$) as external instruments for the lagged emigration rate. Our first-stage regression, which includes $X^k_{i,t-1}$, the set of controls of the second stage, writes as:

$$m_{i,t-1} = a_0^{we} + a_1^{we}\ln(P_{i,t-1}) + a_2^{we} Natd_{i,t-1} + a_3^{we} Temp_{i,t-1} + \sum\nolimits_k a_k^{we} X^k_{i,t-1} + \epsilon^{we}_{i,t-1} \tag{5}$$

Beine and Parsons (2015) found no direct impact of climatic factors on international migration using migration data in 10-year intervals over the

[7] Fitzgerald et al. (2014) study the political pull factors of international migration in a gravity framework.

[8] Note that in the gravity regressions, we consider the comprehensive migration matrices, including all country pairs.

[9] We do not implement the weather-based IV strategy in the cross-sectional setting.

period 1960–2000. On the contrary, Marchiori et al. (2012) and Coniglio and Pesce (2014) identified an effect on net flows using annual data. Table A.2.c in the Online Appendix shows that emigration rates decrease with population size and increase with the number of natural disasters and with temperature shocks when using data in 5-year intervals. Compared to our gravity-based strategy, the R^2 of this first-stage regression is smaller, ranging from 0.16 (for the P2 indicator) to 0.33 (for the CL indicator). Still, in the second stage, the effect of emigration on democracy remains significant in many cases, and both short-run and long-run effects are similar to those obtained with the gravity-based IV strategy. The F-stat of the second stage is much smaller though, sometimes falling below the threshold of 10 (for the EF and P2 regressions notably).

We consider our weather-based strategy as a robustness check. Weather shocks and natural disasters are exogenous and have been used to instrument migration in several studies (e.g., Munshi, 2003; Yang and Choi, 2007). However, we do not consider them as perfect instruments as they can be correlated with our dependent variable. On the one hand, they could affect institutions through income shocks or through the risk of conflicts (Brückner and Ciccone, 2011; Nel and Righarts, 2008). On the other hand, more democratic countries might suffer less from natural disasters (Kahn, 2005). To partially reduce concerns about exclusion restrictions, we consider the occurrence, and not the incidence, of natural disasters.

2.2.3. SYS-GMM strategy

As third identification strategy, we use the SYS-GMM estimator with internal instruments only.[10] This technique accounts for unobservable heterogeneity, and potential endogeneity and persistence of the other regressors.

Estimating Eqs. (2) and (1) requires defining a set of explanatory variables affecting the quality of institutions. Introducing correlated controls can therefore generate identification problems among the correlated variables. In a panel setting, we could solve this problem by controlling for time fixed effects, α_t, and country fixed effects, α_i. Although they cannot capture determinants that are both country- and time-specific, such fixed-effects account for many unobservable characteristics that jointly affect emigration and institutions. In our estimation strategy, we do not consider a within transformation to control for unobserved heterogeneity, because the results would become too imprecise for several reasons. First, we know that in a dynamic panel data model, the standard fixed effect estimator is biased and inconsistent in panels with a short time dimension, the so called Nickell bias (Nickell, 1981). Second, as Hauk and Wacziarg (2009) point out, the within estimator tends to exacerbate the measurement error bias and to understate the impact of the explanatory variables in dynamic panel data models with regressors that are both time persistent and measured with errors. This point is particularly crucial if the right-hand-side variables are highly time persistent, as is the case here. Under fixed effect estimation, therefore, eliminating heterogeneity bias may come at the cost of exacerbating measurement error bias.[11]

Under particular assumptions, the SYS-GMM estimator controls for unobserved heterogeneity and partly corrects for the deficiencies of the FE estimator.[12] It combines the regression in differences with the regression in levels in a single system. The instruments used in the first differentiated equation are the same as in Arellano and Bond (1991), while the instruments for the equation in level are the lagged differences of the corresponding variables. SYS-GMM requires an additional moment condition for the level equation, such that first differences of pre-determined explanatory variables must be orthogonal to the country fixed effects. This holds when the process is mean stationary, a difficult condition to test. Nevertheless, even when the stationarity condition is unlikely to be fully satisfied, Hauk and Wacziarg (2009) show that the estimation biases of SYS-GMM are systematically smaller in magnitude than those resulting from weak instruments in the Arellano-Bond approach, or than the Nickell bias under dynamic fixed effects. The validity of the additional moment conditions associated with the level equation has been tested using the Hansen difference test for all GMM instruments.

A second advantage of SYS-GMM is that it enables us to deal with the endogeneity of other regressors using internal instruments. For example, the existing literature has studied the impact of human capital and development on institutions, however it is obvious that institutions affect economic performance and the incentives to acquire human capital. The same issue arises with GDP per capita, trade and foreign aid. In addition, using the lagged dependent in (1) also induces potential biases in the estimation. We conduct SYS-GMM estimations with the alternative sets of time-varying controls (human capital, GDP per capita, trade, and foreign aid), considering each control as potentially endogenous.

There is neither formal tests nor precise guidance to identify the optimal number of lags in the SYS-GMM specification. As a rule of thumb, Roodman (2009) suggests to keep the number of instruments lower than the number of countries. Too many instruments can in fact overfit the endogenous variables. At the same time, too few instruments can lead to weak instrumentation problems. In our estimations, each explanatory variable (with the exception of the time fixed effects) will be instrumented for using its first to third lags, as in Spilimbergo (2009). However, our results are fairly robust to the choice of the instruments set.

For each SYS-GMM regression, we report the p-value of the Hansen test for joint validity of all the instruments. Although SYS-GMM works less well under a few specifications, we keep the same lag structure for all the specifications to be transparent and not to choose "ad hoc" internal instruments for each specification and/or indicator.[13]

The SYS-GMM estimator will also be used to check the robustness of the results to the inclusion/exclusion of certain countries whose characteristics may exacerbate reverse causality problems (e.g., socialist countries, sub-Saharan African countries, oil-exporting countries, and MENA countries) and to examine whether our results are driven by skill-specific emigration rates.

2.3. Data

Our data set is a five-year unbalanced panel spanning the period between 1985 and 2010, where the start of the date refers to the dependent variable (i.e., $t = 1985$, $t - 1 = 1980$). In our sample, we consider developing countries only (according to the World Bank Classification in 2008), and they enter the panel if they are independent at time $t - 1$.[14] The country sample is selected on the basis of the availability of the data. Table 1 provides summary statistics and number of observations for our dependent and control variables, calculated considering

[10] In a previous version of this paper, we also combined internal and external instruments. Results are similar and are available upon request.

[11] For example, this can explain why, in the growth literature, human capital variables have often been found insignificantly different from zero (or with negative signs) in panel fixed-effects applications (see Islam, 1995). Hauk and Wacziarg (2009) show that Monte Carlo simulations are in line with these results found in the literature. In addition, even if the model is dynamic they also show that the first-difference GMM estimator does not perform better in terms of bias properties. For example, the Monte Carlo simulations regarding the effect of human capital accumulation on growth display very close results to the fixed effect estimates, suggesting that the weak instrumentation problem may be prevalent in this case.

[12] See also Blundell and Bond (1998), and Bond et al. (2001), who suggest that system GMM is the most appropriate estimator in dynamic panel data models when time series are very persistent.

[13] It should also be noticed that the Hansen J-test rests on strong assumptions and can have low power. See Roodman (2009).

[14] In our sample, Antigua and Barbuda, Chile, Latvia, Lithuania, Russia and Uruguay are classified as developing countries. Chile, Lithuania, Russia and Uruguay became high-income countries in 2013. Latvia did it in 2010, and Antigua and Barbuda in 2009. Hungary became a high-income country in 2008 but slipped back to the upper–middle-income group in 2013.

Table 1
Summary statistics for time-varying variables.

	Obs.	Mean	Std dev	Min	Max
PR index	766	.4779	.3413	.0000	1.000
CL index	766	.4856	.2851	.0000	1.000
EF index	445	.5745	.1150	.1780	.7930
P2 index	654	.5500	.3244	.0000	1.000
Emigration rate	766	.0550	.0967	.0001	.5193
Human capital	568	.0382	.0404	.0006	.2491
GDP per capita (logs)	745	7.819	.9652	5.016	9.811
Trade (as % of GDP)	683	.7620	.4061	.0108	3.587
Net ODA (as % of GNI)	655	.0978	.1307	−.0010	1.451

Note: Samples including developing countries only (see Table A.1.a in the Online Appendix).

Table 2
Correlation rates between democracy indicators.

	PR	CL	EF	P2
PR	1.000	–	–	–
CL	.8852	1.000	–	–
EF	.4162	.4850	1.000	–
P2	.8402	.7904	.4647	1.000

the largest sample that we use across indicators and estimation techniques.[15]

2.3.1. Democracy

Data on democracy are taken from the Freedom House data set, from the Economic Freedom of the World Project, and from the POLITY IV data set.

The Freedom House publishes the political rights (PR) and civil liberties (CL) indices. They are based on perception measures gathered through expert coding based on news reports, NGOs and think tanks evaluations, and surveys administered to large number of professionals. For the PR index, the questions are grouped into three sub-categories: electoral processes, political pluralism and participation, and functioning of the government. The CL questions are grouped into four subcategories: freedom of expression and belief; association and organization rights; rule of law and personal autonomy; and individual rights. The sum of each country's sub-category scores translates to a rating from 1 to 7, with a higher score indicating more freedom. Following Acemoglu et al. (2008) we transform these indices so that they lie between 0 and 1, with 1 corresponding to the most-democratic set of institutions.

We also consider Economic Freedom of the World (EFW), an index which measures the degree to which countries' policies and institutions support economic freedom. Five broad areas are distinguished: (1) size of government; (2) legal structure and security of property rights; (3) access to sound money; (4) freedom to trade internationally; and (5) regulation of credit, labor and business. The index is placed on a scale from 0 to 10. We also normalize it between 0 and 1.[16] The ratings are determined by combining real indicators (such as "size of government", taken from the IMF) with answers to survey questions on other modules (such as "independence of the judicial system", taken from perception reports — e.g., the Global Competitiveness Report form the World Economic Forum, or "regulatory restrictions" taken from the World Bank's "Doing Business" database).

Finally, another measure of democracy from the POLITY IV data set is also considered. Polity IV indicators of democracy measure the general openness of political institutions and combine several aspects such as: the presence of institutions and procedures through which citizens can express effective preferences about alternative policies and leaders; the existence of institutionalized constraints on the exercise of power by the executive power; and the guarantee of civil liberties to all citizens in their daily lives and in acts of political participation. In our data set we consider a composite index (Polity2), that ranges from -10 to $+10$. This index is also normalized between 0 and 1. Note that while the "political rights", "civil liberties" and "economic freedom of the world" indices are largely based on public perception measures and can therefore be seen as a reflection of contemporaneous de facto institutional quality, the Polity 2 indicator is based on expert coding of legal documents and can therefore be interpreted more as a *de jure* measure.[17]

Table 2 presents the correlation table between the various institutional indicators. The first three indices (PR, CL, Polity2) exhibit pairwise correlation rates between 0.8 and 0.9; their correlation rate with EFW is around 0.45.

2.3.2. Emigration

For emigration data, we use the IAB database (Brücker et al., 2013). Focusing on 20 OECD destination countries, they computed emigration stocks and rates of the population aged 25 years and older by gender and educational attainment in five-year intervals from 1980 to 2010, i.e. seven data points per country of origin.

Two alternative databases are worth mentioning: Ozden et al. (2011), and Artuç et al. (2015). They cover more destinations but less data points per country of origin. Ozden et al. (2011) provide data from 1960 to 2000 in ten-year intervals for the whole population of migrants (including children). Given the availability of our democracy indicators, using this database would limit the number of data points per country to three (1980, 1990 and 2000) in the dynamic panel framework. Artuç et al. (2015) only provide data for the years 1990 and 2000 (henceforth referred to as the ADOP database). Still, in our cross-sectional setting, we will estimate our model using more comprehensive emigration rates (to all OECD destinations, and also to all countries of the world) from Artuç et al. (2015) and for the year 2000. This will enable us to increase coverage and to distinguish between emigration to OECD versus non-OECD destinations.

The IAB data were obtained by harmonizing national censuses and population registers from the receiving countries. When building their large-scale data set, Brücker et al. (2013) had to deal with inevitable gaps in the data. As in the other databases, interpolations and/or imputations were used when census data were missing or were not sufficiently detailed to identify the bilateral stocks of migrants. This is a less important issue in our context as we use aggregate emigration rates and (bilateral) missing values are scarce for the major OECD destinations. Comparing IAB data with those from Artuç et al. (2015), the 20 destination countries covered represent more than 90% of the OECD total immigration stock. In addition, the correlation between the IAB and ADOP measures of emigration stocks to the 20 destinations equals 0.986 for the year 2000.

2.3.3. Other data

Data on human capital are based on Barro and Lee (2013) and concern the population aged 25 and over, in line with emigration data. Some regressions in the Online Appendix will exploit the human capital indicators of the IAB database, where the levels of education in countries for which the Barro and Lee's data are missing are imputed. Data on GDP per capita, population, trade, and official development assistance (ODA) are taken from the Penn World Tables and from the World Development Indicators. Data on legal origins are from La Porta et al. (1999), who provide a set of time-invariant binary variables characterizing the origin of national law.[18] Ethnic fractionalization data are

[15] Table A.1 in the Online Appendix presents the list of countries in our sample including the largest number of observations.

[16] It should be noticed that EFW is a continuous index, and there are no countries that has a grade of 0 or 10. In order to normalize this index, we simply divided the original index by 10.

[17] It goes without saying that there is a good deal of discrepancy between de facto and de jure indicators. See Hallward-Driemeier and Pritchett (2011) in the case of the "Doing Business" data.

[18] Five systems are distinguished: French, German, British, Scandinavian and Socialist.

Table 3
Cross-sectional results.
Estimated coefficients for the emigration rate.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Controls	None	None	HumCap	Ethnic	GDPpc	Trade	ODA	Legal	Geogr.	Regions	All
PR index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Emig	1.058***	2.140***	1.577***	2.108***	1.668***	1.770***	1.903***	1.769***	2.158***	1.664***	4.260**
	(.2234)	(.4870)	(.5971)	(.5146)	(.4453)	(.4041)	(.3859)	(.4247)	(.8063)	(.5532)	(1.939)
Obs	138	138	99	135	134	130	123	136	116	138	81
KPW	–	47.66	13.69	44.86	54.93	61.92	73.93	62.13	10.44	41.28	13.76
CL index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Emig	1.174***	2.179***	1.904***	2.095***	1.751***	1.795***	1.954***	1.870***	2.401***	1.932***	4.103***
	(.1390)	(.3980)	(.4330)	(.4085)	(.3401)	(.2972)	(.3057)	(.3398)	(.6766)	(.4530)	(1.424)
Obs	138	138	99	135	134	130	123	136	116	138	81
KPW	–	47.66	13.69	44.86	54.93	61.92	73.93	62.13	10.44	41.28	13.76
EF index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Emig	.4249***	.5133***	.5236***	.5183***	.3565***	.4525**	.4349***	.4626***	.4405***	.2343	.1760
	(.1208)	(.1583)	(.1628)	(.1444)	(.1372)	(.2068)	(.1353)	(.1445)	(.1309)	(.1731)	(.4158)
Obs	79	79	75	79	78	78	71	79	77	79	64
KPW	–	13.70	13.19	14.41	12.74	21.16	12.40	13.70	9.06	11.00	10.63
P2 index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Emig	.9496***	1.137**	1.297**	1.105**	.9318**	.9333	1.048**	1.014**	.4915	.3576	.4600
	(.2281)	(.5017)	(.5606)	(.4995)	(.4560)	(.6525)	(.4737)	(.4786)	(.7253)	(.4973)	(1.431)
Obs	118	118	96	117	116	114	105	118	113	118	80
KPW	–	20.82	15.08	20.33	19.73	14.61	21.70	22.25	7.247	19.65	4.324

Notes: Robust standard errors clustered by country in parentheses. Col (1) shows OLS results. Col (2) to (11) show 2SLS results; total emig rate is instrumented using gravity-based, predicted emigration rates. KPW: Kleibergen–Paap rk Wald F statistics to be compared with the Stock-Yogo critical values for weak instrumentation.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

taken from Alesina et al. (2003). Latitude, prevalence of malaria and other geographic and cultural bilateral data are taken from the CEPII database and from Sachs (2003), respectively. Data on natural disasters are obtained from the EMDAT-database whereas monthly data on temperature are taken from Mitchell et al. (2003). The EMDAT database provides the number of natural disasters, the total number of deaths, the number of affected people, and damages in US$. To partially reduce concerns regarding violations of the exclusion restriction, we consider the number of natural disasters and we disregard their incidence. We aggregate natural disasters by periods of five years. As for temperature, we average the levels over periods of five years and expressed them in percentage of deviation from the 1975–2010 average, as in Beine and Parsons (2015).

3. Results

The results are organized in five sub-sections. We first use cross-sectional data to estimate the long-run relationship between emigration and institutional quality depicted in (2) using the OLS and 2SLS regressions with external instruments. Second, we use panel data to estimate dynamic specification (1) with pooled OLS and 2SLS regressions under two sets of external instruments: gravity-based instruments, and weather shocks. Third, we re-estimate our dynamic model using the SYS-GMM estimator with internal instruments. Fourth, we conduct a sensitivity analysis to check the robustness of our results to the exclusion of certain groups of countries (socialist countries, oil-producing countries, sub-Saharan African countries, and MENA countries) and to the exclusion of some periods. Finally, we estimate the dynamic model using skill-specific emigration rates to investigate whether the effect of emigration on institutions varies by education level. In the last two sub-sections, we only rely on the SYS-GMM estimator. In all cases, the analysis is conducted on four institutional indicators: the Freedom House indicators of political rights (PR) and civil liberties (CL), the index of Economic Freedom of the World (EF), and the Polity 2 index (P2).

3.1. Cross-sectional analysis

Tables 3 reports OLS and 2SLS estimates for specification (2) using data for 2000 for all variables.[19] We only report the estimated coefficients of the emigration rate and their standard errors, which are robust and clustered by country. In OLS regressions (column 1), the estimated coefficient of the emigration rate is positive and statistically significant for each indicator. In columns 2, we correct for endogeneity using the gravity-based 2SLS strategy, introducing various controls separately in columns 3 to 10, and all of them jointly in column 11.

The baseline regressions in column 1 show that the emigration coefficient is positive and statistically significant for all democracy indicators. Compared with OLS, the 2SLS coefficients are larger, suggesting that OLS coefficients might suffer from a reverse causality bias: emigration rates decrease when institutions improve (particularly when political rights and civil liberties improve).

In columns 3 to 10, we add our eight controls one at a time to avoid collinearity problems. Estimated coefficients for control variables are provided in Tables A.3.a to A.3.d in the Online Appendix. The share of tertiary educated residents is always positive and significant for each indicator, with exception of the Economic Freedom of the World (EF) index. The number of observations drastically decreases when human capital is included (from 138 to 99 observations). The coefficient of GDP per capita is always positive and significant. It is worth noticing that we do not instrument our control variables. Hence, we only identify correlation relationships between democracy and our controls. In most cases, the coefficients of trade, net ODA, ethnic fractionalization and legal dummies are not significant with the exception of the EF indicator, which decreases with ethnic fractionalization and with foreign aid. Geographic variables and regional dummies are usually significant.

[19] We consider the year 2000 as we will use alternative measures of emigration rates available for that year. The results are robust to using other years.

Table 4
Robustness of cross-sectional results.
Alternative measures of the emigration rate — index of political rights (PR).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PR index	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
Emig20.IAB	1.058***	–	–	–	–	2.140***	–	–	–	–
	(.2234)					(.4870)				
Emig20.ADOP	–	.9633 ***	–	–	–	–	1.646***	–	–	–
		(.1941)					(.3434)			
Emig34.ADOP	–	–	.9707***	–	.9891***	–	–	1.619***	–	1.747***
			(.1917)		(.2007)			(.3385)		(.3554)
Emig.non-OECD	–	–	–	−.0454	−.2515	–	–	–	−.5885	−.7157
				(.3852)	(.3385)				(.5939)	(.5873)
Constant	.4437***	.4347***	.4302***	.4974***	.4421***	.3763***	.3919 ***	.3870***	.5256***	.4156***
	(.0329)	(.0324)	(.0327)	(.0372)	(.0385)	(.0387)	(.0349)	(.0356)	(.0437)	(.0439)
Obs	138	134	134	134	134	138	134	134	134	134
R^2	.1019	.0984	.1010	7.8e−5	.1034	–	–	–	–	–
KPW	–	–	–	–	–	47.66	90.07	92.18	41.77	23.16

Notes: Robust standard errors clustered by country in parentheses. Col (1) and (6) show OLS and 2SLS results with IAB data on emigration to 20 OECD countries. Col (2) and (7) show OLS and 2SLS results with ADOP data on emigration to 20 OECD countries. Col (3) and (8) show OLS and 2SLS results with ADOP data on emigration to 34 OECD countries. Col (4) and (9) show OLS and 2SLS results with ADOP data on emigration to non-OECD countries. Col (5) and (10) show OLS and 2SLS results with ADOP data on emigration to 34 OECD and to all non-OECD countries. Emigration rates are instrumented using gravity-based, predicted emigration rates. KPW: Kleibergen–Paap rk Wald F statistics to be compared with the Stock-Yogo critical values for weak instrumentation.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

We find that the effect of emigration on political rights and civil liberties is strongly robust to the inclusion of standard control variables. Adding these controls does not affect the significance and the size of the emigration coefficient. In addition, the Kleibergen–Paap Wald rk (KP) F-statistic for weak identification appears to be very large in all specifications, except when human capital or geographic controls are included. In the first case, the sample size drastically decreases. In the second case, geographical variables are correlated with the explanatory variables used in our "zero-stage" gravity model: the quality of the first-stage strongly decreases when geography is included. However, the KP F-statistic value is higher than the critical values reported by Stock and Yogo (2005) although lies below the most demanding one (16.38).[20]

The effect of emigration on the EF indicator is also stable, except when geographic controls are included. As far as the Polity 2 index is concerned, the effect of emigration is less robust; we lose significance under three specifications. As expected, the regression including all controls (column 11) is problematic: the number of observations and the KP F-stat decrease, and many coefficients are inflated due to multicollinearity.

Globally, Table 3 is suggestive of a positive and robust causal effect of emigration on the de facto indicators of democracy. Similar effects are found for political institutions and economic institutions, with long-run effects ranging between 1.6 and 2.2 for the PR index, and between 1.8 and 2.4 for the CL index. Overall, this means that a 10-percentage point increase in the emigration rate raises standardized democracy indices by 16 to 24 percentage points in PR and CL, that is, by 47 to 84 percent of their standard deviations as reported in Table 1. The effect on the P2 index, our *de jure* indicator of democracy, is much less stable and sometimes insignificant. Regarding the EF index, the long-run effect ranges only from .4 to .5, implying that a 10 percentage-point increase in emigration raises the index by 4 to 5 percentage points (that is, by 15 percent of its standard deviation).

The IAB emigration rates are based on census data collected in 20 OECD destination countries. Although these countries represent about 90% of the foreign-born population living in OECD countries in 2000, we also estimated our cross-sectional model using the comprehensive measures of migration from ADOP for the year 2000.[21] In Table 4, we depart from the parsimonious specifications 1 and 2 of Table 3 and re-estimate the model using the ADOP emigration rates to 20 OECD countries, to 34 OECD countries, and to non-OECD countries. It is worth noticing that migration to non-OECD destinations accounts for 42.2% of the stock of migrants aged 25 and over and originating from developing countries. Columns 1 to 5 show the OLS results. Columns 6 to 10 show the 2SLS results obtained with gravity-based instruments.[22] Results are only provided for the PR index.[23]

For each de facto indicator of democracy, very similar OLS and 2SLS results are obtained when using alternative (and more comprehensive) emigration rates to OECD countries. The coefficient is slightly smaller with the ADOP data because we divide total emigration stocks by different measures of the native population. On the contrary, emigration to non-OECD (i.e. less democratic countries) is never significant. Hence, restricting the sample of destination countries to 20 OECD member states and disregarding non-OECD destinations is not a limitation. In line with Spilimbergo (2009), we conclude that what matters for institutions is the emigration rate to democratic states. As far as Polity 2 is concerned, emigration to OECD countries remains positive and highly significant, whereas emigration to non-OECD is negative and significant. We do not want to push the interpretation of this result too far as estimation for the Polity 2 index will be less stable in the panel frameworks with external and internal instruments.

[20] To test for the quality of our instruments we consider the framework provided by Stock and Yogo (2005), which is a generalization of the well-known Staiger-Stock "rule-of-thumb" of a value of 10. The null being tested is that instruments are weak in the sense that IV estimates provide hypothesis tests with large size distortions. With one endogenous variable and one excluded instrument, the Stock and Yogo (2005) critical values (maximal IV size) are: 16.38 (10% maximal IV size); 8.96 (15% maximal IV size); 6.66 (20% maximal IV size); 5.53 (25% maximal IV size). Note that the Stock and Yogo critical values are only strictly appropriate when errors are IID. However, it is common to use them as a guideline also in the presence of non-IID errors.

[21] We identified countries for which the IAB dataset accounts for less than 70% of total emigration to OECD in the year 2000. These include 23 countries (Azerbaijan, Belarus, Brazil, Bulgaria, Burkina Faso, Burundi, Dem. Rep. of Congo, Rep. of Congo, Georgia, Kazakhstan, Kyrgyzstan, Libya, Lithuania, Moldova, Morocco, Romania, Russia, Rwanda, Tajikistan, Turkmenistan, Ukraine, Uzbekistan, Yemen). Most of them are Socialist or Sub-Saharan African countries that will be excluded in our robustness analyses. In unreported regressions (available from the authors upon request), we estimated the model without these 23 countries and obtained very similar results.

[22] To construct instruments, we run gravity regressions with different samples of destination countries (20 OECD, 34 OECD, or all non-OECD destinations).

[23] Regression results for the other indicators are provided in Tables A.4.a to A.4.d of the Online Appendix.

Table 5
Dynamic panel model with gravity-based instruments.
Estimated coefficients for the lagged dependent and the emigration rate.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Controls	None	None	HumCap	Ethnic	GDPpc	Trade	ODA	Legal	Geogr.	Regions	All
PR index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.PR	.8113***	.7917***	.7759***	.7925***	.7623***	.7850***	.7836***	.7900***	.7531***	.7619***	.6156***
	(.0221)	(.0237)	(.0261)	(.0241)	(.0249)	(.0256)	(.0261)	(.0244)	(.0311)	(.0288)	(.0458)
L.Emig	.2833***	.5277***	.3607***	.4831***	.5031***	.5186***	.4896***	.5178***	.5541***	.4770***	1.413***
	(.0489)	(.1160)	(.1220)	(.1188)	(.1214)	(.1069)	(.1065)	(.1108)	(.1702)	(.1421)	(.4622)
Obs	766	766	568	749	745	683	655	756	643	766	449
KPW	–	67.84	16.59	68.78	73.54	89.37	82.23	86.24	13.69	58.15	17.07
CL index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.CL	.8358***	.8109***	.7980***	.8099***	.7721***	.8148***	.8007***	.8150***	.7837***	.7932***	.6586***
	(.0200)	(.0224)	(.0254)	(.0219)	(.0239)	(.0247)	(.0257)	(.0227)	(.0279)	(.0253)	(.0412)
L.Emig	.2017***	.3978***	.2006**	.3418***	.3773***	.3320***	.3875***	.3990***	.2288*	.4042***	.7810***
	(.0434)	(.0828)	(.0900)	(.0817)	(.0853)	(.0754)	(.0871)	(.0783)	(.1201)	(.1012)	(.2987)
Obs	766	766	568	749	745	683	655	756	643	766	449
KPW	–	64.20	16.67	63.88	70.01	81.08	77.44	81.23	13.80	54.79	16.93
EF index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.EF	.7651***	.7673***	.7502***	.7580***	.7273***	.7549***	.7757***	.7692***	.7346***	.7628***	.6963***
	(.0287)	(.0274)	(.0280)	(.0286)	(.0283)	(.0276)	(.0300)	(.0261)	(.0289)	(.0326)	(.0309)
L.Emig	.1365***	.1209***	.1239***	.1236***	.0952***	.0758*	.1036***	.1228***	.1069**	.1031**	.1247
	(.0350)	(.0405)	(.0407)	(.0363)	(.0339)	(.0443)	(.0371)	(.0422)	(.0444)	(.0524)	(.1255)
Obs	445	445	424	445	439	431	403	445	433	445	369
KPW	–	18.32	17.88	19.74	18.08	22.11	17.08	18.52	17.58	16.35	15.25
P2 index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.P2	.7919***	.7870***	.7661***	.7804***	.7606***	.7871***	.7885***	.7872***	.7651***	.7285***	.6124***
	(.0258)	(.0254)	(.0309)	(.0280)	(.0298)	(.0258)	(.0267)	(.0253)	(.0338)	(.0364)	(.0520)
L.Emig	.1355*	.2498	.1886	.2365	.2307	.3524*	.2165	.2412	.2157	.1559	.8149
	(.0707)	(.1720)	(.1988)	(.1670)	(.1731)	(.1818)	(.1633)	(.1601)	(.2071)	(.1883)	(.5025)
Obs	654	654	543	649	642	605	569	653	623	654	443
KPW	–	19.42	14.24	19.14	18.22	19.85	21.95	20.65	8.380	19.33	8.167

Notes: Robust standard errors clustered by country in parentheses. Col (1) shows OLS results. Col (2) to (11) show 2SLS results; total emig rate is instrumented using gravity-based, predicted emigration rates. KPW: Kleibergen–Paap rk Wald F statistics to be compared with the Stock-Yogo critical values for weak instrumentation.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

3.2. Panel analysis with 2SLS

Table 5 reports pooled OLS and 2SLS estimates for dynamic specification (1). Standard errors are robust and clustered by country. Compared to the cross-section regressions, we now control for the level of the lagged dependent variable and use panel data in 5-year intervals. The pooled OLS regression in column 1 confirms that institutional indicators are persistent and positively correlated with the emigration rates. In columns 2 to 11, we provide 2SLS results with our gravity-based instrumentation strategy. The latter builds on Feyrer (2009) and consists in introducing time dummies and interactions between these dummies and the log of distance in our pseudo-gravity regression.

We gradually add our sets of controls following the same structure as in Table 3. We only report the estimated coefficients of the lagged dependent and of the emigration rate.[24] The share of tertiary educated residents and the level of GDP per capita are always positive and significant for each indicator. Depending on the specification, the coefficient for the lagged dependent usually varies between .75 and .80. This means that it takes 14 to 17 years (2.77 to 3.47 periods of 5 years) to close half of the gap with the long-run level of institutional quality when a shock occurs.[25] Our 2SLS results therefore confirm a positive causal effect of emigration on institutions. This is the case for all indicators except the Polity 2 index, confirming our prior on de facto versus *de jure* democracy. As in the cross-sectional analysis, the coefficient of emigration increases when emigration is instrumented, reflecting the reverse causality problem of the OLS estimates. The KP F-stat of our second stage is always very large. The smallest levels are obtained when geographic controls are included (as in the cross-sectional framework, geographic controls are strongly correlated with the distance variables used in the pseudo-gravity, "zero-stage" regression) and, to a lesser extent, when human capital is included (which causes a drop in the number of observations, from 766 to 568). However, in this last case, the KP F-stat is larger than the most demanding Stock-Yogo critical value of 16.38 (10% maximal IV size).

The magnitude of the coefficient (i.e., the short-run effect) varies between .35 and .55 for the Freedom House index of political rights (PR), between .20 and .40 for the Freedom House index of civil liberties (CL), between .08 and .12 only for the index of Economic Freedom of the World (EF), and is rarely significant for Polity 2 (P2). This means, for example, that a 10-percentage point change in the emigration rate increases the PR index by 3.5 to 5.5% in the short-run. Estimation of the dynamic specification confirms that larger effects are found for perceived (de facto) levels of democracy than for the *de jure* indicator P2. Given (2), the long-run effects are obtained by multiplying the short-run coefficient by $(1-\beta)^{-1}$), i.e. by 4 to 5 depending on the specification. Remarkably, the implied long-run effects are almost identical to those obtained in the cross-sectional setting. For example, a 10-percentage point increase in the emigration rate increases the PR index by 16 to 22% in the long-run, that is, by 47 to 64% of the standard deviation, as in the cross sectional framework. The long-run effect on the CL index is slightly smaller than in cross-section, ranging from 12 to 21% (i.e., 42 to 74% of the standard deviation), but in the same order of magnitude. The long-run effect on the EF index is in line with cross-sectional results, ranging from 3 to 5% (i.e., 13 to 21% of the

[24] Estimated coefficients for control variables are provided in Tables A.5.a to A.5.d in the Online Appendix.
[25] In a dynamic model such as (1), the number of periods that a country needs to close x percent of the gap between its current level of democracy and the steady state can be proxied by $-\ln(1-x)/(1-\beta)$.

Table 6
Dynamic panel model with weather-based instruments.
Estimated coefficients for the lagged dependent and the emigration rate.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Controls	None	None	HumCap	Ethnic	GDPpc	Trade	ODA	Legal	Geogr.	Regions	All
PR index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.PR	.8113***	.7787***	.7716***	.7829***	.7559***	.7748***	.7772 ***	.7804***	.7426***	.7486***	.6154***
	(.0221)	(.0248)	(.0260)	(.0245)	(.0255)	(.0263)	(.0264)	(.0247)	(.0347)	(.0299)	(.0500)
L.Emig	.2833***	.6376***	.4708	.5646***	.5892***	.6451***	.5535***	.6299***	1.039	.6356***	2.713**
	(.0489)	(.1757)	(.2895)	(.1809)	(.1760)	(.1761)	(.1520)	(.1706)	(.8680)	(.1745)	(1.326)
Obs	766	751	568	734	736	674	646	744	637	751	449
KPW	–	10.65	4.11	9.94	9.98	13.06	12.59	11.76	2.28	11.12	2.54
JP	–	0.3809	.5686	.4198	.4127	.4901	.2515	.3757	.3129	.3996	.6852
CL index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.CL	.8358***	7881***	.7742***	.7912***	.7532***	.7974***	.7800 ***	.7951***	.7559***	.7752***	.6579***
	(.0200)	(.0254)	(.0289)	(.0242)	(.0258)	(.0267)	(.0271)	(.0246)	(.0358)	(.0269)	(.0440)
L.Emig	.2017***	.5619***	.5321**	.4936***	.5350***	.4643***	.5287***	.5765***	.9917	.5722***	1.581**
	(.0434)	(.1467)	(.2703)	(.1470)	(.1447)	(.1342)	(.1331)	(.1416)	(.8298)	(.1350)	(.7795)
Obs	766	751	568	734	736	674	646	744	637	751	449
KPW	–	10.99	4.17	10.52	10.42	14.08	12.62	12.16	2.41	11.14	2.45
JP	–	.3835	.5993	.4927	.4633	.4650	.2345	.4163	.3549	.2199	.7744
EF index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.EF	.7651***	.7654***	.7445***	.7556***	.7273***	.7587***	.7822 ***	.7669***	.7388***	.7603***	.6945***
	(.0287)	(.0264)	(.0273)	(.0279)	(.0287)	(.0267)	(.0315)	(.0255)	(.0283)	(.0318)	(.0311)
L.Emig	.1365***	.1343	.1666*	.1431*	.0956	.0067	.0565	.1400	.0123	.1381	−.0228
	(.0350)	(.0906)	(.0979)	(.0848)	(.0842)	(.1261)	(.0881)	(.0873)	(.1572)	(.0887)	(.2211)
Obs	445	445	424	445	439	431	403	445	433	445	369
KPW	–	3.60	3.90	4.25	3.33	2.42	3.80	3.51	1.50	4.52	4.46
JP	–	.0111	.0208	.0245	.0216	.0043	.0634	.0259	.0112	.0246	.2379
P2 index	OLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
L.P2	.7919***	.7835***	.7620***	.7791***	.7580***	.7657***	.7866 ***	.7839***	.7643***	.7230***	.6233***
	(.0258)	(.0282)	(.0339)	(.0298)	(.0324)	(.0324)	(.0305)	(.0277)	(.0344)	(.0383)	(.0595)
L.Emig	.1355*	.3852	.3159	.3294	.3442	.9995*	.3106	.3749	.4407	.4395	1.482
	(.0707)	(.3804)	(.4827)	(.3911)	(.3740)	(.5886)	(.3769)	(.3676)	(1.133)	(.3752)	(1.479)
Obs	654	648	543	643	636	599	563	647	617	648	443
KPW	–	4.47	2.96	4.58	3.99	6.04	3.89	4.45	1.66	3.51	1.12
JP	–	.4580	.5757	.5313	.4931	.4728	.3461	.4329	.2979	.4313	.6044

Notes: Robust standard errors clustered by country in parentheses. Col (1) shows OLS results. Col (2) to (11) show 2SLS results; total emig rate is instrumented using climatic variables. KPW: Kleibergen–Paap rk Wald F statistics to be compared with the Stock-Yogo critical values for weak instrumentation. JP: Hansen J test P-value.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

standard deviation). The only difference is that we lose significance for the Polity 2 index. As usual, the regression including all controls (column 11) is problematic, due to multicollinearity.

As a robustness check, Table 6 reports pooled OLS and 2SLS estimates when weather-based instruments are used. We follow exactly the same structure as in Tables 3 and 5. Although the R^2 of the first stage remains above .30 for the PR and CL index, it falls to .23 for the EF index and to .16 for the P2 index.[26] Consequently, the KP F-stat of the first stage is poor for the EF and P2 regressions, thus indicating a weak instrumentation problem. Nevertheless, weather shocks are generally relevant instruments for the Freedom House indices of political rights (PR) and civil liberties (CL), with the exceptions of columns 3, 9, and consequently, 11 (specification with all the controls), where we are not able to reject the null of weak instruments. This is because geographic controls in column 9 are too highly correlated with natural disasters, and human capital in column 3 kills the effect of weather shocks in the first stage; the KP F-stat falls to 4.11.[27] For all the other columns, the KP F-stat is always above the least demanding Stock and Yogo critical values (and in general above/close to the traditional "rule of thumb" of 10).[28]

Focusing on the PR and CL indices, the overidentification test for the validity of instruments (Hansen J test) is satisfied in all specifications. The short-run effect with weather-based instruments varies between .55 and .65 for the Freedom House index of political rights (PR), between .45 and .55 for the Freedom House index of civil liberties (CL). The coefficient for the lagged dependent is very stable and varies between .77 and .78. Again, long-run effects of emigration on the PR and CL indices of democracy are in the same order of magnitude albeit greater as in the previous settings. For example, a 10-percentage point increase in the emigration rate increases the PR index by 24 to 28% in the long-run (i.e., 70 to 82% of the standard deviation), and increases the CL index by 19 to 24% (i.e., 67 to 84% of the standard deviation).

3.3. Panel analysis with SYS-GMM

Tables 7 reports SYS-GMM estimates for dynamic specification (1). One advantage of this method is that it accounts for the endogeneity of all regressors, including the lagged dependent. In Column 1, we consider the parsimonious specification without controls. In columns 2 to 5, we add four sets of time-varying controls (human capital, the log of GDP per capita, trade and foreign aid as percentage of GDP), treat all

[26] Table A.2.c in the Online Appendix shows the results of the first-stage regressions (5) when controls are not included. Estimated coefficients for control variables are provided in Tables A.6.a to A.6.d.

[27] The latter result is caused by the drop in the number of observations (from 766 to 568). If we use the IAB imputed levels of education for the missing countries and re-estimate the model for political norms, the KP F-stat reaches 10.4. Interestingly, the coefficient of the emigration rate equals .6976 and becomes significant at the one percent level; the coefficient of human capital equals .2512 but is only significant at the 10 percent level. These results are available upon request.

[28] With one endogenous variables, and 3 excluded instruments, the critical values are: 22.30 (10% maximal IV size); 12.83 (15% maximal IV size); 9.54 (20% maximal IV size); and 7.80 (25% maximal IV size).

Table 7
SYS-GMM with internal instruments.
Estimated coefficients for the lagged dependent and the emigration rate.

	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Controls	None	HumCap	GDPpc	Trade	ODA	None	HumCap	GDPpc	Trade	ODA
	Index of political rights (PR)					Index of civil liberties (CL)				
L.dependent	.7635***	.7758***	.7712***	.7790***	7227.***	.6706***	.7471 ***	.6671***	.7225***	.6524***
	(.0413)	(0350.)	(.0359)	(.0453)	(.0472)	(.0399)	(.0365)	(.0429)	(.0467)	(.0454)
L.Emig	.3163***	.1977*	.3249***	.3424***	.3697***	.4074***	.1731**	.3755***	.3416 ***	.4212***
	(.0768)	(.1045)	(.0842)	(.1006)	(.0874)	(.0823)	(.0786)	(.0858)	(.1031)	(.0870)
Obs	766	568	745	683	655	766	568	745	683	655
AR1.p	7.9e−11	1.3e−08	2.2e−10	1.8e−10	7.3e−09	7.0e−12	3.2e−09	3.0e−11	2.8e−10	1.9e−09
AR2.p	.7329	.9619	.7310	.2717	.3256	.9380	.9696	.9731	.9840	.9436
Hansen.p	.2260	.1429	.2078	.3733	.4275	.0132	.0335	.0193	.0741	.0527
Diff.Hansen	.3330	.1160	.3240	.4200	.9000	.0920	.0240	.1720	.0410	.0210
j	40	57	57	57	57	40	57	57	57	57
N.g	139	99	135	133	132	139	99	4135	133	132
	Index of economic freedom (EF)					Index of Polity 2 (P2)				
L.dependent	.7595***	.6698***	.7022***	.6969***	.7455***	.7590***	.7193 ***	.7693***	.7635***	.7318***
	(.0490)	(.0534)	(.0532)	(.0546)	(.0562)	(.0449)	(.0495)	(.0442)	(.0417)	(.0484)
L.Emig	.1393**	.1497***	.1282**	.1722***	.1056**	.1307	.1806	.1916	.3054**	.1953*
	(.0569)	(.0537)	(.0547)	(.0603)	(.0417)	(.1288)	(.1305)	(.1258)	(.1512)	(.1089)
Obs	445	424	439	431	403	654	543	642	605	569
AR1.p	2.1e−06	1.0e−05	1.5e−06	4.6e−05	8.2e−05	7.4e−10	1.7e−07	1.1e−09	5.4e−09	8.5e−08
AR2.p	.0650	.0896	.0663	.1126	.0694	.4664	.7794	.4670	.2800	.2470
Hansen.p	.1866	.5912	.2556	.2230	.2598	.0653	.1287	.0728	.4947	.1476
Diff.Hansen	.1470	.8570	.8000	.5420	.7210	.5070	.3530	.3060	.9850	.5390
j	40	57	57	57	57	40	57	57	57	57
N.g	79	75	78	79	73	120	96	118	117	113

Notes: Robust standard errors clustered by country in parentheses. One-step SYS-GMM estimator. AR(1) and AR(2): p-values of Arellano-Bond test for serial correlations. Hansen J test: p-values for the null hypothesis of instrument validity. Diff.Hansen: p-values for the null hypothesis of the joint validity of additional instruments used in SYS-GMM. All variables are instrumented using their own 1st to 3rd lags. In addition, SYS-GMM uses 1st differences lagged one period as instruments for the level equations. J indicates the number of instruments. N.g indicates the number of countries.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

explanatory variables as predetermined and instrument them using their first to third lags.[29] We do not control for time invariant variables (ethnic fractionalization, legal origin, geography and regional dummies) as the SYS-GMM estimator accounts for unobserved heterogeneity. Standard errors are robust and clustered by country group.

The SYS-GMM analysis confirms the cross-sectional and panel results obtained with the 2SLS methods. The coefficient for the lagged dependent varies between .70 and .77. This means that it takes 12 to 15 years (2.3 to 3.0 periods of 5 years) to close half the gap with the long-run level of institutional quality when a shock occurs. Importantly, our SYS-GMM estimates for emigration remain positive and statistically significant at usual significance levels, except for the (*de jure*) Polity 2 index. In the SYS-GMM setting, the short-run effects of emigration are identical to those obtained with the 2SLS estimator. The estimated coefficient of emigration varies between .20 and .37 for the Freedom House index of political rights (PR), between .17 and .42 for the Freedom House index of civil liberties (CL), and between .11 and .17 for the index of Economic Freedom of the World (EF).

It is worth noticing that the size of the coefficient and its significance level are smaller when human capital is accounted for, in spite of the fact that human capital is weakly significant. This is mainly because the inclusion of human capital drastically reduces the number of observations (from 766 to 568 observations). In Table A.8 in the Online Appendix, we compare results obtained with alternative measures of human capital taken from the Barro and Lee's database (i.e. the proportions of individuals with primary schooling in columns 2 and 3, with secondary schooling in columns 4 and 5, and with tertiary schooling in columns 6 and 7) for the PR indicator. Results obtained with primary and secondary education are never significant, independently of whether emigration is included in the regression. Hence, the insignificance of these variables is not driven by the collinearity with emigration rates. The best specification is the one including the proportion of college-educated, although this variable is significant at the 10% threshold only. In the last column of Table A.8, we exploit the human capital indicators of the IAB database, which imputes the levels of education in countries where Barro and Lee's data are missing. Compared to the parsimonious specification of column 1, this leaves the number of observations and the estimated coefficients of the emigration rate unchanged, while human capital becomes insignificant.

Recall that the literature on education and democracy is inconclusive: while Acemoglu et al. (2005b) found that education has no explanatory power for democracy, Bobba and Coviello (2007), Castello-Climent (2008) and Murtin and Wacziarg (2014) found a positive and significant effect. Our results suggest that the effect might be there but does not prove to be highly robust to specification choices and instrumentation strategies.[30]

Long-run effects of emigration are in the same order of magnitude, albeit slightly smaller (due to a smaller coefficient for the lagged dependent) than in the 2SLS cross-sectional and panel frameworks. Using results from the parsimonious specification, a 10-percentage point change in the emigration rate increases the PR, CL and EF indices respectively by 13, 12 and 6 percentage points in the long-run (i.e., respectively 38, 42 and 26 percent of the relevant standard deviations). The AR(2) row tests the null hypothesis that the error term does not exhibit second-order serial correlation. The Hansen J-test of overindentifying restrictions and the difference Hansen test indicate that the moment

[29] Estimated coefficients for control variables are provided in Tables A.7.a to A.7.d in the Online Appendix.

[30] This could be due to differences in time coverage: Castello-Climent (2008) considered the 1965–2000 period in 5-year intervals. Murtin and Wacziarg (2014) used data from 1870 to 2000 in 10-year intervals; they conducted robustness checks by sub-periods but their shortest period goes from 1960 to 2000.

Table 8
Dynamic panel results by sub-sample.
Parsimonious specification — Index of political rights (PR).

	(1) Socialist countries dropped	(2) Sub-Saharan countries dropped	(3) Oil prod. countries dropped	(4) MENA countries dropped	(5) Year 1980 dropped	(6) Years 1980–85 dropped	(7) Years 2005–10 dropped	(8) Year 2010 dropped	(9) Balanced panel
L.PR	.7086***	.8032***	.7663***	.7407***	.7407***	.6805***	.7755***	.7691***	.7796***
	(.0496)	(.0505)	(.0375)	(.0414)	(.0513)	(.0566)	(.0601)	(.0455)	(.0401)
L.Emig	.3687***	.2613***	.2839***	.2868***	.3157***	.4696***	.2533 *	.2755***	.3044 ***
	(.0820)	(.0820)	(.0723)	(.0757)	(.0935)	(.1528)	(.1346)	(.0997)	(.0761)
Constant	.1222***	.0807***	.0938***	.1113***	.0999***	.1213***	.1168 ***	.1211***	.0839***
	(.0288)	(.0288)	(.0238)	(.0255)	(.0282)	(.0305)	(.0302)	(.0244)	(.0247)
Obs	646	498	696	696	651	534	489	627	690
AR1.p	3.3e−09	1.1e−05	1.0e−09	1.9e−10	1.0e−12	1.1e−07	2.2e−07	5.9e−10	1.7e−09
AR2.p	.5514	.7768	.7154	.7271	.4767	.7994	.7822	.7515	.7734
Hansen.p	.2332	.3985	.2188	.2198	.1030	.0643	.0542	.0781	.1248
Diff.Hansen	.699	.303	.457	.281	.241	.092	.080	.193	.181
j	40	40	40	40	31	22	22	31	40
N.g	112	93	127	127	139	139	138	138	115

Notes: Robust standard errors clustered by country in parentheses. One-step SYS-GMM estimator. AR(1) and AR(2): p-values of Arellano-Bond test for serial correlations. Hansen J test: p-values for the null hypothesis of instrument validity. Diff.Hansen: p-values for the null hypothesis of the joint validity of additional instruments used in SYS-GMM. All variables are instrumented using their own 1st to 3rd lags. In addition, SYS-GMM uses 1st differences lagged one period as instruments for the level equations.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

conditions are satisfied and the instruments are valid in most regressions, with exception of the CL index. Although, for CL, the test works better under alternative lag structures, we have used a common lag structure for all specifications for transparency reasons.[31]

3.4. Robustness by sub-sample

The above results suggest that emigration positively affected institutional quality in developing countries between 1980 and 2010. In this section, we conduct three sets of robustness checks. The results are presented in Table 8 for the Freedom House Political Rights Index (PR).

First, we investigate whether our results could be driven by the inclusion of countries sharing specific characteristics. Building on the parsimonious specification (1) in Table 7, we re-estimate the model using four alternative samples of countries and relying on the SYS-GMM estimator. We first exclude socialist countries (defined on the basis of the legal origin dummy) in columns 1. The rationale for doing this is that emigration was legally restricted in these countries prior to the transition while the fall of the Berlin wall drastically affected the evolution of institutions and of emigration patterns. One may also be concerned by the fact that the pre- and post-transition trajectories of human capital have been peculiar in socialist countries (see Acemoglu et al., 2005b). We exclude sub-Saharan African countries in column 2. Sub-Saharan African countries are on average less stable politically than the other countries in our sample. We exclude oil-exporting countries in column 3. Several studies have pointed out a negative correlation between oil exports (and of natural-resource dependence in general) and democracy (see Ross, 2001; Tsui, 2011). We exclude MENA countries (Middle East and Northern Africa) in column 4. These countries are mostly populated by Muslims, and the proportion of Muslims has often been used as a control variable in the literature (Castello-Climent, 2008; La Porta et al., 1999). These four groups all exhibit average emigration rates below the full sample 5.5 percent mean (3.4% in socialist countries, 2.3% in sub-Saharan Africa, 2.3% in oil producing countries, and 3.4% in the MENA region).

The estimated coefficient for emigration is always positive and highly significant in all sub-samples. The long-run effect of a 10-percentage point increase in emigration on PR varies between 11 and 13 percentage points (i.e., 32 to 42% of the standard deviation), which is clearly in line with the full-sample results. As discussed above, the 20-destination IAB database imperfectly captures the emigration from Socialist and Sub-Saharan African countries. These mismeasurement issues do not affect our main findings.

Second, one of the assumptions of the SYS-GMM estimator is that first differences of predetermined explanatory variables are orthogonal to the fixed effects, which imply that the series in level have constant correlation over time with individual fixed effects (Bun and Sarafidis, 2013). Bun and Sarafidis (2013) provide an alternative specification of the constant correlated effects assumption, which is that deviations of the initial in-sample conditions from the respective steady state behavior are not systematically related to the level of the steady state itself. In our case, this assumption can be questioned if the secular trends in democratic quality and in emigration rates follow highly non-monotonic dynamics. One way to alleviate these concerns is to check the robustness of our results using subsamples in terms of the length of the observation period thus changing both the length and the in-sample initial conditions. In columns 5 and 8, we estimate the model after eliminating the first or last time series observation, respectively. The estimated coefficient of the emigration rate remains positive and highly significant with a quite stable coefficient. The long-run effect of a 10-percentage point increase in emigration on PR is around 11 percentage points in both cases (i.e., 32% of the standard deviations). In columns 6 and 7, we eliminate the first two and last two periods, respectively. Although the number of observations falls, the effect of emigration remains positive and significant, however the Hansen J test and the Difference Hansen test are weaker (but still suggests that our instruments are valid at the 5% significance level).[32]

Finally, it could be argued that using an unbalanced panel is problematic as countries are included in the sample after gaining independence, which might coincide with an episode of democratization. In column 9, we therefore estimate our model using a fully balanced sample. Again, the effect of emigration is positive and significant. The long-run effect of a 10-percentage point increase in emigration on PR equals 11 percentage points.

[31] For instance, in the specification without additional controls, instrumenting the lagged dependent and the emigration rate with their third to fifth lags, we obtain (i) statistically significant results for both the lagged dependent and the total emigration rate, (ii) a Hansen test p-value of 0.233, and (iii) a difference Hansen test p-value of 0.133.

[32] Results do not change if we consider the same number of countries (138) across the different sample periods.

Table 9
Dynamic regressions with skill-specific emigration rates.
Parsimonious specification — index of political rights (PR).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.PR	.7635***	.7820***	.7506***	.7547***	.7983***	.7804***	.7779***
	(.0413)	(.0383)	(.0427)	(.0407)	(.0386)	(.0395)	(.0462)
L.Emig	.3163***				.3197***	.3277***	.7354***
	(.0768)				(.0823)	(.0823)	(.2528)
L.Emig.High		.8447***		−.5235			
		(.2158)		(.5611)			
L.Emig.Low			.4720***	.6643**			
			(.1214)	(.2696)			
L.Share.High					.0771		
					(.1228)		
L.Share.Low						−.0407	
						(.1266)	
L.Emig × L.PR							−.5472***
							(.3046)
Constant	.0877***	.0806***	.0946***	.0953***	.0371	.1018***	.0741***
	(.0235)	(.0230)	(.02241)	(.0237)	(.0601)	(.0480)	(.0230)
Obs	766	766	766	766	766	766	766
AR1.p	7.9e−11	9.3e−11	5.9e−11	4.3e−11	1.3e−10	1.9e−10	1.8e−10
AR2.p	.7329	.7212	.7399	.7479	.7491	.7422	.7349
Hansen.p	.2260	.2230	.2296	.1521	.4110	.3112	.4015
Diff.Hansen	.333	.282	.302	.255	.217	.267	.489
j	40	40	40	57	57	57	57
N.g	139	139	139	139	139	139	139

Notes: Robust standard errors clustered by country in parentheses. One-step SYS-GMM estimator. AR(1) and AR(2): p-values of Arellano-Bond test for serial correlations. Hansen J test: p-values for the null hypothesis of instrument validity. Diff.Hansen: p-values for the null hypothesis of the joint validity of additional instruments used in SYS-GMM. All variables are instrumented using their own 1st to 3rd lags. In addition, SYS-GMM uses 1st differences lagged one period as instruments for the level equations.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

3.5. Testing for skill-specific effects

Finally, we explore whether the effect of emigration could be governed by the education level of emigrants (and not only by their number). This means that we allow for the effect of emigration in Eq. (1) to vary with the education level of emigrants. The technology becomes:

$$I_{i,t} = \alpha + \beta I_{i,t-1} + \gamma\left[m^H_{i,t-1} + \rho m^L_{i,t-1}\right] + \sum\nolimits_k \delta_k X^k_{i,t-1} + \varepsilon_{i,t} \tag{6}$$

where $m^H_{i,t-1}$ measures the emigration rate of individuals with tertiary education and $m^L_{i,t-1}$ is that of the less educated; the parameter ρ captures the relative effectiveness of low-skilled emigrants in promoting democracy compared with the highly-skilled.

We use the SYS-GMM estimator. We start from the parsimonious specification including only the lagged dependent and the total emigration rate (see column 1 of Table 7). We only provide results for the PR index in Table 9.

Our main strategy to capture potential heterogeneous effects for different types of migrants consists in splitting the total emigration rate by education level. More precisely, we compute skill-specific emigration rates ($m^H_{i,t-1}$ and $m^L_{i,t-1}$) by dividing the number of high-skill and low-skill emigrants by the total native population aged 25 and over. We use the same denominator as in our computation of the total emigration rate.[33] We include $m^H_{i,t-1}$ and $m^L_{i,t-1}$ separately in columns 2 and 3, and find estimated coefficients of .8447 and .4720, respectively. Nevertheless, we cannot infer from these regressions that high-skill emigration has a greater impact on democracy than low-skill emigration. Indeed, the data reveal that $m^H_{i,t-1}$ and $m^L_{i,t-1}$ are linked by a relationship of proportionality.[34] If $m^L_{i,t-1} \simeq 2m^H_{i,t-1}$ (as in our sample), this means that $[m^H_{i,t-1} + \rho m^L_{i,t-1}]$ is alternatively equal to $m^H_{i,t-1}(1+2\rho)$ and to $m^L_{i,t-1}(1+2\rho)/2$. Regressing democracy on high-skill emigration gives a coefficient, $\gamma(1+2\rho)$, that should be twice as large as the one obtained from the regression of democracy on low-skill migration, $\gamma(1+2\rho)/2$. Hence, nothing can be inferred from the comparison between columns 2 and 3. In our dataset, $m^H_{i,t-1}$ and $m^L_{i,t-1}$ exhibit a correlation of about 80%. Although it is not recommended to include them jointly in the same regression, we did so for completeness in column 4 of Table 9. High-skill emigration loses significance whereas the effect of low-skill emigration increases but is only significant at 5%.

Comparing the benchmark regression on $m_{i,t-1}$ (column 1) to that on $m^H_{i,t-1}$ (column 2), however, is instructive. If ρ is close to one (i.e. same effectiveness of high-skill and low-skill emigration), the emigration coefficient in the second regression, $\gamma(1+2\rho)$, should be approximately three times as large as γ, the coefficient in the first regression. Using the same reasoning, the coefficient of the regression with low-skill emigration, $\gamma(1+2\rho)/2$, should be approximately 1.5 times as large as γ. This is exactly what Table 9 reveals. At first glance, this suggests that high-skill and low-skill emigration exert a similar impact on institutions. This does not mean that the selection of emigrants cannot influence the quality of institutions through other channels. As high-skill emigration rates are twice as large as the low-skill ones, emigration reduces the stock of human capital of the origin country and this could be detrimental to democracy. However, our analysis reveals that the direct effect of human capital does not prove to be robust to specification choices and instrumentation strategies. Hence, we cannot conclude that positive selection in emigration is harmful to democracy. This result is confirmed by the regression in columns 5 and 6, in which we interact the total emigration rate with the proportion of highly-educated or low-skilled individuals among emigrants. The coefficients of these interaction terms, *L. Share. High* and *L. Share. Low*, are insignificant. Altogether, the results do not support the existence of heterogeneous effects across skill groups.

In addition, we also considered sub-samples excluding the countries with the highest shares of both high-skill and low-skill emigrants. Unreported results show that the effect of emigration increases when we drop the quartile of countries with the highest shares of college graduates among emigrants, and conversely if we drop the quartile of

[33] These variables are denoted by *L. Emig. Low* and *L. Emig. High* in Table 9.
[34] See Figure A.1 in the Online Appendix.

countries with the lowest shares. However, we cannot infer from these results that less educated emigrants are more effective at improving institutions. Indeed, eliminating the top and bottom quartiles based on a different criterion (GDP per capita, or level of democracy) gives similar results: the effect of emigration is greater in poor countries with initially bad institutional quality. Rather, we conclude that the effect of emigration is nonlinear, a result confirmed in column 7 of Table 9 where we introduce an interaction term between emigration and the lagged dependent variable. The beneficial effect of emigration on democracy decreases with the lagged level of democracy itself.

4. Conclusion

This paper empirically investigates the overall impact of emigration on institutions in a large sample of developing countries. We find that openness to migration (measured by the total emigration rate) contributes to improve institutional quality (as measured by standard indicators of democracy and economic freedom) in the migrants' origin countries. Interestingly, this result holds for our three (out of four) indicators that can be considered as indicators of de facto institutional quality (the Freedom House's "Political Rights" and "Civil Liberties" indicators, and the Simon Fraser Institute's "Economic Freedom of the World" indicator), but not for the "Polity 2" indicator of the Polity IV Project, an indicator of *de jure* institutional quality.

Overall the results appear quite robust across specifications and estimation methods. They are also robust to the use of different sub-samples of countries (e.g., excluding oil-producing countries, former socialist countries, or sub-Saharan countries), time-periods (e.g., excluding the first or last sub-periods) and migrants' type (skilled versus unskilled). Remarkably, the cross-sectional estimates are fully in line with the implied long-run relationship obtained from dynamic panel regressions. In terms of magnitude of the coefficients, the dynamic panel regressions give a short-run effect around .4 to .5, quite stable across specifications; given that the coefficient of the lag-dependent is .7, this gives a long-run effect of 1.5 to 2, very similar to the one obtained in the cross-section. In other words, a ten percentage point increase in the emigration rate increases the main indicators of institutional quality by about 5 percentage points in the short-run and 15 to 20 percentage points in the long run (i.e., 45 to 60% of the standard deviation of the relevant democracy indicator).

Finally, we note that our main result is fully driven by emigration to rich, highly democratic countries, suggesting that the effect of emigration on home-country institutional outcomes is destination-specific. Indeed, when we use alternative migration data sources allowing to disentangle the effect of emigration to OECD versus non-OECD destinations, the effect of emigration to the latter is virtually zero. We therefore conclude that emigration to liberal democracies played an important positive role in determining institutional and political change in developing countries.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jdeveco.2015.12.001.

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