

# Emigration and Political Contestation

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## Abstract

How does migration affect global patterns of political violence and protest? While political scientists have examined the links between trade and conflict, less attention has been paid to the links between migration and conflict. In this paper, we show that greater emigration reduces domestic political violence by providing exit opportunities for aggrieved citizens and economic benefits to those who remain. Emigration also reduces non-violent forms of political contestation, including protests and strikes, implying that high emigration rates can produce relatively quiescent populations. However, larger flows of emigrants to democracies increase non-violent protest in autocracies, as exposure to freer countries spreads democratic norms and the tools of peaceful opposition. We use instrumental variables analysis to account for the endogeneity of migration flows and find robust results for a range of indicators of domestic violence and protest from 1960 to 2010.

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## Introduction

In 2011, the Arab Spring produced political contestation unmatched in the Arab world for decades. Yet conditions on the ground—including high youth unemployment and low economic growth—were little changed from the past. One factor that had changed was the opportunity to emigrate, as the 2008 financial crisis shrank emigration flows to Europe by 80% for Morocco, 95% for Tunisia, and 88% for Algeria from 2008 to 2011 (DEMIG 2015). Is there a link between emigration and political contestation?

We argue that emigration powerfully shapes a country's likelihood of both violent and non-violent contestation. Specifically, we show that higher emigration rates predict less political contestation across a range of measures, including civil war, insurgencies, protests, and strikes. In addition, greater emigration to democracies shifts opposition tactics in autocracies toward non-violent activity.

Higher rates of emigration reduce contestation through two mechanisms: the safety valve effect and economic benefits. Emigration attracts those unhappy with the state of the country, selecting out political opponents and average citizens seeking better economic conditions, especially unemployed young men (Hirschman 1970; 1978). This leaves fewer potential recruits for opposition movements and insurgencies, and by providing an outside option, reduces the incentive to fight.

Emigration also improves economic well-being (Clemens 2011). Remittances sent back by migrants increase household welfare and have multiplier effects throughout the economy. Emigration also increases trade (Gould 1994), foreign investment (Leblang 2010), and aid (Bermeo and Leblang 2015). Together, these effects reduce economic deprivation, leading to fewer grievances and higher opportunity costs for joining opposition movements. Moreover, greater economic resources can improve a regime's repressive capacity.

Left at this, emigration should generally lead to less politically active populations, bolstering the survival of autocratic and democratic regimes alike. However, we argue that emigration can *increase* non-violent contestation when emigrants travel from autocracies to democracies. Exposure to democratic freedoms can inspire demands for political reform at home. Simultaneously, migrants absorb norms of non-violent contestation and learn the associated

tools through exposure to protest, civil society groups, and unions. In turn, these migrants either return home or transfer these skills and values through what Levitt (1998) calls “social remittances.”

Testing the relationship between emigration and contestation confronts a serious endogeneity problem. As the Syrian civil war demonstrates, civil conflict often sparks large flows of emigrants. Further, contestation and emigration may spring from the same source: unhappiness with the ruling regime. To demonstrate a causal effect, we use an instrumental variables analysis that leverages variation in dyadic emigration patterns from exogenous geographic and socioeconomic factors. Using more than 110,000 data points for dyadic emigration, the analysis aggregates up to produce exogenous country-level estimates of total emigration and the share of emigrants going to democracies, adapting a technique first applied by Frankel and Romer (1999) to trade.

Our results show that a higher emigration rate (relative to population) reduces both violent and non-violent contestation, using a range of dependent variables. The effect holds in both autocracies and democracies and is robust to controlling for potential confounders like development, trade, foreign aid, and regional diffusion. It’s also stronger during economic crises. In comparison, more emigration to democracies predicts more non-violent contestation, but only in autocracies. In addition to this quantitative evidence, we present illustrative case studies of Portugal, Spain, and Morocco.

Although international migration is among the most significant forces in the global economy, with more than 250 million migrants currently living abroad (UN 2017), we have limited understanding of its political effects on sending countries. Connecting migration to political violence and protest can help us better understand how to limit violent contestation globally and encourage non-violent contestation in autocracies. Additionally, this paper highlights how emigration to consolidated democracies can not only help those in the developing world, but also benefit Western democracies through enhanced political stability abroad.

Finally, this paper illuminates the relationship between opportunities for exit and voice, an enduring subject since Hirschman (1970; 1978). In particular, it serves as one of the first global tests of Hirschman’s claim that “the presence of the exit alternative can...

atrophy the development of the art of voice” (Hirschman 1970, 43). This can aid our understanding of conflict’s origins, including the role of international economic flows and domestic opportunities. In particular, our results illustrate how individual strategic choices undergird societal conflict and protest.

## **Existing Work on Migration and Political Contestation**

The theoretical work most relevant to migration and contestation remains Hirschman (1970; 1978; 1993), who posits exit and voice as alternative responses of dissatisfied customers or citizens. Since emigration and contestation both stem from regime opposition, we might expect to see a positive relationship between the two, as with the simultaneous protest movement and mass exodus from East Germany in 1989 (Hirschman 1993). However, Hirschman’s argument does not imply that emigration *causes* higher levels of contestation or vice-versa. To the contrary, the presence of easy exit incentivizes less voice and, in the long run, reduces the skills and organization needed for effective voice (Hirschman 1970).

Despite this influential theoretical grounding, the empirical relationship between migration and contestation has been understudied. Although the conflict literature has examined structural factors making civil conflict more likely (*e.g.*, Fearon and Laitin 2003; Collier and Hoeffler 2004) and characteristics that make individuals more likely to fight (*e.g.*, Humphreys and Weinstein 2008), migration has been neglected despite potentially tying into both mechanisms. Studies that do address this relationship mostly focus on *immigration’s* effect on conflict, (*e.g.*, Salehyan and Gleditsch 2006; Salehyan 2008).

Other work looks at how diasporas fuel conflict. Ethnic groups living abroad frequently have deep-seated conflicts with the regime or rival domestic groups that lead them to support extremists and try to protect their co-ethnics from persecution (Posen 1993; Collier and Hoeffler 2004). In turn, diasporas often fund conflicts through remittances to armed groups. For example, Irish migrants to the U.S. have helped fund IRA activities since the late nineteenth century (Lucassen 2005), while Tamils living abroad provided much of the LTTE’s funding (Wayland 2004). Collier and Hoeffler (2004) find that larger diasporas in the U.S. increase the probability of civil war onset in sending countries. More recently, Miller and Ritter (2014) ar-

gue that diasporas foster civil war by providing resources to insurgent groups. However, these studies do not convincingly account for the endogeneity of their main variables, nor discount the possibility of reverse causation from conflict to emigration. Further, as Adamson (2013) argues, there are many diasporas which have not supported conflict, thus raising the question of when and how diasporas are mobilized.

A few other recent papers examine how emigration relates to civil violence.<sup>1</sup> In a working paper, Preotu (2016) relates emigration to civil conflict using a similar identification strategy as the current paper. She finds that greater predicted emigration to OECD states is negative for civil conflict. We present a broader analysis by including all emigration destinations, predicting both violent and non-violent contestation, and distinguishing between country types among the emigration targets.

Cross-national work on emigration and non-violent contestation is much less extensive. In the most relevant work, Barry et al. (2014) finds that higher emigration freedom reduces anti-regime protest in modern autocracies, but only when there are economic opportunities abroad. However, they do not directly test the effect of emigration itself. It thus remains an open question whether emigration has a causal effect on various forms of political contestation and whether emigrant destinations matter.

## **Theory on Emigration and Political Contestation**

We argue that both total emigration and where emigrants go shape political contestation. Total emigration decreases both non-violent and violent contestation by providing a peaceful outlet for potential dissidents and economic benefits that reduce deprivation and enable governments to invest in security. However, when emigrants move to democracies, they increase their demands for change at home (Miller and Ritter 2014; Moses 2011), while also absorbing the norms and organizational tools of peaceful contestation. Thus, greater flows to democracies can facilitate peaceful protest. This is not necessarily a major threat for all regimes, as

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<sup>1</sup> For instance, Okamoto and Wilkes (2008) analyze how opportunities for exit and voice predict ethnic rebellion.

nearly half of all migrants now move to wealthy autocracies—such as the Gulf Cooperative Council states and Singapore—or within the developing world (World Bank 2017).

In our theory and empirics, we include all types of migrants: temporary and permanent, refugees and economic migrants, low-skill and high-skill. Given the available migration data, we cannot test differences among these groups, but we expect the mechanisms to differ only slightly in magnitude.<sup>2</sup> However, we emphasize we are capturing average treatment effects across a diverse population.

### **The Safety Valve Effect of Emigration**

Hirschman (1970; 1978; 1993) famously argued that disaffected populations can choose two methods of opposition: exit and voice. In line with Hirschman, we expect that easier exit through emigration opportunities should reduce voice, including both peaceful and violent forms of opposition. We call this the *safety valve effect*.

Most migrants move for primarily economic reasons—less than 10% of migrants were refugees in 2014 despite a spike in flows (World Bank 2017)—but many also trade off political concerns at the margins. Thus, on an individual basis, emigration first selects out the most economically frustrated individuals, whose frustrations would otherwise slowly build. These are precisely those individuals (disproportionately young men) most ripe for recruitment into rebel groups (Humphreys and Weinstein 2008). Freer exit further selects out populations politically unhappy with the regime, with activists, opposition voters, and non-allied ethnic groups the most motivated to leave. As a result, emigration pushes out citizens who would be active members of the opposition if they lacked opportunities abroad, generating a self-selected population more loyal to the regime. As Goodwin (2001) argues, revolutions develop when domestic groups have “no other way out” in the face of repressive and unresponsive states. Emigration provides the way out.

Emigration opportunities also influence those left behind. Similar to the “brain gain” effect in which people gain education in hopes of emigrating (Chand and Clemens 2008), the

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<sup>2</sup> For instance, high-skill migrants may more often serve as opposition leaders, but still require motivated followers to mobilize, so the safety valve effect should hold across economic classes. A longer time abroad may heighten normative exposure, but return migration magnifies normative diffusion back home, so the role of migration permanence is ambiguous.

possibility of leaving should make individuals less likely to take up arms, which would compromise their ability to emigrate.<sup>3</sup> Emigration's effect on the labor market at home also provides increased job opportunities for those left behind. Thus, emigration impedes opposition organization by those remaining both by removing the most likely recruits and raising the opportunity costs for those left behind. The result is a population-wide shift to exit away from voice.

Understanding these dynamics, political leaders strategically use emigration to quash opposition. Many European countries that restricted exit prior to the 1848 revolutions embraced emigration to sap the strength of popular movements. The British government used emigration to move Irish activists to the colonies (Hirschman 1978). When allocating visas under the Bracero Program—the agricultural guestworker program between the U.S. and Mexico—the Mexican government favored areas with opposition party support and hotbeds of insurgency in the Cristeros War (Moses 2011, 129-130). Even the Soviet Union initially allowed free exit to purge political opponents (Light 2012).

### **The Economic Benefits of Emigration**

In addition to providing an outside option, emigration brings economic benefits that dampen political contestation and violence. Chief among these benefits are remittances, which at over \$400 billion per year are now three times the size of official development aid and make up more than 10% of GDP in 25 countries (World Bank 2017).

Remittances have complex effects on anti-regime support. On the downside for regimes, remittance income can reduce the effectiveness of state patronage, making citizens more willing to support opposition parties (Pfutze 2014) and protest (Escriba-Folch, Meseguer and Wright 2018). However, this assumes remittances don't affect citizens' attitudes toward the regime or augment regime resources. Through these other channels, we expect the net effect of remittances to reduce contestation. Remittances improve economic conditions and in turn increase regime support (Ahmed 2017; Tertychnaya et al. 2018). Because they are counter-cyclical, remittances are especially beneficial during economic crises, key moments of regime vulnera-

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<sup>3</sup> This is especially true for emigration to OECD states, as many prohibit terrorist supporters from immigrating, which often restricts rebel combatants.

bility. Remittances also benefit non-receiving families through multiplier effects (Taylor 1999). Additionally, while remittances go directly to families, they typically free up public resources that can be redirected to social spending or the state's security forces (Ahmed 2012; Easton and Montinola 2017).

Large-scale emigration has a variety of other systemic economic effects (Clemens 2011). Emigration leads to more trade (*e.g.*, Gould 1994), foreign investment (*e.g.*, Leblang 2010), and aid (Bermeo and Leblang 2015), all of which increase economic activity. Clemens (2011) shows that emigration can dramatically expand poor countries' economies, with fully free migration theoretically increasing global GDP by 50–150%. Finally, large-scale emigration reduces the supply of domestic workers, leading to greater opportunities and wages for those left behind.<sup>4</sup> While some lament the effects of “brain drain” (the emigration of the highly educated) on economic capacity (Miyagiwa 1991), this may also have a pacifying effect by reducing a population likely to develop grievances against the state if unemployed.

### **Emigration to Democracies and Peaceful Contestation**

While emigration overall should lower contestation, more emigration to democracies should shift opposition tactics to peaceful contestation in autocracies. We expect this to occur through the spread of democratic norms, increased capacities among return migrants after gaining exposure to non-violent opposition, and freedom to organize abroad.

As several recent studies have shown, even brief periods living in stable democracies can shift migrants' attitudes in a more pro-democratic direction (Chauvet and Mercier 2014; Levitt 1998; Pérez-Armendáriz and Crow 2010; Spilimbergo 2009). While living in a democracy, migrants experience how different political life can be, varying from the routine (*e.g.*, few officials asking for bribes) to the more fundamental (*e.g.*, workers' rights, a free press, and legal equality). For example, a leader of Mongolia's opposition noted that emigrants' and foreign students' experiences in Europe led them to no longer “sympathize with the one-party

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<sup>4</sup> For example, Hatton and Williamson (1998, 224) find that emigration to the New World led to a 33% increase in Italian and Irish wages.



ideology” and instead join the 1989-90 protest movement (Kaplonski 2012, 48). Migrants thus often learn that life is better in democracies.<sup>5</sup>

Migrants spread the resulting norms when they return home or through contact with friends and family.<sup>6</sup> These *social remittances* can have powerful effects on sending communities (Levitt 1998; Pérez-Armendáriz and Crow 2010). While we focus here on democratic behavior, migration and travel can spread a variety of norms. For example, communities in Mali with many emigrants to countries with strong norms against female genital mutilation show decreased support for the practice (Diabate and Mesplé-Somps 2015).

The spread of democratic norms should be especially likely to increase support for *peaceful* opposition against *autocratic* regimes. Populations with higher demands for democracy should be more likely to target autocratic regimes back home. Further, the types of non-violent political contestation prominent in democracies, such as protests and strikes, become natural tools for migrants socialized into democratic behavior. This is even more likely if experiences abroad demonstrate the efficacy of non-violence (Chenoweth and Lewis 2013).

In addition, migrants to democracies have opportunities for peaceful political participation (such as protest and union activity) that can build critical political capacities and social capital. For example, several Norwegian labor leaders in the early twentieth century worked with labor unions in the U.S., where they learned how to mobilize politically for greater rights, including extended suffrage (Moses 2011). Similarly, Sellars (2017) finds that Mexicans deported from the U.S. in the 1930s played an important role in the agrarian movements back home that led to land reform.

Finally, emigrants to democracies often have greater room to organize opposition to the regime at home. These migrant organizations then often link to domestic opposition groups as well as global networks. This room to organize is notably not available to emigrants to autocracies (Ruhs 2013) nor to violent groups, which are typically restricted even by democracies.

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<sup>5</sup> An empirical concern is that migrants to democratic countries may have had democratic leanings to begin with. This is another reason we instrument for emigration destinations.

<sup>6</sup> According to the OECD, between 20% and 50% of migrants return home within five years of migrating, which in 2013 amounted to about 82 million people (Wahba 2015).

Autocratic governments understand the potential for norm diffusion and frequently monitor their emigrant populations. The Chinese Communist Party, for example, supports the Chinese Students and Scholars Association, which provides services for Chinese students living in the West, while informing on their activities to the government (Saul 2017). Below, we document how the Moroccan government infiltrated migrant groups in Europe for similar reasons.

In sum, we arrive at the following two hypotheses:

**Hypothesis 1:** *All else equal, higher rates of emigration should predict less violent and non-violent political contestation.*

**Hypothesis 2:** *All else equal, higher rates of emigrants moving to democracies should predict more non-violent political contestation in autocracies.*

## Illustrative Cases

We now consider three cases that illustrate our mechanisms: Portugal, Spain, and Morocco.

### Portugal

Portugal stands out for its high emigration rate during its autocratic period and the strong connection between emigration and political activism during its transition. Seeking better economic opportunities, hundreds of thousands of Portuguese workers moved abroad after World War II, most going to Western Europe. By 1975, Maxwell (1997, 23) estimates that 1.5 million Portuguese (more than one-sixth the population) worked abroad, including 700,000 in France. However, emigration was highly uneven across Portugal, with exit higher in rural northern districts (Brettell 1979).

Comparing emigration rates and later protest activity within Portugal supports our argument. Using Portuguese census data, we calculate the cumulative emigration rates for each district from 1960-70, then compare this to political activity from 1970-83, a period encompassing the 1974 Carnation Revolution and ensuing democratic transition.<sup>7</sup> Our count of con-

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<sup>7</sup> Portuguese population data was taken from Statistics Portugal at [www.inec.pt](http://www.inec.pt).

tentious political events (combining protests and armed actions) is drawn from the SPEED dataset (SPEED Project 2012).

The difference between high- and low-emigration areas is stark: The 9 districts with an emigration rate above 11% of the population featured only 7 contentious events, compared to 106 events in the remaining 13 districts (Figure 1). In total, high-emigration districts represented 21% of the population in 1970, but only 6% of the ensuing political events.

The negative relationship between emigration and political activism is reflected in the political culture (Brettell 1979). The north's high emigration gave rural workers an easy exit that reduced the attraction of political opposition: "They were raised to find solutions to their economic problems not through political pressuring and collective action but through individual initiative and emigration" (Brettell 1979, 290). Even those too young to emigrate were socialized to prefer exit and "gave no thought to involving themselves in forms of political action" (Brettell 1979, 292). The high standard of living (from wages or remittances) and economic opportunities made emigrant areas more resistant to direct activism. The result was wide-scale political apathy and a view that the 1974-76 revolution was a Lisbon affair. As Brettell (1979, 292) concludes, "emigration on its own works in many ways to keep emigrants essentially non-political and relatively conservative."

Although emigration had a pacifying effect overall, the high proportion of emigrants going to Europe had a democratizing influence on the population. Returning emigrants from Europe "brought with them modern Western values," which "began to erode the ideological corporatist superstructure that dominated the minds of the rural populations" (Manuel 1998, 143). These pro-Western emigrants thus supported democratization and voted for moderate democratic parties in the key transitional elections of 1975 and 1976 (Bermeo 2007). However, this did not necessarily translate into contentious political activism. As Brettell (1979, 293) notes, Portuguese emigrants in France had unusually little contact with French unions or organized political opposition. The result was a gradual diffusion of pro-Western attitudes, but with a relatively passive mode of expression.

## **Spain**

The relationship between emigration and quiescence is even stronger in Spain despite a lower overall emigration rate. In Spain, the 22 provinces with the highest emigration rates from 1964-70 (above 2% of province population) featured only 8 contentious events combined from 1970-77, a period that saw a fraught transition to democracy. In contrast, the remaining 29 provinces had 164 combined events. The 22 high-emigration provinces represented 39% of the population in 1970, but fewer than 5% of contentious events (Figure 2).<sup>8</sup>

Just as in Portugal, Spanish emigrants were a constituency for democracy when they returned home. Those abroad were often organized into unions with contacts with Spanish opposition groups, which served as a political crash course (Foweraker 2003) and provided organizational tools for the emerging unions at home (Townson 2007). While emigration led to fewer protests in poorer rural districts, exposure to democratic Europe increased support for democracy and organized labor.

## **Morocco**

Morocco illustrates both how emigration can reduce contestation overall and how governments concerned about democracy-focused emigration attempt to reduce its effects. After independence, Moroccan emigration took off when the government signed bilateral treaties with West Germany, Belgium, the Netherlands, and France. In turn, increased emigration coincided with a decrease in the total number of contentious events (Figure 3).

Emigration served as a safety valve for ethnoregional tensions in Morocco. Post-independence, the government encouraged emigration from the eastern Rif mountain area, the southwestern Sous region, and the High Atlas mountains (De Haas 2007). Prior to colonization, these regions had largely been autonomous, only coming under French control in 1933, and became restive at independence (Moore 1970). Emigration was also encouraged from the northern Rif area, which had been under Spanish control and faced secessionist pressures. Each area was targeted through direct recruitment and control over passports and visas into Europe (De Haas 2007).

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<sup>8</sup> Spanish population data was taken from the National Institute of Statistics at [www.ine.es](http://www.ine.es).

Given that most emigration went to Europe, it represented a potential channel for the transfer of democratic norms. However, the Moroccan government anticipated this and attempted to mitigate the threat. First, government recruiters would often select poorly educated citizens for emigration in hopes that they would be less likely to engage politically or through union activity in the host country (De Haas 2007). Second, the Moroccan government maintained ties with emigrants abroad to help ensure that only monetary, and not social, remittances would return. The government placed spies in its embassies, state-sponsored mosques, and regime-controlled emigrant associations (Brand 2010; De Haas 2007). Through these organizations, migrants were discouraged from joining unions, organizing independent associations, or participating in local elections (De Haas 2007).

Nevertheless, they were unable to prevent all organization. Some emigrants coalesced as a left-wing opposition abroad, initially through activities with established European unions (De Haas 2007). Emigrants went on to establish the *Association des Marocaines de France* in 1962, which had links to the *Union National des Forces Populaires* (UNFP), the main opposition party (De Haas 2007). When migrants organized, their families back home often faced harassment, as did the emigrants if they returned. For example, Moroccan officials confiscated the passports of trade unionists who returned home on vacation after French authorities outed them (De Haas 2007). It was only in the early 1990s that King Hassen II and later King Mohammed VI implemented reforms reducing the repression of emigrants, largely to increase their economic engagement with Morocco through remittances and investment (De Haas 2007).

## **Data and Empirical Analysis**

### **Main Variables**

We now turn to testing the relationship between emigration and political contestation, using a global country-year sample from 1960 to 2010. All sending and receiving states are included when predicting dyadic emigration. For predicting contestation, the sample is then

limited to non-OECD sending states, as nearly all political violence takes place in the developing world.<sup>9</sup>

For dyadic migration data, we rely on the World Bank (Özden et al. 2011), which provides migrant stocks for every pair of countries every 10 years from 1960 to 2010. As the data is based on both sending and receiving countries' census figures, it picks up undocumented and refugee flows and accounts for under-reporting by single countries. However, it does not distinguish between permanent and temporary migration, economic and forced migration, or legal and illegal migration. To maximize our sample, we linearly interpolate the emigration stocks data, which is reasonable given that stocks are generally stable on the scale of a decade. However, as a check, we confirm our results after removing all interpolated data.

To measure democracy, we use the dichotomous coding from Boix, Miller and Rosato (2013), which has been updated to 2010. We test our models in a full sample of non-OECD sending countries, then restrict the sample in turn to autocracies and democracies. We also test whether emigration to democracies differs in its effects on sending countries. Our chosen outcome variables and controls are detailed below.

### **Identification Strategy**

We want to derive the causal effect of emigration on several forms of political contestation. However, we cannot rely on tests of observed emigration due to both omitted factors and reverse causation: migrants flee countries during civil conflict and states' strategically manipulate emigration. Although we show the results using actual emigration for comparison, we cannot credibly interpret them as causal.

To solve this endogeneity problem, we use an instrumental variables (IV) setup in which we predict emigration from exogenous geographic and socioeconomic characteristics. Specifically, we first run a directed-dyad model that predicts emigration as a population share between each pair of countries. We then aggregate to the country-year level to generate a predicted measure of total emigration, as well as the *net* emigration to democracies. These predicted

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<sup>9</sup> A robustness check including OECD countries confirms our results.

measures are used as our instruments for total emigration and net democratic emigration, respectively.

This technique was originally adopted by Frankel and Romer (1999) to predict bilateral trade and then applied to migration by Ortega and Peri (2014) and Preotu (2016). In each case, the authors estimate a dyadic stage, aggregate to total predicted trade or emigration, and then run an IV model. This paper is distinguished by considering a larger sample of destination countries, predicting different outcomes, and investigating the characteristics of migration partners rather than just total migration.

For the dyadic analysis, we begin by estimating a regression of the following form:

$$EmShare_{ijt} = \alpha_0 + \alpha_1 \ln(Population_{it}) + \alpha_2 \ln(Population_{jt}) + \alpha_3 \ln(Distance_{ijt}) + \mathbf{X}_{ijt}\beta + \mathbf{W}_{it}\delta + \mathbf{Z}_{jt}\lambda + \gamma_t + \varepsilon_{ijt} \quad (1)$$

where  $EmShare_{ijt}$  is the share of migrants (as a fraction of population) from country  $i$  to country  $j$  in year  $t$ .<sup>10</sup> The sample includes all sending and receiving states. Equation (1) is known as a *gravity model*, frequently used in the trade literature.  $\mathbf{X}$ ,  $\mathbf{W}$ , and  $\mathbf{Z}$  are control variables that refer in turn to dyadic characteristics, the source country  $i$ , and the receiver  $j$ .  $\gamma_t$  is a linear control for the year.

We include variables that reflect that most economic migrants (and even many refugees) choose their destinations based on relative wealth and travel costs (Massey et al. 1993) and that are exogenous to political contestation. For dyadic factors, we include the logged distance between capitals and dummies for six categories of contiguity (capturing shared borders and separation by water) to approximate the cost of migration;<sup>11</sup> an indicator of whether one country is a former colony of the other, a strong predictor for migrant networks; and a dummy for shared language, which makes migration easier (Melitz and Toubal 2014). We control for each country's logged population and logged average income. Finally, we include interactions between the receiving country's population and distance, population and shared borders, and

<sup>10</sup> An alternative setup is to use the total number of emigrants (logged) from  $i$  to  $j$  as the dependent variable. Results are similar with this strategy, but we find that using emigration share is a stronger instrument.

<sup>11</sup> The miles between capitals comes from Gleditsch and Ward (2001). Contiguity data is from Correlates of War Project (2007).

the same interactions for income, as large, wealthy neighbors are especially likely targets for emigration. Because population and income may directly predict our outcome variables, we control for both in our outcome equations. Results from the estimation of Equation (1) are shown in Appendix Table A1 (page 1).

Equation (1) allows us to calculate the yearly *predicted* population share of emigration from country  $i$  to  $j$  as follows:

$$\widehat{EmShare}_{ijt} = \hat{\alpha}_0 + \hat{\alpha}_1 \ln(\text{Population}_{it}) + \hat{\alpha}_2 \ln(\text{Population}_{jt}) + \hat{\alpha}_3 \ln(\text{Distance}_{ijt}) \\ + \mathbf{X}_{ijt} \hat{\beta} + \mathbf{W}_{it} \hat{\delta} + \mathbf{Z}_{jt} \hat{\lambda} + \hat{\gamma}_t$$

We then sum expected dyadic emigration across all receivers to create a total expected emigration share:

$$\widehat{EmShare}_{it} = \sum_j \widehat{EmShare}_{ijt}$$

We change all negative predicted values to 0, which applies to 7.9% of the sample. Our final measure is correlated with the actual emigration share at 0.40. We can think of  $\widehat{EmShare}$  as an estimate of how much emigration we should expect from a country independent of politics and current contestation.

We then calculate how democratic country  $i$ 's expected emigration targets are, as captured by the net emigration to democracies versus autocracies:

$$\widehat{EmShare}_{it}(\text{Net Democracy}) = \left( \sum_j \widehat{EmShare}_{ijt} \times Dem_{jt} \right) - \left( \sum_j \widehat{EmShare}_{ijt} \times (1 - Dem_{jt}) \right)$$

where  $Dem_{jt}$  is the binary democracy measure from Boix, Miller and Rosato (2013). This is correlated with the actual net democratic share at 0.49.

We use these two measures as instruments to predict the equivalent variables constructed from actual emigration: *Emigration Share* and *Net Democratic Emigration*.<sup>12</sup> This setup allows us to test emigration by leveraging its variation from geography and external countries' socioeconomic characteristics. Actual migration will vary from our instruments based on conflict, senders' exit policies, and receivers' immigration policies. However, the instruments pick

<sup>12</sup> The former varies from 0.0003 to 0.56, whereas the latter varies from -0.21 to 0.54.



up a great deal of exogenous variation, well exceeding common benchmarks for strong instruments (shown below).

Our identification strategy rests on the assumption that our predicted emigration measures are uncorrelated with the error term in our outcome equations (called the exclusion restriction). Since we control for domestic population and income in all outcome equations, the remaining variation in the instruments is driven by characteristics of *external* countries.<sup>13</sup> Thus, our design leverages exogenous changes in predicted emigration when other countries become wealthier or more populous over time. It also exploits exogenous changes in the democratic share of emigration when a likely emigration target democratizes. As a result, the exclusion restriction is highly likely to hold (Frankel and Romer 1999; Ortega and Peri 2014).

To bolster our causal claims, we conduct several further analyses. First, we include controls for other types of economic exchange that follow a gravity-type relationship, such as trade, aid, and foreign direct investment (FDI).<sup>14</sup> If our predicted emigration measures were simply tracking general economic or political engagement, controlling for these other measures should change our results. We show that our findings are robust (see Table 3).

Second, one might still worry that some of the variables in Equation (1) directly predict the outcomes. In response, we test the robustness of our results to individually removing variables from Equation (1). If a variable violated the exclusion restriction, the test without this variable should greatly change in magnitude. Results are in fact highly consistent (see Appendix Figures A1-A2, pages 2-3).

Third, one might be concerned that our instruments happen to correlate with more stable or less violent countries, accounting for our results. We examine placebo tests predicting irregular turnover, coups, and state purges using our IV design. Our theory predicts that emigration reduces mass contestation, but not necessarily intra-elite violence. Indeed, we find no relationship (see Appendix Table A2, page 4). Thus, predicted emigration is specifically related to forms of political instability requiring mass participation.

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<sup>13</sup> Colonial relations and shared language are partial exceptions (since they are dyadic relationships), but our results hold after dropping both variables from Equation (1).

<sup>14</sup> We include some of these variables only in checks as they are potentially post-treatment.

## Dependent Variables

Our outcome variables capture a range of political contestation, both violent and non-violent. We begin with measures of civil war and conflict. *Civil War* is taken from the Correlates of War Project (Sarkees and Wayman 2010), which defines this as an internal conflict with at least 1,000 deaths in a single year. *Civil Conflict*, from the UCDP database, has a lower bar of 25 deaths (Sundberg, Eck and Kreutz 2012). We predict both binary variables using an IV-probit model. We also predict a multi-valued *Conflict Intensity* measure from UCDP (Sundberg, Eck and Kreutz 2012). This is coded as 0 for no conflict, 1 for conflicts with 25–1,000 deaths in a year, 2 for conflicts with no more than 1,000 deaths, and 3 for more than 10,000 deaths. For all non-binary outcomes, we predict this using two-stage least squares (2SLS).<sup>15</sup> For each of these variables, we remove cases corresponding solely to external involvement in others' internal conflicts.

We next turn to measures of violent opposition movements. The NAVCO dataset codes for the existence and size of violent and non-violent opposition campaigns (Chenoweth and Lewis 2013). To measure *NAVCO Violent Movement Size*, we consider the largest such movement in the country and distinguish the ordinal categories of no movement, a movement with fewer than 10,000 members, between 10,000 and 100,000, between 100,000 and 1 million, and above 1 million.<sup>16</sup> Further, we consider the incidence of guerilla movements, revolutions, and riots from the Banks (1976) and Norris (2008) data. We use IV-probit to predict whether any such incident occurs, which we call *Violent Events (Any)*. In a separate test, we use 2SLS to predict *Violent Events (Number)*, the total number of violent incidents.

To capture non-violent movements, we first test *NAVCO Non-Violent Movement Size*, measured and tested in parallel with violent movements. We then consider the incidence of peaceful protests and strikes from the Banks (1976) and Norris (2008) data, again predicting any such event and the absolute number. This enables us to compare similar measures of non-violent and violent contestation.<sup>17</sup>

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<sup>15</sup> We follow common practice in applying 2SLS to both ordinal and count data.

<sup>16</sup> Results are similar using a simple binary variable for the presence of a violent campaign.

<sup>17</sup> Moving beyond event data, we also predict V-Dem's expert-coded measure of anti-regime activity and find similar results (Coppedge et al. 2016). See Appendix Table A6, page 8.

## Control Variables

Although we stress our identification strategy for credible causal inference, we also control for several factors that may predict political contestation and emigration. In particular, any variables in the predicted emigration calculation that could directly predict conflict need to be controlled for in the outcome equation. Thus, we control for *GDP/capita* (logged, in real 2000 dollars, from Haber and Menaldo 2011; World Bank 2015) and *Population* (in millions, from Heston et al. 2011).

We control for *Trade* (imports and exports as % of GDP, from Heston et al. 2011) to account for international economic engagement. We want to ensure it is emigration specifically and not general economic ties that produce our results. However, since emigration predicts trade, controlling for it risks post-treatment bias. We therefore check our results after removing *Trade*, as well as controlling for FDI and aid. As important economic variables, we control for natural resource wealth (fuel revenues per capita, in thousands of 2009 dollars, from Ross 2013) and *Economic Growth* (the average percentage change in *GDP/capita* over the past two years). As important conflict predictors, we control for *Land Area* (in millions of square kilometers, from World Bank 2015) and *Ethnolinguistic Fractionalization* (from Roeder 2001).<sup>18</sup>

To account for diffusion, we control for the regional average of the dependent variable (excluding the country itself) and region fixed effects. We also include the *Polity* democracy score as both a linear and quadratic term to test the claim that countries at middle values of *Polity* are especially unstable (Marshall and Jaggers 2014). In a check, we add a control for civil liberties protections. We also control for the average *Polity* level in the surrounding region. Lastly, since both emigration and conflict vary over time, we include year fixed effects in all models.

## Empirical Results

### Emigration Share

We first discuss the effect of total emigration share on our dependent variables, with the effect of democracy-focused emigration discussed below. We begin in Table 1 by relating *Em-*

<sup>18</sup> Results are similar controlling for mountainous terrain (from Fearon and Laitin 2003).

*emigration Share* to *Civil War* using our IV framework, with predicted emigration as the instrument. The first set of models rely on a simple specification, only controlling for average income, region fixed effects, and year fixed effects. The next set of models add the remaining controls. For each set of controls, we test the effect in the full non-OECD sample, then restricted to autocracies and democracies. For comparison, we also show the non-IV results (using probit) in the full non-OECD sample.

The results for total emigration are clear. *Emigration Share* has a strong and significantly negative effect on *Civil War* in each model, with the strongest effect in autocracies. The magnitude of the coefficient does not change much across models, except that it increases from the probit to IV results, consistent with reverse causation from civil war to actual emigration. Tests from the first stage of IV-probit show that the instrument is a very strong predictor of *Emigration Share*. The Kleibergen-Paap F statistic (a weak-instrument test) is above 100 in each model, well over the rule of thumb of 10 for strong instruments.

Figure 4 shows the magnitude of the effect in autocracies (top panel) and democracies (bottom panel), derived from Models 7 and 8 (with other variables held at their means). The estimated effect is substantively large: Moving from no emigration to 5% of the population emigrating (about a standard deviation) shifts the likelihood of civil war from 35.8% to 10.8% in autocracies, and from 28.3% to 13.7% in democracies. For autocracies, this is roughly equivalent to the effect of shifting a country's average income from that of modern Madagascar to modern Singapore.

These results sharply contrast with arguments that diasporas increase the likelihood of conflict, such as Collier and Hoeffler (2004) and Miller and Ritter (2014). We robustly find the opposite, suggesting that previous findings may stem from reverse causation or spurious correlations. Among the controls, average income and fuel revenues are negatively related to civil war in autocracies. Population size has opposite effects in the two regime types.<sup>19</sup>

Table 2 shows the effects of emigration share on eight other dependent variables, which are listed in the left column and divided between violent and non-violent events. The first

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<sup>19</sup> Middle ranges of *Polity* correlate with civil war, which is partly explained by *Polity*'s use of 0 (a middle value) to code for state collapse.

column shows non-IV results for comparison.<sup>20</sup> The remaining three columns show IV results for the full sample, then restricted to autocracies and democracies. Each displayed coefficient represents a distinct model. The full control set is included, with the only variation being the regional average of the dependent variable. As a result, the first-stage results of each IV model are virtually identical to those shown in Table 1.

The results are highly consistent across event types. For the non-IV models, *Emigration Share* is significantly negative in all but one model. For the IV results, *Emigration Share* is significantly negative (at the .001 level) for each outcome in the full sample and the autocratic sample. For the democratic sample, all results are negative, but one misses significance. The effect of emigration is stronger in autocracies for all nine total outcome variables, especially for movement sizes. Results are very similar in magnitude for the comparable violent and non-violent outcomes, indicating that emigration operates similarly in disrupting different types of opposition movements. However, results are slightly stronger for higher-intensity civil wars compared to *Civil Conflict*, likely because they require larger numbers of recruits.

Figure 5 shows the estimated probability of at least one violent and non-violent event in autocracies (top panel) and democracies (bottom panel). These are derived from the IV-probit models summarized in Table 2. The effect is substantively large, with a 5% increase in *Emigration Share* nearly halving the chance of each event type in autocracies. The effect is slightly weaker in democracies, where the equivalent effect reduces events by about 20%.

### **Robustness Checks for Emigration Share**

Table 3 includes several robustness checks for three of our dependent variables: *Civil War*, *NAVCO Violent Movement Size*, and *NAVCO Non-Violent Movement Size*.<sup>21</sup> Each check adjusts the corresponding models in Table 2 and again each coefficient represents a distinct model. The first nine checks retain the IV setup, whereas the tenth tests the actual emigration share.

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<sup>20</sup> We use a probit for binary outcomes, ordered probit for ordered outcomes, and negative binomial for the count variables.

<sup>21</sup> Results for the remaining dependent variables are similar. We also examine a measure of anti-regime activity from Coppedge et al. (2016) and find similar results (see Appendix Table A6, page 8).

The first five checks adjust the control variables. Because trade is predicted by emigration and similar dyadic relationships as emigration, controlling for it may generate post-treatment bias. The first check therefore removes *Trade* from the controls. We also consider the possibility we are not controlling *sufficiently* for international economic engagement. In the following checks, we add controls for FDI (net inflow as % of GDP, from World Bank 2015), foreign aid (official development assistance as % of GDP, from World Bank 2015), and both together.<sup>22</sup> In the fifth check, we include a 0-1 *Liberal Freedom Index* to further account for the political environment (Coppedge et al. 2016).<sup>23</sup>

Since the timing of emigration's effects is unclear, we next lag the emigration variables by 10 or 20 years. Both predicted emigration in the first stage and actual emigration (the endogenous variable) are lagged. The eighth check includes OECD countries in the sample, expanding the full sample by 22%. The ninth check only includes non-imputed emigration data for the endogenous variable, rather than the linearly imputed values used elsewhere. This limits the sample to every 10 years from 1960-2010.

Our IV results are highly robust to these checks. In total, 75 of 81 checks remain significant (at the .05 level). All results remain negative and effect magnitudes are similar across checks. Results are especially consistent as the controls are adjusted, making it unlikely that our results are proxying for general international connectedness. The stronger effect of emigration in autocracies also holds across 26 of 27 checks.

In the final check in Table 3, we test the actual emigration share in a non-IV model. This repeats findings shown earlier, but now divides by regime type. We find a negative, but attenuated and inconsistently significant effect on each outcome. Again, this weaker negative relationship is consistent with reverse causation from conflict to emigration, balancing against the negative relationship from emigration to conflict.

Lastly, to test the sensitivity of our IV technique, we consider nine alternatives to the emigration prediction equation, each removing specific variables from Equation (1) (Appendix

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<sup>22</sup> Although these factors may also generate post-treatment bias given that emigration predicts FDI and aid, our approach is to consider the full range of results under different assumptions and confirm their consistency.

<sup>23</sup> Appendix Table A3 (page 5) further shows the results hold controlling for autocratic regime types and stratifying by regime type, personalism, and the presence of elections.

Figure A1, page 2). Each alternative is then used as a new instrument. Results are highly robust, with 26 of 27 alternatives retaining significance at the .05 level (and 24 of 27 at the .001 level). With one exception, the results are so consistent that no alternative coefficient varies more than 18% in magnitude from the original. This casts serious doubt on the possibility that one of these predictor variables violates the exclusion restriction, as its removal should then meaningfully change the result.

### **Emigration to Democracies**

We now consider whether the *destinations* of emigrants matters for political contestation. Table 4 displays the IV models testing *Net Democratic Emigration* on our outcomes. These are identical to the *Emigration Share* models in Table 2, with only the instrument and endogenous variable changed.<sup>24</sup>

For the violent outcomes, results are inconsistent and mostly insignificant. Within autocracies, for instance, *Net Democratic Emigration* has a small negative effect on *Civil War* and *Civil Conflict*, the opposite effect on total violent events, and no discernible effect on the remaining three outcomes.

Results are starkly different for the non-violent outcomes. In each case, *Net Democratic Emigration* is significantly positive in the autocracy sample. In contrast, the effects are negative or null in the democracy sample. Further, the substantive effect in autocracies is quite large. If we shifted emigration from a net 5% of the population going to autocracies to a net 5% going to democracies, the likelihood of at least one protest or strike more than triples (from 10.1% to 33.8%).<sup>25</sup>

Thus, unlike for *Emigration Share*, we find a highly specific effect: democracy-focused emigration predicts a particular style of contentious opposition in a particular regime type. This supports the idea that exposure to democracies generates grievances in emigrants and autocratic sending communities, triggering opposition (Miller and Ritter 2014). That this opposition only takes non-violent forms further supports our theory on the diffusion of democratic norms regarding legitimate political contestation. As with emigration share, we test the sen-

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<sup>24</sup> The instrument remains a strong first-stage predictor, with the Kleibergen-Paap F test ranging from 67.8 to 385.3 for the *Civil War* tests.

<sup>25</sup> The results are robust to the variations shown in Table 3.

sitivity to nine IV alternatives (Appendix Figure A2, page 3). Results retain significance at the .05 level in all models (and 25 of 27 at the .001 level).

### Extensions: Testing the Mechanisms

Finally, we examine several tests aimed at validating our mechanisms. First, we consider whether emigration’s effects are moderated by economic crises. During a crisis, grievances toward the incumbent regime raise the likelihood of civil conflict and protest (Barry et al. 2014; Humphreys and Weinstein 2008). This is precisely when the safety valve and economic mechanisms should most strongly reduce opposition, as frustrated dissidents can leave and the crisis is softened through increased remittances. We find support for this, with emigration’s negative effect heightened during negative growth or inflation crises (see Appendix Table A4, page 6).

Second, we further examine how *Net Democratic Emigration* affects the character of anti-regime groups. We test a *Pro-Democratic Opposition* variable coded by averaging measures of whether the opposition follows democratic rules, works through legal channels, and participates in elections, from Coppedge et al. (2016). Validating our democratic diffusion argument, we find that greater migration to democracies produces more pro-democratic oppositions in autocracies but has no effect in democracies (Appendix Table A6, page 8).

Third, we consider two other receiving state characteristics: wealth and freedom of association. Specifically, we test expected mean level of *GDP/capita (ln)* and *Freedom of Association* (a three-valued measure from Cingranelli and Richards 2014) of receiving states, again using an IV setup (see Appendix Table A5, page 7).<sup>26</sup> Although more mixed than our other results, the findings follow our expectations. Wealthier receivers should generate greater economic benefits, reducing contestation. Indeed, seven of nine coefficients on average *GDP/capita (ln)* are negative, with four significant. Countries with greater freedom of association should give migrants more opportunities to learn organizational skills and start opposition movements

<sup>26</sup> We compute the exogenous expected mean of variable  $X$  in receivers as follows:

$$\widehat{EmShare}_{it}(X) = \sum_j \widehat{EmShare}_{ijt} X_{jt} / \widehat{EmShare}_{it}$$



abroad, shifting tactics to peaceful contestation. Indeed, average *Freedom of Association* is consistently negative for violent contestation and positive for non-violent contestation, with five of nine coefficients significant.<sup>27</sup> Along with our democracy results, the findings point to nuanced effects of receiver characteristics.

## Conclusion

This paper returned to Hirschman's lasting, but understudied, idea that exit can "atrophy the development of the art of voice" (Hirschman 1970, 43). Using an instrumental variables design, we found that increased emigration indeed reduces both violent and non-violent contestation, revealing how the rising international flow of people can shape global patterns of conflict.

Civil violence and protest both require groups of people who are unhappy with the political regime and dedicated to changing it. Emigration provides disaffected citizens an opportunity to move abroad instead, leaving behind a more supportive population. In addition, emigration raises wages and lowers unemployment by shrinking the labor force and increasing remittances, trade, FDI, and aid. This further reduces grievances and increases the opportunity costs for joining opposition movements and insurgencies.

Yet we argue the Hirschman framework provides an incomplete picture of exit's effects on voice. Not all emigration is equal: emigrants to democracies transmit democratic norms back home, while return migrants bring the tools of civil society organization and protest. Together, we found this increases the propensity for peaceful contestation in autocracies. This is particularly significant given research showing that non-violence is most effective at achieving regime change and democratic stability (Chenoweth and Lewis 2013).

This paper makes both scholarly and policy contributions. To scholars, it counters work arguing that diasporas inflame conflict (Collier and Hoeffler 2004; Miller and Ritter 2014). Future research can investigate how emigration opportunities deter individuals from joining violent movements, explore factors that moderate emigration's effect (extending our findings

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<sup>27</sup> The same pattern holds if limited to autocracies, with six of nine coefficients significant.

on emigrant destinations and economic crises), and predict which diasporas mobilize during ongoing conflicts (e.g., Adamson 2013).

Our research also highlights the role of emigrants in non-violent contestation, prompting several questions regarding how migrants' normative ideas, experiences, and connections to groups abroad influence democratic movements. How often do emigrants lead these movements? Do some democratic destinations contribute more to non-violence through greater openness or their treatment of immigrants? Further, do emigrants typically replicate the style of democracy they experience abroad?

Finally, to policymakers in wealthy democracies, our results highlight a neglected benefit of opening their doors to immigration. Emigration to democracies can reduce global conflict, while also shifting the contestation that does result in a more peaceful direction.

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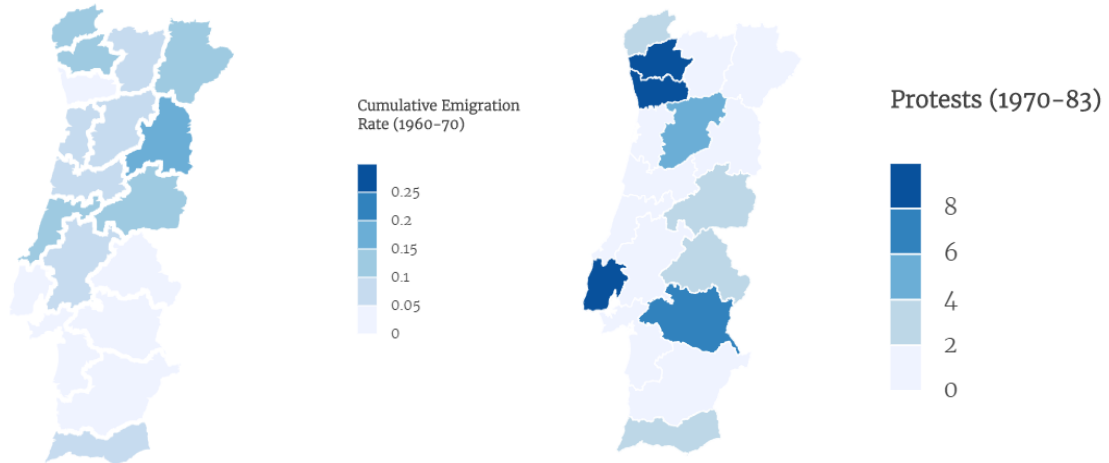
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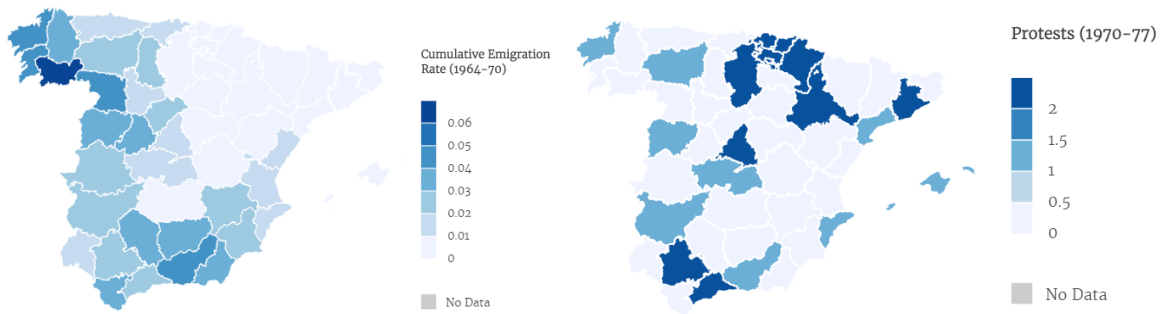
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## Portugal



**Fig. 1:** The figures show the cumulative emigration rate and number of protests by district in Portugal.

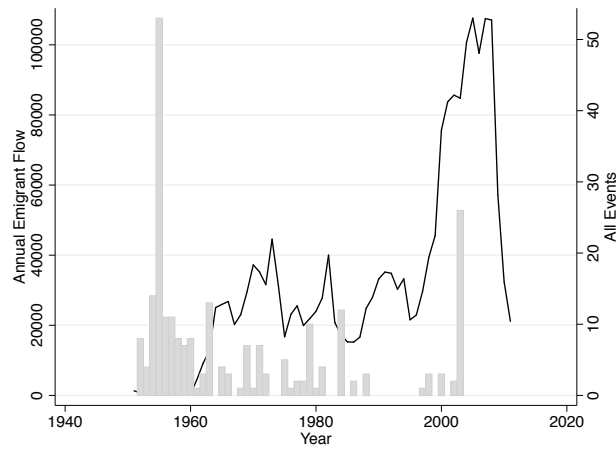
## Spain



**Fig. 2:** The figures show the cumulative emigration rate and number of protests by province in Spain.



## Morocco



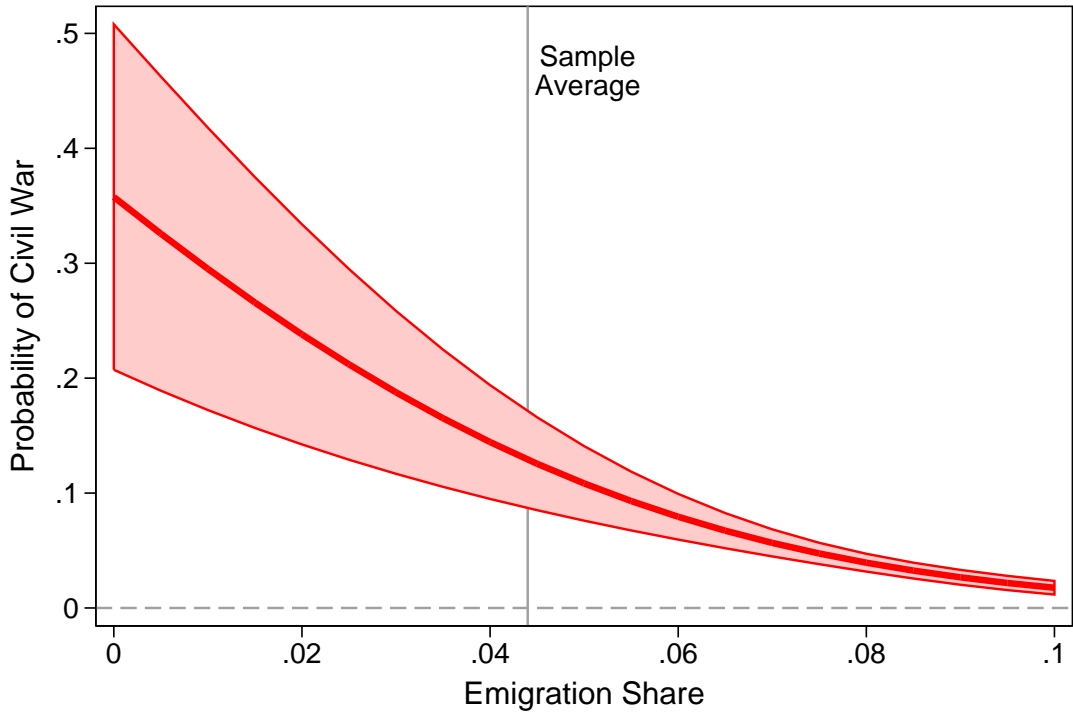
**Fig. 3:** The figures show the yearly emigration flow and number of contentious events. Data on contentious events is from SPEED Project (2012) and is the total number of events in the country in the year. Emigration data is from DEMIG (2015) and was calculated from the immigration flow from Morocco for the following reporting states: Austria, Belgium, Canada, Chile, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Israel, Mexico, Netherlands, New Zealand, Portugal, Slovakia, South Africa, Spain, Switzerland, U.S., and Uruguay.

**Table 1: Instrumental Variables Models Predicting Civil War**

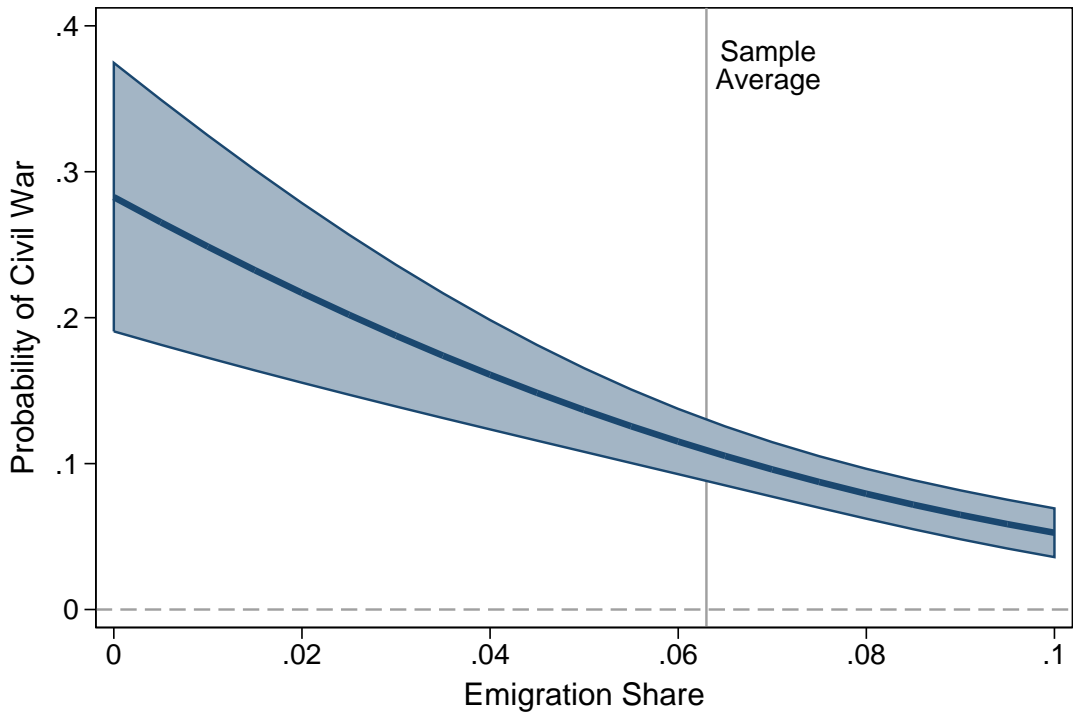
	Probit		IV		Probit		IV	
	All	All	Auth.	Dem.	All	All	Auth.	Dem.
DV = <i>Civil War</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Second Stage</b>								
<i>Emigration Share</i>	-7.065*** (-8.20)	-16.302*** (-16.08)	-18.463*** (-13.35)	-12.051*** (-8.33)	-4.228*** (-4.87)	-13.530*** (-7.41)	-19.835*** (-7.56)	-13.722*** (-6.97)
<i>GDP/capita (ln)</i>	-0.346*** (-10.85)	-0.355*** (-11.39)	-0.363*** (-10.47)	-0.436*** (-4.88)	-0.172*** (-3.66)	-0.231*** (-5.27)	-0.295*** (-6.03)	0.316** (2.95)
<i>Regional Civil Wars</i>					-2.787*** (-5.81)	-2.721*** (-6.19)	-2.619*** (-4.93)	-4.449*** (-4.15)
<i>Trade</i>					-0.441*** (-3.30)	-0.196 (-1.51)	0.033 (0.26)	-0.959* (-2.19)
<i>Fuel Revenues Per Capita</i>					-0.059** (-3.04)	-0.042** (-2.76)	-0.046** (-2.78)	-0.162 (-1.29)
<i>Economic Growth</i>					-0.029*** (-3.93)	-0.018* (-2.51)	-0.017* (-2.11)	-0.016 (-1.09)
<i>Population</i>					0.001* (2.22)	0.001** (3.25)	-0.004** (-2.74)	0.001*** (3.53)
<i>Land Area</i>					0.059* (2.19)	-0.024 (-0.70)	0.061 (1.76)	-0.295*** (-7.07)
<i>Ethnolinguistic Fractionalization</i>					0.269* (2.01)	0.019 (0.14)	-0.275 (-1.79)	1.031** (2.86)
<i>Regional Polity</i>					-0.029 (-0.07)	1.232** (3.05)	1.725** (2.99)	-0.314 (-0.35)
<i>Polity</i>					-0.003 (-0.53)	-0.005 (-0.86)	0.008 (0.77)	0.062 (1.62)
<i>Polity</i> <sup>2</sup>					-0.010*** (-8.87)	-0.010*** (-8.37)	-0.006*** (-3.44)	-0.029*** (-5.65)
Region Fixed Effects?	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects?	Y	Y	Y	Y	Y	Y	Y	Y
<b>First Stage</b>								
$\widehat{Em}(\text{Population Share})$		0.708*** (19.84)	0.568*** (18.57)	1.458*** (9.46)		0.585*** (13.70)	0.446*** (9.83)	1.363*** (7.88)
Other Controls?	Y	Y	Y	Y	Y	Y	Y	Y
N	5,130	5,130	3,656	1,465	4,501	4,501	3,119	1,380
Countries	161	161	124	97	140	140	113	84
BIC	3257.60	-13295.32	-10315.97	-2141.57	2730.67	-11821.44	-8834.28	-2191.74
Weak ID Test (Kleib.-Paap F)		454.13	340.68	178.53		213.72	155.29	102.20

*Notes:* The table shows probit (Models 1 and 5) and IV-probit models testing the effect of emigration on civil war. Models 1-2 and 5-6 include all non-OECD countries from 1960-2007. Models 3 and 7 restrict the sample to autocracies, while Models 4 and 8 include democracies. The IV first stage is shown at the bottom. The models show a strong negative effect of emigration on civil war. *t* statistics (based on robust standard errors) are in parentheses. \**p* < 0.05, \*\**p* < 0.01, \*\*\**p* < 0.001

### Emigration and Civil War in Autocracies



### Emigration and Civil War in Democracies



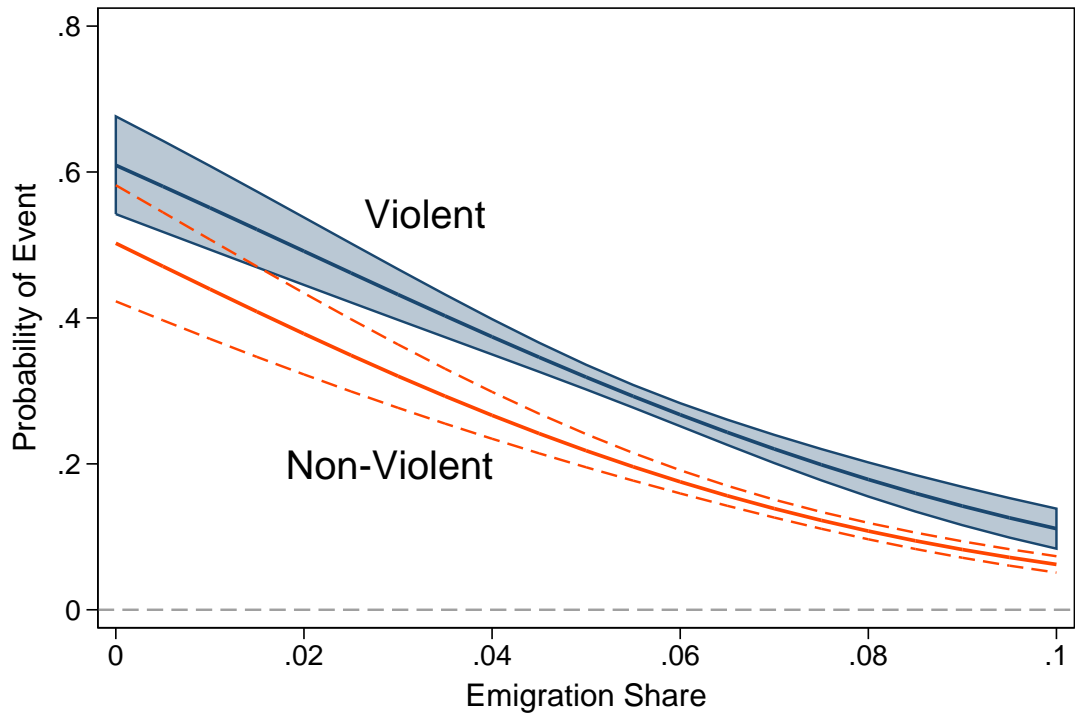
**Fig. 4:** The figures show the estimated likelihood of civil war given different values of predicted emigration share, derived from Models 7 and 8 in Table 1. The top and bottom panels show the predicted probabilities restricted to autocracies and democracies, respectively.

**Table 2: Effect of Emigration Share on Contestation**

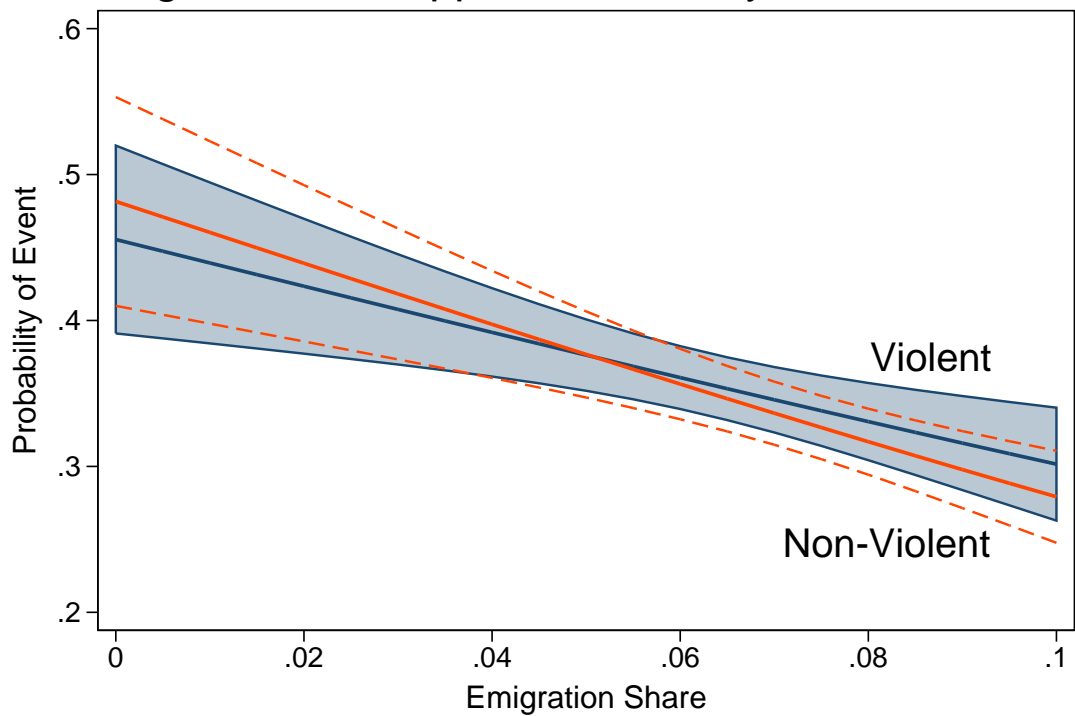
Dependent Variables	Non-IV	IV		
	All	All	Auth.	Dem.
<b>Violent Outcomes</b>				
<i>Civil Conflict</i>	-4.885*** (-6.36)	-6.938*** (-3.82)	-17.451*** (-6.46)	-4.354* (-2.06)
<i>Conflict Intensity</i>	-4.635*** (-6.15)	-4.323*** (-4.67)	-10.646*** (-6.11)	-1.982*** (-3.63)
<i>NAVCO Violent Movement Size</i>	-6.264*** (-7.40)	-5.266*** (-6.72)	-8.006*** (-6.18)	-1.319** (-3.08)
<i>Violent Events (Any)</i>	-1.868*** (-3.66)	-10.465*** (-7.47)	-16.698*** (-9.47)	-5.638*** (-3.44)
<i>Violent Events (Number)</i>	-2.177* (-2.47)	-11.072*** (-5.54)	-22.533*** (-6.40)	-1.358 (-0.98)
<b>Non-Violent Outcomes</b>				
<i>NAVCO Non-Violent Movement Size</i>	-2.371 (-1.65)	-4.302*** (-5.99)	-6.702*** (-5.33)	-1.507*** (-3.43)
<i>Non-Violent Events (Any)</i>	-1.526** (-2.96)	-12.428*** (-9.27)	-18.553*** (-11.65)	-6.964** (-4.34)
<i>Non-Violent Events (Number)</i>	-3.052** (-3.00)	-16.888*** (-7.10)	-26.403*** (-6.72)	-4.348*** (-3.33)

*Notes:* The table shows estimated effects of emigration share on several outcomes (listed at left). Samples of non-OECD countries vary by column. Each coefficient in the Non-IV column is from a separate probit, ordered probit, or negative binomial model. In the remaining columns, each coefficient represents a separate instrumental variables model (IV-probit for binary outcomes, 2SLS for others). Emigration's negative effect holds for both violent and non-violent contestation, with the effect strongest in autocracies. *t* statistics (based on robust standard errors) are in parentheses. \* $p < 0.05$ , \*\* $p < 0.01$ , \*\*\* $p < 0.001$

## Emigration and Opposition Activity in Autocracies



## Emigration and Opposition Activity in Democracies



**Fig. 5:** The figures show the estimated probability of at least one non-violent or violent event in autocracies (top panel) and democracies (bottom panel), derived from the corresponding instrumental variable models in Table 2.

**Table 3: Robustness Checks for Emigration Share**

DV =	Civil War			NAVCO Violent			NAVCO Non-Violent		
	All	Auth.	Dem.	All	Auth.	Dem.	All	Auth.	Dem.
Trade Removed	-14.792*** (-9.23)	-20.122*** (-8.44)	-16.169*** (-9.54)	-6.244*** (-8.25)	-9.395*** (-7.76)	-2.594*** (-5.74)	-4.329*** (-6.34)	-6.766*** (-5.76)	-1.328*** (-3.69)
FDI Added	-13.687*** (-7.48)	-20.620*** (-7.11)	-13.724*** (-5.83)	-5.308*** (-5.94)	-5.441*** (-3.69)	-1.983*** (-3.56)	-4.426*** (-5.16)	-3.220* (-2.50)	-1.798** (-3.17)
Aid Added	-15.602*** (-8.65)	-24.060*** (-11.10)	-13.816*** (-6.42)	-5.192*** (-6.59)	-9.070*** (-6.11)	-0.812* (-2.16)	-3.876*** (-5.67)	-6.264*** (-4.82)	-0.921** (-2.77)
FDI/Aid Added	-15.259*** (-8.38)	-24.675*** (-10.89)	-13.788*** (-6.07)	-5.451*** (-5.86)	-6.931*** (-3.94)	-1.418** (-3.06)	-3.872*** (-4.82)	-2.898* (-2.09)	-1.105** (-2.60)
Freedom Added	-12.464*** (-6.11)	-20.843*** (-7.62)	-12.794*** (-6.02)	-5.019*** (-5.97)	-8.848*** (-5.31)	-0.979* (-2.46)	-4.598*** (-5.75)	-8.335*** (-5.04)	-1.276*** (-3.38)
Lagged 10 Years	-20.915*** (-7.34)	-26.192*** (-7.69)	-23.659*** (-5.48)	-8.677*** (-6.06)	-12.009*** (-5.06)	-2.542*** (-3.73)	-7.087*** (-5.27)	-11.602*** (-4.59)	-1.350* (-2.33)
Lagged 20 Years	-16.047** (-3.27)	-9.997 (-1.08)	-66.845*** (-7.41)	-7.191*** (-4.09)	-7.771** (-2.91)	-1.015 (-1.11)	-10.145*** (-5.01)	-15.232*** (-4.16)	-1.105 (-1.23)
OECD Included	-15.358*** (-4.85)	-19.892*** (-7.57)	-16.851*** (-10.49)	-6.740*** (-4.49)	-7.897*** (-5.80)	-1.902 (-1.43)	-7.702*** (-4.87)	-7.601*** (-5.46)	-3.085* (-2.43)
No Imputed Values	-13.870*** (-3.35)	-19.849*** (-3.94)	-18.678 (-1.86)	-5.377* (-2.50)	-8.637* (-2.35)	-0.856 (-0.95)	-4.277** (-2.66)	-4.941* (-2.20)	-2.335* (-2.06)
Actual Emigration	-4.228*** (-4.87)	-6.403*** (-5.71)	-0.528 (-0.46)	-6.264*** (-7.40)	-7.557*** (-7.60)	0.601 (0.36)	-2.371 (-1.65)	-2.066 (-1.19)	-8.184*** (-3.51)

*Notes:* The table shows robustness checks for the effect of emigration on three dependent variables. Each coefficient represents a distinct model. The checks remove trade as a control; add FDI, foreign aid, both FDI and aid, and a liberal freedom index as controls; lag the instrument (and endogenous variable) by 10 and 20 years; add OECD countries to the sample; remove all imputed values of emigration; and use actual emigration rather than an IV model. The negative effect of emigration on contestation is highly robust, with 75 of 81 IV models remaining negatively significant and all remaining negative. *t* statistics (based on robust standard errors) are in parentheses. \* $p < 0.05$ , \*\* $p < 0.01$ , \*\*\* $p < 0.001$

**Table 4: Effect of Democracy-Focused Emigration (IV Models)**

<b>Dependent Variables</b>	<b>All</b>	<b>Auth.</b>	<b>Dem.</b>
<b>Violent Outcomes</b>			
<i>Civil War</i>	-1.711 (-0.92)	-4.503* (-1.98)	15.513*** (5.73)
<i>Civil Conflict</i>	-4.838*** (-3.59)	-5.009** (-2.79)	-12.800*** (-5.43)
<i>Conflict Intensity</i>	-0.772 (-1.42)	-1.399 (-1.63)	-1.640 (-1.38)
<i>NAVCO Violent Movement Size</i>	0.311 (0.48)	0.012 (-0.42)	-0.811 (-0.30)
<i>Violent Events (Any)</i>	0.726 (0.54)	2.367 (1.31)	-1.348 (-0.32)
<i>Violent Events (Number)</i>	3.639** (2.67)	5.695** (3.11)	5.291 (1.29)
<b>Non-Violent Outcomes</b>			
<i>NAVCO Non-Violent Movement Size</i>	0.915* (2.48)	2.644*** (4.07)	-1.244* (-2.11)
<i>Non-Violent Events (Any)</i>	3.738** (2.81)	8.746*** (4.80)	-0.460 (0.25)
<i>Non-Violent Events (Number)</i>	4.902*** (3.59)	11.222*** (5.53)	-1.731 (-0.51)

*Notes:* The table shows the estimated effects from instrumental variables models of net emigration to democracies on several dependent variables listed at left. The samples vary by column, with all restricted to non-OECD sending countries. Each coefficient represents a separate IV model. Democracy-focused emigration is positive for non-violent contestation, but only in autocracies. *t* statistics (based on robust standard errors) are in parentheses. \* $p < 0.05$ , \*\* $p < 0.01$ , \*\*\* $p < 0.001$