Emigration and Political Contestation

Margaret E. Peters · Michael K. Miller

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 can 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

Late on November 9, 1989, border guards manning the Berlin Wall looked up with shock to find thousands of East Germans suddenly demanding to be let through to West Berlin. Earlier that night, the regime's sleep-deprived press agent incorrectly reported that East Germans were now free to leave, effective immediately. The panicking checkpoint commander facing the largest crowd called his superiors and asked what to do. His instructions were simple: "seek out the 'more aggressive' people at the checkpoint, note down their names, and let them through with a special stamp" on their passport that would ensure they couldn't return (Sebestyen 2009, 354). Once these instigators were gone, they reasoned, the crowds would be easier to control. Although this strategy failed the East Germans, was their logic sound? Is there a link between emigration and political contestation at home?

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. Due to the field's focus on immigration in wealthy democracies, our results represent a rare finding on migration's impact on politics in *sending* countries.

This finding cuts in both directions. Higher emigration reduces contestation, while sudden restrictions in exit opportunities can inflame domestic protest and conflict. In 2011, the Arab Spring produced political contestation unmatched in the region for decades despite conditions on the ground—including high youth unemployment and low economic growth—that were little changed from the past. Yet one factor that had changed was the opportunity to emigrate, as the 2008 financial crisis shriveled emigration flows to Europe by 80% for Morocco, 95% for Tunisia, and 88% for Algeria from 2008 to 2011 (DEMIG 2015).

Higher rates of emigration reduce contestation through two mechanisms: the safety valve effect and economic benefits. Emigration attracts those unhappy with the state of their country, selecting out political opponents and citizens seeking better economic conditions, especially unemployed young men (Hirschman 1970; N.d.). This leaves fewer potential recruits for

opposition movements and insurgencies, skims off their most effective leaders, 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 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 (Miller and Peters 2020). However, we argue that it also matters *where* emigrants go. 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." Thus, greater migration from autocracies to democracies mitigates the general pacifying effect of emigration and can even *increase* non-violent contestation.

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 devel-

opment, trade, foreign aid, and regional diffusion. To bolster our causal claims, we show that emigration does not predict coups or other elite-led violence. To validate our mechanisms, we show that emigration's effect is stronger during economic crises and when emigrants travel to wealthier countries. In comparison, more emigration to democracies predicts more peaceful contestation and more pro-democratic opposition movements, but only in autocracies. Supplementing this quantitative evidence, we present illustrative case studies of Morocco and Taiwan.

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 a 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 peaceful contestation in autocracies. Additionally, this paper highlights how emigration to democracies can mutually help populations in the developing world and 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; N.d.). 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; N.d.; 1993), who presents 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).

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 sometimes fund conflicts through remittances to armed groupsCollier 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 (2013) argue 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, including the possibility of reverse causation from conflict to emigration.

Although some diasporas have meaningfully contributed to keeping civil conflict alive back home, this appears to be the exception, not the rule. As Adamson (2013) argues, many diasporas have not supported conflict, let alone been pivotal for it, thus raising the question of when and how diasporas are mobilized. Further, the relevant counterfactual for our study is not whether diaspora support for conflict exists, but what effect this support would have if these political opponents were still in their home countries. We argue that financial support is unlikely to outweigh the damage to organization and recruitment from having many of the most dedicated opponents living abroad.

A few other recent papers examine how emigration relates to civil violence and non-violent contestation.[1] In a working paper, Preotu (2016) relates emigration to civil conflict using a

[1] For instance, Okamoto and Wilkes (2008) analyze how opportunities for exit and voice predict ethnic rebellion.

similar identification strategy as the current paper. She finds that greater predicted emigration to 20 OECD states is negative for small-scale civil conflict but is not robustly related to civil war. However, these OECD states account for less than half of emigration and are uniformly democratic. We present a broader analysis by including all emigration destinations, distinguishing among the emigration targets, and predicting both violent and non-violent contestation. Regarding non-violent conflict, Barry et al. (2014) finds that emigration freedom reduces anti-regime protest within autocracies, but only when there are sufficient economic opportunities abroad. However, they do not directly test the effect of emigration itself.

In sum, it 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. The social movement literature has identified several different factors that affect whether a movement successfully mobilizes. The earliest literature focused on the role of grievances (e.g., Gurr 1970). Later scholars in the "political process" school noted that grievance is almost ubiquitous and focused instead on resources and political opportunities (e.g., McAdam et al. 2001). More recently, scholars have emphasized taking culture and emotions into greater account (e.g., Goodwin, Jasper and Polletta 2009).

Building from this varied literature, we argue that emigration can impede social movements by reducing the resources—especially manpower and leadership—available to groups and alleviating many of the grievances that social movements are built on. Specifically, total emigration decreases both non-violent and violent contestation by providing a peaceful outlet for potential dissidents, as well as economic benefits that reduce deprivation and enable governments to invest in security.

However, emigration from autocracies to democracies can increase organizational resources for non-violent contestation through exposure to democratic opposition and open environments in which to organize. Exposure to democracy can also change the cultural meaning of government behavior, increasing demand for change at home (Miller and Ritter 2013; Moses

2011). Thus, greater flows to democracies can facilitate peaceful protest. This is not necessarily a mortal threat for autocracies allowing open emigration, as nearly half of all migrants from autocracies move to wealthier autocracies, such as the Gulf Cooperative Council states (Group 2016).

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 paucity of migration data. We expect the mechanisms to differ only slightly in magnitude.[2] However, we emphasize we are capturing average treatment effects across a diverse population.

The Safety Valve Effect of Emigration

The social movements that organize contentious politics require resources to prosper, of which the most important is manpower. Mass politics of the type we examine—protests, strikes, and civil conflict—depend critically on large-scale participation. In what we call the *safety valve effect*, migration restricts this participation by siphoning off the most likely opposition members and leaders, while decreasing the incentives for opposition for those left behind.

The most critical effect on the manpower available to movements is through economic emigration. Most migrants move for primarily economic reasons—less than 10% of migrants were refugees in 2014 despite a spike in flows (Group 2016)—although many also trade off political concerns at the margins. On an individual basis, emigration therefore first selects out the most economically frustrated individuals, who may not be politically active but whose opposition could otherwise slowly build.

Economic migrants, who are disproportionately young men, are precisely those individuals most ripe for recruitment into protest movements and rebel groups. Humphreys and Weinstein (2008) find that participation in an armed group depends on an individual's economic position, the costs and benefits of joining, and social pressure. Emigration mitigates these individual motives by securing a viable outside option (Okamoto and Wilkes 2008; Sellars

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

2019), pushing out citizens who would be targeted as recruits for anti-regime movements if they were still at home. In the language of Goodwin (2001), revolutions develop when domestic groups have "no other way out" when facing repressive states. Emigration provides the way out.

Groups politically opposed to the regime—including activists, opposition voters, and non-allied ethnic groups—are also disproportionately likely to leave, especially if they face a risk of imprisonment, economic punishment, or worse. Although the most committed may stay to oppose the regime, many will prioritize personal safety and move abroad, if only to bide their time. Repressive regimes are particularly likely to target the most politically active and threatening opposition members, driving out the most likely leaders of anti-regime movements. Although many opposition leaders have fled abroad and returned to help take down regimes, from Portugal's Mário Soares to Iran's Ayatollah Khomenei, this effect still saps domestic movements of their most critical members. As a result, emigration generates a self-selected population more loyal to the regime.

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 prospect of leaving should make individuals less invested in home country politics. Describing high-emigration regions in Portugal under the *Estado Novo* dictatorship, Brettell (1979, 292) notes that the young were socialized to prefer exit and "gave no thought to involving themselves in forms of political action. . . . , [keeping] emigrants essentially non-political and relatively conservative." Prospective economic migrants should also be less likely to take up arms, which can compromise their ability to emigrate.[3] Lastly, by tightening the labor market at home, emigration provides greater job opportunities and wages for those left behind, reducing economic grievances and increasing the opportunity cost of anti-regime opposition.

More subtly, emigration flows can disrupt the critical social connections among potential dissidents, hampering collective action and constricting the spread of anti-regime ideology and recruitment (Humphreys and Weinstein 2008; Sellars 2019). For instance, East German

[3] This is especially true for emigration to OECD states, as many prohibit terrorist supporters, which often restricts rebel combatants.

dissidents recognized that "the turnover created as people left the country meant there was little familiarity or communication within the group anymore" (Mohr 2018, 279).

In sum, emigration impedes opposition organization by removing the most likely recruits and potential leaders, and harming incentives and organization for those left behind. The safety valve effect thus greatly limits the resources for contentious politics, resulting in a population-wide shift to exit and away from voice.

The Economic Benefits of Emigration

In addition to providing an outside option, emigration brings economic benefits that reduce grievances, dampen political contestation, and provide resources for the government. 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 (Group 2016). 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). Further, social movements can draw on remittances for funding, although it is unlikely remittances can make up for the loss of domestic manpower.

On the plus side for regimes, remittances improve economic conditions and in turn increase regime support (Tertytchnaya et al. 2018). Remittances also benefit non-receiving families through multiplier effects (Taylor 1999). Because they are counter-cyclical, remittances are especially beneficial during economic crises, key moments of regime vulnerability. 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). In turn, these expanded state resources directly counteract any reduced dependence on state patronage. On net, we therefore expect remittances to reduce contestation.

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.[4] Clemens (2011)

[4] Aid can also be stolen by incumbent regimes to maintain power (Bermeo 2016).

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.[5] 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.

Through remittances and greater links to the international economy, emigration is likely to increase economic health. Scholars have long noted that economic performance predicts support for incumbent politicians and regimes, including in autocracies (Barry et al. 2014). Emigration thus helps to stave off political grievances and political opposition, leading to less contentious politics.

Emigration to Democracies and Peaceful Contestation

While emigration overall should lower contestation, more emigration to democracies should shift opposition tactics to peaceful contestation within autocracies. We expect this to occur through three channels: the spread of *democratic norms* and resulting opposition to autocratic rule, increased *capacities* among return migrants, and *freedom to organize* abroad. Each effect increases social movements' resources or political opportunities, although they may operate on a longer time-scale than the safety valve and economic mechanisms.

As several recent studies show, even brief periods living in stable democracies can shift migrants' attitudes in a more pro-democratic direction (Camp 2003; Chauvet and Mercier 2014; Levitt 1998; Pérez-Armendáriz and Crow 2010; Kapur 2014; Spilimbergo 2009).[6] 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). Migrants thus often learn that life is better in democracy and become dissatisfied with their home-country politics. For example, a leader of Mongo-

[5] For example, Hatton and Williamson (1998, 224) find that emigration to the New World led to a 33% increase in Italian and Irish wages.

[6] An empirical concern is that migrants to democracies may have democratic leanings to begin with. Several of these survey-based studies therefore exploit as-if random variation in emigration destinations, as we do using an instrumental variables technique.

lia's opposition noted that emigrants' and foreign students' experiences in Europe led them to no longer "sympathize with the one-party ideology" and instead join the 1989-90 protest movement (Kaplonski 2012, 48).

Migrants spread the resulting norms when they return home or through contact with friends and family.[7] These *social remittances* can have powerful effects on sending communities (Levitt 1998). In Mexico, Pérez-Armendáriz and Crow (2010) find that relatives of migrants to the U.S. and Canada are more tolerant, civically engaged, and more than three times as likely to protest compared to others. While we focus here on democratic behavior, migration and travel can spread a variety of norms. For instance, communities in Mali with high emigration to countries opposed to female genital mutilation show decreased support for the practice (Diabate and Mesplé-Somps 2015).

This diffusion effect also extends to political elites (Gift and Krcmaric 2017). Mercier (2016) shows that developing country leaders who lived abroad produce more pro-democratic changes. Grewal (2020) finds that Tunisian elites who lived in Western democracies became more supportive of democratic norms. As he quotes one Ennahda MP, those lacking these migration experiences "will never appreciate [freedom] like we do because we tasted it. We actually experienced citizenship, experienced democracy, experienced living together with others" (533).

The spread of democratic norms should be especially likely to increase support for *peaceful* opposition against *autocratic* regimes. The types of non-violent political contestation prominent in democracies, such as protests and strikes, become natural tools for migrants and political leaders socialized into democratic behavior. This is even more likely if experiences abroad demonstrate the efficacy of non-violence (Chenoweth and Lewis 2013). Populations with higher demands for democracy should be especially likely to target autocratic regimes back home as the spread of norms alters the cultural acceptance of political activities once taken for granted (Goodwin, Jasper and Polletta 2009; Pérez-Armendáriz and Crow 2010). What was once seen as gift-giving to local officials may now be seen as bribery. Election fraud once met with a shrug may now trigger nationwide protest (Chauvet and Mercier 2014). Gradually, greater demand for democratic behavior should spread across the population.

[7] 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).

In addition, migrants to democracies have opportunities for peaceful political participation (such as protest and union activity) that can strengthen 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 and suffrage (Moses 2011). Similarly, Mexicans deported from the U.S. in the 1930s contributed to the agrarian movements back home that led to land reform (Sellars 2017). Zhang (2016) finds that Chinese citizens who witnessed protests in Hong Kong and Taiwan became more civically engaged back home. In contrast, migrants are unlikely to gain experiences in violent non-state organizations in democracies. Thus, migrants to democracies are more likely to both believe in non-violence and to have the organizational tools to wield it effectively.

Finally, emigrants to democracies usually have greater room to organize opposition movements abroad, as we discuss for Morocco and Taiwan. These migrant organizations can then link to domestic opposition groups or global networks (Betts and Jones 2016). 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.

A remaining question is how democratic emigration's contribution to non-violent contestation in autocracies balances against the pacifying safety valve and economic effects. As a theoretical matter, this is unclear. We therefore limit our prediction to the claim that, within autocracies, democratic emigration produces more non-violent contestation *relative* to autocratic emigration. Critically, this suffices to prove our theoretical point that *where* emigrants go matters. In testing, we compare the magnitude of effects.

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, within autocracies, higher rates of emigrants moving to democracies should predict more non-violent political contestation compared to emigrants moving to autocracies.*

Illustrative Cases

We now consider two cases that illustrate our mechanisms: Morocco and Taiwan.

Morocco: The safety valve and regime concerns over democratic norms

Morocco illustrates both how emigration can reduce contestation overall and how governments concerned about democracy-focused emigration attempt to block its effects. Emigration as a tool of pacification goes back to the colonial regime. In the 1920s, the Resident General ordered French labor recruiters to target emigrant workers "from the regions of Marrakesh and Mogador, zones of dissidence" and noted that "every departure of a Moroccan immigrant removed one rifle" from the opposition (quoted in Iskander 2010, 39). Contemporary French observers also noted the pacifying benefits of remittances from France to Morocco, arguing "the Moroccan South received from France a large portion of the resources that allowed it to live, and its pacification occurred almost as much in [French] factories as in its mountains" (quoted in Iskander 2010, 39).

After independence, emigration was a major part of King Hassan II's strategy to reduce political contestation. Emigration began in earnest when Morocco signed bilateral treaties with West Germany, Belgium, the Netherlands, and France in 1963. As Iskander (2010, 32) argues, Hassan aimed to gain support from rural elites as a countervailing force to urban opposition. To develop the agricultural export sector, the state needed a rural, landless labor force without creating an excess of rural laborers who might migrate to the cities and join ongoing protests. Instead, the regime shipped excess workers to Europe (Iskander 2010, 33). Further, the regime sent labor recruiters to areas considered especially restive (De Haas 2007; Iskander 2010, 44). In turn, increased emigration coincided with a decrease in contentious events (Figure 1).

Insert Figure 1 here

Caption: The figure shows the yearly emigration flow from Morocco to 22 OECD states (black line) and yearly number of contentious events (grey bars). Data on contentious events is from SPEED Project (2012). Emigration data is from DEMIG (2015).

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

Nevertheless, the government was unable to prevent all organization. Some emigrants organized left-wing opposition abroad, initially through activities with established European unions (De Haas 2007) and Moroccan university students (Iskander 2010). Emigrants established the *Association des Marocaines de France* in 1962, which had links to Morocco's main opposition party (De Haas 2007; Iskander 2010). Emigrant workers organized union activity in France and were disproportionately represented in immigrant labor protests in the 1970s (Iskander 2010, 81). These labor activities were not limited to France: In the 1980s, Moroccan labor organizations across Europe created the Common Charter for the Coordination of Moroccan Democratic Associations in Europe to share information about the regime's repressive activities (Iskander 2010, 111).

Migrants spread these norms and organizational tools back home through labor activities and pro-development organizations. A notable example of the latter is the group *Migrations et Développement* (M/D). Founded by migrants from the Sous region which lacked adequate public goods, M/D spurred the creation of self-funding village associations that determined village needs through a consultative process (Iskander 2010, 184). As M/D activists noted, "Before, people didn't even know how to have a conversation [about] development needs. Now, they understand the need to organize, they know how to talk to the state" (quoted in Iskander 2010, 189). These proto-democratic associations sufficiently threatened the regime's grip on power that it refused to provide organizational funds for M/D or similar groups (Iskander 2010, 189-190).

When migrants organized, their families back home often faced harassment, as did the emigrants themselves if they returned. Hundreds of labor organizers who returned to Morocco on vacation found themselves arrested, disappeared, or their passports confiscated (De Haas 2007; Iskander 2010, 106). The regime also recruited emigrant workers to physically intimidate unionized emigrants and tried to co-opt them (Iskander 2010, 106, 114). Only in the early 1990s did Hassan and King Mohammed VI reduce emigrant repression, largely to encourage economic engagement with Morocco through remittances and investment (De Haas 2007; Iskander 2010). This was only moderately successful as emigrants, as one editorial put it, "want political compensation for their economic contribution. . . . They want real participation in the management of the country; if not. . . they will shut off the [remittance] faucets" (quoted in Iskander 2010, 185).

In sum, Morocco has tried to balance the safety valve and economic benefits of emigration against the threat of democratic norm transmission. Although the state initially reduced contestation through emigration, it has also faced increasing organization by labor unions and village groups organized by emigrants. This supports the contrasting effects of these mechanisms, while also confirming that the democratic contagion effect may operate on a longer time-scale.

Taiwan: Elite emigration and the spread of democratic norms

Unlike the Moroccan government, Taiwan's autocratic ruling party, the Kuomintang (KMT), did not purposefully use emigration as a safety valve. Instead, emigration stemmed from a lack of domestic economic and educational opportunities: in the 1960s and 1970s, approximately 52,000 students left Taiwan to attend university or post-graduate programs, primarily in the U.S. (Chang 1992, 30). Although these students left to gain advanced training, not to seek out democracy, their time abroad changed their views. According to a 1979 survey of Taiwanese in the U.S., most did not plan to return to Taiwan due to limited political and academic freedoms and fewer economic opportunities (Chang 1992, 33). Only about 5% of student emigrants returned to the island in the 1960s, increasing to about 18% in the 1980s (Chang 1992, 28, 35).

The emigrants who stayed in the U.S. were soon seen as providing economic support and linkages back to Taiwan. Many students sent remittances once they started working, becoming critical foreign exchange earners (Chang 1992, 42). Further, they provided personal links to the U.S. economy for trade and investment, critical sources for the country's economic development (Chang 1992, 42). Finally, after the U.S. and U.N. de-recognized Taiwan as the government of China, emigrants in the U.S. became unofficial ambassadors helping to maintain American support for Taiwan's independence (Chang 1992, 43).

Although the KMT sought to limit emigrant organization through spying and targeted repression (Wright 1999), the regime could not limit the spread of democratic norms completely, even within the party. Former President Lee Teng-hui represents one of the most important examples of democratic norm transmission. Lee attended graduate school in economics at Cornell from 1965 to 1968, the height of the civil rights and anti-war movements. Previously a member of the Communist Party, Lee became a committed democrat during his time at Cornell (Gift and Krcmaric 2017, 691). He also recognized the importance of peaceful contestation, writing "that lack of democracy must be confronted with democratic methods" (Lee 1995). After Lee took power, he successfully and peacefully transformed the country from a one-party state to a full democracy. Lee was supported by other moderate KMT members who had obtained doctorates in the U.S. and earlier pushed to allow opposition periodicals to be published in 1979 (Domes 1981, 1024).

Democratization in Taiwan was not solely a top-down project, with a significant role played by peaceful popular activism. Prominent opposition leaders like Yao Chia-wen and Lu Hsiu-lien, along with many of their supporters, studied in the U.S. (Domes 1981, 1016). Much of the opposition's intellectual basis and energy came from universities, including from scholars who studied abroad. About a third of the student returnees to Taiwan ended up teaching in the universities (Chang 1992, 38). These intellectuals created several publications that spread democratic ideals and organized usually peaceful protests (Guuying 1982, 35-36). In turn, the university students they taught were key to pressuring Lee into democratizing on a faster timetable than planned, particularly through a peaceful sit-in in March 1990.

Taiwan represents a successful case of peaceful transition to democracy. Some reforms that opened up space for change were initiated by party members who had studied in the U.S. Much of the opposition had also spent time in the U.S. and adopted norms of peaceful, democratic contestation. Challenged by large, peaceful protests, President Lee responded not with violence but with reform.

Data and Empirical Analysis

Main Variables

We now rigorously test 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.[8]

For dyadic migration data, we rely on the World Bank (Özden et al. 2011), which provides migrant stocks for each 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 measurement error by single countries. However, it does not distinguish between types of 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 2015. 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

[8] A robustness check including OECD countries confirms our results.

We want to derive the causal effect of emigration on several forms of political contestation. However, we cannot simply test observed emigration due to both omitted factors and reverse causation: migrants flee countries during civil conflict and states strategically manipulate emigration. Although we show 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-dyadic 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 democratic character of emigrant destinations. These predicted measures are used as our instruments for total emigration and 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:

$$\begin{aligned} EmShare_{ijt} = \alpha_0 + \alpha_1 \ln(Population_{it}) + \alpha_2 \ln(Population_{jt}) + \alpha_3 \ln(Distance_{ijt}) \\ + \mathbf{X_{ijt}}\beta + \mathbf{W_{it}}\delta + \mathbf{Z_{jt}}\lambda + \gamma_t + \varepsilon_{ijt} \end{aligned} \tag{1}$$

where $EmShare_{ijt}$ is the migration stock from country i to country j in year t (as a share of i's population).[9] The sample includes all sending and receiving states. Equation (1) is known as a *gravity model*, frequently used in research on trade. $\mathbf{X}$, $\mathbf{W}$, and $\mathbf{Z}$ are control variables that refer in turn to dyadic characteristics, the source country i, and the receiver j. γ_t is a linear year control.

[9] Results are similar using the number of emigrants (logged) as the dependent variable.

We include variables that are exogenous to political contestation and that reflect the fact most economic migrants (and even many refugees) choose their destinations based on relative wealth and travel costs (Massey et al. 1993). For dyadic factors, we include the logged distance between capitals and dummies for six categories of contiguity (e.g., shared borders and separation by water) to approximate migration costs;[10] an indicator of whether one country colonized the other, a strong predictor for migrant networks; and a dummy for shared language, which facilitates migration (Melitz and Toubal 2014). We include 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 the same interactions for income, as large, wealthy neighbors are especially likely emigration targets. Because population and income may directly predict our outcome variables, we control for both in our outcome equations. Results from estimating Equation (1) are shown in Appendix Table A1.

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

$$\widehat{EmShare}_{ijt} = \widehat{\alpha}_0 + \widehat{\alpha}_1 \ln(Population_{it}) + \widehat{\alpha}_2 \ln(Population_{jt}) + \widehat{\alpha}_3 \ln(Distance_{ijt}) + \mathbf{X_{ijt}}\widehat{\beta} + \mathbf{W_{it}}\widehat{\delta} + \mathbf{Z_{jt}}\widehat{\lambda} + \widehat{\gamma}_t$$

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.

[10] The miles between capitals comes from Gleditsch and Ward (2001). Contiguity data is from Correlates of War Project (2007).

We then calculate how democratic country i's expected emigration targets are. Our first measure is the *net* emigration to democracies versus autocracies:

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

where Dem_{jt} captures whether the receiver is democratic. Hence, if 6% of a population goes to democracies and 2% to autocracies, the net democratic share is 4%. This prediction is correlated with the actual net democratic share at 0.49. Our second measure is the expected *fraction* of emigrants going to democracies, which is correlated with the actual fraction at 0.56. Each allows us to test whether democratic-focused emigration more strongly predicts the outcomes relative to autocratic-focused emigration.

We use these measures as instruments to predict the equivalent variables constructed from actual emigration: *Emigration Share*, *Net Democratic Emigration*, and *Fraction Democratic Emigration*.[11] 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 up a great deal of exogenous variation, well exceeding common benchmarks for strong instruments (shown below).[12]

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.[13] 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 demo-

[11] These respectively vary from 0.0003 to 0.56, -0.21 to 0.54, and 0.005 to 1. There is ample variation within countries over time: the within-country standard deviation of *Emigration Share* is 2.9% of the population.

[12] Although using predicted measures normally requires a standard error adjustment, the IV setup already adjusts for uncertainty, so we follow existing work and use robust standard errors (Frankel and Romer 1999; Preotu 2016). However, Appendix Table A2 confirms that the main results are robust to a two-stage bootstrapping procedure.

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

cratic 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).[14] 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). We also find largely consistent results when adding receiver and/or sender country fixed effects to Equation (1) (Tables A2-A3).[15]

Third, one might be concerned that our instruments happen to correlate with more stable or less violent countries, accounting for our results. In response, 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 A4). Thus, predicted emigration is specifically related to forms of political instability requiring mass participation.

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

[14] We include some of these variables only in checks as they are potentially post-treatment.

[15] For emigration share, of the 18 IV coefficients using Equation (1) fixed effects, all retain the same sign and 14 remain significant. Interestingly, with both fixed effects, emigration only significantly predicts violent events in autocracies and non-violent events in democracies. We also tested these models controlling for the country mean of the dependent variable in the outcome equation, accounting for country-specific factors. Although a demanding test, results hold with the noted dependence on regime type. For net democratic emigration, estimates retain significance for two of the three non-violent events, with the third narrowly missing.

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 if the conflict has totaled more than 1,000 deaths, and 3 if it features more than 1,000 deaths in the current year. We predict all non-binary outcomes using two-stage least squares (2SLS).[16] 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 country's largest such movement and distinguish the ordinal categories of no movement, a movement with fewer than 10,000 members, 10,000–100,000, 100,000–1 million, and above 1 million.[17] 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 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.[18]

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

[16] We follow common practice in applying 2SLS to both ordinal and count data.
[17] Results are similar using a simple binary variable for the presence of a violent campaign.
[18] 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 A8.

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. Further, as important conflict predictors, we control for natural resource wealth (fuel revenues per capita, in thousands of 2009 dollars, from Ross 2013); *Economic Growth* (the average percentage change in $GDP/capita$ over the past two years), *Land Area* (in millions of square kilometers, from World Bank 2015), and *Ethnolinguistic Fractionalization* (from Roeder 2001).[19]

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. In Table 1, we relate *Emigration Share* to *Civil War* using our IV framework, with predicted emigration as the instrument. We test the effect in the full non-OECD sample, then restricted to autocracies and democracies. 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 comparison, we also show the non-IV results (using probit) in the full non-OECD sample.

[19] Results are similar controlling for mountainous terrain (from Fearon and Laitin 2003).

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 2 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 contrast with arguments that diasporas increase the likelihood of conflict, such as Collier and Hoeffler (2004) and Miller and Ritter (2013). 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.

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 next column shows non-IV results for comparison.[20] 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. The first-stage results of each IV model are virtually identical to those shown in Table 1.

[20] We use a probit for binary outcomes, ordered probit for ordered outcomes, and negative binomial for the count variables.

Table 1: Instrumental Variable Models Predicting Civil War

	Probit	IV			Probit	IV		
	All	**All**	**Auth.**	**Dem.**	**All**	**All**	**Auth.**	**Dem.**
DV = *Civil War*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Second Stage								
Emigration Share	−7.065***	−16.302***	−18.463***	−12.051***	−4.228***	−13.530***	−19.835***	−13.722***
	(−8.20)	(−16.08)	(−13.35)	(−8.33)	(−4.87)	(−7.41)	(−7.56)	(−6.97)
GDP/capita (ln)	−0.346***	−0.355***	−0.363***	−0.436***	−0.172***	−0.231***	−0.295***	0.316**
	(−10.85)	(−11.39)	(−10.47)	(−4.88)	(−3.66)	(−5.27)	(−6.03)	(2.95)
Regional Civil Wars					−2.787***	−2.721***	−2.619***	−4.449***
					(−5.81)	(−6.19)	(−4.93)	(−4.15)
Trade					−0.441***	−0.196	0.033	−0.959*
					(−3.30)	(−1.51)	(0.26)	(−2.19)
Fuel Revenues Per Capita					−0.059**	−0.042**	−0.046**	−0.162
					(−3.04)	(−2.76)	(−2.78)	(−1.29)
Economic Growth					−0.029***	−0.018*	−0.017*	−0.016
					(−3.93)	(−2.51)	(−2.11)	(−1.09)
Population					0.001*	0.001**	−0.004**	0.001***
					(2.22)	(3.25)	(−2.74)	(3.53)
Land Area					0.059*	−0.024	0.061	−0.295***
					(2.19)	(−0.70)	(1.76)	(−7.07)
Ethnolinguistic Fractionalization					0.269*	0.019	−0.275	1.031**
					(2.01)	(0.14)	(−1.79)	(2.86)
Regional Polity					−0.029	1.232**	1.725**	−0.314
					(−0.07)	(3.05)	(2.99)	(−0.35)
Polity					−0.003	−0.005	0.008	0.062
					(−0.53)	(−0.86)	(0.77)	(1.62)
$Polity^2$					−0.010***	−0.010***	−0.006***	−0.029***
					(−8.87)	(−8.37)	(−3.44)	(−5.65)
Region Fixed Effects?	Y	Y	Y	Y	Y	Y	Y	Y
Year Fixed Effects?	Y	Y	Y	Y	Y	Y	Y	Y
First Stage								
$\widehat{Em}(Population\ Share)$		0.708***	0.568***	1.458***		0.585***	0.446***	1.363***
		(19.84)	(18.57)	(9.46)		(13.70)	(9.83)	(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 demonstrating a negative effect of emigration on civil war. The models differ by including all non-OECD countries, only autocracies, and only democracies. The IV first stage is summarized at the bottom. t statistics (based on robust standard errors) are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Insert Figure 2 here

Caption: The figures show the estimated likelihood of civil war given 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.

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 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 3 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*.[21] Each check adjusts the corresponding models in Table 2 with, each coefficient representing a distinct IV model.

First we 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.[22] In the fifth

[21] Results for the remaining dependent variables are similar.
[22] Although these factors may also generate post-treatment bias, our approach is to consider the full range of results under different assumptions and confirm their consistency.

Table 2: Effect of Emigration Share on Contestation

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

Notes: The table shows estimated effects of emigration share on several measures of contestation (listed at left). Samples 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 IV model. Emigration's negative effect holds across types of contestation, with the effect strongest in autocracies. t statistics (based on robust standard errors) are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

check, we include a 0-1 *Liberal Freedom Index* to further account for the political environment (Coppedge et al. 2016).[23]

Insert Figure 3 here

Caption: 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 models in Table 2.

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,

[23] Appendix Table A5 further shows the results hold controlling for autocratic regime types and stratifying by regime type, personalism, and the presence of elections.

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.

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^{***}	-20.122^{***}	-16.169^{***}	-6.244^{***}	-9.395^{***}	-2.594^{***}	-4.329^{***}	-6.766^{***}	-1.328^{***}
	(-9.23)	(-8.44)	(-9.54)	(-8.25)	(-7.76)	(-5.74)	(-6.34)	(-5.76)	(-3.69)
FDI Added	-13.687^{***}	-20.620^{***}	-13.724^{***}	-5.308^{***}	-5.441^{***}	-1.983^{***}	-4.426^{***}	-3.220^{*}	-1.798^{**}
	(-7.48)	(-7.11)	(-5.83)	(-5.94)	(-3.69)	(-3.56)	(-5.16)	(-2.50)	(-3.17)
Aid Added	-15.602^{***}	-24.060^{***}	-13.816^{***}	-5.192^{***}	-9.070^{***}	-0.812^{*}	-3.876^{***}	-6.264^{***}	-0.921^{**}
	(-8.65)	(-11.10)	(-6.42)	(-6.59)	(-6.11)	(-2.16)	(-5.67)	(-4.82)	(-2.77)
FDI/Aid Added	-15.259^{***}	-24.675^{***}	-13.788^{***}	-5.451^{***}	-6.931^{***}	-1.418^{**}	-3.872^{***}	-2.898^{*}	-1.105^{**}
	(-8.38)	(-10.89)	(-6.07)	(-5.86)	(-3.94)	(-3.06)	(-4.82)	(-2.09)	(-2.60)
Freedom Added	-12.464^{***}	-20.843^{***}	-12.794^{***}	-5.019^{***}	-8.848^{***}	-0.979^{*}	-4.598^{***}	-8.335^{***}	-1.276^{***}
	(-6.11)	(-7.62)	(-6.02)	(-5.97)	(-5.31)	(-2.46)	(-5.75)	(-5.04)	(-3.38)
Lagged 10 Years	-20.915^{***}	-26.192^{***}	-23.659^{***}	-8.677^{***}	-12.009^{***}	-2.542^{***}	-7.087^{***}	-11.602^{***}	-1.350^{*}
	(-7.34)	(-7.69)	(-5.48)	(-6.06)	(-5.06)	(-3.73)	(-5.27)	(-4.59)	(-2.33)
Lagged 20 Years	-16.047^{**}	-9.997	-66.845^{***}	-7.191^{***}	-7.771^{**}	-1.015	-10.145^{***}	-15.232^{***}	-1.105
	(-3.27)	(-1.08)	(-7.41)	(-4.09)	(-2.91)	(-1.11)	(-5.01)	(-4.16)	(-1.23)
OECD Included	-15.358^{***}	-19.892^{***}	-16.851^{***}	-6.740^{***}	-7.897^{***}	-1.902	-7.702^{***}	-7.601^{***}	-3.085^{*}
	(-4.85)	(-7.57)	(-10.49)	(-4.49)	(-5.80)	(-1.43)	(-4.87)	(-5.46)	(-2.43)
No Imputed Values	-19.290^{***}	-25.992^{***}	-18.166^{**}	-7.031^{**}	-11.160^{*}	-1.592	-3.259	-2.626	-2.665
	(-5.44)	(-9.29)	(-2.87)	(-2.50)	(-2.35)	(-1.15)	(-1.40)	(-0.74)	(-1.38)

Notes: The table shows robustness checks for emigration's effect 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; and remove all imputed values of emigration. The negative effect of emigration on contestation is highly robust, with 73 of 81 IV models remaining negatively significant and all remaining negative. t statistics (based on robust standard errors) are in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Our IV results are highly robust to these checks. In total, 73 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 25 of 27 checks.

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 Figure A1). 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). 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 across outcomes. We also follow Young (2020)'s advice to examine the robustness to dropping individual countries (Appendix Figure A3) and the results remain significant (t-value < -5) after dropping any country from the sample.

Emigration to Democracies

We now consider whether the *destinations* of emigrants matters for political contestation. Table 4 displays the IV models alternatively testing *Net Democratic Emigration* and *Fraction Democratic Emigration* on our outcomes. These are identical to the *Emigration Share* models in Table 2, with only the instrument and endogenous variable changed. Because *Fraction Democratic Emigration* doesn't encompass the magnitude of emigration, we include *Emigration Share* as a second endogenous variable, along with its instrument in the first stage.[24]

For the violent outcomes, results are inconsistent and mostly insignificant. Results are starkly different for the non-violent outcomes. In each case, the democratic emigration measures are significantly positive in the autocracy sample and negative or null in the democracy sample. Further, the substantive effect in autocracies is quite large. If we shift 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%). From

[24] The instruments remain strong first-stage predictors, with the Kleibergen-Paap F test 52.6 or higher in the *Civil War* tests. To get convergence, the fraction models are tested with 2SLS.

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

	Net Democratic Emigration			Fraction Dem. Emigration		
Dependent Variables	**All**	**Auth.**	**Dem.**	**All**	**Auth.**	**Dem.**
Violent Outcomes						
Civil War	−1.711	−4.503*	15.513***	−0.062	−0.134**	0.415**
	(−0.92)	(−1.98)	(5.73)	(−1.37)	(−3.08)	(2.72)
Civil Conflict	−4.838***	−5.009**	−12.800***	−0.266***	−0.057	−0.469**
	(−3.59)	(−2.79)	(−5.43)	(−4.85)	(−1.03)	(−2.85)
Conflict Intensity	−0.772	−1.399	−1.640	−0.449***	−0.264*	−0.086
	(−1.42)	(−1.63)	(−1.38)	(−3.90)	(−2.15)	(−0.28)
NAVCO Violent Movement Size	0.311	0.012	−0.811	−0.272**	−0.157	−0.244
	(0.48)	(−0.42)	(−0.30)	(−3.01)	(−1.69)	(−1.08)
Violent Events (Any)	0.726	2.367	−1.348	−0.147*	−0.007	−0.339
	(0.54)	(1.31)	(−0.32)	(−2.03)	(−0.09)	(−1.87)
Violent Events (Number)	3.639**	5.695**	5.291	−1.086***	0.161	−4.552***
	(2.67)	(3.11)	(1.29)	(−3.63)	(0.64)	(−4.40)
Non-Violent Outcomes						
NAVCO Non-Violent Movement Size	0.915*	2.644***	−1.244*	0.152	0.331**	−0.356
	(2.48)	(4.07)	(−2.11)	(1.89)	(3.28)	(−1.94)
Non-Violent Events (Any)	3.738**	8.746***	−0.460	0.088	0.220**	−0.471*
	(2.81)	(4.80)	(0.25)	(1.22)	(3.06)	(−2.28)
Non-Violent Events (Number)	4.902***	11.222***	−1.731	0.052	0.837**	−3.224***
	(3.59)	(5.53)	(−0.51)	(0.17)	(2.67)	(−4.07)

Notes: The table shows the estimated effects from IV models of net emigration to democracies and fraction of emigration to democracies on several dependent variables (listed at left). Samples vary by column. 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. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

the democratic fraction models, we can also calculate when the positive democratic effect outweighs the negative emigration share effect: If all emigrants go to democracies, the total effect is positive for protest when up to 3.9% of the population leaves.[25]

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 2013). That this opposition primarily takes non-violent forms further supports our theory on the diffusion of democratic norms regarding legitimate political contestation. More generally, it demonstrates that *where* emigrants go can matter as much as the total numbers.

Extensions: Testing the Mechanisms

[25] The results are robust to the variations shown in Table 3, as well as the nine IV alternatives (Appendix Figure A2).

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 (Appendix Table A6).

Second, we further examine how *Net Democratic Emigration* affects the character of anti-regime groups. We test a *Pro-Democratic Opposition* variable that averages measures of whether the opposition follows democratic rules, works through legal channels, and participates in elections, using 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 A8).

Third, we consider two other receiving state characteristics: wealth and freedom of association. Specifically, we test the expected mean level of *GDP/capita (ln)* and *Freedom of Association* (a three-valued measure from Cingranelli and Richards 2014) in receiving states, again using an IV setup (Appendix Table A7). Although more mixed than our other results, the findings follow our expectations. Wealthier receivers should generate greater economic benefits, reducing contestation. Indeed, we find consistent evidence for this (see Appendix Table A7). Countries with greater freedom of association should give migrants more opportunities to learn organizational skills and start opposition movements abroad, shifting tactics to peaceful contestation. Indeed, average *Freedom of Association* is consistently negative for violent contestation and positive for non-violent contestation. 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 con-

testation, revealing how the rising international flow of people can shape global patterns of conflict.

Civil violence and protest both require groups of citizens who are unhappy with the political regime and dedicated to changing it. Emigration provides the disaffected with 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 2013). Future research can investigate how emigration opportunities deter individuals from joining violent movements, explore factors that moderate emigration's effect (extending our findings on emigrant destinations and economic crises), and predict which diasporas mobilize during ongoing conflicts (e.g., Adamson 2013). Our work also highlights questions about how migrants' normative ideas, experiences, and connections to groups abroad influence democratic movements. How often do emigrants lead these movements? What types of migration are most likely to lead to these movements? Do some democratic destinations contribute more to non-violence through greater openness or their treatment of immigrants?

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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