

Abel Escribà-Folch, [Covadonga Mesequer](#) and Joseph Wright  
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# Remittances and Protest in Dictatorships

Abel Escribà-Folch,<sup>\*</sup> Covadonga Meseguer,<sup>†</sup> and Joseph Wright<sup>‡§</sup>

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## Abstract

Remittances – money migrant workers send back home – are the second largest source of international financial flows in developing countries. As with other sources of international finance, such as foreign direct investment and foreign aid, worker remittances shape politics in recipient countries. We examine the political consequences of remittances by exploring how they influence anti-government protest behavior. While recent research argues that remittances have a pernicious effect on politics by contributing to authoritarian stability, we argue the opposite: remittances increase political protest in non-democracies by augmenting the resources available to potential political opponents. Using cross-national data on a latent measure of anti-government political protest, we show that remittances increase protest. To explore the mechanism linking remittances to protest, we turn to individual-level data from eight non-democracies in Africa to show that remittance receipt increases protest in opposition areas but not in progovernment regions.

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Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the American Journal of Political Science Dataverse within the Harvard Dataverse Network, <https://doi.org/10.7910/DVN/TVZQG6>.

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<sup>\*</sup>Universitat Pompeu Fabra.

<sup>†</sup>London School of Economics and Political Science

<sup>‡</sup>Pennsylvania State University

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## Introduction

Do remittances spur anti-regime protests? Remittances – money migrant workers send back home – are the second largest financial inflow for developing countries, only behind foreign direct investment.<sup>1</sup> As a result, there is growing interest in understanding how out-migration and remittances influence a number of outcomes in recipient countries (Kapur, 2014) such as poverty, growth, exchange rate regimes, institutional quality, and public spending (Chami et al., 2008; Singer, 2010; Doyle, 2015). Importantly, researchers posit that political regimes may mediate how remittances influence these outcomes (Catrinescu et al., 2009; Abdih et al., 2012; Ahmed, 2013; Tyburski, 2014; Easton and Montinola, 2016).

One strand of this literature examines how remittances influence political stability. Given the growing size of inflows, some posit that remittances are similar to oil rents or foreign aid, and thus stabilize authoritarian governments (Ahmed, 2012). Yet remittances differ from these other inflows; they are private transfers sent by migrant workers that accrue directly to households, not recipient governments. Indeed, others show that remittances can promote democratization in dominant party autocracies by undermining electoral support for incumbent parties (Pfütze, 2012; Escribà-Folch, Meseguer and Wright, 2015). In the decades since the Cold War ended, electoral defeat of incumbents has been the most common way autocracies collapse (Geddes, Wright and Frantz, 2014).

This paper explores how remittances influence protests and whether political context mediates this relationship. While protest constitutes standard politics in democracies, anti-incumbent mobilization can destabilize dictatorships – under some circumstances propelling regime collapse and democratization (Bratton and van de Walle, 1997; Chenoweth and Stephan, 2011; Rivera and Gleditsch, 2013). We show that remittances are associated with protests in autocratic regimes, but not in democracies. Remittances thus contribute to political change via anti-regime mobilization as well as by undermining electoral support for incumbent. Notably, popular uprisings are the second most common way – after electoral defeat – autocracies collapsed since 1989 (Geddes, Wright and Frantz, 2014). Large, sustained anti-government protests have precipitated the downfall of

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<sup>1</sup>In 2015, according to UNCTAD and World Bank data, developing countries received \$681 billion in FDI, while remittances amounted to \$431.6 billion.

numerous autocracies in the recent decades, including the revolutionary wave in Eastern Europe Communist regimes, the Color Revolutions in post-Soviet regimes, and the Arab Spring in the Middle East and North Africa.<sup>2</sup>

Understanding how international factors, such as migrants' remittances, influence contentious politics contributes to research on the international dimensions of regime stability. Research on protest in autocracies is relatively scarce though. Cross-national studies mostly focus on domestic determinants such as economic conditions, elections, technology, and political opportunities (Bratton and van de Walle, 1997; Beaulieu, 2014; Brancati, 2014; Chenoweth and Ulfelder, 2017), while researchers concentrate on diffusion and economic globalization as international explanations for protest (Bunce and Wolchik, 2006; Beissinger, 2007; Bellinger and Arce, 2011; Gleditsch and Rivera, 2017). To the extent scholars examine migration, they focus on emigration, not financial inflows (Barry et al., 2014). Further, research linking remittances to political behavior generally focuses on new democracies – especially Mexico – and yields inconclusive results. We contribute to this literature by examining how remittances influence protest, especially in autocratic contexts.

We posit two competing theories. The first argues that remittances *decrease* anti-government protest by either: (1) reducing grievances against political incumbents; or (2) by providing dictatorships with more resources to fund patronage and repression. A second theory suggests that remittances *increase* protest by either: (1) augmenting the resources available to political opponents; and/or (2) by severing clientelistic links between individuals and the state, which reduces support for incumbent governments. We argue that political regimes mediate the impact of remittances: these dynamics are more likely in non-democratic, low-income contexts, for several reasons. First, the marginal effect of additional (remitted) income should be higher in societies where groups have limited access to resources due to state restrictions. Second, the effect of additional resources on protest should be strongest in contexts where institutionalized mechanisms for voicing demands are constrained. Finally, weakening clientelistic practices should be more important in autocracies because patronage is a more critical survival strategy in such regimes. Using global data on a latent

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<sup>2</sup>As popular revolts ousting autocratic leaders have become more common, the incidence of coups has decreased. See Andrea Kendall-Taylor and Erica Frantz, “Autocrats now more vulnerable to being ousted by revolt,” *The Washington Post*, 9 April 2014.

measure of anti-government protest, we show that remittances increase protest in autocracies but not in democracies.

We then turn to individual-level data from eight non-democracies in Africa to adjudicate between the two mechanisms linking remittances to anti-regime protest in autocracies. To examine whether remittances augment resources for political opponents or reduce tacit supporters' dependence on state-clientelism, we test whether the influence of remittances on individual behavior varies according to political preferences. To capture them, we construct a measure of progovernment support in both regions and districts within non-democratic countries. The evidence shows that remittance receipt increases protest in opposition areas but not in pro-government ones. Importantly, this research design reveals that context mediates the influence of remittances within non-democracies, providing evidence consistent with the contention that remittances increase resources for mobilization in autocracies in areas where anti-incumbent sentiment is strong.

## Remittances and Political Behavior

Existing evidence on the political consequences of remittances is not conclusive because it does not examine how political context mediates the influence of remittances. On one hand, there is growing evidence that remittances cause recipients to disengage from politics by reducing electoral turnout and depressing support for incumbent parties among those left behind (Goodman and Hiskey, 2008; Pfutze, 2012; Dionne, Inman and Montinola, 2014; Pfutze, 2014; Escribà-Folch, Meseguer and Wright, 2015).<sup>3</sup> Further, Doyle (2015) shows that remittance recipients are less likely to support leftist parties because remittances reduce recipients' support for redistribution through taxation. As countercyclical flows, remittances may reduce economic grievances, leading to disengagement from local politics (Bravo, 2009; Goodman and Hiskey, 2008). Indeed, research on Latin America suggests that remittances make recipients less dependent on state-delivered goods (Adida and Girod, 2011; Aparicio and Meseguer, 2012; Duquette, 2014), which can explain why remittances reduce incumbent support (Pfutze, 2014; Díaz-Cayeros, Magaloni and Weingast, 2003).

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<sup>3</sup>Conversely, Germano (2013) finds that remittance recipients were less likely to punish the incumbent party in the 2006 Presidential Mexican election.

Alternatively, numerous studies suggest that remittances empower recipients. Both monetary (and social) remittances are associated with more non-electoral political participation, such as activism in civic associations, contacting local officials, attending political meetings, and persuading others in political discussions (Levitt, 1998; Burgess, 2005; Goodman and Hiskey, 2008; Pérez-Armendáriz and Crow, 2010; Dionne, Inman and Montinola, 2014; Córdova and Hiskey, 2015).

Remittances may influence anti-government protest as well. However, there is little research on this and less so in autocratic contexts. Dionne, Inman and Montinola (2014) show that remittance receivers are more likely to protest in Africa, while Barry et al. (2014) argue that open emigration policies reduce protest in non-democracies by allowing dissenters to leave. Yet open emigration policies may increase protest if migrant remittances decline when economies weaken in destination countries. Focusing on other forms of contentious politics, Regan and Frank (2014) find that remittances reduce the risk of civil war onset during economic crises, while Miller and Ritter (2014) find that remittance inflows increase it.

## **Autocracies, Protests, and Remittances**

We present two competing sets of hypotheses linking remittances to protest. We then posit that the mechanisms underpinning a *positive* macro-relationship between remittances and protest are likely to obtain in autocracies, but not in democracies.

### **Remittances Dampen Protest**

Two mechanisms suggest remittances reduce anti-regime protest: individual grievance and government substitution. First, grievance-based approaches to contentious politics posit that economic or political deprivation motivates individuals to dissent (Gurr, 1970). Comparative evidence shows that poor economic conditions and relative deprivation are correlated with protests, especially in non-democratic and weak polities (Brancati, 2014). Remittances may thus discourage protests by providing families with additional (external) income. Existing evidence indicates that remittances are an important source of income for households in many developing countries, resulting in less poverty (Adams and Page, 2005; World Bank, 2006*a*) and more consumption and investment

(World Bank, 2006*b*; Fajnzylber and López, 2007; Chami et al., 2008; Adida and Girod, 2011). If remittances increase economic and, in turn, political satisfaction with the status quo, they could induce disengagement from politics (Germano, 2013; Regan and Frank, 2014). Similarly, remittances may insulate recipients from local economic conditions, prompting less political participation to hold decision-makers accountable (Bravo, 2009; Goodman and Hiskey, 2008). Barry et al. (2014) also posit – but do not test – that remittances mitigate protest by increasing the opportunity cost of challenging the regime. Hence, countercyclical remittance inflows may have a compensation and insurance function (Frankel, 2011; Doyle, 2015) that demobilizes citizens.

A second argument contends that remittances reduce protests via governments’ policies. By increasing tax revenue from consumption levies, remittances may augment governments’ revenues, thereby increasing funds to buy support.<sup>4</sup> Even if not generating extra state-revenue, remittances may still allow governments to divert public resources away from public goods: by increasing households’ income, remittances permit governments to substitute patronage spending and repression for public goods spending (Abdih et al., 2012; Ahmed, 2012; Tyburski, 2014; Easton and Montinola, 2016). Diverting resources to patronage and military spending may increase citizen loyalty and improve the coercive capacity of the regime, which in turn reduce protest opportunities. Because patronage and coercion are more important survival strategies in non-democracies, the substitution effect should be strongest in autocracies.

Both the grievance and substitution mechanisms yield the same macro-level expectation: *remittances reduce anti-government protest, especially in autocracies.*

## **Remittances Foster Protest**

Alternative mechanisms suggest that remittances should increase protest, again more so in autocracies than in democracies. First, the resource model of political participation contends that higher individual or household income should increase political engagement (Brady, Verba and Scholzman, 1995). Similarly, modernization theory posits that more income – and consequent social transfor-

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<sup>4</sup>There is some controversy here. In general, though, remittances are largely non-taxable due to tracking difficulties and high elasticity (Ahmed, 2012; World Bank, 2006*c*). Singer (2012) suggests that remittances increase revenue via consumption taxes, however Escribà-Folch, Meseguer and Wright (2015) find no evidence of this in autocracies.

mations – leads to emergent popular demands for political liberalization. A remittance-driven boost to resources could thus increase political participation by fostering organizational capacity, coordination, self-perceived effectiveness, value changes, and available time among individuals who oppose the incumbent government (White et al., 2015).

Remittances may not only prompt protests by increasing individual or household resources, but also by directly funding opposition political groups in recipient countries. Additional external funding can increase the opposition’s organizational and mobilizational resources and, in turn, boost its anti-regime collective action capacity (Burgess, 2014). This mechanism entails emigrants seeking to deliberately influence politics in home countries (Kapur, 2010). For example, O’Mahony (2013) and Nyblade and O’Mahony (2014) demonstrate that emigrants from developing countries send more money home at election time. Others find that remittances increase the risk of civil war onset and terrorist attacks by boosting resources available to armed groups (Mascarenhas and Sandler, 2014; Miller and Ritter, 2014). Additional resources may also increase other forms of contentious participation such as protest. Remittances thus constitute an external ‘political investment’ (O’Mahony, 2013) to fund opposition political activity and mobilize citizens.

A second theory posits that remittances reduce the marginal utility of government transfers and thus sever clientelistic ties between citizens and the regime. Remittances ‘liberate’ the former from the latter. While this logic may explain why tacit regime supporters demobilize by, for example, failing to turn out to vote, the logic extends to all citizens who are economically reliant on incumbent regimes. The demobilization argument outlined above assumes that income (and possibly a public good) is the sole component of an individual’s utility function. However, if individuals’ also value political or ideological preferences, remittance income increases autonomy from state-delivered goods, activating latent dissatisfaction with the regime (Pfütze, 2014). External income finances private consumption and local public goods that substitute for direct transfers and public services provided by the state.<sup>5</sup> As a result, “[c]itizens with alternative sources of income can better afford to make ‘ideological investments’ in democratization and oppose the regime” (Magaloni and Kricheli, 2010, 128). Severing clientelistic ties not only reduces electoral support for the

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<sup>5</sup>See Adida and Girod (2011) and Duquette (2014) on Mexico, Chaudhry (1989) on Yemen, and Diedhiou (2015) on Senegal.



regime, but may also free citizens to protest. Existing evidence shows that remittances are related to lower turnout for incumbent parties in new democracies and dictatorships (Pfütze, 2012, 2014; Escribà-Folch, Meseguer and Wright, 2015); but existing research is silent regarding protest.

The impact of these two mechanisms, we posit, should be stronger in non-democracies. While the resource model does not explain how participation is shaped by domestic political opportunities, we note that these opportunities vary considerably across different political contexts. The mobilizing effect of remittances should be stronger in autocratic contexts, for two reasons. First, the marginal effect of additional (remitted) income should be higher in low-income, autocratic environments. Similar to other forms of investment, remitted resources likely have positive but decreasing marginal returns in political effectiveness. Individuals and opposition groups in autocratic polities have limited access to resources and face organizational restrictions from the state. By providing funding for training, campaigning, and coordinating, remittances should increase opposition mobilizational capacity more strongly in these contexts. Conversely, in democracies, where opposition parties have access to ample (often public) resources for campaigning and other forms of participation, the marginal effect of remittances on mobilizational capacity should be smaller.

Second, given a remittance-driven increase in mobilization capacity, political context constrains the available choices for contentious political activity (Meyer, 2004). In democracies, additional resources allow dissenters to increase mobilization and influence through institutional channels of contestation. In contrast, in contexts where institutional channels are limited, additional resources should boost non-institutional forms of political engagement, such as protest (Tarrow, 1994). Indeed, Machado, Scartascini and Tommasi (2011) find that more contentious and unconventional types of participation are more common in polities with weaker institutions and, hence, fewer political opportunities. Additional income thus increases the capacity for political mobilization, allowing dissenting citizens to engage in alternative forms of participation that are constrained by domestic opportunity structures (White et al., 2015). In democracies, remittance recipients might vote less often, but these recipients may also have more resources to organize at the local level, to participate in and fund political parties, or to lobby politicians (Burgess, 2005; Pérez-Armendáriz and Crow, 2010; Córdova and Hiskey, 2015). However, in dictatorships – where political exclusion

is extensive, institutional channels to voice demands are restricted, and elections are unlikely to oust incumbents – remittances should increase contentious political engagement such as anti-regime protest.

Finally, the exit mechanism – or weakening clientelistic ties – should also be stronger in non-democratic polities. Although clientelism is present in many electoral democracies, extensive patronage networks used to co-opt potential political opponents are a key instrument of political survival in autocracies, where incumbent coalitions monopolize and politicize state resources (Bueno de Mesquita et al., 2003). Consequently, if remittances provide citizens with an exit option from state-delivered goods, this additional income may increase protest.

Both the resource and exit mechanisms support the following alternative expectation: *remittances increase anti-government protest, especially in autocracies.*

## Cross-National Data and Analysis

**Protest** To measure protest, we use data from Chenoweth, D’Orazio and Wright (2014). This paper constructs a latent protest variable from an item response theory (IRT) model that combines information from multiple existing datasets. Updates to the method employ an IRT approach that is dynamic in the treatment of the item-difficulty cut-points of the latent variable (Fariss, 2014); the model employs a Poisson distribution for count data.<sup>6</sup> Appendix A provides more information, including the list of the eight extant protest data sets used to construct the protest variable.

**Remittances** The main independent variable, *Remittances*, is from the World Development Indicators (WDI). We use the logged value of the lagged two-year moving average of real remittances, in constant U.S. dollars. Theoretically, we expect remittances to augment resources used in subsequent protest.

**Control variables** To model how remittances influence protest, we account for potential confounders that may determine both. We control for structural factors associated with protest capacity

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<sup>6</sup>Using a latent estimate from a negative binomial distribution yields similar results; see replication files.

and the size of remittance flows: *GDP per capita* and *Population*, both logged and lagged one year. Poor economic growth may cause grievance that fuels protest, while bad economic times may also cause citizens to elicit more remittances from abroad. We include the lagged two-year moving average of *Growth*. Migrant flows – rather than remittances themselves – may explain protest, particularly if dissenting citizens exit rather than protest; we control for *Net migration* lagged one year. These variables are from the WDI.

Further, protest in neighboring-countries can spur protest via diffusion effects as the Arab Spring and the Colored Revolutions illustrate (Bunce and Wolchik, 2006; Beissinger, 2007; Gleditsch and Rivera, 2017), while neighboring-country economic factors that produce those protests may, in turn, influence remittance flows. We include a measure of *Neighbor protest*, defined as the lagged mean latent level of protest in countries with capital cities within 4000km of the target country’s capital. Finally, we include an indicator of multiparty election period because elections may spur anti-government protests (Beaulieu, 2014; Hafner-Burton, Hyde and Jablonski, 2014), and evidence indicates elections attract remittances inflows (O’Mahony, 2013). *Election* periods are the calendar year of a multiparty election or the year prior to or afterwards.<sup>7</sup>

## Estimation

The linear model specification includes country and (5-year) time-period effects as well as baseline control variables ( $\mathbf{X}_{i,t-1}$ ): GDP per capita, population, neighbor protest, net migration, and election period.<sup>8</sup>

$$Protest_{i,t} = \alpha_0 + \beta_1 Remit_{i,t-1:t-2} + \beta_3 \mathbf{X}_{i,t-1} + \eta_t + \zeta_i + \varepsilon_{i,t} \quad (1)$$

$Protest_{i,t}$  is the latent mean of protest,  $Remit_{i,t-1:t-2}$  is the key explanatory variable,  $\mathbf{X}_{i,t-1}$  is a set of covariates,  $\eta_t$  are period effects,  $\zeta_i$  are country fixed effects, and  $\varepsilon_{i,t}$  is the error term that allows for regime-case clustering. The sample includes 102 non-OECD countries coded as autocracies or democracies in Geddes, Wright and Frantz (2014), from 1976 to 2010. *Autocracy* is

<sup>7</sup>Multiparty data is from Hyde and Marinov (2012): executive and legislative elections (`type`) where opposition was allowed (`ne1da3`) and where the ballot contained multiple candidates (`ne1da5`).

<sup>8</sup>A Hausman test indicates a fixed effects estimator produces different estimates than a random effects estimator.

a binary indicator of regime type.

### OLS test

The first column of Table 1 reports results from the OLS test: the estimate for  $\beta_{Remit}$  – for a pooled sample of autocracies and democracies – is positive but not significant. Estimates for *Growth* (negative) and *Elections* (positive) are in the expected direction and significant. The second column adds the interaction term ( $Remit \times Autocracy$ ) to the specification.<sup>9</sup> The interaction term is positive and statistically significant. The estimate for  $\beta_{Remit}$  – the marginal effect in democracies – is roughly zero; the estimate of  $\beta_{Remit} + \beta_{Remit \times Autocracy}$  – the marginal effect in autocracies – is positive and significant.

The latent protest variable is not a raw count of protests but rather an aggregation and scaling of existing information on protests. Substantively, the estimate for remittances in dictatorships, which is shown in Figure 1, indicates an increase in remittances from the 10th to the 90th percentile increases protest by just over 0.5 units.<sup>10</sup> A similar increase in protest within a country is observed in Indonesia from 1996 to 1998 and in Mali from 1989 to 1991. In both of these cases, anti-regime protests led directly to autocratic regime breakdown and democratic transition. For further comparison, we note that a similarly sized increase in economic growth, a strong correlate of protest identified in prior literature, *reduces* protest by roughly 0.25 units. Substantively, this indicates that remittances have roughly twice as large an effect on protest as economic growth.

This finding for protest in autocracies is robust to dropping or adding covariates: civil conflict, oil rents, foreign aid, trade, capital openness, movement restrictions, and refugee population outside the country.<sup>11</sup> We find support in models that use random- instead of fixed-effects, with errors clustered on country instead of regime-case, and when estimating heteroscedasticity and autocorrelation consistent (HAC) errors. Modeling the time trend in the data differently or denominating the remittance variable by GDP or by population yields similar results.

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<sup>9</sup>To ensure common support, we estimate an OLS model for autocracies only, yielding a similar result. Kernel regression estimates indicate the average marginal effect in autocracies is positive and statistically different from the average marginal effect in democracies, which is roughly zero. See Appendix B.

<sup>10</sup>See Figure B-1 as well.

<sup>11</sup>See Table B-1.

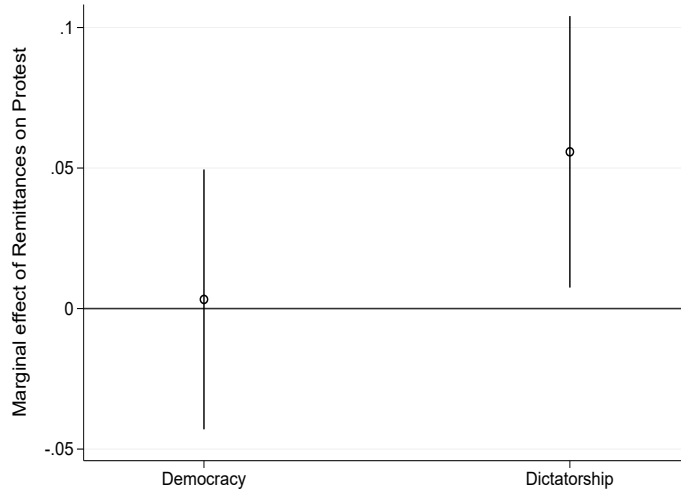


Figure 1: *Marginal effect of remittances on protest.*

## 2SLS-IV tests

OLS tests account for unobserved cross-sectional factors that jointly determine remittances and protest but may suffer from endogeneity, either as the result of mismeasured remittances or unmodeled strategic behavior. For example, if would-be protesters seek external resources such as remittances to finance (or ameliorate the costs of) protest, an estimate of  $\beta_{Remittances}$  in equation (1) may be biased upwards. If, alternatively, regimes that are likely to face protests restrict the flow of private external resources in anticipation of anti-government protest, then an estimate would be biased towards zero.

To address endogeneity, we construct an instrument from the time trend for received remittances in high-income OECD countries and a country's average distance from the coast. Remittances received by citizens in high-income OECD countries mostly come from other high-income OECD countries (World Bank, 2011, 12). Thus domestic factors in OECD countries that influence remittance receipts from other high-income OECD countries also determine the extent to which migrants from non-OECD countries who work in OECD countries send remittances home. Remittances received in high-income OECD countries are unlikely to directly influence political change in remittance-receiving non-OECD countries except through their indirect effect on remittances sent to other countries. We account for the possibility that remittances received in OECD coun-

tries reflect global economic trends that also influence domestic politics in developing countries by modeling calendar time in various ways.

The high-income OECD remittance trend varies by year. To add cross-sectional information, we weight the trend by the natural log of the inverse average distance from the coast.<sup>12</sup> Distance from the coast is correlated with ease of emigration and therefore emigrant population and remittances received, but domestic political behavior does not endogenously determine this geographic feature. Other ways through which coastal distance might influence politics are captured in GDP per capita, population, neighbor protest, and, most importantly, country fixed-effects. The 2SLS-IV model is the following, where  $Z_{i,t}$  is the excluded instrument:

$$Protest_{i,t} = \alpha_0 + \widehat{Remit}_{i,t} + \mathbf{X}_{i,t} + \eta_t + \zeta_i + \varepsilon_{i,t}^1 \quad (2)$$

$$Remit_{i,t} = \alpha_0 + Z_{i,t} + \mathbf{X}_{i,t} + \eta_t + \zeta_i + \varepsilon_{i,t}^2 \quad (3)$$

The third column of Table 1 shows results from a first stage, where the excluded instrument predicts the endogenous remittance variable. The F-statistic is larger than the weak ID critical value. The fourth column reports results from the outcome equation. The estimate for  $\beta_{Remittances}$  – the average marginal effect across all regimes – is positive but not statistically significant. The estimate size is roughly the same as OLS estimate in column 1. The next three columns report results from a 2SLS test with the interaction between remittances and dictatorship. With two endogenous variables, remittances and the interaction, there are two first-stage equations (reported in columns 5-6). Adding the interaction between dictatorship and the excluded instrument identifies these equations. The estimate for  $\beta_{Remit}$  is negative but not significant, while that for  $\beta_{Remit} + \beta_{Remit \times Autocracy}$  is positive and significant. The linear combination of the two – the marginal effect in dictatorships – is positive and significant.

The 2SLS estimate is substantially larger (0.364) than the OLS estimate (0.084). One interpretation is that the OLS estimate is biased downward due to mismeasurement or unmodeled strategic behavior. Another interpretation acknowledges that the 2SLS estimates likely models remittances from wealthy OECD countries and not remittances from oil-rich monarchies in the Middle East.

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<sup>12</sup>See Appendix C on instrument construction.

If higher-skilled migrants seeking a better life in advanced democracies (OECD) remit earnings to family members in developing countries, this resource may be sent with a political agenda, while remittances from migrants working in oil-rich autocracies may be sent without it.

Robustness tests in Appendix C show the result remains consistent when: (a) adding the OECD growth trend to the specification; (b) altering the way the time trend is modeled; (c) dropping control variables; (d) adding control variables; or (e) denominating remittances by GDP or population. The main finding remains when dropping each country from the sample, one at a time. Appendix D addresses missing data issues; the OLS result is slightly stronger when using multiply imputed data.

## **Microfoundations: Individual-level analysis**

The macro-results indicate that remittances increase protest in autocracies. However, designs using aggregate data cannot adjudicate between the mechanisms linking the two. To explore the micro-foundations of our argument, we discuss how political preferences shape the effect of remittances on protest in autocracies and derive some observable implications. We want to know whether remittances increase protest in autocracies because they provide resources to regime opponents that previously could not afford investments in contentious activities (resource mechanism); or alternatively, whether remittances increase protest because individuals caught up in clientelistic exchanges are now able to exit those networks and oppose the regime (exit mechanism). In the first case, remittances should increase protest among citizens who live where clientelism is less pervasive and regime-support lower. However, if remittances have a “liberating” effect, an increase in protest should be largest in progovernment areas where clientelistic networks are strongest.

The resource-theory suggests that additional remittance income augments individuals’ and groups’ capacity to mobilize. However, the model does not account for individuals’ political preferences and does not distinguish between regime supporters and opponents: additional resources would increase mobilization among both groups alike (Chenoweth and Ulfelder, 2017). Ideological distance from the incumbent regime is a good proxy for capturing underlying propensity to oppose the regime. Those with political preferences furthest from the regime are unlikely to be part of the

ruling coalition because the cost to the regime of buying their support is higher than for those closer to the incumbent. If remittances constitute additional resources that increase political mobilization, group funding, and coordination, this effect should be concentrated among individuals (or areas) with low levels of regime-support. The resource mechanism linking remittances to protest should therefore be strongest amongst ideological opponents outside the regime’s patronage network and weakest in stronghold areas with strong clientelistic ties to the regime.

Individual political preferences also shape how the exit mechanism works. This argument suggests that remittances sever clientelistic ties between regime supporters and the state by increasing the price of loyalty the government must pay to retain support. Since regime opponents are unlikely to benefit from clientelistic exchanges in the first place, the exit mechanism – and consequent increase in economic autonomy – should increase protest among tacit regime supporters tied into the regime’s coalition. Existing evidence suggests that remittances dampen turnout for incumbents in dominant-party regimes (Escribà-Folch, Meseguer and Wright, 2015), and that this decrease in support is strongest where clientelism is prevalent and where regime support is traditionally higher (Pfutze, 2012, 2014).<sup>13</sup> If remittances allow individuals to exit the incumbent regime’s clientelistic network to protest against the regime, then the evidence linking remittances to protest should be strongest in areas where the regime’s patronage network is most extensive and its historical support highest.

Using individual-level data helps disentangle the resource mechanism from the exit mechanism because although both mechanisms predict an aggregate increase in protest, they generate different expectations at the subnational level. If the resource mechanism is correct, remittances should increase protest *among citizens most opposed to the regime or more ideologically distant from it*. Alternatively, if the exit mechanism is correct, remittances should increase protest *in regime stronghold areas* with strong ties to the regime’s clientelistic network. However, as previous research has shown (Pfutze, 2014), weakening clientelistic networks may only lead to political apathy among loyalists rather than activating dissent. In this case, we would not observe an increase in protest among remittance recipients in stronghold areas.

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<sup>13</sup>In Mexico, Pfutze (2014) finds that electoral disengagement among previous regime supporters occurred in stronghold municipalities.



To explore how remittances influence protest behavior in non-democracies, we utilize Afrobarometer data from the 2008 survey for *all countries coded as non-democracies* by Geddes, Wright and Frantz (2014): Botswana, Burkina Faso, Mozambique, Namibia, Tanzania, Uganda, Zambia, and Zimbabwe. Burkina-Faso and Zimbabwe are among the top ten emigrant countries in the region; and they include major migration corridors, such as Burkina Faso-Cote d’Ivoire, Zimbabwe-South Africa, and Mozambique-South Africa (World Bank, 2016). In sub-Saharan Africa (SSA), remittances are a large source of foreign income: inward remittances in 2014 amounted to \$34.5bn, similar in size to FDI inflows (\$36.5bn) (World Bank, 2016). According to the 2008 Afrobarometer, in Zimbabwe and Burkina Faso, the percentage of respondents that declare receiving remittances is among the highest in SSA.

There is little research on the relationship between remittances and politics in SSA – let alone on the relationship between remittances and protest. Bratton (2008) suggests that emigration and remittances may be a release valve for dissenting citizens that contributes to regime survival in Zimbabwe. Some suggest that remittances may induce political apathy by, for example, reducing voter turnout (Ebeke and Yogo, 2013; Dionne, Inman and Montinola, 2014). Using Afrobarometer data, Dionne, Inman and Montinola (2014) find that remittances correlate with protest; however they do not explore the mechanism underlying this relationship nor whether there exists subnational variation. Others show that remittances decrease support for democracy among those who value economic stability over political freedoms (Konte, 2016).

To adjudicate between the alternative mechanisms by which remittances may have an effect on protest in African autocracies, we model an individual-level indicator of protest, derived from the following question: **Q23C: Attend a demonstration or protest march**. We group the three ‘Yes’ outcomes together (**Yes, once or twice, Yes, several times, Yes, often**) and group the two ‘No’ responses together, while treating ‘Don’t know’ responses as missing. Twelve percent of respondents in the eight non-democracies report participating in a protest.<sup>14</sup>

The main explanatory variable is an ordered measure of frequency of remittance receipt: **Q87: How often receive remittances**.<sup>15</sup> Roughly 14 percent of respondents report receiving remit-

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<sup>14</sup>Ordered variable tests yield similar results (Appendix E).

<sup>15</sup>There is no variable indicating the monetary value of remittances received. We also employ a binary indicator of

tances: 14.6 percent of remittance receivers protest while 11.6 percent of non-receivers protest.

To examine whether remittances augment resources for political opponents or “liberate” tacit supporters from clientelistic ties to the regime, we test whether the influence of remittances varies according to the *local political context* and, therefore, we create a continuous measure of progovernment support as the average level of reported government support in a geographic area, either region or district. We choose this geography-based strategy for delineating the extent of progovernment support for two related reasons.

First, clientelism often entails the exchange of local-level electoral support for local public goods (Ichino and Nathan, 2013; Nathan, 2016; Young, 2009). Studies of voting behavior focus on locally nonexcludable public goods (e.g. Baldwin, 2015; Carlson, 2015; Rozensweig, 2015; Ejdemyr, Kramon, and Robinson, 2016); and studies of ethnic favoritism suggest this occurs via targeted spending on local improvements such as schools and bore holes (e.g. Baldwin, 2015; Kramon and Posner, 2016).<sup>16</sup> Clientelistic practices may therefore require local elites (or brokers) to monitor local-level incumbent support and to supply local goods (Baldwin, 2015; Koter, 2013). In short, a *local geographic* measure of incumbent support, we posit, captures a salient feature of clientelism in these eight countries in 2008.

Second, using geographic location to capture clientelistic operations that underpin incumbent support circumvents inference issues that arise when relying on individual survey responses to distinguish regime supporters from opponents in political contexts where opponents face the prospect of state-led violence. Self-reported data on voting intentions is likely to suffer from non-response bias in non-democratic settings because there is a threat of political violence against opponents. Indeed, a non-trivial share of respondents refused to answer questions about which party they support. For example, in Zimbabwe, where the survey was implemented during an election period in which the regime targeted opposition supporters, nearly a third of respondents refused to answer the question about *the political party for which they vote*.

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remittance receipt that groups the positive responses (‘Less than once a year’, ‘At least once a year’, ‘At least every 6 months’, ‘At least every 3 months’, and ‘At least once a month’) together and groups negative responses together, while treating ‘Don’t know’ as missing (Appendix E).

<sup>16</sup>Clientelistic exchange based on local public goods provision is prevalent in many parts of the world (Stokes et al., 2013).

The left panel of Figure 2 shows the non-response rate for a question about *the political party for which [respondents] vote* is considerably higher than for other questions on the survey. Further, the right panel shows the non-response rate is positively correlated with Freedom House scores (higher scores indicate *fewer* political rights). Consistent with the contention that self-reported data on party voting may be unreliable, we find that, after controlling for a host of individual-level demographic and economic variables, the strongest predictor of refusing to respond to the party support question is whether the survey respondent resides in a region with more opposition supporters.<sup>17</sup>

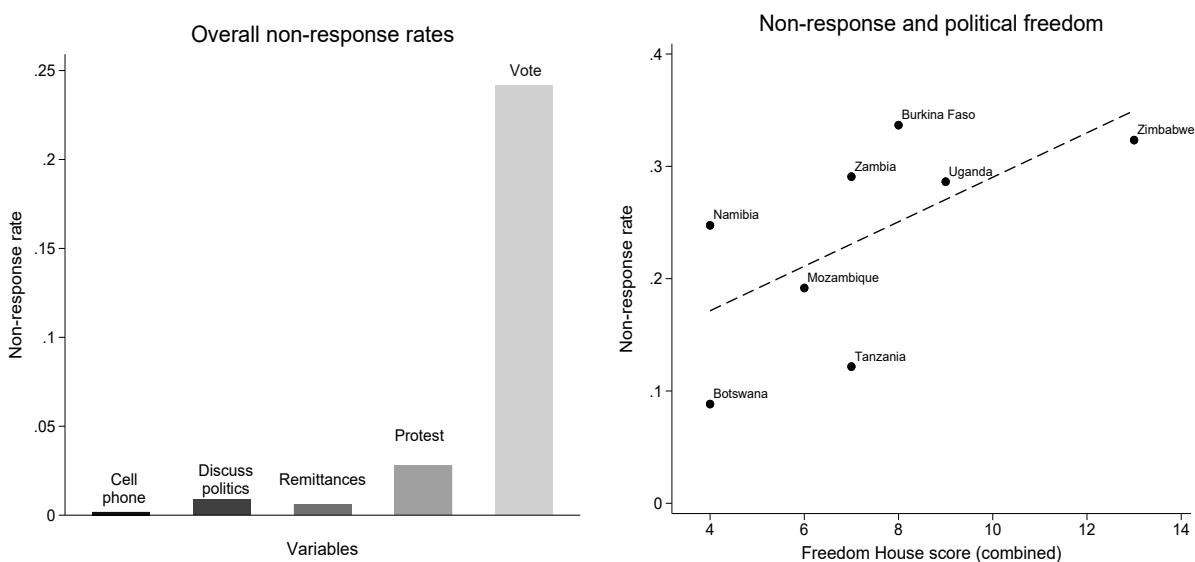


Figure 2: *Non-response rates*. Higher Freedom House scores indicate *less* political freedom.

Rather than using self-reported data on individual voting intentions, we posit that geographic differences better capture the distinction between opposition areas and progovernment areas. To measure the extent of progovernment support in geographic areas, we utilize the `region` and `district` variables from the survey. We employ two levels of geographic aggregation in the analysis because we have no a priori expectation about the relevant size of the geographic units that most closely match distributional cleavages in different countries in distinct types of regimes.<sup>18</sup>

<sup>17</sup>See Appendix E.

<sup>18</sup>The region unit is the area stratification used for sampling; the survey “stratif[ies] the sample according to the main sub-national unit of government (state, province, region, etc.) and by urban or rural location”

We calculate the average level of support for the incumbent within each geographic unit (region or district) by combining responses from three Afrobarometer questions – Q49A: **Trust president**, Q49E: **Trust ruling party**, and Q70A: **Presidential performance** – to construct a scaled index at the individual level.<sup>19</sup> Then we take the mean level of this scaled index for all respondents in each region or district. This provides a continuous measure of region- or district-level incumbent support (bounded at 0 and 1), which we call *progovernment*. If respondents refuse to respond to these questions because they fear reprisal for answering sensitive questions, this non-response is registered by lowering the mean level of incumbent support in the district. Rather than throwing out information from non-responses, which could bias estimates, we use this information to measure the geographic area’s *lack* of incumbent support.

To account for potential confounders, we include: cellphone usage, travel, age (log), education, wealth, male, and employment status. In addition to standard demographic (gender, age, and education) and economic variables (wealth and employment), this list includes cellphone usage because it facilitates protest by lowering coordination costs and is an essential technology for transferring remittances (Ebeke and Yogo, 2013; Manacorda and Tesei, 2016). Finally, respondents who travel more, either due to their individual preferences or endowments, may view exit – rather than voice – as a less costly form of dissent. These individuals may also be more likely to have family abroad, which causes them to travel more and to receive remittances. The specification is:

$$Protest_{ij} = \beta_1 Remit_{ij} + \beta_2 (Remit_{ij} \times Progovernment_j) + \beta_3 \mathbf{X}_{ij} + \delta_j + \epsilon_{ij} \quad (4)$$

Because we use a continuous region- or district-level measure of incumbent support to measure clientelism, the combination of  $\beta_1 + (\beta_2 \times Progovernment)$  estimates the marginal effect of remittances in districts with varying levels (0 to 1) of progovernment support.

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(<http://www.afrobarometer.org/surveys-and-methods/sampling-principles>). Samples in models with fixed-effects employ data from 185 regions but only 439 (of 614 total) districts with variation in the outcome variable. Appendix E reports random effects models that contain all regions and districts, even those with no variation in the dependent variable.

<sup>19</sup>We create a binary indicator of incumbent support from the responses for each question such that “somewhat”/“approve” and “a lot”/“strongly approve” indicate support. Between three and eight percent of those queried do not respond to these questions, in contrast to 24 and 38 percent non-response for other potentially politically-sensitive questions. The three items have an alpha score of 0.69.

In addition to individual-level covariates ( $\mathbf{X}_{ij}$ ), the specification includes region (or district) fixed-effects ( $\delta_j$ ) to account for differences across regions (districts).<sup>20</sup> The model thus accounts for geographic factors that may be correlated with emigration, the cost of receiving remittances, and the likelihood of protest, such as distance from the border and topography. The model also accounts for geographic differences in government attempts to thwart remittance receipt in known opposition areas. This design thus captures local geographic differences with respect to vote-buying, regime neglect, repression, and protest opportunity structure. This is important because opposition areas may offer more opportunities for protest.<sup>21</sup> The identifying information is the difference between remittance receivers and non-receivers in relatively *progovernment* regions relative to this difference in less progovernment (or opposition) regions. Inference thus stems from comparing individuals with others in the *same geographic location*.

## Results

The first column of Table 2 reports results from a conditional logit estimator with **region** as the geographic unit, allowing a direct comparison of remittance recipients to non-recipients within the same region: remittance receipt is positively correlated with protest participation. The second column includes the interaction between *Remit* and *Progovernment*, the estimate of which is negative and statistically significant. This indicates that a positive association between remittances and protest decreases as regional progoverment support increases.

The estimate for  $\beta_{Remit} + \beta_{Remit \times Progov}$  at low levels of regional progovernment support (10th percentile of the region progovernment distribution, or 20 percent progovernment support) is reported at the bottom of column 2. This estimate of the marginal effect of remittances in opposition regions is positive and statistically significant. In contrast, the estimate for the marginal effect of remittances in stronghold regions (90th percentile of the region progovernment distribution, or 76 percent progovernment support) is small, negative, and not statistically significant. This finding suggests that remittances are associated with protest in opposition regions but not in strongly

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<sup>20</sup>One component of the multiplicative interaction term (*Progovernment*) is omitted from the specification because this variation is captured in the region (district) fixed effects ( $\delta_j$ ). We employ a conditional logit that partials out the constant and cluster errors by region (district).

<sup>21</sup>Appendix E discusses this point.

progovernment regions.

The latter two columns of Table 2 report results from models that use district – instead of region – as the geographic unit. Grouping respondents from all districts together, the estimate of  $\beta_{Remit}$  is positive and statistically significant in column 3. Results from the interaction model in column 4 are similar to those reported in column 2. The estimate of  $\beta_{Remit} + \beta_{Remit \times Progov}$  at low levels of district progovernment support is positive and statistically significant, while the estimate at high levels of progovernment support is again negative and statistically indistinguishable from zero.

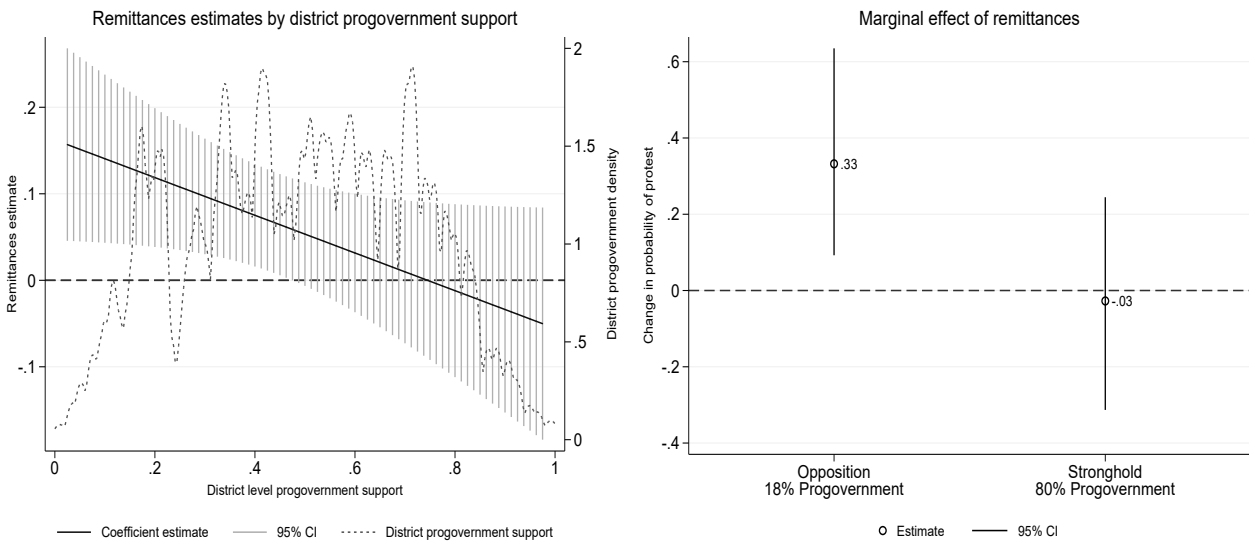


Figure 3: *Marginal effect on Protest participation.* Point estimates and 95 percent confidence intervals, based on Table 3, columns 3 and 4.

The left plot of Figure 3 shows how the estimate of  $\beta_{Remit}$  varies across a range of values for district-level progovernment support, using the more conservative estimates reported in column 4. The size of the estimate, depicted on the left vertical axis of the left plot, is positive and statistically significant in districts with low levels of progovernment support, up to about 0.5 on the horizontal axis. The estimate for remittances is positive and significant in districts with 50 percent or less support for the government, which is roughly the median value at the individual level. This estimate, however, turns negative (though not significant) at high levels of progovernment support – roughly 0.75 on the horizontal axis.

The right plot of Figure 3 reports the substantive effect of an increase in remittance receipt from the lowest level in the survey (0) to the highest level (5).<sup>22</sup> In opposition districts (those with 18 percent progovernment support) the marginal effect of remittance receipt at the highest level is to increase the probability of protest by 33 percent, which is a large effect. In contrast, in stronghold districts (80 percent progovernment) the marginal effect is roughly zero.

Appendix E discusses robustness tests: split-sample models to ensure the findings hold across a range of values for progovernment support; results from random-effects models; specifications without controls and with more control variables; specifications using a binary measure of remittance receipt and ones using an ordinal measure of protest; specifications that include ethnic group fixed-effects; and tests that leave-out one country at a time to ensure the findings are not dependent on any one country. In all these tests – using both region and district-level measures of progovernment support – we find consistent results.

We also test a treatment effects model using coarsened exact matching to trim data in the district-level model, allowing comparison of remittance recipients with similar individuals who do not receive remittances. In addition to matching individuals on the covariates in the main analysis, we match on distinct individual indicators of grievance. These indicators are based on antigovernment sentiment; relative material deprivation; fear of the incumbent regime; and paying bribes (a proxy for corruption). Again we find consistent results.

Overall, the individual-level analyses show that remittances increase protest in autocracies via a resource mechanism rather than through an exit effect. Remittances increase protest propensity in opposition districts/regions, but such effect is not evident in progovernment regions and districts. Note also that the effect of remittances in progovernment areas is negative but not significant, so we do not find support for the disengagement hypothesis at the individual level either.

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<sup>22</sup>Simulations conducted using the observed-values approach (Hanmer and Kalkan, 2013). The 10th percentile of the district-level progovernment support variable is 0.18; the 90th percentile is 0.80.

## Conclusion

This article contributes to research on the consequences of remittances and out-migration for sending countries by exploring how remittances influence anti-regime protests. Our contribution is threefold. First, while most research on remittances and political behavior analyzes different types of participation and focuses on democratic countries (especially Mexico and other Latin American democracies), we explore how political regimes mediate the link between remittances and protest in autocracies. Second, our macro-level tests use global data on a latent measure of anti-government protest that combines information of multiple existing datasets. Finally, we utilize individual-level survey data from eight African non-democracies to test the causal mechanisms linking remittance receipt to protest.

Popular uprisings are an increasingly common way of ousting autocracies; and we show that remittances spur anti-regime protests in authoritarian contexts. Remittances may thus help advance political change. Our individual-level tests are consistent with the contention that remittances increase protest by augmenting the resources available to political opponents.

This paper examines financial remittances. Yet, social remittances may also spur protest in dictatorships because the flow of ideas, democratic values, and political behaviors should have a greater marginal effect on individuals in sending countries where these democratic practices and values are otherwise restricted, namely autocracies. However, this effect would only hold if the main destination of migrants are democracies (Rother, 2009). Exploring this possibility is an important avenue for future research. Researchers should also examine how our finding varies by autocratic regime type. While dominant party regimes are more institutionalized and frequently hold (semi-competitive) elections, personalist dictatorships are more exclusive and offer fewer avenues for expressing dissent. Following our logic, we would expect our results to be stronger in these autocracies.

The findings have important policy implications. Besides direct democracy promotion, wealthy democracies interested in advancing political liberalization in foreign countries may want to adopt migration policies that facilitate the flow of remittances to autocratic countries. Remittances erode electoral support for incumbent parties and, as this article finds, they also embolden individuals



and groups to take to the streets. Hence, contrary to other forms of foreign income such as oil, aid, or foreign investment, remittances have the potential to destabilize autocracies.

Table 1: Remittances and anti-government protest

<i>Dependent variable</i>	OLS		2SLS		2SLS Interaction		
	Protest		Remit	Protest	Remit	Remit $\times$ Dict.	Protest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Remit	0.036*	0.000		0.048			-0.243*
	(0.02)	(0.02)		(0.09)			(0.11)
Remit $\times$ Autocracy		0.062*					0.608*
		(0.02)					(0.19)
GDP pc (log)	0.298*	0.341*	-0.569	0.405*	-0.530	-1.016*	0.839*
	(0.15)	(0.15)	(0.34)	(0.20)	(0.34)	(0.49)	(0.31)
Population (log)	0.907*	0.818*	-1.410*	1.230*	-0.808	0.256	0.379
	(0.28)	(0.27)	(0.71)	(0.37)	(0.70)	(0.69)	(0.52)
Neighbor protest	0.186	0.144	-0.329	0.253	-0.146	0.357	-0.147
	(0.18)	(0.17)	(0.32)	(0.24)	(0.29)	(0.32)	(0.29)
Growth	-0.017*	-0.018*	0.041*	-0.023*	0.040*	0.037*	-0.033*
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Net migration migr	0.053	0.041	-0.134	0.072	-0.083	0.072	-0.035
	(0.09)	(0.08)	(0.10)	(0.12)	(0.10)	(0.15)	(0.12)
Election	0.074*	0.064*	-0.044	0.099*	-0.009	0.114	0.000
	(0.03)	(0.03)	(0.06)	(0.04)	(0.06)	(0.06)	(0.06)
Autocracy	0.192*	-0.708*	-0.274	0.261*	5.282*	9.918*	-8.484*
	(0.08)	(0.33)	(0.17)	(0.11)	(1.41)	(1.84)	(2.74)
Instrument			1.488*		1.984*	0.325	
			(0.22)		(0.27)	(0.28)	
Instrument $\times$ Autocracy					-0.804*	0.628*	
					(0.20)	(0.27)	
$\beta_{Remit} + \beta_{Remit \times Dict.}$		0.063*					0.365*
		(0.02)					(0.17)
F-statistic				45.1		11.7	
Weak ID critical value				16.4		7.0	

\*  $p < 0.05$ . Years: 1976 - 2010. 102 countries; 2429 observations in columns 1-2; 2428 observations in columns 3-7. Country- and period-fixed effects included in all specifications but not reported. Standard errors clustered on regime-case.

Table 2: Remittances and protest

	Region effects		District effects	
	(1)	(2)	(3)	(4)
Remittances	0.070*	0.196*	0.064*	0.162*
	(0.03)	(0.07)	(0.03)	(0.06)
Remit $\times$ Progovernment		-0.273*		-0.218
		(0.14)		(0.12)
Cell phone	0.355*	0.359*	0.311*	0.312*
	(0.08)	(0.08)	(0.09)	(0.09)
Age (log)	-0.188	-0.191	-0.168	-0.170
	(0.12)	(0.12)	(0.11)	(0.11)
Education	0.086*	0.085*	0.090*	0.089*
	(0.03)	(0.03)	(0.02)	(0.02)
Wealth	0.285*	0.286*	0.284*	0.286*
	(0.13)	(0.13)	(0.12)	(0.12)
Male	0.195*	0.196*	0.195*	0.195*
	(0.06)	(0.06)	(0.06)	(0.06)
Employment	0.042	0.040	0.042	0.042
	(0.05)	(0.05)	(0.05)	(0.05)
Travel	0.001	0.006	0.037	0.042
	(0.07)	(0.07)	(0.07)	(0.07)
Respondents	10069	10069	8626	8626
Regions/Districts	185	185	469	469
$\beta_{Remit} + \beta_{Remit \times Progov} \times 10\text{th ptile}$		0.142*		0.123*
		(0.05)		(0.04)
$\beta_{Remit} + \beta_{Remit \times Progov} \times 90\text{th ptile}$		-0.011		-0.010
		(0.05)		(0.05)
Observations	10069	10069	8626	8626
Regions/districts	185	185	469	469

\*  $p < 0.05$ . Year: 2008. Region (district) fixed-effects included in all specifications (not reported). *Progovernment* constituent term omitted because it only varies by region (district) and the specification includes fixed region (district) effects. Standard errors clustered on region (district). Eight non-democracies are: Botswana, Burkina Faso, Mozambique, Namibia, Tanzania, Uganda, Zambia, and Zimbabwe.

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