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Remittances and Protest in Dictatorships

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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 antigovernment 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 nondemocracies by augmenting the resources available to potential political opponents. Using cross-national data on a latent measure of antigovernment political protest, we show that remittances increase protest. To explore the mechanism linking remittances to protest, we turn to individual-level data from eight nondemocracies in Africa to show that remittance receipt increases protest in opposition areas but not in progovernment regions.*

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, at: https://doi.org/10.7910/DVN/TVZQG6.

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 (FDI).[1] 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; Doyle 2015; Singer 2010). Importantly, researchers posit that political regimes may mediate how remittances influence these outcomes (Abdih et al. 2012; Ahmed 2013; Catrinescu et al. 2009; Easton and Montinola 2017; Tyburski 2014).

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 (Escribà-Folch, Meseguer, and Wright 2015; Pfutze 2012). 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 article explores how remittances influence protests and whether political context mediates this relationship. While protest constitutes standard politics

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[1] In 2015, according to UNCTAD and World Bank data, developing countries received $681 billion in FDI, whereas remittances amounted to $431.6 billion.

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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 incumbents. 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 numerous autocracies in 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.[2]

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 (Beaulieu 2014; Brancati 2014; Bratton and van de Walle 1997; Chenoweth and Ulfelder 2017), whereas researchers concentrate on diffusion and economic globalization as international explanations for protest (Beissinger 2007; Bellinger and Arce 2011; Bunce and Wolchik 2006; 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) 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) 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 nondemocratic, 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 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 nondemocracies 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 nondemocratic countries. The evidence shows that remittance receipt increases protest in opposition areas but not in progovernment ones. Importantly, this research design reveals that context mediates the influence of remittances within nondemocracies, 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 (Dionne, Inman, and Montinola 2014; Escribà-Folch, Meseguer, and Wright 2015; Goodman and Hiskey 2008; Pfutze 2012, 2014).[3] Further, Doyle (2015) shows that remittance recipients are less likely to support leftist parties because remittances

[2] 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," *Washington Post*, April 9, 2014.

[3] Conversely, Germano (2013) finds that remittance recipients were less likely to punish the incumbent party in the 2006 presidential Mexican election.

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 (Díaz-Cayeros, Magaloni, and Weingast 2003; Pfutze 2014).

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

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, whereas Barry et al. (2014) argue that open emigration policies reduce protest in nondemocracies 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, whereas 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 nondemocratic 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 2006a) and more consumption and investment (Adida and Girod 2011; Chami et al. 2008; Fajnzylber and López 2007; World Bank 2006b). 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 (Doyle 2015; Frankel 2011) 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.[4] 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; Easton and Montinola 2017; Tyburski 2014). 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 nondemocracies, the substitution effect should be strongest in autocracies.

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

[4]There is some controversy here. In general, though, remittances are largely nontaxable due to tracking difficulties and high elasticity (Ahmed 2012; World Bank 2006c). Singer (2012) suggests that remittances increase revenue via consumption taxes; however, Escribà-Folch, Meseguer, and Wright (2015) find no evidence of this 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 Schlozman 1995). Similarly, modernization theory posits that more income—and consequent social transformations—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 prompt protests not only 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 (Pfutze 2014). External income finances private consumption and local public goods that substitute for direct transfers and public services provided by the state.[5] As a result, "citizens 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 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 (Escribà-Folch, Meseguer, and Wright 2015; Pfutze 2012, 2014); but existing research is silent regarding protest.

The impact of these two mechanisms, we posit, should be stronger in nondemocracies. 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 noninstitutional 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

[5]See Adida and Girod (2011) and Duquette (2014) on Mexico, Chaudhry (1989) on Yemen, and Diedhiou (2015) on Senegal.

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; Córdova and Hiskey 2015; Pérez-Armendáriz and Crow 2010). 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 nondemocratic 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 article constructs a latent protest variable from an item response theory (IRT) model that combines information from multiple existing data sets. 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.[6] Appendix A in the supporting information (SI) 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 2-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 and the size of remittance flows: *GDP per capita* and *Population*, both logged and lagged 1 year. Poor economic growth may cause grievance that fuels protest, and bad economic times may also cause citizens to elicit more remittances from abroad. We include the lagged 2-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 1 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 (Beissinger 2007; Bunce and Wolchik 2006; Gleditsch and Rivera 2017), and 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 4,000 kilometers 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 afterward.[7]

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.[8]

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

$Protest_{i,t}$ 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, η_t are period effects, ζ_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 a binary indicator of regime type.

[6] Using a latent estimate from a negative binomial distribution yields similar results; see replication files.

[7] Multiparty data are from Hyde and Marinov (2012): executive and legislative elections (`type`) where opposition was allowed (`nelda3`) and where the ballot contained multiple candidates (`nelda5`).

[8] A Hausman test indicates a fixed effects estimator produces different estimates than a random effects estimator.

TABLE 1 Remittances and Anti-Government Protest

	OLS		2SLS		2SLS Interaction		
	Protest		Remit	Protest	Remit	Remit × Dict.	Protest
Dependent Variable	**(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 × 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	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 × 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	

Note: Years: 1976–2010; 102 countries; 2,429 observations in columns 1–2; 2,428 observations in columns 3–7. Country and period fixed effects included in all specifications but not reported. Standard errors clustered on * $p < .05$.

Ordinary Least Squares (OLS) Test

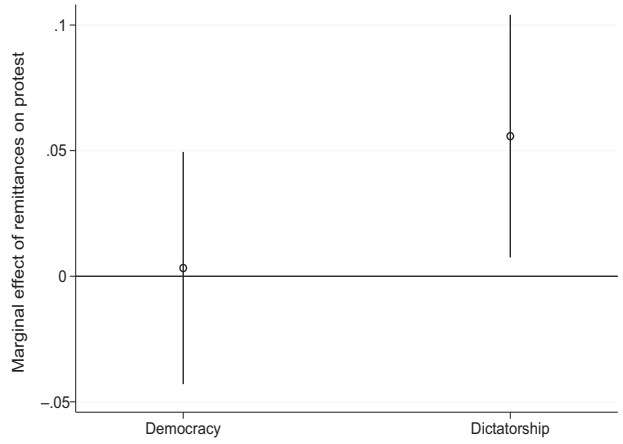
The first column of Table 1 reports results from the OLS test: The estimate for β_{Remit}—for a pooled sample of autocracies and democracies—is positive but not significant. Estimates for *Growth* (negative) and *Elections* (positive) are in the expected direction and significant. The second column adds the interaction term (*Remit* × *Autocracy*) to the specification.[9] The interaction term is positive and statistically significant. The estimate for β_{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.[10] 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*

[9]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 SI Appendix B.

[10]See SI Figure B-1 as well.

FIGURE 1 Marginal Effect of Remittances on Protest



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.[11] We find support in models that use random instead of fixed effects, with errors clustered on country instead of regime case, and when estimating heteroskedasticity- 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.

Two-Stage Least Squares Instrumental Variable (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 upward. 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 toward 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 countries 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.[12] 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:

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

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

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 the 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 β_{Remit} is negative but not significant, whereas that for $\beta_{Remit} + \beta_{Remit\times Autocracy}$ is

[11] See SI Table B-1.

[12] See SI Appendix C on instrument construction.

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 estimate likely models remittances from wealthy OECD countries and not remittances from oil-rich monarchies in the Middle East. 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, whereas remittances from migrants working in oil-rich autocracies may be sent without it.

Robustness tests in SI 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. SI 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 microfoundations 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 who 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 among 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).[13] 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

[13]In Mexico, Pfutze (2014) finds that electoral disengagement among previous regime supporters occurred in stronghold municipalities.

research has shown (Pfutze 2014), weakening clientelistic networks may only lead to political apathy among loyals rather than activating dissent. In this case, we would not observe an increase in protest among remittance recipients in stronghold areas.

To explore how remittances influence protest behavior in nondemocracies, we utilize Afrobarometer data from the 2008 survey for all countries coded as nondemocracies by Geddes, Wright, and Frantz (2014): Botswana, Burkina Faso, Mozambique, Namibia, Tanzania, Uganda, Zambia, and Zimbabwe. Burkina-Faso and Zimbabwe are among the top 10 emigrant countries in the region; they include major migration corridors, such as Burkina Faso–Côte 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 $34.5 billion, similar in size to FDI inflows ($36.5 billion; World Bank 2016). According to the 2008 Afrobarometer, in Zimbabwe and Burkina Faso, the percentage of respondents who 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 (Dionne, Inman, and Montinola 2014; Ebeke and Yogo 2013). Using Afrobarometer data, Dionne, Inman, and Montinola (2014) find that remittances correlate with protest; however, they do not explore the mechanism underlying this relationship or 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 nondemocracies report participating in a protest.[14]

The main explanatory variable is an ordered measure of frequency of remittance receipt: `Q87: How often receive remittances`.[15] Roughly 14% of respondents report receiving remittances: 14.6% of remittance receivers protest whereas 11.6% of nonreceivers 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; Ejdemyr, Kramon, and Robinson 2018; Munshi and Rozensweig 2015), 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).[16] 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.

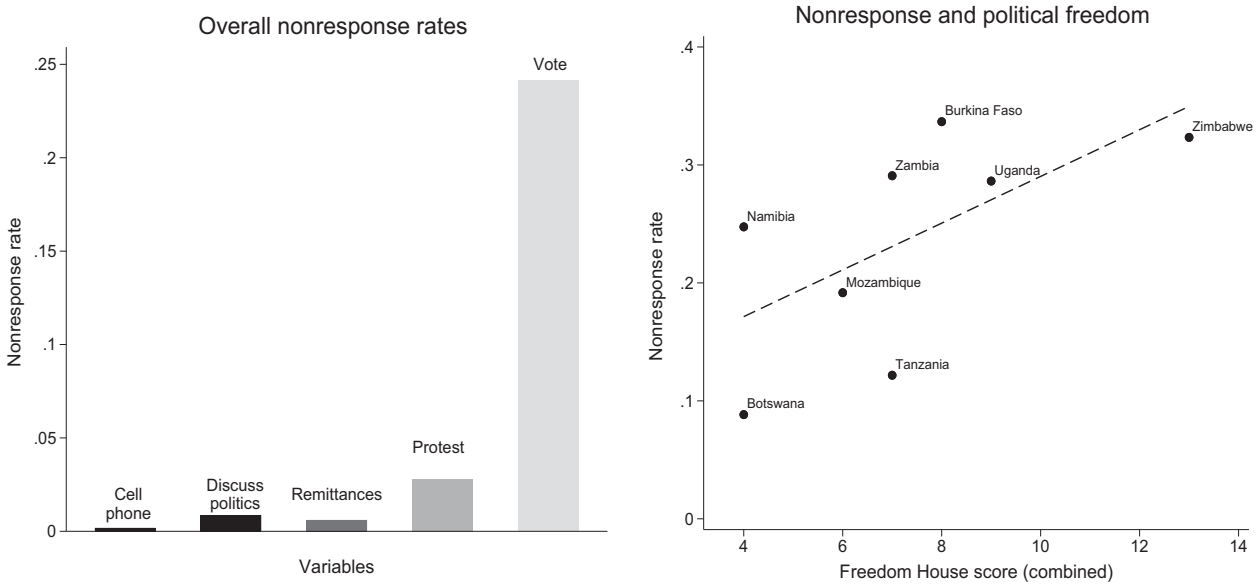
Second, using geographic location to capture clientelistic operations that underpin incumbent support circumvents inference issues that arise when relying on individual survey responses to distinguish regime supporters from opponents in political contexts where opponents face the prospect of state-led violence. Self-reported data on voting intentions are likely to suffer from nonresponse bias in nondemocratic settings because there is a threat of political violence against opponents. Indeed, a nontrivial 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.*

[14]Ordered variable tests yield similar results (SI Appendix E).

[15]There is no variable indicating the monetary value of remittances received. We also employ a binary indicator of remittance reciept 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 (SI Appendix E).

[16]Clientelistic exchange based on local public goods provision is prevalent in many parts of the world (Stokes et al. 2013).

FIGURE 2 Nonresponse Rates
***Note*: Higher Freedom House scores indicate *less* political freedom.**



The left panel of Figure 2 shows the nonresponse 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 nonresponse 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.[17]

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.[18]

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.[19] 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 nonresponse is registered by lowering the mean level of incumbent support in the district. Rather than throwing out information from nonresponses, which could bias estimates, we use this information to measure the geographic area's *lack* of incumbent support.

[17]See SI Appendix E.

[18]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" (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. SI Appendix E reports random effects models that contain all regions and districts, even those with no variation in the dependent variable.

[19]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 3 and 8% of those queried do not respond to these questions, in contrast to 24 and 38% nonresponse for other potentially politically sensitive questions. The three items have an alpha score of .69.

To account for potential confounders, we include cell phone 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 cell phone 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.

In addition to individual-level covariates ($\mathbf{X_{ij}}$), the specification includes region (or district) fixed effects (δ_j) to account for differences across regions (districts).[20] 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.[21] The identifying information is the difference between remittance receivers and nonreceivers 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.*

[20]One component of the multiplicative interaction term (*Progovernment*) is omitted from the specification because this variation is captured in the region (district) fixed effects (δ_j). We employ a conditional logit that partials out the constant and cluster errors by region (district).

[21]SI Appendix E discusses this point.

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 nonrecipients 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 progovernment 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% 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% 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 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 β_{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, whereas the estimate at high levels of progovernment support is again negative and statistically indistinguishable from zero.

The left plot of Figure 3 shows how the estimate of β_{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% 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.

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 × 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)
$\beta_{Remit} + \beta_{Remit \times Progov} \times$ 10th pctile		0.142*		0.123*
		(0.05)		(0.04)
$\beta_{Remit} + \beta_{Remit \times Progov} \times$ 90th pctile		−0.011		−0.010
		(0.05)		(0.05)
Observations	10,069	10,069	8,626	8,626
Regions/districts	185	185	469	469

Note: Year: 2008. Region (district) fixed effects are 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 are clustered on region (district). The eight nondemocracies are Botswana, Burkina Faso, Mozambique, Namibia, Tanzania, Uganda, Zambia, and Zimbabwe.

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).[22] In opposition districts (those with 18% progovernment support), the marginal effect of remittance receipt at the highest level is to increase the probability of protest by 33%, which is a large effect. In contrast, in stronghold districts (80% progovernment), the marginal effect is roughly zero.

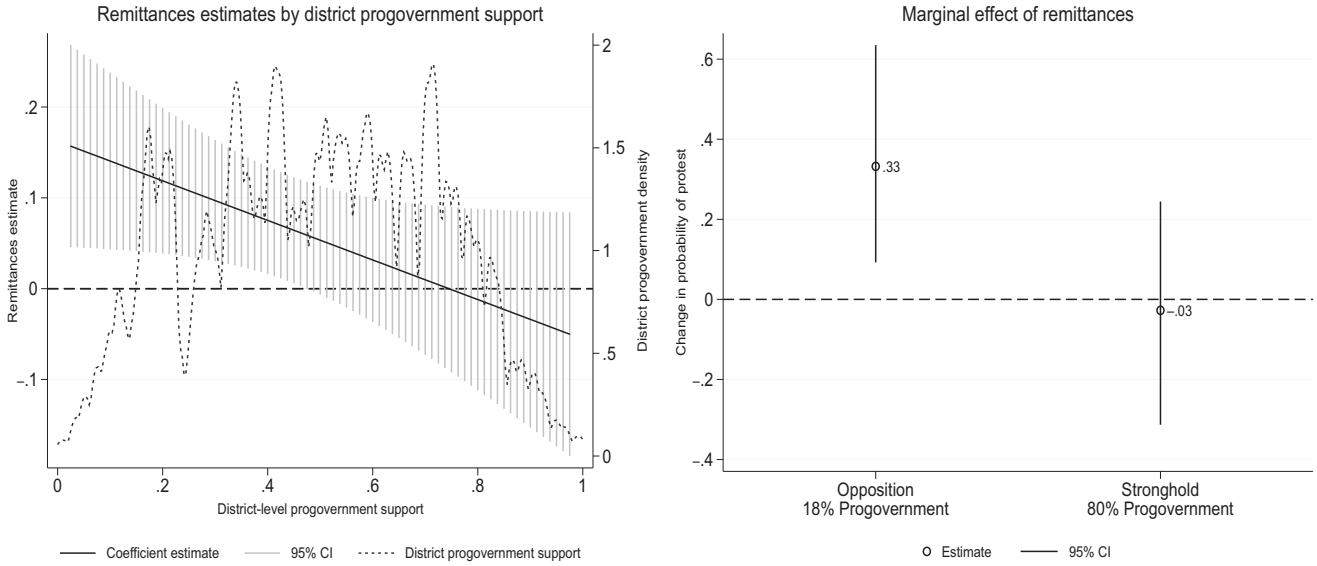
SI Appendix E discusses robustness tests: split-sample models to ensure the findings hold across a range of values for progovernment support; results from random effects models; specifications without controls and with more control variables; specifications using a binary measure of remittance receipt 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 anti-government 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

[22] Simulations were 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.

FIGURE 3 Marginal Effect on Protest Participation
***Note*: Point estimates and 95% confidence intervals are based on Table 3, columns 3 and 4.**



mechanism rather than through an exit effect. Remittances increase protest propensity in opposition districts/regions, but such an 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.

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 macrolevel tests use global data on a latent measure of anti-government protest that combines information of multiple existing data sets. Finally, we utilize individual-level survey data from eight African nondemocracies 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 article 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 is 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.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Appendix S: Sample and summary statistics
Appendix A: Latent protest data
Appendix B: Protest OLS robustness tests
Appendix C: 2SLS-IV diagnostics and robustness tests for the macro analysis
Appendix D: Multiply imputed data for the macro analysis
Appendix E: Afrobarometer data