

Investing in Voice: Remittances and Varieties of Political Participation

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Abstract

Remittances are the single most important source of foreign finance for emerging economies worldwide, yet political economists have not studied the political effects of remittances as much as they have other forms of global economic flows. I argue that remittances increase the participation of recipients in the macro-economy, changing and amplifying their political-economic interests. The best way for them to advocate these interests is not through voting, but by finding non-electoral pathways for exercising political voice. I test this theory using several different estimators, accounting for self-selection and country level institutional differences which could attenuate the personal effect of remittances. I find robust evidence in favor of my argument across specifications using Wave 7 of the Afrobarometer. My findings imply that recent work focusing on the linkages between remittances and democracy should consider the ways in which individuals can advocate their interests beyond formal electoral channels.

Keywords: *remittances, political participation, demonstrations, voice*

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1 Introduction

Transnational remittance flows – monetary transfers sent by migrants to family and friends back home – over the past half century have become one of the single most important sources of foreign finance for developing countries and the people living within them. Remittances account for upward of 20% of GDP in some states according to World Bank estimates, and are far more shock-resistant than FDI, portfolio investment flows, and foreign aid (Ratha et al., 2022). Recent data from Afrobarometer (2020) show that over a fifth of households in Africa rely on money being sent by friends and family abroad on a regular basis. In some countries included in the Afrobarometer survey, the share of households receiving remittances rises as high as 47%.¹ However, as one recent article has noted “scholars have just recently begun to explore the political consequences” of remittances (Córdova and Hiskey, 2021, p.945). How do these monetary flows impact the politics and preferences of recipients?

Other scholars have examined migrant transnational engagement and expatriate voting rights extensively (e.g. Barsbai et al., 2017; Duquette-Rury, 2014; Mazzucato and Kabki, 2009; Wellman, 2021). However, comparatively few have examined the spillover effects of remittances on the political engagement of recipients. What studies do exist in this vein typically argue that recipients are less likely to engage in political activity, because their remittance income insulates them from political mismanagement and allows them to substitute for publicly funded goods (Ahmed, 2017; Germano, 2013; Tertychnaya et al., 2018). Having no need for its services, why would remittance receiving households bother engaging the state? Similarly, in his classic text *Exit, Voice, and Loyalty*, A. O. Hirschman (1970) famously argued that members of a political community can respond to decreases in government quality in two ways: protest or migration. Within this framework, the use of the exit option (migration) precludes the exercise of the voice option (protest).

I argue that this view of the consequences of remittances on political behavior is incomplete, and that increases in financial capital should make remittance recipients *more*, not less willing to become politically engaged. Previous contributions to the domestic political consequences of transnational remittance flows primarily look at electoral measures of political engagement, such as electoral turnout rates (Germano, 2013), incumbent approval rates (Tertychnaya et al., 2018), and the likelihood of voting for incumbents (Ahmed, 2017). While

¹The Gambia

these are undoubtedly important and relevant political behaviors, there are many other ways for individuals in emerging economies to advocate their political interests. These include making direct claims on public goods from local representatives (Auerbach and Kruks-Wisner, 2020; Kruks-Wisner, 2018) as well as joining demonstrations or protests (Escribà-Folch et al., 2018; Harris and Hern, 2019). A complete view of the political effects of remittances ought to consider these non-electoral forms of political participation.

Theorists working from the new economics of labor migration (NELM) tradition emphasize that migration decisions are not made by individuals, but by larger social units such as households (see Massey et al., 1993, pp.436–440 for an overview). While one member of the household may indeed ‘exit’ the polity as Hirschman described, others often stay behind, and can still use their voice to demand increases in government quality. Drawing on work from developmental economics that emphasizes how remittances are used by recipients, I argue that households that receive them will have a greater stake in political economic affairs. Economic empowerment, through both the consumption and investment effects of remittances on households, does not only provide households the *means* of advocating their interests (as in Escribà-Folch et al., 2018), but will also change their political preferences for public policy. For example, the more individuals own, the more they are likely to oppose redistribution and progressive taxation, because they stand to lose more than they otherwise would from such policies (Doyle, 2015; López García and Maydom, 2021).² Combining these various insights, I predict that remittance income should lead to increased levels of political participation through non-electoral channels among recipients.

I subject my hypotheses to a battery of methodological tests, including fixed-effects, nearest neighbor and propensity score matching, and hierarchical mixed-effects models, using data from the seventh wave of the Afrobarometer survey. My results provide broad support for my argument: contrasted with comparable non-recipients, those who receive remittances are more likely to engage in several non-electoral forms of political action, including collective claim-making of the state, contacting local and national politicians, and joining demonstration and protest movements. These effects hold across different institutional and economic contexts, suggesting that remittances matter not only for already democratic countries, but for more authoritarian ones as well.

My theoretical and empirical contributions suggest that those who study the effects of

²But see Acevedo (2020) for an important caveat.

international economic outcomes on political behavior in emerging economies should focus not only on electoral behavior, but other forms of political participation as well. Being exposed to the global economy may have countervailing effects on disparate forms of political behavior and a complete understanding of the consequences of remittances (as well as other forms of exposure to global economics) requires that we pay attention not just to voting and incumbent approval, but also to other forms of political behavior. The findings presented here also help provide the micro-foundations of other work looking at the macro-political effects of remittances and emigration (Bastiaens and Tirone, 2019; Bearce and Park, 2019; Escribà-Folch et al., 2015, 2022; Miller and Peters, 2020; Peters and Miller, 2022). Furthermore, while some other scholars have used micro-level data to examine the effects of remittances on attitudes (Doyle, 2015; López García and Maydom, 2021), comparatively few have tested whether such attitudinal changes result in actual changes in political behavior. My paper provides such a test. Lastly, my paper extends the study of remittances and political behavior from Latin America, which has been studied extensively (Acevedo, 2023; Adida and Girod, 2011; Córdova and Hiskey, 2015, 2021; Crow and Pérez-Armendáriz, 2018; Pérez-Armendáriz and Crow, 2010), to Africa, which has received far less attention in the literature.

In the rest of the paper, I first lay out my theory, which rests on two sub-sections. First, I discuss how remittances are used by the households that receive them, then how households in emerging economies can most effectively advocate their interests. Together, these sections form the basis of my hypothesis formulation. I then move on to a discussion of my data and methods before presenting my results and reflecting on the value added of my paper for the literature on the political economy of remittances.

2 Remittances and Political Participation

Remittances are not merely private monetary transfers which exist outside of the realm of politics, although it is easy to think of them as such. Academic and policy narratives have focused overwhelmingly on remittances as “development from below,” meant to succeed where official development assistance (ODA) failed. Remittances, so the argument commonly goes, flow directly to households, thus circumventing regulatory capture and corruption. Many academics have chimed into this debate, rightfully pointing out that, while they may indeed be beneficial to recipient households, remittances are no panacea for global development, and the

optimism that has accompanied much of the remittance discourse does not adequately consider the lived experiences of migrants, transnational families, and their communities (de Haas, 2005; Kapur, 2003; Skeldon, 2008).

These debates notwithstanding, it is important to recognize that remittances fundamentally alter domestic political coalitions through both their aggregate effect on the macro-economy, and their influence on private political preferences. The resultant macro-economic effects of remittances have been studied quite extensively. For example, remittances give government some leeway in manipulating the exchange rate (Culver, 2022; Singer, 2010), increase the likelihood of central bank independence in autocracies (Garriga and Meseguer, 2019), give governments incentives to change their citizenship regimes (Leblang, 2017), and lower spending on public goods such as social welfare (Ambrosius, 2019; Doyle, 2015; Ebeke, 2012). All of these effects subsequently affect the overall state of the national economy, thus changing migratory flows, leading to further changes in the volume of remittances, and fundamentally altering a country's political economy. For better or worse, some states, such as the Philippines, have turned emigration and remittances into a veritable industry (Blank, 2011).

However, the micro-economic foundations of such macro results are not well understood. This leads to uncertain causal mechanisms: is the association between inward remittances and decreased public spending due to citizen preferences (as in Doyle, 2015), or due to elite preferences (as in Ambrosius, 2019)? If it is the former, we should see remittance recipients be more politically active than non-recipients, going out of their way to lobby for lower fiscal burdens and achieve lower public spending. If the latter, we should see recipients be less demanding of public goods, and thus be less politically active, leaving incumbent elites to use their freed up resources on other goals such as maintaining autocratic rule (Ahmed, 2012, 2019). What is at stake here is a proper understanding of the agency of remittance recipients, and whether they disengage from politics due to increased economic security (Ahmed, 2017; Germano, 2013; Tertychnaya et al., 2018), or leverage that security and invest their increased financial capital into political voice (Escribà-Folch et al., 2015, 2018, 2022).

2.1 How Remittances Are Used

I argue that remittances should make citizens *more*, not less engaged with domestic politics. Remittance recipients are more likely to spend money on real estate, healthcare, sanitation, and other long term investments than non-recipients (Adams and Cuecuecha, 2013; Airola, 2007;

Nsiah and Fayissa, 2014). Remittances can also be used for consumption goods, and greater levels of consumption equally tie recipients further into the macro-economy. Furthermore, the development economics literature provides extensive evidence to suggest that a substantial part of remittances are invested by recipient households (Airola, 2007; Nsiah and Fayissa, 2014). Indeed, Adams and Cuecuecha (2013) find that Ghanaian households receiving international remittances spend 12.35% more on durable goods, and between 3% and 5.7% more on health, education, and housing than comparable households without this additional source of income. While those numbers may seem trivial, the authors note that “it is important to note that they take place in a Sub-Saharan African country (Ghana) where household incomes are quite low and are only a fraction of household incomes in other remittance-receiving countries” (ibid, p.38). Evidence also suggests that remitted income is reinvested by households into enterprises and entrepreneurship (Kakhkarov, 2018). All such investments require substantial capital, and government mismanagement of the economy (or outright corruption) can seriously diminish the return on investment that they can expect.

Consider, by way of example, the returns to education in emerging economies. Educating one’s children takes considerable investment, and remittances often help meet these needs (Airola, 2007; Nsiah and Fayissa, 2014). However, the returns to education will only pay dividend if there are adequate jobs available for educated individuals. Cronyism, a lack of investment in sectors requiring highly skilled labor, and the neglect of attracting foreign investment could all signal that the government is unwilling to help foster an economic environment in which education is worth the investment. In such a situation, the remitted capital sunk into education is effectively lost. Households prefer to see greater returns on investment, and so remittances provide a reason for engaging in political action. Alternatively, if good jobs for high-skilled individuals are unavailable, emigration becomes the most cost-effective option for households. Indeed, education is strongly associated with both the desire to emigrate and the realization of actual emigration (Leblang and Helms, 2023). However, migration is itself a costly affair as well, and may carry risks that the household is unwilling to bear. Given the costs and risks associated with migrating, households with educated breadwinners will still prefer the availability of local well paying jobs, all other things equal.

Remittances are used not only as long-term investments, but also increase consumption and welfare for recipient households and (through the multiplier effect) their local communities. Cross-national evidence in the context of Africa suggest that remittances alleviate poverty as

well as leading to greater human capital investments (Anyanwu and Erhijakpor, 2010). When households are less likely to face material deprivation thanks to remittance flows, they are more active within the macro-economy, have more free time to advocate their political interests, and have a greater stake in public affairs as they diversify away from subsistence agriculture into broader economic exchange. Remitted income allows families to substitute for public goods, further incentivizing them to stake their claim in decision making (Adida and Girod, 2011). In doing so, remittance recipients become more exposed to the national and international political economy, increasing their stakes in political affairs. Increased income, consumption, and investment also make people net contributors to public finances, making them more emotionally involved in politics and the process of public spending, as well as the legitimacy of government projects (Busemeyer and Garritzmman, 2017; Marx and Nguyen, 2018). Indeed, one of the more consistent findings in the literature on remittances is that they change attitudes towards public finance (Acevedo, 2020; Doyle, 2015; López García and Maydom, 2021). All these factors increase their willingness to engage the state.

Furthermore, recipients, through their increased access to foreign capital, are more directly affected by political economic decisions of the state. Remittances are by necessity exchanged from foreign currency into domestic currency, and thus expose their beneficiaries to the political process of exchange rate management. Indeed, remittance senders and recipients have been shown to be attentive to changes in the exchange rate (Nekoei, 2013; Western Union, 2023; Yang, 2008). This should increase recipients' stake in political management of the macro-economy and thus their political engagement, especially in countries where exchange rates are already politically salient (Aklin et al., 2022). Remittances also promote financial inclusion and banking rates (Aga and Martinez Peria, 2014; Ajefu and Ogebe, 2019), thus exposing respondents to government intervention in the financial sector. Mismanagement of the economy can lead to banking runs, placing one's hard earned savings in jeopardy, which will make households more cognizant of the role of government in managing the economy.

In summary, remittances provide not only the *ability* to engage the state through greater economic security, but also a *reason* to engage the state given how they change the economic interests of recipients. Work in development economics suggests that remittances can be used in a variety of ways, from investment for future returns (Adams and Cuecuecha, 2013; Aga and Martinez Peria, 2014; Ajefu and Ogebe, 2019; Kakhkarov, 2018) and the provision of health care and education for the household (Airola, 2007; Nsiah and Fayissa, 2014) to immediate con-

sumption and poverty alleviation (Anyanwu and Erhijakpor, 2010). For most people, economics and welfare are fundamentally political issues, and changes in one's economic position lead to changes in their economic interests, including their preferences over redistribution and taxes (Doyle, 2015; López García and Maydom, 2021), as well as their attentiveness to political issues like the exchange rate (Nekoei, 2013; Western Union, 2023; Yang, 2008). For those who invest their remitted income, concerns likely arise over the quality of government, and the likelihood of expropriation and bad governance, given that they have more to lose than they otherwise would.

This is related to the argument made by some scholars about the pro-democratic effect of remittances (Escribà-Folch et al., 2015, 2022), who also predict that remittances give citizens more freedom to advocate for their preferred political policies. Escribà-Folch et al. (2018) argue very similarly that remittances give recipients the financial freedom to engage in protests. However, unlike these studies, I make no predictions about the preferred form of political organization of remittance recipients. Previous work has shown that remittance recipients are not necessarily more pro-democratic than non-recipients (Konte, 2016), and that downward fluctuations in remittances can cause support for such non-democratic events as military coups (Acevedo, 2023). However, it is not a necessary prerequisite for my theory that citizens hold pro-democratic attitudes. Rather, I wish to point out that remittances change preferences of recipients along more political-economy dimensions than just that of democratic governance. Furthermore, my theory incorporates more recent insights into the causes of demonstrations in developing economies: they are more likely to be about valence issues and the state of the economy, rather than demands for regime change (Harris and Hern, 2019).

Although foreign income from remittances could indeed insulate citizens from adverse income shocks due to political failure (Ahmed, 2017; Tertychnaya et al., 2018), they also increase the amount of capital that citizens stand to lose by endemic corruption or gross mismanagement of the economy by the government. As noted famously by Tversky and Kahneman (1991), individuals are more willing to accept risk when attempting to minimize losses. We can think of engaging the state as a form of risk: it incurs an immediate cost, but there is a chance one's efforts result in not losing the household's hard earned gains through remittances. At this point one might wonder why, given that engaging the state is costly, time-consuming, and does not guarantee success, recipients invest the remitted financial capital in the first place? As demonstrated above, the types of investing and consumption behavior households engage in thanks to remittances are crucial to their health and well-being. They include literal life savers

like sanitation, as well as education for one's children: an investment with immense future payoffs for the family. Even if there is a high probability of government mismanagement of the economy,³ the potential payoff of investing in durable goods makes it a worthwhile choice for households.

Preexisting studies indeed emphasize that remittance recipients invest in public and durable goods despite this giving the government an incentive to deliver poorer governance (Ahmed, 2012). By extension, engaging the state and attempting to mitigate government failure also becomes worth it due to the potential returns on investment. When households' engagement in the political economy rises through investment and consumption, it changes their material interests, as well as their capacity to advocate for those material interests. Given that direct communication with political agents allows for the sending of explicit signals (Verba et al., 1995, p.10), one should expect remittance investment to lead to increased political participation. In summary, remittances give recipients strong incentives to make demands of their political representatives.

2.2 Making Their Voice Heard

This leads to the important follow-up question of *how* people can most effectively engage the state given their interests and the institutional context within which they find themselves. While voting may spring immediately to mind as the quintessential form of political advocacy for citizens, and indeed voting has been used in previous studies of the effect of remittances on political engagement (e.g. Germano, 2013), such avenues may not be optimal for remittance recipients for several reasons. Elections take place relatively infrequently, and elected politicians and voters thus face a commitment problem: after being elected, a politician with a short time horizon faces a time inconsistency problem, and has a large incentive to deviate from their promises to voters (Alesina, 1988; Becher, 2016; Duggan and Martinelli, 2017). Given strong *ex post* incentives to renege on *ex ante* promises to citizens, elected office holders will likely choose not to follow through on said promises. Therefore, when the problems citizens face are immediate issues, the political business cycle would dictate that elections are generally a sub-optimal way to pursue

³Alternatively, one could argue that households are less likely to invest in durable goods when they know that the government is authoritarian, more likely to mismanage the economy, and therefore that they are more likely to see the value of their investments dwindle. I acknowledge this is one potential way for individuals to react to poor governance. However, if indeed this is a concern for households, then it should bias my estimates towards the null. This means that the results I showcase later in the paper should be seen as a lower bound to the "true" effect of remittances on political action. Furthermore, as shown in the appendix, I find no evidence for an interactive effect between remittances and regime type in motivating political participation.

one's interests.

The time inconsistency problem is exacerbated by informational asymmetry, which is common in all states but all the more so for emerging economies where remittances are generally more important for the financial well-being of households. The elected politician has private information about her performance which constituents often lack in developing country contexts (Pande, 2011). Indeed, the literature suggests that remittance recipients may misattribute their economic gains to incumbent politicians (Tertychnaya et al., 2018), further emphasizing the informational asymmetries that make keeping office holders accountable difficult. Politicians can fall back upon claiming that events were out of their hands due to prior commitments, exogenous economic shocks, or natural disasters. Without full information regarding the politician's actual capabilities and opportunities, citizens will not be able to judge whether an elected official is a "slacker" or not. Therefore, elections may not be the most optimal avenue for politically engaged citizens in emerging economies, which are the main destination for remittances, to advocate their interests.

Instead, I argue that remittance recipients mostly advocate their preferred preferences through non-electoral channels. In many developing and emerging economies, widespread pessimism about elections exist among the citizenry, who instead resort to "claim-making" to demand welfare and public services (Auerbach and Kruks-Wisner, 2020; Kruks-Wisner, 2018). Accountability in such local cases is much more clearly delineated, solving the information asymmetry problem. Furthermore, because local brokers have much longer time horizons, not having term limits, time inconsistency and commitment problems are also ameliorated. Citizens may also turn to demonstrations or protests as more direct ways of advancing their interests given politicians with little interest in responding to citizens outside of the political business cycle. This account is substantiated by Harris and Hern (2019), who find that demonstrations and protests in Sub-Saharan Africa are largely motivated by valence issues; cases where governments fail to provide poor service delivery and citizens use their voice to demand better results.

As noted in the American context, voting in and of itself gives politicians, both local and national, very limited information about the preferred policies of citizens (Schlozman et al., 2012, p.3). Directly contacting or petitioning political actors, meanwhile, allows people to explicitly state their concerns and demands (Verba et al., 1995, p.10), thus making it more likely that preferred policies are adopted (or opposed policies abandoned). Political participation takes time and money (Brady et al., 1995, p.273), and remittances can provide both by substituting for

wages or subsistence agriculture. While such conjectures about non-electoral participation were originally designed based on American political life, they apply to developing and emerging economies as well. If anything, the prevalence of clientelism in remittance reliant economies means that elections may be entirely irrelevant for promoting policies in line with the household's economic preferences. Previous work has shown that emigration and remittances allow households to escape clientelist ties (Pfutze, 2014), and thus may decrease electoral turnout in line with existing research findings (Germano, 2013).⁴

Thus, we should expect remittances to increase political action and participation, but mostly regarding those forms of political participation that are non-electoral. While electoral channels of influence are undoubtedly important, the influence of remittances on non-electoral forms of political participation have hardly been examined within in the literature, with only limited exception (Córdova and Hiskey, 2021; Escribà-Folch et al., 2018; Pérez-Armendáriz and Crow, 2010). My specific hypotheses are that remittances increase the recipient's penchant to discuss politics with friends and family, their willingness and capability to band together with other citizens to make demands of the government, their propensity to attend demonstrations, and their willingness to directly contact either local councilors or members of parliament. Such a diverse group of behaviors, including both individual and collective forms of participation, as well as more and less costly ones, ought to provide a broad test of the general expectations laid out in this paper. While the baseline probability of all of these behaviors are different, as the descriptive statistics presented later in the paper will show, they are all expected to be at least partially caused by the intensity of political economic interests at the household level. Therefore, remittances are hypothesized to increase each of these behaviors. Although one might expect that remittances affect these different behaviors differently, as they vary widely in cost, risk, necessity of collective action, I surprisingly end up finding a remarkably consistent effect of remittances across these various behavioral indicators, which leaves open the possibility of follow-up work on this topic, and the question of which people employ which strategies as substitutes or complementarities under which conditions. I also include an aggregate "participation index" in my models, discussed in further detail in the appendix, and expect that remittances increase it as well.

⁴I do not include voting as an outcome in the models presented here, as they follow a completely different data generation process than non-electoral forms of participation. The relationship between remittances and voting is affected by clientelism (Pfutze, 2014) as well as local economic and political conditions (Córdova and Hiskey, 2021), I find no evidence of such conditional effects for non-electoral political behavior in Africa in my appendix.

3 Data & Research Design

In this analysis, I use African survey data taken from Wave 7 of the Afrobarometer survey (Afrobarometer, 2020). Among the countries included within this survey there exists significant variation in levels of economic development, democracy, and the importance of remittances for households. Therefore, the focus on Africa makes substantive sense, allowing for cross-country comparisons of the impact of political and economic development. This wave is the most suited for this analysis, as it is the most recent one to directly ask respondents about the extent to which they are financially dependent on remittances. Wave 7 surveyed between 1,200 and 2,400 people each in 34 countries. The total number of observations is 45,823. The survey includes cross-country weighting to ensure that it is representative of the population across the continent. Individual level predictors incorporated in the analysis include whether the respondent is an urban or rural resident, their gender, their age, their level of education, their employment status, their score on a 4-point poverty index (where higher scores indicate greater lived poverty and material deprivation), whether they or a family member have migrated in the past (which includes family members currently living abroad), whether they own a mobile phone, and their command of at least one European language.

The main explanatory variable – remittances – is recoded to a dummy variable, which takes the value 1 if the respondent reports being at least sometimes dependent on remittances, 0 otherwise. The dependent variables – whether the respondent discusses politics, whether they have joined others to demand government action, whether they have joined a protest, and whether they have ever contacted their local councilor or MP – are all recoded to binary in order to facilitate the use of the logistic estimator. Ordered logistic estimators would be inadvisable here due to the likely violation of the proportional odds assumption. I also use factor analysis to predict values for a latent “propensity for political participation” variable using the other response variables. More information on the construction of this index is provided in the appendix. Descriptive statistics of all the relevant individual level predictors, dependent variables, and their cutoffs are provided in Table 1.

As shown in the matching balance tables in the appendix, remittance recipients are more likely to be urban residents, to have completed at least secondary education, to own a mobile phone, and to speak at least one European language. Unsurprisingly, they are vastly more likely to report having migrated themselves in the past, or having had a household member do so. They

Table 1: Descriptive Statistics

	Mean	Standard Deviation	Min	Max
Remittances	0.213	0.409	0	1
Urban	0.445	0.497	0	1
Male	0.500	0.500	0	1
Education	0.310	0.464	0	1
Employment	0.338	0.479	0	1
Language	0.304	0.463	0	1
Past Migration	0.252	0.432	0	1
Lived Poverty	1.210	0.911	0	4
Age	37.24	14.94	18	106
Mobile Phone	0.889	0.315	0	1
GDP/capita	5,181	4,766	1,193	22,149
Polyarchy	0.527	0.170	0.131	0.798
British colony	0.581	0.493	0	1
French colony	0.366	0.482	0	1
Discuss	0.621	0.486	0	1
Request	0.239	0.427	0	1
Demonstrate	0.103	0.308	0	1
Contact Local	0.230	0.417	0	1
Contact MP	0.121	0.322	0	1
Participation Index	0.007	0.733	-0.634	2.443

Education has secondary education as cut-off point. Language indicates mastery of European language. GDP per capita is normalized with purchasing power parity, and logged in the statistical models. Cross-country survey weights are applied only to the means.

Sources: Afrobarometer Wave 7, World Bank WDI, V-Dem v13, COLDAT.

are only slightly less likely to be employed (although this could be either because unemployment causes migration, or because remittances substitute for wages), and only score slightly lower on the Lived Poverty Index. These results are in line with the general consensus of migration scholarship, which argues that those who migrate (and by extension the families that receive remittances) are usually neither the poorest of society (as they generally lack the resources to emigrate) nor the most affluent (as they generally lack the aspiration to emigrate) (de Haas, 2021; Leblang and Helms, 2023).

Country level predictors for all the models are sourced from the World Bank World Development Indicators for the economic variables (World Bank, 2023), and the Varieties of Democracy project for the political variables (Coppedge et al., 2023). Data on historical colonial legacies comes from COLDAT (Becker, 2019). Because not all countries in Wave 7 of the Afrobarometer were surveyed in the same year, I manually add relevant predictors based on the year the survey was conducted in a given country, using the available data for that country

in that year. The replication data for this exercise will be made available on Dataverse upon acceptance of this manuscript.

3.1 Baseline Results

I start by estimating several baseline models. These include MLE-estimated logit and OLS regression estimations, both including country level fixed effects, cross-country survey weights, and several control variables at the individual level as listed in Table 1. The results of these models, the response variables for which are those from Table 1, are presented in Table 2. The country-level fixed effects coefficients are omitted for space. Across the specifications, remittances increase non-electoral political participation, the results are statistically significant, and in a practical sense comparable to more obvious predictors of participation. For example, the practical effect of remittances on joining a protest is comparable to the effects of gender and education, and the coefficient of remittances for the index variable, estimated via OLS, is comparable to the effect of education and urban residence, conditional on the other control variables in the model. While the coefficient is not as comparable for each of the response variables, some being estimated by logit and others by OLS, the effect is consistently as expected.

Coefficients from the logistic estimator can be notoriously difficult to interpret substantively. To ameliorate this issue, I present predicted probability plots below to illustrate the effect of remittances on the probability of engaging in several forms of political participation in Figure 1. In these plots, all other covariates are held at their mean values. While the minima and maxima in each of the subgraphs are different, their ranges and scales are the same. The index model has been omitted to facilitate comparison. In general, the marginal effect plots indicate that receiving remittances leads to about a three to four percentage point increase in the predicted probability of engaging in non-electoral forms of political participation, keeping other covariates at their means.

Interestingly, the linear effect of remittances on political action is more or less constant between models, even for those outcomes where the base probability is much lower. For example, individuals who do not receive remittances have a predicted probability of about 61.5% to discuss politics, and those who do have a predicted probability of about 66%. Colloquially speaking, this makes those who receive remittances only marginally more likely to discuss politics with their peers. On the other hand, individuals who do not receive remittances have a predicted probability of about 8% to join demonstrations, whereas those who do have a predicted probability of about

Table 2: The Effect of Remittances on Political Behavior

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.196*** (4.22)	0.227*** (6.54)	0.360*** (6.00)	0.241*** (4.78)	0.309*** (4.87)	0.114*** (6.96)
Urban	-0.0208 (-0.51)	-0.425*** (-9.48)	0.173** (3.01)	-0.428*** (-9.02)	-0.338*** (-6.84)	-0.120*** (-10.15)
Male	0.610*** (13.64)	0.484*** (12.95)	0.411*** (11.02)	0.563*** (9.79)	0.505*** (9.63)	0.214*** (15.08)
Education	0.614*** (10.88)	0.183*** (4.85)	0.295*** (4.33)	0.265*** (4.79)	0.267*** (4.62)	0.124*** (7.63)
Employment	0.235*** (6.33)	0.180*** (4.39)	0.150* (2.46)	0.217*** (4.94)	0.246*** (4.93)	0.0840*** (6.32)
Language	0.138 (1.31)	-0.0746 (-1.63)	0.214* (2.14)	-0.109 (-1.69)	0.0252 (0.32)	-0.00290 (-0.13)
Lived Poverty	0.0730** (3.49)	0.202*** (7.35)	0.208*** (5.52)	0.142*** (4.70)	0.140*** (4.21)	0.0658*** (6.85)
Past Migration	0.173*** (4.47)	0.293*** (6.46)	0.312*** (7.16)	0.313*** (6.79)	0.383*** (6.50)	0.132*** (8.22)
Age	-0.00109 (-0.85)	0.00848*** (6.59)	-0.0116*** (-5.46)	0.0161*** (12.45)	0.0145*** (8.06)	0.00384*** (7.91)
Mobile Phone	0.368*** (5.06)	0.211*** (5.52)	0.140 (1.61)	0.430*** (9.07)	0.279* (2.69)	0.126*** (7.84)
Country FEs	YES	YES	YES	YES	YES	YES
<i>N</i>	43,869	43,671	43,276	43,753	43,698	42,616

Fixed Effects Logit/regression models. Based on Afrobarometer Wave 7. Test statistics in parentheses.

Includes cross-country sample weighting.

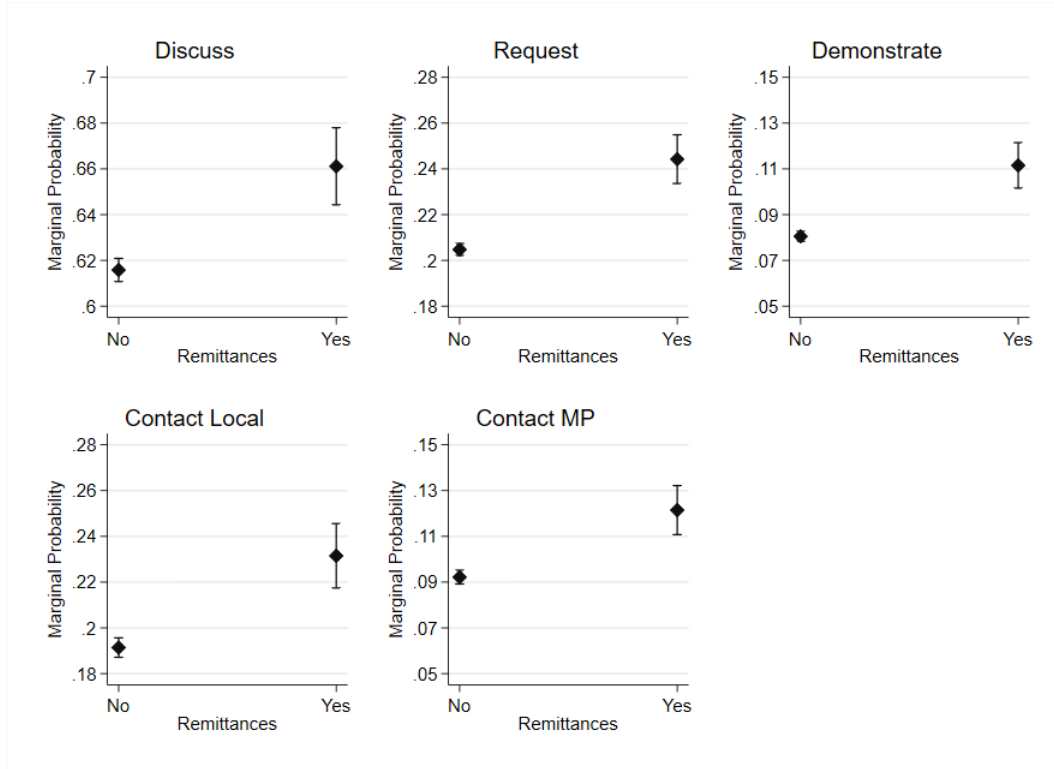
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

11%. Here, a smaller percentage point increase makes respondents much more likely to give the response of interest, because the base outcome is less probable. Substantively, it seems like remittances matter comparatively more for stronger forms of participation than for weaker ones. It is clear from Figure 1 that the practical significance of remittances for political participation is particularly pronounced for rarer and more extreme realizations of participation.

3.2 Methodological Pitfalls

Although I include country-level fixed effects, one could still be concerned about two potential sources of confounding: self-selection of households into migration (and therefore remittance

Figure 1: Predicted Probability Plots



recipience) and heterogeneous treatment effects across national contexts. In order to address likely concerns about non-random selection of family members into migration – and therefore omitted variable bias in any regression using remittances as an explanatory variable – I use propensity score and nearest neighbor matching to build a more similar comparison pool for recipients and non-recipients. I use propensity score matching on whether the respondent is an urban resident, their employment status, their level of education, their gender, a host of variables associated with their country (purchasing power GDP per capita, Polyarchy scores, and colonial heritage), whether they speak English, Portuguese or French, their lived poverty score, mobile phone ownership and whether they or a family member have migrated in the past. This last variable also captures whether any household member is currently living abroad. The nearest neighbor matching algorithm uses exact matching on country (effectively the same as having country fixed effects in a regression model), and weighted matching on urban residency, gender, employment, education, language, poverty, mobile phone ownership and prior emigration.

Note that including emigration in the models provides a very strong control against omitted variable bias. From a research design perspective, the concern is that emigration (and therefore remittances) and political participation have a prior common cause. Whatever such a cause may be, it is likely to be the source of prior emigration of household members. Thus,

controlling for household migration should account for a large amount of variation that could be chalked up to omitted variable bias. Most importantly, including this variable accounts for the concerns of households with the quality of politics and public service provision, which we know to be a major cause of both emigration and participation and claim-making (Hirschman, 1970). What remains are potential confounders that enter the data-generation process post-emigration, but prior to receiving remittances. Considering emigration often takes place with the express aim of generating remitted income, the claim towards internal validity of the research design is considerably strengthened by controlling for prior emigration. While a research design that relies on selection on observables is unable to fully account for all potential unobservable factors, inclusion of broad material concerns (the lived poverty and unemployment variables) and cross-country differences in affluence (through exact matching on country and propensity score matching on purchasing power GDP per capita) should increase our belief that the design will yield valid estimates of causal effects.

In order to get at the problem of heterogeneous treatment effects, I use mixed level models (sometimes called hierarchical or nested models in the literature) that allow the slopes of the regression coefficients to vary across countries. Other scholars have shown that the political effects of remittances are conditional on national context (Tyburski, 2014), and therefore seriously investigating this option is an important step in the research design. The advantage of such a model is that we can examine the effect of remittances on varieties of political participation by country, while making use of the efficiency that the large sample size of the pooled data provides the researcher. Estimating a mixed level model also accounts for the potential violation of the i.i.d.⁵ assumption necessary for estimating generalized linear models if one were to estimate a standard logistic regression based on the pooled data regardless of cross-country differences: one cannot assume that the error distribution of observations from one country is similar to that of those from another. A mixed level model goes beyond fixed effects model in addressing this issue by allowing the slope of the regression coefficient to vary across the levels in which the individual observations are nested.

3.3 Matching Models

I now move to my matching models, the aim of which is to improve the claim to causality that my argument posits. One potential inferential problem of linear models, such as those presented

⁵Independently and identically distributed residuals.

Table 3: Accounting for Self-Selection with Matching

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
ATE (PSM)						
Remittances	0.0429*** (6.18)	0.0399*** (6.26)	0.0494*** (10.01)	0.0452*** (7.11)	0.0341*** (7.00)	0.121*** (10.68)
ATE (NNM)						
Remittances	0.0423*** (5.58)	0.0418*** (5.96)	0.0352*** (7.45)	0.0493*** (6.95)	0.0369*** (6.59)	0.130*** (10.60)
<i>N</i>	43,869	43,671	43,276	43,753	43,698	42,616

Propensity Score Matching: Based on Afrobarometer V7. *t* statistics in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

in Table 2, is that they inappropriately compare incomparable observations and extrapolate from extreme outlier observations. By contrast, matching models select counterfactual control observations for the treated units so as to maximize their comparability. In order to interpret these effects as causal, we have to make the assumption that assignment to treatment (remittances) is as good as random after matching on observed covariates, i.e. that there is conditional independence of treatment and control. Furthermore, the propensity score models require a sizable overlap in propensity scores between treatment and control. Table 3 shows the results of the matching models. Matching balance tables and propensity score overlap plots are provided in the online appendix, and generally support the interpretation that the matching procedure improved the quality of the counterfactual non-remittance recipient cases greatly, especially as it regards the effect of prior household emigration. The common support plots suggest no violations of the common support assumption, even absent any trimming in the tails of the propensity score distributions. Unlike the fixed effect models, the matching models do not allow for the incorporation of survey weights. This means that respondents from countries that are over-sampled relative to their population size vis-à-vis the rest of the continent will have an outsized effect on the coefficients.

As in Table 2, Table 3 shows strong support for my expectations: remittances increase all forms of non-electoral participation. The nearest neighbor models have less variation to exploit, owing to their exact matching on country, but still have coefficients and standard errors comparable to the less restrictive propensity score matching models. Since treatment and control observations are nonparametrically selected for comparison, the causal identification assumptions are less stringent than those for standard linear models (Morgan and Harding, 2006). Additional checks on the validity of the matching estimator results – balance tables and overlap plots – are provided

in the appendix.

3.4 Mixed Effect Models

Despite the inclusion of country-fixed effects in the previous models, one potential concern that remains regarding the preceding results is the role of heterogeneous treatment effects. Fixed effects change the intercept of observations per country, but do not incorporate the potential for heterogeneous treatment effects, which could paint an incomplete picture. Do the effects of remittances on participation vary on a country-to-country basis? In order to address this question, I utilize multi-level mixed effect models. This approach allows for the estimation of a separate country-level slope variance parameter. The results from the mixed effect models are presented in Table 4. As with the matching models, for each of the 7 dependent variables the effect of remittances is statistically significant with p -values below 0.001. The sign of the non-electoral forms of participation is positive, as expected. Survey weights are not included in these models.

In order to aid statistical power, I only allow the slope of the remittance coefficient to vary (the intercept varies across countries by default). The random intercept and slope effects should be interpreted not as normal coefficients, but rather as estimates of the cross-country variance of the parameters. Being variances, these effects are always positive, and larger values indicate that the effect of remittances varies greatly by country, whereas smaller values indicate that the effects are very similar across countries. Thus, one can interpret the random slope effects only in light of the slope of the relevant variable. The size of these effects suggest that, while the effect of remittances varies by country in size, nowhere is the random slope effect large enough to suggest that there are some countries where the slope of the remittance coefficient is negative, rather than positive. These results are somewhat surprising given previous findings in the literature, which emphasize the effects of remittances conditional on local political and economic conditions (Bastiaens and Tirone, 2019; Córdova and Hiskey, 2021; Crow and Pérez-Armendáriz, 2018; Tyburski, 2014). I expand upon how these findings relate to my prior expectations in the appendix, where I also show interactions of remittances with potentially mediating factors identified by the existing literature, including regime type and levels of economic development (Bastiaens and Tirone, 2019; Córdova and Hiskey, 2021). Surprisingly, the results largely show null-effects.

Table 4: Accounting for Effect Heterogeneity with Random Slope Effects

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.211*** (5.08)	0.232*** (6.20)	0.355*** (6.12)	0.264*** (5.73)	0.333*** (5.71)	0.120*** (7.75)
Urban	-0.00789 (-0.34)	-0.433*** (-16.56)	0.122*** (3.41)	-0.416*** (-15.55)	-0.297*** (-8.69)	-0.118*** (-15.66)
Male	0.608*** (28.89)	0.488*** (20.33)	0.403*** (12.10)	0.547*** (22.23)	0.469*** (14.68)	0.211*** (30.34)
Education	0.603*** (22.62)	0.187*** (6.34)	0.301*** (7.76)	0.252*** (8.43)	0.265*** (7.05)	0.124*** (14.46)
Employment	0.237*** (9.99)	0.170*** (6.46)	0.132*** (3.69)	0.225*** (8.43)	0.263*** (7.75)	0.0864*** (11.23)
Language	0.121*** (3.77)	-0.0474 (-1.37)	0.236*** (5.10)	-0.0748* (-2.09)	0.0919* (2.06)	0.0106 (1.03)
Lived Poverty	0.0741*** (5.69)	0.206*** (14.32)	0.197*** (9.95)	0.148*** (9.96)	0.148*** (7.82)	0.0677*** (15.80)
Past Migration	0.166*** (6.58)	0.280*** (10.27)	0.309*** (8.60)	0.306*** (11.06)	0.383*** (11.10)	0.131*** (16.08)
Age	-0.000779 (-1.09)	0.00892*** (11.20)	-0.0102*** (-8.60)	0.0159*** (19.95)	0.0150*** (14.71)	0.00404*** (17.10)
Mobile Phone	0.364*** (10.61)	0.245*** (6.11)	0.158* (2.42)	0.410*** (9.45)	0.295*** (5.25)	0.128*** (10.92)
Log GDP	-0.129 (-1.49)	-0.257** (-2.79)	0.292** (2.76)	-0.0760 (-0.98)	-0.0552 (-0.66)	-0.0283 (-1.50)
Polyarchy	0.379 (1.00)	-0.0237 (-0.06)	0.507 (1.08)	0.498 (1.42)	0.0482 (0.13)	0.0772 (0.93)
British	0.211 (1.29)	0.293 (1.68)	-0.304 (-1.52)	0.242 (1.57)	0.0118 (0.07)	0.0416 (1.16)
French	0.145 (0.91)	-0.242 (-1.43)	0.0325 (0.17)	-0.297* (-2.00)	-0.604*** (-3.74)	-0.0981** (-2.82)
Constant	0.116 (0.17)	-0.304 (-0.43)	-5.476*** (-6.69)	-2.496*** (-4.26)	-2.977*** (-4.71)	-0.326* (-2.24)
Random Intercept	0.128*** (3.98)	0.144*** (3.92)	0.187*** (3.85)	0.113*** (4.37)	0.129*** (3.91)	0.0058*** (3.81)
Random Slope Remittances	0.0325* (2.43)	0.0158 (1.68)	0.0551* (2.20)	0.0491** (2.81)	0.0700** (2.61)	0.0054** (2.98)
<i>N</i>	44,006	43,805	43,404	43,888	43,831	42,738

Mixed Effects Logit/regression models. Based on Afrobarometer Wave 7. Test statistics in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.5 Alternative Explanations

One alternative explanation for the results presented here is that of social and political remittances: perhaps it is the transference of ideas from (potentially) democratic regimes abroad which activates remittance recipients (Barsbai et al., 2017; Córdova and Hiskey, 2015; Maydom, 2017), rather than their changed economic interests. Political remittances are likely strongly correlated with financial remittances, making this a potential alternative explanation, which the large-N quantitative results presented here are unable to parse out. However, preexisting evidence in the African context supports the economic remittances theory over the political remittances theory. First and foremost, Konte (2016) finds that remittances do not necessarily increase pro-democratic attitudes among remittance recipients. If the political remittances theory were sufficient in accounting for all variation explained in my models, one would expect that recipients have more favorable evaluations of democracy than non-recipients, yet this is not borne out by the existing data.

I also estimate several interactive models, provided in the appendix, where remittances and preferences over democracy are interacted and the response variables are the same as in my baseline models. The interactive effect is statistically significant and positive for three out of the five non-electoral forms of behavior, indicating that remittances are more likely to activate those individuals with pro-democratic preferences. Importantly, however, the baseline results remain statistically significant and positive, indicating that remittances also make households without pro-democratic attitudes more politically active. Second, existing research on protest movements in Africa by Harris and Hern (2019) shows that the majority of protests are not about regime type, but about economic policy. These observations, combined with the robust association between remittances and protest movements in my results, provides further evidence that political participation rises because of economic interests, not just ideological preferences.

4 Conclusion

As opposed to the existing literature, which focuses on how remittances supposedly cause recipients to become detached from politics (Ahmed, 2017; Tertychnaya et al., 2018), I have given evidence that suggests that remittances increase the propensity of citizens to engage in non-electoral political behavior. The behaviors examined here included discussing political affairs, joining others to petition the government, joining demonstrations, and directly contacting local

and even national politicians. Various model specifications incorporating potential inferential concerns – including the non-random selection of observations into the treatment condition and heterogeneous treatment effects – all provide robust evidence for this position. The findings presented here suggest that political economists and scholars of political and economic development should pay greater attention to both the behavioral and attitudinal changes instilled in citizens through the receipt of remittances, in terms of their social (Barsbai et al., 2017; Maydom, 2017) as well as their economic effects (Acevedo, 2023).

The implications of this research for studies linking migration, remittances, and politics are potentially profound. Recent years have seen the emergence of a research agenda looking at the divergent pressures facing authoritarian governments in developing economies as it regards their emigration policies (Miller and Peters, 2020; Peters and Miller, 2022). These studies argue that governments choose to promote emigration when the prospective consequences are mostly economic (increases in remittances) and restrict it when the prospective consequences are mostly political (citizens being exposed to pro-democratic norms abroad). The results presented in this paper provide a potential complication for this compelling argument: even if the consequences of emigration are primarily economic, it could still lead to greater levels of protest by changing the material interests and capabilities of recipients. Not all demonstrations are pro-democratic, however, and regimes that are capable at responding to such pressures may still be able and willing to use emigration policy as a tool of attracting foreign finance and having a “safety valve” for citizens dissatisfied with the political status quo.

Some cautionary notes are nevertheless appropriate. The results presented here seemingly provide extra reason for the “remittance optimism” that has captured the development community over the past two decades (de Haas, 2005; Kapur, 2003; Skeldon, 2008). Certainly, remittances empowering households not only economically, but indirectly politically as well, is reason to praise them as a bottom-up tool for development. But it must be noted that emigration is costly, and not necessarily available to all members of society. The poorest citizens and denizens of emerging economies may have the aspiration to emigrate, but not the capacity to do so (de Haas, 2021; Leblang and Helms, 2023). Thus, while remittances could increase the political voice of households who can afford migration, researchers and practitioners should not lose track of those to whom this path is not available.

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Appendices

A Constructing the Participation Index

One can conceptualize an individual's propensity to engage the state to protect their invested remittances as a latent variable. It is unobserved (and unobservable), but it does have observable implications: the behavior of individuals. Within the latent variable framework, one can use the observed indicators, which are assumed to be caused by a common factor (the unobserved latent variable), to predict likely values of the unobserved variable to be further used in statistical analyses. I use the first five response variables as observed indicators of the propensity to engage the state in non-electoral forms of political action. Factor loadings and Uniquenesses of the factor analysis model are provided below.

Factor Analysis of Participation Index

	Factor Loadings	Uniqueness
Discussing Politics	0.2461	0.9394
Joining with Others to Request Government Action	0.4650	0.7838
Joining Demonstration	0.3227	0.8959
Contacting Local Counselor	0.5592	0.6873
Contacting Member of Parliament	0.5208	0.7288

B Balance Tables, Common Support & Discussion

As the tables show, the main differences between treated (remittance receiving) and untreated (not remittance receiving) respondents are whether they reside in an urban or rural area (recipients are more likely to be urban), whether they have completed at least secondary education (recipients are generally better educated), and whether they speak at least one European language adequately (recipients are more likely to do so). All of these variables likely affect an individual's expected utility from migration, and so their strong imbalance across treated and untreated observations in the raw sample should be of little surprise. Educated individuals and those who speak at least one European language have a job market advantage post-migration compared to their compatriots who lack these skills. Furthermore, *ex ante* migration costs are lower for those who are urban residents. The strongest imbalance is, as one might expect, a history of emigration among the members of the household. The imbalance for this variable is more than twice as large as any

of the other variables in every single model. However, post-matching, these differences become negligible.

Across nearly all specifications and all variables, balance is improved in the matched sample compared to the raw sample. There are a handful of models where at least one variable is less balanced, but these are generally small differences. Furthermore, the nearest neighbor matching (NNM) models have no such cases, and only one variable ever has a larger absolute normalized difference than 0.025 in these models (the Lived Poverty Index). Because the NNM models also have exact matching on the respondent's country of residence (meaning that the matched absolute difference on any country-level covariates will be constrained to zero), and these models provide strong evidence for my hypotheses in Table 3, we can safely assume that the differences in the propensity score matching (PSM) models do not meaningfully bias the estimates, given the standard assumptions of the matching approach discussed in the methodology section of the paper.

Lastly, matching estimators work best when there is overlap in the propensity scores for units in the treatment condition and units in the control condition. In other words, there should be appropriate counterfactual units for the treated units among the observations assigned to control. When this assumption is violated, extraordinary explanatory power is placed on only a handful of outlier counterfactuals (units with a high probability being assigned to treatment, but getting control, or vice versa). The overlap plot provided below (after the matching balance tables) strongly support the assumption that there is adequate common support in the propensity scores.

Discussing Politics – Propensity Score

	Raw	Matched
Urban	0.133	0.015
Male	0.008	0.012
Secondary Education	0.156	0.008
Employed	-0.092	0.012
European Language	0.118	0.011
Lived Poverty	-0.069	-0.014
Prior Migration	0.450	0.001
Age	-0.038	0.004
Mobile Phone	0.116	0.006
Log GDP / Capita PPP	-0.053	0.035
V-Dem Score	-0.005	0.019
British Colony	-0.014	-0.028
French Colony	-0.043	0.010

Joining with Others to Request Government Action – Propensity Score

	Raw	Matched
Urban	0.131	0.014
Male	0.009	0.006
Secondary Education	0.157	0.014
Employed	-0.093	0.003
European Language	0.118	0.014
Lived Poverty	-0.071	-0.019
Prior Migration	0.451	0.000
Age	-0.038	-0.011
Mobile Phone	0.118	0.001
Log GDP / Capita PPP	-0.050	0.026
V-Dem Score	-0.005	0.010
British Colony	-0.014	-0.016
French Colony	-0.042	0.019

Joining Demonstration – Propensity Score

	Raw	Matched
Urban	0.131	0.004
Male	0.009	0.015
Secondary Education	0.159	0.006
Employed	-0.091	-0.007
European Language	0.121	0.000
Lived Poverty	-0.069	-0.017
Prior Migration	0.450	-0.001
Age	-0.041	-0.001
Mobile Phone	0.118	0.000
Log GDP / Capita PPP	-0.052	0.033
V-Dem Score	-0.005	0.021
British Colony	-0.011	-0.006
French Colony	-0.044	-0.007

Contact Local Counselor – Propensity Score

	Raw	Matched
Urban	0.133	0.012
Male	0.008	0.002
Secondary Education	0.157	0.004
Employed	-0.091	0.000
European Language	0.120	0.001
Lived Poverty	-0.070	-0.004
Prior Migration	0.448	0.002
Age	-0.038	0.002
Mobile Phone	0.115	-0.004
Log GDP / Capita PPP	-0.050	0.027
V-Dem Score	-0.004	0.018
British Colony	-0.013	-0.022
French Colony	-0.042	0.010

Contact Member of Parliament – Propensity Score

	Raw	Matched
Urban	0.134	0.013
Male	0.009	0.012
Secondary Education	0.158	-0.003
Employed	-0.090	-0.004
European Language	0.121	-0.004
Lived Poverty	-0.070	0.005
Prior Migration	0.449	0.003
Age	-0.040	0.000
Mobile Phone	0.117	0.006
Log GDP / Capita PPP	-0.050	0.027
V-Dem Score	-0.005	0.004
British Colony	-0.012	-0.027
French Colony	-0.043	0.014

Participation Index – Propensity Score

	Raw	Matched
Urban	0.130	0.008
Male	0.011	0.005
Secondary Education	0.160	-0.004
Employed	-0.092	-0.005
European Language	0.123	-0.004
Lived Poverty	-0.070	-0.007
Prior Migration	0.448	0.004
Age	-0.042	0.006
Mobile Phone	0.117	0.012
Log GDP / Capita PPP	-0.051	0.039
V-Dem Score	-0.002	0.022
British Colony	-0.011	-0.009
French Colony	-0.044	0.014

Discussing Politics – Nearest Neighbor

	Raw	Matched
Urban	0.133	0.001
Male	0.008	-0.008
Secondary Education	0.156	0.000
Employed	-0.092	-0.020
European Language	0.118	0.001
Lived Poverty	-0.070	-0.041
Prior Migration	0.450	0.001
Age	-0.038	-0.038
Mobile Phone	0.116	0.006

Joining with Others to Request Government Action – Nearest Neighbor

	Raw	Matched
Urban	0.131	0.001
Male	0.009	-0.009
Secondary Education	0.157	0.000
Employed	-0.093	-0.020
European Language	0.118	0.001
Lived Poverty	-0.071	-0.042
Prior Migration	0.451	0.001
Age	-0.038	-0.038
Mobile Phone	0.118	0.006

Joining Demonstration – Nearest Neighbor

	Raw	Matched
Urban	0.131	0.000
Male	0.009	-0.008
Secondary Education	0.159	0.000
Employed	-0.091	-0.020
European Language	0.121	0.001
Lived Poverty	-0.069	-0.043
Prior Migration	0.450	0.001
Age	-0.041	-0.041
Mobile Phone	0.118	0.006

Contact Local Counselor – Nearest Neighbor

	Raw	Matched
Urban	0.133	0.001
Male	0.008	-0.008
Secondary Education	0.157	0.000
Employed	-0.091	-0.020
European Language	0.120	0.002
Lived Poverty	-0.070	-0.041
Prior Migration	0.448	0.001
Age	-0.038	-0.038
Mobile Phone	0.116	0.006

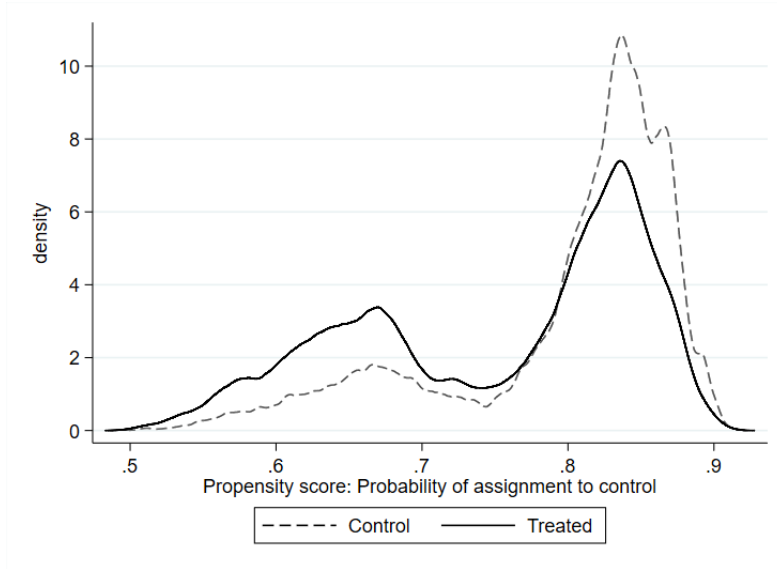
Contact Member of Parliament – Nearest Neighbor

	Raw	Matched
Urban	0.134	0.001
Male	0.009	-0.008
Secondary Education	0.158	0.000
Employed	-0.090	-0.020
European Language	0.121	0.002
Lived Poverty	-0.070	0.042
Prior Migration	0.449	0.001
Age	-0.040	-0.038
Mobile Phone	0.117	0.006

Participation Index – Nearest Neighbor

	Raw	Matched
Urban	0.130	0.000
Male	0.011	-0.008
Secondary Education	0.160	0.000
Employed	-0.092	-0.020
European Language	0.123	0.002
Lived Poverty	-0.070	-0.043
Prior Migration	0.448	0.001
Age	-0.042	-0.040
Mobile Phone	0.117	0.006

Propensity Score Overlap Plot



C Sequential Causal Models: Assessing Phantom Counterfactuals

When a causal model is sequential post-treatment, researchers ought to take into consideration the potential for “phantom counterfactuals” (Slough, 2023). This paper presents such a model, as post-treatment (the household receives remittances or not) the household can then invest their newly realized wealth or not, and subsequently chooses to participate in political life or not. Phantom counterfactuals occur when part of the game tree becomes unidentified due to truncation of choices for agents. The canonical example is voter registration and voting. After administration of treatment and control, some people will register to vote, and some will not. Then, among those who registered, some will vote and some will not. However, those who did not register to vote cannot vote (legally), thus truncating the data in a fashion similar to post-treatment mortality in bio-statistics. Under this condition, the full counterfactual of the control condition is unidentified, and an average treatment effect (ATE) on electoral turnout cannot be estimated. However, if post-treatment decision trees are symmetrical, the ATE can be estimated under the standard assumptions of causal identification.

Is the proposed decision tree symmetrical post treatment or not? When families receive remittances they can choose to invest them in durable goods such as sanitation or education, or not. Then, those who did invest them can choose to engage the state or not. Those who did not invest them can **also** choose to engage the state or not: the decision set is not truncated in either path of the game tree, even though the probability of participation goes down in one part of the tree but not another. This is, after all, what the theory predicts. Therefore, under the

standard causal identification assumptions of the selection on observables design (as discussed in the Data & Research Design section), the results presented in the main paper can indeed be interpreted as valid causal estimates unaffected by the problem of phantom counterfactuals.

D Assessing Cross-Country Random Slopes

One advantage of mixed effect models is the possibility of estimating random slope effects across nests (in this case countries), as done in Table 4. Subsequently, the researcher can attempt to explain this variation in slopes across countries by using interactive effects. Given previous findings in the literature (Tyburski, 2014), I had expected to similarly find remittances matter more in some circumstances than others. My prior expectations included the following hypotheses. First, remittances matter more for Muslim households due to the uniqueness of the *hawala* system, and the social expectations that come along with it. Second, remittances matter more in less democratic countries, because these are the most likely to be clientelist and remittances help recipients escape clientelism (Pfutze, 2014). Third, remittances matter more for poorer households and in poorer countries, as they are more likely to make a significant impact on a household's capacity to invest in durable goods, following the theory in the paper.

As the tables below show, I find no consistent evidence for the relevance of any of these variables. There are, however, some individual interactive effects which are significant both statistically and substantively. Higher scores on the Lived Poverty variable (indicating more poverty) seemingly raise the marginal effects of receiving remittances: while this is in line with my expectations, the results are only significant for two out of the seven models. Generally, the effects do not confirm my expectations, and their inconsistency combined with the relative small random slope estimate of Table 4 should be cause for caution in the interpretation of what few interactions are significant. In conclusion, future studies should address why remittances matter more for motivating political participation in some countries than others.

Mixed Level Interactive Model: Islam

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.165*** (3.52)	0.254*** (5.71)	0.0225 (0.46)	0.288*** (5.42)	0.332*** (5.05)	0.124*** (7.04)
Muslim	0.104** (2.93)	0.212*** (5.49)	-0.0297 (-0.79)	0.0552 (1.33)	0.0936 (1.75)	0.0513*** (4.45)
Remittances x Muslim	0.134 (1.92)	-0.0610 (-0.90)	0.0190 (0.27)	-0.0731 (-0.93)	0.00769 (0.08)	-0.0118 (-0.50)
Full Controls	YES	YES	YES	YES	YES	YES
<i>N</i>	43,869	43,671	44,037	43,753	43,698	42,616

t statistics in parentheses: control variables are the same as those shown in Table 4.

Mixed level interactive model: Based on Afrobarometer 7, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Mixed Level Interactive Model: Democracy (Polyarchy)

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.241 (1.84)	0.114 (0.99)	0.0430 (0.33)	0.121 (0.78)	0.185 (1.01)	0.0774 (1.59)
Polyarchy	0.384 (1.01)	-0.0587 (-0.14)	0.607 (1.28)	0.446 (1.27)	-0.00544 (-0.01)	0.0718 (0.86)
Remittances x Polyarchy	-0.0571 (-0.24)	0.228 (1.08)	-0.0260 (-0.11)	0.266 (0.98)	0.277 (0.85)	0.0808 (0.92)
Full Controls	YES	YES	YES	YES	YES	YES
<i>N</i>	43,869	43,671	44,037	43,753	43,698	42,616

t statistics in parentheses: control variables are the same as those shown in Table 4.

Mixed level interactive model: Based on Afrobarometer 7, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Mixed Level Interactive Model: Democracy (Personal Perception)

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.172*** (3.56)	0.232*** (4.78)	0.0452 (0.86)	0.251*** (4.42)	0.240*** (3.37)	0.106*** (5.87)
Perceived Democracy	-0.0436 (-1.70)	0.0738* (2.52)	-0.108*** (-4.01)	0.129*** (4.27)	0.0745 (1.90)	0.0206* (2.41)
Remittances x Perceived Democracy	0.0816 (1.49)	0.00949 (0.16)	-0.0393 (-0.70)	0.0450 (0.75)	0.168* (2.29)	0.0347 (1.95)
Full Controls	YES	YES	YES	YES	YES	YES
<i>N</i>	41,556	41,383	41,667	41,443	41,401	40,519

t statistics in parentheses: control variables are the same as those shown in Table 4.

Mixed level interactive model: Based on Afrobarometer 7, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Mixed Level Interactive Model: Lived Poverty

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.214*** (3.91)	0.224*** (4.11)	0.0591 (1.07)	0.192** (3.16)	0.339*** (4.46)	0.0860*** (4.45)
Lived Poverty	0.0746*** (5.17)	0.205*** (12.62)	0.125*** (8.20)	0.133*** (7.94)	0.149*** (6.86)	0.0614*** (12.86)
Remittances x Lived Poverty	-0.00262 (-0.09)	0.00637 (0.20)	-0.0261 (-0.84)	0.0603 (1.84)	-0.00492 (-0.12)	0.0291** (2.95)
Full Controls	YES	YES	YES	YES	YES	YES
<i>N</i>	43,869	43,671	44,037	43,753	43,698	42,616

t statistics in parentheses: control variables are the same as those shown in Table 4.

Mixed level interactive model: Based on Afrobarometer 7, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Mixed Level Interactive Model: National Affluence

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.709 (1.66)	-0.306 (-0.80)	-0.419 (-0.96)	1.052* (2.11)	-0.175 (-0.30)	0.227 (1.40)
Log GDP	-0.124 (-1.44)	-0.267** (-2.89)	-0.192 (-1.78)	-0.0535 (-0.65)	-0.0738 (-0.84)	-0.0275 (-1.45)
Remittances x Log GDP	-0.0607 (-1.17)	0.0662 (1.42)	0.0544 (1.04)	-0.0965 (-1.60)	0.0615 (0.88)	-0.0130 (-0.66)
Full Controls	YES	YES	YES	YES	YES	YES
<i>N</i>	43,869	43,671	44,037	43,753	43,698	42,616

t statistics in parentheses: control variables are the same as those shown in Table 4.

Mixed level interactive model: Based on Afrobarometer 7, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

E Alternative Explanation: Political Remittances

It is very likely that political remittances, the political ideas and norms that circulate globally between diasporas and communities origin, are correlated with financial remittances. Given that I argue that it is financial remittances, not political remittances, which matter for my theory, this is an important alternative explanation to test. If political remittances were a sufficient explanation for the entirety of the results presented here, then we should see increased levels of political participation among remittance recipients only for those respondents who also prefer democracy over other regime types. Thus, when interacting remittances with pro-democratic attitudes, if the political remittances theory provides a correct and sufficient explanation, the slope of remittances should not be significant and the interaction should be positive and significant. If the financial remittances theory provides a correct and sufficient explanation, the slope should be significant and positive and the interaction should not be significant. I expect both theories to matter and neither to be individually sufficient, meaning that both the baseline slope and the interaction should be statistically significant and positive. Based on Afrobarometer Wave 7, this is exactly what I find. These results provide evidence that both the economic effect of financial remittances and the normative effect of political remittances matter for recipient households in Africa.

FE Interactive Model: Pro-Democratic Attitudes

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index
Remittances	0.173*** (3.74)	0.172*** (5.19)	0.229*** (3.64)	0.244*** (4.17)	0.249*** (3.91)	0.0946*** (5.37)
Pro-Democratic Attitudes	-0.348*** (-9.13)	-0.115** (-3.17)	-0.0247 (-0.35)	-0.139* (-2.64)	-0.0653 (-1.25)	-0.0539*** (-3.78)
Remittances x Pro-Dem Attitudes	0.101 (1.72)	0.214*** (3.65)	0.371*** (4.92)	-0.00203 (-0.02)	0.185* (2.04)	0.0697** (3.09)
Full Controls	YES	YES	YES	YES	YES	YES
Country FEs	YES	YES	YES	YES	YES	YES
N	42,065	41,897	41,557	41,966	41,924	41,004

Fixed Effects Logit/regression models. Based on Afrobarometer Wave 7. Test statistics in parentheses.

Includes cross-country sample weighting.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$