Emigration, Welfare, and Politics in Central and Eastern Europe

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Abstract

How does large scale emigration affect politics in the peripheral states of the European Union? While a large literature looks at the political consequences of immigration in the more affluent member states, comparatively few scholars have taken up the mantle of examining the political effects of large-scale emigration in Central and Eastern Europe. From a political economy perspective, high levels of emigration, which is concentrated in the younger, more progressive parts of society, changes the makeup of both the labor force and the electorate. This article investigates how emigration and its associated economic and political consequences affect policy making and politics in CEE countries. Evidence is provided from a difference-in-difference estimator and supplementary analyses of government and individual level data. The findings suggest that EU membership leads to fundamental demographic changes, which affect the dominant forms of programmatic competition, and that governments reacting to this extend their stay in government.

1 Introduction

The European Union (EU) fundamentally changed the economic and political trajectories of its new member states when they joined between 2004 and 2013. Access to the Common Market enabled economic growth in the new member states, while the Copenhagen Criteria

led them to pursue institutional reform. Among the most important changes for these countries were the consequences of intra-European migration flows (Head and Mayer 2021; Marques 2010; Rojas-Romagosa and Bollen 2018). While individual citizens benefited from mobility and access to high-wage labor markets, the free movement of people has had a less positive aggregate effect on new member states. Estimates suggest that some states lost a quarter of their entire population to emigration in the immediate post-accession period (Kyriazi and Visconti 2023, p.6). Given these magnitudes, it should be no surprise that citizens living in the new member states generally rank emigration as a greater concern than immigration (Kyriazi et al. 2023, p.564). How has large scale emigration affected the political economy of the new member states of the EU?

I contribute to answering this question by looking at the effect of emigration on political economy in Central and Eastern Europe (CEE), providing robust evidence of a causal link between joining the EU and changes in the political demography of the new member states. Furthermore, I extend the existing literature by showing that changes in welfare attitudes and expenditures have increased government tenure length. My argument is that the freedom of movement provided by membership in the EU lowers the cost of exiting the polity, therefore making migration a comparatively more attractive strategy for individuals who might otherwise decide to challenge the incumbent government through the formal political process (Auer and Schaub 2024; Hirschman 1970; Kelemen 2020; Peters and Miller 2022; Sellars 2019). My main research contribution is showing that these demographic changes lead to the formation of a new winning coalition based on the elderly dependent parts of the population, which are easily placated by targeted welfare spending. Large scale emigration can thus be leveraged by incumbent elites to strengthen their grip on power (Miller and Peters 2020; Portes 2010).

Part of this argument has been acknowledged by previous contributions to the literature on European politics and political economy. While demographic changes and aging populations are hardly unique to CEE (Western and Southern European and East Asian countries have faced similar shifts), what is of particular importance in this case is that these shifts are fundamentally political in origin through the role of emigration (Bruzelius 2021; Hirschman 1970; Lim 2023; Roos 2023). Furthermore, this shift has profound consequences for democratic competition in these countries (Auer and Schaub

2024). While EU membership was seen for a long time as a positive for democratization and good governance reform, these contributions, as well as my own, problematize this truism. While previous studies have made a link between emigration and decreasing levels of democratic competition in CEE (e.g. Kelemen 2020), this article adds causal estimates of the effect EU membership on changing demographics and public expenditures to move the discussion forward.

Leveraging the staggered accession to the EU of the new member states over the first two decades of the twenty-first century and using a difference-in-differences design that accounts for variation in treatment timing (Callaway and Sant'Anna 2021), I show that accession to the Common Market has led to notable demographic changes in new member states. Specifically, I find that the elderly dependency ratio – the number of pensioners relative to the size of the active labor force – increased significantly for countries that joined the EU, but not for comparable countries that did not join. More importantly, these demographic shifts have subsequently changed the programmatic politics and expenditures of CEE governments. Because non-random emigration changes the public's preferences over redistribution (Petrova and Sznajder Lee 2023), incumbent governments have a strong incentive to retool the dominant types of redistribution.

After establishing that accession to the Single Market fundamentally changes labor markets, political demography, and targeted welfare spending, I next show that EU members from CEE countries that increase spending on the elderly dependent parts of the population stay in office longer. I do so using government level data for Czechia, Estonia, Hungary, Latvia, Lithuania, Poland, Slovakia, and Slovenia between 2003 and 2019, taken from the Comparative Political Data Set (Armingeon et al. 2023*ab*), using a negative binomial model to estimate their expected tenure length. The results provide support for the argument that welfare expenditure targeted at the elderly increases government survival, suggesting governments that react strategically to changed electorates are rewarded. I further substantiate my argument about government support by the elderly parts of the population using individual-level data for these same countries from the European Social Survey.

This study thus addresses an important gap in the literature recently identified by scholars of European integration and politics. It contributes to our understanding of the political economic consequences of emigration in the European Union (Auer and Schaub

2024; Bruzelius 2021; Dancygier et al. 2024; Kyriazi and Visconti 2023; Kyriazi et al. 2023; Lim 2023; Roos 2023). The paper also speaks to the relationship between European and global economic integration and redistribution (Bremer and Bürgisser 2023; Bruzelius 2021; Margalit 2011; Rodrik 1997; Scharpf 1997). While redistribution can be a purely benevolent way to compensate the losers of integration, its political implications can also be targeted to particular components of society to the interest of political elites. Despite well documented opposition to immigration in countries like Hungary and Poland, most citizens from these countries consider emigration a greater societal problem than immigration (Kyriazi et al. 2023, p.564). Therefore, providing a better understanding of how emigration affects the politics of welfare in these countries is important (Bruzelius 2021; Roos 2023). My findings suggest that breaking the illiberal coalition comes down to a greater engagement in politics of the younger, more highly educated, and more progressive parts of society, a suggestion that is anecdotally buttressed by the 2023 Polish election that unseated the PiS, where turnout among young voters increased by 18% points.

While the motivation of the paper regards the new CEE members of the EU, the theoretical framework is broadly applicable to other high emigration countries with aging populations. Any government could, in principle, react to changed electorates due to emigration in strategic ways to entrench their position (Miller and Peters 2020; Portes 2010), regardless of whether they are the new or old member states of the EU, or indeed any country. The choice to look at CEE is therefore mostly one of convenience, as the accession of the new member states makes for a strong research design to provide an initial test of the hypothesis (Auer and Schaub 2024). Future research should unpack how these dynamics work out in other countries around the world.

2 The Single Market and Emigration

The most famous and widely cited study of the nexus between the quality of government and emigration comes from A.O. Hirschman (1970), who argued that members of a political community can respond to decreases in government quality in two ways. They can "voice" their dissatisfaction through protesting, demonstrating, petitioning politicians, or, in democracies, voting. Alternatively, they can "exit" the polity altogether, which in the

context of states would mean emigration. A third option, "loyalty," implies that individuals accept the status quo as more desirable than either outside option. Whether any given individual opts for exit, voice, or loyalty depends on the expected costs and benefits of these three strategies relative to one another. That is to say, what are the relative opportunity costs of one's outside options? As the cost of exercising voice rises, for example due to greater government repression, loyalty and exit become more likely. Conversely, if the cost of exit is lowered due to emigration becoming less costly by stripping away visa and work permit requirements abroad, exit becomes the more likely option compared to loyalty and voice.

The EU's freedom of movement principle greatly affects the cost of migration for citizens who consider exiting their country of origin. Under this principle, citizens of EU countries have the right to settle and work in any other EU member state without obtaining a visa or work permit. For those countries that are part of the Schengen Treaty, they are furthermore able to move across national borders without any sort of document check, further lowering the costs of exit. Indeed, it is well documented in the econometric literature that freedom of movement increased migration between both the old and new member states of the Union, even for those countries that did not join the Schengen area until later (Head and Mayer 2021; Rojas-Romagosa and Bollen 2018). This has had great ramifications, especially for those states where more people left. Estimates suggest that Bulgaria and the Baltic countries lost between 16% and 26% of their population in the immediate post-accession period, the majority of which was concentrated in the working age population (Kyriazi and Visconti 2023). Estimates for the region as a whole suggest that 9% of the total CEE population migrated away permanently since 2004 (Auer and Schaub 2024). Since those who leave are expected to be net contributors to the government's budget, this put enormous pressure on labor markets and public finances.

In turn, employers have lobbied CEE government to implement policies that incentivize younger people to stay behind and start families, rather than move abroad in search of higher wages and better public goods (Petrova and Sznajder Lee 2023). Such policies fit with the "traditional family values" which conservative parties in CEE countries have championed for some time. However, it is unclear if, or to what extent, such policies were successful. The majority of people migrating from Eastern member states to the more

affluent EU-15 countries are younger than their compatriots, and tend to hold more progressive attitudes (Lim 2023). It is not at all obvious that explicitly conservative policies would cause those individuals to stay, given their values. As a result, and as I will show empirically, EU membership has led to a "greying" of the population in CEE member states. As younger people emigrated, the elderly stayed behind, changing the interests of the median voter over matters of redistribution. This has occurred because both the makeup of the electorate and the attitudes of those who remained behind have changed (Lim 2023).

2.1 Emigration, Social Spending and Electoral Competition

The demographic changes brought about by joining the Single Market lead to subsequent changes in the labor market, which in turn affect fiscal politics and incentives for reelection seeking incumbent governments. It is well documented that those who leave the country tend to be higher educated and younger (Lim 2023), as well as more progressive and supportive of liberal values (Auer and Schaub 2024). Due to their education and their age, this group of people is expected to contribute strongly to the tax base, providing finances for the government's coffers, and their loss puts governments under pressure to find either different sources of income, or cut into existing programs. By contrast, those who are least likely to exit the country through migration are the elderly and the infirm, who are net recipients of public finances. Demographic changes brought about by high levels of emigration and aging populations put significant strain on government budgets. As the Common Market facilitates emigration by lowering the cost of exit, I expect it to change a country's demography, increasing the proportion of pensioners relative to the labor force, which is known in demographic research as the dependency ratio.

Hypothesis. *Compared to countries that do not join the Single Market, countries that join develop a higher elderly dependency ratio.*

While joining the European Union gives CEE countries access to several different funds – most notably the five European Structural and Investment Funds (ESIFs) – these cannot be full substitutes for the loss of taxation income. These funds are earmarked in very specific ways, determined by the European institutions, and any attempt to use money from, for example, the Common Agricultural Policy to make up for changes in public finance are

impossible. Out of the five ESIFs, the European Social Fund Plus would be most in line with the incentives faced by net emigration countries, but its goal is specifically to boost employment, not the well-being of the retired. Ultimately, and most importantly, we simply do not empirically observe a relation between use of the European Social Fund and public investment (Staehr and Urke 2022). In line with these observations, European states faced with fiscal constraints have increasingly prioritized some forms of social spending over others in the post-Great Recession period (Bremer and Bürgisser 2023). If European funds could substitute for other sources of public finance, such decisions would be unnecessary.

Therefore, CEE countries with dwindling labor forces and aging populations are faced with a need to comprehensively reform their pension and social security systems in order to keep public finances sustainable. While pension reforms are partially due to international peer dynamics and social emulation, domestic political considerations and financial pressures remain relevant (Brooks 2005; Hennessy and Steinwand 2014; Verbič and Spruk 2019). Interestingly, however, emigration creates divergent economic and political pressures on the social security system of a country. On the one hand, emigration is concentrated among younger, more highly educated individuals with better paying jobs (Lim 2023). As they leave the domestic labor market, governments lose out on substantial income tax revenues, which puts pressure on the public budget and could necessitate fiscal consolidation. In the post-Recession European context, such pressures are all the more acute, as the EU has pushed for austerity and "fiscal responsibility" in order to avoid further debt crises (Bremer and Bürgisser 2023). CEE governments face the simultaneous pressures of aging populations, fewer tax paying citizens in the labor force, and top-down political constraints from the EU not to run large budget deficits. Therefore, emigration creates an economic incentive to keep social security spending sustainable by reforming or lowering pensions.

On the other hand, as the share of pensioners in the population increases, so does their political power. Old-age benefits are pure club goods, meaning that they are directly targeted to a particular part of the population, which enjoys its benefits while excluding other groups in society (Knutsen and Rasmussen 2018). This makes them an ideal way to "buy off" the loyalty of a particular part of the electorate. The existing literature emphasizes the political origins of pension schemes, and their importance in causing citizen beneficiaries

to provide support for incumbent regimes (Grünewald 2021*ab*; Knutsen and Rasmussen 2018; Mares 2005). Previous research has established that emigration changes both the makeup and preferences of the domestic electorate (Lim 2023). With many younger, more progressive citizens choosing to exit the polity through emigration, the marginal returns to exercising voice for those who remain behind increases, as there are fewer alternative demands to compete with them, and fewer voices who make mutually exclusive demands of the central government. The core insight here is that emigrants are not a random sample of the population economically, and thus in their political-economic demands. As a result, governments have a significant incentive to shift public expenditure to placate those who stay behind more, resulting in greater health and pension expenditures.

Of course, expatriates from CEE countries are not excluded from home country elections, and so they could remain a relevant part of the winning coalition for reelection oriented governments. However, while emigrants remain technically part of the electorate, expatriate voting turnout remains significantly lower in CEE countries than resident citizen turnout. The smallest gap in turnout among CEE EU states can be observed in Slovenia, and even there nationwide voter turnout is more than double that of external voter turnout. In the most extreme cases, such as Hungary, Lithuania, Poland, and Romania, overall turnout rates are stable above 50%, while external voter turnout stands between 0% and 5% (Kostelka 2017, p.1074). A rational government concerned with finding the minimally costly winning coalition for reelection will therefore focus targeted expenditures on non-migrant citizens, rather than expatriates. In summary, emigration changes not only local labor markets, but local electorates as well. Since the electorate that stays behind skews more towards pensioners and the elderly, incumbent governments who are motivated by reelection will have to spend resources on this crucial part of the electorate if they want to stay in power.

European welfare state politics have increasingly revolved around cross-class and value-based coalitions, rather than traditional class structures (Gingrich and Häusermann 2015; Häusermann 2006). Furthermore, competition over the welfare statement has evolved to be more about trade-offs between different benefits, rather than the overall size of the welfare state (Bremer and Bürgisser 2023; Häusermann, Kurer and Traber 2019). These changes in the dominant structure of party competition have problematized the traditional

mode of welfare politics, where left-wing parties have typically advocated for greater public expenditures, and right-wing parties have advocated for greater fiscal austerity. Häusermann, Kurer and Traber (2019) find that far right voters are among the strongest opponents of pension cutbacks, suggesting that the cleavage structure of pension and welfare politics has fundamentally changed.

Prior to 2011, many CEE countries, both those that ended up joining the EU and those that did not, enacted sweeping privatization reforms in the public sector, regardless of the ideology of the governing parties (Appel and Orenstein 2013). Some authors have interpreted the rise of populism in CEE as a reaction to this neoliberal consensus among established centre-left and -right parties (Orenstein and Bugarič 2022), as populist regimes in the region have reversed many (though not all) of the pre-crisis cuts, for example through lowering the retirement age and offering drug benefits to the elderly (Ost 2018, p.115). The choice to offer policies such as these depends greatly on the incentives faced by incumbent governments. My argument identifies one such incentive: the demographic makeup of the electorate in a context of high emigration.

As noted by Bremer and Bürgisser (2023, p.35), "social policies create their own support coalitions because citizens' narrow self-interest shapes their social policy priorities: people react strongly to cuts in spending from which they benefit". More so than many other political economy questions, distributive decisions about where to spend finite resources of social expenditure hit people directly in their personal financial circumstances. These are precisely the situations where people are most likely to mobilize politically around economic issues (Tilley, Neundorf and Hobolt 2018). For most pensioners in CEE countries, public pension and retirement schemes remain the bulk of their livelihoods. Therefore, they are likely to react strongly to any change in government policy regarding pension schemes and government funded healthcare. This puts governments in a bind, as they need to balance pressures to maintain long-term sustainable social security programs without running budget deficits (the economic or fiscal sustainability dimension), and maintain public support for such policies (the political sustainability dimension). Given changes in the electorate, the cuts are more likely to fall upon other expenditures than those benefiting pensioners and the elderly. In sum, emigration creates a dilemma where policy makers must choose between the economic and political sustainability of their social safety nets and

pension schemes (Vučković and Škuflić 2021). I expect the political incentive to dominate the economic incentive due to changes in the effective electorate brought about by large scale emigration.

Hypothesis. *Compared to countries that do not join the Single Market, countries that join will see increased public spending on social policies that benefit the elderly, such as pensions and healthcare.*

I expect this strategy to be an effective one to retain political power. While Klomp and de Haan (2013, p.261) find limited evidence for the existence of political business cycles among some new EU member states (Bulgaria, Cyprus, Czechia, and Romania), they find no evidence for aggregate election induced fiscal expenditures in Estonia, Hungary, Lithuania, Poland or Slovakia. However, their main outcome variable is average government spending on health, education, and social security as share of GDP. Incumbents are strategic actors that seek to maximize reelection by rewarding key supporters, but neglect parts of the electorate outside of the minimum winning coalition (Bueno de Mesquita et al. 2002). After all, resources not spent on guaranteeing reelection are resources that the regime can consume for its own enjoyment. Thus, aggregate social spending could hide differential dynamics at play through different spending categories (for example, spending on education aids mostly younger individuals, while spending on health and pension benefits aids older individuals). Furthermore, their data covers the period of 1975–2005. Given that some of these countries were not yet part of the Common Market when their panel ends, an extension for these countries would be appropriate.

The final link in the causal chain of my theory pertains to the consequences of increased welfare spending. In line with the political business cycle and political budget cycle literature (Brender and Drazen 2005; Castro and Martins 2018; Lohmann 1998; Nordhaus 1975; Potrafke 2020; Schultz 1995), I argue that increased spending on programs that benefit the elderly and pensioners will lead to lower rates of government turnover conditional on EU membership. This is because non-migrant elderly citizens benefit greatly from increased health and pension expenditures, and reward the incumbent government for their improved economic position (Tilley, Neundorf and Hobolt 2018). This occurs irrespective of the long term fiscal sustainability of such policies, as voters have short time

horizons. Therefore, I expect electorates of new member states to reward governments that target welfare spending at the elderly and pensioners, which have become more important components of the electorate due to the freedom of movement principle.

Hypothesis. *Among new EU member states in Central and Eastern Europe, compared to governments that do not increase public health and pension expenditure, countries that do increase such expenditures will stay in office longer.*

3 Difference-in-Differences Analysis

In order to test my hypotheses, I collect data from the *World Development Indicators* (World Bank 2023), the advantage of which is that they cover both members and non-members of the EU. I look at CEE countries that join the EU between 2004 (the first former Communist cohort) and 2013 (Croatia, the last country to join the EU at time of writing), as well as other CEE countries that are at various stages of applying for EU membership during the time-frame, but never actually join the Single Market. The combined data covers 11 treated countries and 11 control countries, with observations spanning the time frame of 2001 through 2019.The existence of a comparison pool that never enters treatment is a prerequisite of using the staggered treatment adoption design with panel data (Callaway and Sant'Anna 2021). While it could be argued that there are differences between these countries irrespective of EU member status during the study's time-frame, such concerns are not expected to affect the analysis for two reasons.

First, my pool of never treated countries includes several countries that are considered for EU membership during the time-frame. These countries have pursued structural economic and political reforms similar to the EU members, with the one crucial difference being that they never join the Single Market. This makes my counterfactual pool of observations more appropriate than if I were to use a panel of the entire European neighborhood. Second, the identifying assumption of the difference-in-differences estimator is not that there are no baseline differences between treated and untreated units. Rather, the identifying assumption of difference-in-differences is the parallel trends assumption, which states that observations would have had the same trends over time in the absence of treatment. This could be violated due to either pre-treatment differences in trends, or due to compound

treatment effects. I test for the former, and find no evidence for it in any of the comparisons (as shown the pre-treatment leads effects in Figures 1 and 2), except for the 2013 Cohort, which consists of only a single country: Croatia. The latter is by definition untestable, but the literature provides no reason to expect EU membership to have any effect on my outcomes of interest, except through its freedom of movement principle. An overview of all countries by treatment status is provided in Table 1.

Table 1: Country Cases

May 2004 Cohort	Never Treated
Czechia	Albania
Estonia	Armenia
Hungary	Azerbaijan
Latvia	Bosnia-Herzegovina
Lithuania	Georgia
Poland	Kosovo
Slovakia	Moldova
Slovenia	Montenegro
January 2007 Cohort	North Macedonia
Bulgaria	Serbia
Romania	Ukraine
July 2013 Cohort	
Croatia	

3.1 Results

The effect of EU membership on dependency ratios for CEE countries using the Callaway and Sant'Anna estimator (2021) is shown in Figure 1. The estimand, which is shown on the y-axis, is the average treatment effect for the treated (ATT). The graphs clearly show that, post-accession, the relative size of the elderly dependent population to the active labor pool increases by several percentage points. Effects are strongest for the 2004 and 2007 cohorts, and the parallel trends assumption is not satisfied for the 2013 cohort. Considering only a single observation makes up this group (Croatia), this is not wholly surprising, and the other two cohorts still provide ample evidence for the premise that EU membership and the Common Market negatively affected the labor pool size of new members relative to the pension dependent part of the population.

How have these demographic changes impacted public expenditures? Figure 2a shows that governments have increased spending on healthcare, benefiting more elderly

Figure 1: EU membership and dependency ratios for CEE countries
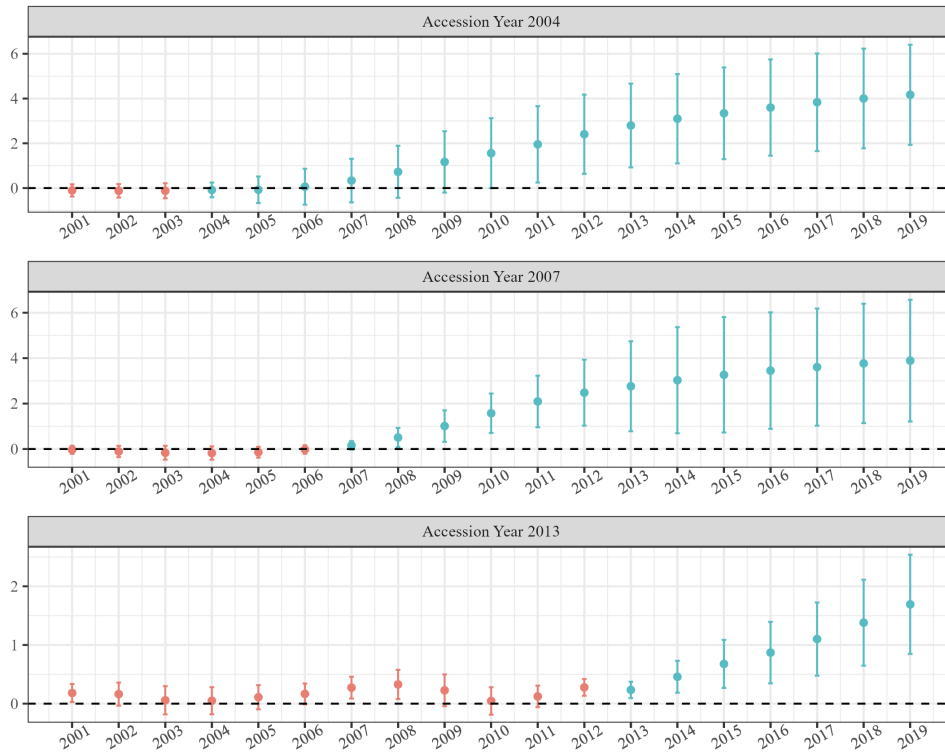
parts of the population. Another public welfare expenditure that benefits the elderly disproportionally is of course pension and retirement funds. However, data coverage for this category is extremely lacking for countries in the control group especially. Therefore, I focus on health expenditure in my analysis. Health expenditure remains a valid component of social expenditure to include here, because the elderly are still a disproportionate beneficiary of healthcare spending, even if it is not fully a club good. Future studies should try to get better data on pension expenditures in the European periphery to facilitate further research.

Government healthcare expenditure is normalized per capita and purchasing power in these models. Effects for the 2004 and 2007 cohorts are clear, while the effect for Croatia is inconclusive due to the violation of the parallel trends assumption. One concern could be that Figure 2a is indicative of an increased wealth enabling greater spending, rather than a targeted form of redistribution in the political business cycle. If this were indeed the case, we should see not only government, but private health expenditure increase as well, as individuals have more money to spend on their own healthcare. Figure 2b shows the effect of EU membership on private healthcare expenditure. Here, the difference-in-differences

estimator shows no significant effect of EU membership; if anything the point estimate suggests near null effects, indicating that the increase in government expenditure observed in Figure 2a is due to political decisions, rather than general economic circumstances. All results are robust to the incorporation of anticipation effects up to two years (in general, accession negotiations end two years before actual accession) as shown in the Online appendix.

Figure 2: EU membership and health expenditure

(a) Government health expenditure (b) Private health expenditure
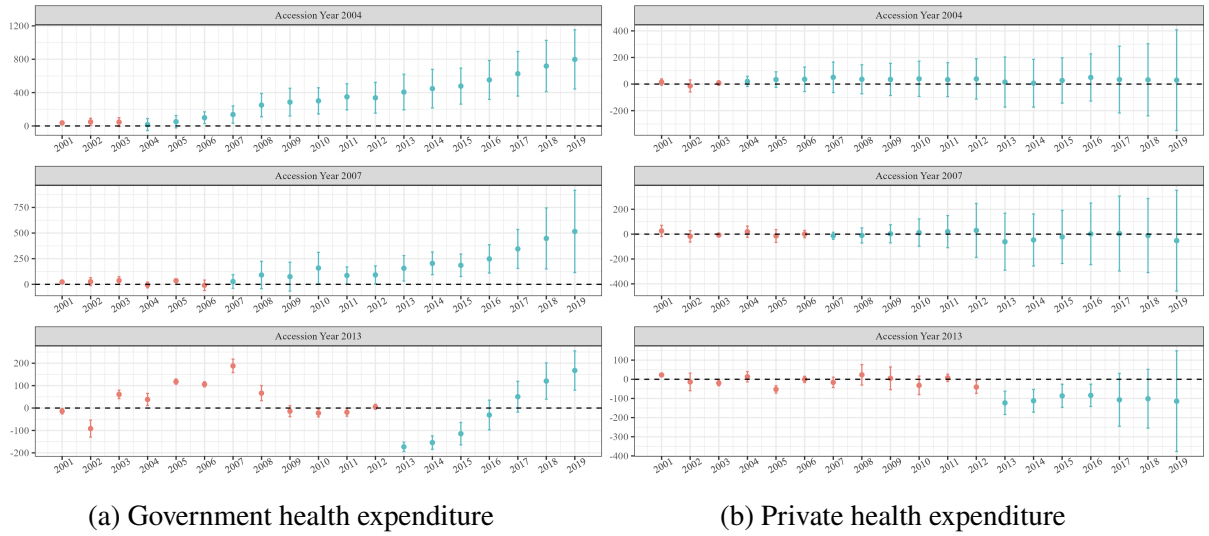
4 Government Tenure Analysis

Having shown that joining the EU leads to fundamental changes in a country's political demography and welfare expenditures, the next logical question would be to ask whether these expenditures are associated with a government's political success. In order to test Hypothesis 2.1, I estimate several Cox semi-parametric proportional hazards model, using data from the Comparative Political Data Set (Armingeon et al. 2023*ab*) and World Bank. Using a survival model in this context is more sensible than directly relying on binary re-election indicators, as a survival model allows for the estimation of the risk of losing office at any point in the time series, rather than just at the point of election. This allows us to capture both elections, as well as alternative forms of losing office, such as popular protests. As election timing is often endogenous in the electoral systems of the countries under consideration, this is an important strength of survival models in this case. Furthermore,

the effect of increased government expenditures may vary over the time of incumbency, being stronger later in the life cycle of incumbent governments (as election comes closer) and weaker earlier in its tenure. Survival models allow such over time differences to be visualized, and as Figure 3 shows, this is exactly what the paper finds.[1]

I use only the countries that join the EU in 2004 and are included in Table 1, and the unit of analysis is the government. This results in a medium-*N* 56 observations in my data. 2003 is the starting point of the analysis, as this includes governments who are incumbent while their country is in the EU. 2019 is taken as the end point to predate the COVID epidemic, and exclude any effects it may have had on welfare expenditure and political survival of incumbent governments. The choice of countries is due to limitations in the coverage of the Comparative Political Data Set. In my models, the outcome of interest is the number of days that a government stays in office, and the main explanatory variable is the government's average per capita health expenditure over its tenure (by purchasing power parity as in Figure 2 and logged in the statistical models). As additional controls I include the old age dependency ratio, net migration, the government's ideology (coded 1 if it is a right-wing government, 0 otherwise), the number of coalition partners, average unemployment during the tenure, average GDP per capita growth during the tenure, and the incidence of major scandals during the government's tenure. The controls are added one-by-one to show robustness of the results.

4.1 Results

Table 2 shows the results of my models. Across specifications, government health expenditure is positively associated with its expected survival in office, despite modest degrees of freedom. Note that the coefficient of survival models indicates the relationship to failure at any point in the survival curve: negative coefficients indicate lower probabilities of failure, i.e. a greater probability of continued survival. While the statement that government expenditure on the population is positively associated with its degree of popularity and stay in power is hardly controversial, it should be noted that previous estimations of the effect of political business cycles on government tenure in CEE countries reached mixed results

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[1]The appendix includes alternative estimators and specifications, including OLS, logit GLM, Poisson, and negative binomial estimators.

(Klomp and de Haan 2013). My results suggest that, rather than taking aggregate spending as the main explanatory variable, researchers should theorize which types of spending are those preferred by the government's winning coalition, and base their estimations on those models (Bueno de Mesquita et al. 2002). Given that incumbent governments are not benevolent welfare maximizing agents, but rather seek reelection at the lowest possible cost, disaggregating welfare expenditure by recipient type makes substantive sense. Since EU accession shifts the average age of the electorate upward, one may reasonably expect that targeted welfare expenditure will move towards pensions and health care. The estimates presented in Table 2 substantiate these assertions. Note that controls are chosen to provide possible prior common causes of health care expenditure and government survival: only coefficients of the government health care expenditure variable are identified in these models and the coefficients of the controls should not be interpreted in a causal fashion (Keele, Stevenson and Elwert 2020).

In order to facilitate interpretation of the results, Figure 3 shows the survival probability in office across the range of the explanatory variable based on the last model of Table 2, where darker colors correspond to higher levels of government health expenditure. At the lowest expenditure level the predicted probability of surviving another day at 100 days in office is nearly 0%. By contrast, the highest expenditure level has a 65% to survive an additional day at this point. Of course, unlike the preceding difference-in-differences analysis, the models from Table 2 are not explicitly causally identified, and may suffer from endogeneity. For example, if a government in the data anticipated *ex ante* that it would be short-lived, and decreased its healthcare expenditure in response to that anticipation, it will induce some level of simultaneity in the results. However, the fact that nearly every government in the data ran on the same party platform in subsequent elections should provide some level of faith in the associative results presented here.

In order to further emphasize that it is the political behavior of the elderly that drives increased government tenure length, I analyze data from Wave 9 of the ESS,[2] combining aggregate country level and individual level covariates to predict satisfaction with the government. I restrict my effective sample to inhabitants of Bulgaria, Czechia, Estonia, Croatia, Hungary, Latvia, Lithuania, Poland, Slovenia, and Slovakia who are 60 years old

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[2]Wave 9 was collected prior to the COVID-19 pandemic.

Table 2: Health expenditure and government survival

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Gov. health exp.	-1.224**		-1.362***	-1.198*	-1.380**	-1.351**	-1.361**	-1.710**
	(0.384)		(0.387)	(0.474)	(0.487)	(0.497)	(0.497)	(0.574)
Elderly dep. ratio		-0.001	0.019	0.017	0.040	0.043	0.039	0.035
		(0.034)	(0.036)	(0.035)	(0.039)	(0.040)	(0.041)	(0.041)
Scandal			0.542	0.554	0.666*	0.653*	0.596	0.633
			(0.320)	(0.321)	(0.330)	(0.332)	(0.347)	(0.350)
Net mig.				-0.00001	-0.00000	-0.00000	-0.00000	-0.00000
				(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Right wing gov.					-0.500	-0.498	-0.461	-0.569
					(0.316)	(0.317)	(0.324)	(0.336)
Unemployment						0.012	0.015	-0.016
						(0.042)	(0.043)	(0.049)
Coalition partners							0.094	0.068
							(0.158)	(0.158)
GDP pc growth								-0.067
								(0.053)
N	56	56	56	56	56	56	56	56
R-squared	0.166	0.00001	0.206	0.211	0.245	0.246	0.251	0.272
Max. R-squared	0.998	0.998	0.998	0.998	0.998	0.998	0.998	0.998
Log Likelihood	-167.259	-172.353	-165.892	-165.713	-164.472	-164.433	-164.256	-163.478
Wald Test	10.170** (df = 1)	0.000 (df = 1)	13.320** (df = 3)	13.690** (df = 4)	16.600** (df = 5)	16.830** (df = 6)	16.670* (df = 7)	17.790* (df = 8)
LR Test	10.188** (df = 1)	0.0004 (df = 1)	12.923** (df = 3)	13.279** (df = 4)	15.762** (df = 5)	15.839* (df = 6)	16.194* (df = 7)	17.750* (df = 8)
Score (Logrank) Test	10.471** (df = 1)	0.0004 (df = 1)	13.644** (df = 3)	14.086** (df = 4)	17.149** (df = 5)	17.330** (df = 6)	17.419* (df = 7)	18.426* (df = 8)

***p < .001; **p < .01; *p < .05

Figure 3: Health expenditure and government tenure (Model 3)
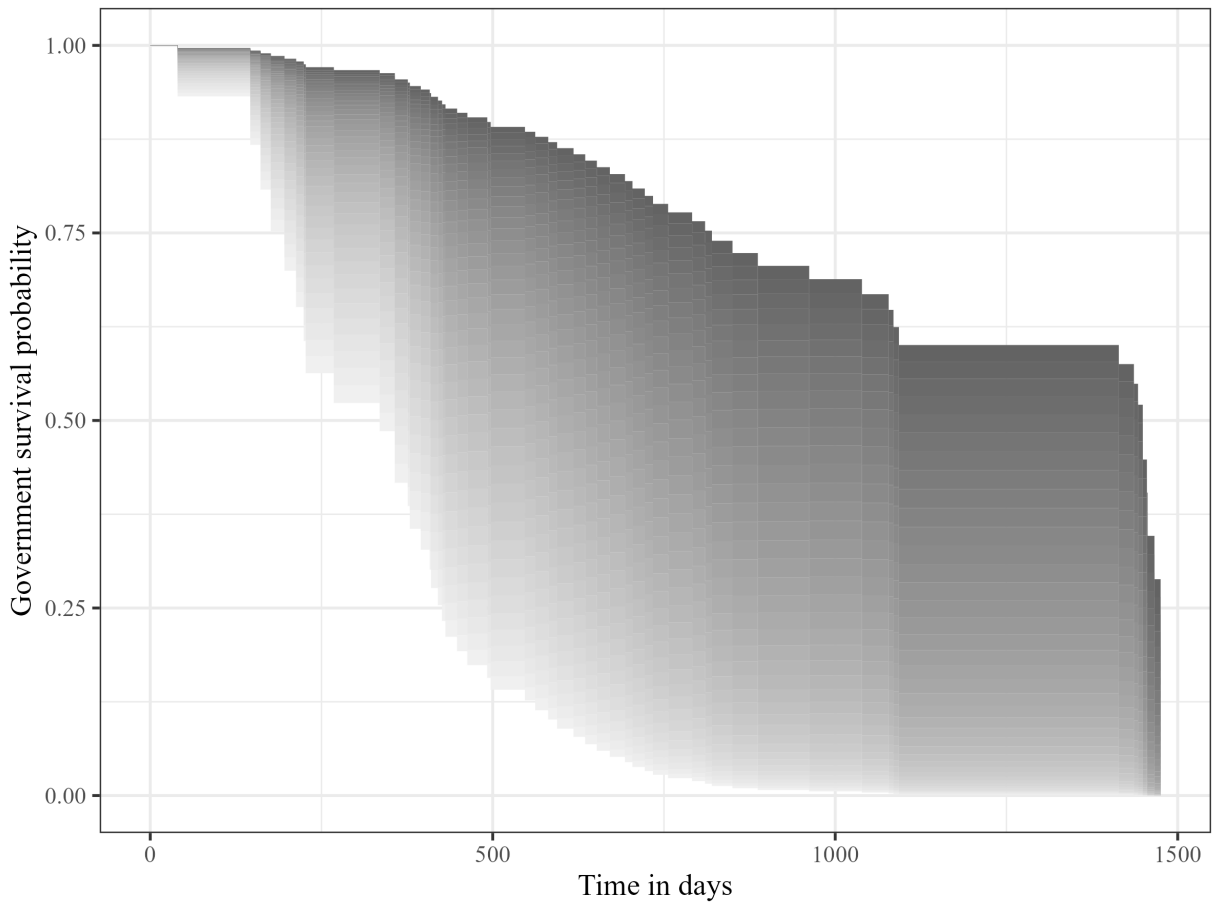
or older. I predict their satisfaction with their governments using their personal satisfaction with public health, country-level health expenditure per capita, aggregate normalized emigration, the country's old age dependency ratio, and the ideology of the incumbent government. I also include year fixed effects for the dates of the interviews.[3] Results are shown in Table 3, where I estimate both an OLS model and a logistic GLM. In the former, the outcome variable is satisfaction with the government on a 10-point scale, whereas the latter recodes the outcome variable to be 0 (a score of 5 or lower) or 1 (a score of 6 or higher). In both specifications, aggregate health expenditure is positively associated with government satisfaction, emphasizing the role of the elderly components of the electorate to keep the government incumbent. In the appendix, I show that this part of the population does not respond similarly to overall social spending: it is specifically targeted welfare spending that increases satisfaction with the incumbent government among the elderly part of the population.

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[3]Wave 9 was collected over two years.

Table 3: Health expenditure and government satisfaction

	OLS					Logit				
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9	Model 10
Health expenditure per capita	0.002***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.0004***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Health satisfaction		0.357***	0.381***	0.381***	0.370***		0.263***	0.290***	0.295***	0.285***
		(0.012)	(0.012)	(0.012)	(0.012)		(0.013)	(0.013)	(0.013)	(0.013)
Normalized emigration			-1.160***	-0.309*	-0.022			-1.115***	-0.334*	0.026
			(0.111)	(0.151)	(0.154)			(0.110)	(0.151)	(0.157)
Dependency ratio				-0.140***	-0.193***				-0.126***	-0.196***
				(0.017)	(0.018)				(0.017)	(0.019)
Right wing gov.					0.394***					0.451***
					(0.048)					(0.048)
Constant	2.330***	1.445***	1.943***	5.997***	7.188***	-2.156***	-3.024***	-2.573***	0.949	2.465***
	(0.118)	(0.114)	(0.123)	(0.507)	(0.524)	(0.113)	(0.128)	(0.135)	(0.489)	(0.510)
N	6138	6107	6048	6048	6048	6138	6107	6048	6048	6048
R-squared	0.042	0.166	0.182	0.191	0.200					
Adj. R-squared	0.042	0.166	0.181	0.190	0.199					
Residual Std. Error	2.540 (df = 6134)	2.369 (df = 6102)	2.347 (df = 6043)	2.335 (df = 6042)	2.322 (df = 6041)					
F Statistic	90.155*** (df = 3; 6134)	304.178*** (df = 4; 6102)	335.958*** (df = 4; 6043)	285.349*** (df = 5; 6042)	251.610*** (df = 6; 6041)					
Log Likelihood						-3549.116	-3290.776	-3213.737	-3186.522	-3141.225
AIC						7106.232	6591.553	6437.474	6385.043	6296.450

***p < .001; **p < .01; *p < .05

5 Conclusion

This article has shown that, by joining the Common Market with its Freedom of Movement principle, the new member states of Central and Eastern Europe have undergone fundamental transformations not only in terms of their economics and labor markets, but also their politics and targeted welfare spending. Joining the EU leads to significant out migration to higher wage countries, which strongly affects the size of the labor market and the dependency ratio. Furthermore, emigration is non-random with respect to both socio-economic and political characteristics of individuals (Lim 2023). Consequently, incumbent governments shift targeted welfare expenditure towards the elderly, and become less likely to lose reelection. My contribution also adds causal estimates of some effects previously highlighted by the econometric literature (Petrova and Sznajder Lee 2023).

The findings presented here provide further evidence that contemporary political conflict about the welfare state in Europe regard not its overall size, but rather relative spending on different priorities, such as pensions, education, or health (Bremer and Bürgisser 2023). As such, it feeds into the ongoing debate of how countries with aging populations, both in Europe and beyond, are likely to respond to the economic and political pressures that such demographic changes generate. Where these are exacerbated by emigration, conflict over public finances are all the more likely to become bitter and hard fought, as governments have to balance greater demand for pension and health expenditures, with a lower supply of money to finance these demands. The struggle to balance the economic and political sustainability of welfare provides governments a strong incentive to systematically favor some parts of the electorate over others. In aging societies, this will mean greater provisions for the elderly, at the cost of other segments of the population. The more dominant a faction the elderly are in the minimum winning coalition, the stronger this effect will be. While the research design of this article was tailored to studying the effects of emigration on welfare politics and programmatic competition in Central and Eastern Europe, its theoretical framework could be generalized to other regions of the world with high levels of emigration and aging populations, including older member states such as Italy and Greece.

Naturally, these results are but a small part of the much larger puzzle of program-

matic competition and political economy within the European Union in general, and its Central and Eastern member states in particular. Most importantly, the October 2023 defeat of the PiS coalition in Poland provides an important caveat to the argument made here. Turnout of voters under 30 jumped over twenty percentage points: from 46% in 2019 to 64%, and support of the incumbent illiberal party decreased significantly among this group. The results shown in this study should therefore not be seen as an argument in favor of the inevitability of the rise of a consistently conservative electorate in the Eastern European member states. Future research should investigate the factors that led to the 2023 Polish electoral results and its marked increase in electoral turnout among younger voters, as well as other success stories of pro-democratic movements in CEE.

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A   Robustness to anticipation effects

Elderly dependency ratio with anticipation
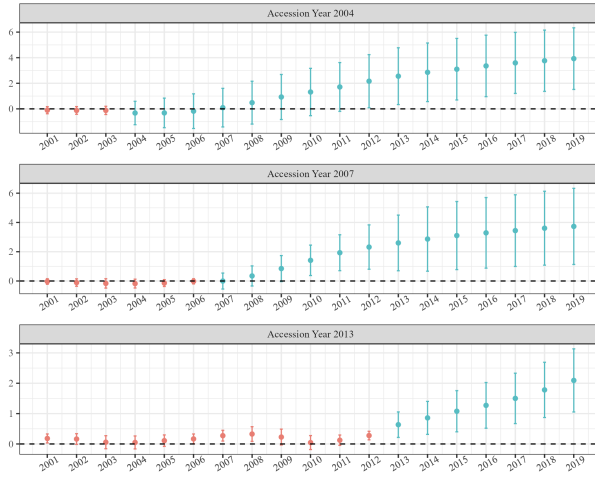
Government health expenditure with anticipation
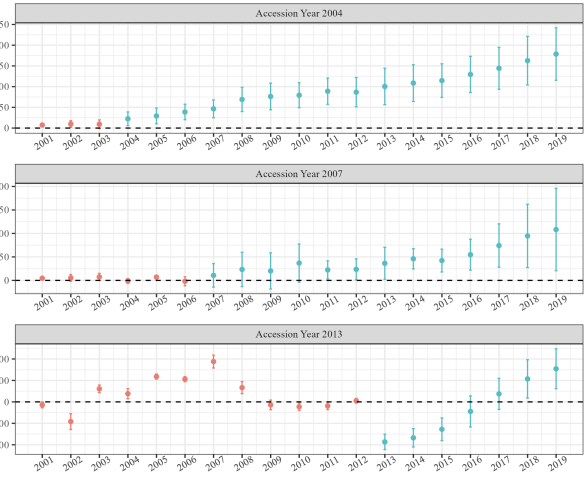
Private health expenditure with anticipation
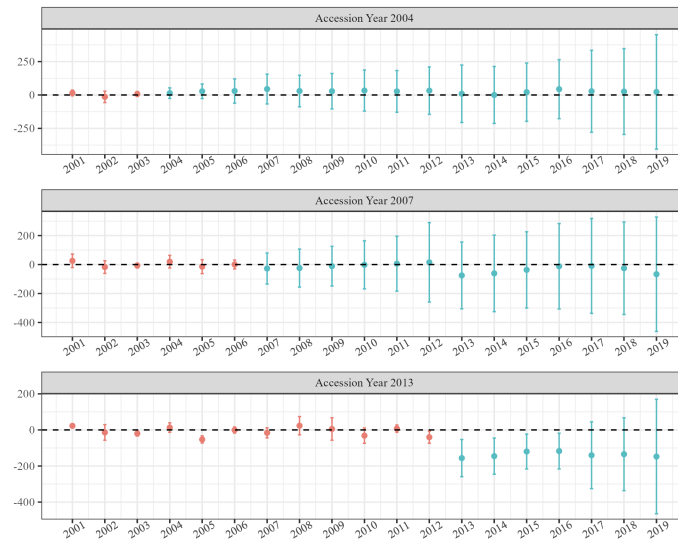
B Robustness to estimator choice

B.1 TWFE-regression (2004 cohort only)

	Old age dep. ratio	Gov. health exp.	Priv. health exp.
Post-treatment	6.444***	649.008***	481.493***
	(0.411)	(51.442)	(32.732)
Treatment status	5.276***	1095.552***	323.517***
	(0.454)	(54.517)	(34.437)
Diff-in-Diff	1.879***	434.406***	36.368
	(0.312)	(38.595)	(24.208)
Constant	14.894***	4.386	94.501**
	(0.394)	(48.463)	(30.673)
TWFE	YES	YES	YES
N	380	349	348
R-squared	0.959	0.943	0.835
Adj. R-squared	0.954	0.936	0.816
Residual Std. Error	1.201 (df = 341)	142.084 (df = 311)	89.088 (df = 310)
F Statistic	209.764*** (df = 38; 341)	139.560*** (df = 37; 311)	42.546*** (df = 37; 310)

***$p < .001$; **$p < .01$; *$p < .05$

B.2 Synthetic control

Old age dependency ratio: gap synthetic control and treated unit
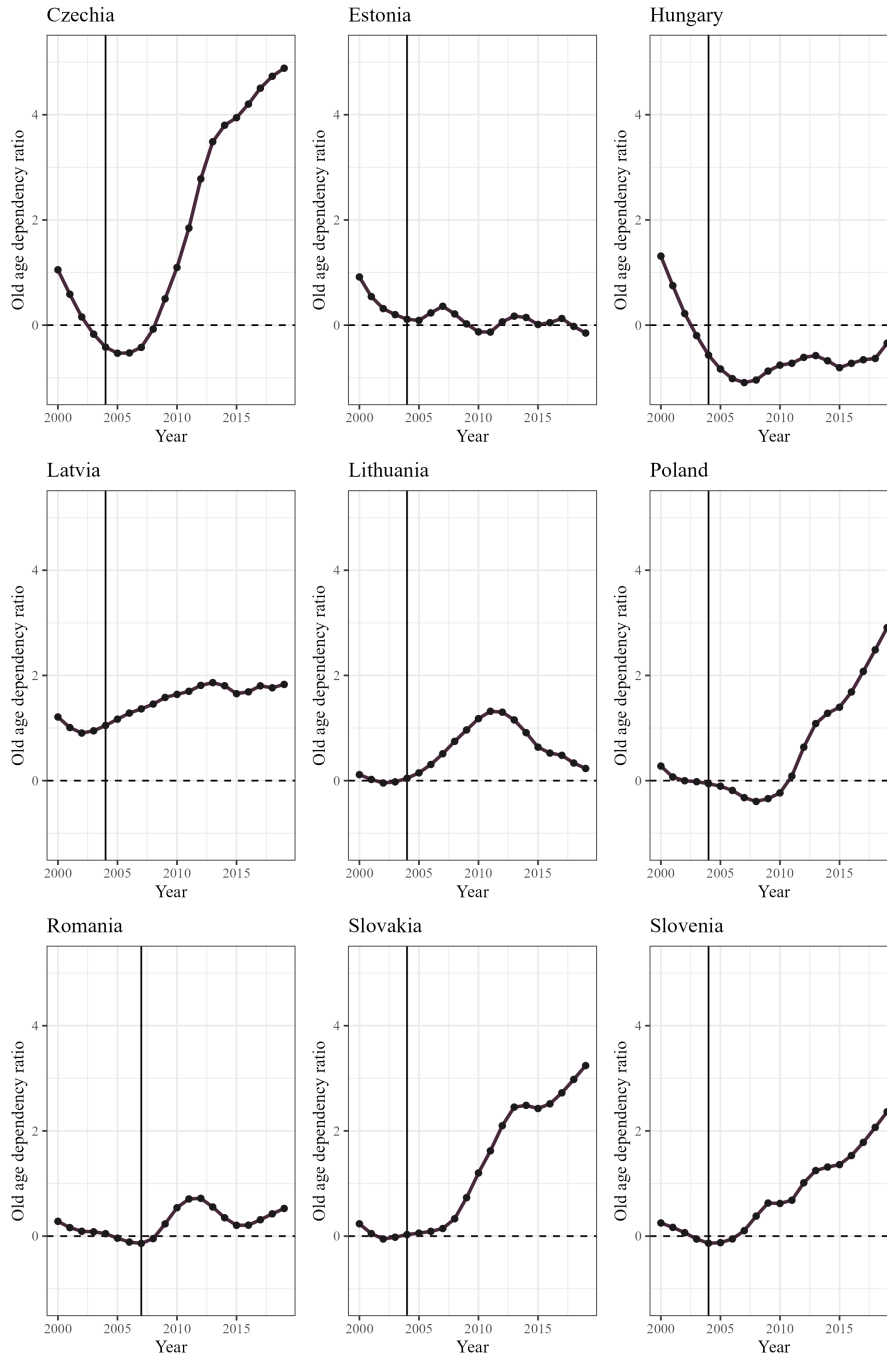
Government health expenditure: gap synthetic control and treated unit

Private health expenditure: gap synthetic control and treated unit

Synthetic controls for Bulgaria and Croatia

C   Robust tenure analyses

OLS

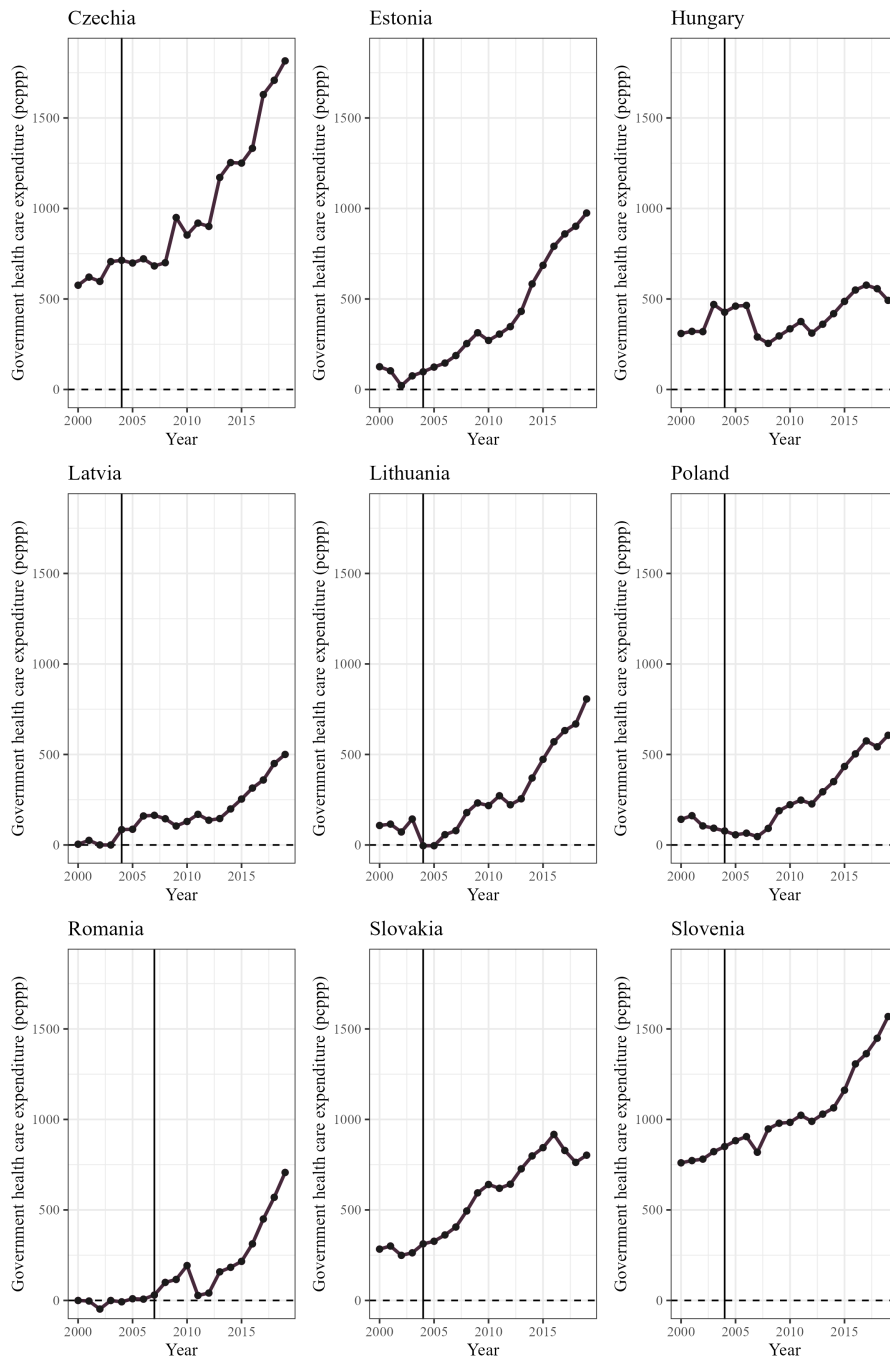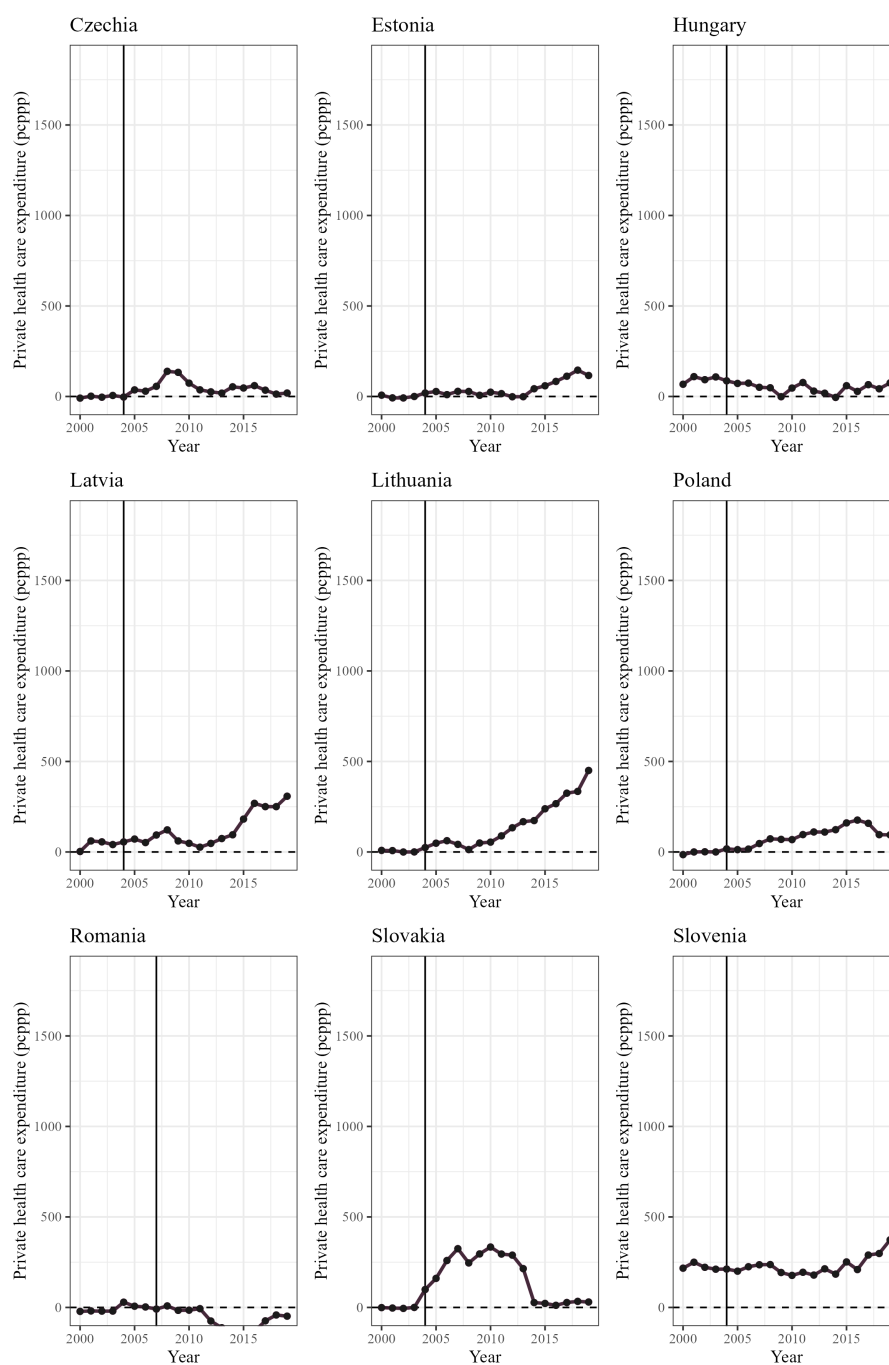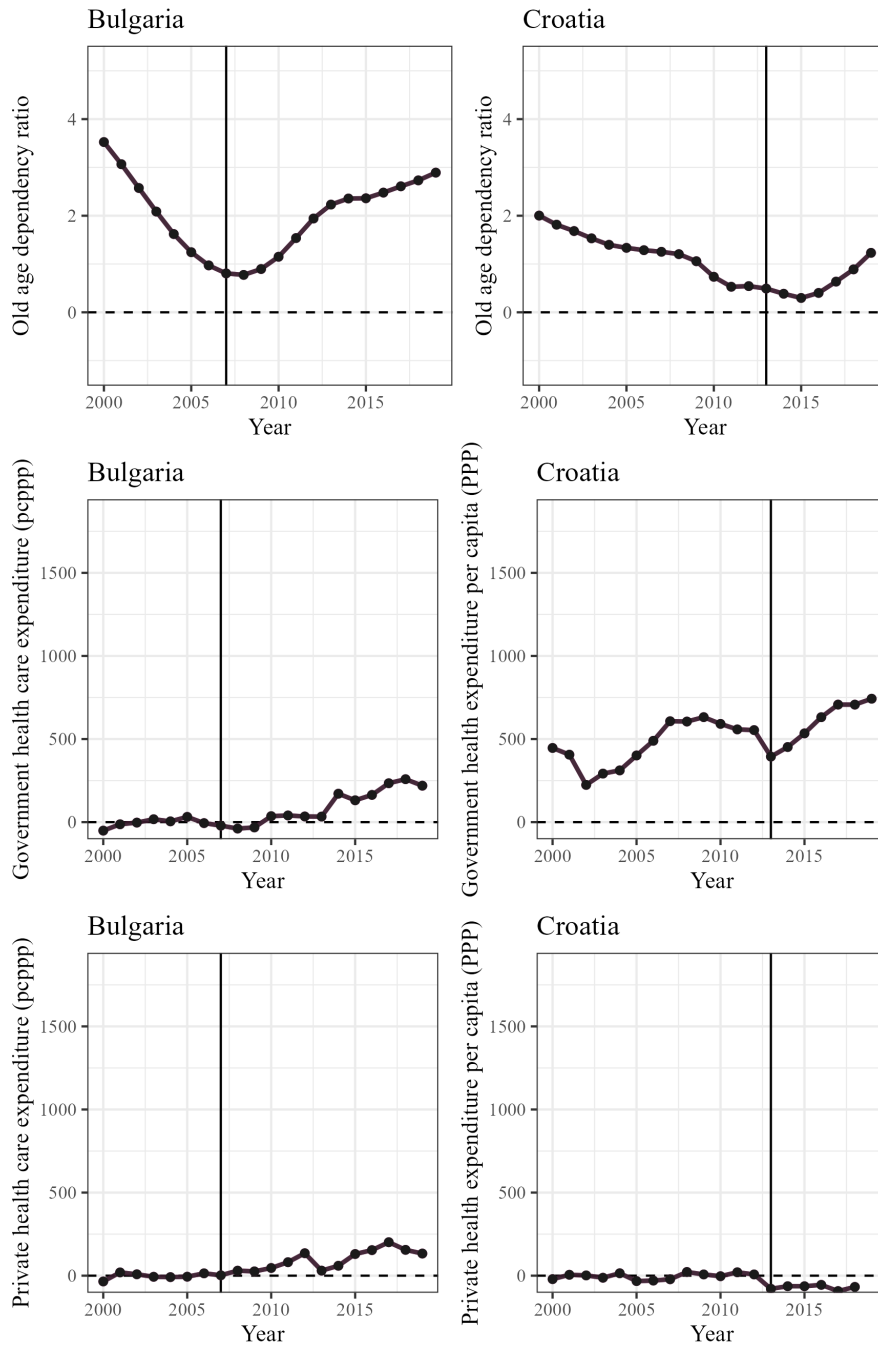|	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Gov. health exp.	366.869**	457.021**	501.035**	505.201**	498.591**	560.253**
	(120.174)	(163.172)	(164.592)	(177.396)	(180.495)	(202.126)
Elderly dep. ratio	1.616	−5.189	−12.993	−12.806	−11.631	−9.197
	(13.793)	(14.426)	(15.321)	(15.723)	(16.372)	(16.832)
Scandal		−225.301	−264.823*	−265.381*	−256.908	−256.333
		(128.022)	(129.853)	(131.426)	(135.794)	(136.538)
Net mig.		−0.001	−0.001	−0.001	−0.001	−0.0004
		(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Right wing gov.			161.496	161.809	152.920	166.136
			(114.452)	(115.702)	(120.683)	(122.832)
Unemployment				1.008	0.935	8.551
				(14.964)	(15.108)	(18.750)
Coalition partners					−15.107	−11.950
					(51.638)	(52.119)
GDP pc growth						12.681
						(18.302)
Constant	−1890.896*	−2293.803*	−2496.410*	−2539.324	−2476.629	−3094.482
	(868.731)	(1098.807)	(1097.716)	(1278.945)	(1308.713)	(1589.533)
N	56	56	56	56	56	56
R-squared	0.152	0.201	0.231	0.231	0.233	0.241
Adj. R-squared	0.120	0.138	0.154	0.137	0.121	0.111
Residual Std. Error	391.157 (df = 53)	387.123 (df = 51)	383.416 (df = 50)	387.291 (df = 49)	390.956 (df = 48)	393.090 (df = 47)
F Statistic	4.749* (df = 2; 53)	3.202* (df = 4; 51)	3.010* (df = 5; 50)	2.459* (df = 6; 49)	2.080 (df = 7; 48)	1.861 (df = 8; 47)

$^{***}p < .001$; $^{**}p < .01$; $^{*}p < .05$

Poisson

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Gov. health exp.	0.544***	0.691***	0.749***	0.753***	0.749***	0.813***
	(0.012)	(0.016)	(0.017)	(0.018)	(0.018)	(0.019)
Elderly dep. ratio	0.001	−0.007***	−0.018***	−0.017***	−0.017***	−0.014***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Scandal		−0.345***	−0.399***	−0.399***	−0.393***	−0.385***
		(0.013)	(0.013)	(0.013)	(0.014)	(0.014)
Net mig.		−0.00000*	−0.00000	−0.00000	−0.00000	0.00000
		(0.00000)	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Right wing gov.			0.233***	0.233***	0.228***	0.244***
			(0.011)	(0.011)	(0.012)	(0.012)
Unemployment				0.001	0.001	0.010***
				(0.002)	(0.002)	(0.002)
Coalition partners					−0.008	−0.005
					(0.005)	(0.005)
GDP pc growth						0.016***
						(0.002)
Constant	2.704***	1.983***	1.692***	1.653***	1.690***	1.015***
	(0.087)	(0.110)	(0.112)	(0.129)	(0.132)	(0.154)
N	56	56	56	56	56	56
Log Likelihood	−6055.359	−5680.212	−5466.660	−5466.473	−5465.320	−5428.641
AIC	12116.720	11370.420	10945.320	10946.940	10946.640	10875.280

***p < .001; **p < .01; *p < .05

Negative binomial

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Gov. health exp.	0.641***	0.688**	0.744**	0.723**	0.716**	0.793**
	(0.179)	(0.241)	(0.242)	(0.258)	(0.260)	(0.289)
Elderly dep. ratio	0.006	−0.001	−0.010	−0.011	−0.009	−0.007
	(0.021)	(0.021)	(0.023)	(0.023)	(0.024)	(0.024)
Scandal		−0.271	−0.315	−0.310	−0.290	−0.292
		(0.189)	(0.191)	(0.191)	(0.196)	(0.195)
Net mig.		0.00000	−0.00000	−0.00000	−0.00000	−0.00000
		(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Right wing gov.			0.218	0.218	0.204	0.223
			(0.169)	(0.169)	(0.174)	(0.176)
Unemployment				−0.008	−0.009	−0.0001
				(0.022)	(0.022)	(0.027)
Coalition partners					−0.030	−0.025
					(0.074)	(0.074)
GDP pc growth						0.017
						(0.026)
Constant	1.926	1.830	1.525	1.785	1.870	1.118
	(1.294)	(1.625)	(1.617)	(1.863)	(1.886)	(2.272)
N	56	56	56	56	56	56
Log Likelihood	−407.727	−406.667	−405.819	−405.755	−405.686	−405.506
θ	2.962*** (0.535)	3.066*** (0.555)	3.152*** (0.572)	3.159*** (0.573)	3.166*** (0.574)	3.184*** (0.578)
AIC	821.453	823.333	823.637	825.511	827.371	829.013

***$p < .001$; **$p < .01$; *$p < .05$

D Alternative tenure analysis: reelection outcomes

Reelection (binary)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Gov. health exp.	0.166		0.463	−0.279	0.111	0.217	−0.338	−0.265
	(0.651)		(0.734)	(0.917)	(0.962)	(1.015)	(1.081)	(1.205)
Elderly dep. ratio		0.074	0.046	0.074	0.007	0.010	0.057	0.059
		(0.077)	(0.080)	(0.085)	(0.092)	(0.093)	(0.097)	(0.098)
Scandal			−1.059	−0.985	−1.425	−1.414	−1.121	−1.120
			(0.790)	(0.810)	(0.881)	(0.875)	(0.865)	(0.864)
Net mig.				0.00003	0.00004	0.00004	0.00003	0.00004
				(0.00003)	(0.00003)	(0.00003)	(0.00003)	(0.00003)
Right wing gov.					1.547*	1.559*	1.370	1.394
					(0.770)	(0.776)	(0.837)	(0.854)
Unemployment						0.031	0.038	0.049
						(0.092)	(0.098)	(0.125)
Coalition partners							−0.682*	−0.681*
							(0.344)	(0.344)
GDP pc growth								0.015
								(0.110)
Constant	−1.786	−2.452	−4.739	−0.122	−2.095	−3.203	1.336	0.599
	(4.526)	(1.925)	(5.147)	(6.215)	(6.601)	(7.411)	(7.992)	(9.623)
N	55	55	55	55	55	55	55	55
Log Likelihood	−35.420	−34.978	−33.984	−32.993	−30.694	−30.638	−28.475	−28.465
AIC	74.840	73.957	75.967	75.986	73.387	75.276	72.950	74.931

***p < .001; **p < .01; *p < .05

Reelection (margin)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Gov. health exp.	-5.265		-5.966	-6.355	-4.627	-5.605	-5.264	-4.057
	(3.295)		(3.532)	(4.546)	(4.497)	(4.831)	(4.910)	(5.552)
Elderly dep. ratio		0.442	0.534	0.544	0.247	0.203	0.146	0.193
		(0.380)	(0.394)	(0.404)	(0.419)	(0.429)	(0.445)	(0.459)
Scandal			0.529	0.596	-0.861	-0.731	-1.127	-1.094
			(3.525)	(3.593)	(3.564)	(3.595)	(3.698)	(3.729)
Net mig.				0.00002	0.00001	0.00000	0.00001	0.00002
				(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Right wing gov.					6.304	6.229	6.698*	6.992*
					(3.141)	(3.165)	(3.308)	(3.392)
Unemployment						-0.236	-0.233	-0.086
						(0.407)	(0.411)	(0.514)
Coalition partners							0.755	0.834
							(1.418)	(1.439)
GDP pc growth								0.244
								(0.508)
Constant	30.666	-16.616	22.313	24.851	16.593	26.666	23.361	11.204
	(22.867)	(9.415)	(24.304)	(30.661)	(30.056)	(34.888)	(35.695)	(44.009)
N	55	55	55	55	55	55	55	55
R-squared	0.046	0.025	0.080	0.080	0.150	0.156	0.161	0.165
Adj. R-squared	0.028	0.007	0.026	0.007	0.064	0.051	0.036	0.020
Residual Std. Error	10.669 (df = 53)	10.786 (df = 53)	10.680 (df = 51)	10.784 (df = 50)	10.471 (df = 49)	10.543 (df = 48)	10.623 (df = 47)	10.711 (df = 46)
F Statistic	2.552 (df = 1; 53)	1.353 (df = 1; 53)	1.479 (df = 3; 51)	1.093 (df = 4; 50)	1.733 (df = 5; 49)	1.480 (df = 6; 48)	1.291 (df = 7; 47)	1.140 (df = 8; 46)

***p < .001; **p < .01; *p < .05

E Alternative treatment individual level: total social expenditure

	Model 1	Model 2	Model 3	Model 4	Model 5
Social expenditure (% GDP)	0.051	0.075**	0.021	0.018	0.015
	(0.028)	(0.027)	(0.028)	(0.029)	(0.029)
Health satisfaction		0.369***	0.382***	0.383***	0.376***
		(0.014)	(0.014)	(0.014)	(0.014)
Normalized emigration			-0.721***	-0.660***	-0.029
			(0.127)	(0.184)	(0.207)
Dependency ratio				-0.011	-0.109***
				(0.023)	(0.027)
Right wing gov.					0.365***
					(0.056)
Year FE	-0.363***	-0.101	-0.124	-0.134	0.044
	(0.089)	(0.083)	(0.083)	(0.086)	(0.090)
Constant	3.779***	1.440**	2.841***	3.165***	5.241***
	(0.549)	(0.519)	(0.573)	(0.904)	(0.955)
N	4554	4535	4535	4535	4535
R-squared	0.008	0.142	0.148	0.148	0.156
Adj. R-squared	0.007	0.142	0.147	0.147	0.155
Residual Std. Error	2.549 (df = 4551)	2.369 (df = 4531)	2.361 (df = 4530)	2.361 (df = 4529)	2.350 (df = 4528)
F Statistic	17.199*** (df = 2; 4551)	250.268*** (df = 3; 4531)	197.094*** (df = 4; 4530)	157.691*** (df = 5; 4529)	139.760*** (df = 6; 4528)

***p < .001; **p < .01; *p < .05

Investing in Voice:
Remittances and Varieties of Political Participation

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Under contract with *Cambridge Elements*

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 by finding nonelectoral 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. Remittances thus affect not only economic development, but political development as well. However, I also find that remittances decrease voter turnout, and that this effect is attenuated by party-level clientelist practices. 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*

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). Given that equal political participation in society is key for sustainable and equitable development, this project investigates how 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; De Vries et al., 2024; Germano, 2013; Tertytchnaya 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

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[1] The Gambia

(Tertytchnaya et al., 2018), and the likelihood of voting for incumbents (Ahmed, 2017). These findings suggest that remittances could elevate households economically, but lead them to become disengaged from the political process. While these measures of voting 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 demonstrations and protest movements. However, they are not more likely to vote than non-recipients, as

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

suggested by the existing literature (Germano, 2013). Interaction effects with country-level covariates indicate that these effects hold across different institutional and economic contexts, suggesting that remittances matter not only for already democratic or affluent countries, but for more authoritarian and poorer ones as well.

My theoretical and empirical contributions suggest that those who study the effects of international economic outcomes on political behavior and unequal access to the political apparatus 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 microfoundations 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 changes in political behavior. My project provides such a test. Lastly, this project 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 less attention in the literature (Konte, 2016). Latin America is a region with many democracies and people emigrating to the United States. By contrast, my sample incorporates more authoritarian states with varied migration flows, including a large share of South-South migration in overall flows. This thus provides an important measure of external validity to previous findings.

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; Tertytchnaya 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, education, sanitation, and other long term investments than non-recipients (Adams and Cuecuecha, 2013; Airola, 2007; Askarov and Doucouliagos, 2020; 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. 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; Askarov and Doucouliagos, 2020; Mishra et al., 2022; 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. Previous research has suggested that remittances decrease, rather than increase, further outmigration (Helms and Leblang, 2022). 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 remittances, 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 Garritzmann, 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 macroeconomy 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; Askarov and Doucouliagos, 2020; Nsiah and Fayissa, 2014) to immediate consumption and poverty alleviation (Anyanwu and Erhijakpor, 2010; Mishra et al., 2022). 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; Tertytchnaya 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

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[3]Alternatively, 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.

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 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 (De Vries et al., 2024; Tertytchnaya 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 house-hold'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).

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 chapter. While the baseline probability of all of these behaviors are different, as the descriptive statistics presented in the next chapter 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

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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.

3   Data & Research Design

In this analysis, I use African survey data taken from Wave 7 of the Afrobarometer survey. 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, whether they have ever contacted their local councilor or MP, and whether they self-report having voted in the most recent national election – 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 non-electoral 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.

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	0322	0	1
Participation Index	0.007	0.733	-0.634	2.443
Voting	0.755	0.427	0	1

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.

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 are only slightly less likely to be employed (although this could be either because unemployment causes migration, or because remittances substitute for wages), and 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 and the author's website upon acceptance of this manuscript.

3.1 Baseline Results

I start by estimating several baseline models. These include logit and OLS regression estimations, both including country level fixed effects, cross-country survey weights, and relevant 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. Note that all the mentioned variables are binary indicators, meaning they are directly comparable. 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. The results of remittances on the likelihood of voting is negative, which is in line with both theory provided in this paper and the previous literature (Germano, 2013).

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

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Table 2: The Effect of Remittances on Political Behavior

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index	Voting
Remittances	0.196***	0.227***	0.360***	0.241***	0.309***	0.114***	-0.149***
	(4.22)	(6.54)	(6.00)	(4.78)	(4.87)	(6.96)	(-3.05)
Urban	-0.0208	-0.425***	0.173**	-0.428***	-0.338***	-0.120***	-0.432***
	(-0.51)	(-9.48)	(3.01)	(-9.02)	(-6.84)	(-10.15)	(-7.64)
Male	0.610***	0.484***	0.411***	0.563***	0.505***	0.214***	0.113*
	(13.64)	(12.95)	(11.02)	(9.79)	(9.63)	(15.08)	(2.47)
Education	0.614***	0.183***	0.295***	0.265***	0.267***	0.124***	(0.0516)
	(10.88)	(4.85)	(4.33)	(4.79)	(4.62)	(7.63)	(0.89)
Employment	0.235***	0.180***	0.150*	0.217***	0.246***	0.0840***	0.163***
	(6.33)	(4.39)	(2.46)	(4.94)	(4.93)	(6.32)	(3.69)
Language	0.138	-0.0746	0.214*	-0.109	0.0252	-0.00290	-0.251***
	(1.31)	(-1.63)	(2.14)	(-1.69)	(0.32)	(-0.13)	(-3.67)
Lived Poverty	0.0730**	0.202***	0.208***	0.142***	0.140***	0.0658***	-0.00148
	(3.49)	(7.35)	(5.52)	(4.70)	(4.21)	(6.85)	(-0.08)
Past Migration	0.173***	0.293***	0.312***	0.313***	0.383***	0.132***	-0.116**
	(4.47)	(6.46)	(7.16)	(6.79)	(6.50)	(8.22)	(-2.95)
Age	-0.00109	0.00848***	-0.0116***	0.0161***	0.0145***	0.00384***	0.0323***
	(-0.85)	(6.59)	(-5.46)	(12.45)	(8.06)	(7.91)	(12.71)
Mobile Phone	0.368***	0.211***	0.140	0.430***	0.279*	0.126***	0.120*
	(5.06)	(5.52)	(1.61)	(9.07)	(2.69)	(7.84)	(2.22)
Country FEs	YES	YES	YES	YES	YES	YES	YES
N	43,869	43,671	43,276	43,753	43,698	42,616	39,385

Fixed Effects Logit/regression models. Based on Afrobarometer Wave 7. Test statistics in parentheses.
Includes cross-country sample weighting.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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. Remittance recipients are approximately three percentage points less likely to self-report voting.

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

Figure 1: Marginal Effect Plots
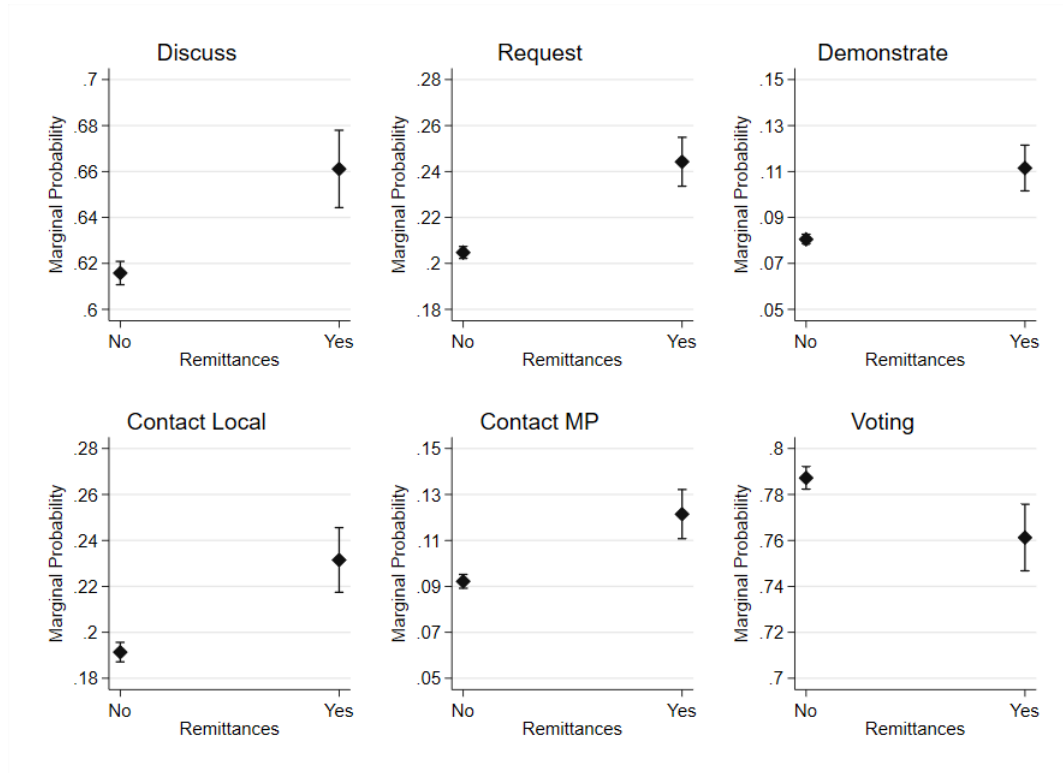
11%. Here, a smaller percentage point increase makes respondents much more likely to give the response of interest, because the base outcome is less probable. Substantively, it seems like remittances matter comparatively more for stronger forms of participation than for weaker ones. It is clear from 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 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 (Córdova and Hiskey, 2021; 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.[4] assumption necessary for estimating generalized linear models

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[4]Independently and identically distributed residuals.

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 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-a-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, while decreasing the likelihood of voting. 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

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Table 3: Accounting for Self-Selection with Matching

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index	Voting
ATE (PSM)							
Remittances	0.0429***	0.0399***	0.0494***	0.0452***	0.0341***	0.121***	-0.0487***
	(6.18)	(6.26)	(10.01)	(7.11)	(7.00)	(10.68)	(-7.10)
ATE (NNM)							
Remittances	0.0423***	0.0418***	0.0352***	0.0493***	0.0369***	0.130***	-0.0199***
	(5.58)	(5.96)	(7.45)	(6.95)	(6.59)	(10.60)	(-2.82)
N	43,869	43,671	43,276	43,753	43,698	42,616	39,385

Based on Afrobarometer V7. t statistics in parentheses.

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

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 8 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 and that of electoral participation is negative, 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

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Table 4: Accounting for Effect Heterogeneity with Random Slope Effects

	Discuss	Request	Demonstrate	Contact Local	Contact MP	Index	Voting
Remittances	0.211***	0.232***	0.355***	0.264***	0.333***	0.120***	-0.167***
	(5.08)	(6.20)	(6.12)	(5.73)	(5.71)	(7.75)	(-3.73)
Urban	-0.00789	-0.433***	0.122***	-0.416***	-0.297***	-0.118***	-0.427***
	(-0.34)	(-16.56)	(3.41)	(-15.55)	(-8.69)	(-15.66)	(-15.41)
Male	0.608***	0.488***	0.403***	0.547***	0.469***	0.211***	0.127***
	(28.89)	(20.33)	(12.10)	(22.23)	(14.68)	(30.34)	(4.97)
Education	0.603***	0.187***	0.301***	0.252***	0.265***	0.124***	0.0443
	(22.62)	(6.34)	(7.76)	(8.43)	(7.05)	(14.46)	(1.42)
Employment	0.237***	0.170***	0.132***	0.225***	0.263***	0.0864***	0.165***
	(9.99)	(6.46)	(3.69)	(8.43)	(7.75)	(11.23)	(5.83)
Language	0.121***	-0.0474	0.236***	-0.0748*	0.0919*	0.0106	-0.214***
	(3.77)	(-1.37)	(5.10)	(-2.09)	(2.06)	(1.03)	(-5.62)
Lived Poverty	0.0741***	0.206***	0.197***	0.148***	0.148***	0.0677***	-0.00470
	(5.69)	(14.32)	(9.95)	(9.96)	(7.82)	(15.80)	(-0.30)
Past Migration	0.166***	0.280***	0.309***	0.306***	0.383***	0.131***	-0.119***
	(6.58)	(10.27)	(8.60)	(11.06)	(11.10)	(16.08)	(-4.02)
Age	-0.000779	0.00892***	-0.0102***	0.0159***	0.0150***	0.00404***	0.0326***
	(-1.09)	(11.20)	(-8.60)	(19.95)	(14.71)	(17.10)	(32.47)
Mobile Phone	0.364***	0.245***	0.158*	0.410***	0.295***	0.128***	0.152***
	(10.61)	(6.11)	(2.42)	(9.45)	(5.25)	(10.92)	(3.34)
Log GDP	-0.129	-0.257**	0.292**	-0.0760	-0.0552	-0.0283	-0.398***
	(-1.49)	(-2.79)	(2.76)	(-0.98)	(-0.66)	(-1.50)	(-3.47)
Polyarchy	0.379	-0.0237	0.507	0.498	0.0482	0.0772	1.272*
	(1.00)	(-0.06)	(1.08)	(1.42)	(0.13)	(0.93)	(2.51)
British	0.211	0.293	-0.304	0.242	0.0118	0.0416	-0.0332
	(1.29)	(1.68)	(-1.52)	(1.57)	(0.07)	(1.16)	(-0.15)
French	0.145	-0.242	0.0325	-0.297*	-0.604***	-0.0981**	-0.435*
	(0.91)	(-1.43)	(0.17)	(-2.00)	(-3.74)	(-2.82)	(-2.05)
Constant	0.116	-0.304	-5.476***	-2.496***	-2.977***	-0.326*	2.874***
	(0.17)	(-0.43)	(-6.69)	(-4.26)	(-4.71)	(-2.24)	(3.25)
Random Intercept	0.128***	0.144***	0.187***	0.113***	0.129***	0.0058***	0.0329*
	(3.98)	(3.92)	(3.85)	(4.37)	(3.91)	(3.81)	(2.27)
Random Slope Remittances	0.0325*	0.0158	0.0551*	0.0491**	0.0700**	0.0054**	0.228***
	(2.43)	(1.68)	(2.20)	(2.81)	(2.61)	(2.98)	(3.98)
N	43,869	43,671	43,276	43,753	43,698	42,616	39,385

*M*ixed Effects Logit/regression models. Based on Afrobarometer Wave 7. Test statistics in parentheses.

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

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).

However, it is noteworthy that the Random Slope Variance Parameter is much larger compared to the size of the coefficient for the model estimating propensity to vote. This suggests that, while the effect of remittances on voting may indeed be moderated by institutional variables, their effect on non-electoral participation is much more consistent across different contexts. 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.

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. This suggests that the economic and

political remittances channels are complimentary and both operate simultaneously. 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.

3.6 Remittances, Clientelism, and Voter Turnout

Regarding the negative relationship between remittances and voter turnout in the preceding models, the established literature suggests two potential mechanisms, both premised on the role played by clientlist party ties. On the one hand, Pfutze (2014) argues that remittances allow households to escape clientelist ties in the long run, because remittances allow households to substitute goods otherwise provided by clientelist parties with remitted income. On the other hand, González-Ocantos et al. (2018) argue that remittance receiving households are more attractive targets of party machines in the short run, because their distributive preferences change compared to non-receiving households. This dynamic has mostly been examined in the context of Latin America. However, it has been largely untested in the context of Africa, despite the prevalence of clientelism and the importance of remittances for this region. Here, I examine to what extent remittances affect individual-level turnout propensity for clientelist and non-clientelist parties.

I take party-level clientelism data from the Democratic Accountability and Linkages Project (DALP, Freeze and Kitschelt, 2010), and link this to the individual level Afrobarometer data by matching on party name for respondents' answer to the question of which political party they generally support. I then test for the interaction between the party's clientelism score from DALP and whether an individual receives remittances based on the Afrobarometer. All other controls are the same as in Table 2. Results are visualized in Figure 2. These results show that the decrease in voter turnout is driven by individuals who support the least clientelist parties. At the lowest level of party clientelism, the difference in the probability of voting between remittance recipients and non-recipients is approximately 20%-points. By contrast, at the highest level of party clientelism, ther is no statistically significant difference in the probability of voting between recipients and non-recipients. Note furthermore that, for non-recipients, there is no discernible effect of party-level clientalism on voter turnout. These results are more in line

with the theoretical model proposed in González-Ocantos et al. (2018) than that proposed in Pfutze (2014). However, it must be acknowledged that the former is fundamentally concerned with short-term dynamics, whereas the latter focuses on the long-term. As such, it remains an open question whether the result shown in Figure 2 is time invariant or dependent on contextual development of party systems.

Figure 2: Relationship Remittances, Clientelism, Voting
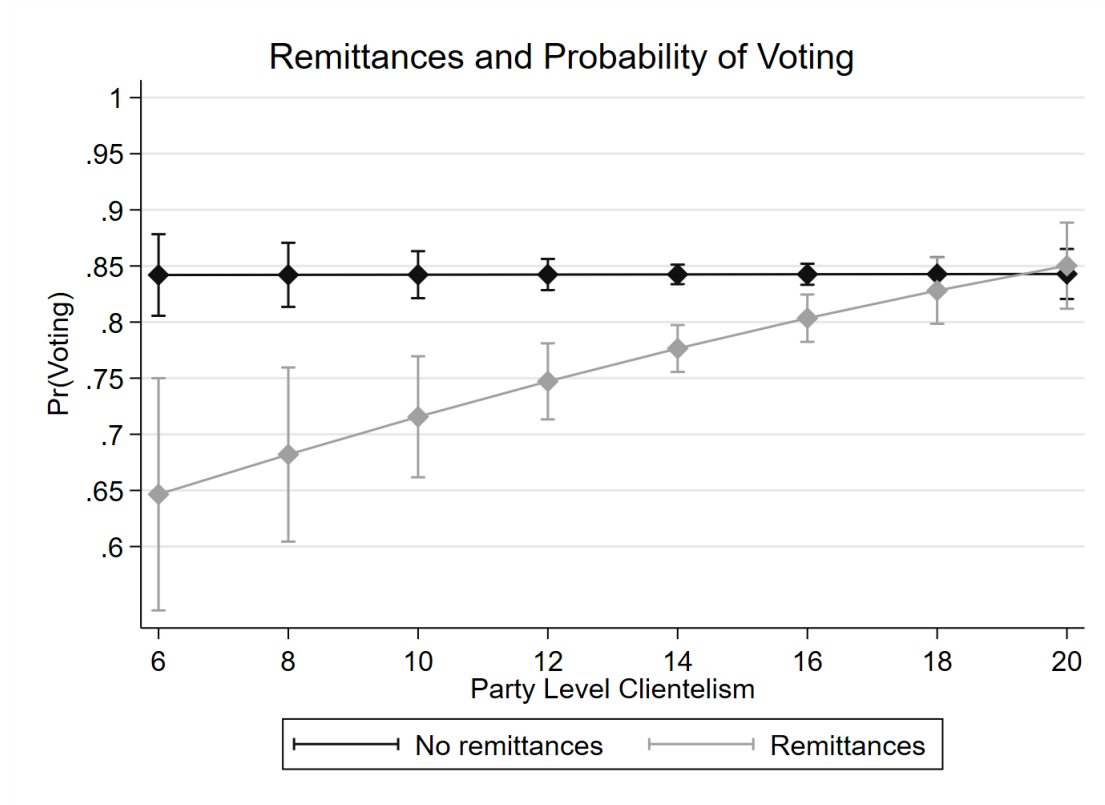
4 Conclusion

As opposed to the existing literature, which focuses on how remittances supposedly cause recipients to become detached from politics (Ahmed, 2017; Tertytchnaya 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

Voting – Propensity Score

	Raw	Matched
Urban	0.127	0.001
Male	0.000	0.012
Secondary Education	0.159	0.003
Employed	-0.083	-0.011
European Language	0.101	0.010
Lived Poverty	-0.066	0.006
Prior Migration	0.445	0.000
Age	-0.013	0.007
Mobile Phone	0.117	0.005
Log GDP / Capita PPP	-0.058	0.030
V-Dem Score	0.007	0.007
British Colony	-0.027	-0.014
French Colony	-0.033	0.003

Voting – Nearest Neighbor

	Raw	Matched
Urban	0.127	0.003
Male	0.000	-0.009
Secondary Education	0.159	-0.001
Employed	-0.083	-0.020
European Language	0.101	-0.001
Lived Poverty	-0.066	-0.038
Prior Migration	0.445	-0.002
Age	-0.013	-0.033
Mobile Phone	0.117	0.008

Propensity Score Overlap Plots

36
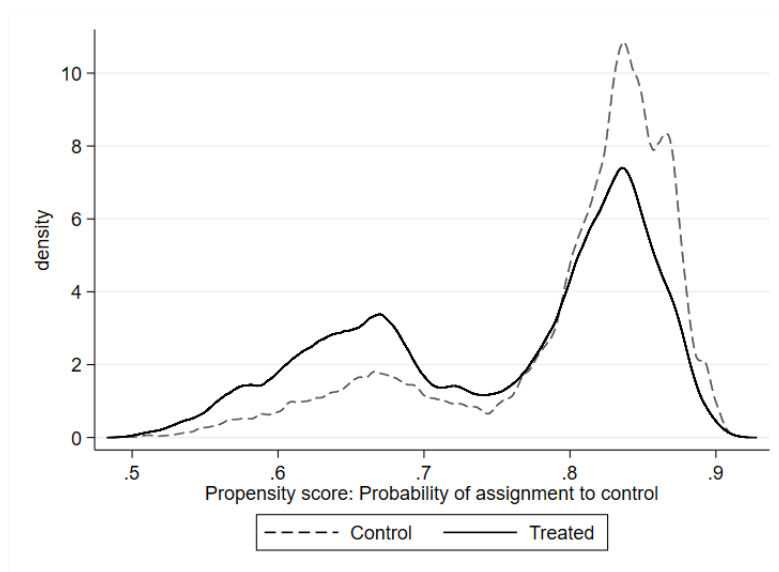
C   Sequential Causal Models: Assessing Phantom Counterfactuals

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 (ibid).

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 inside the text), 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	Voting
Remittances	0.165***	0.254***	0.0225	0.288***	0.332***	0.124***	-0.236***
	(3.52)	(5.71)	(0.46)	(5.42)	(5.05)	(7.04)	(-4.66)
Muslim	0.104**	0.212***	-0.0297	0.0552	0.0936	0.0513***	0.126***
	(2.93)	(5.49)	(-0.79)	(1.33)	(1.75)	(4.45)	(2.74)
Remittances x Muslim	0.134	-0.0610	0.0190	-0.0731	0.00769	-0.0118	0.198*
	(1.92)	(-0.90)	(0.27)	(-0.93)	(0.08)	(-0.50)	(2.55)
Full Controls	YES	YES	YES	YES	YES	YES	YES
N	43,869	43,671	44,037	43,753	43,698	42,616	39,385

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	Voting
Remittances	0.241	0.114	0.0430	0.121	0.185	0.0774	-0.0827
	(1.84)	(0.99)	(0.33)	(0.78)	(1.01)	(1.59)	(-0.59)
Polyarchy	0.384	-0.0587	0.607	0.446	-0.00544	0.0718	1.289*
	(1.01)	(-0.14)	(1.28)	(1.27)	(-0.01)	(0.86)	(2.54)
Remittances x Polyarchy	-0.0571	0.228	-0.0260	0.266	0.277	0.0808	-0.161
	(-0.24)	(1.08)	(-0.11)	(0.98)	(0.85)	(0.92)	(-0.64)
Full Controls	YES	YES	YES	YES	YES	YES	YES
N	43,869	43,671	44,037	43,753	43,698	42,616	39,385

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	Voting
Remittances	0.172***	0.232***	0.0452	0.251***	0.240***	0.106***	-0.192***
	(3.56)	(4.78)	(0.86)	(4.42)	(3.37)	(5.87)	(-3.38)
Perceived Democracy	-0.0436	0.0738*	-0.108***	0.129***	0.0745	0.0206*	0.277***
	(-1.70)	(2.52)	(-4.01)	(4.27)	(1.90)	(2.41)	(8.76)
Remittances x Perceived Democracy	0.0816	0.00949	-0.0393	0.0450	0.168*	0.0347	0.0598
	(1.49)	(0.16)	(-0.70)	(0.75)	(2.29)	(1.95)	(0.95)
Full Controls	YES	YES	YES	YES	YES	YES	YES
N	41,556	41,383	41,667	41,443	41,401	40,519	37,290

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	Voting
Remittances	0.214***	0.224***	0.0591	0.192**	0.339***	0.0860***	-0.0984
	(3.91)	(4.11)	(1.07)	(3.16)	(4.46)	(4.45)	(-1.65)
Lived Poverty	0.0746***	0.205***	0.125***	0.133***	0.149***	0.0614***	0.00934
	(5.17)	(12.62)	(8.20)	(7.94)	(6.86)	(12.86)	(0.52)
Remittances x Lived Poverty	-0.00262	0.00637	-0.0261	0.0603	-0.00492	0.0291**	-0.0596
	(-0.09)	(0.20)	(-0.84)	(1.84)	(-0.12)	(2.95)	(-1.72)
Full Controls	YES	YES	YES	YES	YES	YES	YES
N	43,869	43,671	44,037	43,753	43,698	42,616	39,385

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	Voting
Remittances	0.709	-0.306	-0.419	1.052*	-0.175	0.227	0.144
	(1.66)	(-0.80)	(-0.96)	(2.11)	(-0.30)	(1.40)	(0.30)
Log GDP	-0.124	-0.267**	-0.192	-0.0535	-0.0738	-0.0275	-0.394***
	(-1.44)	(-2.89)	(-1.78)	(-0.65)	(-0.84)	(-1.45)	(-3.44)
Remittances x Log GDP	-0.0607	0.0662	0.0544	-0.0965	0.0615	-0.0130	-0.0377
	(-1.17)	(1.42)	(1.04)	(-1.60)	(0.88)	(-0.66)	(-0.66)
Full Controls	YES	YES	YES	YES	YES	YES	YES
N	43,869	43,671	44,037	43,753	43,698	42,616	39,385

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 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	Voting
Remittances	0.173***	0.172***	0.229***	0.244***	0.249***	0.0946***	-0.165**
	(3.74)	(5.19)	(3.64)	(4.17)	(3.91)	(5.37)	(-3.07)
Pro-Democratic	-0.348***	-0.115**	-0.0247	-0.139*	-0.0653	-0.0539***	-0.317***
Attitudes	(-9.13)	(-3.17)	(-0.35)	(-2.64)	(-1.25)	(-3.78)	(-6.97)
Remittances x Pro-Dem	0.101	0.214***	0.371***	-0.00203	0.185*	0.0697**	0.0551
Attitudes	(1.72)	(3.65)	(4.92)	(-0.02)	(2.04)	(3.09)	(0.84)
Full Controls	YES	YES	YES	YES	YES	YES	YES
Country FEs	YES	YES	YES	YES	YES	YES	YES
N	42,065	41,897	41,557	41,966	41,924	41,004	37,742

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

Includes cross-country sample weighting.

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

The Prodigal Child Returns? Attitudes Towards Return Migration in a Developing Economy

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Abstract

Data estimates suggest that up to half of all migrants return to the country of origin within 5 years of leaving. Return migration is known to be a boon for the local economy and a catalyst for political reform. However, these effects are conditional on successful reintegration, which is dependent on the preferences of non-migrants. What causes negative attitudes towards return migration, given its significant potential economic benefits? I argue that non-migrants are concerned about both the economic and political competition of returnees. Non-migrants prefer to welcome back migrants who can bring back home financial capital and employment, but will oppose competitors on the job market when unemployment is high. Furthermore, non-migrants are concerned about the potential role of return migrants as norm entrepreneurs. I test my hypotheses with a conjoint survey experiment conducted in Colombia, as well as an analysis of the 2016 peace referendum.

1 Introduction

Migration is arguably the most politically charged component of contemporary globalization, and it has become an increasingly important facet of the foreign policies of both democratic and authoritarian states (Gamlen 2019; Graham 2019; Miller and Peters 2020). Diasporas have helped their home countries access global finance through remittances, and have helped usher in democratic reform in many developing countries (Chauvet, Gubert and Mesplé-Somps 2016). However, migration is often a temporary event for individuals. Data estimates suggest that up to 50% of migrants return to their country of origin within 5 years of migrating (Wahba 2014), a number which grows even further when taking into account labor migrants specifically (Bijwaard, Schluter and Wahba 2014; van Stiphout-Kramer et al. 2023). Another recent empirical study suggests that 1 in every 4 migration events is an instance of return (Azose and Raftery 2019). Even in the context of ongoing conflict, migrants abroad often choose to return home, for example in the case of the ongoing war in Ukraine.

In general return migration is associated with strong positive sociotropic effects, aiding local economic development (Bucheli and Fontenla 2022), increasing demands for government accountability (Batista and Vicente 2011), and depressing criminal violence (Bucheli, Fontenla and Waddell 2019). However, despite these positive sociotropic effects, firms in emerging economies have been found to discriminate against returnees (Abarcar 2016), and returning refugees often face violence from their compatriots when coming back home (Schwartz 2019). This limits the potential of return migration to lead to positive spillovers for society. What determines whether return migrants reintegrate successfully? Existing explanations focus on the role of conflict between returnees and non-migrants (International Organization for Migration 2016; Mueller and Kuschminder 2022). Therefore, understanding what determines the attitudes of stay-behind locals is an important step in understanding the conditions under which migrants can contribute meaningfully to their communities of origin after return, especially in the context of developing countries and post-conflict settings.

By now there exists a prolific literature on what determines attitudes towards immigrants in North America and Western Europe. Among the emphasized explanations are labor market competition and complimentarities (Baccini, Lodefalk and Sabolová 2024; Dancygier and Donnelly 2013; Malhotra, Margalit and Mo 2013; Pardos-Prado and Xena 2019; Scheve and Slaughter 2001), sociotropic attitudes (Hainmueller and Hopkins 2015; Jeannet 2018; Valentino

et al. 2019), and cultural anxiety effects (Clayton, Ferwerda and Horiuchi 2021; Goldstein and Peters 2014; Hedegaard and Larsen 2023; Semyonov et al. 2023; Ward 2019). However, the focus of these studies is almost exclusively on attitudes in the advanced economies of the OECD. Others have rightfully pointed out that a nativist backlash against immigrants can occur in emerging economies and non-OECD countries as well (Helms 2024; Singer and Quek 2022; York 2022). Yet, even these studies tend to focus on first-time immigrants: newcomers to the polity who are more often than not racially or ethnically distinct from the native population. While we have some indication of the attitudes of non-migrants to those who leave (Kustov 2022), we have little knowledge about the attitudes towards emigrants that make the decision to return to their country of origin. Given the importance of return migrants for their countries of origin, there is a pressing need to incorporate returnees in our understanding of what drives opposition to global human mobility.

Returnees[1] are not comparable to first-time migrants on several economic, political, and ethnic dimensions, and therefore we cannot assume that existing explanations for attitudes towards immigrants apply, even for those studies that examine attitudes in non-OECD economies (Kustov 2022; Singer and Quek 2022; York 2022). For example, while Colombians may perceive Venezuelan migrants to be politically to the left of them (Holland, Peters and Zhou 2024), we cannot assume that their perception of fellow Colombian nationals returning from Venezuela are similar. Furthermore, research in development economics shows that returnees tend to bring home savings and are more likely to start their own businesses (Abainza and Calfat 2018; Martin and Radu 2012; Wahba 2014). While concerns over immigrants in Western countries may indeed be due to concerns over labor market competition or fiscal burdens (Scheve and Slaughter 2001), return migrants are far less likely to be labor market competitors: instead they often bring employment to local communities by generating entrepreneurship (Hagan and Wassink 2020). Furthermore, while immigrants are ethnically distinct from natives and this may generate anxiety among native populations (Semyonov et al. 2023; Ward 2019), returnees are not directly visually distinguishable from non-migrant compatriots.

If mainstream accounts of attitudes towards immigrants cannot account for backlash against returnees, what can? I argue that the preferences of non-migrants over reintegration are moderated by both economic and political characteristics of the returnees. Specifically, locals

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[1]In this paper, I choose to focus on individuals born in a particular country, who leave that country as adults and come back after a particular period spent abroad. This excludes individuals born in the country who left as children as well as second generation migrants who may contemplate return. This helps mitigate concerns about perceived cultural differences between returnees and natives.

will oppose not only prospective returnees that represent economic competitors (either as labor or as entrepreneurs), but also those that are likely to bring home political norms that go against locals' own preferences, while preferring returnees that are labor market complimentary and support the same political norms. Furthermore, locals will generally be more positive towards return migrants that bring home financial capital in the form of savings. I test these predictions using a conjoint experiment fielded in Colombia in the Spring of 2024. Colombia makes for a good testing ground of my hypotheses, as return migration is a salient political topic due to the legacy of the civil conflict and the reintegration of both rebels and refugees. Furthermore, many of those who have returned to Colombia since 2016 have become active in local advocacy groups and political organizations (Díez Jiménez, Cabrera Izquierdo and Márquez Guerra 2021). I also provide observational evidence that concerns over the economic and political effects of return migrants moderated votes for the 2016 Peace Referendum, providing some measure of external validity for my experimental results in a high-stakes, closely contested election.

This paper contributes to several research agendas in political science and political economy. First and foremost, it extends a small but growing group of studies that examines attitudes to different forms of migration outside of the OECD by focusing on a group of migrants not previously considered by the literature: returnees (Helms 2024; Holland, Peters and Zhou 2024; Kustov 2022; Singer and Quek 2022; York 2022). Economic and political conflict between migrants and non-migrants is not unique to post-industrial democracies, and understanding the conditions under which such conflict becomes more inflamed matters for the building of peaceful and prosperous societies.

Second, it adds to the literature on the political consequences of migration and return for developing countries by flipping the script and examining the roles and attitudes of non-migrant locals, and how they react to returnees who may have an effect on the political status quo (Barsbai et al. 2017; Batista and Vicente 2011; Bucheli and Fontenla 2022; Bucheli, Fontenla and Waddell 2019; Chauvet, Gubert and Mesplé-Somps 2016; Chauvet and Mercier 2014; Gaikwad, Hanson and Toth 2023; Mercier 2016; Pérez-Armendáriz and Crow 2010; Spilimbergo 2009; Tuccio, Wahba and Hamdouch 2019; Vari-Lavoisier 2016). Return migrants can have far reaching effects on their home countries' politics and economy. Extending our understanding of these effects to attitudinal and behavioral changes among local non-migrants helps build a more thorough understanding of the economic and political consequences of human mobility.

Third, its empirical application speaks to the conditions under which the resolution

of civil conflict will be supported by citizens (Blair and Wright 2022; Branton et al. 2019; Esparza et al. 2020; Hazlett and Parente 2023; Kennard, Sonin and Wright 2021; Pechenkina and Gamboa 2022; Tellez 2019). Sustainable peace can only be achieved if the underlying causes of conflict do not flare up again between those who attempt to reintegrate, and those who never left. Understanding the drivers of conflict between return migrants and non-migrant locals therefore matters extensively for how we think about the politics of development, post-conflict peace building, and the political and economic role of the diaspora in the modern transnational state.

The theory and results presented in this paper should be of interest not only to political scientists and political economists studying migration, integration, and development due to its novel predictions and results, but also to public policy professionals. The International Organization for Migration has recently published work highlighting the possible positive developmental consequences of return migration, but also acknowledges that these positive externalities are moderated by successful reintegration upon return (International Organization for Migration 2022). My results suggest that public policy in migrant destination countries should emphasize migrant employment and savings schemes, and limit barriers to migrant transnational capital flows so that individuals can bring financial capital home with them upon return. Furthermore, countries of origin can help successful reintegration by incentivizing the reinvestment of money saved abroad to maximize positive spillovers.

2   The Consequences of Return Migration

Governments in developing countries are aware of the positive consequences of both emigration and return migration, and try to enact policies that maximize economic gains while balancing potentially detrimental political consequences for the regime (Graham 2019; Miller and Peters 2020; Peters and Miller 2022). Remittances send from abroad by migrants can stabilize access to global capital due to their counter-cyclical nature, and return migrants possess valuable human capital that can be turned towards productive use. While working or studying abroad, migrants gain knowledge, and form networks that can help promote trade, foreign direct investment, and financial services (Leblang and Helms 2023). The idea of "diaspora politics" has gained ground in recent years to capture the various policies enacted by governments to keep in touch with their citizens (or the descendants of their citizens) living abroad (Gamlen 2019). Such policies are

usually enacted with the goal of incentivizing migrants to send home financial and human capital in order to aid with the economic growth of the home country. The return of migrants achieves both, as they bring home earned savings as well as skills and innovative ideas (Tuccio, Wahba and Hamdouch 2019; Wahba 2014). Broadly speaking, the literature separates the consequences of return migration into two categories: economic and political / normative.

2.1 Economic consequences

While refugees and family migrants occasionally return to the country of origin as well, the majority of those who return are labor migrants and students (van Stiphout-Kramer et al. 2023). The initial reason for emigration of these two groups is explicitly economic, and so it is no surprise that the bulk of returnee studies focuses on the economic consequences of return. This research has found that, compared to similar non-migrants, returnees are more likely to be entrepreneurs, a feature that is typically attributed to a combination of financial and human capital garnered abroad, both of which facilitate setting up a successful business venture of one's own (Martin and Radu 2012). In fact, some have shown that return migrants are not only more likely to be self-employed, their business ventures are also more likely to take on additional employees (Hagan and Wassink 2020, p.536). Furthermore, returnees raise demand for durable goods such as real estate, providing further jobs for local citizens in construction (Abainza and Calfat 2018). In general, economic research has shown that migrant return leads to local economic development, aiding not only employment and wages, but also health care access and school attendance, while lowering food poverty (Bucheli and Fontenla 2022). Return migration has also been found to lead to decreases in violent crime rates in Mexico (Bucheli, Fontenla and Waddell 2019). In summary, returnees not only improve the economic and social health of their own households, but also those of the communities they return to.

It is important to note that these benefits are conditional on migrants returning voluntarily and having been gainfully employed while abroad, and that not all returnees are equally successful in achieving their migration goals. While some migrants acquire savings and skills abroad, others are unable to find employment and return without financial or human capital. The existing literature typically distinguishes between return of failure, return of conservatism, return of retirement, and return of innovation (Cerase 1974; Kunuroglu, van de Vijver and Yagmur 2016). These categories vary according to their economic success in the destination country, the eventual goal of their migration journey, and their intended activity in the home country.

The last group – the innovators – may or may not have had an ex ante intent to return, but ended up integrating reasonably well abroad, acquiring not only financial capital, but also social and human capital, potentially even seeing themselves as agents of change for their country of origin. Paradoxically, they are also most likely to perceive cultural stigma in return due to their adoption of norms overseas (Mueller and Kuschminder 2022). This observation further emphasizes the need to disentangle the causal mechanisms that lead to returnee-local conflict. If the explanation was wholly economic, one would expect to see successful returnees perceive less stigma than non-successful returnees.

2.2   Political consequences

Return migration has political consequences as well as economic ones. Individuals educated or employed abroad usually pick up political norms in the host country, and can promote those norms upon their return to the country of origin (Gaikwad, Hanson and Toth 2023; Mercier 2016; Spilimbergo 2009). While foreign migrants can become politically active as well, they face high barriers to doing so, making returnees a far more likely case for gaining political influence after (return) migration. Case study evidence suggests that return migrants were instrumental in enabling regime change in Senegal (Vari-Lavoisier 2016), and Mali (Chauvet and Mercier 2014; Chauvet, Gubert and Mesplé-Somps 2016). Even when they do not directly help usher in a new political era, they still help mobilize non-migrants and raise electoral turnout, suggesting that returnees' experience abroad matters not only for markets, but for states as well (Bucheli and Fontenla 2022; Pérez-Armendáriz and Crow 2010). Interestingly, these effects are driven by both grass-roots organizers demanding bottom-up accountability (Batista and Vicente 2011; Pérez-Armendáriz and Crow 2010), and elites imposing democratic reforms from above after being educated abroad (Mercier 2016; Spilimbergo 2009).

In explaining these effects, the literature relies on theories of norm transfer. While abroad, migrants are faced with political institutions, cultures, and norms different from their own. To some extent, migrants internalize these norms, and update their expectations of what they can legitimately expect from their home country government. As a result, returnees are more likely to demand accountability after they return. Batista and Vicente (2011) show, using a costly action experiment conducted in Cape Verde, that returnees are not only more likely to demand transparency and accountability from their representatives, their norms are transferred to their local communities. Furthermore, these effects are far stronger for returnees

who came back from more democratic countries. Similarly, Barsbai et al. (2017) find that Moldovan migrants returning from the EU decrease local support for the Communist Party, whereas returnees from Russia have no such effect. These results suggest that norms transfer is the mechanism through which return migration affects home country changes in political culture and institutions. As norm adoption is dependent on the political conditions in the host country, this literature emphasizes the conditional effects of return as a function of host country regime type and political liberties.

Some authors place an important caveat to findings that suggest the pro-democratic effect of return migration. Changing the norms of migrants to be more pro-democratic and favorable to social out-groups requires that they actually benefit from liberalism and democracy while abroad. Rother (2009) shows that emigrants from the Philippines update their preferences in favor of democracy only when their rights are not violated in practice, taking into account the level of democracy in the host country. Similarly, Fan et al. (2020) find that Chinese students in the United States become less in favor of democratic norms if they experience discrimination or harassment while abroad. These are valid concerns, and the results presented later in this article should be interpreted with this point in mind. For the purpose of this article, it is assumed that migrants residing in more democratic countries will adopt more democratic norms on average, a fair assumption given the findings in the literature (Barsbai et al. 2017; Maydom 2017; Mercier 2016; Pérez-Armendáriz and Crow 2010; Spilimbergo 2009; Tuccio, Wahba and Hamdouch 2019). Looking forward to my empirical strategy, willingness to become involved in politics and the initial migration destination are separately randomized in my experimental study in order to address any concerns one may still have. In the observational component of the study, political preferences of the diaspora are inferred from their actual voting record.

Of course, return migration is a phenomenon with a double selection process. First comes the decision to emigrate, then the decision to return or not, and when (Wahba 2014). This suggest that returning migrants may have had political differences with non-migrants prior to emigrating, which is an argument that has recently received considerable attention in studies of intra-European migration (Auer and Schaub 2024; Lim 2023). However, such baseline differences are not anticipated to be sources of opposition to return for several reasons. First, if concerns over pre-existing differences fully accounted for opposition to returnees, then locals should be positively predisposed to the exit of those same individuals. However, existing research has indicated that this is not generally the case: emigration is often viewed negatively

by local citizens (Kustov 2022). Second, experimental research has shown that migrants do in fact pick up norms while abroad and that differences in social and political attitudes between migrants and non-migrants cannot be wholly chalked up to pre-existing baseline differences. Having to invest in foreign languages and being exposed to foreign norms can affect attitudes of migrants (Gaikwad, Hanson and Toth 2023). Even in cases where migrants are exploited for their labor, merely being experiencing foreign institutions and political ideas allows for a meaningful comparison with the situation in the home country, and cause return migrants make political demands upon return (Gamio 1930).

Despite these great studies, there is virtually no research about how citizens in countries of origin view the diaspora or prospective returnees, despite their potential to affect both the economy and broader political norms and values. Given that successful reintegration upon return strongly affects the development potential of return migration (International Organization for Migration 2022; Kunuroglu, van de Vijver and Yagmur 2016; Wahba 2014), and returnees are not universally embraced by their compatriots (Abarcar 2016; International Organization for Migration 2016; Mueller and Kuschminder 2022; Schwartz 2019), this question is not without import. In order to improve upon the development impact of return migration, it is important to understand how those who stayed behind respond to returnees, which kinds of returnees are evaluated most positively, and by extension how governments in both host and origin societies can improve reintegration to help promote sustainable local economic development. I now move on to developing a theory that provides predictions for under which conditions returnees are welcomed or opposed by non-migrants.

3 Economic and Political Competition

There are good reasons to expect returnees to be fundamentally different from other types of migrants in ways that meaningfully affect the expectations of existing theories on public attitudes toward migration. Just as citizens in immigration countries have a sense of moral obligation to those already residing in the country (Margalit and Solodoch 2022), citizens in emigration countries may have a sense of moral obligation towards returnees due to a shared nationality. Alternatively, natives may be weary of returnees, as they represent new insights, norms, and values gained abroad. Natives may also see emigrants as "traitors" to the homeland, taking with them important resources for national development, and thus view their return negatively

for sociotropic reasons (Kustov 2022). On the other hand, if returning migrants bring more resources home with them, they may be evaluated more positively. How are returnees different from other forms of migrants, and how might this affect how non-migrant locals react to their return?

First of all, because they return to their country of origin, anxieties pertaining to ethnic, racial, or religious diversity are likely to be less acute. Existing studies on attitudes to immigrants emphasize the importance of perceptions of "cultural threat" for both Western (Semyonov et al. 2023; Valentino et al. 2019; Ward 2019), and non-Western countries (Singer and Quek 2022; York 2022). Return migrants, by definition, do not represent the same level of threat to natives, as they speak the same language and grew up in the same communities prior to leaving the country. Indeed, as a subset of all migrants, those who return voluntarily are those who are more likely to have retained the habits and norms of their countries of origin, especially compared to migrants who decide to integrate in the host society and are not committed to return. Furthermore, return migrants are visually non-distinguishable from natives, and are thus less likely to prime xenophobic attitudes based on racism and ethnocentrism. Therefore, arguments centered around racial or ethnocentric as explanations of attitudes to migrants are unlikely to be as persuasive for return migration as they are for immigration.

However, despite visual similarities, economic considerations may still be cause opposition to return migrants. Kennard, Sonin and Wright (2021) show that those residing in regions more affected by negative economic shocks are less likely to support negotiating an end to civil war, and reintegrating rebel fighters. Other authors have shown that, even though returnees may decrease insurgent violence by raising reservation wages, they can also increase social conflict due to competition for scarce resources (Blair and Wright 2022). Thus, even when returnees are ethnically homogeneous and similar to locals, and their return is associated with a strong positive sociotropic effect – ending civil conflict – local economic conditions may still determine opposition to inward migration, even from co-ethnics. Locals can anticipate either labor market or price-demand shocks due to returnees and such anticipations moderate support for the reintegration of returnees, especially when local economic conditions are precarious.

Potential concerns over labor markets and resource competition notwithstanding, return migrants are, economically speaking, not equivalent to other types of migrants. Whereas most initial migrants (or for that matter returning insurgents as in Kennard et al., 2021) are likely job seekers, and thus potential labor market competitors to natives, returnees are not unambiguously

a threat to the employment opportunities of non-migrants. Economic research has shown that, compared to those who do not migrate in the first place, returnees are more likely to be self-employed (Martin and Radu 2012; Wahba 2014). Returnees also tend to generate local employment by increased demand for construction jobs (Abainza and Calfat 2018). These types of return migrants are not labor market competitors: if anything they should increase demand for labor by generating employment opportunities and access to financial capital, tightening, rather than loosening local labor markets, and thus raising the wages of non-migrants, rather than competing for the same jobs. Stay-behind job seekers therefore likely support return migration, if they believe returnees will offer employment opportunities. By contrast, small local firms, who may not be able to compete with returnees' accumulated financial and human capital, will likely oppose returnee reintegration for fears of losing their market share.

Additionally, research has shown that natives may oppose migrants not due to direct labor market competition, but rather because of concerns over the higher fiscal burden potentially imposed by immigrants. In the context of immigration into North America and Western Europe, scholars have argued that opposition to immigration may run through cost-benefit expectations regarding social services and public finance, rather than labor markets (Cordero, Zagórski and Rama 2023; Scheve and Slaughter 2001). Because of ideas that immigrants make more use of public services while contributing less to taxes,[2] people may oppose immigration due to concerns over public spending. Again, however, such concerns do not translate clearly to the context of return migration. Those returnees who managed to be meaningfully employed in the host country bring home savings (Martin and Radu 2012; Wahba 2014), and tend to improve investment in local public goods (Bucheli and Fontenla 2022). In general, this suggests that non-migrants will have a preference for "successful" returnees (Cerase 1974; Kunuroglu, van de Vijver and Yagmur 2016): those who return with financial capital that can offset potential greater demands on public services, or help provide for public goods through private investment.

Hypothesis 1. *Non-migrants prefer returnees who bring home financial capital in the form of savings.*

Hypothesis 2. *Non-migrants who are employed by others or seeking employment prefer returnees intending to start their own business, and non-migrants who are self-employed prefer returnees*

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[2]The economic literature provides no consensus on whether the migrant welfare burden is real or not. For the present discussion, this is irrelevant: what matters is that many people *perceive* migrants to make a disproportionate use of public services while contributing less to public finances

intending to become employed by others.

The potential influence of return migrants is not merely economic, however. Returnees do not only bring home financial or human capital, but also political capital. A bevy of studies from the social remittances literature has shown that members of households with a returnee from a democratic country hold stronger pro-democratic attitudes than households with no returnees or households with returnees from autocratic countries (Batista and Vicente 2011; Barsbai et al. 2017; Maydom 2017; Pérez-Armendáriz and Crow 2010; Tuccio, Wahba and Hamdouch 2019). Furthermore, return migrants that adopted pro-democratic attitudes abroad are more likely than non-migrants to publicly advocate and work towards the liberalization of their polities, if they returned to an autocratic or flawed-democratic home country (Chauvet and Mercier 2014; Chauvet, Gubert and Mesplé-Somps 2016; Vari-Lavoisier 2016).

Much like the economic effect of returnees can generate winners and losers on an economic level, so too can their political entrepreneurship generate winners and losers on a political level. Not every citizen living in an emerging economy holds pro-democratic attitudes, and support for such undemocratic events such as military coups remains fairly high across countries (Acevedo 2023; Konte 2016). For those that do not hold pro-democratic attitudes, returnees from democratic countries, who may engender political change, can be a threat to their preferred regime type and a favorable status quo. Furthermore, norm transfers could be less positive as well. For example, Tuccio and Wahba (2018) find that labor migrant return from more conservative countries to Jordan can lead to lower female empowerment in the household. Again, those who hold preferences that run against those that could be promoted by return migrants will oppose those returnees settling in their communities.

Hypothesis 3. *Non-migrants with (non-)democratic preferences prefer returnees from (non-)democratic countries.*

Hypothesis 4. *Non-migrants with (non-)democratic preferences prefer returnees who (do not) intend to become politically active, conditional on the migrant returning from a democratic country.*

These hypotheses capture various components of and reasons for return. In doing so, the theory captures many different types of return migrants, including returning labor migrants, refugees, and political exiles. Nevertheless, I do not include reason for emigration in my

hypotheses for two reasons. First, while these categories are used extensively in public discourse, they are difficult to delineate clearly in academic work, as many refugees and political exiles also end up employed and in a position to gather savings, while labor migrants may have had secondary political reasons for emigration. To the extent that there are systematic differences between these groups, they are captured by the hypotheses and variables used in this study. Second, returning labor migrants and refugees likely do not label themselves explicitly as such when returning. The case of Colombia, discussed later in the paper, illustrates this dynamic well. In its *Colombia Nos Une* program (Colombia Unites Us), the government makes no distinction between returnees that were labor migrants or refugees, and so locals have only the impact of returnees after return to go on while former their attitudes and evaluations of them.

In summary, I argue that the determinants of preferences over return migrants are both economic and political: I argue that non-migrants are as much concerned with the potential economic impact of returnees (based on whether or not they bring home savings, as well as their prospective role in the local labor market) as with their prospective political impact as norm entrepreneurs (based on which country they return from and whether or not they intend to become involved in politics). Overall, those who do not migrate should prefer returnees who provide labor market complimentarities and who offset demand for public services by bringing private savings, as well as returnees who intend to promote political norms in line with their own preferences.

4   Returnees and the Colombian Civil War

I test my hypotheses in the context of Colombia, where two civil wars, *La Violencia* (1948–1958) and the so-called Colombian conflict (1986–2016), have caused large scale internal displacement and outward international migration. This makes it an excellent case to provide an initial test for my novel theory, as in the post conflict era (2016 onward) repatriation and returnee reparations have become a politically salient topic (Sánchez Léon 2021). Returning migrants are not just the passive target of politics, however, they also actively make claims on resources provided by the state after return. Local estimates suggest that about one in three returnees have become active in some sort of local political or advocacy group since returning (Díez Jiménez, Cabrera Izquierdo and Márquez Guerra 2021, p.201). The political facets of return migration makes Colombia a good first test of my theory.

During *La Violencia*, Liberal and Conservative communities segregated spatially in response to the violence, as members of both groups were targeted and discriminated against by the other (Steele 2017, p.69). Indeed, even where Liberals wanted to return to their communities of origin, their ability to do so was often hampered by their political affiliation: local Conservative rulers demanded Liberals openly change their political affiliation or be driven off (Steele 2017, pp.70–71). During the second civil war, displaced communities and individuals were those with high levels of social capital and political organization, as they were deliberately targeted by insurgents and counter-insurgents (Steele 2017). This matters because it means the hypothesis that non-migrants prefer or oppose returnees who intend to become politically active is both testable and mirrors the political reality of Colombia. The idea that returnees will organize politically is believable in this context, because their penchant for organization is what led to them being targeted for forced displacement in the first place. The empirical record supports this assertion (Díez Jiménez, Cabrera Izquierdo and Márquez Guerra 2021).

After a first referendum on a peace deal between several armed groups and the Colombian government narrowly failed to get majority support on October 2, 2016, the Congress of Colombia ratified a revised peace deal in November of the same year. Article 5.1.3.5 of the agreement includes provisions for the return of displaced persons.[3] Far from being empty rhetoric, existing findings emphasize that government officials are indeed more responsive to victims of the conflict than they are to regular citizens (Barceló and Vela Barón 2024). Interestingly, the Colombian diaspora was heavily involved in advocating for the peace process (Bermudez 2011), suggesting that their political preferences aligned with those of the incumbent President Santos. During the Referendum, expatriate Colombians overwhelmingly supported the Peace Deal, although there were some exceptions to this rule, most notably Colombians living in the United States.

Given the history of *La Violencia* and the politically targeted nature of the displacements in the more recent conflict (Steele 2017), negotiating the return and reparations for IDPs and refugees has been fraught with conflict over how many can return, how their claims to land should be resolved, and how many reparations are appropriate, especially in locales where largely conservative landowners have an economic stake in preventing redistribution (Sánchez Léon 2021). Given these concerns about both the economic and political impact of returnees on local communities, Colombia makes for a good case for the testing of my hypotheses. Returnees have

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[3]A full English translation of the agreement is available through the Peace Agreements Database of the University of Edinburgh: https://www.peaceagreements.org/viewmasterdocument/1845, last retrieved the 10th of February, 2024. The full text of Article 5.1.3.5 is provided in the appendix.

been politicized through the peace process, and emigration patterns hold a particularly political connotation due to *La Violencia*.

While the Colombian case is illustrative in many ways, it is worth acknowledging the limits of its external validity. Migration and return migration in are salient topics in Colombia on account of its history of political violence. Displacement was politically targeted throughout the conflicts of the 20th and 21st centuries (Steele 2017), and the referendum itself was therefore lobbied by the Colombian diaspora (Bermudez 2011). In addition to large migration flows due to the legacy of the Colombian Conflict and Civil War, economic migration to the United States, Spain, and, prior to the collapse of the petrochemical industry and the economy, Venezuela was substantial as well. While Colombia's history of emigration being caused by both economic need and civil violence is hardly unique in the world (e.g. Sri Lanka, Mali, Mexico), the results presented later in the paper should be interpreted with these characteristics in mind. It is left up to future research to investigate if, and if so, how, these attitudes to returning migrants differ in situations of purely economic migration, versus repatriation after conflict.

5   Conjoint Experimental Design

I conduct a conjoint experiment to test my hypotheses. I designed and fielded a original survey fielded in Colombia during February and March of 2024. I recruited 1,572 respondents[4] from a panel maintained by the research firm Netquest, which has a strong track record of facilitating survey research in Latin America (Zhirkov and Smilan-Goldstein 2023). I used quota sampling to achieve a panel that matched the Colombian population in the breakdown of gender, age, and socio-economic status. Prior to completing the experimental tasks, respondents answered several questions regarding their political opinions, current labor market status, and migration background.[5]

Because there are divergent characteristics of immigrants, each of which may determine preferences of readmission among locals (potentially conditional on characteristics of the locals), the most obvious methodological approach to this problem would be the conjoint experimental approach, which has been widely used in existing studies of attitudes towards migrants (Clayton, Ferwerda and Horiuchi 2021; Hainmueller and Hopkins 2015; Hedegaard and Larsen 2023). The

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[4]The small deviation with the pre-registered sample size is due to an issue with the stopping rule when implementing the survey. The panel remains a random sample, and remains balanced on the main variables of interest.

[5]The research was approved by the author's IRB under protocol IRB-SBS #6356. An anonymized version of the study's preregistration can be found at https://osf.io/esm5f/?view_only=112ea2970c674d25a049d61c1931368b

conjoint design allows the researcher to test competing hypotheses: are attitudes predominantly about whether returnees bring home savings, their prospective political impact due to norm entrepreneurship, or their anticipated effect upon the local labor market? The conjoint method allows for the generation of comparable treatment effects across various competing dimensions. Furthermore, the conjoint method helps mitigate social desirability bias compared to standard survey experiments (Horiuchi, Markovich and Yamamoto 2022), which matters for a study that asks respondents about a potentially sensitive subject matter.

My conjoint experiment is set up as follows. After filling out a brief battery of questions looking to establish the political and economic preferences of respondents, as well as their baseline attitudes towards policies facilitating return as stipulated by Article 5.1.3.5 of the Peace Agreement, each respondent is shown two profiles of potential returnees, and then asked who they would prefer to see move into their community: the so-called forced choice design. Respondents are also asked to respond to a 10 point ordinal scale indicating their overall willingness to welcome the prospective returnee into their community. As in the majority of the literature on conjoint experiments and migration preferences (Hainmueller and Hopkins 2015; Zhirkov and Smilan-Goldstein 2023), I take as my main unit of analysis the profile, and as my main outcome whether or not that profile was selected as the preferable one by the respondent. Each respondent evaluated six pairs of profiles, meaning the total number of observations is $1,572 \times 2 \times 6 = 18,864$. I provide several robustness checks using the profile score as the outcome variable in the replication materials. Those robustness checks verify the results of the primary experimental analysis

Each returnee profile has four attribute values, which are independently randomized. These variables, and their potential values, are listed in Table 1. Hypotheses 1 and 2 are tested by the first two (economic) variables, and Hypotheses 3 and 4 are tested by the latter two (political) variables. In the experiment, the order of variables is randomized to account for the chance that the first attribute observed could consistently outperform the other variables. However, each respondent still views all their own profiles in the same order. To account for differences between respondents, I cluster standard errors at the respondent level, as is common practice for the conjoint method (Hainmueller and Hopkins 2015).

The choice for having the United States and Venezuela as potential values for the Country variable stems from both the theoretical literature and the empirical situation in Colombia. Theoretically, the literature emphasizes that returnees from democratic countries tend to bring

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Table 1: Experimental Variables and Potential Values

Variable Name	Potential Values
Savings	Has savings earned abroad
	Has no savings
Employment	Intends to start their own business
	Intends to work for someone else
Country	United States
	Venezuela
Politics	Wants to get involved in politics
	Does not want to get involved in politics

home pro-democratic norms, and that returnees from non-democratic countries may bring home non-democratic norms (Barsbai et al. 2017). Out of all countries with Colombian migrants, the United States and Venezuela host the most Colombians out of democratic and authoritarian countries respectively.[6] I include a pretest to establish a baseline for preferences by asking respondents whether they believe the government should facilitate the return of Colombian expatriates. Through the original survey, I also collect data on respondents' type of residence (urban or rural), their political leanings, their employment status, whether they have ever lived outside of Colombia themselves or whether they have family currently living abroad, as well as their preference for democracy. These variables can be used to predict baseline preferences for returnees, as well as indicators of heterogeneous treatment effects. In the main body of the article, I focus only on those interactions that were preregistered. The full text of the survey preamble – in both English and Spanish – and conjoint experiment task – in Spanish – are provided in the online appendix.
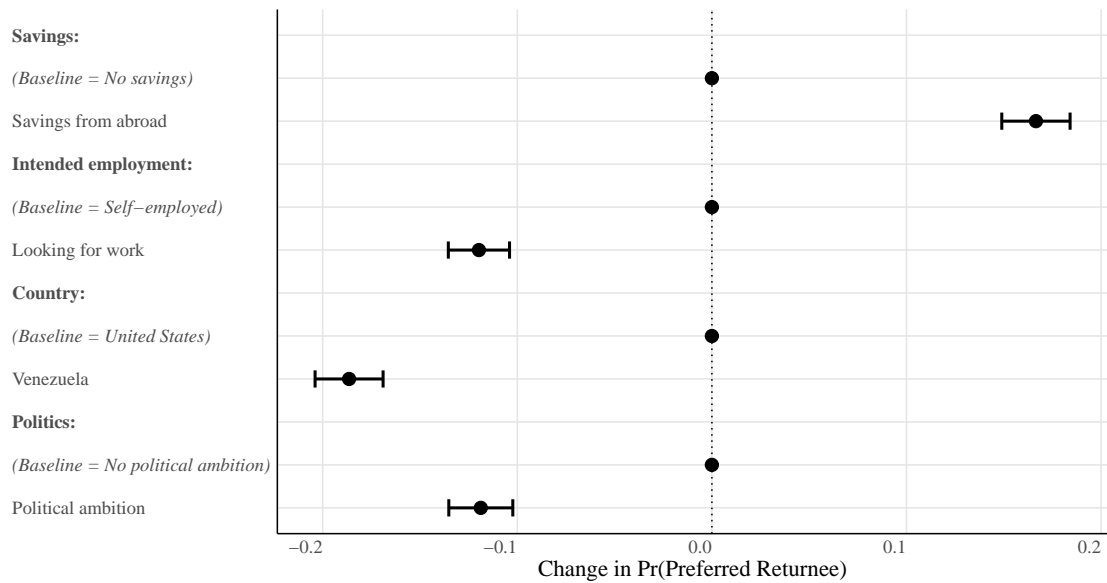
5.1 Results

The results of the unconditional effects of the variables from Table 1 are shown graphically in Figure 1. Results presented are based on the forced choice design, but are robust to using ratings as the outcome instead, as shown in the replication materials. Respondents have a strong preference for returnees with savings, and returnees intending to become self-employed. Conversely, respondents strongly disfavored returnees coming from Venezuela, as well as returnees who reported a desire to become politically active. The results of the savings variable provide

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[6]While concerns may exist that American and Venezuelan returnees do not bring home the same amount of financial capital due to vastly different levels of economic development and unemployment, the separate randomization of the Savings variable should account for this. Nevertheless, in my replication materials i show that the effect of the Savings variable does not depend on the Country variable, which is what one would see if respondents believe returnees from Venezuela bring home fewer savings than returnees from the United States.

strong support for Hypothesis 1, as savings earned abroad were a strong predictor of support for returnees. Regarding the next Hypothesis, I code as 1 any respondent who is employed by a private firm or the state, or who is actively seeking employment, and 0 otherwise. Results are shown graphically in Figure 2. At the 5% level, I find a statistically significant difference between the treatment effect in the two subgroups for the Employment variable. However, against my expectations, those who are self-employed are more opposed to job seekers as are those who are job seekers themselves.

Figure 1: Unconditional AMCEs

While not part of this study's preregistration, the existing literature provides a possible explanation for the results of Figure 2. Research by Hainmueller and Hopkins (2015) suggests that individuals evaluate migrants not based on concerns over competition rooted in factor-endowment models, but rather through sociotropic attitudes. To the extent that locals view those who intend to become self-employed positively for sociotropic reasons, i.e. based on their contribution to the community overall, this could account for the results in Figure 2. However, this potential explanation was not preregistered, nor was the conjoint experiment designed to account for it. Future research should explicitly design experimental conditions meant to test for sociotropic economic evaluations of returnees.

Regarding the political variables, I code as 1 any respondent who reports preferring democracy at any and all times, 0 otherwise.[7] Results are shown graphically in Figure 3. I find

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[7]The 0-category includes people who responded "In some circumstances, a strong non-democratic government may be preferable" or "For someone like me, it does not matter what kind of government we have".

Figure 2: Employment AMCE conditional on respondent's employment status
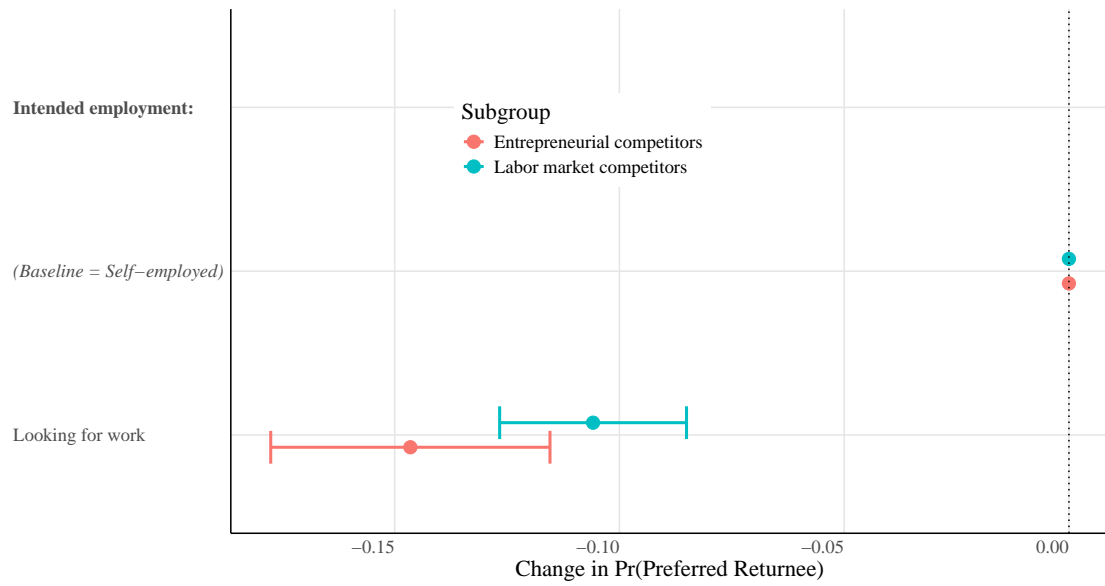
that those who prefer democracy have a stronger preference for returnees from the United States vis-à-vis returnees from Venezuela. Therefore Hypothesis 3 is backed up by the experimental results from my sample. However, the triple interaction between country of return, political ambitions, and respondent preferences for democracy are statistically insignificant, despite the effect being in the hypothesized direction. I am thus unable to find full support for Hypothesis 4. Overall, the experimental results provide strong support for Hypotheses 1 and 3, but not for Hypotheses 2 and 4.

Figure 3: Political AMCEs conditional on respondent's attitude to democracy
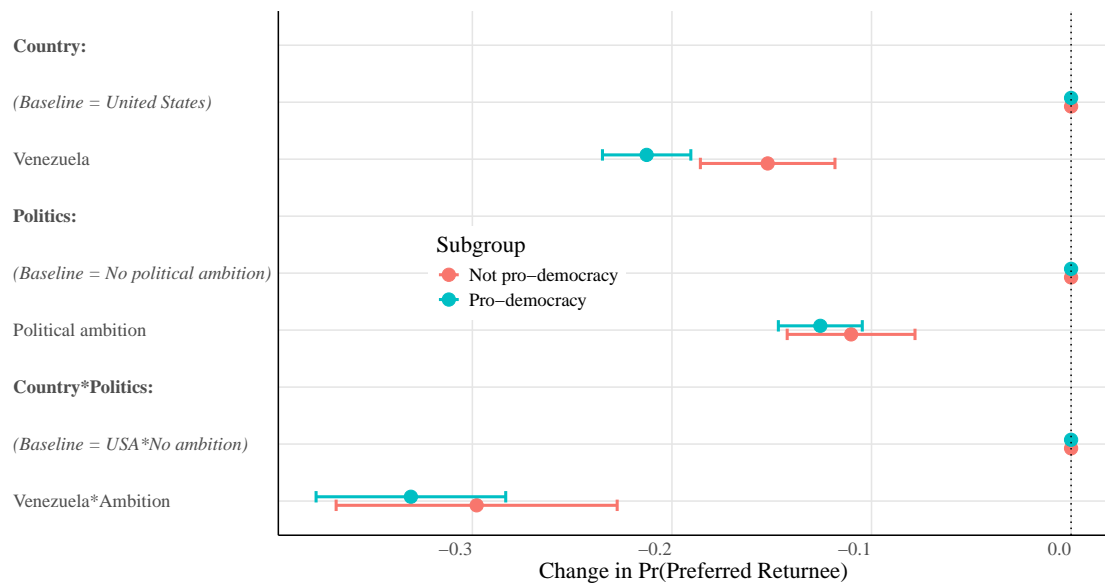
6 Observational Research Design

The experimental results presented above provide support for some of my hypotheses. However, are these stated preferences reflective of actual political behavior? In order to address this important issue, I provide a supplementary analysis based on the results of the 2016 referendum on the Colombian peace agreement.[8] The 2016 referendum, its defeat, and subsequent amended adoption by the Colombian Congress, mark arguably the single most important political event in Colombia this millennium. If the implications of my theory – that is to say, if the potential local effect of returnees matters for political behavior – prove to have predictive power vis-à-vis its outcome, it can be said to matter for actual political outcomes broadly conceived.

Previous explanations for support for the referendum focus on two variables: exposure to violence during the conflict and trust in and support for the Santos administration, which brokered the agreement (Branton et al. 2019; Esparza et al. 2020; Pechenkina and Gamboa 2022; Tellez 2019). Both are argued to increase support for peace, however, statistical support for the effect of political affiliation is more robust than that of exposure to violence (Hazlett and Parente 2023). Zooming in on specific arrangements in the agreement, Matanock and Garbiras-Díaz (2018) find that aspects of the deal that granted concessions and political power to former rebels were particularly unpopular, and even more so if they were endorsed by ex-insurgents. The role of displacement and migrant return is generally not considered among the relevant explanatory factors. Furthermore, the economic and redistributive effects of peace are left out of explanations for referendum support. Considering concerns over economic competition during peace building and post-conflict recovery have been shown to be important causes of opposition to peace (Blair and Wright 2022; Kennard, Sonin and Wright 2021), this omission is concerning, and my contribution is a first step to rectifying this lacuna.

Within this setting, one observable implication of my theory is that regions where more individuals were displaced during the conflict should have lower support for the peace agreement, as those who were not displaced are concerned about having to compete in loose labor markets or share scarce land resources. Compared to returning labor migrants, returning refugees are less likely to have access to earned savings. The hypothesized effect of net refugee flows should be conditional on the actual state of the labor market: where unemployment is low voters have little reason to be concerned about the level of competition that returnees may present. Thus, I

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[8]Unlike the experimental study, the observational component of the paper was not preregistered.

test whether the interaction between net refugee flows and municipal employment indicates that voters oppose potential returnees when labor markets are loose. I use municipal level voting results of the referendum, where the YES share of all valid YES and NO votes is the outcome of interest. There is considerable variation to explore in this outcome: at the municipal level, the lowest YES vote share is 16.2% in the municipality of *Vetas*, whereas the highest is 100% in *Morichal Nuevo*. A histogram of the distribution of this outcome at the municipal level is shown in Figure 4.

Figure 4: Distribution of yes vote share (municipalities)
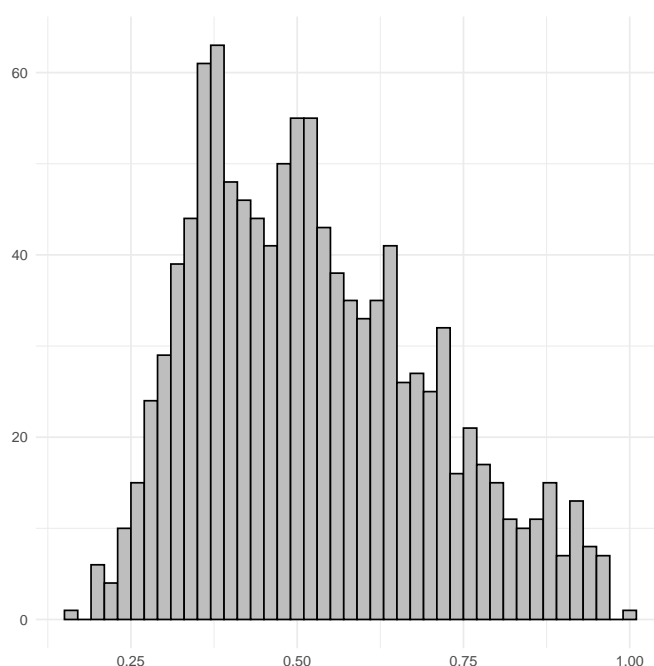
Data on emigration and violence come from the municipal data of the Economics Faculty of the University of the Andes (CEDE).[9] I use total number of FARC attacks, homicides, and subversive actions over the course of the conflict as controls for wartime violence. Results are robust to the inclusion of alternative forms of violence and violence perpetrated by other actors, as shown in the replication materials. I also include support for president Santos as an additional control, given that it has been by far the most influential explanation for the referendum results (Hazlett and Parente 2023).[10] Lastly, I include controls for municipal population, cocaine production, and quality of local government.

Unfortunately, exact data on municipal unemployment levels in 2016 are not available

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[9]https://datoscede.uniandes.edu.co/es/

[10]In the online appendix, I estimate models excluding support for Santos due to its potential role as a post-treatment collider. Results are robust to the exclusion of this variable.

even in the CEDE municipal data. Local unemployment at the time of the referendum would have been the best indicator for likely concerns over labor market competition due to returnees. Instead, I rely on the 23 departmental capitals for which unemployment data is available. While data on unemployment for all municipalities in 2016 is unavailable, the Colombian statistics office, DANE, does publish moving trimester unemployment data for the 23 departmental capitals. Thus, I test for the interactive effect of unemployment with net normalized refugee flows directly for this more limited panel, using unemployment data that runs from July to September 2016 (the referendum took place October 2, 2016).

For the second observable implication of my theory, Hypotheses 3 and 4 suggest that the effect of potential returnees on support for peace should also be conditional on the political characteristics of the municipality. My theory implies that, where return migrants and locals have similar political preferences, support for return and reintegration should be higher. By contrast, in municipalities where locals have opposing viewpoints to potential returnees, there ought to be greater opposition to peace. I test this by including an interaction between net normalized refugee flows and support for the incumbent President Santos, whose peace proposal garnered considerable support from the Colombian diaspora (Bermudez 2011). Since the diaspora supported Santos' peace deal, we should expect those municipalities where support for Santos was greater to be less concerned with potential returnees than municipalities where support for Santos was lower, and therefore that the interaction between support for Santos and refugee flows should be a significant predictor in the model. This model requires us to assume that local voters had some level of awareness of the preferences of potential returnees. While possibly problematic in other settings, the unique circumstances of the politically targeted nature of displacement during the conflict makes this assumption plausible in the Colombian context (Steele 2017).

Of course, there are important baseline differences between municipalities with different levels of (relative) refugee flows. Municipalities with high levels of inward and outward refugee flows differ systematically from municipalities that had neither displacement nor hosted refugees. To account for as many sources of confounding as possible, I include controls for several known drivers of support for peace as well as potential causes of displacement. These include several indicators of wartime violence, support for the incumbent president Santos, a measure of population size, local cocaine production indicators on the intensive and extensive margin, and an index score for municipal good governance. Reverse causation is furthermore unlikely

due to the temporal sequencing of the variables: refugee flows are measured between 1993 and 2015, and the referendum vote took place in 2016. Nevertheless, it is important to acknowledge that this is design that relies on a selection on observables strategy. Interpreting the results as causal requires assuming that net normalized refugee flows are as good as randomly assigned conditional on the included variables. This assumption may not be tenable. Nevertheless, this exercise helps provide external validity to the experimental results provided earlier.

6.1   Results

I run a total of three models. The first model contains as covariates the net number of refugees (outward minus inward) between 1993 and 2015 normalized by the 2016 municipal population, the number of FARC attacks between 1993 and 2010, the number of FARC homicide victims between 2003 and 2013, the total number of subversive actions between 2003 and 2015, and the support for President Santos in the runoff of the 2014 Presidential Election. The second model has all the same covariates as the first, but adds an interaction between the normalized refugee variable and support for President Santos. The third model directly tests for the interactive effect of unemployment, but only for the 23 departmental capital cities, which are the only municipal units for which unemployment data is available. All models are estimated using ordinary least squares regression.

Since refugees are measured by subtracting inward from outward refugee flows, greater potential numbers of returnees are indicated by positive numbers on the refugee variables. If the labor market competition story is correct, the coefficient should be negative when unemployment is high. Conditional on high levels of concern about labor market looseness, measured by unemployment, higher values of the net normalized refugee variable (indicating greater out-migration during the conflict and therefore greater numbers of potential returnees with peace) should be associated with lower support for peace, meaning the coefficient should be positive (or at least less negative than under conditions of no labor market concern). If the political competition story is correct, the coefficient of net normalized refugee flows should be positive when support for Santos is high, indicating that there is more openness to returnees when support for President Santos is higher.

As can be seen in Table 2, higher levels of refugees leaving the municipality are associated with higher levels of support for the peace deal, even keeping constant support for President Santos and exposure to violence perpetrated by the FARC. However, this effect is mediated by the

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Table 2: Potential returnees and support for peace in the 2016 referendum

	Support for peace		
	(1)	(2)	(3)
Net norm. refugees	0.067***	0.007	1.703*
	(0.009)	(0.022)	(0.570)
Support for Santos	0.006***	0.006***	0.005***
	(0.0002)	(0.0002)	(0.001)
Unemployment			−0.030*
			(0.013)
Refugees × Santos		0.001**	
		(0.0004)	
Refugees × unemployment			−0.173**
			(0.050)
FARC attacks	−0.001***	−0.001**	−0.001
	(0.0004)	(0.0004)	(0.001)
FARC homicides	−0.003*	−0.003	0.003
	(0.002)	(0.002)	(0.005)
Subversive actions	0.001	0.001	0.002
	(0.001)	(0.001)	(0.003)
Population	−0.000	−0.000	0.000
	(0.000)	(0.000)	(0.000)
Cocaine (ext.)	0.005	0.004	−0.003
	(0.009)	(0.009)	(0.051)
Cocaine (int.)	0.00001**	0.00001*	−0.001
	(0.00001)	(0.00001)	(0.001)
Good government index	−0.001***	−0.001***	0.002
	(0.0003)	(0.0003)	(0.003)
Constant	0.306***	0.320***	0.368
	(0.024)	(0.024)	(0.217)
N	1,101	1,101	23
R^2	0.678	0.680	0.891
Adjusted R^2	0.675	0.677	0.782

*$p < .05$; **$p < .01$; ***$p < .001$

potential of returnees to compete on loose labor markets, as well as support for Santos. Where voters face high levels of unemployment, higher levels of potential returnees are associated not with higher levels of support, but rather with lower levels of support, as predicted by my theory. Furthermore, in municipalities where support for Santos is low, the effect of refugee flows on peace practically disappears, also as predicted. All results are robust to alternative controls for insurgent violence, and decomposing net flows into normalized inward and outward refugee flows, as shown in the replication materials

Figure 5 visualizes the effects of the interactive variables on support for peace. Where unemployment is below 11%, the effect of net refugee flows is positive, meaning potential returnees are associated with greater support for peace. Above that mark, the dynamic shifts, and potential returnees become associated with lower support for peace. The negative sign and significance of the interaction underscores the importance of my hypotheses on attitudes toward return migration, not only in the abstract setting of a survey experiment, but also in the real world application of the 2016 Colombian peace referendum. Similarly, the interaction between support for Santos and net normalized refugee flows emphasize that this is not only an economic process, but a political one as well. I acknowledge that this is a far more crude test of my hypotheses than the preceding conjoint experiment, which was able to identify attitudinal effects conditional on individual economic and political covariates. Nevertheless, these results help illustrate the applicability of the proposed causal mechanisms in a setting with direct material consequences for political actors.

7 Conclusion

In this paper, I have presented a new theory about attitudes to immigration by looking at a class of migrants previously not considered by the literature: returnees. Given that a large part of migrants return home after several years (Wahba 2014; van Stiphout-Kramer et al. 2023), and that the developmental prospects of communities are maximized when reintegration goes smoothly (Mueller and Kuschminder 2022), the findings presented here are important for both academic and practical purposes. On the one hand, it extends our understanding of the political-economic causes of opposition to migration in emerging economies, contributing to a small but growing literature (Helms 2024; Holland, Peters and Zhou 2024; Kustov 2022; Singer and Quek 2022; York 2022). On the other, these results may help governments and civil society in

Figure 5: Marginal effect of refugee outflows (1993–2015) on support for peace

(a)

(b)
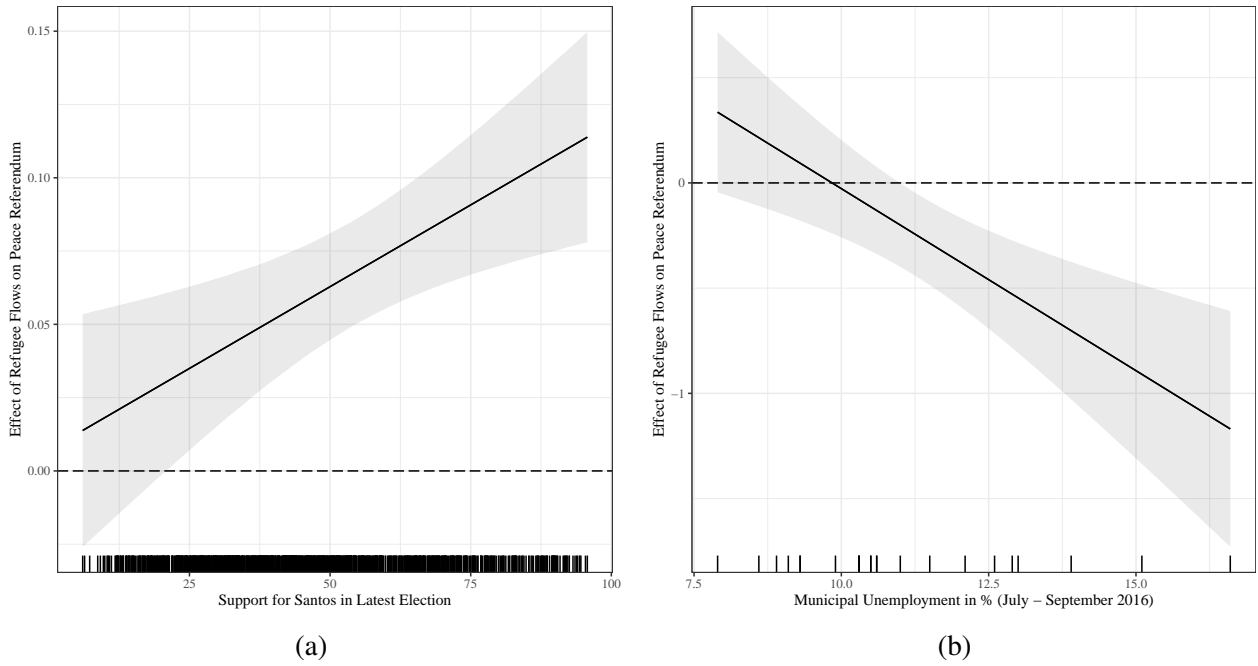
emerging economies tailor their return policies in order to maximize successful reintegration and development policies.

While the focus of this paper has been on Colombia, its results are generalizable to other settings. High volumes of returnees that are active as both economic and political entrepreneurs are common to many other developing countries. For example, in recent years return migration from the United States to Mexico was about 75% of the size of immigration from Mexico to the USA, and while flows from Mexico to the United States have decreased since the 1990s, flows from the United States to Mexico have increased over the same time frame (Azose and Raftery 2019, p.119). Some of those who return become engaged in local and national politics, inspired by their own experience of emigration (Schwartz 2008). Similar dynamics have been observed in Sub-Saharan Africa, where returnees also became politically active to try to change the status quo (Chauvet and Mercier 2014; Chauvet, Gubert and Mesplé-Somps 2016; Vari-Lavoisier 2016). Though empirical verification is needed, these studies lend credence to the position that my theory will have explanatory power outside of Colombia.

The results of my novel experiment imply that governments in migrant home and host countries should cooperate to facilitate the safe and effective transfer of migrant savings to improve local development and returnee acceptance. This could take the form of savings and retirement transfer schemes, as well as international banking cooperation to improve capital mobility for migrant workers. Furthermore, since non-migrants prefer returnees who intend to be

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entrepreneurs in their neighborhoods, my results also suggest that migrant sending communities should work to lower barriers to returnees starting their own small businesses. The implications of my study matter for academia as well. Multiple studies have noted the potential positive effects of returnees for local politics and democratic reform (Bucheli and Fontenla 2022; Chauvet and Mercier 2014; Tuccio, Wahba and Hamdouch 2019). The results of my conjoint experiment suggest that, such beneficial effects notwithstanding, non-migrants have an apprehensive view of returnees with political ambitions. Future studies should further unpack how and under what conditions such reservations can be ameliorated.

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A Peace agreement repatriation clause (highlights added)

5.1.3.5. Collective processes of return of displaced persons and reparations of victims abroad

In developing this Agreement and in the context of the conflict, the National Government will introduce specific collective territorial- and gender-based programs to return and relocate displaced persons on the one hand, and accompanied and assisted return plans for victims abroad on the other. The coordination of such plans will be strengthened at territorial level by other aspects of the Victim Reparation Policy, particularly collective reparation and land restitution programmes, and by implementation of the agreement "Towards a New Colombian Countryside: Comprehensive Rural Reform", where applicable.

Measures will be taken accordingly to guarantee collective or individual returns and relocations in conditions of safety and dignity according to a voluntary approach involving the following elements:

- **Identification of territories:** return and relocation plans will prioritise areas in which the DPTFs are implemented and other areas in which collective reparation plans are developed and will be coordinated with land restitution processes.

- **Interinstitutional coordination:** return and relocation plans will be coordinated, where applicable, with the various plans and programmes agreed, particularly the DPTFs, rural housing and water plans, measures to provide access to land, income generation, boosting of the small-scale farmer economy and programmes to clear and decontaminate areas of APM, IED, UXO, or ERW, and with land restitution processes.

- **Security in territories for return:** in areas in which return and relocation plans are to be prioritised, the Government will set up the security measures necessary to guarantee life and personal integrity in communities, which will always participate in this process.

- **Strengthening of community advocates:** The Government will take the necessary measures to strengthen the community advocates (*defensores comunitarios*) programme, and in particular their functions of protection and promotion of human rights, so that they can effectively monitor the processes of land restitution, return and relocation of displaced persons and victims abroad, including refugees and exiles, which form part of these processes and can support and assist the victims in order to guarantee access to the institutional

services offered with regard to realisation of their rights.

The implementation of these processes of returns and relocations will require the cooperation of specialised and interdisciplinary teams, capable of ensuring the participatory process and use of local resources.

With regard to the large number of victims who had to leave the country as a consequence of different human rights violations and breaches of international humanitarian law (IHL) during the conflict, the National Government, in fulfillment of this Agreement, will strengthen the programme of recognition and reparation of victims abroad, including refugees and exiles victimised during the conflict, by means of the implementation of "supported and assisted return" plans. The assisted return will consist of promoting condition to facilitate their return to the country and the construction of their life project, including decent reception conditions through the coordination of these plans with the specific institutional services offered, to progressively guarantee access to basic rights, decent employment, housing, health and education at all levels according to each person's individual needs. Priority will be given to their relocation to the places they had to leave, respecting the wishes of the victim. The Government will adopt the necessary measures to coordinate these plans, where appropriate, with the different plans and programmes agreed, in particular the DPTFs.

All this is without prejudice to the different measures that, in an end-of-conflict scenario, have to be adopted to drive forward and promote the return of exiles and other Colombians who left the country because of the conflict.

B Conjoint information

B.1 Conjoint preamble (English)

Study Information Sheet

Study Title: Attitudes Towards Return Migration in Colombia

Protocol #: 6356

Please read this study information sheet carefully before you decide to participate in the study.

Purpose of the research study: The purpose of the study is to understand attitudes of Colombian citizens to fictional Colombian migrants currently residing abroad, who wish to return home.

What you will do in the study: You will take a survey in which you will answer questions about your employment status and political opinions, but your participation will be fully anonymous. No personalized information will be collected. You will then be shown six pairs of hypothetical return migrants, each with distinct characteristics. Please indicate which of the Colombian citizens living abroad you would most rather see return to your community. You can skip any question that makes you uncomfortable and you can stop the survey at any time.

Time required: The study will require about ten to fifteen minutes of your time.

Risks: There are no anticipated risks to this study.

Benefits: There are no direct benefits to you for participating in this research study. The study may help us understand how Colombians living abroad who want to return can be best reintegrated into our society.

Confidentiality: The information that you give in the study will be anonymous. Your name and other information that could be used to identify you will not be collected or linked to the data.

Voluntary participation: Your participation in the study is completely voluntary.

Right to withdraw from the study: You have the right to withdraw from the study at any time without penalty. However, once your responses are submitted there is no way for the them to be deleted since this study is anonymous.

How to withdraw from the study: If you want to withdraw from the study, you can close the browser. There is no penalty for withdrawing: you will still receive full payment for the study. Because the data are not connected to your identity, you cannot withdraw after you submit your data.

Payment: You will receive what the survey company normally pays you.

Using data beyond this study: The researcher would like to make the information collected in this study available to other researchers after the study is completed. The researcher will remove any identifying information (such as your name, contact information, etc.) connected to the information you provide. The researcher will share all the information collected in this study (not just your individual file) with other researchers for future research studies, including but not limited to replication of the researcher's work. The researcher will make the information available on a public website such as Dataverse. Researchers of future studies will not ask your permission for each new study. The other researcher will not have access to your name and other information that could potentially identify you nor will they attempt to identify you.

You may also report a concern about a study or ask questions about your rights as a research subject by contacting the Institutional Review Board listed below.

B.2 Conjoint preamble (Spanish)

Página electrónica de información del estudio

Título del estudio: Actitudes hacia la migración de retorno en Colombia

Protocolo #: 6356

Antes de decidir si participar o no en el estudio, lea atentamente esta hoja de información del estudio.

Propósito del estudio de investigación: El propósito del estudio es comprender las actitudes de los ciudadanos colombianos hacia los migrantes colombianos hipotéticos que actualmente residen en el exterior y que desean regresar a su país.

¿Qué harás en el estudio?: Realizará una encuesta en la que responderá preguntas sobre su situacián laboral y sobre algunos temas políticos. Su participación será totalmente anónima y no se recopilará información personalizada. Luego, se le mostrarán seis pares de migrantes hipotéticos de retorno, cada uno con características diferentes. Deberá, entonces, indicar cuál

de esos dos ciudadanos colombianos hipotéticos que viven en el exterior preferiría que regresara a su comunidad. Tenga en cuenta que puedes omitir cualquier pregunta que le haga sentir incómodo y que puede detener la encuesta en cualquier momento.

Tiempo requerido: El estudio requerirá entre diez y quince minutos.

Riesgos: No se prevén riesgos para este estudio.

Beneficios: No habrá beneficios directos para usted por participar en este estudio de investigación. Sin embargo, el estudio puede ayudarnos a comprender cómo los colombianos que viven en el exterior y que desean regresar al país pueden reintegrarse de la mejor manera a nuestra sociedad.

Confidencialidad: La información que usted proporcione en el estudio será anónima. Su nombre y otra información que podría usarse para identificarlo no serán recopilados ni vinculados a los datos.

Participación voluntaria: Su participación en el estudio es completamente voluntaria.

Derecho a retirarse del estudio: Tiene derecho a retirarse del estudio en cualquier momento sin penalización. Sin embargo, una vez enviadas sus respuestas, no hay forma de eliminarlas ya que este estudio es anónimo.

¿Cómo retirarse del estudio?: Si desea retirarse del estudio, puede cerrar el navegador. No hay penalización por retirarse: aún recibirá el pago completo por el estudio. Debido a que los datos no están relacionados con su identidad, no puede retirarlos después de enviar sus datos.

Pago: Recibirá lo que normalmente le paga la empresa encuestadora.

Use de datos más allá de este estudio: Al investigador le gustaría que la información recopilada en este estudio esté disponible para otros investigadores una vez finalizado el estudio. El investigador eliminará cualquier información de identificación (como su nombre, información

de contacto, etc.) relacionada con la información que usted proporcione. El investigador compartirá toda la información recopilada en este estudio (no solo su archivo individual) con otros investigadores para futuros estudios de investigación, incluidos, entre otros, replicando este estudio. El investigador pondrá la información a disposición en un sitio web público como Dataverse. Los investigadores de estudios futuros no le pedirán permiso para cada nuevo estudio. El otro investigador no tendrá acceso a su nombre ni a otra información que pueda identificarlo ni intentará identificarlo.

También puede informar cualquier inquietud sobre un estudio o hacer preguntas sobre sus derechos como sujeto de investigación comunicándose con la Junta de Revisión Institucional que se detalla a continuación.

B.3 Choice task example (Spanish)

Figure A6: Conjoint choice task

Pregunta 1

Revise cuidadosamente las opciones que se detallan a continuación y luego responda las preguntas.

¿Cuál de estas opciones prefieres?

	Opción 1	Opción 2
Empleo previsto	Tiene la intención de iniciar su propio negocio	Tiene la intención de iniciar su propio negocio
País de retorno	Estados Unidos	Venezuela
Política	No quiere involucrarse en política	Quiere involucrarse en política
Ahorros	Tiene ahorros ganados en el extranjero	No tiene ahorros

○ Opción 1

○ Opción 2

Indique qué tan favorables considera ambas opciones.

Menos favorable   Más favorable
0   1   2   3   4   5   6   7   8   9   10

Opción 1

○——————————————————————————————

Opción 2

○——————————————————————————————

B.4 Ethical principles

The survey experiment and data collection were designed with the highest standards of ethical research in mind. Remuneration of respondents went through NetQuest, the owner of the online

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panel used to recruit respondents. Netquest follows the European General Data Protection Regulations (GDPR), placing it well beyond legally mandated American data protection standards. Respondents were quota sampled on age, gender, and socio-economic status to generate a diverse, representative sample of the Colombian population. In order to minimize the potential for trauma recollection in the context of the Colombian conflict, no reference to the Conflict, refugees, or victims was made at any point in the survey. Instead, the survey referenced "return migrants" broadly, in line with the approach that has been taken by the Colombian government itself. At no point during data collection was the true purpose of the experiment obfuscated, and the survey reiterated several times that the conjoint profiles viewed by individuals were entirely fictional. All laws of the Republic of Colombia were followed in the collection and analysis of the data.

B.5 Randomization check

Based on the randomization scheme (uniform with an equal chance for all variable values to be shown), we should observe a roughly .5 probability for assignment to treatment across all relevant subgroups. This is indeed the case, as shown below.

Table A3: Randomization check

	Pr(Assignment to treated level)				
	Pooled	Work = 1	Work = 0	Democracy = 1	Democracy = 0
Savings	.498	.5	.491	.506	.49
Employment	.502	.504	.498	.501	.509
Country	.502	.504	.502	.51	.494
Politics	.498	.502	.493	.504	.497

B.6 Conjoint results (tables)

B.6.1 Unconditional model (Figure 1)

Table A4: AMCEs Figure 1

	Change in Pr(Preferred Returnee)
Savings	0.1665***
	(0.0090)
Intent to work for others	−0.1197***
	(0.0080)
Venezuela	−0.1864***
	(0.0089)
Political ambition	−0.1188***
	(0.0084)
N	18,864

*p < .05; **p < .01; ***p < .001
Number of respondents: 1,572

B.6.2 Model conditional on employment (Figure 2)

Table A5: Conditional AMCEs Figure 2

	Change in Pr(Preferred Returnee) due to "Intent to work for others"
Among labor market competitors (work = 1)	−0.1059***
	(0.0106)
Among entrepreneurs (work = 0)	−0.1465***
	(0.0158)
Difference	0.0407*
	(0.0191)
N	16,008

*p < .05; **p < .01; ***p < .001
Number of respondents: 1,334

B.6.3 Model conditional on democratic attitudes (Figure 3)

Table A6: Conditional AMCEs & ACIEs Figure 3

Change in Pr(Preferred Returnee) due to "Venezuela"	
Among respondents with pro-democratic attitudes (democracy = 1)	−0.2127***
	(0.0113)
Among others (democracy = 0)	−0.1520***
	(0.0172)
Difference	−0.0606**
	(0.02058)

Change in Pr(Preferred Returnee) due to "Political ambition"	
Among respondents with pro-democratic attitudes (democracy = 1)	−0.1256***
	(0.0107)
Among others (democracy = 0)	−0.1102***
	(0.0163)
Difference	−0.0154
	(0.0195)

Change in Pr(Preferred Returnee) due to "Venezuela × Political ambition"	
Among respondents with pro-democratic attitudes (democracy = 1)	−0.3308***
	(0.0242)
Among others (democracy = 0)	−0.2978***
	(0.0359)
Difference	0.0431
	(0.0330)
N	16,620

*$p < .05$; **$p < .01$; ***$p < .001$
Number of respondents: 1,385

B.7   Preregistration

Study Information

Hypotheses

The experimental component of the study tests the following hypotheses:

1. Non-migrants prefer returnees who bring home financial capital in the form of savings.

2. Non-migrants who are employed by others or seeking employment prefer returnees intending to start their own business, and non-migrants who are self-employed prefer returnees intending to become employed by others.

3. Non-migrants with (non-)democratic preferences prefer returnees from (non-)democratic countries.

4. Non-migrants with(non-)democratic preferences prefer returnees who (do not) intend to become politically active, conditional on the migrant returning from a democratic country.

Design Plan

Study type

Experiment - A researcher randomly assigns treatments to study subjects, this includes field or lab experiments. This is also known as an intervention experiment and includes randomized controlled trials.

Blinding

For studies that involve human subjects, they will not know the treatment group to which they have been assigned.

Personnel who interact directly with the study subjects (either human or non-human subjects) will not be aware of the assigned treatments. (Commonly known as "double blind")

Is there any additional blinding in this study?

No response

Study design

I will test the hypotheses with a conjoint survey experiment on a sample representative of the adult Colombian population. Respondents will be presented with descriptions of pairs of hypothetical return migrants and asked which of the pair they would rather see return. Attributes of

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the countries presented to respondents will be the following: whether they have savings from abroad, whether they intend to become self-employed or be employed by others, whether they come back from the United States (democracy) or Venezuela (autocracy), and whether they intend to become involved in politics. Potential effects' heterogeneity with respect to all attributes except savings will be explored along respondents' attitudes towards democracy, self-placement on the left-right scale and their employment status.

Randomization

Values for all experimental attributes will be randomized with uniform distributions. As there are only 4 potential values to randomize, I restrict profile randomization to have at least a single difference between them. The order of attributes will be randomized between respondents (kept the same for each individual respondent).

Sampling Plan

Existing data

Registration prior to creation of data.

Explanation of existing data

No response

Data collection procedures

I collect respondents from a panel provided by survey firm Netquest, which has a strong track record of facilitating survey research in Latin America (Zhirkov and Smilan-Goldstein 2023). I used quota sampling to achieve a panel that matched the Colombian population in the breakdown of gender, age, and socio-economic status.

Sample size

Overall sample size: 1,694

Gender quotas: 836 men, 858 women

Age quotas:

402 18 through 24,

364 25 through 34,

294 35 through 44,

265 45 through 54,

200 55 through 64,

114 65 through 74,

55 75 and up

Socio-economic status:

269 strata 1

490 strata 2

582 strata 3

187 strata 4

120 strata 5

46 strata 6

Sample size rationale

No response

Stopping rule

No response

Variables

Manipulated variables

For each conjoint profile, the following variables and potential values will be randomized. English version:

English version:

Savings:

- Has savings from abroad

- Does not have savings

Prospective employment:

- Intends to be self-employed

- Intends to work for someone else

Politics:

- Wants to be involved with politics

- Does not want to be involved with politics

Country of Return:

- Venezuela

- United States

Spanish translation:

Ahorros:

- Tiene ahorros ganados en el extranjero

- No tiene ahorros

Empleo Previsto:

- Tiene la intención de iniciar su propio negocio

- Tiene la intención de trabajar para otra persona

Política:

- Quiere involucrarse en política

- No quiere involucrarse en política

País de Retorno:

- Venezuela

- Estados Unidos

Measured variables

Outcome variables:

- Forced choice design, i.e. binary indicator if a profile is preferred

- Ordinal ranking for each profile

Conjoint variables

- Whether the hypothetical returnee has savings

- The prospective employment of the hypothetical returnee

- Which country the hypothetical returnee is returning from

- Whether the hypothetical returnee wants to be involved in politics

Other survey variables

- Whether the respondent is an urban or rural resident

- Whether the respondent considers themselves liberal, conservative, or other

- The respondent's employment status

- Whether the respondent has ever lived outside of Colombia

- Whether the respondent has family living abroad

- The respondent's attitude towards democracy

- Whether the respondent believes in general if the government should aid the return of Colombians abroad.

Analysis Plan

Statistical model

Average marginal component effects (AMCEs) from the conjoint experiment will be estimated using a standard OLS regression. Standard errors will be clustered on the respondent level. As a robustness check, I also estimate average component preferences (ACPs) as causal estimands (see Ganter 2023).

To explore the hypothesized heterogeneity of effects, the same regression will also be estimated for (1) those who prefer democracy versus those who do not, (2) those who are self-employed versus those who are employed by others versus those not in the labor market, (3) those with conservative preferences versus those with liberal preferences, and (4) those who have lived abroad or have family abroad versus those who have / do not.

Transformations

The employment variable (which is multinomial) will be recoded into two binary variables, one identifying self-employed individuals, the other individuals outside of the labor market, with the baseline category being individuals employed by others or looking for employment (i.e. those who will face most competition on the labor market by returnees intending to be employed by others).

The variables capturing the respondent's history abroad or family abroad will be combined into a single variable that equals 1 if either of the first two variables equal 1.

All other variables will be recoded to exclude "Don't Know / Prefer not to Answer"

Inference criteria

No response

Data exclusion

No response

Missing data

No response

Exploratory Analysis

No response

Other

No response

C Conjoint robustness extensions

C.1 Sensitivity analysis

The results of the conjoint experiment placed in the main body of the paper include some of the most important drivers of opposition to return migration based on the theoretical discussion. However, other potential factors could matter for attitudes to returnees, such as education. If respondents infer such factors from others that are included in the survey, there is a problem of "informational equivalence" (Dafoe, Zhang and Caughey 2018). How strong would such inferences have to be in order to threaten the conclusions of the manuscript? Using sensitivity analysis (Cinelli and Hazlett 2020), I show that the partial correlation between (for example) the Savings factor and omitted factors would have to exceed an R2-value of 0.4, assuming the effect of the omitted factors on preferences is approximately the size of the largest predictor of attitudes: coming from Venezuela as opposed to the United States. While sensitivity analysis cannot provide evidence for whether such a confounder exists, it seems implausible that such a large part of the variation in Savings leads to inferences about omitted variables.

Figure A7: Sensitivity analysis: Savings as treatment, Country as baseline
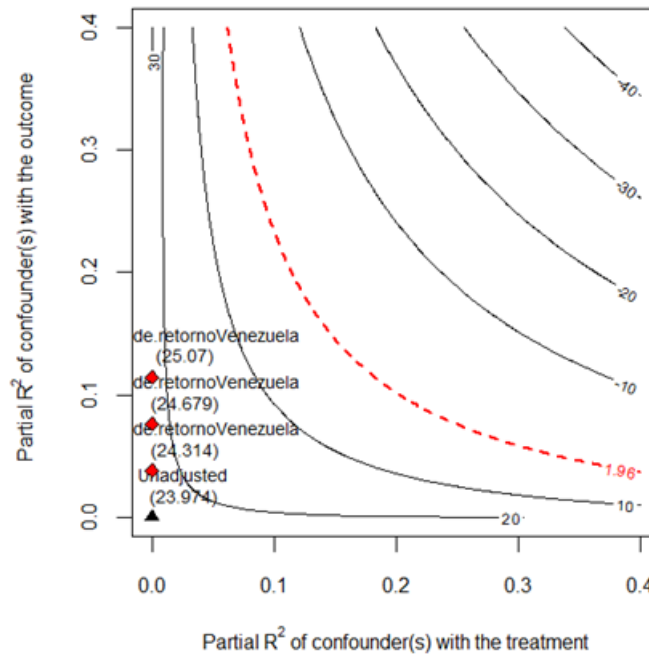
C.2 Ideological competition

Based on the discussion of concerns over political competition raised in the main body of the manuscript, one could wonder whether survey respondents were concerned about additional forms of normative influence of returnees besides those along preferences for regime type. For example, competition could take place along ideological (left-right) fault lines. This is especially relevant because Venezuela is a left-wing regime and Venezuelans are thought to be more left-wing than they are by Colombians(Holland, Peters and Zhou 2024). Do preferences for returnees from the USA and Venezuela differ by respondents' ideology? To test for this possibility, I conduct a subset analysis for respondents who considered themselves left- or right-wing on the political spectrum respectively. Results indeed suggest that right-wing respondents penalize returnees from Venezuela more than left-wing respondents, especially if the respondents also intend to become politically active. However, correlation between ideology and regime type preferences are largely uncorrelated at the individual respondent level. This suggests broad support for the theory of political competition elucidated in the paper, as both ideology and regime type concerns are prevalent in the minds of respondents when evaluating possible returnees.

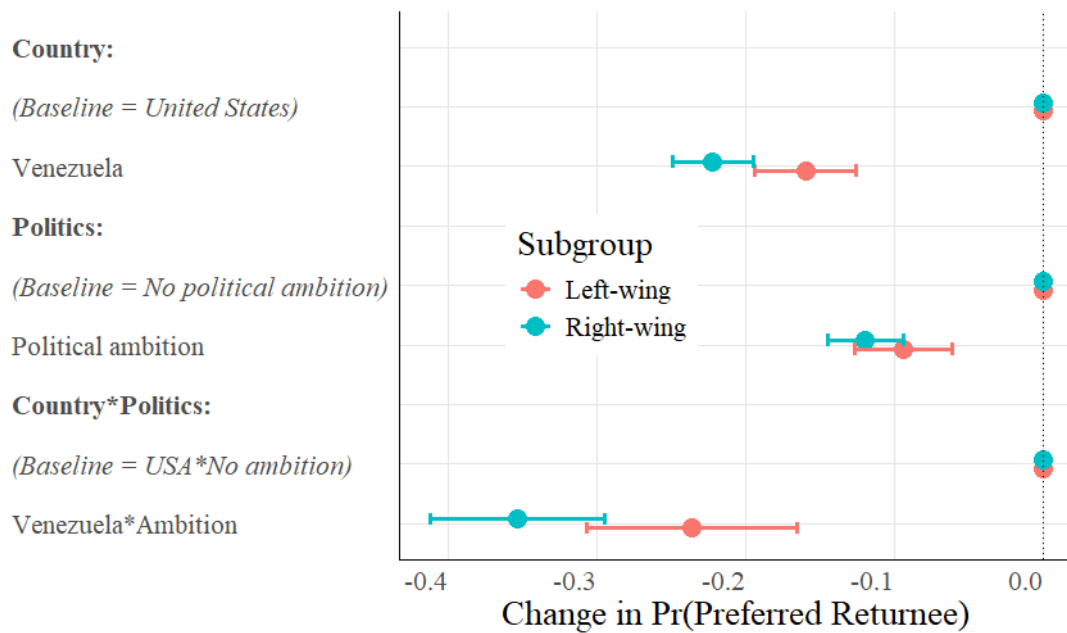Figure A8: Political AMCEs conditional on respondent's ideology

Table A7: Correlation ideology and regime type preferences

	Left-wing	Right-wing
Pro-democracy	305	463
Not pro-democracy	122	183

Chi-square test with 1 d.f.: χ^2 = 0.007, p-value = 0.9311
Note: *data at the respondent level*

D Descriptive statistics

D.1 Conjoint survey respondents

Table A8: Descriptive statistics for conjoint analysis

	Mean	Std. Dev.	Min	Max	N
Support for Democracy	.69	.46	0	1	1,464
Right-wing	.44	.5	0	1	1,552
Work	.48	.5	0	1	2,036
Former migrant	.18	.38	0	1	1,667
Has family abroad	.76	.43	0	1	1,660

D.2 2016 Municipalities

Table A9: Descriptive statistics for observational analysis

|	Mean	Std. Dev.	Min	Max	N
Proportion YES	.53	.17	.16	1	1, 122
Percentage Points Santos	48	21	6	96	1, 122
Net. Norm. Refugees	.17	.36	−.64	2.7	1, 122
Unemployment	11	2.1	7.9	17	23
Population	41, 738	249, 109	262	7, 300, 918	1, 122
Cocaine (int)	.17	.38	0	1	1, 122
Cocaine (ext)	86	625	0	16, 960	1, 122
Good Gov. Index	64	9.5	30	91	1, 122
FARC Attacks	4.8	8.9	0	90	1, 122
FARC Homicides	.35	2.2	0	43	1, 122
Subversive Actions	2.3	7.1	0	103	1, 122

D.3 Distribution of net normalized refugee flows

Figure A9: Net normalized refugee flows, by municipality
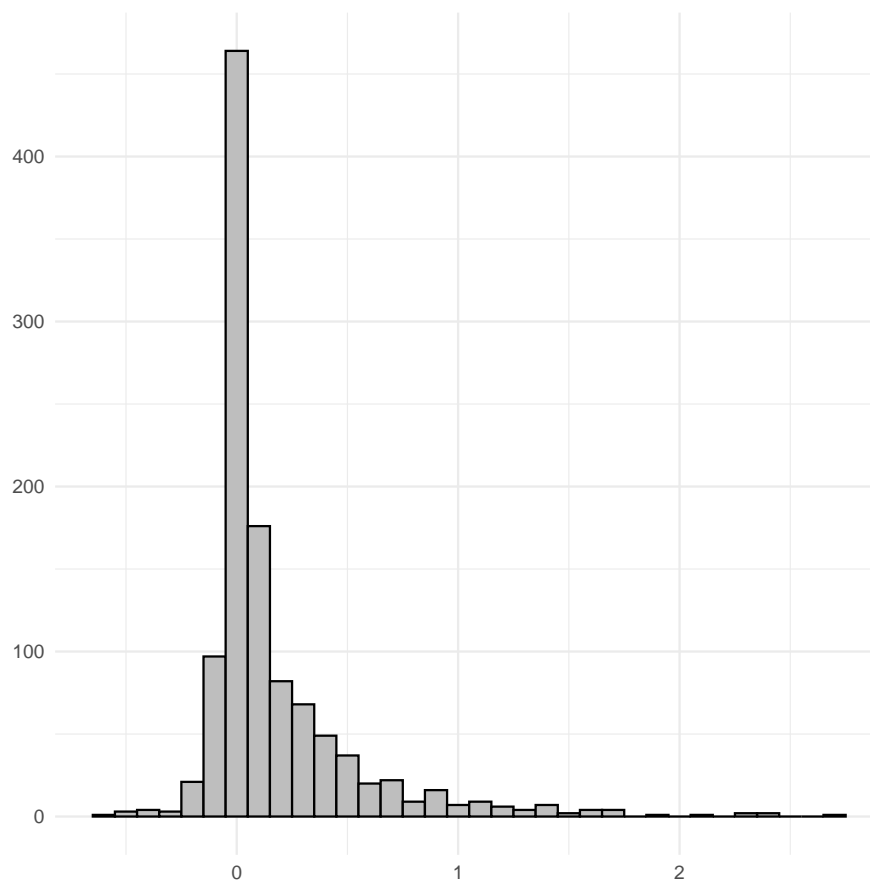
D.4 Distribution of support for president Santos

Figure A10: Support for President Santos, by municipality
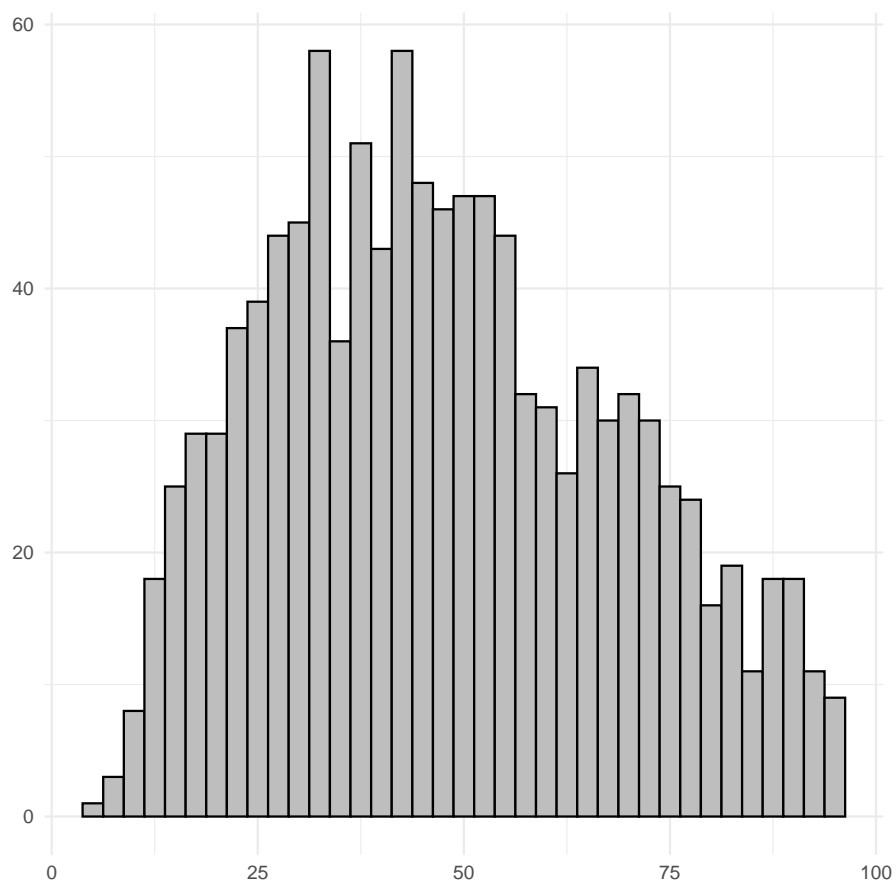
E Support for Santos as a possible collider

If support for President Santos was affected by net refugee flows, then it becomes a possible collider in the causal graph and controlling for it can induce bias in OLS estimates by opening a backdoor path to common causes of support for Santos and support for peace. To account for this possibility, I re-estimate models 1 and 3 from Table 2 excluding support for Santos. Results are presented below and do not diverge meaningfully from those shown in the main body of the text.

Table A10: Excluding support for Santos

	Support for peace	
	(1)	(2)
Net norm. refugees	−0.122***	−2.610**
	(0.014)	(0.797)
Unemployment		−0.043*
		(0.016)
FARC attacks	−0.004***	−0.004*
	(0.001)	(0.002)
FARC homicides	−0.001	0.009
	(0.002)	(0.007)
Subversive actions	0.005***	0.004
	(0.001)	(0.004)
Refugees × unemployment		0.248**
		(0.069)
Constant	0.513***	0.996***
	(0.006)	(0.174)
N	1,122	23
R^2	0.115	0.558
Adjusted R^2	0.112	0.392

*$p < .05$; **$p < .01$; ***$p < .001$