"Remittances are a Political Blessing and not a Curse"

by

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2nd draft (November 2015), prepared for IPES 2015.

Abstract: This paper offers a two-part challenge to the proposition that migrant remittances act as a political curse by hindering democratization. It first challenges the logic that remittances allow less democratic governments to divert public goods spending towards patronage spending. This spending substitution hypothesis is directly tested and shown to have little empirical support: in less democratic regimes, remittances have no effect in reducing public goods spending related to education and health and no effect in increasing military spending as a primary patronage good in more autocratic regimes. Second, it builds an argument through modernization theory to explain why remittances should facilitate democratization: remittances increase national income and promote economic development, both increasing the societal demand for democracy and the state's willingness to supply it. Testing the hypothesis that remittances should be associated with movement towards democracy, it presents supportive evidence using operational measures focused on executive constraints and contested elections.

Acknowledgments: The first draft of this paper was presented at APSA 2015 in San Francisco. We thank Faisal Ahmed, Gabriella Montinola, Dennis Quinn, Rachel Wellhausen, and Joe Wright for helpful comments and suggestions.

Scholars and policymakers have proposed that various international economic flows, including certain commodities and money, act as a political curse by hindering democratization. A political curse argument was first advanced for commodities that are traded internationally like oil and other natural resources (e.g., Mahdavy 1970, Huntington 1991, Ross 2001). A similar political curse argument was subsequently offered for foreign aid (e.g., Easterly 2006; Djankov, Montalvo, and Reynal-Querol 2008; Moyo 2009). Most recently, a political curse has been advanced for remittances (e.g., Abdih, Chami, Dagher, and Montiel 2012; Ahmed 2012; Ahmed 2013, Berdiev, Kim, and Chang 2013), a growing international capital flow associated with migrant workers in a foreign country sending money back to their families in the home country that now exceeds both foreign aid and portfolio investment flows in volume.

Not surprisingly, scholars have also challenged these political curse arguments. Habor and Menaldo (2011) questioned the empirical proposition that oil is negatively associated with democracy, although it is important to note the response offered by Andersen and Ross (2014).[1] Both theoretically and empirically, Altincekic and Bearce (2014) challenged the political curse proposition as it has been applied to foreign aid.[2] In this paper, we turn to the most recent political curse proposition focused on migrant remittances.[3] As indicated by our title, we argue that not only should remittances not be

[1] Dunning (2008) offered a qualified response to the oil curse logic, arguing that oil can be blessing when there is higher income inequality (but nonetheless a curse with lower income inequality).

[2] Morrison (2009) offered a qualified response to both the oil and aid curse, arguing that these economic flows only hinder democracy in autocratic regimes but that they enhance democracy in already democratic regimes.

[3] In addition to the political curse literature, this proposition also fits within the growing political science literature on the causes and consequences of international migration and remittances (e.g., Singer 2010; Leblang 2010; Bearce and Hutnick 2011; Breunig, Cao, and Luedtke 2012; Fitzgerald, Leblang, and Teets 2014; Peters 2014; and Peters 2015). For a literature review, see Mosley and Singer (2015). As noted by these authors (ibid, 295), a "key theme" in this literature concerns "substitution", or whether remittances replace "government-provided public goods and welfare programs." We address this theme and provide some aggregate evidence to show that they do not.

expected to hinder democracy, these international capital flows should actually enhance the prospects for democratization in remittance-receiving countries (i.e. serve as a political blessing).

Our contrary argument is advanced in two sequential parts. We first challenge the causal logic offered by both Abdih et al. (2012) and Ahmed (2012) that remittances allow governments to divert public goods spending towards patronage spending. Per their substitution hypothesis, when citizens can fund their education and health care via remittance inflows, more autocratic governments shift their spending away from public goods towards private goods, thus allowing them to solidify their power base. We directly test this spending shift hypothesis and show that it has little empirical support: net remittance inflows have no significant effect in reducing public goods spending related to education and health or in increasing military spending (as a primary private/patronage good) in less democratic regimes.

In the first part of our paper, we also explain why one should not expect to observe this government spending substitution effect due to migrant remittances. This explanation centers on two related considerations: 1) empirical results showing that remittances increase per capita national income, economic growth, and development and 2) the theory underlying Wagner's Law that a richer society demands more of its state in terms of public goods provision. Thus, there is a remittance income effect that offsets the expected substitution effect in terms of public goods.

In the second part of our argument, we also build from the first consideration above to explain why remittances should facilitate democratization. Per modernization theory, economic development both increases the societal demand for democracy and the state's willingness to supply a more democratic political regime. Thus, if remittances promote economic development by increasing national income, then we argue that remittances should also be expected to promote political development in terms of greater democracy.

We then test the hypothesis that remittance inflows should be associated with movement towards democracy, a relationship that was not directly considered either by Ahmed (2012) or by Abdih

et al. (2012); the former looked at government survival and the latter at corruption. In a deliberately restrictive specification with country fixed effects (often not included in democracy models), our results show that net remittance inflows can be associated with movement towards greater democracy, using two different operational measures (one focused on executive constraints and the other on contested elections). We also test for causality through our intermediate variables – those related to economic modernization - and for reverse causality, showing that remittance inflows cannot be strongly explained by political regime type.

From the outset, it is important to acknowledge that our paper is not the first to question a political curse associated with remittances. Tyburski (2014) presents results to show how remittances worsen corruption control in countries with closed polities (consistent with a remittance curse), but also improve corruption control in more open polities (consistent with a remittance blessing). But these results treat political regime type as an independent variable and, thus, do not ultimately address the research question in this paper: do remittances help or hurt democracy? Escribà-Folch, Meseguer, and Wright (2015) also argue that remittances make voters less dependent on patronage in party-based dictatorships, thus increasing the probability of democratization in this set of autocracies. But our argument for a political remittance blessing applies more broadly. Indeed, we present evidence to show how remittances promote democracy even when excluding party-based dictatorships.

Part 1: Remittances and Government Spending

We begin this section by describing the political curse logic offered by Abdih et al. (2012) and Ahmed (2012). Given that these papers make a similar argument, we treat them here as a theoretical package. Compared to oil/natural resources and foreign aid, both papers acknowledge that remittances are not an obvious economic flow to be associated with a political curse. As offered by Abdih et al. (2012, 657): "Because remittance inflows differ from natural resources inflows in several ways, at first glance one might not expect them to have similar adverse effects on institutional quality." For Ahmed

(2012, 149), the comparison was made to foreign aid: "Unlike aid, which goes directly to a government's coffers, remittances are received by households and tend to be poorly tracked and thus untaxed by [i.e., not serving as revenue for] the government."

Since remittances accrue to households and not to governments (unlike state oil and foreign aid), the causal logic underlying a political remittance curse requires some description. Following Abdih et al. (2012, 659), "remittances reduce the value that the representative household places on an extra unit of the government-provided public good, because they increase the ability of the government to meet its own needs. Consequently, the cost to the government of diverting resources for its own purposes is reduced and the government does so, cutting back on its provision of the public good." Such a diversion has implications for democracy (ibid 665, fn. 5): "this reduced cost to the government of diverting resources for its own use can be interpreted as a reduced incentive on the part of households to discipline the government", thus resulting in greater corruption and a less democratic political regime.

Ahmed (2012, 151) labeled this causal story as the "substitution effect": remittances allow more autocratic governments to substitute private/patronage spending for public/welfare spending. This substitution logic, consistent with many models of democratization, associates greater private goods with more autocratic regimes (and greater public goods with more democratic regimes). Ahmed (2012 150) specifically identified "education and health services" as welfare/public goods. He did not specifically identify any category of government spending as patronage/private goods, but, for operational purposes, we classify military spending as such an example based on his argument that "military officials" (ibid) are a key group towards which economic and political rents must be redistributed within a more autocratic regime.

There are thus two hypotheses associated with this proposed remittance substitution effect. The first is that *in more autocratic regimes, net remittance inflows should be negatively correlated with government spending on education and health* (as welfare/public goods). The second substitution hypothesis is that *in*

more autocratic regimes, net remittance inflows should be positively correlated with government spending on the military (as a primary patronage/private good). It is important to note that the proposed remittance substitution effect requires both hypotheses to be true. For example, it may be the case that remittances can be associated with less public goods spending in more autocratic regimes, but if remittances cannot also be associated with more private goods spending, then it would be hard to explain how autocratic government survival should be facilitated by this economic flow. Likewise, if remittances are associated with greater military spending but not with lesser education/health spending in more autocratic regimes, then it becomes difficult to understand how this economic flow would have reduced citizen demand for the public goods more associated with democratic regimes.

These two linked hypotheses were not tested at all by Abdih et al. (2012). Ahmed (2012, 163) offered a small sample test, looking at the variation in government subsidies and transfers broadly defined. But this dependent variable represents a wide segment of government spending that potentially includes both welfare and patronage goods. So we will more directly test this proposed substitution effect using these three narrower categories (e.g., education, health, and military) of government spending.

Substitution Hypothesis Testing

Using data with the country/year unit of analysis, we test these two hypotheses by estimating equations (1) and (2) below. Since the data on government spending in these different categories are not available for the same set of country/years, we estimate equation (1) twice: once for *Education Spending* and again for *Health Spending*. Consequently, we present a series of three government spending models: one for *Education*, a second for *Health* and a third for *Military Spending* with the first two representing public goods and the third as a specific category of private goods.[4]

[4] These three dependent variables are all measured as a share of GDP, using data from the World Bank (2014).

$$(1)\ \textit{Education/Health Spending}_{it} = B_1\ \textit{Education/Health Spending}_{it-1} + B_2\ \textit{Remittances}_{it-1} + B_3\ (\textit{Remittances}*\text{Democracy})_{it-1} + B_4\ \textit{Democracy}_{it-1} + B_x\ \textit{Controls}_{it-1} + e$$

$$(2)\ \textit{Military Spending}_{it} = B_1\ \textit{Military Spending}_{it-1} + B_2\ \textit{Remittances}_{it-1} + B_3\ (\textit{Remittances}*\textit{Democracy})_{it-1} + B_4\ \textit{Democracy}_{it-1} + B_x\ \textit{Controls}_{it-1} + e$$

As the primary independent variables, these equations include *Remittances* and its interaction with some measure of *Democracy* (*Remittances*Democracy*).[5] For the latter, we begin with the 21-point *Polity* score, rescaled so that the least democratic country/year is coded as 0 (and the most democratic is coded as 20).[6] Since the *Polity* measure is dominated by constraints on the executive (with checks and balances as a key feature of a more democratic political system), we will also make use of another *Democracy* measures more focused on contested elections (to be described below). Given the interaction term, the *Remittances* constitutive term captures the effect of remittances in the *least* democratic regime (or where *Democracy*=0).

The first substitution hypothesis, concerning remittances and public goods, predicts for equation (1) that B_2, the coefficient on the *Remittances* constitutive term, should be negatively signed, indicating that remittances reduce *Education* and *Health Spending* in the least democratic (most autocratic) regimes. This first hypothesis also predicts for equation (1) that B_3, the coefficient on the *Remittances* interaction term, should be positively signed, indicating that this substitution effect disappears in more democratic regimes.

Conversely, the second substitution hypothesis about remittances and private goods predicts the opposite set of signs for *Military Spending* in equation (2): B_2, the coefficient on the *Remittances*

[5] Using data from the World Bank (2014), *Remittances* is measured as a share of GDP (like the dependent variables), subtracting remittance outflows from remittance inflows (since most country/year observations are associated with at least some remittance flows in both directions) to create a measure of net remittance inflows. Other scholars have measured remittances as a share of GDP considering *only* remittance inflows. We view this as potential misspecification, especially for the argument presented in this paper concerning the income effect of remittances, since remittance inflows add income to the national economy, but remittance outflows subtract income, or wealth. Thus, our operational measure needs to account for flows in both directions.

[6] The *Polity* measure comes from Marshall, Gurr, and Jaggers (2014).

constitutive term, should be positively signed, indicating that remittances increase *Military Spending* in the least democratic (most autocratic) regimes. The second hypothesis also requires that B_3, the coefficient on the *Remittances* interaction term, should be negatively signed, indicating that this private goods diversion disappears in more democratic regimes.

As control variables, our specification includes *Oil* and *Aid* as the two other economic flows often associated with a political curse.[7] To the extent that broader economic flows associated with international trade may also have an effect on government spending, we include a measure of net *Exports* in our specification.[8] To make sure that the estimated correlations associated with these various economic flows do not stem primarily from movement in the denominator, we include the country/year's Gross Domestic Product measured in billions of US dollars (*GDP*) as an independent variable. We also control for its economic development expressed in terms of the logged value of GDP per capita (*GDPpcln*).[9] Finally, this specification (and all others in this paper) includes a full set of unit fixed effects. The descriptive data for these variables (and all others to be added later) appear in our appendix.

Table 1 (panel A) presents this set of three government spending models using *Polity* as our democracy measure and with all independent variables lagged one year. While the signs on *Remittances* and *Remittances*Polity* are consistent with the proposed substitution effect, there is only one statistically significant result: the negative sign for the *Remittances* constitutive term in the *Education Spending* model.

[7] *Oil* and *Aid* are also measured as a share of GDP using data from the World Bank (2014). Like *Remittances*, *Aid* is a net measure, subtracting government repayments of low interest loans (a loss of national income) from the receipt of new grants and low interest loans (representing gains to national income).

[8] *Exports* are also measured as a share of GDP using data from the World Bank (2014). The logic for a net exports measure (exports minus imports as a share of GDP) parallels our income logic for the measure of net remittance inflows: exports add income to the national economy, while imports represent an income loss (goods enter the national economy in exchange for money paid out to foreign producers).

[9] Our data for *GDP* and *GDPpcln* are expressed in 2005 U.S. dollars and come from the World Bank (2014).

Even if we treat this model as consistent with remittances reducing educational spending in more autocratic regimes (despite the lack of statistical significance for *Remittances*Polity*), there is no strong substitution effect in terms of *Military Spending* as a private good. In other words, even if remittances reduce the government's effort towards this public good, it does not strongly translate into greater private goods as captured by military spending. If one is concerned that the results here are only weak here because of potential attenuation bias associated with a specification that includes both a lagged dependent variable and unit fixed effects (Nickell 1981), then we can report that the results are even weaker if we drop the fixed effects. For example, the statistically significant *Remittances* coefficient in the *Education Spending* model would lose strength and statistical significance without the unit fixed effects: -0.002 (0.005).

[Table 1 here]

If the weak results in Panel A of Table 1 are not an artifact of attenuation, or Nickell, bias, then perhaps they are weak because of our operational measure for democracy. To test if this is indeed the case, we use an alternative democracy measure in Panel B of Table 1: the dichotomous democracy/ dictatorship measure (*DD*) coded by Cheibub, Gandhi, and Vreeland (2010), updating the ACLP measure from Przeworski et al. (2000). *DD* follows a minimalist definition of democracy based on the presence/absence of contested elections and is coded as 1 for democratic regimes and 0 for non-democracies. We view these results as even weaker for the substitution hypotheses: when using *DD*, only half the signs are even consistent with substitution expectations (which is what we would expect by chance alone). While the *Remittances* interaction term achieves statistical significance in the *Health Spending* model, this isolated result simply associates remittances inflows with greater health spending in more democratic regimes, but there is no strong evidence of remittances leading to a reduction in health spending in the most autocratic regimes (this would require a statistically significant negative coefficient for the *Remittances* constitutive term). As before, these weak results are not due to the

presence of fixed effects in our specification. Indeed, without fixed effects, we find that remittances increase (not decrease) health spending in the most autocratic regimes.

Another possibility for the weak results in Table 1 may be the short time lag. Perhaps the remittances substitution effect can only be observed over a longer time frame. To test for this possibility, we report a parallel set of results in Table 2, now with a *five* year time lag on the independent variables. In Panel A of Table 2 using the *Polity* measure for democracy, there are no statistically significant *Remittances* coefficients in the three spending models. In Panel B using the *DD* measure, there are also no statistically significant *Remittances* coefficients. Furthermore, the signs on the *Remittances* coefficients are uniformly positive in Panel B, suggesting that remittances can be weakly associated with more government spending in all categories and with reductions in none.

[Table 2 here]

To summarize across the 12 *Spending* models in Tables 1 and 2, there is not even one model where the two *Remittances* coefficients (constitutive and interactive) are both consistent with the proposed substitution effect in less democratic regimes. Furthermore, this is not simply a story of statistical insignificance: the signs on the *Remittances* coefficients are often inconsistent with the substitution hypotheses. We acknowledge that there are other possible democracy measures and alternative statistical specifications; thus, other scholars may be able to find more supportive results (e.g. Easton and Montinola 2015). But our results certainly suggest a lack of robust empirical support for the substitution hypothesis that underlies the remittance curse proposition.

Why No Remittance Substitution Effect?

But in arguing against a remittance political curse, it is not enough to demonstrate empirically that one cannot observe a substitution effect associated with migrant remittances. One should also try

to *explain* why remittances should not lead to the expected reduction in public goods. Our explanation proceeds in two steps. The first is to show that remittance inflows increase national income. The second associates this increase in income with a greater demand for public goods. Thus, while the basic logic of the substitution effect may be operative, we cannot observe the expected reduction in the supply of public goods because it is offset by an increase in citizen demand for the same following an income effect. Stated differently, the causal logic underlying the remittance political curse is incomplete by ignoring a countervailing remittance income effect.

Several papers in the Economics literature show that migrant remittances help reduce poverty and promote economic development in the remittance-receiving country (e.g. Stark and Lucas 1988, Adams and Page 2005, Acosta et al. 2008, Catrinescu et al. 2009). For the purposes of our explanation, it does not matter exactly *how* remittances have this effect (e.g. it could be through increased consumption or investment or some combination of both), but it is important that remittances have a positive effect on national economic development. Since other papers have challenged this relationship (e.g. Chami, Fullenkamp, and Jahjah 2003; Barajas et al. 2009) and because it will also be central to our remittance blessing argument in the second part of the paper, it is important to make another empirical demonstration here.

We do this with the same data/variables used in the previous subsection, now treating *GDPpcln* – a standard measure of economic development - as the dependent variable and *Remittances* as the primary independent variable. As before, our econometric specification includes a lagged dependent variable, so we are effectively measuring *the change in* economic development, along with fixed effects to address endogeneity (e.g., country specific factors that explain both economic development and net remittance inflows).

The first column in Table 3 presents this model of economic development, and the results in Panel A show a positive and statistically significant coefficient for *Remittances* lagged one year. Given the logged value of the dependent variable (*GDPpcln*), this coefficient (0.0019***) can be read as a semi-

elasticity, indicating that a one percent increase in the net remittance inflow share of GDP can be associated with a 0.2 percent increase in GDP per capita [100*(exp(0.0019)-1)]. On a comparative basis, this remittance development effect is about 60 percent greater than the same for *Oil* and for *Aid*, which are all measured on a similar basis (as a share of GDP).

[Table 3 here]

In the second column of Table 3, we estimate a model with an even more fundamental measure of economic development - the annual growth rate of GDP per capita (*Growth*) - as the dependent variable. In Panel A, *Remittances* (lagged one year) once again takes on a statistically significant positive coefficient (0.15***), indicating that a one percent increase in the share of net remittance inflows can be associated with a 0.15 percent *increase* in the economic growth rate in the following year. As shown in the appendix, a one standard deviation increase in *Remittances* is approximately 5.6, which can be associated with a 0.8 percent increase in the annual per capita growth rate in the next year.

Of course, economic development, or modernization, is associated with more than just increases in national income. So in the third column of Table 3, we use an even broader indicator for modernization: the human development index (*HDI*), a composite measure (scaled 0-1) that includes life expectancy, education, and per capita income.[10] The results in Panel A also show a statistically significant effect for *Remittances* lagged one year even with fixed effects and a lagged dependent variable. The latter is notable because HDI is a very sticky country/year measure; if we were to drop the lagged dependent variable, the coefficient for *Remittances* would more than quadruple to 0.0034*** (0.0009).

In Table 3 Panel B, we present a parallel sequence of economic development/growth models with all independent variables now lagged five years. Over this longer time frame, *Remittances* have an

[10] Our *HDI* data come from the United Nations Development Programme (2014). To avoid listwise deletion, we interpolate the missing internal observations for each country's *HDI* time-series.

even greater effect on the change in *GDPpcln*, the dependent variable in the first column: a four-fold increase from 0.0019*** with a one year lag in Panel A to 0.008*** with a five year lag in Panel B.

Given that the dependent variable in the second column is the per capita economic growth rate *measured annually*, we should not expect to observe a larger effect with a five-year lag; in fact, one might expect to observe a diminished effect on the annual growth rate five years later. But even five years later, *Remittances* have a statistical significant positive effect on the annual per capita economic *Growth* rate (0.12***).

In the third column of Panel B, one can observe the positive effect of *Remittances* over a five-year time frame on *HDI*, our broadest measure of economic modernization. The effect of remittances with a five year lag on the human development index represents an almost six-fold increase in what was observed with a one year lag: from 0.00008* in Panel A to 0.00045** in Panel B.

From the argument and evidence that remittances promote long-term income growth and economic modernization, we now move to the second step in our explanation: the demand for many public goods increases with income. The positive relationship between income and the demand for public goods has been offered as the basic logic underlying Wagner's law (Wagner 1892) that economic development leads to an expansion in the size of government, both in terms of civilian spending (including public goods, welfare services, and infrastructure) and in non-military state activity (including rule of law and contract enforcement). As an empirical statement, Wagner's law has much supportive evidence (e.g., Chang 2002, Akitoby et al. 2006, Lamartina and Zaghini 2011), including our own results in Tables 1 and 2: the statistically significant positive coefficients for *GDPpcln* in the models for *Education* and *Health Spending*, but not for *Military Spending*.

The theoretical logic that has been offered to explain the empirical result known as Wagner's law concerns societal *demand* for these non-military goods and services, which is expected to increase with modernization. The latter exposes new economic vulnerabilities, leading citizens to demand more protective goods (e.g. health care) from the state. Modernization also creates new economic

opportunities, leading citizens to demand that the government provide additional public goods like infrastructure and education. As Meltzer and Richard (1981, 923) summarized on this point: "government is a luxury good so that there is a positive relation between the relative size of government and the level of real income." As citizens get wealthier, on average, through remittances (or through any other income stream), they should demand more government spending in categories like education and health.

In fact, there is already country-specific evidence consistent with this income demand effect *through remittances*. In El Salvador, Edwards and Ureta (2003) show how remittances have a large impact on school attendance and retention, increasing the demand for further education at higher levels. Yang (2008) makes a similar demonstration for remittances in the Philippines. In Mexico, Frank et al. (2009) present evidence showing that remittances increase the demand for health care services.

The key point here is that even if the basic logic of the remittance substitution hypothesis is correct, its impact is offset by a remittance income effect. Stated differently, if we understand the remittance substitution effect as a potential supply-side story - autocratic governments want to supply fewer public goods so they can pay for greater private goods - they are unable to reduce this supply due to increased demand for public goods associated with the remittance income effect.

Alternatively, if we understand the remittance substitution effect as a demand-side story[11] - citizens need fewer public goods when they can pay for some of their own education and health care - the remittance income effect is offsetting on the demand-side. For example, while citizens may need less state-provided *elementary* education when they can pay for private primary schools through remittances, this primary schooling also increases the demand for state-provided *secondary and tertiary* education. Likewise, while citizens may want less state-provided *primary* health care when they can pay

[11] The authors who argue for remittance substitution (e.g. Abdih et al. 2012, Ahmed 2012) do not clearly state whether it is better understood as a supply-side or a demand-side effect. So we address it here as both.

for medical emergencies through remittances, they will also demand more state-provided *preventive* health-care as a result of their survival.

To summarize thus far, we have empirically demonstrated that there is no robust evidence consistent with a public goods substitution effect for migrant remittances, which is the primary causal story for why there would be a remittance political curse. We have also explained why one should not expect to observe a remittance substitution effect for public goods: because remittances produce an offsetting increase in demand for (non-military) government goods and services through an income effect. But this analysis only counters remittances as a political curse; it does not demonstrate that migrant remittances serve as a political blessing. We now turn to this second step in our argument.

Part 2: Remittances and Democratization

Our argument for how remittances should serve as a political blessing builds on two related foundations. The first comes from the empirical results already presented (in Table 3) showing that remittances inflows promote income growth and economic development. The second is modernization theory proposing that income growth and economic development facilitate democracy. Having established the first foundation in the previous section, this section will focus on the second before testing our hypothesis that migrant remittances are positively associated with movement towards a more democratic political regime (i.e. democratization).

A Broad Modernization Theory

Modernization theory has its origins in Lipset's (1959) study, and the positive relationship between economic development and democracy has subsequently been identified as "the strongest empirical generalization we have in comparative politics to date" (Boix 2003, 1-2).[12] Our theory of

[12] Przeworski et al. (2000) offered an empirical challenge to modernization theory, arguing that economic development does not promote democratic transitions (i.e. no "endogenous democracy"); it

modernization also includes recent arguments about the effect of inequality on democracy (e.g., Boix 2003, Ansell and Samuels 2014)[13] since income inequality *is endogenous to income growth* (i.e. inequality stems from the relative differences in income growth between/among socio-economic classes). As we will explain below, the demand for asset protection in Ansell and Samuels (2014) works through income growth in the rising middle class (which may be facilitated by remittances). Likewise, the elite's willingness to supply greater redistribution per Boix (2003) works through income growth in the poorer lower class (which may also be facilitated by remittances).[14] Our modernization theory is thus broad because it includes both asset protection and redistribution. It is also broad because it applies to political development across a variety of less democratic regimes and is not fit to a specific regime type (e.g. party-based dictatorships).

To consider the effect of income growth on democratization, we begin in a non-democratic political system with three socio-economic classes. At the top is a small group of rich elites, who support the autocratic regime. Opposed to the autocratic regime are the rising middle class and the large poor lower class. The autocratic regime includes a regressive tax system with the middle and lower classes paying more than the rich elites, and the state uses most of this tax revenue to pay for patronage/private goods in an effort to maintain the support of the rich elite. But households in both

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only helps democracies to survive and consolidate (i.e. "exogenous democracy"). But, in fact, their statistical evidence is consistent with endogenous democracy (ibid 124, 133). Furthermore, with data/sample improvements and statistical corrections, Boix and Stokes (2003) and Epstein et al. (2006) provide even stronger evidence consistent with endogenous democracy. We thus do not provide another empirical demonstration within this paper.

[13] These scholars place their arguments about the effect of inequality on democracy into modernization theory, instead of contrary to modernization theory as done by Acemoglu and Robinson (2006).

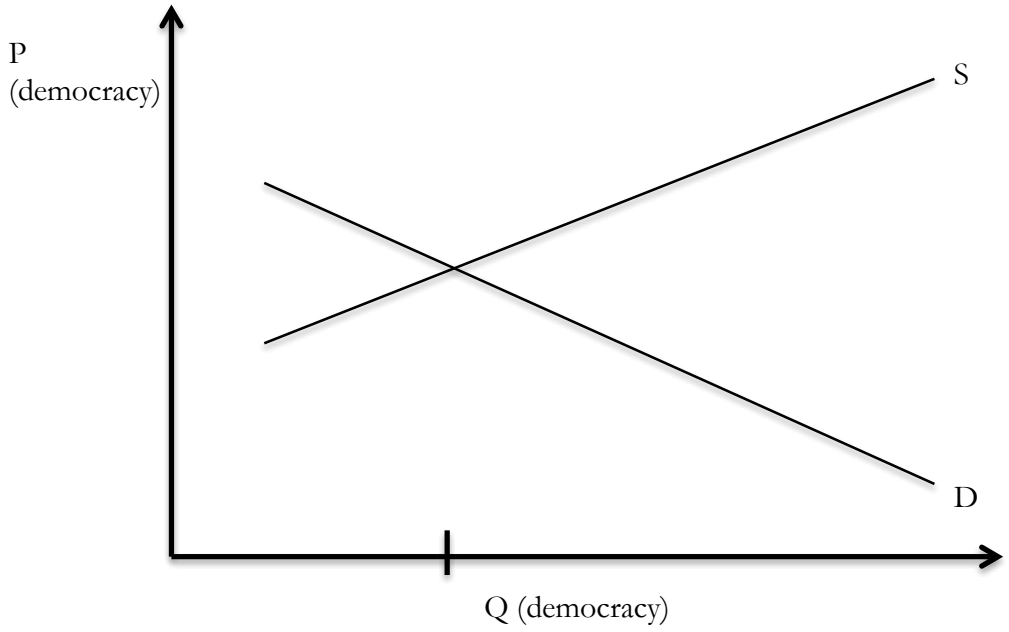
[14] Since these scholars (Ansell and Samuels 2014, Boix 2003) offer contrary results on the empirical relationship between inequality and democracy, it is important to note that remittances have an ambiguous effect on income inequality (e.g., Acosta et al. 2008, Barham and Boucher 1998). To the extent that middle-class families receive more remittances, their resulting income growth may increase inequality vis-à-vis the poorer lower class. But if lower-class families receive more remittances, then their income growth may reduce inequality relative to the richer classes.

the middle and lower classes can send family members to work abroad, and their income would be increased by remittances.

Because they are poor, households in the lower class demand at least some redistribution on the part of the state. To facilitate this economic redistribution, including greater public goods like education and health care, the poor also want a greater political voice (i.e. more democracy) following the standard redistributionist logic in many democratization models. However, the rising middle class is less interested in redistribution and more concerned with asset protection, following the logic offered by Ansell and Samuels (2014). The middle class demands more public goods such as infrastructure to facilitate their business growth and greater law and order to protect their existing economic assets. To better guarantee this asset protection, the rising middle class also demands greater political representation in exchange for their taxation (i.e., "no taxation without representation").

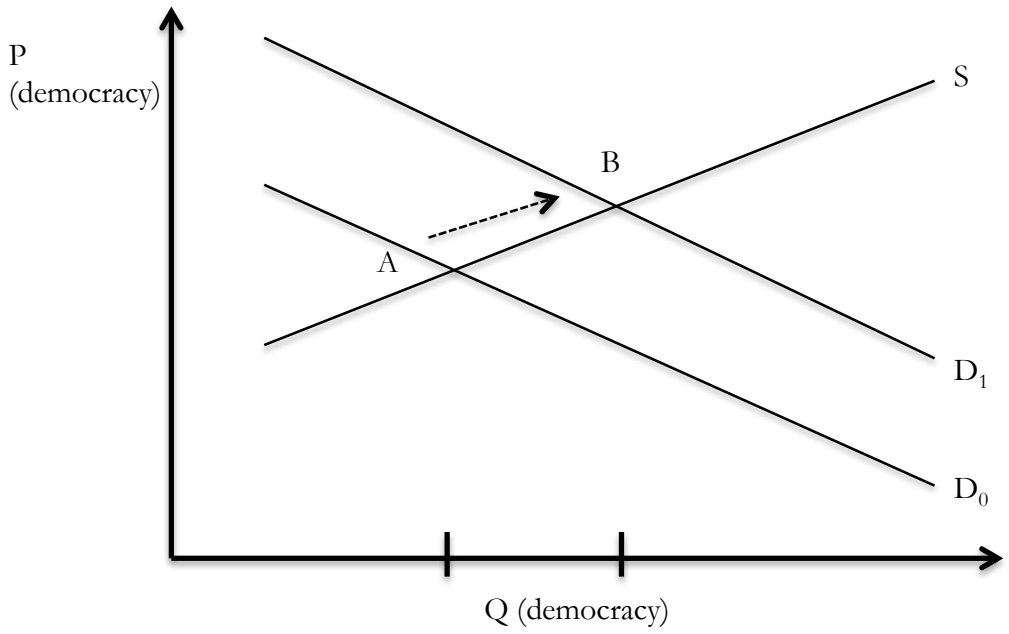
This broad version of modernization theory treats more protective government services, greater welfare goods, and increased political representation as a more democratic package. In this regard, it is important to note the theoretical correspondence between the logic underlying Wagner's law (income growth leads to greater demands for government services and welfare goods) and modernization theory (income growth leads to greater demands for political representation and democracy). We illustrate this logic using a supply and demand diagram, focused on the price and quantity of democracy (the latter being our dependent variable.

Figure 1: The Supply and Demand for Democracy (without Remittances)



The quantity (Q) of this democratic package is given on the x-axis in Figure 1. Citizens who demand these features pay for them with their tax revenues, although the tax system can also be expected to become somewhat more progressive with greater democracy (more on this below). The tax revenues paid by citizens to support their demand for protective services, welfare goods and political representation effectively represent the price (P) for democracy on the y-axis in Figure 1. When the price is high, citizens should demand a lower quantity of protective services and welfare goods, along with less political voice to safeguard them. Hence, one observes a downward sloping demand (D) schedule. When citizens are willing to pay a higher price for these related features, the state should be more willing to supply them. Thus, the supply (S) schedule is upward sloping with regards to price. As illustrated in Figure 1, the demand and supply schedules intersect at an equilibrium that includes a relatively low quantity of democracy. One can think of this as representing the non-democratic political regime (described above) in a lower-income situation that does not include remittances inflows.

Figure 2: Increased Demand for Democracy (with Remittances)



But with income growth due to remittances (or any other income-generating factor), the demand for the democratic package increases at any price. Stated differently, the basic logic of modernization theory is that citizens with rising incomes are willing to pay more to support their demand for increased protective services, welfare goods, and political representation. This expectation is illustrated in Figure 2 with an upward shift in the demand schedule from D_0 to D_1. Holding supply constant, this income-driven demand shift moves the equilibrium from A to B, the latter associated with a greater quantity of democracy (i.e. democratization).

Income growth (due to remittances and/or other factor) may also increase the state's willingness *to supply* a more democratic regime. Modernization theory has traditionally been understood primarily as a demand-side story, but Boix's (2003) theory for why reduced inequality makes the elite more willing to accept democratic reform adds a supply-side logic. We make use of a parallel argument focused on income growth.

As noted above, democratic reform includes a more progressive tax system, which means that the elite must provide a larger share of the tax revenue to support the government's protective services and welfare goods. This would be very costly for the elite when the rest of the citizens are poor and thus unable to pay so much in taxes (even when their tax rate is high). But as middle and lower class citizens get richer (via remittances), they are correspondingly able to pay more in taxes,[15] meaning that the shift to a more progressive tax system need not be so large, or so expensive for the rich elite, to support the same quantity of the democratic package.[16] Stated differently, at a constant tax rate for the middle and lower classes, the state receives more tax revenue as these citizens get richer. And with more tax revenue coming from the lower and middle classes, the state does not need to increase taxes so much on the rich elite (i.e. a less progressive tax shift). Hence, supplying more democracy becomes less costly for the rich elite and also for the state, relying on the former's political support.

The state's increased willingness to supply a more democratic regime is illustrated in Figure 3 with an outward shift in the supply schedule from S_0 to S_1. This shift moves the supply and demand equilibrium from B to C, further increasing the quantity of democracy. The key point here is that income growth can lead to greater democracy through demand- *and/or* supply-side effects. Furthermore, this modernization theory should apply broadly: to any factor that increases average income among the masses, including but not limited to remittance inflows. It also potentially applies across a variety of less democratic political regimes and is not specifically fit to any single autocratic regime type (e.g., party-based dictatorships, military governments, personalist autocracies, and/or monarchies).

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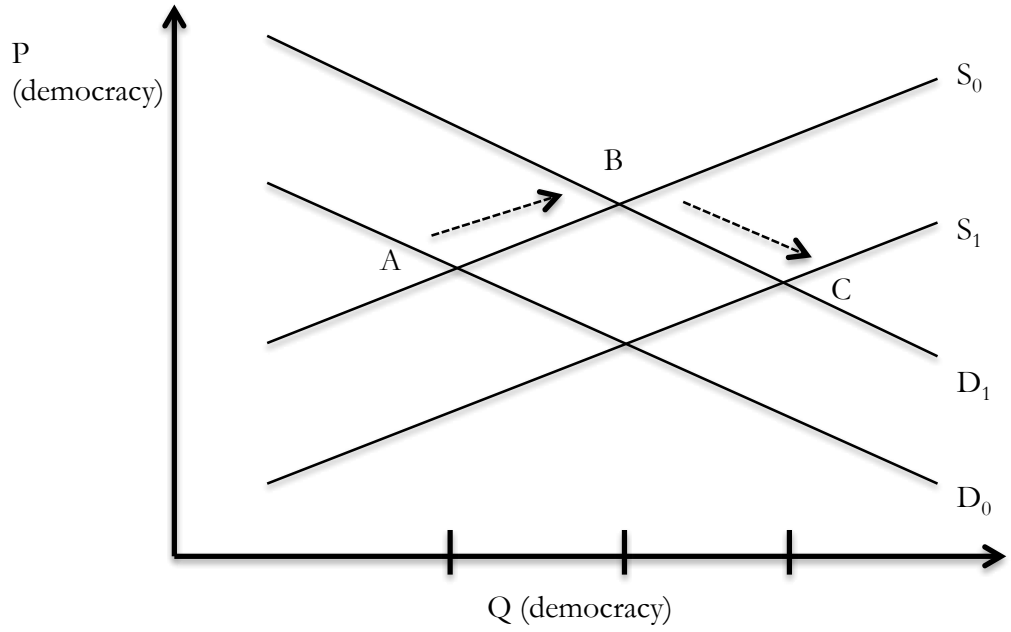
[15] We recognize that remittances are often unreported as income, thus they would not increase tax revenue primarily through an income tax. But if lower and middle class citizens use their remittances to increase their consumption, then the state receives more revenue through sales taxes (Singer 2012). And to the extent that remittances fund productive investments, the state may also receive more revenue through value-added taxes.

[16] This understanding is consistent with the empirical evidence showing that democracies often have less progressive, or more regressive, tax systems than one might expect (e.g., Cheibub 1998, Timmons 2010, Garcia and Von Haldenwang 2015).

Figure 3: Increased Supply of Democracy (with Remittances)



Hypothesis Testing

Our hypothesis about migrant remittances, working through this broad version of modernization theory, is that *net remittance inflows should be positively associated with democratization.* Following Mainwaring (1992, 298 *emphasis in original*), we define democratization as "movement *toward* democracy", which may or may not include a democratic transition (defined as when a formerly autocratic regime crosses some threshold to become democratic). As noted in the introduction, the scholars arguing for a political remittance curse did not directly test this relationship. Ahmed (2012) looked at government survival, but autocratic governments may survive *by engaging in limited democratic reform.* Abdih et al. (2012) considered corruption, but this concept is not a direct measure of political regime type even if democracies usually have less corruption than autocracies.

We test the hypothesis above by estimating multiple versions of equation (3) below. As operational measures for *Democracy*, our dependent variable, we use - in sequence – the same two

indicators as introduced in Table 1 (as independent variables): *Polity* and *DD*. The first focuses on executive constraints (or checks and balances) and the second on the presence/absence of contested elections. As shown in equation (3), our models include a lagged dependent variable, which means that we are estimating *the change in Democracy*, controlling for its previous level.

$$(3)\quad Democracy_{it} = B_1 Democracy_{it-1} + B_2 Remittances_{it-1} + B_x Controls_{it-1} + e$$

In terms of the control variables, our models of political regime change take a reduced-form specification, meaning that we deliberately omit, as controls, the intermediate variables (from modernization theory) between remittances and democracy. These include variables like GDP per capita, economic growth, life expectancy, and education. Stated differently, since we expect remittances to have their democratizing effect *through* economic development and income growth, operational measures related to these concepts should not be included in our right-hand side specification.

We do, however, directly control for the other economic flows that might influence the political regime: *Oil*, *Aid*, and *Exports* more generally. We also include *GDP* as a control variable to make sure that the denominator in our economic flow measures does not drive the results. And as before, our specification also includes a full set of unit fixed effects, which are often not included in cross-national time-series models of democracy to deal with endogeneity in the form of selection (i.e., remittance inflows are not randomly assigned and may be driven by some country-specific factor that also explains its political regime).

[Table 4 here]

Estimates of this *Democracy* specification are presented in the first two columns of Table 4. In the first column, *Polity* is the operational measure and *Remittances* lagged one year takes on a statistically significant positive coefficient (0.034***), consistent with a political remittance blessing. In terms of the other economic flows, we find evidence of a political oil curse (consistent with the results in

Andersen and Ross 2014) and a weak aid blessing (consistent with results offered by Goldsmith 2001 and Wright 2009). In terms of the substantive significance of our *Remittances* coefficient, a one-standard deviation increase in *Remittances* (~5.6) translates into an expected 0.2 increase in the *Polity* score in the next year. If this effect seems small, then it is useful to compare it to the estimated *Oil* curse effect: the former is about 50 percent larger than the latter, although these economic flows move democracy in different directions. A similar comparison can be made between the blessing effect of *Remittances* and the same for *Aid*: the former is 70 percent greater than the latter.

In the second column of Table 4, the dichotomous *DD* becomes our operational measure for democracy. In a specification with unit fixed effects, this represents a very strong test for our hypothesis since we lose any country time-series that does not experience either a democratic transition (*DD* moving from *0* to *1*) or a democratic reversal (*DD* moving from *1* to *0*); hence N=1108. But even with this alternative democracy indicator and a reduced sample size, *Remittances* lagged one year again takes on a statistically significant positive coefficient (0.15**), which would be 50 percent larger if we dropped the lagged dependent variable (0.22*). Using *DD*, we also find evidence consistent with a political oil curse and a foreign aid blessing, but the results in the second column show that the substantive impact of *Remittances* is larger than that of either *Oil* or *Aid*.

Having established a positive association between remittances and democratization, how can we be certain that this relationship is consistent with our theory of remittance-driven economic modernization? To assess how much of this positive relationship comes through economic modernization, we now add the three dependent variables from Table 3 (*GDPpcln*, *Growth*, and *HDI*) to our specification as independent variables to see how much they attenuate the effect of *Remittances* on democracy. If the observed remittance effect in the first two columns stems largely from the remittances effect on economic development/modernization, then we should be able to observe that the inclusion of variables for national income, economic growth, and human development significantly reduces the effect of *Remittances* in our democracy models.

These variables are added to the *Polity* model in the third column and to the *DD* model in the fourth column of Table 4. When including these intermediate variables, one can observe that *Remittances* loses statistical significance in the *Polity* model. Comparing this coefficient from the first column (without *GDPpcln*, *Growth*, and *HDI*) to the same in the third column, the impact of *Remittances* is reduced by more than 50 percent (from 0.034*** to 0.016). Thus, while we cannot say that the entire remittance blessing effect comes from economic modernization, the results in the third column indicate that a large portion of the remittance blessing effect is consistent with this causal story.[17] This portion is even larger in the *DD* model: comparing the *Remittances* coefficient from the second column (the reduced-form equation) to the same in the fourth column (adding *GDPpcln*, *Growth*, and *HDI*), the remittance blessing effect is cut by about 75 percent (from 0.15** to 0.04).

This attenuation of the remittance blessing effect with the inclusion of variables for economic growth and development is consistent with our causal argument concerning modernization. But how can we be certain that the statistically significant positive *Remittances* coefficients in the first two columns of Table 4 are not driven by reverse causality? Do more democratic regimes simply allow greater remittance inflows?

To test for this possibility, we reverse the independent and dependent variables from equation (3), regressing *Remittances* on each of our two *Democracy* measures, in sequence. In the fifth column of Table 5, *Polity* becomes an independent variable, and one can observe no statistically significant relationship between *Remittances* and *Polity* lagged one year, although the *Polity* coefficient is positively signed. In the sixth column, we repeat this exercise using *DD*, and the results here also fail to show a statistically significant relationship between *Remittances* and *DD* lagged one year. We read these results to indicate that the positive association between democracy (however measured) and net remittance

[17] Consistent with modernization theory, *HDI* - our broadest operational measure for economic development - takes on a positive sign in these democracy models. The negative sign on *GDPpcln* is an artifact of collinearity with *HDI*; their bivariate correlation is greater than 0.9. Without *HDI*, *GDPpcln* would take on a positive coefficient, also consistent with modernization theory.

inflows is stronger when *Remittances* is lagged than when the democracy variable is lagged, suggesting that the primary causal relationship runs from remittances to greater democracy, rather than from democracy to greater remittances.

In Tables 2 and 3 (Panel B), we looked at the effect of remittances over a longer time frame, using a five year lag. We now do the same with our two democracy measures as the dependent variable, an exercise with two purposes. The first is to establish the longer-term democratization effect of remittances, and the second is to address further the direction of causality. If the primary causal relationship runs from remittances to greater democracy, then we should be able to observe an even greater democratization effect with a longer time lag on *Remittances*. Conversely, if the primary causality runs from democracy to greater remittances, then we should observe an attenuation of the *Remittances* coefficient compared to the results with a one year lag.

[Table 5 here]

These two models are presented in the first two columns of Table 5. With *Polity* as the dependent variable, the *Remittances* coefficient grows to 0.094* with a five year lag, a three-fold increase from what was observed in Table 4 with a one year lag. With *DD* as the dependent variable, the *Remittances* coefficient grows to 0.53*** with a five year lag, a greater than three-fold increase from the same model in Table 4 with a one year lag.

As mentioned in the introduction, Escribà-Folch, Meseguer, and Wright (2015) also present evidence consistent with a remittance political blessing in party-based dictatorships based on the logic that this economic flow makes voters less dependent on patronage. We offer no objection to their causal story, but our argument for a remittance political blessing intends to apply more broadly than just party dictatorships. And since Escribà-Folch, Meseguer, and Wright have already shown a

remittance blessing in this set of autocratic regimes, it is important to demonstrate that our results are not driven by a blessing effect *unique* to party-based dictatorships.

To make this demonstration, we drop all country/year observations associated with a party dictatorship, using the classification of autocratic regime types offered by Geddes, Wright, and Frantz (2014). The results using this restricted sample (excluding party dictatorships) are presented in the third and fourth columns of Table 5, and they show statistically significant evidence consistent with a political blessing for remittances. On this basis, we conclude that the remittance blessing found by Escribà-Folch, Meseguer, and Wright (2015) in a relatively small sample of party dictatorships does not drive the full sample results found in our paper. The hypothesized broad-based remittance political blessing is also present among other types of less democratic regimes, including military governments, personalist autocracies, and monarchies.

Finally, for readers who may be concerned that our measure of *net* remittance inflows may be producing peculiar results based on the negative values associated with country/year observations that send more remittances than they receive, we drop all observations that take on a negative value for *Remittances*. This leaves a restricted sample that consists of *developing* countries that receive as much or more remittances than they send, omitting the net senders that consist mostly of *developed* countries. These results, presented in the last two columns of Table 5, show continued evidence of a political blessing for remittances and precisely where we would expect to find it most: in the developing countries that are net remittance receivers. Our full sample results are thus not an artifact of including net remittance senders.

Conclusion

This paper offers a two-part challenge to the proposition that remittances hinder democratization, or act as a political curse in the countries that receive them. It first addresses the

causal logic underlying this curse proposition: that remittances allow less democratic governments to divert public goods spending towards patronage spending. We directly test this substitution hypothesis and find that it has little empirical support. Remittance inflows have no effect in reducing public goods spending related to education and health. They also have no effect in increasing military spending as a primary patronage good in less democratic regimes. We next explain why one should not observe this substitution effect as a result of migrant remittances: remittance inflows increase national income, and a richer society *demands* more of its state in terms of public goods provision. Thus, any remittance substitution effect to reduce public goods is offset by a demand-side income effect to increase them.

In the second part of our argument, we build from the empirical results showing that remittance inflows increase national income. Per a broad version of modernization theory, the economic development that can be associated with remittances both potentially increases the societal demand for and the state's willingness to supply a more democratic political regime. We thus hypothesize that remittances should facilitate (not hinder) democratization, and our results provide evidence to show that net remittance inflows can be associated with change towards greater democracy. We also show how this positive association between remittances and democracy stems in large part from the modernization effect associated with this economic flow and that it cannot be explained by more democratic governments simply allowing greater remittance inflows. We thus conclude that remittances do not act as a political curse; instead, they function as a political blessing.

Appendix: Descriptive Statistics

Variable	Mean	Std. Dev.	Min.	Max.
Education Spending	4.59	1.99	0.78	44.33
Health Spending	3.67	2.02	0.48	9.93
Military Spending	2.32	2.23	0.001	39.62
Remittances	1.92	5.57	-18.08	60.65
Oil	3.88	10.35	0	88.82
Aid	4.48	8.31	-0.72	164.42
Exports	-5.19	14.29	-118.26	47.61
GDP	307.4	1107.3	0.21	14231.57
GDPpcln	7.92	1.61	4.73	11.38
Growth	2.05	4.63	-47.31	36.77
HDI	0.62	0.18	0.19	0.94
Polity	3.47	6.75	-10	10
DD	0.56	0.50	0	1

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Table 1: Models of Government Spending with One Year Lag.

A. Using *Polity*

DV:	*Education Spending*	*Health Spending*	*Military Spending*
LDV	0.802***	0.792***	0.682***
	(0.033)	(0.034)	(0.077)
Remittances	-0.026*	-0.006	0.016
	(0.013)	(0.009)	(0.014)
*Remittances * Polity*	0.003	0.0003	-0.001
	(0.002)	(0.0006)	(0.001)
Oil	0.007	0.002	-0.010
	(0.009)	(0.004)	(0.016)
Aid	0.014	0.004	0.001
	(0.009)	(0.003)	(0.003)
Exports	-0.004	-0.0001	0.001
	(0.004)	(0.002)	(0.004)
GDP	-0.00004	0.00016***	0.00004*
	(0.00004)	(0.00005)	(0.00002)
GDPpcln	0.261***	0.296***	-0.217*
	(0.074)	(0.071)	(0.119)
Polity	-0.002	0.004	-0.004
	(0.006)	(0.005)	(0.007)
N	1646	2127	2587
R^2	0.877	0.929	0.849

B. Using *DD*

DV:	*Education Spending*	*Health Spending*	*Military Spending*
LDV	0.802***	0.750***	0.662***
	(0.030)	(0.032)	(0.087)
Remittances	0.005	-0.007	-0.012
	(0.016)	(0.006)	(0.011)
*Remittances * DD*	0.001	0.013*	0.021
	(0.018)	(0.008)	(0.015)
Oil	0.003	-0.005	-0.016
	(0.010)	(0.005)	(0.019)
Aid	0.002	-0.001	-0.0004
	(0.009)	(0.002)	(0.0043)
Exports	-0.003	0.001	-0.001
	(0.005)	(0.002)	(0.004)
GDP	-0.00005	0.0002***	0.00004
	(0.00004)	(0.0001)	(0.00003)
GDPpcln	0.240***	0.518***	-0.196
	(0.069)	(0.091)	(0.121)
DD	0.160*	0.070	-0.007
	(0.082)	(0.048)	(0.088)
N	1587	1981	2239
R^2	0.858	0.878	0.853

Fixed effects included, but not reported.
OLS coefficients with robust standard errors clustered on the country.
Statistical significance: *** p<0.01, ** p<0.05, * p<0.1 (two tailed).

Table 2: Models of Government Spending with Five Years Lag.

A. Using *Polity*

DV:	*Education Spending*	*Health Spending*	*Military Spending*
LDV	0.605*	0.243***	0.188***
	(0.342)	(0.083)	(0.063)
Remittances	0.019	-0.005	0.024
	(0.043)	(0.034)	(0.026)
*Remittances * Polity*	-0.0001	0.003	-0.001
	(0.0028)	(0.002)	(0.001)
Oil	0.016	-0.004	0.011
	(0.022)	(0.009)	(0.010)
Aid	0.018	-0.006	0.002
	(0.021)	(0.013)	(0.006)
Exports	0.002	0.009	0.008*
	(0.015)	(0.007)	(0.005)
GDP	-0.00010	0.0007***	0.00025***
	(0.00012)	(0.0002)	(0.00004)
GDPpcln	0.601*	0.739***	-0.587***
	(0.356)	(0.230)	(0.200)
Polity	-0.022	0.010	0.002
	(0.039)	(0.012)	(0.014)
N	1216	1623	2095
R^2	0.391	0.590	0.138

B. Using *DD*

DV:	*Education Spending*	*Health Spending*	*Military Spending*
LDV	0.556*	0.198***	0.187***
	(0.303)	(0.076)	(0.063)
Remittances	0.002	0.020	0.002
	(0.035)	(0.013)	(0.014)
*Remittances * DD*	0.013	0.019	0.018
	(0.041)	(0.019)	(0.017)
Oil	0.021	-0.005	0.012
	(0.021)	(0.010)	(0.010)
Aid	0.011	-0.004	0.001
	(0.018)	(0.012)	(0.006)
Exports	-0.003	0.008	0.008*
	(0.013)	(0.008)	(0.004)
GDP	-0.0001	0.0007***	0.00025***
	(0.0001)	(0.0002)	(0.00004)
GDPpcln	0.629*	0.900***	-0.604***
	(0.350)	(0.221)	(0.195)
DD	0.149	0.124	-0.084
	(0.209)	(0.127)	(0.141)
N	1300	1835	2169
R^2	0.336	0.556	0.138

Fixed effects included, but not reported.
OLS coefficients with robust standard errors clustered on the country.
Statistical significance: *** p<0.01, ** p<0.05, * p<0.1 (two tailed).

Table 3: Models of Economic Development / Growth.

A. With One Year Lag

DV:	*GDPpcln*	*Growth*	*HDI*
LDV	0.973***	0.202***	0.994***
	(0.004)	(0.035)	(0.004)
Remittances	0.0019***	0.151***	0.00008*
	(0.0004)	(0.037)	(0.00004)
Oil	0.0012***	0.090***	0.00008**
	(0.0004)	(0.034)	(0.00004)
Aid	0.0011***	0.132***	0.00008**
	(0.0004)	(0.045)	(0.00003)
Exports	-0.0001	-0.014	0.00001
	(0.0002)	(0.019)	(0.00002)
GDP	0.000002	-0.0004*	-0.0000003
	(0.000002)	(0.0002)	(0.0000002)
N	4352	4342	3373
R^2	0.999	0.055	0.999

B. With Five Year Lag

DV:	*GDPpcln*	*Growth*	*HDI*
LDV	0.824***	-0.068***	0.962***
	(0.022)	(0.024)	(0.017)
Remittances	0.008***	0.117***	0.0005**
	(0.002)	(0.039)	(0.0002)
Oil	0.0001	-0.055	0.0002
	(0.0010)	(0.044)	(0.0002)
Aid	0.003***	0.050**	0.0003
	(0.001)	(0.024)	(0.0002)
Exports	0.001	-0.019	0.0001
	(0.001)	(0.015)	(0.0001)
GDP	0.00002	-0.0006**	-0.0000009
	(0.00001)	(0.0003)	(0.0000011)
N	3769	3759	2827
R^2	0.991	0.001	0.993

Fixed effects included, but not reported.
OLS coefficients with robust standard errors clustered on the country.
Statistical significance: *** p<0.01, ** p<0.05, * p<0.1 (two tailed).

Table 4: Democracy Models with One Year Lag.

DV:	*Polity*	*DD*	*Polity*	*DD*	*Remittances*	*Remittances*
LDV	0.865***	5.042***	0.844***	5.156***	0.888***	0.834***
	(0.012)	(0.295)	(0.016)	(0.430)	(0.025)	(0.030)
Remittances	0.034***	0.149**	0.016	0.035		
	(0.011)	(0.065)	(0.012)	(0.122)		
Oil	-0.023***	-0.126***	-0.019***	-0.181***	-0.004	-0.002
	(0.007)	(0.031)	(0.007)	(0.038)	(0.008)	(0.007)
Aid	0.020*	0.094**	0.016	0.104	0.011	-0.012
	(0.010)	(0.042)	(0.010)	(0.071)	(0.018)	(0.011)
Exports	0.011**	0.034	0.008	0.028	-0.004	-0.003
	(0.005)	(0.023)	(0.005)	(0.031)	(0.012)	(0.009)
GDP	0.000015	0.026**	0.000005	0.030*	-0.0000052	-0.000009
	(0.000018)	(0.011)	0.000026	(0.018)	(0.0000074)	(0.000010)
Polity					0.008	
					(0.006)	
DD						0.185
						(0.122)
GDPpcln			-0.713**	-9.095***		
			(0.274)	(2.672)		
Growth			-0.002	0.020		
			(0.006)	(0.043)		
HDI			4.595***	39.758***		
			(1.531)	(12.717)		
N	3805	1108	3105	817	3746	3724
R2 (Pseudo)	0.944	0.706	0.937	0.745	0.950	0.936

Fixed effects included, but not reported.
OLS (logit) coefficients with robust standard errors clustered on the country.
Statistical significance: *** p<0.01, ** p<0.05, * p<0.1 (two tailed).

Table 5: Democracy Models with Five Year Lag.

	Full Sample		No Party Dictatorships		Remittances ≥ 0	
DV:	*Polity*	*DD*	*Polity*	*DD*	*Polity*	*DD*
LDV	0.413***	1.628***	0.347***	1.457***	0.404***	1.999***
	(0.043)	(0.389)	(0.045)	(0.394)	(0.049)	(0.489)
Remittances	0.094*	0.532***	0.109**	0.443***	0.085**	0.417***
	(0.050)	(0.159)	(0.045)	(0.148)	(0.042)	(0.142)
Oil	-0.074***	-0.130**	-0.079***	-0.127**	-0.055	-0.118***
	(0.026)	(0.058)	(0.027)	(0.059)	(0.035)	(0.043)
Aid	0.085**	0.237***	0.064**	0.252***	0.041	0.257**
	(0.034)	(0.071)	(0.030)	(0.086)	(0.034)	(0.103)
Exports	0.033*	0.046	0.042**	0.051*	0.032	0.035
	(0.017)	(0.028)	(0.019)	(0.030)	(0.020)	(0.050)
GDP	0.0001	0.032	0.0001	0.028	0.0005	0.023
	(0.0001)	(0.020)	(0.0001)	(0.022)	(0.0006)	(0.014)
N	3319	883	2907	764	2104	542
R^2 *(Pseudo)*	0.714	0.444	0.711	0.414	0.721	0.423

Fixed effects included, but not reported.
OLS (logit) coefficients with robust standard errors clustered on the country.
Statistical significance: *** p<0.01, ** p<0.05, * p<0.1 (two tailed).