The Personal Vote and the Efficacy of Education Spending

Allen Hicken University of Michigan
Joel W. Simmons Stony Brook University

In this article we explore the ways in which incentives to cultivate a personal vote affect the efficiency of education spending in developing democracies. We argue that where the electoral system provides incentives for political particularism, resources are allocated less efficiently and the effect of increased spending on literacy is diminished. We test our hypotheses using data on education spending and performance in over 40 developing democracies since 1980. We find that though personal vote systems spend just as much on education as party vote systems, particularism in personal vote systems dampens the marginal effect of increased education spending on illiteracy and at its highest levels, incentives to cultivate a personal vote completely undermine the positive effects of increased education spending on literacy.

An important distinction between various electoral systems is the extent to which they generate incentives for politicians to cultivate a personal versus a party vote (Carey and Shugart 1995). Some electoral systems encourage candidates to develop a network of voters and donors tied directly to the candidate himself. This personal support network is typically much narrower than the constituency that emerges when a premium is placed on building and defending the party label. This distinction not only affects which strategies candidates for office employ on the campaign trail, but should also shape their behavior in office. Indeed, there is good evidence that incentives to cultivate a personal vote are correlated with particularism and pork barrel politics, less attention to the provision of national public goods, and a higher degree of corruption by elected officials (Ames 1995; Chang and Golden n.d.; Cox and Thies 1998; Golden 2003; Samuels 1999).

The assumption by much of the existing literature is that the pursuit of a personal vote via particularism or other means comes at a substantial cost in terms of public welfare. But is this really the case? This basic empirical question has been surprisingly underaddressed in the extant literature. And importantly, one can imagine counterarguments for why the systemic costs associated with particularism are minimal—largely offset by gains in other areas. We might think of pork barrel politics, for example, as simply a mechanism for dividing up national public goods to local constituencies. There may be costs in terms of lost economies of scale, but in exchange we get specialization—policies that are better calibrated to local needs and conditions.[1] Similarly, we might view pork, not as sand in the works of an otherwise efficient policy machine, but rather as the grease necessary to get the machinery to operate at all (Alston and Mueller 2005). In short, while recognizing that there may exist some extreme examples of high costs associated with particularism (Italy springs to mind), we might wonder whether personal vote systems are discernibly less efficient and effective at providing public goods than party vote systems. This is the question we tackle in this article.

To assess the effect of incentives to cultivate a personal vote on the provision of public goods, we begin by separating the amount of spending on programs normally conceptualized as public goods from the effectiveness of that spending. We argue that there is little reason to expect personal vote systems to spend substantially more or less, overall, on public services like education and

Allen Hicken is assistant professor of political science, University of Michigan, Ann Arbor, MI 48109-1045 (ahicken@umich.edu). Joel W. Simmons is assistant professor of political science, Stony Brook University, Stony Brook, NY 11794-4392 (joel.simmons@gmail.com).

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[1] Indeed, this is precisely the defense politicians often use for such behavior.

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health care.[2] However, the ways in which these public services budgets are distributed should vary significantly between different kinds of electoral systems. In personal vote systems, larger portions of public services budgets should go towards items for which politicians (a) can claim personal credit, and (b) can directly and personally control the distribution. We hypothesize that the costs of this type of distribution are substantial and come in at least two forms: efficiency losses and losses due to corruption. Thus we expect the same amount of public goods provision (for example achieving a certain infant mortality rate) to be more costly in personal vote systems compared to party vote systems. Put differently, the same amount of spending should purchase fewer public goods in personal vote systems and, as a result, public goods will be undersupplied in personal vote systems compared to party-centered systems.

We test our hypotheses using cross-national data on education spending and illiteracy for about 40 countries between the years 1980 and 1998. The focus on education variables is useful and interesting for several reasons. First, while education does not meet the strict definition of a public good it does produce the kinds of externalities that are often associated with public or collective goods. These positive, growth-enhancing externalities include technological innovation (Papapeorigiou 2003), greater labor productivity and higher earnings for labor (Glewwe 2002), greater agricultural productivity (Huffman 2001), less income inequality (Birdsall, Ross, and Sabot 1995), greater public health (Glewwe 2002), greater investment in physical capital (Acemoglu 1996), and the agglomeration of economic activity (Amiti and Pissarides 2002).

A focus on education variables also allows us to speak quite clearly about the distribution of government spending. We may not readily think of education policy as an instrument that politicians can manipulate for particularistic ends. Scholars of pork and particularism have tended to focus on infrastructure spending or social welfare policy. However, education spending, like all budgetary categories, can be allocated in particularistic ways even if it is typically denoted as a "public service." In fact, given that education spending makes up a sizeable share of total government spending in most developing democracies, it would be surprising if politicians did not try to particularize at least some aspects of education policy.[3] Where governments allocate a large share of total education spending towards public goods, we should observe improvement in literacy rates throughout the population. If, on the other hand, governments allocate a large share of total education spending to pork/particularism, narrow interests may benefit, but there need not be substantial improvements in literacy rates in the country. A final reason we use these variables is that doing so allows us to address the large and growing literature on the politics of government public service spending. Much of this literature is dedicated to the study of how political institutions (mostly regime type) and increased economic integration influences the amount of money governments spend on education and health care services for the population, (e.g., Lake and Baum 2001; Stasavage 2005), but little of it explores the extent to which that spending translates into education and health outcomes.

Our findings can be summarized as follows. Incentives for particularism dampen the marginal effect of increased education spending on illiteracy. In fact, at the highest levels, such incentives completely undermine the positive effects of increased education spending on literacy. In short we find that the effect of greater spending on education outcomes depends on the incentives to cultivate a personal or a party vote. Finally, we find no relationship between our measure of particularistic incentives and the level of education spending, which indicates that the effects of particularistic incentives operate through how a given pool of resources is allocated rather than the level of spending.

Theory

Over the past few years, a number of political scientists and economists have explored the ways in which electoral systems shape policymaking processes and outcomes. We are learning that the rules governing elections have substantial effects on policy, though these effects are largely indirect. For example, electoral systems help determine which actors get a seat at the table and which are excluded. Electoral systems also play a key role in determining how many actors are likely to have a say in policymaking through their effect on the number of political parties and party cohesion. Finally, electoral systems generate incentives for politicians to pursue certain policies over others via their effect on the nature of a politician's constituency (the groups to which he or she responds). It is this latter link between electoral incentives and policy that we explore in this article.[4]

[2] In truth, education and health care are collective rather than public goods since they are neither nonrivalrous nor nonexcludible.

[3] In our sample of countries, education spending's share of total government spending averages 15.7 percent.

[4] This is not to suggest that these are the only institutional incentives that affect economic policy. The literature on the effects of political institutions on policy processes and outcomes is large and includes a focus on the role of such institutional variables as regime type (or the separation of power; Persson and Tabelini 2003), the presence

We know that the broader the constituency to which politicians are accountable, the stronger the incentives to provide broad public goods. Alternatively, where politicians have very narrow constituencies, they tend to pursue goods and services that can be targeted to smaller groups (Bueno de Mesquita et al. 2004; Cox and McCubbins 2001; Franzese, Nooruddin, and Long-Jusko 2004; Lake and Baum 2001; Olson 2000; Stasavage 2005). Scholars interested in the effects of electoral systems have explored this relationship with an eye towards the different incentives generated by majoritarian versus proportional electoral systems. Several scholars argue, with some empirical support, that majoritarian systems are associated with greater targeting of goods and services to narrow (geographic) groups compared to PR systems (Milesi-Fereti, Perotti, and Rostagno 2002). On the other hand, other scholars contend that PR, especially closed-list PR, is more prone to corruption than other types of electoral systems (e.g., Persson and Tabellini 2003). However, within PR systems we observe a marked difference between those that privilege the cultivation of a party vote (closed-list PR) and those that privilege an individual's reputation and hence the cultivation of a personal vote (e.g., open-list PR). Open-list PR, for example, is associated with higher levels of perceived corruption than closed-list PR (Chang and Golden n.d.).

Chang and Golden (n.d.) are representative of a growing number of scholars interested in moving beyond the PR-majoritarian distinction, as useful as it may sometimes be, to focus instead on the extent to which the electoral and party rules generate incentives for a party vote and party-centered campaigns or a personal vote and candidate-centered campaigns (e.g., Ames 1995; Carey and Shugart 1995; Golden 2003; Golden and Chang 2001; Hicken 2006; Nielson 2003; Samuels 1999). The central argument in this literature is that under some electoral systems candidates for office distinguish themselves from their competitors primarily on the basis of partisan differences.[5] During election campaigns it is the party's label and reputation that are the key assets for candidates and parties. Typically these are systems where party leaders have strict control over party nominations and where a candidate's success is completely dependent on the party's success. Under other sets of rules, however, personal reputation and candidate-focused appeals supplant partisan appeals as the dominant strategy for candidates and the primary cue for voters. Incentives to cultivate a personal vote tend to be strongest where there is significant intra-party competition and where party leaders exercise only weak control over access to the party label.

If electoral incentives affect politicians' behavior on the campaign trail, should we not also expect these incentives to shape their behavior once in office? The answer appears to be yes. In personal vote systems, legislators place a greater emphasis than do their party vote counterparts on policies for which they (a) can claim personal credit, and (b) directly and personally control the distribution. Thus, studies of highly personalized systems such as Brazil, Italy, Japan, and Thailand find that the legislators in these systems tend to rely greatly on pork and particularism (Ames 1995; Chang and Golden 2005; Cox and Thies 1998; Golden 2003; Samuels 1999).

What we don't know is the extent to which the incentives to cultivate a personal vote and the resulting particularism ultimately affect public goods outcomes. After all, Japan and Italy are among the wealthiest countries in the world while Thailand and Brazil are two of the best performers in the developing world. This raises several questions. Are public goods truly undersupplied in personal vote systems? Does pork barrel spending come at the expense of spending on needed public goods or is it a relatively innocuous disaggregation of public goods expenditures (a different means to the same end)? In short, do personal vote systems really do a poorer job overall at producing needed public policies?

We argue that indeed, countries with a strong party vote should do a better job at supplying public goods than those with a strong personal vote. This is because the emphasis on policies that individual politicians can directly control and claim credit for generates two sets of costs. First, and perhaps most straightforwardly, personal vote systems are extremely vulnerable to corruption. The combination of the means (access to government resources), opportunity (control by politicians over some of those resources), and motive (the need to raise campaign funds) that can arise in personal vote systems is a recipe for official corruption. It's not surprising, then, that the personal vote and candidate-centered campaigns are regularly associated with high levels of corruption in the literature (Chang 2005; Chang and Golden n.d.; Cox and Thies 1998; Hicken 2006). Corruption, via diversion of state resources and weaker economic performance, ultimately reduces the amount of resources available to spend on public goods.[6]

of central banks (Cukierman, Webb, and Neyapti 1992), fiscal institutions that enhance or decrease fiscal transparency (Alt and Lassen 2006), and the number of veto players (Cox and McCubbins 2001). Where appropriate and feasible we control for other institutional variables in our analyses.

[5]There may also be important differences between candidates and parties within a given system (Cox and Thies 1998; Golden and Chang 2001; Samuels 1999).

[6]See Mauro (1995) for the link between corruption and economic performance.

Even if corruption is low, however, the process of carving up social welfare spending into targetable, politician-controlled morsels often carries severe efficiency costs.[7] To begin with, the diversion of government funds to a particular district imposes costs on other districts and may come at the expense of national policy goals (Lyne and Carroll 2006). In short, the morselization of public spending implies a decision to forego potential gains from economies of scale. This loss might be worthwhile if offset by a more efficient delivery of needed goods and services to smaller constituencies—i.e., specialization. For example, politicians armed with information about the most pressing education needs in their district could potentially make more efficient use of education funds in that district than would be possible if spending were mandated and controlled by central government officials. However, given politicians' electoral incentives it is perhaps not surprising that their first priority is often not efficiency. Instead, their primary concern is to use the funds at their disposal in ways that help them develop and maintain the personal support network necessary for reelection. When allocation and distribution are based primarily on political rather than economic calculations, dead weight losses occur and as a result outcomes may depart quite substantially from any measure of economic or social efficiency.[8]

A couple of examples from the Philippines underscore the way in which public goods provision may suffer where there are strong incentives to cultivate a personal vote. Filipino legislators have access to large amounts of pork. Traditionally they spend a good portion of their pork allotment on various health care, education, and infrastructure projects for their district.[9] These politicians contend they are meeting needs that would otherwise go unmet and in the process promoting economic development and reducing poverty. But in fact, pork is primarily a political, not a developmental tool (Cruz and Chua 2004). For example, legislators often give large donations to hospitals in their districts to care for those who lack the capacity to pay. However, only a legislator's political supporters are typically eligible for this subsidized care. Patients must present an ID card or a letter signed by the politician identifying them as a political supporter before receiving care (Cruz and Chua 2004).

Politically driven inefficiencies in the Philippines are not confined to the provision of health care. In the area of education, politicians often award scholarships to students in their districts. The primary distribution criterion for these awards is not financial need or academic merit, but rather the political affiliation of the students or their parents.[10] Politicians also spend enormous sums every year from various pork barrel funds to purchase textbooks for schools. Yet there remains a chronic shortage of required textbooks in most Filipino schools—as few as one textbook for eight students. How can this be given the amount spent on textbooks each year? It seems that politicians prefer to spend their funds on supplementary textbooks and education materials rather than on the required texts used in core courses. The reason: the Department of Education has established limits on the price of required textbooks while there is no such ceiling for supplementary textbooks and materials (Chua 2000). Politicians prefer to purchase these supplementary textbooks at greatly inflated prices since the higher margins on these books translate into bigger "commissions" or kickbacks for the purchasing politician. The net result—schools are awash in overpriced education materials they don't need while lacking the basic texts needed to educate students (Chua 2000).

Consider, as a contrast, the spending incentives where the party's reputation is at a premium. In such circumstances the goal of winning a nontrivial number of seats in the legislature provides an incentive for parties and their membership to use policies that appeal to a broader segment of the population. Doing so enables the party to form a positive relationship with a large swath of the voting public. As a result, party-centered systems should be associated with politicians who are relatively more concerned about achieving the efficiency and economies of scale associated with broad public goods provision due to the benefits for the party label.

In addition to the incentives to appeal to broader constituencies, party-centered systems also enjoy stronger party discipline than do candidate-centered systems. First, because the system places a premium on the party's reputation relative to individual candidates', individual party members are less likely to defect from the party's collective goals in pursuit of narrowly targeted goods that do little to benefit the party as a whole. Second, party

[7] Cox and McCubbins (2001) distinguish between two types of pork—morsels (localized public goods allocated on the basis of political calculations) and rents.

[8] For example, in both Italy and India politicians have maintained nonmeritocratic, ineffective bureaucracies despite numerous calls for reform. They do so because ineffective bureaucracies produce many more opportunities for raising necessary campaign funds (Wade 1985), intervening in the bureaucracy on behalf of constituents, and using government jobs to reward supporters (Golden 2003).

[9] In 2001 at least 29 percent of countrywide development funds (a major source of legislative pork) were spent on health, education, or infrastructure projects (authors' calculations from Cruz and Chua 2004).

[10] Similarly, in Brazil Souza reports that only children who had a letter from the relevant politician were allowed to register for school (Souza 1997, 145, quoted in Lyne and Carroll 2006).

leaders in party-centered systems are typically delegated greater authority to protect the party reputation and impose discipline on party backbenchers. Party leaders' control of ballot access and order, campaign financing, and legislative/postlegislative careers enables them to rein in personalism/particularism while promoting policies that benefit the party collectively (Cox and McCubbins 2001; Grossman and Helpman 2005; Nielson 2003).[11]

In summary, we propose that candidate-centered electoral rules will correlate with a systematic undersupply of public goods and that spending on budgetary categories nominally called "public services," such as public spending on education and health, will be associated with corruption and efficiency losses.[12] Note that our argument about the public goods consequences of particularism and the personal vote does not include directional prediction about the amount of spending on public services. Instead, we expect to see no relationship between the personal vote and aggregate levels of spending. Particularistic spending is not a substitute for the level of spending on public services—public services expenditures themselves can be turned to particularistic purposes without having to resort to the politically risky move of slashing spending on public services. In other words, we don't very often expect to see legislators slashing education spending to free up more resources for pork. (If such were the case we should observe a negative relationship between particularism and total education spending.)[13] Instead, as we argued previously, particularism in personal vote systems should generally take the form of policy morselization or commodification *within* existing amounts of spending. Less efficient targeted spending may displace more encompassing forms but total spending on public services need not significantly change. With this logic in mind, our argument has important implications about the effect of democracy in public goods and public service spending. A number of scholars have found a positive relationship between democracy and spending on public services (Lake and Baum 2001; Stasavage 2005). Scholars often find that democracy is correlated with greater education spending and by extension some reason that democracy should also be correlated with better education outcomes (Stasavage 2005). We counter that improved education outcomes will not necessarily follow from a move to democracy and the resulting increase in spending. Specifically, if our argument is correct, we should find that in some democracies strong incentives to cultivate a personal vote will undermine or negate the impact of increased spending on education outcomes.

Research Design

We test these ideas using data from a sample of democratic less developed countries between the years 1980 and 1998. We code a country as being democratic if its Polity score is greater than 0 on a scale of –10 to 10.[14] We test our hypothesis regarding the relationship between incentives to cultivate a personal or party vote and public goods provision by investigating whether those incentives modify the effects of education spending on the level of illiteracy. Our dependent variable is the percent of the population over the age of 15 years that is illiterate.[15] We note that in our sample the illiteracy variable is right-skewed to a significant degree; the vast majority of countries in our sample fall within the range of 5–30% illiteracy, but some countries have values in excess of 60%. To ensure that

[11] Of course there are important variations within party-centered systems. Particularly important is whether party leaders are accountable to voters (see Carey 1998; Coppedge 1994; Hawkins and Nielson 2003).

[12] Note that our argument is not that party vote systems always promote public goods. Our argument is merely that, on average, needed public policies should be undersupplied in candidate-centered systems vis-à-vis party-centered systems. An interesting follow-up to this article would be to examine variation within party-centered systems. It is possible that at extreme levels of party dominance over candidates, party cliques and cartels can emerge which are unresponsive to the demands of voters outside their traditional constituencies, to the detriment of public service provision (see Coppedge, 1994; Crisp and Rey, 2001; Hawkins and Nielson 2003). Unfortunately, the nature of the available data, described below, does not allow us to separate out hyper-centralized party systems from other less extreme party-centered systems. To the extent they exist, the presence of such hyper-centralized cases in our dataset should bias the results *against* our hypothesis.

[13] Nor must particularism take the form of add-ons to government budgets—expenditures over and above what the government would otherwise allocate. (If such add-ons are common we should observe an inflationary effect of particularism on government budgets.)

[14] Since our hypotheses relate specifically to how voting rules and institutions shape the incentives of legislators, we exclude dictatorships from the sample. Note that Polity scores are not given for three countries in our dataset, Bahamas, Belize, and Cape Verde. However, an alternative classification of democracies/nondemocracies (Golder 2005; Przeworski et al. 2000) lists each as democratic over most of the period for which we have data: Bahamas (1973–2000), Belize (1981–2000), Cape Verde (1991–2000). For the full period, Freedom House's Political Freedom score has an average score of "partly free" for Cape Verde.

[15] Our use of the illiteracy variable explains why we limit our sample only to developing democracies. High-income countries have largely eliminated the illiteracy problem and since illiteracy tends to move only slowly over time and with a trend toward decreasing illiteracy, these countries do not appear to be comparable to countries where large swaths of the population are still illiterate. Indeed, the United Nations Educational, Scientific, and Cultural Organization (UNESCO), the source of our illiteracy data (via the World Bank's World Development Indicators database), does not even collect such data for high-income countries on the basis of estimates that these countries have literacy rates well in excess of 95%.

these extreme observations do not drive our results, we use logged values of illiteracy rates.

Independent Variables

We rely on the variables and coding scheme developed by Carey and Shugart to operationalize the extent to which the political setting places a premium on cultivating a personal or party vote. They offer three variables, *Ballot, Pool,* and *Vote,* each of which is coded 0, 1, or 2, where higher values denote greater incentives to cultivate a personal vote. *Ballot* measures "the degree of control party leaders exercise over ballot rank in electoral list systems" (1995, 418). A score of 0 indicates a situation in which the leader presents a ballot that voters cannot change, 1 represents systems in which leaders present party lists that voters can change, and 2 is assigned if leaders do not control the ballot at all. *Pool* codes whether votes for one candidate affect the number of seats won in that district by the party as a whole. A score of 0 means that "a vote for any candidate of a given party is counted first as a vote for the whole party list for the purpose of determining how many seats are to be allocated to the list" (421), 1 indicates that votes are pooled across candidates or factions in the same party, and 2 indicates that no pooling occurs at all. Lastly, *Vote* codes the rules articulating the nature of the voter's choice. A score of 0 means that voters cast one vote and must cast it for a party, 1 indicates that voters have multiple votes and can cast them for multiple candidates, and 2 indicates a system where voters have a single vote and must choose from among several candidates.

Wallack et al. (2003) collect data based on Carey and Shugart's scheme for 158 countries, and we use these data for our analyses.[16] Wallack et al. make a couple of changes from the original Carey and Shugart scheme, however. First, rather than coding *Ballot, Pool*, and *Vote* as strictly trichotomous variables, they code them as the "weighted averages . . . for each group of legislators that is elected under a different set of rules" (2002, 13). They also create a variable, *parindex,* that is the average of *Ballot, Pool*, and *Vote.*[17] We will use each of the indicators in the analysis below.

[16]Wallack et al. collect these data for the years 1975–2001. Our database spans the range 1980–1998, due to the coverage of our education spending variable.

[17]Wallack et al. disagree with Carey and Shugart regarding how to code single-member districts. Carey and Shugart propose that SMDs are essentially closed-list systems in particularly small districts and they suggest coding *Ballot* as 0 in the presence of SMD systems. They also propose coding *Pool* as equal to zero in the case of SMD systems. They argue that in the presence of a "list" of one candidate, votes are pooled across the entire list. Wallack and her coauthors disagree with both suggestions. Instead, they code SMD systems 0 for *Ballot* only where the majority of districts are multimember, closed-list, and proportional. Otherwise, they assign SMD systems a score of 1 in the *Ballot* variable. With respect to *Pool* they code "single member districts as two on the *Pool* scale because they do not receive additional electoral support if other candidates from their party are successful in other districts" (7).

Estimation

To test our hypothesis that personal vote systems are associated with efficiency losses with respect to public goods provision, we estimate the following equation:

$$Illiteracy_i = a + \beta_1 Spending_i + \beta_2 Institution_i + \beta_3 Spending_i * Institution_i + \beta X_i + \varepsilon_i, \quad (1)$$

where subscript *i* indicates a particular country, *Institution* indicates either *parindex* or one of its constituent parts, *Ballot*, *Pool*, or *Vote*. *Spending* is public education spending (% GDP), and *X* refers to a set of controls including regional indicators, GDP per capita, and the percent of the population living in rural areas. GDP per capita controls for the observation that wealth gives governments more revenues they can use to fund programs like education initiatives. It also captures the degree to which the average citizen has access to some private source of education supply. The regional indicators account for very clear regional trends with respect to illiteracy such as the fact that many governments in East and Southeast Asia, learning from each other, dramatically reduced illiteracy since the 1960s (World Bank 1993) and the communist governments of Eastern Europe and the Soviet Union went to great lengths to reduce illiteracy to teach and spread the communist ideology (von Kopp 1992, 103–4). We add the regional dummies to control for these and other regional trends that will influence the level of literacy in a particular country.[18] To gain additional leverage on country-level heterogeneity, we also control for the percent of the population living in rural areas.[19] We expect that illiteracy reduction is relatively easier in urban populations, where

[18]Including the regional indicators is not an innocuous decision as our results are not robust to their exclusion. That said, we have reason to believe that there are variables that occur at the regional level that are correlated both with spending and with illiteracy. If left unaccounted for we risk running into problems of omitted variable bias. Ideally, we would like measures of those omitted variables directly, but we do not have such data either for a large enough sample of countries or a long enough span of time. Also, we are aware of our small sample size and the problems we would face should we include many more variables in the model. While a second-best option, we think the regional controls are an effective and parsimonious way of dealing with this issue.

[19]We have also estimated models controlling for the country's age-dependency ratio, whether a country is parliamentary or presidential, and Transparency International's Corruption Perception Index. Our substantive results hold when controlling for these variables. We do not show them here to preserve space. Additionally, we do not show the corruption results because of that variable's lack of coverage temporally and cross-nationally.

a centrally located school in an urban area can benefit a large share of the population.

We estimate equation 1 on a cross-section of just under 40 democratic countries. While the use of a time series cross-section (TSCS) sample of countries is increasingly common in studies of comparative political economy, and we will make use of a TSCS sample in a robustness check, we believe a cross-sectional model is the most appropriate choice given the nature of the data. The independent variables of interest for us are institutions that are time-invariant for most countries and change only rarely for others. The TSCS format would likely call for the inclusion of country effects (in fact, Wald tests indicate we should include country dummies), which will be highly correlated with the time-invariant variables and would therefore wash out any effects of the institutions that are the focus of our study. A cross-sectional design is more appropriate. We estimate all cross-section models with White's heteroskedastic-consistent standard errors.

Expectations

Our theory leads us to expect the following: β_1 in equation 1 should be negative, telling us that education spending *reduces* illiteracy when either *parindex, Ballot, Pool,* or *Vote* equal 0 (i.e., when those variables identify a party-centered political system). However, β_3 should be positive, indicating that education spending is *less effective at reducing illiteracy* in personal vote systems. That is, assuming a negative coefficient on β_1, a positive coefficient on β_3 is consistent with our proposition that personal vote systems are associated with efficiency losses with respect to the provision of public goods and services. Governments may increase spending, but rather than aiming to reduce illiteracy and attaining the growth consequences thereof, leaders opt to spend on pork. Additionally, in such systems corruption may decrease further the efficacy of education spending for illiteracy reduction. Table 1 lists the sample of included countries with their values on the key variables. It also presents the summary statistics.

Results

Table 2 presents our main results in a series of seven models. The first estimates the effect of education spending on the illiteracy variable without including the interaction term between education spending and any of our institutional variables. Model 2 estimates equation 1 including *parindex* as the institutional variable. Model 3 replaces the original, continuous coding of *parindex* with a dichotomous measure, while Model 4 uses a trichotomous measure. We recode the variable as there are only a few scattered observations that do not take values of 0, 1, or 2 in the original continuous coding. This is relevant since we want to know the range of the modifying variable where the marginal effect of education spending is statistically significantly different from zero and where it is not. For those values of the modifying variable where there are only one or two observations, it is impossible to tell whether failure to reject the null hypothesis occurs because it is an accurate estimate of the truth (i.e., there is in *actuality* too much variation at the level of the institution in question to reject the null hypothesis) or because there are simply too few cases from which to draw information. By recoding the variables, we avoid this issue. We use trichotomous codings of *Ballot, Pool,* and *Vote* respectively for the same reason. Those results are displayed in Models 5–7 in Table 2.[20]

We start our discussion with Model 1. Here we find that most regions of the world have lower illiteracy rates than in sub-Saharan African countries and that GDP is negatively correlated with illiteracy. Notably, we find no statistically significant effect of education spending on the illiteracy rate.[21]

Model 2 tells a more nuanced story. Here, the coefficient on education spending is negative, which tells us that when *parindex* equals 0, education spending reduces illiteracy. This is as expected. Substantively, the sizes of the coefficients are notable. These results say that a one-unit increase in education spending (e.g., moving from 1% of GDP to 2%) corresponds with an 8.5% decrease in the log of illiteracy. Since illiteracy itself is in percentage form, we need to go further to get a sense of what this coefficient means. In our sample, the mean illiteracy score is 19.6%. Therefore, a one-unit increase in education spending, when *parindex* equals 0, would decrease

[20] In the dichotomous *parindex* coding scheme, values above 1 are coded as 1 and values less than or equal to 1 are coded as 0. For all of the trichotomous recodings, values of the original continuous variable below 0.5 are coded as 0, values between .5 and 1.5 are coded as 1, and we give values above 1.5 a score of 2. The distribution of the dichotomous *parindex* measure is 50% are 0 and 50% are 1. For the trichotomous *parindex* variable, 32% of cases are scored 0, 39% are scored 1, and 29% have a score of 2. The distribution of the *Ballot*, *Pool*, and *Vote* are as follows. For *Ballot:* 34% are scored 0, 58% are 1, and 8% are 2. For *Pool:* around 47% are 0, around 18% of cases are 1, and 34% are 2. For *Vote:* 34% are 0, 29% are 1, and 37% are 2.

[21] At first glance the fact that East Asian states have significantly lower illiteracy rates may seem paradoxical given that these same states have relatively high *parindex* scores, indicating strong incentives to cultivate a personal vote. Three things are worth noting in this regard. First, recall that the reference category is Africa and so Table 2 reveals only that East Asian states in our sample are more literate than the African states. Second, much of the focus on decreasing illiteracy in East Asia occurred under nondemocratic governments and hence these lie outside the theory and our sample. Nevertheless, we need to control for these earlier policy decisions. Third, our theory does not predict that personalized systems cannot reduce illiteracy, only that they will be less efficient at doing so compared to less personalized systems.

TABLE 1 List of Countries in Cross-Sectional Database and Summary Statistics

Country	Polity Score	Illit. Rate	Ed. Spend	Parindex	Ballot	Pool	Vote	DFFITS	Influence Weight
Armenia	3.5	2	2	1	.7	1.4	.94	.24	1
Bahamas	.	6.2	3.9	1.7	1	2	2	.15	1
Belize	.	11	5	1.7	1	2	2	−.5	.69
Bolivia	.63	27	3	.28	.17	.33	.33	.033	1
Botswana	7.5	37	6.3	2	2	2	2	−.58	.58
Brazil	1.5	22	3.8	.67	1	0	1	.46	.74
Cape Verde Is.	.	40	3.5	.12	.18	0	.18	.17	1
Chile	2.2	7.5	3.6	0	0	0	0	−.73	.47
Colombia	6.1	14	2.6	.67	1	1	0	.51	.66
Costa Rica	10	7.5	4.7	0	0	0	0	−.67	.51
Dominican Rep.	.71	24	1.9	0	0	0	0	.17	1
Ecuador	3.6	16	3.3	0	0	0	0	−.32	1
El Salvador	.78	31	2.5	0	0	0	0	.47	.72
Fiji	6.4	15	5.1	1.7	2	2	1	.1	1
Gambia	4.8	78	4.1	1.7	1	2	2	.54	.63
Honduras	1.7	35	3.6	0	0	0	0	.39	.88
India	8.6	55	3	1.7	1	2	2	.7	.49
Israel	9.4	13	6.7	0	0	0	0	.9	.38
Jamaica	9.8	21	4.9	1.7	1	2	2	.18	1
Latvia	8	.21	5.8	.67	1	0	1	−.38	.89
Lebanon	1.6	24	2.2	.67	1	0	1	−.9	.38
Lithuania	10	.54	5.5	.67	.5	1	.5	.033	1
Malaysia	5.3	25	5.4	1.7	1	2	2	1.6	.22
Mauritius	9.6	23	3.9	.67	1	0	1	−.35	.96
Namibia	6	22	8.6	0	0	0	0	.77	.44
Pakistan	1.1	68	2.4	1.7	1	2	2	1.5	.23
Panama	.43	13	4.7	1	1	0	2	−.071	1
Peru	2.1	18	3.1	0	0	0	0	.097	1
Philippines	2	9.7	2.4	1.4	.93	1.9	1.3	−.67	.51
Russia	4.8	.58	3.6	1.3	1	2	1	.35	.98
Slovenia	10	.39	5.5	1	1	.044	2	.65	.52
South Africa	4.9	22	5.9	1.3	.83	1.3	1.7	−.0031	1
Sri Lanka	6.3	13	3	1.2	.93	.92	1.9	−2.7	.13
Thailand	.02	11	3.6	1.5	1.5	2	1	−1	.33
Trinidad-Tobago	8.6	10	4.3	1.7	1	2	2	.000079	1
Ukraine	6.5	.47	6.2	.83	.5	1	1	−.5	.68
Uruguay	4.5	4.4	2.8	1.3	1	1	2	−1.1	.3
Venezuela	6.5	14	4.5	0	0	0	0	.34	1
Mean	5.0	19.5	4.1	.88	.72	.89	1.0		
Standard Deviation	3.3	17.8	1.5	.68	.55	.90	.83		
Minimum	.01	.2	0	0	0	0	0		
Maximum	10	78.4	2	2	2	2	2		

illiteracy in a country with the mean value from about 19.6% to about 18%.[22]

Notice, however, that the positive coefficient on the interaction term suggests that the marginal effect of

[22] Similarly, for the country with the median value of illiteracy in our sample, an increase in education spending when *parindex* equals 0 will decrease illiteracy from 14.6% to just over 13% of the population.

TABLE 2 Estimated Effects of Education Spending and Incentives to Cultivate a Personal Vote on Illiteracy Rates

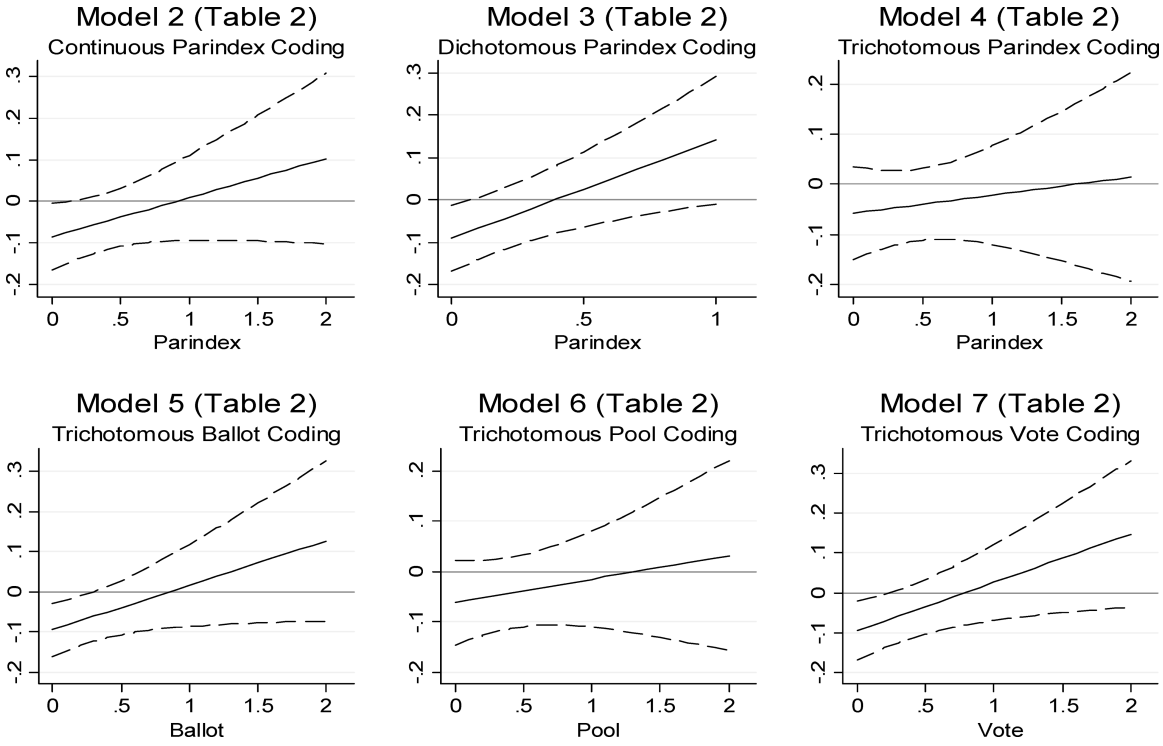
Parindex Coding	–	*Continuous*	*Dichotomous*	*Trichotomous*	–	–	–
	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**	**Model 7**
Ed. Spending	**−0.030**	**−0.085***	**−0.090***	**−0.057**	**−0.094****	**−0.062**	**−0.096****
	(0.045)	**(0.048)**	**(0.047)**	**(0.056)**	**(0.040)**	**(0.051)**	**(0.045)**
Parindex		**−0.433**	**−1.078****	**−0.110**			
		(0.364)	**(0.460)**	**(0.381)**			
Ed. Spend ∗ Parindex		**0.094**	**0.232****	**0.036**			
		(0.071)	**(0.098)**	**(0.077)**			
Ballot					**−0.568**		
					(0.376)		
Ed. Spend ∗ Ballot					**0.111**		
					(0.065)		
Pool						**−0.180**	
						(0.331)	
Ed. Spend ∗ Pool						**0.047**	
						(0.067)	
Vote							**−0.574***
							(0.294)
Ed. Spend ∗ Vote							**0.122***
							(0.063)
GDP	−0.552***	−0.550***	−0.644***	−0.542***	−0.526***	−0.524***	−0.597***
	(0.147)	(0.156)	(0.159)	(0.151)	(0.156)	(0.159)	(0.157)
Rural Pop.	0.008	0.006	0.002	0.007	0.007	0.008	0.003
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.007)
L.America/Carib	−0.645***	−0.698***	−0.753***	−0.652***	−0.706***	−0.654***	−0.738***
	(0.196)	(0.186)	(0.186)	(0.203)	(0.186)	(0.199)	(0.184)
Post-Soviet	−2.549***	−2.581***	−2.518***	−2.565***	−2.556***	−2.538***	−2.654***
	(0.236)	(0.225)	(0.215)	(0.242)	(0.217)	(0.242)	(0.221)
South Asia	−0.629	−0.430	−0.303	−0.594	−0.480	−0.536	−0.262
	(0.437)	(0.559)	(0.482)	(0.568)	(0.476)	(0.585)	(0.509)
East Asia	−0.973***	−0.929***	−0.849***	−1.015***	−0.879***	−0.976***	−0.939***
	(0.307)	(0.286)	(0.250)	(0.297)	(0.310)	(0.314)	(0.263)
Mid. East	−0.047	−0.048	−0.209	0.002	0.014	−0.037	−0.044
	(0.324)	(0.316)	(0.369)	(0.345)	(0.288)	(0.362)	(0.280)
Constant	7.694***	8.038***	9.039***	7.695***	7.877***	7.583***	8.616***
	(1.484)	(1.531)	(1.625)	(1.473)	(1.574)	(1.538)	(1.604)
R-squared	0.869	0.867	0.881	0.862	0.869	0.862	0.875
N	38	38	38	38	38	38	38

*p < 0.1, **p < 0.05, ***p < 0.01; Standard errors in parentheses. Sub-Saharan Africa is the excluded regional category.

increasing education spending on illiteracy reduction is *weaker* where strong incentives to cultivate a personal vote are present. That is, in personal vote systems, education spending *reduces illiteracy less* than in party vote systems. Kam and Franzese (2007) and Brambor, Clark, and Golder (2005) recommend that we plot the marginal effect of education spending on illiteracy over the range of *parindex* to get a better sense of the processes at work here as the significance tests we report are uninterpretable given the interaction term.[23] Figure 1 displays six graphs.

[23]We thank Brambor, Clark, and Golder for granting us access to their STATA .do file, which we use to create all the marginal effect graphs in this article.

FIGURE 1 The Marginal Effect of Education Spending on Illiteracy as Incentives to Cultivate a Personal Vote Increase (Using Continuous, Dichotomous, and Trichotomous *Parindex* Measures and Trichotomous Measures of *Ballot*, *Pool*, and *Vote*)



In each, the solid line represents the marginal effect of education spending, while the dashed lines are the 90% confidence intervals around that marginal effect. The top left graph presents results from Model 2 in Table 2, which uses the original continuous coding of *parindex*. The center graph in the same row uses the dichotomous measure and the last graph uses the trichotomous measure. The bottom row of graphs comes from Models 5–7 from Table 2.

Starting with the first graph in the top row, we see results that support our hypothesis. The marginal effect of education spending on illiteracy is significantly negative only when *parindex* indicates the lowest incentives to cultivate a personal vote and the highest incentives to campaign on the party's reputation. As incentives to cultivate a personal vote increase, the confidence interval around the marginal effect are on either side of the 0 line, and we fail to reject the null hypothesis that the marginal effect of education spending on illiteracy is 0. Notice also the positive slope to the marginal effect line. These results are consistent with our argument that when incentives to cultivate a personal vote are high, legislators place a small premium on the economies of scale associated with public goods and prefer instead to allocate budgetary resources in a form they can easily distribute and for which they can claim credit when voters go to the polls. The middle graph in the figure, using the dichotomous measure, supports this conclusion as well. When we use the trichotomous measure, however, the results are somewhat less supportive.[24] Although a similar trend exists in that the marginal effect of education spending tends toward zero with increasing particularism, we find no value of *parindex* where the effect of education spending is a statistically significant predictor of illiteracy rates.

The bottom row of graphs investigates our hypothesis further by using the trichotomous codings of the three constituent variables.[25] Once again we observe that education spending decreases illiteracy only when the institutional variable in question indicates low levels of political particularism and high incentives for candidates to cultivate the party vote. The lone exception to this trend is

[24]Recall that in the dichotomous and trichotomous codings, we have data only for the integer values of the modifying variable.

[25]Using the original continuous codings does not affect the substantive results we report here.

the model using *Pool*. In that case, there is no value of the modifying institution where education spending is statistically significant.

Robustness Checks

We turn now to an assessment of the stability of our estimates. There is some evidence of influential outliers and in a small sample like ours, these can be especially damaging. Using Model 2 from Table 2, we create partial regression plots with respect to education spending, *parindex,* and their interaction respectively and show them in the top graphs in Figure 2. We have labeled the countries that look to be influential. Four cases, Malaysia, Pakistan, Sri Lanka, and Uruguay, have either DFFITS values above the threshold or Cook's Distance scores above that threshold.[26] We examine the effects of these observations by estimating a series of models, inserting dummies for each of these observations. The results are displayed in Table 3. Our conclusions do not change with the inclusion of these dummies. In each, education spending is negative but the interaction between spending and *parindex* is positive.[27] That said, including dummy variables is not an altogether appealing strategy for dealing with outliers as there is no theoretical reason to add more variables to the model. Nor is dropping these observations appealing. As Granato, Inglehart, and Leblang note, in a small sample like ours, dropping cases discards useful information and perhaps more importantly, "one imagines a situation where after deleting observations and re-estimating the model other influential cases are identified and are removed. This process continues until few interesting observations remain" (1996, 623). We thus conduct a robustness check described in Welsch (1980) and used by Granato, Inglehart, and Leblang to further analyze the stability of our results.[28] Welsch suggests a bounded-influence estimator conducted as follows. First, estimate OLS and calculate the DFFITS for all observations. Then, estimate the model of interest using weighted least squares with weights calculated as follows:

(a) Weight = 1 if $|\text{DFFITS}| \leq .34$.
(b) Weight = $(.34/|\text{DFFITS}|)$ if $|\text{DFFITS}| > .34$.

Welsch suggests the value .34 as it provides for approximately 95% asymptotic efficiency. We conduct this procedure and show the DFFITS and weights in the last columns of Table 1. The results of bounded-influence estimation are displayed in Table 3. The corresponding marginal effect graph is shown in the bottom left of Figure 2. The substantive conclusions do not change when we use this procedure. The marginal effect of education spending is negative when *parindex* equals 0, but the positive coefficient on the interaction term suggests that this effect of education spending is reduced in personal vote systems.

To this point, we have found what we think are interesting and novel results. The extent to which electoral rules and institutions provide incentives for legislators to cultivate a personal reputation has an important modifying effect on the efficacy of education spending at reducing illiteracy. When such incentives are low and incentives to cultivate the party's reputation are high, education spending has a statistically significant downward pressure on illiteracy rates. By contrast, where the incentives to cultivate a personal vote increase, we cannot reject the null hypothesis of no effect of increasing education spending on illiteracy. These results hold for our summary indicator of personal versus party-centered systems and for the constituent parts of that summary indicator. The conclusions do not appear to be due to influential outliers.

Furthermore, the effect of the personal vote seems to work entirely through how personalism affects the *allocation* of education spending, rather than on the *level* of education spending. In a set of four models, we tested whether the measures of particularism predict the level of education spending and we found none of the indicators had a statistically significant effect. We also regressed illiteracy rates on both education spending and *parindex* additively but without their interaction. The p-value on the index's coefficient was 0.98.[29]

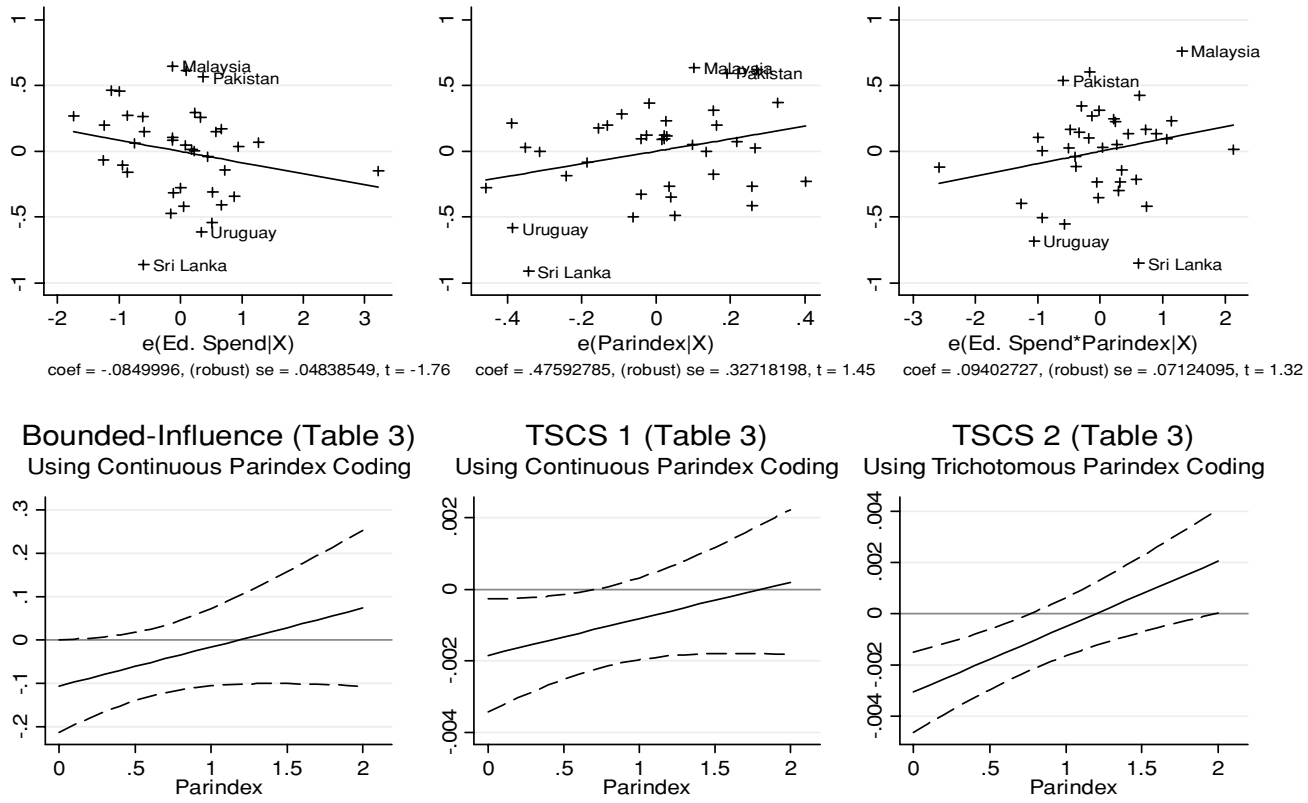
[26] We follow Granato, Inglehart, and Leblang (1996) and consider a case influential in a small sample according to its Cook's Distance if $C_i > 4/n$. Our cutoff point for the DFFITS scores comes from Bollen and Jackman (1985), who suggest $2 * (k/n)^{(1/2)}$.

[27] The marginal effect figures closely resemble those already displayed and so are not presented here. The only model that does not conform to what we have shown already is the model controlling for Uruguay. In that model the marginal effect of education spending just barely becomes insignificant at the 90% level when *parindex* equals 0.

[28] We thank an anonymous reviewer for this suggestion.

[29] When we exclude education spending, to account for the possibility that the causal chain from particularism to illiteracy runs through the level of education spending, the index's p-value is 0.93. Finally, we recognize the possibility that the results we report in Table 2 can be interpreted differently. For example, the results in Model 2 in Table 2 could also be interpreted as suggesting that increasing incentives to cultivate a personal vote *reduces* illiteracy at low levels of education spending. We explored this possibility by plotting the marginal effect of *parindex* on illiteracy throughout the range of education spending and found no value of education spending where we could reject the null hypothesis of no effect of *parindex* on illiteracy. The same holds for *Ballot* and *Pool*. The lone exception to the trend is *Vote*, where at low values of education spending there is a reduction in illiteracy as *Vote* increases. The

FIGURE 2 Top Row: Added-Variable Plots Form the Baseline Models for Variables Education Spending, *Parindex*, and Their Interaction. Bottom Row: Results from Bounded-Influence Estimation and Models TSCS 1 and TSCS 2 (Both from Table 3)



TSCS Results

As a final sensitivity analysis, we test our hypothesis using a TSCS sample of countries. Even as our cross-sectional results are supportive (and, we believe, the more appropriate model given the structure of the data), a TSCS sample can be useful for several reasons. First, a TSCS sample allows us to control for likely endogeneity better than we can in our small cross-section sample. In our equations, we have included GDP per capita on the right-hand side. However, in the theory, we noted the effects of literacy and human capital formation for economic development. There is, then, reason to believe that illiteracy and GDP per capita have an endogenous relationship. Our cross-section sample is too small, however, to account for that relationship reliably. With a TSCS model, we can lag our right-hand-side variables one period, which grants us some relief from the endogeneity concern.

areas where this occurs represent only about 25% of our sample, however. (We thank Sven Wilson for raising this issue.) The results of all of these additional models are available from the authors upon request.

A second reason we turn to a TSCS sample is so that we can have a more precise sample of democratic countries. A country is included in our cross-section sample if its average Polity 2 score is greater than 0 for the entire period over which we have data. Such a coding scheme may be problematic in that it will exclude countries that have had democratic episodes that were shorter than their episode of dictatorship as well as those countries whose democratic episodes were only moderately democratic (say, a Polity score of 6), but whose autocratic episode were more extreme (a value of −10). A TSCS sample can make these issues less pronounced since a country can be democratic for some shorter period and still be included in the sample.

That said, some estimation issues warrant discussion before discussing the TSCS results. First, because literacy rates move only very slowly over time, we need to be careful in how we model the temporal dependence. Tests from a TSCS database using yearly data reveal that illiteracy exhibits a unit-root when the data are at that level of detail. To avoid the concerns unit-roots cause for statistical inference, we estimate the relevant equations using panels of five years each, again between the years 1980 and 1998,

TABLE 3 Robustness Checks: Baseline Model Estimated with Country Dummies for Potential Outliers and TSCS Models Using Five-Year Panels and Three-Step Procedure for Estimating Panel Data with Slowly Moving or Invariant Independent Variables. (Coefficients in *bold italics* in the TSCS Models Denote That We Treat the Corresponding Variable as Slowly Moving/Time-Invariant.)

	Malaysia	Pakistan	Sri Lanka	Uruguay	Bounded-Influence	TSCS 1	TSCS 2
Ed. Spending	−0.081*	−0.097**	−0.115**	−0.076	−0.105	***−.002****	***−.003*****
	(0.046)	(0.046)	(0.043)	(0.050)	(0.064)	***(0.001)***	***(0.001)***
Parindex	−0.208	−0.540	−0.625*	−0.243	−0.412	***.003***	***−.003***
	(0.332)	(0.344)	(0.336)	(0.357)	(0.319)	***(0.004)***	***(0.004)***
Ed. Spend ∗ Parindex	0.045	0.115	0.128*	0.061	0.089	***.001***	***.002*****
	(0.062)	(0.068)	(0.067)	(0.070)	(0.071)	***(0.001)***	***(0.001)***
GDP	−0.524***	−0.513***	−0.451***	−0.564***	−0.517***	−.008***	−.011***
	(0.146)	(0.146)	(0.138)	(0.160)	(0.149)	(0.002)	(0.002)
Rural Pop.	0.009	0.007	0.008	0.002	0.005	***.001*****	***.001*****
	(0.007)	(0.007)	(0.007)	(0.008)	(0.006)	***(0.000)***	***(0.000)***
L.America/Carib	−0.669***	−0.680***	−0.689***	−0.736***	−0.679***	***−.024*****	***.026*****
	(0.184)	(0.191)	(0.195)	(0.198)	(0.181)	***(0.003)***	***(0.003)***
Post-Soviet	−2.530***	−2.558***	−2.561***	−2.703***	−2.583***	***−.058*****	***−.052*****
	(0.215)	(0.222)	(0.218)	(0.241)	(0.219)	***(0.006)***	***(0.006)***
South Asia	−0.588	−0.665	0.201	−0.518	−0.367	***.031*****	***.031*****
	(0.546)	(0.586)	(0.325)	(0.546)	(0.367)	***(0.004)***	***(0.005)***
East Asia	−1.226***	−0.907***	−0.892***	−0.983***	−0.916***	***−.124*****	***−.122*****
	(0.257)	(0.286)	(0.292)	(0.297)	(0.225)	***(0.004)***	***(0.004)***
Mid. East	−0.000	−0.009	−0.013	−0.214	−0.082	***−.125*****	***−.121*****
	(0.330)	(0.310)	(0.306)	(0.330)	(0.399)	***(0.005)***	***(0.005)***
Malaysia	0.991***						
	(0.269)						
Pakistan		0.934					
		(0.548)					
Sri Lanka			−1.464***				
			(0.171)				
Uruguay				−0.813***			
				(0.223)			
LDV						.951***	.955***
						(0.002)	.002
Constant	7.644***	7.728***	7.269***	8.317***	7.875***	.067**	.088***
	(1.436)	(1.471)	(1.424)	(1.608)	(1.438)	(0.026)	.026
R-squared	0.882	0.879	0.905	0.877	0.923	–	–
Obs	38	38	38	38	38	96	96
Countries	–	–	–	–	–	50	50

*p < 0.1, **p < 0.05, ***p < 0.01; Standard errors in parentheses; Sub-Saharan Africa is the excluded regional category.

where the observation for each country *i*, in panel *p*, is the average value of that variable for every year in that panel for which we have data.

Another concern is how we account for unit-heterogeneity. Wald tests indicate unit-specific indicators are needed in the model to avoid omitted variable bias, but the inclusion of country dummies will minimize the explanatory power of our key independent variables—many of which are nearly time-invariant (i.e., *parindex)* or temporally sluggish (education spending as a percent

of GDP and the percent of the population living in rural areas). One alternative specification is to use a random effects model, but the strict exogeneity assumptions that go along with that model are likely to be violated with our data.

Our approach is to follow the strategy outlined in Plümper and Troeger (2004). Their suggestion is to use a three-step approach where the first stage estimates the relationship between the dependent variable and the fixed effects; stage 2 uses the vector of fixed effects coefficients reported in stage 1 as the dependent variables and includes only the slowly moving and time-invariant independent variables on the right-hand side. The final stage places the original dependent variable on the left-hand side of the equation and includes all independent variables of interest and also the stage 2 residuals on the right. Stage 3, then, includes all of the variables of theoretical interest to us while also including that part of the country-specific effects that are uncorrelated with the slowly moving and time-invariant independent variables. With this procedure, we can estimate a model with invariant and slowly moving right-hand-side variables, include the necessary country effects, and are not subject to the strict exogeneity assumptions included in the random effects model. Plümper and Troeger's Monte Carlo simulations show that this approach has better properties than a random effects model, pooled OLS, or the Hausman-Taylor instrumental variable approach. The authors write:

> This procedure adopts the robustness of the fixed effects model and allows for correlation between explanatory variables and the unobserved individual effects. The effects of time varying factors are consistently analyzed and remain unbiased. Since fixed effects are robust with respect to potential correlation between right hand side variables and the individual specific effects the exogeneity of explanatory variables is not required. (9)[30]

Using this approach, we use the same controls as in the baseline cross-section models. However, education spending, *parindex,* GDP per capita, and rural population are entered in 1-period lags to indicate our expectation that the effects of these variables take time to manifest in observable improvements in literacy. Our sample includes countries that have an average Polity score greater than 0 for two consecutive five-year panels. That is, to enter into the sample, the country must be democratic in the present period and in the previous panel when the education-related resources were allocated.[31]

The results are displayed in the last two columns of Table 3. In these models, the coefficients in bold italics denote the variables we treat as slowly moving/time invariant. TSCS 1 uses the continuous measure of *parindex* and TSCS 2 uses the trichotomous coding.[32] Turning to the estimated coefficients in TSCS 1, we see that the lagged dependent variable is somewhat close to 1 indicating that we should still be wary of a unit-root, but the t-statistic is large, giving us confidence that we are safe from such concerns. The control variables also perform as expected with illiteracy decreasing with wealth, decreasing with urbanization, and most regions of the world performing better on illiteracy statistics than sub-Saharan Africa. Here we see further evidence in support of our hypothesis. In model TSCS 1, education spending reduces illiteracy when *parindex* equals 0, but as before, this effect of education spending is less negative with increases in the index.[33] TSCS 2, using the trichotomous *parindex*, tells the same story.

The last two graphs in the bottom row of Figure 2 use the results from TSCS 1 and TSCS 2 to graph the marginal effect of education spending on illiteracy. As before, the efficacy of education spending on illiteracy reduction is contingent upon institutions that shape which reputation receives a political premium. The marginal effect of education spending is negative and significant in party-centered systems, but trends toward zero with increases in *parindex* and becomes statistically insignificant.

Conclusion

Our aim in this article is to better understand the ways in which electoral and party systems affect public goods

[30] For other recent work using this three-stage approach, see Blaydes (2006). We thank Thomas Plümper and Vera E. Troeger for allowing us access to their STATA .ado file, with which we conducted this procedure.

[31] We have a total of four panels (1980–1984; 1985–1989; 1990–1994; and 1995–1998), but use of 1-period lags drops the first panel from the sample.

[32] We have also estimated models that include time effects. These do not change the substantive interpretations here. They are available upon request.

[33] Admittedly, the coefficients in these models are smaller than what we observed with the cross-section results. In all likelihood this is due to the inclusion of the lag of the dependent variable, which as Achen (2000) points out, can minimize the explanatory power of time-trended independent variables and bias those coefficients toward zero.

provision. To understand this relationship we need to look not only at how incentives to cultivate a personal vote shape spending decisions, but also whether/under what conditions those decisions affect the policy outcomes we ultimately care about. In our attempt to examine the effects of spending on public goods provision we have focused on education spending and illiteracy. Our core findings can be summarized as follows. First, we find that incentives to cultivate a personal vote dampen the marginal effect of increased education spending on illiteracy and at their highest levels particularistic incentives completely undermine the positive effects of increased education spending on literacy. Second, there appears to be no significant relationship between our measure of particularistic incentives and the level of education spending. In other words, the effects of the personal vote on illiteracy operate through the efficiency of education spending rather than on the level. While democracies may spend more on education that nondemocracies (as several scholars have found), the effect of greater spending on education outcomes depends on the type of democratic institutions in place.

There are several intriguing questions worth exploring in future work. To begin with, do the patterns we observe in education policy appear in other areas as well, for example health or infrastructure? Could we more directly observe actual forms of spending that would allow us to track the differences in the form of spending in personal and party vote systems? At present we can say that the spending and illiteracy results are consistent with our theory but we do not directly observe the way in which education budgets are allocated. To do so we would need detailed data on allocations within the education budget. We are currently exploring the feasibility of collecting such data. It would be worth exploring further the differences in the way each component of *parindex* affects illiteracy. Finally, an interesting question is whether you can have too much of a good thing—whether extremely high levels of party centeredness undermine the incentives to provide needed public policies. As discussed earlier we were unable to explore this question here given the nature of the available data, but we hope to return to the question in future work.

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