

# Opportunistic Political Cycles in Social Spending: An Examination of Transition and Consolidated Democracies in Latin America

Lorena G. Barberia  
Fundação Getúlio Vargas  
[lorena.barberia@gmail.com](mailto:lorena.barberia@gmail.com)

George Avelino  
Fundação Getúlio Vargas  
[George.Avelino@fgv.br](mailto:George.Avelino@fgv.br)

## **Abstract**

In this paper, we show that education, health and social security expenditures did not increase during elections. Based on a panel of fifteen Latin American democracies from 1973 to 2000, we show that there are important increases in social spending in the inaugural year of a new presidential administration. We argue that social policy is used by Latin American democracies as an instrument to reward voters after winners enter office and not as a tool to manipulate outcomes before elections as commonly argued in the literature.

**Keywords:** Political budget cycles; Elections; Social spending; Latin America.

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## 1. Introduction

The literature on political budget cycles has produced important insights on the extent to which politicians attempt to manipulate government fiscal policies to influence electoral outcomes. In particular, some of the strongest evidence produced to date suggests that electoral cycles are particularly marked in the case of fiscal policy (Drazen 2000; Franzese 2002a). Oftentimes, it is also asserted that developing countries and new democracies are most susceptible to fiscal manipulation (Block 2002; Akhmedov and Zhuravskaya 2004; Brender and Drazen 2005, 2007). The experiences of recently re-established Latin American democracies in a period marked by episodes of heightened macroeconomic volatility followed by the adoption of painful stabilization measures provides fertile ground for testing political budget cycle theories and recently formulated arguments on the acuteness of these patterns for young fragile democratic regimes in developing countries.

An important insight of the political budget cycle (PBC) theoretical framework is that incumbents will prioritize consumption spending by increasing transfers and targeting projects with high immediate visibility (Rogoff 1990). Motivated by the premise that politicians will target expenditures to influence election outcomes, research efforts have been directed at examining if there are variations in the composition of government expenditures depending on whether they are also more perceptible to voters and if the credit for such policies is more easily attributed to individual politicians or levels of government.<sup>1</sup> The electoral-economic cycle affects of transfer payments, such as social security and social assistance, are particularly well-suited for testing PBC theory given their magnitude both in terms of the number of citizens involved and the absolute and relative volume of payments relative to real disposable personal and national income.

Grounded in theoretically rigorous definitions of democracy, transitions and elections, in this paper we test if social spending was used to manipulate election

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<sup>1</sup> Another important and well-studied pattern of politically-motivated spending directs resources by geography. A classic reference is Gavin Wright's (1974) study of New Deal spending in U.S. elections.

outcomes in Latin American democracies. We present evidence to show that there are no significant increases in transfers in the run-up to presidential elections. These results persist once we test for whether electoral cycles differ between countries given that some democracies in Latin America have only recently democratized. We argue that our results, which are due to superior measures of the institutions that capture the nature and degree of political competition in Latin America, support research that has argued that elections are fundamentally distinct in Latin America.

Between the late 1970s and 1990s, the majority of countries in Latin America experienced a founding election marked by the participation of formerly banned political parties and the retreat of the military (Huntington 1991).<sup>2</sup> Indeed, transitions to democracy occurred in Argentina, Bolivia, Brazil, Chile, Dominican Republic, Ecuador, El Salvador, Guatemala, Peru and Uruguay between 1978 and 1990. Democracy returned to in Panama and Paraguay in the mid 1990s and Mexico in 2000.

This paper seeks to answer two main questions. Are elections catalysts for increases in welfare spending in Latin America? Are elections held during democratic transitions more likely to provoke larger changes in budget allocations for programs whose beneficiaries are large groups of voters? In order to answer these questions, we undertake an in-depth, cross-national examination of how social policy instruments are used by Latin American democracies to try to influence reelection prospects. The effect of electoral politics is tested by looking at how presidential elections impact the relative share of expenditures in education, health and social security between 1973 and 2000. The hypothesis of whether pre-election government spending tends to exceed post-election efforts for social policy measures is robustly tested. To analyze the specific impact of democratic transitions, the paper examines whether elections during democratic transitions produce a distinct pattern from those held once presidential democracies are consolidated.

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<sup>2</sup> The failed fourth term re-election bid of Joaquín Balaguer in the Dominican Republic in 1978 is an exception.

The paper is structured in the following way. Section 2 reviews existing political budget cycle theory and empirical evidence on social spending during elections, as well as findings that have argued that elections in the early stages of democratization may cause countries to be particularly vulnerable to fiscal policy manipulation. Section 3 describes the time-series-cross-sectional data set employed for hypothesis testing. Section 4 introduces the model specifications that will be used for hypothesis testing in this paper and the battery of alternative models that are adopted to check the findings for robustness. Section 5 presents and discusses the results of the empirical analysis. The final section concludes the paper with a summary of the key findings.

## **2. Review of the Literature**

There is a robust literature on political budget cycles that has focused attention on if electoral cycles are stronger and more persistent for outlays on social expenditures and in particular social security transfers. Empirical work on transfer cycles has largely been developed based on the study of democracies in developed countries where empirical evidence has generally confirmed a marked rise in social spending prior to both national and local elections (Tufte 1978; Alesina 1988; Keech and Pak 1989; Hicks and Swank 1992; Mebane Jr. 1994; Schultz 1995; Kneebone and McKenzie 2001).

Tufte's (1978) research has become one of the seminal studies on electoral cycles and is illustrative of the arguments commonly found in subsequent research on political welfare cycles. Focusing on national elections in the United States between 1960 and 1976, Tufte describes the dramatic increase in transfers preceding presidential elections which totaled \$200 billion per year by 1976. He reports that social security benefit increases, which comprise half of total transfers and include payments for old age, survivors, disability and health insurance, were more likely in the year preceding presidential elections and that benefit increases were also timed to begin in the early months of the year in advance of U.S. national elections in November. The increases in payroll taxes paid by the working population, however, only took effect at the beginning of the calendar year. These earlier findings have also been confirmed by empirical studies

using multivariate regression analysis. For example, Alesina (1988) confirms that transfers, which are measured as total personal payments to U.S. citizens less personal contributions to social insurance as a share of GNP, follow a political budget cycle in a study of U.S. elections between 1961 and 1985.

Scholars have pointed out that the electoral payoff from investing in education, health and social security may not produce the same rewards for politicians at the ballot box as these programs benefit different constituencies whose interests are often competing. Cox and McCubbins (1986) develop a model to show electoral as a redistributive game in which each candidate promises redistributions of welfare among the various groups in their constituencies. Building on this theoretical framework, Mebane (1994) argues that in pay-as-you-go social insurance schemes payers want to lower benefits (to pay lower taxes), while recipients want taxes raised to increase social security payments. He posits that it is furthermore important to distinguish between means-tested beneficiaries (e.g. lower-income groups who are less educated, poorer and less apt to vote), non-means tested recipients (e.g. retirees who are very apt to vote) and taxpayers. In a study of monthly transfer payments and contributions in the United States from 1948-1987, he finds that in U.S. elections the expenditures on means-tested programs (e.g. unemployment, social assistance, etc.) and payroll taxes were slightly reduced.

Persson and Tabellini (2005) further refine this argue by arguing that spending on social programs, like social insurance and pensions, are examples of broad redistributive programs that benefit large groups of the population only in developed countries. The authors argue that many of the programs of the modern welfare state in developed countries cannot easily be tailored to the specific demands of well-defined groups of citizens and that these programs are evaluated in a similar fashion by large groups of beneficiaries (2005: 20). However, these same authors argue that the same pattern is not found in developing countries as spending on education, health and pension benefits in developing countries generally aid smaller segments of the urban population.

The arguments formulated by Mebane (1994) and Persson and Tabellini (2005) both underscore that the nature of political budget cycles and the effects of social policy instruments may be distinct depending on whether governments seek to appease wealthy taxpayers or to capture the support of voters in lower income deciles and depending on the type of welfare state that has been established. To the extent that government spending is directed at government works and non-means tested social programs, it may be that taxpayers and beneficiaries have similar interests. In this case, dramatic shifts in budget allocations prior to elections are less likely to occur. On the other hand, it is likely that there will be greater internal conflict in welfare states that favor greater provision of services and benefits for the poor as taxpayers are forced to bear the burden of these costs and derive minimal benefits. In these cases, the direction of fiscal manipulation may be in either direction.

Empirical research to examine if similar patterns exist with respect to electoral cycles for redistributive social spending in developing countries has been limited.<sup>3</sup> In Latin America, the only study to examine the impact of elections on social security transfers is a study of presidential and midterm elections in Mexico under PRI party dominance and non-competitive elections. In a study of 14 federal elections (both presidential and midterm Congressional elections), González (2002) finds that the growth rate of the share of Mexican central government expenditures on current transfers, which include subsidies to consumption, aid for cultural and social development, social security payments, and other nonfinancial transfers, did not increase in the previous six pre-election quarters, but did increase in the quarter of the election itself. Claiming to capture increased levels of “democratization” during elections as measured by lower scores on the Index of Political Coercion and the Autocracy Index, she argues that greater levels of

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<sup>3</sup> A much larger set of research on political budget cycles in developing countries has focused on infrastructure and public investment. These studies have also generally confirmed stronger patterns for electoral cycles on infrastructure in developing countries. González (2002) confirms a election cycle in infrastructure investments in Mexican federal elections. For state legislative assembly elections in India over the period 1960-1994, Khemani (2000) finds that there is a positive and large effect on road construction by state public works departments prior to voting periods. Drazen and Eslava (2009) confirm higher levels and budget shares of government spending directed at including infrastructure investments (both urban road and rail networks and water and energy power plants) in the year prior to elections in Colombian municipalities from 1987 to 2000.

“democracy” exacerbate political budget cycles as the PRI responded to the growing threat of losing power by spending more and more resources in election campaigns to ensure its victory. With the development of Mexico’s political institutions leading to improvements in transparency and accountability, she predicts that the election effect will increase as democratization increases.

A larger number of studies have examined non-social security social spending in developing and transition countries. In the only cross-national study that has included both developed and developing democracies, Persson and Tabellini (2005) do not find that there are statistically significant increases in social spending prior to or after executive elections in presidential democracies between 1960 and 1998. Country-specific case studies, however, have found partial evidence in support that social spending rises prior to elections. In a cross-municipality study of expenditures in Colombia, one of Latin America’s oldest and most stable democracies, Drazen and Eslava (2009) find that municipalities increased spending on housing and health (in levels and as a share of the total budget) in election years, but did not find that similar patterns occurred for education. The magnitudes of these electoral cycle effects, moreover, are not insignificant. Indeed, these authors report that health investments increased at rates higher than public infrastructure investments prior to municipal elections.

Based on a micro-level analysis of the Peruvian Social Fund (FONCODES) created during Alberto Fujimori’s first term in office, Schady (2000) finds that the distribution of expenditures for community projects between 1991 and 1995 increased significantly before elections to provinces where returns were expected to be large in elections for the constituent assembly (1992) and a referendum (1993) and for turncoat provinces in Fujimori’s reelection bid (1995). Based on these results, he speculates that election cycles may be greater in small-scale, targeted poverty alleviation programs as governments may have reduced abilities to manipulate large-scale subsidy or universalist social programs to influence returns at the ballot box.

The inferences that can be drawn from studies on political welfare cycles in Latin America, however, are limited. There has been no region-specific cross-national study and the three country studies that have examined social spending during elections focus on three countries that have evolved very differently in terms of their democratization. The studies of Mexico and Peru are particularly problematic as they are plagued with the same problems that have been cited for studies of political budget cycles for the region (Barberia and Avelino 2008). Namely, these studies are based on analysis of non-competitive elections. Of the fourteen Mexican presidential “elections” analyzed by González, the PRI was victorious with candidates earning virtually uncontested victories. Indeed, José López Portillo earned 100 percent of the vote in the 1976 election. The three Peruvian “elections” analyzed by Schady (2000) occurred following Fujimori’s shutting down of Congress in April 1992.<sup>4</sup> In this study, we seek to remedy the paucity of evidence for political welfare cycles by examining whether there are marked patterns for social spending during elections across the region versus particular countries. We also employ careful coding for democracy, transitional and consolidated stages of democracy to test if these patterns persist depending on the level of consolidation of this regime.

### **3. Data**

#### *Expenditures on Social Security, Health and Education*

The dependent variables in this paper are drawn from annual data on education,<sup>5</sup> health care<sup>6</sup> and social security<sup>7</sup> drawn from the International Monetary Fund (IMF)’s *Government Finance Statistics* (GFS)(International Monetary Fund 2006). Rather than using the data in its original form, the dataset used in this study is the revised dataset developed by Kaufman and Segura (2001). In each case, the dependent variable is the share of social spending relative to total central government expenditures. This

<sup>4</sup> As will be subsequently explained, Fujimori’s entire presidency from 1991-2000 is not included in this study on election cycles in social spending as his rule is considered authoritarian.

<sup>5</sup> Education expenditures include spending on pre-primary and primary education, secondary education, postsecondary education, tertiary education, subsidiary services to education, and R&D on education (International Monetary Fund 2001).

<sup>6</sup> Health expenditures include spending on medical products, appliances, equipment, outpatient services, hospital services, public health services, and R&D in health (International Monetary Fund 2001).

<sup>7</sup> Social security includes benefits paid related to sickness and disability, old age, survivors, family and children, unemployment and social protection R&D (International Monetary Fund 2001).



measurement of the dependent variable as a share of the government budget is intended to capture the relative importance of each function vis-à-vis other services or goods that the government might choose to purchase.

There are four differences in the dataset employed in this paper with respect to social spending as compared to the original series employed by Kaufman and Segura (Kaufman and Segura-Ubiergo 2001) and Segura (Segura-Ubiergo 2007). First, the dataset used in this study expands the original data set (1973-1997) by including three additional years of expenditures on education, health and social security for the period between 1998 and 2000. Kaufman and Segura (2001) calculate welfare spending net of interest payments based on the argument that these proportions of central government spending are long-term commitments and do not reflect the decisions of the governments in power from 1973 to 2000.<sup>8</sup> The original dataset was updated by adding collected data from IMF GFS yearbooks following the methodology employed by Kaufman and Segura (2001). Second, an additional country, Panama, was added to the dataset. A fairly complete accounting of fiscal and social expenditures exists for Panama for the period under examination in the IMF's *Government Finance Statistics* volumes.

Third, in contrast to Segura-Ubiergo (2007), this study treats censored observations as randomly missing and do not attempt to model this aspect of sample selection. Segura-Ubiergo (2007:128) reports that he used ECLAC social spending to fill the missing gaps for Peru and Venezuela. Given that it is not clear that IMF Government Finances Statistics data and ECLAC social spending data are reported on the same basis, this procedure requires significant assumptions that were not deemed advisable. Finally, available data for 2001 to 2003 were not added. The IMF altered its methodology for collecting government expenditure data after 2000. Statistics are now reported on both a cash and accrual basis, but the two methodologies are reported without reconciliation.

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<sup>8</sup> A future research task will be to test the differences in the results obtained from including interest payments as a share of total government spending.

One additional observation on the data is in order. Statistical strategies to modify the data in which there are significant jumps or outliers were not adopted. There are two periods in two countries where there are significant jumps in social security spending in the data set. Social security spending for Brazil was included in the overall government budget starting in 1989 following the enactment of the 1988 Constitution. Prior to this date, pensions had been accounted for in a special budget account and not included in central government expenditure accounts. The inclusion of these expenditures results in significant increases in the pension share of the budget for 1989 and 1990 and thus the annual share of the budget allocated to social security in this period for Brazil is overstated during these years. El Salvador undertook a reform of its social security system in 1998 that included privatization. There is a significant jump in social security spending between 1997 and 1998; expenditures increased from 6.18% of the total budget to 27.3%. For both countries, the data are reported in the original form.

#### *Democracy and Election Data*

The sample includes only democratic years in Latin America between 1973 and 2000. A minimalist definition of democracy was adopted to code democratic years based on an updated version of the Álvarez, Cheibub, Limongi and Przeworski (1996) and Przeworski, Álvarez, Cheibub and Limongi (2000) datasets published by Cheibub and Ghandi (2004). Modifications were made to the dataset based on the classification of democratic and authoritarian regimes in Nohlen (2005) for specific periods in six countries and are discussed in detail in Barberia (2008). It is important to note that some countries enter only in some years. For example, the democracy rule temporarily excludes countries like Argentina (between 1976 and 1982) and Chile (between 1973 and 1988).

To test for differences between election and non-election years, a dichotomous dummy variable that codes one for the year of a presidential election was created. This data is drawn from the *Latin American Democracy Codebook for Latin America* from 1980 to 2000 prepared by Avelino (2006). Election dates from 1970 to 1979 were added

based on information reported in Nohlen (2005) and the *Political Database of the Americas* (Center for Latin American Studies at Georgetown University 2007).

In order to code for the different stages of the electoral cycle, a series of dummy variables were created based on two standard approaches in the literature. The “rule of the semester” measure that codes all elections prior to June 1<sup>st</sup> as “1” in the previous year was adopted. For example, if an election occurred in September 1973 as it did in Argentina, this year is coded as the election year. If an election occurred in February 1974 as it did in Costa Rica, 1973 is coded as the election year. An alternative method following the rule of the year was also used. Under this measure, the year was coded as one if a presidential election occurred in that particular year from January to December. Thus, under the rule of the year methodology “1” indicates that the election occurred after January 1<sup>st</sup> and before December 31<sup>st</sup> of the year in question and otherwise the value of “0” was assigned. The alternative method will be used as a check on the results reported in the paper, but will not be presented in the discussion that follows. For both approaches, dummy variables for the year prior to an election and the year following an election were also created.

The dataset includes 62 presidential elections and 71 legislative elections and a total of 188 non-election democratic years. Of the 15 countries in the sample, all countries had at least one presidential election. On average, there are 3.5 presidential elections for country. Mexico is the only country that has one election (e.g. Vicente Fox’s election in 2000). In 55 presidential elections, voters also choose congressional representatives. The paper limits attention to the years of presidential elections and do not include 16 midterm legislative elections. The focus on presidential elections follows the literature; studies that have included congressional elections have not found that these elections have distinct impacts on electoral cycles (Drazen 2000). It is also pragmatic as the purpose of this paper is to test both election and post-electoral periods and the inclusion of congressional elections results in some years counted as falling into both categories. Of the 62 presidential elections in the sample, only 32 are the same if either the rule of the year or semester classifications are adopted. The rule of the semester

codification rule is adopted as the preferred measure of election cycles as it is more accurate as compared to the rule of the year classification. Of the sixty-two elections, there are five cases in which a president was re-elected. The cases are Carlos Saúl Menem in Argentina in 1995, Fernando Henrique Cardoso in Brazil in 1998, Joaquín Balaguer in the Dominican Republic in 1990 and Hugo Chávez in Venezuela in 2000.

To test for differences between elections held during democratic transition and established democracy years, a dichotomous dummy variable that codes one for transitional democracy election years was created. The beginning of democratic transition is defined as the year of the inauguration of the first democratic regime following a period of authoritarian rule (Avelino 2000, 2005). The onset of stable democracy is defined as the second consecutive democratic turnover in which there is a change in the political party controlling the presidency following the criteria stipulated by Huntington (1991), who defends the two-turnover test as an unambiguous measure of the resilience of democracy. This definition is also consistent with the definition of democracy adopted in this study following Álvarez, et al (1996) who argue that this regime is characterized by the opposition rising to power through elections.

It should be noted that Costa Rica and Venezuela did not undergo democratic transitions during the period. Table 1 lists the 63 presidential elections that took place in the sample and denotes the 29 elections that took place during democratic transitions in boldface. It should be noted that due that social expenditure data were not available for 8 elections in five countries (Bolivia [1985], Ecuador [1996 and 1998], Guatemala [1999], Paraguay [1997], Venezuela [1988, 1993 and 1998].) Two of these elections are considered to have taken place in the democratic transition period and the remaining five in consolidated democracies.

**Table 1. Presidential Elections in Latin America, 1973-2000**

| Country            | Presidential Election Dates                                   |
|--------------------|---|
| Argentina          | <b>9/1973, 9/1983, 5/1989, 5/1995, 10/1999</b>                |
| Bolivia            | <b>6/1980, 7/1985, 5/1989, 6/1993, 6/1997</b>                 |
| Brazil             | <b>1/1985, 11/1989, 10/1994, 10/1998</b>                      |
| Chile              | <b>12/1989, 12/1993, 12/1999</b>                              |
| Costa Rica*        | 2/1974, 2/1978, 2/1982, 2/1986, 2/1990, 2/1994, 2/1998        |
| Dominican Republic | <b>5/1978, 5/1982, 5/1986, 5/1990, 5/1994, 6/1996, 5/2000</b> |
| Ecuador            | <b>4/1979, 1/1984, 1/1988, 7/1992, 7/1996, 6/1998</b>         |
| El Salvador        | <b>3/1984, 3/1989, 4/1994, 3/1999</b>                         |
| Guatemala          | <b>11/1985, 11/1990, 11/1995, 11/1999</b>                     |
| Mexico             | <b>7/2000</b>   |
| Panama             | <b>5/1994, 5/1999</b>   |
| Paraguay           | <b>5/1993, 5/1998</b>   |
| Peru               | <b>5/1980, 4/1985</b>   |
| Uruguay            | <b>11/1984, 11/1989, 11/1994, 11/1999</b>                     |
| Venezuela*         | 12/1973, 12/1978, 12/1983, 12/1988, 12/1993, 12/1998, 7/2000  |

Notes: \* No democratic transition elections. Elections during democratic transitional period according to the founding election and one turnover of political power test are indicated in boldface. There are 2 elections (Mexico and Venezuela) that ended in 2000 and therefore do not have a post-election year (rule of the semester). Source: Elaborated by the authors.

One of the key challenges in analysis of elections is the extent to which they may be endogenous as oftentimes the end of a particular regime is not pre-determined, but coincides with economic crises (Przeworski and Limongi 1993; Haggard and Kaufman 1997). There are a few reasons why the endogeneity of elections does not seem to be a significant problem for the questions that will be explored in this paper. First, the problem of simultaneity bias is much more severe in political budget cycle studies that employ economic growth, unemployment and inflation as dependent variables as declines in the performance of these variables are precisely what tend to trigger the collapse of particular administrations. Second, unlike parliamentary democracies, elections are typically held on a fixed schedule in presidential democracies such as those found in Latin America. Of course, there are some notable exceptions. Fueled by rampant hyperinflation, Siles Suazo anticipated presidential elections one year earlier than the end of his term in 1985 in Bolivia. In Argentina, Raúl Alfonsín similarly ceded power earlier than anticipated though only a few months earlier than planned. The robustness of the results reported in Section 4 without both of these elections will be tested and reported. Third, control variables in estimations include measures of per capita economic growth to capture the possible endogenous relationship that exists between government spending and elections.

### *Control Variables*

Several demographic and economic variables are included as control variables. Demographic characteristics of the population are likely to impact government spending. Two demographic variables representing the fraction of the population aged 15–64 and 65 and over are employed as controls. A higher percentage of the working age population and the elderly populations are expected to positively increase the pressures exerted by voters for higher budget allocations and the worsening of budget deficits. Unless otherwise noted, the control variables data are from the World Bank's *World Development Indicators* (2007).

In the last three decades of the 20<sup>th</sup> century Latin America underwent significant urbanization rising from nearly 55 percent of the population to over 70 percent by 2000. Higher degrees of industrialization and urbanization tend to be correlated with a larger working class population. Ames (1987) asserts that more urbanized nations also tend to have populations that are better organized politically.<sup>9</sup> Therefore, the percentage of the population living in urban areas is included to control for the heightened responsiveness of politicians to the demands of urban groups when they are subject to electoral competition.

Controls are also used to account for the level of heterogeneity in income and growth rates across countries. The level of economic development, which is defined as the real gross domestic product per capita and measured in constant US\$ 2000 dollars, is included in the model to control for Wagner's Law that holds that the level of public spending will be positively correlated with levels of economic development. Higher levels of per capita income are expected to be correlated with higher levels of government spending.

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<sup>9</sup> Ames (1987:79)

A control for GDP growth lagged one year was also included. The increase in available resources resulting from economic growth in year  $t-1$  should lead to increased demands for redistribution by voters in year  $t$ . In contrast to the countercyclical pattern found in Western industrialized democracies, primary spending has been found to be procyclical in Latin America (Gavin and Perotti 1997; Aldunante and Martner 2006). Accordingly, governments in Latin America respond by increasing spending during periods of economic growth and reducing expenditures during recessions and crises. Thus, the coefficient for GDP growth lagged one period is expected to be positive.

Both trade and financial liberalization increased dramatically in Latin America during the 1990s. Thus, measures of trade integration and capital mobility both aim to gauge the degree of an economy's integration with world markets. Trade is calculated as the sum of imports and exports relative to GDP, where the denominator is calculated by converting domestic local currency to current US\$ based on exchange rate conversions. A measure of capital mobility based on the decision rules outlined by Quinn (1997) is used. The data are drawn from Avelino, Brown and Hunter (2005).<sup>10</sup>

#### 4. Estimation Procedure and Model Specification

The baseline model to test the effect of elections can be specified as:

$$Y_{i,t} = \alpha + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 * ELEC_{i,t} + \beta_4 * ELEC_{i,t+1} + c_i + \epsilon_{it} \quad (1)$$

The first dependent variable that will be tested is the share of aggregate social spending (health, education and social security combined) as a share of total government spending. Two additional dependent variables will be employed: the budget share devoted to education and health and the proportion of the total budget that is channeled to social security. This model tests whether there are differences in social spending prior to elections by including a dummy variable, ELEC, for the election year. The expected sign of this coefficient is positive and statistically significant from zero.

<sup>10</sup> See Avelino, Brown and Hunter (2005) for a more detailed explanation of this variable.

The model also includes a one-period lag of the dependent variable,  $Y_{i,t-1}$ , and a vector of control variables,  $Z$ , as described earlier. The index  $i$  refers to the  $N$  observational units (or panels), and  $t$  indexes the  $T$  time periods. The term  $c_i$  contains country-specific unobserved effects that impact welfare spending, as well as the democratic character of the regime in a given country and  $\alpha_t$  represents year dummies. The error term,  $\epsilon_{it}$ , is an error term associated with unit  $i$  at time  $t$ .

In a second stage, a model is used to test whether elections produce differences if the president was selected in an election that took place during the transitional democracy period (TDELEC). The second model that will be tested can be specified as:

$$Y_{i,t} = \alpha_t + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 *ELEC_{i,t} + \beta_4 *TDELEC_{i,t} + c_i + \epsilon_{i,t}. \quad (2)$$

The marginal effect of an election during the democratic transition phase,  $\beta_4$ , measures the marginal difference of elections that take place prior to a democracy satisfying Huntington's two turnover test. If the hypothesis that elections in new democracies result in greater levels of government spending is correct,  $\hat{\beta}_4$  should be positive and statistically significant from zero and  $\hat{\beta}_3 + \hat{\beta}_4$  (the total effect of an election in a new democracy) should be statistically significant from zero. On the other hand,  $\hat{\beta}_3$  (the effect of elections in established democracies in the election year) should not be statistically significant from zero.

Theoretical models that have been used to guide contemporary PBC research have largely drawn upon the Rogoff (1990) model. With respect to the post-election period, Rogoff (1990) points out that "pre- and post-election values of observable fiscal policy variables tend to be positively correlated (when variables are measured in deviations from pre- and postelection means)." A series of subsequent models have yielded different predictions on whether government spending will increase or decrease in the post-



election period (Alt and Lassen 2006; Shi and Svensson 2006; Streb and Torrens 2009).<sup>11</sup> Therefore, a third model will be estimated including a dummy variable for the year following elections.

$$Y_{i,t} = \alpha + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 * ELEC_{i,t} + \beta_4 * ELEC_{i,t+1} + c_i + \mu_{it} \quad (3)$$

In a fourth stage, a model is used to test whether elections produce differences if the president was selected in an election that took place during the transitional democracy period (TDELEC) and also controlling for the pre and post electoral years. The fourth model that will be tested can be specified as:

$$Y_{i,t} = \alpha + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 * ELEC_{i,t} + \beta_4 * ELEC_{i,t+1} + \beta_5 * TDELEC_{i,t} + \beta_6 * TDELEC_{i,t+1} + c_i + \mu_{i,t} \quad (4)$$

The marginal effect of an election during the democratic transition phase,  $\beta_5$ , measures the marginal difference of elections that take place prior to a democracy satisfying Huntington's two turnover test. If the hypothesis that elections in new democracies result in greater levels of government spending is correct,  $\hat{\beta}_5$  should be

positive and statistically significant from zero and  $\beta_3 + \beta_5$  (the total effect of an election in a new democracy) should be statistically significant from zero. Similarly, the parameter  $\hat{\beta}_6$  measures the marginal difference between post-election years in democratic transition and non-transitional democracies. If the hypothesis that democratic transition elections result in lower levels of government spending in the year after elections is correct,  $\hat{\beta}_6$  should be positive and statistically significant from zero and  $\hat{\beta}_4 + \hat{\beta}_6$  (the total effect of a democratic transitional election) should be statistically

significant from zero. On the other hand,  $\beta_3$  and  $\hat{\beta}_4$  (the effect of elections in established democracies in the election and post-election years) should not be statistically significant from zero.

<sup>11</sup> We are grateful to Jorge Streb for his insight on the important distinctions that should be made between Rogoff and these models and their implications for the direction of the fiscal policy response.

Based on the assumption that past levels of government spending influence the levels of expenditures in future years, a lagged dependent variable is included in each specification. A series of measures were taken to check for consistency and robustness of the results that will be reported below. First, pooled ordinary least squares regressions (OLS) with panel corrected standard errors (column 1) were estimated.<sup>12</sup> Subsequently, country fixed effect estimates (column 2) and year fixed effects on top of country fixed effects (column 3) were undertaken.<sup>13</sup> In a second stage, two Generalized Methods of Moments (GMM) procedures were used: the Arellano and Bond (1991) first-differenced GMM estimator (GMM-Diff) and the Blundell and Bond (1998) system GMM estimator (GMM-System).<sup>14</sup> GMM estimates were also carried out with and without control for year fixed effects (columns 4-7). Therefore, the tables presented below consist of a total of seven columns. Two lags of the dependent variable were used in the GMM difference and systems equations. The four specifications present GMM estimates using the Arellano-Bond (difference) and Blundell-Bond (system) procedures with orthogonal deviations to adjust for an unbalanced panel and collapsed to minimize the number of instruments following the recommendations of Roodman (2007). Per capita GDP and growth were also included as endogenous variables in the GMM estimations. For GMM estimates standard errors are reported as t-statistics based on Windmeijer (2005) finite sample correction and corrected for serial correlation and heteroskedasticity.<sup>15</sup>

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<sup>12</sup> The model will be estimated with the Stata XTPCSE command.

<sup>13</sup> For fixed T, Nickell (1981) demonstrates that the within groups estimate of the coefficient is likely to be biased downward of the order  $1/T$ , where T is the length of the panel. Thus, the magnitude of the bias in the fixed effects estimates can be calculated in the within-group estimator for a dynamic model with fixed individual effects. The exact magnitude depends on which sample and indicator is used as some countries do not report data for the entire period. In a panel of all countries from 1973 to 2000, the maximum length of the sample is 28 years and the minimum length is 12 years for two countries (Brazil and Paraguay). Hence, the bias from using a fixed effects estimator in these regressions is likely range from 3.6% ( $1/28$ ) to 8.3% ( $1/12$ ).

<sup>14</sup> The exercise and commands for GMM estimation are based on Roodman (2006) and were carried out using Stata 10.

<sup>15</sup> The results of an error correction model (ECM), which is also appropriate for highly persistent series, with panel corrected standard errors based on the *first difference* of the dependent variables was also carried out and did not change the findings reported in this paper.

## 5. Results

In the tables that follow, the estimates for the coefficients will be presented for the effect of elections on aggregate social spending (panel A), as well as the effects when spending is disaggregated by the share of the budget spent on health and education combined (panel B) and on pensions (panel C). Relative to the base category, which is defined as all other democratic years, the estimations yield surprising results that contrast with findings for industrialized democracies and the patterns reported previously in the country-specific literature for Latin America. The coefficients on the effect of election cycles reported in the tables that follow are based on the rule of the semester.<sup>16</sup>

The results presented in Tables 2, 3, 4 and 5 provide only the coefficient estimations for the key variables of interest that pertain to elections for presentation purposes. It should be noted, as introduced earlier, that the regressions were estimated with controls for the lag of the dependent variable, per-capita GDP, one lag of per capita GDP growth, the fraction of the population over age 65, the fraction of the population between the ages 15 and 64, the fraction of the population living in urban areas, trade openness and capital mobility.

### *Election Year Effects*

Theoretical and empirical predictions drawing upon Tufte would suggest that we should expect to find higher social spending in the year of elections in Latin American democracies. In fact, the dummy variable measuring the effect of an election year on social spending is not statistically significant in regressions for either total social spending or health and education budget shares in Table 2. There is some evidence that social security expenditures contract prior to the election of a new president relative to all other democratic years, but the coefficient estimate on the election year is only statistically significant in the country-level fixed effects in Pooled OLS and GMM One-Step System equations.

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<sup>16</sup> The same models were also estimated using the rule of the year definition for elections. In these specifications the estimated coefficients generally were the same sign, but weaker in magnitude.

**Table 2. Political Budget Cycles in Latin America, 1973-2000: The Effect of Elections on Social Spending**

| <i>Dependent Variable:</i>  | (1)                | (2)   | (3)  | (4)                                     | (5)   | (6)                                 | (7)   |
|---|--------------------|---|--|---|---|-------------------------------------|---|
| <i>A. Social Spending/Total Central Government Expenditures<sup>a</sup></i> | Pooled OLS<br>PCSE | Pooled OLS PCSE<br>with country fixed<br>effects (f.e.) | Pooled OLS PCSE<br>with country and<br>year f.e. | GMM One-Step<br>First Diff <sup>b</sup> | GMM One-Step<br>First Diff with<br>year f.e. <sup>a</sup> | GMM One-Step<br>System <sup>a</sup> | GMM One-Step<br>System with year<br>f.e. <sup>a</sup> |
| Election Year   | -0.337<br>(1.128)  | -0.570<br>(0.977)                                       | -0.283<br>(0.977)                                | -0.555<br>(1.240)                       | -0.287<br>(0.864)   | -0.653<br>(1.374)                   | -0.391<br>(1.105)                                     |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.78               | 0.84  | 0.86   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 13                                      | 38  | 18                                  | 43  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.315                                   | 0.567   | 0.779                               | 0.807   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.522                                   | 1.000   | 0.683                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 0.973                               | 1.000   |
| <b><i>B. Health and Education/Total Central Government Expenditures</i></b> |                    |   |  |   |   |                                     |   |
| Election Year   | 0.011<br>(0.743)   | -0.307<br>(0.659)                                       | -0.327<br>(0.692)                                | -0.548<br>(0.884)                       | 0.838<br>(1.284)  | 0.602<br>(0.740)                    | 0.057<br>(0.735)                                      |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.85               | 0.89  | 0.90   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 13                                      | 38  | 18                                  | 43  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.319                                   | 0.223   | 0.130                               | 0.239   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.341                                   | 1.000   | 0.909                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 0.909                               | 1.000   |
| <b><i>C. Social Security/Total Central Government Expenditures</i></b>      |                    |   |  |   |   |                                     |   |
| Election Year   | -0.521<br>(0.388)  | -0.663*<br>(0.369)                                      | -0.556<br>(0.359)                                | -0.799<br>(0.531)                       | -0.663<br>(0.464)   | -1.059*<br>(0.552)                  | -0.799*<br>(0.403)                                    |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.89               | 0.92  | 0.93   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 13                                      | 38  | 18                                  | 43  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.663                                   | 0.857   | 0.767                               | 0.807   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.223                                   | 1.000   | 0.718                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 0.632                               | 1.000   |

Notes: The covariates include lags of the dependent variable, per-capita GDP, one lag of per capita GDP growth, the fraction of the population over age 65, the fraction of the population between 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. In those cases that are noted, country and year dummy variables were included in regressions but were not reported above for presentation purposes. Pooled OLS regressions were estimated with panel corrected standard errors that correct for groupwise heteroskedasticity and contemporaneous correlations of the errors. Standard errors in parentheses and significance levels are as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

<sup>a</sup> Two lags of the dependent variable were used in the GMM difference and systems equations. The four specifications present GMM estimates using the Arellano-Bond (difference) and Blundell-Bond (system) procedures with orthogonal deviations to adjust for an unbalanced panel and collapsed to minimize the number of instruments following the recommendations of Roodman (2007). Per capita GDP and growth were also included as endogenous variables in the GMM estimations. For GMM estimates standard errors are reported as t-statistics based on Windmeijer (2005) finite sample correction and corrected for serial correlation and heteroskedasticity.

<sup>b</sup> The Arellano-Bond tests for first-order and second-order serial correlation are reported for all GMM models. The tests were carried out on the first-differenced residuals. The p-values are the probability of rejecting the null hypothesis of no autocorrelation.

**Table 3. Political Budget Cycles in Latin America, 1973-2000: The Effect of Elections in Social Spending in Consolidated and Transitional Democracies**

| <i>Dependent Variable:</i>  | (1)                | (2)   | (3)  | (4)                                     | (5)   | (6)                                 | (7)   |
|---|--------------------|---|--|---|---|-------------------------------------|---|
| <i>A. Social Spending/Total Central Government Expenditures<sup>a</sup></i> | Pooled OLS<br>PCSE | Pooled OLS PCSE<br>with country fixed<br>effects (f.e.) | Pooled OLS PCSE<br>with country and<br>year f.e. | GMM One-Step<br>First Diff <sup>a</sup> | GMM One-Step<br>First Diff with<br>year f.e. <sup>a</sup> | GMM One-Step<br>System <sup>a</sup> | GMM One-Step<br>System with year<br>f.e. <sup>a</sup> |
| Election Year   | -0.004<br>(2.071)  | -1.037<br>(1.736)                                       | -0.656<br>(1.763)                                | 0.284<br>(0.910)                        | -0.320<br>(1.223)   | 0.133<br>(1.302)                    | 0.298<br>(1.359)                                      |
| Transitional Democracy Election Year  | -0.559<br>(2.608)  | 0.075<br>(2.323)  | -0.137<br>(2.133)                                | -2.214<br>(2.878)                       | 0.369<br>(3.538)  | -1.006<br>(2.351)                   | -1.569<br>(2.481)                                     |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.78               | 0.84  | 0.86   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 14                                      | 39  | 19                                  | 44  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.394                                   | 0.467   | 0.804                               | 0.725   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.478                                   | 1.000   | 0.674                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 0.961                               | 1.000   |
| <i>B. Health and Education/Total Central Government Expenditures</i>        |                    |   |  |   |   |                                     |   |
| Election Year   | 0.316<br>(1.201)   | -0.724<br>(1.094)                                       | -0.705<br>(1.054)                                | -0.333<br>(0.635)                       | 0.384<br>(1.082)  | 0.608<br>(0.674)                    | 0.566<br>(0.796)                                      |
| Transitional Democracy Election Year  | -0.490<br>(1.330)  | 0.778<br>(1.318)  | 0.693<br>(1.238)                                 | -0.399<br>(1.370)                       | 1.104<br>(2.601)  | 0.228<br>(1.576)                    | -0.758<br>(1.460)                                     |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.85               | 0.89  | 0.90   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 114                                     | 39  | 19                                  | 44  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.305                                   | 0.198   | 0.136                               | 0.235   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.320                                   | 1.000   | 0.986                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 1.000                               | 1.000   |
| <i>C. Social Security/Total Central Government Expenditures</i>             |                    |   |  |   |   |                                     |   |
| Election Year   | -0.323<br>(1.667)  | -0.151<br>(1.451)                                       | 0.155<br>(1.364)                                 | -0.106<br>(1.801)                       | 0.255<br>(1.546)  | -0.399<br>(1.843)                   | 0.273<br>(0.883)                                      |
| Transitional Democracy Election Year  | -0.026<br>(2.012)  | -0.809<br>(1.842)                                       | -0.869<br>(1.660)                                | -0.781<br>(2.859)                       | -1.079<br>(2.849)   | -0.431<br>(2.586)                   | -1.268<br>(1.388)                                     |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.89               | 0.92  | 0.93   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 14                                      | 39  | 19                                  | 44  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.674                                   | 0.802   | 0.736                               | 0.909   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.238                                   | 1.000   | 0.997                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 1.000                               | 1.000   |

Notes: The covariates include lags of the dependent variable, per-capita GDP, one lag of per capita GDP growth, the fraction of the population over age 65, the fraction of the population between 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. In those cases that are noted, country and year dummy variables were included in regressions but were not reported above for presentation purposes. Pooled OLS regressions were estimated with panel corrected standard errors that correct for groupwise heteroskedasticity and contemporaneous correlations of the errors. Standard errors in parentheses and significance levels are as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

<sup>a</sup> Two lags of the dependent variable were used in the GMM difference and systems equations. The four specifications present GMM estimates using the Arellano-Bond (difference) and Blundell-Bond (system) procedures with orthogonal deviations to adjust for an unbalanced panel and collapsed to minimize the number of instruments following the recommendations of Roodman (2007). Per capita GDP and growth were also included as endogenous variables in the GMM estimations. For GMM estimates standard errors are reported as t-statistics based on Windmeijer (2005) finite sample correction and corrected for serial correlation and heteroskedasticity.

<sup>b</sup> The Arellano-Bond tests for first-order and second-order serial correlation are reported for all GMM models. The tests were carried out on the first-differenced residuals. The p-values are the probability of rejecting the null hypothesis of no autocorrelation.

### *Transitional Democracies*

Given that a significant share of Latin American countries experienced a transition to democracy since 1979, the results reported earlier in Tables 2 could be driven by the failure to account for the effects of electoral competition following authoritarian rule prior to the consolidation of democracy as argued by Brender and Drazen (2005).<sup>17</sup> Therefore, the model was estimated with a dummy variable to measure those elections that took place prior to the year in which the less than two turnover criteria for new democracies was satisfied by coding these as transitional democracy election years. The evidence in Table 3 is robust in rejecting this hypothesis. The coefficient estimates are not statistically significant neither in the case of consolidated or transitional democracies.

As a further check, the same model was run using the measure of new democracy proposed by Brender and Drazen (the first four democratic elections).<sup>18</sup> The results are not presented, but the coefficient estimates were not statistically significant for either the budget share of social spending or the two disaggregated measures. In contrast to the transitional democracy measure, however, the coefficient sign for the effect of elections in new democracies is positive in the case of health and education spending and oscillate in sign for the budget share devoted to social security.

### *The Effect on Social Spending of the Year after the Election*

Political business and budget cycles models generally posit that spending should increase in the year of the election. Some models have also hypothesized that spending increases in the year following the election. Following Rogoff, we might expect to see higher social spending allocations should occur in election and post-election years. While

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<sup>17</sup> Block, Ferree and Singh (2003) and Remmer (1993) hypothesize that we should see higher peaks in economic policy performance during "foundational" elections. To test whether the findings for transitional democratic years are driven by founding elections, the models were also estimated excluding the thirteen elections that were the first election following military rule. The results reported in Table 3 were confirmed.

<sup>18</sup> The results are available from the authors.

the dummy variable measuring an election year is now positive, it is not statistically significant in regressions for either total social spending, social security or health and education budget shares. In contrast, elections do appear to have a strong and robust impact on social spending once newly elected democratic administrations enter office. These findings support the arguments made by Franzese (2002b), who argues that electioneering should be even higher after leaders assume office as candidates counter-promise expenditure increases in the case of transfers. After controlling for economic and socio-demographic factors, the regression estimations suggest that there is a fairly large and robust increase in social spending in the first year following an election. The results in Table 4 are particularly striking if we compare them to the results reported by Barberia and Avelino (2008) showing that government spending as a share of GDP declines both in the year of and the year following presidential elections in Latin America.

In Table 4, the coefficient estimates are presented for the dummy variables controlling for both the year of the elections and the following year. These results yield surprising and unexpected results. In the year after winning office, there is a marked increase in the share of the budget devoted to social spending ranging from 2.8 to 3.8 percent (panel A). The coefficient estimates on the post-election year are positive and statistically significant in the case of the share of the budget devoted to social security (panel C), but not in the case of health and education expenditures (panel B). Together, these results seem to suggest that the increase in the allocation of the budget to social spending is driven by larger allocations in social security in the inaugural year of a new presidency.

Together, the results presented in Table 4 are suggestive of the argument that election-induced changes in the shares of the budget directed at education, health and pensions are most relevant for the inaugural year of a presidential mandate. Social spending does not appear to be used to manipulate elections, as much as it is used to reward voters once new governments enter office. To further confirm these results, we re-ran the regressions substituting the rule of semester for the rule of year method of classifying election years. The results confirmed a positive increase in social spending

and the budget share devoted to social security in the year after elections. This evidence seems to support our argument that the budget increases are not due to miscalculations by the previous administration, but instead more adequately explained as the result of policy measures introduced by the newly elected president and congress.

### *Transitional Democracies and the Electoral Cycle*

Given arguments suggesting that electoral cycles should be more marked in the case of more fragile democracies, Table 5 tests whether social policy is more responsive in democratic governments following the transition from authoritarian rule with controls for the year of and the year following the election. Dummy variables are also included to control for the election and post-election year for established democracies (those with at least two consecutive turnovers in power). The omitted category is all other democratic years. If the argument of the distinction between transitional and consolidated democracies is correct, we should expect to find that the coefficients that measure the marginal effect of elections in transitional democracies as positive and statistically significant and the coefficient for election and post-election years in consolidated democracies should not be statistically significant from zero.



**Table 4. Political Budget Cycles in Latin America, 1973-2000: The Effect of Elections and Post-Election Years on Social Spending**

| <i>Dependent Variable:</i>  | (1)                | (2)   | (3)  | (4)                                     | (5)   | (6)                                 | (7)   |
|---|--------------------|---|--|---|---|-------------------------------------|---|
| <i>A. Social Spending/Total Central Government Expenditures<sup>a</sup></i> | Pooled OLS<br>PCSE | Pooled OLS PCSE<br>with country fixed<br>effects (f.e.) | Pooled OLS PCSE<br>with country and<br>year f.e. | GMM One-Step<br>First Diff <sup>a</sup> | GMM One-Step<br>First Diff with<br>year f.e. <sup>a</sup> | GMM One-Step<br>System <sup>a</sup> | GMM One-Step<br>System with year<br>f.e. <sup>a</sup> |
| Election Year:  | 0.707<br>(1.486)   | -0.080<br>(1.295)                                       | 0.282<br>(1.332)                                 | -0.412<br>(1.565)                       | 0.527<br>(1.370)  | 0.733<br>(1.420)                    | 0.448<br>(1.482)                                      |
| Post-Election Year <sub>+1</sub>  | 3.256**            | 2.894**   | 2.838**  | 2.860                                   | 2.385   | 3.750*                              | 2.903**   |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.79               | 0.84  | 0.86   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 14                                      | 39  | 19                                  | 44  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.319                                   | 0.530   | 0.833                               | 0.731   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.581                                   | 1.000   | 0.854                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 1.000                               | 1.000   |
| <i>B. Health and Education/Total Central Government Expenditures</i>        |                    |   |  |   |   |                                     |   |
| Election Year:  | 0.246<br>(0.787)   | -0.138<br>(0.701)                                       | 0.032<br>(0.751)                                 | -0.329<br>(1.036)                       | 1.177<br>(1.463)  | 0.953<br>(0.852)                    | 0.599<br>(1.034)                                      |
| Post-Election Year <sub>+1</sub>  | 0.726<br>(0.782)   | 0.509<br>(0.680)  | 0.946<br>(0.728)                                 | 0.924<br>(0.781)                        | 0.863<br>(0.943)  | 1.578<br>(1.001)                    | 1.520<br>(1.098)                                      |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.85               | 0.89  | 0.90   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 14                                      | 39  | 19                                  | 44  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.348                                   | 0.236   | 0.111                               | 0.246   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.305                                   | 1.000   | 0.903                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 0.912                               | 1.000   |
| <i>C. Social Security/Total Central Government Expenditures</i>             |                    |   |  |   |   |                                     |   |
| Election Year:  | 0.517<br>(1.188)   | 0.182<br>(1.035)  | 0.352<br>(1.003)                                 | 0.019<br>(1.230)                        | 0.224<br>(0.939)  | 0.105<br>(1.502)                    | 0.340<br>(1.241)                                      |
| Post-Election Year <sub>+1</sub>  | 2.567**<br>(1.089) | 2.438***<br>(0.946)                                     | 1.945**<br>(0.959)                               | 2.125<br>(1.367)                        | 1.557<br>(1.182)  | 2.511<br>(1.652)                    | 1.879<br>(1.312)                                      |
| Observations  | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared   | 0.89               | 0.92  | 0.93   |   |   |                                     |   |
| Number of Instruments   |                    |   |  | 14                                      | 39  | 19                                  | 44  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>(p value)                      |                    |   |  | 0.669                                   | 0.820   | 0.759                               | 0.755   |
| Hansen test for joint validity of instruments (p value)                     |                    |   |  | 0.257                                   | 1.000   | 0.944                               | 1.000   |
| Diff. Sargan tests for all system instruments (p value)                     |                    |   |  |   |   | 0.997                               | 1.000   |

Notes: The covariates include lags of the dependent variable, per-capita GDP, one lag of per capita GDP growth, the fraction of the population over age 65, the fraction of the population between 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. In those cases that are noted, country and year dummy variables were included in regressions but were not reported above for presentation purposes. Pooled OLS regressions were estimated with panel corrected standard errors that correct for groupwise heteroskedasticity and contemporaneous correlations of the errors. Standard errors in parentheses and significance levels are as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

<sup>a</sup> Two lags of the dependent variable were used in the GMM difference and systems equations. The four specifications present GMM estimates using the Arellano-Bond (difference) and Blundell-Bond (system) procedures with orthogonal deviations to adjust for an unbalanced panel and collapsed to minimize the number of instruments following the recommendations of Roodman (2007). Per capita GDP and growth were also included as endogenous variables in the GMM estimations. For GMM estimates standard errors are reported as t-statistics based on Windmeijer (2005) finite sample correction and corrected for serial correlation and heteroskedasticity.

<sup>b</sup> The Arellano-Bond tests for first-order and second-order serial correlation are reported for all GMM models. The tests were carried out on the first-differenced residuals. The p-values are the probability of rejecting the null hypothesis of no autocorrelation.

**Table 5. Political Budget Cycles in Latin America, 1973-2000: The Effect of Elections and Post-Election Years on Consolidated and Transitional Democracies**

| <i>Dependent Variable:</i>                                       | (1)                | (2)   | (3)  | (4)                                     | (5)   | (6)                                 | (7)   |
|--|--------------------|---|--|---|---|-------------------------------------|---|
| <i>A. Social Spending/ Government Budget<sup>a</sup></i>         | Pooled OLS<br>PCSE | Pooled OLS PCSE<br>with country fixed<br>effects (f.e.) | Pooled OLS PCSE<br>with country and<br>year f.e. | GMM One-Step<br>First Diff <sup>a</sup> | GMM One-Step<br>First Diff with<br>year f.e. <sup>a</sup> | GMM One-Step<br>System <sup>a</sup> | GMM One-Step<br>System with year<br>f.e. <sup>a</sup> |
| Election Year <sub>t</sub>                                       | 0.983<br>(2.114)   | -0.469<br>(1.781)                                       | 0.008<br>(1.770)                                 | 0.921<br>(0.985)                        | 0.091<br>(1.264)  | 1.142<br>(1.259)                    | 1.191<br>(1.351)                                      |
| Election Year <sub>t+1</sub>                                     | 2.475*<br>(1.395)  | 1.212<br>(1.170)  | 1.668<br>(1.298)                                 | 1.499<br>(1.218)                        | 0.638<br>(1.033)  | 2.607**<br>(1.202)                  | 1.757<br>(1.083)                                      |
| Transitional Democracy Election Year <sub>t</sub>                | -0.469<br>(2.579)  | 0.907<br>(2.372)  | 0.764<br>(2.175)                                 | -2.133<br>(3.016)                       | 1.239<br>(3.763)  | -0.839<br>(2.179)                   | -1.313<br>(2.520)                                     |
| Transitional Democracy Election Year <sub>t+1</sub>              | 1.377<br>(2.197)   | 3.194<br>(2.064)  | 2.315<br>(1.823)                                 | 2.465<br>(3.354)                        | 3.132<br>(2.791)  | 1.848<br>(2.727)                    | 1.921<br>(2.433)                                      |
| Observations   | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared  | 0.79               | 0.85  | 0.86   |   |   |                                     |   |
| Number of Instruments  |                    |   |  | 16                                      | 41  | 21                                  | 46  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>( <i>p value</i> )  |                    |   |  | 0.355                                   | 0.445   | 0.835                               | 0.753   |
| Hansen test for joint validity of instruments ( <i>p value</i> ) |                    |   |  | 0.493                                   | 1.000   | 1.000                               | 1.000   |
| Diff. Sargan tests for all system instruments ( <i>p value</i> ) |                    |   |  |   |   | 0.734                               | 1.000   |
| Total Effect of an Election in a Transitional Democracy          | 0.908<br>(3.580)   | 4.101<br>(3.569)  | 0.191<br>(1.960)                                 | 0.331<br>(4.974)                        | 4.370<br>(5.577)  | 1.009<br>(3.611)                    | 0.607<br>(3.972)                                      |
| Total Effect of an Election in a Consolidated Democracy          | 3.457<br>(2.896)   | 0.743<br>(2.451)  | 3.143<br>(1.707)                                 | 2.420<br>(1.884)                        | 0.728<br>(1.974)  | 3.748<br>(1.690)                    | 2.948<br>(2.160)                                      |
| <b><i>B. Health and Education Spending/Government Budget</i></b> |                    |   |  |   |   |                                     |   |
| Election Year <sub>t</sub>                                       | 0.546<br>(1.225)   | -0.686<br>(1.126)                                       | -0.550<br>(1.091)                                | -0.306<br>(0.866)                       | 0.552<br>(1.349)  | 1.108<br>(0.881)                    | 1.079<br>(1.232)                                      |
| Election Year <sub>t+1</sub>                                     | 0.827<br>(1.220)   | -0.122<br>(1.099)                                       | 0.251<br>(1.065)                                 | 0.117<br>(1.352)                        | 0.316<br>(1.677)  | 2.114<br>(1.669)                    | 1.403<br>(2.227)                                      |
| Transitional Democracy Election Year <sub>t</sub>                | -0.481<br>(1.326)  | 1.056<br>(1.354)  | 1.139<br>(1.267)                                 | -0.087<br>(1.574)                       | 1.356<br>(2.736)  | -0.093<br>(1.624)                   | -0.705<br>(1.634)                                     |
| Transitional Democracy Election Year <sub>t+1</sub>              | -0.192<br>(1.338)  | 1.206<br>(1.307)  | 1.373<br>(1.229)                                 | 1.367<br>(2.048)                        | 0.888<br>(1.830)  | -0.935<br>(1.772)                   | 0.178<br>(2.351)                                      |
| Observations   | 209                | 209   | 209  | 199                                     | 199   | 203                                 | 203   |
| R-squared  | 0.85               | 0.88  | 0.90   |   |   |                                     |   |
| Number of Instruments  |                    |   |  | 16                                      | 41  | 19                                  | 46  |
| Arellano-Bond test for AR(2) <sup>b</sup><br>( <i>p value</i> )  |                    |   |  | 0.308                                   | 0.196   | 0.112                               | 0.235   |
| Hansen test for joint validity of instruments ( <i>p value</i> ) |                    |   |  | 0.494                                   | 1.000   | 0.999                               | 1.000   |
| Diff. Sargan tests for all system instruments ( <i>p value</i> ) |                    |   |  |   |   | 0.721                               | 1.000   |

**C. Social Security/Government Budget**

|  |                  |                   |                   |                   |                   |                   |                   |
|--|------------------|-------------------|-------------------|-------------------|-------------------|-------------------|-------------------|
| Election Year  | 0.440<br>(1.701) | 0.391<br>(1.490)  | 0.681<br>(1.376)  | 0.372<br>(1.859)  | 0.533<br>(1.898)  | 0.277<br>(2.023)  | 0.842<br>(1.075)  |
| Election Year <sub>t+1</sub>                                     | 1.706<br>(1.269) | 1.437<br>(1.141)  | 1.502<br>(1.109)  | 0.883<br>(0.674)  | 0.787<br>(1.867)  | 0.965<br>(0.705)  | 1.099<br>(0.812)  |
| Transitional Democracy Election Year                             | 0.042<br>(1.985) | -0.275<br>(1.875) | -0.429<br>(1.695) | -0.483<br>(2.788) | -0.599<br>(3.425) | -0.290<br>(2.513) | -1.041<br>(1.375) |
| Transitional Democracy Election Year <sub>t+1</sub>              | 1.506<br>(1.738) | 1.879<br>(1.690)  | 0.872<br>(1.482)  | 2.240<br>(2.087)  | 1.334<br>(3.551)  | 2.741<br>(2.294)  | 1.291<br>(1.739)  |
| Observations   | 209              | 209               | 209               | 199               | 199               | 203               | 203               |
| R-squared  | 0.90             | 0.92              | 0.94              |                   |                   |                   |                   |
| Number of Instruments  |                  |                   |                   | 16                | 41                | 21                | 46                |
| Arellano-Bond test for AR(2) <sup>b</sup><br>( <i>p value</i> )  |                  |                   |                   | 0.686             | 0.758             | 0.753             | 0.826             |
| Hansen test for joint validity of instruments ( <i>p value</i> ) |                  |                   |                   | 0.564             | 1.000             | 1.000             | 1.000             |
| Diff. Sargan tests for all system instruments ( <i>p value</i> ) |                  |                   |                   |                   |                   | 0.721             | 1.000             |
| Total Effect of an Election in a Transitional Democracy          | 1.547<br>(2.714) | 1.605<br>(2.832)  | 0.443<br>(2.488)  | 1.756<br>(3.428)  | 0.734<br>(6.556)  | 2.451<br>(3.467)  | 0.250<br>(2.177)  |
| Total Effect of an Election in a Consolidated Democracy          | 2.147<br>(2.382) | 1.828<br>(2.139)  | 2.183<br>(1.950)  | 1.254<br>(2.242)  | 1.320<br>(3.675)  | 1.241<br>(2.468)  | 1.941<br>(1.723)  |

Notes: The covariates include lags of the dependent variable, per-capita GDP, one lag of per capita GDP growth, the fraction of the population over age 65, the fraction of the population between the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. In those cases that are noted, country and year dummy variables were included in regressions but were not reported above for presentation purposes. Pooled OLS regressions were estimated with panel corrected standard errors that correct for groupwise heteroskedasticity and contemporaneous correlations of the errors. Standard errors in parentheses and significance levels are as follows: \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

<sup>a</sup> Two lags of the dependent variable were used in the GMM difference and systems equations. The four specifications present GMM estimates using the Arellano-Bond (difference) and Blundell-Bond (system) procedures with orthogonal deviations to adjust for an unbalanced panel and collapsed to minimize the number of instruments following the recommendations of Roodman (2007). Per capita GDP and growth were also included as endogenous variables in the GMM estimations. For GMM estimates standard errors are reported as t-statistics based on Windmeijer (2005) finite sample correction and corrected for serial correlation and heteroskedasticity.

<sup>b</sup> The Arellano-Bond tests for first-order and second-order serial correlation are reported for all GMM models. The tests were carried out on the first-differenced residuals. The p-values are the probability of rejecting the null hypothesis of no autocorrelation.

Due to the small number of elections in our sample for which there is available social spending data and the elevated number of dummy variables, however, the regression results presented in this table should be viewed with caution. These results, rather, are presented to as the natural corollary to the test that should be conducted given the findings thus far revealed from previous models. Table 5 suggests that democracies that have had at least two turnovers between opposition parties are more likely to increase social spending in the inaugural year of a newly elected president's administration. These results are only weakly confirmed in the case of aggregate social spending, however. The coefficient estimates in panel A predict that post-election year social spending will increase by 0.75 to 3.15 percent of the total budget for an established democracy. The magnitude of this change in budget allocation is not insignificant. Argentina spent more than 25.5 billion pesos on health, education and pension expenditures of its total budget of 46 billion pesos in 2000 (equivalent to roughly 15 percent of its GDP). If we use a midpoint estimate of 2 percent as an estimate of the increase in social spending allocations in the year following elections, this would be equivalent to nearly a 1 billion peso increase in social spending once democracy has been consolidated. The final rows for panels A, B and C presented the results of the estimated total effect of elections for consolidated and transitional democracies. Although not statistically significant, the signs and magnitudes of the coefficients suggest that the increase in post-election social spending is higher for consolidated democracies.

## **6. Conclusion**

There is a need for greater understanding of the differences and commonalities that Latin America has with other developing regions and advanced, industrialized democracies. This paper has undertaken a more theoretically grounded exploration of political cycles in fiscal policy performance for Latin America during the most profound and widespread period of democratization by examining what happens to social spending during elections, a category notable for its "visibility" to voters. It has attempted to address several gaps in past empirical research by considering what happens to

performance measures prior to and immediately after multi-party competitive elections in the transition period following authoritarian and after the consolidation of democracy.

Applying a battery of specifications, we have presented compelling evidence that questions past research on political budget cycles. Competitive elections do not appear to operate as catalysts for social spending in Latin America. Rather, this paper provides evidence to suggest that consolidated democracies are more apt to undertake increased social spending in a presidential administration's first year in office. In addition, we have shown that there are differences in the social policy response depending on whether a democracy is in a transition stage or has been consolidated. However, the divergence between more fragile democracies and their consolidated counterparts are not as predicted by Brender and Drazen (2005 and 2007). In the period following authoritarian rule and prior to two turnovers in power, transitional democracies are not more likely to engage in redistributive spending. One possible reason for this result might be that it takes time for new democracies to consolidate their power base and institute redistributive programs. In other words, the effect of new democracies may be cumulative versus immediate.

The results in this paper give support to theories that argue that the driving force of social policy responsiveness due to democratic turnovers is greater *after* elections. In this sense, our findings support the arguments of Franzese (2002b) for social spending detected for elections in industrialized democracies. However, we also show that there are important differences in Latin American democracies that contrast markedly with empirical evidence found in developed democracies, case study specific literature for particular Latin American democracies and theoretical predictions. Thus far, there has been limited exploration of models that study political welfare cycles in developing democracies. The findings in this study further stress the need for further research on the patterns driving the experiences of democracies in developing countries and those in Latin America in particular.

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