

FUNDAÇÃO GETÚLIO VARGAS
ESCOLA DE ADMINISTRAÇÃO DE EMPRESAS DE SÃO PAULO

LORENA GUADALUPE BARBERIA

DISTRIBUTIVE POLITICS IN LATIN AMERICA:

The Impact of Democracy, Elections and Globalization on the Welfare State

SÃO PAULO
2008

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LORENA GUADALUPE BARBERIA

DISTRIBUTIVE POLITICS IN LATIN AMERICA:

The Impact of Democracy, Elections and Globalization on the Welfare State

Tese apresentada à Escola de Administração de Empresas de São Paulo, como requisito para a obtenção do título de Doutor em Administração Pública e Governo.

Campo de conhecimento:
Finanças Públicas

Orientador: Prof. Dr. George Avelino Filho

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Resumo: A tese busca examinar dois desenvolvimentos de grandes conseqüências na América Latina nas últimas três décadas do século XX. Ela procura testar as teorias sobre políticas distributivas examinando os efeitos da democracia e da globalização no estado de bem-estar na América Latina utilizando dados de séries temporais para 15 países entre 1973 e 2000.

Palavras-Chave: América Latina, Políticas sociais, Democracia, Eleições, Globalização, Estado de bem-estar.

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Dedicatoria

Para mi mamá, Alba Elisa Agustina Balboni Barberia, quien me enseñó de nunca desistir, de nunca dejar de querer aprender y de hacer todo con mucho amor.

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ABSTRACT

This dissertation is focused on examining two developments of great consequence in Latin America in the three last decades of the 20th century. It tests theories of distributive politics by examining the effects of democracy and globalization on the welfare state in Latin America. The study emphasizes that the politics of resource allocation decisions are best understood by measures of social spending relative to the national budget instead of GDP. Using time-series data for 15 nations between 1973 and 2000, it examines how three key political factors influence the responsiveness of the welfare state in Latin America: a) the democratic character of political institutions; b) the electoral institutions that channel voter preferences to bring in to power new democratic governments with a mandate; and, c) the degree of integration of states into the global economy. Based on a battery of specifications, the study shows that democracies allocate greater shares of their budget to public health and education and reduce regressive pension benefits. It demonstrates, however, that expenditures begin to favor more entrenched power groups after the period of democratic transition ends and democracy is consolidated. It shows that social policies in Latin America are used as an instrument to reward voters and not a tool to manipulate the outcome of elections. The dissertation also provides evidence that more open economies in Latin America seek to compensate citizens by increasing pensions, but that globalization has not triggered a similar increase of investments in health and education.

Keywords: Latin America, Social Policy, Democracy, Elections, Globalization, Welfare state.

RESUMO

A tese busca examinar dois desenvolvimentos de grandes conseqüências na América Latina nas últimas três décadas do século XX. Ela procura testar as teorias políticas distributivas por meio do exame dos efeitos da democracia e da globalização no estado de bem-estar na América Latina. O estudo enfatiza que as decisões políticas sobre alocação de recursos são mais bem compreendidas por medidas relativas ao gasto no orçamento nacional em vez dos contabilizados no PIB. Utilizando dados de séries temporais de 15 países entre 1973 e 2000 estuda três fatores políticos chaves que influenciam as respostas dos estados de bem-estar na América Latina: a) o caráter democrático das instituições políticas; b) as instituições eleitorais que canalizam as preferências eleitorais conduzindo ao poder novos governos democráticos; e, c) o grau de integração dos países na economia global. Baseado numa bateria de especificações, o estudo mostra que as democracias alocam uma parcela maior de seus orçamentos para a saúde pública e educação e reduzem benefícios previdenciários regressivos. Demonstra que os gastos começam a favorecer grupos de poder mais estabelecidos depois do termino das transições democráticas e quando a democracia está consolidada. Mostra que políticas sócias na America Latina são usadas para remunerar eleitores e não como uma ferramenta de manipulação das eleições. A tese também evidencia que as economias latino-americanas mais abertas procuram compensar seus cidadãos por meio do aumento das pensões, mas que a globalização não produz um semelhante aumento dos investimentos em educação e saúde.

Palavras-chave: América Latina; Políticas sociais; Democracia; Globalização, Estado de bem-estar.

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Resumo da Tese

Esta tese está centrada no exame de dois desenvolvimentos de grandes conseqüências na América Latina ao longo das últimas três décadas do século XX. Ela busca testar teorias de políticas distributivas examinando os efeitos da democracia e da globalização no bem-estar na América Latina. Na literatura de economia política e de ciência política comparada, tem sido dada atenção considerável ao exame de se a democratização que se espalhou pela América Latina nas décadas recentes instalaria governos mais atentos ao bem-estar de seus cidadãos. Na medida em que as economias latino-americanas instituíram uma dramática liberalização de seu comércio internacional e de seus mercados financeiros emergiu também forte interesse na compreensão das respostas dos governos às demandas crescentes por uma melhor distribuição de renda e por uma maior proteção social das populações vulneráveis. Este interesse é especialmente elevado dado que as forças de integração dos mercados nacionais na economia mundial em um contexto de maior volatilidade financeira e fraco crescimento econômico têm limitado a amplitude de respostas dos governos às necessidades de proteção de seus cidadãos.

Existem três importantes debates. O primeiro concentra-se em saber se as democracias, comparativamente aos regimes autoritários, são mais suscetíveis de investir recursos em função da presença de instituições que forçam a representação das demandas e interesses sociais. O segundo debate concentra suas atenções em examinar se as campanhas eleitorais na América Latina agem como catalisadores temporários ou se os eleitos implementam crescentes compromissos de bem-estar sustentáveis no longo prazo. Um terceiro debate procura saber se a crescente integração comercial e financeira ao mercado global tem reduzido a capacidade de resposta da América Latina em estender e sustentar a proteção social de seus cidadãos. Esta tese procura clarear esses três debates baseada na análise sistemática de se instituições políticas impactam os compromissos governamentais de bem-estar.

Até este momento o consenso sobre como as políticas impactam os gastos com bem-estar na América Latina ainda não foi alcançado. Estudos quantitativos têm produzido conclusões contraditórias e dessa forma mesmo as pesquisas que consideram um número pequeno de questões permanecem inconclusivas. Os estudos não têm conseguido alcançar consenso em questões fundamentais e importantes fatores políticos, centrais no período sendo estudado, não foram ainda examinados. Entre os fatores não estudados estão incluídas

diferenças que podem existir entre os períodos de transição de uma regime autoritário para uma democracia e após sua consolidação. A não consideração desses fatores pode introduzir um viés importante e distorcer as hipóteses das pesquisas passadas.

Por outro lado, os estudos têm sofrido uma variedade de problemas em suas conclusões estatísticas relacionados à medição dos fatores explicativos: o impacto das propriedades dinâmicas e variações entre países. Democracia, democratização e eleições são as variáveis-chave independentes de interesse nesta tese. Frequentemente medidas mais rigorosas e teoricamente fundamentadas destes conceitos têm sido pouco exploradas na literatura. Além disso, existem consideráveis razões para se preocupar com o fato dos gastos governamentais poderem ser séries de tempo altamente persistentes. No entanto, os estudos raramente reportam os resultados dos testes econométricos de raiz unitária ou as medidas tomadas que consideram este problema. Eles também frequentemente agrupam um conjunto de países latino-americanos sem uma discussão das variações *dentro* e *entre* os mesmos.

Neste estudo abordam-se tais problemas num prisma metodológico que considera a medição e as preocupações com as especificações. Esta tese procura remediar estes problemas adotando uma metodologia que se preocupa com as medições e as especificações utilizadas para testar hipóteses. Utilizando uma base de dados de uma série temporal de 28 anos para 15 países, se analisa os impactos no bem-estar social da democracia e da globalização no contexto dos modelos que consideram as necessidades de bem-estar e a estrutura econômica na América Latina. Entretanto, os resultados das *time-series-cross-section* (TSCS) relatados nesta tese não se baseiam no modelo utilizado para a sua obtenção. Ao invés disso, as hipóteses serão testadas com uma bateria de modelos diferentes e os resultados serão relatados com a atenção voltada para a significância estatística dos mesmos e sua consistência ao longo de várias especificações.

Visando estabelecer o contexto para esta discussão, é importante rapidamente revisar a expansão do estado do bem-estar na América Latina. Os programas de bem-estar na América Latina, que surgem na maioria dos países depois de 1930, foram elaborados para atender segmentos específicos das elites do aparato burocrático dos estados, nomeadamente os militares, os juizes e os servidores públicos. A cobertura ao setor formal, aos trabalhadores da classe média, somente foi estendida de forma gradual (MESA-LAGO, 1989). Na medida em que sua consolidação era alcançada entre os anos de 1950 a 1970 os governos começaram a

enfrentar novos desafios (ABEL; LEWIS, 2002). A política de saúde começou a mudar seu foco de saneamento público, higiene e campanhas preventivas contra doenças infecciosas para a construção de um sistema de saúde mais completo, com foco em cuidados primários e atenção hospitalar de alta complexidade. Sistemas de educação que haviam expandido o ensino primário voltaram-se à expansão do ensino secundário e superior. Os países passaram a se envolver também com as mudanças necessárias para a construção de sistemas de seguro social compreensivos, uma vez que os modelos bismarkianos baseados na extensão da cobertura para força-de-trabalho assalariada provaram ser inadequados para a inclusão de trabalhadores informais e os empregados por conta própria (MESA-LAGO, 1989).

A crise da dívida externa dos anos de 1980 e seu legado de inflação, déficits fiscais e instabilidade cambial foram os vetores do impulso para que vários países latino-americanos se tornassem a primeira geração de economias em desenvolvimento a adotar reformas liberalizantes. Governos latino-americanos adotaram reformas abrangentes para dar arranque ao crescimento econômico. O pacote de reformas incluía a liberalização das importações, a liberalização do mercado financeiro doméstico, as privatizações e reformas no mercado de trabalho.¹ Estas mudanças políticas prometiam mais emprego e crescimento econômico. Em muitos países latino-americanos, a “primeira fase” das reformas macroeconômicas foi seguida por uma “segunda fase” de reformas das políticas sociais, que deram início a transformações voltadas à busca de uma maior eficiência econômica e maior eficiência de seus sistemas de bem-estar social (KAUFMAN; NELSON, 2004).

Contrariamente às expectativas, a década de 1990 foi marcada por fraco crescimento econômico, desemprego, instabilidade macroeconômica, expansão do emprego informal e aumento da desigualdade (STALLINGS; WELLER, 2001). Após períodos de regras autocráticas que predominaram na formulação de políticas em vários países, muitos foram surpreendidos em ver novas e frágeis democracias implementarem crescentes reformas neoliberais impopulares. A perplexidade cresceu quando os regimes persistiram com a continuidade das reformas apesar de seus fracos desempenhos. O relativo sucesso que alguns governos obtiveram com as reformas macroeconômicas pode em parte ser atribuído ao alcance das medidas compensatórias de proteção social. Apesar dos programas sociais terem sido impactados de forma adversa devido à contínua carência de recursos e os choques

¹ O Chile é o único país que implementou as reformas uma década mais cedo, durante a década de 70.

econômicos recorrentes, o esforço fiscal direcionado aos gastos sociais cresceu dramaticamente nos anos de 1990. Este padrão se alinha com os modelos de políticas que sugerem que os governos democráticos da região estariam mais preocupados com a alocação dos gastos sociais na direção dos eleitores de baixa renda e da classe média, quando do retorno às democracias.

No entanto, um exame dos gastos sociais melhor e mais aprofundado revela que podem ser necessárias explicações mais complexas. Se o orçamento médio de bem-estar é desagregado, confirma-se um aumento acentuado da parcela dos recursos governamentais direcionado à saúde e à educação. É claro, porém, que este resultado é muito menos significativo que o crescimento exponencial dos recursos alocados para a seguridade social ao longo das últimas três décadas do século XX. A preocupação central desta tese, portanto, é compreender como a onda de democratização e abertura comercial e financeira impactou o esforço fiscal dos estados de bem-estar latino-americanos.

O estudo verifica os impactos das políticas democráticas e da globalização no bem-estar social no contexto dos modelos que consideram as necessidades de bem-estar e as estruturas econômicas na América Latina utilizando dados de séries temporais para 15 países entre 1973 e 2000. A análise baseia-se no exame dos determinantes políticos ligados ao desempenho do bem-estar para: Argentina, Bolívia, Brasil, Chile, Costa Rica, República Dominicana, Equador, El Salvador, Guatemala, México, Panamá, Paraguai, Peru, Uruguai e Venezuela. Embora cinco países tenham sido omitidos devido à falta ou a não comparabilidade dos dados, a amostra representa uma considerável cobertura de região. Os países da América Latina incluídos neste estudo representam mais de 90% da população e mais de 90% do PIB regional.

A tese está organizada em 7 capítulos, que incluem esta introdução. Os Capítulos 2 e 3 servem de fundamentação para a análise empírica que será desenvolvida nos próximos três capítulos via introdução dos dados e da codificação das variáveis políticas compilados para esta tese e servem também para resumir a metodologia de pesquisa, o modelo básico e as especificações que serão testadas nos Capítulos 4, 5 e 6. Um capítulo final apresenta as conclusões da tese. Nos parágrafos seguintes, estão expostos os objetivos-chave relacionados a esses capítulos.

O Capítulo 2 descreve a base de dados dos gastos governamentais que são utilizados na análise empírica desta tese e a codificação das variáveis democracia e eleições para a América Latina. O capítulo detalha os critérios utilizados para codificar os dados sobre democracia, transições democráticas e eleições. A codificação das variáveis democracia e eleições estão baseadas em sólida fundamentação teórica encontrada na literatura especializada.

A análise empírica da tese está baseada em uma TSCS. O Capítulo 3 foca dois assuntos-chave das TSCS, que foram testados para selecionar a metodologia dos capítulos empíricos. O primeiro e talvez mais importante problema diz respeito aos desafios colocados pela heterogeneidade das unidades ou pelos efeitos fixos dos países quando da análise dos dados das TSCS. O segundo e mais complexo problema está relacionado às estimativas de modelos onde os dados não são estacionários ou seguem uma raiz unitária. Baseado no resultado desta análise o estudo conclui que uma bateria de especificações é uma metodologia mais adequada para o trabalho empírico apresentado nos capítulos subsequentes. O capítulo resume o custo-benefício e os desafios colocados pelas diferentes especificações do modelo e como elas influenciam as inferências realizadas nos demais capítulos.

Nos Capítulos de 4, 5 e 6, a tese procura testar três fatores políticos que influenciaram a capacidade de resposta de bem-estar dos países latino-americanos nas últimas três décadas do século XX:

- 1) O caráter democrático das instituições políticas;
- 2) As instituições eleitorais que canalizam as preferências dos eleitores em conduzir ao poder novos governos democráticos com um mandato; e,
- 3) O grau de integração dos estados na economia global.

O primeiro fator-chave testado no Capítulo 4 é a democracia. A tese argumenta que as democracias podem ser caracterizadas por meio de uma distribuição mais equitativa do poder político e estes tipos de regimes, quando comparados aos regimes autoritários, são mais aptos a redistribuir a renda. Assim, o capítulo investiga se os gastos públicos em saúde e educação crescem em governos democráticos na América Latina. Ele examina também se governos vitoriosos em eleições competitivas são mais aptos a diminuir a regressividade dos benefícios de seguridade social favorecendo grupos minoritários de eleitores. Outro teste verifica se o impacto da democracia persiste para além do período de transição. Dessa forma, o capítulo

explora se compromissos com o bem-estar são sustentados nas democracias consolidadas onde o poder foi alternado entre partidos de oposição por pelo menos duas vezes.

O segundo fator-chave refere-se à realização de eleições, constituindo o foco do Capítulo 5. Eleições é a mais básica e direta medida de democracia. Esse capítulo testa se o impacto da democracia resulta em um aumento temporário dos compromissos de bem-estar antes das eleições, como também investiga se os resultados eleitorais conduzem a mudanças na alocação de recursos no primeiro ano de mandato de um novo governo. Fortes margens eleitorais possibilitam aos governos mandatos mais sólidos. Entretanto, maior investigação é conduzida para confirmar se os compromissos governamentais com o bem-estar são impactados pelo tamanho da margem de votos entre os dois candidatos que lideraram o processo eleitoral. O capítulo conclui com um teste do desempenho fiscal e social em eleições nos períodos de transição democrática e em períodos posteriores à consolidação da democracia.

A preocupação central do Capítulo 6 é o estudo do terceiro fator que se concentra na análise do impacto da integração econômica e financeira no gasto social. Este estudo argumenta que a abertura da América Latina para a economia global tem imposto pressões contrabalanceadas em seus governos nacionais. De um lado, os governos são pressionados a aumentar a eficiência de suas economias e fazem isto por meio de reformas que reduzem os custos do trabalho. Por outro lado, eles buscam também ampliar a rede de proteção social para proteger seus cidadãos dos choques adversos da abertura de mercados. Assim, o que é testado nesse capítulo refere-se a como o aumento da abertura comercial e da mobilidade do capital impactam a dinâmica política que envolve os compromissos com o bem-estar.

No conjunto, as conclusões dos capítulos acima buscam integrar cinco linhas de investigação: os estudos de bem-estar nos países em desenvolvimento; os estudos sobre democracia na América Latina; os estudos das eleições; os estudos da integração global e os estudos de metodologia na ciência política. No capítulo final da tese as conclusões são agrupadas para mostrar como a democracia e a globalização moldam os compromissos do estado de bem-estar na América Latina. O trabalho como um todo evidencia que o estudo dos determinantes políticos da dinâmica do estado de bem-estar na América Latina é um campo fértil de investigação para os estudiosos da economia política e postula que muitas das conclusões de trabalhos anteriores necessitam ser reexaminadas sob o enfoque metodológico

desenvolvido nesta tese. Muitas das conclusões desta tese complicam a percepção dominante sobre as políticas redistributivas na América Latina.

1. Introduction

This dissertation is focused on examining two developments of great consequence in Latin America in the last three decades of the 20th century. It seeks to provide a test for theories of distributive politics by examining the effects of democracy and globalization on the welfare state in Latin America. Within the comparative political science and political economy literatures considerable attention has been paid to examining whether the wave of democratization that has spread across Latin America in recent decades would install governments more attentive to the welfare of their citizens. As Latin American economies have instituted a dramatic liberalization of their trade and financial markets, strong interest has also emerged in understanding the response of governments to demands for greater income redistribution and for greater social protection of vulnerable populations. This interest is especially great as the very forces of market integration with world markets have also limited the extent to which governments have been able to respond to shield citizens from greater economic volatility and lackluster economic growth.

Three important debates exist. The first debate centers on whether democracies are more likely to invest resources in distributive programs as compared to authoritarian regimes due to the presence of institutions that force representation of societal demands and interests. The second debate concentrates its attention on examining on whether electoral competitions act as a temporary fiscal catalyst in Latin America or if elected officials implement longer-term, sustained increases in welfare commitments. A third debate focuses on whether increased trade and financial integration with global markets has reduced the responsiveness of Latin American governments to spending that extends and sustains social protection. This dissertation seeks to shed light on all three debates based on a systemic analysis of whether political institutions and globalizations impact government welfare commitments.

To date, consensus on how politics matters for welfare spending in Latin America has not been reached. Quantitative studies have produced contradictory findings and thus research on even narrow sets of questions remains inconclusive. Not only have studies failed to reach consensus on fundamental questions, important political factors that are central to the period being studied have yet to be explored. These include research directed at examining the important differences that may exist between the transition period in a democracy following

authoritarian rule and once the regime has been consolidated. The failure to account for these factors may introduce significant bias and distort the hypothesis testing carried out in past research.

In turn, studies have suffered from a variety of problems related to the measurement of explanatory factors and the failure to explore the impact of dynamics and cross-section variation on statistical findings. Democracy, democratization and elections are the key independent variables of interest in this dissertation. Oftentimes, more rigorous and theoretically grounded measures of these concepts have been neglected in the literature. In addition, there are reasons to be concerned that data on government expenditures may be highly persistent. However, studies rarely report the results of unit root tests or the measures taken to address this problem. Studies also often pool a group of Latin American countries without discussion of *within* versus *across* country variation.

This dissertation seeks to remedy these problems by undertaking a methodological approach that addresses both measurement and specification concerns. Using a 28-year time-series data for 15 nations, the study assesses social welfare impacts of democratic politics and globalization in the context of models that account for the impact of welfare needs, economic capabilities and economic structure in Latin America. Moreover, the time-series-cross-section (TSCS) findings reported in this dissertation do not rely on the model used to obtain them. Rather, the hypothesis will be tested with a battery of different models and results will be reported with attention to whether statistically significant outcomes are consistent across several specifications.

In order to set the stage for this discussion, it is helpful to briefly review the expansion of the welfare state in Latin America. Surging in most countries after 1930, welfare programs in Latin America were designed to cover specific elite segments of the state's bureaucratic apparatus, namely the military, the judiciary and civil servants. Coverage to the formal sector, middle class workers was extended only gradually (Mesa-Lago 1989). As the consolidation of the expansion stage was completed in the 1950s to 1970s, governments began to confront new challenges (Abel and Lewis 2002). Health policy began to shift from focusing on public sanitation, hygiene and prevention campaigns against infectious disease to the building of a more complete public health care systems with emphasis on primary and tertiary care. Education systems that had expanded primary schooling now began to target the expansion of

secondary and university education. Countries also began to grapple with the changes required to build comprehensive social insurance systems as Bismarckian models based on the extension of coverage for the salaried labor force proved inadequate for addressing the issues of inclusion of self-employed and informal workers (Mesa-Lago 1989).

The external debt crisis and its legacy of inflation, fiscal deficits, exchange rate instability and debt servicing problems propelled several democratizing Latin America countries to be the first generation of developing economies to adopt market opening reforms. In the mid to late 1980s and 1990s, Latin American governments adopted comprehensive and unprecedented reforms to ignite economic growth. The reform package included import liberalization, domestic financial liberalization, privatizations and labor reforms.² These policy changes promised higher growth and employment rates. The “first-phase” of macroeconomic reforms was followed by a “second-phase” of social policy reforms in many Latin American countries that was initiated transformations to bring about greater efficiency and efficiency of their welfare states (Kaufman and Nelson 2004).

Contrary to expectations, the 1990s was marked by lackluster growth, macroeconomic instability, unemployment, increases in informal employment and increased inequality (Stallings and Weller 2001). After the long periods of autocratic rule that had dominated policymaking in many countries, many were surprised to see new, fragile democracies implementing increasingly unpopular neoliberal reforms. The perplexity only increased when regimes persisted in continuing with unpopular policies despite poor performance records. The relative success that some governments had in implementing macroeconomic reforms may be due in part to their adoption of countervailing social protection measures. Although welfare programs were adversely impacted by continued resource constraints and reoccurring economic shocks, the fiscal effort directed at social spending increased dramatically during the 1990s. This pattern aligns with theoretical models of politics that suggest that democratic governments in the region would be more responsive to reallocating social expenditures towards lower and middle class voters with the return to democracy.

Yet, a more in depth examination of social expenditures reveals that more complex explanations may be necessary. If the average welfare budget is disaggregated, a marked

² Chile is the only country that implemented reforms a decade earlier in the 1970s.

increased in the share of government resources directed to health and education is confirmed, but it is equally clear that this result is much less marked than the exponential increase in social security allocations over the course of the last three decades of the 20th century. Understanding how the democratization wave and the opening to trade and financial world markets impacted the fiscal effort of Latin American welfare states is the central preoccupation of this dissertation.

The study assesses social welfare impacts of democratic politics and globalization in the context of models that account for welfare needs, economic capabilities and structure in Latin America using time-series data for 15 nations between 1973 and 2000. The analysis of this dissertation is based on an examination of the political determinants of welfare performance for fifteen Latin American countries: Argentina, Bolivia, Brazil, Chile, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Mexico, Paraguay, Panama, Peru, Uruguay, and Venezuela. Though six nations of the region are omitted due to missing or noncomparable data, this represents a quite comprehensive coverage of the region. The Latin American countries included in this study account for more than 90 percent of the population and 90 percent of GDP of the region.

The dissertation is comprised of 7 chapters including this introduction. Chapters 2 and 3 serve as the foundation for the empirical analysis that will then be carried out in the next three chapters by introducing the dataset and codebook that were compiled for this dissertation and summarizing the research methodology, the baseline model and the specifications that will be tested in chapters 4, 5 and 6. A final chapter concludes the dissertation. In the paragraphs that follow, a brief overview of the key objectives of each chapter is presented in further detail.

Chapter 2 describes the database on government fiscal and social expenditures and a codebook for democracy and elections for Latin America that were compiled and will be used for the empirical analysis in the dissertation. It describes in detail the dataset and the criteria that were used to code the data on democracy, democratic transitions, and elections. The democracy and elections codebook for Latin America is based on solid theoretical foundations in the specialized literature.

The empirical analysis in this dissertation is based on a time-series-cross-sectional (TSCS) dataset. Chapter 3 focuses on two key issues for TSCS that were tested to select the research methodology for the empirical chapters of this dissertation. The first and perhaps more straightforward issue concerns the challenges posed by unit heterogeneity, or the time-invariant differences across countries, in time series-cross section data analysis. The second and more complex issue concerns the problems of estimations of data that are stationary or follow a unit root. Based on the results of this analysis, the chapter concludes that a battery of specifications is a preferred methodology for the empirical work presented in the next three chapters. The chapter also summarizes the tradeoffs and challenges posed by different model specifications and how these influence the inferences drawn in subsequent chapters.

In Chapters 4, 5 and 6, the dissertation seeks to test how three key political factors influence the responsiveness of the welfare state in Latin America in the last three decades of the 20th century:

- 1) The democratic character of political institutions;
- 2) The electoral institutions that channel voter preferences to bring in to power new democratic governments with a mandate; and,
- 3) The degree of integration of states into the global economy.

The first key factor that will be tested is democracy in Chapter 4. This study argues that democracies can be characterized by more equitable distributions of political power and as a result these types of regimes are more apt to redistribute income when compared to authoritarian regimes. Thus, it investigates if public education and health spending rise in democratic governments in Latin America. The chapter also examines if governments elected in competitive elections are more apt to decrease regressive pension benefits favoring smaller groups of voters. A further test of the impact of democracy is that the effect should persist beyond the transitional democratic period following military rule. Thus, the chapter further explores whether welfare commitments are sustained in the period after the first two turnovers of power to competing opposition parties.

The second key factor is elections and it constitutes the focus of Chapter 5. Elections are the most basic and direct measure of democracy. This chapter tests if the impact of democracy results in a temporary increase in welfare commitments before elections and also

investigates if the ballot box works to bring about changes in resource allocations in a presidential administration's inaugural year in office. Finding that stronger electoral margins give governments more solidly backed mandates, the study further investigates if the commitments of government's to welfare are impacted by the size of the margin between the two leading candidates. The chapter concludes with a test on the differences in fiscal and social performance between elections in the transition democratic period and the period after democracy is consolidated.

The central preoccupation of Chapter 6 is the third factor, which studies the extent of an economy's integration with world trade and financial markets. This study argues that the opening of Latin America to the global economy has placed countervailing pressures on governments. On the one hand, governments are driven to bring about greater efficiency in their economies and do so by implementing reforms to reduce labor costs. On the other hand, governments also seek to strengthen safety net to shield citizens from the adverse shocks of market opening. Thus, this chapter tests whether increased trade openness and capital mobility impact the political dynamics surrounding welfare commitments.

Together, the findings from these chapters seek to integrate five often distinct strands of literature: the study of the welfare state in developing countries, the study of democracy in Latin America, the study of elections, the study of global integration, and the study of political methodology. In the concluding chapter of this dissertation, the findings are brought together to show how democracy and globalization shape the commitments of the welfare state in Latin America. The conclusion notes that the study of the political determinants of welfare state dynamics in Latin America holds great promise for scholars of political economy and posits that many prevailing findings need to be reexamined in light of the methodological considerations developed in this dissertation. Many of the findings reported complicate the dominant wisdom of redistributive Latin American politics.

2. A Database on Government Fiscal and Social Expenditures and a Codebook on Democracy and Elections for Latin America

2.1 Introduction

Given that the purpose of this study is to examine the effect of national political dynamics on national level spending, it was necessary to compile a database for the sample of 15 countries in Latin America and the Caribbean. This chapter describes the database on government fiscal and social expenditures compiled that will be used for the empirical analysis in the dissertation. This is the data that will be employed as the dependent variable in the analysis.

A codebook for democracy and elections for Latin America was also put together and was then used to code the variables that will be used for democracy, democratic transitions, and elections. These variables represent the key independent variables of the dissertation. The variables and the definitions that were adopted are only briefly introduced in this chapter.

The sample is comprised of Argentina, Bolivia, Brazil, Chile, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Mexico, Paraguay, Panama, Peru, Uruguay, and Venezuela. Though six nations of the region are omitted due to missing or noncomparable data, this represents a quite comprehensive coverage of the region. The Latin American countries included in this study account for more than 90 percent of the population and 90 percent of GDP of the region.

2.2 Government Fiscal and Social Expenditures

The dependent variable in the empirical chapters are all based on annual data on the central government total expenditures and expenditures by welfare function as reported in the International Monetary Fund (IMF)'s *Government Finance Statistics* (GFS) (International Monetary Fund 2006) using both printed versions and CD versions of the data. A number of discrepancies were found between hard copy yearbooks and the electronic data reported in

CD-rom.³ Appendix 2A provides a summary of the data set that was assembled and the inconsistencies that were identified. This is the source used in the majority of studies that analyze the impact of political determinants on welfare expenditures. There are two exceptions. Brown and Hunter (Brown and Hunter 1999) and Avelino, Brown and Hunter (2005) use social spending data reported by the U.N. Economic Commission for Latin America and the Caribbean (ECLAC).⁴

Yearly reports of total central government revenues for each country in the sample were also compiled. Data of the share of expenditures allocated to education,⁵ health care⁶ and social security⁷ relative to total government spending were calculated for each year between 1973 and 2000.⁸ An aggregate measure of all three expenditures is used to measure welfare effort. Social spending is also grouped by expenditures on health and education combined and the share spent on social security transfers.

Using the same data source from the International Monetary Fund, Kaufman and Segura (2001) report social spending data for 1973-1997 and Drazen (2005) reports fiscal data for the same group of countries. In both cases, these datasets were more complete than the raw data that was assembled from past IMF GFS reports.⁹ In order to ease comparability of this dissertation with the findings of several studies that use these same IMF GFS data, the final estimations reported in this dissertation use an updated version of the Kaufman and Segura and the original Drazen datasets. The dataset compiled by Drazen for fiscal data is used in its original form and no additional changes bear mention.

There are four differences in the dataset employed in this dissertation with respect to social spending as compared to the original series employed by Kaufman and Segura

³ For a discussion on the advantages and disadvantages of GFS data for Latin America and the problems in electronic versus print form of the data, see Gavin and Perotti (1997).

⁴ Both ECLAC and IMF data report figures as reported by governments to these bodies. The difference is that each measure follows a different methodology to calculate the level of expenditures allocated for social expenditures.

⁵ 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).

⁶ 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).

⁷ 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).

⁸ Housing expenditures were not included as part of social spending. These expenditures are typically small.

⁹ Appendix 2A describes the problems that were identified with the IMF GFS data on a country-by-country basis and the coverage of the data for each country.

(Kaufman and Segura-Ubiergo 2001) and Segura (Segura-Ubiergo 2007). First, the dataset used in this study includes three additional years of expenditures on education, health and social security. 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 2000.¹⁰ The original dataset was updated by adding collected data from IMF GFS yearbooks for the period between 1998 and 2000 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.

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

¹⁰ 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.

from 6.18% of the total budget to 27.3%. For both countries, the data are reported in the original form.

Table 2.1 presents a summary of the numbers of years available for the data on total government and social spending on a per country basis and for the total sample. The dataset comprises a maximum of 420 observations for 15 Latin American countries from 1973 to 2000 (15 countries by 28 years). The panel is unbalanced due to a large number of missing observations. Although the sample period is 1973 to 2000, data for many countries cover shorter periods. Although the total number of observations is less for government spending, this data suffers from fewer interruptions than the social spending data in which there are significant number of gaps.

Table 2.1 Summary of Government Finance Statistics for Latin America Dataset, 1973-2000

Country	Fiscal Data			Welfare Expenditures		
	Start Date	End Date	# of Years	Start Date	End Date	# of Years
Argentina	1983	2000	18	1973	2000	28
Bolivia	1974-1981	1985-2000	24	1973-1982,	1986-2000	26
Brazil	1985	1998	12	1973-1994	1997-1998	24
Chile	1973	1999	27	1973	2000	28
Costa Rica	1973	2000	28	1973	2000	28
Dominican Republic	1973	2000	28	1973	2000	28
Ecuador	1973	2000	28	1973	1994	22
El Salvador	1973	2000	28	1974	2000	27
Guatemala	1973	2000	28	1973-1997	2000	26
Mexico	1980	2000	21	1973	2000	28
Panama	1973	2000	28	1973	2000	28
Paraguay	1989	2000	12	1973-1987	1989-1993	20
Peru	1986	2000	15	1973-1986	1999-2000	16
Uruguay	1973	2000	28	1973	2000	28
Venezuela	1979	2000	28	1973-1986	1999-2000	16
Total Number of Observations			353			373

The data offer several advantages.¹¹ First, the data follow a similar accounting methodology prescribed by the IMF and thus are comparable across countries. Second, expenditures are detailed at a high level of disaggregation and therefore allow a closer look at government preferences over specific types of welfare expenditures. Third, the data on shares of fiscal expenditures on welfare and disaggregated by function (education, health and social security) permit an analysis of the relative importance of each function vis-à-vis other services or goods that the government might choose to purchase.

¹¹ The implicit assumption in these comparisons is that spending is a good proxy for social welfare protection. Data on the variation in the scope and quality of coverage social welfare protection for Latin American countries are not available as continuous series for the same period (1973-2000). If data were available, a further analysis could be carried out to examine the changes in social spending analyzed in this chapter for different types of welfare protection schemes.

2.3 A Codebook for Democracy and Elections

This dissertation departs from the premise that democratization is a clear-cut process and its effects are best measured by a dichotomous variable that codes one for democracies and zero for the residual category of authoritarian regimes. This measure is based on a minimalist definition of democracy and builds on the seminal work of Schumpeter (1942) and more recently Sartori (1987). A democratic regime is defined as one in which the executive and the legislature are both filled by “contested elections.” The data employed draws on an updated version of the Álvarez, Cheibub, Limongi and Przeworski (1996) and Przeworski, Álvarez, Cheibub and Limongi (2000) datasets by Cheibub and Gandhi (2004) and a modification of these datasets by Avelino (2006) for Latin America from 1980-2000. The description of democratic and authoritarian regimes in Nohlen (2005) served as a key reference to cross-examine the dataset. For the full classification of the entire data set, see Appendix 2B.

Drawing upon on the classification of democratic and authoritarian regimes in Nohlen (2005), this study adopts coding that differs from Cheibub and Gandhi (2004) for specific periods in 7 countries. First, Bolivia was recoded as authoritarian in 1978 as a *coup d'état* took place on July 21st and thus Bolivia is not considered to have returned to democracy until 1983.¹² Under Ernesto Beckmann Geisel and João Baptista de Oliveira Figueiredo, Brazil was recoded as non-democratic between 1978 and 1984. According to Nohlen (2005), in the Dominican Republic the main opposition party did not participate in elections in 1970 or 1974 and the first free and competitive elections were held in 1978. Therefore, the period between 1970 and 1977 was recoded as authoritarian.

In the case of Ecuador, the period from 2000 to 2002 is not considered authoritarian as power was transferred to the Vice-President and thus succession followed constitutional law. As Nohlen (2005) considers Marco Vinicio Cerezo Arévalo the first constitutionally elected president in 20 years, Guatemala was recoded as authoritarian from 1970 to 1981. Panama is not considered to have returned to democracy until the inauguration of Ernesto Pérez Balladares' term in 1994, the years between 1989 and 1993 were recoded as authoritarian. Cheibub and Gandhi (2004) consider Paraguay as autocratic during the entire decade of the

¹² For a detailed analysis, see Nohlen (2005) pages 125-6.

1990s. Following Nohlen (2005), Paraguay was recoded as a democracy since Juan Carlos Wasmosy's term inauguration in 1993 to 2000.

A further disaggregation of democratic regimes by whether they are stable or transition democracies also measured in dichotomous terms is employed to examine the impact of new democracies on welfare commitment. Following the codification employed by Avelino (2005) to classify transitional regimes in Latin America, the second measure of democratization tries to capture the effects of the different stages of democracy by drawing a distinction between “transitional” and “established” democratic governments.¹³ The beginning of democratic transition is defined as the year of the inauguration of the first democratic regime following a period of authoritarian rule. 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. The rationale for adopting this criterion is based on the view that the alternation in power within democracy is theoretically closer to the definition of democracy, which stresses that democracy is a regime in which incumbents lose power through elections and willingly relinquish power to the winner.

Table 2.2 summarizes the 15 countries and the respective years in which authoritarian, democratic transitions and stable democratic regimes existed between 1973 and 2000. Democracies overall predominate, but the number of authoritarian years is approximately 42.6 percent of the sample. Nearly all countries were ruled by authoritarian regimes in the 1970s and the average authoritarian regime years in the sample is 10.3 years. Of the total 241 years of democracy, 53.1 percent are “new” democracy years, which had an average duration of 9.85 years. The average number of established years of democracy satisfying the two turnover test is 9.4 years.

¹³ Avelino (2005) adopts a definition of the beginning of an authoritarian transition as established by a singular event that reflects the internal split in the authoritarian coalition, such as the announcement of elections, the draft of a new constitution, a national plebiscite, or a public statement by authoritarian leaders about their intentions to liberalize based on information drawn from the comparative case study literature. He divides regime transitions into four categories: Stable Authoritarian, Transitional Authoritarian, Transitional Democratic, and Stable Democratic for 19 Latin American governments from 1980-2000.

Table 2.2. Codification of Political Regimes in Latin America, 1973-2000

Country	Authoritarian	"New" Democracies	"Established" Democracies
Argentina	1976-1982	1973-1975, 1983-1998	1999-2000
Bolivia	1973-1982	1983-1988	1989-2000
Brazil	1973-1984	1985-1993	1994-2000
Chile	1973-1989	1990-2000	–
Costa Rica	–	–	1973-2000
Dominican Republic	1973-1977	1978-1995	1996-2000
Ecuador	1973-1978	1979-1987	198-2000
El Salvador	1973-1983	1984-1993	1994-2000
Guatemala	1973-1985	1986-1995	1996-2000
Mexico	1973-1999	2000	–
Panama	1973-1993	1994-2000	–
Paraguay	1973-1992	1993-2000	–
Peru	1973-1979, 1990-2000	1980-1989	–
Uruguay	1973-1984	1985-1994	1995-2000
Venezuela	–	–	1973-2000
# of Observations	179	128	113

The data used in the chapters on electoral cycles is based on the same coding methodology. As this study departs from the premise that elections are only a valid measure of democratic transition following the return of democracy with the inauguration of a democratic president, the sample was confined to countries with democratic political institutions. This permits a total of 15 countries in the panel, but some of them enter only in some years. For example, the rule temporarily excludes countries like Argentina (between 1976 and 1982) and Chile (between 1973 and 1988). The definitions and coding methodology employed for elections will be described in detail in Chapter 5.

2.4 Conclusion

This chapter has introduced the dataset and codebook that were compiled for the analysis carried out in this dissertation with attention to specifying the steps that were taken to ensure addressing important methodological and measurement issues. With respect to government expenditure data, several different strategies were undertaken to explore the potential bias and limitations of the available data. Careful coding based on theoretically grounded definitions were adopted with respect to the political variables that will be tested. The next chapter summarizes the research methodology, the baseline model and the specifications that will be tested in chapters 4, 5 and 6 with the data described in this chapter.

3. Research Methodology and Model Specification: Unit Heterogeneity and Dynamics in Time Series-Cross-Section Data

3.1 Introduction

Students of political economy, comparative politics and political science more generally often work with cross-country data that includes variation within and across countries (units) and time (years). This dissertation is based on hypothesis testing using a time-series-cross section (TCSC) data model to examine differences in government and social spending for 15 Latin American countries between 1973 and 2000. This chapter seeks to address two key issues that had to be examined in order to test and select the research methodology for the empirical chapters of this dissertation. The first and perhaps more straightforward issue concerns the challenges posed by unit heterogeneity, or the time-invariant differences across countries, in time series-cross section data analysis. The second and more complex issue concerns the problems of estimations of data that are stationary or follow a unit root. Both issues are of serious cause of concern during research design and the subjects have generated significant discussion as there are contrasting viewpoints and prescriptions emphasized by different researchers on the most suitable methods (Bartels 1996; Beck and Katz 2004b; Wilson and Butler 2007; Plümper and Troeger 2004b, 2004a; Adolph et al. 2005; Beck and Katz 2004a, 2001, 1995; Wawro 2002; Plumper et al. 2005). To date, however, these subjects have received recent, albeit limited attention (Wilson and Butler 2007).

As is discussed below, the challenges posed for hypothesis testing in a time-series-cross-section series dataset are exceedingly complex. The majority of empirical studies reviewed for this dissertation report results based on a single regression estimation method. In a few cases, studies report two or three alternative specification methods or adopt an alternate estimation strategy for computing standard errors. In most cases, however, a particular methodology is utilized with insufficient discussion of the implications of unit heterogeneity and dynamics and how these may influence findings. Moreover, although various specifications are possible, different disciplines have generally emphasized particular sets of estimation methods over other alternatives. This is verified in comparison of political science and economic research reviewed in this dissertation. For example, panel corrected standard

errors¹⁴ in pooled Ordinary Least Squares (OLS) regressions and error correction models are rarely used in economics, but are considered standard tools in comparative politics. In turn, General Method of Moments estimations (GMM) are rarely used in political science, but quite common in economics (Wawro 2002).

The discussion on methodological issues of regression estimation is particularly important for a study focused on the political determinants of welfare spending as some of the most significant papers on political methodology in recent years have precisely centered on debates of the findings of studies focused on welfare states in industrialized democracies (Huber and Stephens 2001; Beck and Katz 2004b; Plumper et al. 2005; Wilson and Butler 2007; Kittel and Winner 2005; Podestà 2006). While an intense debate is underway with respect to these issues in literature aimed at the politics of the welfare state in developed democracies, the issues have not been explored thus far with sufficient discussion and cross-examination with regard to developing democracies and the Latin American welfare state in particular. This chapter is an effort to fill this lacuna and also contribute to a larger discussion on these issues by political methodologists.

The baseline model of this dissertation can be summarized as:

$$Y_{i,t} = \beta_1 Y_{i,t-1} + \beta_2 \mathbf{Z}_{i,t} + \beta_3 \mathbf{X}_{i,t} + c_i + \mu_{i,t}. \quad (1)$$

Thus, the study departs from the assumption that past levels of government spending influence the levels of expenditures in future years and therefore a lagged dependent variable must be included in models seeking to examine the effects of political determinants and globalization on fiscal and social policy performance. The variables on the right-hand side of the model include a one-period lag of the dependent variable, a vector of control variables, \mathbf{Z} , and a vector of variables, \mathbf{X} , that will be used for hypothesis testing. The index i refers to the N observational units (or panels), and t indexes the T time periods. The term c_i contains time-invariant, country-specific unobserved effects. The error term, μ_{it} , is an error term associated with unit i at time t . As is discussed in detail in the sections that follow, this model allows for dynamics via the lagged dependent variable and unit heterogeneity in the mean via i .

¹⁴ For a description of OLS panel-corrected standard errors, see Beck and Katz (1995).

The sections that follow focus on why unit heterogeneity and unit roots pose important challenges for multivariate analysis and report the results after tests for country and time effect are carried out for the underlying models in Chapters 4, 5 and 6. The chapter then shifts to reviewing the problems and benefits posed by adopting different model specifications and how these are impacted by highly persistent series. The results of tests carried out on the fiscal and social spending data that will be used as dependent variables in the empirical chapters for unit roots and serial correlation are also reported. The consequences of these characteristics of the data for estimation techniques including pooled OLS, Fixed Effects (FE), First Differences (FD), Autoregressive Distributed Lag (ADL), the Error Correction Model (ECM), the Anderson-Hsiao First-Differenced Estimator, the General Method of Moments First Differences (GMM-Differences) and System (GMM-System) estimators are discussed.¹⁵ Following this review, the battery of specifications that will be used in each chapter is introduced. Rather than adopting a single specification model to use in the empirical chapters of this dissertation, this chapter argues that there are tradeoffs and challenges posed by different model specifications and the key is to understand how the specified model impacts and influences our ability to draw inferences.

This chapter consists of three main sections. First, it introduces the issue of unit heterogeneity and its implications for the empirical models employed in Chapter 4, 5, and 6. Second, it reviews the challenges posed in working with time series-cross-section (TSCS) research with dynamics and the results of the tests undertaken to diagnose unit roots. Finally, it outlines the reasons why the alternative dynamic specifications were adopted in the dissertation and review the benefits and challenges of each specification method, as well as review how the methods differ. The emphasis in the sections that follow is to outline the key methodological issues that were contemplated in working with the time series-cross sectional data that were used in the empirical chapters of this dissertation. The specific models developed in each chapter will be introduced and discussed in detail in chapters 4 through 6 and as a result they are not detailed in this chapter though each of the models used in subsequent sections of this dissertation are drawn from the discussion summarized here.

¹⁵ It should be noted that the models that will be reviewed in this chapter are not intended to all available solutions. Instead, discussions have been focused on those models most commonly employed in the body of literature that has been reviewed for this dissertation. For example, Kiviet (1995) has proposed a procedure to remove the bias from the LSDV estimator that has been demonstrated to perform at least as well as pooled OLS with PCSEs in Monte Carlo simulations. However, Beck and Katz (2004b) point out that the implementation of this solution is difficult and rarely used in practice as the routine is not available in standard statistical software packages, such as STATA, and can not be applied to unbalanced panels.

Prior to moving into a detailed discussion, however, it should be noted that the focus on unit heterogeneity and state dependence for model specification in this chapter is not intended to imply that other critical methodological issues were not explored for the design of the research explored in this dissertation.¹⁶ Issues concerning research design are addressed throughout the study. Chapter 1 and 2 of this dissertation, for instance, focus on the definition of the research question, the theories of the relevant body of literature, the cases and observations that were selected for this study and measurement error issues. The empirical chapters that follow this one, in turn, focus on issues of casual inference explaining how theory was used to select key variables for hypothesis testing, why explanatory variables were introduced to control for the possibility of omitted variable bias in past research and the steps that were taken to confirm the robustness of the results. The concluding chapter focuses on highlighting how the findings in the dissertation should be used in the construction of a longer-term research agenda to help refine theory on redistributive politics in Latin America.

3.2 Unit Heterogeneity

One of the most challenging problems posed by work based on time series-cross sectional data is deciding if data can be pooled into a single sample (Bartels 1996). This problem is manifest in studies on the determinants of fiscal and social policy performance in which some authors depart from the premise that it is permissible to group both developed and developing countries, while others concentrate on a single region seeking to create more homogenous units (e.g. European countries or Latin America).

Given the heterogeneity that exists between the countries that are being analyzed, researchers are often challenged for the pooling together of time-series-cross-section data. The solution most commonly adopted to account for heterogeneity across countries is the inclusion of dummy variables for $n-1$ units that transform the equation into an estimation of within group (country) variation in which all variables are replaced by their unit-centered averages (Beck and Katz 2004b). If the original model is: $y_{i,t} = \alpha_i + \beta X_{i,t} + c_i + u_{i,t}$, the fixed effects model (FE) corresponds to: $y_{i,t} - \bar{y}_i = \beta(x_{i,t} - \bar{x}_i) + (u_{i,t} - \bar{u}_i)$. In undertaking this

¹⁶ Guiding texts on methodology that were foundational for the research design include King, Keohane and Verba (1994) and Brady and David Collier (2004).

transformation, researchers seek to control for errors that would lead one to accept an effect when it is not really there due to omitted variable bias produced by the absence of controls for time-invariant, unobserved unit heterogeneity (Green et al. 2001; Wilson and Butler 2007). Thus, controls for unobserved unit effects are introduced to avoid a false positive result (type 1 errors). As is confirmed by inspection of the transformed model, fixed effects models explain how intra-unit changes of x (e.g. regime type) effect intra-unit changes of y (e.g. social spending). This specification is useful for revealing what happens to the dependent variable as equally-sized short-term deviations within a particular unit of observation across time occur (Beck and Katz 2001).

Two drawbacks have been noted with respect to the use of this specification in cross-country analysis. First, this type of transformation is particularly poor for variables that are either invariant or change rather slowly over time (e.g. regime type). Though the implications of this flaw is often overlooked in empirical work, Beck (2001) caution that variables that change very slowly are highly correlated with country effects and as a result “sluggish variables” are measured imprecisely by fixed effects. In addition, Zorn (2001) argues that it is often the case that researchers are more interested in the long-term effect of these variables on a particular dependent variable. Plumper, Troeger and Manow (2005) emphasize this guidance, “ There is one crucial exception where the inclusion of country dummies actually complicates the interpretation of the estimation. If either a *level effect* of at least one variable or a time invariant variable of theoretical interest exists, the inclusion of country dummies is problematic because it suppresses the level effect (333).”¹⁷

The consequences of the inclusion of fixed effects are important for this dissertation as many of the key variables that will be tested are indeed slowly changing. This is best illustrated by focusing on a particular variable and the consequences of country dummies in regression estimations. There are considerable differences in the degree of openness, which is measured as the level of exports and imports as a share of GDP, in Latin America in the period under study. Yet, fixed effects treats increases the movement of an increase in openness from 10 to 20 percent in the same fashion as an increase from 80 to 90 percent. It may be, however, that the level effect of trade openness matters in such a manner that both movements produce different effects on government and social spending.

¹⁷ It should be noted that there are similar implications from the use of year dummies in regression estimation (Plumper et al. 2005).

Second, inferences about the inter-unit variation can not be drawn from fixed effects models.¹⁸ Indeed, there are important reasons why the rate of change of an independent variable will not necessarily exert the same influence within a country as it will across countries. This is best illustrated by an example. Zorn (2001) argues that the tentative relationship that has been found between trade openness and economic aid may be due to the fact that the potential effects of market openness and trade lies in differing effects *across countries* rather than *across time*. In other words, it may be that the short-term changes in openness *within* a country explain marginally very little of the change in the propensity to receive economic aid. On the other hand, it may be more likely that more open countries (relative to their average) may be more likely to receive economic aid. In instances in which there is the necessity to distinguish between the effects within and between countries, fixed effects may not be the most appropriate method to be adopted (Zorn 2001).

As a result of these properties of FE estimation, Beck (2001) warns that the control of unit heterogeneity can also lead to the rejection of an effect that really does matter (i.e. a type II error or a false negative result). Beck and Katz (2001) advocate careful analysis of the necessity of using fixed effects when the independent variables of interest vary little from year to year. They suggest moving beyond F-tests that may be overly liberal in rejecting null hypothesis of no effects and advocate an additional test, the Schwarz criterion (otherwise known as the Bayesian information criterion (BIC) as this statistic has a larger penalty for the lack of parsimony in model specification relative to the F-test commonly used to test the null hypothesis of no effects.¹⁹ In a subsequent analysis, the same authors go a step further and advise,

“we do worry that when the null of no fixed effects is only marginally rejected that the overall interpretive costs of including fixed effects may be more costly than the extra bias from excluding them (if that bias is seen to be small). Thus with large T’s, where we can often reject the null that even a very small estimated fixed effect is zero, we must be very careful in deciding about whether or not to include fixed effects in our model. Where the estimated effects are large and clearly significant, there is no doubt that they should be included in the model) (Beck and Katz 2004b).”

¹⁸ Zorn (2001) points out that random effects is also flawed for distinguishing between versus within effects as it is based on a variance-weighted average of the between and within estimators. An alternative and promising model advocated by Beck and Katz (2004a; 2004b) is the random coefficients model (RCM). For a discussion of methods to explore inter-unit variation versus intra-unit variation, see Zorn (2001) and Beck and Katz (2004a).

¹⁹ It should be pointed out that Beck and Katz do not recommend that the BIC be used to decide between different types of regression models (e.g. fixed effects versus GMM), but rather to decide if country dummies (units) are necessary within a regression specification.

On the other hand, Beck and Katz (2004b) clearly defend that there are situations in which there is significant homogeneity across units and that the remaining cross-sectional differences can be explained by the observed independent variables (Beck 2001). In these cases, the authors defend that there are good theoretical reasons to exclude unit dummy variables for each country in pooled ordinary least squares.

3.2.1 Results for Tests on Fixed Effects

In order to examine the implications of including country dummies in the models that will be tested in the empirical chapters of this dissertation, tests were carried out to compare outcomes with and without controls for unobserved unit fixed effects. The pooled OLS models presented in each chapter were tested with F-tests and the BIC to determine if controls to account for unit heterogeneity should be included. The results of these tests generally did not indicate that specifications should be estimated with controls for time invariant country effects based on the BIC. As predicted by Beck and Katz (2004b), F-tests supported the hypothesis that country dummies should be included. Due to these results, models with and without unit and time effects will be reported.

3.3 Dynamics, State Dependence and Unit Roots

As had been introduced earlier, the baseline model can be summarized as:

$$Y_{i,t} = \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 X_{i,t} + c_i + \mu_{i,t} \quad (1)$$

The inclusion of a lagged dependent variable on the right hand side of the equation is a conventional method to handle dynamics. This specification, which is commonly referred to as the Least Squares Dummy Variable Estimator (LSDV), violates the assumption of strict exogeneity introducing the possibility of a feedback effect of y_{it} to future values of x_{it} (Wooldridge 2002). Dynamics represents a serious modeling challenge and there are different specifications that can be utilized depending on whether there are unit roots. This section discusses the general recommendations of political methodologists, economists and statisticians on the models that are best suited for dynamics and the implications in the cases of stationary and non-stationary data. It should be noted that the models that will be reviewed in this chapter are not intended to cover all available estimation solutions. Instead, the

discussion in this chapter focuses on those models most commonly employed in the body of literature that has been reviewed for this dissertation. In addition, appropriate methods for the models employed by scholars of comparative political and economic dynamics, who work with time-series-cross-section data with larger time dimensions (T) and smaller numbers of observations (N), are emphasized as compared to the types of data employed by microeconomists for panel analysis where the time dimension tends to be for fewer years and the number of observations significantly larger.

The modeling of regression estimation with a lagged dependent variable on the right hand side of the equation poses challenges for traditional regression estimation methods. The key question that must be resolved for model specification of an equation that includes lags of the dependent variable on the right hand side is diagnosing whether this is a case of a stationary series (Woolridge 2002). Provided the series is not a unit root or highly persistent, Woolridge (2002) shows that consistent estimation can be produced with pooled OLS by computing standard errors that are robust to arbitrary serial correlation and heteroskedasticity. Pooled OLS coefficients, however, will be biased (Hsiao 1986; Woolridge 2002).²⁰ Bond (2002) notes that the coefficients will be upwardly biased. Similarly, within groups estimation procedures will produce consistent, but biased estimates (Woolridge 2002; Hsiao 1986).²¹ Nickell (1981) has shown that the potential bias of the within groups estimator is of order 1/T. Thus, the within groups estimator is consistently biased downward.

Despite the recognized biases in pooled OLS, some researchers defend their continued use in cases where series are stationary. Beck and Katz (2004b) point out that in the case of TSCS data the number of T are often greater than 10 and even as high as 30. In these cases, they go on to argue that the proposed fixes to the biases for Pooled OLS analysis with a

²⁰ Woolridge (2002) shows that consistent estimation can be produced with pooled OLS by computing standard errors that are robust to arbitrary serial correlation and heteroskedasticity. He argues that the assumption of strict exogeneity is not necessary in cases of fixed-T, large-N panel data. He warns, however, that "If T is large relative to N, the asymptotics may be misleading. Fixing N while T grows or letting N and T both grow takes us into the realm of multiple time series analysis: we would have to know about the temporal dependence in the data, and we would have to assume some form of weak dependence (175)." He goes on to add that "in a pure time series case, or in a panel data case with $T \rightarrow \infty$ and N fixed, we would have to assume that [the coefficient on the lagged dependent variable] $\beta_1 < 1$." In other words, the consistency of the pooled OLS estimator in cases of TSCS data is attained in cases in which the data do not follow a unit root. For a discussion of the implications of using Pooled OLS, see Woolridge (2002) pp.173-178. In Stata, he advises the implementation of pooled OLS with a robust (unit) option to adjust standard errors appropriately.

²¹ For a discussion on the problems posed by the introduction of the lagged dependent variable for fixed effects estimation and the tests that should be carried out for the estimation model, see Woolridge (2002) pp. 299-305.

lagged dependent variable involving complicated alternative specifications may not be worth their costs.^{22,23} This viewpoint is also affirmed by Judson and Owen (1999) who test a dynamic model with pooled OLS, unit dummies and a lagged dependent variable (LSDV) and compare these biases to alternative specifications in Monte Carlo simulations for stationary data as T ranges from 5 to 30 years.²⁴ The authors find that the bias for the coefficient on the lagged dependent variable from pooled OLS is more serious as T decreases, but the bias for the coefficient of the independent variables, which are the key variables of interest to comparative political scholars, is less serious. Based on comparisons, the authors conclude that the LSDV model is more appropriate in cases where T are as high as 30 and panels are unbalanced relative to alternative, more sophisticated estimation techniques.²⁵

To illustrate the differences between regression estimation with a lagged dependent variable in cases with and without a unit root, Beck and Katz (2004b) re-test the results obtained in a study based on a stationary series of the growth of GDP with fixed effects by Garrett and Mitchell (2001) for 14 OECD countries from 1966-1990 and one based on a non-stationary series by Huber and Stephens (2001) of social security spending in 16 OECD countries from 1960-1985. In the case of the first study, Beck and Katz note that the coefficient on the lagged dependent variable is 0.16. They argue that due to the rapid speeds of adjustment there are no major differences in using a model with AR1 errors, a model with a lagged dependent variable (LDV) or an autoregressive specification. To illustrate their argument, the authors replicate the results using all three specifications and show that the LDV estimates perform just as well as alternative specifications. For stationary series, the authors advocate starting estimation with a lagged dependent variable (LDV) model and testing for serial correlation. With serious error correlation, they advise that statistical tests be used to explore whether the alternative specifications of an AR1 error or an ADL model are preferable.

²² The authors test the lagged dependent variable (LSDV) model against the Anderson-Hsiao (A-H) model and the Kiviet correction model (1995) showing that the LSDV clearly outperforms the A-H model based on Monte Carlo experiments and performs just as well as the Kiviet correction model without the cost of estimation.

²³ Brender and Drazen (2005) defend the use of fixed effects estimation arguing that the extent of bias is relatively small.

²⁴ The models that will be discussed subsequently in this section are GMM First Differences, Anderson-Hsiao First Differenced Estimator and Kiviet's correction.

²⁵ The authors recommend GMM Arellano-Bond estimates in cases of T less than or equal to 10 and either GMM or A-H estimators when T=20.

After carrying out various panel root tests that indicate that social security spending as a share of GDP is a highly persistent series in a study for OECD democracies by Huber and Stephens (2001), Beck and Katz confirm that the data follow a non-stationary trend. In the case of the presence of unit roots, pooled regression estimates of time-series-cross-section data be they obtained either with an AR1 error, a lagged dependent variable (LDV) model or an autoregressive distributed lag (ADL) model are inappropriate (Beck and Katz 2004b). The authors assert that the correct methodology to be employed should be limited to examining the short-term effects of changes in the independent variables on short-term changes in social security spending. As they summarize, “using LDVs [lagged dependent variables] is similar to modeling first differences, and we should not model first differences if we want to model lags (15).” The authors cite the results obtained from a first-differenced specification without a lagged dependent variable to conclude that the earlier Huber and Stephens findings based on the specification of a Panel Corrected Standard Errors (PCSE) AR(1) model are due to the improper specification that yielded biased estimates.²⁶

Thus, Beck and Katz (2004b) advise that two distinct approaches should be undertaken depending on the non-stationary properties of the dependent variable data. In cases where data are not highly persistent, the authors emphasize that model selection should be driven by the inclusion of dynamics and consideration of what the speed of adjustment to a level change in the independent variable of interest, x , looks like. If the interest is in an independent variable that is purported to have a one-time immediate impact, they advocate that an AR1 formulation seems plausible.²⁷ Alternatively, if x is expected to have impacts slowly over time with the final long term effect being much larger than the immediate effect, they argue that a Lagged Dependent Variable specification might be a better fit. In studies where independent variables are a combination of both one-time immediate adjustments (e.g. institutions) and longer-term adjustments (e.g. economic growth whose speed of adjustment is similar to the speed of adjustment of the error process), they argue that a good compromise might be a modified version of an autoregressive distributed lag (ADL) specification that could be represented by the following equation:

²⁶ Specifically, Beck and Katz estimate the following FD model: $\Delta \text{SSBEN} = \text{Constant} + \Delta \text{Aged} + \Delta \text{GDP} + \Delta \text{Female Labor} + \Delta \text{Christian Democracy} + \Delta \text{Left} + \Delta \text{Unemployment} + \Delta \text{Female Labor} * \text{Left} + \text{Vetogates}$. The authors do not find statistically significant coefficients for any of the political variables that are central to the arguments of Huber and Stephens. It should be noted that a lagged dependent variable is not included in the First Difference model tested by Beck and Katz (2004b).

²⁷ The model can be generalized as $y_t = \beta x_t + \varepsilon_t$ where the error term is serially correlated.

$$Y_{i,t} = \beta X_{i,t} - \phi \beta X_{i,t-1} + \gamma Z_{i,t} + \phi Y_{i,t-1} + v_{i,t} \quad (2)$$

where x is a variable that can be characterized by a longer-term adjustment process and z is a one-time effect (e.g. institutions).²⁸ The authors add one caveat, however. As can be readily seen the full ADL specification uses both the lagged and current year values for independent variables. Beck and Katz therefore warn that this specification may result in harming the robustness of results confirmed with alternative simpler specifications as multicollinearity is an important concern.

In highly persistent series, Beck and Katz (2004b) concur with Wooldridge (2002) that variables must first be differenced before they are used in linear regression models. Estimation can be performed with OLS or instrumental variable (Wooldridge 2003). Beck and Katz (2004) advocate the “first-difference (FD)” model as an unbiased specification that permits the analysis of the effect of short-term changes in the independent variables on the dependent variables. The model can be summarized as:

$$y_{i,t} - y_{i,t-1} = \beta(x_{i,t} - x_{i,t-1}) + (u_{i,t} - u_{i,t-1}) \quad (3).^{29,30}$$

Beck and Katz (2004) undertake a slightly modified approach suggesting that it is possible to include key variables in *levels* in the FD model. Estimation with first-differences has a drawback that the transformation can greatly reduce the variation in the explanatory variables (Wooldridge 2003).³¹ Podestà (2006) criticizes FD models in studies of the political determinants of the welfare state in developed countries. He raises two main arguments. First and foremost, he notes that theories on the dynamics of the welfare state are not about short-term, transitory effects. Rather, explanations center on variables (e.g. globalization, partisan effects) that take considerable time to materialize. Thus, he argues, “although a first

²⁸ The generic form of the ADL specification would be: $Y_{i,t} = \beta X_{i,t} + \delta Y_{i,t-1} + \psi X_{i,t-1} + v_{i,t}$. For a more detailed discussion, see pp. 16-18 and pp. 21 and 22.

²⁹ Beck and Katz (2004b) also note that the GLS procedure for dealing with AR1 errors is pseudo-differences. They argue that “choice between modeling levels or first differences (or nearly first differences) is econometric, not substantive (18).”

³⁰ It should be underscored that Beck and Katz (2004) do not recommend the estimation of FD with a lagged dependent variable. As Wooldridge (2002) notes, in the presence of serial correlation first-differencing with a lagged dependent variable yields inconsistent estimates for a fixed T as $N \rightarrow \infty$. For a discussion, see pp. 301-302.

³¹ A sign of the reduced variation in the explanatory variables is usually confirmed by very large standard errors. To address this problem, Wooldridge (2003) recommends undertaking an analysis based on longer differences in time than year to year changes. For a discussion, see pp.440-446. This is a task for future research.

difference model performs better from an econometric point of view, it is unable to test the hypothesized long-term relationships underlying welfare state dynamics (539).” Second, model specification must not be guided solely by econometric theory.

Podestà (2006) advocates estimation with an Error-Correction Model (ECM). This alternative model in highly persistent series is also advocated by Woolridge (2003) and Beck (Forthcoming). Although only a few studies in comparative politics have used this technique, a significant number of these precisely have been performed on welfare state dynamics in both developed and developing democracies (Kaufman and Segura-Ubiergo 2001; Iversen and Cusack 2000; Podestà 2006) as they are argued to be preferable to FD models permitting the measurement of the effects of both long-term and short-term dynamics on fiscal and social spending. Beck (Forthcoming) notes that research on the suitability of ECM to TSCS is still in its infancy. The single equation ECM model can be summarized as:

$$Y_{i,t} - Y_{i,t-1} = \beta(X_{i,t} - X_{i,t-1}) - \lambda X_{i,t-1} + \gamma Z_{i,t} + \phi Y_{i,t-1} + v_{i,t} \quad (4)^{32}$$

Several alternative estimators have also been proposed to handle dynamics based on instrumental variables.³³ Woolridge (2002) points out that in the presence of unobserved effects, instrumental variable estimation, specifically Generalized Method of Moments (GMM) instruments, can be utilized as they have the advantage of accounting for unobserved country specific effects and allowing for the endogeneity resulting from the presence of the lagged dependent variable in the regressions.^{34,35} These estimators include the Arellano and Bond (1991) first-differenced GMM estimator (GMM-Diff) and the Blundell and Bond (1998) system GMM estimator (GMM-System).³⁶ As is the case with other instrumental variable-based estimation techniques, estimation based on Generalized Method of Moments

³² For a discussion of the benefits of a single equation versus two-step ECM, see Wooldridge (2003) and Podestà (2006). Podestà (2006) argues that theoretically more adequate for modeling welfare state dynamics as given that social spending does not adjust with all variables also adjusting themselves back to equilibrium (e.g. economic structures).

³³ For example, Bond (2002) and Beck and Katz (2004) cite Kiviet and maximum likelihood estimators (MLEs). Other alternatives include Hausman-Taylor estimators (generalized IV estimators). For a discussion on this method, see Wooldridge (2002) pp.325-328.

³⁴ See Woolridge (2002) pp. 299-305 for a discussion of the efficiency of GMM estimators with a lagged dependent variable.

³⁵ The use of instrumental variables after first differencing as suggested by Anderson and Hsiao (A-H) (1982) is also possible, though regarded as less efficient than GMM. For Anderson-Hsiao First-Differenced 2SLS, the second lag of the dependent variable is used as an instrumental variable for the lagged dependent variable in first-differences, $(Y_{i,t-1} - Y_{i,t-2})$.

³⁶ The references to GMM estimates all refer to the one-step versions of both GMM-First Differenced and GMM-System estimators. As Bond (2002) summarizes, most applied work refers to one-step estimators given that there are very modest efficiency gains from two-step versions.

(GMM) instruments requires great attention in the selection of the instruments. Both types of models are based on methods that generate instruments sets whose number grows quadratically in T (Roodman 2007b). Similar to the Fixed Effect transformation, the Arellano-Bond (GMM-First Differences) eliminates time invariant unit-effects and avoids introducing disturbances for periods earlier than $t-1$ into the transformed error term by estimating the “first-differenced” model:

$$y_{i,t} - y_{i,t-1} = \beta(x_{i,t} - x_{i,t-1}) + (u_{i,t} - u_{i,t-1}) \quad (4)$$

in which suitable lagged levels are used as instruments for first-differences (Bond 2002). Blundell-Bond (GMM-System) proposed an alternative estimator that is based on an additional restriction on the initial conditions of y_{i0} such that it is drawn from a steady-state distribution (Woolridge 2002; Bond et al. 2002). The parameter is identified using lagged differences of the dependent variable dated $t-1$ and earlier as instrumental variables for the levels equations.³⁷

Blundell and Bond (1998) point out that estimators relying on lagged levels will perform poorly as instruments for first differences if unit effects are present in a dynamic panel framework. They propose GMM system estimators that will instrument levels with differences and argue that these estimators will be more efficient than the first differenced GMM estimator in cases of persistent series and small samples. Woolridge (2002) concurs noting that the additional restrictions are well-suited from problems where the coefficient on the lagged dependent variable is close to one, but notes that when the coefficient equals one (e.g. the series follows a unit root) it makes no sense to assume that the series follows a steady-state distribution.³⁸

GMM estimators can be particularly valuable in the context of single equation, autoregressive distributed lag models and GMM System estimators in particular are well-suited for persistent series. However, these methods also pose problems for use in time-series-cross-section series as they were originally developed for panel data with much larger N and fixed T samples. Bond (2002) underscores that GMM estimators “based on too many moment conditions can be subject to potentially severe overfitting biases for GMM estimations with

³⁷ For an excellent pedagogic explanation of the differences between both models, see Roodman (2006).

³⁸ In these cases, Woolridge (2002) refers the researcher to consider conditional maximum likelihood methods. For a discussion, see pp. 304-5.

small samples (14).³⁹ Roodman (2007b) points out that first-differenced GMM often suffers from weak instrumentation as the instruments can overfit endogenous variables “failing to expunge their endogenous components and biasing coefficient estimates.”⁴⁰ He also underscores that system GMM, works only under initial conditions in which initial differences are uncorrelated with the units’ fixed effects, circumstances that he emphasizes are exceptional. Roodman (2007) illustrates the difficulty in satisfying this criterion by noting that appropriate application of system GMM in cross-country economic growth empirical work requires that there be no systematic relationship between a country’s fixed effect and its distance from its conditional steady state in whenever the study period begins.

To summarize this discussion and the consensus on the recommendations to be followed, Table 3.1 presents a summary of the specifications that have been recommended to be employed in the case of both unit roots and stationary data for the TSCS data. This list is not intended to exhaust all options, but only summarize the models reviewed in this section. It is also a simplification only intended to provide a brief overview of the general direction of the recommendations.

Table 3.1 Model Specification Strategy for TSCS Data with and without Unit Roots

	Highly Persistent, Non-Stationary Data	Stationary Data
Dependent variable in levels or differences	First-Difference (Δy)	Level (y)
Include lag of dependent variable	No	Yes
Types of specifications employed that yield consistent estimates	First Differences (FD); Error Correction Model (ECM), and GMM First Differences Estimators (Arelando-Bond)	Pooled OLS AR1 ADL Fixed Effects (FE) A-H 2SLS GMM System Estimators (Bond and Blundell)

Source: Elaborated by the author.

³⁹ Therefore, only instruments corresponding to $Y_{i,t-2}$ and $Y_{i,t-3}$ were used as instruments for $(Y_{i,t-1} - Y_{i,t-2})$ in GMM one-step estimations employing GMM first-differences and GMM system techniques in the empirical chapters of this dissertation.

⁴⁰ In his review, Roodman (2007) concludes, “Perhaps the lesson to be drawn is about the difficulty and importance of finding good instruments. Internal instruments appear to have serious limitations... It is all too easy to employ complicated estimators without fully appreciating their risks—indeed sometimes it takes years for their disadvantages to come to light. If those risks include a propensity for false positives they are particular serious because of the way research and publication processes favor positive results. (28).”

3.3.1 Results of Tests for Unit Roots

This section reports the results from tests for state dependence. Research on the implications of unit root tests in time-series-cross-section data remains controversial and an area of intense research (Banerjee 1999; Maddala and Wu 1999; Pedroni 2004). In particular, unbalanced panel data, cross-section and spatial dependence and finite samples are among issues that complicate the detection of unit roots in panel settings. Given that all of these issues are significant concerns in the data that will be employed in this dissertation, the results that are presented should be interpreted with the understanding that they are not definitive and will be subject to more sophisticated testing in a future stage of research.⁴¹

In order to examine the extent of persistence in the series for fiscal and social expenditures explored in this dissertation, a simple Dickey Fuller test of both fiscal and social policy variables to see if there is evidence of unit roots was performed. The model that was tested for each dependent variable in this test is:

$$\Delta Y_{i,t} = \alpha + \theta Y_{i,t-1} + \lambda_t + e_{i,t}$$

The results of the estimated coefficients and their significance levels are reported in Table 3.2. In all cases the coefficients except fiscal balances, the coefficients are close to 0.9 suggesting that there is strong evidence to suspect that the series are highly persistent.

Table 3.2 Testing for Unit Roots: Dickey-Fuller Test for TSCS Data

	Government Expenditure /GDP	Government Revenue /GDP	Fiscal Balance /GDP	Social Spending /Budget	Social Security /Budget	Health + Education /Budget
Lagged Dependent Variable	0.938	0.940	0.693	0.879	0.959	0.937
Standard Error	(0.021)	(0.022)	(0.034)	(0.028)	(0.016)	(0.041)
Number of Observations	335	334	334	351	351	351
R2	0.888	0.886	0.531	0.804	0.907	0.860

Following Wooldridge(2003), the dependent variables for fiscal and social policy were tested for state dependence to see if current levels depend on the level of the dependent variable in the previous year controlling for unobserved country effects using the Augmented

⁴¹ Future research includes a plan for further testing of this complicated issue, as well as further exploration of the implications of cointegration.

Dickey-Fuller (ADF) test for unit root with a time trend.⁴² The model that was tested for each dependent variable in this test is:

$$\Delta Y_{i,t} = \alpha + \theta Y_{i,t-1} + \delta \Delta Y_{i,t-1} + \lambda_t + c_i + e_{i,t}$$

where $\theta = \rho - 1$ is used to test the null hypothesis that $\theta = 0$ against the alternative that $\theta < 0$. The xtfisher test in Stata that is appropriate for calculating the ADF test for unit root with unbalanced panel data was used. The null hypothesis in this case is that *all* series are nonstationary and the alternative is that there is at least one series that follows a unit root. For each of the six variables, the test was conducted with a time trend was included for fiscal and social policy series as they clearly have an upward trend over time and an intercept was included as the variables do not have a zero mean.

Table 3.3 Testing for Unit Roots: Augmented Dickey-Fuller Test for TSCS Data

Variable	p-value
Government Expenditures/ GDP	0.000
Government Revenue/ GDP	0.002
Fiscal Balance/ GDP	0.002
Social Spending/ Budget	0.254
Social Security/ Budget	0.378
Health and Education / Budget	0.489

Notes: Tests were conducted with one lag of the dependent variable and a trend for time was included.

The results reported in Table 3.3 suggest that we have statistically significant evidence to reject the null hypothesis that all of the series follow a unit root in the case of total government expenditures, government revenues and fiscal deficits. In the case of social spending, however, the results do not indicate that there is statistically significant evidence to allow us to reject the null hypothesis that social expenditures in all fifteen countries follow a unit root. These results are not, however, very conclusive as they give us no indication of the value of θ (e.g. whether it exceeds 0.8), nor what is happening with respect to specific countries. Indeed, Maddala and Wu (1999) note that tests for unit roots in panel settings are not sufficiently well-designed as “the tests are almost all tests for the hypothesis that all series are stationary vs. all series are non-stationary, or that all series are cointegrated vs. that none is cointegrated. This is almost always a hypothesis of questionable value to test (650).”

⁴² This test replicates the methods employed by Wooldridge (2003) pp. 608-613.

Given these concerns, the next stage of testing that was undertaken seeks to break down the analysis further by applying unit root tests for individual units and deriving the predicted value of θ for each country for all measures of fiscal and social policy performance to see if there is evidence of persistence. Table 3.4 presents the results of the estimated coefficient for the lagged value of government spending, revenue and fiscal balances. The test was conducted with a trend and one lag of the dependent variable. The ADF test proposed implemented for each country separately, rejects the hypothesis of nonstationarity for 4 countries in the case of fiscal spending, 4 countries in the case of taxation, and 4 countries in the case of fiscal balances at the 10% or lower levels. This represents 26.6 percent of the countries for which budget information is available in Latin America. Although not statistically significant in the majority of cases, the predicted coefficients are not close to one for any of the three variables.

Table 3.4 Augmented Dickey Fuller Test on Fiscal Policy Variables for Specific Countries($\hat{\theta}$)

	Government Spending/GDP	Government Revenue/GDP	Fiscal Balance/GDP
Argentina	0.518	0.168*	0.778
Bolivia	0.683	0.876	0.491
Brazil	-0.493*	-1.602**	-0.542
Chile	0.701	0.565	0.391**
Costa Rica	-0.105***	0.689	0.565
Dominican Republic	0.483*	0.628	0.479
Ecuador	0.343	0.070**	0.371*
El Salvador	0.722	0.503	0.561
Guatemala	0.510	0.329	0.458
Mexico	0.483	0.383	0.510
Panama	0.575	0.239	0.473
Paraguay	-0.632***	-0.371	-1.251***
Peru	-0.288	0.450	-0.017
Uruguay	0.421	0.343	0.351
Venezuela	0.233**	0.208**	0.129***

Notes: Interpolated Dickey-Fuller ***1% ; **5% ; and *10% Critical Values

ADF tests were also carried out for the share of social spending relative to total government expenditures, as well as the budget shares disaggregated by whether resources are spent on health and education, or pensions. The tests reported in Table 3.5 were conducted with the same specification employed earlier that includes a linear trend and one lag of the dependent variable. In these cases, it was impossible to reject the null hypothesis of a unit root for all three measures of social spending. However, once again, the predicted coefficients were generally below 0.8. The results reported in Tables 3.5 and 3.6 on the outcomes of ADF tests for fiscal and social policy performance in Latin America between 1973 and 2000 should be interpreted with caution and doubt. Woolridge (2003) points out that it is very difficult to reject unit root hypothesis if the data follows a trend that is close to a unit root when using

small sample sizes with the test employed and described above. As the data in our sample for each country (n=15) is quite small, it is therefore likely that the results reported above are due to the small sample size. Unfortunately, no additional data for longer periods is available to permit sensitivity analysis.

Table 3.5 Augmented Dickey Fuller Test on Social Policy Variables ($\hat{\theta}$)

	Social Spending/Budget	Social Security/Budget	Health + Education/ Budget
Argentina	0.425	0.514	0.351
Bolivia	0.335	0.365	0.632
Brazil	-0.023	0.040	0.175
Chile	0.559**	0.767	0.842
Costa Rica	0.292	0.446	0.240
Dominican Republic	0.765	0.758	0.654
Ecuador	0.252	0.475	0.269
El Salvador	0.922	0.428	0.438
Guatemala	0.739	0.566	0.606
Mexico	0.713	0.781	0.581
Panama	0.645	0.677	0.463
Paraguay	0.609	0.771	2.092
Peru	0.488	-0.061	0.548
Uruguay	0.261	0.194	0.695
Venezuela	-0.224	-0.249	-0.199

Notes: Interpolated Dickey-Fuller ***1%; **5%; and *10% Critical Values

Tests for serial correlation in the presence of unit root (non-stationarity) were also carried out by exploring whether the residuals appear stationary following estimation of the dynamic specification with the lagged dependent variable. In order to check for stationarity in the residuals, an autoregression of the residuals on their lags was estimated for each of the fiscal and social policy variables. This test is employed to rule out the potential for a spurious regressions (Wooldridge 2003; Beck 2008). The Stata command `xtserial` to test for serial correlation, which tests if the coefficient on the lagged residuals in a regression of the lagged residuals on the current residuals is -0.5 , was used.⁴³ The results of these tests with the null hypothesis testing that there is no serial correlation are reported in Table 3.6. For all six variables, the test statistics confirm that the errors are serially correlated (AR(1)) in first differences. Moreover, this test was confirmed whether or not the lagged dependent variable was included.

⁴³ This test is proposed by Wooldridge (2002), see pp. 282-283 and pp. 314-315.

Table 3.6 Testing for Serial Correlation

Dependent Variable	Government Expenditure /GDP	Government Revenue /GDP	Fiscal Balance /GDP	Social Spending /Budget	Social Security /Budget	Health + Education /Budget
without a lag of dependent variable						
F-Statistic	55.199	6.173	14.157	32.196	10.982	189.941
Prob > F	0.000	0.026	0.002	0.000	0.005	0.000
with a lag of the dependent variable						
F-Statistic	64.626	18.823	45.233	47.393	30.941	123.103
Prob > F	0.000	0.001	0.000	0.000	0.000	0.000

The results presented in this section do not provide conclusive evidence that the data can be characterized as highly persistent series (and perhaps even a unit root). The evidence is far from decisive as different analyses provide distinct interpretations. In particular, the results presented in Tables 3.4 and 3.5 provide suggestive evidence that the series may not follow a unit root process, while the evidence suggestive of persistence is stronger in Tables 3.2 and 3.3. As has been reviewed, testing of unit roots remains a controversial area of research in panel data econometrics. Moreover, the conclusiveness of testing is particularly controversial for TSCS data such as the one used in this dissertation given the finite time series length, potential for cross-sectional dependence and unbalanced panel data structure. It is possible that these factors, rather than the unit root properties of the data, are driving the results. The next section describes the strategy that will be employed in the empirical chapters.

3.4 Model Specification

There are different possible specifications that could be adopted for the analysis of the baseline model introduced earlier:

$$Y_{i,t} = \beta_1 Y_{i,t-1} + \beta_2 \mathbf{Z}_{i,t} + \beta_3 \mathbf{X}_{i,t} + c_i + \mu_{i,t}. \quad (1)$$

Model choice should be driven by both theoretical considerations and the results of the diagnostic tests on the time-series-cross-section data (Beck and Katz 2004b). As this section has tried to emphasize, there are good theoretical reasons to emphasize whether short-term or level effects drive welfare state dynamics in Latin America. The literature on welfare state dynamics in Latin America suggests that level changes versus short-term effects should be the focal point. Hypothesis testing of level effects, however, is particularly challenging if the data follow a unit root.

The results presented in Section 3 provided mixed evidence that the series are highly persistent (and perhaps even a unit root) with the presence of serial correlation. Given the uncertainty regarding the stationary character of the data, this study adopts an estimation strategy to model both the cases of stationary and non-stationary data in the next three empirical chapters. The specifications are based on diagnostics and the recommendations of the literature as reviewed in this chapter. Therefore, tests on the effects of key independent variables on the *level* of welfare state development will be carried out using three models: (1) pooled OLS with panel corrected standard errors;⁴⁴ (2) the Arellano and Bond (1991) first-differenced GMM estimator (GMM-Diff); and, (3) the Blundell and Bond (1998) system GMM estimator (GMM-System).⁴⁵ To examine whether results are altered when estimation is carried in models more appropriate for highly persistent series, the results of a fourth model that is an error correction model (ECM) with panel corrected standard errors based on the *first difference* of the dependent variable are also reported in Appendices 4A, 5A and 6A.⁴⁶ Estimation was carried out with and without fixed effects to address issues of unit heterogeneity.⁴⁷ Thus, the pooled OLS, GMM and ECM estimates will also be estimated with and without controls for time and unit heterogeneity. As the results with and without fixed effects are reported in the tables presented in Chapters 4, 5 and 6 there will be a total of 7 columns in each table. Similarly, the ECM models reported Appendices 4A, 5A and 6A will also have 3 columns.

Bond (2002) proposes a consistency check for GMM estimates. While both estimates are recognized as being consistent, Bond (2002) argues that the estimate of β_I in pooled OLS can be regarded as an approximate upper bound of the consistent estimated coefficient and the fixed effects estimator can be regarded as the lower bound in a model requiring the use of a

⁴⁴ The model will be estimated with the Stata XTPCSE command.

⁴⁵ The exercise and commands for GMM estimation are based on Roodman (2006) and were carried out using in Stata 10.

⁴⁶ Further testing using FD and GMM models on the dependent variable in differences could also be carried out as a further test. In this case, it is important to note that the FD specification *would not* include a lag of the dependent variable. In contrast, the ECM and GMM models *would* include a lag of the dependent variable on the right hand side of the equation.

⁴⁷ 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).

lagged dependent variable as a control variable in the regression model.⁴⁸ Following Bond, pooled OLS and fixed effects estimates be used to establish a feasible range to check the results derived from GMM estimations. Thus, estimation with fixed effects with heteroskedasticity-consistent estimates of the standard errors was also undertaken for cross-checking and model calibration purposes. It was confirmed that pooled OLS and fixed effects performed as upper and lower boundaries for GMM estimations.

While there are strong reasons for employing GMM estimation in a dynamic specification, the implementation of this technique requires making several important decisions given the finite sample nature of the data used in this dissertation. As Roodman (2007a) notes, both the difference and system GMM estimators generate moment conditions prolifically with instrument count quadratic in the time dimension. To address this problem GMM estimations are modeled by limiting the number of lags to two periods, collapsing the number of instruments and using orthogonal deviations to adjust for an unbalanced panel following the recommendations of Roodman (2007a). Standard errors are also reported as t-statistics based on Windmeijer (2005) finite sample correction. Due to these problems the tests normally used to check for the consistency of the instruments (Hansen test for joint validity of instruments and Difference Sargan tests for all system instruments) generate high values. In some cases, both test statistics report with a p value of 1.⁴⁹ Rather than following an alternate strategy (i.e. increasing the number of instruments) to attain acceptable test results, good practice recommendations are followed and therefore estimations are based on the minimum number of instruments.

3.5 Conclusion

This chapter has attempted to outline a number of estimation issues raised by working with time-series cross-sectional structure of the data. As the discussion has attempted to underscore, there is no perfect recipe for which models are more appropriate in empirical studies. Instead, great care needs to be undertaken to both understand the characteristics of the data being

⁴⁸ Bond (2002) goes on to state, “Thus, we might hope that a candidate consistent estimator will lie between the OLS and Within Groups estimates, or at least not be significantly higher than the former or significantly lower than the latter...In cases where the AR(1) model seems well specified and this pattern is not observed, we might suspect either inconsistency or severe finite sample bias (5).”

⁴⁹ Roodman (2006) notes that the tests are prone to weakness as the tests grow weaker the more instruments are used; problems which are more serious in finite samples (12).

analyzed and the suitability of models for the questions being explored. Diagnostics must be undertaken prior to model selection to ensure obtaining consistent estimates. Moreover, once models are selected it is critical to understand what effects models are designed to capture to avoid making errors in drawing causal inferences.

As will be reviewed in detail in the empirical chapters, most studies that examine the political determinants of fiscal and social policy performance exceptionally discuss whether the issues of unit heterogeneity and unit roots were explored and how they may affect reported findings. Results are also tested on few model specifications. This dissertation undertakes a different approach in order to address these concerns. Based on the results of the tests reported in this chapter on the degree of unit heterogeneity and state dependence, estimation specifications that address both the stationary and non-stationary cases for TSCS in each of the following three chapters are employed. Furthermore, a battery of specifications to test the robustness of findings and to ensure that the results are not driven by reliance on one method is undertaken.

4. Democracy, Autocracy and Social Spending

4.1 Introduction

Within the comparative political science and political economy literatures considerable attention has been paid to examining whether the wave of democratization that has spread across Latin America in recent decades would install governments more attentive to the welfare of their citizens. Two important debates exist. The first debate centers on whether democracies are more likely to invest resources in social programs as compared to authoritarian regimes due to the presence of institutions that force representation of societal demands and interests. The second debate focuses on whether increased trade and financial integration with global markets has forced Latin American governments to reduce social spending and is the focus of Chapter 6 of this dissertation. After having reviewed the methodological challenges for the analysis of welfare state dynamics in Chapter 3, this chapter focused on advancing understanding of the first debate by examining if democratic regimes are more responsive to societal demands. The next chapter will focus on the direct effects of democracy on welfare commitments by studying the effect of election cycles on government fiscal and social policy performance.

This chapter reexamines earlier findings on the impact of democracy on social expenditures considering common problems of time-series-cross-section (TSCS) data analysis and a new data set for a sample of fifteen countries in Latin America and the Caribbean for the period between 1973 and 2000. A series of recent studies have been undertaken on the impact of regime type for the allocation of resources for health, education and social security in Latin America. These studies have produced generally inconclusive findings on the effect of democracy on the propensity of Latin American governments to buttress the social safety net. In some cases democracies outperform authoritarian counterparts in welfare spending; while other studies point to the contrary outcome.

A related question that has received scant attention is whether “new” democracies are more “fragile” and therefore more “vulnerable” to political pressures to increase welfare

expenditures relative to mature, established elected governments. Many Latin American democracies are relatively recent in origin and may be more “susceptible” to institutional weaknesses that make them “more sensitive” to the demands of voters. Thus, they may have higher propensities to redistribute income by increasing the provision of social assistance, public health and education. To advance understanding on this question, this chapter explores whether the impact of democratic regimes on social expenditures is driven by the experience of new Latin American democracies. Given that a source of the inconclusiveness of earlier studies may be tied to methodological problems, a battery of alternative specifications are undertaken to test the robustness of findings reported in earlier research.

The chapter finds that there are no differences in the total amount of resources allocated as part of the national budget for social programs between democratic and authoritarian regimes over the last three decades in Latin America. There are, however, major differences in the priorities democracies give to health and education expenditures versus social security transfers. As a share of the total central government budget, democratic regimes spend more on health and education and allocate fewer resources for social security relative to non-democratic governments. Moreover, the propensity of democracies to invest greater shares of the budget in health and education does not depend on the post-authoritarian transition period. These results suggest that the theorized differences between democracies and autocratic regimes with respect to social protection are fundamentally important. Future research should not abandon the dichotomy between both types of regimes in favor of more gradient or cumulative measures of democracy.

These results are noteworthy as they challenge past research. This chapter argues that the differences in the central patterns we observe in the literature with respect to whether Latin American democracies reduce social spending in the context of globalization may be due to differences in measurement and methods. The majority of studies have employed measures of social spending relative to the size of the economy. This chapter emphasizes that the politics of resource allocation decisions are best understood by measures that consider social expenditures as a share of the total budget. As was discussed in Chapter 3, the hypothesis that social spending data in Latin America is highly persistent can not be ruled out. To address these concerns, the hypotheses on the effects of globalization on social spending for Latin America were tested with a battery of specifications. There is generally strong correlation between the sign and significance levels across the multivariate estimation

techniques that were tested in this chapter. These results suggest that the conclusions reached in this chapter are fairly robust as they are not highly contingent on the method used to obtain them.

The chapter is structured in the following way. Section 2 reviews the literature on the responsiveness of democratic and authoritarian governments to welfare commitments and examines research findings in the context of Latin America's rapid and thorough economic transformation over the course of the late 20th century. This review includes attention to theories on the effects of new and established democracies. Section 3 presents the measures of democracy and autocracy that will be employed for hypothesis testing and discusses the methodological difficulties in past empirical studies on social spending in Latin America. Section 4 introduces the model estimation and specifications that will be used to address these issues. Section 5 reports and discusses the results of the empirical analysis. A variety of statistical methods are analyzed in order to check the robustness of the relationship given concerns that the data may not be stationary. The final section concludes the chapter with a summary of the key findings.

4.2 Review of the Literature on Democracy and Social Spending

Social policy instruments are important mechanisms for achieving the redistribution of income from the rich to poor and for shielding vulnerable populations from economic shocks. Over the course of the last century, Latin American welfare states underwent significant expansion and in some cases augmented coverage to the entire population. While the scope of coverage expanded and became more comprehensive in many countries, benefits remained very unequal. Given the wave of democratization that advanced across the region in late 20th century, a debate has emerged on whether democracies, which are characterized by more equitable distributions of political power, are more apt to redistribute income from the wealthy to the poor, in contrast with authoritarian regimes. Research on the allocation of social spending resources is an important lens for understanding this question, as well as helping to elucidate whether democracy in Latin America is an independent or intervening variable depending on whether elected governments are recently installed or established regimes. This section briefly summarizes the theoretical frameworks that have been developed to explain the responsiveness of different regimes to social needs and then shift to

examining the empirical research on the impact of democracy on social spending for Latin America.

4.2.1 Democracy and Redistribution

In societies with a more marked incidence of poverty and inequality such as that found in Latin America, the redistribution of resources via welfare programs may be especially effective as a political support generating mechanism for leaders seeking to retain office. An important and critical debate in political science, political economy and the social sciences more generally has focused on examining whether democracies, which are characterized by more equitable distributions of political power, are more apt to redistribute income from the wealthy to the poor in contrast to authoritarian regimes. On one side of the dispute there are those who argue that democratic regimes generally choose policies that are more favorable to the poor than nondemocratic regimes because they are driven by electoral competition. In contrast, recently developed political exchange models argue that authoritarian regimes are equally engaged in political trades and no less likely to invest in social welfare to secure citizen support for remaining in office.

A focal and longstanding body of scholarship has posited that democracy plays a critical function in equalizing influence on resource allocation. According to this literature, competitive elections are the key mechanism to linking the responsiveness of politicians and governments to societal demands as the desire to win elections induces politicians' to align their platforms and policies with citizen demands (Meltzer and Richard 1981; Boix 1998; Olson 1982). As early as the first half of the nineteenth century scholars such as Alexis de Tocqueville and Karl Marx theorized that the extension of voting rights to the poor and property-less would lead to policy shifts resulting in higher taxes and increased levels of redistribution (Przeworski and Limongi 1993; Wintrobe 2007). As Przeworski and Limongi (1993) summarize these models posit that, "if the median voter is decisive and the market-generated distribution of income is skewed downward, as it always is, majority equilibrium (if one exists) will call for a greater equality of incomes (53)."

This theoretical framework predicts that democracies will redistribute more than autocracies and they will also provide a greater share of public services that seek to respond to the interests of society as a whole. Olson (1993) and McGuire and Olson (1996) develop models to show that redistributive majorities in democracies will tax citizens at lower levels

and provide greater levels of government services for citizens relative to autocracies who will extract higher revenues, but spend less on government keeping a portion for their own personal gain.⁵⁰ Drawing upon public choice theory of the state as a monopoly producer of public services, Lake and Baum (2001) argue that the low barriers to exit and costs of participation in democracy result in the state functioning as a regulated monopoly providing larger quantities of goods at lower prices. In authoritarian regimes, however, they note that the political market is less contestable because there is a lower degree of citizen participation and a higher cost for the leader if he loses power. Less contestable political markets permit greater levels of rent extraction by authoritarian regimes and in turn lower levels of public good service provision. Thus, these models suggest that authoritarian regimes will give less emphasis to universal programs (e.g. primary education, primary care health services, etc.). In their study of the political origins of dictatorship and democracy, Acemolgu and Robinson (2006) concur with this reasoning concluding that “pro-majority policies [will] coincide with pro-poor policies, especially a greater tendency to redistribute income away from the rich to the poor (19).”

An alternative framework contends that even if authoritarian regimes are motivated by different incentives as they are not concerned with short-term popularity or reelection, they still face strong political incentives that trigger them to allocate public spending, including welfare expenditures, to augment their political survival (Kirkpatrick 1982; Wintrobe 1998; Ghandi and Przeworski 2007). Wintrobe (1998) develops a political exchange model to explain how dictators, who are primarily motivated by seeking to maximize power and personal consumption, make trades with citizens offering policies in exchange for support and engage in repression to contain opposing forces that threaten their power. Dictators seek the backing of interest groups, such as the military that have the power to depose the regime, but they also direct resources to appease broader elements of the population (Wintrobe 1998).⁵¹ McGuire and Olson (1996) show how secure self-interested autocrats with monopoly control over tax revenue have incentives to provide public goods to society as these generate higher levels of economic growth and higher personal rewards for leaders who derive their earnings

⁵⁰ In positing that democracies will distribute more resources than dictatorships, Mancur Olson (1993) warns that these resources will not necessarily be allocated equally among voters. He points out that there are significant obstacles to collective mobilization by majorities relative to smaller organized interest groups (including political parties, unions and social movements) that form “distributional coalitions” to pressure governments for targeted policies. He predicts that upper and middle income groups will benefit disproportionately from government policies as they are probably more articulate and politically active than poorer groups.

⁵¹ For a review of these models and discussion of their implications, see Wintrobe (2007).

from the duties excised on citizens. While McGuire and Olson conclude that it is possible for autocrats to provide public services, they warn that their model also shows that if rulers have a shorter-term horizon or a less encompassing interest, the so-called “stationary bandit” will be transformed into a “roving bandit” plundering society and providing no public goods.⁵²

Models that emphasize the political exchanges that take place in authoritarian regimes have reached different predictions on the propensity of dictatorships to redistribute greater shares of resources relative to democratic regimes. Scholars such as Wintrobe argue that there will not be substantive differences between democracies and dictators with respect to resource allocation and distribution though the latter may tend to tax their populations more heavily to extract resources to finance repression, loyalty and personal consumption (Wintrobe 2007). Such a view is also espoused by Mulligan, Gil and Sala-i-Martin (2004) who conclude from empirical analysis that political regimes do not impact economic and social policies. Democracies, they conclude “may have important impacts on the degree of competition for public office, but that their effects on public policies are insignificant (72).” On the other hand, McGuire and Olson (1996) stress that stationary bandits will still provide less public services than democracies because as they summarize “a majority whose members earn income in the market has a more encompassing interest than an autocrat, so optimization by such a majority therefore necessarily generates outcomes better than autocracy for every market participant (90).”

4.2.2 *Empirical Research on the Responsiveness of the Welfare State*

The majority of the empirical research on the politics of welfare has focused on analyzing the experiences of developed, industrialized Western European democracies (Cameron 1978; Huber and Stephens 2001; Hicks and Swank 1992; Swank 2002; Moene and Wallerstein 2002; Pierson 2001; Rodrik 1997; Garrett 2001; Hicks and Zorn 2005).⁵³ Given

⁵² McGuire and Olson use the term “stationary bandit” to explain the difference between authoritarian regimes that steal and those who behave. Under certain conditions, they argue that dictators will behave and promote pro-growth policies and thus the term stationary refers to the fact that the dictator will not steal and pillage. For a critique of the stationary bandit model, see Wintrobe (2007).

⁵³ There have been a few studies that include both developed and developing countries. In these studies, however, there are additional challenges to drawing inferences as it has been argued that there are significant differences in the political and economic conditions between both groups of countries. In order to control for these differences, most studies attempt to evaluate commonalities on welfare response by controlling for regional or unit heterogeneity, as well as factors including the level of economic development and demographic

the evidence that advanced industrialized democracies have significantly increased government spending in the late 20th century, research has focused on examining what aspects of democracy including party and politician ideology and the relative strength of organized civil society groups (unions and workers) are most critical to explaining the responsiveness of social spending in industrialized democracies. Thus far, however, limited research has been directed at examining how the welfare states has evolved in developing countries (Haggard and Rudra 2005).

Particular attention has been directed at examining their social safety response of Latin America as the inception and transformation of the welfare state in these countries has coincided with dramatic shifts in the level of integration of these economies with global markets. In contrast to European nations, the welfare state in Latin America in the post-war period developed in many countries in closed, protected economies that had implemented import-substitution industrialization (Huber 1996; Solimano 2005).⁵⁴ Authoritarian regimes also predominated. However, notwithstanding their expanded scope and coverage, the programs that are traditionally part of the welfare state in Latin America are significantly different from those provided to citizens in industrialized Western democracies both in terms of the level of government resources directed at the social safety net and the types of services offered to citizens. For example, in advanced democracies, expenditures on unemployment and social assistance are significant portions of welfare spending. In contrast, these programs are insignificant in most Latin American countries. Nevertheless, the design and social safety net provisions for Latin American citizens are among the most advanced in the developing world.

In a seminal study focused on seventeen Latin American regimes, Ames (1987) argues that "survival politics," conceived as the desire to remain in office, drove budget allocations including for social welfare programs during the period between post World War II and the pre-debt Crisis period (1947-1982), an era characterized by expansionary fiscal policies, industrialization via import substitution, inflation and democratic breakdowns in the region.

characteristics of the population. Scholars have argued that the extent to which both groups may be following a similar convergence trajectory have yet to be validated (Rudra 2007). For examples of this research and its findings, see Rodrik (1997) and Persson and Tabellini (2005).

⁵⁴ As Huber summarizes, "ISI created urban constituencies for social insurance, that is employed middle and working classes with an interest in protection from loss of earnings due to accidents, illness, and old age. Politically, passage of social insurance schemes was facilitated by the fact that employers did not really have to absorb the costs of their contributions but rather could pass them on to consumers because they were operating in protected markets. (144)"

Contending that social spending is closely correlated with political business cycles, Ames argues that the more contested are elections (not necessarily democracy per se) in more polarized societies, the greater governments increased the magnitude of social expenditures.⁵⁵ He presents evidence that indicates that bureaucratic-authoritarian regimes were more likely to reduce social programs targeted towards benefitting middle and working classes and increase military spending and repressive state capacity in order to limit the pressures exerted by rival, majority interest groups.⁵⁶ Ames shows that these regimes allocated expenditures to favor the interests of their power base – the military, technocrats, domestic bourgeoisie and foreign capital. He finds that Latin American governments seeking to pacify the military during political crisis cut health expenditures, which could be considered the social program that benefits the poor and the working class. On the other hand, education and pension expenditures, which are two social programs that most benefit the middle class (namely, the parents of children in public primary schools, teachers, university students and retired formal sector workers), were the least likely to be reduced. However, Ames posits that authoritarian regimes in Latin America are not insular to popular pressures. As an example he notes that education spending jumped in the first budget following the 1964 coup in Brazil towards primary education, but as mounting student demonstrations increased resources were reallocated to appease middle-class students engaged in protests against the regime.

While early findings suggested that more democratic periods were associated with higher levels of social spending, later research has not been able to confirm a statistically significant relationship between democracy and the share of the aggregate sum of expenditures devoted to the social security, health and education. Table 4.1 summarizes the seven empirical studies that have been undertaken directed at examining the impact of democracy on social spending for Latin America including the effects on education and health versus social security programs. Thus far, there has not been robust support for the hypothesis that democracies are more likely to increase aggregate social spending relative to authoritarian regimes in Latin America in the post-external debt crisis period. This finding has only been confirmed consistently in three studies.

⁵⁵ Ames (1987) includes all elections, even those that took place under autocracy.

⁵⁶ It should be noted that the period analyzed by Ames was marked by the dominance of autocracy in the region. For example, Ames (1987: 12) reports that between 1945 and 1982, administrations were ended with an election in 82 instances and with a coup d'état in 51 instances in the 17 countries that comprised the sample used in his study. He only reports two cases in his dataset in which governments were overthrown by popular revolt.

Table 4.1. Cross-National Empirical Studies of the Impact of Democracy and Globalization on Social Sector Spending for Latin America

Authors	Countries	Period	Hypothesis Testing Method	Source for Measure of Democracy	Includes Controls for Unit Heterogeneity	Democracy		
						Social Spending	Social Security	Health and Education
Brown and Hunter (1999)	17	1980-1992	Pooled OLS with panel corrected standard errors (PCSE)	Dichotomous measure of democracy based on Polity III	No	Democracy increases social spending.	n.a.	n.a.
Kaufman and Segura-Ubiergo (2001)	14	1973-1997	Error Correction Model (ECM) with panel corrected standard errors (PCSE)	Dichotomous measure of democracy based on Polity IV	Yes	Democracy is not statistically significant.	Democracy reduces social security expenditures in a few cases.	Democracy increases education and health spending in a few cases. Health and education expenditures are not disaggregated.
Huber, Mustillo and Stephens (2004)	22	1970-2000	Pooled OLS with robust-cluster estimator of the standard errors (AR1 model)	Cumulative measure of democracy based on Polity IV	No	n.a.	Democracy is not statistically significant.	Democracy is not statistically significant. Health and education expenditures are not disaggregated.
Avelino, Brown and Hunter (2005)	19	1980-1999	Pooled OLS with PCSE	Categorical definition of democracy based on Álvarez, et. al. (1996)	Yes	Democracy increases social spending.	Democracy is not statistically significant.	Democracy increases education, but not health spending.
Avelino (2005)	17	1980-2000	Pooled OLS with PCSE	Categorical definition of democracy based on Álvarez, et. al. (1996)	Yes	Democracy increases social spending, but countries undergoing transitions to democracy spend less than established democracies.	n.a.	n.a.
Wibbels (2006)	12	1970-1995	ECM with PCSE	Polity IV score	Yes	Democracy increases social spending in one of two cases.	Democracy is not statistically significant.	Democracy increases education and health spending in a few cases. Health and education expenditures are not disaggregated.
Segura-Ubiergo (2007)	14	1973-2003	ECM with PCSE	Dichotomous measure of democracy based on Polity IV	Yes	Democracy is not statistically significant.	Democracy is not statistically significant.	Democracy increases education and health spending in a few cases. Health and education expenditures are not disaggregated.

Notes: The table only reports findings for estimated coefficients with significance levels of 10% or lower. For ECM models, the sign and significance level of the independent variable in levels was used.

Source: Elaborated by the author.

In a study on between 1980 and 1992 in a sample of seventeen countries, Brown and Hunter (1999) find that Latin American democracies allocate greater resources to social programs relative to authoritarian regimes. At low initial levels of per-capita income, Latin American democracies were more likely to increase the level of welfare spending relative to authoritarian regimes. Democracies were also more likely to increase social spending in response to declining rates of economic growth. Thus, the authors conclude that democracies respond to economic constraints by increasing the level of social spending. One of the puzzles presented by Brown and Hunter is elected governments do not significantly enhance social spending in the wealthiest economies in Latin America. The authors posit that at higher income levels, political pressures on regimes are exerted not only through elections, but also through other channels, such as NGOs and unions, and that these explain the fact that there are not significant differences beyond income levels of \$6,000 per capita. In contrast in poorer economies the authors argue, electoral competition is more decisive.

In a subsequent study, Avelino, Brown, and Hunter (2005) examine the impact of democratization on social spending in a sample of 19 countries from 1980 to 1999. In contrast to the 1999 study by the two of the same authors, they add additional controls for trade and capital integration.⁵⁷ The authors show that more open democracies spend more on social policies than their authoritarian counterparts. Wibbels (2006) also reports some weak evidence that unit increases in a country's democracy score is associated with greater social spending based on results from 1970 to 1995 for 12 Latin American countries.

A number of studies, however, argue that regime type has a weak and inconsistent impact on total welfare expenditures. In a study for fourteen Latin American countries during the period following the first petroleum crisis in 1973 to 1997 Kaufman and Segura-Ubiergo (2001) report that the impact of democracy on annual changes in aggregate social expenditures is not statistically significant.⁵⁸ In a further extension of this work, Segura-

⁵⁷ The authors note that the positive and statistically significant coefficient on democracy is stable whether different measures of trade openness ((Imports + Exports)/GDP) are tested.

⁵⁸ The interpretation of these results is problematic as the empirical models tested in the study employ a dummy variable for democracy and an additional dummy variable that classifies whether democratic and authoritarian regimes are closely tied to labor unions and/or have programmatic orientations toward the mass electorate (popular). This variable is also statistically insignificant in the equations on aggregate social spending. This design and the results produced could be interpreted as providing evidence that democracy does not impact welfare spending after control of the political base of support of the regime. On the other hand, it is also possible

Ubierno (2007) using the same data updated to 2003 confirms earlier findings of the insignificance of democracy in explaining changes in aggregate welfare spending.

In contrast, there is some suggestive evidence that the incentives for authoritarian and democratic leaders may induce regimes to direct different levels of government resources to human capital investments (e.g. education and health programs) or the protection of the elderly and formal workforce (e.g. social security). With respect to spending on education and health, the evidence is stronger and more consistent. Several studies confirm that democracies allocate a greater share of budgets to these programs. Several interpretations have been offered for this result. Some scholars have emphasized the progressive character of health and education programs that give the poor incentives to mobilize to attempt to secure a greater share of resources allocated for this social objective, while other studies have speculated that the response may be due to the responsiveness of democracies to powerful interest groups. Avelino, Brown and Hunter (2005) emphasize that changes in resource allocations are much more difficult for democratic regimes to impose on politically, influential interests such as teacher's unions, social security lobbies, and health care professionals.

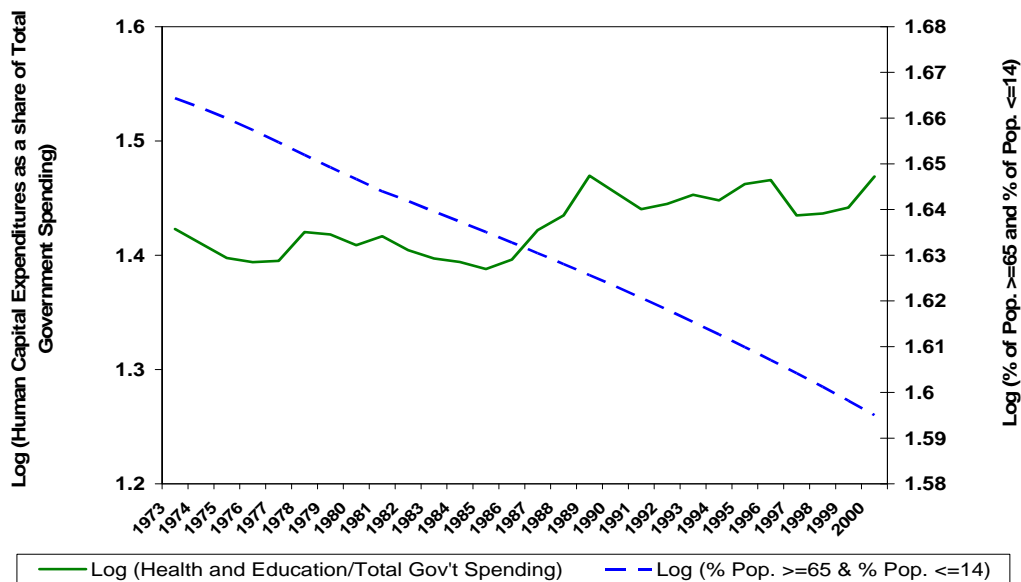
As Figure 4.1 below shows, the share of the population below the ages of 14 and over the age of 65, which are presumably the major beneficiaries of health and education services, declined over the course of the last three decades of the 20th century. The rate of expenditures on health and education, however, increased slightly suggesting that a greater share of resources is being allocated on a per citizen basis. Early evidence produced by Sloan and Tedlin (1987) suggests that this pattern may precede the post-debt period. They find that Latin American democracies showed the highest increase in health expenditures relative to GNP as compared to traditional authoritarian, bureaucratic authoritarian and communist regimes between 1960 and 1980.⁵⁹ In the cross country regressions with controls for trade and financial openness, Avelino, Brown and Hunter (2005) find a positive and statistically robust correlation between education spending and democracy, but no evidence of improved performance with respect to health. The authors argue that increased expenditures on

that the inclusion of both democracy and popular-based governments introduces multicollinearity that renders it difficult to assess the effect of both of the independent variables on social spending.

⁵⁹ In addition to health expenditures, the authors also analyze the differences in outputs of public investments in education (enrollment rates, etc.) between both types of regimes. They conclude that democracies are mediocre in educational output performance. These results (based on the analysis of the output of social programs) are problematic for drawing inferences and making comparisons with budget expenditures (inputs for social programs).

education can be interpreted as a means of improving “efficiency” in an era of globalization by supplying employers with more productive workers.

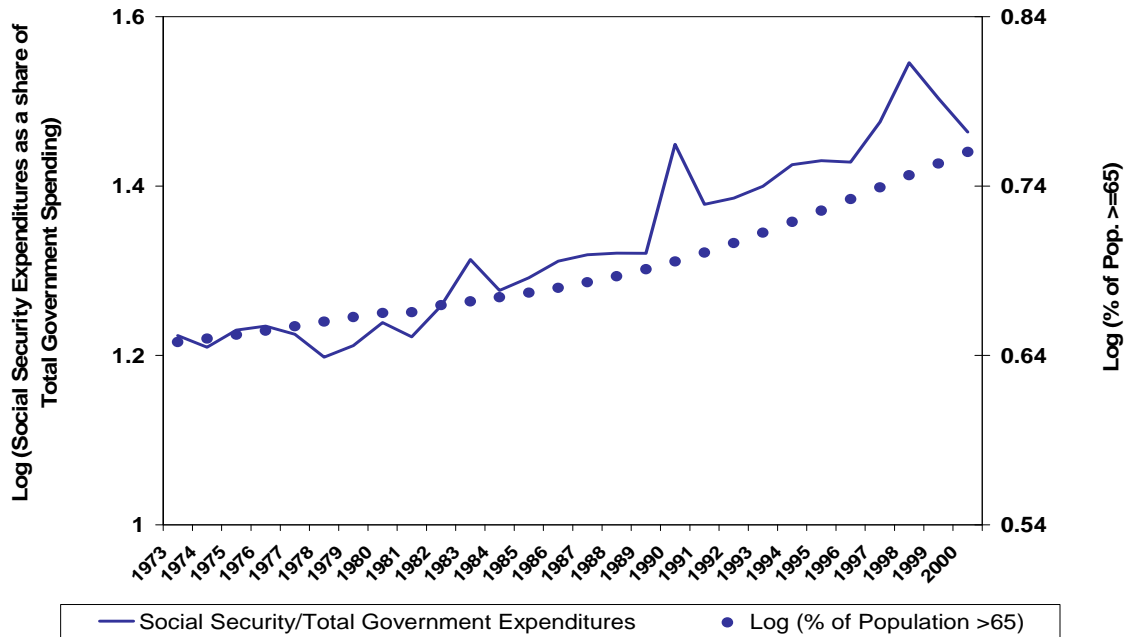
Figure 4.1. Health and Education Spending and the Proportion of the Population below 14 and over 65 in Latin America, 1973-2000
(average for 15 countries in sample)



Source: International Monetary Fund, *Government Finance Statistics*.

The empirical evidence on the political determinants of social security expenditures, however, is much weaker. To date, Kaufman and Segura (2001) have been the only study that has found statistically significant differences suggesting that democratic regimes spend less on social security than autocracies. The authors argue that the divergence in social spending is due to the size of the constituency in democratic periods. Thus, social security is predicted to decrease or at least not increase as the poor have little incentive to call for greater spending on pensions given their regressive character. In a follow-up study, Segura-Ubierno (2007) is unable to confirm this earlier finding. Other empirical studies have failed to detect a statistically significant and robust relationship. In the sample of fifteen countries analyzed in this chapter, the average proportion of the over 65 population has steadily increased from 4.5 to 5.5 percent. Moreover, as Figure 4.2 details, social security spending has increased steadily in a pattern that appears to be proportionate to the share of the population demanding these transfers.

Figure 4.2. Social Security Spending and the Proportion of the Population over 65 in Latin America, 1973-2000
(average for 15 countries in sample)



Source: International Monetary Fund, *Government Finance Statistics*.

Studies have additionally underscored that political motivations are complex and often multi-faceted. Ames (1987) points out that governments may target social spending, but seek to distribute benefits to different groups. For instance, governments might increase education expenditures by directing resources to basic schooling to draw massive electoral support from lower-class and intermediate groups and invest in higher education to appease the urban upper classes. Avelino, Brown and Hunter (2005) similarly stress that it is incorrect to assume that strategies are mutually exclusive. Instead, they argue that we should expect democracies to choose to compensate certain groups (e.g. social security spending) and also invest in human capital (e.g. health and education spending). Once appropriate controls are used for domestic and international factors, democracies are not found to spend greater shares of resources on social security or health. In contrast, the authors report that elected officials allocate greater shares of resources to public education echoing earlier research by some of same researchers (Brown and Hunter 2004).

Thus far, there are contrasting views on whether Latin American democracies invest greater shares of government resources for social spending. Notwithstanding theoretical foundations for why democracies are more sensitive to popular pressures, empirical studies have thus far provided weak evidence of statistically significant increases in welfare spending under competitively elected governments. Even if political motivations induce both democrats and autocrats to channel government resources to enhance powers, the mechanisms and strategies employed by regimes seem to differ significantly. One particularly important phase that may help understand the differences that motivate both regimes is to examine what happens to social spending during the transition period when authoritarians hand over rule to leaders that are elected through multiparty competitive elections. Thus far, there has been limited research to examine whether these results may be due to differences in social safety net resource allocations between recently established and consolidated democracies. There are important reasons why the monotonic assumption of democratic regimes on welfare spending may be incorrect in Latin America and why this question merits greater attention.⁶⁰ It is to this factor that we now turn to focus on.

4.2.3 *New Democracies*

Following a period dominated by authoritarian governments, the politics of Latin America were deeply transformed by a wave of democratization that started in the late 1970s with the election of Jaime Roldós Aguilera in Ecuador (Huntington 1991). Indeed, transitions to democracy occurred in Argentina, Bolivia, Brazil, Chile, Dominican Republic, Ecuador, El Salvador, Guatemala, Peru and Uruguay between 1978 and 1990. While Costa Rica, Colombia and Venezuela have a long and uninterrupted democratic history that has persisted through most of the 20th century, other nations in the region, such as El Salvador and Guatemala, have been ruled by authoritarian regimes for the majority of the 20th century. The inauguration of José Napoleon Duarte and Vinicio Cerezo, the elected leaders of these respective countries, were markedly distinct from prior transitions as the point marking the beginning of now more than two decades of democracy. In other nations, such as Brazil and Argentina democratic and authoritarian regimes had oscillated for a significant part of the 20th century. The fact that these

⁶⁰ There are also important nuances and differences within authoritarian regimes. However, unlike the transition to democracy which is recognized as a clear-cut process and the establishment of a consolidated democratic regime for which several methods have been developed and tested, there has not been much empirical work aimed at developing criteria for discerning between stable and transitory authoritarian governments.

political transitions occurred more or less concurrently with economic crisis and openings to world markets gives special salience to whether new democracies were more responsive in their allocation of budgets towards education, health and social security.⁶¹

There has been considerable research suggesting that there are significant differences between established and new democracies and their impact on economic policy outcomes in developing countries and Latin America in particular (Sloan and Tedin 1987; Remmer 1990; Haggard and Kaufman 1997; Gasiorowski 2000; Weyland 2002; Rodrik and Wacziarg 2005; Bernhard et al. 2003; Brender and Drazen 2005). Several explanations have been offered for why new democracies are more likely to produce poor economic outcomes relative to consolidated democracies.⁶² Przeworski (1991) notes that coalitions demanding greater redistribution are a key variable driving democratization. Accordingly, it is argued that incoming elected governments during transitions are less able to respond effectively to critical economic challenges because they come to power facing a huge backlog of unfulfilled demands. Based on the recognized confluence of economic and political crisis that usually precipitate democratic transitions, scholars have argued that newly elected governments find themselves needing to adopt policies that are unsustainable in the medium to long-run given the high stakes involved threatening a reversion to autocracy (Haggard and Kaufman 1989). Haggard and Webb (1993) cite the experiences of incoming elected governments in Argentina, Bolivia and Brazil in the aftermath of the debt crisis as examples of countries that pursued expansionist, unsustainable policies to acquiesce to popular demands resulting in economic havoc and rampant hyperinflation. Bernhard, Reenock and Nordstrom (2003) emphasize the time contingency of democratic transitions. Incoming governments lack long-term track records that insulate them from the perception that short-term policy setbacks are more major efficacy and/or effectiveness problems. As a result, these regimes are judged more harshly for their failures to overcome legacies of dictatorship.

⁶¹ The exception to this wave of democratization is the reversion of Peru to authoritarianism in the 1990s. In April 1992, Alberto Fujimori and the Peruvian military closed down the Peruvian Congress. In this chapter, Fujimori's entire presidency from 1991-2000 is classified as authoritarian and appropriately coded for the entire period following the rules of Przeworski, Alvarez, Cheibub and Limongi (2000). Social spending data for Peru is only available between 1973 and 1986 and 1999-2000. All of the models were also estimated without the two reverted years for Peru and the results of the findings reported below remain the same.

⁶² A slightly different viewpoint is offered by Mancur Olson (1996), who contends that distributional coalitions are more powerful in older, more established democracies. In established democracies, older and more entrenched patterns of power asymmetries tend to prevail.

The reasons that have been postulated for the purported weakness of new democracies have been contested. Remmer (1990) argues that new democracies have a distinct advantage as the “political goods” produced by the transition give elected governments capital that compensate voters for declining per capita incomes. Indeed, this “honeymoon” effect was emphasized by Hirschman (1987) who observed that, “new democratic governments [post-transition Argentina and Brazil in the 1980s], especially when they take over from detested authoritarian regimes, have to cope with a new burst of combativeness of social groups. But at the same time, the new governments can call upon a special reserve of goodwill and trust as a result of the political liberties and human rights that they have restored or established (28).” The strongest argument put forward to counter claims that new democracies poorly manage their economies is their survival. As Remmer (1990) notes, “Despite repeated prognoses of collapse, every Latin American democracy, whether old or new, weathered the first eight years of the debt crisis (320).”

Part of the reason for the survival of these new democracies in Latin America despite the severity of the crisis confronted in the 1980s and 1990s may be due in part to this honeymoon effect. New democracies have unique strengths that enable them to respond to economic crisis more effectively than old democracies. With reference to the resiliency of new democracies in the aftermath of the debt crisis, Remmer concludes that “political leaders are aware that the rise and the fall of democracy in Latin America have corresponded less to the whims of the voting majority than to the concerted opposition of business and military elites (335).” Cognizant and responding to these pressures, honeymoon democracies may have been more willing to adopt responsible economic management practices including stabilization packages producing improved economic outcomes over the medium-term.

Sparked by this debate, considerable attention has been directed at exploring the impact of democratic transitions on macroeconomic variables including economic growth, inflation, external indebtedness, fiscal deficits, unemployment and wages in developing countries with cross-country multivariate regression analysis. Newer and less established democracies have not been found to grow at reduced rates or exhibit greater levels of macroeconomic volatility (Rodrik

and Wacziarg 2005).⁶³ In addition, recent evidence suggests that there are important differences with respect to how new democracies balance their budgets and spend tax dollars. Brender and Drazen (2005), who conceive the experience of new democracies as represented by the first four competitive elections, presents robust evidence that fiscal deficits and public expenditures increase significantly during democratic transitions in both developed and developing countries between 1960 and 2001.⁶⁴ The authors contend that new democracies are more susceptible to information problems that may either affect voters or politicians. As a result, the authors hypothesize that political budget cycles are more likely to occur in countries with less of an electoral history, and hence less exposure to pre-electoral fiscal spending sprees. Fiscal manipulation may work when voters lack the necessary information to draw inferences about the state of the economy, as well as the ability to process that information correctly. As they note, “in many new democracies, even basics like the collection of data and reporting it to the public are not well established, so that fiscal manipulation is easier to engage in (1290).” In addition, incumbents in new democracies may have difficulties in identifying pivotal voters due to information problems and as a result have to distribute transfers to woo voters more widely, implying higher aggregate government deficits.

New democracies may increase public spending to increase their political clout, but there may be more stringent limits on the overall magnitude of changes in resource allocations for these types of governments as compared to established democracies. It has been argued that newly installed democratic regimes are less likely to engage in radical efforts to redistribute resources (McGuire and Olson 1996). The reason offered for the moderation of economic management in new democracies is that extreme policy measures threaten their security and authority. This point was most eloquently formulated by O’Donnell and Schmitter (1986) who argued that the less radical redistribution, the more likely the transition will succeed, and the greater are the survival chances of democratic regimes. As they note, “All previously known transitions to political democracy have observed one fundamental restriction: it is forbidden to take, or even to checkmate the king of one of the players. In other words, the property rights of the bourgeoisie are inviolable (69).”

⁶³ Rodrik and Wacziarg (2005) define new democracies as the first five years following transition.

⁶⁴ Brender and Drazen (2005) only examine democracies. The criteria used to select the sample of developing and developed democracies is a cutoff score ranging between 0 and 10 in Polity IV.

There is a significant lacuna of broad-based, empirical evidence on democratization and social policy resource allocations in Latin America. Thus far, only a single study has examined the effect of democratic transitions on social spending in a multivariate setting. Avelino (2005) reports that both new and established democracies, which are defined depending on whether the elected government has survived a single turnover in executive power following founding democratic elections, devote greater priority to aggregate social spending as compared to authoritarian regimes. However, contrary to theoretical expectations, higher levels of social spending are also associated with consolidated democracies in the study of 17 Latin American countries between 1980 and 2000. Kaufman and Segura-Ubiergo (2001) and Segura-Ubiergo (2007) test for whether the lagged level and the yearly change in democracy is significant in explaining welfare expenditures. The results of their multivariate analysis do not indicate a statistically robust relationship between democratic change and social spending. Perhaps due to the difficulty in assessing what yearly changes in democratic regimes exactly measures, the authors are silent on the theoretical significance of this specification and do not explore its significance in either study.

There are compelling reasons to test the effect of new democracies on social spending using measures that capture these distinct types of democratic regimes. Failure to find evidence of enhanced performance for new democracies would indicate that increased responsiveness to social spending is not a ubiquitous feature of the transition but rather an important feature of regime type irrespective of its time contingency. On the other hand, we might expect to see differences in how resources are allocated for public health, education and social security when new democracies assume power. Democratic transitions are moments of dramatic increases in citizen representation and franchise and also uncertainty. Younger democracies in Latin American may be more responsive to voters or alternatively may be less able to distribute resources more equitably and prioritize social spending.

4.3 Econometric Models and Estimation Methods: Time-Series-Cross-Section Analysis of Democracy and Social Spending

4.3.1 Methodological Issues

The inconclusive results of previous research may be the result of inappropriate methods. There are three key methodological problems with previous research: (1) measurement error of the dependent and key independent variables; (2) dynamic specification; and (3) simultaneity bias. Of the three problems, (2) and (3) are the most serious and none of the aforementioned studies of democracy and social spending control for these problems. The sub-sections that follow summarize each problem and the solutions that will be adopted to address each issue and used in the multivariate analysis in Section 5.

4.3.1.1 Measurement Error

Most studies discussed in this chapter have preferred to utilize social spending as a share of GDP as the dependent variable. There are two significant problems with the measure traditionally used in studies analyzing the relationship between democratization and welfare expenditure in Latin America. The problems introduced in this measure are best illustrated with a few examples. First, measuring welfare effort as a share of GDP is strongly impacted by macroeconomic fluctuations. The last three decades of the 20th century were marked by distinct phases of macroeconomic and fiscal administration in Latin America. Whereas the 1970s can be characterized as a period of economic and fiscal expansion financed through increased borrowing from international markets, Latin America entered the debt crisis and underwent a period characterized by economic contraction, budget austerity, and capital flow reversal starting in 1982. While these pressures eased in the early 1990s, a number of financial crises slowed growth again by the mid and particularly late 1990s. These changes influence the behavior of social spending measured as a share of GDP. During fiscal contractions governments may be forced to reduce social spending but proportionately less so than other expenditures. However, the change in public expenditure on social spending measured as a percentage of GDP would show a negative change, whereas welfare spending share of government expenditure would show a positive change.

Second, the measure of social spending relative to GDP is also distorted by the relative size of governments. Governments that comprise a larger share of their national economy will be analyzed as dedicating a similar effort to social programs, though some countries are only investing a relatively small share of their budget on welfare. Third, the social spending data employed the majority of studies is based on the amount of central government expenditures directed at social programs in education, health and social security. For countries in Latin America, such as Argentina and Brazil, that have undertaken significant fiscal decentralization of education and health spending in the mid 1990s the allocation of resources towards these sectors may have increased, but the indicator that reports welfare expenditure levels as a share of the economy would be decreasing and therefore incorrectly indicating that these expenditure levels are decreasing. The problem may be particularly serious for Latin American countries that underwent fiscal decentralization and democratization.

For these reasons the modeling of the dependent variable as social spending relative to GDP may not adequately measure the allocative priority given to social welfare within the economy as a whole. Therefore, the dependent variable is modeled in relative terms, which is measured as the level of social spending relative to total government spending. This measure permits us to examine the priority that governments place on social spending within a given public budget and thus to more directly capture the effect of political pressures on spending.

Democracy and democratization are the key independent variables of interest in this chapter. Oftentimes, more rigorous and theoretically grounded measures of both terms have been neglected in the literature. Most studies on the impact of elected regimes on social spending in Latin America have employed either a continuous measure of democracy based on the Polity III and the updated Polity IV dataset created by Marshall, Jagger and Gurr (2005) or a trichotomous classification of Latin American regimes (democratic, semi-democratic and authoritarian) elaborated by Mainwaring, et. al.(2001) to create a dichotomous measure of democracy based on a specific threshold score.⁶⁵ The sole exception to this problem is the two studies by Avelino,

⁶⁵ In the Polity IV dataset, a country's given score on democracy in a given year is based on a ten-point scale, which assigns a score for the competitiveness of political participation (1-3), the competitiveness of executive recruitment (1-2), the openness of executive recruitment (1), and the constraints on the chief executive (1-4). The negative version of the same indices is used to classify authoritarian regimes and the two scales are combined to yield a single democracy-autocracy score varying from -10 to +10.

Brown, and Hunter (2005) which defines democracy on the categorical criteria employed by Álvarez, et. al. (1996) that will also be employed in this chapter. Departing from the premise that democratization is a clear-cut process, the use of the alternate graded measures fail to reflect the dramatic differences between authoritarian and democratic regimes resulting in measurement validity problems (King et al. 1994).

Kaufman and Segura-Ubiergo (2001) create a dichotomous measure based on the Polity III and IV data sets in which they classify a regime as democratic if it scores 6 or greater. Huber, Mustillo and Stephens (2004) use the same data set to create a cumulative measure of democracy based on the scores each country accrued from 1945 to 2000. Segura-Ubiergo (2007) classified all semi-democratic regimes as being democratic if they were above a given threshold in the Polity data and thus created a dichotomous regime measure. The problems posed by methods based on Polity classifications of democratic regimes in Latin America in the period in question occur for several years in four countries. Using this criteria, Guatemala is considered a democracy in 1986 ten years after the inauguration of Vinicio Cerezo's term (Nohlen 2005). Mexico is considered a democracy after 1997, although Vicente Fox is only inaugurated in 2000. Panama is classified as a democracy in 1989 when Guillermo Endara is installed as President following the U.S. military intervention, five years earlier than the first election after the invasion. Paraguay is coded as a democracy in 1992, although Juan Carlos Wasmosy is only inaugurated into office in August 1993 following elections in May of the same year.

A similar problem concerns the strategy employed by Wibbels (2006) who includes democracy as a continuous variable based on a country's Polity IV score in each year. An interpretation of this measure as Álvarez et al (1996) argue is difficult. The increase in democratization resulting from a particular increase in a country's Polity score is not equivalent. This is best illustrated with an example. A shift from an "almost authoritarian" democratic regime with a low Polity score may have equally as important a quantitative impact as a shift within a democracy that moves from a transition to a consolidated stage.

To address this issue, this study departs from the premise that democratization is a clear-cut process and its effects are best measured by a dichotomous variable that codes one for

democracies and zero for the residual category of authoritarian regimes. This measure is based on a minimalist definition of democracy and builds on the seminal work of Schumpeter (1942) and more recently Sartori (1987). A democratic regime is defined as one in which the executive and the legislature are both filled by “contested elections.”

As the review of the literature in Section II of this chapter has attempted to highlight, with few exceptions the empirical work on new democracies has been based on poorly developed definitions that largely neglect the theoretical body of literature that has been dedicated to the question in political science. The political economy studies of Rodrik and Wacziarg (2005) and Brender and Drazen (2005) have both set arbitrary time caps to differentiate between younger and older elected governments. In fact, age is not the deterministic criterion that differentiates elected government type. Established democracies are not only older, they have developed and exercised mechanisms to share power among groups with diverging interests and it is this feature that should be employed to test whether redistributive politics operate differently when elected governments have yet to fully develop the power to endure turnovers in power. As Chapter 2 described, new democracies are modeled with a measure that is directed at appropriately measuring the consolidation of democracy based on the two turnover test proposed by Huntington (1991). This measure is used because it is a tough and rigorous measure that provides a clear cut mechanism for dividing democracies by their stage of development.

4.3.1.2 Alternative Dynamics

A second and more problematic methodological difficulty has to do with the model used to capture the dynamic specification on the impacts of democratization and globalization on social spending in Latin America. Three of the seven published studies summarized in Table 4.1 employ pooled time series cross-section data and estimation by Ordinary Least Squares (OLS) with Beck-Katz panel corrected standard errors (PCSE), one employs an AR1 model with PCSEs and three use an Error Correction Model Beck-Katz panel corrected standard errors.⁶⁶ PCSEs are used to correct OLS standard errors for groupwise heteroskedasticity and contemporaneous correlation of the errors due to global processes that affect all the countries in the sample in a given year (Beck and Katz 1995). Based on the assumption that past levels of government

⁶⁶ The error correction model is recommended by Beck and Katz to address the problem of unit roots.

spending on social services influence the levels of expenditures in future years on social security, education and health services, a lagged dependent variable is included in all of the cited papers. Given that the lag model specification violates the assumption of strict exogeneity, the possibility of a feedback effect of y_{it} to future values of x_{it} is introduced (Woolridge 2002). Under these conditions, regression estimation based on OLS with panel corrected standard errors (PCSE) or within groups estimation is likely to provide biased estimates (Woolridge 2002; Hsiao 1986).⁶⁷ The magnitude of the bias in the estimates reported in previous studies depends on the sample size and how many countries do not have data for the entire period (Nickell 1981).

Recognizing the potential the bias in OLS coefficients, Beck and Katz (2004b) advocate careful data diagnostics to test for common statistical problems and also advise the need to use different dynamic models depending on the results for the specific data set.⁶⁸ As discussed in further detail in Chapter 3, the problems posed for time-series-cross-section (TSCS) data analysis with unit roots are a serious cause of concern. Yet, none of the aforementioned seven studies report tests for state dependence on the share of government social spending as a share of total spending controlling for unobserved country effects, nor do they publish the results of alternative dynamic models.⁶⁹ This is not a problem specific to these studies. As Wilson and Butler (2007) have noted, within the political science literature the subject has received recent, albeit limited attention. As explained in further detail below, the chapter will test whether the published results are robust to alternative dynamic structures that are different from the model proposed by Beck and Katz.

⁶⁷ For a discussion on the problems posed by the introduction of the lagged dependent variable and the tests that should be carried out for the estimation model, see Woolridge (2002) pp. 299-305. Brender and Drazen (2005) argue that the potential estimation bias introduced is of order $1/T$, where T is the length of the panel. Hence, they conclude that the bias from using a fixed effects estimator in these regressions is likely to be small.

⁶⁸ Beck and Katz (2004b) note that in the case of time-series-cross-section (TSCS) data the number of T are often greater than 10 and even as high as 30. In these cases, they argue that the proposed fixes to the biases for OLS regression analysis involving complicated alternative specifications may not be worth their costs. They therefore recommend that prior to adopting alternative specifications to OLS, tests should be carried out to explore if the size of the inconsistency is small or substantial.

⁶⁹ Segura-Ubierno (2007:147) reports that the results of his paper were also carried out using robust regression (rreg), Maximum Likelihood (ML), Generalized Methods of Moments, and Generalized Least Squares (Prais-Winsten correction for serial correlation) and that these different models did not produce major substantive changes in his results. He does not discuss if tests were conducted to explore the problems of unit roots and stationarity, but does report that ECM model was used to address unit roots.

4.3.1.3 Simultaneity Bias

A final methodological difficulty is caused by simultaneity bias, a problem that is largely ignored in most studies that examine the impact of democracy on social spending. Controls for this issue are particularly important because of the potential endogeneity between government spending and democratization. The findings on the causal effects of democracy on welfare effort may also be due to the inverse process whereby social spending affects democracy. Thus, the inferences emerging from this body of literature might be contaminated by reverse causation. In order to address the question of simultaneity bias, empirical estimation will include models using instrumental variables. These models will be discussed in the next section.

4.3.2 Estimation Procedure and Model Specification

The baseline model was introduced in Chapter 3. In this chapter tests are carried out on the effect of democracy. We can thus summarize the model adding specificity for the key independent variable of interest in this chapter:

$$Y_{i,t} = \beta_1 Y_{i,t-1} + \beta_2 \mathbf{Z}_{i,t} + \beta_3 \mathbf{Democracy}_{i,t} + c_i + \mu_{i,t}. \quad (1)$$

The dependent variable is the share of social spending as a share of total government spending, $Y_{i,t-1}$ is a one-period lag of the dependent variable, \mathbf{Z}^{70} is a vector of control variables and α_i represent year dummies. 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 openness of a given country. The error term, $\mu_{i,t}$, is an error term associated with unit i at time t . In addition to modeling how democracy impacts aggregate social spending, the disaggregated measures of education, health and social security budget shares will be tested as dependent variables in order to examine whether the impact of regime type on welfare spending is of similar magnitude for different types of social spending expenditures.

⁷⁰ There is one control variable, GDP growth, that is lagged one year. To simplify notation, it is not added as a separate variable in the equation.

Thus, the model seeks to explore how changes in the level of openness impact welfare state dynamics in Latin America. As has been discussed in detail in Chapter 3, hypothesis testing is particularly challenging if the data follow a unit root. Given the uncertainty regarding the stationary character of the data, a test on the *level* of welfare state development will be carried out using three models: (1) pooled OLS with panel corrected standard errors;⁷¹ (2) (3) the Arellano and Bond (1991) first-differenced GMM estimator (GMM-Diff); and, (4) the Blundell and Bond (1998) system GMM estimator (GMM-System).⁷² To examine whether results are altered when estimation is carried in models more appropriate for highly persistent series, the results of a fourth model that is an error correction model (ECM) with panel corrected standard errors based on the *first difference* of the dependent variable will also be reported in Appendix 4A. Estimation was carried out with and without fixed effects to address issues of unit heterogeneity.⁷³ Thus, the pooled OLS, GMM and ECM estimates will also be estimated with and without controls for time and unit heterogeneity. As the results with and without fixed effects are reported in the tables presented below there will be a total of seven columns.

4.4 Data

4.4.1 The Social Spending Data

The dependent variable in this chapter is based on annual data on central government total expenditures and expenditures by welfare function as reported in the International Monetary Fund (IMF)'s *Government Finance Statistics* (GFS) (International Monetary Fund 2006). Data on the share of expenditures allocated to education, health care and social security relative to total government spending were compiled based on the procedures and criteria explained in

⁷¹ The model will be estimated with the Stata XTPCSE command.

⁷² The exercise and commands for GMM estimation are based on Roodman (2006) and were carried out using in Stata 10.

⁷³ 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).

Chapter 2.⁷⁴ An aggregate measure of all three expenditures relative to total government spending is used to measure welfare effort. Social spending is also grouped by the share of expenditures on health and education combined and the share spent on social security transfers.

4.4.2 The Democracy and Democratic Transition Data

This study departs from the premise that democratization is a clear-cut process and its effects are best measured by a dichotomous variable that codes one for democracies and zero for the residual category of authoritarian regimes. The data are drawn from the Democracy codebook presented in Chapter 2. A democratic regime is defined as one in which the executive and the legislature are both filled by “contested elections.” The beginning of democratic transition is defined as the year of the inauguration of the first democratic regime following a period of authoritarian rule. 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.

The data employed draws on an updated version of the Álvarez, Cheibub, Limongi and Przeworski (1996) and Przeworski, Álvarez, Cheibub and Limongi (2000) datasets by Cheibub and Ghandi (2004) and a modification of these datasets by Avelino (2006) for Latin America from 1980-2000. The classification of democratic and authoritarian regimes in Nohlen (2005) was used to cross-examine the coding employed in the aforementioned datasets. The full classification of the data is discussed in Chapter 2 and detailed in Appendix 2B.

4.4.3 Control Variables

Several demographic and economic variables are included as control variables; each is drawn from the theoretical literature. Demographic characteristics of the population are likely to impact education, health care and social security spending. Two demographic variables representing the fraction of the population aged 15–64 and 65+ are employed as controls in the

⁷⁴ Housing expenditures were not included as part of social spending. These expenditures are typically small.

regressions where total welfare expenditures and social security transfers are the dependent variables. A higher percentage of elderly people in the population is expected to be positively related to social security transfers and health spending. Unless otherwise noted, the control variables data are from the World Bank's *World Development Indicators* (2007).

In the last three decades of the 20th 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.⁷⁵ 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.

Given the heterogeneity in income and growth rates across the region, it is important to include economic variable controls. The first is the level of economic development, defined as the real gross domestic product per capita and measured in constant US\$ 2000 dollars. Per capita income is included in the model to control for Wagner's Law, which 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 social spending.

A control for GDP growth lagged one year was 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 and this pattern is even more acute in the case of social spending (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.

⁷⁵ Ames (1987:79)

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 imports + exports/GDP, where the denominator is calculated by converting domestic local currency to current US\$ based on exchange rate conversions. This measure is affected by the size of the economy and by changes in the exchange rate, but the inclusion of country dummies as in the model is assumed to correct for these effects.

Capital mobility is included as its expected impact on social spending is expected to be distinct from trade flows.⁷⁶ 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).⁷⁷ Quinn attempts to measure the intensity of capital controls through a more detailed reading of the IMF's *Annual Report on Exchange Arrangements and Exchange Restriction*. The degree of capital account restrictions, *Capital mobility*, is measured from 0 to 4 with the higher values given to more open economies. The extent of controls on international financial regulation, *Financial openness*, is rated similarly on a scale of 0 to 14 and measures the degree of liberalization of both the capital and current account plus the adherence to international legal agreements that regulate exchange and capital flows. It has been standard in the literature to include a control for capital mobility based on the premise that responses to government policy outcomes will be higher when there is greater capital mobility. This variable has not been found to be particularly significant in empirical studies on social spending. One reason that has been offered is that foreign investors do not react by shifting investments based on changes in the resources allocated to government social programs (Segura-Ubiergo 2007).

⁷⁶ The correlation between trade integration and capital mobility is surprisingly low for the sample (0.3321). There is a wide variation and sometimes negative relationship between trade openness and capital mobility across countries in Latin America.

⁷⁷ See Avelino, Brown and Hunter (2005) for a more detailed explanation of this variable.

4.5 Results of Time-Series-Cross-Section Analysis: The Effect of Democracy and Democratic Transitions

The data analysis for this chapter uses a variety of time-series-cross-section models to test the robustness of the suggested relationship between democracy and social spending.⁷⁸ The empirical results presented below offer fairly strong and robust results that showing how regime type has important and distinct impacts on both health and education expenditures and social security transfers. The sign and significance levels of the coefficients are confirmed across estimations. Several important findings are contrary to past empirical analysis. The sections that follow analyze and discuss separately the results on the impact of democracy and democratic transitions on aggregate and disaggregated social spending. Afterwards, the chapter discusses what the empirical results reveal about the impact of alternative specifications for welfare commitments in Latin America.

4.5.1 Testing the Effects of Democracy

The regressions reported in Table 4.2, 4.3 and 4.4 yield three important findings: (1) there are no differences in the total amount of resources allocated for social programs as part of the central government budget between democratic and authoritarian regimes over the last three decades in Latin America; (2) democracies allocate less resources for social security relative to non-democratic governments; and, (3) democracies spend more on health and education relative to authoritarian regimes.

Table 4.2 provides evidence that democratic regimes in Latin America do not have predictable impacts on aggregate social spending as a share of total government expenditures across the seven types of models employed in hypothesis testing in this chapter. However, political control by elected governments does matter a great deal when social spending is disaggregated. The most surprising and unexpected result is the finding indicating that democracies spend less on pensions relative to authoritarian regimes. The average share of the

⁷⁸ In addition, the models were also estimated with two lags of the dependent variable on the right hand side. These models did not register differences in the significance levels and signs of the coefficients.

budget spent on social security increased dramatically between 1973 and 2000 from 16.7 percent of the budget to 29.1 percent. Relative to health and education expenditures, these payments are the portion of social expenditures that have experienced the greatest increase over the last three decades and have risen particularly more during the last fifteen years when democracies surpassed authoritarian regimes as the predominant regime in the region. The pooled OLS models also confirm that democracies with larger shares of the population that are over the age of 65 are more likely to increase pensions as a share of the budget. Table 4.3 demonstrates this result is robust and confirmed whether the analysis is based on pooled OLS with PCSE, pooled OLS with PCSE, year and country fixed effects, or GMM difference estimations.

The findings of the propensity of democracies to reduce social security expenditures contrast in important ways with past research on earlier and shorter periods in Latin American politics. Moreover, the estimated impact of democracy on social security is not only statistically significant, it is substantively important as well. For the average country in the sample, 48.1 percent of the central government budget is allocated to social security payments.⁷⁹ The estimates predict that democracies will allocate between 1.1 percent and 2.2 percent less of its budget on average for pensions. To better illustrate this with an example. Mexico spent 48 percent of its budget on social security. By 2000, the share of Mexico's budget devoted to social security was equivalent to 7.4 percent of GDP. In the same year Mexico's budget represented the equivalent of 15.4 percent of GDP and its economy was estimated to be over US\$ 581 billion. Using a low estimate of 1.5 percent, social security expenditures are predicted to be 0.23 percent less of GDP after democratization. This represents the equivalent of a US\$ 1.3 billion.

After controlling for per capita income levels, economic growth, globalization and demographic factors, the models predict that the share of the budget devoted to education and health is higher in democracies as compared to authoritarian regimes. The results reported in Table 4.4 are robust across all but GMM system specifications. Specifically, democratic regimes are predicted to allocate greater shares of the budget to education and health when the model is estimated using pooled OLS with PCSE, pooled OLS with PCSE, year and country fixed effects, and GMM difference estimators.

⁷⁹ The actual share is less as this percentage is relative to the budget after interest payments have been deducted since we are following the methodology proposed by Kaufman and Segura (2001).

Table 4.2. The Effect of Democracy on Social Spending in Latin America, 1973-2000*Dependent Variable: Share of Welfare Spending as a Share of Total Government Spending*

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One-Step First Differences ^a	(5) GMM One- Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.769*** (0.035)	0.471*** (0.047)	0.470*** (0.049)	0.687*** (0.169)	0.555** (0.206)	0.720*** (0.168)	0.720*** (0.180)
% of population aged 65 and over _t	0.232 (0.217)	3.041*** (0.669)	0.117 (0.875)	1.826 (1.852)	0.283 (1.389)	-0.055 (0.692)	0.522 (0.718)
% of population aged 15 to 64 _t	0.721*** (0.156)	0.151 (0.191)	0.035 (0.192)	0.036 (0.181)	0.117 (0.263)	0.782 (0.662)	0.509 (0.557)
%urban population _t	-0.067** (0.029)	0.350*** (0.116)	-0.099 (0.212)	0.257** (0.089)	-0.290 (0.257)	-0.208 (0.350)	-0.032 (0.234)
GDP per capita _t (constant 2000 US\$) (WDI)	0.000 (0.000)	-0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	-0.003 (0.002)	0.002 (0.004)	0.000 (0.003)
GDP per capita growth _{t-1} (annual %) (WDI)	0.025 (0.049)	-0.035 (0.051)	-0.017 (0.066)	-0.196 (0.153)	-0.222 (0.162)	-0.011 (0.096)	0.088 (0.112)
Trade Openness _t	-0.030* (0.018)	-0.027 (0.025)	-0.076*** (0.029)	-0.026 (0.036)	-0.058 (0.048)	-0.031 (0.058)	-0.033 (0.051)
Capital Mobility Index _t	0.748** (0.353)	0.591 (0.429)	0.496 (0.471)	0.253 (0.798)	0.427 (0.628)	0.640 (0.891)	0.521 (0.795)
Democracy _t	-0.030 (0.471)	-0.139 (0.680)	0.134 (0.794)	-0.172 (0.898)	0.783 (1.410)	0.715 (1.242)	-0.241 (1.069)
Constant	-27.678*** (6.234)	-25.796*** (4.951)	42.665* (21.778)			-25.466 (19.883)	-13.142 (19.639)
Observations	351	351	351	331	331	351	351
R-squared	0.80	0.84	0.85				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (<i>p value</i>) ^b				0.466	0.594	0.556	0.476
Number of Instruments				12	38	16	42
Hansen test for joint validity of instruments (<i>p value</i>)				0.216	1.000	0.980	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a One lag of the dependent variable was 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 (2007a). 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.

^b 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 4.3. The Effect of Democracy on Social Security Spending in Latin America, 1973-2000*Dependent Variable: Share of Social Security Expenditures as a Share of Total Government Spending*

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One-Step First Differences ^a	(5) GMM One- Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.789*** (0.035)	0.510*** (0.048)	0.528*** (0.048)	0.711*** (0.189)	0.664** (0.284)	0.844*** (0.147)	0.750*** (0.159)
% of population aged 65 and over _t	0.628*** (0.218)	2.761*** (0.684)	1.722* (0.891)	1.510 (1.034)	1.277 (1.546)	0.321 (0.521)	0.787 (0.668)
% of population aged 15 to 64 _t	0.491*** (0.168)	-0.296** (0.149)	-0.295** (0.149)	-0.215 (0.182)	-0.195 (0.232)	0.326 (0.278)	0.318 (0.321)
%urban population _t	0.006 (0.045)	0.477*** (0.129)	0.408** (0.189)	0.348** (0.144)	0.296 (0.233)	-0.033 (0.106)	0.013 (0.102)
GDP per capita _t (constant 2000 US\$) (WDI)	-0.000 (0.000)	-0.001 (0.000)	-0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	0.000 (0.001)
GDP per capita growth _{t-1} (annual %) (WDI)	0.092** (0.047)	0.043 (0.049)	0.053 (0.056)	-0.013 (0.079)	-0.044 (0.133)	0.051 (0.050)	0.093 (0.080)
Trade Openness _t	-0.082*** (0.023)	-0.085*** (0.028)	-0.111*** (0.032)	-0.071** (0.025)	-0.092** (0.036)	-0.061 (0.044)	-0.089 (0.053)
Capital Mobility Index _t	0.626** (0.257)	1.257*** (0.400)	1.424*** (0.451)	0.751 (0.580)	1.058 (0.979)	0.480 (0.503)	0.563 (0.564)
Democracy _t	-1.181** (0.462)	-2.148*** (0.748)	-1.925** (0.874)	-1.699*** (0.343)	-1.443** (0.494)	-0.661 (0.708)	-1.534* (0.846)
Constant	-23.334*** (6.954)	-20.893*** (7.638)	-9.660 (17.517)			-14.563 (11.838)	-13.409 (13.735)
Observations	351	351	351	331	331	351	351
R-squared	0.91	0.92	0.93				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (<i>p value</i>) ^b				0.511	0.668	0.509	0.574
Number of Instruments				12	38	16	42
Hansen test for joint validity of instruments (<i>p value</i>)				0.198	1.000	0.766	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a One lag of the dependent variable was 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 (2007a). 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.

^b 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 4.4. The Effect of Democracy on Health and Education Spending in Latin America, 1973-2000

Dependent Variable: Share of Health and Education Expenditures as a Share of Total Government Spending

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One-Step First Differences ^a	(5) GMM One- Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.806*** (0.051)	0.547*** (0.085)	0.536*** (0.090)	0.110 (0.653)	0.029 (0.272)	0.267 (0.275)	0.466* (0.264)
% of population aged 65 and over _t	-0.388*** (0.117)	0.169 (0.483)	-1.683* (0.859)	-6.983 (12.494)	-3.002* (1.485)	-1.086 (1.365)	-0.643 (0.988)
% of population aged 15 to 64 _t	0.161 (0.130)	0.405** (0.169)	0.310* (0.163)	0.190 (1.282)	0.851** (0.304)	0.610 (0.787)	0.387 (0.576)
%urban population _t	-0.060 (0.047)	-0.130 (0.137)	-0.504** (0.244)	0.110 (0.665)	-1.099* (0.566)	-0.203 (0.509)	-0.074 (0.326)
GDP per capita _t (constant 2000 US\$) (WDI)	0.000 (0.000)	0.000 (0.000)	-0.001 (0.001)	0.015 (0.025)	-0.003 (0.004)	0.000 (0.005)	-0.001 (0.003)
GDP per capita growth _{t-1} (annual %) (WDI)	-0.062 (0.042)	-0.066 (0.043)	-0.055 (0.051)	-0.329 (0.271)	-0.432** (0.181)	-0.091 (0.144)	-0.040 (0.062)
Trade Openness _t	0.051*** (0.016)	0.059** (0.026)	0.040 (0.030)	-0.130 (0.351)	0.031 (0.068)	0.130 (0.075)	0.100 (0.078)
Capital Mobility Index _t	0.048 (0.258)	-0.744** (0.376)	-1.017** (0.426)	-0.448 (1.536)	-1.173 (1.128)	0.197 (0.826)	0.160 (0.731)
Democracy _t	1.127*** (0.359)	1.914** (0.809)	1.994** (0.941)	1.062 (3.291)	3.436** (1.336)	2.754 (2.197)	2.330 (2.211)
Constant	-2.180 (5.414)	-3.446 (5.563)	50.862** (25.058)			-5.615 (18.871)	-2.880 (18.589)
Observations	351	351	351	331	331	351	351
R-squared	0.86	0.89	0.90				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (<i>p value</i>) ^b				0.383	0.215	0.260	0.242
Number of instruments				12	38	16	42
Hansen test for joint validity of instruments (<i>p value</i>)				0.923	1.000	0.899	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.642	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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a One lag of the dependent variable was 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 (2007a). 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.

^b 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.

Some comments on the differences across specifications are also in order. First, the results are generally consistent whether measured by pooled OLS, pooled OLS with country and time fixed effects, as well as in GMM First-Differenced and GMM-System models. The results presented in Tables 4.2, 4.3, and 4.4 are also confirmed in estimations tests with an Error Correction Model and are presented in the Appendix 4A. As discussed in Chapter 3, Bond (2002) notes that the pooled OLS and fixed effects estimators provide boundaries for checking the results of GMM estimates. In the case of social spending and pension budget shares, the GMM specifications yield estimates that fall within the predicted range. There calibration of GMM estimates in the case of health and education spending, however, do not seem to be as precise. By presenting the models with and without controls for unit and time invariant effects, the results presented in Tables 4.3 and 4.4 also provide some insights into the differences within and across units. The estimated coefficients on democracy are stronger in magnitude after fixed controls, but remain within the 95% confidence interval predicted by the pooled model reported in the first column.

4.5.2 Testing the Effects of New Democracies

Tables 4.5, 4.6 and 4.7 seek to explore whether the responsiveness of democracy to citizen welfare in Latin America is explained by whether countries have had less than two-turnovers of elected governments between factions.⁸⁰ The specifications all test the same model employed in Tables 4.2 (social spending), Table 4.3 (social security) and Table 4.4 (health and education spending) adding a new variable that codes one if the specific year in question was a democratic transition year (*New Democracy*). This coefficient ($\hat{\beta}_4$) measures the marginal impact of being a new democracy and thus the total effect of a new democracy is derived from adding this coefficient with the coefficient measuring democratic regimes (*Democracy or* ($\hat{\beta}_3$)). In order to confirm the hypothesis that results are driven by new democracies, the estimated coefficient for consolidated democratic regimes ($\hat{\beta}_3$) should not be different from zero and the combined coefficient for the effect of social spending in new

⁸⁰ It should be noted that there were vast increases in social security spending for Brazil between 1989 and 1990 when social security spending briefly rose to comprise 90.6 percent and 156.4 percent of the budget following the enactment of the 1988 Brazilian Constitution in those two years. All of the models in this chapter including these tables were run under an alternative strategy where these expenditures were replaced by imputation; the sign and significance levels of the coefficients remain as reported in these tables.

democracies should be statistically significant from zero ($\hat{\beta}_3 + \hat{\beta}_4$). The combined effect of both coefficients and its statistical significance is reported in the last row of the tables. The omitted category is authoritarian regime years.

The results presented in Tables 4.5, 4.6 and 4.7 yield surprising findings and provide evidence that there are some notable differences between newer and more established democracies. New democracies are not found to spend greater shares of resources in welfare programs as compared to authoritarian and consolidated regimes. New democracies are more likely to cut social security pensions as compared to established democracies. Earlier results had suggested that democracies were more likely to reduce pensions and increase investments in health and schooling. Table 4.6 provides evidence to suggest that both consolidated and transition democracies reduce social security expenditures, but that the total effect of pension reductions is stronger and more robust across specifications in the case of new democracies. The estimated coefficient on democracy which captures the effect of established democracies is negative and statistically significant in the GMM first-differenced estimation in the equations controlling for fixed effects and without country dummies. Although the marginal effect of younger democracies with less than two turnovers between rival political parties is not statistically significant, the total effect is predicted to be statistically significant at the 99% confidence interval across all specifications.

After controlling for differences between consolidated and new democracies, Table 4.7 provides evidence in support of the hypothesis that transition democracies allocate greater resources health and schooling as compared to established elected governments and authoritarian regimes. Although the marginal effect is not statistically significant, the estimations of the total effect of new democracy is statistically significant in all cases with the exception of system GMM estimations with and without controls for fixed effects. The findings in Table 4.6 and 4.7 are similarly confirmed by ECM estimates reported in the Appendix 4A.

Table 4.5. Response of Social Spending to Democratic Transitions in Latin America, 1973-2000*Dependent Variable: Share of Welfare Spending as a Share of Total Government Spending*

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One- Step First Differences ^a	(5) GMM One-Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.762*** (0.034)	0.467*** (0.046)	0.465*** (0.048)	0.682*** (0.164)	0.567** (0.204)	0.699*** (0.157)	0.741*** (0.172)
% of population aged 65 and over _t	0.267 (0.219)	3.363*** (0.682)	0.532 (0.883)	1.942 (1.885)	0.647 (1.609)	0.172 (0.760)	0.424 (0.697)
% of population aged 15 to 64 _t	0.741*** (0.160)	0.106 (0.193)	-0.002 (0.192)	0.020 (0.182)	0.066 (0.215)	0.763 (0.643)	0.488 (0.539)
%urban population _t	-0.066** (0.029)	0.411*** (0.118)	0.003 (0.212)	0.282** (0.105)	-0.155 (0.249)	-0.146 (0.333)	-0.059 (0.234)
GDP per capita _t (constant 2000 US\$) (WDI)	0.000 (0.000)	-0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	-0.003 (0.002)	0.002 (0.004)	0.000 (0.003)
GDP per capita growth _{t-1} (annual %) (WDI)	0.031 (0.050)	-0.030 (0.051)	-0.015 (0.065)	-0.190 (0.142)	-0.209 (0.154)	0.019 (0.096)	0.096 (0.118)
Trade Openness _t	-0.039** (0.019)	-0.023 (0.025)	-0.073** (0.028)	-0.025 (0.036)	-0.056 (0.047)	-0.037 (0.053)	-0.034 (0.048)
Capital Mobility Index _t	0.737** (0.346)	0.582 (0.432)	0.477 (0.475)	0.253 (0.774)	0.383 (0.614)	0.714 (0.843)	0.419 (0.731)
Democracy _t	0.898 (0.671)	-1.739 (1.083)	-1.709 (1.158)	-0.753 (1.255)	-1.093 (1.455)	1.350 (1.245)	0.504 (1.455)
New Democracy _t	-1.586** (0.774)	1.455* (0.860)	1.789** (0.873)	0.515 (1.082)	1.671 (1.873)	-1.065 (1.223)	-1.113 (1.147)
Constant	-28.077*** (6.328)	-27.416*** (4.934)	37.503* (21.683)			-25.620 (19.335)	-12.078 (17.746)
Observations	351	351	351	331	331	351	351
R-squared	0.81	0.84	0.85				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (<i>p value</i>) ^b				0.446	0.494	0.657	0.491
Number of Instruments				13	39	17	43
Hansen test for joint validity of instruments (<i>p value</i>)				0.184	1.000	0.975	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						1.000	1.000
<i>Total Effect of New Democracy</i>	-0.689 (.561)	-0.284 (0.672)	0.08 (0.782)	-0.238 (0.847)	0.577 (1.231)	0.284 (1.149)	-0.609 (0.668)

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. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a One lag of the dependent variable was 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 (2007a). 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.

^b 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 4.6. Response of Social Security to Democratic Transitions in Latin America, 1973-2000*Dependent Variable: Share of Social Security Expenditures as a Share of Total Government Spending*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pooled OLS	Pooled OLS with country fixed effects (f.e.)	Pooled OLS with country and year f.e.	GMM One-Step First Differences ^a	GMM One-Step First Diff. with year f.e. ^a	GMM One-Step System ^a	GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.788*** (0.035)	0.509*** (0.049)	0.526*** (0.049)	0.709*** (0.188)	0.671** (0.286)	0.836*** (0.144)	0.752*** (0.159)
% of population aged 65 and over _t	0.645*** (0.219)	2.805*** (0.688)	1.871** (0.939)	1.510 (1.043)	1.361 (1.623)	0.374 (0.519)	0.787 (0.651)
% of population aged 15 to 64 _t	0.493*** (0.168)	-0.303* (0.163)	-0.309* (0.158)	-0.210 (0.186)	-0.205 (0.228)	0.327 (0.273)	0.316 (0.315)
%urban population _t	0.008 (0.044)	0.485*** (0.157)	0.445* (0.236)	0.342** (0.141)	0.332 (0.259)	-0.025 (0.107)	0.014 (0.100)
GDP per capita _t (constant 2000 US\$) (WDI)	-0.000 (0.000)	-0.001 (0.000)	-0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	0.000 (0.001)
GDP per capita growth _{t-1} (annual %) (WDI)	0.096** (0.047)	0.044 (0.050)	0.054 (0.056)	-0.015 (0.075)	-0.043 (0.132)	0.060 (0.050)	0.099 (0.080)
Trade Openness _t	-0.087*** (0.023)	-0.084*** (0.028)	-0.109*** (0.032)	-0.071** (0.025)	-0.091** (0.036)	-0.066 (0.042)	-0.090 (0.052)
Capital Mobility Index _t	0.612** (0.257)	1.255*** (0.400)	1.416*** (0.450)	0.757 (0.593)	1.037 (0.968)	0.481 (0.498)	0.559 (0.563)
Democracy _t	-0.707 (0.706)	-2.369 (1.534)	-2.578 (1.660)	-1.554** (0.704)	-1.987** (0.921)	-0.270 (0.818)	-1.176 (1.096)
New Democracy _t	-0.811 (0.685)	0.200 (0.878)	0.632 (1.039)	-0.127 (0.670)	0.490 (0.860)	-0.627 (0.754)	-0.496 (0.703)
Constant	-23.273*** (6.919)	-21.107*** (8.160)	-11.546 (19.556)			-14.673 (11.491)	-13.469 (13.309)
Observations	351	351	351	331	331	351	351
R-squared	0.91	0.92	0.93				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (p value) ^b				0.522	0.667	0.519	0.579
Number of Instruments				13	39	17	43
Hansen test for joint validity of instruments (p value)				0.198	1.000	0.766	1.000
Diff. Sargan tests for all system instruments (p value)						1.000	1.000
<i>Total Effect of New Democracy</i>	<i>-1.517***</i> <i>(0.450)</i>	<i>-2.168***</i> <i>(0.804)</i>	<i>-1.945***</i> <i>(0.887)</i>	<i>-1.681***</i> <i>(0.317)</i>	<i>-1.497***</i> <i>(0.483)</i>	<i>-0.896</i> <i>(0.747)</i>	<i>-1.672**</i> <i>(0.821)</i>

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. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a One lag of the dependent variable was 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 (2007a). 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.

^b 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 4.7. Response of Health and Education to Democratic Transitions in Latin America, 1973-2000*Dependent Variable: Share of Health and Education Expenditures as a Share of Total Government Spending*

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One- Step First Differences ^a	(5) GMM One-Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.801*** (0.050)	0.542*** (0.083)	0.531*** (0.089)	0.101 (0.579)	0.030 (0.264)	0.259 (0.282)	0.475* (0.269)
% of population aged 65 and over _t	-0.388*** (0.118)	0.396 (0.451)	-1.482* (0.891)	-6.515 (12.534)	-2.445 (1.812)	-1.270 (1.342)	-0.646 (0.961)
% of population aged 15 to 64 _t	0.166 (0.133)	0.374** (0.181)	0.294* (0.171)	0.213 (1.150)	0.781*** (0.263)	0.681 (0.771)	0.386 (0.543)
%urban population _t	-0.061 (0.048)	-0.086 (0.163)	-0.453 (0.280)	0.110 (0.548)	-0.897 (0.512)	-0.284 (0.506)	-0.083 (0.306)
GDP per capita _t (constant 2000 US\$) (WDI)	0.000 (0.000)	0.000 (0.000)	-0.001 (0.001)	0.015 (0.025)	-0.002 (0.003)	0.001 (0.005)	-0.001 (0.003)
GDP per capita growth _{t-1} (annual %) (WDI)	-0.061 (0.042)	-0.062 (0.044)	-0.054 (0.051)	-0.321 (0.225)	-0.426** (0.178)	-0.099 (0.139)	-0.022 (0.051)
Trade Openness _t	0.049*** (0.016)	0.062** (0.026)	0.042 (0.030)	-0.118 (0.342)	0.032 (0.064)	0.123 (0.070)	0.094 (0.073)
Capital Mobility Index _t	0.038 (0.262)	-0.758** (0.376)	-1.036** (0.423)	-0.466 (1.559)	-1.200 (1.167)	0.090 (0.794)	0.130 (0.684)
Democracy _t	1.503** (0.715)	0.714 (1.761)	1.021 (1.900)	0.554 (3.530)	0.814 (2.915)	3.875 (2.652)	3.293 (3.337)
New Democracy _t	-0.610 (0.753)	1.098 (1.064)	0.951 (1.198)	0.536 (3.078)	2.324 (2.030)	-1.486 (1.645)	-1.441 (2.442)
Constant	-2.115 (5.360)	-4.609 (6.200)	48.296* (26.211)			-6.122 (17.954)	-3.068 (17.568)
Observations	351	351	351	331	331	351	351
R-squared	0.86	0.89	0.90				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (p value) ^b				0.374	0.225	0.266	0.250
Number of instruments				13	39	17	43
Hansen test for joint validity of instruments (p value)				0.923	1.000	0.899	1.000
Diff. Sargan tests for all system instruments (p value)						0.642	1.000
<i>Total Effect of New Democracy</i>	<i>0.893***</i> (0.331)	<i>1.812***</i> (0.869)	<i>1.972***</i> (0.954)	1.089 (2.835)	3.138*** (1.501)	2.388 (1.914)	1.853 (1.719)

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. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a One lag of the dependent variable was 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 (2007a). 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.

^b 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.

Together these tables suggest that democratic transitions are characterized by greater redistributive pressures that translate into real changes in budget allocations towards more progressive social spending and away from more regressive expenditures. The transition to democracy opens a window of opportunity that permits less organized groups to gain greater weight in the decision-making process increasing the likelihood of radical shifts away from appeasing the same groups favored by authoritarian regimes. However, budget allocations towards the poor dissipate over the long-run in democracies. In short, after the uncertainty surrounding the first years of democratic transition end, the allocation of public expenditures begins to favor more entrenched power groups and move away from health and education expenditures that benefit the poor.

4.5.3 Lessons Learned: Does Method Matter?

This chapter has undertaken a hypothesis testing strategy that is based on testing the model with a battery of different specifications. The primary motivation for undertaking this strategy was the considerable evidence suggesting that the data on social expenditures in Latin America employed in this chapter were highly persistent. Concerns were raised that standard stationary methods might be misleading for understanding the determinants of social spending (Beck and Katz 2004). Several of the results in terms of the signs and magnitude levels of the coefficient estimates between the different specifications conform with the specialized econometrics literature on the outcomes that should be expected with highly persistent series.

Whereas prior studies on the impact of regime type on social spending have relied solely on one method (either pooled OLS estimation with time and unit effects or an error corrected model with panel corrected standard errors, time and unit controls), this chapter has tested hypothesis employing a battery of seven specifications for each question. Table 4.8 summarizes the results obtained from five of these different specifications employed in this chapter: pooled OLS with PCSE and country unit dummies, pooled OLS with time and unit dummies, GMM first difference and system estimates with time fixed effects controls. The results of the ECM model are also summarized and reported in Appendix 4A. There is generally strong correlation between the sign and significance levels across the specifications that were tested in this chapter. In six of the nine tests, all five specifications produce the same predicted sign and magnitude of the

coefficient in question. In the other three cases, the nonrobustness is modest, as the sign and significance levels of the coefficients are the same in two of the three specifications. These results suggest that the conclusions reached in this chapter are fairly robust as they are not highly contingent on the method used to obtain them.

Table 4.8. Robustness of Results to Alternative Dynamic Specifications

Dependent Variable	Independent Variable	Predicted Sign	Number of Studies where variable was significant and predicted sign (Total=7)	Results of Pooled OLS with PCSE and country f.e.	Results of Pooled OLS with country and year f.e.	Results of GMM First Differences with year f.e.	Results of GMM System with year f.e.	Results of ECM with country and year f.e.	Predicted hypothesis confirmed in at least 3 specifications
<i>Social Spending</i>	Democracy	Positive	3	Not significant	Not significant	Not significant	Not significant	Not significant	Not significant
	Established Democracy	Positive	1	Not significant	Not significant	Not significant	Not significant	Not significant	Not significant
	Total Effect New Democracy	Negative	1	Not significant	Not significant	Not significant	Not significant	Not significant	Not significant
<i>Social Security</i>	Democracy	Not significant	4	Negative	Negative	Negative	Negative	Negative	Negative
	Established Democracy	None	None	Not significant	Not significant	Not significant	Not significant	Not significant	Not significant
	Total Effect New Democracy	None	None	Negative	Negative	Negative	Negative	Negative	Negative
<i>Health and Education</i>	Democracy	Positive	4	Positive	Positive	Positive	Not significant	Positive	Positive
	Established Democracy	None	None	Not significant	Not significant	Not significant	Not significant	Not significant	Not significant
	Total Effect New Democracy	None	None	Positive	Positive	Positive	Not significant	Positive	Positive

Notes: Estimated coefficient signs are only reported if statistical significance was 10% or lower. The reported coefficients in the case of the ECM model are for the lagged level of the variable ($x_{i,t-1}$).

The results reported for all hypotheses tested in this chapter are based on a battery of specifications. As a result, the conclusions reached in this chapter are fairly robust as they are not highly contingent on the method used to obtain them. One of the major lessons learned from this exercise is that there is a need for extensive sensitivity testing as part of the research process. In the case of this chapter, some of the findings suggest that alternative dynamic specifications will significantly challenge central findings derived from the simple lagged dependent variable model or the fixed effects model. As Butler and Wilson (2007) advise, “Given a field in which everyone is painfully aware that theoretical concepts sometimes have weak empirical analogues and where data collection is often error-ridden, highly aggregated, or otherwise problematic, the bar for confirming theories with regression analysis should be very high (119).”

Based on previous studies reviewed in this chapter, Table 4.8 presents a prediction of the expected sign of the impact on social spending on the three key variables: democracy, established and democratic transition governments. The fourth column presents the number of studies in which the predicted hypothesis was confirmed out of the total of seven studies that have been published to attempt to gauge the level of consensus that prevails regarding the effect of these three variables on aggregate social spending, health and education expenditures and social

security transfers. The highest number of studies to confirm a predicted hypothesis is four and this is only attained with respect to two of the nine outcomes. Four of the nine hypotheses have not previously been tested and there are therefore no prior predictions for comparison. The last column summarizes the results predicted by at least three specifications in this study. Most of the previous results on the impact of democracy on social expenditures in aggregate and disaggregated terms are not robust to alternative specifications. Only one of five hypotheses was confirmed. In the other four cases, the results were not as predicted.

4.6 Conclusions

In light of the waves of democratization and integration with commercial and financial global markets, a small but growing literature has emerged examining the responsiveness of the social policy agendas of Latin American governments to the demands of citizens. This chapter has presented robust evidence to show that there are major differences between the expenditure pattern of democracies and authoritarian regimes with respect to health and education expenditures and social security transfers. As a share of the total central government budget, democratic regimes spend more on health and education relative to non-democratic governments. However, the propensity of democracies to invest greater shares of the budget in health and education depends on whether the regime is in transition. There are slightly different dynamics underway in new democracies relative to the patterns observed in their established counterparts. During the transition stage following an authoritarian regime, democracies allocate fewer resources for social security and spend more on health and education. Together, these findings suggest that different political logics appear to be at work with respect to whether social spending is directed towards health and education programs or social security transfers.

The results of this chapter pose important questions on how findings could be interpreted. There is partial evidence that by “the tyranny of the ballot box” lower-income individuals in new democracies impose redistributive social programs on the better-off. Health and education spending increase in democracies, but these changes in budget allocations appear to be short-term as they end with the establishment of two turnovers in political power following multiparty

elections. On the other hand, these results could also support the hypothesis that expenditure increases are responding to powerful teachers' unions and health care professionals.

The evidence also suggests that democracies in Latin America, all else equal, allocate proportionately less benefits for the middle and upper classes and therefore have limited the resources channeled towards mostly regressive pension programs. Social security expenditures, which are funded in part by corporate income taxes, payroll taxes and employer contributions, increased dramatically over the last three decades. Yet, all else equal, the share of the budget allocated to pensions is lower in democracies that have experienced less than two consecutive turnovers of power between distinct factions.

Other questions are beyond the scope of this chapter, but nevertheless bear mention as they highlight that greater understanding is needed on the mechanisms that explain superior democratic performance in allocating resources to enhance equity and reduce poverty. It has been argued that the instances in which voters in Latin America hold politicians accountable for election promises at the ballot box are rare. Citizens vote on personalities versus issues. Democracies are weakly institutionalized and therefore there is a limited ability to exert pressure on governments to make them more accountable. In addition, even in cases where voters do hold politicians accountable, it is unlikely that citizens will have access to information on specific technical issues related to government spending on social services to inform their votes. In some Latin American countries, most of the middle class and workers have opted out of the public health and education systems. These points counter the implicit assumption of the literature on welfare spending that not only assumes that education, health and social security policies are leading issues for voters, but that candidates have incentives to implement policies because voters have information available to judge and punish those governments who break their promises. Moreover, the evidence that democracies have directed fiscal resources to health and education expenditures and therefore brought greater reductions in poverty and enhanced equity is thus far limited and weak. In the next chapter, the findings in this chapter will be tested with respect to elections to attempt to precisely gauge the direct impact of this aspect of democracy on social spending.

5. Electoral Cycles and Fiscal and Social Policy Performance in Latin America

5.1 Introduction

The previous chapter showed that democracies are more likely to invest resources in health and education programs as compared to authoritarian regimes. Furthermore, the propensity of democracies to invest greater shares of the budget in health and education was also shown to be influenced by the post-authoritarian democratic transition period. Given the superior performance of democracies with respect to social policy investments, this chapter turns to examining the direct effects of democracy seeking to see if the differences in responsiveness to societal demand are driven by election cycles.

This chapter seeks to answer four questions. Are electoral competitions catalysts for fiscal and social policy performance in Latin America? Do short-term changes in the allocation of resources quickly disappear, or do election-induced changes in government spending and in the shares of the budget directed at education, health and pensions persist in the inaugural year of a presidential mandate? Are governments during democratic transitions more likely to implement larger changes in budget allocations following elections as compared to established democracies? Do closer electoral margins result in higher shares of government and social spending prior to and after elections?

The literature on political business and budget cycles has produced important insights on the extent to which politicians attempt to manipulate government fiscal and monetary policies to influence electoral outcomes. In particular, some of the strongest evidence produced to date suggests that there is an electoral cyclicity in fiscal spending and that patterns are especially pronounced for government transfers, in particular social security (Franzese 2002a; Drazen 2000). Oftentimes, it is also asserted that the evidence of these cycles is stronger in developing countries. There are, however, three important shortcomings of the existing literature. First, empirical studies on political budget cycles that include developing democracies have largely proceeded without analysis of the distinct characteristics that distinguish democratic regimes and elections in particular in these countries from their

industrialized counterparts. Second, it has largely proceeded without exploring how political budget cycles impact changes in the composition of social spending. Finally, it has altogether neglected the important contrasts that exist between elections in Asia, Africa and Latin America.

The influence of elections differs depending on the type of voters and magnitude of influence these exert in different political economies. Given variations in international and domestic contexts, studies of electoral demand for redistribution and responsiveness by governments in a wide range of countries and regions are important. Yet, multinational research of the impact of elections on total government spending in Latin America has been limited to five studies and there has been no systematic cross-country multivariate study on the consequences of elections for government spending on social spending in health, education and social security (Ames 1977, 1987; Remmer 1993). Understanding the impact of electoral politics for Latin America is especially important given the powerful wave of democratization and the pent-up demands for greater redistribution of government expenditures. The pressures faced by newly elected Latin American democratic governments, which have struggled to bring about improved economic and social policy performance in the face of weak institutions, widespread poverty, growing inequality and volatile economies, stand in stark contrast to those weighing on developed industrialized democracies and their extensive welfare systems. In comparative perspective, however, the region can clearly be distinguished from other developing countries given the longevity, albeit interrupted, of its democratic and welfare and institutions. There is a need for greater understanding of the differences and commonalities that Latin America has with other developing regions and advanced, industrialized democracies.

One of the weaknesses of many studies of political budget cycles in developing countries has been with respect to research design and methodology. The method most commonly employed is the inclusion of a dummy variable for the election year to control for differences in spending during the year of the election as compared to all other years. In some cases, as will be discussed below, studies include both democratic and authoritarian periods. These studies are often also plagued by the inclusion of multiparty, competitive elections in democracy as the variable that measures the effect of electoral cycles. Researchers have included single party elections in developing countries and elections under autocracy. While other studies adopt a more theoretically apt approach of only examining democratic years,

much of the literature on political budget cycles in developing countries has neglected to account for differences that exist between elections that are easily won by comfortable electoral margins and those where presidential candidates struggle with highly uncertain chances of winning. There are also unique characteristics that are embodied in democratic governance following authoritarian rule and the elections that characterize democratic transitions between civilian administrations. Yet, studies devoted to affirming the robust presence of election cycles in developing democracies have mostly neglected to explore these issues or control for how they might effect the patterns being reported.

This chapter seeks to address a gap in the literature by undertaking an in-depth, cross-national examination of how fiscal and social policy instruments are used by Latin American democracies to enhance reelection prospects and to respond to constituencies in the winner's first year in office. The effect of electoral politics is tested by looking at how presidential elections impact the total budget and composition of distributive social spending in Latin America democratic elections between 1973 and 2000. The hypothesis of whether pre-election government spending tends to exceed post-election efforts for fiscal and social policy measures is explored. In addition, the chapter examines whether there are differences between the elections that take place under transitional and established democracy. In order to analyze the impact of elections, the chapter also tests to see if elections that end with closer electoral margins produce greater changes in the budget composition and share for health, education and pensions.

The chapter is structured in the following way. Section 2 reviews existing theory on the behavior of democracies prior to and after elections with respect to total government spending, budget deficits and social expenditures more specifically, as well as findings that might suggest why elections during transitional democratic periods may prove to be particularly important and distinct. 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 chapter 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 chapter with a summary of the key findings.

5.2 Fiscal and Social Policy Performance and Electoral Cycles: A Review of the Literature

This section reviews the literature on political business and most specifically budget cycles highlighting what theory predicts should happen with respect to how governments spend resources and manage their budgets. It then briefly assesses the empirical evidence on fiscal policy patterns and elections in developing countries with emphasis on Latin America. This discussion helps to set the parameters for a discussion on the extent to which fiscal resources are directed at social policies during and after elections and the sparse empirical evidence produced to date. The section concludes with a summary of the competing theories that have been offered to explain why political budget and welfare cycles may be more or less pronounced in Latin America.

5.2.1 *Political Business and Budget Cycles*

Since their inception, a crucial assumption of political *business* cycles models is that voters choose leaders on the basis of economic variables and accordingly the degree, nature and timing of economic policies exerts influence on citizen decisions at the ballot box.⁸¹ The electoral motivations that may guide government policies were described by Kalecki (1943) and discussed by Schumpeter (1939) in his study of business cycles, but the theoretical framework to describe the “opportunistic (office-seeking)” motivations of politicians were developed formally by Nordhaus (1975) and Tufte (1978). In these early and subsequent models based on the same assumptions, elected leaders in control of monetary policy were able to successfully manipulate the economy by surprising myopic voters who were limited in forming their opinions based solely on *past* incumbent performance and inflation rates. More recent theories have made important advances in two important realms. First, they have incorporated forward-looking, rational expectations. Second, they have further advanced research on the effects of right and left-wing party orientation on macroeconomic outcomes during and after elections (Alesina 1987).⁸² Building on Rogoff and Siebert’s (1988) and Rogoff’s (1990) rational opportunistic political budget cycle models, recent research has

⁸¹ For a critique and dissenting view of political budget cycles, see Alt and Chrystal (1981).

⁸² For a valuable summary of the findings emerging from partisan cycles, see Franzese (2002a). The effects of ideological orientation on the findings reported in this chapter will be a task for future research.

focused on examining how fiscal policy is utilized by incumbents to enhance reelection prospects (Drazen 2000).

Arguing that monetary surprises are an unconvincing driving force for political *business* cycles, recent studies including Drazen (2000), Brender and Drazen (2005) and Brender and Drazen (2007) have reinvigorated efforts to develop and test models that emphasize *fiscal policy* as the driving force, especially for opportunistic cycles.⁸³ The basic rationale behind models that emphasize the political *budget* cycle is that governments will manipulate fiscal policy, in part, in order to obtain electoral success. Models based on this theoretical framework posit that the size of the fiscal stimulus is the catalyst variable and that monetary policy will reflect fiscal impulses. This framework predicts that incumbents have incentives to signal competence to rational voters by increasing expenditures in highly visible budget activities before elections, such as personal transfers, service provision and public works. In turn, central banks will respond to increasing government expenditures with money growth to quell pressures on interest rates.

While theory predicts that expenditures will increase prior to elections, the theorized and empirical results of what will happen to government spending once winners are in office have failed to find consistent patterns. Rogoff's (1990) model of political budget cycles posits that government spending will continue to increase in the period immediately after a leader assume office, a pattern similarly emphasized by Tufte (1978), Ames (1987) and Franzese (2002b). In developed democracies Franzese (2002b) finds that transfers increase both the year before and the year after elections with electioneering being higher after leaders assume office. He postulates that both incumbents and challengers counter-promise expenditure increases with elections rewarding candidates who promise more with greater credibility. As he summarizes, "the election serves as a filter for credibility \times promised largesse. Thus, especially if newly seated governments are most productive (another empirical regularity), post-electoral electioneering will be greater and more certain than pre-electoral (391)." On the other hand, Remmer (1993) argues that elections in Latin America give leaders the political capital to enact reform given voter's preferences for reduced income volatility and inflation.

⁸³ There is a rich strand of the literature that has emphasized the influence of partisan cycles on economic policies and outcomes. These studies are not reviewed in this chapter. For a review and discussion of the impact of party structure and ideology to explain monetary and fiscal policy electoral cycles, see Alesina (1988), Drazen (2000) and Franzese (2002a).

She posits that government spending will decrease following elections as decision-makers quickly move to stabilize the economy.

Empirical research on political budget cycles for developing countries is recent and less systematic. There have been a few multinational studies that have carried out either developing country only or cross-region specific research for Africa and Latin America (Remmer 1993; Ames 1987; Block 2002a, 2002b; Block et al. 2003; Schuknecht 2000; Mejía Acosta and Coppedge 2001; Persson and Tabellini 2003; Amorim Neto and Borsani 2004; Brender and Drazen 2005). The majority of these studies have drawn inferences based on results derived from estimations that are problematic. Studies have tested the effect of elections relative to both democratic and authoritarian periods in developing countries (Ames 1987; Block 2002b, 2002a; Block et al. 2003). Thus, the inclusion of the election dummy tests whether ballot box competition matters relative to all other years irrespective of regime type. For example, Block (2002a) finds a marked increase in presidential election-year public expenditures on current consumption goods and away from public investment, which are both measured in budget shares, in 69 developing countries between 1975 and 1990. However, he includes both competitive and non-competitive elections thus confounding interpretation as to the processes that are driving the cycles being detected. Studies often also examine any election that took place irrespective of whether it was a multiparty, competitive election under a democratic regime (Mejía Acosta and Coppedge 2001; Gonzalez 2002). For example, Mejía Acosta and Coppedge (2001) include Mexican presidential elections dominated by the Partido Revolucionario Institucional (PRI) during the 1980s and 1990s and compare these elections with the outcomes from decisions in Argentina, Brazil, Chile, Ecuador, Venezuela and Uruguay after democracy had returned to each of these countries. Some studies, including Schuknecht (2000) who studies fiscal policy cycles in 24 developing countries between 1973 and 1992, do not even enter into a discussion of the country selection or mention consideration of democracy as part of the criteria for selecting election years.

While other studies have adopted a more theoretically apt approach by only examining democratic years for developing democracies, they still suffer from problems of research design. Some studies combine both developing and developed countries, despite strong evidence that the patterns of democratization and the level of institutional development differ substantially (Persson and Tabellini 2003; Brender and Drazen 2005). Indeed, Brender and Drazen (2005) precisely argue that the results in earlier studies are driven by political budget

cycles in “new democracies” and that fiscal manipulation no longer is statistically significant for established democracies once these samples are appropriately separated. Brender and Drazen (2005), however, seem to disregard the political science literature on the properties of new democracies basing their analysis on an arbitrary definition in which the first four competitive elections are considered to be the “new democratic” period.

Empirical research directed at developing countries has also neglected to control for differences in the types of elections that take place in more contested political environments and at the onset and consolidation stages of democracy. Political theory is rich in arguments on the differences that exist between elections that are easily won by comfortable electoral margins and those where presidential candidates struggle with highly uncertain chances of winning. There are also unique characteristics embodied in the first democratic elections following authoritarian rule. Yet, studies devoted to affirming the robust presence of election cycles in developing democracies have mostly neglected to explore these issues or control for how they might effect the patterns being reported (Block et al. 2003).

The evidence confirming that governments appear to be timing expenditures to strategically coincide with elections has been confirmed in the case of Argentine gubernatorial elections and South Korean presidential and national assembly elections (Kwon 2005; Jones 1997). Yet, closer reading of the studies often cited as supporting evidence of political budget cycles indicates that a significant portion of this research fails to consistently confirm its predicted findings (Khemani 2000; Gonzalez 2002). Indeed, research directed at detecting electoral manipulations in government spending and tax policies in Latin America has tested for patterns at the national, provincial (or state) and municipal levels. Despite the fairly large number of studies for the region, the empirical evidence of political budget cycles, however, is weak.⁸⁴

Multinational studies of “electioneering” in government expenditures in Latin America have thus far reached divergent conclusions. Ames (1977) finds that government expenditures rose prior to and after the 65 elections that took place in seventeen Latin American countries between 1945 and 1972, although only post-election spending proved to

⁸⁴ In a related vein, Borsani (2003) examines electoral cycles for economic growth, inflation, unemployment in twelve Latin American countries between 1978 and 1998. He finds that economic performance worsens in the year after the election, but there are no statistically significant differences in the dependent variables he tests in the election year.

be statistically significant. For the same group of countries, Ames (1987) reports that government expenditures increased by 6.3 percent in the pre-election year and decreased by 7.6 percent in the year after the 82 elections that took place between 1947 and 1982. In a study of eight South American democracies during the 1980s, Remmer (1993) reports that the quarterly percentage change in the fiscal balance is heterogeneous across countries. She argues that elections in Latin America during the 1980s provided leaders with greater political capital to enact reform given voter's preferences for reduced income volatility and inflation.⁸⁵ Underscoring the importance of the macroeconomic context in the region, she posits that there is evidence of an "anti-political business cycle" (402) in presidential elections in Argentina (1989), Bolivia (1985), Brazil (1989), Ecuador (1984 and 1988), Peru (1990) and Venezuela (1988). The general trend in these cases is a decrease in government spending following elections and in some cases a contraction also occurs prior to elections.

More recent cross-country studies that seek to explain the political determinants of government spending and budget deficits in Latin America have also failed to produce robust findings (Amorim Neto and Borsani 2004; Mejía Acosta and Coppedge 2001). Controlling for a multiplicity of political determinants in a study of eight Latin American countries between 1983 and 1998, Mejía Acosta and Coppedge (2001) do not find statistically significant findings for the effect of electoral cycles on government spending. The authors do, however, find that fiscal deficit worsen during elections. These findings are confirmed in a study of the influence of presidential and cabinet effects in ten Latin American countries between 1980 and 1998 by Amorim Neto and Borsani (2004), who argue that the fiscal difficulties in election years are driven by the reluctance of governments to increase taxes rather than spend greater shares of resources. However, this hypothesis has not yet been explored robustly as neither study has tested whether differences in tax collection effort are impacted by the electoral calendar.

Some researchers have chosen to focus on electoral cycles within a single country; the evidence they have uncovered in support of political budget cycles is equally mixed. In a study of Mexican government central government spending between 1957 and 1997, Gonzalez (2002) does not find robust evidence that the quarterly growth rate of public spending is correlated with pre-election or election quarters during fourteen presidential and

⁸⁵ Remmer (1993) excludes "founding" elections in the nine countries she analyzed for the 1980s.

midterm elections. In some cases, the results are contrary to theoretical predictions. For example, Drazen and Eslava (2005) find that current government expenditures decrease in pre-election years in Colombian municipalities. With the exception of Remmer (1993), however, the Latin American single country and multinational empirical studies are problematic for drawing inferences on the impact of competitive elections on political budget cycles as many of these studies examine electoral cycles by including elections that took place under autocracy.

5.2.2 *Political Welfare Cycles*

An important insight of the political budget cycle 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 choose far more direct and targeted tools, research efforts have been directed at examining if there are variations in expenditures by type depending on whether they are also more perceptible and if the credit for such policies is more easily attributed to individual politicians or levels of government.⁸⁶ 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, particular attention has been focused on the electoral-economic cycle affects of personal transfer payments from governments, such as social security and social assistance. Indeed, among the types of government spending, the persistence and magnitude of the electoral trend of transfer payments has been noted across both developed and developing countries.

The empirical evidence in developed democracies has generally found that electoral cycles are stronger and more persistent for outlays on social expenditures and in particular social security transfers in both national and local elections (Tufte 1978; Alesina 1988; 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

⁸⁶ 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.

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. Tufte also draws attention to the fact that the largest increase in transfers occurred in Richard Nixon's reelection bid in 1972. The Democrat-dominated U.S. Congress countered the 5 percent increase proposed by the Republican Party president by approving a 20 percent increase resulting in the arrival of checks to 32 million voters on the eve of the election and the good news was communicated in a letter to retirees expressly attributing the benefit to Richard Nixon. Indeed, in this contested election, Tufte notes that almost every type of transfer accelerated in the months preceding voting and decelerated afterwards. Using multivariate regression analysis, 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.

While there has been a much more in-depth examination of budget cycles for developing countries, the case-specific empirical evidence on electoral cycles for redistributive social spending has been very limited.⁸⁷ Table 5.1 presents a summary of the cross-national work on the impact of elections on fiscal and social policy interventions in developing countries. Only four of the seventeen studies have tested whether social expenditures are timed to coincide with elections. The two studies that have focused on the impact of elections on social security transfers have produced contrary findings. In a study of presidential and midterm elections in Mexico under PRI party dominance and non-competitive elections, Gonzalez (2002) reports 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

⁸⁷ Studies have also generally found stronger patterns for electoral cycles on government spending for infrastructure. Khemani (2000) finds that state legislative assembly elections have a positive and large effect on road construction by state public works departments in India over the period 1960-1994, but no effect on deficits or government consumption spending. Drazen and Eslava (2005) confirm higher levels and budget shares of government spending directed at development projects including infrastructure, power and water plants and road construction in the year prior to elections in Colombian municipalities from 1987 to 2000.

increase in the quarter of the election itself. Contrary to theoretical predictions in developed democracies, however, Drazen and Eslava (2005) find that the level and share of municipal expenditures on transfers to retirees and payments to temporary workers decrease before mayoral elections in Colombian from 1987 to 2000.⁸⁸

Very few studies have focused on non-social security welfare spending in developing countries. The empirical work is limited to one cross-national study including both developed and developing democracies, a cross-municipality study of expenditures in Colombia and a micro-level analysis of social fund expenditures in Peru (Persson and Tabellini 2003; Schady 2000; Drazen and Eslava 2005). The evidence in these studies has yielded inconclusive findings. Drazen and Eslava (2005) report evidence that Colombian municipalities increased the level of spending on education (though only in levels and not as a share of the total budget) in election years. However, they do not find that municipal expenditures on housing and health were significantly increased in amount or proportion during 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. Thus, political cycles have been reported for expenditures on social fund programs and education, but not for aggregate welfare or public health expenditures in past research.

⁸⁸ Part of the explanation for Drazen and Eslava's (2005) finding that there are not cycles in municipal-level transfers to retirees in Colombian municipal elections may be due to the extent to which local politicians are able to take credit for national-level social policies. Following a series of reforms, education, health and water are now provided in a decentralized fashion in Colombia.

Table 5.1. Cross-National Empirical Studies of the Impact of Elections on Fiscal and Social Policy Interventions in Developing Countries*

Authors	Countries and Types of Elections	Period	Methodology	Includes Non-Competitive Elections†	Number of Elections	Impact of Elections		
						Fiscal Balance or Government Expenditures	Social Security Transfers	Health and Education Expenditures
Ames (1977)	Presidential elections in 17 Latin American countries	1945-1972 (annual)	Generalized least squares (GLS)	Yes	65	Pre-election and post-election years increase spending	Not analyzed	Not analyzed
Ames (1987)	Presidential elections in 17 Latin American countries	1945-1982 (annual)	GLS	Yes	82	Pre-election year increase in spending and post-election year decrease in spending	Not analyzed	Not analyzed
Remmer (1993)	Presidential and midterm elections in Argentina, Bolivia, Brazil, Colombia, Ecuador, Peru, Uruguay, Venezuela	1980-1991 (quarters)	Ordinary least squares (OLS)	No	14	Weak evidence in Argentina and Bolivia that fiscal deficit increases in pre-election year and decreases in post-election year	Not analyzed	Not analyzed
Gonzalez (2000)	Mexico presidential and midterm elections	1957-1997 (quarters)	OLS	Yes	14	No evidence of changes in fiscal balance	Growth rate of transfer spending increases in election quarter, but not pre or post-election year	Not analyzed
Schady (2000)	Presidential, constituent assembly and referendum elections	1991-1995	OLS	Yes	3	Not analyzed	Not analyzed	Social fund expenditures increase in pre-election year
Khemani (2000)	State legislative assembly elections in 14 major states in India	1960-1994 (annual)	OLS with Instrumental Variables	No	77	Election years do not effect fiscal deficits primarily because consumption spending is reduced.	Not analyzed	Not analyzed
Jones, Sanguinetti and Tomamasi (2000)	Argentine provincial gubernatorial elections	1985-1996	Three-stage least squares	No	Not specified	Current government expenditures increase in election years	Not analyzed	Not analyzed
Schuknecht (2000)	National elections in 24 developing countries (sample countries are unspecified)	1973-1992 (annual)	Fixed Effects (FE)	Not specified	Not specified	Fiscal deficits increase, but total expenditure does not increase in the election year	Not analyzed	Not analyzed
Mejia Acosta and Coppedge (2001)	Presidential elections in 7 Latin American countries	1983-1998 (annual)	OLS with Panel Corrected Standard Errors	Yes	Not specified	Government spending is not responsive to electoral cycles. Fiscal deficits worsen depending on electoral calendar.	Not analyzed	Not analyzed
Block (2002a)	National elections in 69 developing countries	1975-1990 (annual)	Generalized Method of Moments (GMM)-Difference	Yes	93	Current government expenditures increase in pre and election years	Not analyzed	Not analyzed
Block (2002b)	Presidential elections in 44 Sub-Saharan African countries	1980-1995 (annual)	OLS, FE and GMM-Difference	Yes	67	Fiscal deficits and public spending increase during election years and decrease in the post-election year	Not analyzed	Not analyzed
Persson and Tabellini (2003)	National elections in 60 developed and developing countries	1960-1998 (annual)	FE	Yes	522	Current government expenditures decrease and fiscal surplus improves in pre-election years, but neither are significant in election years	No statistically significant relationship between elections and social security transfers	No statistically significant relationship between elections and welfare spending
Block, Ferree and Singh (2003)	Presidential elections in 44 Sub-Saharan African countries	1980-1995 (annual)	OLS, FE and GMM-Difference	Yes	67	Current government consumption increases in election years	Not analyzed	Not analyzed
Amorim and Borsani (2004)	Presidential elections in 10 Latin American countries	1980-1998 (annual)	GLS with Panel Corrected Standard Errors	Yes	Not specified	Election years do not effect fiscal spending. Fiscal deficits worsen during election years.	Not analyzed	Not analyzed
Drazen and Eslava (2005)	1119 Colombian municipal elections	1987-2000 (annual)	GMM	No	6	Current government expenditures decrease in pre-election years	Transfers (in levels and shares) decrease in pre-election year	Education spending (in levels) increases in pre-election year, but not in shares
Kwon (2005)	South Korean presidential and national assembly elections	1988-1997 (quarters)	OLS with Prais-Winsten procedure	No	3	Current government expenditures increase in pre-election years	Not analyzed	Not analyzed
Brender and Drazen (2005)	National elections in 68 developing and developed countries	1960-2001 (annual)	FE and GMM-Difference	Yes	548	Fiscal deficit increases in election years, but government expenditures increase only for new democracies	Not analyzed	Not analyzed

† The criteria used to classify if elections were included in non-democratic countries is based on the criteria of Alvarez et. al.*

*Three of the studies included in this table include both developed and developing countries: Shi and Svensson (2002), Persson and Tabellini (2003) and Brender and Drazen (2005).

Source: Elaborated by the author.

The inferences that can be drawn from studies on political welfare cycles, however, are problematic as they are plagued with the same problems that have been cited for studies of political budget cycles in developing countries. Two of the four studies that include Latin American countries in their tests for political welfare cycles include non-competitive elections. Of the fourteen Mexican presidential “elections” analyzed by Gonzalez, 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 1992.⁸⁹ Furthermore, the only multinational study of political welfare studies is based on elections in both developed and developing countries (Persson and Tabellini 2003).

The literature suggests several competing hypotheses to explain why political welfare cycles may be more or less pronounced given particular conditions. Four types of explanations have been offered to account for augmented fiscal policy cycles during and after elections. Studies have emphasized the conflicts between taxpayers and beneficiaries over redistributive policies, the closeness of elections, the newness of democratic institutions and the impact of decentralization. Depending on these factors, the nature of political competition may produce distinct outcomes either enhancing or reducing political budget and welfare cycles. The sections that follow briefly review each of these explanations, the empirical evidence and the implications for Latin America.

5.2.3 Redistributive Politics

Politicians depend on multiple constituencies with interests that are often competing. Electoral politics can be viewed as a redistributive game in which each candidate promises redistributions of welfare among the various groups in their constituencies (Cox and McCubbins 1986). Building on this theoretical framework, Mebane (1994) emphasizes that redistributive politics pushes policymakers to choose between taxpayers, tested-program recipients (e.g. unemployment, social assistance, etc.) and non-tested-program recipients (e.g. social security, public education, etc.). In his study of monthly transfer payments and

⁸⁹ 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 and appropriately coded for the entire period following the rules of Przeworski, Álvarez, Cheibub and Limongi (2000).

contributions in the United States from 1948-1987, he confirms the tension between the opposing interests of benefit recipients and payroll-tax-payers. Whereas Tufte's emphasized that incumbents seek to manipulate retirees, he argues that the intended target during elections are taxpayers and finds evidence that there are both reductions in payroll taxes and reductions in benefits during U.S. elections.

In a study of 60 parliamentary and presidential democracies in developed and developing countries between 1960 and 1998, Persson and Tabellini (2005) find that taxes are cut and budget deficits increase prior to elections. The authors also analyze aggregate welfare expenditures as a share of GDP and find that there are not statistically significant increases in social spending prior to or after executive elections in a panel of developed and developing countries. The authors conjecture that social spending targets very different types of voters in developing and developed countries (2005: 50). Whereas social welfare expenditures are broad redistributive programs that benefit large groups of the population in developed countries, they hypothesize that 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 (2003) both underscore that the nature of political cycles depends on the degree of asymmetry of interests between beneficiaries and tax payers. There are several implications that can be derived for Latin America. 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. In cases where governments seek to appease wealthy taxpayers, government and social expenditure may contract prior to elections. However, if governments seek to capture the support of voters in lower income deciles they may increase spending.

5.2.4 Contested Elections

Broad consensus exists that government spending increases more prior to elections where the incumbent faces greater competition and outcomes are more uncertain (Frey and Schneider 1978b; Franzese 2002a; Frey and Schneider 1978a; Schultz 1995). Yet, the incentive to manipulate government spending is not the same in every election; rather, it is directly related to the probability of losing office. In elections in which victory is not insured, the marginal benefits of fiscal policy manipulation are greater and politicians will therefore be more likely to direct resources for election motivated reasons. Schultz (1995) contends that political budget cycles occur because incumbent governments act at the margins. He argues that politicians increase transfers when elections are highly contested as the marginal benefits they gain from “buying” votes is greater than the costs once returned to office. As Schultz summarizes, “the marginal benefit of winning additional votes through economic policy manipulation decreases with the number of votes the incumbent can already count upon (84).” Using polling data, he finds strong robust evidence that more closely contested elections, increase transfers in the quarter preceding voting in a study of nine parliamentary elections in Great Britain from 1961 to 1992.

In consolidated democracies in Latin America, closely contested elections should drive greater increases in government spending. Yet, in contrast to Schultz’ theoretical predictions, Ames (1987) does not find that electoral margins explain which incumbents (newly elected governments) allocated greater shares of resources prior to (after) assuming office between 1947 and 1982.⁹⁰ He argues that this is because “the stimulative effect of a close election” is offset by the executive’s political weakness as president’s who win with very small margins often must govern with a hostile legislature (1987: 27). With the spread of democracy in the region since this earlier period, elections in many Latin American countries have become extremely contentious. Indeed, as Payne, Zovatto and Mateo Diaz (2007) note there are nine elections where the front runner in the first round was defeated in the second round between 1978 and 2000.⁹¹ The electoral margin of the winning candidate has been very narrow in recent elections. Indeed, the closest elections in Latin America in the last three decades were won by Gonzalo Sanchez de Losada with a 0.37 percent margin in Bolivia in 1989 and Ricardo Lagos with a 0.45 percent margin in Chile in 1999.

⁹⁰ Ames (1987) tests both the margin between the leading candidates in the election, as well as low margins, which he defines as elections that were decided with less than 10 percent of a lead for the winning candidate. Both the election margin and the more contested measure of elections were not statistically significant.

⁹¹ For example, frontrunner Mario Vargas Llosa defeated Alberto Fujimori by a 3.5percent electoral margin (32.6 percent vs. 29.1 percent) in the 1990 Peruvian presidential elections, but he later lost by 24.8 percent of the vote (37.6 percent to 62.4 percent) (Schady 2000).

On the other hand, several Latin American democracies have also reduced presidential office terms and approved constitutional changes to permit reelection over the last three decades. In a survey of democratic governance concluded in 2005, the IDB reported that fourteen of the eighteen countries allow reelection at some point, either immediately after or following one presidential term (Payne et al. 2007).⁹² Five Latin American countries also reduced the length in office of presidents between 1978 and 2005.⁹³ For example, both Argentina and Brazil reduced presidential terms to four years and also established the possibility of reelection. Reduced terms and the possibility of reelection both may have promulgated the propensity for there to be a reduced likelihood of political budget and welfare cycles in Latin America as the victories of most presidential incumbents have been quite comfortable. For example, Argentine President Carlos Saúl Menem was reelected in 1995 with a 20.6 percent lead over Jose Bordón. In Brazil, Fernando Henrique Cardoso also easily won his bid for a second consecutive presidential term over Luiz Inácio “Lula” da Silva with a 21.4 percent margin in 1998. It should be noted, nonetheless, that presidential reelection is a recent phenomenon that has become a feature common in the region only in the 1990s.

5.2.5 *Political Institutions in New Democracies*

One of the most often cited arguments used to argue why we should expect to find greater political budget cycle effects in developing democracies focuses on the level of development of their political institutions (Persson and Tabellini 2003; Keefer 2005; Keefer and Khemani 2005; Brender and Drazen 2005; Gonzalez 2002). These studies argue that the dynamics of political competition are very distinct in new democracies due to both the experience level of voters and the level of maturity of political institutions. As voters lack experience and information to hold elected officials accountable in democracies that have recently transitioned from authoritarian rule, they are more apt to believe campaign promises and can therefore be more easily manipulated by politicians in the first few elections. In addition, political institutions such as the legislature, the judiciary, central banking authorities,

⁹² There are four countries that prohibit reelection: Guatemala, Honduras, Mexico, and Paraguay.

⁹³ Although the overall trend in the region moved towards shorter terms, Venezuela and Bolivia increased the length of the executive term by one additional year; the length in office is six and five years respectively. (Payne et al. 2007).

and the media may not be autonomous or institutionalized in the early stages of democracy. An important test of these theories is the impact of elections on government spending in the early stages of democracies as the explanations cited to explain the vulnerability of democracies to these pressures should be even greater in the elections prior to the consolidation of democracy.

In their classic study of transition from authoritarian rule, O'Donnell and Schmitter (1986) posit that the end of transitions from authoritarianism to democracy is marked by foundational elections in which power is effectively transferred from the military to civilians. Seeking to examine the implications of founding elections on political budget cycles, Block, Ferree and Singh (2003) test whether there are higher peaks in economic policy performance during "foundational" elections in sub-Saharan African countries between 1980 and 1995. The authors offer several hypotheses for why politicians have an enhanced capacity to manipulate economic policy in the first election following authoritarian rule. Authoritarian incumbents may have greater discretion to manipulate expenditures prior to elections. Non-democratic leaders who are reluctantly holding elections may also dig in deep to government coffers to scare off the opposition as the winners will undoubtedly have to undertake painful stabilization measures. New democracies may have reduced capacities to check and balance the powers exerted by the executive branch. In addition, voters may also be more credulous rendering non-democratic rulers with greater power to manipulate fiscal and monetary policies. Based on 65 presidential elections, they report that multiparty competitive elections (22 of total elections) are associated with higher monetary growth and government consumption as a share of GDP. However, they are not able to prove their hypothesis that founding elections have an additional effect on government spending.⁹⁴

Przeworski (1991) calls attention to the fact that democratization is driven in great part by Keynesian coalitions that coalesce to demand greater redistribution. Accordingly, it is argued that incoming elected governments during transitions are less able to respond effectively to critical economic challenges because they come to power facing a huge backlog of unfulfilled demands. Based on the recognized confluence of economic and political crisis

⁹⁴ The interpretation of the empirical model is problematic as the authors include single party elections in their econometric analyses and note that these are roughly half of total elections in the sample. Given that many of these non-competitive elections may also involve the participation of incumbent authoritarian rulers, it is unclear if foundational elections were appropriately measured. As will be discussed in the section on election data utilized in this chapter, I only examine the impact of *multiparty* competitive elections on government and social spending.

that usually precipitate democratic transitions, scholars have argued that newly elected governments find themselves needing to adopt policies that are unsustainable in the medium to long-run given the high stakes involved threatening a reversion to autocracy (Haggard and Kaufman 1989).

Some suggestive evidence that increases in political competition during the transition to democracy fuel increase political budget cycles is provided by Gonzalez (2002) in her study of autocratic Mexican presidential elections between 1957 and 1997. 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 “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.

In contrast to the process of democratization in sub-Saharan Africa where a significant share of elections were single party elections (only 33 of the 65 elections between 1980 and 1995 involved competition by more than one party!), all elections in Latin America following the transition to democracy have involved multiparty competition. Moreover, it should be noted that the majority of countries in Latin America experienced a founding election between the 1970s and 1990. 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. In all but one case founding elections were marked by the participation of formerly banned political parties and the retreat of the military (Huntington 1991). Moreover, with the exception of the 1978 failed 4th term reelection bid of Joaquín Balaguer in the Dominican Republic, authoritarians retreated and did not participate in the elections in all other twelve instances. If the arguments posited by Gonzalez and Block, et. al are correct, we should expect to find that the magnitude of political budget and welfare cycles is greater in new versus established democratic regimes.

5.2.6 *Decentralization*

A final explanation focuses attention on the level of decentralization of social programs, a process that has been particularly important for some countries in Latin America. In countries where social policy has been devolved to sub-national governments, increases in social spending may not be as clearly attributed to the politicians who have spearheaded these welfare improvements. Such is the case made by Kneebone and McKenzie (2001) in Canadian provincial elections. The authors find that governments increase spending on education, roads, and culture and decrease spending levels on hospitals, welfare, and business subsidies in election years (with all expenditures measured relative to provincial GDP). They argue that the emphasis by incumbents on education, roads and culture during election years reflects the attempt to signal competence to voters by directing expenditures to categories (education and transportation) that are visible and activities where provincial governments are clearly identified as having the primary responsibility (versus the federal government). In contrast, they point to the decreased level of spending on health as explained by the fact that though visible, this area of public service is less clearly attributed to competent provincial governments as responsibilities are shared with the federal government.

These findings have some important implications for studies of national-level social spending in Latin America as they suggest that the level of decentralization of social services may be an important determinant of the potential cyclicity of government spending. In countries in which social service delivery has been significantly devolved to local governments, it may be that central government expenditures are not as vulnerable to attempts to target these resources so as to influence election outcomes. On the other hand, it may be that decentralization has resulted in augmented electioneering of social security transfers as national candidates are not able to take credit for the building of new schools and inauguration of new hospitals.

5.3 Data

5.3.1 *Fiscal and Social Spending Data*

The dependent variable in this chapter is based on annual data on central government total expenditures and expenditures by welfare function as reported in the International Monetary Fund (IMF)'s *Government Finance Statistics* (GFS) (International Monetary Fund 2006). Data on the total and budget share of expenditures allocated to education, health care and social security relative to total government spending were compiled based on the procedures and criteria explained in Chapter 2. In all fifteen countries the fiscal calendar year follows the calendar year. It should be noted that the terms fiscal balance and deficit will be used interchangeably in the chapter as most countries ran persistent budget deficits throughout the period. However, a positive value of the fiscal balance is a budget surplus.

5.3.2 *Democracy and Election Data*

As described in Chapter 2, a minimalist definition of democracy was adopted to code democratic years based on the updated versions of the Przeworski, Álvarez, Cheibub and Limongi (2000) dataset compiled by Cheibub and Ghandi (2004). The sample includes only democratic years in Latin America between 1973 and 2000. 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). Appendix 2B describes the democracy codebook used in this study in detail.

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). To double check information, all

coding was compared with the *Database on Political Institutions* (DPI) created by Beck et al. (2001).⁹⁵

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 1st 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 1st and before December 31st 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 chapter, 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.

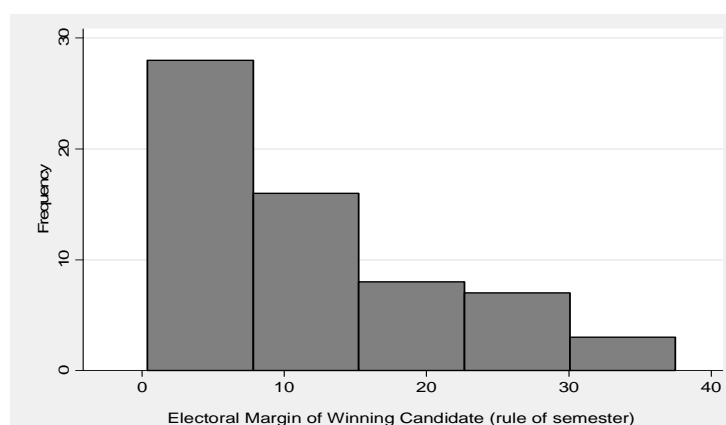
The dataset includes 62 presidential elections and 71 legislative elections and a total of 188 non-election democratic years. Of the 15 countries in the sample, all countries had at least one presidential election. On average, there are 3.5 presidential elections for country. Mexico is the only country that has one election (e.g. Vicente Fox’s election in 2000). In 55 presidential elections, voters also choose congressional representatives. The chapter 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 chapter is to test both pre 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,

⁹⁵ In a few cases, errors were detected in the coding of election years in the DPI database for Latin America and the coding changes that were necessary are described in Appendix 2B. This information suggests that DPI data for Latin American democracies should only be used after careful checking for consistency.

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 1994, Ricardo Lagos in Chile in 2000, Joaquín Balaguer in the Dominican Republic in 1990 and Hugo Chavez in Venezuela in 2000.

In order to examine whether closer elections impact fiscal and social spending budget shares, a variable to measure the electoral margin of the leading 2 candidates in each competitive election between 1973 and 2000 was created based on the percentages reported in Nohlen (2005) and the *Political Database of the Americas* (Center for Latin American Studies at Georgetown University 2007). In cases where there were two rounds of voting, the first round was adopted. Argentina held two presidential elections in 1973. Only the results of the September 1973 elections were included in the dataset. The wide variation in electoral margins over the course of the three decades is striking. On average, presidents win elections with margins of 11.6 percent of a lead over the 2nd challenger. However, as Figure 5.1 demonstrates the distribution of electoral margins is left-skewed with more than half of all 62 elections having been won by less than 10 percent of the vote.

Figure 5.1. Electoral Margins in Latin America, 1973-2000



Source: Elaborated by the author.

To test for differences between election 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 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 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. In the case of Bolivia, the June 1980 presidential election of Hernán Siles Zuazo in Bolivia is not coded as a democratic transitional election as he could not assume due to a coup d'état although he finally assumes power in October 1982. Table 5.2 lists all presidential elections that took place in the sample and denotes the 30 elections that took place during democratic transitions in boldface.

Table 5.2. Presidential Elections in Latin America, 1973-2000

Country	Presidential Election Dates
Argentina	9/1973, 10/1983 , 5/1989 , 5/1995 , 10/1999
Bolivia	7/1985 , 5/1989, 6/1993, 6/1997
Brazil	1/1985 , 11/1989 , 10/1994, 10/1998
Chile	12/1989 , 12/1993 , 12/1999
Costa Rica*	2/1974, 2/1978, 2/1982, 2/1986, 2/1990, 2/1994, 2/1998
Dominican Republic	5/1978 , 5/1982 , 5/1986 , 5/1990 , 5/1994 , 6/1996, 5/2000
Ecuador	4/1979 , 1/1984 , 1/1988, 7/1992, 7/1996, 6/1998
El Salvador	3/1984 , 3/1989 , 4/1994, 3/1999
Guatemala	11/1985 , 11/1990 , 11/1995, 11/1999
Mexico	7/2000
Panama	5/1994 , 5/1999
Paraguay	5/1993 , 5/1998
Peru	5/1980 , 4/1985
Uruguay	11/1984 , 11/1989 , 11/1994, 11/1999
Venezuela*	12/1973, 12/1978, 12/1983, 12/1988, 12/1993, 12/1998, 7/2000
Total Number of Elections	62
Number of Democratic Transitional Elections	30

Notes: * No democratic transition elections. Elections during democratic transitional period are indicated in boldface. There are 3 elections (Dominican Republic, Mexico and Venezuela) that ended in 2000 and therefore do not have a post-election year.

Source: Elaborated by the author.

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. There 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 chapter. 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, Raul Alfonsín similarly ceded power earlier than anticipated though only a few months earlier than planned. The robustness of the results reported in Section 5 without both of these elections will be tested. Third, control variables in estimations include measures of per capita economic growth to precisely capture the possible endogenous relationship that exists between government spending and elections. In addition, it should be noted that the social policy instruments employed in this chapter are expressed as percentages of the budget and not GDP.

5.3.3 Control Variables

Several demographic and economic variables are included as control variables; each is drawn from the theoretical literature. Demographic characteristics of the population are likely to impact education, health care and social security spending. Two demographic variables representing the fraction of the population aged 15–64 and 65+ are employed as controls. A higher percentage of elderly people in the population is expected to be positively related to social security transfers and health spending. The specification of the budget share of health and education spending model includes controls for the age of the population who are children below the age of 14 instead of the working age population. A higher percentage of children in the population is expected to be positively related to education spending. Unless otherwise noted, the control variables data are from the World Bank's *World Development Indicators* (2007).

In the last three decades of the 20th 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.⁹⁶ 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.

⁹⁶ Ames (1987:79)

Given the heterogeneity in income and growth rates across the region, it is important to include economic variable controls. The first is the level of economic development, defined as the real gross domestic product per capita and measured in constant US\$ 2000 dollars. Per capita income is included in the model to control for Wagner's Law, which 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 social spending.

A control for GDP growth lagged one year was 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 and this pattern is even more pronounced in the case of social spending (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 imports + exports/GDP, where the denominator is calculated by converting domestic local currency to current US\$ based on exchange rate conversions. This measure is affected by the size of the economy and by changes in the exchange rate, but the inclusion of country dummies as in the model is assumed to correct for these effects.

Capital mobility is included as its expected impact on social spending is expected to be distinct from trade flows.⁹⁷ 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).⁹⁸ Quinn attempts to measure the intensity of capital controls through a more detailed reading of the IMF's *Annual Report on Exchange Arrangements and Exchange Restriction*.

⁹⁷ The correlation between trade integration and capital mobility is surprisingly low for the sample (0.3321). There is a wide variation and sometimes negative relationship between trade openness and capital mobility across countries in Latin America.

⁹⁸ See Avelino, Brown and Hunter (2005) for a more detailed explanation of this variable.

The degree of capital account restrictions are measured from 0 to 4. It has been standard in the literature to include a control for capital mobility based on the premise that responses to government policy outcomes will be higher when there is greater capital mobility. This variable has not been found to be particularly significant in empirical studies on social spending and the reason may be that foreign investors do not react by shifting investments based on changes in the resources allocated to government social programs (Segura-Ubiergo 2007). Finally, the model also takes into account the impact of time effects. Year dummies are used to account for the important differences in regional and international conditions over the course of the time period.

5.4 Econometric Models and Estimation Methods

5.4.1 Estimation Procedure and Model Specification

The baseline model was introduced in Chapter 3. In this chapter tests are carried out on the effect of elections. In a first stage, the model that will be tested for political budget cycles in fiscal and welfare policy for this chapter can be specified as:

$$Y_{i,t} = \alpha_i + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 ELEC_{i,t} + \beta_4 (ELEC)_{i,t+1} + c_i + \mu_{i,t} \quad (1)$$

The three measures of fiscal policy that are used are total government spending as a share of GDP, total revenue collection as a share of GDP and the budget balance as a share of GDP. The three measures of welfare policy follow the same measures utilized in the earlier chapter on the effect of democracy for social spending. In addition to modeling how election cycles impact aggregate social spending, disaggregated measures of education and health spending and social security transfers are also tested in order to examine whether the impact of elections on the share of social spending is of similar magnitude for different types of expenditures. In each specification lags of the dependent variable are included on the right-hand side. This model tests follows the literature and tests whether there are differences in 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. A dummy variable for the year following elections is also included. Z is a vector of control variables as described earlier and α represents year dummies. 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. The error term, μ_{it} , is an error term associated with unit i at time t .

In a second stage, a model is used to test whether close electoral margins between the winning presidential candidate and the 2nd runner up effects fiscal and social spending performance. In this second model, the parameter estimate for $\hat{\beta}_3$ is the effect of competition in the year of the election on the dependent variable. The expected sign of this coefficient is negative as smaller electoral margins are expected to translate into larger increases in government expenditures before elections. The parameter estimate for $\hat{\beta}_4$ is the effect of the election margin in the year following the election of the president. If a president was elected in a close election, the national leader might be expected to have less overall political support and will therefore be less apt to institute significant changes in the first year in office. If this is the case, the coefficient for margins in the year following elections should not be statistically significant from zero.

$$\text{Log}(Y_{i,t}) = \alpha_i + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 \text{Log}(\text{Margin}_{i,t}) + \beta_4 \text{Log}(\text{Margin}_{i,t+1}) + c_i + \mu_{i,t}. \quad (2)$$

In a third 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 (NEWDEMELEC). The third model that will be tested can be specified as:

$$Y_{i,t} = \alpha_i + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 (\text{ELEC})_{i,t} + \beta_4 (\text{ELEC})_{i,t+1} + \beta_5 (\text{NEWDEMELEC})_{i,t} + \beta_6 (\text{NEWDEMELEC})_{i,t+1} + c_i + \mu_{i,t}. \quad (3)$$

The marginal effect of an election during the democratic transition phase, $\hat{\beta}_5$, 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 and social spending is correct, $\hat{\beta}_5$ should be positive and statistically significant from zero and $\hat{\beta}_3 + \hat{\beta}_5$ (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 and social spending 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, $\hat{\beta}_3$ 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.

Due to the limited number of observations, a fourth model that controls for both founding elections and electoral margins was not undertaken.⁹⁹ With sufficient observations, such a model would improve the ability to discern the impact of the effects of both contested and founding elections on fiscal and social policy performance. This remains a task for future research.

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 that will be tested in this chapter. Given that the lag model specification violates the assumption of strict exogeneity, the possibility of a feedback effect of y_{it} to future values of x_{it} is introduced (Woolridge 2002). As has been discussed in detail in Chapter 3, hypothesis testing is particularly challenging if the data follow a unit root. Given the uncertainty regarding the stationary character of the data, the *level* of welfare state development will be tested using three models: (1) pooled OLS with panel corrected standard errors,¹⁰⁰ (2) the Arellano and Bond (1991) first-differenced GMM estimator (GMM-Diff); and, (3) the Blundell and Bond (1998) system GMM estimator (GMM-System).¹⁰¹ To examine whether results are altered when estimation is carried in models more appropriate for highly persistent series, the results of a fourth model that is an error correction model (ECM) with panel corrected standard errors based on the *first difference* of the dependent variable will also be reported in the Appendix 5A. Estimation was carried out with and without fixed effects to address issues of unit heterogeneity.¹⁰² Thus, the pooled OLS, GMM and ECM estimates will be estimated with and without controls for time and unit heterogeneity. As the results with and without fixed effects are reported in the tables presented below there will be a total of seven columns.

⁹⁹ This formulation would test the effect of both democratic transitional and contested elections by interacting terms. The fourth model could be specified as: $Y_{i,t} = \alpha + \beta_1 Y_{i,t-1} + \beta_2 Z_{i,t} + \beta_3 (\text{Margin})_{i,t} + \beta_4 (\text{Margin})_{i,t+1} + \beta_5 (\text{Democratic Transitional Election} \times \text{Margin})_{i,t} + \beta_6 (\text{Democratic Transitional Election} \times \text{Margin})_{i,t+1} + c_i + \mu_{i,t}$.

¹⁰⁰ The model will be estimated with the Stata XTPCSE command.

¹⁰¹ The exercise and commands for GMM estimation are based on Roodman (2006) and were carried out using in Stata 10.

¹⁰² 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).

5.5 Time-Series-Cross-Section Analysis of Fiscal and Social Policy Performance and Electoral Cycles

As we have reviewed briefly, the predictions on the patterns for political budget and welfare cycles have only been superficially and poorly tested for Latin America. This section presents the results of a series of hypotheses that are tested with respect to political cycles for fiscal and social policy based on the three specifications outlined in the previous section. The first model tests whether total budget or social spending vary in the election and post-election years. The second specification examines whether additional differences exist for election and post-election years when races are tightly disputed. The third model examines whether the patterns on electoral cycles persist once controls for elections during democratic transitions and established democracy are introduced. The coefficients on the effect of election cycles reported in the tables that follow are based on the rule of the semester.¹⁰³ To anticipate one important conclusion of this chapter, the battery of tests below provide compelling evidence that elections do not have a monotonic impact on fiscal and social policies. Based on each of the three specifications, this section first presents the results for political budget cycles and how elections impact government spending (panel A), tax collection (panel B) and fiscal deficits (panel C). It then moves to discuss the regression results for aggregate social spending (panel A), pensions (panel B) and health and education expenditures (panel C).

Results for Political Budget Cycles

Table 5.3 examines whether there are peaks in government spending, revenue collection and the fiscal deficit in the year of and the year following a presidential election in Latin America. All of the dependent variables are measured as a share of GDP. For presentation purposes only the results for the two dummy variables, election year and post-election year, are presented although the covariates include control 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

¹⁰³ 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.

of the population living in urban areas, trade openness, capital mobility and year effects.¹⁰⁴ The base group is all other non-election and non-post election democratic years.

Political budget cycle theory predicts that expenditures will increase prior to elections, but as we reviewed earlier there are contrasting views of what should happen to government spending once winners are in office. Whereas some models including Rogoff's (1990) political budget cycle framework predict that spending will increase in the first year after elections, Remmer (1993) argues that elections in Latin America give leaders the political capital to enact reform. As a result, government spending is theorized to decrease following elections as decision-makers quickly move to stabilize the economy. The results in Table 4.3 do not support Rogoff's (1990) model and lend some support to Remmer's theory. Government spending not only decreases in the election year, it also decreases in the year following. These findings are robust across most specifications. The coefficients are consistently the same sign and statistically significant at the ten percent or less level.

The results in panel B align more closely with the predictions of the political budget cycle literature. In the year of elections, less tax revenue is collected. Yet, as fiscal expenditures decrease as well in the year of elections, panel C confirms that the budget deficit does not increase. It bears mention that the fiscal balance can be either a negative or positive value. A positive coefficient on fiscal balance is thus measuring an improvement in the government's fiscal balance. As government expenditures continue to decrease, fiscal surpluses improve considerably once the winning candidate enters office. These results counter findings reported by earlier empirical studies. Both Mejía Acosta and Coppedge (2001) and Amorim Neto and Borsani (2004) had reported statistically significant evidence that budget deficits worsen in election years. In this study, these trends are tested for a larger group of countries over a longer time period and do not find evidence to confirm their findings. In addition, the evidence produced contrasts with Ames (1987), who had not found a post-election effect for budget deficits.

¹⁰⁴ I also estimated the same model including a dummy variable for the pre-election years in the sample. The statistical significance of the election year and post-election year dummies were verified, but the dummy variable on the pre-election year was insignificant. The same model was also estimated with the rate of inflation. In most cases inflation was not statistically significant. Therefore, the results reported in the tables that follow do not include either the pre-election year or the rate of inflation.

Contested Elections

Table 5.4 tests the effect of contested elections by employing a variable of the actual margin in the year of the election and the year following when the winner takes office. This model allows us to test whether the trends detected earlier persist after control for the degree of electoral competitiveness. The results presented in Table 5.4 report the effect of the log transformation of electoral margins at time t and $t+1$ on the log transformation of the dependent variable in the case of government spending and tax revenue collection as a share of GDP. These variables seek to test the marginal difference of contested elections on fiscal policy. The base category is non-election democratic years. In the case of fiscal balances, the dependent variable is in percentage form and was not logged.

The results provide strong evidence to confirm that government spending is responsive to greater political competition. The coefficient on the difference between the winning and 2nd runner up in an election year is negative and statistically significant in most specifications in panel A of Table 5.4. Government spending is more likely to be reduced by a smaller amount by incumbent governments competing in tight elections in the year of elections. The estimated coefficient for electoral margins in the year following the election is also negative and statistically significant across specifications except in the case of the GMM System estimates. If candidates enter office with small leads over competitors, they are more likely to reduce spending in the first year of their administration by a smaller amount. On the other hand, tax collection efforts do not seem to be impacted by electoral cycles. Neither the coefficient for the election or post-election year is statistically significant in panel B of Table 5.4. The coefficient estimate for the effect of the election on fiscal balance (a negative number if a fiscal deficit) in panel C is positive for both the election and post-election years, but the estimates are only statistically significant in the case of the year following elections. As the coefficient is positive, this result implies that the fiscal deficit becomes significantly less negative e.g., the deficit share increases in the first year in office during election years if election margin was smaller.

Table 5.3. Political Budget Cycles in Latin America, 1973-2000): The Effect of Pre and Post-Election Years

<i>Dependent Variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>A. Government Spending/GDP</i>	Pooled OLS PCSE	Pooled OLS PCSE with country fixed effects (f.e.)	Pooled OLS PCSE with country and year f.e.	GMM One-Step First Diff ^a	GMM One-Step First Diff with year f.e. ^a	GMM One-Step System ^a	GMM One-Step System with year f.e. ^a
Election Year _t	-0.690* (0.359)	-0.517 (0.331)	-1.168*** (0.328)	-1.093** (0.497)	-1.486*** (0.432)	-0.431 (0.359)	-1.173*** (0.260)
Election Year _{t+1}	-0.997*** (0.322)	-0.859*** (0.295)	-1.206*** (0.296)	-1.552* (0.815)	-1.608** (0.738)	-0.773* (0.376)	-1.151** (0.459)
Observations	226	226	226	211	211	218	218
R-squared	0.88	0.90	0.92				
Number of Instruments				14	39	19	44
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.246	0.312	0.295	0.390
Hansen test for joint validity of instruments (<i>p value</i>)				0.201	1.000	0.914	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						1.000	1.000
<i>B. Government Revenue /GDP</i>							
Election Year _t	-0.331 (0.425)	-0.482 (0.392)	-0.776** (0.378)	-0.292 (0.342)	-0.871** (0.363)	-0.549** (0.218)	-1.121** (0.422)
Election Year _{t+1}	0.532 (0.384)	0.298 (0.360)	0.023 (0.336)	0.162 (0.197)	-0.159 (0.343)	-0.170 (0.120)	-0.600 (0.367)
Observations	225	225	225	210	210	217	217
R-squared	0.85	0.88	0.91				
Number of Instruments				14	39	19	44
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.689	0.281	0.417	0.093
Hansen test for joint validity of instruments (<i>p value</i>)				0.120	1.000	0.502	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.921	1.000
<i>C. Fiscal Balance/GDP</i>							
Election Year _t	-0.238 (0.415)	-0.418 (0.396)	-0.096 (0.378)	-0.162 (0.400)	0.216 (0.539)	-0.259 (0.362)	-0.116 (0.297)
Election Year _{t+1}	0.972*** (0.356)	0.758** (0.339)	0.843*** (0.323)	0.592 (0.498)	1.050** (0.483)	0.450 (0.420)	0.816** (0.300)
Observations	225	225	225	210	210	217	217
R-squared	0.46	0.52	0.63				
Number of Instruments				14	39	19	44
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.594	0.715	0.367	0.140
Hansen test for joint validity of instruments (<i>p value</i>)				0.015	1.000	0.275	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.534	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 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%.

^aTwo 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 (2007a). 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. ^bThe 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 5.4. Political Budget Cycles in Latin America, 1973-2000: The Effect of Contested Elections

<i>Dependent Variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>A. Log(Government Spending/GDP)</i>	Pooled OLS PCSE	Pooled OLS PCSE with country fixed effects (f.e.)	Pooled OLS PCSE with country and year f.e.	GMM One-Step First Diff ^a	GMM One-Step First Diff with year f.e. ^a	GMM One-Step System ^a	GMM One-Step System with year f.e. ^a
Log (Margin _t)	-0.019** (0.008)	-0.013 (0.008)	-0.024*** (0.008)	-0.029 (0.017)	-0.030** (0.013)	-0.012 (0.008)	-0.025*** (0.008)
Log (Margin _{t+1})	-0.023*** (0.008)	-0.018** (0.008)	-0.024*** (0.008)	-0.041 (0.027)	-0.029* (0.015)	-0.017* (0.009)	-0.022 (0.013)
Observations	226	226	226	211	211	218	218
R-squared	0.88	0.90	0.92				
Number of Instruments				14	39	19	44
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.417	0.470	0.896	0.842
Hansen test for joint validity of instruments (<i>p value</i>)				0.622	1.000	0.964	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						1.000	1.000
<i>B. Log (Government Revenue /GDP)</i>							
Log (Margin _t)	-0.009 (0.010)	-0.008 (0.009)	-0.009 (0.009)	-0.017 (0.028)	-0.013 (0.011)	-0.008 (0.007)	-0.015 (0.009)
Log (Margin _{t+1})	0.005 (0.009)	0.005 (0.008)	0.003 (0.008)	0.011 (0.031)	-0.002 (0.012)	-0.004 (0.005)	-0.009 (0.011)
Observations	225	225	225	210	210	217	217
R-squared	0.88	0.91	0.93				
Number of Instruments				14	39	19	44
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.336	0.053	0.854	0.352
Hansen test for joint validity of instruments (<i>p value</i>)				0.678	1.000	0.854	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						1.000	1.000
<i>C. Log (Fiscal Balance/GDP)</i>							
Log (Margin _t)	-0.120 (0.204)	-0.157 (0.197)	0.044 (0.180)	-0.001 (0.160)	0.268 (0.277)	-0.108 (0.150)	-0.040 (0.140)
Log (Margin _{t+1})	0.411*** (0.158)	0.338** (0.154)	0.414*** (0.138)	0.305 (0.221)	0.595** (0.272)	0.168 (0.188)	0.343** (0.151)
Observations	225	225	225	210	210	217	217
R-squared	0.46	0.52	0.63				
Number of Instruments				14	39	19	44
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.617	0.827	0.374	0.144
Hansen test for joint validity of instruments (<i>p value</i>)				0.025	1.000	0.342	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.635	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 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%.

^aTwo 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 (2007a). 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. ^bThe 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.

The results of panels A, B and C of Table 5.4 add important nuances to the patterns reported earlier in Table 5.3. In contrast to Ames (1987), the results for Latin America in the post-1973 era suggest that the closeness of elections do influence fiscal policy performance. Governments are more likely to undertake fiscal adjustments in the election and post-election year, but these tendencies are lessened if elections are tight. Budget deficits are more likely to improve in the winning candidate's first year in office, but this proclivity increases if the president received a greater vote of confidence in the election. The degree of competition does not seem to be a determinant of the likelihood that a government will reduce taxes in an election year. Thus, after control for the degree of electoral competition, the results of Table 5.4 suggest that the electoral swings in fiscal deficits reflect a preference of Latin American governments to manipulate expenditures over taxation depending on the closeness of the election competition.

Elections in New Democracies

Given that a significant share of Latin American countries experienced a transition to democracy since 1979, the results reported earlier in Tables 5.3 and 5.4 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)¹⁰⁵ The results reported below are based on a more theoretically grounded definition of new democracies as only elections that took place prior to the year in which the less than two turnover criteria for new democracies was satisfied are coded a transitional democracy election years. In Table 5.5 two parameters for transitional democratic elections to capture the marginal difference of both election and post-election years are introduced. The parameters for election and post-election year now represent the effect of elections when democracy has been consolidated.

The results do not indicate that fiscal performance electoral cycles are driven by elections in transitional democracies. The coefficients of both the election and post-election year are not significant in any of the specifications testing cyclical effects. Rather, the results suggest that the reduction in fiscal spending in the year of the election and year following the election is driven by adjustments made in consolidated democracies. The last row of Panel A

¹⁰⁵ Block, Ferree and Singh (2003) 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, I tested the model excluding the thirteen elections that were the first election following military rule. The results reported in Table 5 were confirmed.

in Table 5.5 presents the results of the combined effect of both the election and post-election year for consolidated democracies. Relative to all other democratic, non-election years, the models predict that Latin American democracies that have undergone at least two turnovers of power reduce spending between 1.4 to 3.3 percent of GDP; the coefficient is strongly robust in most specifications. Although only confirmed by the GMM System model, the results in panel C of Table 5.5 indicate that fiscal balances improve by approximately 1 to 1.6 percent of GDP in the year after elections in established democracies.

Together, the results presented in Tables 5.3, 5.4 and 5.5 allow us to draw some important conclusive findings on fiscal policy performance in Latin American democracies. Contrary to past empirical research in developing democracies, there appears to be an anti-political budget cycle similar to the findings reported by Remmer (1993) for Latin America. Governments are more likely to reduce government spending in the year of the election and the year once the newly elected government is in office. Fiscal balances also improve once winning candidates enter office. There is robust evidence that these trends are driven by elections in established democracies. In contrast, closer elections may be more likely to result in less cutbacks in government spending and smaller decreases in budget deficits. The results also show that both political competition and consolidated democracy have distinct effects on fiscal policy performance in Latin America. These findings suggest that past research that has failed to account for the transitional character of democracy and different levels of competition may be ignoring important factors that impact the effect of democratic elections on fiscal policy.

Table 5.5. Political Budget Cycles in Latin America, 1973-2000: The Effect of Elections in New Democracies

<i>Dependent Variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>A. Government Spending/GDP^a</i>	Pooled OLS PCSE	Pooled OLS PCSE with country fixed effects (f.e.)	Pooled OLS PCSE with country and year f.e.	GMM One-Step First Diff ^a	GMM One- Step First Diff with year f.e. ^a	GMM One-Step System ^a	GMM One-Step System with year f.e. ^a
Election Year _t	-0.690 (0.483)	-0.515 (0.454)	-1.138*** (0.425)	-1.449 (0.870)	-1.645** (0.679)	-0.412 (0.367)	-1.254** (0.475)
Election Year _{t+1}	-0.723* (0.403)	-0.532 (0.404)	-0.998*** (0.387)	-1.609 (1.317)	-1.626 (1.079)	-0.494 (0.737)	-1.079 (0.763)
New Democracy Election Year _t	0.006 (0.582)	0.006 (0.611)	-0.043 (0.545)	0.779 (1.320)	0.375 (0.763)	-0.055 (0.754)	0.183 (0.726)
New Democracy Election Year _{t+1}	-0.590 (0.523)	-0.691 (0.568)	-0.438 (0.521)	0.088 (1.399)	0.048 (1.073)	-0.588 (0.896)	-0.114 (0.866)
Observations	226	226	226	211	211	218	218
R-squared	0.88	0.90	0.92				
Number of Instruments				16	41	21	46
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.230	0.303	0.277	0.397
Hansen test for joint validity of instruments (<i>p value</i>)				0.197	1.000	1.000	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						1.000	1.000
Total Effect of an Election in a Consolidated Democracy	-1.413** (0.700)	-1.047 (0.695)	-2.136*** (0.653)	-3.058 (1.986)	-3.270** (1.544)	-0.905 (0.798)	-2.332** (0.953)
<i>B. Government Revenue /GDP</i>							
Election Year _t	-0.246 (0.595)	-0.279 (0.570)	-0.681 (0.509)	-0.372 (0.385)	-0.852* (0.415)	-0.368* (0.201)	-0.784 (0.536)
Election Year _{t+1}	0.878* (0.514)	0.785 (0.535)	0.153 (0.463)	0.150 (0.425)	-0.136 (0.425)	-0.009 (0.274)	-0.358 (0.502)
New Democracy Election Year _t	-0.196 (0.677)	-0.469 (0.702)	-0.220 (0.615)	0.191 (0.398)	-0.031 (0.366)	-0.378 (0.544)	-0.754 (0.671)
New Democracy Election Year _{t+1}	-0.759 (0.606)	-1.041 (0.666)	-0.283 (0.583)	0.033 (0.674)	-0.036 (0.457)	-0.276 (0.565)	-0.500 (0.693)
Observations	225	225	225	210	210	217	217
R-squared	0.85	0.88	0.91				
Number of Instruments				16	41	19	46
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.692	0.290	0.428	0.102
Hansen test for joint validity of instruments (<i>p value</i>)				0.840	1.000	0.887	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.807	1.000

C. Fiscal Balance/GDP

Election Year _t	-0.022 (0.576)	-0.201 (0.584)	0.014 (0.524)	-0.182 (0.324)	0.296 (0.509)	-0.093 (0.407)	0.162 (0.435)
Election Year _{t+1}	1.202** (0.476)	0.988* (0.520)	0.894** (0.451)	0.420 (0.671)	1.079 (0.689)	0.383 (0.512)	1.007* (0.475)
New Democracy Election Year _t	-0.511 (0.678)	-0.507 (0.730)	-0.269 (0.633)	0.017 (0.458)	-0.182 (0.442)	-0.378 (0.675)	-0.683 (0.661)
New Democracy Election Year _{t+1}	-0.520 (0.574)	-0.491 (0.648)	-0.117 (0.565)	0.360 (0.744)	-0.065 (0.714)	0.147 (0.409)	-0.502 (0.537)
Observations	225	225	225	210	210	217	217
R-squared	0.47	0.52	0.63				
Number of Instruments				16	41	21	46
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.579	0.712	0.365	0.113
Hansen test for joint validity of instruments (<i>p value</i>)				0.094	1.000	0.998	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						1.000	1.000
Total Effect of an Election in a Consolidated Democracy	1.180 (0.821)	0.787 (0.896)	0.908 (0.778)	0.238 (0.715)	1.374 (0.925)	0.289 (0.543)	1.168* (0.662)

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%.

^a 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 (2007a). 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.

^b 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.

5.5.1 *Results for Political Welfare Cycles*

Table 5.6 presents the results for the effect of election and post-election years on aggregate social spending (panel A), as well as the effects when spending is disaggregated by the share of the budget spent on pensions (panel B) and health and education combined (panel C). Relative to the base category, which is defined as all other democratic years, the estimations yield strong results that conform to empirical work for the effect of transfers in industrialized democracies and also contrast markedly with the patterns reported previously in the literature for Latin America.

Theoretical and empirical predictions suggest that higher social spending allocations should occur in election and post-election years. While the dummy variable measuring an election year is 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. 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. Across most specifications, the results also confirm that there is a statistically significant increase in the share of expenditures on health, education and pensions ranging between 2.9 and 3.3 percent of the total government budget. The results in panel A of Table 5.6 are particularly striking if we compare them to earlier results in Table 5.3 that had shown that government spending as a share of GDP declines both in the year of and the year following presidential elections in Latin America. When social expenditures are disaggregated, the sign of the coefficient estimates for the election and post-election years remain positive, however, they are only statistically significant at the 10 percent or less levels for the post-election year in the pooled OLS estimates for social security (panel B) and the GMM system model with control for time invariant effects for health and education expenditures (panel C).

These results 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. 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. In the case of transfers in particular, Franzese (2002b) argues that electioneering should be even higher after leaders assume office as candidates counter-promise expenditure increases. In contrast, Persson and Tabellini (2005) have posited that that social spending targets very different types of voters in developing and developed countries. They hypothesize that spending on education, health and pension benefits in developing countries generally aid smaller segments of the urban population and we should therefore expect to find less responsiveness to elections in these types of programs for developing countries. The results in Table 5.6 lend credence to the framework proposed by Franceze (2002b) for developed democracies and evidence against the prediction of Persson and Tabellini (2005).

Contested Elections

In the case of social transfers in Great Britain, Schultz (1995) argues that the degree of competition is critical to the responsiveness of the welfare regime to electoral cycles. Schultz uses the effect of margins based on polling data to show that the share of social transfers as a share of GDP responds by increasing in the election year in closer elections. In the absence of polling data, this chapter tests the effect of electoral competition in the election and post-election year based on the size of the election margin between the two leading candidates. In addition to the election year effects tested by Schultz, this chapter also seeks to explore whether there are differences in social spending when elected officials enter office in the aftermath of extremely competitive elections. To control for the marginal effect of greater competition, the models tested in Table 5.7 include the election margins in each of the sixty two contested elections in Latin America in the year of and the year following the election between 1973 and 2000 as independent variables.

Table 5.6. Political Welfare Cycles in Latin America: The Effect of Pre and Post-Election Years

<i>Dependent Variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>A. Social Spending/Government Budget</i>	Pooled OLS PCSE	Pooled OLS PCSE with country fixed effects (f.e.)	Pooled OLS PCSE with country and year f.e.	GMM One-Step First Diff ^a	GMM One- Step First Diff with year f.e.	GMM One-Step System	GMM One-Step System with year f.e. ^a
Election Year _t	0.814 (1.521)	-0.199 (1.294)	0.288 (1.355)	-0.501 (1.157)	0.510 (1.043)	-0.296 (1.144)	0.649 (1.145)
Election Year _{t+1}	3.266** (1.371)	2.829** (1.178)	2.912** (1.246)	2.835 (2.152)	3.003* (1.603)	2.781 (2.003)	3.126* (1.546)
Observations	207	207	207	201	201	207	207
R-squared	0.79	0.85	0.86				
Number of Instruments				13	39	17	43
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.507	0.859	0.295	0.865
Hansen test for joint validity of instruments (<i>p value</i>)				0.744	1.000	0.821	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.626	1.000
<i>B. Social Security/Government Budget</i>							
Election Year _t	0.639 (1.208)	0.137 (1.022)	0.578 (1.005)	-0.234 (0.988)	0.239 (0.859)	0.243 (1.074)	0.722 (1.021)
Election Year _{t+1}	2.556** (1.093)	2.264** (0.934)	1.998** (0.950)	1.974 (1.451)	1.563 (1.309)	2.291 (1.628)	2.033 (1.236)
Observations	207	207	207	201	201	207	207
R-squared	0.89	0.93	0.94				
Number of Instruments				13	39	17	43
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.768	0.593	0.952	0.920
Hansen test for joint validity of instruments (<i>p value</i>)				0.498	1.000	0.906	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.680	1.000
<i>C. Health and Education/Government Budget</i>							
Election Year _t	0.229 (0.799)	-0.239 (0.702)	-0.210 (0.750)	-0.272 (0.407)	-0.293 (0.448)	-0.123 (0.761)	-0.095 (0.666)
Election Year _{t+1}	0.750 (0.788)	0.579 (0.680)	0.945 (0.721)	0.939 (0.764)	1.308 (0.803)	0.681 (0.587)	1.044* (0.537)
Observations	207	207	207	201	201	207	207
R-squared	0.46	0.52	0.63				
Number of Instruments				13	39	17	43
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.061	0.300	0.256	0.196
Hansen test for joint validity of instruments (<i>p value</i>)				0.429	1.000	0.681	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.805	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 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%.

^aOne lag of the dependent variable was 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 (2007a). 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.

^bThe 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 5.7. Political Welfare Cycles in Latin America, 1973-2000: The Effect of Contested Elections

<i>Dependent Variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>A. Log(Social Spending/Government Budget)</i>	Pooled OLS PCSE	Pooled OLS PCSE with country fixed effects (f.e.)	Pooled OLS PCSE with country and year f.e.	GMM One- Step First Diff	GMM One-Step First Diff with year f.e.	GMM One-Step System	GMM One-Step System with year f.e.
Log (Margin _t)	0.015 (0.010)	0.004 (0.009)	0.005 (0.009)	-0.001 (0.011)	0.014 (0.015)	0.004 (0.011)	0.016 (0.012)
Log (Margin _{t+1})	0.016* (0.009)	0.014* (0.008)	0.017** (0.008)	0.017 (0.011)	0.024** (0.010)	0.018* (0.010)	0.024*** (0.007)
Observations	207	207	207	201	201	207	207
R-squared	0.89	0.91	0.93				
Number of Instruments				13	39	17	43
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.907	0.654	0.967	0.669
Hansen test for joint validity of instruments (<i>p value</i>)				0.302	1.000	0.644	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.905	1.000
<i>B. Log (Social Security/Government Budget)</i>							
Log (Margin _t)	0.020 (0.026)	0.031 (0.023)	0.020 (0.023)	0.011 (0.025)	0.015 (0.037)	0.018 (0.025)	0.014 (0.031)
Log (Margin _{t+1})	0.001 (0.027)	0.028 (0.023)	0.011 (0.025)	-0.003 (0.032)	-0.014 (0.036)	0.008 (0.032)	-0.015 (0.027)
Observations	207	207	207	200	200	207	207
R-squared	0.94	0.96	0.97				
Number of Instruments				13	39	17	43
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.479	0.846	0.575	0.835
Hansen test for joint validity of instruments (<i>p value</i>)				0.535	1.000	0.288	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						1.000	1.000
<i>C. Log (Health and Education/Government Budget)</i>							
Log (Margin _t)	0.016 (0.012)	-0.003 (0.011)	-0.000 (0.011)	-0.011 (0.019)	0.006 (0.009)	0.009 (0.019)	0.013 (0.014)
Log (Margin _{t+1})	0.008 (0.012)	0.006 (0.010)	0.016 (0.010)	0.017 (0.011)	0.023* (0.012)	0.012 (0.012)	0.023** (0.009)
Observations	207	207	207	201	201	207	207
R-squared	0.89	0.92	0.94				
Number of Instruments				13	39	17	44
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.098	0.253	0.366	0.160
Hansen test for joint validity of instruments (<i>p value</i>)				0.254	1.000	0.842	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.414	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 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%.

^a One lag of the dependent variable was 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 (2007a). 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. ^b 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 5.7 suggests that the degree of electoral competition operates distinctly from patterns reported in the case of British parliamentary elections with respect to social policy performance in Latin American democracies. In Latin American democracies, elections decided with greater electoral margins coincided with increases in social spending in the winning government's inaugural year in office. As panel A confirms, the coefficients for the post-election years are positive for aggregate welfare spending. In the case of the post-election year the results for the effect of electoral margins on welfare expenditures are strongly robust as the coefficient estimates are statistically significant in most specifications at the five percent or less level.

Panel C in Table 5.7 suggests that the reductions in social spending associated with highly contested elections can be explained by changes in the share of the budget allocated to health and education. There is robust evidence in the GMM first difference and system estimations, as well as the ECM estimations in Appendix 5A, that the share of the budget allocated to health and education expenditures is more likely to be increased when the election was decided with a large margin of victory for the winning candidate in the year following the election. In contrast, the electoral effects on social security pensions are not discernible as the coefficients for the election year and post-election margins reported in Panel B are not statistically significant. Intuitively, this evidence could support the argument that electoral victories with greater margins give new administrations more solidly backed mandates and political capital to initiate social policy innovations directed at the health and education sectors. In this regard, the evidence suggests the size of the election margin is critical in explaining the propensity of newly elected governments to increase health and education budget allocations in the first year in office. However, there are grounds for caution as the results are not confirmed in the pooled OLS.

Elections in New Democracies

Given arguments suggesting that electoral cycles should be more marked in the case of new democracies, Table 5.8 tests whether social policy is more responsive in democratic governments following the transition from authoritarian rule. Dummy variables are also included to control for established democratic regimes that have had at least two consecutive turnovers in power and the omitted category is all other democratic years. If the argument of the distinction between new and consolidated democracies is correct, we should expect to

find that the coefficients that measure the marginal effect of elections in new democracies are positive and statistically significant and the coefficient for election and post-election years in consolidated democracies should not be statistically significant from zero.

The results in Table 5.8 suggest 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 most strongly confirmed in the case of aggregate social spending. The coefficient estimates in panel A predict that post-election year social spending will increase by 1.3 to 2.6 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.

In this case of new democracies, the results in panels A, B and C of Table 5.8 suggest that the increase in post-election social spending is higher than for consolidated democracies. However, the results for new democracies are weaker as post-election year welfare and social security expenditures are only estimated to be higher with five percent statistical significance in the case of the pooled OLS model with control for unit effects. In this specification, social spending is predicted to increase by 4.1 percent and pensions by 3.0 percent in the year after the election. The Error Correction Model results in Appendix 5A also confirm that there is an increase in aggregate social spending and health and education expenditures in the year following the election.

Table 5.8. Political Welfare Cycles in Latin America, 1973-2000: The Effect of Elections in New Democracies

<i>Dependent Variable:</i>	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>A. Social Spending/Government Budget</i>	Pooled OLS PCSE	Pooled OLS PCSE with country fixed effects (f.e.)	Pooled OLS PCSE with country and year f.e.	GMM One-Step First Diff	GMM One- Step First Diff with year f.e.	GMM One-Step System	GMM One-Step System with year f.e. ^a
Election Year _t	1.103 (2.142)	-1.142 (1.744)	-0.319 (1.780)	-1.291 (1.514)	-0.144 (1.178)	0.305 (0.992)	0.667 (0.830)
Election Year _{t+1}	2.562* (1.413)	0.638 (1.155)	1.396 (1.330)	0.459 (0.747)	1.317** (0.513)	2.935 (1.787)	2.306** (1.021)
New Democracy Election Year _t	-0.544 (2.623)	1.923 (2.374)	1.319 (2.156)	1.581 (3.070)	1.295 (2.808)	-1.024 (2.969)	0.056 (2.237)
New Democracy Election Year _{t+1}	1.290 (2.210)	4.099** (2.044)	2.964 (1.817)	4.310 (3.456)	3.145 (2.602)	-0.247 (3.721)	1.530 (2.099)
Observations	207	207	207	201	201	207	207
R-squared	0.79	0.85	0.86				
Number of Instruments				15	41	19	45
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.570	0.995	0.620	0.960
Hansen test for joint validity of instruments (<i>p value</i>)				0.762	1.000	0.999	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.975	1.000
<i>B. Social Security/Government Budget</i>							
Election Year _t	0.494 (1.715)	-0.410 (1.438)	0.273 (1.365)	-0.623 (1.197)	-0.069 (0.998)	-0.298 (1.093)	0.360 (1.055)
Election Year _{t+1}	1.699 (1.276)	0.667 (1.095)	1.128 (1.103)	0.516 (0.597)	0.923 (0.871)	1.408 (0.871)	1.232 (0.852)
New Democracy Election Year _t	0.273 (2.018)	1.118 (1.854)	0.658 (1.675)	0.773 (1.966)	0.589 (2.097)	1.021 (1.867)	0.681 (1.818)
New Democracy Election Year _{t+1}	1.570 (1.741)	2.981* (1.649)	1.690 (1.461)	2.653 (2.294)	1.201 (1.956)	1.616 (2.438)	1.497 (1.767)
Observations	207	207	207	201	201	207	207
R-squared	0.90	0.93	0.94				
Number of Instruments				15	41	19	45
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.893	0.636	0.987	0.845
Hansen test for joint validity of instruments (<i>p value</i>)				0.495	1.000	1.000	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.988	1.000

C. Health and Education/Government Budget

Election Year _t	0.614 (1.229)	-0.638 (1.133)	-0.544 (1.082)	-0.777 (0.780)	-0.454 (0.941)	0.766 (1.159)	0.614 (0.718)
Election Year _{t+1}	0.905 (1.226)	-0.003 (1.108)	0.300 (1.054)	-0.104 (0.770)	0.508 (0.868)	1.665 (1.334)	1.200 (0.906)
New Democracy Election Year _t	-0.747 (1.335)	0.800 (1.348)	0.717 (1.270)	0.984 (2.047)	0.316 (1.831)	-1.768 (2.972)	-1.305 (1.584)
New Democracy Election Year _{t+1}	-0.298 (1.350)	1.092 (1.296)	1.274 (1.225)	1.874 (1.414)	1.467 (1.289)	-1.851 (2.517)	-0.324 (1.415)
Observations	207	207	207	201	201	207	207
R-squared	0.85	0.89	0.91				
Number of Instruments				15	41	19	45
Arellano-Bond test for AR(2) ^b (<i>p value</i>)				0.062	0.297	0.257	0.202
Hansen test for joint validity of instruments (<i>p value</i>)				0.501	1.000	0.825	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.933	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 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%.

^a One lag of the dependent variable was 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 (2007a). 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. ^b 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.

In the aftermath of the external debt crisis, most Latin American countries adopted adjustment programs in the 1980s and 1990s. In this chapter, we have explored the extent to which democracies adhered to fiscal adjustments during elections and shown that the commitment to improving fiscal balances was persistent and marked. The purpose of this section has been to examine if democracies protect the poor from painful budget cuts during or immediately after elections. The results presented show quite strongly that fiscal adjustments were combined with important increases in social spending in the inaugural year of a new presidential administration. However, the results are not as robust and conclusive as we would hope as the tests verifying that democracies are more inclined to prioritize welfare expenditures are not confirmed as strongly across specifications in the tests on disaggregated social spending directed at pensions, health and education.

5.6 Conclusion

This chapter has undertaken a more theoretically grounded exploration of political cycles in fiscal and social policy performance. It has attempted to address several gaps in past empirical research by considering what happens to performance measures prior to and immediately after elections, when there is increased competition and in the transition period following rule by an authoritarian regime prior to the consolidation of democracy. Applying a battery of specifications to data on Latin American democracies, this study has found compelling evidence that questions past research on political budget cycles in developing democracies. Electoral competitions do not appear to operate as catalysts for fiscal policy as hypothesized to function in models of political budget cycles. Rather, the evidence presented in this chapter shows quite conclusively that they serve as opportunities for governments to undertake fiscal adjustments in the year of competition and after the election has been won.

The results do not support Rogoff's (1990) political budget model that predicts that government spending will increase in the year of and the year following elections. In addition, the responsiveness of tax policy to elections is also proven to be non-applicable to democracies in the region after controlling for the degree of electoral competition and transitional democratic elections. Instead, this chapter has found support confirming earlier empirical findings documented by Remmer (1993) for the 1980s in Latin American democracies in which she argues there is evidence of an "anti-political business cycle". Total

government expenditures as a share of GDP not only decrease in the election year, they also decrease in the year following elections once the new government has been inaugurated. As government spending decreases following elections, decision-makers quickly move to stabilize the economy and fiscal surpluses also improve in the post-election year. This pattern lends support to Remmer's hypothesis that the primary effect of elections in Latin America is to give leaders the political capital to enact reform.

However, this chapter also adds a slightly more nuanced view of Remmer's original findings shedding light on how budget cuts have impacted social policy in Latin America. Whereas governments undertake fiscal adjustment to reduce budget deficits, this chapter shows that democracies enact measures to protect the poor. Social spending is confirmed to increase in a presidential administration's first year in office. Moreover, governments elected with larger electoral margins are more likely to undertake fiscal adjustments, but they are also more likely to increase allocations towards redistributive social programs. In turn, governments elected with smaller election margins will lack the political capital to adopt more significant fiscal austerity measures or institute large-scale investments in social programs.

The results also suggest that the fiscal and social policy response differs depending on whether a democracy is in a transition stage or has been consolidated. In the period following authoritarian rule and prior to two turnovers in power, new democracies have a reduced capacity to adopt fiscal austerity in the pre and post election period. Consolidated democracies, on the other hand, exert greater fiscal discretion both before and after elections. With respect to social spending, the distinctions between transition and consolidated regimes are less distinct. In both cases, democracies make efforts to protect the poor from budget cutbacks after entering office. However, new democracies are less 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.

Thus far, there has been limited exploration of models that study political welfare cycles in developing democracies. The results in this chapter give support to theories that argue that the driving force of fiscal and social policy responsiveness due to democratic turnovers is greater *after* elections. In this sense, the findings in this chapter support the arguments of Franzese (2002b) for social spending detected for elections in industrialized

democracies. However, this chapter also helps to better explain the conditions that make this pattern robust for Latin American democracies in light of the important differences that contrast markedly with developed democracies.

6. Globalization and the Welfare State in Latin America

6.1 Introduction

Chapter 3 of this dissertation applied a battery of tests to show that the wave of democratization that has spread across Latin America in recent decades has helped to install governments that are more attentive to the welfare of their citizens as compared to authoritarian counterparts in particular with respect to expenditures on health and education. The purpose of this chapter is to examine whether increased trade and financial integration with global markets has forced Latin American governments to reduce social spending after controlling for the responsiveness of democratic regimes. This chapter reexamines past research on the impact of globalization on social expenditures considering common problems of time-series-cross-section (TSCS) data analysis using a new data set for a sample of fifteen countries in Latin America and the Caribbean for the period between 1973 and 2000.¹⁰⁶

A series of recent studies have been undertaken on the impact of international trade and financial market integration for the allocation of resources for health, education and social security in Latin America, but these empirical studies have produced generally inconclusive findings. Some studies have produced evidence indicating that increased exposure to the world economy has caused Latin American governments to increase social protection, while others have shown that social spending has been reduced in those countries with higher levels of international trade and capital flows. The empirical results of this chapter suggest globalization has induced an “efficiency” response in Latin America that has included the adoption of fiscal policies to reduce labor costs (e.g. lower social security transfers) in order to retain country competitiveness. At the same time, the results also support the hypothesis that governments seek to compensate certain groups in an era of globalization. After controlling for the level of trade integration, higher rates of capital mobility result in greater budget allocations for pensions. However, globalization has not induced governments to undertake human capital investments (e.g. increase health and education budget allocations). After controlling for the level of trade openness, there is some evidence to suggest that greater degrees of capital mobility reduced budget shares allocated to health and education.

¹⁰⁶ The countries are Argentina, Bolivia, Brazil, Chile, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Mexico, Paraguay, Panama, Peru, Uruguay, and Venezuela. All of the tables and figures that will be presented for Latin America in this chapter will refer to this fifteen country sample.

These results are noteworthy as they challenge past research. This chapter argues that the differences in the central patterns we observe in the literature with respect to whether Latin American democracies reduce social spending in the context of globalization may be due to differences in measurement and methods. The majority of studies have employed measures of social spending relative to the size of the economy. This chapter emphasizes that the politics of resource allocation decisions are best understood by measures that consider social expenditures as a share of the total budget and employ this method to test hypotheses on the effect of globalization on social spending. Most studies have also relied on a single measure of trade and capital mobility to test theories of globalization's effects on social spending. In this chapter, several different measures of both trade and capital market integration are employed to test if the differences in coefficient signs and magnitudes depend on the data used to measure both independent variables. The results confirm that the patterns detected depend on which metrics are used.

As we discussed in Chapter 2, the hypothesis that social spending data in Latin America is highly persistent can not be ruled out. To address these concerns, the hypotheses on the effects of globalization on social spending for Latin America are tested employing a battery of specifications. There is generally strong correlation between the sign and significance levels across the multivariate estimation techniques that were tested in this chapter. These results suggest that the conclusions reached in this chapter are fairly robust as they are not highly contingent on the method used to obtain them.

The chapter is structured in the following way. Section 2 reviews the literature on the effects of enhanced trade and financial integration on welfare state responses in Latin America in the context of the region's rapid and thorough economic transformation over the course of the late 20th century. Section 3 presents the measures of globalization that will be employed for hypothesis testing with a time series cross sectional data set for Latin America. Section 4 introduces the model estimation and specifications that will be used. A variety of statistical methods are analyzed in order to check the robustness of the relationship given concerns that the data may not be stationary. Section 5 reports and discusses the results of the empirical analysis. The final section concludes the chapter with a summary of the key findings.

6.2 Review of the Literature

The dramatic liberalization of trade and financial markets undertaken by Latin American economies in the last decades of the 20th century have transmitted additional demands on governments for greater income redistribution from the rich to poor and for greater social protection of vulnerable populations from economic shocks. Yet, integration with world markets has also limited the extent to which governments have been able to respond to shield citizens from greater economic volatility and lackluster economic growth. This section briefly summarizes the theoretical frameworks that have been developed to explain the responsiveness of governments to globalization and then shift to examining the empirical research on the impact of trade and financial integration on social spending for Latin American countries.

From the earliest political economy models scholars have recognized that various combinations of domestic institutional arrangements and international exposure can have varying efficacy on the ability of governments to employ fiscal policy. A key concern for scholars of the welfare state has been focused on trying to understand its response to the increased flows of goods, capital and technology between and among developed and developing economies given the marked increase in the magnitude, complexity and speed of these forces in the postwar period. The majority of the empirical research on the politics of welfare has focused on analyzing the experiences of developed, industrialized Western European democracies (Cameron 1978; Hicks and Swank 1992; Quinn 1997; Rodrik 1997, 1998; Iversen and Cusack 2000; Huber and Stephens 2001; Swank 2002; Moene and Wallerstein 2002; Pierson 2001; Garrett 2001; Garrett and Mitchell 2001; Hicks and Zorn 2005).¹⁰⁷ Sparked by the expansion of government spending as trade integration advanced in European and North American democracies, empirical studies have presented varying levels of supporting evidence that industrialized democracies have sought to protect the welfare of their citizens by implementing policies that lessen the negative impact of the opening of their

¹⁰⁷ There have been a few studies that include both developed and developing countries. In these studies, however, there are additional challenges to drawing inferences as it has been argued that there are significant differences in the political and economic conditions between both groups of countries. In order to control for these differences, most studies attempt to evaluate commonalities on welfare response by controlling for regional or unit heterogeneity, as well as factors including the level of economic development and demographic characteristics of the population. Scholars have argued that the extent to which both groups may be following a similar convergence trajectory have yet to be validated (Rudra 2007). For examples of this research and its findings, see Rodrik (1997) and Persson and Tabellini (2005).

economies.¹⁰⁸ Research has focused on examining what aspects of democracy including party and politician ideology and the relative strength of organized civil society groups (unions and workers) are most critical to explaining the responsiveness of government spending to increased integration with world markets and the concomitant rise in job insecurity, unemployment and declining wages in North America and Europe.

Thus far, limited research has been directed at examining how welfare states in developing countries have responded to globalization (Haggard and Rudra 2005). Within this body of research, particular attention has been directed at examining the social safety response of Latin America as the inception and transformation of the welfare state in these countries has coincided with dramatic shifts in the level of integration of these economies with global markets. Indeed, scholars have noted a marked and distinct pattern as from the onset the commitments to the welfare state evolved differently in Latin American countries than patterns found in developed countries and part of the reason for the differing evolutionary path has been attributed to the interaction between international and domestic factors (Huber 1996; Solimano 2005). In contrast to European nations, the welfare state in Latin America in the post war period developed in many countries in closed, protected economies that had implemented import-substitution industrialization.¹⁰⁹ In the aftermath of the external debt crisis and its legacy of inflation, fiscal deficits, exchange rate instability and debt servicing problems, several democratizing Latin America countries were also among the first generation of developing economies to adopt market opening policies. In the late 1980s and 1990s, Latin American governments adopted comprehensive and unprecedented market-oriented reforms to ignite economic growth. The reform package included import liberalization, domestic financial liberalization, privatizations and labor reforms.¹¹⁰ These reforms promised higher growth and employment rates.

For Latin American nations well-endowed in unskilled workers, the impact of increased imports from industrialized countries rich in skilled workers was expected to result in greater demand for unskilled labor and reductions in wage differentials between skilled and

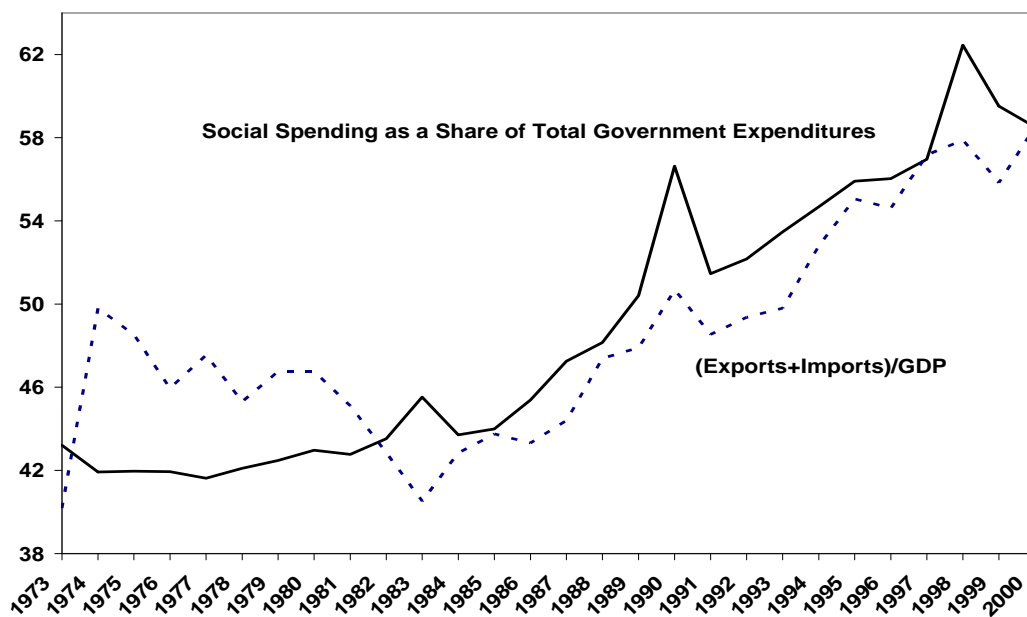
¹⁰⁸ It should be noted that most studies do not analyze welfare spending per se, but rather government spending.

¹⁰⁹ As Huber summarizes, “ISI created urban constituencies for social insurance, that is employed middle and working classes with an interest in protection from loss of earnings due to accidents, illness, and old age. Politically, passage of social insurance schemes was facilitated by the fact that employers did not really have to absorb the costs of their contributions but rather could pass them on to consumers because they were operating in protected markets. (144)”

¹¹⁰ Chile is the only country that implemented reforms a decade earlier in the 1970s.

unskilled labor.¹¹¹ Contrary to expectations, the 1990s was marked by lackluster growth, macroeconomic instability, unemployment, expanding informal employment and higher inequality (Stallings and Weller 2001). After the long periods of autocratic rule that had dominated policymaking in many countries, many were surprised to see new, fragile democracies implementing increasingly unpopular neoliberal reforms. The perplexity only amplified when regimes persisted in continuing reforms despite poor performance records. Scholars have cited this evidence to argue that Latin American governments face “efficiency” pressures that have forced them to adopt conservative fiscal policies to reduce labor costs, boost exports and lure capital inflows.¹¹² According to this viewpoint, government social policy choices are heavily constrained by world markets forcing governments to reduce costs in order for economies to remain competitive.

Figure 6.1. Social Spending and Trade Openness in Latin America, 1973-2000
(average for 15 countries in sample)



Sources: International Monetary Fund, *Government Finance Statistics* and World Development Bank, *World Development Indicators*.

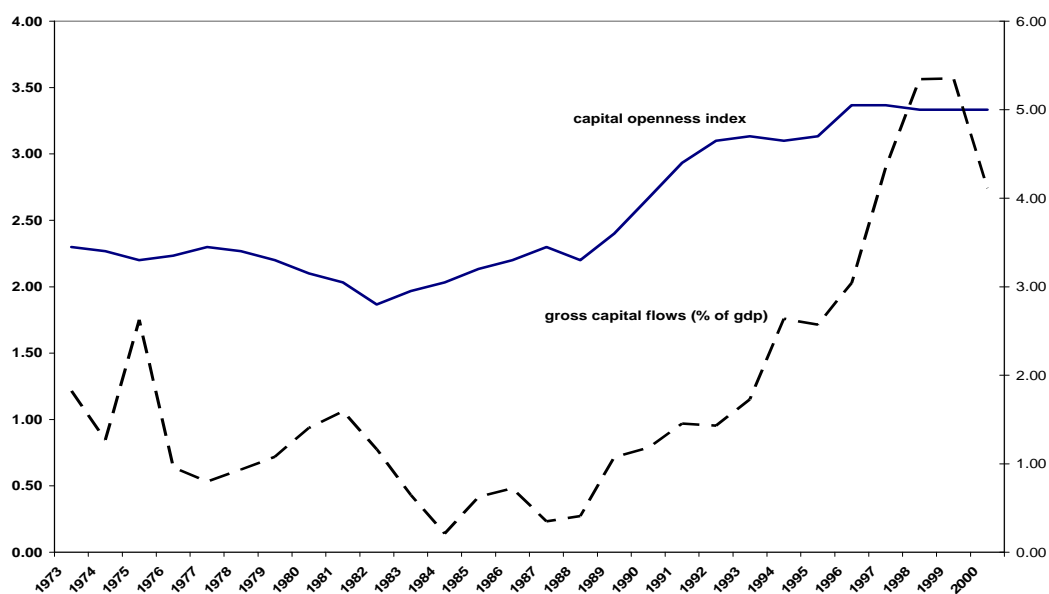
An alternative viewpoint has underscored the convergence in government responses to globalization. Similar to industrialized democracies that increased social insurance investments, Latin American democracies also adopted “compensating” policies. With the

¹¹¹ These are the standard predictions of the Heckscher-Ohlin-Samuelson factor endowments model (Rodrik 1997).

¹¹² The characterization of “compensation” and “efficiency” responses to globalization are terms that were first applied by Garret (2001) to denote both strategies.

return to democracy, theoretical models of politics predicted that democratic governments in the region would be more responsive to reallocating social expenditures towards lower and middle class voters. Evidence in favor of this governmental response is corroborated in Figure 6.1 that confirms a sustained increase in the average level of social expenditures as a share of the budget from the external debt crisis in the early 1980s to the end of the 20th century coinciding with the increasing outward orientation of the sample of fifteen Latin American countries studied in this chapter. The early 1980s is also significant as it represents a turning point. The subsequent decade and a half are marked by increased social spending, trade and financial openness and democratization in Latin America (see Figure 6.2). The increase in social spending is even more remarkable if one takes into account the fact that government expenditure overall contracted throughout the 1980s and only began to increase after 1991 (see Figure 6.3).

Figure 6.2. Capital Account Openness Index and Gross Capital Flows in Latin America, 1973-2000
(average for 15 countries in sample)

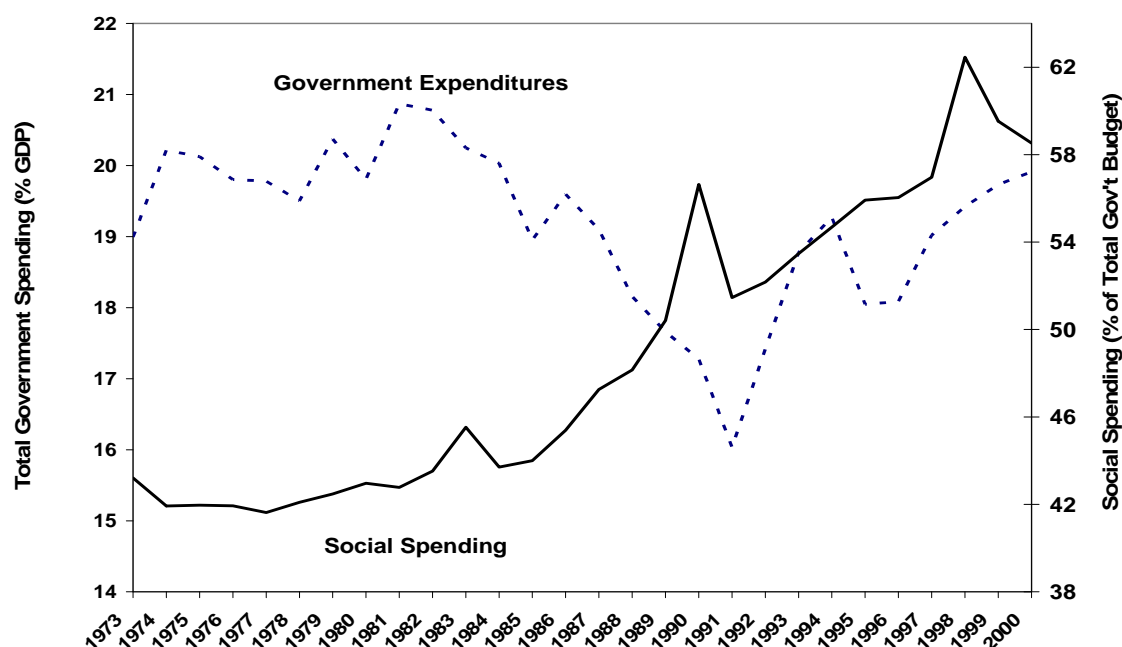


Source: World Development Bank, *World Development Indicators* and updated capital openness index for Latin America developed by Avelino, Brown, Hunter (2005).

More in depth empirical analysis with controls for key factors that explain divergences in the level of welfare spending among Latin American countries, however, has shown that increases in trade and financial openness have more complex effects on government fiscal and social policies. Several recent studies that analyze the impact of political regimes on welfare expenditures using data on government spending published by the IMF have concluded that

trade openness is correlated with lower levels of social spending in Latin America.¹¹³ The first empirical study to confirm this relationship robustly for the region was performed by Kaufman and Segura-Ubiergo (2001) and based on a study of fourteen Latin American countries during the period following the first petroleum crisis in 1973 to 1997. In this study, the authors find that greater levels of trade integration are associated with reductions in social security transfers, which are the components of social spending that provide the most direct protections from vulnerability to market forces, and aggregate social spending.

Figure 6.3. Government and Social Spending in Latin America, 1973-2000
(average for 15 countries in sample)



Sources: International Monetary Fund, *Government Finance Statistics* and *International Financial Statistics*.

Using the same data updated to 2003, Segura-Ubiergo (2007) confirms these earlier findings concluding that globalization introduces efficiency pressures on Latin American welfare states. He concludes that the substantial negative effect of trade openness on social spending is driven by (a) the pressures imposed by key actors on governments to adopt fiscal responsibility; and, (b) the weakening of the “organic link” between labor and political parties to pressure governments for compensatory policies as a result of reductions in manufactured labor employment, increases in informality and the fall in wages which caused a weakening in

¹¹³ Both ECLAC and IMF data report figures as reported by governments to these bodies. The difference is that each measure follows a different methodology to calculate the level of expenditures allocated for social expenditures.

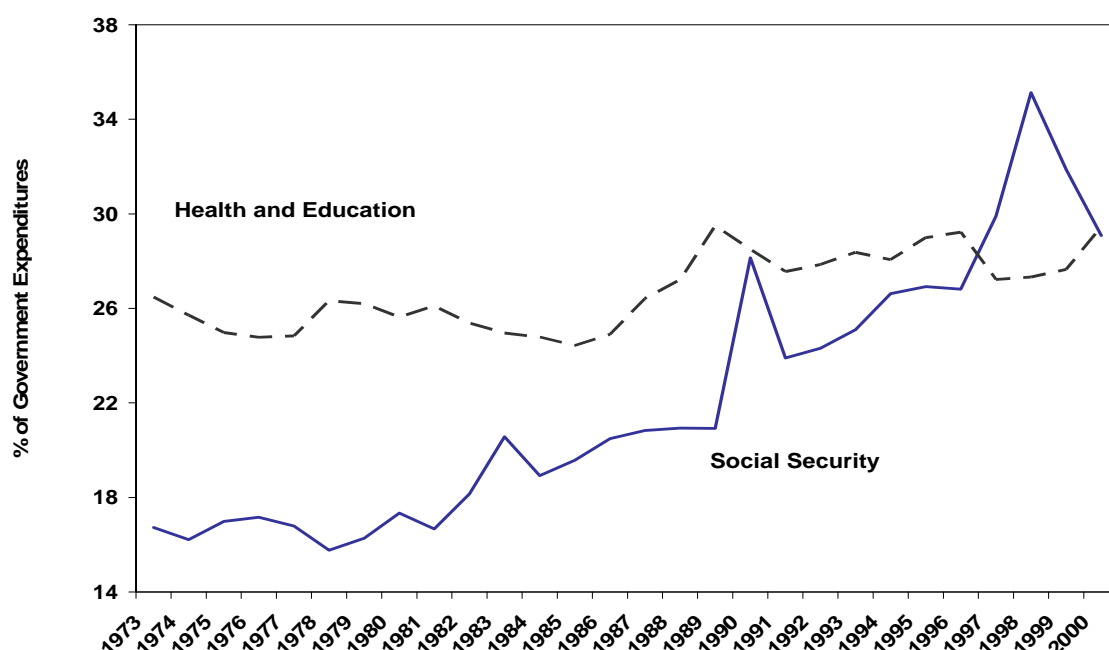
the link between employment and social protection. Wibbels (2006) reaches the same conclusion with respect to the effect of trade openness on total social spending and social security transfers arguing that globalization has reduced the potential countercyclical response of Latin American economies. These studies are less conclusive, however, on the effects of trade integration for health and education spending as the coefficient on trade openness is not found to be statistically significant.¹¹⁴

The arguments defended by Kaufman and Segura-Ubiergo (2001), Wibbels (2006) and Segura-Ubiergo (2007) of labor's losing political power due to global market pressures contrasts with the empirical data that shows that the large increases in social spending since the mid 1980s have been driven by social security expenditures that have surpassed combined spending on health and education in the 1990s (see Figure 6.4). Social security expenditures as a share of the budget have increased from approximately 17 percent of the total budget between 1973 and 1982 to nearly 30 percent in the 1990s. Education and health spending, on the other hand, have remained fairly constant averaging 26 percent in the former and 28 percent in the latter period. The level of variation of social security fluctuations is also much higher. Whereas the standard deviation of the average share of education and health spending as a share of the budget was 1.5 percent from 1973 to 2000, the variation in social security spending is three times greater.

Closer examination of tax burden in Latin America reveals that social security taxes make up a minor share of the total tax burden in most countries. Aldunate and Martner (2006) report that on average social security compromises only 3.1% of GDP in Latin America (out of a total of 18% of GDP collected) whereas the average in OECD countries is 9.5% (out of an average of 36.3% of GDP) and 11.6 in the European Union (out of an average of 40.5% of GDP). Of course, there are some notable exceptions. The most notable is Brazil where social security taxes make up 15.4% of GDP (out of a total tax burden of 37.4% of GDP).

¹¹⁴ Wibbels (2006) does report a statistically positive coefficient on trade openness for health and education spending, but only in the case of a positive economic shock, but his findings for all other specifications are negative and statistically significant.

Figure 6.4. Health and Education Expenditures and Social Security Transfers in Latin America, 1973-2000
(average for 15 countries in sample)



Sour

ce: International Monetary Fund, *Government Finance Statistics*.

In contrast to these studies, two studies have produced robust evidence to support the hypothesis that democracies are more likely to increase welfare expenditures in Latin America in the post-external debt crisis era. Avelino, Brown, and Hunter (2005) examine the impact of democratization on social spending in a sample of 19 countries from 1980 to 1999 controlling for trade and capital integration.¹¹⁵ The authors show that more open democracies spend more on social policies than their authoritarian counterparts and argue that more globalized Latin American democracies should not be considered as either choosing to compensate or promote efficiency. Rather, they emphasize that elected governments are more prone to compensate certain groups by spending more on social security and also promote greater levels of efficiency by increasing the share of government resources devoted to education. The authors note that these results are not stable if different measures of trade openness ($(\text{Imports} + \text{Exports})/\text{GDP}$) are used in the same specification. The results of Avelino, Brown and Hunter were obtained with an alternative measure of trade openness in which GDP is measured in PPP rates in the denominator rather than the standard exchange

¹¹⁵ The difference in social spending outcome predicted by the model depending on trade openness measure is tested and discussed in Section V.

rate conversion measure.¹¹⁶ These findings have also been supported by research done by Huber, Mustillo and Stephens (2004) who report that trade openness is positively linked with investments in health and education in a sample of 22 Latin American countries from 1970 to 2000, but fail to find a statistically robust relationship between trade and social security expenditures.¹¹⁷

Table 6.1 summarizes the empirical studies that have been undertaken directed at examining the impact of globalization on social spending for Latin America including the effects on the components of social spending programs in education, health and social security. Trade openness has been consistently reported to reduce both aggregate social spending (four of five studies) and pension expenditures (three of five studies). On the other hand, the evidence of the effect of trade openness on health and education expenditures is more inconclusive. Of the five studies that test for this effect, three do not find a statistically significant relationship. While Huber, Mustillo and Stephens (2004) find that trade openness increases expenditures on health and education, Avelino, Brown, and Hunter (2005) examine both expenditure items separately and only find a statistically significant positive coefficient for the effect of trade openness on government expenditures for education.

¹¹⁶ Avelino, Brown and Hunter (2005) report a negative and not statistically significant sign on the coefficient estimate for trade openness if the standard exchange rate measure is used.

¹¹⁷ The sample adopted by the authors is unconventional as they include five English-speaking nations not commonly considered part of Latin America. These countries are Barbados, Belize, Guyana, Jamaica and Trinidad and Tobago.

Table 6.1. Cross-National Empirical Studies of the Impact of Globalization on Social Sector Spending for Latin America

Authors	Countries	Period	Hypothesis Testing Method	Includes Controls for Unit Heterogeneity	Trade Openness Measure	Capital Openness Measure	Impact of Globalization		
							Social Spending	Social Security	Health and Education
Kaufman and Segura-Ubiergo (2001)	14	1973-1997	Error Correction Model (ECM) with panel corrected standard errors (PCSE)	Yes	Exchange Rate Measure	Morely, Machado and Pettinato (1999) Index of Capital Account Liberalization	Trade Openness decreases social spending. Capital mobility increases social spending.	Trade Openness decreases social security spending. Capital mobility is not statistically significant.	Trade Openness is not statistically significant. Capital mobility increases expenditures.
Huber, Mustillo and Stephens (2004)	22	1970-2000	Pooled OLS with robust-cluster estimator of the standard errors (AR1 model)	No	Exchange Rate Measure	Net Inflows of FDI/GDP	n.a.	Trade openness is not statistically significant. Net inflows of FDI not statistically significant.	Trade openness increases human capital spending. Net inflows of FDI not statistically significant.
Avelino, Brown and Hunter (2005)	19	1980-1999	Pooled OLS with panel corrected standard errors (PCSE)	Yes	Exchange Rate & PPP	Updated Quinn (1997)'s Capital Account Restrictions Index (1-4)	Trade Openness (PPP) increases social spending. Trade Openness (Exchange Rate measure) decreases social spending. Capital mobility is not significant.	Trade Openness (PPPs) increases social security spending. Capital mobility is not significant.	Trade Openness (PPPs) increases education spending, but does not impact health spending. Capital mobility is not significant.
Avelino (2005)	17	1980-2000	Pooled OLS with panel corrected standard errors (PCSE)	Yes	Exchange Rate Measure	Updated Quinn (1997)'s Capital Account Restrictions Index (0-4)	Trade Openness and Capital Mobility decrease social spending.	n.a.	n.a.
Wibbels (2006)	12	1970-1995	ECM with PCSE	Yes	Exchange Rate Measure	Garrett, Brune, Guisinger and Sorens' (2001) Index of Capital Account Openness (0-8)	Trade Openness decreases social spending. Capital mobility increases social spending.	Trade Openness decreases social security spending. In some regressions, capital mobility increases social spending.	Trade openness is not statistically significant. Capital mobility both increases and decreases spending depending on controls.
Segura-Ubiergo (2007)	14	1973-2003	ECM with PCSE	Yes	Exchange Rate Measure	Author's own index of capital account openness (0-4)	Trade Openness decreases social spending. Capital mobility increases social spending.	Trade Openness decreases social security spending. Capital mobility increases social security spending.	Trade Openness and Capital Mobility are not statistically significant.

Notes: The table only reports findings for estimated coefficients with significance levels of 10% or lower. For ECM models, the sign and significance level of the independent variable in levels was used.

Source: Elaborated by the author

Whereas the effect of trade flows has been shown to be an important determinant of social spending, the aforementioned studies have reported generally inconclusive findings with respect to capital mobility. In most cases, the estimated coefficient on capital account regulation has been found to be positive and statistically significant for aggregate social spending and not significant for disaggregated measures. Kaufman and Segura-Ubiergo (2001) argue that this inconclusiveness is due to the fact that foreign investors do not react by shifting investments based on changes in the resources allocated to government social programs. The lack of significance of capital account openness on social spending is perplexing particularly because part of the theories that posit that globalization introduces pressures on governments rests on the volatility of economies to international capital flows (Rodrik 1998).

From a theoretical viewpoint, several arguments have been put forward as to why increased openness to capital may induce greater pressures on governments in developing countries. Under more liberalized regimes, governments will find it more difficult to excise taxes on capital due to its increased ease of entry and exit. With fully functioning and liberalized capital markets, the extent and magnitude of government's to effectively employ fiscal policy can be derived from the standard Mundell-Fleming macroeconomic framework.¹¹⁸ This framework demonstrates that the more open the economy in countries who adopt fixed exchange rate, the less flexibility countries have to maneuver monetary policy and the greater reliance on fiscal policy. The opposite occurs in the case of countries that choose to float their currencies as they lose the ability to utilize fiscal policy as an independent instrument with fully functioning capital markets.

Empirical work in industrialized democracies has tended to confirm the Mundell-Fleming model showing that fiscal (monetary) policy will be employed to a greater extent to manipulate pre-electoral outcomes when countries operate under a fixed (floating) exchange rate regime and capital is mobile. This pattern is confirmed by Oatley (1999) and Clark and Hallerberg (2000) for a sample of OECD countries. However, the evidence in developing countries, suggests that patterns may be different. Block, Ferree and Singh (2003) test whether incumbent governments that belong to the *Communauté Financière d'Afrique* (CFA)

¹¹⁸ Under openness, the Mundell-Fleming model shows that governments can not adopt the following three policies jointly: capital mobility, fixed exchange rates, and independent monetary policy (Krugman and Obstfeld 2000).

zone in sub-Saharan Africa utilize fiscal policy more actively during elections. Contrary to theory, they find that there is greater existence of election-year increases in government consumption for non-CFA zone countries. In Latin America, no studies have been carried out to date to examine these patterns on whether there are differences in fiscal and monetary policy by exchange regime type and none have tested the implications of exchange rate regime type on social spending.¹¹⁹

Important changes have taken place in Latin America with respect to monetary and exchange rate policies that suggest that the constraints on the abilities of governments to manipulate government spending to win at the polls have increased over time particularly after the 1970s. In a recent study, Frieden, Ghezzi and Stein (2000) undertake a classification of exchange rate regimes in Latin America between 1960 and 1994. They present empirical evidence suggesting that the majority of countries in the region have moved towards more flexible arrangements particularly in the 1990s in contrast to the early 1970s. For example, in 1973 (the year marking the collapse of the Bretton Woods system) 90 percent of Latin American countries had fixed exchange rate regimes. During 1989-1994, the average number of countries operating under such an arrangement had declined to thirty eight percent. On the other hand, some Latin American countries have either maintained fixed or adhered to binding types of exchange rates. Argentina adopted a near-decade long currency board arrangement that collapsed in 2001 and some countries, such as Ecuador and Panama, operate under the U.S. dollar.

There have also been fundamental changes in the role of central banks particularly as countries have moved towards floating exchange regimes that permit greater degrees of monetary policy effectiveness for stimulating the economy. Earlier, we had reviewed Drazen's (2000) model that draws attention to fiscal policy as the main driver of electoral changes in economic indicators. One of the key insights of this framework is that effective manipulation of government stimulus policies is significantly harder due to the separation in the actors that control fiscal policy (elected politicians in the executive and legislative branches) and monetary policy (independent central bank authorities). He argues that

¹¹⁹ Part of the reason for the lack of significance of capital mobility in past research may be due to the fact that studies have failed to include controls for fixed exchange regimes. The testing of this hypothesis will be a task for future research as data coding fixed and flexible exchange rate regimes for Latin America was not obtained. It should be noted that a series of studies have uncovered electorally driven patterns with respect to exchange rate policies (Frieden et al. 2000; Frieden and Stein 2001; Blomberg et al. 2005).

independent central banks generally choose to pursue accommodating policies in response to fiscal expansions to avoid sharp changes in interest rates. Thus, the response of the central bank depends on its degree of independence and whether it is concerned in stabilizing increases in government spending (or tax cuts).

In the United States, Drazen notes that the Federal Reserve generally seeks to “lie low” during elections and thus empirical tests confirm a pattern in which monetary growth follows an electoral cycle. Since the early 1980s, central bank authorities have gained independence in several Latin American countries. It is possible that these institutions have become more focused on economic stabilization and as a result more responsive to quelling fiscal spending increases so as to not increase inflationary pressures. On the other hand, central bank independence, which is closely correlated with exchange regime type, is fairly recent and has mostly occurred in the 1990s. It is likely that central banks were more apt to enhance rather than quell fiscal policies in earlier periods and in countries where these institutions lack independence.

In sum, there is an important debate underway on whether increased trade and financial integration with global markets has forced Latin American governments to reduce social spending. It is clear that the response of Latin American welfare states contrast markedly with the experiences found in North America and Europe democracies, but the exact pattern of governmental responses remains in dispute. While the majority of empiric studies produced to date suggests that higher degrees of economic opening have been associated with contractions in the social safety net, there are some studies that provide strong evidence to contest these conclusions showing that there may also be compensating efforts by Latin America regimes to globalization. However, the body of evidence is scant and based on few specifications.

6.3 Measures of Globalization: Trade Openness and Capital Mobility

The empirical models that will be employed in Section 5 seek to test how the welfare state response in Latin America is impacted by differing levels of trade integration and capital mobility. As has been underscored in the review of the literature in Section 2, the findings on the response of Latin American welfare states to globalization are unstable. Depending on the different measures of trade openness ($(\text{Imports} + \text{Exports})/\text{GDP}$) used, studies have shown

that there are differences in the direction and magnitude of the response. With respect to capital mobility, the coefficient is not statistically significant either when measured based on an index measure of capital account openness or in terms of overall flows in the majority of past research. In order to test the robustness of the findings reported in this chapter, different measures of both trade openness and capital mobility were tested to examine if there are differences in the predicted relationship depending on the definition employed. This section briefly explains the different measures of trade openness and capital mobility that will be subsequently tested.

6.3.1 Trade Openness

The issue of how to appropriately measure openness to international trade is a matter that has incited significant discussion. In order to test the robustness of the relationship between globalization and social spending, four different measures of an economy's integration with world markets are tested in this chapter. The first measure is based on trade calculated as $\text{imports} + \text{exports}/\text{GDP}$, where the denominator is calculated by converting domestic local currency to current US\$ based on official exchange rate conversions. This measure is the most common measure employed in past research. It has been criticized as a poor measure due to the variation introduced by the size of the economy and by changes in the exchange rate. Given that it may be that export-intensive versus import-intensive economies face distinct pressures and therefore respond differently in terms of social spending, the study also examines the effects of trade openness when exports and imports are tested as separate effects in the regression model.

Following Avelino, Brown, and Hunter (2005) the results of the same estimations were also verified to see if there were differences when trade is measured as a percentage of GDP based on purchasing power parity. Two alternative trade openness variables are tested. The first measure, TRADESHPPP, follows Avelino, Brown, and Hunter (2005) and is calculated as the percentage of GDP based on purchasing power parity as reported in the World Bank's World Development Indicators. The second variable, OPENK, is drawn from the Penn World Tables and represent total trade as a percentage of GDP in constant prices (Heston et al. 2006). The correlation between TRADESHPPP and the exchange-rate measure

of trade openness (Exports+Imports/GDP in US\$) is 0.595. The data on GDP in PPP is only available from 1975 onwards thus the estimates are based on slightly less observations. The correlation between OPENK and the exchange-rate measure of trade openness (Exports+Imports/GDP in US\$) is 0.700.

6.3.2 Capital Mobility

In the empirical studies, controls have been introduced for trade openness based on several different indexes of capital account openness. These include indices developed by Quinn (1997), Morely et al. (1999) and Garret et al. (2001). In order to measure the effect of openness to international capital, the overall level of openness to financial flows from abroad in terms of regulations by using an index of capital mobility based on the decision rules outlined by Quinn (1997) is tested. Quinn measures the intensity of capital controls through a more detailed reading of the IMF's *Annual Report on Exchange Arrangements and Exchange Restriction*. The degree of restrictions on the capital account is scored from 0 to 4 on an annual basis with increases measuring reduced constraints on financial flows. The data are drawn from Avelino, Brown and Hunter (2005).¹²⁰ The correlation between trade openness and capital mobility is surprisingly low for the sample (0.332). Quinn also develops a measure of financial openness to capture the degree of control exerted on both the capital and current account. The degree of financial openness on an annual basis is scored from 0 to 14 with increases measuring reduced degrees of restrictions.

Given concerns raised that the exposure to risk based on the coding of capital controls may not be strongly correlated with actual capital flows, gross capital flows (private capital inflows plus outflows as a percentage of GDP) are also tested. Following Haggard and Rudra (2005) total flows is used to capture the total exposure to risk from international capital movements, rather than measures that would capture the direction of flows. It should be noted that this measure has also been criticized given that private capital flows often respond to macroeconomic volatility (as opposed to national policies with respect to foreign direct investment). The correlation between the index score of capital mobility and gross capital flows is 0.287. The correlation between the index of financial openness and gross capital flows is 0.296.

¹²⁰ See Avelino, Brown and Hunter (2005) for a more detailed explanation of this variable.

6.4 Estimation Procedure and Model Specification

Earlier, we had introduced the baseline model. Adding specificity for the two variables that will be tested in this chapter, trade openness and capital mobility, the model can be summarized as:

$$Y_{i,t} = \beta_1 Y_{i,t-1} + \beta_2 \mathbf{Z}_{i,t} + \beta_3 \text{Trade Openness}_{i,t} + \beta_4 \text{Capital Mobility}_{i,t} + c_i + \mu_{i,t}. \quad (1)$$

The dependent variable is the share of social spending as a share of total government spending, $Y_{i,t-1}$ is a one-period lag of the dependent variable, \mathbf{Z}^{121} is a vector of control variables and α_t represent year dummies. 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 openness of a given country. The error term, μ_{it} , is an error term associated with unit i at time t . In addition to modeling how trade openness and capital mobility impact aggregate social spending, the disaggregated measures of education, health and social security budget shares as dependent variables will also be tested in order to examine whether the impact of openness on welfare spending is of similar magnitude for different types of social spending expenditures.

Thus, the model seeks to explore how changes in the level of openness impact welfare state dynamics in Latin America. As has been discussed in detail in Chapter 3, hypothesis testing is particularly challenging if the data follow a unit root. Given the uncertainty regarding the stationary character of the data, the *level* of welfare state development will be tested using three models: (1) pooled OLS with panel corrected standard errors;¹²² (2) the Arellano and Bond (1991) first-differenced GMM estimator (GMM-Diff); and, (3) the Blundell and Bond (1998) system GMM estimator (GMM-System).¹²³ To examine whether results are altered when estimation is carried in models more appropriate for highly persistent series, the results of a fourth model that is an error correction model (ECM) with panel

¹²¹ There is one control variable, GDP growth, that is lagged one year. To simplify notation, it is not added as a separate variable in the equation.

¹²² The model will be estimated with the Stata XTPCSE command.

¹²³ The exercise and commands for GMM estimation are based on Roodman (2006) and were carried out using in Stata 10.

corrected standard errors based on the *first difference* of the dependent variable are also reported in Appendix 6A. Estimation was carried out with and without fixed effects. In order to address issues of unit heterogeneity, the pooled OLS, GMM and ECM estimates will also be estimated with and without controls for time and unit heterogeneity. As the results with and without fixed effects are reported in the tables presented below there will be a total of seven columns.

6.5 Results of Time-Series-Cross-Section Analysis: The Effects of Globalization

This section presents the results of hypothesis testing across a battery of different specifications and using different measures of trade openness and capital mobility. The battery of tests applied to examine the effects of the dramatic increase in trade and financial openness on social spending present robust evidence confirming that globalization is associated with reduced levels of social spending in aggregate terms. The evidence of a retrenchment in social spending is confirmed for both aggregate and social security budget shares, thus supporting the argument that there has been an “efficiency” response in Latin America that has included the adoption of fiscal policies to reduce labor costs (e.g. lower social security transfers) in order to retain country competitiveness. At the same time, the results also support the hypothesis that governments seek to compensate certain groups in an era of globalization. After control for the level of trade openness, higher rates of capital mobility result in greater budget allocations for pensions. However, the results suggest that trade and capital flows do not impact health and education in the same fashion as social security. There is some, albeit limited, evidence to indicate that higher capital mobility leads to reductions in the share of the budget directed towards health and education. Thus, we do not find that globalization has provoked governments to increase human capital investments (e.g. increase health and education budget allocations). The pattern is confirmed across different specifications, but is less robust when tested with different measures of trade openness and capital mobility.

6.5.1 Results for Trade Openness

The results in Table 6.2, 6.3 and 6.4, which are replications of Tables 4.2, 4.3 and 4.4 presented in Chapter 4, confirm that higher degrees of economic opening to foreign trade are associated with contractions in the social safety net in Latin America. Although the coefficient on trade openness measured as the share of exports and imports relative to GDP based on exchange rates is negative in all seven specifications in Table 6.2, it is only statistically significant at the 10% or lower level in two cases. The two cases are the model for pooled OLS with no controls for unit or time invariant effects and the pooled OLS case where controls are adopted for country and year fixed effects. There are two possible interpretations to this result. A first interpretation is that globalization has limited effects on the retrenchments in social spending in Latin America. In fact, Tables 6.3 and 6.4 suggest that an alternative explanation may be at work. With respect to one aspect of globalization (trade openness) globalization has induced an “efficiency” response in Latin America that has included the adoption of fiscal policies to reduce labor costs (e.g. lower social security expenditures) in order to retain country competitiveness.¹²⁴ There is strong evidence to confirm this interpretation. Table 6.3 provides strongly robust evidence that trade openness measured as the share of imports and exports relative to the size of the economy diminishes social security spending.

¹²⁴ An alternative explanation is that both “efficiency” and “compensatory” responses may be counterbalancing each other.

Table 6.2. The Effect of Globalization on Social Spending in Latin America, 1973-2000

Dependent Variable: Share of Welfare Spending as a Share of Total Government Spending

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One-Step First Differences ^a	(5) GMM One- Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.769*** (0.035)	0.471*** (0.047)	0.470*** (0.049)	0.687*** (0.169)	0.555** (0.206)	0.720*** (0.168)	0.720*** (0.180)
% of population aged 65 and over _t	0.232 (0.217)	3.041*** (0.669)	0.117 (0.875)	1.826 (1.852)	0.283 (1.389)	-0.055 (0.692)	0.522 (0.718)
% of population aged 15 to 64 _t	0.721*** (0.156)	0.151 (0.191)	0.035 (0.192)	0.036 (0.181)	0.117 (0.263)	0.782 (0.662)	0.509 (0.557)
%urban population _t	-0.067** (0.029)	0.350*** (0.116)	-0.099 (0.212)	0.257** (0.089)	-0.290 (0.257)	-0.208 (0.350)	-0.032 (0.234)
GDP per capita _t (constant 2000 US\$) (WDI)	0.000 (0.000)	-0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	-0.003 (0.002)	0.002 (0.004)	0.000 (0.003)
GDP per capita growth _{t-1} (annual %) (WDI)	0.025 (0.049)	-0.035 (0.051)	-0.017 (0.066)	-0.196 (0.153)	-0.222 (0.162)	-0.011 (0.096)	0.088 (0.112)
Trade Openness _t	-0.030* (0.018)	-0.027 (0.025)	-0.076*** (0.029)	-0.026 (0.036)	-0.058 (0.048)	-0.031 (0.058)	-0.033 (0.051)
Capital Mobility Index _t	0.748** (0.353)	0.591 (0.429)	0.496 (0.471)	0.253 (0.798)	0.427 (0.628)	0.640 (0.891)	0.521 (0.795)
Democracy _t	-0.030 (0.471)	-0.139 (0.680)	0.134 (0.794)	-0.172 (0.898)	0.783 (1.410)	0.715 (1.242)	-0.241 (1.069)
Constant	-27.678*** (6.234)	-25.796*** (4.951)	42.665* (21.778)			-25.466 (19.883)	-13.142 (19.639)
Observations	351	351	351	331	331	351	351
R-squared	0.80	0.84	0.85				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (<i>p value</i>) ^b				0.466	0.594	0.556	0.476
Number of Instruments				12	38	16	42
Hansen test for joint validity of instruments (<i>p value</i>)				0.216	1.000	0.980	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order correlation are reported for all models. For GMM estimations, 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 6.3. The Effect of Globalization on Social Security Spending in Latin America, 1973-2000*Dependent Variable: Share of Social Security Expenditures as a Share of Total Government Spending*

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One-Step First Differences ^a	(5) GMM One- Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.789*** (0.035)	0.510*** (0.048)	0.528*** (0.048)	0.711*** (0.189)	0.664** (0.284)	0.844*** (0.147)	0.750*** (0.159)
% of population aged 65 and over _t	0.628*** (0.218)	2.761*** (0.684)	1.722* (0.891)	1.510 (1.034)	1.277 (1.546)	0.321 (0.521)	0.787 (0.668)
% of population aged 15 to 64 _t	0.491*** (0.168)	-0.296** (0.149)	-0.295** (0.149)	-0.215 (0.182)	-0.195 (0.232)	0.326 (0.278)	0.318 (0.321)
%urban population _t	0.006 (0.045)	0.477*** (0.129)	0.408** (0.189)	0.348** (0.144)	0.296 (0.233)	-0.033 (0.106)	0.013 (0.102)
GDP per capita _t (constant 2000 US\$) (WDI)	-0.000 (0.000)	-0.001 (0.000)	-0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)	0.000 (0.001)
GDP per capita growth _{t-1} (annual %) (WDI)	0.092** (0.047)	0.043 (0.049)	0.053 (0.056)	-0.013 (0.079)	-0.044 (0.133)	0.051 (0.050)	0.093 (0.080)
Trade Openness _t	-0.082*** (0.023)	-0.085*** (0.028)	-0.111*** (0.032)	-0.071** (0.025)	-0.092** (0.036)	-0.061 (0.044)	-0.089 (0.053)
Capital Mobility Index _t	0.626** (0.257)	1.257*** (0.400)	1.424*** (0.451)	0.751 (0.580)	1.058 (0.979)	0.480 (0.503)	0.563 (0.564)
Democracy _t	-1.181** (0.462)	-2.148*** (0.748)	-1.925** (0.874)	-1.699*** (0.343)	-1.443** (0.494)	-0.661 (0.708)	-1.534* (0.846)
Constant	-23.334*** (6.954)	-20.893*** (7.638)	-9.660 (17.517)			-14.563 (11.838)	-13.409 (13.735)
Observations	351	351	351	331	331	351	351
R-squared	0.91	0.92	0.93				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (<i>p value</i>) ^b				0.511	0.668	0.509	0.574
Number of Instruments				12	38	16	42
Hansen test for joint validity of instruments (<i>p value</i>)				0.198	1.000	0.766	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order serial correlation are reported for all models. For GMM estimations, 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 6.4. The Effect of Globalization on Health and Education Spending in Latin America, 1973-2000

Dependent Variable: Share of Health and Education Expenditures as a Share of Total Government Spending

	(1) Pooled OLS	(2) Pooled OLS with country fixed effects (f.e.)	(3) Pooled OLS with country and year f.e.	(4) GMM One-Step First Differences ^a	(5) GMM One- Step First Diff. with year f.e. ^a	(6) GMM One-Step System ^a	(7) GMM One-Step System with year f.e. ^a
Lagged Dependent Variable _{t-1}	0.806*** (0.051)	0.547*** (0.085)	0.536*** (0.090)	0.110 (0.653)	0.029 (0.272)	0.267 (0.275)	0.466* (0.264)
% of population aged 65 and over _t	-0.388*** (0.117)	0.169 (0.483)	-1.683* (0.859)	-6.983 (12.494)	-3.002* (1.485)	-1.086 (1.365)	-0.643 (0.988)
% of population aged 15 to 64 _t	0.161 (0.130)	0.405** (0.169)	0.310* (0.163)	0.190 (1.282)	0.851** (0.304)	0.610 (0.787)	0.387 (0.576)
%urban population _t	-0.060 (0.047)	-0.130 (0.137)	-0.504** (0.244)	0.110 (0.665)	-1.099* (0.566)	-0.203 (0.509)	-0.074 (0.326)
GDP per capita _t (constant 2000 US\$) (WDI)	0.000 (0.000)	0.000 (0.000)	-0.001 (0.001)	0.015 (0.025)	-0.003 (0.004)	0.000 (0.005)	-0.001 (0.003)
GDP per capita growth _{t-1} (annual %) (WDI)	-0.062 (0.042)	-0.066 (0.043)	-0.055 (0.051)	-0.329 (0.271)	-0.432** (0.181)	-0.091 (0.144)	-0.040 (0.062)
Trade Openness _t	0.051*** (0.016)	0.059** (0.026)	0.040 (0.030)	-0.130 (0.351)	0.031 (0.068)	0.130 (0.075)	0.100 (0.078)
Capital Mobility Index _t	0.048 (0.258)	-0.744** (0.376)	-1.017** (0.426)	-0.448 (1.536)	-1.173 (1.128)	0.197 (0.826)	0.160 (0.731)
Democracy _t	1.127*** (0.359)	1.914** (0.809)	1.994** (0.941)	1.062 (3.291)	3.436** (1.336)	2.754 (2.197)	2.330 (2.211)
Constant	-2.180 (5.414)	-3.446 (5.563)	50.862** (25.058)			-5.615 (18.871)	-2.880 (18.589)
Observations	351	351	351	331	331	351	351
R-squared	0.86	0.89	0.90				
Average Time Series Length	23.4	23.4	23.4	22.1	22.1	23.4	23.4
Arellano-Bond test for AR(2) (<i>p value</i>) ^b				0.383	0.215	0.260	0.242
Number of instruments				12	38	16	42
Hansen test for joint validity of instruments (<i>p value</i>)				0.923	1.000	0.899	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)						0.642	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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order serial correlation are reported for all models. For GMM estimations, 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 6.4 suggests there is only weak evidence that higher rates of trade openness are associated with higher budget shares for health and education. The estimated coefficient on trade openness measured as the share of exports and imports relative to GDP based on exchange rates is only positive and statistically significant in the case of the pooled OLS regression and the pooled OLS regression controlling for time invariant effects. This evidence could be interpreted as suggestive that globalization also provides incentives for Latin American governments to undertake human capital investments (e.g. increase health and education expenditures), but these incentives appear to be more limited.

Some comments on the differences across specifications are also in order. First, the results are generally consistent whether measured by pooled OLS, pooled OLS with country and time fixed effects, as well as in GMM First-Differenced and GMM-System models. The results presented in Tables 6.2, 6.3, and 6.4 are also confirmed in estimations tests with an Error Correction Model and are presented in Appendix 6A. As cited previously, Bond notes that the pooled OLS and fixed effects estimators provide boundaries for checking the results of GMM estimates. In the case of social spending and pension budget shares, the GMM specifications yield estimates that fall within the predicted range. There calibration of GMM estimates in the case of health and education spending, however, do not seem to be as precise.

Given the hypothesized effect of trade openness on social spending, we would expect that productivity enhancing social expenditures should increase in those countries where exports comprise a greater share of the size of the economy. In contrast, increases in import expenditures are expected to have a negative effect on social spending as it is argued that it is this mechanism that most directly transmits the impact of globalization on local wages. Therefore, the model is tested to see whether the effects of trade are distinct when exports and imports are included separately. These predictions are not confirmed. As detailed in Table 6.5, however, neither greater import nor export levels seem to reduce the share of the budget allocated to social security or health and education.

Table 6.5. Testing the Effect of Exports and Imports on Disaggregated Social Spending

	<i>Social Security Budget Share</i>				<i>Health and Education Budget Share</i>			
	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a
Exports _t (%GDP)	0.008 (0.051)	-0.106 (0.082)	-0.081 (0.061)	-0.010 (0.090)	0.057 (0.079)	0.071 (0.120)	0.091 (0.157)	0.219 (0.138)
Imports _t (%GDP)	-0.075 (0.066)	0.005 (0.083)	-0.023 (0.062)	-0.074 (0.111)	-0.006 (0.088)	-0.030 (0.131)	-0.076 (0.139)	-0.112 (0.128)
Capital Mobility Index _t	0.497 (0.310)	1.105** (0.500)	0.600 (0.749)	0.492 (0.592)	0.019 (0.347)	-0.825 (0.609)	-0.958 (1.017)	-0.160 (0.528)
Democracy _t	-1.261** (0.603)	-1.802 (1.159)	-1.378*** (0.400)	-1.846* (0.918)	1.383*** (0.495)	2.040 (1.264)	3.400** (1.399)	2.745 (2.249)
Observations	345	345	325	345	345	345	325	345
Average Time Series Length	23	23	21.7	23	23	23	21.7	23
R-squared	0.91	0.93			0.86	0.90		
Number of instruments			39	43			39	43
Arellano-Bond test for AR(2) (<i>p value</i>) ^b			0.595	0.657			0.240	0.292
Hansen test for joint validity of instruments (<i>p value</i>)			1.000	1.000			1.000	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)				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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order serial correlation is reported for GMM estimations. For GMM estimations, the tests were carried out on the first-differenced residuals. The p-values are the probability of rejecting the null hypothesis of no autocorrelation.

Following Avelino, Brown and Hunter (2005) the results of the same estimations were also verified to see if there were differences when trade is measured as a percentage of GDP based on purchasing power parity. As reviewed earlier, Avelino, Brown, and Hunter (2005) had find that there is a difference in the behavior of social spending with respect to changes in trade openness depending on whether an exchange rate or PPP measure is used. Tables 6.6 and 6.7 present only the coefficients for the alternative trade openness measure (Tradeshppp and OpenK), capital mobility and democracy. The same control variables were employed as in the specifications presented earlier in Tables 6.2, 6.3, and 6.4. We do not find evidence in support of the argument that PPP measures change the sign and significance levels of the trade openness effect on social spending. The sign of the coefficients estimated using exchange rate measures of GDP is confirmed, but the robustness of the relationships reported in the earlier tables is confirmed in fewer cases. Trade openness measured as the share of imports and exports relative to the size of the economy in PPP terms diminishes social security spending, but the coefficient estimates are only significant in the pooled OLS regressions.¹²⁵ With respect to health and education expenditures, trade openness increases the share of the budget allocated to these items in the GMM System estimates. To further test these results, a second measure, OPENK, that is drawn from the Penn World Tables and represent total trade as a percentage of GDP in constant prices is used (Heston et al. 2006).¹²⁶ These estimates also confirm the sign and significance results obtained when the trade measure relative to GDP in PPP terms is used.

¹²⁵ The correlation between TRADESHPPP and the exchange-rate measure of trade openness (Exports+Imports/GDP in US\$) is 0.595. The data on GDP in PPP is only available from 1975 onwards thus the estimates are based on slightly less observations.

¹²⁶ The correlation between OPENK and the exchange-rate measure of trade openness (Exports+Imports/GDP in US\$) is 0.700.

Table 6.6. Testing the Effects of PPP-based Trade Openness on Social Spending (Tradeshppp)

Dependent Variable:	Aggregate Social Spending Budget Share				Social Security Budget Share				Health and Education Budget Share			
	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a
Tradeshppp _t	0.011 (0.016)	0.019 (0.062)	0.009 (0.050)	0.008 (0.049)	-0.021*** (0.008)	0.036 (0.044)	0.039 (0.051)	-0.015 (0.020)	0.033 (0.023)	-0.018 (0.057)	-0.030 (0.055)	0.095* (0.053)
Capital Mobility Index _t	0.361 (0.426)	0.202 (0.486)	-0.147 (0.511)	-0.099 (0.613)	0.241 (0.313)	1.021* (0.527)	0.828 (1.205)	0.068 (0.411)	-0.021 (0.362)	-0.896 (0.635)	-0.969 (0.853)	-0.319 (0.549)
Democracy _t	0.157 (0.488)	0.526 (1.159)	0.207 (1.097)	-0.140 (0.891)	-1.127* (0.609)	-1.817 (1.359)	-1.512* (0.718)	-1.255** (0.537)	1.401*** (0.500)	2.257 (1.406)	3.095** (1.065)	2.542 (2.270)
Observations	332	332	312	332	332	332	312	332	332	332	312	332
Avg. Yrs.	22.13	22.13	20.8	22.13	22.13	22.13	20.8	22.13	22.13	22.13	20.8	22.13
R-squared	0.80	0.93			0.90	0.93			0.86	0.90		
Instruments #			37	41			37	41			37	41
Arellano-Bond test for AR(2) (<i>p value</i>) ^b			0.414	0.207			0.915	0.601			0.298	0.276
Hansen test for joint validity of instruments (<i>p value</i>)			1.000	1.000			1.000	1.000			1.000	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)				1.000				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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order serial correlation is reported for GMM estimations. For GMM estimations, 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 6.7. Testing the Effects of PPP-based Trade Openness on Social Spending (OPENK)

Dependent Variable:	Aggregate Social Spending Budget Share				Social Security Budget Share				Health and Education Budget Share			
	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a
OpenK _t	0.005 (0.008)	0.008 (0.028)	0.026 (0.052)	0.016 (0.025)	-0.013** (0.005)	-0.001 (0.021)	0.001 (0.019)	-0.013 (0.017)	0.018 (0.012)	0.007 (0.023)	0.039 (0.057)	0.055** (0.025)
Capital Mobility Index _t	0.472 (0.296)	0.143 (0.451)	0.106 (0.703)	0.155 (0.591)	0.300 (0.232)	0.922** (0.447)	0.465 (0.817)	0.201 (0.512)	0.038 (0.236)	-0.861** (0.427)	-1.135 (1.095)	-0.100 (0.496)
Democracy _t	0.037 (0.463)	0.376 (0.828)	1.012 (1.710)	-0.103 (1.230)	-1.154*** (0.433)	-1.626* (0.881)	-1.278** (0.495)	-1.512* (0.831)	1.294*** (0.352)	1.934** (0.903)	3.602** (1.358)	2.637 (2.316)
Observations	351	351	331	351	351	351	331	351	351	351	331	351
Avg. Yrs.	23.4	23.4	22.07	23.4	23.4	23.4	22.07	23.4	23.4	23.4	22.07	23.4
R-squared	0.81	0.85			0.91	0.93			0.86	0.89		
Instruments #			38	42			38	42			38	42
Arellano-Bond test for AR(2) (<i>p</i> value) ^b			0.629	0.458			0.691	0.638			0.242	0.282
Hansen test for joint validity of instruments (<i>p</i> value)			1.000	1.000			1.000	1.000			1.000	1.000
Diff. Sargan tests for all system instruments (<i>p</i> value)				1.000				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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order serial correlation is reported for GMM estimations. For GMM estimations, the tests were carried out on the first-differenced residuals. The p-values are the probability of rejecting the null hypothesis of no autocorrelation.

6.5.2 Results for Capital Mobility

After controlling for the level of trade openness, there is some evidence to suggest that greater degrees of capital mobility increase pension allocations and reduce health and education expenditures in central government budgets. In Table 6.3 the coefficient measuring the degree of capital mobility is positive suggesting that the liberalization of the capital account is associated with increased social security expenditures. This pattern buttresses the hypothesis that compensatory strategies are being undertaken by countries that are more open to foreign capital flows. This finding is noteworthy as it challenges scholarship that has emphasized that the threat of capital flight is the most critical in imposing constraints on government policy choices to increase efficiency in developing countries.

The results suggest a slightly different story. After controlling for trade openness, those countries with additional openness due to the ease of entry and exit of foreign capital are more likely to compensate workers with social security transfers. This interpretation, however, should be viewed with caution as the statistical significance of the coefficient is not robust across all specifications. The results are only statistically significant in three of the seven specifications reported in Table 6.3. Moreover, estimations based on an ECM also yield statistically significant results showing that liberalization of the capital account increases social security pensions as reported in Appendix 6A.

The opposite pattern is observed with respect to human capital expenditures. After controlling for trade openness, greater reductions in the restrictions on the flow of capital reduces the share of the budget allocated to schooling and health in the case of the pooled OLS estimates controlling for time and country invariant fixed effects in Table 6.4. Further testing with the ECM model indicates that the negative sign on the coefficient for capital openness is driven by a short-term effect that is reversed in the long-run as the lagged level value of the capital account coefficient is positive (see Appendix 6A).

Table 6.8 tests whether there are any differences if the expanded financial openness measure of capital and current account liberalization is used to evaluate the impact of capital flows on social spending. The coefficient on financial openness is negative and statistically significant in three of the four specifications in the case of aggregate social spending and pensions. Latin American countries that liberalized the flow of capital allocated greater shares

of budgets to social spending and these effects are due to increases in pension payments as opposed to funding for schooling and healthcare that are once again predicted to decrease with greater degrees of flexibility in the movement of capital though less robustly. Appendix 6A presents the results of the same model estimated with an error correction model in which the findings reported in Table 6.8 are confirmed.

Concerns have also been raised that the exposure to risk based on the coding of capital controls is an inadequate measure as it may or may not correlate highly with actual capital flows.¹²⁷ An alternative measure of capital mobility based on measured as gross capital flows (private capital inflows plus outflows as a percentage of GDP) was also tested. Following Haggard and Rudra (2005) total flows are used to capture the total exposure to risk from international capital movements, rather than measures that would capture the direction of flows. In contrast with the degree of regulation of the capital account, the results reported in Table 6.9 provide some evidence to suggest that Latin American countries with higher rates of foreign capital inflows and outflows are more likely to reduce the total share of the budget directed at social spending. This finding is noteworthy as it is the first time in any specification that both the coefficient on trade and capital openness are both negative in all specifications tested in this chapter.

¹²⁷ The correlation between capital mobility and gross capital flows is 0.287. The correlation between financial openness of both capital and current accounts and gross capital flows is 0.296.

Table 6.8. Testing the Effect of Financial Openness, 1973-2000

Dependent Variable:	Aggregate Social Spending Budget Share				Social Security Budget Share				Health and Education Budget Share			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Pooled OLS PCSE	Pooled OLS with country and year f.e.	GMM First Differences with year f.e. ^a	GMM System with year f.e. ^a	Pooled OLS PCSE	Pooled OLS with country and year f.e.	GMM First Differences with year f.e. ^a	GMM System with year f.e. ^a	Pooled OLS PCSE	Pooled OLS with country and year f.e.	GMM First Differences with year f.e. ^a	GMM System with year f.e. ^a
Trade Openness _t	-0.034* (0.018)	-0.086*** (0.026)	-0.072 (0.046)	-0.031 (0.047)	-0.081*** (0.024)	-0.116*** (0.031)	-0.092** (0.036)	-0.082 (0.048)	0.048*** (0.016)	0.036 (0.029)	0.014 (0.058)	0.089 (0.078)
Financial Openness _t	0.313** (0.144)	0.495** (0.210)	0.520** (0.229)	0.181 (0.305)	0.213** (0.098)	0.792*** (0.164)	0.519* (0.290)	0.148 (0.206)	0.071 (0.110)	-0.342* (0.192)	-0.222 (0.378)	0.301 (0.343)
Democracy _t	-0.078 (0.468)	0.306 (0.781)	0.811 (1.281)	-0.275 (0.914)	-1.175** (0.464)	-1.755** (0.856)	-1.303*** (0.328)	-1.519* (0.856)	1.106*** (0.364)	1.962** (0.938)	3.363** (1.458)	2.450 (2.115)
Observations	351	351	331	351	351	351	331	351	351	351	331	351
Avg. # Years	23.4	23.4	22.1	23.4	23.4	23.4	22.1	23.4	23.4	23.4	22.1	23.4
R-squared	0.80	0.85			0.91	0.93			0.86	0.89		
# Instruments			38	43			38	42			38	42
Arellano-Bond test for AR(2) (<i>p value</i>) ^b			0.557	0.411			0.557	0.567			0.254	0.247
Hansen test for joint validity of instruments (<i>p value</i>)			1.000	1.000			1.000	1.000			1.000	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)				1.000				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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order serial correlation is reported for GMM estimations. For GMM estimations, 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 6.9. Testing the Effects of Gross Capital Flows on Social Spending

Dependent Variable:	Aggregate Social Spending Budget Share				Social Security Budget Share				Health and Education Budget Share			
	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a	(1) Pooled OLS PCSE	(2) Pooled OLS with country and year f.e.	(3) GMM First Differences with year f.e. ^a	(4) GMM System with year f.e. ^a
Trade Openness _t	-0.034* (0.019)	-0.075*** (0.027)	-0.060 (0.042)	-0.045 (0.070)	-0.089*** (0.022)	-0.096*** (0.032)	-0.084*** (0.024)	-0.107* (0.053)	0.052** (0.020)	0.027 (0.034)	0.005 (0.049)	0.106 (0.067)
Gross Capital Flows _t	0.092 (0.082)	-0.289*** (0.103)	-0.215** (0.089)	-0.033 (0.198)	0.152** (0.064)	-0.094 (0.082)	-0.052 (0.124)	0.058 (0.117)	-0.069 (0.075)	-0.169 (0.112)	-0.192 (0.120)	-0.153 (0.090)
Democracy _t	-0.008 (0.498)	0.984 (0.919)	1.131 (1.414)	-0.041 (1.094)	-1.225** (0.486)	-1.503 (1.017)	-1.097** (0.424)	-1.604 (1.048)	1.166*** (0.379)	2.362** (1.152)	3.571*** (1.195)	2.265 (2.768)
Observations	348	348	328	348	348	348	328	348	348	348	328	348
Avg. # Years	23.2	23.2	21.9	23.2	23.2	23.2	21.9	23.2	23.2	23.2	21.9	23.2
R-squared	0.81	0.85			0.91	0.93			0.86	0.89		
# Instruments			38	42			38	42			38	42
Arellano-Bond test for AR(2) (<i>p value</i>) ^b			0.440	0.358			0.604	0.591			0.291	0.256
Hansen test for joint validity of instruments (<i>p value</i>)			1.000	1.000			1.000	1.000			1.000	1.000
Diff. Sargan tests for all system instruments (<i>p value</i>)				1.000				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 the ages 15 and 64, the fraction of the population living in urban areas, a dummy variable for democratic years. The dummy variable for authoritarian regimes is omitted and is the default category. 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%.

^a Two lags of the dependent variable were used in the GMM difference and systems equations. The final 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 (2007a). 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.

^b The Arellano-Bond tests for first-order and second-order serial correlation is reported for GMM estimations. For GMM estimations, the tests were carried out on the first-differenced residuals. The p-values are the probability of rejecting the null hypothesis of no autocorrelation.

6.5.3 Summary of Key Findings

Based on previous studies reviewed in this chapter, Table 6.10 presents a prediction of the expected sign of the impact on social spending of the two key variables: trade openness and capital mobility. The third column presents the number of studies in which the predicted hypothesis was confirmed out of the total of seven studies that have been published to attempt to gauge the level of consensus that prevails regarding the effect of these two variables on aggregate social spending, health and education expenditures and social security transfers. The seven studies refer to those reviewed and summarized in Table 6.1 of this chapter. The highest number of studies to confirm a predicted hypothesis is four and this is only attained with respect to one of the six outcomes. The final column summarizes whether the prevailing hypothesis in the literature was confirmed or not by at least three of the seven specifications in this chapter. Four of the six hypotheses in past studies were confirmed. Specifically, we confirm the finding that globalization as measured by trade openness is more closely associated with a retrenchment in social spending in Latin America. In two cases, however, results were not robust in the majority of past research. Past research has reported a positive correlation between social spending and capital mobility that was not confirmed. On the other hand, we do find robust evidence to show that the liberalization of the capital account is associated with increases in social and pension budget expenditures.

Table 6.10. Robustness of Results to Alternative Dynamic Specifications

Dependent Variable	Independent Variable	Predicted Sign	Number of Studies where variable was significant and predicted sign (Total=7)	Results of Pooled OLS with PCSE	Results of Pooled OLS with country and year f.e.	Results of GMM First Differences	Results of GMM System	Results of ECM with country and year f.e.	Predicted hypothesis confirmed in at least 3 specifications
<i>Social Spending</i>	Trade Openness	Negative	4	Negative	Negative	Not significant	Not significant	Negative	Negative
	Capital Mobility	Positive	3	Positive	Not significant	Not significant	Not significant	Not significant	Not significant
<i>Pensions</i>	Trade Openness	Negative	3	Negative	Negative	Negative	Not significant	Negative	Negative
	Capital Mobility	Not significant	3	Positive	Positive	Not significant	Not significant	Positive	Positive
<i>Health + Education</i>	Trade Openness	Not significant	3	Positive	Not significant	Not significant	Not significant	Not significant	Not significant
	Capital Mobility	Not significant	3	Not significant	Negative	Not significant	Not significant	Positive	Not significant

Notes: Estimated coefficient signs are only reported if statistical significance was 10% or lower. The reported coefficients in the case of the ECM model are for the lagged level of the variable ($x_{i,t-1}$).

Whereas prior studies on the impact of globalization on social spending in Latin America have mostly relied solely on one method (either OLS estimation or an error corrected model with panel corrected standard errors), this chapter has tested hypothesis employing a

battery of specifications for each question. Table 6.10 summarizes the results obtained from five different specifications employed in this chapter: pooled OLS with PCSE, pooled OLS with control for unit and time effects, GMM first difference and GMM system and error correction model estimates. There is generally strong correlation between the sign and significance levels across the specifications that were tested in this chapter. ECM estimates generally corroborate pooled OLS results. On the other hand, few coefficients are statistically significant in both pooled OLS and GMM estimations. These results suggest that the conclusions reached in this chapter are fairly robust as they are not highly contingent on the method used to obtain them.

6.6 Conclusion

In light of the waves of democratization and integration with commercial and financial global markets, a small but growing literature has emerged examining the responsiveness of the social policy agendas of Latin American governments to the demands of citizens. This chapter has sought to reexamine past findings that argue that globalization has induced an “efficiency” response in Latin America based on concerns about how the stationary property of the data and the measurement differences across studies may have influenced past research. The results support the view that globalization has generally had a negative influence on the Latin American welfare state based on the results of tests on the effects of trade openness.

However, the findings are not completely pessimistic as they point to the fact that the effects of globalization are much more complex. Higher rates of capital mobility also result in greater budget shares for social security spending after controlling for trade openness. This result is noteworthy as it challenges scholarship that has emphasized that the threat of capital flight is the most critical in imposing constraints on government policy choices to increase efficiency in developing countries. After controlling for trade openness, those countries with additional openness due to the ease of entry and exit of foreign capital are more likely to compensate workers with social security transfers.

7. Conclusion

This dissertation has focused on examining whether the wave of democratization that has spread across Latin America in recent decades has installed governments more attentive to citizen welfare in light of the dramatic liberalization of trade and financial markets. The results presented in this dissertation offer important insights on what explains the differences in the central patterns observed in the literature with respect to whether Latin American democracies reduce social spending in the context of globalization.

Beginning with Chapter 2, this study has shown that part of the reason for the divergence in past results on welfare dynamics in Latin America may be due to the difference in measurement. The majority of studies have employed measures of social spending relative to the size of the economy. This research emphasizes that the politics of resource allocation decisions are best understood by measures that consider social expenditures relative to the budget. By focusing specifically on the differences in budget allocations for social expenditures, this dissertation sheds greater insight on the forces that operate within a regime on how resources are distributed and avoiding misattribution that might be caused by alternative measures of welfare effort relative to GDP. Democracy, democratization and elections are the key independent variables of interest in this dissertation. By developing and testing more rigorous and theoretically grounded measures of these concepts, this dissertation has increased the precision of the variables being tested.

The results of the research also show that methods do matter. The framework developed in Chapter 3 highlights that there is significant complexity to research design and methodology. Not only do factors have multiple and contrasting effects on welfare state dynamics, but hypothesis testing is particularly challenging if the data follow a unit root. Given that test results suggests that data on social spending in Latin America can not be ruled out as being highly persistent, the dissertation subsequently adopted an empirical strategy based on a battery of alternative dynamic specifications to test the consistency of the estimates. There is generally strong correlation between the sign and significance levels across the specifications that were tested in each chapter. These results suggest that the conclusions reached in this dissertation are fairly robust as they are not highly contingent on the method used to obtain them. The reported findings hold up under a variety of alternative

specifications, which makes these results stronger than earlier studies on the impact of democratization and globalization on social spending in Latin America.

This dissertation has focused on examining how three key political factors influence the responsiveness of the welfare state in Latin America in the last three decades of the 20th century:

- 1) The democratic character of political institutions;
- 2) The electoral institutions that channel voter preferences to bring into power new democratic governments with a mandate; and,
- 3) The degree of integration of states into the global economy.

The first key factor is democracy. Chapter 4 provides a test for theories that argue that democracies are characterized by more equitable distributions of political power and as a result this regime is more apt to redistribute income from the wealthy to the poor as compared to authoritarian governments. The chapter offers evidence to show that public education and health spending rise in democratic governments in Latin America. Governments elected in competitive elections are also more apt to reduce regressive pension benefits favoring smaller groups of voters.

Chapter 4 contributes to a further refinement on how redistributive politics are shaped by the transition to democracy. The transfer of power from authoritarian regimes to popularly elected governments opens a window of opportunity that permits less-organized and lower-income groups to secure greater weight in the decision-making process that translates into real changes in the allocation of government budgets towards redistributive social programs. Consequently, Latin American democracies are more likely to increase expenditures directed at schooling and healthcare in the period prior to the first two turnovers of power to competing opposition parties. However, the chapter shows that the allocation of public expenditures begins to move away from health and education expenditures that benefit the poor and favor more entrenched power groups after the period of democratic transition ends and democracy is consolidated.

The second key factor is elections. Elections are the most basic and direct measure of the exercise of democracy. Given the findings in Chapter 4 that show a heightened welfare response by democratic regimes, Chapter 5 focuses on whether government responsiveness is

driven by efforts to manipulate electoral outcomes. The research presented in Chapter 5 of this dissertation shows that welfare commitments depend on the election cycle in Latin American democracies, but not as typically theorized. Instead, robust evidence is presented to show that democracies adhered to fiscal adjustments during and immediately after elections. The fiscal restraint commitment is persistent and marked. However, governments are more likely to undertake fiscal adjustments in the inaugural year when they enter office with greater election leads and when democratic governments have undergone at least two turnovers of power.

Chapter 5 also underscores that democracies protect the poor from painful budget cuts immediately after elections. It provides robust evidence to show that fiscal adjustments were combined with important increases in social spending in the inaugural year of a new presidential administration. Thus, social policy is used by Latin American democracies as an instrument to reward voters once a new administration has entered office and not as a tool to manipulate outcomes before elections as commonly argued in the literature. Presidents elected with large electoral margins and in consolidated democracies will increase allocations towards redistributive social programs. Putting these findings together with the results from Chapter 4, the dissertation concludes that there are important differences in redistributive spending between new and established democracies. However, social investments are not immediately implemented in the first year of a presidential mandate in a new democracy. Rather, the results in these chapters suggest that redistribution is cumulative.

The third factor is the extent of an economy's integration with world trade and financial markets. Chapter 6 examines and shows that the opening of Latin America to the global economy has placed countervailing pressures on governments. On the one hand, governments are driven to bring about greater efficiency in their economies and do so by implementing reforms to reduce labor costs. Indeed, social security budget shares in particular decrease in economies that are engaged in exporting and importing goods and services across their borders. On the other hand, governments also seek to strengthen safety net to shield citizens from the adverse shocks of market opening. In more open Latin American countries where foreign capital flows are less tightly constrained, democracies seek to compensate citizens by increasing pensions. These results are noteworthy as they challenge scholarship that has emphasized that the threat of capital flight is the most critical in imposing constraints on government policy choices to increase efficiency in developing countries. After controlling

for trade openness, those countries with additional openness due to the ease of entry and exit of foreign capital are more likely to reallocate government budgets towards compensatory social security programs.

Viewed together, the chapters underscore that democracy and globalization have complex impacts on the Latin American welfare state. Democratization has propelled governments to prioritize redistributive social programs and reduce allocations on regressive pension programs. Globalization puts countervailing pressures on governments to both reduce labor costs and compensate workers with greater pension support. The economic forces of redistribution and global market exposure are channeled through political coalitions and constraints to produce different welfare outcomes in Latin American democracies depending on the type of social spending in question. The direction of government responses differ radically depending on whether spending is directed at social security transfers or health and education. Social security is closely linked with the extent of an economy's integration with world commercial and capital markets, but the same forces do not seem to ignite greater investments in health and schooling in Latin America. In contrast, democratization is closely linked with human capital investments.

The study of the political determinants of welfare state dynamics in Latin America holds great promise for scholars of political economy most particularly in light of mounting and persistent inequalities. This dissertation has raised important methodological considerations and presented findings that question prevailing hypotheses in the literature. It also has outlined an agenda for future research on Latin American welfare state dynamics that builds on continuing to join and counter test the findings obtained from research developed by political scientists and economists in separate research tracks. This agenda includes a) further testing of models with alternative specifications advocated by these respective fields; b) more in-depth analysis of the dynamics operating on the separate responses of health and education to democratization and globalization; c) exploration of how retrenchment, privatization and decentralization may add further nuances to the observed patterns analyzed in this volume; and, 4) case study specific research on the country experiences of both new and consolidated democracies.

Appendix 2A. Analysis of Government Finance Statistics for Latin America: Consolidated Central Government Spending

Country	1st Year of Available Data	Last Year of Available Data	# of Years	Comments	Scale	Budgetary vs. Actual Expenditure Data	Differences with Kafuman & Segura dataset
Argentina	1972	2004	32	The data for 1972-1981 was missing in GFS Historical Database so the data had to be added into the database with printed data from the GFS 1983 Yearbook. Data from 1972-1981 reported in millions of pesos argentinos. This data was converted to newly released 1992 pesos. Data from 1982-1989 reported in millions of pesos (new pesos introduced after australes were removed as currency) confirmed in GFS 2000 yearbook. Data through 2001 are on a cash basis. From 2002 onwards, data are reported on an accrual basis. Manually had to enter sub-categories for economic affairs spending and other from 1990-2002.	Millions of Pesos.		Additional data for 1972, 1998-2005
Bolivia	1975-1981, 1983-1984	1986-2005	27	The data for 1975-1981 was missing in GFS Historical Database so the data had to be added into the database with printed data from the GFS 1987 Yearbook in thousands of Bolivianos. From 1996 onwards, the structure of expense changed. The central government transferred health and education functions to other levels of government, which resulted in a significant reduction of wages and salaries paid by the central government and an increase of central government transfers to other levels of government. From 2002 onwards, data are reported on an accrual basis. Data prior to 2002 are reported on a cash basis. Data are presented on a net basis (i.e., after consolidation).	Millions of Bolivianos.		K&S report data for 1973-1974; Additional data for 1998-2005
Brazil	1972-1994	1997-1998	23	The data for 1972-1981 was missing in GFS Historical Database so the data had to be added into the database and is reported in billions of cruzeiros. The data for 1980-89 is in millions of reais. Only the data for 1990-1994, 1997-1998 was available from the GFS Yearbook. Data through 1998 are on a cash basis.	Millions of Reais.		Three additional years of data: 1972, 1997-1998
Chile	1973	2005	32	Data accords with GFS 1987 yearbook and are reported in billions of pesos. Data through 2001 are on a cash basis. From 2002 onwards, data are reported on an accrual basis.	Billions of Pesos.		Additional data for 1998-2005
Colombia	1982-1986		4	The data for 1974-1981 is only available for state & local governments in GFS Historical Database. For the central government, only data for 1982-1986 are available. The data for 1990-1999 was added based on data reported in GFS 2000 Yearbook on a gross basis (i.e. before consolidation). The data for 1998 and 1999 had to be manually entered from the GFS annual yearbook as it was not reported in the CD-rom data and is on a gross basis. From 2001 onwards, data are reported on an accrual basis.	Billions of Pesos.	Yes.	Not in sample
Costa Rica	1972	2005	33	The data is reported in colones and accords with printed data in GFS 1987 Yearbook. Data through 2004 are on a cash basis. Starting in 2004, data for subsectors of central government are presented on gross basis (i.e., before consolidation).	Millions of Colones.	Yes.	Additional data for 1972, 1998-2005
Dominican Republic	1973-2000	2002-2004	29	The data accords with printed data in GFS 1983 Yearbook and is reported in millions of pesos. Through 2004, the subsectors of central government are presented on a net basis (i.e., after consolidation). Data through 2004 are on a cash basis.	Millions of Pesos.	Yes.	Additional data for 1998-2004
Ecuador	1974	1990	16	Data only available for budgetary central government (vs. consolidated central government spending). From 1990 onwards, data are reported on a gross basis (before consolidation).	Suaces. GDP data had to be converted from Millions of US\$.	Yes.	Data series more complete as it includes 1973-1994
El Salvador	1973-1989	1998-2003, 2005	22	The data for 1973-1982 was missing in GFS Historical Database so the data had to be added into the database and is reported in millions of colones according to the GFS 1982 and 1987 Yearbooks. The data from 1982-1989 was missing and also had to be added according to the GFS 1992 Yearbook in millions of colones. The data for 1990-1997 are reported on a gross basis (before consolidation). The data from 1990-2000 are reported in millions of colones and data from 2001-2005 are reported in millions of US dollars. Data from 1998-2005 are presented on a net basis (i.e., after consolidation). Starting in 2002, data are on an accrual basis. Data through 2001 are on a cash basis.	Colones. GDP data and government spending data had to be converted from Millions of US\$.	Yes.	Additional data for 1998-2003 and 2005
Guatemala	1974-1989		14	The data for 1974-1979 was missing in GFS Historical Database so the data had to be added into the database using the printed data in the GFS 1983 Yearbook in millions of quetzales. 1980 missing. The data for 1981-1989 was missing and had to be added per the GFS 1992 Yearbook in millions of quetzales. The data for 1990-1994 and 2000-2005 is on a gross basis (before consolidation).	Millions of Quetzales.	Yes.	Data series more complete as it includes 1973-1997
Honduras	1972	1979	7	The data for 1972 was missing in GFS Historical Database so the data had to be added into the database based on the GFS 1983 Yearbook.	Millions of Lempiras.		Not in sample
Mexico	1972	2000	28	The data accords with printed data in GFS 1983 Yearbook and is reported in billions of pesos.	Millions of Pesos.		Additional data for 1972, 1998-2000
Nicaragua	1972-1980	1990-1994	12	The data did not accord with either the GFS 1983, 19987 or 1992 yearbooks and had to be manually entered.	Millions of Córdobas.		Not in sample
Panama	1973	2001	28	The data accords with printed data in GFS 1983 and 1992 Yearbooks and is reported in millions of balboas.	Millions of Balboas.		Not in sample
Paraguay	1972-1987	1989-1993	19	The data accords with printed data in GFS 1983 and 1993 Yearbooks and is reported in millions of guaranies. The data for 1990-1993 is on a gross basis (before consolidation).	Billions of Guaranies.	Yes.	Additional data for 1972
Peru	1973-1986		13	The data for 1973-1981 was missing in GFS Historical Database so the data had to be added into the database based on data reported in the GFS 1983 and 1987 Yearbooks in billions of soles. The data for 1982-1986 was missing and was added based on data reported in the GFS 1992 Yearbook. Starting in 1999, data for subsectors of central government are presented on a gross basis (i.e., before consolidation) except for total spending. The data for 1999-2003 was added as it was missing from the cd-rom database, but included in the print version of the yearbook.	Millions of Nuevos Sol Yes.		Same
Uruguay	1972	2001	29	The data accords with the statistics presented in the GFS 1987 Yearbook and is reported in millions of new pesos. Starting in 2002, the data is presented on a gross basis (before consolidation) except for total spending.	Millions of Pesos.	Yes.	Additional data for 1972, 1998-2001
Venezuela	1972-1986	1999-2005	20	The data for 1972-1982 was missing in GFS Historical Database so the data had to be added into the database based on GFS 1983 Yearbook. The data for 1983-1986 was missing in GFS Historical Database so the data had to be added into the database based on GFS 1992 Yearbook.	Billions of Bolivares		Additional data for 1972, 1998-2005

Source: International Monetary Fund, *Government Finance Statistics*, [Cd-Rom], Washington, DC: International Monetary Fund and various print versions of the same statistical yearbook.

Appendix 2B. Latin America Democracy Codebook, 1970-2005

This codebook builds on an earlier democracy codebook compiled by Avelino (2006). The data used in the empirical chapters of this dissertation comprises annual data for 15 countries over almost 30 years (1973-2000). The final sample comprises 15 countries and excludes Colombia, Honduras and Nicaragua. The information for these countries is still included in the notes that follow, but each country is denoted with an “†” to denote that it was not included in the statistical analysis reported in the chapters.

Defining Democracy and Regime Transitions

A dichotomous dummy variable for the political regime, which codes one for democracies and zero for the residual category of authoritarian regimes, was employed. This measure of a minimalist definition of democracy builds on the seminal work of Schumpeter (1942) and more recently Sartori (1987) and considers a democratic regime as one in which the executive and the legislature are both filled by “contested elections.” The data employed draws on an updated version of the Przeworski, Álvarez, Cheibub and Limongi (2000) dataset by Cheibub and Ghandi (2004). As this database only classifies regimes up until 2002, the database was updated for 2003-2005 by the author.

Drawing upon on the classification of democratic and authoritarian regimes in Nohlen (2005), the author adopted coding that differs from Przeworski, et.al (2000) and Cheibub, et.al. (2004) for specific periods in seven countries. First, Bolivia was recoded as authoritarian in 1978 as a *coup d'état* took place on July 21st and thus Bolivia is not considered to have returned to democracy until 1982. Under Ernesto Beckmann Geisel and João Baptista de Oliveira Figueiredo, Brazil was recoded as non-democratic between 1978 and 1984. According to Nohlen (2005), the main opposition party did not participate in elections in 1970 or 1974 and the first free and competitive elections were held in 1978 in the Dominican Republic. Therefore, the period between 1970 and 1977 was recoded as authoritarian. In the case of Ecuador, the period from 2000-2002 is not considered authoritarian as power was transferred to the Vice-President and thus succession followed constitutional law. As Nohlen considers Marco Vinicio Cerezo Arévalo the first constitutionally elected president in 20 years, Guatemala was recoded as authoritarian from 1970 to 1981. Panama is not considered

to have returned to democracy until the inauguration of Ernesto Perez Balladares' term in 1994, the years between 1989 and 1993 were recoded as authoritarian. Paraguay was recoded as a democracy since Juan Carlos Wasmosy's term inauguration in 1993 to 2002.

Avelino (2005) adopts a definition of the beginning of a authoritarian transition as established by a singular event that reflects the internal split in the authoritarian coalition, such as the announcement of elections, the draft of a new constitution, a national plebiscite, or a public statement by authoritarian leaders about their intentions to liberalize based on information drawn from the comparative case study literature. He divides regime transitions into four categories: Stable Authoritarian, Transitional Authoritarian, Transitional Democratic, and Stable Democratic for nineteen Latin American governments from 1980-2000. Following the codification employed by Avelino to classify transitional regimes in Latin America, the second measure of democratization tries to capture the effects of the different stages of democracy by drawing a distinction between "transitional" and "stable" democratic governments. A similar definition was used to update the data and to code the period between 1970 and 1979 drawing upon on the classification of democratic and authoritarian regimes in Nohlen (2005). The beginning of democratic transition is defined as the year of the inauguration of the first democratic regime following a period of authoritarian transition.

The onset of stable democracy is defined as the second 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. The rationale for adopting this criterion is based on the view that the alternation in power within democracy is theoretically closer to the definition of democracy, which stresses that democracy is a regime in which incumbents lose power through elections and willingly relinquish power to the winner.

Defining Elections

The data used in the chapters on electoral cycles comprises annual data for 15 countries over almost 30 years (1973-2000). The final sample comprises 15 countries and excludes Colombia, Honduras and Nicaragua. As this study departs from the premise that elections are only a valid measure of democratic transition following the return of democracy

with the inauguration of a democratic president, I confine the sample to countries with democratic political institutions. This permits a total of 15 countries in the panel, but some of them enter only in some years. For example, the 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). I added election dates from 1970 to 1979 using information reported in Nohlen (2005) and the *Political Database of the Americas* (Center for Latin American Studies at Georgetown University 2007). I also updated the database to 2005 drawing upon election results reported by the IPU's *Parline* database (2007). To double check information, I compared all coding with the *Database on Political Institutions* (DPI) created by Beck et al. (2001). In a few cases, errors were detected in the coding of election years in the DPI database for Latin America and the coding changes that were necessary are denoted in brackets [] and comments are made in CAPS.

In the case of electoral cycle dummy variables, two variables were created. The first variable follows the rule of the year and codes 1 if an election occurred in that particular year from January to December. Thus, "1" indicates that the election occurred after January 1st and before December 31st of the year in question and otherwise the value of "0" was assigned. The other measure adopted the rule of the semester which codes all elections prior to June 1st as "1" in the previous year. Dummy variables for the year prior to an election and the year following an election were also created. The dataset includes 63 presidential elections and 71 legislative elections and a total of 241 democratic years. Of the 15 countries in the sample, all countries had at least one presidential election. Mexico is the only country that only has one election (e.g. Vicente Fox's election in 2000). On average, there are 3.5 presidential elections for country. In 55 presidential elections, voters also choose congressional representatives. In the chapter, I limit attention to the years of presidential elections and do not include 16 midterm legislative elections.

To test for differences between foundational election and normally scheduled election years, a dichotomous dummy variable that codes one for the year of a presidential election in which power was transferred from the military to a civilian leader was created (Przeworski

and Cheibub 1999). Three countries do not have a foundational election: Bolivia, Costa Rica and Venezuela. In the simple there are 13 founding elections meeting this criteria. Although he finally assumes power in October 1982, the June 1980 presidential election of Siles Zuazo in Bolivia is not coded as a foundational election as he cannot assume due to a coup d'état. Competitive elections occurred throughout the period in Costa Rica and Venezuela.

In order to examine whether closer elections impact social spending budget shares, I created a variable to measure the electoral margin of the leading 2 candidates in each competitive election between 1973 and 2000 based on the percentages reported in Nohlen (2005) and the *Political Database of the Americas* (Center for Latin American Studies at Georgetown University 2007). In cases where there were two rounds of voting, the first round was adopted. Argentina held two presidential elections in 1973. I only use the results of the September 1973 elections in the dataset.

I also created a variable that was scored as one if the president was re-elected in the specific election year. Of the sixty-three 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 1994, Ricardo Lagos in Chile in 2000, Joaquín Balaguer in the Dominican Republic in 1990 and Hugo Chavez in Venezuela in 2000.

A detailed discussion of the coding adopted for each country is described below.

COUNTRIES

1. ARGENTINA

Authoritarian Government: 1966-1972 and 1976-1982

Democracy (1): since 9/1973 when Peron is elected

Democracy (2): since 12/83 with the inauguration of Alfonsin's term

Authoritarian transition (1): 1972

Authoritarian transition (2): 1982 (Defeat in Malvinas War (04/82), General Reynaldo A. Bignones' call for elections in 06/82

1st Turnover: with the end of Alfonsin's term (07/1989) when Carlos Menem assumes power (turnover).

2nd Turnover: with the end of Menem's 2nd term (12/1999) when Fernando de la Rúa assumes power (turnover)

Presidential Elections: 9/1973 [DPI MISCODED AS 3/1974], 10/1983, 5/1989, 5/1995, 10/1999 and 4/2003

Legislative Elections: 9/1973, 10/1983, 9/1987, 5/1989, 8-12/1991, 10/1993, 5/1995, 10/1997, 10/1999, 10/2001 and 10/2003, 10/2005.

Foundation Elections: 9/1973, 10/1983

2. BOLIVIA

(1979 DIFFERENT FROM CHEIBUB AND GHANDI (2004))

Democracy: since 10/1982 (1983).

Authoritarian Government: 1970-1978

Authoritarian transition: 1978-1982 (1989) (after Banzer's deposition in 1978, 7 military and 2 civil governments between 1978 and 1982). Lidia Gueiler is elected President by the Congress in 16/11/79. Siles Zuazo (UDP) wins Presidential Elections in 06/80 but cannot assume, he will finally assume in 10/1982.

1st Turnover: with the end of Siles Suazo (UDP)'s term and the inauguration of Victor Paz Estenssoro (A-MNR)'s term in 08/1985 (turnover).

2nd Turnover: with the inauguration of Jaime Paz Zamora's term in 08/1989 (turnover).

Presidential Elections: 1980 [This election year was deleted since coup takes place in same year and president only assumes in 1982], 7/1985, 5/1989, 6/1993, [DPI MISSING] 6/1997, 6/2002.

Legislative Elections: 7/1985, 5/1989, 6/1993, [DPI MISSING] 6/1997 and 6/2002.

Foundation Election: None.

3. BRAZIL

(1979-1984 DIFFERENT FROM CHEIBUB AND GHANDI (2004))

Democracy: since 03/1985 when Jose Sarney assumes Presidency

Authoritarian government: 1964-1981

Authoritarian Transition: 1982-1984 with the 1982 Chamber and Senate elections

1st Turnover: when Sarney's term ends and power is transferred to Fernando Collor de Mello in 03/90.

2nd Turnover: when Fernando Henrique Cardoso (PSDB)'s is elected as PMDB loses power of presidency.

Presidential Elections: 1/1985, 11/1989, 10/1994, 10/1998, 10/2002.

Legislative Elections: 11/1986, 10/1990, [DPI MISSING 10/1994], 10/1998, 10/2002.

Foundation Election: 1/1985

4. CHILE

Democracy (1): 1970-1972

Democracy (2): since 03/1990.

Authoritarian government: 1973-1987

Authoritarian Transition: Since the military defeat in the national Plebiscite of 10/1988

1st Turnover: none, since Patricio Aylwin's is substituted by Eduardo Frei from the same Concertación Alliance (turnover), who is substituted by another "Concertación" president Ricardo Lagos in 2000.

Presidential Elections: 9/1970, 12/1989, 12/1993, 12/1999 [DPI CODED 1/2000 2nd Round], and 12/2005

Legislative Elections: 12/1989, 12/1993, 12/1997, 12/2001, and 12/2005

Foundation Election: 12/1989

5. COLOMBIA[†]

Democracy: since 1958

No Transition period.

Presidential Elections: 4/1970, 4/1974, 4/1978, 5/1982, 5/1986, 5/1990, 6/1994, 6/1998 [DPI MISCODED AS 3/1999], and 5/2002

Legislative Elections: 4/1970, 4/1974, 2/1978, 3/1982, 3/1986, 3/1990, 10/1991 [DPI MISSING], 3/1994, 3/1998 [DPI MISCODED AS 3/1999], and 3/2002

6. COSTA RICA

Democracy: since 1949.

No transition period.

Presidential Elections: 2/1970, 2/1974, 2/1978, 2/1982, 2/1986, 2/1990, 2/1994, 2/1998, 2/2002

Legislative Elections: 2/1970, 2/1974, 2/1978, 2/1982, 2/1986, 2/1990, 2/1994, 2/1998, 2/2002

Foundation Election: None.

7. DOMINICAN REPUBLIC

(1972-1977 DIFFERENT FROM CHEIBUB AND GHANDI (2004))

Democracy: since 1978. According to Nohlen, the main opposition party did not participate in elections in 1970 or 1974. The first free and competitive elections were held in 1978 and PRD wins the elections. The following five elections up until 1994 were very problematic and there were widespread accusations of fraud. Balaguer agreed to a shorter 2 year term and elections were held again in 1996.

Authoritarian Transition: 1977

Democratic Transition: when Balaguer (PRSC) allows Silvestre Antonio Guzmán Fernández (PRD) to assume power (08/1978)

1st Turnover: when Joaquin Balaguer (PRSC)'s returns to power in 08/1986.

2nd Turnover: when Leonel Fernandez of PLD wins election in 6/1996

*Mainwaring, et.al. classify as authoritarian from 1994-1995. According to Nohlen, the 1994 elections were also branded fraudulent and Balaguer's victory was questioned. A Pacto por la Democracia was signed and elections were held in 1996.

The elections are as follow in terms of party control:

1978- PRD wins PRSC (Beginning of Democracy)

1982- PRD

1986- PRSC (1st turnover)

1990-PRSC

1996- PLD wins over PRD. (2nd turnover)

2000-PRD wins

Presidential Elections: 5/1978, 5/1982, 5/1986, 5/1990, 5/1994, 6/1996, 5/2000, 5/2004

Legislative Elections: 5/1978, 5/1982, 5/1986, 5/1990, 5/1994, 5/1998, 5/2002

Foundation Election: 5/1978

8. ECUADOR

(DIFFERENT FROM CHEIBUB AND GHANDI (2004) THAT CODED 2000-2002 AS AUTHORITARIAN YEARS)

Democracy: since 1979

Authoritarian Transition: 1976-1978 (1989)(Removal of the General Guillermo Rodríguez Lara in 1976, Constitutional Approval by Plebiscite in 01/1979

Start Date of Democratic Transition: since the end of the Aguilera/Hurtado administration 79/84 - Hurtado, the vice-president, assumed in 1981 due to the death of president Jaime Roldós Aguilera

1st Turnover: when Osvaldo Hurtado from the Center-Left coalition gives power to the rightist Social Christian León Febres Cordero in 1984.

2nd Turnover: when Febres Cordero transfers power to Rodrigo Borja of the social-democratic candidate of the ID party in 08/1988

On January 21, 2000, during demonstrations in Quito by indigenous groups, the military and police refused to enforce public order. Demonstrators entered the National Assembly building and declared a three-person "junta" in charge of the country. Field-grade military officers declared their support for the concept. During a night of confusion and negotiations, President Mahuad fled the presidential palace. Vice President Gustavo Noboa took charge and Mahuad went on national television to endorse Noboa as his successor. Congress met in emergency session in Guayaquil the same day, January 22, and ratified Noboa as President of the Republic. Noboa turned over the government on January 15, 2003, to his successor, Lucio Gutierrez, a former army colonel who first came to public attention as a member of the short-lived "junta" of January 21, 2000.

Presidential Elections: 4/1979, 1/1984, 1/1988, 7/1992, 7/1996, 6/1998 [2nd round 7/1998], 11/2002

Legislative Elections: 4/1979, 1/1984, 6/1986, 1/1988, 6/1990, 5/1992, 5/1994, 5/1996, 5/1998, 10/2002

Foundation Election: 4/1979

9. EL SALVADOR

Democracy: since 06/1984 with the inauguration of José Napoleon Duarte's term-

Authoritarian Transition: since the election for the Constitutional Assembly in 03/1982.

1st Turnover: with the inauguration of Alfredo Cristiani's term in 06/1989

2nd Turnover: with the inauguration of Armando Calderon's term in 06/1994

Presidential Elections: 3/1984, 3/1989, 4/1994, 3/1999, 3/2004

Legislative Elections: 3/1985, 3/1988, 3/1991, 3/1994, 3/1997, 3/2003

Foundation Election: 3/1984

10. GUATEMALA

(DIFFERENT FROM CHEIBUB AND GHANDI (2004) THAT CODED 1970-1981 AS DEMOCRATIC YEARS)

Democracy: since 01/1986 with the inauguration of Vinicio Cerezo's term whom Nohlen, et. al. recognize as the first constitutionally elected president in 20 years

Authoritarian Transition: since 08/1983 with the removal of General Rios Montt by Gen. Mejia, the authoritarian government announces a political opening.

1st Turnover: when Christian Democrat Vinicio Cerezo (DCG) transfers power to the conservative Jorge Antonio Serrano Elias(MAS) (01/1991). Following Jorge Antonio Serrano Elias' (01/1991) self-coup attempt in 1992, Pres. Ramiro de Leon Carpio of the Unity of the National Centre (UCN) is elected as president in 06/1993 in parliamentary elections.

2nd Turnover: with the inauguration of Alvaro Arzu (PAN) and the "New Right" coalition win in January 1996.

Presidential Elections: 11/1985, 11/1990, 11/1995 [2nd Round 1/1996], 11/1999 [2nd Round 12/1999], 11/2003

Legislative Elections: 11/1985, 11/1990, 8/1994, 11/1995, 11/1999, 11/2003

Foundation Election: 11/1985

* Elections

Elections were held for all the seats in Parliament in accordance with the constitutional reform of January 1994. General elections had previously been held in August 1994

- Marco Vinicio Cerezo Arévalo (Christian Democratic Party): 1986-1991
- Jorge Antonio Serrano Elías Solidarity Action Movement): 1991-1993
- Ramiro de León Carpio (non-party): 1993-1996
- Álvaro Arzú Yrigoyen (National Advancement Party): 1996-2000
- Alfonso Portillo (Guatemalan Republican Front): 2000-2004

11. HONDURAS[†]

(DIFFERENT FROM CHEIBUB AND GHANDI (2004) AS 1972 IS CODED AS DEMOCRACY)

Democracy (1): Short-lived democracy between 1971-72. There were constituent assembly elections in 1957 which appointed Ramón Villeda as President, and itself becoming a national Congress with a 6-year term. In October 1963, conservative military officers preempted constitutional elections and deposed Villeda in a bloody coup. These officers exiled PLH members and governed under General Oswaldo López until 1970. A civilian president for the PNH, Ramón Ernesto Cruz, took power briefly in 1970 until, in December 1972, Oswaldo López staged another coup.

Democracy (2): since 01/1982 with the inauguration of Roberto Suazo Cordova (Partido Liberal)'s term.

Authoritarian Transition: since 04/1980 with the elections for the Constitutional Assembly.

1st Turnover: with the inauguration of Rafael Leonardo Callejas (PN)' term in 01/1990

2nd Turnover: with the inauguration of Carlos Roberto Reina (PL)'s term in 01/1994

Presidential Elections: 11/1981 [DPI MISCODED AS NON EXECUTIVE ELECTION YEAR], 11/1985 [DPI MISSING MONTH], 11/1989, 11/1993, 11/1997, 11/2001, 11/2005

Legislative Elections: 11/1981, 11/1985, 11/1989, 11/1993, 11/1997, 11/2001, 11/2005

Foundation Election: 11/1981

12. MEXICO

Democracy since the inauguration of Vicente Fox's (PAN) term in 12/2000.

Authoritarian Transition: since 07/1988 (authoritarian transition since the beginning of Salinas' election)

No 1st Turnover

Pre-democracy elections 7/1982, 7/1988, 8/1994, 1/1997

Presidential Elections: 7/2000

Legislative Elections: 7/2000, 7/2003

Foundation Election: 7/2000

13. NICARAGUA[†]

Democracy: since 11/1984 with the election of Daniel Ortega as elected president and a national assembly elected

Authoritarian Transition: since 1979 with the Sandinista Revolution and the end of Somoza's authoritarian government.

1st Turnover: since 04/1990 with the inauguration of Violeta Chamorro's term

2nd Turnover: with Daniel Ortega's term in 01/2007

1984

Daniel Ortega Saavedra, Sandinista National Liberation Front (FSLN)

1990

Violeta Barrios de Chamorro, National Opposition Union (UNO –PLC included)

1996

Arnoldo Alemán Lacayo, (AL) Liberal Alliance led by the Partido Liberal Constitucionalista (PLC) wins elections against Daniel Ortega (FSLN)

2001

Enrique Bolaños Geyer - Constitutional Liberal Party (PLC); but, he broke with PLC to help form the Alliance for the Republic (APRE) once in office.

Presidential Elections: 11/1984, 2/1990, 10/1996, 11/2001

Legislative Elections: 11/1984, 2/1990, 10/1996, 11/2001

Foundation Election: 11/1984

14. PANAMA

(DIFFERENT FROM CHEIBUB AND GHANDI (2004) AS 1989-1994 ARE CODED AS AUTHORITARIAN YEARS)

Democracy with the inauguration of Ernesto Perez Balladares' term in 09/94 (PRD as part of coalition Pueblo Unido) against Mireya Moscoso.

Transition: since the U.S. military intervention and the beginning of the term of Guillermo Endara, former candidate (by ADOC), in 12/1989.

1st Turnover: when Mireya Moscoso (AUDD) term inaugurated in 09/1999.

2nd Turnover: when Martin Torrijos' (PRD) term is inaugurated in 09/2004.

Presidential Elections: 5/1994, 5/1999, 5/2004

Legislative Elections: 5/1994, 5/1999, 5/2004

Foundation Election: 5/1994

15. PARAGUAY

(DIFFERENT FROM CHEIBUB AND GHANDI (2004) AS 1993-2004 ARE CODED AS DEMOCRATIC YEARS)

Democracy: since 08/1993 (inauguration of Juan Carlos Wasmosy's (ANR-PC) term who won elections held in 05/1993)

Authoritarian Transition: After the fall of 33 years of Stroessner's rule with the coup lead by Andres Rodrigues in 2/1989 since 03/1989 (Rodrigues' coup, election and term)

Cubas Grau won elections held in 05/1998 and his term is inaugurated in 08/15/1998, but he is impeached in 03/28/1999 with the murder of the vice-president José Maria Argaña. The president of Congress Luiz Gonzales Macchi took the Presidency in 03/1999.

1st Turnover: None since Cubas Grau(ANR-PC), who succeeds Wasmosy in 1999, since both are from the same party ANR. The same is valid for Luiz Gonzalez Macchi and most recent presidential winner in 2003 election, Nicanor Duarte(ANR-PC).

Presidential Elections: 5/1993, 5/1998, 4/2003

Legislative Elections: 5/1993, 5/1998, 4/2003

Foundation Election: 5/1993

16. PERU

Democracy (1): since 07/1980 (Fernando Belaunde Terry's term)

Democracy (2): since 04/2001 with election of Alejandro Toledo (Perú Posible) term is inaugurated in 07/28/2001

Authoritarian Transition (1): since 06/1978 (elections for the Constitutional Assembly)

Authoritarian Transition (2): since 2000 with Fujimori's term (which coded as entirely authoritarian given Alvarez et. al. rules). Fujimori staged coup d'état on 5 April 1992.

Fujimori resigned in 11/2000, and the power is given to the president of Peruvian Congress Valentín Paniagua until elections in May and June of 2001 when Alejandro Toledo (Perú Posible) is elected.

1st Turnover: since 07/1985 with the inauguration of Alan Garcia's term (Partido Aprista Peruano).

1st Turnover (2): since 28 July 2006, Alan García (APRA) was sworn in as President of the Republic for the second time.

Presidential Elections: 5/1980, 4/1985, 4/1990, and 4/2001

Legislative Elections: 5/1980, 4/1985, 4/1990, and 4/2001

Foundation Election: 5/1980, 4/2001

17. URUGUAY

Democracy (1):- Between 1971-1973 the democratic regime gradually gave way to a military dictatorship until congress was dissolved in 1973.

Democracy (2): since 03/1985 (inauguration of Julio Maria Sanguinetti's term (Colorado))

Authoritarian Transition: 11/1980 (military defeat in national Plebiscite)

1st Turnover: with Alberto Lacalle (Blanco)'s term in 03/1990

2nd Turnover: with inauguration of Sanguinetti's term in 03/1995

Presidential Elections: 1971, 11/1984, 11/1989, 11/1994, 11/1999, 10/2004 [DPI MISSING]

Legislative Elections: 1971, 11/1984, 11/1989, 11/1994, 11/1999, 10/2004 [DPI MISSING]

Foundation Election: 11/1984

18. VENEZUELA

Democracy: since 1958.

No transition period

Presidential Elections: 12/1973, 12/1978, 12/1983, 12/1988, 12/1993, 12/1998, 7/2000

Legislative Elections: 12/1973, 12/1978, 12/1983, 12/1988, 12/1993, 12/1998, 7/2000, 12/2005

Foundation Election: None

Appendix 4A. Error Correction Model (ECM) Results on Democracy and New Democracy

Table 4A.1 Results of Error Correction Model (ECM) Estimations for the Effect of Globalization and Democracy on Social Spending

Dependent Variable	ΔWelfare/Budget			ΔSocial Security/Budget			ΔHealth and Education/Budget		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	ECM case	ECM with country fixed effects (f.e.)	ECM with year and country f.e.	ECM case	ECM with country f.e.	ECM with year and country f.e.	ECM case	ECM with country f.e.	ECM with year and country f.e.
Lagged Dependent Variable _{t-1}	-0.261*** (0.033)	-0.541*** (0.048)	-0.540*** (0.049)	-0.222*** (0.034)	-0.517*** (0.048)	-0.502*** (0.048)	-0.196*** (0.050)	-0.480*** (0.082)	-0.482*** (0.085)
D. % of population aged 65 and over _t	-10.929* (5.675)	13.782 (11.566)	-19.936 (17.087)	-9.183 (5.609)	-5.975 (7.785)	-18.198 (13.129)	-0.304 (5.021)	18.353* (9.870)	-0.122 (15.189)
% of population aged 65 and over _{t-1}	0.535** (0.223)	3.327*** (0.763)	-0.477 (1.243)	0.820*** (0.220)	3.192*** (0.803)	1.621 (1.201)	-0.327** (0.153)	0.042 (0.509)	-2.044* (1.231)
D. % of population aged 15 to 64 _t	3.941** (1.828)	-1.928 (2.356)	0.307 (2.835)	2.370* (1.420)	-0.491 (0.739)	1.868** (0.761)	1.018 (1.730)	-1.370* (2.011)	-0.333 (0.901)
% of population aged 15 to 64 _{t-1}	0.884*** (0.166)	0.019 (0.487)	0.381 (0.233)	0.585*** (0.164)	-0.325** (0.150)	-0.244 (0.526)	0.158 (0.152)	-0.419 (0.175)	-0.781* (0.182)
D. %urban population _t	1.921** (0.805)	0.460 (0.860)	-1.041 (0.923)	0.506 (1.090)	-1.571 (0.978)	-2.110* (1.138)	1.084 (1.136)	1.951* (1.119)	1.229 (1.281)
%urban population _{t-1}	-0.089** (0.034)	0.332** (0.132)	-0.157 (0.264)	0.005 (0.050)	0.550*** (0.126)	0.431** (0.182)	-0.070 (0.054)	-0.217 (0.135)	-0.568** (0.258)
D. GDP per capita _t	-0.003* (0.002)	-0.003** (0.001)	-0.003* (0.002)	-0.003* (0.001)	-0.002* (0.001)	-0.002 (0.002)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)
GDP per capita _{t-1}	0.000* (0.000)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.000)	-0.000 (0.001)	-0.000 (0.001)	0.000* (0.000)	0.000 (0.000)	-0.000 (0.001)
D. GDPpercapita growth _{t,2}	0.036 (0.062)	-0.010 (0.059)	-0.009 (0.073)	0.121** (0.058)	0.074 (0.051)	0.063 (0.060)	-0.070 (0.051)	-0.074 (0.048)	-0.055 (0.056)
GDPpercapita growth _{t,3}	0.004 (0.059)	-0.056 (0.066)	-0.118 (0.083)	0.085 (0.058)	0.018 (0.059)	-0.011 (0.069)	-0.063 (0.051)	-0.064 (0.053)	-0.085 (0.061)
D. Trade Openness _t	-0.068* (0.040)	-0.025 (0.043)	-0.079* (0.047)	-0.164*** (0.053)	-0.138*** (0.050)	-0.177*** (0.053)	0.102* (0.057)	0.112** (0.052)	0.098* (0.058)
Trade Openness _{t-1}	-0.044** (0.020)	-0.035 (0.028)	-0.091*** (0.030)	-0.088*** (0.021)	-0.080** (0.031)	-0.110*** (0.036)	0.044*** (0.016)	0.046* (0.026)	0.028 (0.032)
D. Capital Mobility Index _t	-0.081 (0.864)	0.548 (0.828)	-0.657 (0.903)	1.350* (0.818)	1.798** (1.856)	-0.051 (1.806)	-1.582* (0.906)	-1.128 (0.827)	-1.697* (2.153)
Capital Mobility Index _{t-1}	1.016*** (0.324)	0.837* (0.218)	0.099 (0.619)	0.721*** (0.244)	1.194** (0.467)	1.098** (0.152)	0.130 (0.234)	0.322* (0.355)	0.318* (0.467)
Democracy _t	0.129 (0.456)	0.016 (0.712)	0.697 (0.845)	-1.119** (0.442)	-2.331*** (0.701)	-1.827** (0.814)	1.227*** (0.376)	2.261*** (0.732)	2.396*** (0.899)
Constant	-37.465*** (6.888)	-18.788** (7.782)	46.914** (23.515)	-29.543*** (7.060)	-26.384*** (8.711)	-10.703 (19.961)	-2.744 (6.415)	7.539 (7.827)	53.797** (26.479)
Observations	337	337	337	337	337	337	337	337	337
R-squared	0.14	0.28	0.34	0.14	0.30	0.36	0.14	0.29	0.34

Notes: 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%. All variables preceded by "D." are measured in first-differences.

Table 4A.2. Results of Error Correction Model (ECM) Estimations for the Effect of New Democracy on Social Spending

Dependent Variable	ΔWelfare/Budget			ΔSocial Security/Budget			ΔHealth and Education/Budget		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	ECM case	ECM with country fixed effects (f.e.)	ECM with year and country f.e.	ECM case	ECM with country f.e.	ECM with year and country f.e.	ECM case	ECM with country f.e.	ECM with year and country f.e.
Lagged Dependent Variable _{t-1}	-0.265*** (0.033)	-0.544*** (0.048)	-0.555*** (0.048)	-0.223*** (0.034)	-0.517*** (0.049)	-0.504*** (0.048)	-0.199*** (0.050)	-0.485*** (0.080)	-0.486*** (0.084)
D. % of population aged 65 and over _t	-10.471* (5.728)	15.034 (11.518)	-18.499 (17.198)	-8.919 (5.511)	-5.649 (7.686)	-17.462 (13.205)	-0.108 (4.880)	19.286** (9.691)	0.568 (15.135)
% of population aged 65 and over _{t-1}	0.545** (0.226)	3.713*** (0.788)	0.069 (1.271)	0.822*** (0.221)	3.298*** (0.827)	1.899 (1.280)	-0.329** (0.151)	0.281 (0.500)	-1.828 (1.263)
D. % of population aged 15 to 64 _t	3.912** (1.823)	-1.891 (0.839)	0.238 (0.906)	2.340* (1.405)	-0.475 (0.738)	1.830** (0.757)	1.006 (1.718)	-1.108 (2.007)	-0.348 (0.902)
% of population aged 15 to 64 _{t-1}	0.891*** (0.169)	0.826* (0.489)	0.369 (0.234)	0.584*** (0.164)	-0.342** (0.161)	-0.269* (0.161)	0.159 (0.154)	0.287 (0.355)	0.301 (0.464)
D. %urban population _t	1.721** (0.794)	0.466 (0.847)	-1.091 (0.928)	0.405 (1.152)	-1.570 (0.979)	-2.136* (1.132)	1.029 (1.182)	1.972* (1.124)	1.224 (1.279)
%urban population _{t-1}	-0.089** (0.035)	0.404*** (0.131)	-0.034 (0.266)	0.006 (0.049)	0.571*** (0.153)	0.494** (0.231)	-0.070 (0.055)	-0.172 (0.160)	-0.520* (0.293)
D. GDP per capita _t	-0.002 (0.002)	-0.003** (0.001)	-0.004** (0.002)	-0.003* (0.001)	-0.002* (0.001)	-0.002* (0.002)	-0.000 (0.001)	-0.000 (0.001)	-0.002 (0.001)
GDP per capita _{t-1}	0.000 (0.000)	-0.000 (0.001)	-0.001 (0.001)	-0.000 (0.000)	-0.000 (0.001)	-0.000 (0.001)	0.000* (0.000)	0.000 (0.000)	-0.000 (0.001)
D. GDPpercapita growth _{t-2}	0.039 (0.063)	-0.004 (0.059)	-0.003 (0.072)	0.123** (0.058)	0.075 (0.051)	0.066 (0.060)	-0.069 (0.051)	-0.070 (0.048)	-0.052 (0.056)
GDPpercapita growth _{t-3}	0.007 (0.060)	-0.044 (0.066)	-0.109 (0.082)	0.087 (0.057)	0.022 (0.060)	-0.006 (0.070)	-0.062 (0.051)	-0.057 (0.054)	-0.082 (0.061)
D. Trade Openness _t	-0.070* (0.039)	-0.022 (0.043)	-0.077 (0.047)	-0.165*** (0.053)	-0.137*** (0.050)	-0.176*** (0.053)	0.101* (0.058)	0.114** (0.052)	0.098* (0.058)
Trade Openness _{t-1}	-0.049** (0.021)	-0.032 (0.028)	-0.087*** (0.030)	-0.090*** (0.021)	-0.079** (0.031)	-0.108*** (0.036)	0.043** (0.017)	0.049* (0.027)	0.029 (0.033)
D. Capital Mobility Index _t	-0.012 (0.865)	0.473 (2.371)	-0.692 (2.836)	1.378* (0.819)	1.775** (1.862)	-0.060 (1.808)	-1.559* (0.905)	-1.420* (0.830)	-1.726* (2.150)
Capital Mobility Index _{t-1}	0.986*** (0.322)	-0.039 (0.217)	0.053 (0.620)	0.702*** (0.248)	1.190** (0.468)	1.092** (0.527)	0.118 (0.244)	-0.432 (0.186)	-0.791* (0.190)
Democracy _t	0.701 (0.644)	-1.795 (1.115)	-1.309 (1.232)	-0.853 (0.700)	-2.838* (1.502)	-2.837* (1.593)	1.412* (0.724)	1.090 (1.660)	1.578 (1.819)
New Democracy _t	-0.971 (0.726)	1.610* (0.887)	1.899** (0.920)	-0.452 (0.656)	0.450 (0.907)	0.950 (1.016)	-0.302 (0.746)	1.048 (1.036)	0.778 (1.162)
Constant	-37.218*** (6.899)	-20.500*** (7.822)	39.989* (23.705)	-29.264*** (6.988)	-26.861*** (9.273)	-14.303 (22.310)	-2.574 (6.267)	6.529 (8.300)	51.011* (27.918)
Observations	337	337	337	337	337	337	337	337	337
R-squared	0.15	0.29	0.34	0.14	0.30	0.36	0.14	0.29	0.34

Notes: 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%. All variables preceded by "D." are measured in first-differences.

Appendix 5A. Error Correction Model Estimations of Political Budget and Welfare Cycles

Table 5A.1. ECM Estimations of Political Budget Cycles: Pre and Post-Election Year

Dependent Variable: (Government Expenditures/GDP)	(1)	(2)	(3)
	ECM with PCSE	ECM with PCSE and country fixed effects (f.e.)	ECM with PCSE and country + time f.e.
Election Year _t	-0.776** (0.362)	-0.604* (0.342)	-1.119*** (0.338)
Election Year _{t+1}	-1.063*** (0.341)	-0.914*** (0.312)	-1.110*** (0.305)
Observations	224	224	224
R-squared	0.13	0.27	0.41

Table 5A.2. ECM Estimations of Political Budget Cycles: The Effect of Contested Elections

Dependent Variable: (Government Expenditures/GDP)	(1)	(2)	(3)
	ECM with PCSE	ECM with PCSE and country fixed effects (f.e.)	ECM with PCSE and country + time f.e.
Log (Margin) _t	-0.019** (0.008)	-0.015* (0.008)	-0.024*** (0.008)
Log (Margin) _{t+1}	-0.024*** (0.008)	-0.020** (0.008)	-0.022*** (0.008)
Observations	224	224	224
R-squared	0.15	0.25	0.39

Table 5A.3. ECM Estimations of Political Budget Cycles: The Effect of New Democracy

Dependent Variable: (Government Expenditures/GDP)	(1)	(2)	(3)
	ECM with PCSE	ECM with PCSE and country fixed effects (f.e.)	ECM with PCSE and country + time f.e.
Election Year _t	-0.742 (0.480)	-0.557 (0.454)	-1.025** (0.429)
Election Year _{t+1}	-0.873** (0.431)	-0.600 (0.424)	-0.848** (0.393)
New Democracy Election Year _t	-0.058 (0.577)	-0.077 (0.612)	-0.177 (0.550)
New Democracy Election Year _{t+1}	-0.378 (0.523)	-0.625 (0.576)	-0.552 (0.525)
Observations	224	224	224
R-squared	0.13	0.27	0.41

Table 5A.4. ECM Estimations of Political Welfare Cycles: Pre and Post-Election Year

Dependent Variable: (Social Spending/ Budget)	(1)	(2)	(3)
	ECM with PCSE	ECM with PCSE and country fixed effects (f.e.)	ECM with PCSE and country + time f.e.
Election Year _t	0.546 (1.541)	-0.186 (1.354)	0.343 (1.403)
Election Year _{t+1}	3.306** (1.489)	2.875** (1.277)	2.698** (1.298)
Observations	204	204	204
R-squared	0.18	0.39	0.45

Table 5A.5. ECM Estimations of Political Welfare Cycles: The Effect of Contested Elections

Dependent Variable: (Social Spending/ Budget)	(1)	(2)	(3)
	ECM with PCSE	ECM with PCSE and country fixed effects (f.e.)	ECM with PCSE and country + time f.e.
Log (Margin _t)	0.014 (0.009)	0.005 (0.009)	0.006 (0.009)
Log (Margin t+1)	0.021** (0.009)	0.018** (0.009)	0.017* (0.009)
Observations	204	204	204
R-squared	0.13	0.32	0.41

Table 5A.6. ECM Estimations of Political Welfare Cycles: The Effect of New Democracy

Dependent Variable: (Social Spending/ Budget)	(1)	(2)	(3)
	ECM with PCSE	ECM with PCSE and country fixed effects (f.e.)	ECM with PCSE and country + time f.e.
Election Year _t	0.587 (2.116)	-1.229 (1.761)	-0.388 (1.818)
Election Year _{t+1}	2.208 (1.516)	1.009 (1.285)	1.383 (1.388)
New Democracy Election Year _t	-0.121 (2.519)	2.067 (2.304)	1.549 (2.134)
New Democr acy Election Year _{t+1}	1.960 (2.058)	3.425* (2.028)	2.616 (1.919)
Observations	204	204	204
R-squared	0.18	0.39	0.46

Appendix 6A. Error Correction Model Estimations for Globalization

Table 6A.1 Results of Error Correction Model (ECM) Estimations for the Effect of Globalization and Democracy on Social Spending

Dependent Variable	ΔWelfare/Budget			ΔSocial Security/Budget			ΔHealth and Education/Budget		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	ECM case	ECM with country fixed effects (f.e.)	ECM with year and country f.e.	ECM case	ECM with country f.e.	ECM with year and country f.e.	ECM case	ECM with country f.e.	ECM with year and country f.e.
Lagged Dependent Variable _{t-1}	-0.261*** (0.033)	-0.541*** (0.048)	-0.549*** (0.049)	-0.222*** (0.034)	-0.517*** (0.048)	-0.502*** (0.048)	-0.196*** (0.050)	-0.480*** (0.082)	-0.482*** (0.085)
D. % of population aged 65 and over _t	-10.929* (5.675)	13.782 (11.566)	-19.936 (17.087)	-9.183 (5.609)	-5.975 (7.785)	-18.198 (13.129)	-0.304 (5.021)	18.353* (9.870)	-0.122 (15.189)
% of population aged 65 and over _{t-1}	0.535** (0.223)	3.327*** (0.763)	-0.477 (1.243)	0.820*** (0.220)	3.192*** (0.803)	1.621 (1.201)	-0.327** (0.153)	0.042 (0.509)	-2.044* (1.231)
D. % of population aged 15 to 64 _t	3.941** (1.828)	-1.928 (2.356)	0.307 (2.835)	2.370* (1.420)	-0.491 (0.739)	1.868** (0.761)	1.018 (1.730)	-1.370* (2.011)	-0.333 (0.901)
% of population aged 15 to 64 _{t-1}	0.884*** (0.166)	0.019 (0.487)	0.381 (0.233)	0.585*** (0.164)	-0.325** (0.150)	-0.244 (0.526)	0.158 (0.152)	-0.419 (0.175)	-0.781* (0.182)
D. %urban population _t	1.921** (0.805)	0.460 (0.860)	-1.041 (0.923)	0.506 (1.090)	-1.571 (0.978)	-2.110* (1.138)	1.084 (1.136)	1.951* (1.119)	1.229 (1.281)
%urban population _{t-1}	-0.089** (0.034)	0.332** (0.132)	-0.157 (0.264)	0.005 (0.050)	0.550*** (0.126)	0.431** (0.182)	-0.070 (0.054)	-0.217 (0.135)	-0.568** (0.258)
D. GDP per capita _t	-0.003* (0.002)	-0.003** (0.001)	-0.003* (0.002)	-0.003* (0.001)	-0.002* (0.001)	-0.002 (0.002)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
GDP per capita _{t-1}	0.000* (0.000)	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.000)	-0.000 (0.001)	-0.000 (0.001)	0.000* (0.000)	0.000 (0.000)	-0.000 (0.001)
D.GDPpercapita growth _{t-2}	0.036 (0.062)	-0.010 (0.059)	-0.009 (0.073)	0.121** (0.058)	0.074 (0.051)	0.063 (0.060)	-0.070 (0.051)	-0.074 (0.048)	-0.055 (0.056)
GDPpercapita growth _{t-3}	0.004 (0.059)	-0.056 (0.066)	-0.118 (0.083)	0.085 (0.058)	0.018 (0.059)	-0.011 (0.069)	-0.063 (0.051)	-0.064 (0.053)	-0.085 (0.061)
D. Trade Openness _t	-0.068* (0.040)	-0.025 (0.043)	-0.079* (0.047)	-0.164*** (0.053)	-0.138*** (0.050)	-0.177*** (0.053)	0.102* (0.057)	0.112** (0.052)	0.098* (0.058)
Trade Openness _{t-1}	-0.044** (0.020)	-0.035 (0.028)	-0.091*** (0.030)	-0.088*** (0.021)	-0.080** (0.031)	-0.110*** (0.036)	0.044*** (0.016)	0.046* (0.026)	0.028 (0.032)
D. Capital Mobility Index _t	-0.081 (0.864)	0.548 (0.828)	-0.657 (0.903)	1.350* (0.818)	1.798** (1.856)	-0.051 (1.806)	-1.582* (0.906)	-1.128 (0.827)	-1.697* (2.153)
Capital Mobility Index _{t-1}	1.016*** (0.324)	0.837* (0.218)	0.099 (0.619)	0.721*** (0.244)	1.194** (0.467)	1.098** (0.152)	0.130 (0.234)	0.322* (0.355)	0.318* (0.467)
Democracy _t	0.129 (0.456)	0.016 (0.712)	0.697 (0.845)	-1.119** (0.442)	-2.331*** (0.701)	-1.827** (0.814)	1.227*** (0.376)	2.261*** (0.732)	2.396*** (0.899)
Constant	-37.465*** (6.888)	-18.788** (7.782)	46.914** (23.515)	-29.543*** (7.060)	-26.384*** (8.711)	-10.703 (19.961)	-2.744 (6.415)	7.539 (7.827)	53.797** (26.479)
Observations	337	337	337	337	337	337	337	337	337
R-squared	0.14	0.28	0.34	0.14	0.30	0.36	0.14	0.29	0.34

Notes: 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%. All variables preceded by "D." are measured in first-differences.

Table 6A.2. Results of Error Correction Model (ECM) Estimations for the Effect of Financial Openness and Democracy on Social Spending

Dependent Variable	ΔWelfare/Budget			ΔSocial Security/Budget			ΔHealth and Education/Budget		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	ECM case	ECM with year fixed effects (f.e.)	ECM with year and country f.e.	ECM case	ECM with year f.e.	ECM with year and country f.e.	ECM case	ECM with year f.e.	ECM with year and country f.e.
Lagged Dependent Variable _{t-1}	-0.258*** (0.034)	-0.251*** (0.035)	-0.553*** (0.049)	-0.217*** (0.033)	-0.206*** (0.034)	-0.503*** (0.049)	-0.202*** (0.051)	-0.195*** (0.053)	-0.486*** (0.087)
D. % of population aged 65 and over _t	-10.939* (5.657)	-15.717** (6.967)	-17.988 (16.451)	-8.739 (5.529)	-14.261** (6.584)	-16.512 (12.713)	-0.634 (5.240)	0.201 (5.862)	-0.109 (15.957)
% of population aged 65 and over _{t-1}	0.518** (0.214)	0.626*** (0.209)	-0.688 (1.131)	0.782*** (0.211)	0.908*** (0.214)	1.134 (1.247)	-0.346** (0.164)	-0.358** (0.171)	-1.718 (1.146)
D. % of population aged 15 to 64 _t	3.951** (1.774)	3.704** (1.820)	-0.947 (2.624)	2.242* (1.351)	2.124 (1.298)	0.058 (1.649)	1.079 (1.758)	0.941 (1.782)	-0.749 (2.187)
% of population aged 15 to 64 _{t-1}	0.911*** (0.161)	0.788*** (0.172)	0.117 (0.224)	0.581*** (0.162)	0.457*** (0.159)	-0.222 (0.146)	0.173 (0.146)	0.172 (0.175)	0.316* (0.186)
D. %urban population _t	1.879** (0.837)	1.779** (0.855)	-0.880 (0.869)	0.519 (1.115)	0.399 (1.182)	-1.922 (1.189)	1.083 (1.152)	1.124 (1.213)	1.193 (1.298)
%urban population _{t-1}	-0.091*** (0.034)	-0.084** (0.034)	-0.181 (0.263)	0.002 (0.050)	0.005 (0.051)	0.402** (0.184)	-0.071 (0.053)	-0.066 (0.055)	-0.567** (0.263)
D. GDP per capita _t	-0.003* (0.002)	-0.003* (0.002)	-0.004** (0.002)	-0.003** (0.001)	-0.003* (0.002)	-0.002 (0.002)	-0.000 (0.001)	-0.001 (0.001)	-0.002 (0.001)
GDP per capita _{t-1}	0.000 (0.000)	0.000 (0.000)	-0.001 (0.001)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.001)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.001)
D.GDPpercapita growth _{t-2}	0.042 (0.062)	0.051 (0.074)	-0.012 (0.073)	0.118** (0.059)	0.103 (0.066)	0.054 (0.059)	-0.061 (0.052)	-0.033 (0.063)	-0.047 (0.056)
GDPpercapita growth _{t-3}	0.008 (0.059)	-0.033 (0.079)	-0.129 (0.083)	0.087 (0.057)	0.044 (0.065)	-0.024 (0.068)	-0.063 (0.052)	-0.055 (0.065)	-0.082 (0.062)
D. Trade Openness _t	-0.070* (0.040)	-0.112** (0.046)	-0.079* (0.046)	-0.163*** (0.052)	-0.209*** (0.056)	-0.173*** (0.052)	0.098* (0.058)	0.100 (0.064)	0.093 (0.058)
Trade Openness _{t-1}	-0.045** (0.020)	-0.040* (0.021)	-0.089*** (0.029)	-0.083*** (0.021)	-0.078*** (0.021)	-0.107*** (0.035)	0.044*** (0.016)	0.044*** (0.016)	0.028 (0.032)
D. Financial Openness Index _t	0.572 (0.409)	0.577 (0.444)	0.792* (0.434)	0.916*** (0.294)	1.030*** (0.300)	1.226*** (0.290)	-0.367 (0.385)	-0.460 (0.409)	-0.459 (0.423)
Financial Openness Index _{t-1}	0.382*** (0.131)	0.228 (0.147)	0.330 (0.261)	0.226*** (0.086)	0.045 (0.094)	0.576*** (0.184)	0.089 (0.105)	0.116 (0.113)	-0.293 (0.226)
Democracy _t	0.001 (0.459)	-0.581 (0.554)	0.737 (0.822)	-1.153*** (0.447)	-1.812*** (0.624)	-1.825** (0.777)	1.189*** (0.389)	1.248** (0.609)	2.432*** (0.911)
Constant	-39.198*** (6.657)	-31.490*** (7.747)	46.575* (24.050)	-29.341*** (6.998)	-20.174*** (6.888)	-10.853 (21.262)	-3.607 (6.159)	-5.143 (7.588)	53.956** (27.454)
Observations	337	337	337	337	337	337	337	337	337
R-squared	0.14	0.20	0.34	0.14	0.24	0.37	0.13	0.18	0.33

Notes: 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%. All variables preceded by "D." are measured in first-differences.

Table 6A.3. Results of Error Correction Model (ECM) Estimations for the Effect of Capital Flows and Democracy on Social Spending

Dependent Variable	ΔWelfare/Budget			ΔSocial Security/Budget			ΔHealth and Education/Budget		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	ECM case	ECM with year fixed effects (f.e.)	ECM with year and country f.e.	ECM case	ECM with year f.e.	ECM with year and country f.e.	ECM case	ECM with year f.e.	ECM with year and country f.e.
Lagged Dependent Variable _{t-1}	-0.260*** (0.032)	-0.261*** (0.034)	-0.572*** (0.055)	-0.227*** (0.035)	-0.227*** (0.035)	-0.510*** (0.048)	-0.197*** (0.060)	-0.187*** (0.060)	-0.491*** (0.104)
D. % of population aged 65 and over _t	-6.341 (6.263)	-14.103** (7.017)	-16.125 (17.047)	-5.276 (6.317)	-11.888* (7.063)	-20.908 (14.508)	-0.606 (6.775)	-1.319 (7.092)	6.752 (18.057)
% of population aged 65 and over _{t-1}	0.635*** (0.231)	0.690*** (0.247)	-0.933 (1.400)	0.921*** (0.203)	0.953*** (0.193)	1.021 (1.325)	-0.325* (0.167)	-0.291* (0.156)	-1.841 (1.449)
D. % of population aged 15 to 64 _t	3.433* (2.037)	3.375 (2.183)	-1.402 (3.048)	2.237 (1.672)	2.241 (1.625)	0.595 (2.110)	0.759 (2.235)	0.648 (2.235)	-1.650 (2.461)
% of population aged 15 to 64 _{t-1}	0.898*** (0.157)	0.846*** (0.186)	0.171 (0.236)	0.624*** (0.181)	0.595*** (0.193)	-0.118 (0.183)	0.144 (0.200)	0.104 (0.243)	0.248 (0.227)
D. %urban population _t	1.615* (0.969)	1.539 (1.005)	-1.424 (1.177)	0.333 (1.213)	0.228 (1.337)	-2.653* (1.358)	0.991 (1.324)	1.000 (1.413)	1.490 (1.452)
%urban population _{t-1}	-0.122*** (0.034)	-0.113*** (0.036)	-0.061 (0.329)	-0.029 (0.058)	-0.023 (0.061)	0.480** (0.221)	-0.065 (0.064)	-0.056 (0.069)	-0.530* (0.321)
D. GDP per capita _t	-0.003* (0.002)	-0.004** (0.002)	-0.003* (0.002)	-0.003* (0.002)	-0.003* (0.002)	-0.002 (0.002)	-0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)
GDP per capita _{t-1}	0.001** (0.000)	0.001** (0.000)	-0.000 (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.001)	0.000 (0.000)	0.000 (0.000)	-0.000 (0.001)
D.GDPpercapita growth _{t-2}	0.040 (0.066)	0.044 (0.077)	-0.012 (0.074)	0.115* (0.063)	0.102 (0.069)	0.058 (0.061)	-0.060 (0.060)	-0.036 (0.075)	-0.047 (0.070)
GDPpercapita growth _{t-3}	0.013 (0.064)	-0.044 (0.082)	-0.113 (0.084)	0.084 (0.067)	0.044 (0.073)	0.001 (0.068)	-0.052 (0.063)	-0.057 (0.078)	-0.086 (0.074)
D. Trade Openness _t	-0.048 (0.040)	-0.105** (0.047)	-0.072 (0.053)	-0.153** (0.063)	-0.208*** (0.068)	-0.167*** (0.060)	0.106 (0.070)	0.104 (0.079)	0.093 (0.070)
Trade Openness _{t-1}	-0.040* (0.023)	-0.042* (0.025)	-0.093*** (0.030)	-0.093*** (0.023)	-0.097*** (0.023)	-0.113*** (0.038)	0.052** (0.022)	0.052** (0.022)	0.032 (0.043)
D. Gross Capital Flows _t	0.112 (0.110)	0.051 (0.122)	-0.208 (0.142)	0.221*** (0.083)	0.134 (0.103)	-0.008 (0.085)	-0.124 (0.098)	-0.095 (0.123)	-0.180 (0.120)
Capital Flows _{t-1}	0.169* (0.094)	0.062 (0.117)	-0.445*** (0.136)	0.174** (0.075)	0.048 (0.112)	-0.217* (0.112)	-0.026 (0.090)	-0.010 (0.141)	-0.191 (0.149)
Democracy _t	-0.017 (0.378)	-0.645 (0.500)	1.408 (0.926)	-1.198** (0.502)	-1.723** (0.743)	-1.358 (0.956)	1.156*** (0.391)	1.035 (0.713)	2.550** (1.136)
Constant	-35.082*** (7.357)	-30.562*** (9.310)	23.014 (24.026)	-28.782*** (7.537)	-24.150*** (8.239)	-24.114 (17.616)	-2.080 (8.991)	-1.572 (11.247)	43.529* (25.257)
Observations	334	334	334	334	334	334	334	334	334
R-squared	0.14	0.20	0.34	0.15	0.24	0.37	0.13	0.18	0.33

Notes: 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%. All variables preceded by "D." are measured in first-differences.

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