ESSAYS ON THE POLITICAL ECONOMY OF FISCAL AND MONETARY POLICY

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DOCTOR OF PHILOSOPHY

Cameron A. Shelton
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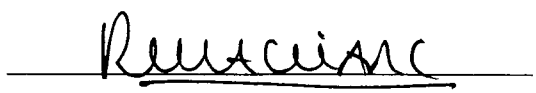

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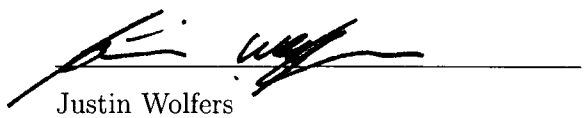

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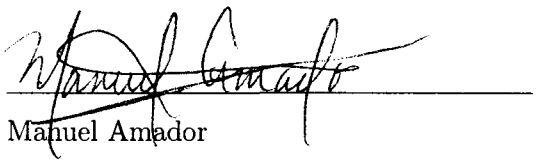

Manuel Amador

Approved for the University Committee on Graduate Studies.

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Introduction

While my own interests encompass most fields of economics, I have always been particularly interested in the role of institutions in shaping the incentives and expectations of market participants and the outcomes of public choice. Political economy is one of the most active fields in economics and draws interest from scholars and practitioners in a wide range of other fields as well. By political economy I mean the implications of politics for economic outcomes. In these chapters, the economic outcomes are macroeconomic indicators such as output growth, unemployment, inflation, and interest rates; and the pattern of government expenditures at different levels of government and on different categories. The political structures considered include the timing of elections, the ability of parties to commit to future policy, the electoral system. Patterns in the distribution of citizens' primitive preferences also play an important explanatory role. One may think of a political system as a machine for transforming citizens' preferences into public choice. Variation in the machine's output can be due to variation in either the input, or to a change in the structure of the machine.

My three chapters represent three distinct contributions to the political economy of fiscal and monetary policy. In chapter one I examine the effect of elections, as surprise changes in aggregate demand policy, on macroeconomic forecasts. In chapter two I examine patterns of government expenditure across various categories of expenditure and levels of government to study the response of government expenditure to shifts in demand for public spending. Chapter three investigates to what extent redistributive government spending is directed to garner votes. In each case, my goal is a novel reduced-form test with the power to distinguish between competing theories where such distinction has, in the past, been difficult. The scope of the studies varies by chapter: my sample for chapter one is a set of twelve advanced economies, for chapter

two it encompasses over one hundred countries, and for chapter three I focus solely on the United States.

Chapter One: Election Surprise and the Economy, is first of all a test of the rational partisan theory (RPT) of political business cycles. The theory attempts to explain, within a model based on rational expectations, the observation that electing left-wing governments leads to a post-election boom while electing right-wing governments leads to a post-election slowdown. As I argue to open the chapter, previous tests of RPT actually have remarkably little power to distinguish between it and its predecessors based on nave expectations. By looking at the direct response of expectations to election surprise, this chapter establishes that the resolution of uncertainty over future aggregate demand policy which takes place on election day can result in large movements in the forecasts of large corporations. In the process, I discover a great deal of country-level heterogeneity. Through further investigation, I reveal that the nature of political competition can explain a lot of the country-level heterogeneity. In other words, some political systems are more susceptible to politically induced economic volatility than others. I also discover correlation between the degree to which forecasts respond to partisan surprise and the national brand of capitalism. Corporatist economies like Germany, where firms tend to coordinate activities via non-market mechanisms, tend to be less responsive to changes in aggregate demand policy than market economies like the UK. This suggests that the brand of capitalism—institutions of wage bargaining, corporate governance, vocational training, and worker monitoring—may actually influence the effectiveness of fiscal and monetary policy.

Chapter Two: The Size and Composition of Government Expenditure, is a collective test of the leading theories of the government size literature. Theories in this literature are usually formulated to explain a broad correlation between total government expenditures and a particular variable such as openness to trade or a measure of income inequality. In this chapter I estimate all coefficients in complete specification so as to avoid omitted variables bias. More importantly, I use government expenditure data on individual categories and various levels of government to formulate nuanced tests of these theories. Looking at a more detailed pattern of correlations allows me to push the theories more seriously and delivers new results on the effects on the pattern of government expenditure of openness to trade, per capita income,

income inequality, and political freedom, results which challenge our understanding of the determinants of government expenditure.

Chapter Three: Tactical vs. Ideological Redistribution, takes a look at taxes and transfers in the United States. In contrast to the previous chapters, this one looks at household level data. The question is how much the net transfer position of households depends on their political importance. Candidates competing for votes may seek to promise favorable tax and transfer policies to certain groups in order to either convert wavering voters or ensure that their own supporters turn out to vote. Using data on voting behavior, I measure the political importance of a voter as a function of the marginal benefit, in expected votes, to the party that directs taxes and transfers to that voter. This analysis reveals that parties are far better served by focusing on turning out their own supporters rather than converting the opposition. The second stage shows that while net transfers to politically important households are nontrivial, they are a small part of the action, swamped by the effects of family income.

These chapters are almost entirely empirical. I have a deep appreciation for elegant mathematical models and I think, at least partly because of my background in physics and my propensity to drag in Greek symbols, the faculty expected me to be a theorist. But my early attempts failed largely because they weren't sufficiently well motivated: they were models in search of a phenomenon. So I decided that I needed to understand the motion of the machine before explaining how it works. I got my hands dirty and I have not, as yet, washed them off to go back to the drawing board. There will be time enough later. For similar reasons, I have attempted to keep my analysis positive rather than normative. Political systems rarely permit first best solutions and indeed are often not characterized by a single metric over which efficiency can be defined. The first goal of the political economist must be to understand the system and its attendant tradeoffs before attempting to define or import criteria for normative analysis. These chapters go some way toward explaining the mechanisms by which preferences and the institutions which channel them affect fiscal and monetary policy. They give answers to several existing questions, provide a wealth of empirical results, and open up interesting avenues for further research.

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Chapter 1

Electoral Surprise and the Economy

Traditional tests of the partisan political business cycle lack power because they are based on the duration of the cycle and do not allow economists to distinguish between theories based on adaptive and rational expectations. However, the rational partisan theory predicts strong and robust effects on expectations which sharply differentiate it from theories based on adaptive expectations. Using a panel of economic forecasts for 11 OECD countries and newly assembled opinion poll data, I examine the relationship between electoral surprise and economic forecasts, directly measuring the central mechanism of rational partisan theory. I find that the rational partisan mechanism is present and important in a sub-sample of the countries. There is a great deal of country-level variation in the strength of the cycle which can be explained by the nature of party competition and institutions of economic and political governance. Countries featuring clear and stable partisan divides over economic policy exhibit stronger responses to electoral surprise and statist economies are more vulnerable to partisan political cycles than market-based economies. Finally, the cycle in real variables is stronger than that in price variables suggesting independent central banks mitigate the partisan political business cycle.

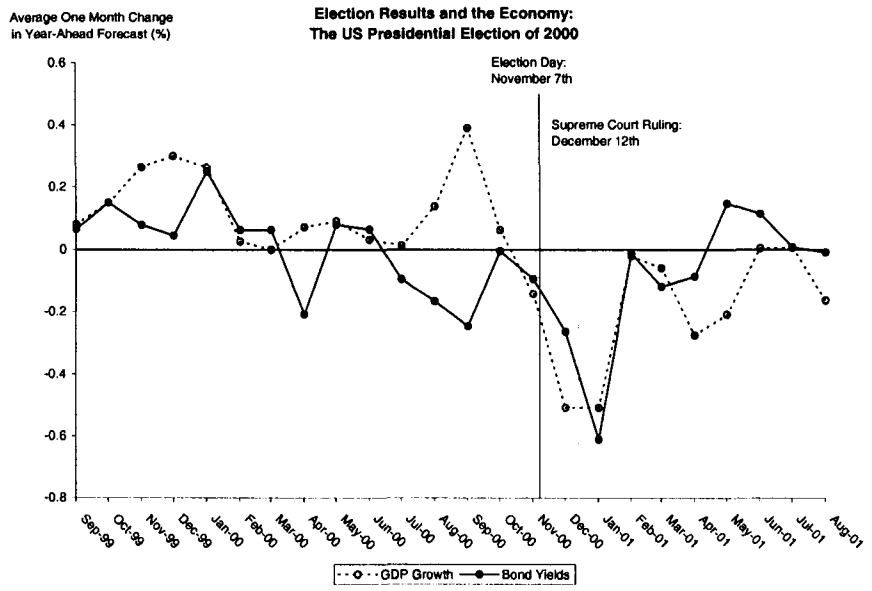
1.1 Introduction

The National Election Study conducted over the two months prior to the US Presidential election of November 7th, 2000 showed Democratic candidate Al Gore with a nine point lead over Republican candidate George W. Bush among intended voters. However, election day proved the contest was essentially a dead heat and, due to ballot recounts in Florida, did not produce a victor. For 36 days the identity of the victor was in doubt until, late in the night on December 12th, the US Supreme Court ruled to halt the vote recounts resulting in a victory for Bush on the morning of December 13th.

During this period from early September 2000 through late December 2000 as the election slipped from Gore to Bush, projections of US GDP growth in 2001 from the Consensus Economics panel of forecasters show a major shift in expectations. As election day revealed a virtual tie rather than the expected Gore victory, the average forecast of 2001 calendar year GDP growth dropped half a percentage point. Upon resolution of the standoff in Bush's favor by the Supreme Court, the average of GDP growth forecasts dropped another half point. Expected bond yields also plunged, witnessing a one-time drop of six tenths of a point in the month following the Supreme Court ruling (see figure 1.1). This episode points to an intriguing connection between the information revealed during elections and the subsequent performance of the economy.

I examine the link between election outcomes and subsequent economic expectations in industrialized countries. There are three main contributions. First, the use of expectations data to test partisan political business cycle theories is a major innovation with significantly increased power to distinguish between alternate theories. I find strong support for the rational partisan theory (Alesina 1987). Second, I introduce two data sets: a commercially available data set on expectations which is new to this literature and a set of pre-electoral opinion polls measuring voter-intent which I have assembled for this study. Finally, I investigate the institutional basis of the rational partisan theory, characterizing how economic and political institutions amplify or dampen the partisan political business cycle. I find significant cross-country variation in the magnitude of the cycle, much of which can be explained by the nature of party competition, the character of electoral institutions, and the national brand

Figure 1.1: Reactions to the Disputed US Presidential Election of 2000



of capitalism.

Political business cycle theories seek to explain that portion of cyclic behavior in macroeconomic variables which is related to the timing, characteristics, and outcomes of elections. The goal is to elucidate the mechanism by which electoral politics introduces additional fluctuations in the economy and to answer questions such as "Which electoral systems are prone to produce economic volatility?" and "Which institutions of government mitigate (or exacerbate) this volatility?" Understanding the relationship between institutions and political business cycles is important both positively and normatively. Documenting the effect of institutions on political cycles helps illuminate the mechanism generating these cycles while an understanding of the generating mechanism informs the design of electoral systems and institutions of government.

The partisan political business cycle is a collection of facts concerning the relation between election results and post-electoral economic performance. In his seminal article and subsequent book, Hibbs (1977, 1987) presents evidence of partisan effects on output, unemployment, and inflation in a dozen industrialized democracies. Left-wing administrations preside over periods of higher output growth, lower unemployment,

and higher inflation compared to their right-wing counterparts. Further work has shown these partisan effects tend to be temporary, disappearing within a year or two of the election (Alesina 1988, Alesina-Sachs 1988, Alesina-Rosenthal 1995, Alesina-Roubini-Cohen 1997). There is also evidence that the magnitude of these partisan effects is related to the degree to which the election outcome was a surprise (Cohen 1993, Alesina-Roubini-Cohen 1997).

There exist two main theories to explain these facts: the traditional partisan theory (Hibbs 1977) and the rational partisan theory (Alesina 1987). The traditional partisan theory (PT) relies on adaptive inflation expectations to generate a relatively stable short-run Phillips curve. The partisan policy-maker then chooses his party's preferred point on the Phillips curve. The result is partisan differences in economic performance the duration of which is governed by the speed with which expectations adjust.

The rational partisan theory (RPT) is based on an expectations-augmented Phillips curve derived from a simple wage-contract framework. Output growth, y_t, is assumed to be inversely related to the growth in real wages, $w_t - \pi_t$. Lower case letters indicate logarithmic growth rates.

$$y_t = \bar{y} - [w_t - \pi_t] \tag{1.1}$$

In equilibrium, the growth of nominal wages, w_t, is set equal to the inflation rate, π_t, to clear the competitive labor market.[1] However, it is assumed that wage contracts must be negotiated before actual inflation is revealed so it is to expected inflation that nominal wage growth is equated as unions and employers attempt to keep real wages consistent with full employment. Thus

$$w_t = \pi_t^e \tag{1.2}$$

Inflation expectations are rational and thus only unexpected aggregate demand shocks affect output. An election serves as such a shock. On the eve of an election, it is uncertain which party will be in power next year. As a result, rational inflation expectations are an average of the preferred inflation policies of the parties weighted by the probabilities of each party being elected. When the election takes place,

[1] $\bar{y}$ is the natural rate of output growth.

there is a sudden resolution of this uncertainty as a winner is produced and the party (and associated economic policy) in power next year is identified. But wage contracts are fixed in the short term and can adjust to this change only with a lag. Thus in the period immediately after the new administration takes office, there is a gap between nominal wage growth and the new inflation policy—in effect a surprise inflation or deflation—leading to an expansion or contraction in the economy. This delivers partisan differences in economic performance which endure until new wage and price contracts are signed.

Alesina, Roubini, and Cohen (1997) attempt to differentiate between the traditional and the rational partisan theories by looking at the duration of these partisan effects. They claim that while RPT predicts temporary effects, the traditional theory results in permanent partisan differences over the entire term of office. In a panel of 18 OECD democracies covering 1960-1993, they find partisan differences in growth and unemployment are confined to the first year or two after the election while partisan differences in inflation persist throughout the entire term of government. While this work is valuable in documenting the behavior of the partisan political business cycle, it does not satisfactorily distinguish between PT and RPT.

Essentially, *both* theories predict transitory effects in output and unemployment, albeit with different mechanisms governing the duration of the partisan effect. Alesina-Roubini-Cohen (ARC) indicate that partisan effects decay within two years. They contend this is consistent with wage-contracts with an average length of 1-2 years. However, it is also consistent with adaptive expectations and sufficiently rapid adjustment of expectations. The parameter values are plausible either way so it is difficult to convincingly reject either theory with a measurement of the cycle's length.

The persistence of macroeconomic variables poses another difficulty for tests based on the duration of partisan effects by adding a layer between observation and theoretical interpretation. For example, unemployment displays strong hysteresis; the removal of a shock to unemployment doesn't necessarily result in the return of unemployment to its previous level (Blanchard 2000). While much of the literature on hysteresis in unemployment focuses on raw materials prices and labor productivity growth shocks, the concept is relevant to changes in policy. The effects of a shock to unemployment delivered by a partisan shift in policy may last longer than it takes for the wages and prices to readjust because of the need to replace skills that have

decayed during unemployment and lost job-specific capital that was destroyed. If the duration of partisan effects is not a simple function of wage and price adjustment, then tests based on the length of the cycle have low power.

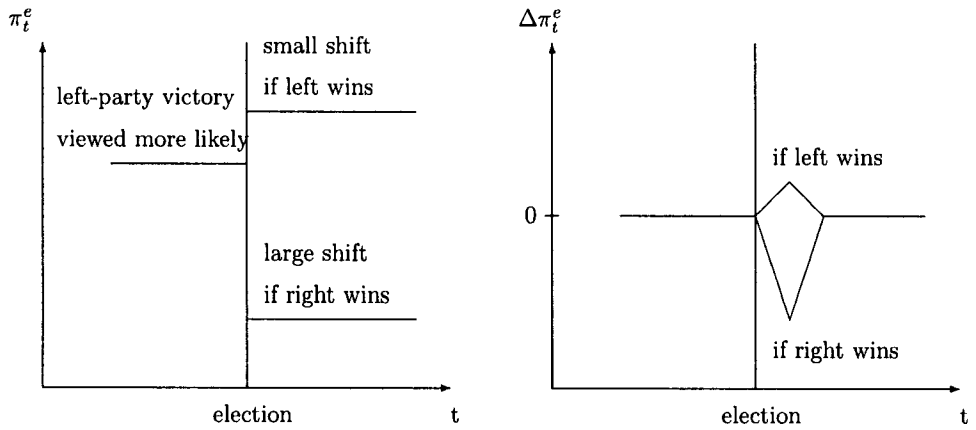
Since PT and RPT are both formulated to explain observed partisan political business cycles, any test based on the cycle itself is likely to suffer from low power. Luckily, the mechanisms of these two theories imply very different behavior of expectations. Under RPT, the resolution of electoral uncertainty acts as an unexpected shock to aggregate demand: it is the information revealed on election day that drives the partisan political business cycle. In PT, by contrast, information revealed on election day goes unremarked: expectations adjust only gradually as the policy of the new administration is revealed through the performance of the economy. These two sources of expectations changes operate on very different time scales. Thus to test RPT, we ought to look at the behavior of expectations in the neighborhood of the election. Under RPT, an election should trigger a sharp change in expectations. Under PT it will not.

Furthermore, under RPT the magnitude of the change in expectations will depend on the degree to which the election results are a surprise. If it is highly likely that the left will win, then the left's platform will already be incorporated into expectations, the left's actual victory on election day will carry very little information, and consequently, expectations will react only a little to this event. On the other hand, should the right emerge as the victor, the surprise is much greater, expectations will have much further to adjust, and thus the change in expectations will be much greater (see figure 2).

I test RPT by measuring the change in expectations across an election to determine whether (a) the direction in which expectations shift depends on the identity of the victorious party and (b) the magnitude of the shift depends on the degree to which that party's victory was unexpected. The degree to which the election result is a surprise is quantified by comparing pre-election opinion polls to election results. To my knowledge, this is the first study which has attempted to test partisan theories using expectations data. It is also one of a small number of studies which controls for the magnitude of election surprise.[2]

[2]Earlier studies using poll data include Chappell-Keech (1988), ARC (1997), Heckelman (2002), and Berleman-Markwardt (2003). In their chapter 5, ARC use option-pricing techniques to convert

Figure 1.2: The Behavior of Expected Inflation Under RPT



Pre-election inflation expectations are a weighted average of the inflation levels that would prevail under the alternate policies of the left and right. The weights are the relative probabilities of election. Once election day results are revealed, expectations change discontinuously to match the new policy. The more surprising the election result, the further the forecasts must shift. Similarly, the greater the distance between the policies of the left and right, the larger the shift.

I focus attention on one particular mechanism linking electoral outcomes to the macro-economy. Uncertainty over election outcomes (and therefore over future aggregate demand policy), when resolved by an election, results in sharp movements in expectations which translate into macroeconomic fluctuations (Alesina 1987). I have obtained a panel of data detailing forecasts of several macroeconomic variables for 11 OECD countries from 1989-2004. I have also collected opinion poll data for the 36 elections in the countries and years covered by the forecast data. Matching these unique data sets, I regress the post-electoral change in economic forecasts on the election results. Then, after establishing that the strength of the response of expectations varies by country, I show that controlling for institutional characteristics can explain a great deal of this variation.

I find that election surprise does have strong partisan effects on economic forecasts but only in those countries with a certain institutional structure. In particular, countries where the political left and right are stable and defined by economic policy rather

pre-election poll data for the US into election probabilities giving a more sophisticated measure of surprise. But in the rest of their book, they concentrate only on elections resulting in a change in government partisanship (left to right or vice versa).

than other political issues exhibit stronger cycles. Statist economies display stronger cycles than market-based economies. And the United States displays a stronger cycle than the rest of the sample, even after controlling for other affects.

1.2 Method

1.2.1 Specification

The purpose of the study is to estimate that discontinuous jump in expectations which the RPT purports will result from electoral surprise.

Structurally we can derive the form of the regression equation and predictions on its coefficients by looking at the data generating processes resulting from the macroeconomic theory behind the RPT. The basic model underlying the RPT is Barro-Gordon (1983a,b). Consider the equilibrium levels of inflation (π^*) and unemployment (U^*) under this framework.[3]

$$\begin{aligned} U_t &= -(\pi_t - \pi_t^e) + \epsilon_t \\ \pi_t^e &= \tilde{\pi} - \frac{1}{\theta}\tilde{U} \\ \pi_t^* &= \tilde{\pi} - \frac{1}{\theta}\tilde{U} + \frac{1}{1+\theta}\epsilon_t \\ U^* &= \frac{\theta}{1+\theta}\epsilon_t \\ \pi_t^e &= E_{t-1}[\pi_t] \end{aligned}$$

Where $(\tilde{\pi}, \tilde{U})$ is the government's ideal point in Phillips-curve space so that policy is characterized by the triple $(\tilde{\pi}, \tilde{U}, \theta)$, and $U = 0$ can be considered the long run natural rate of unemployment.[4] The demand shocks ϵ_t are assumed i.i.d. with mean zero and variance σ^2.

[3] for an excellent summary of the framework and derivation of these results, see Drazen (2000a).

[4] θ is the parameter signifying the relative weight of inflation in the government's loss function (larger θ implies the government cares more about inflation relative to unemployment). The Barro-Gordon loss function is $\mathcal{L}_t = (U_t - \tilde{U})^2 + \theta(\pi_t - \tilde{\pi})^2$. Alesina (1987) uses essentially a specific parameterization of this loss function.

In the absence of a shift in policy, expectations remain constant.[5]

$$\begin{aligned} \Delta\pi_t^e &= 0 \\ \Delta U_t^e &= 0 \end{aligned}$$

But if policy shifts from $(\tilde{\pi}, \tilde{U}, \theta)$ to $(\tilde{\pi}', \tilde{U}', \theta')$ then we get a change in expectations

$$\Delta\pi_t^e = [\tilde{\pi} - \tilde{\pi}'] - [\frac{\tilde{U}}{\theta} - \frac{\tilde{U}'}{\theta'}] \quad (1.3)$$

$$\Delta U_t^e = 0 \quad (1.4)$$

If the policy shift is anticipated, this change will occur in step with the change in policy. If the policy shift is unanticipated, expectations will shift with a lag, during which the real economy will react according to equation (1.3).

If we characterize differences in the political left and right as involving at least one of the following: (i) $\tilde{U}^L < \tilde{U}^R \leq 0$, (ii) $\tilde{\pi}^L > \tilde{\pi}^R$, (iii) $\theta^L < \theta^R$, then we can see from equation (1.3) that inflation expectations will increase (decrease) when policy shifts toward the left (right).

Due to the time-inconsistency fundamental to the Barro-Gordon framework, we will get a permanent shift in realized inflation whether or not the policy change is anticipated. But realized unemployment will be affected only to the extent that the change is unanticipated and only so long as expectations remain unadjusted. Hence the change to policy is reflected in inflation forecasts but never alters unemployment forecasts. By the time forecasters register that the surprise change in policy has an effect on unemployment, it is too late: the effect is temporary, will not last until next period, and thus does not enter into their forecasts made the following period.

While the theory does not extend to the behavior of other macroeconomic variables, we may expect that real variables such as output will follow the pattern for

[5]While the rational representative agent does not specifically formulate expectations over unemployment (or output) in the model, we can add such expectations formation without affecting the model since unemployment expectations do not feed back into the economy as inflation expectations do. Thus, simply imagine that in addition to forming expectations over inflation, which are economically important, the representative agent also forms rational expectations over unemployment, with no effect on the economy.

unemployment and prices such as interest rates and bond yields will follow the pattern for inflation. Thus upon running equation (1.5) for a particular dependent variable, we would expect γ to take the sign specified by the 'strict' interpretation column of table 1.1. However, in all likelihood, the lack of movement in real variables depends on theoretical assumptions. Due to the demonstrated persistence of unemployment and other macroeconomic variables, it is likely there will be an effect on real variables. The 'lenient' interpretation column of table 1.1 summarizes the expected sign of the coefficient under persistence.

The preceding derivation implies a rather sparse specification in which change in expectations is regressed on an indicator of policy change. Clearly in this chapter, where the focus is on elections as the generators of economic cycles, the indicator would be a measure of election day results. However, we should also want to include on the RHS any other factors which agents use in forecasting. In a highly persistent world, rational agents would do well to use past values of a variable as predictors of its future value, adjusted by whatever shocks they observe. Thus I have included the recent history of the variable in question on the RHS along with the election-day shock that is the central feature of the regression. This is important to ensure that a forecaster's tracking of the underlying trend in a variable is not mistaken for a response to election surprise. So the basic specification for this study takes the form

$$\Delta\xi^{e}_{f,t} = \alpha + \sum_{j=1}^{3} \beta_j \left[\xi_{c,t-3(j-1)} - \xi_{c,t-3j}\right] + \gamma SURPRISE_{c,t} + \epsilon_{i,t} \tag{1.5}$$

$$\Delta\xi^{e}_{f,t} \equiv \xi^{e}_{f,t+o+w} - \xi^{e}_{f,t+o} \tag{1.6}$$

Where ξ is one of the macroeconomic variables for which panelists record forecasts. I look at seven of these variables: GDP growth, CPI inflation, 90 day interest rates, 10 year bond yields, the level of unemployment, growth in household consumption, and growth in business investment. The first subscript, f, refers to the forecaster which is the cross-sectional unit of analysis. In cases where the variable is constant across all forecasters within the same country, this subscript has been changed to c to remind the reader (and the writer) of this fact. The second subscript refers to the time dimension of the panel and is indexed monthly. All data is monthly except for GDP growth data, which is quarterly (and therefore only changes every third period).

The dependent variable is the change in expectations over a period of time called the measurement window. The measurement window is defined by indices w and o which control it around the election date: w is the width of the window and o, the offset, conrtols the position of the window relative to the election. This is discussed in more detail in section 1.2.2. The form of the error terms and the method of estimation is discussed in section 1.2.3.

The summation on the RHS consists of the changes in the realization of the variable ξ over the three most recent quarters.[6] The centerpiece of the regression is SURPRISE, which is meant to capture the shift in expectations due to election-day results. SURPRISE captures the magnitude and direction of the post-election jump illustrated in the left-hand panel of figure 1.2. The jump in expectations depends on the liberal and conservative alternatives as well as the ex-ante probabilities assigned to victory by either side.

The first step in the construction of SURPRISE is to classify, using Banks' Political Handbook of the World, the final government produced by each election on a five-point scale from left to right

$$GOV = \begin{cases} 0 & : \text{ left} \\ 0.25 & : \text{ center-left} \\ 0.5 & : \text{ center} \\ 0.75 & : \text{ center-right} \\ 1 & : \text{ right} \end{cases}$$

This is intended as a crude measure of the degree to which the economic policy of the new government is partisan. Single party governments earn a pure left or right classification while coalitions encompassing parties with differing economic ideologies earn a diluted center-left, center-right, or even dead-center classification.

The next step is to determine which alternative governments the election is to decide between. For the United States, the alternatives are a Democratic president

[6] These are not quarters as defined by the calendar year but rather they are the three most recent three month periods. Thus in May, the three most recent "quarters" cover February-May, November-February, and August-November. For GDP growth, this will actually correspond to the change over the past three calendar quarters because data is quarterly. But as the other series are monthly, this allows for the effects of the most recent past data.

(GOV=0) or a Republican president (GOV=1). Similarly, for Britain the alternatives are a Labor government (GOV=0) or a Conservative government (GOV=1). But for Japan, the important electoral question is not whether the LDP heads the government, but whether the LDP must form a coalition government which dilutes its policies (GOV=0.75), or whether it can poll a majority (GOV=1). Similarly, in the Canadian general elections of 1993, 1997, and 2000 support for the Liberal party was so overwhelming that the important electoral margin was not between pure left and pure right, but between pure left (GOV=0) and center-left (GOV=0.25). Finally, in the Dutch elections (1998, 2002, and 2003), three parties of roughly equal size implied that neither left nor right would obtain a majority. Thus in this case the policy alternatives were center-left (GOV=0.25) and center-right (GOV=0.75). Excepting these three countries, the alternatives are taken to be left-wing majority (GOV=0) and right-wing majority (GOV=1).

Having established the likely alternatives, the task is to assign ex-ante probabilities to these alternatives, or rather, to the probability that the more conservative alternative is realized on election day. This probability is generated using pre-electoral opinion poll data. Think of an opinion poll as a repeated draw from a trinomial distribution (voter prefers party R, party L, or some other party) with unknown parameters q_r and q_l which indicate the probability a given respondent prefers party R or party L. If v^i denotes the vote-share party i receives in pre-electoral opinion polls and N is the size of the poll, then for large N[7], the difference $v_i - v_j$ is normally distributed with standard error

$$\sigma_{ij} = \sqrt{\left[\frac{v_i(1-v_i)}{N} + \frac{v_j(1-v_j)}{N} + 2\frac{v_i v_j}{N}\right]} \tag{1.7}$$

Then the probability that $q_r > q_l$ and thus the right wing party will win election day given the polling results v_r, v_l is given by

$$P = \Phi\left(\frac{v_r - v_l}{\sigma_{rl}}\right) \tag{1.8}$$

where Φ is the cumulative standard normal distribution.[8]

[7]The opinion polls I use range in size from 1000 to 9000 respondants.

[8]This formulation focuses only on sampling error. I have worked out a more complicated model in which surveys may also mis-estimate the relative turnout between parties. Estimating such a

This method works for the cases where the important margin is competition between two parties. But for those elections (Japan 1990, 1993, 1996, 2000, 2003 and Canada 1993, 1997, 2000) where the electoral uncertainty concerns whether a single party i can win a majority of the seats, the standard error is much simpler:

$$\sigma_i = \sqrt{\frac{v_i'(1 - v_i')}{N}} \tag{1.9}$$

$$P = \Phi\left(\frac{v_i' - 0.5}{\sigma_i}\right) \tag{1.10}$$

The sole complication arises from the disconnect between vote share and seat share. While poll data measures expected vote share, parliamentary governments rely on support from a majority of seats. The well-known phenomenon is that small parties tend to garner a significant fraction of the national vote, but are unable to muster a majority in a corresponding fraction of the districts and so fail to win seats commensurate with their vote share. But simply omitting parties which fail to poll 5% of the vote and renormalizing the remaining parties brings vote share and seat share broadly in line. Thus vote shares v_i' in equations 1.9 and 1.10 have been adjusted by omitting parties supported by less than 5% of the respondents.

The final value of SURPRISE is given by

$$SURPRISE_{ct} = G_{ct} - L_{ct} - (R_{ct} - L_{ct})P_{ct} \tag{1.11}$$

where G_{ct} is the value of the final government on the five point scale following the election in country c at time t, L_{ct} and R_{ct} are the values of the left and right alternatives on that same scale, and P_{ct} is the probability from equation 1.8 or 1.10. Each of the steps in its construction can be viewed in light of figure 1.2. Classifying the alternatives at stake in the election (R_{ct} and L_{ct}) on the five point scale is akin to measuring the distance between the liberal and conservative alternatives. Converting poll data to the expected probability of a conservative victory (P_{ct}) and then scaling by the distance between the two alternatives ($R_{ct} - L_{ct}$) gives a measure of the pre-election expectation. Subtracting this from the resulting government (G_{ct}) thus yields a measure of the degree to which the ex-post election result differed from the ex-ante

model gives broadly similar results.

Table 1.1: Hypotheses

variable name	varible symbol	predicted coefficient on SURPRISE	
		strict	lenient
GDP growth	y	$\gamma = 0$	$\gamma < 0$
Unemployment	u	$\gamma = 0$	$\gamma > 0$
Growth in Household Consumption	c	$\gamma = 0$	$\gamma < 0$
Growth in Business Investment	i	$\gamma = 0$	$\gamma < 0$
Consumer Price Inflation	cp	$\gamma < 0$	$\gamma < 0$
90-day Interest Rates	sr	$\gamma < 0$	$\gamma < 0$
10-year Bond Yields	lr	$\gamma < 0$	$\gamma < 0$

expectation.

1.2.2 The Measurement Window

The measurement window associated with period t identifies the pair of surveys from which $\Delta\xi^e_{f,t}$ is calculated. Index w from equation 1.6 determines the width of the window, the number of months between the two surveys. The offset, o, determines which expectations change is associated with the election. For a given election, index the last survey before election day as survey t. Then the window associated with the results of that election is the one defined by surveys t+o+w and t+o. All other windows of width w enter the regression associated with a period of no election.

I choose w and o to reflect which surveys contain the forecasters' response to the information revealed in the election. Ideally, the measurement window ought to be as narrow as possible so as to isolate the effect of election surprise from other noise. But in practice, it is hard to localize the effect of election surprise to a single month because of two types of variation. The first is variation in when the panelists incorporate election results in their forecasts. This varies by forecaster as some are quicker to respond than others, but it also varies by election. Surveys are evenly spaced, arriving every 4 or 5 weeks but elections don't always take place at the same point between adjacent surveys. Sometimes the next survey comes many weeks after the election, sometimes it comes the very next day. The second type is variation in when the electoral uncertainty is resolved. Is it resolved by the actual vote result, is it delayed due to the process of coalition formation, or is it resolved earlier as voter intent becomes clear prior to the actual vote? The pattern of resolution is unique to an election.

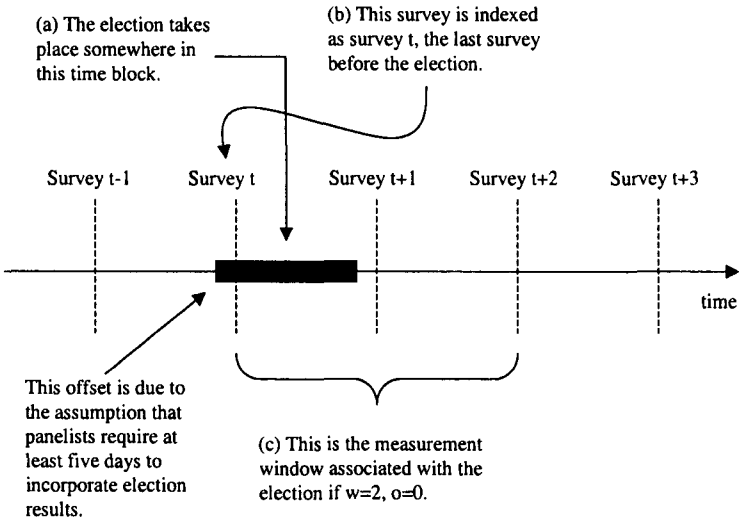


Figure 1.3: The Measurement Window

For the purposes of this study I have assumed panelists require at least five days to respond to the results of an election. The panelists in question are large corporations with formal forecasting departments who respond to Consensus on a monthly basis and whose work is likely used for internal purposes far more frequently. If elections are important and expectations are forward-looking, then it seems likely that such departments would update forecasts with alacrity. This five-day figure becomes important due to the fact that some elections take place almost immediately before the next Consensus survey. If an election falls fewer than five days prior to a survey, that survey is coded as the last survey *before* the election rather than the first survey after the election (see figure 1.2.2).

The second consideration is the fact that electoral surprise may not be delivered only on election day itself but may be partially resolved both before and after the election as well. The projected fortunes of the parties rise and fall in the months prior to the election. Perhaps, in the final months of the campaign, it becomes increasingly clear which party will carry the election with the result that a good portion of the electoral uncertainty is resolved over the previous months and less remains for the election-month pop. However, the revelation of information during the campaign period is comparatively slow, mere wobbles compared to the swoop of election day.

Thus widening the window to include such movements is probably not worth the extra noise. Consequently, I have focused on election day and the immediate aftermath by choosing $o = 0$.

Just as electoral information may be revealed prior to election day, so may it be delayed until after election day if the identity of the government (and thus the impending economic policy) is not fully determined by the voting. The most common example of this is the habitual post-electoral formation of a governing coalition in a parliamentary system when no party has obtained an outright majority of seats in the election. The disputed 2000 US presidential election constitutes another, altogether rarer, form of delay. In each case, the identity of the government and thus of subsequent policy is not conclusively determined until after the formal election date. [9] To account for this I have chosen a two-month measurement window, $w = 2$. This is largely to capture what seem to be significant effects in the second month after the election. This is the case especially in many European countries with proportional representation, leading me to believe that a good part of election uncertainty is resolved by the post-electoral formation of the governing coalition rather than on election day. It is also an attempt to include panelists who take slightly longer to update their forecasts.[10]

Unfortunately, a two-month window generates autocorrelation in the LHS variable. If the LHS variable is the difference in the level of ξ_t across the past two months, but is recorded with monthly frequency, then these windows overlap. What this means is that any particular month-to-month change in ξ_t is recorded in two consecutive values of $\Delta\xi_t$, guaranteeing that $\Delta\xi_t$ exhibits spectacularly high autocorrelation. For this and other reasons explained in the following section, I allow for autocorrelation in my error term.

[9] I have not attempted to itemize the resolution of electoral surprise in parliamentary countries by documenting the date of coalition formation. However, in the case of the disputed election of 2000, I have taken the election date to be the day the US Supreme Court halted the recount (December 12th) rather than the day of the election (November 7th).

[10] I have also run $w = 1, 3$ for robustness. Not surprisingly, enlarging the measurement window results in larger point estimates. It also results in greater significance, to a point, after which the standard errors grow much faster than the coefficient, indicating one is marginally including more noise without much more signal. Since the character of the results is largely unchanged, I have stuck with my original $w = 2$.

1.2.3 Estimation Technique

The next question is how to estimate equation (1.5). At each point in time for each country, we draw a number of different forecasting models (panelists) from the urn and record their forecasts. Having multiple panelists for each country is something like having repeated draws of the same experiment. So we have to think carefully about the error structure when combining across panelists and countries to form a single cross-sectional panel.

The fact that several observations have been made on the same country on the same date raises the possibility of contemporaneous correlation: all the panelists from one country have something in common in their forecasting due to the fact that they are looking at the same country. The trick is to articulate how this translates into deviant behavior in the error covariance matrix. Differences in the frequency with which official data is released and differences in the persistence and volatility of the underlying economic variables ought not result in different forecasting behavior, simply in greater variance of the independent (and presumably also the dependent) variables.[11] But the fact that several panelists are all looking at the same jitters means they'll all change expectations at the same time. And if the jitters are country-specific and not accounted for by the set of explanatory variables, then their econometric errors from equation (1.5) will be correlated.

Imagine a country suffers a shock which is country-specific and of uncertain magnitude. This shock doesn't show up in any of the RHS variables until official data comes in that reflects actual changes. Presumably expectations react more quickly than official data, so there is at least some period during which panelists' reaction to the shock shows up in the error term.[12] And it will show up in the error term for all the panelists in a given country but not for those panelists in other countries. Thus the errors of panelists from the same country are subject to contemporaneous

[11] Most countries in this sample release price and unemployment data weekly and GDP data quarterly.

[12] An excellent example of this is the terrorist attacks of September 11th, 2001. The data show a swift and sizable reaction by panelists predicting severe negative consequences for the US economy. This reaction in expectations was certainly not captured by any of the explanatory variables. And while panelists predicted dips in other countries as well, the movement of expectations was understandably much higher in the US than for any other country. Clearly then, the error terms for the months after September 2001 were large and in the same direction for all US panelists. While 9/11/2001 is exceptional in magnitude, other country-specific events undoubtedly produce similar, if smaller, effects.

correlation.

$$Cov\left[\epsilon_{f,t}, \epsilon_{f',t}\right] \neq 0 \quad \text{for} \quad f, f' \in c \tag{1.12}$$

The shock itself is simply unexplained variation. But the correlation of the error terms may affect my standard errors. In fact, if the errors are positively correlated among forecasters observing the same country (as is expected), then random effects will underestimate the standard errors. What is required then, are panel corrected standard errors which account for covariance in the disturbances across panels, known as clustering by country.

Another possible problem is serial correlation in the errors. Gallo et al present evidence of herding behavior in Consensus data for the US, UK, and Japan for 1993-1996 and suggest that such herding might generate first order serial correlation in forecast errors as forecasters attempt to hit the group mean or follow a leader. I am not concerned with forecast accuracy and forecasting errors in this chapter but herding does imply the possibility of first-order serial correlation in the errors of my basic specification.

The herding discussed by Gallo et al would imply negative serial correlation. Imagine the economy is hit with a shock about which panelists have heterogeneous opinions, resulting in heterogeneous changes to their forecasts. Then, according to the story, before answering the second post-shock survey, the panelists look at their position relative to their peers in the first survey after the shock and adjust toward the mean forecast. This behavior would result in negative autocorrelation of my LHS variable as those who reacted most strongly to the event will then take a step back in the other direction once they have observed their peers. The story remains unchanged if the panelists all follow a particular leading analyst rather than reverting to the mean.[13]

However, visual inspection of the data suggests a different pattern. If one sorts forecasts from bullish to bearish, panelists seem to stake out positions within the array of forecasts and adhere to them for several consecutive surveys rather than

[13]This analysis assumes that a panelist's reaction to the event is uncorrelated with his previous position relative to the panel mean. If, in reaction to positive news, those who were previously pessimists react more strongly than those who were previously optimists, then we might support positive serial correlation of the errors as a result of herding.

herding toward the mean or toward a leading forecaster. This high persistence in the forecasts is probably due to the lengthy forecast horizon and thus the long feedback time. However, this implies autocorrelation in the level, but not necessarily in the first difference of the forecasts. Indeed, I find no evidence of the negative serial correlation in the first difference of the errors that would come from herding. However, since I am using a two-month measurement window, there is strong serial correlation in the dependent variable (see section 1.2.2 for an explanation), so I nonetheless control for serial correlation.

Given these factors, I estimate the model by GLS, specifying an error-covariance matrix which allows for contemporaneous correlation across panelists within the same country (equation 1.12) plus first order serial correlation. Thus the error term is

$$\epsilon_{it} = \rho\epsilon_{i,t-1} + \eta_{it} \tag{1.13}$$

where $|\rho| < 1$ and η_{it} is iid with zero mean and constant variance.

1.3 Two New Data Sets

1.3.1 A Panel of Economic Forecasts

To measure expectations, I employ forecast data from Consensus Economics. The data consist of monthly surveys covering eleven countries—Canada, France, Germany, Italy, Japan, Netherlands, Norway, Spain, Sweden, the UK, and the US—from October 1989 to July 2004.[14] For each country-month, the survey records individual forecasts on a number of macroeconomic variables by a number of panelists, typically commercial and investment banks, large firms, and think tanks. The exact number of panelists, the depth of the survey, varies by country and month. Individual panelists are identified and can be tracked through the data. The duration of their stay in the sample varies by panelist: some panelists are in the sample consistently for years, others regularly miss a few surveys a year, and some simply make a brief one or two month appearance. Table 1.2 summarizes the number of panelists per

[14]Norway was added to the sample in June 1998 while Spain, Netherlands, and Sweden were added in January 1995. Switzerland is part of the survey since June 1998 but has been dropped from this study due to difficulty finding Swiss poll data.

Table 1.2: Number of Panelists per Survey (N), by Country

Country	Obs	Mean	Std.Dev.	Min	Max
Canada	2674	15.1	2.0	11	20
France	3111	16.1	3.6	6	24
Germany	4721	25.4	3.1	12	32
Italy	2308	12.9	2.8	6	21
Japan	3489	15.7	4.0	5	23
Netherlands	1060	9.3	1.6	7	14
Norway	731	10.1	1.4	6	12
Spain	1489	12.3	2.0	7	17
Sweden	1419	12.5	2.2	6	17
UK	5354	30.4	4.7	18	39
USA	4606	25.0	3.4	16	33

Only panelists that have recorded a forecast for consumer price inflation are counted in this table.

country. The variables for which forecasts are formed for all countries include output growth, consumer price inflation, 90-day interest rates, and 10-year bond yields. For the G7, forecasts are also made on the level of unemployment, the growth of private consumption, and the growth of business investment. Producer price inflation, the level of exports and imports, and the trade balance are also widely available while housing starts and auto sales are included for a few countries. Individual panelists do not necessarily deliver forecasts for every variable included in a country survey. Nonetheless, for the seven variables mentioned in the first two lists, which will form the core of this study, the response rate is extremely high.

Two aspects of this data are particularly exciting. First, the high frequency not only boosts the observations but enables one to look at the high frequency responses to events that are likely to characterize expectations. A look at the summary statistics in table 1.3 shows that expectations are often stable but are capable of large changes in a single month. The $\Delta\xi$ variables are the first difference of a panelist's forecasts of ξ. The sample average for these variables is close to zero and the inter-quartile range is usually quite small. However, the standard deviation is large. This suggests that individual panelists adjust infrequently- perhaps every few months on average, but that such adjustments can be large and swift. Although many observations bring no adjustment, this does not necessarily imply infrequent updating of information or rule out rapid responses to events. High frequency data allows a more precise examination of the timing of panelists' responses to events and information.

Table 1.3: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max	Iqr
y	29925	2.44	0.87	-2.2	6.2	1.0
u	24103	7.68	2.75	0.6	14.0	4.5
c	29809	2.30	0.92	-2.5	6.3	1.1
i	29551	4.40	3.13	-14.7	20.6	3.5
cp	29851	2.43	1.27	-1.7	10.4	1.3
sr	28278	5.09	2.65	0.0	15.5	2.1
lr	27744	6.25	2.12	0.3	14.7	3.5
Δy	29181	-0.03	0.33	-4.8	4.3	0.1
Δu	23351	0.01	0.27	-3.0	4.4	0.0
Δc	29036	-0.02	0.35	-4.7	4.6	0.0
Δi	28692	-0.08	1.34	-21.3	17.1	0.2
Δcp	29046	-0.02	0.27	-4.8	3.9	0.1
Δsr	26893	-0.04	0.41	-5.1	5.4	0.3
Δlr	26285	-0.03	0.35	-4.6	3.0	0.3
SURPRISE > 0	9	0.27	0.38	0.02	1.00	
SURPRISE $= 0$	16	0	0	0	0	
SURPRISE < 0	9	-0.25	0.28	-0.96	-0.04	

$\Delta\xi$ refers to the one month change in variable ξ.
IQR refers to the inter-quartile range: the difference between the 25th and 75th percentiles.

Table 1.4: Average Pairwise Correlation of Forecasts Within the Same Country

Country	y	Δy
Canada	0.60	0.30
France	0.66	0.27
Germany	0.65	0.31
Italy	0.67	0.23
Japan	0.51	0.27
Netherlands	0.68	0.19
Norway	0.50	0.19
Spain	0.53	0.21
Sweden	0.44	0.15
UK	0.53	0.25
USA	0.59	0.34

Δy refers to the one month change in y

The other exciting aspect of the data is the existence of multiple panelists per country. If each panelist is endowed with an information set and a forecasting model, then having multiple panelists is like a repeated experiment with multiple draws of information set and forecasting model. This increases the variation in the dataset and increases confidence in the generality of the results so long as the forecasts display independent variation. For each variable, I calculate the average pairwise correlation between the forecasts of two panelists located in the same country. This gives a measure of how much independent variation exists among different panelists' forecasts of the same variable. I then repeat the exercise for the first difference of each variable, which speaks to the cohesion in the sample over the short-run. The average pairwise correlation in output growth forecasts ranges between .44 and .68 which seems quite low. This suggests to me that panelists are endowed with significantly different information sets and/or employ a variety of forecasting models. The average pairwise correlation of the month-to-month change in the forecast of output growth is much lower, ranging from .15 to .34. This indicates that the direction in which one panelist's forecast moves tells very little about the direction in which the forecast of another panelist (in the same country) will move. In other words, panelists don't strongly agree on how the picture has changed from month to month. This is further information that panelists either focus on different sets of information or carry different interpretations of what that information means for future output growth.

Thus the data indicate that panelists bring significantly different information to

the sample, that panelists tend to move in different directions in the short-run, and that panelists tend to adjust every few periods rather than every month. What I will show is that despite this chaotic picture, elections result in strong short-run movements in the sample as a whole.

For each variable, there are two forecast horizons: the current calendar year (current-year) and the following calendar year (year-ahead). This means that the forecast horizon is not actually a fixed distance from the date of the survey, but comes closer as the end of the year approaches and then leaps back again as the new calendar year is reached. For example, in the March 1996 survey, panelists record their forecasts for output growth for the 1996 calendar year and output growth for the 1997 calendar year. In the December 1996 survey, they record forecasts of output growth for the 1996 calendar year (which is all but over) and the 1997 calendar year. Then, in the January 1997 survey, panelists record forecasts for output growth over the 1997 calendar year and the 1998 calendar year. Clearly, as the year passes, the forecast horizons move closer to the survey date. As a result, forecasts for the current calendar year tend to become more tightly clustered toward the end of the year as the bulk of the uncertainty is resolved.[15] This effect is also seen in the year ahead data series, but is much weaker since the entire period with which the forecast is concerned remains in the future.

The central regression in this chapter, equation (1.5), incorporates the first difference of the expectation, $\Delta\xi_t^e$. However, if one were to blindly take the first difference of either the current year or the year ahead data series, one would end up with data displaying spurious seasonality. Figure 1.4 illustrates. Imagine that one is interested in using the year-ahead data to calculate a 2-month measurement window for the variable ξ. Thus $\Delta\xi_t^e = \xi_{t+2}^e - \xi_t^e$. This generates a consistent data series for the months of January through October: in each case the forecast horizon is two months nearer on the far side of the mesurement window than it is on the near side. For example, in April of 1992, we are taking the difference between $\xi_{June1992}^e$, when the forecast horizon is 18 months distant, and $\xi_{April1992}^e$, when the forecast horizon is 20 months distant. However, because the forecast horizon is tied to the calendar year,

[15]For the 90-day interest rate and the 10-year bond yield the forecast horizons are fixed at 3 months and 12 months ahead rather than being tied to the calendar year and thus do note exhibit this effect. For these variables I use the twelve month horizon.

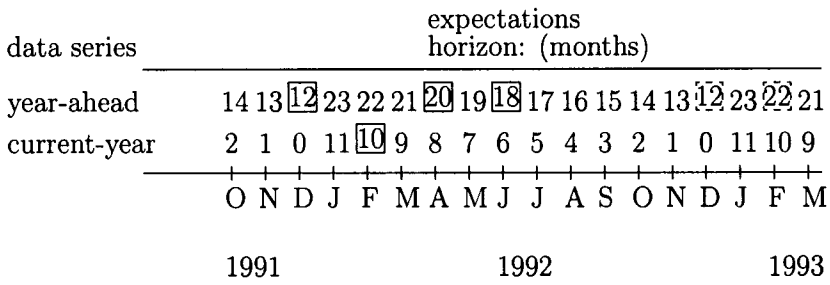


Figure 1.4: Consistent Horizons. Constructing the series $\Delta\xi_t = \xi_{t+2} - \xi_t$ requires both the year-ahead and the current-year data series. Using only the year-ahead works for t between January and October where the forecast horizon changes by two months across the window (middle pair of boxes). But in the last two months of the year, the window stretches across the new year when the horizon jumps forward, creating a mismatch between the two edges of the window (dotted boxes). The solution is to use some of the current-year data to ensure consistent horizons at both edges of the measurement window (left-most pair of boxes).

this causes a problem for December 1992. Now we are differencing a forecast for calendar year 1994 from a forecast for calendar year 1993: this is comparing apples and oranges. So we must use some of the data from the shorter forecast horizon, the current year data, to ensure a smooth data series. February 1992 and December 1991 are shown as an example in figure 1.4. As a result, the forecast horizon on the far edge of the measurement window varies from 10-21 months depending on the time of year, but the difference in horizon between the leading and trailing edges of the measurement window is always 2-months.

1.3.2 Pre-Electoral Poll Data

To measure the extent of the surprise contained in the election result requires a measure of the expected election outcome. The natural place to look is polling data. Hibbs suggested using electoral preference polls over ten years ago in his review article (Hibbs 1992) and several years prior, Chappell and Keech did so in their study of US unemployment (Chappell-Keech 1988). In recent years, interest in the effect of endogenous elections on the partisan theory has led a few authors to pursue time series poll data of party preference for other developed countries. Unfortunately, reliable

time series poll data have proven difficult to acquire. The number of countries in those few studies that utilize poll data is quite small compared to typical cross-country studies of political business cycles which usually feature at least a dozen countries and sometimes as many as one hundred. Heckelman (2002) studies Canada, Germany, and the United Kingdom, chosen "because of the availability of continuous poll data". Berleman and Markwardt (2003) manage to assemble data for six countries (Australia, France, Germany, Sweden, the UK, and the US) from a variety of sources. Because there is no central source of poll data, finding reliable time-series data for a lengthy period covering multiple elections is a challenge which must be revisited for each country in the study with no guarantee of compatibility.

Because I study information revealed in a short period around the election date, I have little need for time series poll data. For my purposes, it is sufficient to measure the level of support for the left and right over the few months prior to the election as a measure of the probability of either side achieving victory while the pre-electoral forecasts are being formed. Comparing the poll data to the actual election result gives a measure of the electoral surprise realized over the measurement window. Thus electoral surprise and the change in expectations are measured across the same window.

While pre-electoral poll data exists for many countries, it has not, to my knowledge, been compiled before. Table 1.10 documents the sources from which I have compiled my pre-electoral polling data. The data are based on in-person or telephone interviews featuring the question: "If a {general, parliamentary, presidential} election were held {tomorrow, Sunday}, which {party, candidate} would you vote for?"[16] The raw data yield the frequency with which each candidate or party garnered support as well as frequencies of respondents who are uncertain. For each party i, I have defined v^i as the percentage of survey respondents who expressed an intention to vote for party i. Many polls repeat the question for those who express uncertainty, asking them "Are you leaning toward any particular party?" I have not counted these separate respondents, focusing solely on voters who profess to have made their decision. To form the final v^r (v^l) I have summed the v^i of all right (left)-wing parties which garner at least 5% of the votes in the election. I use the Mannheim Eurobarometer

[16]The surveys actually use one of a number of closely related questions. I have nested these questions to emphasize their essential compatibility. To extract a particular question from the nested question, simply treat each set of brackets as a list of options from which one entry ought to be chosen, as appropriate for the country in question.

definitions of which parties belong to the left and right.

The data have been collected from three different types of sources: general elections studies, public opinion polls by public opinion agencies, and opinion polls by major newspapers. My preferred source was general elections studies. The goal is to gain a measure of pre-electoral political support at the front edge of the measurement window. Thus I selected the general election study if available and conducted fewer than three months prior to the election. If this was unavailable, I simply chose the most prominent opinion poll available during that period, either from a public opinion agency, or a major newspaper (whose polls are usually conducted by public opinion agencies). All of the polls are scientific and feature at least one thousand respondents selected to represent the national electorate (no regional polls, no respondents ineligible to vote, no internet polls). In two cases (Italy 1994, Spain 1996), I could find no poll data so these have been coded missing. The sources for each election and the dates over which the field work for the polls were done are noted in table Table 1.10. These data are used to produce the SURPRISE variable. Summary statistics for SURPRISE are included in table 1.3. The largest values come from US 2000 and Spain 2004.

1.3.3 Economic and Political Outcomes

Data on election dates and outcomes have been assembled using Banks' Political Handbook of the World plus national election institutes and include the date of the election, the vote shares of and seats allocated to each party, and the ideology of the post-electoral government. The sample consists of 36 elections.[17] Changes of power not associated with elections have been omitted due to the difficulty in obtaining measures of political surprise which are consistent with the surprise contained in the elections.[18] These data are used to produce the POL variable. The sample is well-balanced between the left and right: 17 left-wing governments, 15 right-wing governments, and 2 centrist coalitions. Most governments are given a pure left or

[17]In most countries I have chosen legislative elections for the lower house. In the United States, the only presidential country in the data set, I look at presidential elections. This is consistent with ARC and other authors.

[18]For an attempt to combine both latent and electoral surprise, see Berlemann and Markwardt 2003.

right designation, only 4 out of 36 earn center-left, center-right, or dead center classifications.

Macroeconomic data for the eleven countries have been gathered from the IMF and the EIU. All data is monthly with the exception of data on GDP growth which is quarterly.

1.4 Results

If the RPT is true, we're looking for a pattern like that shown in figure 1.2. The strict interpretation of the RPT means we're looking in equation (1.5) for negative and significant coefficients on inflation and interest rates and zero coefficient on output growth, unemployment, consumption, and investment. A more lenient interpretation allows for negative coefficients on output growth, consumption growth, and investment growth and a positive coefficient on unemployment. The coefficient on SURPRISE indicates the difference between the effect of right-wing and left-wing surprise on the change in the forecast. Thus a negative coefficient means victory of the right wing induces a relatively negative change (and the left wing therefore induces a relatively positive change) in the forecast for that variable.[19]

1.4.1 Initial Results

I've run equation (1.5) for seven different variables: growth rate of output, level of unemployment, change in household consumption, change in business investment, change in consumer prices, level of 90 day interest rates, and level of 10 year bond yield. The results for the entire sample are presented in table 1.5. All of the coefficients align with the basic partisan theory result: the election of right wing governments results in expectation of economic downturn relative to the election of the left. This significant partisan response of forecasts to elections results is strong evidence for the RPT.[20]

[19] Refer to section 1.2.1 for a discussion of these hypotheses.

[20] As mentioned in the Data section above, the original sample in 1989 consists of the G7; the four other countries are added in two waves in January 1995 (Netherlands, Spain, Sweden) and June 1998 (Norway). Since these countries have been added in descending order of GDP, and larger countries tend to have less volatile GDP growth, there is some small danger of bias in the GDP growth regressions due to sample bias. To be sure, I reran the regressions using only data since June

Table 1.5: Initial Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ξ	GDP Growth	Unemp.	HH Cons.	Bus. Invest.	CPI infl.	90-Day Rates	10 Year Yield
SURPRISE	-0.595	0.007	-0.143	-2.069	-0.025	-0.373	-0.214
	[0.142]***	[0.099]	[0.110]	[0.986]*	[0.017]	[0.105]***	[0.069]**
observations	21102	22704	19791	19853	27658	25638	25002

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%

GLS with dependent variable $\Delta\xi_t^e$
Controls: previous three quarters of change in the realized values of the variable plus a constant.

Here is one way of interpreting the magnitude of the coefficients. In a race which is dead even in the polls ($v^r = v^l$), a left-wing victory delivers SURPRISE=-0.5 while a right-wing victory delivers SURPRISE=0.5. Thus the regression coefficients roughly correspond to the difference between left- and right-wing victory in a race that is too close to call. Which means that upon identification of the victor in such a race, we would expect a movement in expectations, $\Delta\xi_t^e$, of roughly half the value of the coefficient. In sum, the regression coefficient represents the partisan difference in forecasts in a very close race, which equals roughly twice the movement we'd expect to see post-election. The data are measured in percentage points, thus the coefficient on output growth corresponds to a partisan difference in expected output growth of .6% for a sample of 11 OECD countries from 1990-2004. For comparison, ARC report a partisan difference in realized output growth of 1.4% for the United States during the period 1949-1994.[21]

While the signs and magnitudes constitute strong evidence for RPT, there are a few questions which arise from these initial results. First, contrary to a strict interpretation of the theory, forecasts of real variables respond strongly to election surprise. Second, the response of inflation is quite weak, casting some doubt on the formal model emphasizing surprise inflation. It is likely that aggregating data across several countries, even OECD countries, conceals significant differences in their experiences. Perhaps the results are being driven by one country or even one election. As a preliminary step, I've rerun the regressions on the eleven restricted samples in which one country has been omitted. Neither this nor visual inspection of the individual elections and panelists suggests any serious outliers at any level— country, election, or panelist. However, these regressions do exhibit a wide variety of point-estimates implying that there is a great deal of country-level heterogeneity in the direction and magnitude of the effect. I will argue in the next section that institutional differences between countries can explain much of why some countries exhibit systematic election-period effects on forecasts while others do not.

The US exhibits the strongest results. Figure 1.5 displays the behavior of forecasts in the neighborhood of an election after removing the estimated effects of the controls

1998, the date at which the sample was full. The results are not markedly different from the full sample.

[21] See ARC Table 4.2

(The previous three quarters change in ξ plus the constant). Each of the quadrants addresses a different variable. Within each quadrant, the left-hand panel shows the data for the US while the right-hand panel shows the data for the rest of the sample. In each case, the sample has been further split into elections which produced a left-wing victory and elections which produced a right-wing victory so the graphs may be compared to the right-hand panel of figure 1.2.

The difference between the US and the rest of the sample is striking. In three of the four panels, the US data nicely matches the idealized figure in the right-hand panel of figure 1.2. Bond yields are particularly striking. Before the election there is little separation indicating the future victor. Then, at the time of the election, the lines jump apart sharply: a right wing victory produces expectations of fiscal prudence and expected bond yields drop sharply while a left wing victory produces a jump in expected yields. Finally, after only two months, the lines converge and resume a joint course. Because the vertical axis graphs the *change* in expectations, the temporary split in the paths represents a *permanent* partisan difference in forecasts of bond yields for the following year. At the time of the election, expectations jump either up or down depending on which administration is elected, and remain split. On the other hand, the figures for the remainder of the sample don't match the idealized figure 1.2 at all. The immediate effect at election time often takes the "wrong" sign, after which left and right switch back and forth over the next nine months. This clearly doesn't correspond to the RPT.

Two things are evident from the graphs. First, for several macroeconomic variables the US data clearly display partisan effects of election surprise while the remainder of the sample does not. Quite likely the remainder of the sample conceals heterogeneity among the remaining countries. However, the source of country-level heterogeneity is not yet clear. Second, even in the US data which is broadly characterized by strong partisan effects, inflation expectations display little partisan response to electoral surprise.

1.4.2 The Role of Institutions

Three possible explanations for differences in results by country come to mind, all of them due to variation in institutions which affect the RPT but which are unaccounted

for by my basic regression. First, in my sample, the US is the only country with a presidential system of government. Policy in presidential systems may be more volatile as power is relatively centralized in a single office which makes the election more of a winner-take-all proposition. Thus an electoral victory may carry more weight with forecasters in a presidential system resulting in a stronger cycle. The US also employs a majoritarian rather than a proportional electoral rule (there are three other majoritarian countries in the sample), which tends to result in the under-representation of smaller parties who, despite a national presence, cannot win any single district.[22] This may result in cleaner differences between left and right and thus in a stronger effect.

[22]Because of this, the variables 'Majoritarian' and 'Partisan' (defined below) exhibit high positive correlation.

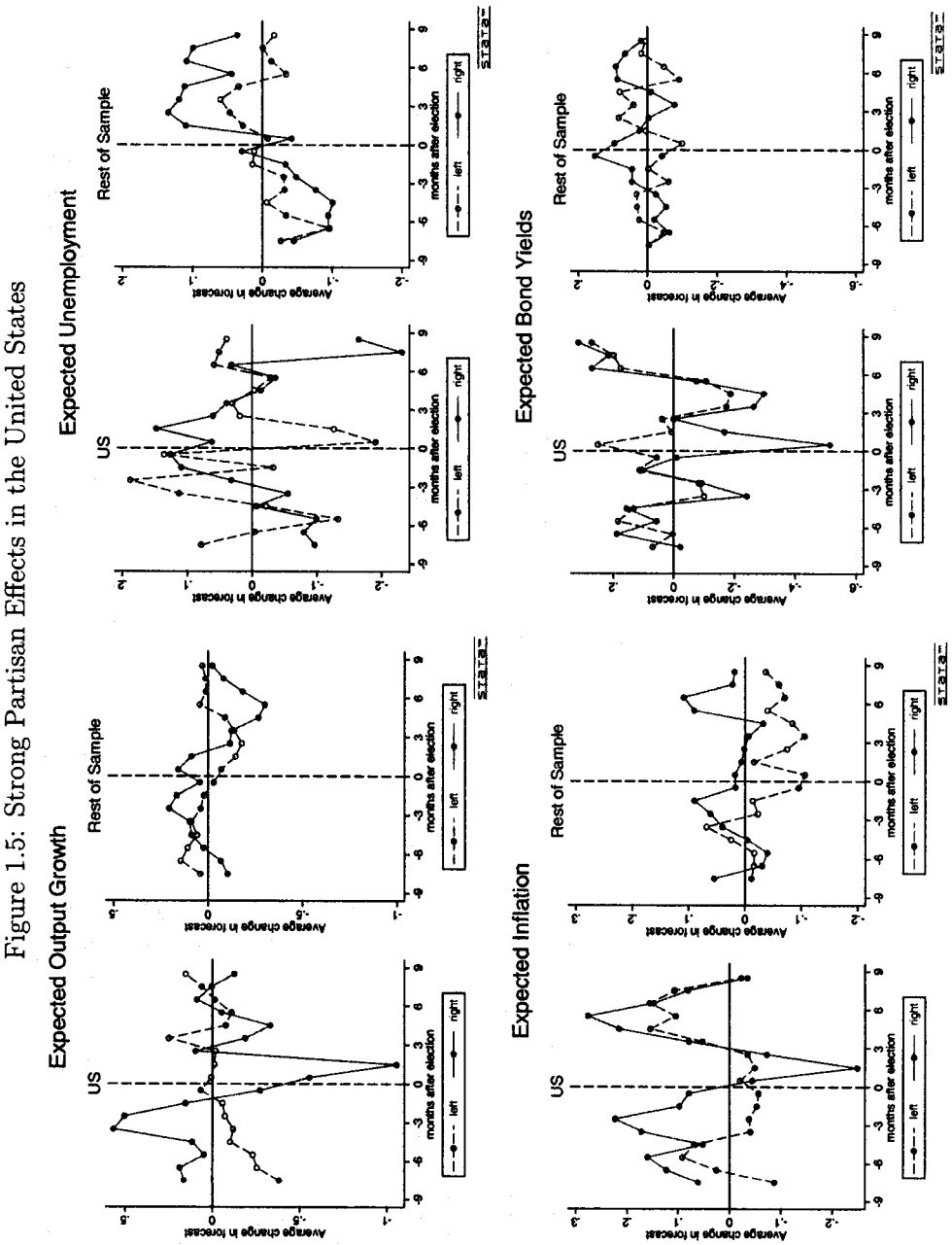


Figure 1.5: Strong Partisan Effects in the United States

Second, the strength of the partisan effect in the RPT depends on a stable left and right representing different economic policies. This condition can break down in at least two ways. First, in countries where political parties are pieces in a shifting pattern of alliances, governing coalitions may stretch across ideological lines, drawing economic policy toward the center. Recalling figure 1.2, the lesser the difference between the government in power and the alternative, the smaller the partisan effect on expectations. In other countries, economic policy is not the major source of contention between the left and the right. In Japan, for instance, foreign policy defines the political spectrum and distinguishes between the parties. If there is little partisan difference in economic policy, election surprise will have little effect on economic forecasts.

Third, the macro-model underpinning the RPT relies on overlapping wage-contracts to deliver the nominal rigidities necessary for surprise inflation. Furthermore, the behavior of wages in the model (equation 1.2) embodies one particular wage-bargaining outcome.[23] But even OECD countries exhibit a considerable variety of wage-bargaining institutions. In fact, OECD countries vary along several institutional dimensions. Perhaps these institutions play a role in the robustness of an economy to partisan political business cycles in particular and policy shifts in general. Hall and Soskice (2001) stress that there are often complementarities between the various institutional choices that firms must make to solve coordination problems relating to their core competencies. When we look at institutions of wage-bargaining, corporate governance, vocational training, and worker monitoring and performance, countries tend to cluster into brands of capitalism. Hall and Soskice emphasize the distinction between the liberal market economies (LMEs) (US, UK, Canada, Australia, New Zealand, Ireland) where "firms coordinate their activities primarily via hierarchies and competitive market arrangements" and the coordinated market economies (CMEs) (Germany, Japan, Switzerland, Belgium, Netherlands, Norway, Sweden, Denmark, Austria) where "firms depend more heavily on non-market relationships to coordinate their endeavors". Given the importance of wage-bargaining in the RPT, there is good reason to believe that different brands of capitalism may result in different brands of partisan cycles.

[23] I don't mean this as criticism of the RPT; such simplifications are necessary and even helpful. I simply point out one complication in taking such a model to a panel of countries.

Table 1.6: Additional Political Variables

Variable	Values
Majoritarian[a]	1: Can, Fra, UK, US 0: Ger, Ita, Jap, Neth, Nor, Spa, Swe
Presidential[b]	1: US 0: Can, Fra, Ger, Ita, Jap, Neth, Nor, Spa, Swe, UK
Partisan[c]	1: Can, UK, US 0.5: Fra, Ger, Spa, Swe 0: Ita, Jap, Net, Nor
Main Policy Axis[d]	1: Can, Fra, Ita, Neth, Nor, Spa, Swe, UK, US 0: Ger, Jap
Type of Capitalism[e]	1: Can, Jap, UK, US 0.5: Fra, Ita, Spa 0 Ger, Neth, Nor, Swe:

[a]'Majoritarian' refers to countries with a majoritarian electoral rule. Based on classification by Persson-Tabellini.

[b]'Presidential' identifies presidential and parliamentary systems of government. Based on classification by Persson-Tabellini.

[c]'Partisan Stability' refers to the degree to which party competition is characterized by a stable and identifiable left and right. This classification is based on that put forth by ARC, which they discuss on pages 145-6.

[d]'Main Policy Axis' refers to whether economic and class conflict constitutes the primary axis of conflict between the left and right. This classification is based on survey data from Huber-Inglehart 1995.

[e]'Type of Capitalism' is meant to capture the confluence of a variety of institutional characteristics such as wage-bargaining institutions, labor laws, and corporate governance. It is based on the classification of liberal vs. coordinated market economies by Hall and Soskice (2001).

To further explore these hypotheses, I have constructed five discrete-valued variables each of which splits the sample of countries into a few subsets. The variables are detailed in table 1.6. The 'Partisan Stability' and 'Main Policy Axis' variables both address the distance between the economic policies of the left and right but in different ways. 'Partisan Stability' captures the degree to which left and right are stable and clearly identifiable alternatives. At one extreme is the United States where left and right are dominated and defined by one party each. At the other end is Japan where a single dominant party fends off a myriad of smaller parties which are simultaneously competing to be either the dominant opposition party or a member of the ruling coalition. In the middle are countries like France and Germany where multiple parties allow for coalitions but these coalitions are relatively stable and predictable. The more stable the identity of the left and right alternatives, the more predictable is the influence of the partisan politics on economic policy and thus the sharper ought to be the RPT effect. 'Main Policy Axis' explores whether the definition of left-right is based on economic management and class conflict or whether, as in Japan and Germany, other dimensions are more prominent. According to survey data from Huber-Inglehart (1995), in Germany economics shares the stage with immigration policy among other issues while in Japan economics is completely overshadowed by foreign policy. In countries where definitions of left and right are based largely on factors other than economic policy, we would expect the RPT to be weaker. 'Presidential' and 'Majoritarian' refer to the form of government and the electoral rule (their respective opposites are Parliamentary and Proportional). 'Type of Capitalism' divides the countries according to their system of capitalism, following Hall and Soskice (2001).[24] I have rerun the regressions including additional terms for the interaction between SURPRISE and these five qualitative variables.

Several things stand out from the results in tables 1.7, 1.8, and 1.9. First, the discrete variables are almost always significant indicating that institutional details play an important role in partisan cycles. Second, the strength of the effect in the US is largely explained by the present set of institutional characteristics suggesting that country-level heterogeneity is attributable to these characteristics. Third, in some

[24]The coding of the 'Mediterranean' capitalist countries (France, Italy, Spain) midway between the liberal and coordinated market economies on a linear scale is meant to capture the sense in which these countries share some characteristics with either archetype.

northern European countries, the left may be so insistent on low unemployment as to generate a different partisan political business cycle. And finally, the results that emerge are strongest in many of the real variables the formal theory predicted to show little or no effect such as output and unemployment. Consumer price inflation is remarkable mainly for its *lack* of response.

Because the US is the only presidential country in my sample, there is no way to distinguish the effects of a presidential system from those due to unobserved characteristics of the United States. Table 1.7 shows that the exceptional strength of the US cycle is not simply due to unobserved characteristics beyond the scope of this study. If this were the case, the coefficient on SURPRISE*Presidential would be large and negative in the regressions on output growth for example. However, with the exception of unemployment, where a significant fraction of the US cycle remains unexplained, the strength of the US cycle can be explained by the institutional factors at hand. This pattern holds a more complete specification including all five institutional variables.

The story of tables 1.7, 1.8, and 1.9 contains three elements. First, the degree of heterogeneity between countries is quite large: some countries exhibit strong responses of expectations to elections surprise while others seem quite immune to it. For example, in a full specification with all institutional variables included, the US exhibits a fitted partisan difference in unemployment expectations of 1.79% while the estimated coefficients for France sum to 0.24%. Thus in the United States whether an unpredictably close race swings right or left will make a one and three quarters percentage point difference in year-ahead unemployment forecasts, but for a similar election in the France the difference in forecasts would be a mere quarter of a point. Some countries are much more sensitive to partisan political change than others.

One possible reason for this is that some countries exhibit wider partisan divides than others. The greater the difference between the policy positions of the left and right, the more weight the election carries. Another possibility is that the government of one country may enjoy more power than the government of another country. The more latitude the future government will enjoy, the greater the weight put on election results. Due to the difficulty of quantifying the relevant variables, I have largely left these possibilities to future work. The institutional variables I examine speak to a third possibility: whether political and economic institutions can dampen the effects

Table 1.7: Electoral Rules

	(1) GDP Growth	(2) Unemp.	(3) HH Cons.	(4) Bus. Invest.	(5) CPI infl.	(6) 90-Day Rates	(7) 10 Year Yield
SURPRISE	0.753	0.996	0.108	-0.079	-0.038	0.032	-0.077
	[0.434]	[0.558]	[0.096]	[0.299]	[0.043]	[0.142]	[0.071]
SURPRISE*	0.13	0.228	0.294	-1.43	-0.038	0.161	0.209
Presidential	[0.059]*	[0.027]***	[0.241]	[1.122]	[0.011]***	[0.033]***	[0.027]***
SURPRISE*	-1.612	-1.123	-0.683	-1.858	0.034	-0.616	-0.273
Majoritarian	[0.439]***	[0.552]*	[0.261]**	[1.188]	[0.049]	[0.151]***	[0.074]***
observations	21102	22704	19791	19853	27658	25638	25002

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%

GLS with dependent variable $\Delta\xi_t^e$
Controls: previous three quarters of change in the realized values of the variable plus a constant.

Table 1.8: Party Competition

	(1) GDP Growth	(2) Unemp.	(3) HH Cons.	(4) Bus. Invest.	(5) CPI infl.	(6) 90-Day Rates	(7) 10 Year Yield
SURPRISE	1.967	1.712	1.497	6.672	-0.214	1.372	0.133
	[0.157]***	[0.290]***	[0.227]***	[0.515]***	[0.129]	[0.169]***	[0.091]
SURPRISE*	1.835	1.448	1.294	5.14	-0.113	1.164	0.076
Main Policy Axis	[0.177]***	[0.225]***	[0.204]***	[0.421]***	[0.103]	[0.171]***	[0.080]
SURPRISE*	-0.877	-0.292	-0.482	-4.768	0.091	-0.707	-0.321
Partisan Stability	[0.332]**	[0.147]*	[0.236]*	[0.947]***	[0.102]	[0.209]***	[0.197]
observations	21102	22704	19791	19853	27658	25638	25002

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%

GLS with dependent variable $\Delta\xi_t^e$
Controls: previous three quarters of change in the realized values of the variable plus a constant.

Table 1.9: Variety of Capitalism

	(1) GDP Growth	(2) Unemp.	(3) HH Cons.	(4) Bus. Invest.	(5) CPI infl.	(6) 90-Day Rates	(7) 10 Year Yield
SURPRISE	-0.748	-0.040	-0.331	-3.333	-0.016	-0.505	-0.252
	[0.028]***	[0.088]	[0.053]***	[0.075]***	[0.021]	[0.046]***	[0.077]***
SURPRISE*	1.545	1.378	0.831	5.652	-0.066	0.853	0.248
Variety of Capitalism	[0.401]***	[0.385]**	[0.194]***	[0.689]***	[0.093]	[0.225]***	[0.150]
observations	21102	22704	19791	19853	27658	25638	25002

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%

GLS with dependent variable $\Delta\xi_t^e$
Controls: previous three quarters of change in the realized values of the variable plus a constant.

of a partisan change in government policy.

The nature of party competition affects the way forecasts respond to election surprise. The coefficients for SURPRISE*Partisan Stability, while not always strongly significant, reveal that the partisan cycle is stronger in countries where clear and stable parties or coalitions occupy the left and right. Countries with multiple parties and coalition governments simply don't exhibit as sharp a reaction to election results. The SURPRISE*Main Policy Axis coefficients are much stronger and similarly supportive. Countries where left and right are defined largely by economic issues ('Axis' = 1) exhibit sharper partisan effects.

While most of the variables fit the pattern, unemployment does not. Countries where economic management defines and divides the left and right seem to suffer less of a partisan effect on expected unemployment even as they suffer more pronounced effects in other variables. The puzzle, once again, is due to combining two countries with vastly different political cycles. The Axis variable combines Germany and Japan as the countries in which left and right are defined largely by non-economic issues. When I allow Germany and Japan to enter separately, not constrained to have the same coefficient, I find that the partisan effect on unemployment expectations is much stronger than the sample average in Germany but much weaker than the sample average in Japan. So despite the salience of non-economic issues, unemployment expectations in Germany exhibit extremely strong partisan response to election surprise.

I suspect that the strong partisan effect in German unemployment expectations is actually due to the exceptionally strong views of the left in Germany and many Scandinavian countries concerning unemployment. In other words, in a group of countries (Sweden, Norway, Germany), there is a larger gap between the policies of the left and right where unemployment is concerned and thus a stronger partisan effect. Because the Consensus data set records unemployment expectations only for the G-7 countries, the effects of these unemployment extremists are seen only in German data. The fact that this effect is seen only in unemployment, without a simultaneous effect in inflation, leads me to believe that the Phillips Curve does not represent the entire space of policy choices which are relevant to partisan political business cycles. In fact, inflation shows remarkably little response to election surprise in any country: a fact to which I will return later.

In sum, examining party politics leads to four conclusions. First, countries with a clear and stable political left and right display stronger partisan cycles than countries characterized by shifting alliances. Second, countries in which the political left-right spectrum does not correlate closely with preferences over aggregate demand policy tend to exhibit a weaker partisan cycle. Third, greater distance between the policy preferences of the left and right leads to a stronger partisan cycle. Finally, the tale of the cycle in German unemployment expectations suggests that a rich variety of partisan differences is relevant in generating partisan political business cycles.

Lastly, I look at the association between the national brand of capitalism and the impact of election surprise on economic forecasts. The coefficients for SURPRISE*Type of Capitalism in the table 1.9 are positive and significant indicating that market economies tend to be more prone to political cycles than statist economies. The coefficients are large and highly significant in most cases. The exceptions are unemployment and inflation. The coefficient on unemployment is positive, indicating the opposite effect (remember that unemployment is counter-cyclic while the other variables are pro-cyclic). More complete specifications show that this is once again due to the extremely strong effect in Germany, where unemployment is an issue of particular salience. And as usual, the coefficient on inflation is quite tame.

This brings me to the final result of note: the behavior of inflation forecasts. Almost without exception, the coefficients for inflation are much smaller than their counterparts for other dependent variables and generally insignificant from zero. And yet this is the place where we expected, from the theory, to see the real action! So why do forecasters predict partisan movements in output and unemployment and none in inflation? My guess is that the lack of movement in inflation expectations is due to the relative independence of central banks in these developed countries over the past fifteen years. While this independence is hotly debated and difficult to pinpoint, forecasters, at least, seem to believe that inflation is safely insulated from swings in political power, even if the rest of the economy is not.

1.5 Conclusion

The rational partisan theory proposes that elections, because they are a source of unanticipated policy change, generate the post-electoral economic volatility known as

the partisan political business cycle. Unfortunately, previous work characterizing the cycle itself has been unable to distinguish between the rational partisan theory and the traditional partisan theory based on adaptive expectations. This chapter solves that problem by looking at the post-electoral behavior of expectations and introduces new data sets containing economic and political forecasts to do so. The primary result is compelling support for the premise of RPT. Forecasters exhibit partisan responses to election results which vary according to the degree to which those results are unexpected. These responses are virtually immediate, implying an anticipation of partisan policy rather than an ex-post reaction to its effects. The magnitudes of the responses match well with the magnitudes of the subsequent partisan effects on realized variables, particularly when country heterogeneity is taken into account. Moreover the effects are strongest in countries where economic policy is the primary axis of politics and are stronger where partisan divides are stable. Each of these factors is consistent with RPT, suggesting that the partisan political business cycle is the result of election surprise rather than delayed adjustment to new policy.

This chapter also uncovers significant country-level heterogeneity in the character and magnitude of the partisan political business cycle among advanced western economies. Significant differences in the structure of political competition (the number, strength, and platforms of the major parties) constitute one source of heterogeneity. The nature of the capitalist system constitutes another source. Liberal market economies, where firm-firm and firm-worker interactions are largely conducted at arm's length by markets and complete contracts, are subject to more pronounced cycles than coordinated market economies, where interactions are more often governed by relationships and incomplete contracts. Since LMEs and CMEs refer to broad groupings of related institutions, this raises the question of exactly which institutions are responsible for these differences and how such institutions serve to dampen the partisan effects of electoral surprise on macroeconomic forecasts.

While the basic mechanism of the RPT garners strong support, certain results call into question the specific RPT mechanism. Real variables display much stronger results than monetary variables. Furthermore, inflation forecasts display virtually no partisan effects suggesting that forecasters believe in the political independence of central banks.

This doubt about the underlying mechanism, coupled with evidence of the importance of political and economic institutions opens an exciting new area of inquiry. Further investigation into the institutional basis of country-level heterogeneity in partisan cycles would form a useful base for the formulation of an updated model of rational partisan political business cycles.

Table 1.10: Opinion Poll Data Sources

Question: "If an election were held tomorrow, for which candidate/party would you vote?"			
Country	Election Date	Source	Time to Election
CAN	10.25.1993	Canadian National Election Study	5 weeks - 1 day
CAN	6.2.1997	Canadian National Election Study	5 weeks - 1 day
CAN	11.27.2000	Canadian National Election Study	5 weeks - 1 day
CAN	6.28.2004	Ipsos-Reid	5 weeks - 1 week
FRA	3.28.1993	Eurobarometer	1 month
FRA	6.1.1997	Eurobarometer	3 months
FRA	6.16.2002	Le Figaro (Ipsos)	1 month
GER	12.2.1990	Eurobarometer	2 months
GER	10.16.1994	German Election Study (Politbarometer)	7 weeks - 1 week
GER	9.27.1998	German Election Study (Politbarometer)	6 weeks - 1 week
GER	9.22.2002	Politiscope	1 month
ITA	4.6.1992	Eurobarometer	1 month
ITA	3.27.1994	.	.
ITA	4.21.1996	Corriere della Sera (Ispo-Cra/Nielsen)	1 month
ITA	5.13.2001	Corriere della Sera (Ispo-Cra/Nielsen)	2 months
JAP	2.18.1990	NHK	1 week
JAP	7.18.1993	NHK	2 weeks
JAP	10.20.1996	Asahi Shimbun	2 weeks
JAP	6.25.2000	Asahi Shimbun	2 weeks
JAP	11.9.2003	Asahi Shimbun	2 weeks
NLD	5.6.1998	Dutch Parliamentary Election Study	5 weeks - 1 day
NLD	5.15.2002	Dutch Parliamentary Election Study	5 weeks - 1 day
NLD	1.23.2003	NRC Handelsblad	2 weeks
NOR	9.10.2001	Verdes Gang (Norsk Gallup)	1 month
SPA	6.6.1993	Eurobarometer	3 months
SPA	3.3.1996	.	.
SPA	3.12.2000	El Pais (Demoscopia)	1 week
SPA	3.14.2004	Instituto Opina para la Cadena SER (Pulsometro)	2 weeks
SWE	10.20.1998	Swedish Election Study	2 months - 1 week
SWE	9.15.2002	SIFO	3 months
UK	4.9.1992	Eurobarometer	1 month
UK	5.1.1997	Eurobarometer	2 months
UK	6.7.2001	British Election Study	1 month - 1 day
USA	11.3.1992	National Election Study	2 months - 1 day
USA	11.5.1996	National Election Study	2 months - 1 day
USA	12.12.2000	National Election Study	2 months - 1 day

Chapter 2

The Size and Composition of Government Expenditure

This chapter collectively explores several leading hypotheses on determinants of government expenditure. The purpose is to unify the government spending literature and avoid omitted variables bias by testing the prominent theories in a comprehensive specification; by so doing to identify persistent puzzles for the current set of theories; and to explore those puzzles in greater depth by looking at the composition of government expenditure and the level of government at which it takes place as well as its magnitude. Using Global Financial Statistics data from the IMF covering over 100 countries from 1970-2000, I look not only at cross-sectional and inter-temporal variation in government expenditure but focus on individual categories of expenditure (such as defense, education, health care) as well as different levels of government (central, state, and local). I elaborate a new framework for bringing Meltzer-Richard (1981) to data and find strong support for the theory. I find strong support for the role of demographic variables such as country size, openness, and inequality. I also find that, contrary to expectations, majoritarian governments do not favor any particular type of expenditure, but simply correlate with reduced expenditure across the board.

2.1 Introduction

Theories of government expenditure tend to focus either on determinants of demand for government services or the structure of the supply of these services. One class of theories postulates mechanisms whereby demographic characteristics correspond to increased demand for a certain type of government service. For example, a larger population of elderly in a country implies a greater demand for social security (as well as a larger fraction of the population receiving it) and thus higher public expenditure on social security. Another source of demand shifters is the flow of ideas. Tanzi and Schuknecht (2000) emphasize the importance of the perceived role of the state in determining the scope of government and thus the size of public expenditure.

These demand-driven theories usually treat the formation of policy as a black-box. They postulate a factor which shifts demand and look for the resulting shift in equilibrium public expenditure. The second major approach focuses on how different government structures translate fixed demand into heterogeneous policies. In this chapter I consider both types of theories.

Many of the explanatory variables nominated by these theories are correlated. As a result, tests which focus solely on one of several variables almost surely suffer from omitted variables bias. The first purpose of this chapter is to gather the prominent theories and test them collectively to avoid such bias.

The second purpose is to use data breaking public expenditure out into categories (defense, education, health care) and different levels of government (local, state, central) to formulate more nuanced tests of the leading theories. Data covering a variety of countries and time periods yields variation in both the independent and the dependent variables. However, data covering multiple levels of government generates a greater *number* of dependent variables. Instead of simply looking at the behavior of total government expenditure, one can now look at how central government and local government expenditure vary independently. Data which breaks down total expenditures into subcategories delivers another important proliferation of dependent variables. The great utility of this multiplicity of dependent variables is that it enables more sophisticated tests of the relevant theories. Theories which lead to identical predications at the aggregate level are distinguishable at a finer level.

For example, Alesina-Wacziarg (1998) assert that public expenditure is a larger

fraction of the economy in less populous countries. Rodrik (1998) asserts that countries which are more open to trade have higher public spending. However, as they both acknowledge, less populous countries also tend to be more open. The key to differentiating between these theories is to look at categories of government expenditure. The Alesina-Wacziarg mechanism operates through those public goods which display increasing returns to scale (transportation, public order and safety). The Rodrik mechanism operates through social protection. Thus the theories predict different patterns of public expenditure and they can be disentangled. Looking at a variety of levels of government is similarly useful.

Focusing on the finer patterns in data disaggregated by category and government level also allows for more sophisticated tests and greater confidence that results are not due to omitted variation. While an omitted variable may produce a coarse pattern at the aggregate level, it is less likely to reproduce the more intricate pattern predicted in the disaggregated data.

Many previous contributions have looked at the behavior of subcategories of public expenditure. For example, Alesina-Wacziarg (1998) look at several categories as well as aggregate spending. The contribution of this chapter is the consistent application of this technique coupled with a similar examination of several levels of government to a broad range of variables.

This technique delivers a variety of interesting results. I find that increased trade openness in developing countries is not reflected in higher public employment as Rodrik (1998) hypothesizes. I find that ethnic fractionalization results in a decentralization of expenditure in certain categories (education, healthcare) but has little effect on other types of expenditure (public order and safety). I find that Wagner's Law is actually driven by demographics: richer countries tend to have more old people and thus tend to spend more on social security. Total spending net of social security actually declines with per capita income. I also test the Meltzer and Richard (1981) median-voter mechanism for redistribution and find strong evidence that greater inequality and greater political access are each correlated with redistribution. And I show that majoritarian governments not only incure lower total expenditure than governments elected by a proportional rule, but they spend less on both public goods and transfers, demonstrating that panel regressions of expenditure shares cannot distringuish between Milesi-Feretti, Perotti, and Rostagno (2002) and

Persson and Tabellini (2004).

The next two sections discuss the choice of estimation method and the data. Section 2.4 addresses each of the main explanatory variables in turn, explaining their history in the literature, my strategy for bringing them to data, and my results. Two other significant exercises are embedded within this section. In my discussion of political rights I elaborate a framework for testing Meltzer-Richard (1981) in a cross-country panel setting. And in the section on demographics I use an example to make the case for separating the dependency ratio into its components (those too young to work and those too old to work). The example shows how constraining the two to enter with the same coefficient can lead to nonsensical results.

2.2 Methodology

The essential exercise is to regress various measures of government expenditure on a vector of explanatory variables in a cross-country panel. The two most important complications present in the data are measurement error in the right hand side variables and country-specific effects that are correlated with regressors.[1]

The first source of measurement error is simply in collection and transmission. The data used in this chapter are macro-indicators collected for over one hundred countries and are often several steps removed from first hand data collection. They often involve estimation rather than measurement. It would be optimistic to assume that these quantities are measured with perfect accuracy. The second source of measurement error is the distance between the measure used and the theoretical concept it is meant to capture. This problem is reflective of both the paucity of available data and the difficulty of pining down many of the theoretical concepts.

[1]Hauk and Wacziarg (2003) use Monte Carlo methods to assess the tradeoff between unobserved heterogeneity and measurement error in the human-capital augmented version of the Solow neoclassical growth model. No such study exists for the literature on government size, at least in part because no prominent model exists to inform the specification, but some of these lessons are likely to be valid. Specifically, choosing between fixed effects and estimators that use some degree of between country variation is a tradeoff between omitted variables bias and measurement bias. Fixed effects solves omitted variables bias but tends to exacerbate bias from measurement error when the right-hand side variables are more persistent than the errors in measurement. Mindful of this tradeoff and without a study for this specific example, I have chosen to use random effect but average the data over five-year periods.

The plethora of candidates for country-specific fixed effects that might be correlated with regressors are a constant temptation for those inclined to a kitchen sink approach. Geographic characteristics such as whether a country is an island or landlocked or split by a large mountain-range may be correlated with its trade openness as inter-national trade is made easier or harder relative to intra-national trade. Features of national history such as the date of independence may be correlated with the institutional structure as new constitutions are written with an eye to concurrently popular political ideas. Cultural and religious identification may be correlated with population growth through shared views on birth control. Since the list is inexhaustible, some degree of bias is inevitable. I have made the choice to stick with those variables which have been repeatedly demonstrated to have first-order effects on patterns of expenditure—population, trade openness, per capita wealth, age demographics, inequality, fractionalization, political and electoral system—relegating discussion of the possible bias to the choice of estimation technique.

There are several possible estimation techniques: random effects on annual data, random effects on data averaged over multi-year periods, fixed effects on annual data, fixed effects on data averaged over multi-year periods, the between-country estimator, or more complex choices resorting to instrumentation. The choice of method is best justified by an appeal to the characteristics of the data at hand.

A look at the summary statistics in tables 2.15, 2.16, and 2.17 reveals that the dependent and independent variables exhibit markedly different breakdowns of total variation into within-group and between-group variation. In annual data, the independent variables vary quite a lot between countries but are almost completely stationary within each country. Those few that do change (population and per capita income) are characterized by relatively steady growth. The dependent variables (the expenditure shares), on the other hand, display annual variation within countries as well as between them. While they do exhibit growth, they also exhibit nontrivial annual variation about the trend.

Clearly then, we cannot hope to describe this annual within-country variation with the set of explanatory variables we have drawn from the literature. Our ambitions must be limited to a description of the between-country variation and the longer trends in the within-country variation. Nor does it make sense to run fixed effects, because most of the variation in the independent variables, being between countries,

will be soaked up by country fixed effects, leaving little variation to evaluate the hypotheses.

The choice, then, is between random effects on the entire sample and the between-country estimator and it is essentially a choice of how much to average across time periods. Averaging each country's annual data across a period of y > 1 years presents an intermediate case between these two methods. Larger y (closer to the between-country estimator) has the benefit of reducing the measurement error to the extent that such error is not autocorrelated. This is done at the cost of reducing the number of observations and removing some of the within-country variation, which may be of interest. The choice y = 5 (leaving 4-6 periods per country) is a common compromise and is the baseline for this study.

The basic specification is

$$\text{Expenditure}_{it} = \alpha + \beta * \text{Explanatory Variables}_{it} + u_i + \epsilon_{it}$$

Of which a specific example would be

$$\begin{aligned} \text{Transfers}_{it} = \; & \alpha + \beta_1 * \ln(\text{population})_{it} \\ & + \beta_2 * \ln(\text{GDP per capita})_{it} \\ & + \beta_3 * \text{openness}_{it} \\ & + \beta_4 * \text{index of ethnic fractionalization}_{it} \\ & + \beta_5 * \text{fraction of population over } 65_{it} \\ & + u_i + \epsilon_{it} \end{aligned}$$

Where i indexes the country and t indexes the 5 year period.

2.3 Data

The primary source of data for this study is the IMF Government Financial Statistics (GFS) dataset. The GFS is a standardized collection of annual national accounts for over one hundred countries. I use the GFS data on government expenditures, which includes all non-repayable payments by any level of government for either

current or capital purposes. GFS classifies expenditure by two methods: either by economic characteristics or by the function or purpose served. Both are used in this study. The former is called the economic classification of government expenditure (ECOG) and breaks total spending into current and capital expenditure and then further into goods and services vs. transfers. The latter is called the classification of the functions of government (COFOG) and breaks total expenditure into categories such as healthcare, education, and defense, each of which include both current and capital expenditure. See diagram 1 for diagrams illustrating how each system of classification breaks down total expenditure into categories. The detailed analysis of how each category is defined and how expenditures are classified is available in A Manual on Government Financial Statistics 1986 which I refer to hereafter as "the GFS manual". The raw data is gross expenditure in local currency so I divide by the contemporaneous GDP in local currency to achieve expenditure as a share of GDP, which is a unit-less measure.

In addition to being cut according to one of two methods of classification, total expenses are also classified according to the level of government: central, state, or local. So a sample GFS series would be expenditure by the central government on education as a share of GDP and observations would be by country-year. In an effort to address total spending at all levels of government I have summed the three levels of government into a category designated general government. This exercise is complicated by the fact that data for state and local government expenditure is often missing for one of two reasons. First, the expenditure in question may exist for the level of government in question, but the IMF was not able to get data on its expenditures for that year. Or, second, there may be no such expenditure, either because the level of government does not exist (e.g. many countries don't have state level governments) or because the level of government does not spend on the category in question (e.g. state and local governments rarely spend on defense). Ideally, general government would be recorded missing if the former were the case but would simply ignore the level in question during aggregation if the latter were the case. Unfortunately, the GFS dataset does not distinguish between the various types of missing data, complicating the calculation of general government expenditure as the missing values need to be sorted into "missing" and "does not exist". I have attempted to do this sorting by hand by looking at the system of government, tracking missing

values through time, and thinking about the categories and levels of government in question.

The fractionalization data is care of ADEKW and is described in their appendix. Data on political systems is care of Persson and Tabellini and is described in their appendix. I sincerely thank these authors for sharing their data. Openness data come from the Penn World Tables and are measured in current prices. My political rights variable is derived from the Gastil index of the same name. The list of questions from which the political rights index is composed and the ranking methodology are available at the Freedom House website. The raw index runs from 1 to 7, with lower numbers indicating greater political freedom. I have taken the inverse of the index to obtain a variable which runs from 1/7 to 1 and in which larger numbers indicate greater political freedom. Some demographics data (per capita GDP and population) come from the Penn World Tables. Other demographics data (Over65, Under15) are taken from the World Bank World Development Indicators Database. Gini coefficients come from the United Nations Development Program World Income Inequality Database (WIID) which includes and builds upon the well-known Deininger-Squire data. Documentation is available from the UNDP website. The UN groups these data into reliable and unreliable data and further categorizes them by the source of the accounts and the population over which they are valid. I have used only data marked reliable and stemming from income or expenditures data covering the entire population.

To form five year panels from annual data, I took the arithmetic averages of the available annual values for each variable.[2] Summary statistics for the variables can be found in tables 2.15, 2.16, and 2.17. With a slew of variables and a bevy of countries, there are, inevitably, gaps in the data. Some variables are available for a wide swath of countries: GDP, population, and many of the broader expenditure categories. But some variables suffer from more limited coverage, most notably the inequality data (Gini) and the political institutions data (majoritaran, presidential). Including these variables in the regressions cuts the number of countries from 90-100 in the basic specifications to 45-50 in the more inclusive specifications. Were the availability of these variables random across countries, this sample cut would imply loss of precision but no bias in the estimates. However, not surprisingly, data for

[2]Because my data stretches 31 years from 1970-2000, the first panel is six years from 1970-1975.

richer countries is more easily come by so the data cuts resulting from the inclusion of these limited-coverage variables can imply significant changes in the coefficients due to sample selection rather than the inclusion of an extra control variable. Hence the importance of considering both basic and extended specifications.

2.4 Results

With four levels of government (general, central, state, local), at least nine expenditure categories (defense, education, healthcare, social security, public order and safety, general public services, wages and salaries, goods and services, and total expenditure plus certain others at times), and several RHS specifications (basic, complete, and alternates highlighting certain variables), recording the regressions runs to forty or fifty excel spreadsheets. Since this is too many for comprehensive reporting, I have included those results on which I base my conclusions. Others are available on request.

2.4.1 Openness

The ratio of imports plus exports to GDP is a common measure of the degree to which an economy is dependent on international trade. In the literature on government finance, this measure has come to be called simply, openness. It was David Cameron (1978) who first convincingly demonstrated a connection between openness and government finance. In a sample of 18 OECD countries[3], Cameron found openness in 1960 to be a strong predictor of the increase in government tax revenues as a share of GDP between 1960 and 1975. His explanation centered on the role of unions: postulating that more open countries were more heavily unionized which, through collective bargaining, lead to greater demand for government transfers in the form of social protection and reeducation. While Cameron's dependent variable was tax revenues, the mechanism he suggests would be better explained through an analysis of expenditures.

Rodrik (1998), taking up where Cameron left off, notes a positive correlation between openness to trade and subsequent government expenditure which is robust to

[3]Cameron's sample includes: Netherlands, Sweden, Norway, Denmark, Belgium, Canada, Britain, Ireland, Austria, Finland, Switzerland, France, Australia, Germany, Spain, Italy, Japan, and the United States.

controls[4] and exclusion of outliers, extends to countries of all income levels (Rodrik's sample includes over one hundred countries), and exists for all available measures of government consumption. But he asserts that Cameron's collective bargaining explanation is unlikely to explain the correlation in the broader sample of countries due to the relative weakness of organized labor in developing countries. Rodik hypothesizes that government expenditure may serve as a form of insurance against external risk. In more open countries, the income streams of households are derived from firms which do more overseas business and are thus subject to greater external risk such as exchange rate risk or supply or demand fluctuations abroad. Assuming some portion of this risk cannot be diversified away, this would generate demand for public insurance against external risk. Rodrik surmises that advanced countries with the requisite administrative capacity mitigate this undiversified external risk through spending on social protection while developing countries, lacking the capacity to administer large-scale social transfer programs, rely on simpler, less-targeted solutions including public employment. In each case the result is an increase in government expenditure as a country becomes more open, ceteris paribus.

Rodrik regresses the log of government consumption over a multi-year period on the log of openness for a previous multi-year period (e.g. log(average government consumption 1990-1992) regressed on log(average openness 1980-1989)). His points are (a) that lagged openness can explain the level of government expenditure; and (b) that past openness can explain subsequent growth in government consumption.[5] It is important to emphasize that openness is lagged in Rodrik's specifications, since his most vocal critics often use simultaneous values for openness and government expenditure in their reduced form regressions. [6]

I test Rodrik's theory in two ways. First, I look to replicate the correlation between trade openness and the level of government expenditure given a broader set of controls. Second and more importantly, I examine which categories of government expenditure are most affected by trade openness. If Rodrik's theory is correct, we

[4]Rodrik controls for the log of per capita GDP, the log of the dependency ratio, the log of urbanization, and dummies for socialism, membership in the OECD, and three geographic regions: Latin America, Sub-Saharan Africa, and East Asia. In a separate regression, he controls for log of area, log of population, and various measures of the composition of the country's exports and imports (primary goods, oil, etc.).

[5]Rodrik (1998), table 1 regressions 1-4 and 5.

[6]Alesina-Wacziarg (1998)

would expect to see higher openness associated with higher levels of expenditure on social protection in developed countries and higher expenditures on wages and salaries in developing countries. So when we run the following regressions

$$\begin{aligned}
\text{Social Protection}_{it} &= \beta X_{it} + \alpha_{sp} Openness_{it} + \mu_i + \epsilon_{it} \\
\text{Wages and Salaries}_{it} &= \beta X_{it} + \alpha_{ws} Openness_{it} + \mu_i + \epsilon_{it} \\
\text{Social Protection}_{it} &= \beta X + \gamma_{sp} Openness_{it} \\
&\quad + \lambda_{sp} Openness_{it} * OECD_{1975} + \mu_i + \epsilon_{it} \\
\text{Wages and Salaries}_{it} &= \beta X + \gamma_{ws} Openness_{it} \\
&\quad + \lambda_{ws} Openness_{it} * OECD_{1975} + \mu_i + \epsilon_{it}
\end{aligned}$$

where X_{it} is the vector of other controls, we are looking for $\lambda_{sp} > 0, \lambda_{ws} < 0$. The extent to which developing countries also use social protection as a form of government insurance against openness is captured by γ_{sp}. Similarly, the extent to which developed countries use government employment to insure against external risk is captured by γ_{ws}. Rodrik's theory doesn't make specific predictions for these coefficients.

I've presented the results to the regressions in table 2.1. In these regressions I've limited the controls to population, wealth, fractionalization, and demographics. Both the inequality data and the political institutions data are available for a more limited sample which is heavily biased towards developed countries. The limited specification ensures the sample remains as wide as possible and with a good representation of developing countries. Specific results are robust to the inclusion of the full range of controls. The data is for general government (expenditures aggregated over all levels of government) and is averaged over five year periods.

The regressions in table 2.1 use contemporary values of openness. Because Rodrik uses lagged values of openness, I also experimented with various lags of openness and didn't find any significant change in the relevant coefficients.

Table 2.1 shows $\alpha_{sp}, \alpha_{ws}, \gamma_{sp}, \gamma_{ws} \approx 0$, $\lambda_{sp} > 0$, and $\lambda_{ws} \approx 0$. Essentially this results in partial justification of Rodrik's theory. On the one hand, expenditure on

social protection seems to depend rather significantly on openness in OECD countries but not at all in developing countries. This is strong support for Rodrik's contention that social protection is a form of insurance for external risk, but that only advanced countries possess the sophisticated infrastructure to support such complex programs.

The actual coefficient is $\lambda_{sp} = 0.027$ meaning an increase in openness of 1% in an OECD country delivers an increase in expenditure on social protection of not quite three one hundredths of a percent of GDP. This may seem like a small effect, but countries can actually differ by quite a lot in their degree of openness. One standard deviation in openness among OECD countries is 40%, which implies a difference in expenditure on social protection of more than 1% in GDP. This use of social protection to mitigate undiversifiable external risk is clearly a very large effect among developed countries.

For further confirmation of the social insurance theory, we can turn to the ECOG and look at the behavior of transfers. Table 2.2 displays evidence that transfers increase with openness, particularly in OECD countries. Furthermore, by comparing regressions 1 and 2, we can see that once again, expenditure on transfers responds to openness in developed countries but not in developing countries.

On the other hand, there is no evidence in the complete sample of 90 countries that developing countries resort to government employment as an alternate form of social insurance against external risk. If they did, we would expect to see either γ_{ws} or λ_{ws} significantly less than zero.[7] But columns 5 and 6 of table 2.1 clearly indicate that total expenditure in both developed and developing countries increases with openness. Furthermore, the rise in developed countries is larger than that attributable to social protection. So if more open countries spend more in total and neither wages and salaries nor social protection can account for the entirety of the increase, on what categories are open countries spending? And is this expenditure explainable in terms

[7]As mentioned earlier, including the full set of controls cuts the sample in half due to the limited availability of data on inequality and political institutions. Including all controls limits the sample to about 45 countries and produces estimates of $\gamma_{ws} > 0$ and $\lambda_{ws} < 0$, the former being significant. However, it turns out that running the basic specification (without controlling for inequality or political institutions) over the reduced sample of 45 countries produces the exact same results. In other words, the significant results are due to the sample rather than the controls. The inequality and political institutions data are more likely to be available for richer countries, so the sample is hardly a representative cross-section for testing Rodrik's theory. So while there is some evidence of correlation between the public wage bill and openness, it is certainly not occurring in those countries that lack the developed infrastructure to implement other forms of social insurance.

of Rodrik's theory of increased demand for social insurance against external risk?

Examination of the other COFOG categories reveals most to be unlikely targets for social insurance. Defense; public safety; healthcare; recreational, cultural and religious activities; transportation and community; agriculture; mining; and energy are obviously poor candidates. Education is a possibility as it may include some funding of retraining for displaced workers, but this is more likely included within social protection.[8] The last two categories are housing and community and other economic affairs. The former includes provision of public housing, a form of social insurance. The latter is even more promising. It includes tourism, hotels, storage and warehousing, and schemes to facilitate labor mobility and reduce the rate of unemployment in distressed or underdeveloped regions. [9] Each of these sub-categories promises to be linked to openness and those dealing with labor mobility and unemployment are clearly forms of social insurance dealing directly with external risk.

Table 2.3 displays regression results for central government expenditures. Expenditures on housing, other economic affairs, and other expenditures increase significantly with greater openness. Expenditures in the other categories (including those not shown) do not.[10] However, notice that the increase is not limited to developing countries, but takes place in across the board. Thus we have not identified an alternate channel of expenditure present only in developing countries.

The broad conclusion is that developed countries do seem to mitigate the increased external risk associated with greater openness through increased expenditure on social protection and other labor market policies. Developing countries, on the other hand, seem to have a much weaker response to undiversifiable external risk: they don't significantly increase their expenditure on social protection, nor do they tend to absorb risk through increased public employment. There seems to be some small response in expenditure on labor market policies (included in other economic affairs).

[8]Education includes full and part-time adult students, vocational students, and scholarships, grants, and loans to students (for example see COFOG category 4.2.2 on page 154 of the GFS Manual). However, unemployment benefits are included under social protection (see COFOG 6.1.4 on page 159). Retraining could also be included in other economic affairs, mentioned in the next paragraph.

[9]GFS Manual page 173.

[10]Other Expenditure is a nontrivial residual category catching expenditures not classified by major group. It accounts for an average of 11% of government expenditure in my sample and seems to consist largely of interest payments and outlays for underwriting public debt. See GFS Manual page 174.

However, most of the increase in expenditure associated with increased openness in developing countries has little to due with social insurance in any form. Coupled with the fact that the effect of openness on total expenditure is three times as large in developed countries as it is in developing countries (column 6 of table 2.1), this suggests that developing countries simply fail to address the undiversified external risk associated with greater openness. Since residents of developing countries are unlikely to have better access to asset markets for diversification of external risk than citizens of developed countries, it would seem they simply live with a higher level of undiversified income risk.

2.4.2 Country Size

Alesina and Wacziarg (1998) offer an explanation for the observed fact that larger countries have smaller government consumption as a share of GDP. Their argument is built on two ideas taken from the literature on country formation.[11] First, assume that sharing non-rivalrous public goods over larger populations results in lower per-capita costs of provision. Assume further that larger populations tend to exhibit greater heterogeneity in preferences over public goods provision. Then equilibrium country size emerges as a tradeoff between the costs of increasingly heterogeneous preferences and the benefits of sharing non-rivalrous public goods over larger populations. The result is that larger countries tend to exhibit lower per capita expenditure on public goods. They regress per capita government consumption of goods and services (not including transfers) on country size and find a negative relationship. Second, to the extent that market size influences productivity[12], smaller countries are more negatively impacted by a closed world trading system. Put differently, smaller countries are more viable under open trading systems because they can benefit from spillovers due to foreign production. Not surprisingly, small countries are more likely to be open to trade. Thus we expect to see a negative relationship between country size and the degree of trade openness, and Alesina-Wacziarg (AW) find just such an effect. In combination, these two effects result in smaller countries being more open to trade and spending less per capita on public goods. Consequently, AW argue that

[11] See Alesina-Spolaore (1997) and Alesina-Spolaore-Wacziarg (1997)

[12] See here the vast literature on monopolistic competition with a variety of goods and inputs and the resulting increasing returns to scope and economy size.

it is not openness, but country size that truly explains government expenditure.

Starting with a simple univariate regression of government consumption on country size (population), AW proceed to control for time period, per capita income, region of the world, urbanization, the degree of ethno-linguistic fragmentation, the dependency ratio, and a democracy index. They also examine results for a variety of subcategories of government spending: public consumption, public consumption net of education and defense, expenditure including transfers and interest payments, defense, education, and public investment.

AW supply a pair of results that support the conceptual underpinnings of their general argument. First, in the regression of per capita government consumption on log of population, the latter has a negative and significant coefficient, supporting the conjecture that larger countries spend less on public goods. Second, when transfers and interest payments are added to government consumption and the regression is rerun, the point estimate is relatively stable but the significance drops markedly suggesting that per capita transfers are unrelated to country size. So, as expected, the effect is visible in public goods but not in transfers. Next, both Wacziarg (2001) and AW demonstrate that country size and openness are negatively related in the presence of a wide range of controls. And these results are replicated in regressions in which more direct measures of trade policy such as tariffs and measures of "outward orientation" are substituted for openness (see Sachs-Warner 1995).

The country size and openness explanations are clearly in direct competition and the interesting questions are: are both of these channels at work separately and if so what is their relative importance? After a detailed analysis of the competing hypotheses, AW propose that perhaps openness explains transfers while country size explains government consumption. In a series of regressions of categories of spending on both variables of interest and their set of controls, they find some evidence for this view. The key lesson to take away is the importance of examining specific categories of government expenditure. Simply cataloging correlations often fails to distinguish between theories whose broad predictions are similar. The high degree of colinearity between country size and openness makes it hard to tell the two theories apart, unless we focus on predictions at a different level of government expenditure. Disaggregating government expenditure into categories over which theories make different predictions can help return power to the tests. In this instance, AW's theory on the effects

of country size hypothesizes an association between population and public goods. In contrast, Rodrik's theory on the effects of openness hypothesizes an association between openness and expenditure on social protection in developed countries and an association between openness and expenditure on wages and salaries in developing countries. Data on government expenditures at the categorical level is available and it is at this level that the theories must be tested because it is here that they can be distinguished by unique predictions.

In this study, I use the log of population (rather than area) as the measure of country size. The issue here is which measure better captures the dimension over which public goods provision is likely to exhibit scale effects. In the categories of spending I examine most closely (education, healthcare, social protection), where spending is tied to individuals, population is the proper dimension. Area is more appropriate for defense spending or development indicators such as roads and telephone lines where the effectiveness of expenditure depends more explicitly on geographic considerations. The question then is, over which categories of government expenditure do we expect scale effects? AW find that per capita education spending seems to be lower in larger countries suggesting some form of increasing returns. One would also expect to see this in general public services, wages and salaries, and goods and services- categories which capture administrative costs that we expect to increase more slowly than population. On the other hand, spending on public safety, and especially healthcare and social protection seem likely to scale with population so we would expect no significant effect of country size (population) on per capita expenditure.

AW submit that the log of population is positively correlated with expenditures on public goods. I look at how the dependency of expenditure on population varies with the level of government in question. Where there are increasing returns to scale in the provision of public goods, providing the same level of service is less costly if administrative costs are spread over a larger population base. The relevant population base is the number of people within the jurisdiction of the government in question. In my study, I use national data on population but have data for three different levels of government: central, state, and local. The point is that the AW hypothesis, if true, implies a correlation between central government expenditures and national population, but not necessarily between *state and local* government expenditures and *national* population. This is so because larger national population necessarily implies

a larger population within the jurisdiction of the central government but does not necessarily imply a larger average population within the jurisdictions of state and local governments because the number of state and local governments is not fixed across countries. More populous countries have more people within the central government's jurisdiction and thus the fraction of GDP spent on public goods provided by the central government ought to be smaller if these public goods do exhibit scale effects. On the other hand, more populous countries probably also have a greater number of local jurisdictions. Thus the population of the average local district increases more slowly than that of the entire country. Thus, if the negative correlation between national population and central government expenditure on public goods as a share of GDP is due to scale effects in the provision of public goods, the correlation between national population and *state and local* expenditure shares ought to be much weaker.

Table 2.4 shows strong effects of population on expenditure on education, public order and safety, general public services, and transportation by the central government: exactly the categories one expects to display increasing returns to scale.[13] Table 2.5 shows that adding the complete set of controls tends to dampen the significance (due to both reduced magnitudes and increased standard errors) but does so without changing the sign of the point estimates.

Perhaps most importantly, Table 2.6 shows how state and local government expenditure displays a different relationship with country size. Namely, in each of the categories of public goods expenditure where we see a decline in expenditure by the central government, we see a countervailing (though more modest) increase in total expenditures by the state and local governments. This is exactly what we'd expect to see in the presence of scale effects, endogenous jurisdictions, and heterogeneity costs. As one progresses from smaller to larger countries, one ought to see scale effects work to reduce the cost of providing public goods at the national level. At the same time, state and local jurisdictions may grow in population leading to a reduction in expenditures. But there will also be more states, cities, and counties in a more populous country so growth of the population in the average local jurisdiction will be slower than growth in the national population. This would argue for a coefficient on

[13]Transportation might respond even better to measuring country size by area rather than population. On the other hand, maybe public investment in mass transit, the efficiency of which is tied to population density, is the most important driver.

country size in the state and local government regressions that was smaller than its central government counterpart, but still negative. The fact that we see a positive coefficient implies a role for the costs of heterogeneity. Assuming that increased population within a jurisdiction results in increasing heterogeneity of preferences over public spending, we would expect another partial effect of increasing country size. Namely, that citizens would prefer to return some spending prerogatives to state and local governments, where population, and thus heterogeneity, are growing less quickly than at the national level. Looking at the results for general government (the aggregation of central, state, and local) in Table 2.6 suggests that the savings from scale effects on the national level dominates the increased spending at state and local levels.

I also run the same regressions for government consumption, confirming the AW results of a very strong dependency of government consumption on country size. Table 2.7 presents these results. Delving into the ECOG categories, it becomes evident that most of the effect comes from wages and salaries rather than employer contributions or other purchases of goods and services. The fact that the effect comes through wages rather than goods and services implies that the returns to scale are in the public employment needed to achieve the distribution of public goods rather than the goods themselves.

The magnitudes of the coefficients are large. The coefficient on the (natural) log of population in regression 1 of table 2.6 is -0.305. One standard deviation in the log of population is roughly 1.8 in this sample. For calibration, this is roughly the difference between the current (2005) populations of the United States and South Korea. Increasing the size of a country by one standard deviation results in an average decrease in education expenditure by the central government of 0.55% of GDP though this is partially offset by expenditure at the state and local levels. The effect on total government consumption at the level of general government is even larger. Here a one standard deviation increase in population leads to a net decrease in total expenditure over all levels of government of 1.3% of GDP.

There are two results of note here. First, the sheer size of the effect and its robustness to different specifications demands attention. Clearly there are significant scale effects in the provision of education, transportation, and public safety, as well as some more general categories. Why education is among the categories displaying this behavior is not immediately obvious as it does not seem to be an industry in

which there are returns to increasing the scale of production. But the traditional fiscal federalism literature points out that the efficient location of an expenditure is the result of a tradeoff between the costs of uniform provision over a population of heterogeneous preferences and the benefits of spillovers. It is easy to believe that both of these forces are at play in the education industry. Perhaps rather than returns to scale in the provision of education, there are high costs to heterogeneity and a need to keep education policy relatively local. Thus, as populations expand, education policy is devolved from the central government to the local governments.

Which leads to the second important result. Namely, that a tendency to devolve policy prerogatives towards state and local governments in more populous countries seems to affect a number of categories of public goods. The results of table 2.6 suggest that both scale effects and costs of heterogeneity are important in the provision of public goods.

2.4.3 Fragmentation

Easterly and Levine (1997) report a strong negative correlation between indices of ethnic fragmentation and measures of public goods (telecommunications networks, transportation network, electricity grids, and education) in African countries. The implication is that greater ethnic fragmentation leads to lower spending on public goods. This may happen either because different ethnic groups have different preferences over the set of public goods to be provided and so fail to agree on expenditure or because an ethnic group's utility from public goods declines when the public goods are shared with other ethnic groups. Alesina-Baqir-Easterly (1999) document a body of work suggesting that preferences about public policy are correlated with ethnicity. They then submit that, in the presence of heterogeneous preferences (in this case driven by ethnicity), interest group activity may encourage, via log-rolling, an increase in targeted expenditure at the expense of public goods provision.[14]

The evidence is based on US fragmentation and expenditures data from three levels of aggregation - cities, metropolitan areas, and counties. Their primary result is the negative correlation between ethnic fragmentation and several measures of public goods expenditure including per capita spending on public education. They also note

[14]The authors note that when measured by expenditure share rather than in levels, the effects will appear even stronger.

that ethnic fragmentation is positively correlated with police spending, probably due to increased violent crime. Surprisingly, expenditure on health and hospitals increases with ethnic fragmentation. Total spending per capita is positively related to ethnic fragmentation in all three samples, supporting the log-rolling theory.

Interestingly, the authors rerun the regressions including both the ethnic fragmentation variable and a similar variable capturing only black vs. non-black heterogeneity. The broader fragmentation coefficient is still significantly different from zero, implying that the impact of ethnic fragmentation on public expenditure in American cities is not just a black vs. non-black issue.

In a follow-up, Alesina-Baqir-Easterly (2000) suggest that increased fragmentation may lead to higher levels of public employment as public officials circumvent opposition to explicit tax and transfer schemes by employing individuals who would otherwise receive transfers. They find some support using US city-level data.

More recently, Alesina et al. (2003) have investigated the effects of fragmentation on economic growth in a panel of almost one hundred countries. The first service of their paper is the development of separate indices for ethnic, linguistic, and religious fragmentation in nearly two hundred countries. Each index is meant to capture the probability that a random draw of two inhabitants will produce individuals of distinct (ethnic, linguistic, or religious) groups. This chapter uses the Alesina et al. (ADEKW) indices.

Among their results, ADEKW report that ethnic fragmentation is negatively associated with the ratio of transfers to GDP, confirming similar results found by Alesina-Glaser-Sacerdote (2001) and Alesina-Wacziarg (1998). They conclude that achieving consensus necessary for redistribution to the needy is more difficult in ethnically diverse societies. They achieve similar but less significant results for their index of linguistic fragmentation. Interestingly, they report a positive relationship between religious fragmentation and redistribution. To explain the difference between the result for religious fragmentation and those for ethnic and linguistic fragmentation, the authors note that while ethnicity and language are largely fixed, religious affiliation is flexible and therefore endogenous. Observed religious fragmentation is often the result of greater tolerance by the government or majority. And this tolerance (or factors leading to it) may explain both observed religious fragmentation and increased transfers.

ADEKW examine government size as one of the possible channels through which fractionalization affects growth. In this study, I break out government expenditure into its components and examine the dependence of those components on the ADEKW measures of fractionalization in the presence of controls. By examining which categories of expenditure respond most heavily to fractionalization, we can hope to shed light on exactly how fractionalization changes preferences for public goods.

From ADEKW we expect measures of fractionalization to be negatively correlated with expenditure on certain types of goods. In tables 2.8 and 2.9, I display regression results for several COFOG categories of publicly provided goods: education, healthcare, public order and safety, and general public goods. Table 2.8 gives a simple specification with minimal controls, Table 2.9 adds the complete set of controls, at the loss of half the countries and two thirds of the data points. Elsewhere, I have typically controlled for all three measures of fractionalization—ethnic, linguistic, and religious—and tested for joint significance. In this case, doing so simply adds a layer of complication to interpretation of the results. As ADEKW have noted, religious fractionalization is often associated with different effects than ethnic and linguistic fractionalization: in many of my regressions it takes the opposite sign. A test for joint significance isn't as useful in a situation where there are clearly multiple effects at work because one does not know which of the effects is driving the test result. In tables 2.8 and 2.9 I have omitted religious fractionalization as the purveyor of a different causal mechanism and include only one of the tightly related measures of ethnic and linguistic fractionalization. While the following discussion refers to tables 2.8 and 2.9 in which ethnic fractionalization is featured, the points remain valid for linguistic fractionalization.[15]

[15]The correlation between the ethnic and linguistic fractionalization coefficients is 0.7. in my full sample of ninety-plus countries. See Table 2.14. I have run the same set of regressions with linguistic fractionalization in place of ethnic fractionalization and the results are extremely similar in both the magnitude and significance of the point estimates in question. The effects of religious fractionalization are completely different. For example, religious fractionalization is correlated with more healthcare expenditure by the central government and less by the state and local governments. This centralization is exactly the opposite of the effect we see associated with ethnic and linguistic fractionalization. ADEKW suggest that religious fractionalization is unique among the three indices because it is endogenous. It is much easier to change your publicly professed religious affiliation than your ethnic or linguistic heritage. They speculate that countries with greater tolerance of minorities are likely to have greater recorded religious fragmentation and that perhaps it is the tolerance

Since the measures of fractionalization are national measures, we would expect to see significant negative effects on central government expenditure on public goods. However, following the logic of the previous section, we would expect the effects on local and state government expenditure to be either independent of national fractionalization, or possibly even positively related to it as public goods expenditure is taken care of by jurisdictions small enough to be relatively homogenous. Tables 2.8 and 2.9 bear out these hypotheses. In general, increased ethnic (or linguistic) fractionalization results in a decentralization of expenditure on education and healthcare without significant movements in public order and safety or general public services.

The basic tenet of fiscal federalism is that increased heterogeneity in preferences should lead to a devolution of fiscal policy prerogatives to lower levels of government where heterogeneity may be less severe. If we assume that ethnic or linguistic affiliation is correlated with preferences over public goods provision, then tables 2.8 and 2.9 can be interpreted as evidence of the principle of fiscal federalism in action: increased heterogeneity as measured by the index of fractionalization is accompanied by decentralization of expenditure. But is there support for the Easterly-Levine hypothesis that an ethnic group's utility from public goods declines when forced to share the public good with other ethnic groups?

On the one hand, education and healthcare involve significantly more interaction between consumers than do transportation networks and public safety. The fact that the fireman protects the homes of another ethnic group can hardly diminish the utility of having the fireman protect one's business. Transportation, telecommunications, and energy grids are similarly impersonal. By contrast, education and healthcare not only involve greater personal interaction in the classroom and waiting room, but may involve a significant skewing of benefits for one group or another. If rich white Californians see their tax dollars being spent on public schools from which they have largely withdrawn their children or hospitals which cater mainly to poorer Hispanics, they may be reluctant to spend public money on these goods.

On the other hand, is this really an example of a disutility to sharing—evidence of

which drives both religious fractionalization and expenditure on public works. Unfortunately, political rights—a decent proxy for "tolerance of minorities"—is completely uncorrelated with religious fractionalization in my sample (correlation of -0.02). See table 2.14.

an ethnocentric utility function? In fact, it can be explained quite well simply by allowing for differences in preferences across ethnicities, driven by persistent differences in wealth. The story about Californian voters doesn't require that white voters suffer a disutility from having their children in the same classrooms as Hispanic children. It simply requires that white voters be richer and less likely to enroll their children in public schools than Hispanic voters. The conclusion that can be drawn is simply that education and healthcare policy are more complex than policy over transportation and public safety and thus that the costs of heterogeneous preferences-driven by ethnicity, incomes, or any other source of heterogeneity-are higher. Thus we see a greater decentralization of expenditure in these complex categories in the face of heterogeneity. To disentangle ethnocentric utility from a simple correlation between ethnicity and preferences over public goods requires a different approach.

2.4.4 Income

One of the earliest hypotheses in the literature on government size is the view that the public sector tends to grow as a society becomes wealthier, commonly known as Wagner's Law. Wagner gave two main reasons in his original work. First, he postulated that as states grow more wealthy they simultaneously grow more complex, increasing the need for public regulatory and protective action to ensure the smooth workings of a modern, specialized economy. Second, he postulated that certain public goods, such as education and cultural enhancements, are luxury goods.

Writing in 1893, Wagner was pontificating on the effects of industrial revolution and urbanization. It's not immediately obvious that the same mechanism is at work in the information age. Nor is it clear why education should be thought of as a luxury good rather than an investment in human capital. Nonetheless, Wagner's Law is still heavily discussed and widely tested. The essence of Wagner's Law is the assertion that the ratio of civilian government expenditure (excluding defense spending) to GDP is positively related to GDP per capita.[16]

Henrekson (1993) notices that the bulk of the support for Wagner's Law derives from regressions in levels and, reiterating the main point from Granger and Newbold (1974), cautions that regression equations specified in levels of time series often lead to

[16]See Henrekson for a discussion of interpreting Wagner's theories and how to bring them to data.

erroneous inferences if the variables are non-stationary. He contends that income and the share of government expenditure, while correlated, are not, in fact, cointegrated, and demonstrates this in Swedish time series data from 1861-1990. He concludes that the correlation reported by other researchers may be spurious.[17]

Taking this lesson to heart, Oxley (1994) examines data on Britain from 1870-1913, and finds evidence that Wagner's Law holds and does satisfy Granger causality.[18] Unfortunately, while addressing the direction of causality, neither author includes a full set of control variables, thus failing to conclusively settle the issue of whether the data actually support Wagner's Law.

My original purpose in including income was as a control rather than to test Wagner's Law. In fact, as one of Wagner's main channels is through spending on regulation, which the GFS does not separately measure, my data is not well suited to a nuanced test of his theory. But whether or not Wagner's theory is correct, there is reason to believe the composition of demand for public expenditure changes with per capita income, making per capita income an important control. Defense and public safety are necessary functions of government which probably tend to grow more slowly with per capita income while products of the welfare state such as expenditure on healthcare and social protection are likely luxuries. As it turns out, analysis of the categories of expenditure offer a powerful explanation of the correlation between per capita income and government size: demographics.

Table 2.10 columns (1)-(8) reveal only lukewarm evidence for Wagner's assertion that education is a luxury and display absolutely no other patterns.[19]The surprising result here is that column (9) shows a very strong negative relationship between per capita income and total expenditure. In other words, I get the result that richer countries tend to have smaller government-the exact opposite of Wagner's Law. To make sense of this result, we need to separate out the within and the between effects as well as pinpointing the categorical source of this surprising result. The trend within most countries is for both income and government expenditure to rise over time, though they are not necessarily cointegrated. Wagner's Law hypothesizes a

[17]See Oxley for a list of papers testing Wagner's Law

[18]Oxley admits that the country and period were chosen to give cointegration of income and government expenditure the best possible chance.

[19]The corresponding regressions with the basic specification allowing more countries display similar results.

specific form of cointegration, namely that higher incomes lead to greater government expenditure.

Digging a little further shows first, that the negative relationship is indeed from within country variation rather than between country variation; and second, that removing the control for demographics completely reverses the results from table 2.10. To be blunt, richer countries tend to have more old people and thus tend to spend more on social security which drives greater total spending. To further test this result I regressed the difference between total expenditure and expenditure on social security on the same set of controls. This non-social-security expenditure declines with income, indicating that while social security is a luxury, the rest of government is a necessity. The elusive Wagner's Law is, in fact, simply a matter of demographics.

2.4.5 Income Distribution and Political Rights

In their seminal paper, Meltzer and Richard (1981) construct a general equilibrium model connecting the size of public sector redistribution (not public sector consumption) to the extent of the franchise and the distribution of wealth. Their economy is populated by a large number of individuals of heterogeneous labor productivity. These individuals perform two activities: they vote on a linear income tax rate whose proceeds are used to finance lump-sum redistribution and they make a labor-leisure choice. Citizens' tax preferences are aggregated by a voting rule. Meltzer and Richard (MR) consider the class of voting rules which result in choice of the tax rate by a decisive individual and focus primarily on majority rule. Under majority rule, the voter with the median income is decisive. Voters' tax preferences are single-peaked about an ideal point which is weakly monotonically decreasing in productivity (and thus in income). Workers with higher than average productivity and income prefer zero taxes and zero redistribution. Workers with lower than average productivity and income prefer positive amounts of taxation and redistribution but voluntarily limit the rate of taxation to limit the distortion of the labor supply. The further a voter's income below the mean, the higher is his preferred tax rate. MR conclude that redistribution in majority rule societies is positively related to a particular measure of skew in the income distribution: the difference between mean and median income.[20]

[20]Krussel and Rios-Rull (1999) extend the MR model to a dynamic setting which allows them to account for the distorting effects of a tax on capital. The result is an extension of the basic MR

In their own test of their model, Meltzer and Richard (1983) attempt to estimate a relationship derived directly from the model rather than a reduced form. Using annual data for the United States for the period 1936-1972, they estimate a linearized version of their solution for the optimal tax rate preferred by the median voter.[21] They use data on social security payments from the census bureau to compute the ratio of the mean to the median income. They split expenses into three categories: public goods, redistribution, and public supply of private goods. Public goods, which includes defense and public safety and some general public services, they discard as outside the bounds of the theory. Pure redistribution, which is basically social protection, is where their theory ought apply most strongly but they also expect to see results for public provision of private goods, which includes education, healthcare and some general public services. In fact, they report coefficients that are significantly different from zero, indicating that the general effect they describe is present, but which fall short of the expected values, indicating that the particular structural form they test is not a perfect description of the mechanism at work.[22]

The MR model also leads to a testable hypothesis concerning extensions of the franchise. Assuming the newly enfranchised earn a lower income than those who already enjoy political rights, an expansion of the franchise will result in a decline in the income of the median voter and thus an increase in the tax rate and level of transfers preferred by the median voter.

Several authors have attempted reduced form tests of this Meltzer-Richard hypothesis with specific natural experiments in mind. Lott and Kenny (1999) examine

result to the distribution of wealth as well as income. They conclude that the basic MR framework over-predicts taxation by omitting this second distortion.

[21]They use the little-known Stone-Geary utility function: $u(c,l) = \ln(c+\gamma) + a\ln(l+\lambda), \lambda > -1$. Where c and l are consumption and leisure respectively and a, *lambda*, and *gamma* are preference parameters. Note that if $\lambda = \gamma = 0$, the function specializes to Cobb-Douglas. The linearization of the resulting equilibrium is $\ln(t) + \ln(1-F) = \ln\frac{1+a}{a} + \ln(\frac{y_m}{y_d} - 1) - \frac{\gamma}{y_d}$ where y_m is mean income, y_d is income of the decisive voter, and F is the distribution of the marginal productivities of the voters. The first term on the RHS is a constant, the second term is the one of interest to their theory, and the third term is a reincarnation of Wagner's Law. One of the main arguments of this chapter is that tests of Wagner's Law must control for some measure of relative income (the second term on the RHS). Their test of Wagner's Law indicates that increasing income strongly increases redistribution but has little effect on public provision of private consumption. They note that the use of aggregate income instead of the reciprocal of median income makes little difference in the results.

[22]The structural form they bring to their model predicts coefficients of 1 while they estimate the coefficients in a range from 0.34 - 0.71 with standard errors on the order of 0.1.

the effects of women's suffrage and other extensions of the franchise on spending using US state level data on expenditure and turnout from 1870-1940. They regress several measures of expenditure (total, education, social services, highways) on measures of additional turnout due to several binary factors (legalization of women's suffrage, imposition of a poll tax, implementation of secret ballots, existence of a literacy test as a voting requirement) and some demographic data (a state's ethnic composition, the local wage rate, the composition of local jobs) plus state and time fixed effects. They find that the increase in voter turnout due to women's suffrage explains a large but not overwhelming part of the drastic increase in state expenditures during this time period: on the order of 20% of a 90% increase in expenditure over the 1910s.

Lott and Kenny note that removing poll taxes and literacy requirements effectively enfranchised poorer segments of the population. These variables, then, speak directly to the MR hypothesis while reminding us that the statutory franchise and effective franchise can differ markedly for a variety of reasons. But the main focus of Lott and Kenny is the extension of the franchise to women. Here the connection to MR is less obvious since, according to Lott and Kenny, the more liberal voting record of women on matters of public finance is not purely due to differences in income. This is an important reminder that while MR focus solely on income (and Krussel and Rios-Rull (1999) have extended the MR result to wealth), preferences over the extent and character of public expenditure vary with other voter characteristics as well. Variation in the extent of the franchise ought to be examined for two characteristics: (a) that a more inclusive franchise means a poorer median voter, and (b) whether increases in the franchise are correlated with other characteristics that correlate with preferences over public expenditure.

While MR limit their analysis to transfer payments, Kenny (1978) demonstrates that expanding the franchise to poorer voters will also have an effect on public goods consumption, but that the size and sign of the effect depends on the relative magnitudes of an income effect and a price elasticity. On the one hand, relative to rich citizens, poorer citizens will substitute from private consumption to public goods because the latter are subsidized by the rich through progressive taxation. On the other hand, poorer citizens will consume fewer public services in general (relative to their richer brethren) due to an income effect. The relative size of these effects determines the effect of extension of the franchise on the extent of government provision of public

goods.

To test this theory, Husted and Kenny (1997) look at the effects of the removal of poll taxes and literacy tests (effectively extensions of the franchise to poorer voters) on government expenditure using biennial US state and local data for 1950-1988. They document a strong increase in the size of welfare spending (transfers) as the decisive voter becomes poorer, confirming the basic MR result. The effect of moving the decisive voter down the income distribution is less clear in the case of government services (public goods). They find coefficients that are of both signs and largely insignificant, leading them to conclude that the income and substitution effects roughly cancel each other out.

Acemoglu and Robinson (2000) suggest another source of evidence of the MR effect. They observe that in Western societies historical peaks in the Kuznets curve—the time evolution of inequality in a developing society—have been followed by extensions of the franchise. They propose the observed extension of the franchise was in fact a deliberate, credible commitment to redistribution enacted by elites looking to quell civil unrest born of the unprecedented inequality.

There are at least three reasons for caution when interpreting tests of Meltzer-Richard. First, because new voters are not always poorer than old voters, changes in the franchise are an imperfect proxy for changes in the income of the decisive voter. Second, newly enfranchised groups may have different preferences due to factors other than income leading to other channels by which extensions of he franchise influence government expenditure. Third, effective representation may not be distributed as evenly as universal franchise suggests. If wealthier citizens are better represented in the political process, then the gap between mean and median income exaggerates the extent to which redistribution via the proposed mechanism will take place. Wealthier individuals may be better represented because they have more time and money to devote to political enterprise: to educating themselves about issues and candidates, to voting, and to pursuing political representation beyond voting via lobbying, contributing to candidates, and so on. While poor individuals can and do organize—in unions, for example—the overwhelming evidence is that the wealthy are more active in a wide variety of forms of political participation: voting, campaign contributions, contacting and working for lawmakers, boycotts, and demonstrations.[23] Lijphart

[23]See Lijphart (1997) for references documenting inequality in political participation in the US

(1997) notes that while voter turnout is less skewed toward the rich than other measures of participation, the pattern is persistent across advanced countries since the time of universal suffrage, and has widened over the past few decades as turnout in advanced democracies has declined.

Meltzer and Richard (MR) implicitly assumed the median income and the decisive vote belonged to one and the same citizen. Since they were dealing with the United States, this may have been a reasonable assumption. But in many countries political rights are, either de jure or de facto, restricted to a privileged minority. In such a case, the decisive voter may no longer earn the median income and thus have different preferences over taxation and redistribution. Since political rights and economic power go hand in hand, it is most likely that countries with restricted political rights will have a decisive voter with preferences weighted towards less taxation and redistribution.[24] In a cross-country sample, then, testing for the MR effect requires inclusion of an index of political rights as well as a measure of the skewness of the income distribution.

Alesina-Rodrik (1994), Bertola (1993), Perotti (1993), Persson-Tabellini (1994) and others have noted that if MR is true, then there is a link between income inequality and the rate of economic growth. Income inequality leads to greater redistribution which, because it is financed by distortionary taxation, will result in slower growth via reduced incentives to save and invest. To investigate the first stage of this proposed channel, Perotti (1996) performs a reduced-form cross-country study of the link between income inequality and redistribution. He includes a binary measure of democracy, the combined income shares of the 3rd and 4th quintiles, and the interaction between the two as well as controls for per capita income and educational attainment. After some investigation of alternate specifications, he concludes that

and other industrialized countries over the last century.

[24]Benabou (1996) points out that most studies simply assume deviations from democracy result in a move of the decisive voter toward wealthier individuals. In fact, Benabou asserts that dictatorships can be biased either for or against the poor. Thus, simply plugging in an indicator for democracy cannot capture the fact that the politically decisive voter may be either above or below the median income in a regime biased for or against wealth. While this is certainly true in theory, in practice, unless the political rights of the poor are upheld by democratic institutions, the rich tend to have a greater hand in political decisions and at the same time those with political power tend to enrich themselves. Either way, the correlation between income and political influence is more likely positive than negative. Thus I believe the Gastil index of political rights is a good proxy for the income ranking of the politically decisive household on fiscal matters.

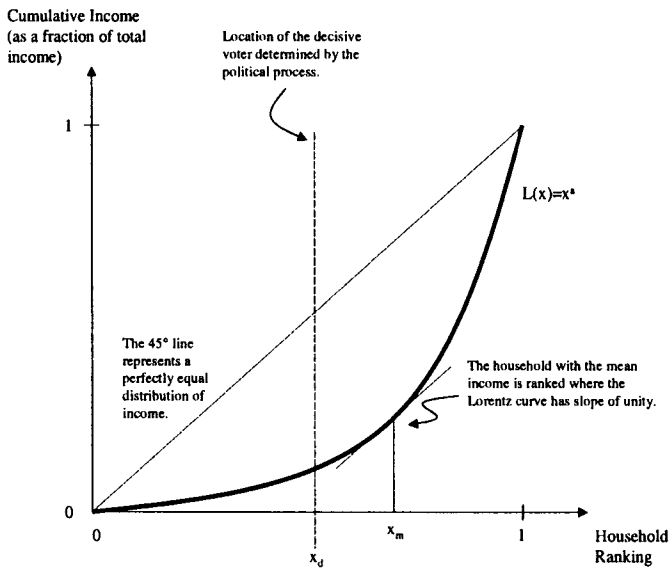
greater income inequality does lead to greater redistribution and that this effect is stronger in democracies. And he submits that these results are robust to a variety of measures of democracy and inequality. However, he finds no effect for the level of democracy alone. Furthermore, he finds the results are not robust to the exclusion of solitary datum. And most damningly, he concludes that the interaction effect is never very strong. His results are similar to those of Persson-Tabellini (1994) and indicative of the family of cross-country results.

When estimating reduced-form versions of MR and related theories, it is important to be honest about exactly how much distance exists between the theory and the regression. Benabou (1996) not only laments the lack of a formal model detailing the link between the extent of democracy and the effect of inequality on redistribution, he doubts the possibility of one without "restrictive functional assumptions." Indeed, there are several difficulties in founding the hypothesis more formally in a way that facilitates closing the gap between reduced-form empirical studies and the model they are intended to test. Benabou himself points one out when he notes that mapping a linear measure of democracy to the relative income of the decisive voter is problematic. Second, Alesina-Rodrik (1994) have challenged the assumption that social spending and transfers are always and everywhere progressive. If spending on social transfers benefits middle class at the expense of both rich and poor, then can evidence regarding such a measure shed light on models of redistributive fiscal policy that predict monotonic redistribution from rich to poor? It is hard to decide what to conclude from such measures. Finally, which measure of inequality ought one use to most closely capture the theory?

In this chapter I use the Gastil index of political rights which, after application of a Barro-transformation, runs from 0-1, a larger value indicating better political rights. My measure of income inequality is the Gini coefficient. I include both of these variables in levels as well as their interaction. Given the prevalent use of Gini data, it is important to construct a link between the Gini coefficient and the MR hypotheses. Consider the following discussion.

First, assume that we can approximate any country's Lorentz curve with the function $L(x) = x_a$ simply by varying the value of a. (see figure 2.1) The x-axis displays the rank of a household by income, normalized to run between 0 and 1. The Lorentz curve at point x plots the cumulative fraction of a society's income which

Figure 2.1: Using Political Rights to Extend Meltzer-Richard



is earned by the households ranked from 0 to x. The Lorentz curve of a society in which all households earned the same income would be a straight line with a slope of unity. The Lorentz curve of a society in which one household earned all the income would be the x-axis plus a point at (1,1). The location of the household with the mean income is given by the point of tangency of $L(x)$ with a line of unit slope and is denoted x_m.

Second, assume that any political regime can be characterized by a type x_d, indicating the ranking by income of the decisive voter over fiscal matters under the regime in question. In a direct democracy with universal suffrage, this decisive voter is the household with the median income: $x_d = 1/2$. In a regime that is biased towards wealthier households, $x_d > 1/2$. In the absence of a better measure of wealth bias, we will assume that increased political rights as measured by the Gastil index result in a decreased wealth bias. It is important to emphasize that while these assumptions may be reasonable, the functional form is completely ad hoc. There is no reason to suppose that the relationship between x_d and the inverse of the Gastil index is linear. The linear specification has been chosen as the simplest option in the face of ignorance. This difficulty in convincingly measuring the decisive household is the weakest link in cross-country studies of the MR theory.

The slope of the Lorentz curve, $L(x)$, at a point x corresponds to the ratio of the income of the household at point x to the average household income. The income of a household of rank x is given by the slope of the Lorentz curve at x. According to the MR theory, the extent of redistribution, R, is related to the difference between the income of the mean household and the income of the decisive household. So we can write down an expression for Meltzer-Richard redistribution in terms of the Lorentz curve.

$$R \equiv \frac{y_m - y_d}{y_m} = \left[1 - \left.\frac{\partial L(x)}{\partial x}\right|_{x_d}\right]$$

Now we can examine how MR redistribution changes if we shift the Lorentz curve or the position of the decisive voter. An increase in a corresponds to a general increase in inequality as the Lorentz curve shifts down and to the right. An increase in x_d corresponds to a *contraction* of the franchise as the decisive voter shifts towards greater income.

$$\frac{\partial}{\partial a}R = -[1 + a\ln(x_d)]x_d^{a-1}$$

$$\frac{\partial}{\partial x_d}R = -a(a-1)x_d^{a-2} < 0$$

$$\frac{\partial^2}{\partial a \partial x_d}R = -[(2a-1) + (a^2 - a)\ln x_d]x_d^{a-2}$$

It turns out that only one of these expressions can be signed over the entire domain: increasing the wealth-bias of the administration results in less redistribution. Changing the shape of the Lorentz curve is more complicated and can result in a change in either direction. However, over the majority of the domain, increasing the exponent a (greater inequality) results in greater redistribution. Basically, as the Lorentz curve shifts down and to the right, points where the slope of $L(x)$ is already less than one (which likely includes x_d) will likely see a decrease in slope. This is not true always and everywhere, but it is true for most combinations of $a > 1, x < x_m$. And a decrease in slope at x_d means the politically decisive voter has become poorer relative to the mean and thus demands more redistribution. Mathematically, it is also true that whenever $\partial R/\partial a > 0$, we also have $\partial^2 R/\partial a \partial x_d > 0$, implying the

cross-term is of the opposite sign. More inequality and a wider franchise each lead to more redistribution, but the effects are not complementary.

Intuitively, greater inequality usually means the decisive household loses ground relative to the mean household. However, the poorer is the decisive voter relative to the mean voter, the less effect widening the franchise can have on the ratio of the decisive household's income to the mean household's income. When the decisive voter is already poor, widening the franchise no longer changes the decisive voter from middle class to poor, it changes the decisive voter from poor to slightly poorer with less effect on redistribution.

Finally, we must consider which expenditure categories most closely embody the kind of redistribution portrayed in the MR model. The most obvious categories are vertically redistributive transfers like social protection. However, the transfers in the MR model are not vertically redistributive, but enjoyed equally across society. In this respect, various measures of public consumption such as government expenditure on healthcare, goods and services, and wages and salaries also ought exhibit the expected behavior, though pure public goods such as defense and public safety ought not. Education is a difficult case. While the benefits of primary and secondary education are widespread, the benefits of tertiary education are mainly enjoyed by households above the median income. As expenditure on tertiary education is a significant fraction of total expenditure on education, especially in developing countries, it is not really a clean test. In sum, if the extended MR model is correct, we would expect to see greater income inequality and a higher index of political rights each positively associated with redistribution and public consumption (social protection, healthcare, goods and services, wages and salaries) but not associated with pure public goods (defense, public safety). But we would expect the interaction term between income inequality and political rights to be negative.

Table 2.11 illustrates the effects of political rights and income inequality on three COFOG categories as well as the ECOG measure of transfers. It is in social protection (COFOG) or transfers (ECOG) that we expect to see the effects; education and healthcare have been included for comparison. And in fact, what we see is a vindication of the Meltzer-Richard theory. Both increased political rights and increased inequality (as measured by the Gini coefficient) result in strong increases in transfers (or social protection) but not so in other categories such as education or healthcare.

One standard deviation in the Gini is 9.4 points in this sample. An increase in inequality of one standard deviation is associated with an increase in transfers of over 2% of GDP. The magnitude of the coefficient on political rights is a bit harder to interpret because it's an ordinal index of dubious cardinality. Furthermore, because the index has been normalized to run between 1/7 and 1, the full coefficient looks enormous because it represents a difference slightly greater than between the most and least democratic nations. A one standard deviation change in this case would result in a 4% increase in transfers: clearly a large effect.

The other result of note is the strong interaction effect of inequality and democracy: it turns out to be negative and highly significant, as predicted by the model. In essence, if the country displays a highly unequal distribution of wealth, enfranchising additional poor voters changes the income of the median voter very little because the median voter is already poor. On the other hand, if the country has a relatively even distribution of wealth, enfranchising additional poor voters moves the decisive voter from the upper-middle class towards the lower-middle class, signaling a real change in the tax rate preferred by the decisive voter.

These results can be seen as a vindication of the maintained hypotheses as well as the Meltzer-Richard model. In particular, the notion that extending the franchise generally results in a poorer politically active median voter seems to be sustained. One alternative is that extensions of the franchise involve the addition of voters who have preferences for greater redistribution based on something other than wealth. For example, Lott and Kenny note that women often have different preferences over public expenditure than men of the same social class. This could certainly explain the coefficient on political rights. Increasing political rights leads to the enfranchisement of a class of voters with preferences for higher redistribution, thus shifting the preferences of the decisive voter towards greater redistribution. And with a stretch, it might be capable of explaining the effects of increased inequality as measured by the Gini. But the negative interaction effect makes alternate explanations based on more nuanced preferences much more difficult.

2.4.6 Demographics

Any attempt to explain variation in government expenditure must pay attention to variation in voter preferences. As direct measurement of such preferences is both laborious and problematic, demographic measures serve as a convenient proxy. I have already discussed how preferences over expenditure may be related to ethnic, linguistic, and cultural identity. There is another important demographic variable that is highly correlated with preference on matters of public finance: age.

Previous studies have controlled for the dependency ratio—the fraction of the population which is either too young or too old to work—which sums together the fractions of the population under 15 and over 65. In regressions on government size, this overlooks an important aspect of demand for expenditures. Retired voters and voters with dependent children display markedly different preferences for expenditure on public services such as education and social security. Therefore one ought instead split the dependency ratio into two separate measures—Under15 and Over65—to allow these measures to enter the regression with different coefficients.

Razin, Sadka, and Swagel (2002) provide an example of how neglecting to split the dependency ratio can produce bizarre results. The authors advertise the rather puzzling fact that in a sample including the US and 12 western European countries for 1965-1992, per capita social transfers are negatively related to the dependency ratio after controlling for openness, a measure of income skewness, per capita GDP growth, the unemployment rate, and the fraction of the employed who are in government pay in a regression with country fixed effects.[25] They proceed to construct an elegant story to demonstrate that this can arise as a special case in an extension of the Meltzer-Richard framework. The puzzle, however, is much more easily explained by correcting their specification to separate the dependency ratio into the fraction of the population under age 15 and the fraction of the population over 65. These two population groups ought to have extremely different effects on the demand for social transfers; restricting the coefficient on these groups to be equal is a mistake. Furthermore, since Under15 and Over65 are highly negatively correlated in most cross-country samples, using the dependency ratio eliminates most of the interesting variation.

Using my data on the same set of countries and similar time period (1970-1999),

[25] Razin-Sadka-Swagel Table 1.

table 2.12 replicates the Razin-Sadka-Swagel results and shows that when the proper specification is used, there is no puzzle. As expected, a greater fraction of retirees in the population is associated with a huge increase in per capita social transfers while the negative effect comes entirely from the youthful fraction of the population. The magnitude of the effect in table 2.12 demonstrates that age is an important control variable once correctly specified.

2.4.7 Political Institutions

Work by Persson, Roland, and Tabellini starts from the premise that a constitution is essentially an incomplete contract assigning decision-making rights to specific groups and individuals. Their goal is to compare alternative political institutions within models of the policy-making process built on rational voters and self-interested politicians. They examine institutional variation along two axes: electoral rules (majoritarian vs. proportional) and legislative structure (parliamentary vs. presidential).[26] One common mechanism in their models is that self-interested politicians siphon resources from public goods to targeted transfers to please a decisive coalition. The electoral rules or legislative structure dictate how to effectively pursue such a coalition-building strategy and thus affect the composition and magnitude of government spending.[27]

Electoral Rules

Their first model is a Downsian model of electoral competition with forward-looking voters. Contrasting majoritarian and proportional voting rules, they find that the former focuses electoral competition on a few key districts, leading to fewer public goods but more redistribution than the latter. In a related model Austen-Smith (2000) generate similar predictions. But Milesi-Ferretti, Perotti, and Rostagno (2002) generate a very different result, asserting that majoritarian systems should produce higher spending on public goods and lower spending on transfers when compared to

[26]See Persson-Roland-Tabellini (1998), Persson-Tabellini (1999), Persson-Tabellini (2000), Persson-Tabellini (2004).

[27]A second theme which is less relevant to this chapter is the attempt to limit politician's use of taxes to supply themselves with rents, a form of public tunneling which may or may not be explicitly illegal depending on the circumstance. In addition to the predictions on government size and the composition of spending, they also generate predictions on the degree of waste and misappropriation in government.

proportional systems.

Milesi-Ferretti, Perotti, and Rostagno (MfPR) derive their hypothesis from the differences between socially defined constituencies and geographically defined constituencies. Majoritarian systems elect one representative from each geographically-defined district. If the distribution of social groups is reasonably stable across districts, this results in a socially homogenous legislature in which legislators differ and thus are judged based on support delivered to their geographic constituency. For example, Barbara Boxer and Diane Feinstein are held accountable more for their representation of California than for their representation of women. As a result, representatives in a majoritarian system will be more concerned with obtaining fiscal support for their geographic constituency than for their social group. In contrast, proportional systems elect representatives who are beholden to a national constituency defined along social lines and so focus on payments to this socially defined constituency. Finally, they note that redistribution (unemployment, reeducation, welfare) is more easily targeted to social groups while public goods (military bases, highways, dams) are more easily targeted to geographic groups. They conclude that representatives under a majoritarian electoral system will pay more attention to spending which can be targeted to their constituents-public goods-while proportionally elected representatives will favor transfers to their social constituency. Hence the association between electoral rules and pattern of public expenditure.

So Persson-Roland-Tabellini and MfPR generate contradictory hypotheses concerning the effects of electoral rules on public expenditure.

Legislative Structure

Persson-Tabellini (1999) analyze legislative structure (presidential vs. parliamentary) in a model of legislative bargaining with retrospective voting. They conclude that the separation of powers which defines a presidential regime results in more competition between policy-makers and thus in smaller, more efficient government with less waste, less redistribution and lower expenditure on public goods. Because it places weight on legislative cohesion, the parliamentary regime is prone to frequent log-rolling and therefore produces larger, more wasteful government but with higher levels of public goods expenditures and more broadly targeted transfers. They conclude that there is a tradeoff between accountability and public-goods provision in legislative design.

And the resolution of this tradeoff has implications for patterns of government expenditure.[28]

Empirical Work

Persson-Tabellini (1999) (PT) test their hypotheses on both electoral systems and legislative structure using cross-country data from a sample of 64 countries classified as democracies in the period 1985-1990.[29] Their measure of government size is the ratio of total central government expenditures to GDP, taken from the IMF Government Financial Statistics. They argue that their theory, based on the institutions of central government, applies best to central government expenditures and try to account for fiscal decentralization in their set of controls. They define expenditures on public goods as the sum of transportation, education, and public order and safety, citing these as the expenditures categories with high public goods content.[30] Their controls include: log of per capita income, log of openness, fraction of population over 65, ethno-linguistic fragmentation, and the ratio of central to total expenditure which they use as a measure of centralization. They examine the effects of political institutions on both government size and public goods expenditure.

Their results offer qualified support for their theory.[31] A presidential system seems to strongly curtail the level of spending in the presence of either electoral system. This result is stronger in total expenditure but also significant in their measure of public goods. The results for electoral systems seem to be of the sign predicted by their own theory but are rather weak in total expenditure and only slightly stronger in the public goods regression.

Meanwhile, in a detailed examination of the effect of district size on expenditure, MfPR find considerable support for their hypothesis. They construct three measures of the degree of proportionality of electoral systems for 40 OECD and Latin American countries. They split government expenditure into three categories: primary

[28]See Persson-Tabellini 2004, Persson-Roland-Tabellini 2000, Persson-Tabellini 1999. For a description of the majoritarian and presidential variables, see Persson-Tabellini 2004. Because of the rarity of major constitutional design, these variables display almost no time-variation in the sample.

[29]Their threshold for democracy is a raw score of 5 or less in the Gastil index of political rights (lower means better rights).

[30]They justify their omission of defense spending by noting that it depends on geopolitical variables that are beyond the cope of the theory and difficult to control for.

[31]See Persson-Tabellini 1999 tables 1 and 2.

expenditure, transfers, and public goods. They proceed to regress each category of government expenditure on each of the three measures of the electoral system, controlling for the log of per capita GDP and the fraction of the population over 65. They find strong support in OECD countries for the proposition that governments elected under a majoritarian rule spend less on transfers than those elected under a proportional rule. Support in Latin American countries is weaker: coefficients are of the right sign but small in magnitude. Support for their hypothesis that majoritarian governments spend more on public goods is similarly weak.

At first review, the results of PT and MPR concerning the effects of electoral rules on government expenditure seem directly contradictory. The former give evidence that majoritarian governments spend less on public goods and claim it as evidence for their theory that majoritarian government skews spending toward targeted transfers. The latter give evidence that majoritarian governments spend less on targeted transfers and claim this supports the notion that majoritarian governments skew spending toward public goods. In fact, these claims are not explicitly contradictory. They are both consistent with the fact that majoritarian governments simply spend less across the board. Both studies offer less-than-complete support for their theories. On the one hand, PT work with a full range of controls, but document an effect on public goods only, completely omitting any work on transfers. On the other hand, MfPR do look at both transfers and public goods but include only minimal controls. Furthermore, they find a strong effect only in transfers. I show that in fact, when categories of government spending are examined individually with a full set of controls, majoritarian governments spend less on every single category.

The idea that majoritarian governments are less profligate is not new. Austen-Smith (2000) and MfPR (2002) both predict that total government expenditure is higher under proportional representation. PT (2004) confirm these predictions in a panel of 80 democracies during the 1990s. But PT (2004) limit their study to total expenditure and do not break out central government expenditure by category. This study shows that their results are replicated in each category of expenditure: that majoritarian government results in smaller expenditures across the board.

Of course, this does not necessarily falsify the theoretical claims of either PT (1999) or MfPR (2002). It could be the case that while a majoritarian government

lowers government expenditure for all categories, it simultaneously "tilts" the incidence of expenditure in favor of some categories at the expense of others. PT argue that the tilt is in favor of transfers at the expense of public goods; MfPR the opposite. The question, of course, is how to measure the tilt in the face of the evident shift. Since it is not possible to rigorously compare the coefficients from the various regressions, new econometric specifications will have to be brought to bear.

Possible Endogeneity

Noting that constitutions are not selected at random, but are in fact carefully crafted to fit the country, time period, and prevailing wisdom, PT (2004) worry that we cannot take constitutional characteristics (e.g. presidential regime, majoritarian electoral system) as exogenous in government spending regressions. The danger comes in two forms. First, if historical variables determining constitutional design also affect policy outcomes (and hence government spending patterns), then omitting any of the proper historical variables violates the conditional independence of the regressors and results in biased OLS coefficients (simultaneity bias). Second, the linear model conventionally estimated is usually interpreted as a local approximation of a more general model. PT point out that since we are comparing very different groups of countries, we might expect the true coefficients to vary across these groups, making the local approximation untenable. To guard against such dangers, PT (2004) focus on relaxing, in turn, both conditional independence and linearity.[32]

Despite the theoretical plausibility of the danger, it seems in practice to have little bite. In their own summary, PT (2004) justify the continued use of traditional panel techniques.

"The three sets of results paint a very consistent picture. If we are willing to assume conditional independence, given a large set of covariates, both constitutional effects are negative These results are robust to relaxing the conditional independence and linearity assumptions and they conform with our theoretical prior." Persson and Tabellini AER (2004)

[32]Persson-Tabellini (2004) discount the additional possible danger of direct feedback from expenditure policy to constitutions by noting that expenditure policy has changed a lot over the past forty years while constitutions have largely remained fixed.

In light of their results, I feel an instrumental variables approach constitutes unnecessary complication.

Table 2.13 presents results for the effects of political institutions for a wide range of spending categories. I've included eight of the major COFOG categories, one ECOG category (transfers), total expenditure, and a composite called public goods. The public goods category is an aggregation of transportation, education, and public order and safety, which matches the category by the same name on which PT 1999 focus.

The overwhelming result from 2.13 is that a majoritarian government reduces spending across the board. This is particularly true in the presence of a parliamentary system of government but also holds broadly under presidential systems. Unfortunately, this means that resolving the differing predictions of MfPR and PT regarding the spending habits of majoritarian governments will be more difficult. Majoritarian governments reduce spending across the board—whether they slash some categories more than others remains to be seen.

2.5 Conclusion

This chapter contributes to the literature on government expenditure in two ways, each of which pays significant dividends. First, the leading theories are examined collectively to avoid omitted variables bias. Second, disaggregating government expenditure by level and category has allowed a more careful examination of the favorite theories, leading to a number of interesting results.

While confirming Rodrik's hypothesis that increased trade openness correlates with greater expenditure on social protection in advanced economies, I find no evidence that developing countries take any steps to mitigate the undiversified income risk that comes with openness to trade. I find that as countries become more populous and more fragmented, expenditure on public goods declines at the national level and increases (though more modestly) at the state and local levels. This shifting from national to local is consistent with scale effects in the provision of public goods. It is also consistent with a federalist approach to preference heterogeneity: as national preferences become more heterogeneous, more spending decisions are shifted to state and local jurisdictions where preferences are relatively homogeneous. I find

that Wagner's Law is actually driven by demographics: richer countries tend to have more old people and thus tend to spend more on social security. Total spending net of social security actually declines with per capita income. I construct a framework for testing the Meltzer and Richard median-voter mechanism in a broad sample of countries with varying degress of political access and I find support for a modified version of the median-voter mechanism. Greater political access and a greater degree of income inequality each correlate with higher transfers as a fraction of GDP. As further evidence, the interaction term is negative: if a country is sufficiently inegalitarian, further enfranchisement has little effect on the wealth of the median voter, who is already poor, and thus has little effect on median-voter redistribution. Finally, I show that majoritarian governments not only incure lower total expenditure than governments elected by a proportional rule, but they spend less on both public goods and transfers, demonstrating that panel regressions of expenditure shares cannot distringuish between Milesi-Feretti, Perotti, and Rostagno (2002) and Persson and Tabellini (2004).

Figure 2.2: Two Methods for the Classification of Government Expenditures

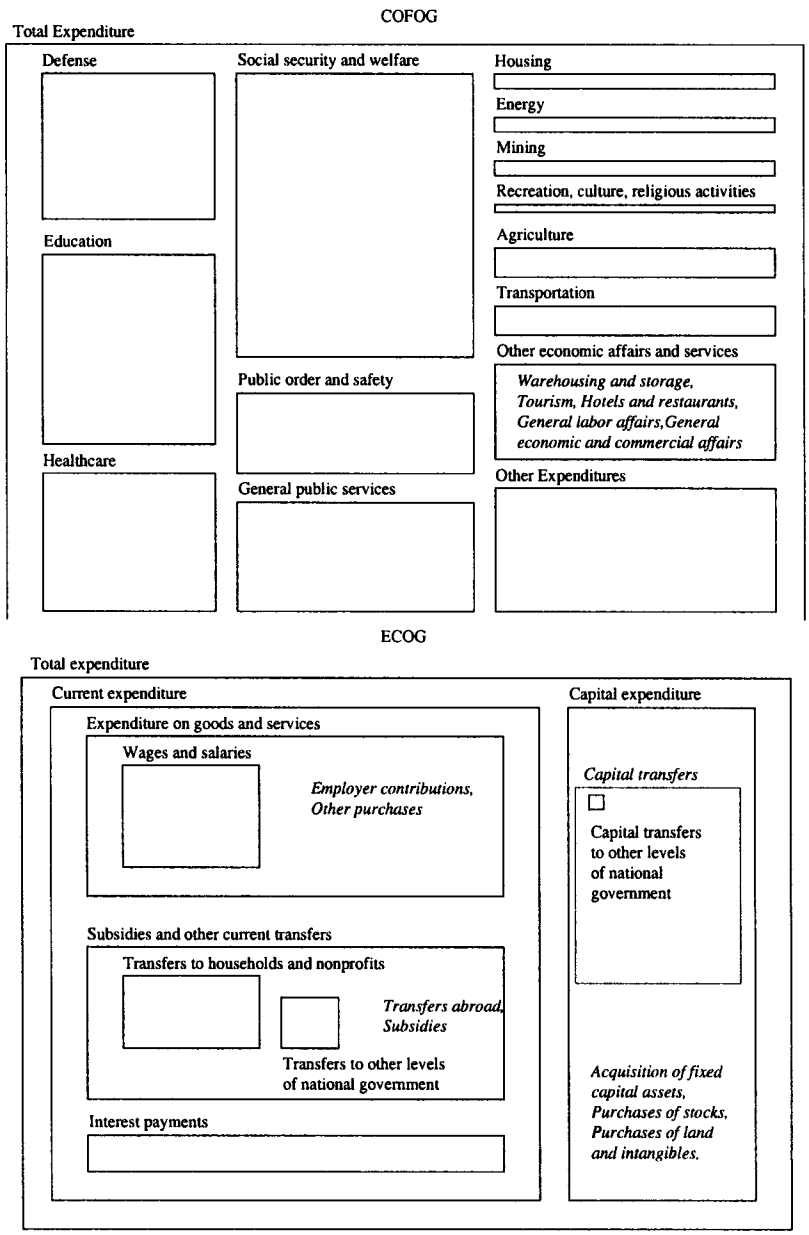


Table 2.1: Openness-I

α γ λ	(1) Social Protection (general)	(2) Social Protection (general)	(3) Wages and Salaries (general)	(4) Wages and Salaries (general)	(5) Total Expenditure (general)	(6) Total Expenditure (general)
Openness	0	-0.002	0.006	0.005	0.045	0.04
(X+M/GDP)*100	(0.007)	(0.006)	(0.006)	(0.006)	(0.019)*	(0.019)*
Openness*		0.027		0.009		0.082
OECD member 1975		(0.011)*		(0.009)		(0.032)*
observations	365	365	394	394	441	441
countries	90	90	94	94	99	99

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: log of population, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, plus a constant.

Table 2.2: Openness-II

	(1) Transfers (central)	(2) Transfers (central)
Openness (X+M/GDP)*100	0.005 (0.010)	0 (0.009)
Openness OECD member 1975		0.062 (0.016)**
observations	414	414
countries	96	96

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: log of population, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, plus a constant.

Table 2.3: Openness-III

	(1) Housing (central)	(2) Education (central)	(3) Other Econ Affairs (central)	(4) Other Expenditure (central)
Openness (X+M/GDP)*100	0.004 (0.001)**	0.007 (0.003)**	0.007 (0.002)**	0.02 (0.008)*
Openness OECD member 1975	-0.002 (0.002)	0.001 (0.005)	-0.002 (0.003)	0.011 (0.012)
observations	379	379	369	378
countries	92	92	91	92

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: log of population, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, plus a constant.

Table 2.4: Country Size-I

	(1) Defense (central)	(2) Education (central)	(3) Healthcare (central)	(4) Social Protection (central)
Log of population	0.037	-0.305	-0.194	-0 013
	(0.211)	(0.098)**	(0.108)	(0.228)
observations	358	379	378	365
countries	90	92	92	90

standard errors in parentheses
*significant at 5%, **significant at 1%

	(5) Public Order and Safety (central)	(6) General Public Services (central)	(7) Transportation (central)	(8) Total Expenditures (central)
Log of population	-0.243	-0.428	-0.200	-0.447
	(0.060)**	(0.130)**	(0.072)**	(0.591)
observations	212	377	371	441
countries	80	92	91	99

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: Openness, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, plus a constant.

Table 2.5: Country Size-II

	(1) Defense (central)	(2) Education (central)	(3) Healthcare (central)	(4) Social Protection (central)
Log of population	0.448	-0.246	0.33	0.145
	(0.386)	(0.164)	(0.190)	(0.420)
observations	150	151	151	145
countries	42	42	42	41

standard errors in parentheses
*significant at 5%, **significant at 1%

	(5) Public Order and Safety (central)	(6) General Public Services (central)	(7) Transportation (central)	(8) Total Expenditures (central)
Log of population	-0.094	-0.241	-0.351	1.122
	(0.080)	(0.162)	(0.110)**	(1.005)
observations	90	151	146	168
countries	32	42	41	44

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: Openness, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, index of political rights, Gini coefficient, Gini*political rights, majoritarian electoral, system dummy, presidential legislative system dummy, majoritarian*presidential, federal system indicator, plus a constant.

Table 2.6: Country Size-III

	(1) Education (central)	(2) Education (state+local)	(3) Education (general)
Log of Population	-0.305 (0.098)**	0.174 (0.106)	-0.14 (0.117)
observations	379	383	379
countries	92	92	92
standard errors in parentheses			
*significant at 5%, **significant at 1%			
	(4) Public Order and Safety (central)	(5) Public Order and Safety (state+local)	(6) Public Order and Safety (general)
Log of Population	-0.243 (0.060)**	0.061 (0.026)*	-0.186 (0.060)**
observations	212	237	212
countries	80	81	80
standard errors in parentheses			
*significant at 5%, **significant at 1%			
	(7) General Public Expenditure (central)	(8) General Public Expenditure (state+local)	(9) General Public Expenditure (general)
Log of Population	-0.428 (0.130)**	0.041 (0.033)	-0.382 (0.132)**
observations	377	381	377
countries	92	92	92
standard errors in parentheses			
*significant at 5%, **significant at 1%			

Controls: openness, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, plus a constant.

Table 2.7: Country Size-IV

	(1) Use of Goods and Services (aggregate)	(2) Use of Goods and Services (central)	(3) Use of Goods and Services (state+local)	(4) Use of Goods and Services (local)
Log of population	-0.709	-1.295	0.518	0.07
	(0.341)*	(0.300)**	(0.239)*	(0.206)
observations	413	413	417	437
countries	96	96	96	102

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: Openness, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, plus a constant.

Table 2.8: Fractionalization-I

	(1) Education (central)	(2) Education (state+local)	(3) Healthcare (central)	(4) Healthcare (state+local)
Log of population	-0.011	0.025	-0.012	0.014
	(0.007)	(0.007)**	(0.008)	(0.006)*
observations	379	383	378	382
countries	92	92	92	92

standard errors in parentheses
*significant at 5%, **significant at 1%

	(5) Public Order and Safety (central)	(6) Public Order and Safety (state+local)	(7) Transportation (central)	(8) Transportation (state+local)
Log of population	0.003	0.005	0.003	-0.002
	(0.004)	(0.002)**	(0.005)	(0.004)
observations	212	237	371	157
countries	80	81	91	46

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: Openness, log of GDP per capita, log of population, fraction of the population Under 15, fraction of the population Over 65, plus a constant.

Table 2.9: Fractionalization-II

	(1) Education (central)	(2) Education (state+local)	(3) Healthcare (central)	(4) Healthcare (state+local)
Log of population	-0.01	0.022	-0.041	0.019
	(0.011)	(0.013)	(0.015)**	(0.014)
observations	151	153	151	153
countries	42	42	42	42

standard errors in parentheses
*significant at 5%, **significant at 1%

	(5) Public Order and Safety (central)	(6) Public Order and Safety (state+local)	(7) Transportation (central)	(8) Transportation (state+local)
Log of population	0.003	0.004	0.016	0.007
	(0.005)	(0.003)	(0.007)*	(0.006)
observations	90	98	146	63
countries	32	34	41	21

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: Openness, log of GDP per capita, log of population, fraction of the population Under 15, fraction of the population Over 65, index of political rights, Gini coefficient, Gini*political rights, majoritarian electoral system dummy, presidential legislative system dummy, majoritarian*presidential, federal system indicator, plus a constant.

Table 2.10: A Look at Wagner's Law

	(1) Defense (aggregate)	(2) Education (aggregate)	(3) Healthcare (aggregate)
Log of per capita GDP	-0.487	0.533	-0.156
	(0.854)	(0.541)	(0.598)
observations	150	151	151
countries	42	42	42
standard errors in parentheses			
*significant at 5%, **significant at 1%			
	(4) Social Protection (aggregate)	(5) Public Order and Safety (aggregate)	(6) General Public Services (aggregate)
Log of per capita GDP	-0.143	0.115	-0.02
	(1.223)	(0.195)	(0.412)
observations	145	90	151
countries	41	32	42
standard errors in parentheses			
*significant at 5%, **significant at 1%			
	(7) Wages and Salaries (aggregate)	(8) Govt Consumption (aggregate)	(9) Total Expenditure (aggregate)
Log of per capita GDP	0.491	-0.177	-5.245
	(0.975)	(1.588)	(2.668)*
observations	158	164	168
countries	42	43	44
standard errors in parentheses			
*significant at 5%, **significant at 1%			

Controls: Openness, log of population, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, index of political rights, Gini coefficient, polrights*Gini, majoritarian electoral system dummy, presidential legislative system dummy, majoritarian*presidential, federal system indicator, plus a constant.

Table 2.11: Testing Meltzer-Richard

	(1) Education (central)	(2) Healthcare (central)	(3) Social Protection (central)	(4) Transfers (central)
Gastil Index of political rights with Barro transformation	2.367 (1.225)	-0.062 (1.828)	12.002 (4.262)**	19.464 (5.862)**
WIID Gini coefficient: high quality, entire population	0.034 (0.025)	-0.011 (0.036)	0.201 (0.084)*	0.231 (0.112)*
Gini*Index of Political Rights	-0.042 (0.030)	0.022 (0.044)	-0.271 (0.102)**	-0.433 (0.142)**
observations	151	151	145	164
countries	42	42	41	43

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: openness, log of population, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, majoritarian electoral system dummy, presidential legislative system dummy, majoritarian*presidential, federal system indicator, plus a constant.

Table 2.12: Dangers of Using the Dependency Ratio

	(1) log of per capita transfers	(2) log of per capita transfers
Openness (X+M/GDP)*100	0.00 (0.00)	0.00 (0.00)
WIID Gini Coefficient: high quality, entire population	0.01 (0.005)**	0.01 (0.004)**
Growth	-1.77 (0.703)*	-1.29 (0.540)*
Unemployment	0.04 (0.006)**	0.01 (0.005)**
Dependency Ratio	-0.07 (0.021)**	
Fraction of Population aged 0-14		-0.02 (0.02)
Fraction of Population aged 65+		0.18 (0.027)**
Constant	14.27 (0.751)**	9.93 (0.652)**
observations	159	178
countries	11	12
standard errors in parentheses *significant at 5%, **significant at 1%		

Table 2.13: Political Institutions

	(1) Education (central)	(2) healthcare (central)	(3) Social Protection (central)	(4) Public Order and Safety (central)
maj	-1.175	-0.616	-4.395	-0.102
	(0.664)	(0.728)	(1.520)**	(0.255)
pres	-2.021	-0.612	-1.404	-0.221
	(0.662)**	(0.737)	(1.538)	(0.312)
majpres	1.808	0.257	3.1	0.029
	(1.228)	(1.307)	(2.686)	(0.452)
observations	151	151	145	90
countries	42	42	41	32

standard errors in parentheses
*significant at 5%, **significant at 1%

	(5) General Public Services (central)	(6) Wages and Salaries (central)	(7) Govt Consumption (central)	(8) Transportation (central)
maj	-0.807	-0.414	-0.714	-0.877
	(0.615)	(1.161)	(1.959)	(0.412)*
pres	-1.981	-2.546	-3.149	-1.274
	(0.624)**	(1.124)*	(1.931)	(0.425)**
majpres	1.274	2.181	3.487	1.491
	(1.098)	(2.059)	(3.526)	(0.736)*
observations	151	158	164	146
countries	42	42	43	41

standard errors in parentheses
*significant at 5%, **significant at 1%

	(9) Transfers (central)	(10) Public Goods (central)	(11) total Expenditure (central)
maj	-5.249	-2.973	-7.063
	(2.353)*	(1.139)**	(3.960)
pres	-3.681	-4.15	-7.185
	(2.330)	(1.391)**	(3.975)
majpres	5.61	3.895	7.464
	(4.219)	(2.044)	(7.239)
observations	164	88	168
countries	43	32	44

standard errors in parentheses
*significant at 5%, **significant at 1%

Controls: log of population, log of GDP per capita, indices of ethnic, linguistic, and religious fractionalization, fraction of the population Under 15, fraction of the population Over 65, index of political rights, Gini coefficient, polrights*Gini federal system indicator, plus a constant.

Table 2.14: Sample Correlations

	log pop	log GDP	Open	Ethnic	Ling.	Relig.	15-	65+	p.rights
log(population in thousands)	1.00								
log(GDP per capita)	-0.05	1.00							
Openness (M+X)/Y	-0.54	0.23	1.00						
Ethnic fractionalization	0.00	-0.56	-0.06	1.00					
Linguistic fractionalization	0.04	-0.46	0.01	0.70	1.00				
Religious fractionalization	-0.02	-0.08	0.08	0.26	0.31	1.00			
% Under 15 years old	-0.09	-0.82	-0.19	0.58	0.40	0.05	1.00		
% Over 65 years old	0.02	0.77	0.11	-0.56	-0.37	-0.06	-0.93	1.00	
Political rights	-0.13	0.73	0.11	-0.45	-0.34	-0.02	-0.69	0.74	1.00

	log pop	log GDP	Open	Ethnic	15-	65+	p.rights	Gini	maj	pres
log(population in thousands)	1.00									
log(GDP per capita)	-0.15	1.00								
Openness (M+X)/Y	-0.66	0.17	1.00							
Ethnic fractionalization	0.10	-0.43	0.02	1.00						
Political rights	0.05	-0.84	-0.19	0.52	1.00					
% Over 65 years old	-0.11	0.83	0.16	-0.58	-0.94	1.00				
% Under 15 years old	-0.20	0.73	0.22	-0.41	-0.72	0.74	1.00			
Gini coefficient	0.12	-0.55	-0.27	0.37	0.73	-0.74	-0.63	1.00		
Majoritarian electoral system	0.237	-0.11	-0.05	0.18	0.08	-0.15	-0.03	0.07	1.00	
Presidential system of government	0.10	-0.43	-0.29	0.34	0.57	-0.55	-0.44	0.66	-0.20	1.00

Table 2.15: Summary Statistics

		Variable	Obs	Mean	Std. Dev. (panel)	Std. Dev. (between)	Std. Dev. (within)	Min	Max	Source
LHS										
general		total expenditure	227	43.545	15.087	13.619	5.664	9.223	86.938	IMF GFS
	COFOG	defense	393	2.938	3.728	2.576	2.322	0.043	46.154	IMF GFS
		education	121	5.578	1.843	1.610	0.944	1.076	10.471	IMF GFS
		health	116	5.008	2.607	2.103	0.777	0.338	14.980	IMF GFS
		social security	116	13.232	6.874	6.594	1.688	0.570	35.062	IMF GFS
		public order and safety	85	1.559	0.526	0.759	0.232	0.696	2.878	IMF GFS
		general public expenditure	121	2.952	1.545	2.255	1.233	0.520	10.450	IMF GFS
	ECOG	wages and salaries	175	9.435	3.203	3.146	1.791	2.957	18.024	IMF GFS
		goods and services	190	16.699	5.466	5.338	2.962	4.210	40.530	IMF GFS
central		total expenditure	501	29.789	11.801	10.472	5.117	8.406	96.220	IMF GFS
	COFOG	defense	393	2.931	3.727	2.575	2.322	0.043	46.154	IMF GFS
		education	428	3.486	1.833	1.690	0.656	0.184	12.184	IMF GFS
		health	427	2.296	1.753	1.703	0.609	0.130	7.927	IMF GFS
		social security	409	6.549	5.999	5.827	1.455	0.059	23.837	IMF GFS
		public order and safety	231	1.288	0.805	0.808	0.222	0.100	4.330	IMF GFS
		general public expenditure	426	3.197	2.601	2.369	1.208	0.248	15.892	IMF GFS
	ECOG	wages and salaries	443	6.729	3.729	3.644	1.335	0.422	21.519	IMF GFS
		goods and services	462	11.391	5.778	5.418	2.344	2.190	39.602	IMF GFS

Table 2.16: Summary Statistics cont.

		Variable	Obs	Mean	Std. Dev. (panel)	Std. Dev. (between)	Std. Dev. (within)	Min	Max	Source
LHS										
state		total expenditure	506	1.576	4.132	3.758	0.843	0.000	25.207	IMF GFS
	COFOG	defense	427	0.003	0.025	0.020	0.009	0.000	0.300	IMF GFS
		education	434	0.375	1.066	0.946	0.383	0.000	5.511	IMF GFS
		health	433	0.231	0.805	0.646	0.372	0.000	6.810	IMF GFS
		social security	432	0.163	0.570	0.475	0.247	0.000	4.856	IMF GFS
		public order and safety	261	0.114	0.308	0.234	0.046	0.000	1.302	IMF GFS
		general public expenditure	432	0.120	0.383	0.348	0.126	0.000	2.941	IMF GFS
	ECOG	wages and salaries	447	0.455	1.300	1.084	0.549	0.000	7.692	IMF GFS
		goods and services	466	0.777	2.126	1.783	0.813	0.000	12.936	IMF GFS
local		total expenditure	257	7.809	7.031	6.442	1.313	0.160	36.787	IMF GFS
	COFOG	defense	428	0.004	0.021	0.022	0.006	0.000	0.181	IMF GFS
		education	142	1.899	1.608	1.353	0.460	0.000	6.089	IMF GFS
		health	137	1.326	1.863	1.118	0.359	0.005	8.824	IMF GFS
		social security	131	1.507	3.323	1.630	0.394	0.001	18.184	IMF GFS
		public order and safety	119	0.265	0.290	0.191	0.057	0.002	1.402	IMF GFS
		general public expenditure	142	0.645	0.396	0.370	0.168	0.008	1.919	IMF GFS
	ECOG	wages and salaries	202	3.321	3.142	2.285	0.984	0.040	15.148	IMF GFS
		goods and services	213	5.194	4.490	3.617	1.381	0.058	19.167	IMF GFS

Table 2.17: Summary Statistics cont.

	Variable	Obs	Mean	Std. Dev. (panel)	Std. Dev. (between)	Std. Dev. (within)	Min	Max	Source
RHS									
	log(population in thousands)	630	8.93	1.81	1.85	0.16	3.73	14.02	PWT 6.1
	Openness (M+X)/Y	630	71.48	46.24	44.43	11.11	11.01	380.10	PWT 6.1
	log(GDP per capita)	629	8.44	1.03	1.01	0.19	6.22	10.51	PWT 6.1
	Religious fractionalization	693	43.27	23.52	23.31	0.00	0.20	86.00	ADEKW
	Ethnic fractionalization	689	43.30	25.94	25.93	0.00	0.00	93.00	ADEKW
	Linguistic fractionalization	672	38.01	28.89	28.80	0.00	0.20	92.30	ADEKW
	Political rights	708	0.47	0.33	0.31	0.13	0.14	1.00	Freedom House
	Gini coefficient	311	38.69	9.39	9.53	2.74	16.63	63.00	UN WIID
	% Under 15 years old	687	35.11	10.11	10.06	2.48	14.59	50.44	World Bank WDI
	% Over 65 years old	687	6.35	4.31	4.22	0.68	1.20	17.86	World Bank WDI
	Majoritarian electoral system	341	0.34	0.47	0.48	0.06	0.00	1.00	Persson-Tabellini
	Presidential system of government	347	0.36	0.48	0.49	0.00	0.00	1.00	Persson-Tabellini
	Federal system	347	0.21	0.41	0.40	0.00	0.00	1.00	Persson-Tabellini

Chapter 3

Tactical vs. Ideological Redistribution

The common picture of redistribution emphasizes at least two separate components: ideological redistribution to redress income disparity, and tactical redistribution for electoral gain. This conception raises two questions which remain largely unanswered. What are the relative magnitudes of these two types of redistribution? To whom is tactical redistribution directed? This second question is often refined to mean: do parties court swing voters or core supporters? I test a version of Dixit and Londregan's model of redistributive politics to measure the relative magnitudes of ideological and tactical redistribution, using Bartels' notion of voting power to broaden the notion of swing voter to include those whose turnout is uncertain. I find that a family's net transfers from the government do depend nontrivially on their electoral importance. However, this tactical distribution is swamped by ideologically-motivated redistribution based on family income which is 11-14 times larger. Looking at the components of net transfers suggests that most of the tactical redistribution toward pivotal demographics occurs through the tax code rather than cash and noncash transfers. My analysis of both voter choice and turnout data allows me to compare the benefits to a party's vote share of converting swing voters to those from mobilizing the core supporters. I find that mobilizing the core is two orders of magnitude more effective than conversion of swing voters. The relative paucity of tactical redistribution suggests candidates and parties have difficulty credibly committing to keep election promises which inhibits the use of tactical redistribution to buy households' votes.

3.1 Introduction

Work on redistribution distinguishes between two components: ideological redistribution to redress income disparity, and tactical redistribution for the electoral gain of political candidates. The relative magnitudes of these two types of transfers remains an open question. In a pair of papers, Dixit and Londregan (1995, 1998) have developed a model of electoral competition which serves as a tractable framework for analyzing the relative importance of ideological and tactical redistribution. In their framework, politicians make campaign promises of net transfers to electorally identifiable groups. They show that net transfers are a linear function of income and voting power; the former reflects ideological redistribution, the latter reflects tactical redistribution. An individual's voting power is the degree to which increasing the net transfers a candidate promises to that individual is increases the candidate's expected vote-count.

Dixit and Londregan emphasize that, in order to maximize their vote-counts, candidates will target groups with a high density of swing voters. Of course, expected vote count also depends on turnout. Since net transfers promised can affect a voter's propensity to turn out, this represents another channel by which tactical transfers can serve to increase vote count. Building on work by Bartels (1998), I extend the notion of a swing voter to include those voters whose turnout is most responsive to a change in the schedule of net transfers. As a result, voting power is a function of demographic characteristics which captures the degree to which increasing a voter's preference for one candidate (through promised transfers) will increase that candidate's expected vote-count.

To the best of my knowledge, existing work on redistribution has focused solely on aggregate measures of transfers. In this chapter, I use US micro-data on taxes and both cash and noncash transfers between households and government to examine how the *net* transfer position of households varies according to income and a measure of political power. Many studies look only at the transfers to a household without looking at the tax burden on that household. Since tax cuts obviously play an important role in electoral politics, this is a mistake. Micro data is compelling because it allows direct and comprehensive measurement of the transfers at the level where voting decisions are made. I choose to look at the family rather than the individual

voter because I assume that it is within the family that the impact of fiscal policy is felt: that transfers to any member of a family affect the voting behavior of all members of the family.

I use the Dixit and Londregan model augmented with Bartels' notion of voting power to measure the relative magnitudes of ideological redistribution toward poor households and tactical redistribution toward households with high voting power. I find that tactical redistribution towards electorally important households, while statistically positive, is dwarfed by redistribution toward poor households: the latter is between 11 and 14 times larger. Furthermore, voting power explains very little of the total variation in taxes and transfers, a further indication of the limited scope of tactical redistribution.

The chapter is organized as follows. The next section describes the the notion of voting power derived from Bartels, the Dixit-Londregan model, and the manner in which I use the available data to estimate construct an index of voting power and estimate the Dixit-Londregan model. Sections 3 and 4 describe the raw data and the constructed index of voting power. Section 5 gives the results of the estimation and interprets them in the light of an inability of candidates and parties to commit to future redistribution. Section 6 discusses possible problems with the measurement strategy and explains why they are unlikely to overturn the conclusions. The final section places the results in context.

3.2 Measurement Strategy

Dixit and Londregan (1995, 1998) provide a model of electoral competition in which transfers are both ideologically motivated and used to buy votes: both ideological and tactical redistribution exist. Two monolithic parties compete for control of fiscal policy. There exists an exogenous distribution of ex-ante income. Taxes and transfers can be targeted across identifiable electoral groups (retirees, blacks, married couples) to alter the distribution of ex-post consumption and parties can make credible commitments over fiscal policy. The left and right parties differ in their social welfare function and thus in their preferences for redistribution. The left party prefers an egalitarian distribution of ex-post consumption. The right party prefers ex-post consumption which mirrors ex-ante income. Parties care both about holding office and

subsequent policy.

The electorate consists of a number of electoral groups, distinguished by demographic characteristics such as age, gender, employment status, and so on. Parties commit to targeted tax and transfer policies, essentially choosing a consumption vector for each electoral group. Voters within a given electoral group have heterogeneous preferences over the distribution of consumption (a social welfare function) as well as their own consumption. Once the platforms are observed, the voters within a given group split into supporters for left and right.

The authors consider the platforms proposed in Nash equilibrium. The result is tax and transfer policy in which the net transfers to electoral group j are a linear function of that group's relative income and relative political power. The resulting first order conditions ensure that net transfers, T_j, (total transfers less total taxes) to a given group of voters, j, in excess of the average transfer, $\bar{T}$, is a separable linear function of the group's income, Y, and "political clout", π, relative to national averages $\bar{Y}$ and $\bar{\pi}$. In the following expression of equilibrium platforms, which is a rearrangement of equation (12) from Dixit and Londregan (1998), κ and η are positive constants.

$$T_j - \bar{T} = -\kappa(Y_j - \bar{Y}) + \eta(\pi_j - \bar{\pi}) \tag{3.1}$$

Essentially, the policy promised by each party is a combination of a linear income tax and group-specific transfers. The former delivers ideological redistribution, the latter serves the tactical purpose of vote-buying. Tactical redistribution is directed at those groups with "high political clout." Dixit and Londregan define political clout as a product of the density of swing voters within a particular group, j, combined with the degree to which voters in group j are willing to trade their ideals for money. Unfortunately, the latter, while conceptually interesting, is particularly difficult to operationalize. It is also hard to see how this would differ systematically across groups except as the marginal utility of income changes, in which case it would simply be a function of the group's income and therefore difficult to identify.

In the DL model, political clout springs from being part of a group with a lot of swing voters. Groups with a high density of swing voters attract tactical redistribution because they give parties a lot of bang for their buck. Conversion of swing voters is

an important motivation for party policy, but parties tuning policy to maximize votes also care about mobilization of those who already support the party (Bartels 1996). A proper measure of voting power ought to estimate both effects. In this chapter, I first estimate π and then estimate κ and η.

The difficulty in estimating equation 3.1 is that income may have both a direct and an indirect effect on net transfers. The direct effect operates through the social welfare function: voters prefer a more equal distribution of income thus parties propose redistribution from rich to poor. The indirect effect operates through the effect of wealth on voting power. I address this issue as well as possible of measurement error in my (estimated) measure of voting power in section 3.6.

The framework I use for estimating voting power as a function of demographic characteristics is developed by Bartels (1998). It is based on a random utility model of voting behavior.

$$v_i = x_i'\beta + \epsilon_i \tag{3.2}$$

$$t_i = x_i'\alpha + (x_i'\beta)^2\gamma + \delta_i \tag{3.3}$$

Here, t_i and v_i are a voter's utilities for turning out and for voting Republican conditional on turning out, δ_i and ϵ_i are uncorrelated i.i.d. standard normal random variables, and vectors α and β and scalar γ are parameters to be estimated. The x_i are a vector of characteristics defining the electoral group to which the individual belongs: race, age, gender, education, marital status, and employment status. So $x_i'\beta$ represents an individual's propensity to vote Republican due to race, age, gender, education, marital status, and employment status. All other factors that influence the individual's vote are represented in the random component, ϵ_i. Similarly, the turnout equation specifies the the voter's propensity to turnout as a function of the same vector of characteristics, x, as well as the voter's intensity of preference, $x_i'\beta$, plus a random term, δ_i, capturing unmeasured factors. The quadratic functional form, $(x_i'\beta)^2$ simply represents the idea that utility from turning out is an increasing function of absolute intensity of preferences.[1]

Within this framework of probabilistic voting, policy affects a voter's partisan

[1]The results are not dependent on the functional form of equation 3.3. Repeating the analysis with $|x_i'\beta|$ in place of $(x_i'\beta)^2$ gives similar final coefficients in the regressions in section 3.5.

propensity, $x_i'\beta$. Parties make policy promises to maximize their expected vote-share. As a result, certain voters—those for whom a small increase in net transfers will generate a large effect on turnout or vote choice—are more important than others. These voters are said to have higher voting power.

The contribution of voter i to the Republican margin of victory is given by $prob_i(turnout) * [prob_i(voteR|turnout) - prob_i(voteD|turnout)]$. In terms of the coefficients, this is

$$\begin{aligned} M &= \Phi(x_i'\beta)\Phi(x_i'\alpha + (x_i'\beta)^2\gamma) - [1 - \Phi(x_i'\beta)]\Phi(x_i'\alpha + (x_i'\beta)^2\gamma) \\ &= 2[\Phi(x_i'\beta) - .5]\Phi(x_i'\alpha + (x_i'\beta)^2\gamma) \end{aligned} \tag{3.4}$$

where Φ is the cdf and ϕ the pdf of the standard normal distribution.

Thus a vote-maximizing party will direct net transfers to those groups (defined by the vector of demographic characteristics, x) where they will get the greatest benefits in terms of increased vote-share. A party targets a program to group i, this increases group i's partisan propensity, $x_i'\beta$, which changes the expected vote-share.[2] The value to the party of a marginal change in a voter i's partisan propensity is given by

$$\pi \equiv \frac{\partial M}{\partial x_i'\beta} = 2\phi(x_i'\beta)\Phi(x_i'\alpha + (x_i'\beta)^2\gamma) + 2[\Phi(x_i'\beta) - .5]\phi(x_i'\alpha + (x_i'\beta)^2\gamma)2x_i'\beta\gamma \tag{3.5}$$

This object represents the *voting power* of individual i and can be calculated from α, β, and γ and x_i. Bartels' voting power includes both conversion of swing voters and mobilization of supporters.

In this chapter, I measure equation 3.1 using Bartels' voting power for π. To do so requires micro-data containing voting behavior (turnout and candidate choice), demographics, and taxes and transfers from the government. Unfortunately, no such data set exists. But I do have one data set with voting behavior and demographics and a second data set with demographics and tax and transfers. To solve the problem, I make the measurement in two steps. In the first step, I use micro-data on voting

[2]The maintained assumption here is that the program can be targeted to any group j but will have the same effect on partisan propensity no matter where it is targeted. In other words, the candidate's technology of transfer is equally efficient at reaching all groups.

behavior (v_i, t_i) and demographics (x_i) to estimate α, β, and γ, giving voting power (π_i) as a function of demographics (x_i). Then I take these coefficients α, β, and γ and take them to the second data set to calculate voting power for these individuals based on their demographic information. This gives voting power, income, and net transfers in the same data set, from which I estimate the following version of equation 3.1.

$$NetTransfers_{j,t} = Constant - \kappa * FamilyIncome_{j,t} + \eta * VotingPower_{j,t-2} + \epsilon_{jt} \quad (3.6)$$

The important thing to note about equation 3.6 is the timing. Voting power is estimated from an election in year t-2 and then paired with tax and transfer data from year t. Waiting two years until the end of the Congressional term ensures measuring what the elected Congress has enacted during its term rather than judging it based on preceding Congresses. Allowing for longer delays between voting power and transfers does not significantly change the pattern of results. Finally, the basic unit of observation for these regressions is the family. Family voting power is simply the sum of voting power for all voting age individuals in a family.

3.3 Data

The two data sets I use are the American National Election Study and the Current Population Survey. There are three types of data: voting data, demographic information, and data on government taxes and transfers. The American National Election Study contains demographic information and voting behavior for a national sample of 1500-2000 people every second year. The Current Population Study is much larger, covering between 130,000 and 180,000 individuals in 55,000 - 65,000 households each year.

Turnout data includes both whether the respondent went to the polls and whether they voted in that particular race. Vote choice is coded by party. Family income includes wage income but not dividend payments or capital gains. Cash transfers consist of disability benefits, education assistance, financial assistance, worker's compensation, public assistance and welfare, veteran's benefits, unemployment compensation,

and survivor's benefits. Noncash transfers consist of the estimated market value of housing subsidies, medicare, medicaid, free school lunches, food stamps, and energy assistance. Taxes include federal taxes, state taxes, and property taxes. Net transfers are the sum of cash and noncash transfers less total taxes. I have data for 1988 - 1998 which enables five snapshots using voting data for the elections for the House of Representatives of 1988, 1990, 1992, 1994, and 1996: the 101^{st} through 105^{th} Congresses. [3]

3.4 Voting Power: Individuals and Families

After estimating equations 3.2 and 3.3 using ANES data from the 1996 election and then calculating equation 3.5 for the 1998 CPS data, I have values of voting power for 94,990 individuals aged 18 and over forming 56,241 families. Among individuals, voting power ranges from 0.088 to 0.832 with a mean of .581 and a standard deviation of .170. Recall that parties direct transfers to both convert swing voters and mobilize likely supporters. An individual's voting power is a function of his susceptibility to conversion and mobilization and equation 3.5 for voting power can be broken down into these two components, which I refer to as a voter's conversion and mobilization factors. It turns out that voting power is driven mostly by the mobilization factor: conversion factors are two orders of magnitude smaller.[4] Perhaps not surprisingly, conversion and mobilization factors display a strong negative raw correlation (-0.66). The more strongly a voter supports a party, the less additional ideological persuasion can do to increase the likelihood of that voter supporting the party in the upcoming election. However, the more strongly a voter supports a party, the greater the return (in expected votes) to increasing the probability that voter actually turns out to vote.

The party directs transfers to group i to increase their partisan preference, $\beta * x_i$. This also has an indirect effect on turnout through γ. These results indicate that the indirect effect on turnout contributes far more to expected vote-share than the direct

[3] I have also estimated equation 3.6 using data from the Presidential elections of 1988, 1992, 1996. Coefficients of family voting power calculated based on voting behavior for House and Presidential elections are highly correlated (.95). Thus the Presidential regressions essentially repeat the story told in the House regressions.

[4] This ratio is not dependent on the functional form of equation 3.3. Repeating the analysis with $|x_i'\beta|$ in place of $(x_i'\beta)^2$ gives the same dominance of mobilization over conversion.

Table 3.1: Voting Power by Demographic Groups

Group	Group Mean	Partial Effect
(Married)	0.674	baseline
Widowed	0.567	-0.069
		(0.001)**
Single/Divorced	0.422	-0.218
		(0.000)**
(White)	0.615	baseline
Black	0.322	-0.222
		(0.001)**
American Indian	0.395	-0.156
		(0.002)**
(Male)	0.579	baseline
Female	0.583	0.008
		(0.000)**
(18-30)	0.494	baseline
30-39	0.592	0
		(0.001)
40-49	0.607	0
		(0.001)
50-64	0.613	-0.003
		(0.001)**
65+	0.608	-0.009
		(0.001)**
(No H.S. Degree)	0.448	baseline
H.S. Degree	0.569	0.12
		(0.001)**
College Degree	0.727	0.256
		(0.001)**
(Employed)	0.58	baseline
Unemployed	0.594	0.049
		(0.001)**

effect on vote-choice. Therefore, in order to maximize expected votes, political parties ought to direct the lion's share of their efforts to mobilizing their core supporters rather than battling for ideologically moderate voters.

Table 3.1 shows a number of interesting comparisons of individual voting power across demographic groups. The first column reports the group mean while the second column reports the coefficient and standard error from the regression of voting power on dummy variables for group membership. Those on the low end of the distribution tend to be black or American Indian, unmarried, and lacking high school education. Those at the very highest end tend to be well-educated, married women. Married voters tend to have significantly higher *individual* voting power than non-married

voters. The gender gap in voting power favors women ever so slightly but women display moderately greater variation in voting power so they outnumber men at both ends of the distribution. The voting power of blacks is barely *half* that of non-blacks. The ANES and CPS data on ethnicity are coarse, labelling many groups as simply 'white'. However, data on Native Americans is available and they also exhibit significantly lower voting power than the rest of the population. The youngest voters, those under age 30, average lower voting power than the rest, but there seems to be little difference between middle-aged voters and retired voters. Interestingly, employment status has virtually no bearing on voting power. Education is an extremely strong indicator of voting power: the voting power of college graduates is 28% higher than that of high school graduates and 62% greater than that of those who have not completed high school.

The regression results in the second column helps sort out which effects are drivers and which effects are simply due to correlations among demographic characteristics. For example, education and marital status remain important factors but the low voting power of the youngest voters (those aged 18 to 30) seems due mainly to the fact that they tend to be less educated (having had less time to finish school) and less likely to be married. Much of the lower average voting power among blacks is due to variation in other demographic variables such as education and marital status. However, racial identification remains significant even when other demographic factors are controlled for.

The dominance of mobilization over conversion and the importance of education suggest to me that the voting power of a demographic group is tied to how politically informed is the average voter in that group. The importance of marriage and the gradual decline in voting power of older voters may indicate that increasing social connections leads to a greater propensity to engage in civic life.

The CPS defines a family as persons "residing together and related by birth, marriage, or adoption." In the 1998 data, the number of voting-age members in the family varies from 1-8 though the overwhelming majority are 1 and 2 voter families: 25,018 single-voter families, 25,329 dual-voter families, 4,542 three-voter families, and 1,352 families with more than three voting-age members. Family voting power ranges from .088 to 4.22 with a mean of .981 and a standard deviation of .540. The distribution of family voting power is essentially bimodal plus a right tail. The two

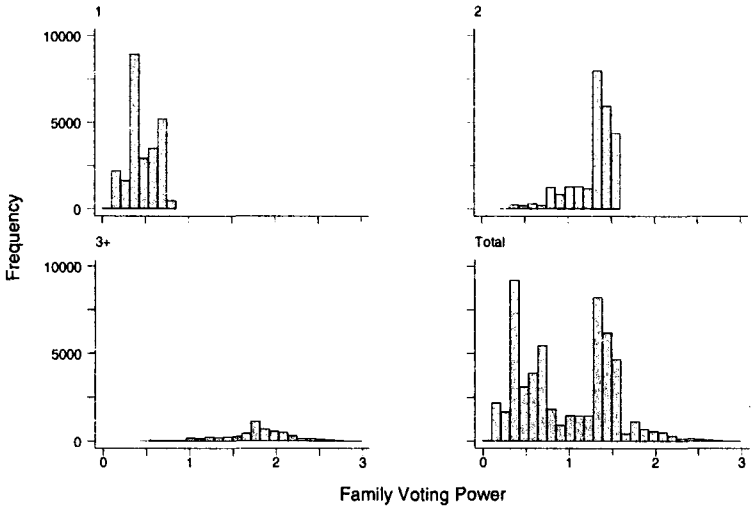


Figure 3.1: Histogram of Family Voting Power by Number of Voters

modes correspond to single- and dual-voter families and the tail is largely due to families with three or more voters.

3.5 Results

The results of the estimation are displayed in table 3.2. Because the independent variables—voting power and income—have such different ranges, I have normalized the variables to make the coefficients comparable. The coefficients have been scaled by the standard deviation of the independent variable so that each coefficient represents the marginal change in net transfers due to a one standard deviation increase in the independent variable.

These basic results are interesting in several ways. Looking at column 1, we see the expected signs: higher income means lower net transfers, greater voting power is associated with an increase in net transfers. The signs accord with the Dixit-Londregan model. But while both these effects are strongly statistically different from zero, κ is an order of magnitude larger than η. The coefficients reported in table 3.2 are for the 105th Congress (1997-98). If we look at the entire sample (101st-105th Congress), the ratio of κ to η ranges from 11 to 14. While κ is larger than η in every snapshot, η is significant and of non-trivial magnitude. Increasing a family's voting

Table 3.2: Effects of Voting Power: 105th Congress

	(1) Net Transfers	(2) Cash Transfers	(3) Noncash Transfers	(4) Taxes
Congress	105^{th}	105^{th}	105^{th}	105^{th}
income $(-\kappa)$	-15503	233	-463	15273
	(47.1)**	(21.5)**	(13.7)**	(36.5)**
voting power (η)	1343	53	521	-769
	(47.1)**	(21.5)**	(13.7)**	(36.4)**
constant	4812	840	1005	-2966
	(90.5)**	(41.3)**	(26.3)**	(70.0)**
observations	56241	56241	56241	56241
R-squared	0.69	0.00	0.03	0.79

standard errors in parentheses
*significant at 5%, **significant at 1%

power in 1996 by one standard deviation increases that family's net transfers in 1998 by roughly \$1300. At this point it is not yet clear what we ought to conclude. While η is certainly non-trivial, it is clearly swamped by κ. Does that make it large or small? What does that imply about the ability of a campaigning candidate to commit to future redistributive policy?

In columns 2-4 of table 3.2, I've broken out the dependent variable into cash transfers, noncash transfers, and taxes to get a better idea of which are the instruments of vote-buying. Again, the coefficients have been normalized to represent the marginal effect of a one standard deviation change in the independent variable. The coefficients on voting power imply that during the 105th Congress, tactical redistribution was directed largely through taxes and noncash transfers while cash transfers are a relatively minor channel.

But it is the R^2 on these regressions that are particularly informative. Notice that the R^2 is high only for taxes which, because the tax code is based on income, is well explained by income. But the R^2 for the other two components of net transfers, which are not directly tied to income, are essentially zero. This suggests that the high R^2 on taxes is due entirely to income and voting power explains almost none of the variation in net transfers. While increased electoral influence does lead to some financial gain, the effect is not a major determinant of transfers and is overwhelmed by income-based redistribution.

The third salient fact about tactical redistribution is that only the smallest component—cash transfers—seems to display partisan effects. Parties in the Dixit-Londregan model propose different tax rates and different group-specific transfers. If redistribution were, in fact, being credibly promised to court voters and subsequently delivered, we would expect such partisan effects. A switch in control of Congress would lead to a switch from a platform based on one party's ideology to one based on the competing party's ideology. One could simply argue that parties' platforms aren't terribly different. After all, Downsian convergence ought not be a surprise. But in this case, I have already shown that mobilization factors dominate conversion factors. This ought to lead parties to court their core voters which would be very different sets of voters.

Table 3.3 shows the progression of the coefficients for the regression of cash transfers on family income and voting power in the 101st-105th Congresses. Recall that control of both the House and Senate switched from Democrat to Republican in 1995. The response of cash transfers to voting power seems to display an effect that mirrors this changes in partisan control of Congress. Notice that η for cash transfers goes from being significant and negative for the Democratic 101st-103rd Congresses to being virtually zero and then significant and positive during the Republican 104th and 105th Congresses. Neither noncash transfers nor taxes display such a strong partisan pattern. (see tables 3.4 and 3.5) Recall that these two components seem to be the much larger part of tactical redistribution, yet they show much smaller partisan differences.

Another way of making the previous point is to look at the fitted values for the average citizen from each income quintile. The first part of table 3.6 breaks out the receipt of transfers for the average individual from each family income quintile. Families are separated into quintiles by income and then, within each quintile, the demographic characteristics are averaged across individuals to calculate the fictional 'average' member of the quintile. For example, the average individual from the third income quintile is 65% married and 9% black while the average individual from the fifth quintile is 28% married and 15% black. The estimates of α, β, and γ are then used to calculate this average individual's voting power, π. Finally, the average individual's voting power and income are used with the estimated κ and η to calculate the fitted values of net transfers to the average individual. For example, the average

Table 3.3: Partisan Effect in Cash Transfers

	(1) Cash Transfers	(2) Cash Transfers	(3) Cash Transfers	(4) Cash Transfers	(5) Cash Tranfers
Congress	101^{st}	102^{nd}	103^{rd}	104^{th}	105^{th}
income $(-\kappa)$	413	361	651	349	233
	(16.8)	(18.8)**	(22.5)**	(23.0)**	(21.5)**
voting power (η)	-267	-139	-293	-11	53
	(16.8)**	(18.8)**	(22.5)**	(23.0)	(21.5)**
constant	1041	1025	1134	933	840
	(30.8)**	(29.6)**	(36.5)**	(42.6)**	(41.3)**
observations	65903	65494	63448	55394	56241
R-squared	0.01	0.01	0.01	0.01	0.00

standard errors in parentheses
*significant at 5%, **significant at 1%

Table 3.4: No Partisan Effect in Non-Cash Transfers

	(1) Non-Cash Transfers	(2) Non-Cash Transfers	(3) Non-Cash Transfers	(4) Non-Cash Transfers	(5) Non-Cash Tranfers
Congress	101^{st}	102^{nd}	103^{rd}	104^{th}	105^{th}
income $(-\kappa)$	-181	-437	-403	-474	-463
	(7.5)**	(9.2)**	(11.0)**	(12.9)**	(13.7)**
voting power (η)	-69	358	233	480	521
	(7.5)**	(9.2)**	(11.0)**	(12.9)**	(13.7)**
constant	1238	1043	1380	1036	1005
	(13.8)**	(14.5)**	(17.8)**	(23.9)**	(26.3)**
observations	65903	65494	63448	55394	56241
R-squared	0.02	0.04	0.02	0.03	0.03

standard errors in parentheses
*significant at 5%, **significant at 1%

individual from a family in the third income quintile pays $5,817 in net transfers. He would receive $4,812 if he had zero income and voting power. He receives another $1,563 because of his voting power but then pays $12,191 because of his income level. The bottom half of table 3.6 shows the results from the same process for the average individual from each quintile of family voting power.

Now compare the source of net transfers by income-quintile with the table by voting power-quintile. My previous statement that the variation in income matters more than the variation in voting power is essentially the fact that κ*income varies more between the income quintiles in than η*voting power does between the voting power quintiles. Since we're interested in the relative magnitudes of tactical and

Table 3.5: No Partisan Effect in Taxes

	(1)	(2)	(3)	(4)	(5)
	Taxes	Taxes	Taxes	Taxes	Taxes
Congress	101^{st}	102^{nd}	103^{rd}	104^{th}	105^{th}
income ($-\kappa$)	10335	9343	11226	13183	15273
	(31.9)**	(29.3)**	(40.5)**	(34.5)**	(36.5)**
voting power (η)	-1216	-469	-913	-574	-769
	(31.9)**	(29.3)**	(40.5)**	(34.5)**	(36.4)**
constant	-2745	-3206	-3606	-3008	-2966
	(58.6)**	(46.0)**	(65.7)**	(63.8)**	(70.0)**
observations	65903	65494	63448	55394	56241
R-squared	0.65	0.68	0.61	0.77	0.79

standard errors in parentheses
*significant at 5%, **significant at 1%

Table 3.6: The 'Average' Individual

Family Income Quintile	Total Net Transfers	κ*Family Income	η*Family Voting Power	Constant
5th	3276	-2772	1237	4812
4th	-993	-7240	1436	4812
3rd	-5817	-12191	1563	4812
2nd	-12252	-18726	1662	4812
1st	-33013	-39577	1753	4812
Family Voting Power Quintile	**Total Net Transfers**	**κ*Family Income**	**η*Family Voting Power**	**Constant**
5th	-184	-5774	778	4812
4th	-3226	-9315	1278	4812
3rd	-5797	-12031	1423	4812
2nd	-9526	-16046	1708	4812
1st	-19800	-26379	1767	4812

ideological redistribution, the question is not simply whether those with high voting power benefit significantly more than those with low voting power. It is whether the benefits to high voting power are comparable to the penalties of high income. The difference in the transfers received due to tactical redistribution between those with the highest voting power and those with the lowest voting power is \$1,767 - \$778 = \$989. Compare that \$989, the difference between an influential voter and an unimportant one, to the difference between a rich voter and a poor one. Here the difference in net redistribution is \$39,577 - \$2,772 = \$36,805. In other words, the difference in net redistribution between a rich and a poor voter is 37 times the difference in net redistribution between an influential voter and an unimportant voter.

The overwhelming majority of the variation in net redistribution is due to progressive income-based redistribution rather than transfers to influential voters.

In sum, (a) tactical redistribution is positive and significant but explains almost none of the total variation in net transfers; (b) tactical redistribution (η) is an order of magnitude smaller than ideological redistribution (κ); and (c) only the least part of tactical redistribution displays the partisan effects one would expect. Together, these imply that while tactical redistribution does take place, it is a relatively minor part of total redistribution.

One reason why this might be the case is that voters could be relatively immune to pecuniary persuasion. If voters care far more about basic ideological issues governing the entire polity—such as how much the ex-post distribution of consumption differs from the ex-ante distribution of income—than about personal transfers, then the expense of buying votes would limit tactical redistribution. There is a wealth of macroeconomic evidence that voters respond to the state of the economy (Kramer, Fair, Tufte, Lewis-Beck). And the microeconomic origins of these correlations between vote-share and macroeconomic indicators are still debated. It is accepted that the economic condition is important, but it is not clear whose economic condition is paramount. Do voters respond to their own family's economic condition—so called pocketbook voting—or are they sociotropic, voting in response to the health of the entire macroeconomy? But Kramer (1983) shows that many of the early studies finding no evidence of pocketbook voting are flawed. And, taking Kramer's methodological criticisms into account, Markus (1988) does find evidence of pocketbook voting. In the review of the literature, Achen and Bartels (2004) argue that on balance, the evidence supports the view that voters do respond to their pocketbooks.

Alternately, these transfers may be used as tactical instruments of electoral politics without being directed toward pivotal demographics. The question then becomes, toward whom are these transfers directed? (And do these groups also benefit from tax policy?) One answer is that rather than electorally pivotal groups, it is politically mobilized groups who receive transfers. The classic model in this vein is Grossman and Helpman's (1994) work on trade protection. Attendant empirical work (*sources*) has found support for the importance of mobilization in trade protection. There is little doubt that these transfers, the product of interest group politics, are significant (see Tullock 1997). But the question remains as to why there is so little tactical

redistribution.

The most likely explanation for the relative paucity of tactical redistribution is that voters simply do not find campaign promises credible because candidates cannot commit to enacting them. Lacking the ability to commit ex-ante to enacting their promises ex-post, candidates simply cannot use tax and transfer policy to buy votes, hence net transfers are dominated by ideological redistribution. Indeed, most explicit transfer programs—veterens' benefits, unemployment benefits, energy assistance, food stamps—are best thought of as ideological redistribution for those cases where income isn't the right indicator and thus the tax code isn't the proper instrument.

The Dixit and Londregan framework is built on electoral competition between two unitary political parties who can credibly commit to enacting their tax and transfer platforms once elected. But it often seems that candidates cannot commit to future policies, that they repeatedly make improbable campaign promises, and often break even reasonable campaign promises once in office. There are strong pressures on politicians to make promises which they cannot keep. A candidate that does not make exaggerated promises would appear bland, unambitious, and uninteresting to voters compared to the one that does. Candidates whose plans are not based on the best-case scenario sound pessimistic, cautious, and incapable and media sound bytes are too short for caveats. Government finances are extremely complex and promises are vague enough to put sober evaluation beyond the resources of a busy public, or even beyond the ability of the media to explain to the busy public. Furthermore, voters seem to have short memories (Achen and Bartels 2004) implying low costs to broken promises, especially those broken early in the term of office. Thus candidates continue to promise lower taxes, more social programs, no military conflicts, and a balanced budget.

As a result, a great number of election promises are broken, some of them spectacularly. In 1932, Roosevelt promised to maintain a balanced budget before presiding over the greatest peace-time expansion of the federal budget in history in response to a recession which was already in full swing during the campaign. Reagan campaigned in 1980 on a pledge not to cut social security benefits but attempted to do so in 1981 and suffered a serious political backlash (prompting Congressional Republicans to label social security the "third rail of American politics"), but persevered and finally

did cut benefits in 1983 with the backing of a bipartisan commission (headed by Alan Greenspan). Perhaps the most famous broken campaign promise in recent years is George H.W. Bush's "read my lips: no new taxes" which was first used in the New Hampshire primary of 1988, became the centerpiece of his election campaign to great effect, and was subsequently broken with a tax hike in the 1990 budget. As Acemoglu and Robinson put it, "the fact that the political system today cannot commit to future redistribution policy seems to be an intrinsic feature of democracy."

A politician's character, the party that they belong to, and scandals and public comportment have a far greater impact on how people vote than specific election promises. Voters respond to competence, personality, and broad ideology more than specific promises and campaign pronouncements. This strongly suggests that in most cases campaign promises are cheap talk and seen as such. In the first two examples listed above, the candidate who made the promise was reelected in the following election and even praised for flexibility and ability to respond to unforseen challenges. Because flexibility and responsiveness in the face of change are valued in a leader, and because voters have little time to evaluate the complexities of promises and subsequent policy for mismatches, campaign promises seem to carry very little commitment for officials once elected. As a result, they are largely uninformative: campaign promises are essentially cheap talk. Hence the emphasis by voters on other signals—party affiliation and personal character—which may actually convey information regarding the candidate's preferences and ability.

Snyder and Ting (2003) submit that parties exist to convey information to voters about a candidate's preferences in the face of a candidate's inability to credibly communicate via campaign pronouncements. They develop a model in which individual candidates have heterogeneous policy preferences over a uni-dimensional space and, because they cannot credibly communicate their preferences or commit to a policy, choose a party label to brand themselves and thereby communicate their preferred point to voters. Voters observe a candidate's party affiliation and the party's ideal point and update their beliefs about a previously unknown candidate. Of course, the informativeness of party affiliation as a signal depends on a restriction of the domain from which candidates can join the party. Therefore, the ability of the party to mitigate the individual candidate's inability to commit and thus to credibly signal his ideal point through party affiliation depends on the ability of the party to control its

membership. The more severely members' preferences are allowed to diverge, the less informative the label becomes.

Grossman and Helpman (2004) point out a natural tension between the preferences of members and those of the party leadership. The former have a regional constituency while the latter has a national constituency. To the extent that districts are heterogeneous, these preferences will diverge. Their analysis of trade policy suggests that the degree to which a unitary party can enforce commitment among its office-holding members is an important determinant of the extent of redistributive trade policy. The party leaders would like to commit to a policy of free trade which gives the party a reputation which is universally attractive to voters. Legislators, however, wish to reward industries in their own constituencies with protectionist trade policy. Because only parties can offer credible signals during a campaign, legislators must choose a party and thereby endorse that party's platform. The party leaders, seeking the broadest possible appeal ex-ante, commit to a platform of free trade. However, ex-post, individual legislators, whose preferences are dictated by the industrial mix in their district, prefer to deviate from party policy and form a majority coalition to enact protectionist policies for the industries in the coalition-members' districts. The authors specify that deviation from the party-line exacts some cost on a legislator. As a result, the degree to which trade policy is protectionist increases as the cost of deviating from the party-line declines. This cost represents the ability of the party to impose discipline and, by so doing, to properly serve as the reputable representative enabling candidates to credibly signal their policy preferences to voters.

There is a similar tension between the national party and any particular candidate in the case of tax and transfer policy. Due to heterogeneity, the partisan preferences and turnout propensities of any given district will differ from those of the country as a whole. Hence individual candidates may prefer to cater to different groups than the national party as a whole. Though unlike trade policy, the national party will not prefer a policy with zero redistribution; they will simply prefer to target a different set of groups for tax breaks and cash transfers than would any individual legislator. Therefore it is not clear that changing the strength of the party label would affect the magnitude of tactical redistribution. However, Milesi-Ferretti, Perotti, and Rostagno (2002) have pointed out that in majoritarian electoral systems, where districts, and therefore constituencies, are geographically defined, individual legislators have

a preference for public goods, which are geographically targetable, over social transfers, which are demographically targeted. Here lies the relevant tension between the party and the legislators. The party leadership prefers redistributive policy driven by transfers (social security, veterens benefits, tax cuts) directed at nationally important demographics (retirees, veterens, the wealthy): individual legislators prefer public goods which are visibly targeted to their own districts (army bases, freeways, research institutes) and for which they can personally take credit. Thus, in the realm of tax and transfer policy, less party-discipline leads to a movement from the transfers favored by national party leadership to geographically targetable public goods favored by individual legislators. The low level of explicit redistribution via taxes and transfers I find in this study evinces the difficulty parties have in controlling their members.

3.6 Robustness

There are two issues to be addressed regarding this measurement strategy. The first issue is identifiability. Since equation 3.6 includes family income, then to the extent that voting power depends on family income, η is not identifiable. To address this issue, I have regressed voting power on its constituent components plus family income. The coefficient on income is small and statistically close to zero, implying that voting power is largely orthogonal to family income and thus that η from equation 3.6 is identified.

Second, because voting power, π, is estimated and therefore measured with error, the coefficient η is biased toward zero. Unfortunately, given the method by which π is constructed, no easy goodness of fit measure exists. Without a measure of goodness of fit for the estimation of π, there is no way of quantifying the severity of this bias.

If we maintain the assumption that Bartels' random utility model of turnout and voter choice is valid, then the greatest source of concern is that the estimates of α, β, and γ may be based on too few demographic variables. The most popular candidate for an omitted variable is family income. To address this concern, I have added income to the list of demographic variables and re-estimated the π_j's. The raw correlation between the original π_j's without income and the new π_j's including income is 0.97. The estimates of κ and η exhibit the same broad patterns mentioned in

the previous section. However the coefficients η in the regressions for net transfers and taxes do increase by 33% (see table 3.7.) This represents a non-trivial increase in the magnitude of the coefficient and suggests that measurement error is, in fact, a source of downward bias in my estimate of η. Addressing this downward bias would require data sets with a more complete set of demographic variables. Bartels (1998) suggests that religious affiliation, geographic region, whether a voter lives in an urban or rural setting, and whether a voter is a union member may play a role in voting power. These variables are all collected by the ANES. The trick is getting these same demographic variables in conjunction with tax and transfer data. Until the estimation with these variables can be made, it is unclear how sever the underestimation is. However, given the relative magnitudes of κ and η, and the 33% increase due to the inclusion of family income, it seems likely that even a full set of demographic variables would still leave the conclusions intact.

3.7 Conclusion

The common picture of redistribution emphasizes at least two separate components: ideological redistribution to satisfy ideological goals, and tactical redistribution for electoral gain. This conception raises two questions which remain largely unanswered. What are the relative magnitudes of these two types of redistribution? To whom is tactical redistribution directed? This second question is often refined to mean: do parties court swing voters or core supporters? The model I have tested here is one in which parties address both ideological and tactical concerns when setting their platforms. Though parties in this model target swing voters, my operationalization of political clout has broadened the notion of swing voter to include mobilization of those whose turnout is uncertain. Since increasing turnout is most useful for those voters who are likely to vote in a party's favor (the core), this allows me to compare the electoral benefits of targeting core vs. swing voters.

My results give direct answers to these questions. Tactical redistribution toward pivotal demographics is non-zero and non-trivial, but ideologically motivated income-based redistribution is between 11 and 14 times larger. In each case, the preferred instrument is clearly the tax code. The response of taxes to voting power is between 21 and 58 times more powerful than that of noncash transfers. It is between 17 and 65

times more powerful than that of cash transfers. Furthermore, voting power accounts for virtually none of the variation of cash and noncash transfers ($R^2 < 0.01$) and likely very little of the variation in taxes. I find that core voters have greater voting power than swing voters thus targeting core voters to increase turnout is a far more powerful strategy than targeting swing voters for conversion.

Given the wealth of evidence in favor of pocketbook voting, the relative paucity of tactical redistribution is something of a puzzle. A likely explanation centers on the inability of candidates to commit to redistributive policies, rendering electoral promises mere cheap talk, and therefore useless and not widespread. While the party might be expected to act as a reputable coordinator of ex-ante platforms and, as a long-lived agent, a guarantor of platform promises, my results suggest that the party's ability to discipline candidates and thus provide a platform for credible commitment is limited.

Table 3.7: Estimating Voting Power with Income

	(1) net transfers	(2) cash transfers	(3) noncash transfers	(4) taxes	(5) net transfers	(6) cash transfers	(7) noncash transfers	(8) taxes
105^{th} Congress	π est. w/o income				π est. w/ income			
income ($-\kappa$)	-15842	182	-556	15468	-15503	233	-463	15273
	(49.3)**	(22.6)**	(14.3)**	(38.2)**	(47.1)**	(21.5)**	(13.7)**	(36.5)**
voting power (w/o income)(η)	1794	144	620	-1029				
	(49.3)**	(22.6)**	(14.3)**	(38.2)**				
voting power (w/income)(η)					1343	53	521	-769
					(47.1)**	(21.5)**	(13.7)**	(36.4)**
constant	4657	744	1037	-2876	4812	840	1005	-2966
	(82.0)**	(37.6)**	(23.9)**	(63.6)**	(90.5)**	(41.3)**	(26.3)**	(70.0)**
observations	56241	56241	56241	56241	56241	56241	56241	56241
R-squared	0.69	0.00	0.04	0.79	0.69	0.00	0.03	0.79

standard errors in parentheses
*significant at 5%, **significant at 1%

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