The Happy Median?
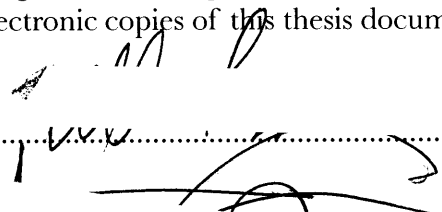## An Examination of the Role of Partisanship on Social Spending

by

Matthew L. Fisher

Submitted to the Department of Political Science in partial
fulfillment of the requirements for the degree of

Master of Science in Political Science, Bachelor of Science in Political Science
September 2010

Signature of Author..

Department of Political Science
September 1 2010

Certified by ....

James Snyder Jr.
Arthur and Ruth Sloan Professor of
Political Science and Professor of Economics
Thesis Supervisor

Certified by ....

Andrea L. Campbell
Hayes Career Development Associate Professor of Political Science
Thesis Committee Member

Accepted by ....

Roger D. Petersen
Professor of Political Science
Chair, Graduate Program Committee
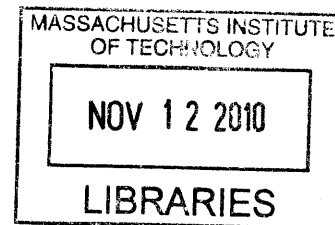
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ABSTRACT:

This thesis examines the effect of party-level partisanship on social spending outcomes. Building a model in which party-politician bargaining plays a central role in determining the passage of bills, this paper develops a theory predicting the superior performance of moderate parties in proposing and passing legislation. Testing this theory using historical roll-call and social spending data, this paper finds that the effects of ideology are not so simple, and that the effects of partisanship on party effectiveness varies with the type of policy examined.

Thesis Supervisor: James Snyder Jr.
Arthur and Ruth Sloan Professor of Political Science and Professor of Economics

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INTRODUCTION

"There are many causes for [Congressional] dysfunction: strident partisanship, unyielding ideology... endless filibusters, holds on executive appointees in the Senate... and a caucus system that promotes party unity at the expense of bipartisan consensus" wrote Democratic Sen. Evan Bayh in his *New York Times* op-ed during the months before his retirement. He continues to blame "the most ideologically devoted elements in both parties" for the lack political progress and compromise in the Senate. The sentiment that ideological divides and party-politics retard political action is widespread: politicians from House Members to Presidents of both parties have railed against partisanship in Congress. Even with this ostensibly bipartisan consensus, the role of ideology and partisanship in the US Congress has not dwindled over the past few decades.

Despite the pervasive belief that partisanship inhibits good government–or at least efficient legislation–few studies examine the mechanisms through which party-level ideology affects policy-outcomes and the magnitudes of those effects. A scientific explanation of these mechanisms is important as Members of Congress are not immune to nationwide swings in ideology; a concrete model on the effects of partisanship would add to the understanding of policy creation. By proposing a formal model of legislation in which partisanship drives the ease of bargaining between parties and politicians, and empirically testing the implications, this paper hopes to shed light on the quantifiable effects of party-level ideology on a specific set of policy outcomes in the US Congress. This paper sidesteps the debate on measures of party strength, cohesion, or support of leadership; it focuses on the *effectiveness* of parties in achieving their long-term goals and sustained platforms regardless of how many legislators toe the party line or defect. Essentially, which type of party is more effective over time: a moderate party or a polarized party?

There are impediments to this study: for instance, both the independent variable—party ideology—and the dependent variable—policy outcomes—are hard to quantify; ideology is even difficult to *define*. In this paper, ideology will be used as "shorthand" for the continuum of positions along which politicians' preferences lie (Poole and Rosenthal 2007). Voting can be thought of as "ideological" when a politician's views on one issue predict his or her views on separate issue (Poole and Rosenthal, 2007). In the US, there have traditionally been two main ideological camps that present competing collections of views across the range of political issues relevant to that era (Aldrich 1995). The continuous nature of this conflict restricts the policy areas available for testing as the points of contention between the two ideologies change regularly. For instance, according to V. O. Key's *Southern Politics*, Strom Thurmond ran as the *progressive* candidate during his first bid for governor of South Carolina; both the time-specific political climate and the constituency in South Carolina determined the relative application of "liberal" and "conservative" in the South Carolina Democratic primary (Key 1949, p. 133).[1] By comparison to his opponents, Thurmond was "progressive" on the issues under debate. Since then, the political debate shifted and Thurmond's views remained more or less constant, marking him conservative by comparison to his fellow US Senators in the following decades.

This paper ignores the evolving fields through which political battles raged, and focuses only on the composition of the armies that fought there, modeling and measuring the effects of a party's "ideological profile" on its effectiveness in the legislature. While the model might be applicable to a wider range of policy contentions between two major parties, to avoid unnecessary complication and an arbitrary ranking of political issues along the ideological

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[1] Alternatively, Sen. Thurmond might have changed his ideological views before entering Congress, but such a complete ideological make-over seems unlikely, especially when considering the remarkable consistency with which Members of Congress retain their ideological stances over time (Poole and Rosenthal 2007, p.7)

spectrum, the empirical sections test only one dimension of political debate—the role of the government in social welfare provision—and examine it between 1940 and 2000. This policy area is especially useful to such my project as, unlike many other policy areas in which the parties change their positions, both the Democrats and Republicans have maintained fairly constant views on government's role in social welfare since the New Deal.

This paper uses roll-call data and historical government spending data to test for the influence of party-level ideological characteristics on social spending outcomes. The social spending dialogue represents an ongoing liberal/conservative cleavage, and will allow for testing the impact of ideology on both proposing and opposing parties. The results of these tests will allow readers to see whether polarized or moderate parties are better at fulfilling their competing social spending agenda. This paper hopes to add to the existing literature on legislative bargaining by formally endogenzing the effects of party-level ideology on the legislative game, specifically as it related to party-politician bargaining. Results from the empirics suggest that party-level ideology is correlated to social spending outcomes, but in ways that vary according to the specific type of social spending. Contradicting this paper's predictions, the effects of partisanship on proposing parties are mixed, decreasing effectiveness in some cases, and improving it in others. Consistent with the model, parties opposing legislation benefit from increased partisanship. Ideology clearly plays a role in these outcomes, but a more nuanced model theory is needed for modeling the mechanisms.

SECTION TWO: LITERATURE REVIEW

In trying to understand the effects of ideological moderation and polarization on policy outcomes, this paper combines various elements of political economy, and is located at the intersection of three fields of literatures: those on legislative bargaining, party politics–specifically the role ideology plays in voting decisions and party influence–and national social spending. The existing literature lays out how politicians come to form parties, whether parties can influence politicians, and through this implicit influence, how non-median outcomes are achieved. Despite the incredibly detailed study of legislative bargaining models and predictions about varying party strength, few papers develop mechanisms for *how* parties influence politicians during the legislative game. Incorporating bargaining between parties and politicians into the legislative process, this paper hopes to create a model capable of explaining waxing and waning party effectiveness, filling this small void. This section will review the related research and findings on the role of parties in the legislature, and their effects on policy outcomes, placing this research in the proper context for consideration.

The study and creation of legislative bargaining models started with Anthony Downs's book *An Economic Theory of Democracy* in which he popularized and fully articulated the ramifications of the Median Voter Theorem (MVT) on the political process (Downs 1957). Using a straightforward model in which two election-motivated groups (or politicians) compete for the majority of a popular vote, Downs shows that, in equilibrium, both groups (or politicians) will offer the same platform so as to maximize the number of votes. The proposed platform will reflect the views of the median voter within the electorate (Downs 1957). The theory, however appealing in its simplicity and logic, fails to mirror real world outcomes in which one frequently witnesses competition between two ideologically disparate politicians or groups, and in which

policy outcomes rarely reflect the median-player's views. The following decades produced a plethora of models that rectified this weakness, and those relevant to this paper can be generalized into two groups: those that explain the variation as a function of individual politicians, and those that rely on parties to drive the perturbation. While the mechanisms might vary, they all share an important characteristic: they explore reasons why outcomes might systematically differ from the predictions of the MVT. In other words, they try to explain the effectiveness of parties or politicians in achieving their individual goals. *The power to effect non-median outcomes is essentially the same as a party's effectiveness* as it represents a party's ability to over-emphasize its weight in the legislative game, creating outcomes that would not occur if all actors had equal weight. This paper, drawing heavily on the frameworks developed below, hopes to accomplish a similar goal via a new method that I hope more realistically captures the functioning of legislatures. The following papers represent the past and current work in the field, showing existing theories about how non-median outcomes can be achieved by politicians and parties.

Those models that do not use on parties to skew the system typically show institutional factors creating unexpected equilibria to explain outcomes observed in politics. The oft-cited Baron and Ferejohn 1989 sets up a divide-the-dollar framework, and shows the importance legislative proposers play in determining the final allocation of goods, and the effects of institutional amendment rules in defining equilibria. Giving all legislators uniform utility functions entirely dependent on their share of the distributive good, they show that outcomes can differ drastically with only minor changes made to the game's rules. Focusing on voting as opposed to amendment rules, Cox 1990 shows the effect of adding votes per voter, abstention rules, and district magnitude on the platforms political candidates.

Less institutionally "rules-based" models like Besley and Coate 1997 and Osborne and Slivinksi 1996 create models comprised of "citizen candidates" in which the individual's abilities and preferences drive the variation from the unadulterated MVT. Besley and Coate 1997 show under circumstances where all candidates have an equal propensity to create and enact their policies once elected, the outcome will be that of the median voter, but in situations–like those in the real world–in which candidates have differing abilities, the winning platforms might be skewed. Osborne and Slavinski 1996 modeling a two candidate system under plurality rule, show that politician platforms *must* be optimally dispersed to avoid the risk of a third candidate entering the race and winning more than a third of the votes. The above four models provide illuminating results, but fail to mirror reality by too large a margin to be useful in understanding how non-median outcomes occur in everyday politics. As this paper seeks to understand real-world outcomes, models that more closely resemble the US political process must be found.

Krebiel 1993 models a simplified US Senate in which some players have more power under certain circumstances than do others. This "pivotal-politics" model highlights the importance of politicians in positions to break filibusters or override vetoes, but could be extrapolated to include any legislator who enjoys a measure of institutionally appointed power. Explaining the effect of these institutional rules, he shows that the US Senate can and will pass non-median policy options under certain circumstances–or not pass *any* policy–depending on the type and location of the policy under discussion, and the President's ideology. Krehbiel 1993 more closely fits reality in the US Senate, but seems less applicable to other legislative bodies such as the House of Representatives that lack the specific institutional characteristics of the Senate bust still exhibit the same non-median outcomes and political gridlock. I turn to the legislative

bargaining literature that includes political parties to inject additional realism and find a mechanism explaining non-median outcomes across the entire US Congress.

The models presented below incorporate political parties as a way to explain the systematic variation of politician platforms and policy outcomes. Many of these papers conceptualize parties as coalitions of various strength comprised of legislators with similar preferences over policy. Cox and McCubbins 1993 additionally describe parties as policy motivated, in contrast to individual politicians whom most of the cited authors view, in the Mayhew tradition, as being election-motivated (Mayhew 1974). Using this discrepancy of goals, authors like Austen-Smith 1984 predict converged parties but diverged politicians as the result of district-elections. Snyder 1994 also uses the disparate goals of parties and legislators, but instead formalizes a model endogenizing parties: individual politicians of similar preferences join a party and create the party platform with a majority-rule vote. Each politician must then run with that platform inside their own district. Assuming that districts vary in their ideologies, such a set predicts that each party in a two-party system will polarize to a different side of the country's median voter as each party's platform will reflect that party's median district. Ansolabehre, Leblanc and Snyder, using the foundations set in Snyder 1994, corroborate the original findings, and show the divergence of parties in a two-party system. The endogenous selection of party platforms provides one way in which parties can skew the median-voter outcome, and such a view is adopted by the model presented in Section Three. These papers present convincing results regarding how parties can skew political outcomes, and they add a level of realism not seen in the previous works. However, most of them take the existence of parties as given, assuming away much of the individual agency politicians enjoy during the course of their terms.

These papers stand to gain from later literature that adds back some individual qualities to politicians.

Two such papers by Jackson and Moselle 2002 and Calvert and Dietz 1998 use the Baron-Ferejohn 1989 framework for legislative bargaining, updating the utility functions of the model's participants, showing that parties serve to enhance legislator utility. Jackson and Moselle choose to insert the public good of "policy" outcome, and Calvert and Dietz create a model in which each legislator cares both about his own share of the private good and his colleague's share. Both models show that once legislators value a good that requires cooperation to gain, there is ample reason to join a party consisting of ideologically like-minded politicians. These parties then collude for the collective good of the party, possibly invalidating the MVT predictions. Diermeier and Feddersen 1998 examine a divide-the-dollar game in which a group of forward-thinking legislators considers both the immediate and long-term ramifications of each vote presented on the floor. If politicians see each floor vote as a indicator or contributor to future control of the agenda—building on the Baron-Ferejohn results of proposer power—then there exists sufficient incentive for politicians to join parties, maximizing their expected share in each round. This model shows that the policy outcomes, the distribution of limited resources among legislators, vary depending on the party of the member proposing. By linking increased utility to group formation, these papers show both that it is valuable for politicians to join a party for highly realistic, logical reasons. Of course, these papers lack the party-level mechanics capable of perturbing the MVT such as those explicitly mentioned in Snyder 1994, Austen-Smith and Banks 1984, but taken along *with* these papers, the mechanisms described by Jackson and Moselle 2002 show a convincing way in which politicians could choose to join a party, and through that party affect non-median policy outcomes.

Despite the full picture painted by the above literature, none discuss how parties and politicians might interact with each other after the passage of a platform or decision to join. As the bulk of votes occur sometime during the legislative term, we must still find mechanisms to understand how and on what basis parties and politicians might treat. Examining the role of parties, some authors create models in which parties themselves are the main actor. These papers illustrate that even at the party-level, the MVT might not accurately characterize the bargaining, and that the outcomes might not be consistent with predictions that consider parties as unitary actors. Austen-Smith and Banks 1988 considers party-level bargaining, and concludes that only in systems with more than two parties will policy results systematically differ from the median. Updating his earlier work, Baron 1991 examines a three-party, two dimensional political system in which each party is randomly selected to propose policy and create a government. He concludes that policy will be set by one of the two larger parties in coalition with the smallest party, with the smaller party's preferences being accorded a weight proportional to their size relative to the larger. Even in governments with only two large parties of standing, a third faction may still play a role in the formation of policy. Such a model is not as foreign to US politics as it may seem: consider the Southern Democrats in coalition with the New Dealers of the forties. While the Southern Democrats were unable to halt the expansion of government as was generally their wont, they were still able to enact several policies of great import to their faction, including the absence of civil rights legislation (Key 1949, ch. 1).

The notion of parties compensating ambivalent or opposed factions with future legislative assurances in return for support on current legislation can be applied to the hole identified in the above literature. Perhaps parties interact with individual politicians differently depending on their relative positions and preferences, just as the smaller party's bargaining position changes given its

size and ideological position in Baron 1991. Digging into this concept, a more complete, unified view of legislative bargaining can be envisioned in which politicians rationally join parties for personal gain, but maintain a large degree of agency over any individual voting decision, prompting the party to enter the game and bargain with legislators, systematically skewing the MVT prediction to varying degrees depending on the party's success. While a more complicated version of events, such a rendition more closely mirrors reality than do many of the alternatives, facilitating a study that tests implications with real world data.

A theory capable of predicting varying levels of political success for parties might help address an oversight of the legislative bargaining theories: the possibility of bargaining breakdown. Foreign to political science research, the possibility of "no outcome" between parties has an established literature in the economics literature. Started in 1983 by Myerson and Satterthwaite's work on bargaining between a single buyer and single seller, later researchers generalized the outcomes, and found new and innovative ways of introducing "breakdown" probabilities. Powell 2006 discusses the applications of breakdowns to conflict and the decision to declare war. McKelvey and Talbot 2000 test the inefficient outcomes predicted by the Myerson-Satterthwaite game against the efficient ones predicted by the Coase theorem. However, the dearth of these models in the legislative bargaining literature creates an odd consequence in which all the games model end with a resolution of some sort—clearly not reflecting reality in which Congressional inaction could be considered the norm.[2] A model capable of predicting cases in which bargaining between politicians and parties is more or less likely might add to the breadth of cases the existing legislative bargaining literature can explain.

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[2] Krehbiel 1993 addresses this dearth with his "pivotal politics" model, but it is explicitly not a bargaining model.

To create such a model, one must understand the way in which parties interact with politicians, what parties have that politicians want, and the effects of the party system on individual politician's actions. Two contradictory views exist in the field: that parties, under certain conditions, can exert large power over their members, and that parties actually have little proven influence. On the side of party-power, Aldrich's 1995 *Why Parties?* lays out the history of American parties and their formation, generalizing from their predictions under which parties might form and the purposes they serve in politics. Further work by Aldrich and Rohde 2000 examines the "strength" of political parties–their ability to gain cooperation and unitary action from their members–more concisely than their previous works, and formalizes their theory of parties known commonly as "conditional party government" (CPG). Seeking to explain the power of parties to influence legislative votes, they opine that "Members' [of Congress] behavior, we assume, may also be shaped to a considerable degree by the legislative party," and list two conditions under which the power parties hold over their members may wax and wane (Aldrich and Rohde 2000). First, that parties more homogenous in their preferences will be stronger, and second, that party strength will increase in the absolute distance between the two ideological ideal points of the party. The mechanism they propose through which parties exert control on their members is endogenous: party members, as a whole, select how much autonomy to award party leadership, and this amount increases as the two conditions are increasingly met. Aldrich and Rohde 2000 also predicts that the above conditions will engender a greater willingness in the party to allocate party resources, and greater strength of the majority party. Aldrich and Battista 2002 tests the CPG theory across 11 states legislatures and relates supporting evidence correlating party cohesion to the distance between competing parties. They also find that the competitiveness of the parties, a near-equal chance that either could control the legislature, predicted polarized

parties (Aldrich and Battista 2002). CPG's main faults as a basis for understanding party power over politicians are its paucity of testable implications, its *ex ante* assumption of parties. The few testable implications contained present expected correlations, and do not show deep attention to the mechanisms connecting the cause and effect. It must be considered along with the legislative bargaining literature for robustness.

CPG's logical predictions inspired a large number of scholarly articles essentially testing the implications in a variety of settings with a number of methods and instruments. Ashworth and Bueno de Mesquita 2004 create a formal model in which politicians can endogenously choose a desired level of party influence, and find that politicians will choose to greater levels of party-discipline in more competitive elections as a means of more effectively capturing the median voter. Such a model neatly sidesteps the need for party-politician bargaining by putting politicians entirely in charge of party strength. Similarly Patty 2008 links the amount of power given to party leadership to the size of the majority party: larger majorities reduce the need for strict party voting, and induce politicians to retain more autonomy as each member's probability of being pivotal decreases. He tests this prediction empirically using NOMINATE data between 1866 and 2004, and finds consistent results with the caveat that NOMINATE data might not be well-specified in Congresses with very large majorities (Patty 2008). Despite using different methods, the model I present in Section Three fits quite well into CPG outcomes, but provides some alternative mechanisms for explaining certain predictions CPG, particularly why polarized parties might allocate larger amounts of resources than more moderate parties. If one stretches CPG's definition of "party strength" to actually mean "the ability of the party to achieve its [non-median] goals," one can see that CPG offers an important mechanisms for inclusion into a model interested in explaining how parties might interact with legislators, particularly the

importance of the party's ideological spread, however CPG and its brainchildren are decidedly

sparse on mechanisms for enforcement and compliance within the legislature.

Cox and McCubbins 1993 describe a mechanism for party influence in which party

leadership is concerned with the reputation and effectiveness of the party as it affects the

electoral outcomes of the next election. To protect these goals, leadership manipulates the floor

agenda, and tries to prevent issues that might divide the party from ever reaching a vote. (Cox

and McCubbins, ch. 5, 9-10). Called the "cartel theory of government," Cox and McCubbin

introduce the party as a cognitive, active player in the legislative game, and endows it with the

ability to forecast future political frictions among its members that could lead to compliance

issues. This theory assumes that politicians can and will deviate from the party line under certain

circumstances, giving the missing agency discussed above back to politicians. Synthesizing Cox

and McCubbin's cartel theory of government and CPG, Lebo, McGlynn, and Koger 2007

propose what they term "strategic party government." Incorporating the electoral importance of

party-reputation and the effects of competition in polarization, they describe a model where

parties retain ability to influence their members, but to do so is costly. Parties therefore choose the

amount of legislative influence that maximizes electoral dividends while minimizing cost to the

party. They test this theory, and find that the greatest determinant of party unity is opposing

party unity. This implies that parties strive for unity in response to electoral competition Lebo

McGlynn and Koger 2007). This model portrays an interesting example of parties exerting direct

influence on politicians, and nicely fills the gaps left by an unmodified version of CPG by

including some amount of legislative agency, and increasing the number of testable implications.

Of course, the role of the party in influencing legislators, particularly the mechanism as

defined in CPG, is not universally accepted. Krehbiel 1993, 1999, and Calvert and Dietz 1996

similarly point out that measuring party strength as a function of party homogeneity creates biased results. Typically, party strength is measured by the percentage of the party that supports the party goal. Increasing homogeneity will almost certainly increase this measure of "party strength," but may in no way be tied to the party's influence over its members. As such, defection rates might be lower and party cohesion higher, but discerning between the party's influence and election-induced preference homogeneity might be impossible (Krehbiel 1999). Krehbiel 2000 shows that this mis-specified causal relationship between voting behavior and party influence is particularly acute in CPG models that predict party strength as a function of polarization. Scenarios in which polarized parties compete would *naturally* exhibit party cohesion as a function of utility-motivated politicians with lexicographic preferences over policy: since little overlap would exist between politicians of opposing parties, party-voting would be almost certain. In this case, it seems like polarized parties influence politicians to a greater degree than moderate ones due to the correlation between party-voting and polarization, but the cohesion is actually caused by the exogenously set preferences of politicians (Krehbeil 2000).

Despite the compelling logic of this counter-argument, the weight of evidence seems to support theories arguing for more party influence. Nokken and Poole 2004 find sparse evidence for party influence by examining politicians that switch parties over time, but Snyder and Groseclose 2000 and 2001 find evidence of party-influence economic and legislative issues by contrasting the roll-call records of lop-sided and close votes. Parties can induce cooperation through promises of committee assignments, leadership positions, campaign funds, legislative pork, and future support on legislation. While party influence exists over a range of issues, party's tend to either perform poorly or relinquish their hold on politicians when it comes to votes over moral issues (Snyder and Groseclose 2000).

Further support for party-influence can be found in Ansolabehere, Snyder, and Stewart 2001 in which Members of Congress were surveyed, and in Cox and Poole 2001 who exploit the implicit party influence left in the roll-call record. In what might be redundant proof, Lawrence Maltzman, and Smith 2006 tests four conceptions of party influence against one another by specifying regressions unique to each of the theories, and testing their fit against the actual roll-call record. While their methods might lead to unobservable biases based on inaccurately specified models, their results show that models built around party voting and loyalty fit the results better than "pivotal politics" explanations.

Building from the CPG, cartel government, and strategic party government models, this paper formalizes a mechanism for vote-persuasion, and attempts to understand the ability of the party to influence votes. This paper takes the view that parties *can* purchase influence over their constituents, but that the amount that they need to spend in order to do so varies with the issue under debate and the relative positions of the party and legislator under consideration. Consistent with Krehbiel 1993, 1999, and 2000, the model developed agrees that preference homogeneity should reduce the need for party-level action, however, it incorporates the consistently proven empirical evidence that suggests parties can, when needed and with cost, influence the vote choice of select politicians over select issues. Similar to the legislative bargaining framework, the literature shown here assumes that parties can influence outcomes, but provides little formal insight into way in which this influence takes place. Using the mechanisms for party-influence described above, this paper augments the literature by formalizing party influence in a legislative bargaining context. This paper adds to the literature not by doing anything substantially new, but by combining two fields of existing research that seemed related.

Many of the empirical tests of party-influence and party-cohesion, including the theory laid out in this paper, require quantitative measures of both candidate and party policy preferences. Of the above tests, Snyder and Groseclose 2000, Cox and Poole 2001, and Patty 2008 use roll-call data to scale legislator onto a continuum of ideology. While the technical methods use vary—some use the NOMINATE coordinates and others use the Heckman-Snyder coordinates—the importance of the results from these scalings makes understanding the strengths and weaknesses of roll-call data important. Hug 2006 cautions against roll-call data, concerned that in many legislators, some votes might be systematically excluded from the roll-call record. Roberts 2007 notes that the inconstant nature of the rules governing records of amendments and amendment voting, especially regarding amendments considered during Committee of the Whole proceedings, might bias the results systematically, and show an increased party-effect. Additionally, Roberts shows that the ideological position of any one legislator might be an artifact of that politician's party's majority status.

These criticisms of roll-cal data, and other like them, necessitate reasoned use. In defense of properly use roll-call scalings, Hill 2001 compares several methods of ideological rating including journalistic accounts and comparison/rating by co-partisan elites all converge with the findings of roll-call approximations. Burden, Caldeira, and Groseclose 2000 present a similar finding, and rate roll-call estimations "among the best measures available."

This paper relies on roll-call approximations for building party-level ideological data. A possible alternative exists in the Manifesto Based Research project (MBR) as presented by Budge, Klingemann et al. 2001. They track the ideological progression of parties over time by engaging tens of knowledgeable analysts to scale party documentation produced to an ideological scale. These ratings are then turned into a line that can be traced over time. The graphs obtained from

MBR regarding US parties they do not overlay perfectly with those produced through the revealed-preferences of roll-call scaling. Of course, both roll-call and MBR have their pros and cons: voting-records belie the influence of agenda setting and "cartel" influence on party members as discussed above, while MBR downplays the disconnect between electoral literature and the actual vote outcomes, unable to control for the fact that a large amount of party-publications may be cheap-talk. Ideally, both roll-call and manifesto-based research would have been included as explanatory variables in this study, but the MBR only records data between 1947 and 1998, dropping almost a sixth of the sample eventually settled upon. More in depth reasoning will be presented in Section 4: Data.

The final component of this paper's theory that merits discussion here is the dependent variable of interest: social spending. The structure and evolution of social spending continues to fascinate political economists, and the logic they have uncovered behind the flows of welfare bear mentioning, if only to understand what the theory in this paper augments in terms of the determinants of spending levels. Lindert 2004 presents a compelling case in which he links the value of social spending as a share of gross domestic product (GDP) to both the absolute value of GDP per capita and the percentage of the population able to vote. He argues that representation in government over time typically followed a pattern in which the wealthy were accorded status first, and only through years of perseverance and debate, were others brought into the government. He points out that, rationally, if political influence were purely restricted to one-man-one-vote, the rich would be heavily taxed to subsidize the poor as long as the income distribution remained highly skewed. (I.e., the mean unequal to the median, with the mean greater.) The transition over time from non-democracy to "elite democracy" in the 19th century to full democracy in the 20th neatly fits the trends in social spending patterns, and can be noticed

by observation when considering the growth of social spending in the US between 1880 and the present. Despite these similar trends, the US wealthy are still not has heavily taxed as the wealthy are in other Western Nations, and the "Robin Hood" syndrome described by Lindert 2004 seems to have not yet materialized (Lindert 2004, Chapter 1). This might represent some non-median outcome.

After surveying the legislative bargaining and party-strength literature, one can see that both fields provide substantial frameworks for explaining how parties can avert the equilibria predicted by the MVT, and how parties interact with politicians, but neither field provides a unified model in which party-politician bargaining is explicitly examined as the main driver for non-median outcomes. By equating non-median outcomes with party effectiveness as described at top of this literature review, the next section will introduce a formal model that uses the disparity between party-level ideology and politician-level ideology to create the friction necessary for party-politicians bargaining, and show how the size of this disparity might lead to more or less effective parties.

SECTION THREE: THE MODEL

The following model, building on the legislative bargaining models of Baron-Ferejohn 1989, Snyder 1994, and Jackson-Moselle 2002, incorporates party ideology into a legislative bargaining context. The ability to skew median outcomes is a good proxy for party effectiveness as it shows the ability of the party to create outcomes that would not occur but for the party's presence. This model posits that party partisanship is an important component of party effectiveness, and examines the role that partisanship plays on the ease of parties achieving their non-median goals. The model formalizes the costs and benefits of partisanship to US parties, and provides a basis for understanding how these factors contribute to the effectiveness of parties interacting their members and achieving their political goals in Congress.

Similar to Jackson-Moselle 2002, this model takes place in an n-member legislator where legislator utility is a function of both a private and a public good. The public good, "policy" is represented by the location of the status quo bill on the ideological spectrum that spans -1 to 1. (-1 the most liberal and 1 the most conservative to fit convention.) Legislators have policy ideal points in this same spectrum representative of their district's median voter. The utility value of the public good to any one legislator is a function of the distance between that point and the legislator's ideal point, decreasing as the distance grows.

The private good is a set of IOUs or side-payments unique to each politician, awarded to them by party apparatuses in return for votes; the value of the private good is initially zero for all politicians. Without considering utility from side-payments, politicians vote sincerely over policy, and are election-motivated. Let the ideal policy preference of politician i's district be denoted y_i. Legislator i's utility can be described by $u_i = -|x - y_i| + z_i$, where x is the location of the status

quo policy on the ideological scale, and z_i is the value of the side-payment offered during bargaining.

Parties are comprised of the politicians they support during national elections. As such, the ideological profile of the party depends on those of its members, and their success come Elections. Campaigning politicians are judged on three criteria by the voters: their individual ideological stance, their party's most recent platform, and the success of the party in achieving the goals laid out in that platform–the party's *effectiveness* in office. An election satisfying the first two of those conditions will create ideologically disparate parties with platforms on either side of the nation's median voter (Ansolabehere, Leblanc, and Snyder). After elections, politicians within each party cast votes creating a platform for that legislative session. Assuming a majoritarian decision process, the result should mirror the median-politician's preference (Snyder 1994). Consistent with the cartel theory of parties as proposed by Cox and McCubbins 1993, this process creates a party whose goals are purely policy-oriented. Parties strive to enact their platforms in order to help their members' reelection campaigns.

This platform selection process creates a dichotomy of interests between election-motivated, district-responsible politicians and the policy-motivated national party: the party's platform affects legislators' reelection probability, but the platform only perfectly mirrors one legislator's ideal preferences. Since the platform is the result of a majoritarian election within the party, it will be the platform most palatable to the greatest number of politicians, but there will always be those politicians whom the platform adversely affects. Thus, the party as a whole has an incentive to support the platform, but individual legislators may have incentive to stray (Snyder 1994). Politicians bind themselves to the party, and take the party platform into consideration when proposing policy as the party's ability will be judged come elections. In

return, parties serve as credible organizations through which promises and side-payments to opposing or indifferent politicians can be made in order to create majorities; the party uses its superior resources to support politicians working toward its policy goals.[3]

Let the two parties be A and B, and the party platforms selected be Y_A and Y_B, corresponding to two different points on the ideological spectrum. Of these two points, one is the platform of the majority party, i.e., $Y_{maj} \in [Y_A, Y_B]$. A few additional assumptions are required for this model to work:

1. That politicians have lexicographic preferences over voting: they will vote for the option that provides them the most utility (Baron Ferejohn 1989, Jackson-Moselle 2002).

2. That the distribution of politicians within the party is independent of the party's placement on the ideological spectrum. While any symmetric distribution is workable, allow politicians to be normally distributed within their parties for tractability.

3. Politicians do not prefer policy to side-payments or side-payments to policy, only the aggregate level of utility their receive from the sum--the two goods are of the same value.

4. In the absence of side-payments, politicians sincerely represent the will of their district's median-voter, and will vote for policies that best increase their chances of reelection.

5. Parties are endowed with a finite amount of resources, and the amount is independent of the party's placement on the ideological scale. I.e., if parties were to receive their

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[3] Levy 2004 proposes an alternate approach to parties: in campaigning within their districts, politicians face credibility concerns over the policies they will actually support once elected to the legislature. Parties provide as commitment mechanisms voters trust, and ensure that politicians cannot stray far from the policies promised during campaigning.

endowments as a random draw from some distribution, partisan parties would receive an endowment from the same distribution as moderate parties.

Once new platforms are chosen, the legislative game begins: a party is chosen at random to propose for that period, and within that party, a politician is then chosen at random to propose his favorite policy (If politician i is chosen, he will propose y_i) Before proposing, the chosen politician weighs the effect of his proposal, anticipating the end-result: through gate-keeping powers, committee-chairs, and institutional agenda-setting powers, the majority party will have a large effect on the bill's final form (Diermeier and Feddersen, p. 611). To represent this power in the model, all policy propositions are tempered by the majority party's platform by one half the distance between the proposer's position and the majority party's. If politician i is chosen as the proposer, the bill resulting from his proposal will not be y_i, but instead $[(Y_{maj} + y_i) / 2.]$ If this tempered policy is closer to the proposer's party platform than the status quo x, the politician will propose. I.e., if the politician chosen belongs to party A, he will propose only if:

$$| Y_A - \frac{(Y_{maj} + y_i)}{2} | < | Y_A - x |$$

If this inequality does not hold, the proposer relinquishes his turn, and the game starts again with a new party chosen at random. This model is designed to keep politicians beholden to their districts to a greater degree than if all politicians were to propose the party platform, but retain some political realism by incorporating a degree of majority-sway. If the selected politician *does* propose a bill, all legislators vote sincerely, weighing the utility gained from the new proposal against utility from the status quo. A model functioning with sincere, individual voting should result in bills pulling the policy toward the median-voter in the legislature. However, parties can complicate this result: if a majority does not organically form around the new bill, the proposer's

party can purchase the additional votes needed for the bill's passage. In honest voting, a majority would form around the proposal if it is closer to the legislature's median than the status quo. Hence, parties will get involved only to change the status quo *away* from the country's median and toward's their party median, skewing the MVT prediction. In this model, parties are portrayed as responsible for outcomes inconsistent with the median voter theorem, and the level of skew from the median towards the party ideal point represents the party's effectiveness.

To purchase votes, the supporting party must make opposing politicians indifferent between the new policy and the status quo. This can be accomplished by offering them the precise amount of the private good z necessary to compensate for the utility lost due to the new bill. Again, z is not a direct cash payment, but may consist for legislative favors, support for district-specific pet-projects, or promises for future considerations with committee assignments. The concepts are vague, but one can imagine a large number of rewards a party could offer politicians on either side of the aisle (Snyder and Groseclose 2000). Once committed to purchasing votes, a party will minimize its costs; logically, the cheapest politicians to compensate are those closest to being indifferent already–generally, those nearest the median of the ideological spectrum. In minimizing costs, one might also expect some deference to Riker's 1962 "size-principle" as parties will spend their limited resources only on the politicians they need. Depending on the institutional rules of the legislature, this could either consist of a bare-majority or a larger supermajority.

Let A be the party chosen to propose. Due to the normal distribution of politicians inside the party, the expected value of $y_{proposer} = Y_A$. Using this to find the expected cost of compensating opposing politician j with z_j:

$$z_j = y_j - \left(\frac{\left(\frac{Y_A + Y_{maj}}{2} \right) + x}{2} \right)$$

where the bracketed term is the location on the ideological spectrum that make an opposing politician indifferent between the status quo and new policy. This is equivalent to compensating the politicians for the utility lost due to the new policy: $u_j = - |x - y_j| + z$:

$$u_j = - |x - y_j| = - \left| \left(\frac{Y_A - Y_{maj}}{2} \right) - y_j \right| + z$$

$$\Rightarrow z = - |x - y_j| + \left| \left(\frac{Y_A - Y_{maj}}{2} \right) - y_j \right|$$

As one can see from both of these equations, as Y_A approaches zero, the compensation cost of opposing politicians decreases. Since Y_A is the party's median, as the bulk party becomes more moderate, the cost of supporting the legislation proposed by its members decreases linearly. Assuming parties are endowed with finite resources to finance z, this result implies that more moderate parties, all else being equal, should be better able to afford the passage of favorable legislation.

Additionally, one notes that the payments of z *increase* in y_j, i.e., as opposing politicians are further away, they become more expensive to coopt. Assuming that both parties are normally distributed, the expected cost of purchasing political support should increase as the opposing party becomes more extreme. All else held equal, polarized opposition parties should be more expensive and therefore better at foiling the attempts of proposers. Since the cost to the proposing party rises with the distance between parties, one also expects to find proposing parties hurt by the absolute distance between the parties, regardless of which party is more moderate or ideological. This prediction is consistent with a related prediction made in Aldrich and Rohde

2000, but with different causality. Aldrich and Rohde 2000 predicts that as the ideological difference between the two parties increases, parties grow stronger, and leaders will more willingly employ the resources as their disposable to ensure party cohesion. A greater distance between parties allows for more spending. In the model presented here, increased distance between parties *requires* party leadership spend more in order to secure needed votes. It also explains the opposite: why more moderate parties would spend less.

Finally, this model suggests that parties will be more apt to accomplish their goals if their politicians are more densely located around the party median. The denser the distribution around the party median, the greater the chance that the politician from the party will mirror the exact wishes of the party. This implies that homogenous parties are more effective than heterogenous parties at achieving party goals, all else held equal. This is straightforward to prove: as above, let party A be chosen to propose, and let $y_{i,A}$ (politician i of party A) be the ideal point of the proposing legislator. As politicians are normally distributed within their parties, the value $y_{i,A}$ is a random variable distributed around the party mean Y_A with variance σ_A as determined by the density of politicians around the mean. Thus, the probability that the distance of the chosen legislator is less than some arbitrary distance "d" can be written:

$$\Pr\left(\mid y_{i,A} - Y_A \mid < d\right) = \Pr\left(\frac{(\mid y_{i,A} - Y_A \mid - Y_A)}{\sigma_A} < \frac{d - Y_A}{\sigma_A}\right)$$

$$= \Phi\left(\frac{d - Y_A}{\sigma_A}\right)$$

As value for σ_A increases, the probability that the distance between $y_{i,A}$ and Y_A is smaller than "d" decreases. In other words, as the variance increases, the expected similarity between a randomly chosen proposer's ideal bill and party's ideal bill decreases. The same logic works for opposing parties: as their variance increases, so does the average distance of a randomly chosen legislator

from his party's platform. Politicians that lie further from their party and closer to the median point on the ideological spectrum will be cheaper for the proposing party to buy. Thus, regardless of whether a party proposes or blocks, low-variance should be correlated with effectiveness.

The following empirical sections of this paper use roll-call data to reveal the preferences Members of Congress, and to build party-level data for testing this model. As testing the model requires a "proposing" and "opposing" party over time, this paper focuses social welfare spending, viewing the Democrats as proposers, and the Republicans as opposers in the time-period since 1940. The following section will introduce the empirics by describing the data and the treatments to which it was subjected.

SECTION FOUR: DATA

As alluded to in the introduction, finding unbiased data representing policy outcomes and party-level ideology is not straightforward. However, that statement is misleading: finding data on government spending and accessing the roll-call records is easy; verifying the data reliably represents the issues under investigation is the difficult part. This section will describe in detail the data and sources used to empirically test the model presented in the previous section.

Two options were considered for testing the model: choose a small subset of congresses and analyze each bill proposed on the floor, analyzing the effectiveness of each party in either supporting or opposing that bill given its placement on the ideological spectrum, or choose one sticky issue and follow it across a long period of time, resulting in a narrower but more detailed analysis. Both are valid; I chose the second. I hope the "narrow but deep" approach will control for exceptional Congresses or unusual years. While a random sample of Congresses from across the history of the US might have controlled for that equally well, choosing one-policy would allow me to better understand the historical determinants of the policy's magnitude over time, allowing for better specifications and a greater ability to recognize and reject aberrations in the results.

This decision placed restrictions on the time-period and policy chosen for examination. The time period had to be characterized by two lasting, easily distinguishable parties with consistent—or at least vaguely recognizable—platforms over time. Furthermore, the time period had to consist of Congresses in which a sufficient number of votes were taken in each session for accurate roll-call analyses. Finally, the period had to exhibit at least one consistent dimension of political debate spanning its entirety. Inside this dimension, a specific policy debate was needed as the dependent variable.

Ultimately, I chose 1940 through 2000. This time period has several advantages over the other party periods as described by Aldrich 1995. First, the familiar liberal-conservative debate over government expansion is sustained throughout this entire period, facilitating inference, and eliminating the need for prolonged historical context. Second, reliable and consistent economic and demographic data are available throughout this period to serve as control variables for the study, a luxury not necessarily existent for the nineteenth or early twentieth centuries.[4] Finally, examining this era allows for tracking the development of American social welfare policy as the dependent variable of interest.

As a dependent variable, social spending has a lot to recommend: it is quantifiable and reliably measured each year by the executive branch, specifically the Office of Management and Budget (OMB). Increased spending on "human resources"--as comprised by the largely redistributive programs geared toward helping the less economically fortunate, children, and the elderly--is a policy, at least since 1940, consistently pursued by the Democrats and consistently opposed by the Republicans. Every new program within this period had to win initial passage through Congress, and most have to fight for appropriations on a yearly basis as well. This policy presents a unique opportunity for testing the effectiveness of parties over time as the major players (the parties) do not change positions, the outcomes vary greatly over the time period.

The spending categories, their composition by function, and their values over time are shown below in Figure 1 and Table 1. (All figures for spending are recorded in millions of chained 2005 dollars, just as received from the OMB.) One notes the categorization of the variables is not particularly fine, nor logically grouped by function: education is recorded together

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[4] While many studies choose to start modern historical examinations after World War II, I chose 1940 in order to increase the sample size. This decision calls into question the validity of the "war years" as observations due to the large macro-economic disturbances that accompanied WWII, but I believe that the control variables ultimately identified are capable of capturing the war-based variation, and hope that the extra observations help more than hurt.

with welfare, income security with government pensions, and health research is separate from other research funding. The decision to use this data represents the best option in a series of trade-offs regarding the reliability: collecting spending data from individual budgets since 1940 presents difficulties as the agencies in charge of the pertinent programs divide and coalesce in irregular, difficult to anticipate ways during the 60 years examined. Furthermore, program-names change without warning. As the US budget presented by the Executive Branch is organized by department and not by function, consistently picking out the programs year-to-year seemed almost impossible. This conclusion necessitated the use of a third-party measure.

The OMB offers several historical datasets, the most germane of which present historical outlay data by function and sub-function. Ideally, this paper would use the data grouped by "sub-function." This level shows all government spending by recognizable program, drawing distinctions between programs such as unemployment compensation and food and nutrition assistance, between primary education and research grants. Unfortunately, data at this level of detail only goes back to the 1962, reducing the sample by more than a third. The alternative to this data, those ultimately chosen, goes back to 1940, but groups the data only by function.

One minor problem with the spending data exist in the OMB recording scheme: in 1976, the US government changed its fiscal year from July-June to October-September. This resulted in an orphaned financial quarter belonging to neither 1976 nor 1977. Adding this to either year would artificially increase appropriations of that year, and so this orphaned quarter was simply excluded from the research. All spending totals presented represent 12 full months of appropriations, but only 156 of the 159 months are used for data (Public Budget Database Guide 2010, p. 4). The OMB spending data are from the agencies responsible for each dispersal. This situation is less than perfect: ideally, the precise numbers appropriated by Congress would would

be used to more accurately represent legislative outcomes, but due to the shifting nature of the agencies, the data are almost impossible to accurate estimate. Totals that were originally appointed to the Department of Health, Education and Welfare in may 1966 may now be recorded under the Department of Education and the Department of Health and Human Services with new names (Public Budget Database Users Guide 2010). I feel safe using these estimates as it seems rational that agencies would use all of the money appointed to them. All are recorded in millions of chained 2005 dollars.

TABLE 1: SOCIAL SPENDING DEFINITIONS

Super-function	Function:	Sub-functions included
Human Resources	– Education, Training, Employment	-Elementary, secondary and vocational education -Higher education -Research and general education aids -Training and employment -Other labor services -Social Services
	– Health	-Health care services -Health research and training -Consumer and occupation health and safety
	– Income Security	-General retirement and disability insurance (excluding social security) -Federal employee retirement and disability -Unemployment compensation -Housing assistance -Food and nutrition assistance -Other income security
	– Social Security	-Social Security payments.
	– Medicare	-Medicare payments

FIGURE 1: SOCIAL SPENDING AS A PERCENT OF TOTAL US GOVERNMENT OUTLAYS[5]
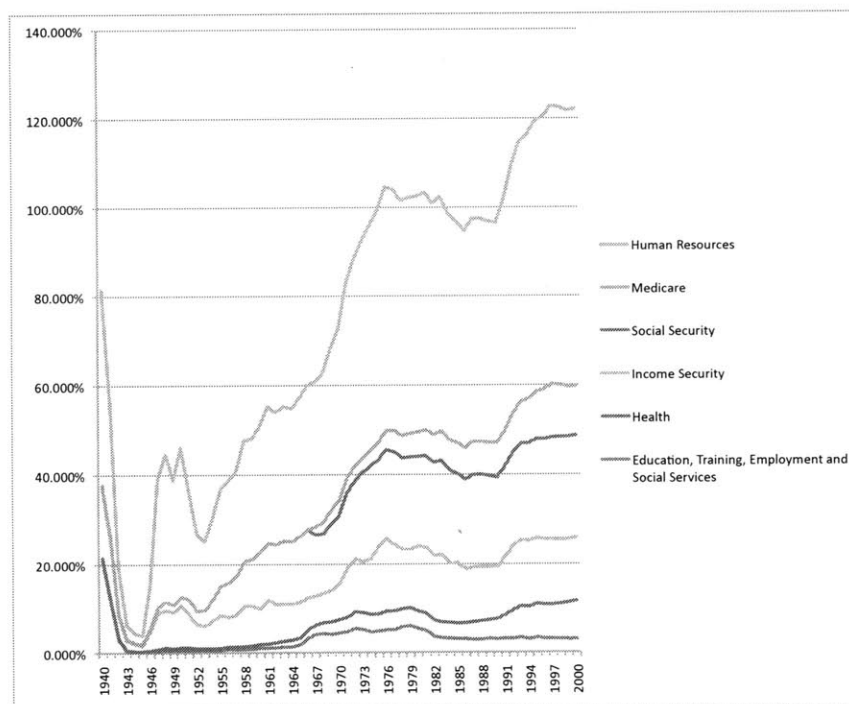
Inspection of Figure 1 shows a huge increase in overall social welfare provision in the United States during the period examined. These figures represent about 2% of the national budget in the mid-forties, but account for more than 60% of the budget by the late nineties (OMB Historical Tables). According to Peter Lindert's 2004 book *Growing Public*, the US experienced this surge in its social-welfare provision as a function of population growth, increased wealth and education, and the percentage of the population with representation in Congress (Lindert 2004). Lindert opines that these demographic factors affect the way citizens

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[5] The percentages in this graph exceed 100%. Disbursements from the Social Security Trust are not counted as "government spending," and have been considered as both "on-budget" spending (1968-1993), and "off-budget" (1935-1968, 1993-present) (SSA Historical). To make sense of this complicated history, the OMB records all social security spending as separate from the US Budget. This is also the case for medicare and the federal employee pension funds that comprise the income security category. Therefore, when taken all together, the full spectrum of Human Resources spending combines to totals larger than the full "on-budget" US government spending.

vote, and so, this paper will include several of those demographic variables as controls.

Population growth was rapid during this time, as was overall GDP growth. These two variables

affect social spending outcomes through their relationships with government tax revenue, and

absolute need in the population for government spending. Based on these same relationships, the

unemployment rate should play a significant role in determining the year-to-year social provision

decisions of Congress. The values of these variables can be seen below, graphed in Figures 3 and

4 by year. GDP figures represent inflation-adjusted, chained-2005 dollar values, and come from

the Bureau of Economic Analysis (BEA). BEA data mostly represents the "best-change"

statistics, with "best-level" revisions occurring every five years after the quinqennial economic

survey. BEA datasets make use of the gross value-added GDP calculation, and sum all

production, value-added, and services in the economy over a one-year period. Detailed input-

output accounts trace the flows of goods and services, and commodity composition of outputs for

both the private and public sectors. The GDP estimates are accurate in capturing all the

production for final use, but might be misleading over the short term as they do not include

production for intermediate use. Yearly revisions are released in July, and update the previous

three years. (Thus, all data used in this paper was subject to three rounds of revision.) Estimates

on production come from a plethora of industry-specific surveys, and form information-sharing

arrangements with the Census Bureau and the Bureaus of Labor Statistics (NIPA Handbook, ch.

1-4).

FIGURE 2: POPULATION AND GDP OF US, 1940-2000

Source: BEA Historical Tables
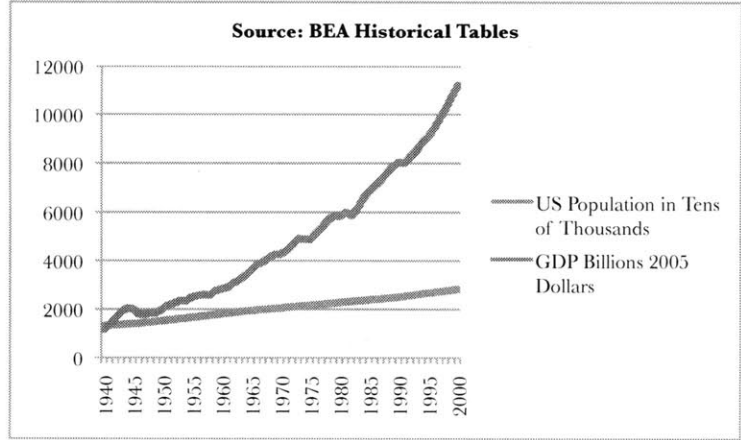
FIGURE 3: UNEMPLOYMENT AND PERCENTAGE 65+

Sources: BLS, Census Bureau
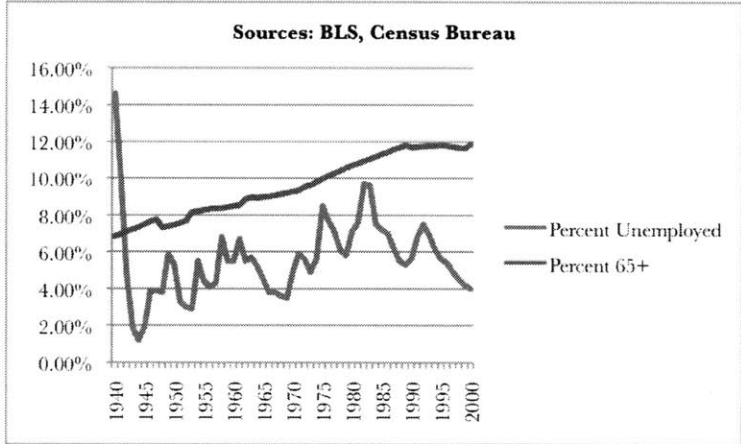
Absolute US population was taken from the Census Bureau's "National Population Estimates" archive, available online in two segments: the years between 1940 and 1999, and then 2000 from a separate data set. The National Population Estimates are created each year by adding all the births taken place since the most recent decennial census, and subtracting all the deaths. (Both are recorded by the National Center for Health Statistics.) As deaths, among the elderly are frequently improperly submitted, Census Bureau analysts adjust the estimate by actuarial death rate among the elderly population, using specific data for every age above 70. Immigration is then added from the American Community Survey (Methodologies for National Population Estimates, 2009).

During this time, not only did the population increase, but the shape of the population changed, driven by the increasing lifespans of American citizens. Such demographics drive the magnitudes of programs specifically targeting the elderly such as social security. While previous decades had seen federal spending on pensions for veterans of the civil war, those benefits were small by comparison to those eventually distributed by the Social Security Administration (SSA) (Skocpol 1992). By 1940, those still collecting their pensions from the civil war were few. As social security makes up such a large share of the government's "human resources" spending, the shape of the population must be accounted for when modeling such expenditures. Data on the population over 65 comes from two sources: the Census Bureau's "National Population Estimates" archive.

Closely linked to the shape of GDP-growth, the unemployment rate also seems a logical exogenous variable for which to control, as Congress might temporarily increase the length of time on which the unemployed can remain on such stipends in times of economic stress. Unemployment numbers come from the Bureau of Labor Statistics, and contain one glitch: through 1946, the BLS counted all unemployed citizens over the age of 14. From 1947 onwards, it only included citizens above the age of 16. This measurement inconsistency means that the unemployment rate in 1947 discontinuously falls relative to its previous levels, and might result in underemphasizing the role of unemployment on social spending. The data are collected through the BLS's Current Population Survey (CPS). The CPS is conducted monthly across thousands of households each, always during the week of the 19th. The selection process is not random, and BLS researchers try to tailor the respondents to a representative sample of the US population. Each respondent is asked a series of demographic questions about their families, and then a series of workforce related economic questions. The methods used by the CPS both in terms of

selection and inference have improved dramatically over the decades, but while imperfect, represents the best available for data for unemployment estimates (BLS Handbook of Methods, Ch. 1).

By controlling for the exogenous variables, this paper hopes to isolate and explain the legislature's role in setting social spending data, specifically the role of party-level ideology. This paper settled on the use of roll-call scaling techniques for assigning ideological preferences, but, of course, other options exist. One enticing alternative for measurement of party-level ideology is presented by the Manifesto Based Research project (MBR) (Budge, Klingemann, et. al 2001).

MBR creates measures of party-level ideology by surveying party literature and platform documents. Ideally, this paper would have included the the MBR party ratings in the empirical section, however two main problems prevent this: first, the MBR data does not cover the full 61 years identified for examination, only the 51 years from 1947 to 1998. Considering the limited initial sample size, decreasing it by an additional 18% seemed unwise. Additionally, the MBR data only measures party ideology every four years, making it a much blunter instrument than the roll-call scalings that can calculate Congressional

FIGURE 4: HECKMAN-SNYDER PARTY IDEOLOGIES

FIGURE 5: SCALED MBR PARTY IDEOLOGIES (MBR KLINGEMANN, 2001)

ideology every two years. (Budge, Klingeman et al., 2001). As one can see in Figure 4 and 5, the roll-call measures of party ideology and the MBR data do not line up. This might be because they differ in the frequency of their approximations, but just as likely reflects a real discrepancy between party literature and the way party members vote. Figure 4 presents the Heckman-Snyder coordinates, but they show a very similar trend to the NOMINATE coordinates. (Figure 4 computes party position by averaging the median member of the House with the median member of the Senate.) The MBR data needed for Figure 5 are provided with Budge, Klingemann et. al., 2001. The main discrepancy between the MBR and roll-call coordinates takes place in the 1980s, when, according to the MBR data, the Democratic party starts moving toward the right, eventually crossing the median in the early nineties. Apparently this shift in rhetoric is not accompanied by a shift in voting, as the Democrats exhibit little rightward movement in the roll-call records.

The roll-call data used came from Keith Poole's website www.voteview.org. The clerks of the House of Representatives and the Senate keep meticulous roll-call records that do not systematically exclude any type of chamber vote however, changing institutional rules in the recording of votes leads to time periods with more votes than others. Roberts 2007 notes that, in the 93rd Congress, the House started including Committee of the Whole on the State of the Union (COW) votes in the roll-call records. The inclusion of COW votes increases the number of votes on amendments by a considerable amount, and can make estimation of party influence over individual legislators more difficult, perhaps even creating such effects when none exist. COW votes generally occur over amendments, and thus, generally, consist of more fragmented votes as the parties fracture into more distinct factional groups evaluating the worth of the amendments to their districts. When measuring party unity over time, the introduction of these

votes into the roll-call record in the early seventies noticeably changes the cohesion scores when compared to the cohesions scores with the COW votes dropped (Roberts 2007). However, this paper is not interested unraveling the motivations of legislator's vote-choices, and it does not use party-level voting cohesion as an explanatory variable. The addition of these COW votes might actually improve some of the estimations of legislator preferences, especially the NOMINATE calculations, as the COW votes introduce a large number of votes with unexpected, mid-party cut-points, facilitating the scaling process. Provided that the values produced via the NOMINATE program are consistent during both pre- and post-COW inclusion, the systematic reporting or non-reporting of votes should not be great concern.

The roll-call data are used to create measures of party ideology. To do so, candidate ideology was estimated using two techniques: Poole and Rosenthal's "W-NOMINATE" scaling, and Heckman and Snyder's linear probability model of binary choice (Poole and Rosenthal 2007; Heckman and Snyder 1996). These two models differ in the methods by which they assign preference-scores to individual politicians, but produce generally consistent estimates of legislator ideal points. The NOMINATE scores are those produced by the W-NOMINATE program available for download from Keith Poole's website, voteview.org. It unfolds binary choice data using a parametric model of legislator utility and the observed voting outcome of the roll-call data. The mathematics behind the NOMINATE program are quite complicated, and best explained in Poole 2005; due to my own failings, a rigorous reproduction here could only obfuscate its functioning. In the abstract, the W-NOMINATE programs uses the "cut-points" between the voting camps, and the assumption of an unchanging signal-to-noise ratio in the roll-call record to order legislators along the ideological spectrum. Using these two pieces of information, the W-NOMINATE program fits the outcomes to a complex non-linear

probabilistic model of vote-choice for each politician (Poole 2005; Poole and Rosenthal 2007, p. 26-28).

The cut-point, or cut-line in the two-dimensional model used in this paper, defines the areas between those who voted yes and those who voted no on a specific issue. Each roll-call has its own cut-point, and it is easily calculated from the roll-call record. However, simply recording the cut-lines only serves to distinguish between those who support any one vote and those who don't; one could order the politicians correctly, but be unable to tell the distance between them. In other words, this provides only an ordinal ranking, not the desired cardinal rankings. Logically, one would assume that those with the most consistent preferences are the most ideological, but without an additional measure, pinpointing the location of legislator ideal points, there is no way of verifying this (Poole 2005; Poole and Rosenthal 2007, p. 26-28).

To rectify this, the NOMINATE programs uses the "errors" generated by the probabilistic vote-choice function described above. Sometimes, based on the other roll-calls by that politician, the model's predictions are inconsistent with the voting record: based on their record, one would expect the legislator to vote yes, but instead he voted no. To use these errors, NOMINATE first processes the roll-call data against an initial configuration of legislators and their binary-choice models to produce the estimations of vote-choices. It then processes the legislators with regards to the estimations and the correct data, trying to maximize the number of "predicted yes"-"actual yes" and "predicted no"-"actual no" as possible. After swapping legislators around from the initial configuration to a new one, this two-step process is repeated until the classification of each legislator stabilizes. This implicitly assumes that the model accurately specifies how preferences decrease in distance from the ideal point; the NOMINATE data use a bell-shaped function (Poole 2005; Poole and Rosenthal 2007, p. 26-28).

In contrast to the complicated Poole and Rosenthal W-NOMINATE scores, The Heckman-Snyder estimations, as described in Heckman and Snyder 1996, are produced by a relatively simple linear probability model. Essentially a discrete-choice model, the Heckman-Snyder process assumes that legislators vote according to a partially or entirely unobserved utility function. By viewing a large of the observed outcomes, modelers can decipher the unobserved preferences governing these choices.

The mechanism is straightforward: assume that legislators face each vote as a choice between "yea" and "nay." On this choice, legislators favor voting "yea" if their utility resulting from the bill's passage would outweigh their utility as a function of the bill's failure. Allowing actors to differ through a heterogenous preference vector included in the utility function, one can build an equation for the probability that one outcome is chosen. Plugging in the observed outcomes--and in this case, controlling the "rank" of the model, or the number of dimensions of individual preferences that affect the outcome--one can then solve for the implicit, heretofore unknown preferences of each politician. Once scaled, these are the estimators (Heckman and Snyder 1996). (The math, while simpler than that needed for the NOMINATE calculations, is still quite complicated. Only confusion could result from any attempt I make to reproduce and explain it accurately. Please see Heckman and Snyder 1996 for a detailed treatment.)

Before running either of these programs, the roll-call data was first cleared of all votes regarding social welfare provision and government provision for the needy or elderly so as to not correlate the dependent data of social spending outcomes with the independent variable built on the vote-choices of individual politicians. To do so, I used a version of the Congressional record from Keith Poole's website that included issue codes assigned according to Aage Clausen's specifications, succinctly described in Clausen and Wilcox 1991 (Poole, voteview.org). I then ran a

simple program that sorted the Congressional roll-call data by the issue-coded data, dropping all votes Clausen determined as "social welfare." The purged votes relate to "social security, public housing, labor regulation, education, urban affairs, employment opportunities and rewards, welfare, medicare, unemployment, minimum wage, legal services, and immigration" (Poole, voteview.org).

After purging the correlation, the roll-call data was then treated to both the W-NOMINATE program and Heckman-Snyder binary-choice model, producing individual legislator coordinates. I imposed a two-dimensional policy space on all of the ideal-point estimations despite the evidence that policy space might be much more complex than such a simple model suggests (Heckman Snyder 1996). The two dimensions I attempted to create were those of "government intervention in the economy" on the x-axis, and "civil rights/race" on the y-axis, mimicking the scalings conducted by Poole and Rosenthal 2007. To this end, I chose northern Democrats to represent the left-most "pro-government" side of the spectrum during scaling, and southern Democrats for the top-most, "anti-civil rights" dimension. While I created two dimensions, I use only the x-axis "government intervention" coordinates in this paper. The civil rights dimension was created to control for what seemed a pervasive and protracted second division in US politics during the time period examined; I feared that without controlling for such, the results of my scalings might reflect unobserved effects of this fissure, and not be illustrative of the liberal-conservative divide (Poole and Rosenthal 2007). Both roll-call programs produce results consistent with each other. The Heckman-Snyder coordinates tend to show larger extremes and bigger swings, but the trends are almost identical.

The decision to use both a NOMINATE program and the Heckman-Snyder technique was spurred by the desire for extra verification—to be sure that any results were not an artifact of

the data-manipulation. The NOMINATE program comes in a variety of flavors, each useful for different types of studies. The W-NOMINATE program, the method chosen here, treats each Congress as a unique occurrence, and does not hold individual legislators constant between Congresses. This reflects this paper's desire to compare a series of "snapshots" of Congress's composition, searching for a correlation to social spending outcomes. The Heckman-Snyder coordinates create similar snapshots, building each legislator's preferences anew each Congress from the way they voted on each roll-call.

Armed with legislator-level preferences, I created party-level ideology using the median member of each party. While very simple, this method is defensible against more complicated alternatives. Other options might be to use the party's median weighted by party leadership, the President's ideology, or some convoluted formula that would introduce new factors into the study. I rejected these alternatives to avoid arbitrarily assigning weights and values to different hierarchical positions within Congress. For instance, if one decides to give the party's leadership increased sway over the party platform, should one use the most senior member, or the party leader, or both? How much extra weight should that legislator have? Ten times more weight? These decisions were beyond my ability to make, and thus I chose the simplest method possible, facilitating any mental correction readers might choose to make of the empirical results. I chose the median not the mean as the model presented in Section 2 so specifies, and that—theoretically—would be the result of a platform created by popular vote within the party.

To measure party-level homogeneity of preferences, the variance of legislator ideal-points around the party's mean is used. While this may seem inconsistent with the use of party-medians as party-platforms, it again represents the most straightforward measure, allowing readers to make their own decisions about its validity, unhindered by complicated metrics or calculation. It

should be noted that the variances are consistently correlated with the distance of the party's

median from zero, and they decrease as that distance increases. This is as predicted by the

conditional party government theory (Aldrich and Rohde 2000).

TABLE 2: STANDARD DEVIATIONS OF PREFERENCES, 1940-2000

	Mean	Std. Dev.	Min.	Max.
Democratic SD, NOM	0.3183	0.1104	0.1253	0.6669
Democratic SD, HS	0.3008	0.1084	0.1272	0.5907
Republican SD, NOM	0.2729	0.0830	0.1442	0.4930
Republican SD, HS	0.2587	0.0980	0.0832	0.5122

In addition to the roll-call votes and ideological measurements, the legislative data needed

for this paper includes Congressional majorities in both the House and the Senate, and the

ideological identification of the President. This data are widely available, but I used the websites

of the clerks from both the Senate and the House. Both the Senate and the House of

Representatives are used in this examination. While the House has more weight in matters of

budget assignment and funding, the Senate votes on new programs and must be accounted for.

Using both the Senate and the House has implications on the empirical specifications used as

members in the houses serve for different times, participate in slightly different institutional

structures, and undergo different socializations. This presents a a choice: either run legislators

from the Senate and the House together in one pool, using those politicians who moved from one

chamber to the other as a way to scale the houese relative to each other, or run the two houses

separately. Both require assumptions: running the two chambers as one group relies on assuming

that those legislators who moved from the House to the Senate kept their ideological views

constant, despite representing a new, larger constituency.[6] Separating the chambers assumes that

the chambers are not influenced by one another, almost treating the Republicans in the House

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[6] This need not be a staggering leap of faith. One could easily imagine that Representatives who become Senators are from those districts that contain and represent the state's median voter, and Poole and Rosenthal 2007 show surprisingly little change in legislator ideal points over time.

and those in the Senate as two separate parties. Hoping to keep the best of these options, I ran

both the House and the Senate in the regression as different panels. These are not true panels as

each observation (year) only has one dependent variable associated with it. To correct for this, I

entered the dependent variables twice in my data, once for each "panel" of observations. This

leaves me with what resembles two strongly balanced panels of 61 observations each, even

though these panels are in fact related with each other, and share dependent variables. This

method was chosen so as to include the maximum amount of data, and both the Senate and

House.

SECTION FIVE: METHODS

The following analysis investigates the relationship between party-level partisanship and social spending outcomes between 1940 and 2000, testing the implications suggested by the model presented in Section 3: to what degree is party-partisanship correlated with non-median outcomes? (Again, in other words, what role does party ideology play in party effectiveness.) The empirical test relies on a series of regression equations designed to isolate the effects of partisanship on social spending from environmental determinants. The spending data are recorded yearly, and increases almost monotonically across the sample. Concerns over obfuscated variables driving any results are justified; the tests below were designed for robustness against the two main threats to credibility: auto-correlated standard errors and autoregressive processes. This section will address these concerns and their circumvention. It will then describe the specific regression variables, and the reasoning behind their form and inclusion.

Regression Methods

Each year, Congress passes a budget, and that budget frequently resembles the previous year's budget. The concerns addressed in this section stem primarily from the appropriations process itself: agencies propose budgets that are subject to review and alteration by Congressional committees and then voted on by the entire House. In deciding allocations, it is possible that Congress simply approves increases based of the previous year's allocation, plus some small, percentage drawn from a distribution. Alternatively, perhaps Congress increases the size of the social spending budget every year, keeping up with population growth. Or Congress decides to increase the social spending budget every-other year, or on odd-years, or on years ending with twos, sevens and nines, or any other random process imaginable. Without controlling for the

range of possibilities, any result might just reflect a random correlation between the real budget

process and party-level partisanship.

Time-series data projects are given to period-to-period correlation. One repercussion of

this is the correlation of error terms in adjacent observations: the error for 1975 might be

systematically related to the error in 1976. This complicates estimation and reduces the accuracy

of the standard errors. The Durbin-Watson statistic is useful in diagnosing the problem. When

ran, the results of the Durbin-Watson test varied by the specific dependent variable–type of

program–used in the regression. They ranged from smaller than 1.2 when examining the

"Education, Training, and Employment Services" variable, to 2.03 when examining the entire

"Human Resources" allocation.[7] As only two of the desired five dependent variables are within

0.15 of the desirable 2.0, regression equations capable of returning correctly specified standard

errors in spite of the auto-correlation between years were needed for testing. Specifications

benefitting from transformations correcting for autoregressive processes of one year lags (AR1)

performed significantly better against the Durbin-Watson test, and so such models were identified

for research. Three models for paneled time-series data were considered: feasible generalized

least squares (FGLS), a Prais-Weinstein estimation with panel-correlated standard errors, and a

Baltagi-Wu transformed generalized least squares (GLS) estimator with fixed effects.

FGLS is an estimation technique similar to the more standard generalized least squares

(GLS) estimator. However, for GLS, the variance-covariance matrix must be identified; FGLS

uses an estimated variance-covariance matrix, making it more widely applicable, but less reliable

under certain circumstances. Comparing several time-series cross-sectional studies from the early

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[7] The Durbin-Watson statistic is a measure of the correlation between the residuals of regression equations from neighboring time-periods. It ranges from zero to four, 0 implying near perfect positive correlation, and 4 near perfect negative correlation. Values of two suggests very little correlation of the residuals.

nineties, Beck and Katz 1995 show that FGLS regressions tend to underestimate standard errors; as the variance-covariance matrix is itself an estimate, the standard-errors should be larger than those typically reported. Augmenting FGLS to accommodate for time-correlated heteroskedasticity or serial correlation also engenders problems in some studies: the widely used "Parks method" only works when the number of time-periods examined is much larger than the number of panels (Beck and Katz 1995). While the ratio of periods to panels in this study is very high (30:1), the overall number of observations is still small–122 including both the Senate and the House, 116 if running a regression with the differences of lagged variables. As the estimated variance-covariance matrix increases in efficiency as number of observations increases– asymptotically approaching the maximum-likelihood estimator–FGLS might be better suited to larger data sets (Beck and Katz 1995).

The alternative suggested by Beck and Katz 1995 is a Prais-Weinstein regression with panel correlated standard errors (PCSE). Even in smaller samples, OLS estimators are efficient; this efficiency facilitates the estimation of a contemporaneous-covariance matrix, allowing a better estimate of the variance-covariance matrix than seen in FGLS, resulting in more accurate standard errors.[8] However, while the OLS provides the best correct standard errors, FGLS estimators with the Parks method transform were designed for estimation in the face of panel-correlated errors, and might be more efficient. Beck and Katz 1995 find that the Parks method estimators are noticeably more efficient than OLS estimators when the contemporaneous correlation is greater than 75%, and the number of periods is more than twice the number of panels (Beck and Katz 1995). There is reason to expect the House and the Senate are *extremely* contemporaneously correlated as they are affected by the same national and economic shocks,

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[8] Regressions ran on my data with the Prais-Weinstein estimator provided larger standard errors–less confidence in the estimates–than regression ran with FGLS. The theory presented in Beck and Katz seems correct.

and the number of periods is significantly more than twice the number of panels. An ideal

specification would combine the efficiency of the FGLS estimation with the more realistic

standard errors of the OLS estimation.

The GLS estimator with a Baltagi-Wu transform fits such a bill. Originally designed for

estimating time-series panel data with missing observations, this method allows for the inclusion

of a fixed-effects term (Baltagi -Wu 1999). The Baltagi-Wu method works as follows:

1. Estimates the coefficient of autocorrelation, rho, on the data using a within-transform.[9]

2. Returns to untransformed data, and transforms according to Baltagi-Wu 1999

3. Runs OLS on the Baltagi-Wu transforms for the creation of residuals

4. Calculates variance from these residuals.

5. Returns to Baltagi-Wu transformed data and runs a GLS transform

6. Estimates coefficients using OLS from the GLS-transformed data.[10]

Through this method, standard errors are generated through an OLS process, but estimation of

the coefficients is done by the more efficient GLS estimator. In this way, I can actually combine

the best aspects of both models. In balanced panels with equally spaced data and no missing

observations, the Baltagi-Wu transform in step 2 conforms to the standard Prais-Weinstein

transformation. The panels in this study are perfectly balanced, and do not display missing data

(Baltagi and Wu 1999).[11] Despite this, the Baltagi-Wu method was chosen over the more standard

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[9] Where the regression is calculated on the value of each observation–every dependent or independent variable in the equation, y, for instance–minus the panel average, plus the panel/time grand average. $y_{i,t} - \bar{y}_i + \bar{y}_{i,t}$ This is done for every variable in the equation. (Stata Support, FAQ xtreg2)

[10] This summarized breakdown was found posted by a David Drukker, a Stata employee on the Stata List-server (http://www.stata.com/statalist/archive/2006-02/msg01040.html) It is corroborated by the Baltagi-Wu 1999 Paper referenced below it.

[11] Again, please remember these are not "true" panels, but rather two sets of independent variables run with the same set of dependent variables reproduced twice. Please see the last paragraph of Section 4 for a fuller explanation.

Prais-Weinstein method as the data display a consistent time-based aberration: each Congress passes two budgets. Every set of roll-call estimations save one are used twice, once for each budget passed under its supervision. Estimating each Congress as a whole as opposed to by year was necessary to ensure a large enough set of roll-call votes for efficient estimation of ideal points in earlier Congresses. This introduces an imbalance in the data where the set of dependent variables and the independent variables have the same number of observations, but different numbers of unique values. While this should not, in theory, bias results or reduce the efficiency of estimates, I felt that the unusual circumstances merited special consideration in choosing an estimator.

The Baltagi-Wu/GLS transformed (BW-GLS) equation allows both random and fixed effects. To ascertain the need for these, I ran a Hausman test comparing the fixed effects equations (always consistent) again the random effects (efficient and consistent under the assumption H_0). As with the Durbin-Watson statistic above, different types of spending had different outcomes. The aggregate "human resources" measure along with "social security" both passed the specification test with random effects, but the remaining measures displayed errors statistically and systematically different under the fixed effects model. This outcome is logical: social security makes up a huge percentage of the human resource aggregate metric; it probably drives the consistency of in the Hausman test. A fixed effects model was chosen.[12]

Steps 1-4 in the BW-GLS process correct for the AR1 disturbance process by calculating the coefficient of correlation from the previous period (rho), and by accurately estimating standard errors from transformed data as opposed to the raw data. The calculation of standard

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[12] As a point of interest, the BW/GLS, fe standard errors are slightly smaller than those produced with the OLS-PCSE model discussed above, but much larger than those produced by the FGLS. The coefficients produced by the BW/GLS closely mirror those produced by the FGLS, but are noticeably different than those produced by the OLS-PCSE estimator. This starkly highlights the trade-off between efficiency and reliability.

errors is carried out by the method suggested by Greene 2007, Chapter 19 for dealing with autoregressive specifications.[13] When including the fixed-effects term, the standard errors are slightly smaller than those created by the OLS with panel-correlated standard errors presented above, but not significantly so, and they do not routinely affect the significance level of the results.

Using the Data

I decided on three measurement methods for each spending category: the absolute share of the budget each category receives, the share of total social spending that category represents, and the logged-difference between the previous year's funding and the current year's funding. While related, they each address a different aspect of the bargaining process: the "share" variable—calculated by dividing the category's yearly spending by the US's total on-plus-off budget expenditure—shows the relative importance of social programs when compared to all other government funding. Presumably, pro-social spending parties want redistributive social programs to take up larger portions of the budget than would opposing parties. This variable increases either as the share of social spending increases *or* as overall size of government spending decreases, holding the category's allocation constant. Either way, it shows the prioritization of social programs relative to other types of government spending as a function of the polarization of political parties. The share variable has the added positive of being inflation-resistant as it is normalized by the overall government spending. The spending data are purportedly reported by the OMB in inflation-adjusted 2005 dollars, however I still worry that the inflation measurements used to adjust were too rough or not well-suited for normalizing government spending. The protection against macroeconomic influences comes at a cost; during large policy shifts—such as

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[13] There is almost no practical difference between these standard errors and the standard calculation as recommended by Baltagi-Wu 1999.

the ending of the cold war–this variable might increase artificially, growing as the denominator

decreases. This increase results from almost purely exogenous factors, not from legislative

bargaining outcomes, and thus could bias the results from this variable.

To control for the concern over the "share" variables responsiveness to non-bargaining

outcomes, I add a set of "adjusted" variables, created by dividing each category's yearly spending

by that year's total human resources appropriation. Each adjusted variable represents the

percentage of total US social spending that was allocated to that category that year. This variable

exhibits less exposure to non-bargaining, exogenous drivers, but is also imperfect: as the majority

of "human resources" is social security, the denominator for this variable is highly inflation-

sensitive. Since 1975, social security payments increase according to a cost-of-living index. Even

before 1975, one can imagine that politicians increased the social security payments when they

lost purchasing power. This introduces a concern as the cost-of-living index (CPI) does not track

inflation indexes such as those used for higher education (HEPI), or healthcare (PPI Physician). If

the inflation rate for these services increases faster than the CPI, and Congress attempts to

maintain purchasing parity, this variable might systematically overestimate the correlation

between the control variables and the dependent variable.

The third measure, the differences of logged spending, measures the percentage change,

year-on-year, in that spending category. This measure should show the effects of party

partisanship on moving the status quo in one direction or the other. The less-refined measure of

log-spending was rejected due to stationarity concerns. This variable is more sensitive to incorrect

inflation adjustments, such as when the higher education price index (HEPI) rises more quickly

than the consumer price index (CPI), but it provides a much clearer, refined view of the parties

pursuing divergent goals over each policy area specifically, not allowing the obfuscation of the US

budget that the share variables include. This is created by taking the differences between the logged values of the social spending categories year-on-year.

Accommodating the different versions of the dependent variable, I created different versions of the control variables in the hopes of facilitating inference. Despite their differences, both equations have controls for the unemployment rate, the population over 65, and changes in GDP. Additionally, the control variables in every equation include a lagged version of the dependent variable. This autoregressive term controls for the likelihood that spending in the present is determined by spending in the past. For the "share" variables, this protects against the concern that legislators simply assign a percentage of the budget to each spending category. For the log-differences specification, this controls for the possibility that each programs is simply assigned an X% increase. As correcting for the AR1 drastically improves the Durbin-Watson statistic in the cases where it was lacking, only one time-period lag is included. In some cases, the lagged-term absorbs the effects of control variables one might expect to be highly significant, however interpretation is generally straightforward. This is particularly noticeable in regressions concerning the social security category: one expects control variables such as the percentage of the population over 65 or the log-population over 65 to be highly significant in driving social security's payouts, but neither are routinely significant.

Corresponding to the differences between the dependent variables, I constructed three different independent variables for use in different regressions. For the "share" specifications, I created a variable reporting the absolute distance of the party-median from zero–"median distance." This number is restricted from zero to one, just as the spending-category's share of the budget is a fraction ranging between zero and one. As each party's median is not restricted to its expected side of the median, this variable actually measures the costs and benefits associated with

being perfectly moderate. This is consistent with the theory presented in Section 3, showing that the costs associated with passing policy should decrease as the party moves toward the center of the spectrum, and towards the other party.[14] This independent variable was created by taking the absolute value of the party's median.

For the log-differences specifications, I use value of the party medians, or "median value" as the independent variable. Theoretically, they can range between -1 and 1, but they are actually restricted to a smaller set. This variable represents the value of being absolutely liberal or conservative, regardless of the distance from zero. Unlike the median-distance variable that tests for the effects of moderation, this tests the effects of the party's ideological value. The log-difference dependent variable shows the percentage change in spending from the previous year, and therefore is not theoretically bound at all, though it tends to vary within a fairly small range. According to the conditional party government theory, the ideological placement of the two parties should be correlated (Aldrich and Rohde 2000). Translating this implication, I expect the that the median-distance values—each party's distance from zero—exhibit positive correlation, and the median-values—each party's actual value on the ideological spectrum—show negative correlation The median-distance variables display very little correlation, while the median value variables are quite negatively correlated. In all specifications, the values for both the Democrats and the Republicans are included.

Using the "share of budget" measure as described above, the third independent variable tests the correlation between social spending and the absolute distance between the party's medians. I constructed this variable by taking the absolute value of the differences between the

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[14] The time periods which in which the parties frequently appear on the "unexpected" sides of the spectrum's zero-point are all predictably in forties and fifties, when the Southern Democrats still exhibited strongly conservative tendencies, and in which Southern Democratic-Republican alliances were on the rise (Key 1949, p. 369-382).

Republican median point and the Democratic median point. While the distance between the parties is not restricted to between zero and one, the "share" measure was used as practical difficulties with the social security spending category strongly dissuaded the use of the log-differences approach. This will be seen clearly in the next section.

In addition to for the effects of party partisanship, two additional party-variables are included: the standard deviation of each party's member's ideal points. These variables measure and represent the effects of the party's preference homogeneity on spending, directly testing the implication in Section Three.

The last variables in the equation concern the legislature: I control for House and Senate majorities, expecting that control of the agenda will play a large role in determining the effectiveness of political parties. Originally, the President's partisan alignment was included as well, but was rarely of any statistical significance or importance. To be sure, F-tests were ran, and the exclusion is justified. Variables for House and Senate are included as 0 or 1 binary variables, taking the value of zero if control is Democratic, and 1 if control is Republican. Ideally, the regressions described in this section would have far fewer variables in them, facilitating inference and decreasing speculation over autocorrelation or co-determination. Unfortunately, due to the complicated nature of policy-creation, stripping out any more of the control variables creates an unrealistic picture of the actual importance of party-ideology on social spending. In all three specifications described, both the NOMINATE estimates and Heckman-Snyder estimates are tested. Below are regression formulae in long form, including all of the possible control variables. The next section will present the results from these tests and discuss them in detail.

Equation 1:

Share of spending$_t$ = α + Share of spending$_{t-1}$ + Democrats median distance$_{i,t}$ + Republican median distance$_{i,t}$ + Democrat stan. dev.$_{i,t}$ + Republican stan. dev.$_{i,t}$ + House majority$_t$ + Senate majority$_t$ + % Unemployed$_t$ + Change in GDP$_t$ + %Population over 65$_t$ + FE$_t$ + ε

Equation 2:

Log-difference spending category$_t$ = α + Log-difference spending category$_{t-1}$ + Democrats median distance$_{i,t}$ + Republican median distance$_{i,t}$ + Democrat stan. dev.$_{i,t}$ + Republican stan. dev.$_{i,t}$ + House majority$_t$ + Senate majority$_t$ + Log unemployed $_t$ + Change in GDP$_t$ + Log population over 65$_t$ + FE$_t$ + ε

Equation 3:

Share of spending$_t$ = α + Share of spending$_{t-1}$ + Absolute distance between party medians$_{i,t}$ + House majority$_t$ + Senate majority$_t$ + % Unemployed$_t$ + Change in GDP$_t$ + %Population over 65$_t$ + FE$_t$ + ε

The outcomes of these tests, the coefficients on the independent variables, will show to what degree party partisanship affects social spending outcomes. Essentially, with the coefficients, we can try and understand the magnitude of the effect that party-level partisanship has on creating non-median outcomes.

SECTION 6: RESULTS

The results vary by spending category, showing support for a strong correlation between ideology and spending outcomes, but limited support for the model presented in Section 3, as the correlations vary between positive and negative. The tests imply that party-level ideology plays a measurable role in determining how well parties can change the MVT predictions, but that its role is more complex than anticipated by the model. Partisanship seems to help blocking parties achieve their goals, but also appears to aid proposing parties in more than half the cases. The mechanism suggested by which a party's effectiveness in bargaining with legislatures is largely driven by party-level ideology must be rejected.

All-in-all, the test consisted of 30 regressions divided into three specifications: budget-share/distance of party medians from zero, log-differences/value of party median, and budget-share/absolute distance between party medians. The full tables are quite dense, and are reproduced in full in the appendix. Table 3 is an example of the regression tables presented to facilitate their explanations.

Before outlining the results, three important notes: first, the health programs category in the share/median-distance regressions–both NOMINATE and Heckman-

TABLE 3: SHARE, INCOME SECURITY	Income Security Share of Spending
Lagged Dependent Variable	0.683616
	(0.059289)***
Distance of Rep's median from zero, nominate	-0.0309
	(0.012369)**
Distance of Dem's median from zero, nominate	0.002222
	(-0.011779)
Stan. Dev. of Rep Ideal Points	-0.016385
	(-0.026493)
Stan. Dev. of Dem Ideal Points	-0.005969
	(-0.015326)
House Majority, Rep=1	0.026217
	(0.005636)***
Senate Majorty, Rep=1	-0.011893
	(0.004481)***
Percent labor force unemployed, 14+ through 46, 16+ after	0.007488
	(0.001213)***
Delta GDP year-on-year, Percent	-0.110632
	(0.034016)***
Percent Population 65+	-
	-
Constant	0.084834
	(0.032965)**
R-Squared	0.9297
F-Test that Error Correlation=0	0.5583
Observations	118
Panels in Regression:	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

Snyder versions–should be entirely discounted as the coefficients for the lagged-dependent variable are greater than one; spending increases regardless of any variable identified in the regressions, and inference might be impossible. Second, please remember that the aggregate "human resources" variable is, by percentage, largely composed of social security. The other programs are relatively small, thus results reported for under the human resources category are largely driven by social security.

Finally, while I applied the model's predictions uniformly across all four unique spending categories, it is important to note that the categories are different. The education, training, employment and social services and health categories are primarily composed of "appropriated" programs for which Congress must re-authorize budgetary allocations frequently–either annually or every time a specific program ends. These stand in contrast to the social security category, almost entirely composed of two large "entitlement" programs that grow yearly to accommodate all eligible citizens. Income security falls somewhere in between as it includes appropriated programs–temporarily increasing the length of unemployment insurance–and entitlement programs–the federal employee pension plans. On initial instinct, one assumes that appropriated programs will display a greater sensitivity to party-partisanship. However, during large overhauls and government reorganizations, entitlement programs come up for review, and during these times, party-partisanship may affect the outcomes. The first part of this section will proceed to test the model on the assumption that it serves to explain variation across both types of policy. A second heading, using the results obtained from the tests, will attempt to unpack this assumption and treat the appropriated and the entitlement programs separately.

The variables standing for party-partisanship return only mixed support for the model, even when considering the significant results alone. Below are the implications of the model, and how they translate into predictions on the empirical tests:

1. That moderate parties should be more effective at passing legislation than ideological parties. In the tests, the median-distance variable for the Democrats are expected to be negatively correlated social spending, and the median-value variables are expected to be positively correlated: as the median-distance variable increases, the party is more partisan, and the model implies that moderate parties will more effectively bargain for social spending. The median-value variable runs from -1 to 1, and increases portray the Democratic party as more moderate, thus the correlation will be positive with increased social spending.

2. That ideological parties should be better at "blocking" or slowing legislation, due to the high cost of their members. The partisanship of the Republicans is rising in both measures of party-ideology; both the median-value and the median-distance variables are expected to be negatively correlated to social spending variables.

3. That the proposing party will be less effective as the distance between the parties grow, regardless of which party is more ideological. Measures of absolute distance between the parties should be negatively correlated with the spending variables.

4. That both proposing and blocking parties will benefit from homogeneity within their parties, as proxied by the standard-deviation of their members' ideal points. As effectiveness for the Democrats can be measured by their ability to increase social spending, the standard deviation of the Democratic party is expected to be negatively correlated to the social spending measures—as they are more homogenous, they will be more effective. The Republicans pursue

the opposite goal, and thus have the opposite expected outcome: the Republican standard deviation is expected to be positively correlated with the social spending variables.

In the majority of regressions, the model's prediction on the partisan advantage in blocking legislation proved accurate. The Republicans limit social spending most successfully when their party median have a high value, close to one. The share/median-distance specifications outperform the log-difference/median-value ones by five supportive significant results to two. The superior performance of the model in the share/median-distance specifications relative to the log-difference/median-value specifications might be explainable: the share/median-distance specification measures the share of the US budget that each spending category receives. Decreasing this share can be done by either effectively fighting a proportional increase in that category relative to the budget, or by increasing government spending as a whole outside that category. The share variable might then, to some degree, measure not only the ability of partisan Republicans to fight increases in social spending, but also their ability to extract larger in-kind political support for pet-projects or conservative policies. Demanding federal funding in return for their votes has the same effect in this measure as slowing the expansion of the program. By contrast to the share specifications, the log-difference specification captures only percentage increases in that one category, omitting the possible bargaining aspects of share-specification.[15] Table 4 shows the coefficients on the Republican ideology variable from each regression in which it was included. (The full tables of these regressions are located in the appendix, tables 10-16.)

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[15] An unintended consequence of this specification is the corollary: If the Democrats increase social spending, but less than they did other liberal spending policies such as environmental protection, then the party will appear less effective than if they had not accomplished such legislation. This problem does not seem to affect the results, though.

TABLE 4: COEFFICIENTS OF REPUBLICAN PARTY IDEOLOGICAL MEASURE

	Share/ Median-Distance, NOMINATE	Share/ Median-Distance, HS Coords	Log-Differences NOMINATE	Log-Differences HS Coords	Adjusted by Social Spend/ Median-distance, NOM	Adjusted by Social Spend/ Median-distance, HS
Human Res.	-0.100771***	-0.052833*	-0.087152	-0.294854***	-	-
Education, Etc.	-0.015024**	-0.012437*	0.294637**	0.38106	0.02797	0.04289***
Health	-0.001981	-0.003226	-0.046338	-0.103658	-	-
Social Security	0.022565***	-0.011908	0.023133	0.02471	0.006682	-0.007923
Income Security	-0.0309**	-0.018687	-0.132227*	-0.063776	-	-

Values are coefficients from regressions, stars indicate significance: *=90%, **=95% ***=99%

All are expected to be negative

Two exceptions to the general proof for "partisans as better blockers" show up. (Actually, several such anomalies exist, but only two of them are significant to the 90% level or higher.) They occur the share-specification of social security, and the log-difference specification of education, training, and social spending–both are the NOMINATE measures. Neither of these show the expected negative correlation with social spending, and display strong *positive* correlations instead. The companion regressions to these two anomalies, the same regressions run with Heckman-Snyder coordinates, either directly contradict the findings in the exceptional two, or show the same result but with much lower significance. An in-depth examination of the deviations from the model will follow in the Section 7, and I will portray these missed predictions as the function of a non-controlled environmental factor in the appropriations process.

In sharp contrast to the corroboration displayed by the Republican opposers, the model's proposer-implications find very little support. The share/median-distance regressions show only *one* regression in which moderate Democrats are significantly correlated to increases in spending, and the Heckman-Snyder version alone shows three cases in which the opposite is strongly suggested. Only the education etc., category supports the model's theory. The log-difference regressions show a similar set of mixed results, but one in which social security spending joins

education to support the model against income security and human resources. Unfortunately, the support from social security is not valid: the log-difference variable essentially measures percentage increases. 1940 was the second social security payment, and the first payment to be consecutively followed by a disbursement the following year (SSA Historical). During the following decade, the scale of social security expanded rapidly. The '40s also saw an extremely moderate Democratic party—probably a function of the conservative Southern Democratic faction. These two facts are not correlated, just contemporaneous: social security was passed into law in 1935, and the large increases in payments were largely a function of the evolving agency, not legislative bargaining. For this reason, the support for the model drawn from social security in the log-differences regressions should not be considered valid. To summarize, it appears that partisan parties make better blockers, but with the exception of education spending, it would appear that partisan parties make better proposers as well. Table 5 shows the coefficients from every variable representing Democratic party ideology. As the variation has an interesting consistency across spending categories, a deeper discussion will appear in Section 7.

TABLE 5: COEFFICIENTS OF DEMOCRATIC PARTY IDEOLOGICAL MEASURES

	Share/ Median-distance, NOMINATE	Share/ Median-distance, HS Coords	Log-Differences/ Median-value, NOMINATE	Log-Differences/ Median-value, HS Coords	Adjusted by Social Spend/ Median-distance, NOM	Adjusted by Social Spend/ Median-distance, HS
Human Res.	-0.009087	0.187264***	-0.188864	-0.397854**	-	-
Education, Etc.	-0.020499***	0.002215	0.497607***	0.481959**	0.008099	-0.066494***
Health	-0.003247	0.000497	-0.129995**	-0.170274*	-	-
Social Security	0.007466	0.02464***	0.163258***	0.133665*	-0.006763	0.023208
Income Security	0.002222	0.059594***	-0.245931**	-0.175064	-	-

Values are coefficients from regressions, stars indicate significance: *=90%, **=95% ***=99%

Please remember that according to the model, share/median-distances are expected to be negatively correlated, and log-difference/median-values are expected to be positively correlated.

The magnitude of the coefficients on both proposer and opposer variables are fairly consistent and large. Interpreting the coefficients shown in Tables 4 and 5 is straightforward.

Consider the share/median-distance regression on education spending: Table 5 shows that a change between the most moderate Democratic parties and the most partisan Democratic party is associated with a 2.05% decrease in the education spending category's share of the budget. (I.e., the education share might be 10% of the budget during the most moderate years, and 7.95% of the budget during the most partisan years, or the budget might grow by 2.5%, but education's allotment stayed stagnant.) The log-differences represent percentage change, and are even simpler: as shown in Table 5, a change Democratic party's median NOMINATE score from the most partisan to the most moderate is associated with a .497% increase in education spending. Due to the size of the sample, outlier cases unduly influence the magnitudes on the coefficients, and so inference focuses on the sign preceding the coefficients, not the magnitudes.

To address the concern that the results from the share/median-distance are actually driven by non-bargaining related affects to the denominator—changes to the US budget that are due to purely exogenous factors—I ran one more specification, examining the correlation between the party median-distances and social spending outcomes adjusted not by total government spending, but by the total spending on social programs--the human resources variable. The results from the majority of the regressions either display no significance for any control variable, or the Heckman-Snyder and NOMINATE coordinates show contradictory results. The full tables are reproduced in the Appendix (Tables 15 and 16). Of concern are the results from the education, eraining, employment, and social services category when using the Heckman-Snyder coordinates: the Republicans look to gain from moderation, directly contradicting the findings from the share/median-distance specification, but supporting the findings from the log-difference equations (Table 4). One possible explanation might be that during debates over education, Republicans tend to extract large spending concessions from Democrats in other parts of budget,

an effect only captured by the "share" variables. The only other significant result occurs between Democratic partisanship and the education, etc. category (Table 5). This result corroborates the results from the previous two specifications: Democrats tend to excel in this category when moderate.

Overall, the predictions of the model regarding the correlation between partisanship and social spending outcomes seem tenuous at best. Unfortunately, the standard deviation predictions exhibit equally lackluster results. The Democrat's results, expected to be consistently negative in all regressions showed achieved only one significant result across all 25 regressions. As it was to the 95% level, this could easily be caused by chance. The Republicans results show a similar ambivalence to homogeneity, achieving statistical significance in three contradictory circumstances. The almost perfect lack of results does not stem from a lack of variation among any of the variables: the standard deviations computed both by W-NOMINATE and Heckman-Snyder system collapse and expand by large percentages of their own value (please see Table 2 in Section Four). The Republicans tend to have tighter parties and lower variation over the course of the study. Despite the failure of the model in predicting the importance of homogeneity, the results show little support for a "big tent" party either; the spread of member ideal points is simply immaterial to the legislative game.

Interpreting the coefficients from the standard deviations is very similar to interpreting the results of the party-ideology variables. The share/median-distance specifications show the effect of moving from the most homogenous (lowest standard deviation) to the least homogenous (highest standard deviation) on the percentage of the budget allocated to each category. The log-differences measure the effect on the percentage increase. As above, the magnitudes are too easily swayed by outliers to merit interpretation, and so I focused on the signs.

TABLE 6: COEFFICIENTS OF REPUBLICAN PARTY STANDARD DEVIATIONS

	Share/Median-Distance, NOMINATE	Share/Median-Distance, HS Coords	Log-Differences NOMINATE	Log-Differences HS Coords
Human Res.	-0.075989	0.036283	-0.037916	0.228684
Education, Etc.	-0.016054	-0.008623	1.096027*	0.257415
Health	-0.008638	-0.005458	-0.415354**	-0.333871
Social Security	0.000729	-0.004502	0.063488	0.041895
Income Security	-0.016385	0.059364**	-0.102352	-0.039346

Values are coefficients from regressions, stars indicate significance: *=90%, **=95% ***=99%

All values expected to be positive.

TABLE 7: COEFFICIENTS OF DEMOCRATIC PARTY STANDARD DEVIATIONS

	Share/Median-Distance, NOMINATE	Share/Median-Distance, HS Coords	Log-Differences NOMINATE	Log-Differences HS Coords
Human Res.	0.044784	-0.144342**	0.085805	-0.193512
Education, Etc.	0.000305	0.015684	-0.438785	0.318559
Health	0.001494	0.00043	0.191397	0.13069
Social Security	0.016422	0.012233	-0.119614	0.024402
Income Security	-0.005969	-0.023653	0.263585	0.206539

Values are coefficients from regressions, stars indicate significance: *=90%, **=95% ***=99%

All values expected to be negative.

In addition to the two specifications testing for the effects of individual party ideology, I ran an additional specification testing for a correlation between the share of social spending and the absolute distance between the party-medians. The "share" measure was selected for two reasons: first, due to the historical happenstance described above, the log-differences measure prohibits inference from the social security spending category. Second, the "share" measure captures the possibility of partisan Republicans demanding equal or greater support for their own spending projects in return for their votes. Unlike the standard deviations, the share/distance specifications, especially the education category, support the model's implications of the model: as the distance between the two parties varies from its lowest to its highest value, the share of the budget devoted to social spending decreases by the coefficient's equivalent percentage value (0.05=5%). (The full regression tables complete with control variables are located in the

appendix, tables 17 and 18.) Of course, the evidence is not pure: the results from the Heckman-

Snyder regressions concerning social security and income security show the distance between the parties as *positively* correlated to spending. The deviation in these regressions mirrors the deviation seen in the party-ideology specifications above: the model fits for education, but the

TABLE 8: COEFFICIENTS OF ABSOLUTE DISTANCE BETWEEN PARTY

	Share/Distance, NOMINATE	Share/Distance, HS Coords
Human Resources	-0.052361**	-0.01079
Education, Etc.	-0.017334***	-0.006668*
Health	-0.002313	-0.001401
Social Security	-0.002313	0.001545
Income Security	-0.007901	0.011124*

Values are coefficients from regressions, stars indicate significance: *=90%, **=95% ***=99%

All values expected to be positive.

opposite results are obtained from income security and social security.

In addition to the results of the test, another curious finding appears in the data: across almost all of the regressions, Republican control of the House is *positively* correlated with social spending in every significant result across all of the data. The variable for House control takes the value one if the House is controlled by Republicans, and zero is the Democrats are in the majority. This positive correlation implies that social spending grows when Republicans are in control of House. The coefficients are displayed in Table 16.

TABLE 9: COEFFICIENTS ON HOUSE MAJORITY, REP=1

	Share/ Median-Distance, NOMINATE	Share/ Median-Distance, HS Coords	Log-Differences NOMINATE	Log-Differences HS Coords	Distance Between, NOMINATE	Distance Between, HS Coords
Human Res.	0.066136***	0.076006***	0.022259	0.055655	0.066325***	0.058908***
Education, Etc.	0.004396	0.003739	0.223679*	0.227432*	0.003598	0.002538
Health	0.001984*	0.001336	-0.059825	-0.065503	0.001834*	0.00157
Social Security	0.00128	0.002424	-0.013483	-0.013886	0.001834*	0.002361
Income Security	0.026217***	0.028166***	0.122258*	0.134187*	0.028716***	0.025015***

Values are coefficients from regressions, stars indicate significance: *=90%, **=95% ***=99%

All values expected to be negative

Considering the goals of the party, one obviously expects these coefficients to be extremely *negatively* correlated with social spending as the Republican party would attempt to keep social spending off the floor when in control of the agenda. Additionally, the magnitude of this unexpected result is consistently large: looking at the share/median-distance specifications for income security, changing control of the House from Democrat to Republican increases spending on income security as a percentage of the budget by more than 2.6%. Regressions on Human Resources show even larger effects. Furthermore, unlike the party-ideology variables, the results do not vary by spending category–all are positively correlated with Republican control of the House. Ignoring the magnitude of the effects as they are susceptible to outliers, this result is truly puzzling, especially compared to the results from the Senate coefficients in which all significant results show the expected *negative* correlation with Republican rule.

If the aberrations were contained to share/median-distance specifications, one could attribute the unexpected results to the Republican ideals of fiscal conservatism across the rest of the budget, artificially boosting the share of social spending. But the log-difference/median-value regressions support this surprising finding, implying that Republicans are associated with larger percentage increases in social spending as well. As Republican control of the House was infrequent during the period under examination–5 Congresses of the 31 in the study–these results must reflect a series of outlier cases, perhaps the Clinton administration, during which social spending increased dramatically despite Republican majorities. As the variation across time is more consistent within the Senate where Majority control frequently bounced between the parties, the results seem more trustworthy.

While puzzling, these results are not entirely uncorroborated in the literature. Warren 2008 examines state legislature undergoing reversals in partisan control and finds the added

benefit to cameral control is surprisingly small when controlling for the relative sizes of the party. This result could speak to the results presented here: Republican control of the agenda might have little effect without taking a large, decisive lead in the seat count. In four of the five instances, Democrats held fewer than 25 seats less than Republicans—a difference less than 5% of the House's total population. Unused to exercising agenda-setting powers and without large majorities the Republican leadership may have been unable to stop the Democrats from carrying on "business as usual" in increasing social spending.

TABLE 10: COEFFICIENTS ON SENATE MAJORITY, REP=1

	Share/ Median-Distance, NOMINATE	Share/ Median-Distance, HS Coords	Log-Differences NOMINATE	Log-Differences HS Coords	Distance Between, NOMINATE	Distance Between, HS Coords
Human Res.	-0.019511	-0.050969***	-0.056721	-0.120496**	-0.020167	-0.022655
Education, Etc.	-0.000753	-0.00251	-0.11814	-0.056467	0.000261	0.000045
Health	-0.001825**	-0.001857*	-0.067743*	-0.101353***	-0.001504*	-0.001368
Social Security	-0.003663	-0.006731**	-0.004359	0.019986	-0.001504*	-0.002783
Income Security	-0.011893***	-0.02063***	-0.121409**	-0.151836**	-0.013622***	-0.015102***

Values are coefficients from regressions, stars indicate significance: *=90%, **=95% ***=99%

All values expected to be negative.

After viewing the results, one can see that the mechanism suggested by the model cannot be correct. The model attempted to link the ability of a party to bargain with its members for cooperation with the party's partisanship. It implied that moderate parties would be more effective in proposing policy as they could more easily sway their constituents. Along with the model's predictions about the important of party-homogeneity, this hypothesis was shown to be incorrect as the evidence points to a more nuanced relationship between partisanship and legislative outcomes. The take-aways from this test should be that party-partisanship plays a role in determining the effectiveness of parties in Congress, but that the mechanism through which

ideology affects outcomes might be through party-politician bargaining. A more detailed

discussion of the model's failings and oversights follows below.

SECTION SEVEN: DISCUSSION

The findings above display mixed evidence for the model presented in Section Three. However, the contradictory results seem to systematically occur within specific spending categories, regardless of the specification. While the model itself is beyond redemption, it might serve as a basis for better understanding the more complex realities of partisanship in legislation. In designing the model, what should have been apparent is that the categories above differ in the way in which they grow. From this disparity, we can divide the four categories into two groups: entitlement policies and appropriated policies. This section provides two possible explanations for how the difference between these policies could have produced the results presented above.

New versus Old

Spending growth in social security and income security—members of the entitlement group—is more or less pre-determined each year; it is a function of the number of people older than 65, and the number of people unemployed.[16] Increasing the magnitudes of these entitlement policies beyond what is determined by demographic forces and economic turmoil requires changing an existing policy with a solid status quo. Over the years, social security has been amended many times with such additions as survivor's benefits, hospital and medical insurance, changes to the pay-roll tax structure, and automatic inflation adjustment. Each of these amendments can be thought of as changing the placement of social security on the ideological spectrum—refinements to the original policy. For the most part, the legislative actions pertaining to social security are even referred to as "amendments," not as bills or acts in their

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[16] Please remember that income security includes the federal employee pension fund.

own right.[17] (Kollman and Soloman-Fears 2001). Each act of legislative bargaining updates or extend an "old" policy.

Alternatively, programs like education, training programs, social services, health, and some of the sub-functions included in the income security category grow more by *new policies* than by incremental changes to old ones. Education provides a good example: until the 1940s, federal support for education was almost entirely land-grants. However, over the next sixty years, tens of programs enacted by Congress granted funding for vocational education, school lunches, library services in rural areas, and support for special-needs that had never existed before. The funding for each existing program might grow to keep up with population growth, but the largest increases in these policy categories come from new programs taking shelter under their umbrellas (Digest of Education Statistics 2009). In these categories, the bargaining over increases in spending occurs over the new pieces of policy, policies that do not yet have a status quo location on the ideological spectrum.

When comparing these two groups to the results produced in Section 6, one notes that policies growing primarily through the addition of "new" programs show limited support for the model: moderate Democratic parties are more successful at increasing those categories, and partisan Republican parties block well. In contrast policies that increase through amendments to "old" programs fit the model least-well. These most noticeably include the social security results and the income-security results, though the human resources results support this as well given the large role social security plays in its composition.

With such a systematic breakdown of the model's goodness of fit, one can imagine that something about the difference between "new" and "old" policies affects the model in a

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[17] When the tax structure changes, the tax acts are referred to individually, not as amendments to social security. (Kollman and Soloman-Fears 2001

predictable way. Consider the two policy types in a legislative bargaining context: programs like social security have a set value on the ideological spectrum at the beginning of each Congress. To increase the social security payment, the Democratic party must be chosen as the proposing party, and then within that party, a member must be chosen whose proposition, once tempered by the majority party, is closer to the Democratic median than the current policy. Assuming that social security is a fairly moderate policy, a moderate Democratic party would rarely have drawn from its ranks a member whose preferences would be extreme enough to propose such legislation; when they did, the movement of the policy would be incremental at best.[18] Alternatively, a partisan Democratic party would produce many more proposals, even if such proposals were more expensive for the party to pass.

Now consider categories like education, training, and social serices: these programs grow when *new* policies are proposed and passed. If the Democrats wanted to increase education spending for science, they might create a new bill funding mobile lab units for high schoolers or a system under which science teachers are rewarded for improved performance. On reaching the floor, this new bill, "Improved Science Competitiveness of American Students for America," would be voted against the alternative of zero funding for the project. While education policy itself had a status quo that existed before the bill, this new bill might be treated as a semi-independent entity. The set of Democrats who would propose such a bill is large as the "status quo" for this policy is an extremely conservative zero. In this case, the Democratic party is in the position of finding the necessary votes to pass the bill, a process similar to the one described by the model, producing results that better fit the model's predictions. This alternative theory is corroborated from the results generated by the share/distance specifications: the education

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[18] Social security seems fairly moderate: it is not extremely redistributive, contributions are capped, and those who pay in receive disbursements, regardless of their need.

category is negatively correlated with the distance between the two parties, but the more predetermined income security category is positively correlated. Proposers excel at passing education and social welfare policies when they are close to their opponent, decreasing the cost of bargaining over a new policy. However, when passing amendments to "old" policies, proposers are better when more distance exists between the parties, increasing the odds that the chosen legislator will be able to propose at all.

Essentially, this modification argues that new programs are in some ways fundamentally different from existing programs during the bargaining game. One could test this hypothesis by examining the set of new or "landmark" policies as defined by a third party that occur in any one spending category over time, and compare the partisan effects on these votes to those increases that occur naturally or demographically within that same category.

Appropriated versus Entitlement

Another–related–way to interpret the results might be to examine how frequently a policy is discussed on the floor on Congress. Instead of dividing the policy world into "new" and "old" policies, we can think about those policies that require yearly appropriations votes, and those that increase as a function of demographics or economic conditions. Here, we distinguish between "appropriated" policies and "entitlement" policies as well, but for a different reason. Appropriated polices, like those found in the education-policy sphere, receive frequent Congressional attention as their budgets must be approved each year by a floor vote. In contrast, policies like social security, since 1975, increase in an automated fashion each year, reacting to changes in the CPI, and are discussed on the floor of Congress rarely. We would expect to see a greater partisan influence among those policies that parties can more frequently bargain over.

Additionally, we would expect to see greater advantage to moderation in policies with more frequent bargaining as moderation facilitates bargaining between politicians and parties. Policies bargained over less frequently will be less affected by partisanship, and might even be postively correlated with partisanship: only very partisan parties propose amendments to the yearly-moderated entitlement policies. Therefore, we expect to see the Social Security and Income Security (as driven by the federal pension programs during times of economic normalcy) show either zero or positive correlation to increased partisanship. On the other hand, categories like education, etc., those frequently revisited by Congress for appropriations bargaining exhibit negative correlation with increased partisanship.

To test this modified version of the model, let's focus on one category, and exploit a discontinuity in time. Through 1975, social security payouts were increased and adjusted by direct Congressional action whenever Congress saw fit to do so. Such adjustments were not made regularly, and were still rather infrequent. From 1976 onward, payments were automatically adjusted for inflation each year, almost eliminating the need for legislative action on the issue (Social Security CRS Legislative History 2001). We would thus expect Social Security outcomes to exhibit a larger correlation with partisanship before 1975 than after. Table 11 shows the results from the regressions run on the restricted time periods with Heckman-Snyder coordinates.

As one can see, the effect of partisanship on social security's share of the overall budget is both larger and more highly significant during the earlier time period. While neither time period exhibits significant results, the Democratic median-distance coefficient hovers just above 80% significance for the regression between 1940 and 1975. Consistent with both the "appropriated-versus-entitlement" and the "new-versus-old" modifications above, we see that proposing parties gain from partisanship when bargaining over an infrequently updated policy with a pre-

TABLE 11: SOCIAL SECURITY'S DISCONTINUITY	Social Security 1940-1975	Social Security 1975-2000
Lagged Dependent Variable	1.010219	0.404303
	(0.038408)***	(0.135451)***
Distance of Rep's median from zero, nominate	-0.014181	0.019854
	(-0.013925)	(-0.012922)
Distance of Dem's median from zero, nominate	0.021348	-0.013573
	(-0.016276)	(-0.015027)
Stan. Dev. of Rep Ideal Points	0.002342	-0.044646
	(-0.030875)	(0.018635)**
Stan. Dev. of Dem Ideal Points	-0.013976	0.000237
	(-0.025809)	(-0.012734)
House Majority, Rep=1	-0.000138	0.01558
	(-0.004251)	(0.004316)***
Senate Majorty, Rep=1	Dropped,	-0.003382
	Collinearity	(-0.002791)
Percent labor force unemployed, 14+ through 46, 16+ after	-0.000078	0.001957
	(-0.001016)	(0.000817)**
Delta GDP year-on-year, Percent	-0.000941	0.09728
	(-0.02485)	(0.035056)***
Percent Population 65+	0.000102	-0.000372
	(-0.000234)	(-0.000279)
Constant	0.003485	0.023046
	(-0.027923)	(-0.032638)
R-Squared	0.9807	0.1264
F-Test that Error Correlation=0	0.6818	0.1515
Observations	68	48
Panels in Regression:	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

determined status quo. In the later era, this partisan-advantage is lost as increases to social security occur mostly in reaction to inflation. As such, one can see that the environmental variables play a much larger role after 1975 than they do before—logical as inflation is co-determined with many of the control variables such as GDP growth and unemployment.

Following the above modification blindly, one might expect to see an even *greater* partisan-advantage after 1975: a party capable of producing legislators so extreme as to propose changing the inflation-adjusted status quo would have to be very partisan. While consistent with the theory, this prediction is not borne out in the results as it simply did not occur in the 25 years examined. Perhaps the US voters declined to elect so partisan party, or perhaps a social security overhaul of a magnitude greater than the inflationary pressures has not yet occurred. Either way, by accurately predicting varying levels of partisan-advantage, one confirms at least one piece of the modified theory: the varying relationships between partisanship and spending outcomes does *not*

imply that the connection between the two is random, merely that the connection depends on a wider spectrum of controls than initially controlled for here.

Taking an even closer look at the evolution of social security, Figure 4 shows a scatter plot of the 1950s, a time during which social security expanded rapidly, largely through increases in eligibility and the enactment of social security disability insurance (SSDI) (Social Security CRS Legislative History, 2001). Figure 4 excludes two outlier years facilitating a closer examination of the remaining results. During a ten year period, demographic drivers lose much of their strength, allowing us to see some of the effects of partisanship in a simple scatter plot without the fear that an uncontrolled environmental variable drives the results. The results support the regressions above, showing a strong partisan advantage in the era before 1975. The vertical axis represents the partisan value of the party, and the horizontal axis shows percentage increases in spending.

FIGURE 4: DEMOCRATIC MEDIANS AND PERCENTAGE CHANGE SOCIAL SECURITY SPENDING
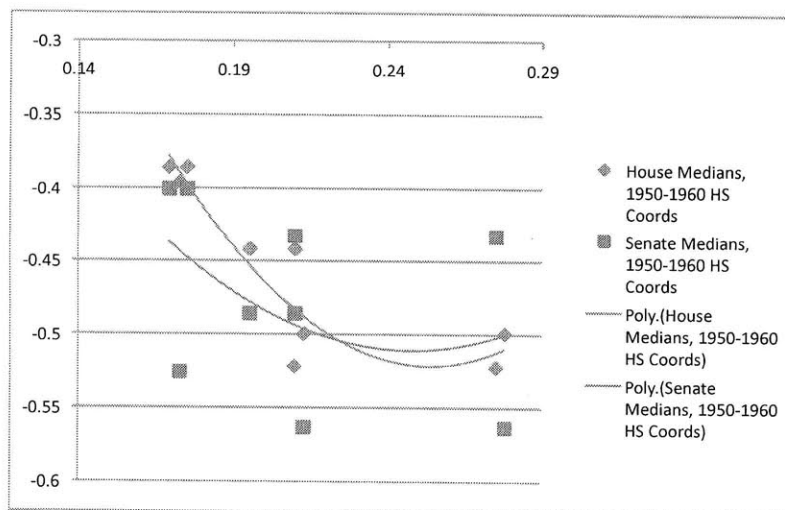
Other such discontinuities exist for quick visual tests. In 1994, social welfare coverage changed from AFDC to TANF, effectively changing from an entitlement program to a block-grant program. Grants are bargained over yearly in Congress. As such, we would expect to see increases in spending more closely correlated with moderate Democratic party ideology after

1994 than before. Figures 5 and 6 show the comparison. (The data for the sub-category of social services spending also comes from the OMB Historical tables, but is only available after 1962, and so was not included as its own category of social spending.)

FIGURES 5: DEMOCRATIC PARTY MEDIAN AND PERCENTAGE INCREASES IN AFDC, 1987-1993
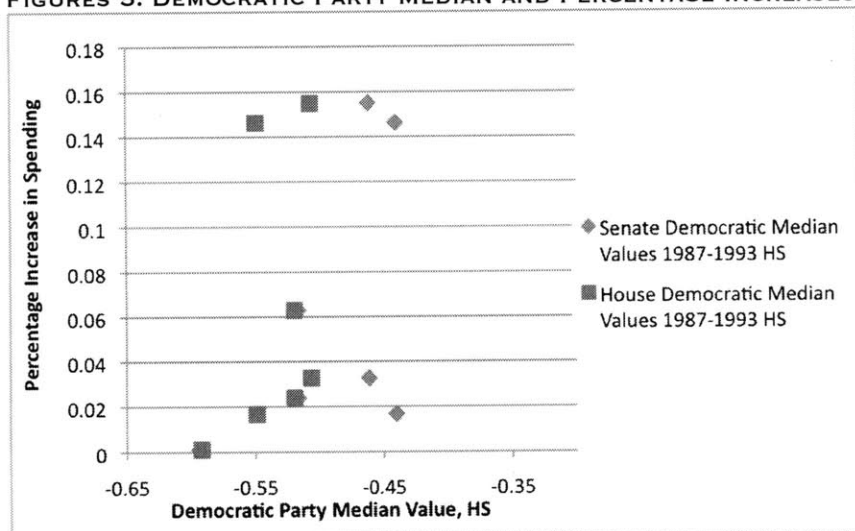

FIGURE 6: DEMOCRATIC PARTY MEDIAN AND PERCENTAGE INCREASES IN TANF, 1994-2000
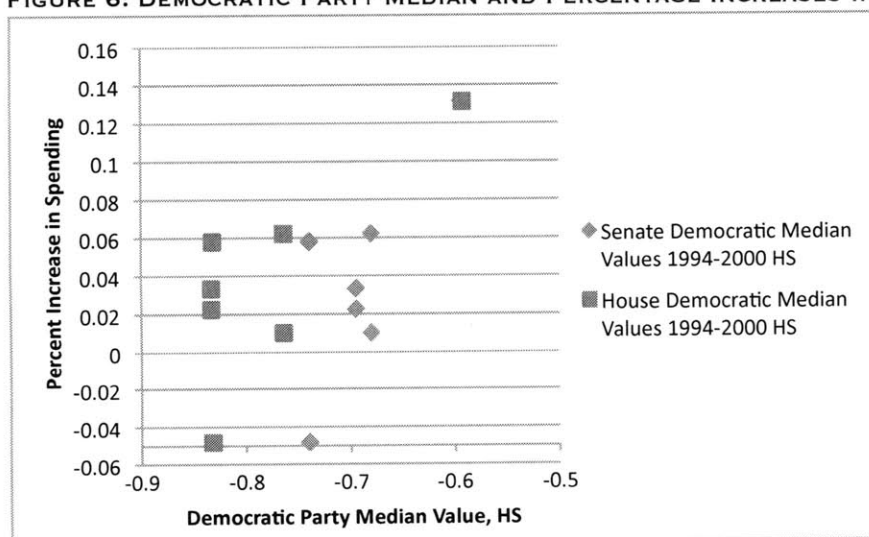
In both cases, the x-axis shows the Democratic Party median values, and the y-axis shows the percentage increases year-on-year. Please note how the the later time-period exhibits a much higher correlation with spending increases, even though the magnitude of the increases is not necessarily larger.

The Health category presents a stumbling block to both modifications proposed. In the share specifications, spending on health programs seems to rise regardless of any control variable, purely as function of its passed value. In the log-difference regressions, health shows a partisan advantage for proposers, but as this category excludes medicare, one would assume that it more closely resembles new and appropriated programs. One possible explanation might be that the costs of healthcare per person have risen more dramatically than almost any other tracked index. If Congress attempted to maintain the purchasing power of its appropriations to Health, not only the absolute dollar amount, it could explain why we see "unexplained" increases, unaccounted for by the control variables that track better with the cost-of-living inflation rate. Understanding the effects of partisanship on healthcare spending might require more specific control variables than accounted for in this paper.

Tests and explanations examining the categories individually or over short periods of time show one avenue of future research: focusing on particular moments of change in the allocation, distribution, or purchasing power of each variable would provide much deeper insights into the actual effects of partisanship. The two modifications try to explain how the model's failure might be explained, but the possibility that these correlations is either random or driven by an unobserved variable is quite real. For example, perhaps the level of partisan influence is driven by the level of public interest given any one topic. In this theory, one could posit that the electorate pays little attention to welfare policy or national education policy, giving the parties more freedom to bargain with politicians and achieve their goals, increasing the effectiveness of moderate parties. Contrariwise, there is deep public interest shown to social security payments and unemployment insurance in times of economic need. Constrained by their constituents, parties might lose their ability to bargain freely with politicians. Without party-politician

bargaining, only individual politician ideology matters and politicians must vote as their districts demand. This produces results that show increased efficacy with increased partisanship as greater partisanship throughout the party would increase cohesion simply through greater homogeneity. To test this, one could compare the results of public opinion polls regarding different policy areas, and look for correlations between how strongly the public cares about any one issue, and the effects of partisanship on that policy's legislative history.

Furthermore, given to the importance of hierarchy in Congress, the inclusion of a statistic showing the party's mean member's time in Congress might prove illuminating in understanding how party goals are affected by longer-term members. Updating the model with a deeper and more extensive form bargaining game between the party and each politician would allow both for the characterization of expected outcomes given a set of assumptions, and would facilitate implementing an explicit step in which breakdowns in negotiations could occur, adding some realism to the model. By blindly assuming that the party and the politicians can always reach an an accommodation, I exhibit the same oversight displayed by most of the legislative bargaining literature. Modeling each party-politician interaction, possibly including assumptions of imperfect information regarding preferences and hold-out prices would substantially improve the model's realism, and might produce predictions more consistent with the results.

Caveats

Unfortunately, modifying the model does not solve every inconsistency in the data. As described above, the log-differences specification is clearly a poor choice for social security. Additionally, the results from log-differences regarding education spending (Table 13 located in the Appendix) are baffling and seem to directly contradict the results from the share-specification.

Why Republicans appear to lose the partisan blocking advantage in that one regression is confusing. Similar to social security, this result might have an exogenous explanation, but more likely it has to do with the composition of the spending category: as it includes training and employment services, this might increase with extremely partisan Republicans as the Democrats extract funding for training programs in return for free-trade propositions and other open-market policies. The characterization of this as an aberration is consistent both with the budget-share regressions, the overall distance specifications.

Despite the model's failure to explain the data, the frequent, significant, and sizable correlations between party-level ideology and social spending outcomes suggest that another mechanism through which partisanship drives effectiveness and skews MVT predictions exists yet uncovered. In my mind, an additional reason is needed to account for the model's poor performance, in addition to the methodological failing in not accounting for different types of spending growth between policies. Due to the complex due to the complex nature of bills and the plethora of riders and private deals on each piece of legislation, specific bills might not represent a pure "public good" as described in the model. As a bill's contents do not have the same effect on each legislator, assuming that each policy-area has the capacity to affect every legislator materially is a stretch. Incentives created by locally-elected politicians might play a much larger or more complex role in legislator utility than assumed in the model, reducing the effect of policy-outcomes on legislator utility, and undermining the model's predictions. This is actually a violation of an assumption made in Section 3: politicians might care less about one preferred policy than another, or they might favor goods given to their district over any policy accomplishment, or each politician might value the party-compensation differently. The model could be wrong simply because legislators might not act rationally, or because their actions are

only rational when considering circumstances left out of the paper. Incorrect assumptions are certain to sabotage any theory, and they might be to blame here as well.

Obviously, the results presented in this section do not fit the model as presented in Section Three, Additionally, several important caveats exist to this study, as well as structural weaknesses. The external validity of this study is extremely limited: not only was it designed for two-party systems, it also uses institutional characteristics specific key to the US system, and thus, the model might not even fit other legislatures. Furthermore, as seen during inference above, policy creation does not occur in a vacuum; a passing knowledge of the policies under examination is essential, and this limits the usefulness of this model. Applying this model to other policy areas would require learning about their histories as well. The model's failure to explain results without such context tremendously limits its usefulness.

Related, the lagged-dependent variables play such a large role in the regressions, and the statistical significance seems so scattered across the variables, that one can almost be certain some other external variable is playing an important role in determining the values presented above. That I must bring in historical events to explain some of the variation should show that omitted variable bias, whether systematic across the entire study or specific to individual regressions must be considered present. The concern voiced earlier—that all spending increases are the result of some unobserved political process or probability distribution function—is still valid.

The structural weaknesses of the test are also fairly apparent: even with fixed effects, the time-series data includes periods of economic crisis, war, and other macro-economic concerns. Clearly 61 years is not a long enough time-period for the effects of these huge disturbances to wash clean. As mentioned in the previous section, the roll-call data itself might deserve some healthy skepticism despite its wide use in the recent past. Finally, the spending data itself was too

blunt an object, and perhaps the specific grouping used by the OMB created, exaggerated, or stifled some results. Such structural weaknesses must be acknowledged, but might not be changed. The results represent the best effort toward candor.

CONCLUSION

Using roll-call and budget data, this paper tried to quantify the effects of party-level partisanship on the growth of social spending in the United States. Overall, the model's predictions fail to explain the relationship between ideology and policy outcomes; the fit varies across different sub-categories of social spending. The results show that, once controlling for environmental factors, party ideology plays a measurable role in determining the importance of social spending to the US budget, perhaps showing that party-level ideology plays real role in determining a parties effectiveness, but not in the way anticipated by the model.

The model's failure to fit the data suggests the mechanism hypothesized is not correct, but the significant effects of ideology on spending outcomes argue for further research into correctly identifying the mechanisms through which parties can use their power to exert ideological influence on legislation. The results consistently show that partisan Republican parties are, *ceteris paribus*, good at stifling social spending legislation, but fails to make a compelling case for the Democrats. The variation found in the results might vary systematically according to the status quo of the policy under consideration, but could also be driven by a variety of other factors included and overlooked in this paper. Due to the model's poor fit, I do not wish to speculate as to *why* partisan Republicans make better blockers–the model's reasoning, that partisan parties are more expensive is appealing, but cannot be proven with rigor, as it does not explain the variation for moderate parties.

Of the four implications, two find consistent support: that partisan parties oppose policy effectively, and that proposers of social policy are less effective as the ideological distance between the two parties grows. The mechanisms through which these two results occur might be those described in this paper–that ideological politicians are harder to compensate for legislative

support—or they might be entirely different. The other two implications—those regarding the importance of preference homogeneity for both parties and the importance of moderation for proposing parties—failed to materialize in a meaningful way. Unfortunately, two for four of the implications is too little proof to reject the null hypothesis that some mechanism other than the one described in this paper drives the correlation between party partisanship and social spending.

As the results do not entirely contradict the model's predictions, this paper would benefit from improvements to the empirical test. In future research, spending data grouped by the finer "sub-function" would greatly improve inference. Indeed, such data would allow for a better understanding of how some programs seem to grow under moderate proposing parties, and others under partisan ones. Obviously, a larger sample would also be beneficial. Additionally, modification of the model to include a second stage in which opposing parties can make competing offers might allow the characterization of a best response function, and lead to more interesting implications for testing. The introduction of a set budget constraint would also lead to more concrete implications, and coupled with the information that programs in which the status quo has a pre-determined value are different, might lead to testing the location of an optimal level of partisanship.

The distinction between entitlement programs and appropriated programs offers many avenues for future research. This study failed to distinguish between these two types of policies when designing the model, and would have benefitted greatly from such foresight. Research regarding the variation between these types of policies might try to find similarly different programs in other policy areas, and examine whether the effects of partisanship are equally bifurcated.

Despite the failure of the model's predictions, the results are still interesting as they shed some light on how sensitive the government might be to valence shocks among the electorate that sweep one party or the other into power. While this paper did not lead to solid predictions regarding the effects of partisanship on the political economy of the United States, the results do show interesting correlations, and future research into this topic might yet yield more concrete results.

APPENDIX:

TABLE 11: SHARE/MEDIAN-DISTANCE REGRESSION, NOMINATE SCORES

	Total HR Share of Spending	Education Training Employment Share of Spending	Health Share of Spending	Social Security Share of Spending	Income Security Share of Spending
Lagged Dependent Variable	0.891938	0.765554	1.024806	0.98339	0.683616
	(0.040301)***	(0.067289)***	(0.020359)***	(0.014665)***	(0.059289)***
Distance of Rep's median from zero, nominate	-0.100771	-0.015024	-0.001981	0.022565	-0.0309
	(0.037717)***	(0.007229)**	(-0.002124)	(0.007805)***	(0.012369)**
Distance of Dem's median from zero, nominate	-0.009087	-0.020499	-0.003247	0.007466	0.002222
	(-0.036321)	(0.006476)***	(-0.001985)	(-0.006849)	(-0.011779)
Stan. Dev. of Rep Ideal Points	-0.075989	-0.016054	-0.008638	0.000729	-0.016385
	(-0.082452)	(-0.019022)	(-0.005489)	(-0.017205)	(-0.026493)
Stan. Dev. of Dem Ideal Points	0.044784	0.000305	0.001494	0.016422	-0.005969
	(-0.052874)	(-0.012183)	(-0.003511)	(-0.011331)	(-0.015326)
House Majority, Rep=1	0.066136	0.004396	0.001984	0.00128	0.026217
	(0.017570)***	(-0.003921)	(0.001053)*	(-0.00307)	(0.005636)***
Senate Majorty, Rep=1	-0.019511	-0.000753	-0.001825	-0.003663	-0.011893
	(-0.013449)	(-0.003379)	(0.000884)**	(-0.002479)	(0.004481)***
Percent labor force unemployed, 14+ through 46, 16+ after	0.007136	0.000106	-	-	0.007488
	(0.003285)**	(-0.000655)	-	-	(0.001213)***
Delta GDP year-on-year, Percent	-0.491308	0.008943	0.00433	-0.014262	-0.110632
	(0.091161)***	(-0.017566)	(-0.00497)	(-0.017404)	(0.034016)***
Percent Population 65+	0.00065	0.000179	0.000051	0.000086	-
	(-0.00042)	(-0.000195)	(-0.00004)	(-0.000094)	-
Constant	0.500315	0.009383	-0.001672	-0.001914	0.084834
	(0.107507)***	(-0.006741)	(-0.00282)	(-0.01853)	(0.032965)**
R-Squared	0.934	0.556	0.9867	0.9887	0.9297
F-Test that Error Correlation=0	0.1727	0.4277	0.3109	0.3118	0.5583
Observations	118	118	118	118	118
Panels in Regression:	2	2	2	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

TABLE 12: SHARE/MEDIAN DISTANCE REGRESSION, HECKMAN-SNYDER COORDINATES

	Total HR Share of Spending	Education Training Employment Share of Spending	Health Share of Spending	Social Security Share of Spending	Income Security Share of Spending
Lagged Dependent Variable	0.264818	0.708542	1.023981	0.97178	0.583551
	(0.062239)***	(0.070242)***	(0.020665)***	(0.013463)***	(0.055462)***
Distance of Rep's median from zero, HS	-0.052833	-0.012437	-0.003226	-0.011908	-0.018687
	(0.029249)*	(0.007402)*	(-0.002244)	(-0.007643)	(-0.011853)
Distance of Dem's median from zero, HS	0.187264	0.002215	0.000497	0.02464	0.059594
	(0.039349)***	(-0.009128)	(-0.002751)	(0.008700)***	(0.013522)***
Stan. Dev. of Rep Ideal Points	0.036283	-0.008623	-0.005458	-0.004502	0.059364
	(-0.081837)	(-0.019483)	(-0.005294)	(-0.015314)	(0.024257)**
Stan. Dev. of Dem Ideal Points	-0.144342	0.015684	0.00043	0.012233	-0.023653
	(0.061087)**	(-0.014035)	(-0.004)	(-0.011952)	(-0.017624)
House Majority, Rep=1	0.076006	0.003739	0.001336	0.002424	0.028166
	(0.016769)***	(-0.003885)	(-0.001068)	(-0.002933)	(0.005338)***
Senate Majorty, Rep=1	-0.050969	-0.00251	-0.001857	-0.006731	-0.02063
	(0.015780)***	(-0.003602)	(0.000985)*	(0.002730)**	(0.004693)***
Percent labor force unemployed, 14+ through 46, 16+ after	0.021042	0.000629	-	-	0.008621
	(0.002444)***	(-0.000629)	-	-	(0.001098)***
Delta GDP year-on-year, Percent	0.301278	0.004727	0.003843	-0.015405	-0.082794
	(0.077497)***	(-0.018654)	(-0.005103)	(-0.017139)	(0.032556)**
Percent Population 65+	0.002201	0.000239	0.00004	0.00005	-
	(-0.002497)	(-0.000168)	(-0.00004)	(-0.000081)	-
Constant	-0.22905	-0.008384	-0.002026	0.01412	0.034976
	(0.013834)***	(-0.007391)	(-0.002691)	(-0.01855)	(-0.028657)
R-Squared	0.415	0.4862	0.9884	0.9911	0.9324
F-Test that Error Correlation=0	0.3803	0.1743	0.3869	0.5407	0.1086
Observations	118	118	118	118	118
Panels in Regression:	2	2	2	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

TABLE 13: LOG-DIFFERENCES/MEDIAN-VALUE REGRESSION, NOMINATE SCORES

	Log-Differenced HR Spending	Log-Differenced Education, Training, Employment Spending	Log-Difference Health Spending	Log-Difference Social Security Spending	Log-Difference Income Security Spending
Lagged Dependent Variable	0.252883	-0.150889	0.205133	-0.151981	-0.083704
	(0.094361)***	(0.087568)*	(0.086433)**	(-0.096384)	(-0.090866)
Value of Republican Median, nominate	-0.087152	0.294637	-0.046338	0.023133	-0.132227
	(-0.079293)	(0.135006)**	(-0.050952)	(-0.040651)	(0.078914)*
Value of Democratic Median, nominate	-0.188864	0.497607	-0.129995	0.163258	-0.245931
	(0.101758)*	(0.142298)***	(0.063961)**	(0.052309)***	(0.096348)**
Stan. Dev. of Rep Ideal Points	-0.037916	1.096027	-0.415354	0.063488	-0.102352
	(-0.315459)	(0.585419)*	(0.205835)**	(-0.16715)	(-0.305496)
Stan. Dev. of Dem Ideal Points	0.085805	-0.438785	0.191397	-0.119614	0.263585
	(-0.191938)	(-0.368361)	(-0.128066)	(-0.102365)	(-0.187875)
House Majority, Rep=1	0.022259	0.223679	-0.059825	-0.013483	0.122258
	(-0.070869)	(0.124154)*	(-0.039078)	(-0.031825)	(0.064999)*
Senate Majority, Rep=1	-0.056721	-0.11814	-0.067743	-0.004359	-0.121409
	(-0.05456)	(-0.106183)	(0.034234)*	(-0.027578)	(0.053094)**
Percent labor force unemployed, 14+ through 46, 16+ after	0.133464	0.222007	-	-	0.311876
	(0.067975)*	(0.097077)**	-	-	(0.062416)***
Delta GDP year-on-year, Percent	-2.923818	2.537111	0.864911	0.152952	-1.626541
	(0.438892)***	(0.616072)***	(0.267357)***	(-0.212133)	(0.459203)***
Percent Population 65+	-0.278948	-0.126917	-0.062434	-0.118268	-0.483462
	(0.099172)***	(-0.194342)	(-0.041927)	(0.035363)***	(0.095303)***
Constant	4.129077	-2.321512	0.240779	2.06445	4.147791
	(0.957785)***	(1.071766)**	(-0.67019)	(0.534500)***	(0.893228)***
R-Squared	0.577	0.0328	0.4571	0.5213	0.4785
F-Test that Error Correlation=0	0.9897	0.3315	0.1767	0.934	0.9954
Observations	116	116	116	116	116
Panels in Regression:	2	2	2	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

TABLE 14: LOG-DIFFERENCES/MEDIAN-VALUE REGRESSION, HECKMAN-SNYDER COORDINATES:

	Log-Differenced HR Spending	Log-Differenced Education, Training, Employment Spending	Log-Difference Health Spending	Log-Difference Social Security Spending	Log-Difference Income Security Spending
Lagged Dependent Variable	0.294942	0.189244	0.184214	-0.112084	-0.157476
	(0.094983)***	(0.099380)*	(0.089374)**	(-0.097684)	(0.088688)*
Value of Republican Median, HS	-0.294854	0.38106	-0.103658	0.02471	-0.063776
	(0.142133)**	(-0.239256)	(-0.092556)	(-0.071158)	(-0.140552)
Value of Democratic Median, HS	-0.397854	0.481959	-0.170274	0.133665	-0.175064
	(0.154576)**	(0.241614)**	(0.095752)*	(0.076825)*	(-0.147149)
Stan. Dev. of Rep Ideal Points	0.228684	0.257415	-0.333871	0.041895	-0.039346
	(-0.3173)	(-0.538043)	(-0.207742)	(-0.170802)	(-0.328021)
Stan. Dev. of Dem Ideal Points	-0.193512	0.318559	0.13069	0.024402	0.206539
	(-0.227456)	(-0.391517)	(-0.147787)	(-0.121367)	(-0.235936)
House Majority, Rep=1	0.055655	0.227432	-0.065503	-0.013886	0.134187
	(-0.071278)	(0.117404)*	(-0.040632)	(-0.033443)	(0.070572)*
Senate Majority, Rep=1	-0.120496	-0.056467	-0.101353	0.019986	-0.151836
	(0.059665)**	(-0.101569)	(0.036912)***	(-0.029219)	(0.061571)**
Percent labor force unemployed, 14+ through 46, 16+ after	0.13193	0.185838	-	-	-1.277707
	(0.068482)*	(0.107100)*	-	-	(0.458084)***
Delta GDP year-on-year, Percent	-2.647593	1.159129	1.004016	0.024395	0.30923
	(0.425652)***	(-0.706687)	(0.256326)***	(-0.208911)	(0.065334)***
Percent Population 65+	-0.257538	-0.116535	-0.033085	-0.130319	-0.464522
	(0.095805)***	(-0.155513)	(-0.040842)	(0.035248)***	(0.097343)***
Constant	3.669721	-0.579205	-0.257549	2.334346	3.433205
	(0.870862)***	(-1.513969)	(-0.637263)	(0.537140)***	(0.813485)***
R-Squared	0.5801	0.3188	0.472	0.517	0.475
F-Test that Error Correlation=0	0.7038	0.5851	0.2699	0.8103	0.9338
Observations	116	116	116	116	116
Panels in Regression:	2	2	2	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

TABLE 15: SPENDING ADJUSTED BY TOTAL SOCIAL SPENDING, MEDIAN-DISTANCE, NOMINATE

NOMINATE, Adjusted Share of Social Spending/Median Distance	Education, etc. Spending Adjusted by total HR Spending	Social Security Spending Adjusted by total HR Spending
Lagged Dependent Variable	0.573854	0.956808
	(0.039745)***	(0.030454)***
Distance of Rep's median from zero, nominate	0.02797	0.006682
	(-0.018986)	(-0.029108)
Distance of Dem's median from zero, nominate	0.008099	-0.006763
	(-0.018649)	(-0.028141)
Stan. Dev. of Rep Ideal Points	0.151221	-0.043928
	(0.044379)***	(-0.065626)
Stan. Dev. of Dem Ideal Points	0.031684	0.012681
	(-0.028979)	(-0.047338)
House Majority, Rep=1	0.005905	-0.016235
	(-0.008014)	(-0.012279)
Senate Majorty, Rep=1	-0.005369	0.014335
	(-0.006574)	(-0.010221)
Percent labor force unemployed, 14+ through 46, 16+ after	0.001527	-0.004052
	(-0.001283)	(0.002119)*
Delta GDP year-on-year, Percent	0.105478	0.221663
	(0.043753)**	(0.063230)***
Percent Population 65+	0.000409	0.000161
	(0.000193)**	(-0.000349)
Constant	-0.179115	-0.189111
	(0.035768)***	(0.054443)***
R-Squared	0.56762	0.9682
F-Test that Error Correlation=0	0.0015	0.7626
Observations	118	118
Panels in Regression:	2	2
Standard errors in parentheses: *=90%; **=95%; ***=99%		

TABLE 16: SPENDING ADJUSTED BY TOTAL SOCIAL SPENDING, MEDIAN-DISTANCE, HECKMAN-SNYDER COORDINATES

HS, Adjusted Share of Social Spending/ Median Distance	Education, etc. Spending Adjusted by total HR Spending	Social Security Spending Adjusted by total HR Spending
Lagged Dependent Variable	0.571487	0.958564
	(0.034400)***	(0.029494)***
Distance of Rep's median from zero, HS	0.04289	-0.007923
	(0.018560)**	(-0.030136)
Distance of Dem's median from zero, HS	-0.066494	0.023208
	(0.022219)***	(-0.034983)
Stan. Dev. of Rep Ideal Points	0.051278	-0.015839
	(-0.034943)	(-0.058521)
Stan. Dev. of Dem Ideal Points	0.078034	0.014121
	(0.027986)***	(-0.048013)
House Majority, Rep=1	0.01314	-0.018453
	(0.007299)*	(-0.012109)
Senate Majorty, Rep=1	0.002587	0.013625
	(-0.006319)	(-0.010462)
Percent labor force unemployed, 14+ through 46, 16+ after	-0.000017	-0.004092
	(-0.001223)	(0.002109)*
Delta GDP year-on-year, Percent	0.104802	0.222061
	(0.040401)**	(0.062415)***
Percent Population 65+	0.00064	0.000014
	(0.000176)***	(-0.00034)
Constant	-0.144955	-0.197438
	(0.034759)***	(0.051804)***
R-Squared	0.5361	0.9698
F-Test that Error Correlation=0	0.0001	0.9696
Observations	118	118
Panels in Regression:	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

TABLE 17: SHARE/DISTANCE REGRESSIONS, NOMINATE SCORES:

	Total HR Share of Spending	Education Training Employment Share of Spending	Health Share of Spending	Social Security Share of Spending	Income Security Share of Spending
Lagged Dependent Variable	0.93201	0.743571	1.022971	1.022971	0.718969
	(0.034693)***	(0.062629)***	(0.019784)***	(0.019784)***	(0.056185)***
Distance between Medians, NOM	-0.052361	-0.017334	-0.002313	-0.002313	-0.007901
	(0.024799)**	(0.005168)***	(-0.001557)	(-0.001557)	(-0.009029)
House Majority, Rep=1	0.066325	0.003598	0.001834	0.001834	0.028716
	(0.017393)***	(-0.00373)	(0.001042)*	(0.001042)*	(0.005732)***
Senate Majorty, Rep=1	-0.020167	0.000261	-0.001504	-0.001504	-0.013622
	(-0.013182)	(-0.003195)	(0.000873)*	(0.000873)*	(0.004564)***
Percent labor force unemployed, 14+ through 46, 16+ after	0.006537	0.000083	-	-	0.007857
	(0.003230)**	(-0.000624)	-	-	(0.001234)***
Delta GDP year-on-year, Percent	-0.525656	0.009401	0.005434	0.005434	-0.109951
	(0.089747)***	(-0.017047)	(-0.004933)	(-0.004933)	(0.034221)***
Percent Population 65+	0.000484	0.000184	0.000037	0.000037	-
	(-0.000354)	(-0.000182)	(-0.000038)	(-0.000038)	-
Constant	0.556	0.004256	-0.004366	-0.004366	0.070032
	(0.099996)***	(-0.005724)	(0.002529)*	(0.002529)*	(0.030545)**
R-Squared	0.9402	0.5576	0.9876	0.9915	0.9241
F-Test that Error Correlation=0	0.1825	0.325	0.3361	0.9321	0.99999
Observations	118	118	118	118	118
Panels in Regression:	2	2	2	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

TABLE 18: SHARE/DISTANCE REGRESSIONS, HECKMAN-SNYDER COORDINATES:

	Total HR Share of Spending	Education Training Employment Share of Spending	Health Share of Spending	Social Security Share of Spending	Income Security Share of Spending
Lagged Dependent Variable	0.914886	0.688692	1.02231	0.978929	0.667619
	(0.037788)***	(0.061301)***	(0.020592)***	(0.013786)***	(0.054503)***
Distance between Medians, HS	-0.01079	-0.006668	-0.001401	0.001545	0.011124
	(-0.019829)	(0.003791)*	(-0.001175)	(-0.003843)	(0.006450)*
House Majority, Rep=1	0.058908	0.002538	0.00157	0.002361	0.025015
	(0.017488)***	(-0.003862)	(-0.001036)	(-0.00313)	(0.005476)***
Senate Majorty, Rep=1	-0.022655	0.000045	-0.001368	-0.002783	-0.015102
	(-0.013776)	(-0.00334)	(-0.000895)	(-0.0027)	(0.004405)***
Percent labor force unemployed, 14+ through 46, 16+ after	0.007614	0.000571	-	-	0.008119
	(0.003309)**	(-0.000628)	-	-	(0.001166)***
Delta GDP year-on-year, Percent	-0.51397	0.011215	0.005467	-0.005141	-0.119652
	(0.092449)***	(-0.017797)	(-0.004963)	(-0.018254)	(0.032949) ***
Percent Population 65+	0.000494	0.000168	0.000038	0.00002	-
	(-0.000369)	(-0.000202)	(-0.000039)	(-0.000087)	-
Constant	0.514908	-0.006972	-0.005077	0.009753	0.074446
	(0.100577)***	(-0.005504)	(0.002487)**	(-0.016948)	(0.030616)**
R-Squared	0.9378	0.5485	0.9871	0.9912	0.9256
F-Test that Error Correlation=0	0.1916	0.4183	0.3335	0.8233	0.9999
Observations	118	118	118	118	118
Panels in Regression:	2	2	2	2	2

Standard errors in parentheses: *=90%; **=95%; ***=99%

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