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THE NATURE, DETERMINANTS, AND CONSEQUENCES OF
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A DISSERTATION APPROVED
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**Abstract**

The allocation of public resources is a core responsibility of the United States federal government, and geographically targeted spending is both practically important and central to major theories of the US Congress. This dissertation advances the study of congressional distributive politics by incorporating a diverse array of methodological approaches to clarify the nature, determinants, and public opinion consequences of the geographic allocation of federal funds by the US Congress. The first two chapters use observational data on federal spending to clarify the nature of distributive policy change and show how legislative procedure shapes policy outcomes. The third and fourth chapters leverage survey experiments to shed light on the role of distributive politics in congressional elections. Together, the four chapters approach congressional distributive politics from different perspectives to generate insights on policy-making, representation, and electoral strategy.

# Contents

# *Introduction*

The allocation of public resources is a core responsibility of the United States federal government. Federal spending decisions carry important consequences for citizens, local and state governments, members of Congress, bureaucratic agencies, and presidents. Research on the geographic distribution of public resources traces back to Laswell's pioneering work, *Politics: Who Gets What, When, How* (Lasswell 1936). Multiple streams of research have developed to further explore this topic, as studies in the fields of legislative studies, public policy, economics, and political behavior aim to shed light on various causes and effects of federal spending decisions. However, these streams of research rarely overlap, and there is still much to learn about distributive politics. This dissertation advances the study of congressional distributive politics by incorporating a diverse array of methodological approaches to clarify the nature, determinants, and public opinion consequences of the geographic allocation of federal funds by the US Congress.

Foundational theories of Congress place geographically targeted spending at the heart of incumbent reelection strategy and institutional organization. Bringing home federal spending projects offers legislators credit claiming opportunities, displays influence in Washington, and dissuades potential challengers from running in upcoming elections (Mayhew 1974; Ferejohn 1974; Cain, Ferejohn and Fiorina 1987). The motivation for securing distributive benefits is universal among legislators, because securing spending projects assists all legislators in their quest for reelection. As a result of this universal demand, distributive benefits provide a tool for legislative coalition leaders to "grease the wheels" of Congress and build support for general interest legislation (Evans 2004; Lee 2003; Adler 2002). Additionally, foundational formal models of congressional lawmaking posit that uncertainty over the composition of future winning coalitions should result in oversized legislative coalitions and the universal distribution of federal funds (Shepsle and Weingast 1981, 1987). In sum, influential theories of Congress conceptualize distributive politics as a key nonpartisan element of congressional lawmaking. Legislators use federal spending projects in their district to build a "personal vote" among thankful constituents, coalition leaders use such projects to build support for legislation across party lines, and no legislator is systematically shut

out of the distributive politics process.

Recent research complicates our understanding of congressional distributive politics by casting doubt on the universal nature of legislator motivation and resource distribution. Multiple studies suggest that the electoral benefits to be gleaned from distributive benefits are conditional on partisanship. Democrats stand to benefit from securing traditional spending projects in their districts, whereas Republicans receive substantially less electoral capital from spending projects (Bickers and Stein 2000; Lazarus and Reilly 2010; Lazarus, Glas and Barbieri 2012; Crespin and Finocchiaro 2013). Further, partisan polarization enhances the party-based conditionality of distributive benefits' electoral value. Distributive benefits are interpreted as a government spending issue during times of high polarization, and opinions on the issue fall along partisan lines (Sidman 2019). Recent research also calls into question the universalistic nature of public resource distribution. Partisanship plays a role in the allocation of federal funds; majority party members of Congress consistently receive more funding in their districts than minority party members (Lazarus 2009; Clemens, Crespin and Finocchiaro 2015; Balla, Lawrence, Maltzman and Sigelman 2002; Albouy 2013; Curry and Donnelly 2021). Additionally, presidential administrations tip the congressional distributive politics scales by sending extra benefits to allies in Congress (Hudak 2014; Berry, Burden and Howell 2010; Kriner and Reeves 2015; Dynes and Huber 2015; Christenson, Kriner and Reeves 2017). Thus, the geographic distribution of federal resources appears, at least in recent years, less universal and nonpartisan than early theories of distributive politics predicted.

The geographically targeted distribution of federal funds is both practically important and central to major theories of the US Congress, but there remains much we do not know about congressional distributive politics. This dissertation advances the field of research in several ways. The first two chapters use observational data on federal spending to clarify the nature of distributive policy change and show how legislative procedure shapes policy outcomes. The third and fourth chapters leverage survey experiments to shed light on the role of distributive politics in congressional elections.

While many extant studies examine the factors that increase legislators' access to distributive

benefits, we know very little about the underlying nature of policy change regarding the geographic distribution of federal funds. Chapter 1 sheds light on this topic by applying the punctuated equilibrium theory (PET) of policy change to congressional distributive politics. I use a stochastic process methodological approach to analyze federal spending in US congressional districts, thereby clarifying the nature of geographic spending change over time. I find that, rather than incremental change, congressional districts tend to experience either no change or major change in levels of federal spending. Further, I identify representative-level factors, such as committee membership, partisanship, and majority party status, that influence patterns of district spending change.

Chapter 2 investigates the connection between legislative procedure and inequality in federal transportation spending. In 2010, the US Congress placed a moratorium on earmarks – congressionally mandated spending projects. But did the earmark moratorium actually rid public policy of earmarks and particularistic spending? I use earmark data and 2010-2020 state-level highway funding metrics to examine the relationship between previously expired transportation earmarks and federal highway funding during the earmark moratorium. I show that old earmarks were institutionalized into the federal highway funding formula during the earmark moratorium. Further, I find that highly earmarked states became even more advantaged during the moratorium. In the case of federal highway funding, the victors of the earmark moratorium were previous pork winners, not porkbusters aiming to root out particularistic spending.

How do incumbents in the US Congress turn federally funded district projects into electoral gains? Previous research suggests members of Congress use credit claiming for distributive benefits to cultivate an impression of influence, portraying themselves as uniquely capable of securing projects for their constituents. In Chapter 3, I develop a targeted theory of congressional distributive politics and argue that public support is granted to legislators for securing the *right* distributive benefits rather than securing the *most* distributive benefits. This chapter uses two survey experiments to achieve two goals. First, I measure constituent demand for different types of federal spending and reveal a surprising degree of partisan convergence on spending priorities. Second, I find robust evidence that legislators' ability to meet constituent demand shapes the effectiveness of

their credit claims. This chapter advances the literature on congressional representation and offers a more complete account of the politics around congressional spending.

Finally, Chapter 4 explores the role of legislator gender in congressional credit claiming. Multiple studies find that congresswomen tend to outperform congressmen in securing district spending (Anzia and Berry 2011; Lazarus and Steigerwalt 2018). However, for legislators to turn distributive benefits into higher approval and electoral rewards, constituents must recognize that public spending has taken place in their community and attribute credit to the correct public official. I ask whether congresswomen face a gender barrier in claiming credit for federal spending projects, and I explore this question through a survey experiment. I find no evidence that legislator gender influences the public's reaction to congressional credit claims, suggesting congresswomen can effectively transform district funding advantages into electoral returns.

The power of the purse represents a core responsibility of the US Congress and a strategic opportunity for its reelection-focused members. Through a combination of observational and experimental analysis, this dissertation sheds light on the geographic allocation of federal funds by the US Congress. The four chapters approach congressional distributive politics from different perspectives to generate insights on policymaking, representation, and electoral strategy.

# *Chapter 1: The Nature of Congressional Spending*

# *A Stochastic Process Approach to Distributive Politics*

**Abstract**

This article aims to bridge the legislative studies literature with Punctuated Equilibrium Theory (PET) by employing a distributional methodology to study patterns of geographic spending change. The geographic location of federal spending is a common topic of interest in legislative studies, but previous research does not clarify the nature of geographic spending change. Using a theoretical approach and methodological tools established in PET, I analyze a dataset of federal spending in US congressional districts from 1983 to 2010. I find that geographic spending change is characterized by a leptokurtic distribution and follows punctuated equilibrium patterns. Further, I identify institutional factors and representative level factors that influence patterns of geographic spending change. I find that Republican control of the US House increases punctuated equilibrium patterns of geographic spending change. At the representative level, Appropriations Committee membership increases district spending stability, while majority party membership and Republican affiliation interact to influence district spending stability.

# Introduction

Foundational research in legislative studies establishes that Members of Congress (MCs) use federal spending projects in their home districts to help ensure reelection (Mayhew 1974; Fenno 1978; Cain, Ferejohn and Fiorina 1987). A large literature, referred to in legislative studies as "distributive politics," has built on this foundation by investigating how MCs and other political actors influence the geographic allocation of public resources. The literature on distributive politics as it relates to the US Congress focuses on the geographic distribution of federal funds, and the dominant questions in this line of research center on legislators' varying ability to secure federal spending projects in their congressional districts. Levels of federal spending in specific geographic locations is the most common dependent variable in such research, but no studies in congressional distributive politics clarify the underlying distribution of geographic spending change.

Policy process research offers a distributional methodology for characterizing the nature of policy change over time, and this approach is commonly used to study public budgeting policy. Such research uses budgetary data organized by policy function, rather than geographic location, and employs distributional methods – starting with histograms of budgetary change distributions and measures of kurtosis – to clarify the nature of policy change. The approach's foundational finding is that essentially all budgets are defined by long periods of stasis and rapid periods of change (Jones, Sulkin and Larsen 2003; Breunig and Koski 2006; Jones, Baumgartner, Breunig, Wlezien, Soroka, Foucault, François, Green-Pedersen, Koski, John et al. 2009). These findings, combined with theoretical reasoning for why policy change is characterized by leptokurtic distributions, have redefined the field's understanding of budgetary policy and provide a foundation for the broader Punctuated Equilibrium Theory (PET) of policy change (Jones, Baumgartner and True 1998; Jones and Baumgartner 2005; Baumgartner and Jones 2015).

Congressional distributive politics research has revealed a lot about the factors that impact legislators' access to distributive benefits, but we know very little about the underlying nature of policy change regarding the geographic distribution of federal funds. Additionally, PET has clarified the nature of budgetary policy change, but it has yet to make full use of insights gleaned

from legislative studies. This article addresses the gulf between congressional distributive politics research and PET research on budgets by exploring three research questions.

First, is geographic spending change characterized by a leptokurtic distribution, or does geographic spending change follow a more normal distribution? Due to the punctuated equilibrium nature of budgetary policy and the additional institutional friction introduced by MCs' parochial interests, I theorize that geographic spending change is leptokurtic. Using a dataset of federal spending in US congressional districts from 1983 to 2010, I find that geographic spending is indeed characterized by a leptokurtic distribution. Though unsurprising, this result extends the foundational PET finding to another type of policy change and carries meaningful implications for congressional distributive politics research.

Second, do institutional factors help explain the stability and volatility of geographic spending change? Scholarship on the US Congress offers insights on institutional factors that plausibly effect patterns of geographic spending change. I theorize that geographic spending change is subject to more punctuated patterns of policy change during times of high polarization and Republican control of the House. I find preliminary support for these hypotheses – both conditions lead to substantially higher l-kurtosis values. These findings add to the growing list of known organization factors that influence patterns of policy change.

Third, is geographic spending change simply downstream of policy change, or do geographic spending concerns play a meaningful role in the policymaking process? Identifying the distributional pattern of geographic spending change is useful for establishing a better understanding of congressional distributive politics. However, for geographic spending change to meaningfully contribute to the PET literature, it cannot simply be a downstream effect of budgetary policy change. I argue that factors at the congressional representative level – Appropriations Committee membership, majority party membership, and party affiliation – plausibly impact geographic spending change patterns. If these representative-level factors predictably influence spending change, it follows that geographic spending concerns play a meaningful role in the budgetary policymaking process. Using a method designed by Epp and Baumgartner (2017) to create an index of budgetary

change as the operational definition of policy instability, I find that Appropriations membership increases district spending stability, while majority party membership and Republican representation interact to influence district spending stability. Republican representation predicts a substantial increase in district spending instability, but only when Republicans are the minority party. These findings support the argument that geographic spending concerns play a meaningful role in the budgetary policymaking process.

In sum, this article taps into the theory and distributional methods established in PET research to investigate the geographic distribution of federal funds to US congressional districts. The findings presented below advance the literature on both congressional distributive politics and PET by describing the distributional nature of geographic spending change and revealing institution-level and representative-level influences on patterns of policy change.

## Congressional Distributive Politics Background

Geographically targeted spending projects play a major role in influential theories of the US Congress. MCs place a high value on distributive benefits, because securing federal spending projects provides opportunities for MCs to claim credit for tangible improvements in their communities and displays influence in Congress (Mayhew 1974; Ferejohn 1974; Cain, Ferejohn and Fiorina 1987). A large literature on congressional distributive politics has built on this foundation by investigating how MCs secure district spending. In addition to the inherent importance of understanding where federal money flows, such research offers insights on congressional representation styles (Fenno 1978; Lazarus and Reilly 2010; Grimmer 2013), institutional power structures (Shepsle and Weingast 1987; Balla et al. 2002; Christenson, Kriner and Reeves 2017), and legislative coalition building (Lee 2000; Evans 2004; Rosenstiel 2023).

The literature on distributive politics as it relates to the US Congress focuses on the geographic distribution of federal funds, and the dominant question in this line of research is on legislators' varying ability to secure distributive benefits for their home districts: how do certain legislator

characteristics influence levels of district spending? Studies have focused on majority party status (Balla et al. 2002; Albouy 2013; Curry and Donnelly 2021), committee membership (Lee 2003; Evans 2004; Clemens, Crespin and Finocchiaro 2015), presidential copartisanship (Hudak 2014; Berry, Burden and Howell 2010; Christenson, Kriner and Reeves 2017), electoral vulnerability (Lazarus 2009; Grimmer 2013), seniority (Lazarus and Steigerwalt 2009), ideological proximity to bureaucratic agency leaders (Bertelli and Grose 2009; Napolio 2021), and legislative bargaining (Lee 2000; Rosenstiel 2023) as determinants of geographically targeted spending outcomes. In sum, this stream of research clarifies MCs' active role in the geographic allocation of federal funds.

Congressional distributive politics research centers on individuals MCs and explores how the characteristics, motivations, and positioning of individual representatives explain where federal money flows. However, existing research does not clarify the underlying nature of policy change concerning geographic spending.

# Punctuated Equilibrium Theory Background

PET offers both a theoretical foundation and distributional methodology for understanding the nature of policy change over time. Incrementalism, a previously dominant policy process model, predicted incremental adjustments and stability. Policy change, however, is often episodic and disjointed. Dissatisfied with incrementalism's inability to predict the reality of policy change, early PET research set out to describe and explain a policy process characterized by both periods of gridlock and periods of dramatic policy change (Jones, True and Baumgartner 1997; Jones, Baumgartner and True 1998; True 2000). The theory is grounded in the concepts of bounded rationality, information processing, and institutional friction. Human decision-makers have limited information processing capacity, and institutional factors – such as operating procedures, rules, cultural norms, and organizational barriers – restrict institutions' ability to quickly adapt policy to changing circumstances (Jones and Baumgartner 2005, 2012; Baumgartner and Jones 2015). As such, policy goes unchanged for long periods of time as issues remain undetected or ignored, and the

policy change that finally occurs after a tipping point is reached is often immense.

Methodologically, PET research employs distributional methods – starting with histograms of policy change distributions along with measures of kurtosis – to clarify the nature of policy change. While PET has expanded far beyond budgeting policy, a foundational finding in the PET literature is that essentially all public budgets are defined by long periods of stasis and rapid periods of change (Jones et al., 2009). The punctuated equilibrium pattern of budgets has been observed at the local level (Jordan 2003; Robinson 2004; Flink 2017), state level (Breunig and Koski 2006, 2020), and federal level (Jones, Sulkin and Larsen 2003) of US government, as well as budgets in other nations (Breunig, Koski and Mortensen 2010). PET has succeeded in generating critical insights on the nature of budgetary policy change, but the concept of budgetary policy used in this stream of research does not align with that of the congressional distributive politics research cited above. Whereas the congressional literature is concerned with where federal money is going, PET research on budgetary policy is concerned with what policy function federal money is funding. In short, rather than spending in geographic locations, PET research focuses on policy functions.

Recent extensions of PET have explored a range of determinants of policy change patterns. For instance, different policy domains experience different change distributions (Breunig and Busemeyer 2012), and multiple studies clarify the aspects of policy domains that increase the likelihood of punctuated policy change (Ryu 2009, 2011; Breunig, Koski and Mortensen 2010). Additionally, the search for organizational characteristics that influence the probability of budgetary punctuations has yielded a number of recent insights. Characteristics of decision-making organizations that impact budgetary change patterns include centralization (Robinson, Caver, Meier and O'Toole Jr 2007), personnel instability (Flink 2017), bureaucratic discretion (Park and Sapotichne 2020), electoral system (Breunig and Busemeyer 2012), executive power (Breunig and Koski 2020), information flows (Fagan 2023), and recency of previous punctuations (Robinson, Flink and King 2014; Flink and Robinson 2020). Finally, in a useful return to original PET theory, Epp and Baumgartner (2017) find that complexity and institutional capacity meaningfully influence budgetary change distributions. Policy domain complexity increases instability while higher institutional capacity

increases stability.

# Theoretical Framework

Tapping into both the legislative studies and PET literatures, I make three theoretical claims on geographic spending change. First, due to the punctuated equilibrium nature of budgetary policy, as well as the friction introduced by MCs' parochial interests, I expect the distribution of geographic spending changes to be leptokurtic. A remarkably robust finding in the PET literature is that frequency distributions of public budget changes are leptokurtic – characterized by slender peaks and fat tails. Jones et al. (2009) refer to this repeated finding as the "general empirical law of public budgets." Because geographic spending outcomes are downstream of public budgets, frequency distributions of geographic spending changes are plausibly characterized by a similar distribution.

I argue that the electoral motivations behind congressional distributive politics contribute to institutional friction, thereby enhancing the punctuated equilibrium patterns of geographic spending policy. Because MCs are motivated to protect federal spending initiatives and programs in their home districts, they are likely to vigorously fight against efforts to move or end such programs. For instance, hundreds of MCs sent letters to the Federal Aviation Administration in attempts to protect air traffic control towers in their districts from being shut down (Mills, Kalaf-Hughes and MacDonald 2016). Because the desire to protect district spending programs stems, at least in part, from electoral motivations rather than effective public policy motivations, MCs' particularism is a form of institutional friction that can lead to error accumulation and eventual policy punctuations. In other words, MCs' electoral interests function as glue in the distributive policymaking process, exacerbating the stick-slip dynamics behind punctuated equilibrium patterns of policy change. The enhanced friction and error accumulation result in longer periods of stasis, leading to more intense punctuations when policy change eventually takes place. Therefore, I predict that geographic spending change follows a leptokurtic distribution.

**Hypothesis 1:** *Annual percentage changes in federal spending in congressional districts follows a leptokurtic distribution.*

Second, I argue that institutional factors predictably influence the patterns of geographic spending change. Partisan polarization is one such factor. While active debate remains around the causes and consequences of partisan polarization in Congress, a general consensus acknowledges that polarization has increased since the mid-1900s (McCarty, Poole and Rosenthal 2016; Theriault 2008; Jacobson 2017). One result of an intense rivalry for partisan control of Congress is that whichever party is in the minority has little incentive to work with the majority party (Lee 2016). As such, congressional gridlock and dysfunction has increased with polarization (Mann and Ornstein 2006; Binder 2015). The "Republican Revolution" of 1994 – when Republicans gained control of the House – is a common marker of the rise of polarization. I use this date as an era defining change and argue that the distribution of geographic spending change from 1983 to 1994 is less punctuated than the distribution of geographic spending change from 1994 to 2010.

**Hypothesis 2:** *The distribution of district spending changes from 1983 to 1994 will have a lower l-kurtosis value than the distribution of district spending changes from 1995 to 2010.*

As partisan polarization has increased, distributive politics has been at the forefront of the partisan battleground. Distributive benefits are considered a partisan issue in times of high polarization (Sidman 2019), as Republicans paint congressionally targeted spending projects as a symbol of congressional profligacy (Frisch and Kelly 2015). Distributive politics, therefore, has become a flashpoint issue when Republicans control the House of Representatives. Debt ceiling showdowns and the moratorium on congressional earmarks serve as examples of this dynamic. Policy change plausibly becomes less stable when partisan warfare engulfs distributive politics. As such, I hypothesize that Republican control of the House of Representatives is associated with more punctuated patterns of geographic spending change.

**Hypothesis 3:** *The distribution of district spending changes when Democrats control the House will have a lower l-kurtosis value than the distribution of district spending changes when Republicans control the House.*

Third, I argue that individual-level characteristics of MCs influence district spending change patterns. This theory is grounded in the concept of attention allocation. Attention is a key component of information processing, and distributive politics takes time and attention on the part of

MCs (Guenther and Searle 2019). Attention allows for smoother information processing, leading to more incremental policy updates and less punctuated patterns of policy change. Therefore, I expect that districts represented by legislators who are plugged-in and attentive to distributive politics will experience less punctuated patterns of spending change than districts represented by legislators who are less attentive to distributive politics.

MC attention to district spending likely stabilizes patterns of district spending change, and there is reason to believe certain MCs give more attention to district spending than others. For instance, MCs on the Appropriations Committee are more likely to pay attention to district spending than other MCs. Whether Appropriations membership increases legislators' influence over spending outcomes is contested in the literature on congressional distributive politics (Lazarus 2009; Berry and Fowler 2016; Hammond and Rosenstiel 2020). Regardless of influence, committee membership indicates increased attention to district spending. Lawmakers interested in district spending likely self-select onto the Appropriations Committee, and the experience of committee membership likely focuses legislators' attention on distributive politics. Because Appropriations members are more attentive to distributive politics, they are likely more attuned to spending needs in their congressional districts. Therefore, I expect Appropriations membership results in lower district spending instability.

**Hypothesis 4:** *Appropriations membership reduces district spending instability.*

Additionally, Republican MCs have less to electorally gain from distributive politics than Democratic MCs (Bickers and Stein 2000; Lazarus and Reilly 2010; Crespin and Finocchiaro 2013; Sidman 2019). Legislators are continuously focused on the prospect of reelection (Mayhew 1974), meaning they are highly attentive to issues that they believe will impact their performance in future elections. Democratic legislators are more likely to consider district spending as a reelection strategy, leading to increased attentiveness to such issues. Conversely, Republicans in Congress are less incentivized to focus their limited time and attention on distributive politics. As a result of this discrepancy in attention, I expect that districts represented by Democratic MCs will experience more stable district spending patterns than districts represented by Republican MCs.

**Hypothesis 5:** *Republican representation increases district spending instability.*

Finally, legislator access to policymaking power likely influences patterns of district spending change. A robust finding in the distributive politics literature is that being in the majority party offers greater access to distributive benefits (Lazarus and Steigerwalt 2009; Clemens, Crespin and Finocchiaro 2015; Albouy 2013; Curry and Donnelly 2021). The majority party does not shut minority party members off from federal spending, but they distribute the majority of new distributive benefits amongst themselves (Balla et al. 2002). Because the majority party maintains power over spending outcomes, minority party districts are less likely to experience substantial increases in spending levels. Any dramatic funding increases are likely to occur in majority party districts. Therefore, I argue that representation by a MC in the majority party leads to additional district spending instability, as majority party MCs' increased access to federal funding allows for the possibility of positive punctuations.

**Hypothesis 6:** *Majority party status increases district spending instability.*

# Data and Methods

Empirically testing the first hypothesis requires descriptive analysis on the nature of annual budget changes from a geographic perspective. The data I use for this research comes from the Federal Assistance Award Data System (FAADS), which records federal transfers to domestic beneficiaries and tracks federal funding into individual congressional districts over time. Bickers and Stein (1996) originally collected and organized spending data starting in FY 1984, and the FAADS dataset has since been extended to FY2010 (Alexander, Berry and Howell 2016; Berry, Burden and Howell 2010; Berry and Fowler 2016). The result is a continuous record of federal spending in congressional districts that covers more than twenty-five years. The comprehensive nature of FAADS data – it includes all federal agencies – and coverage over a twenty-five year period are ideal for examining the distribution of geographic spending changes in US congressional districts.

After adjusting for inflation, I examine the distribution of total annual geographic budget changes in congressional districts. This requires calculating annual change scores for each congressional district. I follow the standard protocol in developing changes scores (Jones, Sulkin and Larsen 2003; Breunig and Koski 2006), taking district spending in a given year, minus district spending in the previous year, divided by district spending in the previous year. Congressional redistricting changes the map of congressional districts, meaning some change scores capture district changes rather than within-district spending changes. Therefore, I only record change scores for districts that cover the same geographic area in both year T and year T-1. I pool this data across years, yielding 9,774 observations. I test whether this data support Hypothesis 1 by generating a histogram of the distribution and calculating an l-kurtosis value.

Next, I explore whether district spending change distributions exhibit differences according to institutional factors – polarization era and party control of the House. To test Hypothesis 2, I separate the data by polarization era (1883-1994 and 1995-2010) and calculate kurtosis values for each of the change score distributions. Similarly, to test Hypothesis 3, I separate the data by which party controlled the House and calculate l-kurtosis values for each of the change score distributions. Because spending usually occurs the year after policy decisions are made, the spending data is lagged by one year. For instance, observations for 1994 in the dataset reflect spending outcomes in 1995.[1]

Finally, I use Epp and Baumgartner's (2017) method for creating an index of budgetary changes to test the hypotheses on representative characteristics. Creating the index involves ranking the change scores for each district-year combination by percentile, re-scaling the percentile rank to a -50 to 50 range (-50 being the most negative change and 50 being the most positive), and squaring this scale. This operational definition of budgetary change has a few benefits. It focuses on the broader concept of instability rather than identifying particular thresholds for different types of change. Additionally, it follows the recommendation of (Breunig and Jones 2011) to make use of

[1]This is a useful but imperfect treatment of timing. Hammond and Rosenstiel (2020) note that around three-quarters of appropriations outlay in the first year. The timing issue generates noise in the data for both this analysis and the representative-level analysis, leading to a more conservative test of hypotheses.

the full range of budgetary change. Finally, the squaring operation underscores the importance of punctuations (Epp and Baumgartner 2017).

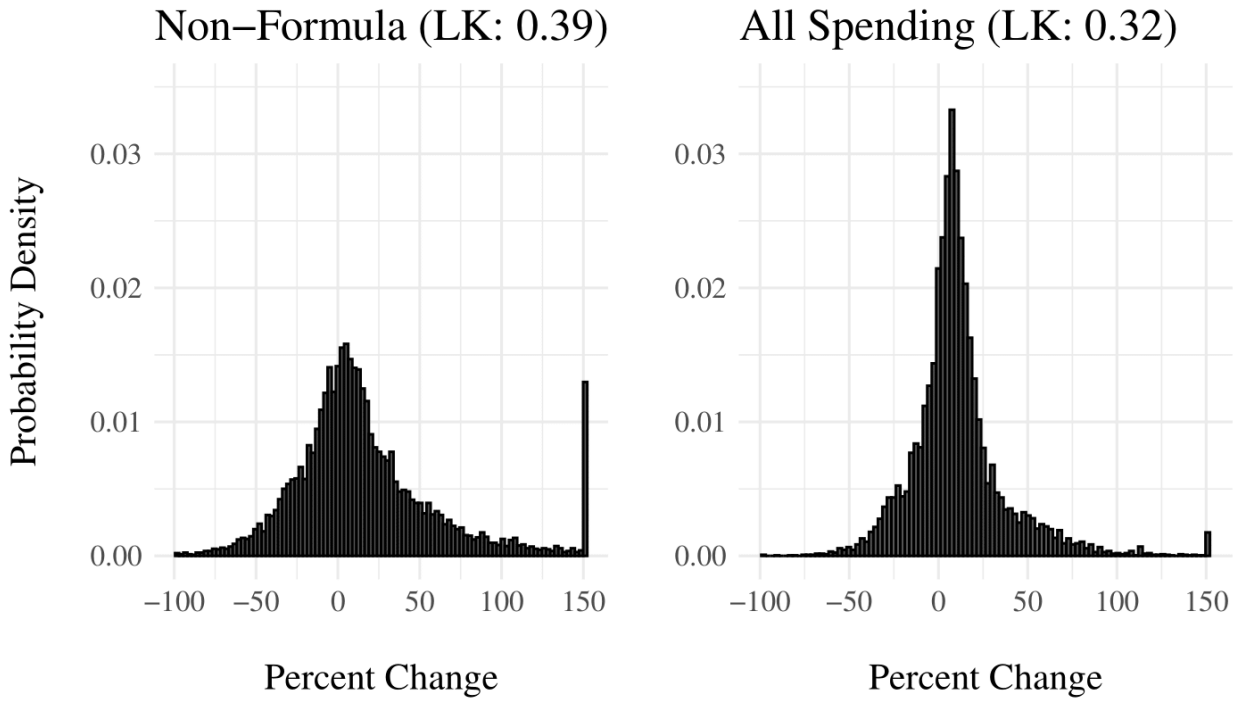
Using the index of district spending change as an operational definition for spending instability, I use OLS regression to test the representative-level hypotheses. Appropriations membership, party affiliation, and majority party status are the three independent variables of interest. I include controls in the model for variables that have been shown in the distributive politics literature to influence district spending levels: tenure in Congress, district competitiveness, and presidential co-partisanship. To account for the same timing issue discussed above, I match district spending in year T with MC characteristics in year T-1. Individual MCs have more influence over non-formula than formula spending, so I primarily focus on results for non-formula spending. However, I also construct and analyze the district spending change index for all spending. Finally, because I argue above that partisan control of the house plays a meaningful role in distributive politics, I include models with an interaction term for majority status and MC party affiliation.

# Results

I test whether the district spending data support Hypothesis 1 by generating a histogram of the distribution of annual district spending change scores and calculating an l-kurtosis value. Figure 1 displays this distribution for non-formula district spending and total district spending. As expected, the histogram visualizations indicate leptokurtic distributions, characterized by slender peaks and fat tails as compared to a normal distribution. Both the non-formula and total district spending change distributions are leptokurtic. Notably, positive punctuations are substantially more likely than negative punctuations, particularly in the non-formula change distribution. This makes sense, as MCs are more likely to secure rather than lose a large amount of district spending. Additionally, the l-kurtosis value of 0.39 for non-formula spending and 0.32 for total spending are substantially larger than that of the normal distribution (0.123). Therefore, the data support Hypothesis 1 – the distribution of annual percentage changes in federal spending in congressional districts is charac-

terized by a leptokurtic distribution.

**Figure 1: Pooled Distribution of Percent Change Scores**




Next, I examine whether patterns of geographic spending change are associated with institutional factors. I use l-kurtosis as a measure of punctuated equilibrium patterns, which is common in the PET literature (Breunig and Koski 2006; Chen and Flink 2022). Table 1 addresses the question of polarization by comparing an era of relatively low polarization (1983-1994) to an era of relatively high polarization (1995-2010). Results generally support expectations. The era of low polarization exhibits lower l-kurtosis in district spending change than the era of high polarization. The bottom two rows of Table 1 separate the l-kurtosis analysis by formula and non-formula district spending, and the finding is robust across this distinction. The bivariate nature of this analysis limits the strength of empirical claims that can be made – the two eras plausibly differ in ways besides polarization – but I report preliminary support for the theory that congressional polarization leads to more punctuated patterns of geographic spending change.

Table 2 includes similar analysis to Table 1, but it compares l-kurtosis values of district spending change distributions based on which party controls the House of Representatives. Because the

**Table 1: L-Kurtosis of District Spending Changes by Era**

| Type of spending | 1983-1994 | 1995-2010 |
|---|---|---|
| All Spending | 0.24 | 0.37 |
| Non-formula | 0.34 | 0.42 |
| Formula | 0.26 | 0.42 |

previous analysis shows that era matters, and Democrats controlled the House in all years in the dataset prior to 1995, I include l-kurtosis values for Democratic control in all years (first column) as well as Democratic control in the years before 1995 (second column) and from 1995 forward (third column). Two things stand out in Table 3. First, the analysis generally supports Hypothesis 3 – regardless of spending type, the l-kurtosis values when Democrats control the House are lower than the l-kurtosis values when Republicans control the House. Second, the l-kurtosis values for the distributions of spending change under Democratic control are effectively the same in the two polarization eras. Therefore, the era difference found in Table 1 appears to stem from the fact that Republicans controlled the House in the latter era and not the earlier era. In other words, partisan control of the House appears to matter more than polarization era in determining patterns of district spending change.

**Table 2 Here: L-Kurtosis of District Spending Changes by Party Control of the House**

| Type of spending | D control All years | D control 1983-1994 | D control 1995-2010 | R control |
|---|---|---|---|---|
| All Spending | 0.24 | 0.24 | 0.22 | 0.39 |
| Non-formula | 0.32 | 0.34 | 0.31 | 0.49 |
| Formula | 0.27 | 0.26 | 0.27 | 0.43 |

Finally, results from the OLS analysis of individual member characteristics' impact on the district spending instability index are reported in Table 3. Columns 1 and 2 display the results for the non-formula district spending change index, which is more susceptible to individual MC efforts, while columns 3 and 4 display results for the district spending change index of all district spending. Hypothesis 5 is moderately supported by the analysis, as Appropriations membership appears to marginally reduce district spending instability. This is particularly true for non-formula district spending, which is where Appropriations membership is expected to matter most. Appropriations

**Table 3: District Spending Instability Regression Results**

| | Non-formula spending | | All spending | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Appropriations Member | -67.93*** | -60.83*** | -37.64* | -24.75 |
| | (22.74) | (22.47) | (22.71) | (21.75) |
| Majority Party | -2.01 | 224.79*** | 29.81* | 449.24*** |
| | (16.41) | (21.81) | (16.39) | (21.11) |
| MC Party: R | 53.58*** | 316.62*** | -19.15 | 467.34*** |
| | (16.08) | (23.20) | (16.07) | (22.47) |
| President's Party | -27.36 | -28.38* | -6.81 | -8.76 |
| | (16.84) | (16.64) | (16.82) | (16.11) |
| Close District | 2.20 | -7.02 | 12.71 | -4.11 |
| | (31.37) | (30.99) | (31.34) | (30.02) |
| Tenure in Congress | 6.27*** | 5.54*** | 10.25*** | 8.92*** |
| | (1.98) | (1.96) | (1.98) | (1.90) |
| Majority * MC Party: R | | -477.95*** | | -884.00*** |
| | | (30.73) | | (29.75) |
| Constant | 813.36*** | 683.34*** | 792.06*** | 551.32*** |
| | (20.65) | (22.05) | (20.62) | (21.35) |
| Num.Obs. | 9757 | 9757 | 9770 | 9770 |
| RMSE | 759.35 | 750.10 | 758.74 | 726.59 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

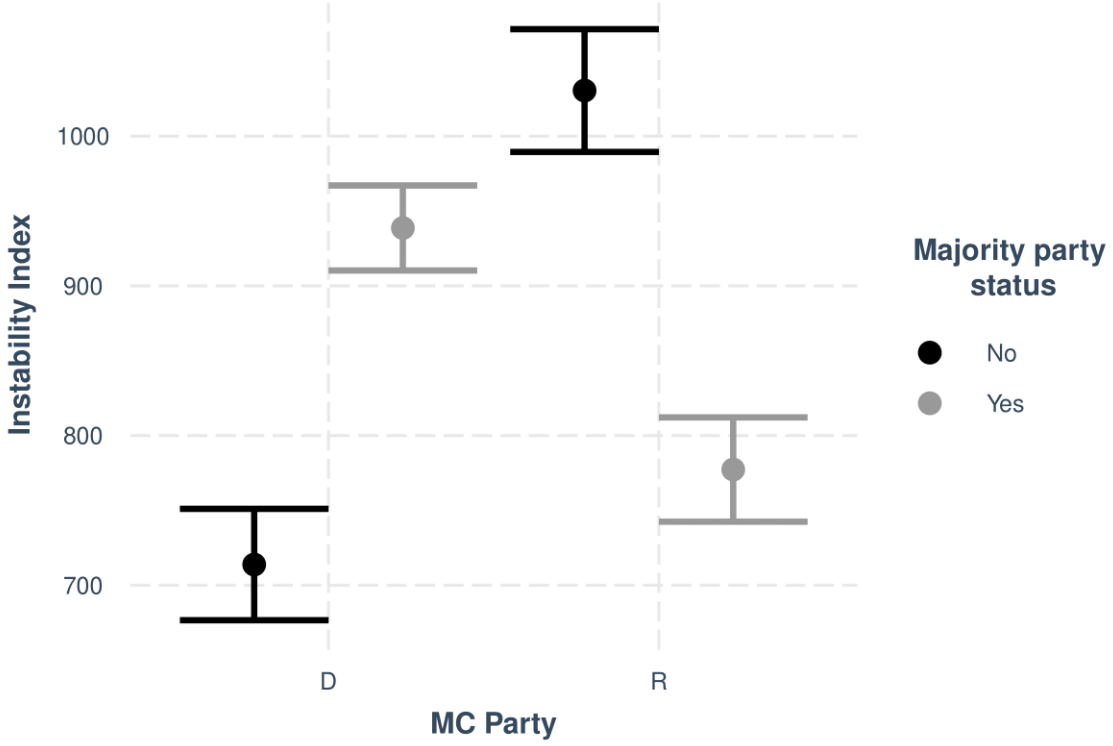
MCs pay more attention to district spending, leading to greater district spending stability. The effect is statistically significant and in the expected direction, though the magnitude of this effect is relatively small. The average effect of Appropriations membership on non-formula district spending instability is around 65 points on the instability scale. This effect equates to a 0.09 standard deviation decrease in instability.[2] Therefore, Appropriations membership only marginally reduces district spending instability.

On the results concerning MC majority status and MC party, columns 2 and 4 reveal a highly interactive relationship. Majority status has a statistically significant and substantially positive effect on district spending instability, but this is only the case for districts with a Democratic representative. The negative interaction term indicates that majority status does not increase instability for Republican-held districts and might even increase stability. Additionally, Republican representa-

[2]Standardized regression results are reported in the appendix.

tion leads to a statistically and substantively significant increase in district spending instability, as expected, but only when Republicans are the minority party. When Republicans are in the majority party, Republican representation leads to increased stability as compared to Democratic representation. Figure 2 displays predicted district spending instability based on model results in the second column of Table 3, thereby clarifying the interactive relationship.

**Figure 2: Predicting District Spending Instability**




An interesting paradox emerges from the combined analyses. Table 2 shows that the distribution of district spending changes when Republicans control the House has a higher l-kurtosis value than when Democrats control the House, but Figure 2 shows that individual congressional districts experience less spending instability when Republicans control the House. A possible theory to connect these findings is that Republican control of the House increases the likelihood of geographic spending gridlock, resulting in a continuation of the status quo and many very low district spending instability scores. However, on rare occasions when a Republican controlled House does depart from the status quo, district spending changes are immense. As a result, districts ex-

perience less instability, on average, when Republicans control the House, but the distribution of district spending changes when Republicans control the House has a higher l-kurtosis value than the distribution of district spending changes when Democrats control the House. Figure A1 in the appendix, a graphical display of the distributions featured in Table 2, offers some support for this theory. The distribution of change scores when Republicans control the House has a taller and narrower peak over zero (no change), while the distribution of change scores when Democrats control the House has larger shoulders.

# Conclusion

This study is designed to bridge the research on congressional distributive politics with PET, resulting in a deeper understanding of the geographic distribution of federal funds in the US. The findings presented above clarify the nature of geographic spending change. Identifying the determinants of legislator access to federal funds is a worthwhile line of research, but the underlying nature of geographic spending change is an important and understudied topic. This article shows that geographic spending change is characterized by the same punctuated equilibrium patterns that PET scholars have repeatedly identified as a fundamental aspect of public budgets, thereby informing our understanding of the dynamics of congressional distributive politics.

Additionally, I extend the foundational punctuated equilibrium finding to a new type of policy change: geographic spending. The initial finding in this extension – that geographic spending change follows punctuated equilibrium patterns – is unlikely to surprise PET researchers. If all public budgets follow punctuated equilibrium patterns, it follows that the geographic spending outcomes of such budgets also follow punctuated equilibrium patterns. I argue that this study theoretically advances PET in its analysis of individual MC characteristics. If geographic spending outcomes were merely downstream of budgetary change, then representative-level factors should have no effect on geographic spending change distributions. I show that representational factors – Appropriations membership, party affiliation, and majority party status – impact patterns of dis-

trict spending change. Therefore, I argue that geographical spending concerns should factor into our understanding of budgetary policy.

This research is a first attempt at considering geographic spending change from a PET perspective, and there are multiple opportunities to extend this line of inquiry. Different geographical entities and types of representation, such as states and Senators, offer ground for further testing of the hypotheses presented above. An exploration of policy where spending location decisions are more directly made, such as specific grant programs and congressional earmarks, provides another extension. Finally, a series of complex interactions occurs between bureaucratic decision-makers and members of Congress on decisions to spend federal money in specific locations (Arnold 1980; Mills, Kalaf-Hughes and MacDonald 2016; Workman 2015), generating additional friction to spending policy. Further research on these interactions has the potential to further our understanding of the policymaking process.

# *Chapter 2: Institutional Earmarks*

# *The Earmark Moratorium and Federal Highway Spending*

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**Abstract**

In 2010, the US Congress placed a moratorium on earmarks – congressionally mandated spending projects. But did the earmark moratorium actually rid public policy of earmarks? I use earmark data and 2010-2020 state-level highway funding metrics to examine the relationship between previously expired transportation earmarks and federal highway funding during the earmark moratorium. Earmarks in the 2005 surface transportation law (SAFETEA-LU) continued to benefit certain states in 2020, even though the projects technically expired in 2009. This is because the funding "formulas" established by all post-2009 surface transportation laws were fully determined by the highway allocation percentage each state received in the preceding year, inclusive of earmarks. Further, I find the relationship between SAFETEA-LU earmarks and state funding disparities strengthened from 2010 to 2020, meaning the expired earmarks increased in policy significance during the moratorium. Highly earmarked states became even more advantaged after the earmarks were institutionalized into the highway funding formula.

# Introduction

Earmarks – congressionally mandated spending for specific projects – have long fascinated congressional scholars due to their traceability to individual members of Congress, their influence in passing legislation, and the signals they send about congressional power structures. But earmarks were presumed to no longer influence policy outcomes in 2020, because Congress placed a moratorium on inserting earmarks into legislation in 2011. Legislators continued to seek out particularistic spending in their district in the absence of earmarks, but they did so by communicating directly with bureaucratic leaders (Mills and Kalaf-Hughes 2015; Mills, Kalaf-Hughes and MacDonald 2016; Kalaf-Hughes and Mills 2016). In addition to seeking out particularized benefits in new ways, I argue that the earmark moratorium led legislators to preserve and extend previous earmarks that benefited their constituents.

The last time earmarks were written into federal surface transportation legislation was the 2005 law (SAFETEA-LU), which authorized highway program funding through 2009. Congress passed multiple surface transportation authorizations under the earmark moratorium, meaning the direct influence of earmarking over policy outcomes ended in 2009 with the expiration of SAFETEA-LU earmarks. However, the funding "formulas" established by all post-2009 surface transportation bills are based solely on the percentage of total highway allocation each state received in the preceding year. Effectively, the state allocation percentages set in FY 2009, inclusive of earmarks, were locked in place and continued to dictate highway funding through 2020. This article examines the resulting influence of SAFETEA-LU earmarks over highway funding from 2010 to 2020, as they were institutionalized into the frozen funding formula.

The frozen highway funding formula and continuous payout of old earmarks from 2010 to 2020 offers a useful window for exploring the institutionalization of previous earmarks during the moratorium. I examine whether the influence of institutional earmarks over policy outcomes grew, contracted, or stayed even during the decade after the earmarks officially expired. I argue that SAFETEA-LU earmarks are meaningfully associated with changes in malapportionment – the over-funding and under-funding of states – from 2010 to 2020. While the original earmarked

projects were based on local and state needs in 2005, the continued payout of earmark funds for previously completed projects likely exacerbated highway funding malapportionment. Rather than fading away under the earmark moratorium, I argue SAFETEA-LU earmarks increased in policy significance during the earmark moratorium.

I utilize earmark data, the SAFETEA-LU federal highway funding formula, and state-level highway statistics to clarify the relationship between institutional highway earmarks and 2010 to 2020 highway funding malapportionment change. To operationalize malapportionment change, I construct multiple measurements of state-level highway program malapportionment and calculate the change in state funding malapportionment between 2010 and 2020. I find SAFETEA-LU earmarks are positively associated with highway funding malapportionment change from 2010 to 2020. By continuously paying out to states for previously completed projects, the institutionalized 2005 earmarks increased in policy significance during the earmark moratorium. To rule out the possibility that 2010-2020 highway malapportionment change is a function of differences between states rather than institutional earmarks, I repeat the analysis for the period in which the SAFETEA-LU funding formula and earmarks were active (2005-2009) and find no such relationship between earmarks and malapportionment change. When the funding formula is unfrozen and earmarks represent active community needs, earmarking levels are unassociated or negatively associated with malapportionment change.

This article makes two contributions to the literature on budgetary policy and distributive politics. First, it adds to a growing line of research on how government spending outcomes are shaped by the nature of lawmaking in the US Congress. For instance, extant research finds a small state funding advantage (Lee 2000) and shows how Congress modifies bureaucratic decision-making tools to shape distributive politics outcomes (Mills 2013). I advance this line of research by revealing how Congress can use funding formulas to institutionalize funding disparities between states. Second, this article advances the literature on distributive politics by detailing the failure of the earmark moratorium to rid highway spending of earmarks. The primary finding paints a picture of congressional particularistic spending as alive and well, though more hidden from public view,

under the earmark moratorium. In the case of federal highway funding, the victors of the earmark moratorium were previous pork winners, not porkbusters aiming to root out particularistic spending.

## Congressional Earmarks

Previous research examines the dynamics of congressional earmarking and the role earmarks play in the policymaking process. The literature on congressional earmarks generally fits into three categories: earmarks' electoral benefits, the determinants of earmark distribution among members of Congress, and earmarks' utility in building legislative coalitions.

Earmarks logically connect to Mayhew's (1974) conception of credit claiming, and extant research speaks to the nature, magnitude, and conditionality of earmarks' electoral value. Earmarks decrease the likelihood of electoral challenge and increase campaign contributions (Bickers and Stein 1996; Rocca and Gordon 2013), but the electoral benefits of earmarking are conditional on successful credit claiming and the relevance of earmarked projects to constituents (Grimmer, Messing and Westwood 2012; Braidwood 2015). Additionally, Lazarus, Glas and Barbieri (2012) find only Democrats reap the electoral benefits of earmarking.

Another branch of research focuses on the determinants of earmark distribution among members of Congress, thereby shedding light on congressional power structures. Membership and leadership on key committees – such as appropriations – are associated with increased access to earmarks (Shepsle and Weingast 1987; Lazarus 2009; Clemens, Crespin and Finocchiaro 2015). Additionally, partisanship influences earmark distribution. Members of the majority party are advantaged when it comes to securing earmarks (Lazarus 2009; Clemens, Crespin and Finocchiaro 2015; Balla et al. 2002). Finally, earmark distribution patterns vary between the House and the Senate (Lazarus 2009; Lee 2003, 2004), and interchamber balancing shapes the overall distribution of earmarks (Shepsle, Van Houweling, Abrams and Hanson 2009).

A third branch of research casts earmarks as a tool for legislative coalition building, high-

lighting earmarking's utility as a policy instrument. Earmarks "grease the wheels" of Congress for coalition leaders to pass general interest legislation, such as highway authorizations, appropriations bills, and trade agreements (Evans 2004). For instance, Lee (2003) finds earmarks to be a critical coalition-building tool used to push the 1998 federal highway authorization bill through the House.

The House Republican Conference in the 113th Congress altered the role of congressional earmarks by placing a moratorium on all earmark requests, and the earmark moratorium remained in place from 2010 to 2022 (Gordon 2018). Recently, after a surge in calls for bringing back earmarks and reconstituting congressionally directed spending (Hudak 2018; Courser and Kosar 2021; NYT 2020), the 117th Congress restored congressionally directed spending. Arguments for restoring congressional earmarks highlight earmarks' legislative value as bipartisan coalition building tools, their negligible budgetary cost, and their ability to address local needs (Evans 2004; Strand and Lang 2018; Crespin, Finocchiaro and Wanless 2009; Lazarus 2010).

What happens when reelection focused members of Congress lose the ability to legislatively mandate federal spending projects in their districts? An innovative line of research from Mills, Kalaf-Hughes, and MacDonald shows that legislators seek out particularistic benefits in other ways (Mills and Kalaf-Hughes 2015; Mills, Kalaf-Hughes and MacDonald 2016; Kalaf-Hughes and Mills 2016). Specifically, members of Congress replace earmarking with letter-marking – "when members of Congress explicitly ask (in writing) the head of an administrative agency to retain or allocate distributive benefits in their districts" (Mills and Kalaf-Hughes 2015, 36). In addition to direct communication with agency leaders, I argue that legislators may seek to protect and extend previous earmarks to ensure the continuation of distributive benefits in their districts. While legislators could not mandate new projects in legislation under the earmark moratorium, they could endeavor to protect existing particularistic funding sources by institutionalizing previous earmarks.

# Earmarks and Federal Highway Funding Policy

Universal demand for highway funding among states means that members of Congress share the desire to bring home transportation funding. "Every state and every congressional district has roads, highways, and bridges, and has members of Congress who take an interest in these funds" (Lee 2004, 189). As such, earmarks have historically played a central role in the passage of surface transportation authorization laws that shape federal highway funding (Lee 2003; Evans 2004; Adler 2002). Additionally, major surface transportation laws were passed both before an after the earmark moratorium, making federal highway finding an ideal policy venue for examining what happened to particularistic spending under the earmark moratorium.

Long-term surface transportation authorizations have historically established policy goals, specified the funding formulas to achieve such goals, and mandated funding for special projects. Formula-based grants to the states are foundational to federal highway funding policy and comprise a large proportion of funds authorized in highway-aid legislation; such grants made up more than 90% of highway funding and allocated over $40 billion to states in 2018 (Kirk 2019). As such, the federal highway funding formula is consequential policy that shapes the nation's infrastructure system.

Federal aid to states for highway construction and maintenance began in 1916 with the Federal-aid Road Act (Lewis, Davis and Grossman 2019). Funding formulas have shifted throughout the program's history as Congress has set new federal highway policy priorities, but factors that vary over time – such as population, federal highway miles, and Highway Trust Fund contributions – have historically dictated the allocation of federal highway funds to the states (Kirk 2019).[3] In 2005, Congress passed the Safe, Accountable, Flexible, Efficient Transportation Equity Act: a Legacy for Users (SAFETEA-LU), the last highway authorization bill to establish funding formulas based on time-varying formula factors. SAFETEA-LU allocated $244 billion to the states through FY 2009 and notoriously established over $20 billion in earmarked projects over the five-

[3] 70% of funding formula was based on static state apportionment percentages for a six-year period – FY 1992 through FY 1998 – though the significant Air Quality Improvement Program was based on time-varying formula factors.

year authorization (Fischer 2005).[4]

After the SAFETEA-LU authorization expired in 2009, Congress passed a series of short-term funding extensions until the three-year Moving Ahead for Progress in the 21st Century Act (MAP-21) was passed in 2012, followed by the five-year Fixing America's Surface Transportation Act (FAST Act) in 2015 (Kirk 2019). MAP-21 and FAST are long-term authorizations, but the funding "formulas" in MAP-21 and FAST are based solely on the percentage of total highway allocation each state received in the preceding year. Effectively, the state allocation percentages set in FY 2009 – the last year of SAFETEA-LU – were locked in place and continued to dictate highway funding in 2020 (Lewis, Davis and Grossman 2019). Functionally, MAP-21 and FAST are more like extensions than new authorizations.

The last year in which the funding formula relied on time-varying formula factors, 2009, acts as the sole determinant of highway apportionment for all subsequent years. Funding in 2009 followed the SAFETEA-LU allocation formula and was populated by 2007 data. Therefore, federal highway funding to the states in 2020 was based on a law passed in 2005 and the state of the country in 2007. The only aspect of the highway funding that relied on current data in 2020 was a rule that each state must be allocated at least 95 cents for every dollar it contributes to the Highway Trust Fund (HTF), and Texas is the only state to trigger this requirement (Lewis, Davis and Grossman 2019). The decision to include earmarks in the 2009 state allocation percentages, rather than rely strictly on the funding formula, ensured the continued policy relevance of SAFETEA-LU earmarks. The $20 billion in SAFETEA-LU earmarks (spread out evenly from 2005 to 2009) accounted for over 8% of the total authorization. In sum, federal highway program funding to the states in 2020 was effectively determined by the state of the country in 2007 and SAFETEA-LU earmarks written in 2005. The expired earmarks continued to impact each state's level of highway funding through 2020, perennially advantaging states whose congressional representatives fought for extra projects in 2005.

[4]The total dollar amount of earmarks is an estimate and differs based on varying definitions.

# Theoretical Expectations

The federal highway funding formula has not drawn on time-varying formula factors data since fiscal year 2009, likely leading to a significant degree of malapportionment – some states receiving more (or less) than they should based on real-world circumstances and transportation policy goals. My central theoretical claim is that SAFETEA-LU earmarks are positively associated with the growth of malapportionment under the earmark moratorium. While the earmarks originally stemmed at least partially from funding needs, their inclusion in the federal highway formula after 2009 is not grounded in anything besides legislative maneuvering. In other words, SAFETEA-LU earmarks better reflected policy goals and state needs in the period of their authorization (2005-2009) than the period after they expired (2010-2020). Lawmakers in 2005 had no way of knowing that their earmarking efforts would continue to pay out for a decade after the earmarks' scheduled expiration, making it unlikely that 2005 earmarking involved long-term policy planning. As such, the relative advantage certain states received via SAFETEA-LU earmarks was enhanced by the institutionalization of the earmarks into the frozen funding formula.

An example of how earmarks could fuel malapportionment change helps clarify the theory. Fatalities on the federal-aid system in each state is a time-varying factor in the SAFETEA-LU funding formula, as such fatalities indicate a need for highway improvements. All else equal, states with more fatalities received greater highway funding under the SAFETEA-LU formula. Similarly, earmarks provide additional funding for states to improve surface transportation, thereby enhancing highway safety. Assuming a state uses earmark projects and funding to improve its highways, fatalities can be expected to fall. This decrease in fatalities would trigger an update in the SAFETEA-LU funding formula, meaning a highly earmarked state would eventually receive less formula funding due to the earmarks addressing demand for highway safety funding. However, under a frozen funding formula with institutionalized earmarks, a reduction in fatalities does not trigger a decrease in earmark funding or formula funding. Over time, highly earmarked states are expected to experience fewer fatalities relative to other states due to extra funding. Their relative demand, therefore, decreases with time, yet their relative funding remains elevated. In other words, if the

SAFETEA-LU funding formula was reactivated in 2020 with its time-varying formula factors, it would show that highly earmarked states received far more than the formula called for relative to lowly earmarked states.

Therefore, I hypothesize that the amount of SAFETEA-LU earmarks a state was awarded in 2005 is positively associated with malapportionment change from 2010 to 2020. Highly earmarked states experienced positive malapportionment change – receiving increasingly more than their fair share. Conversely, states with relatively few SAFETEA-LU earmarks experienced negative malapportionment change, receiving increasingly less than their fair share. Rather than fading in policy significance over time, SAFETEA-LU earmarks increased in influence during the earmark moratorium.

## Data and Measurements

Testing the claim outlined above requires operationalizing two central concepts: SAFETEA-LU earmarking levels and highway funding malapportionment change. The Federal Highway Administration provides transparent figures for SAFETEA-LU earmarks at the state level, as such figures were used in the computation of post-2009 highway funding apportionment. However, a dollar value of earmarks does not accurately operationalize the concept of state earmarking advantage. States vary in size and other characteristics, meaning the equal dollar apportionment of earmarks would not represent equality among states. I operationalize "level of earmarking" by calculating the percentage of each states' fiscal year 2009 highway authorization that is composed of earmarks (*earmark dollars / total FY 2009 authorization dollars * 100*).

An alternative, and more common, measure for earmarking is earmark dollars per capita. Allocation per capita is a useful measure in the distributive politics literature (Lee 2000, 2004), and earmark dollars per capita fits the "level of earmarking" concept. However I argue that earmarks as a percentage of highway funding is a better measure for the analysis below, as population does not map cleanly onto state demand for highway funding. A large state with many highway miles likely

requires more funding than a small state with the same population. Earmark dollars per capita would classify the two states as equal if they received the same number of earmark dollars, masking the difference in demand for funding. Rather than rely on a single characteristic – population – to scale earmarks for states, earmarks as a percent of highway funding is based on the entire funding formula. State population is included in the funding formula and therefore contributes to the measure, but so do the other state-level factors that determine highway funding. I use earmarks as a percent of highway funding in the analysis below, but replacing the measure with earmarks per capita does not meaningfully change the results (see Online Appendix).

Highway funding malapportionment change is based on the idea that funding outcomes have drifted from policy goals since the funding formula was frozen at 2009 funding levels. Funding malapportionment is the difference between how much highway funding a state would have received in a given year with an unfrozen funding formula and how much it actually received. Highway funding malapportionment change from 2010 to 2020, therefore, is the degree to which a state benefited or suffered from the funding formula being frozen for over a decade. A state that experienced population growth and increasing highway usage between 2010 and 2020 suffered from the frozen formula, as increases in highway funding demand were not met with increased highway funding relative to other states. A state that experienced population decline and decreasing highway usage between 2010 and 2020 benefited from the frozen formula, as decreases in highway funding demand were not met with decreased highway funding relative to other states. I operationalize highway funding malapportionment change in three steps: I create state-level measures of "correct" highway funding using time-varying formula factors, I construct malapportionment measures by comparing the "correct" spending measures with how much states actually received, and I calculate changes in state malapportionment between 2010 and 2020.

I define "correct" state highway funding as the amount of federal highway funding each state would receive in a given year if allocation was based on longstanding policy goals and updated time-varying formula factors. The funding formula has changed multiple times throughout history as transportation policy goals change, meaning there is no consensus measure. However, the fund-

ing formulas used to determine highway funding up until the formula was frozen in 2009 rely on similar time-varying formula factors. As such, a valid measure of "correct" federal highway spending in each state must be based on time-varying formula factors that have historically shaped the allocation of highway funds. I collect recent state level data on time-varying formula factors and construct two defensible measures of "correct" funding.

First, I recreate the SAFETEA-LU funding formula – the last formula to use time-varying formula factors – with 2010 and 2020 data. I collect state level data for all time-varying formula factors included in the SAFETEA-LU formula from the Department of Transportation for both 2010 and 2020. Using this data to recreate the multifaceted SAFETEA-LU funding formula is made possible by the Eno Center for Transportation's efforts to recreate the SAFETEA-LU formula for 2018. "The SAFETEA-LU era apportionments use data factors that are readily available and relatively straightforward to calculate" (Lewis, Davis and Grossman 2019, 30), and researchers at the Eno Center created a publicly available excel-based tool that computes each state's highway apportionment under the SAFETEA-LU formula. Using total highway apportionment figures as baselines and time-varying formula factors as inputs, the Eno tool weights the data according to the SAFETEA-LU formula and calculates each state's apportionment. Using Eno's SAFETEA-LU formula template and my collected data on 2010 and 2020 time-varying formula factors, I calculate each states' formula derived apportionment for 2010 and 2020. For instance, if the $38 billion allocated for highways in 2010 was allocated under the SAFETEA-LU formula with 2010 state-level data, Alabama would have been allocated $707 million.

The "correct" counterfactual, therefore, is that the highway funding formula used from 2005-2009 continued to determine funding allocations in 2010 and 2020. This allows for a precise measure of "correct" funding that reflects recent transportation policy goals, though it includes a few assumptions and adjustments to the original SAFETEA-LU formula. For instance, the Eno tool is based purely on time-varying formula factors, meaning it excludes the equity bonus payments program, which was inserted into SAFETEA-LU to engender political support from reluctant mem-

bers of Congress.[5] However, because my aim is to generate valid measures of "correct" appropriations rather than stay true to the original SAFETEA-LU formula, I consider the removal of a political variable from the formula as an improvement.

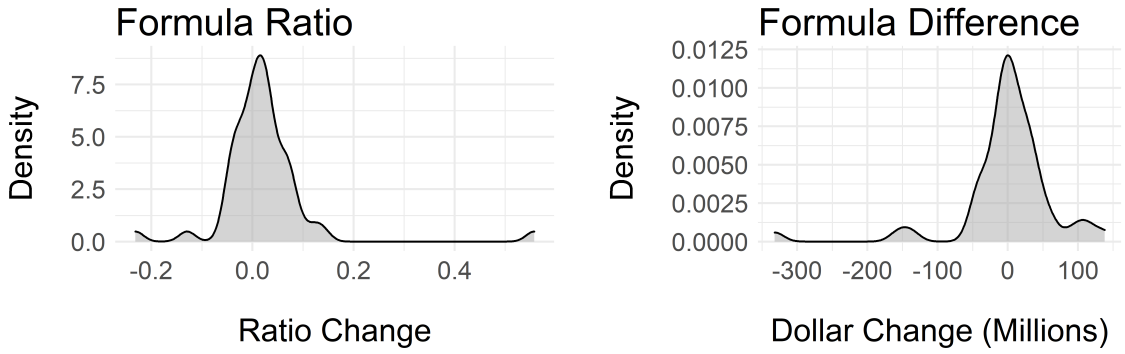
For an alternative "correct" funding measure, I record each states' yearly Highway Trust Fund (HTF) contribution for 2010 and 2020.[6] State return on HTF contributions has long been at the center of federal highway funding considerations and is a highly consequential time-varying formula factor (Lee 2003; Kirk 2019). HTF contribution is a limited measure of "correct" funding because it ignores all other time varying formula factors, but it makes for an easily interpretable alternative measure with high face validity – the metric is often used in debates over proposed highway funding formulas to determine which states are advantaged and disadvantaged. As such, I use HTF contribution as a robustness check on the primary analysis.

To create measures of state highway funding malapportionment, I compare the amount a state received in highway program funding with their "correct" funding measures for that year. I create both a ratio (*actual / correct*) and difference (*actual - correct*) malapportionment measure for each "correct" funding measure. Because Alabama actually received $759 million in 2010 rather than the $707 million called for by the formula, Alabama has a ratio malapportionment score of 1.07 and difference malapportionment score of $52 million for 2010. Finally, I use the malapportionment measures to construct the dependent variable for analysis – malapportionment change. This is achieved by calculating the change in state malapportionment from 2010 to 2020 for each of the malapportionment measures. Positive values indicate positive malapportionment change and negative values indicate negative malapportionment change over time. Alabama's ratio malapportionment score of 1.07 in 2010 sunk to 1.03 in 2020, yielding a -0.04 malapportionment change score. Meaning, Alabama was slightly less advantaged by the frozen funding formula in 2020 than in 2010. Distributions of the two malapportionment change measures used in the primary analysis are shown in Figure 1.

[5] See Online Appendix for a more comprehensive explanation of the formula and its assumptions.

[6] I use a two-year lag on state HTF contributions, meaning 2008 HTF contributions are used for 2010 and 2018 HTF contributions are used for 2020. This follows how HTF contributions are considered by the DOT for the 95% HTF return rule.

**Figure 1: Density Plots of 2010 to 2020 Malapportionment Change Measures**




# Methods

Based on the theory outlined above and the observed data generation process, I estimate two models to explore the relationship between SAFETEA-LU earmarks and state malapportionment change. To test the claim that SAFETEA-LU earmarks positively predict malapportionment change during the formula freeze period, I model the two SAFETEA-LU-based measures of state malapportionment change as a function of the *Percent Earmarks* variable and a list of state-level covariates that are likely to effect state highway funding needs: population (logged), federal aid highway miles (logged), and vehicle miles traveled on federal aid highways (logged).[7] These covariates are included in the model to account for differences between states that plausibly effect highway funding malapportionment. Because highway funding was locked-in under the frozen formula, possible confounding variables are those that plausibly impact demand for highways funds rather than variables that ordinarily impact spending, such as natural disasters or shifts in party control of Congress. The dependent variable, 2010 to 2020 malapportionment change, is distributed according to the normal distribution with mean, $\mu$, and variance, $\sigma^2$. I model $\mu$ as a function of the matrix of state-level covariates and a vector of coefficients $\beta$, to be estimated from the data:

$$y \sim f_N(\mu, \sigma^2)$$
$$\mu = \chi\beta$$

[7]The year in which I take these values matches the year used to re-create the 2010 SAFETEA-LU formula: 2009 population, 2008 federal aid highway miles, and 2008 vehicle miles traveled on federal aid highways.

where $y$ represents state malapportionment change, $\chi$ represents the matrix of state covariates, $\beta$ represents the estimated coefficients, and $\sigma^2$ is a measure of assumed constant variance.

Due to the presence of outliers in the data, I employ robust linear regression to generate coefficient estimates. Next, I use the model estimates to predict values of malapportionment change across the full range of the *Percent Earmarks* variable. To capture the uncertainty around these model predictions, I simulate from the full distributions of $\beta$ coefficients and generate confidence intervals around the predicted malapportionment values.

# Results

Consistent with expectations, I find SAFETEA-LU earmarks are a positive predictor of state malapportionment change from 2010 to 2020. Table 1 displays the coefficient estimates and standard errors for the two models. The mean coefficients for the *Percent Earmarks* variable are positive across both models and reach conventional levels of statistical significance in the ratio model (p-value $< 0.01$). Meaning, states that received high levels of SAFETEA-LU earmark funding benefited from highway funding malapportionment change between 2010 and 2020. The earmarks technically expired in 2009, but their influence over highway funding policy outcomes grew in the decade after their expiration. These findings do not change when when using the alternative independent variable measure – earmarks per capita – or dependent variable measure – return on HTF contribution.[8]

To clarify the magnitude and uncertainty of the relationship between earmarks and malapportionment change, I use the regression coefficients to simulate predictions of 2010 to 2020 malapportionment change across the entire observed range of the *Percent Earmarks* variable. I simulate 1,000 state malapportionment predictions for each hypothetical value of the *Percent Earmarks* variable at 0.1 increments from 3.24% (lowest observed value in the data) to 35.64% (highest ob-

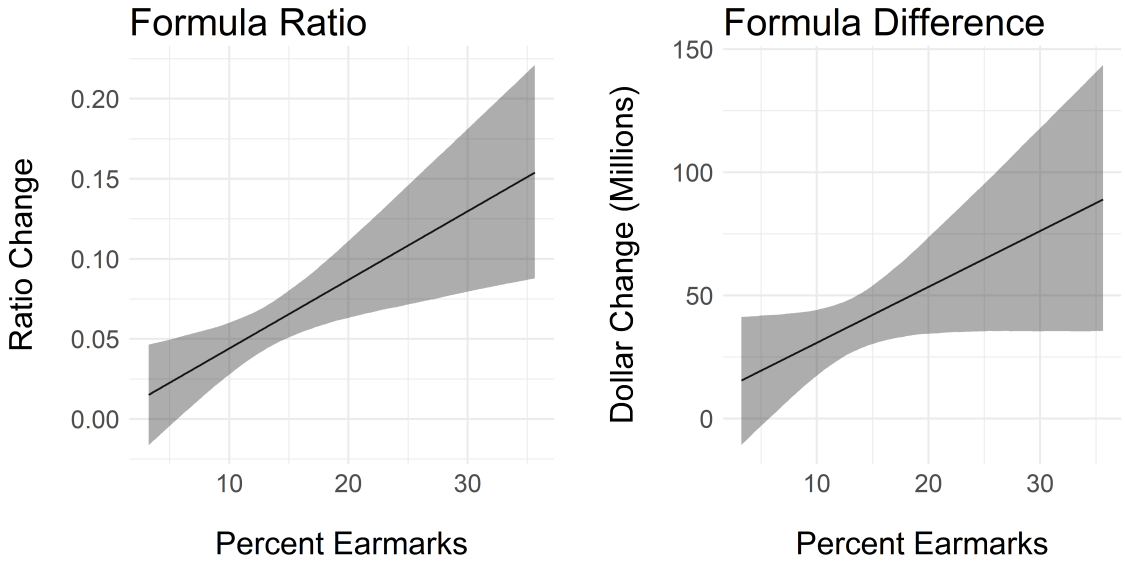
[8]Findings are robust across additional model specifications, including regular OLS regression, controlling for region of the country, controlling for change in the model covariates between 2010 and 2020, and controlling for total 2009 highway spending. See the Online Appendix for all alternative model specifications.

**Table 1: Earmarks and Highway Funding Malapportionment Change: 2010-2020**

| | *Dependent variable:* | |
|---|---|---|
| | Formula Ratio | Formula Difference (In Millions) |
| | (1) | (2) |
| Percent Earmarks | 0.004*** | 2.270* |
| | (0.001) | (1.215) |
| Population (Logged) | −0.008 | 14.422 |
| | (0.049) | (40.528) |
| Federal Aid Highway Miles (Logged) | 0.011 | 5.916 |
| | (0.015) | (12.516) |
| Vehicle Miles Traveled (Logged) | −0.004 | −17.415 |
| | (0.059) | (48.180) |
| Constant | −0.021 | −64.163 |
| | (0.120) | (99.088) |
| Observations | 51 | 51 |
| Residual Std. Error (df = 46) | 0.042 | 32.555 |
| *Note:* Robust Linear Regression | *p<0.1; **p<0.05; ***p<0.01 | |

served value). The simulation draws from the entire distribution of betas generated by the model, thereby capturing model uncertainty across the counterfactual range of earmark values. Figure 2 displays the simulation results with a 95% prediction interval band for both models.

**Figure 2: Predicted State Malapportionment Change by Level of Earmarking**



The findings displayed in Figure 2 are substantively meaningful, and an interpretation of results from the Formula Ratio model contextualizes the observed relationship between institutionalized earmarks and malapportionment change in terms of 2020 highway funding. A mean sized state whose 2009 highway apportionment was made up of 8.9% earmarks (first quartile value) is predicted to receive approximately $0.04 more per formula prescribed dollar in 2020 than in 2010, whereas a mean sized state whose 2009 highway apportionment was made up of 16.7% earmarks (third quartile value) is predicted to receives $0.07 more per formula prescribed dollar in 2020 than in 2010. For a state with the mean formula prescribed highway allocation in 2020 ($850,388,124), the difference between a 0.07 ratio increase and a 0.04 ratio increase is $25,511,644. In sum, the effects of institutionalized earmarks on highway funding malapportionment change carries significant distributive policy consequences.

## Placebo Test: 2005-2009 Malapportionment Change

I argue above that SAFETEA-LU earmarks became disassociated with state need and policy goals as they continued to pay out long after their intended use, resulting in a positive relationship between SAFETEA-LU earmarks and highway funding malapportionment change. The advantage states received from their congressional representatives securing high levels of SAFETEA-LU earmarks grew under the frozen formula, as they continued to receive earmark money long after the earmarked projects concluded. I find support for this theory above – earmarking levels positively predict federal highway funding malapportionment change from 2010 to 2020.

However, it remains possible that the reported relationship stems from unobserved differences between states affecting both earmarking and malapportionment change. Adding controls for observable variables that might affect earmarking and malapportionment change – population, federal aid highway miles, and vehicle miles traveled on federal aid highways, 2009 total highway funding, and region of the country – does not change the reported findings, but it is impossible to control for all potential confounding variables. To test this confounding concern and strengthen my empirical

claims, I employ a placebo test to explore whether the relationship between earmarking and malapportionment change exists during an era with active earmarks and an unfrozen funding formula. That is, when the dynamics that I argue lead to a positive relationship between earmarking and malapportionment change are not present, does a state's level of earmarking still predict highway funding malapportionment change?

I examine highway funding malapportionment change during the SAFETEA-LU period (2005-2009). Highway funding in this period included a formula that used yearly time-varying formula factors, and earmark spending in this period was on active projects deemed worthy of funding in 2005. If the relationship between earmarking and malapportionment change is the result of unobserved differences between states, we would expect to observe a similarly positive relationship between SAFETEA-LU earmarks and malapportionment change from 2005 to 2009. Alternatively, if the above findings are attributable to institutionalized earmarks driving malapportionment change during the earmark moratorium, we should not observe a positive relationship between SAFETEA-LU earmarks and 2005 to 2009 malapportionment change. I argue that institutionalized earmarks under the frozen funding formula are the driving force behind the positive association observed above, so I expect that there is not a meaningfully positive association between earmarking and malapportionment change from 2005 to 2009.

The *Formula Ratio* and *Formula Difference* measures of malapportionment change cannot be used for this analysis, as they are derived directly from the SAFETEA-LU funding formula. Therefore, they are incapable of detecting malapportionment change during the SAFETEA-LU period. However, the two Highway Trust Fund measures of malapportionment change can be replicated for the 2005 to 2009 period. Similar to the SAFETEA-LU funding formula analysis, earmarking is a significant and substantial predictor of malapportionment change in both the *HTF Ratio* and *HTF Difference* models (see Online Appendix for further discussion of this analysis). Using the same data sources and measurement scheme for all variables used in the earlier analysis, I replicate the HTF Ratio and HTF Difference models with 2005 to 2009 malapportionment change as the dependent variable. The first two columns of Table 2 display the 2005 to 2009 analysis of HTF

measures, while the third and fourth columns display the 2010 to 2020 analysis of HTF measures for reference.

**Table 2: Earmarks and Highway Funding Malapportionment Change: Placebo Test**

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | 2005-2009 HTF Ratio | 2005-2009 HTF Difference (In Millions) | 2010-2020 HTF Ratio | 2010-2020 HTF Difference (In Millions) |
| | (1) | (2) | (3) | (4) |
| Percent Earmarks | −0.002 | −1.074** | 0.007*** | 3.548*** |
| | (0.001) | (0.510) | (0.002) | (1.093) |
| Population (Logged) | −0.031 | −28.282 | 0.051 | 19.594 |
| | (0.045) | (17.700) | (0.057) | (36.460) |
| Federal Aid Highway Miles (Logged) | −0.043*** | −6.640 | −0.097*** | −14.070 |
| | (0.013) | (5.238) | (0.018) | (11.260) |
| Vehicle Miles Traveled (Logged) | 0.059 | 4.588 | 0.028 | −14.281 |
| | (0.053) | (20.937) | (0.068) | (43.344) |
| Constant | 0.062 | 86.170** | −0.228 | −96.394 |
| | (0.108) | (42.716) | (0.139) | (89.141) |
| Observations | 51 | 51 | 51 | 51 |
| Residual Std. Error (df = 46) | 0.030 | 14.206 | 0.052 | 34.922 |

*Note:* Robust Linear Regression *p<0.1; **p<0.05; ***p<0.01

Inconsistent with the confounding hypothesis, results reported in Table 2 show no evidence of a positive relationship between SAFETEA-LU earmarks and 2005 to 2009 highway funding malapportionment change. The mean coefficient for the *Percent Earmarks* variable is negative in both models and statistically distinct from zero in the HTF Difference model. Therefore, the substantially positive association between institutionalized earmarks and 2010 to 2020 malapportionment change cannot be cast aside as a spurious relationship based on differences between states, because no such relationship existed in the period preceding the formula freeze. In an era where earmarks reflected current community needs and the funding formula was updated yearly based on time-varying formula factors, the association between earmarking and malapportionment change is nonexistent or negative.

It's worth noting that the placebo window (2005-2009) is substantially shorter than the window

used in the analysis above (2010-2020). As such, it is not a perfect placebo test, because it remains possible that the shorter window did not allow enough time for the relationship between earmarking and malapportionment change to emerge. However, that the coefficient for the *Percent Earmarks* variable is negative in both models and statistically distinct from zero in one model helps mitigate this concern. Further, the negative association between earmarking and malapportionment change from 2005 to 2009 is robust to model specification. When replacing the independent variable with earmark dollars per capita, the association between earmarking and malapportionment change is negative and statistically significant in both models (see Online Appendix for results). The placebo time window is shorter, but the relationship between earmarking and malapportionment change appears to be meaningfully different in the SAFETEA-LU period than the frozen formula period.

In sum, the placebo test adds credibility to findings from the primary analysis on the positive association between institutional earmarks and malapportionment change. The combined analysis does not establish causality, as the research design is unable to rule out all alternative explanations and omitted variables. However, these findings offer a detailed description of an important policy moment and suggest a meaningful relationship between institutional earmarks and malapportionment change.

# Conclusion

Federal highway earmarks officially expired in 2009, yet they continued to influence highway funding policy outcomes in 2020. Their institutionalization into the frozen funding formula ensured their continued policy relevance. Further, the earmarks' association with funding malapportionment strengthened between 2010 and 2020. I find robust evidence of a positive association between earmarks and malapportionment change under the frozen funding formula. States that were advantaged in the 2005 earmarking process became even more advantaged after the earmarks were institutionalized into the frozen highway funding formula in 2010.

Three central takeaways emerge from these findings to inform our understanding of the US

Congress and budgetary policy. First, these findings add to the diverse and growing body of research on how government spending outcomes are shaped by the nature of lawmaking in the US Congress. For instance, Lee (2000) finds that small states are advantaged in distributive politics due to the Senate's equal weighting of states, and Mills (2013) shows how Congress modifies bureaucratic decision-making tools, such as benefit-cost analysis calculations, to shape distributive politics outcomes. The above analysis contributes to this line of research by revealing how Congress can use funding formulas to preserve particularistic benefits and institutionalize funding disparities between states.

Second, public policy researchers document the punctuated equilibrium nature of budgetary policy change – characterized by long periods of stability and major punctuations – stemming from institutional friction (Jones, Sulkin and Larsen 2003; Breunig, Koski and Mortensen 2010). This article identifies frozen funding formulas as a mechanism of institutional friction in congressional distributive politics and highlights institutional earmarks as one way in which funding outcomes can drift away from policy goals over time.

Third, this analysis advances the literature on congressional representation by detailing the failure of the earmark moratorium to rid transportation policy of earmarks. A long literature conceives of earmarks and particularistic spending as foundational to congressional representation (Mayhew 1974; Bickers and Stein 1996; Lazarus 2010) and lawmaking (Evans 2004; Lee 2003). As such, the earmark moratorium is a potential disruption to extant research on Congress and deserves scholarly attention. Recent research shows that members of Congress did not stop seeking particularistic benefits under the moratorium, but instead changed tactics to less visible strategies, such as lettermarking (Mills, Kalaf-Hughes and MacDonald 2016; Kalaf-Hughes and Mills 2016). My findings paint a picture of congressional particularistic spending as alive and well, though more hidden from public view, under the earmark moratorium.

However, this analysis is limited in that it only explores the institutionalization of earmarks into in federal highway funding, one specific form of spending policy. The policy implications discussed above are consequential even if limited to highway funding – SAFETEA-LU appro-

priated $4 billion per year in highway earmarks, while all appropriations earmarks for FY 2009 totaled $19.6 billion (Crespin, Finocchiaro and Wanless 2009) – but whether the concept of institutional earmarks generalizes to other policy domains remains an open question. After all, the normal appropriations bills did not freeze funding levels and institutionalize previous earmarks during the earmark moratorium. I argue the concept of institutional earmarks is useful for distributive politics research moving forward, particularly with the return of earmarks to Congress in 2022. First, there are already calls by members of Congress to again eliminate earmarks,[9] and the potential for institutional earmarks in highway funding and other authorization legislation looms if Congress eliminates earmarking again. Second, continuing resolutions have become the norm in the congressional appropriations process (McClanahan, Bill Jr Heniff, Murray and Lynch 2019), and legislators might look to institutionalize existing earmarks by including them in continuing resolutions. Therefore, examining efforts to institutionalize earmarks in continuing resolutions offers a potentially important next step in distributive politics research.

A few technical limitations of the analysis above are important to note and open the door for further research. First, the structure of the data required analysis with a small number of observations. I show in the Online Appendix that the reported findings are robust to multiple model specifications and measurement strategies, but the small sample size limits the certainty of findings. One avenue for future research is collecting highway funding data over a longer period of time and employing time-series analysis. Additionally, the empirical strategy employed above is unable to make strong causal inferences. The primary analysis reveals a substantive and positive association between a state's level of earmarking and malapportionment change from 2010 to 2020, and the placebo analysis offers supportive evidence that institutional earmarks fueled malapportionment change under the frozen funding formula. However, the former statement is more supported by the analysis than the later, and additional research is needed to confirm the causal role of institutional earmarks in malapportionment change. Finally, the nature of federal highway spending necessitated state-level analysis, but earmarks are generally a highly localized form of distributive

[9]For example: https://www.braun.senate.gov/senator-braun-receive-vote-cut-pork-amendment-eliminate-earmarks-spending-bill

spending. Further research should delve deeper into earmark spending at the local level to examine whether earmark-derived funds under the frozen highway formula kept flowing to particular counties, thereby exacerbating funding inequality, or were spread out within states.

# *Chapter 3: Priority Projects*

# *Constituent Demand and the Benefits of Congressional Spending*

**Abstract**

How do incumbents in the US Congress turn federally funded district projects into electoral gains? Clarifying the connection between federal spending and congressional elections is critical for understanding the institution, as distributive benefits are theorized to enhance representation and facilitate broader policymaking. Extant studies argue that members of Congress use credit claiming for distributive benefits to cultivate an impression of influence, portraying themselves as uniquely capable of securing projects for their constituents. I develop a targeted theory of congressional distributive politics and argue that public support is granted to legislators for securing the *right* distributive benefits rather than securing the *most* distributive benefits. Using two survey experiments to explore constituent demand for different types of spending, I find robust evidence that legislators' ability to meet constituent demand shapes the effectiveness of their credit claims. I also find unexpected partisan convergence in the public's spending priorities and responsiveness to congressional credit claiming. This research advances the literature on congressional representation and offers a more complete account of the politics around congressional spending.

# Introduction

The power of the purse represents a core responsibility of the US Congress, and the allocation of public resources is theorized to play a critical role in the institution's design and functionality. Federally funded district projects are considered prized electoral assets that shape institutional organization (Mayhew 1974; Shepsle 1979; Shepsle and Weingast 1981; Arnold 1990), representation (Fenno 1978; Frisch 1998; Grose 2011), and legislative coalition building (Lee and Oppenheimer 1999; Lee 2000; Evans 2004). Such theories hinge on distributive benefits providing electoral value, making the connection between federal spending and electoral outcomes an essential quantity of interest for understanding policymaking in Congress.

Empirical research suggests the relationship between distributive benefits and electoral outcomes is generally positive (Levitt and Snyder Jr 1997; Stratmann 2013), but conditional on party affiliation; only Democrats stand to electorally benefit from traditional distributive politics (Sellers 1997; Lazarus, Glas and Barbieri 2012; Sidman 2019). In an expansive investigation of credit claiming for distributive benefits, Grimmer, Westwood and Messing (2014) clarify the primary mechanism through which distributive politics is used as an electoral strategy. They find legislators can effectively use credit claiming for federal expenditures to cultivate an *impression of influence*, therein increasing name recognition, perceived influence in Congress, and general support.

In this paper, I theorize that the electoral value of distributive spending is targeted rather than general. The advantage to be gleaned by members of Congress from distributive benefits and credit claiming is determined by how well new spending projects align with constituent demand. In other words, public support is granted to legislators for securing the *right* distributive benefits rather than securing the *most* distributive benefits. By emphasizing constituent demand for different types of spending, I develop an additional quantity of interest concerning congressional credit claiming – the *impression of understanding*. Whereas the impression of influence centers on legislators' capacity to secure funding for local projects, the impression of understanding centers on legislators' recognition of constituent needs.

I use two survey experiments to examine constituent demand for different types of spending and

explore how such demand shapes the consequences of congressional distributive politics. In both surveys, I ask respondents to rate nine common spending categories by level of need in their community. Descriptive analysis reveals Democrats and Republicans to be similar on relative demand for different types of spending, prioritizing the same spending categories. However, Democratic respondents display higher absolute demand for federal spending in their communities.

The first experiment assigns each respondent to a experimental vignette featuring a credit claiming press release from one of their actual US senators for a federally funded project in their state. Critically, the type of secured funding is randomly drawn from the list of nine spending categories the respondent previously rated. The change in respondents' pre/post vignette ratings of the credit claiming senator is used as the dependent variable in the resulting analysis. I find strong evidence for the theory of targeted distributive politics. Credit claiming for high priority projects is far more effective than credit claiming for medium or low priority projects, and the positive relationship between project priority and senator approval remains constant across respondent party affiliation. Constituents reward legislators for bringing home projects based on how well the spending aligns with constituent demand.

Disentangling the *impression of influence* and the *impression of understanding* in congressional distributive politics requires an additional research design, as the first experiment holds influence constant. In the second survey experiment, I explore both legislator influence and constituent spending demand as determinants of credit claiming effectiveness. The experimental structure matches the first experiment: pre-experiment senator ratings, measures of respondent spending priorities, a credit claiming experiment vignette, and post-experiment senator ratings. However, rather than randomly assigning one spending category from the full list of nine, I leverage a 2x2 design that varies spending demand (1st priority / 6th priority) and overall senator influence (top 3 in overall funding / below average in overall funding). I find the spending demand treatment meaningfully increases support for the credit claiming senator, whereas senator influence has no effect. When varied in an experimental design, the effectiveness of congressional credit claiming is shaped by senators' ability to secure the right distributive benefits rather than the most distributive

benefits. In sum, the findings from both experiments provide support for the theory of targeted distributive politics.

This study makes two primary contributions to the literature on congressional representation. First, it offers a more complete account of the politics around congressional spending. I reveal interesting variation in constituent demand for different categories of federal spending, and I find that legislators' ability to meet this demand shapes the effectiveness of their credit claims. Second, this study fills a gap between the literature, which suggests only Democrats stand to benefit from distributive politics, and current developments in Congress, where many Republicans actively seek out and claim credit for federal spending. I show that Republican constituents have an overall lower appetite for federal spending, but they equal Democratic constituents in their willingness to reward legislators for bringing home high priority spending projects.

## Congressional Distributive Politics

Influential theories of the US Congress argue that geographically targeted spending shapes incumbent reelection strategy and institutional organization. Mayhew (1974) includes credit claiming for particularistic benefits as a core activity of MCs, and Fenno (1978) shows that distributive politics is central to legislators' home style. Bringing home federal spending projects helps legislators build a personal vote, display influence in Washington, and dissuade potential challengers from running in upcoming elections (Ferejohn 1974; Cain, Ferejohn and Fiorina 1987; Ferejohn 1986; Arnold 1990; Bickers and Stein 1996). These early theories of congressional distributive politics argue that the motivation for securing distributive benefits is universal among legislators, as securing spending projects assists legislators in their quest for reelection.

As a prized electoral asset, distributive benefits feature prominently in theories of congressional organization and lawmaking. The influential "gains from exchange" model of congressional organization relies on a politics-of-distribution perspective and argues that Congress's institutional structure channels legislative self-interest (Shepsle 1979; Weingast and Marshall 1988; Shepsle

and Weingast 1987). The committee system facilitates influence exchanges, where legislators can seek out the distributive benefits of highest priority in their districts (Adler and Lapinski 1997; Adler 2002).

Additionally, the electoral utility of distributive benefits can be leveraged to build winning legislative coalitions. Evans (2004) finds distributive benefits help legislative coalition leaders "grease the wheels" of Congress and generate support for general interest legislation. Federal spending patterns display evidence of coalition building through distributive benefits. Senate malapportionment means it is more economically efficient to build coalitions of small state senators, and small states are indeed favored in federal spending outcomes (Lee and Oppenheimer 1999; Lee 2000). Different dynamics shape coalition building through distributive politics in the House. House members are more capable of claiming credit for specific projects than general state-level funding, so legislative coalition building occurs through project grants rather than general state allocations (Lee 2003). Taken together, this research shows distributive benefits are used by coalition leaders to build support for broader legislation.

In sum, influential theories on the US Congress place distributive politics as a key element of congressional lawmaking and organization. Legislators use district spending projects to build a personal vote among thankful constituents, committees allow legislators to trade influence and pursue their highest priority benefits, and coalition leaders use distributive benefits to build support for legislation across party lines. These accounts of how the US Congress operates hinge on the electoral value of distributive benefits. Clarifying the electoral effects of distributive benefits, therefore, is critical to understanding congressional representation and organization.

Empirical studies show that distributive benefits can help congressional incumbents secure reelection. Levitt and Snyder Jr. (1997) use an instrumental variable design, leveraging spending outside the district but inside the state, to find that federal spending meaningfully increases congressional incumbents' performance in general elections. Similarly, Stratmann (2013) uses Senate malapportionment and Appropriations Committee membership as instruments for House district spending and finds distributive benefits increase House incumbents' chances of reelection. Further,

distributive benefits impact congressional elections in more ways than a direct effect on voters. Rocca and Gordon (2013) find a robust relationship between defense earmarks and campaign contributions from the defense industry, suggesting distributive benefits offer incumbents a funding advantage.

Congressional communication provides a critical link between spending outcomes and congressional elections, as most voters do not pay enough attention to policy developments to recognize new federal expenditures and accurately assign credit (Mayhew 1974; Arnold 1990; Stein and Bickers 1994; Achen and Bartels 2017). Grimmer et al. (2012, 2014) conduct an expansive investigation of credit claiming for distributive benefits, generating insights on how legislators use credit claiming to build a personal vote by cultivating an *impression of influence*: "creating a reputation as effective at delivering money to the district" (6). On the supply side of credit claiming, they find swing district legislators use credit claiming more aggressively than safe district legislators. On the effects of credit claiming, they find constituents respond more positively to credit claiming for distributive benefits than other types of congressional messaging. Credit claiming increases legislator name recognition, perceived influence in Congress, and general support.

My central theoretical claim is that the electoral value of distributive spending is targeted rather than general. Demand for various types of distributive benefits varies across time and place (Clemens, Crespin and Finocchiaro 2015), and voters prefer that their representatives in Congress secure spending in personally relevant policy areas (Braidwood 2015). The electoral rewards for distributive benefits, therefore, are likely situational and isolated to particular constituencies. In other words, public support is granted to legislators for securing the *right* distributive benefits rather than securing the *most* distributive benefits.

Legislators appear to understand the targeted nature of distributive benefits. In line with Fenno's (1978) conception of congressional representation and home style, Frisch and Kelly (2015) show how members of Congress seek to understand their constituencies and pursue distributive benefits that reflect constituency needs. This strategy is reflected in committee membership, where legislators seek out committee posts that provide access to the distributive benefits of highest priority in

their districts. Similarly, Lazarus (2010) finds legislators are responsive to constituent preferences when seeking out different types of earmarks. Legislators recognize that the electoral rewards to be gleaned from distributive benefits are situational and vary across time and place.

Bringing home high demand projects displays more than just influence in Washington and legislators' capacity to provide positive outcomes for constituents. It displays an understanding of the unique needs of constituents and shows that legislators are in touch with their community. Conversely, securing and claiming credit for unneeded or unwanted projects displays a lack of understanding of the community and can be interpreted as wasteful spending. Legislators stand to benefit from addressing specific constituency needs rather than blanketing constituencies with federal money. As such, I predict that the amount of constituent support a member of Congress receives from securing federal spending is a function of the level of priority constituents assign to the type of secured spending:

**Hypothesis 1:** *As constituent demand for a certain type of spending increases, legislators will receive a greater boost in public opinion for securing that type of spending.*

The literature on congressional communication presents position taking and credit claiming as opposing communication strategies for legislators to present their work in Congress to their constituents (Mayhew 1974; Grimmer 2013). Whereas position taking focuses on broad national policy debates and appeals to more partisan audiences, credit claiming is designed to build a personal vote by generating an *impression of influence* (Grimmer, Messing and Westwood 2012; Grimmer, Westwood and Messing 2014; Gerber, Patashnik and Tucker 2022). By emphasizing constituent demand for different types of spending projects, I develop an additional quantity of interest concerning congressional credit claiming – the *impression of understanding*. Whereas the impression of influence centers on legislators' capacity to bring home federally funded projects, the impression of understanding centers on a legislators' recognition of constituent needs.

Of course, these conceptions of credit claiming are not mutually exclusive. The most effective credit claims display impressive influence along with an understanding of constituent needs. Further, Grimmer et al. (2014) recognize that legislators have little incentive to claim credit for

controversial projects and focus on popular expenditures: "More attractive to legislators, then, are expenditures that elicit a positive response from constituents – such as firefighters, police officers, roads, national parks, homeland security, and local education" (26). However, spending preferences are then treated as more of an assumed constant, with a focus on broadly popular expenditures, than a target of investigation. My focus on the impression of understanding and targeted distributive politics builds on the existing credit claiming framework by investigating how constituent demand for different projects varies, even within broadly positive spending categories, and shapes the effectiveness of congressional credit claiming.

## The Partisan Nature of Distributive Politics

Recent research complicates our understanding of congressional distributive politics by casting doubt on the universal motivation for distributive benefits. Multiple studies find that partisanship conditions the electoral benefits to be gleaned from distributive benefits. Democrats stand to benefit from securing spending projects in their districts, whereas Republicans have little to gain from traditional distributive spending (Sellers 1997; Bickers and Stein 2000; Lazarus and Reilly 2010; Lazarus, Glas and Barbieri 2012; Crespin and Finocchiaro 2013). Polarization enhances the partisan nature of distributive politics, as distributive benefits become part of the partisan battleground (Sidman 2019). Together, recent research suggests there is little electoral incentive for Republicans in Congress to secure traditional distributive benefits in their districts.

However, many Republican legislators continue to actively pursue and claim credit for distributive benefits. House and Senate Republicans combined to request thousands of earmarks – now called "Community Project Funding" – in each of the FY 2022 and FY 2023 appropriations cycles and secured more than $10.5 billion in earmarked funds (Courser, Wuerfmannsdobler and Thorning 2023). Democrats have been more active in pursuing earmarks, but Republicans have secured more than 40% of earmark funding in each chamber since earmarking has returned to the appropriations process. Such work is not a costless activity – securing distributive benefits re-

quires time and political capital (Guenther and Searle 2019). As such, a substantial contingent of Republicans in Congress seemingly believes there is something to be gained from bringing home particularistic spending. The literature on partisanship and polarization uncovers a critical dimension of modern congressional distributive politics, but it does not comprehensively explain this aspect of spending patterns.

My theory of targeted distributive politics can help fill this gap between the literature and recent developments in congressional spending. While Republican legislators may not electorally benefit from securing more money and projects in aggregate, they still benefit from securing specific types of distributive benefits. There is some evidence for this assertion in the literature; Republicans in Congress are found to benefit from federal spending in the form of contingent liabilities (Bickers and Stein 2000; Lazarus and Reilly 2010). I extend this argument to theorize that Republicans benefit more from distributive politics than extant research suggests. If the electoral value of distributive politics is grounded in constituency needs and targeted spending rather than the general provision of federal funds, aggregate district spending figures are an imperfect input for models aiming to clarify the electoral benefits of distributive spending. Studies focused on the relationship between total spending (or project counts) in congressional districts and incumbent electoral performance likely miss this dimension of distributive politics. Even if Republican legislators do not electorally benefit from securing more aggregate spending, they are still likely to benefit from bringing home high demand types of spending.

I argue that partisanship conditions how respondents reward legislators for distributive benefits, but the story is more nuanced than Democrats rewarding legislators for securing particularistic spending and Republicans ignoring or punishing such behavior. Bringing home a high demand project plausibly boosts legislator favorability among both Republicans and Democrats. The partisan difference lies in constituent reactions to lower propriety projects. Because Democrats are positively disposed to government spending, Democratic voters will still reward their representatives for bringing home lower priority projects. Conversely, because Republicans are ideologically opposed to government spending, they are unlikely to reward, and may even punish, legislators

for bringing home low priority projects. Therefore, I argue that the relationship between spending demand and legislator favorability is conditioned by partisanship, in that the difference in support given for high and low priority projects is greater among Republican constituents than Democratic constituents.

**Hypothesis 2:** *The relationship between project priority and increased favorability is greater among Republican constituents than Democratic constituents.*

# Research Design

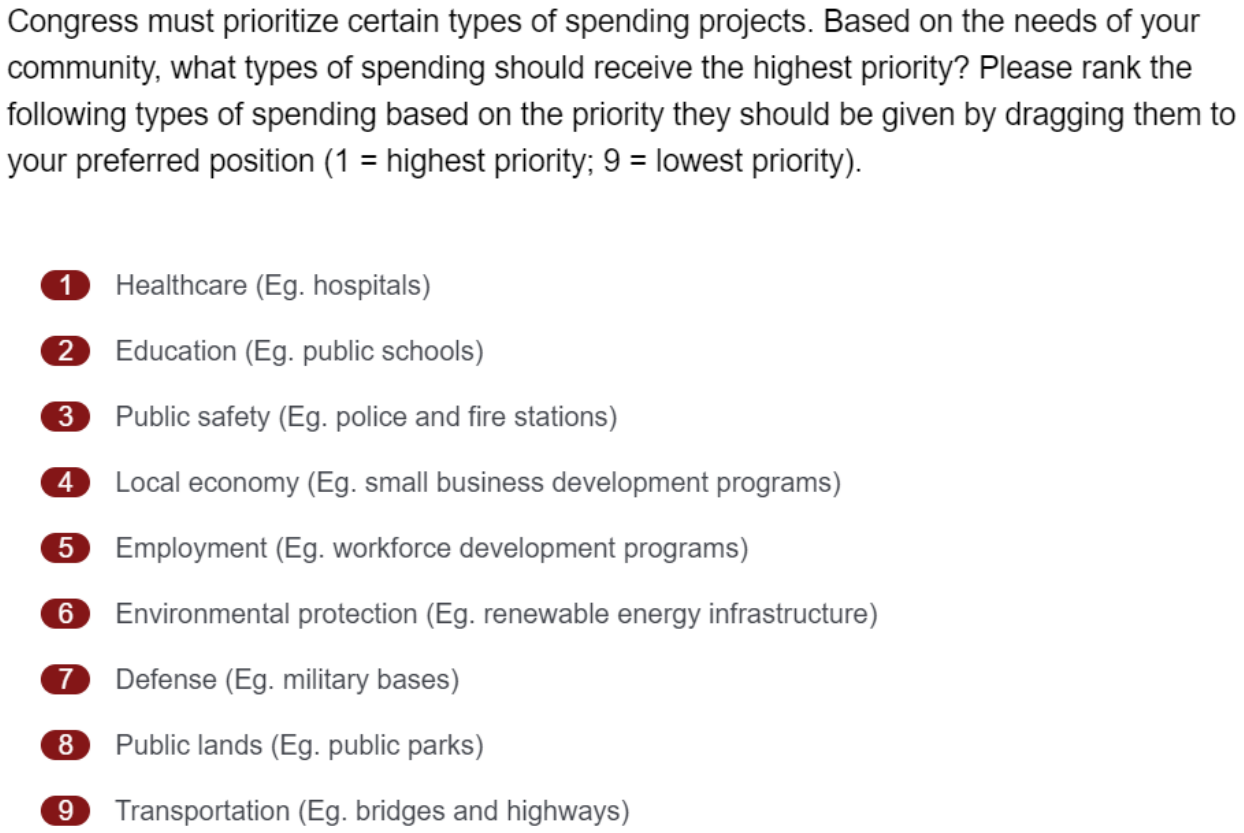
I use a national survey experiment to explore demand for federal spending and test the above hypotheses.[10] Approximately 3,250 respondents were recruited through the Lucid Theorem platform to take a 15-minute survey. The information collected in an early component of the survey is used in conjunction with an experimental design in a later component of the survey to examine how constituents' spending priorities shape their response to congressional credit claiming. The initial survey component measures respondent demand for certain types of federal spending, and the later component includes an experimental vignette featuring a congressional credit claim for a distributive project. Together, this data is well-suited to analyze the role of constituent demand in determining the effectiveness of legislators' distributive politics efforts.

I record two key measurements at the beginning of the survey. First, I ask respondents to evaluate each of their US senators. These evaluations, which consist of a feeling thermometer (0-100) as well as specific ratings on effectiveness (0-10), representation (0-10), and fiscal responsibility (0-10), provide a baseline measure of senator support. Second, I ask respondents to rank nine categories of federal spending by level of need in their community. Project categories include common targets of congressional spending: healthcare, education, public safety, local economy, employment, environmental protection, defense, public lands, and transportation. The display of this question is shown in Figure 1.

Selecting spending categories presents a challenge, as an inherent trade-off exists between

[10] All hypotheses and analyses are stated in a pre-registered analysis plan.

**Figure 1: Issue Ranking Question**




respondent cognitive burden and external validity. Presenting respondents with a comprehensive range of spending possibilities would overload respondents' ability to prioritize, leading to satisficing (Berinsky, Margolis and Sances 2014). However, the range of spending categories must be sufficiently broad to make meaningful observations about variation in demand for different types of spending. As such, my selection of spending categories is designed to accomplish two goals. First, spending categories reflect common targets of congressional distributive politics. I rely on Cassella, Fagan and Theriault (2022), who explore congressionally directed spending in FY 2021, to create a list of the common spending categories. Second, considering the central role of partisanship in American politics, I include spending categories that reflect variation in partisan priorities. Tapping into the literature on party issue ownership (Egan 2013; Fagan 2021), I include three Democratic owned issues (education, healthcare, and environment), three Republican owned issues (public safety, defense, and local economy), and three neutral issues (transportation, em-

ployment, and public lands). The nine spending categories do not constitute a comprehensive list of federal spending, but they cover an externally important range of categories and provide a feasible list for respondents to prioritize. I also include an example of a project for each spending category, ensuring respondents form similar conceptions of the spending categories.

Asking respondents to rank their spending priorities offers an intuitive measure (1-9) of relative demand for different types of spending. However, this measurement strategy likely masks underlying variation in spending demand. For instance, the ranking measure is unable to determine whether a respondent prioritizes a single spending category and has little demand for the eight additional categories. A more precise measurement strategy is needed to gauge the intensity of demand for the nine spending categories, and I create two alternative measures of spending demand to achieve this purpose. First, using the same spending categories and descriptions as the ranking measure, I ask respondents to rate each type of spending based on need in their community on a 0-100 scale. This question provides a more precise measure of spending demand than the ranking question, as it allows respondents to specify how much more they prioritize certain projects relative to other projects. In other words, the 0-100 rating measure operationalizes absolute spending demand intensity for each category rather than relative spending demand between categories. However, this measure fails to capture the trade-off nature of budgetary politics, where policymakers are working with limited resources (Adolph, Breunig and Koski 2020). Funding for one project means less funding for other projects, and the rating measure of spending demand ignores such trade-offs.

Second, I ask respondents to play a resource allocation game, where they are tasked with allocating $10 million towards different categories of spending in their community. The order of the spending categories listed in the resource allocation game matches respondents' previous ranking, such that respondents can work their way down their list of spending priorities while allocating money. Similar resource allocation designs have been used in political science research to measure preferences and priorities in various political contexts (White, Laird and Allen 2014; Touchton and Wampler 2023). This strategy emphasizes the trade-off nature of public budgeting, yet it also offers a precise measure of spending demand. The measure for each project type ranges between

**Figure 2: Experimental Vignette**

*Please read the following news story about your representative in the United States Senate. After which, you will be asked to evaluate the Senator.*

**Senator NAME Secures $8,000,000 for PROJECT in STATE**

Senator NAME announced $8,000,000 in secured federal funding for the STATE SPENDING TYPE Initiative. This project will support upgrades to PROJECT in STATE. "By investing in PROJECT, we can improve the lives of constituents and contribute to the health of local communities," said Senator NAME. "This project addresses the needs of STATE, and I am proud to have championed – and delivered – this favorable investment for my state."

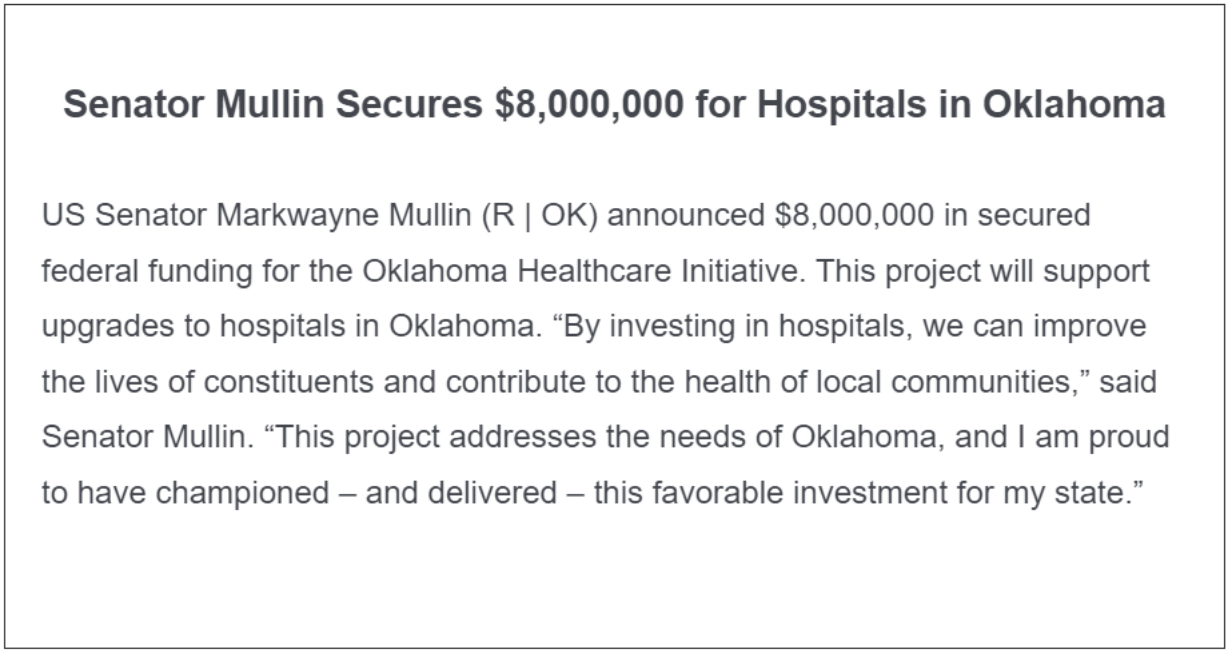
$0 and $10 million for each respondent. In sum, I develop three measures of constituent demand for categories of federal spending, each highlighting different dimensions of spending preferences.

Later in the survey, I embed an experimental vignette featuring one of the respondents' US senators claiming credit for a federally funded district project. The credit claim language remains similar across treatment groups – the dollar amount, length of statement, and general story is constant – but the type of project is randomly varied. Each respondent is assigned to read a credit claim for one project assigned at random from the nine categories presented earlier in the survey. To ensure standardized interpretation of spending categories, the projects in the experimental vignette match the example projects from earlier in the survey. Figure 2 provides a visual outline of the experimental vignette, and Figure 3 displays an example of the experimental vignette.

After reading the credit claim, respondents are asked to evaluate the job performance of each of their US senators on the same ratings used earlier in the survey. I use the pre/post experiment change in support for the US senator in the experimental vignette as the dependent variable in the following analysis. This form of repeated measure survey design increases precision relative to post-only designs without altering treatment effects (Clifford, Sheagley and Piston 2021). The

**Figure 3: Example Vignette**

*Please read the following news story about your representative in the United States Senate. After which, you will be asked to evaluate the Senator.*



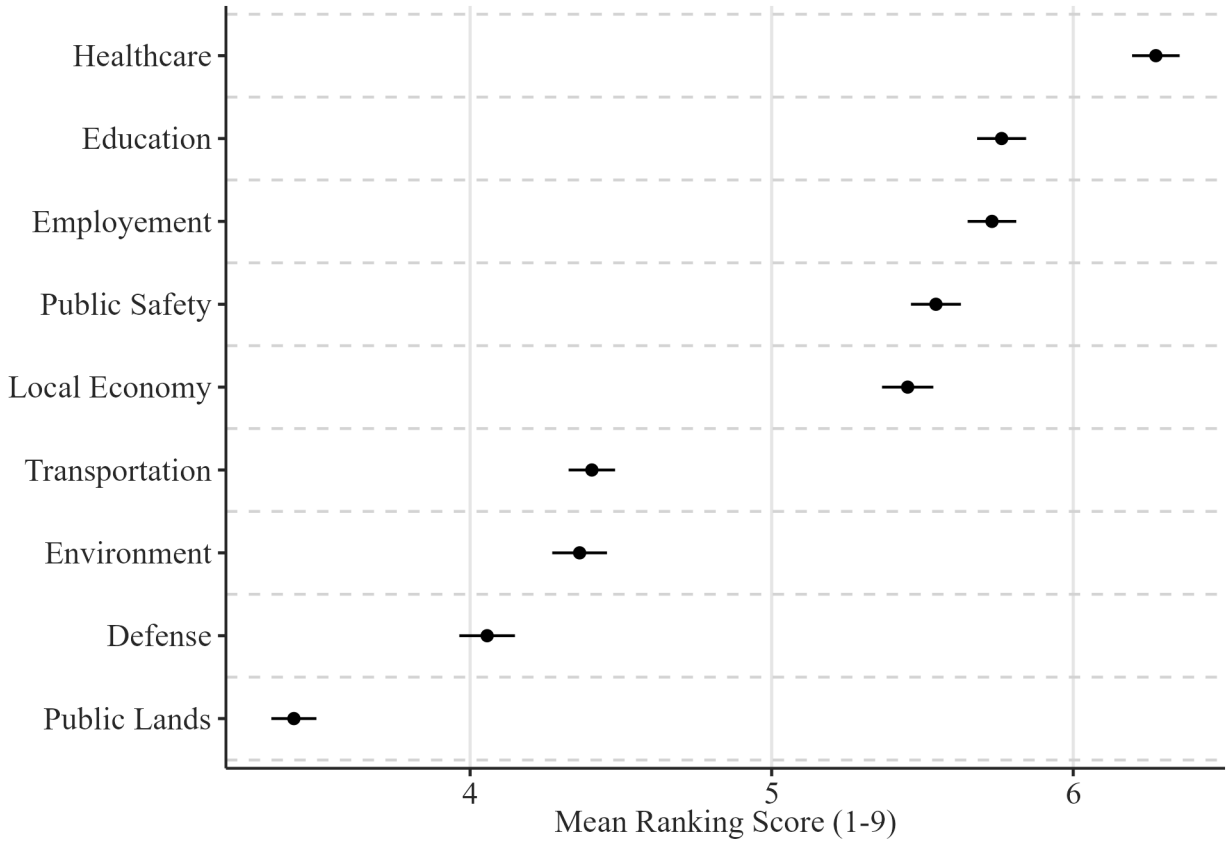

NOTE: This figure displays the healthcare spending treatment for respondents in Oklahoma.

initial questions measure baseline support for the senator, while the post-experiment questions measure support for the senator after she or he has claimed credit for bringing home a distributive project. Thus, the difference in these two measures is the pre-treatment to post-treatment change in support for the senator.

# Descriptive Findings on Public Spending Priorities

Before testing the hypotheses posed above, I describe patterns of demand for different types of district projects. For an initial measure of respondent spending demand, I use the data generated from the spending category ranking activity to calculate a mean ranking score for each spending category. Each respondent was asked to prioritize nine types of spending based on spending needs in their community. The nine spending categories include the most common targets of congressional distributive politics, as well as a balanced assortment of spending types associated with

**Figure 4: Mean Spending Category Ranking Scores**

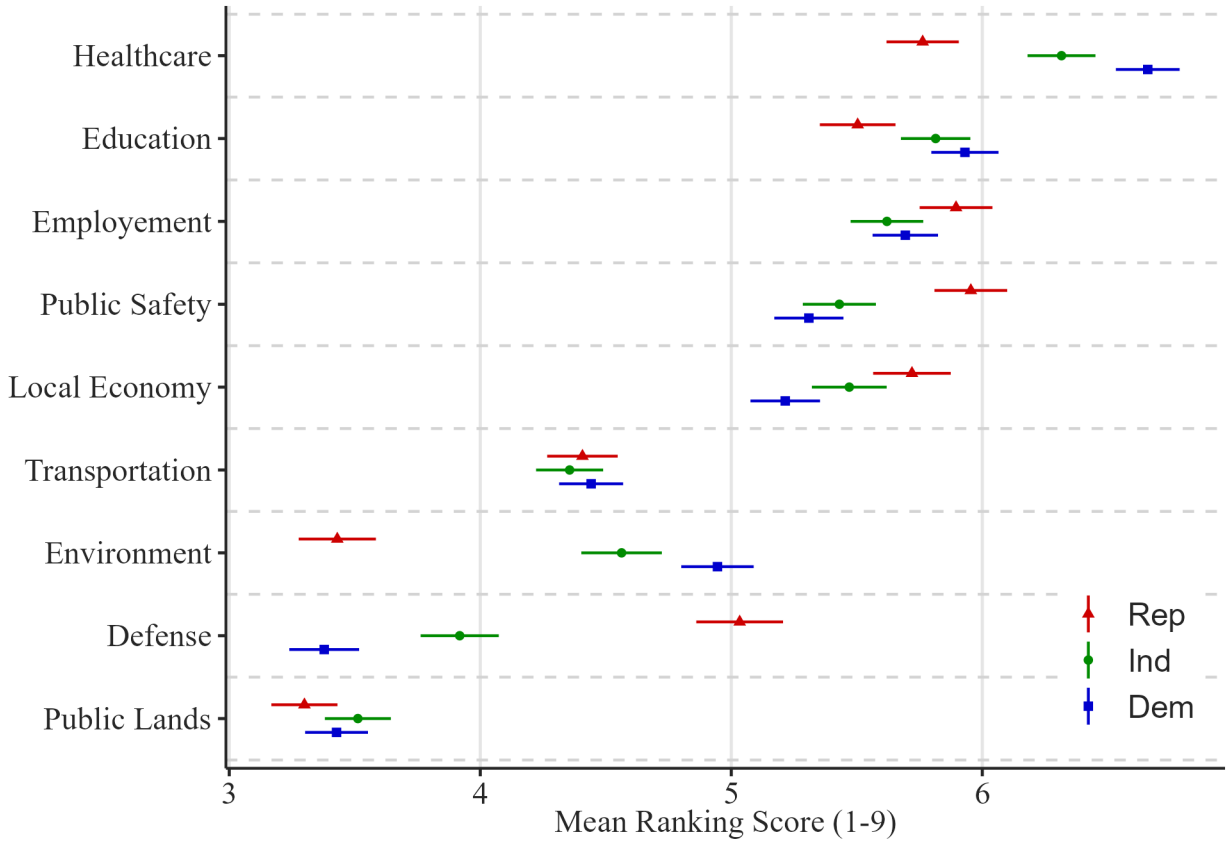



NOTE: Higher scores indicate greater spending demand.

party owned issues. As such, the nine spending categories cover an externally important range of spending types and provide a feasible list for respondents to prioritize. I calculate the mean ranking score for each spending category to operationalize the public's spending priorities.

Figure 4 displays the mean ranking score for each spending category. I reverse code the original measure, meaning higher scores indicate greater respondent demand. On average, the public prioritizes healthcare spending above all other spending categories. A substantial gap separates healthcare (6.27 mean ranking score) from the second highest rated category, education (5.76 mean ranking score). Additionally, Figure 4 shows the nine spending categories offered to respondents can be classified into above average and below average spending priorities. That is, a clear gap separates the top five spending categories – Healthcare, Education, Employment, Public Safety, and Local Economy – from the bottom four spending categories – Transportation, Environment,

**Figure 5: Mean Spending Category Ranking Scores by Party Affiliation**

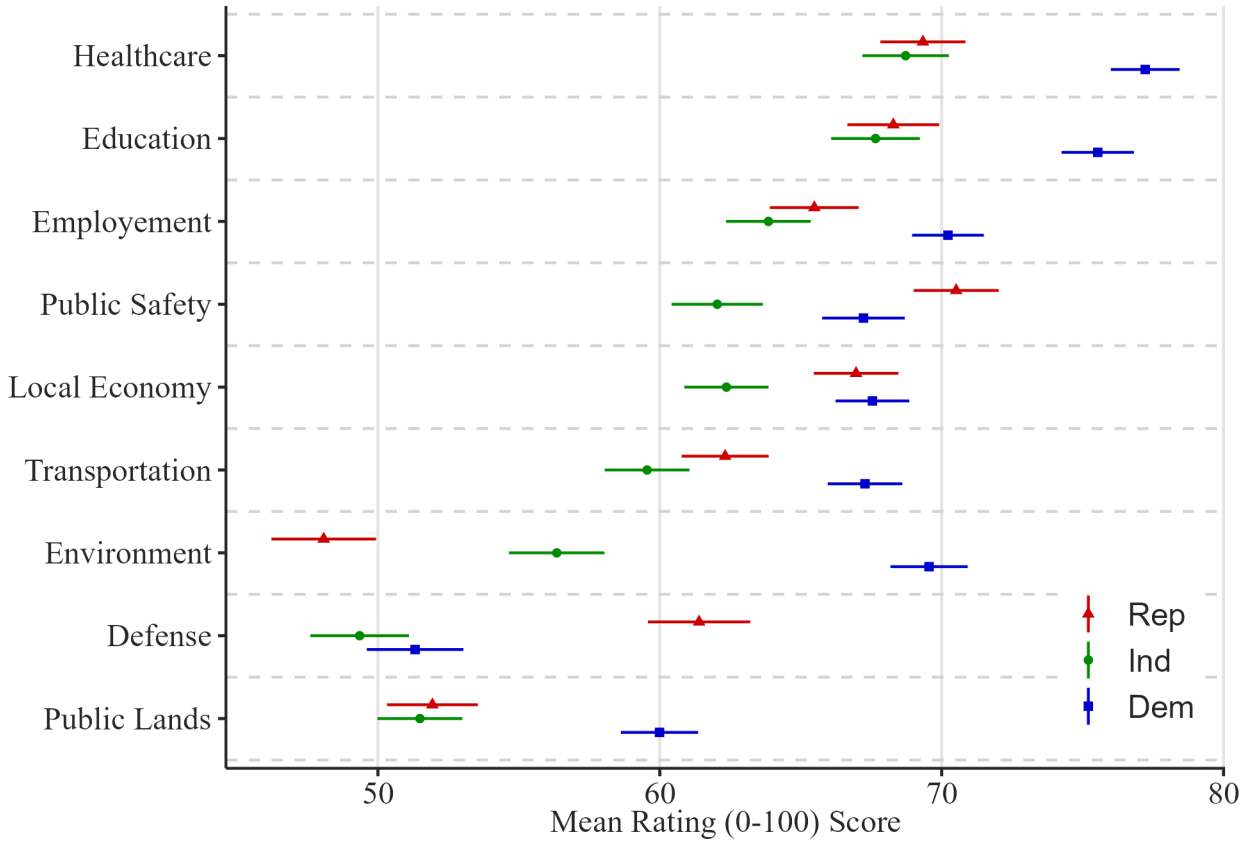



NOTE: Higher scores indicate greater spending demand.

Defense, and Public Lands.

Next, I calculate mean ranking scores for partisan subsets of respondents. Figure 5 displays the mean ranking scores for each spending category, subset by respondent party affiliation. Two notable patterns emerge from Figure 5. First, spending category prioritization does not vastly differ by party affiliation. Though the ordering slightly differs, Democrats, Republicans, and Independents share the same top five spending priorities. Some categories reveal partisan disagreement, such as defense spending and environmental spending, but partisans generally agree on which categories deserve above average prioritization. Second, Democratic respondents exhibit more consensus than Republican respondents on high priority spending categories. A clear order emerges among Democrats' highest priority spending categories, while Republicans' seem to equally prioritize their top four categories.

**Figure 6: Mean Spending Category Rating (0-100) Scores by Party Affiliation**




NOTE: Higher scores indicate greater spending demand.

The patterns in demand noted above generally persist across the ranking, rating, and allocation game measures of spending priority. However, a few additional patterns in spending demand emerge when respondents are asked to express their absolute demand for each spending category rather than their relative prioritization of spending categories. That is, the 0-100 rating measure of spending demand yields additional insights. Figure 6 displays mean ratings (0-100) for each spending category across party affiliation. Without the cap on demand intensity imposed by the ranking question, Democrats' clearly display more overall demand for spending than Republicans and Independents. On average, Democrats rate every spending category higher than Independents and seven out of the nine categories higher than Republicans. Together, Figures 5 and 6 show that Democrats and Republican are similar when it comes to relative spending category prioritization but differ on absolute levels of demand for federal spending.

I use a second survey to further validate these measures of spending demand. First, I replicate the three spending demand questions and observe the exact same patterns in demand as those discussed above. General spending preference and partisan patterns in spending preferences replicate across the two surveys.[11] Second, I leverage a conjoint experiment to explore whether respondents' spending preferences, as measured with the ranking question, predict support for proposed projects in a discrete choice scenario. Results, which are displayed in Appendix E, reveal a clearly positive relationship between respondents' earlier project ranking and their likelihood of choosing a project in the conjoint experiment. In sum, spending demand preferences replicate across different samples and measurement strategies.

# Experimental Results

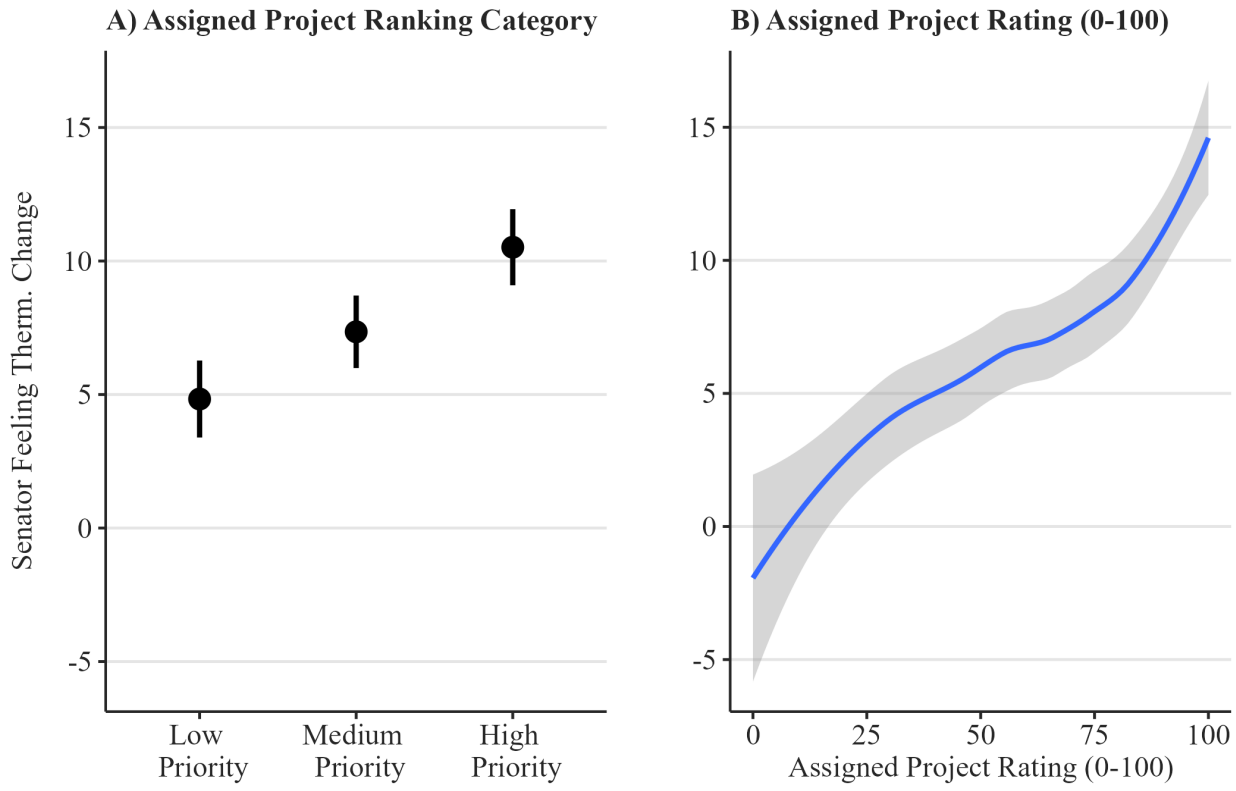
Testing the hypotheses stated above requires operationalizing the concept of alignment between constituent spending demand and the type of spending secured by their representatives in Congress. I use the spending demand measures described above and random assignment of spending type in the experimental vignette to generate the primary independent variable: *Assigned Project Priority (APP)*. Rather than the specific spending category (transportation, education, etc.), the key quantity of interest is respondents' demand for the type of spending they read about in the experimental vignette. Each respondents' APP is based on their rating of the spending category to which they were later assigned. Because respondents have an equal likelihood of being assigned to each type of spending, regardless of their earlier ranking, APP is effectively a randomly assigned treatment condition. The three measures of spending demand offer three options for each respondent's APP measure – ranking (1-9), rating (0-100), and allocation (0-10). [12]

Hypothesis 1 predicts congressional credit claiming effectiveness is a function of constituent demand for the type of spending being claimed. Claiming credit for a high priority project should

[11] See Appendix D for this analysis.

[12] As stated in the pre-registered analysis plan for this study, I use all three measures of the APP measures in the analysis below.

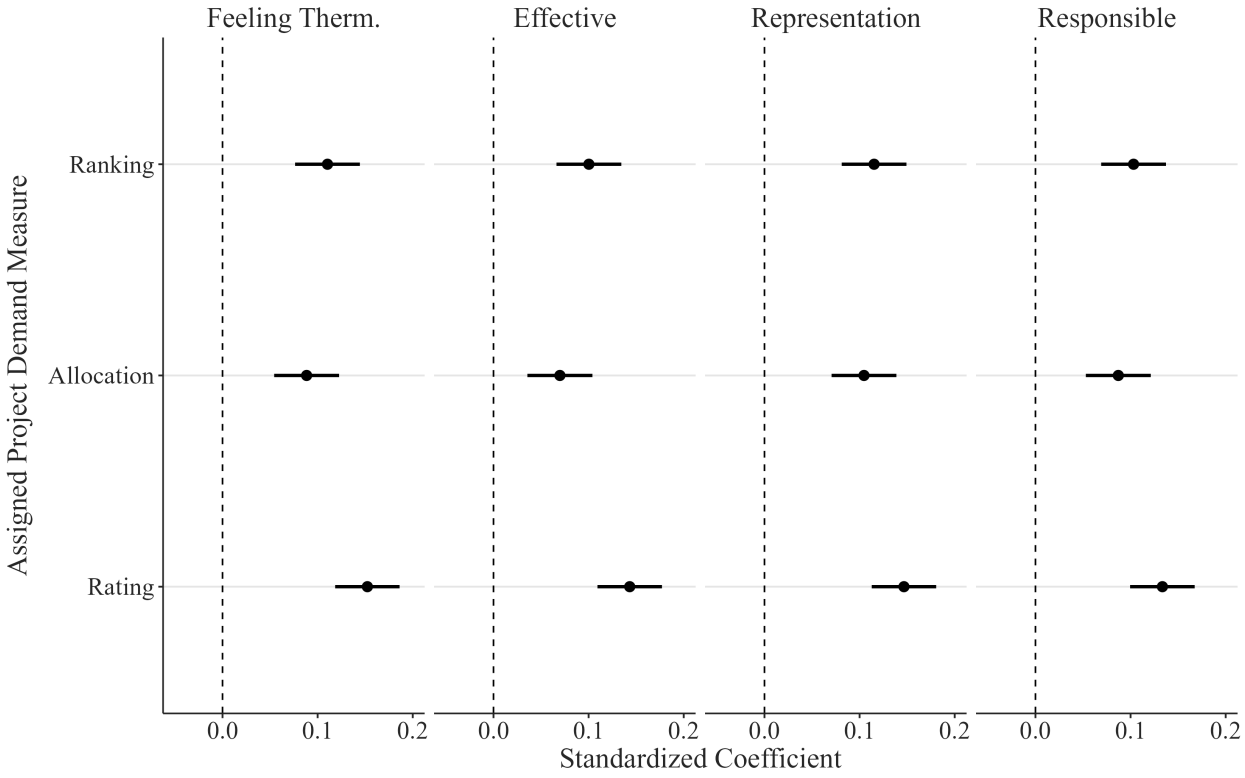
**Figure 7: Project Priority and Credit Claiming Effectiveness**




result in a greater constituent support boost for the senator than claiming credit for a low priority project. Figure 7 offers an initial look at this relationship. Panel A plots the primary dependent variable, the mean pre/post treatment change in general support for the credit claiming senator (0-100 scale), across three levels of the APP ranking measure: respondents assigned a low priority project (bottom three ranking); respondents assigned a medium priority project (middle three ranking); and respondents assigned a high priority project (top three ranking). In line with expectations, securing a high priority project leads to a greater mean increase in support (10.5 points) than securing a medium priority (7.35 points) or low priority project (4.83 points). That senators can meaningfully increase their support by securing low priority projects is somewhat surprising, but securing a high priority project leads to more than twice the boost in public support as compared to a low priority project.

Panel B in Figure 7 focuses on the rating based measure of APP, which ranges from 0-100. Using a locally weighted running line smoother (LOESS) curve with 95% confidence bands, Fig-

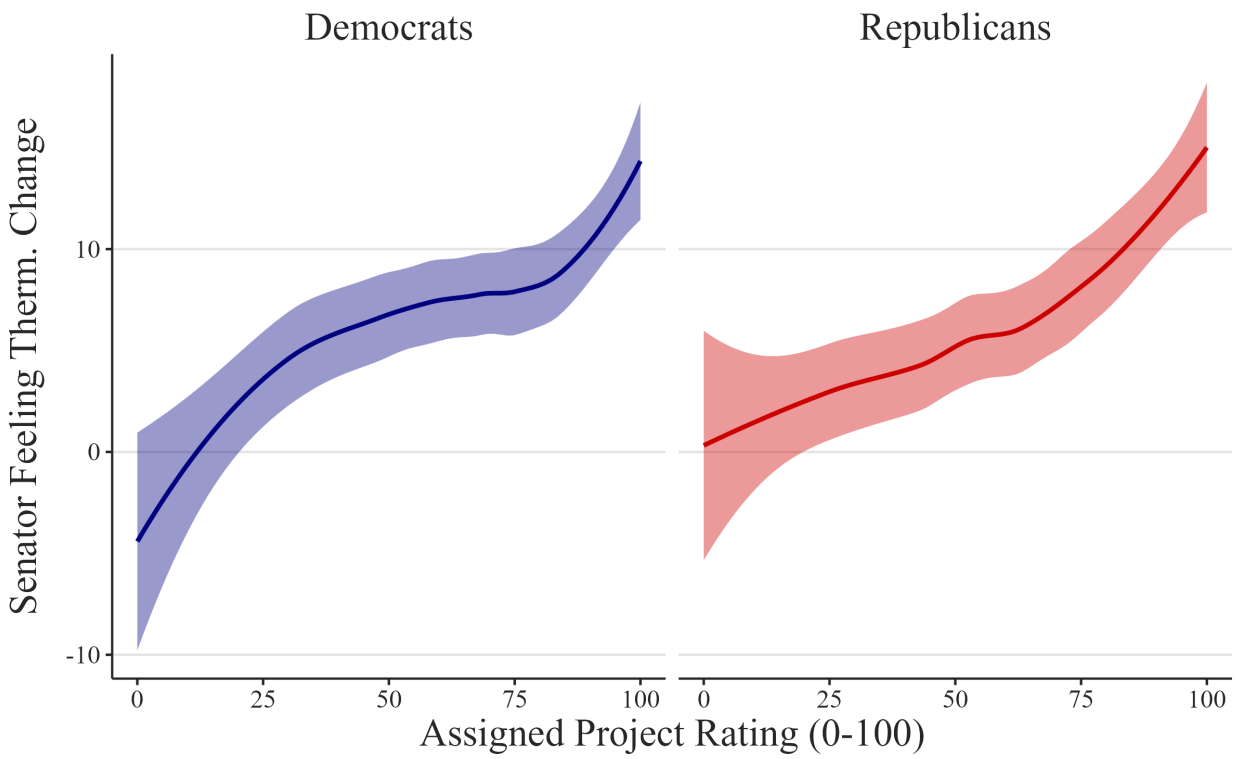
**Figure 8: Estimated Effects of Project Priority on Credit Claiming Effectiveness**




ure 7 displays the pre/post treatment change in support for the credit claiming senator across the full range of the rating APP measure. Panel B reveals a positive, substantial, and roughly linear relationship between the two variables. Credit claiming for projects with very low priority ratings does not lead to an increase in support for the credit claiming senator, while credit claiming for the highest rated projects leads to a nearly 15-point increase in senator approval. As such, both panels in Figure 7 support the prediction of Hypothesis 1. As constituent demand for a certain spending category increases, senators receive a greater boost in public opinion for claiming credit for that type of spending.

Respondents were asked to rate their senators on four quantities both before and after the experimental vignette: feeling thermometer, effectiveness in Congress, representation, and fiscal responsibility. These four pre/post rating differences cover different dimensions of credit claiming effectiveness. To test Hypothesis 1 more comprehensively, I estimate twelve separate regression models, one for each combination of the APP measure and the credit claiming effectiveness mea-

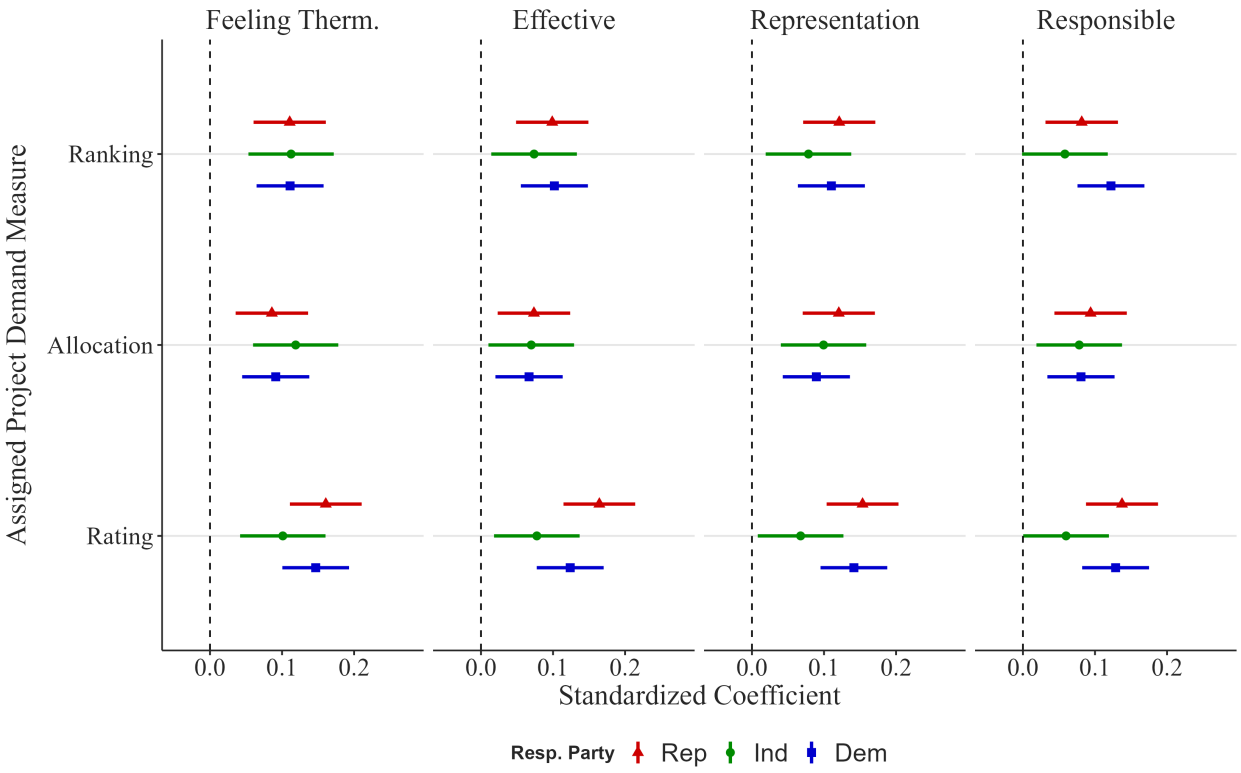
**Figure 9: Project Priority and Credit Claiming Effectiveness, by Party**




sure. Figure 8 displays the standardized regression coefficients with 95% confidence intervals for all twelve models. Standardized regression coefficients are useful in this case, as they allow for the comparison between models with different independent and dependent variable scales. The results are remarkably similar across the twelve regression models. Regardless of operationalization strategy, the APP variable has a significant, substantial, and robust effect on the effectiveness of credit claiming. In sum, Figure 7 reveals spending demand to have a substantively meaningful effect size on credit claiming effectiveness, and Figure 8 displays the robustness of this effect across various measurement strategies.

Hypothesis 2 predicts that the experimental treatment effect will differ based on respondent party. All respondents are expected to reward high priority projects more than low priority projects, but the spending demand effect is predicted to be stronger among Republicans than Democrats. For an initial exploration of this claim, I subset survey responses by respondent party affiliation and replicate the above analysis. Figure 9, using locally weighted running line smoother (LOESS)

**Figure 10: Estimated Effects of Project Priority on Credit Claiming Effectiveness, by Party**




curves with 95% confidence bands, replicates the rating-based APP figure in Panel B of Figure 7 with Democratic and Republican respondent subsets. The slopes of the two LOESS curves do not appear to meaningfully differ. Across the range of the rating APP variable, Democrats and Republicans similarly reward their senator for credit claiming. As such, the results shown in Figure 9 do not support the prediction of Hypothesis 2.

I further evaluate Hypothesis 2 by re-estimating the regression models described above on partisan subsets of respondents. Figure 10 displays the standardized coefficients with 95% confidence intervals resulting from these estimations. Across the twelve independent variable and dependent variable combinations, the effect of APP on credit claiming effectiveness is not meaningfully higher among Republican respondents than Democratic respondents. Rather, the effects are quite consistent across respondent party affiliation. Figure 10 suggests party affiliation does not condition the effect of APP on credit claiming effectiveness. To formally test this proposition, I include a Respondent Party x APP interaction term in the regression models. I find no evidence

of a significant or substantial interaction effect.[13] As such, my findings suggest a rejection of Hypothesis 2. The relationship between spending demand and increased legislator favorability is not greater among Republican constituents than Democratic constituents. Higher constituent spending demand similarly enhances the effectiveness of congressional credit claiming for Republicans, Democrats, and Independents.

## Experiment #2: Impressions of Influence and Understanding

Extant studies emphasize the role of credit claiming in cultivating an *impression of influence* (Grimmer, Westwood and Messing 2014), wherein legislators display a capacity to secure federal funds for constituents. The findings presented above reveal a separate dimension of congressional credit claiming by showing that constituent spending demand shapes the effectiveness of congressional claiming credit for distributive benefits. Legislators who are able to recognize and match their constituents' spending demand stand to gain the most from credit claiming. These findings provide initial support for the argument that credit claiming allows legislators to develop an *impression of understanding*, wherein legislators show an awareness of, and ability to meet, constituent needs.

Disentangling the impression of influence and the impression of understanding requires an additional research design, as the above design varies the target of spending but holds influence constant. I field a second survey experiment for this purpose, recruiting 1,500 new respondents through the Lucid Theorem platform. The structure and design of this experiment generally match that of the first experiment: pre-experiment senator ratings, the three measures of respondent spending priorities, a credit claiming experiment vignette, and post-experiment senator ratings. However, rather than randomly assigning respondents to one credit claim target out of the list of nine spending categories, I leverage a 2x2 experimental design that varies both spending demand and senator influence.

[13] See Tables F17-F20 in the Appendix for the tabular results of these regressions.

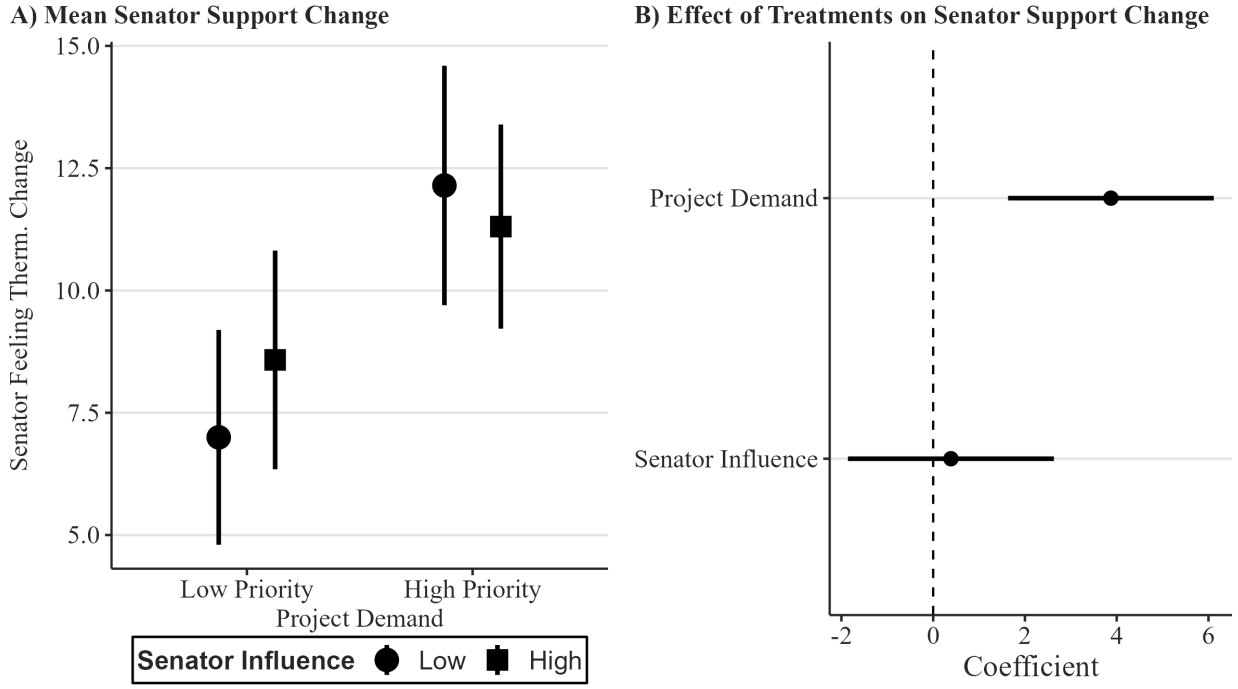
For spending demand, each respondent is assigned to read a credit claim for either their top priority spending category or their sixth (out of nine) priority spending category. That is, respondents have an equal chance of reading about a highly prioritized spending project or a below average spending project, per their rankings. The treatment language of this component generally matches that of the first experiment. However, I add a final paragraph to the news story vignette that varies the credit claiming senator's overall influence. In the high influence condition, the paragraph reads, "In total, Senator {Last Name} secured close to $500 million in funding for projects in {State} in the most recent appropriations legislation, ranking in the top 3 out of 100 US senators." In the low influence condition, the paragraph reads, "In total, Senator {Last Name} secured close to $100 million in funding for projects in {State} in the most recent appropriations legislation, ranking slightly below average (66th out of 100 US senators)." In sum, respondents were assigned to one of four treatment conditions: High Priority + High Influence, High Priority + Low Influence, Low Priority + High Influence, and Low Priority + Low Influence.

The influence treatment is designed to trigger respondents' impression of influence. The $100 million and $500 funding numbers are based on senators' credit claims for congressionally directed spending in the FY2022, FY2023, and FY2024 appropriations packages. However, the total funding numbers alone are unlikely to trigger impressions of influence. Grimmer et al. (2014) show that constituents are generally unresponsive to the differences in the amount of dollars secured. Alternatively, Gerber et al. (2022) show that respondents reward additional funding when they are given a point of reference to evaluate their legislators' credit claiming. The impression of influence, therefore, is more about relative performance than absolute performance. By specifying the senator as a top three performer or a below average performer (66th out of 100 senators), the treatment is designed to explicitly signal the senator's relative influence on expenditures and trigger perceptions of influence.

Panel A in Figure 11 displays the mean pre/post treatment change in support for the credit claiming senator (0-100 scale) across the four vignette treatment conditions. Consistent with the findings above, constituent spending demand clearly influences the effectiveness of the credit

claim. Claiming credit for a high priority project leads to a greater increase in support than claiming credit for a low priority project. This effect is further clarified in Panel B in Figure 11, which plots the average treatment effect (ATE) of the spending demand treatment and the senator influence treatment. I use OLS for this estimation. On average, spending demand has a meaningfully positive effect on support for the senator. The ATE of a high priority project, vis-à-vis a below average priority project, is 3.87 ($p$-value $< 0.01$). Meaning, senators receive a 3.87 greater boost in feeling thermometer ratings (0-100) when credit claiming for a high priority project as compared to a below-average priority project.

**Figure 11: Effect of Vignette Treatments on Senator Support**




Conversely, the experimental variation in senator influence does not appear to influence the effectiveness of the credit claim at increasing support for the senator. The ATE of high senator influence, vis-à-vis below average senator influence, is 0.39 (p = 0.73). Securing more overall funding, in terms of absolute dollar amount and performance relative to other senators, does not lead to higher support for the credit claiming senator.

Together, these findings offer support for the theory of targeted distributive politics outlined above. When varied in an experimental design, the effectiveness of congressional credit claiming

is enhanced by senators' ability to secure the right distributive benefits rather than the most distributive benefits. The frame for this experimental design is investigating the impression of influence and the impression of understanding as complimentary but distinct aspects of congressional credit claiming. To the extent that the experimental design captures these concepts, the latter appears to be more central than the former in explaining patterns of credit claiming effectiveness.

Importantly, these findings in no way dispel the impression of influence as a central component of credit claiming. Rather, they offer support for the impression of understanding as an additional component of congressional credit claiming. Grimmer et al. (2014) recognize that differences in demand for various types of spending influence credit claiming effectiveness (97-103), but the thrust of their argument is that credit claiming allows members of Congress to enhance their support amongst constituents by way of appearing influential over government expenditures. By developing and exploring the impression of understanding, this study makes the case that constituent demand for different types of spending plays a central role in distributive politics and credit claiming. In addition to making legislators appear more influential and effective in Congress, credit claiming helps legislators show that they recognize the unique needs of their constituents.

# Conclusion

Distributive politics has long served as a foundation of the literature on Congress, representation, and American Politics. I build on this foundation to further clarify the consequences of federal spending decisions on public support for members of Congress. I theorize that securing the right distributive benefits is more important for building electoral capital than securing the most distributive benefits, and I test this claim through two survey experiments.

First, I explore variation in demand for different categories of federal spending. I show that Democrats and Republican are similar on relative demand for different categories of spending, prioritizing the same categories, but differ on absolute levels of demand for federal spending. I then find strong evidence for the theory of targeted distributive politics. Senators glean substantially

more public opinion benefits from securing high priority projects than medium or low priority projects. Respondent party affiliation does not condition these effects – the positive relationship between spending demand and senator approval remains constant across respondent party affiliation. I further test the theory of targeted distributive politics in the second survey experiment by varying spending demand and senator influence. Again, I find that spending demand meaningfully shapes the effectiveness of credit claiming. Conversely, the experimental variation in senator influence does not influence the effectiveness of the credit claim at increasing support for the senator.

This study makes two contributions to the literature on congressional representation. First, it offers a more comprehensive account of the politics around congressional spending by introducing and testing a targeted theory of distributive politics. I reveal interesting variation in constituent demand for different types of federal spending and find that legislators' ability to meet this demand shapes the effectiveness of their credit claims. Second, this study fills a gap between the literature, which suggests only Democrats stand to benefit from distributive politics, and current developments in Congress, where many Republicans actively seek out and claim credit for federal spending. I show that Republican constituents have an overall lower appetite for federal spending, but they equal Democratic constituents in their willingness to reward legislators for bringing home high priority spending projects.

These findings also carry practical implications for the US Congress. A critical function of distributive benefits is that they may be used to facilitate lawmaking by "greasing the wheels" of Congress (Evans 2004). However, for spending projects to engender lawmaking and reduce congressional gridlock, members of Congress must believe their constituents desire such projects. By examining the effects of distributive benefits on legislator support, this project sheds light on distributive spending's usefulness as a tool for legislative coalition building.

# *Chapter 4: More Money, Less Credit?*

# *Legislator Gender and Credit Claiming Effectiveness*

This study is published in *Politics & Gender* (DOI: 10.1017/S1743923X23000582)

**Abstract**

Bringing home federal spending projects to the district is a common reelection strategy for members of the U.S. Congress, and congresswomen tend to outperform congressmen in securing district spending. However, for legislators to turn distributive benefits into higher approval and electoral rewards, constituents must recognize that public spending has taken place in their community and attribute credit to the correct public official. I theorize that congresswomen face a gender bias when claiming credit for federal projects, and I test this theory through an online survey experiment. Contrary to expectations, I find no evidence that legislator gender influences the public's reaction to congressional credit claims, indicating that congresswomen can effectively use distributive politics to counter gendered vulnerability in the U.S. Congress. This research advances the literature on gender and politics by investigating whether a gender bias in credit claiming prevents congresswomen from turning their representational efforts into electoral capital.

# Introduction

A key advantage that incumbents in the U.S. Congress hold over electoral challengers is the ability to bring home tangible benefits to constituents. Members of Congress (MCs) use the power of the purse to secure distributive benefits in their districts, and they claim credit for such projects in order to build a personal vote and ensure reelection (Cain, Ferejohn and Fiorina 1987; Fiorina 1981; Mayhew 1974). Critics negatively refer to this practice to as pork barrel spending, but distributive politics provides a venue for productive and meaningful representation and can be used to facilitate congressional lawmaking (Evans 2004; Frisch and Kelly 2015; Lazarus and Reilly 2010). When it comes to bringing home spending projects, congresswomen substantially outperform congressmen. A U.S. congressional district stands to receive more federal funding when represented by a woman (Anzia and Berry 2011; Lazarus and Steigerwalt 2018). However, voters are unlikely to attribute new spending in their community to the right public official, so credit claiming for spending projects is necessary for MCs to turn legislative output into electoral capital (Grimmer, Messing and Westwood 2012). Gender stereotypes influence how voters interpret new information about politicians (Ditonto 2017; Dittmar 2015), and women in politics must navigate a complex campaign messaging environment due to these stereotypes (Bauer and Santia 2022, 2023). An important question, therefore, is whether congresswomen face a gendered challenge in claiming credit for the distributive benefits that they bring home to their constituencies.

This article explores the role of legislator gender in credit claiming for distributive benefits. I theorize that congresswomen face a gender bias when claiming credit for federal projects through two mechanisms. Congresswomen's credit claims are potentially less effective at boosting support than those of congressmen because of gender stereotypes, and congresswomen may be especially vulnerable to attacks that paint the spending as fiscally irresponsible. I use an online survey experiment to test these expectations. The survey experiment features a newspaper excerpt about a fictitious member of Congress. The legislator's gender and the excerpt content are randomly assigned. The content treatments include a nonconsequential announcement (control group), a credit claim, and a credit claim with a critique of the spending as wasteful. After reading the assigned

newspaper except, respondents evaluated the fictitious legislator on a number of characteristics in a post-treatment survey.

Similar to previous research on credit claiming (Grimmer, Westwood and Messing 2014), I find that claiming credit for a federal spending project increases public support for legislators. However, contrary to expectations, I find no evidence that legislator gender plays a substantial role in the credit-claiming process. Credit claiming had an equally positive effect for the woman and man legislator on respondent ratings of general support and effectiveness in Congress. Additionally, a critique of the spending project as wasteful did neutralize the positive effects of credit claiming, but I find no legislator gender-based difference in this effect. The woman legislator and man legislator were rated equally on fiscal responsibility across treatment groups. In sum, I replicate previous findings that credit claiming for spending projects meaningfully influences support for legislators, and I do not find evidence that congresswomen face a tougher credit-claiming environment than congressmen.

Facing gendered vulnerability in congressional elections, congresswomen tend to outperform their male colleagues on representational tasks, including distributive politics (Lazarus and Steigerwalt 2018). This article advances the literature on gender and politics by investigating whether a gender bias in credit claiming prevents congresswomen from using their representational advantage to boost their chances of reelection. I find no evidence that legislator gender influences the public's reaction to congressional credit claims, indicating that congresswomen can effectively use a distributive politics strategy to counter gendered vulnerability in the U.S. Congress.

## Gender, Distributive Politics, and Congressional Elections

Women continue to be substantially underrepresented in the U.S. Congress (CAWP 2023), but the specific role of gender stereotypes and bias in congressional elections is complex. On one hand, women candidates tend to win elections at the same rate as men candidates and appear to be less hampered by gender stereotypes than previously thought (Dolan 2014; Hayes and Lawless 2015;

Schwarz and Coppock 2022; Seltzer, Newman and Leighton 1997). On the other hand, women remain less likely to be recruited for office than men (Sanbonmatsu 2010), are more likely to face electoral challenges (Lawless and Pearson 2008), and must work harder for campaign donations (Jenkins 2007). Therefore, women are less likely to run for office than men (Lawless and Fox 2010), and women who do run tend to have more political experience and qualifications than men who run (Fulton 2012; Pearson and McGhee 2013). Findings of gender neutrality in congressional elections, therefore, mask an underlying gender bias, as women win at the same rate as men even though they come from a higher quality candidate pool (Fulton 2012).

The gender gap in candidate quality appears to lead to a similar gap in legislative performance (Anzia and Berry 2011; Lazarus and Steigerwalt 2018; Volden, Wiseman and Wittmer 2013). Particularly relevant to this research, congresswomen secure more federal funding in their congressional districts than congressmen. Anzia and Berry (2011) find that congresswomen secure around 9% more discretionary district spending than congressmen. In a study on specific spending programs, Lazarus and Steigerwalt (2018) find that congresswomen secured more congressional earmarks and 2009 American Recovery and Reinvestment Act dollars than congressmen. These findings relate to the literature on congressional representation and elections, as district funding is theorized to play a pivotal role in congressional elections by offering incumbents an opportunity to build support among constituents.

Foundational accounts of congressional behavior place the geographic distribution of public resources at the center of legislator reelection strategy (Fenno 1978; Mayhew 1974). Legislators pursue federal funding for projects in their district to build a personal vote and display their influence in Congress to constituents (Cain, Ferejohn and Fiorina 1987; Fiorina 1981). Electorally vulnerable MCs, who have the most to gain from building a personal vote, are particularly keen on securing federal spending in their districts (Ashworth and Mesquita 2006; Lazarus and Reilly 2010). Additionally, states receive more federal funding when they have a senator up for reelection (Shepsle et al. 2009).

Women legislators' advantage on district spending plausibly stems, in part, from the gender-

based electoral challenges. Lazarus and Steigerwalt (2018) compellingly argue that women legislators face gendered vulnerability in elections, causing them to outperform men legislators on a number of constituent-oriented activities. Therefore, securing additional district funding is a strategy that women legislators employ to overcome gendered vulnerability in congressional elections.

However, voters are generally unable to recognize federal spending for projects in their community and attribute credit for such funding to the correct congressional representative (Stein and Bickers 1994). Consequentially, legislators must use messaging tactics, such as press releases and ribbon-cutting ceremonies, to forge connections in constituents' minds between legislative actions and spending projects. Credit claiming for spending projects is necessary for MCs to turn legislative output into electoral capital (Arnold 1990; Grimmer, Messing and Westwood 2012; Grimmer, Westwood and Messing 2014; Mayhew 1974). A legislator stands to electorally benefit from securing distributive benefits only if constituents attribute such benefits to the legislator's work in Congress.

The central question underlying this research is whether a gender bias influences credit attribution for spending projects, thereby precluding congresswomen's advantage at securing federal funds from resulting in an electoral boost. Do women legislators who secure additional district spending to overcome gendered vulnerability in congressional elections face a gender-based barrier in claiming credit for the funding they secure?

# Theory

Credit claiming is an essential component of a distributive politics-based reelection strategy, and I theorize that women legislators face a gender bias in how people react to credit claiming and attribute credit for distributive benefits. I posit two mechanisms through which a credit-claiming gender bias might function.

First, credit attribution potentially occurs through a gendered lens, with women legislators facing a more severe challenge in claiming credit for federal projects than men legislators. Credit-

claiming efforts generally aim to generate the impression that the legislator is influential in Washington and uses such influence to positively impact constituents' lives. Gender stereotypes impact the ways in which voters interpret new information about candidates (Ditonto 2017). Women politicians are stereotyped as having less competence and leadership than men politicians (Dittmar 2015; Meeks 2012; Schneider and Bos 2014), which plausibly harms congresswomen's ability to claim credit for spending projects.

Gender stereotypes play a particularly meaningful role in candidate messaging and communication strategies. Stereotypes around gender lead voters to inherently associate masculine traits with political leadership (Bauer 2020). Women politicians can emphasize masculine traits to mitigate gender stereotypes on expertise, competence, and leadership, but doing so risks a likability backlash (Bauer 2017; Wang, Merolla and Manganiello 2023). Therefore, female candidates must vary their messaging strategies to balance masculine and feminine stereotypes (Carpinella and Bauer 2021; Bauer and Santia 2022, 2023).

Legislators employ credit claiming as a strategy to enhance perceptions of their competence, leadership, and conviction in the eyes of constituents (Grimmer, Westwood and Messing 2014; Mayhew 1974). Credit claiming tends to include language on how the legislator "fought to secure federal funding"[14] and "pushed hard to protect critical programs."[15] As such, credit claiming intersects with the gender stereotypes discussed earlier. Dolan and Kropf (2004) find that congresswomen credit claim more than congressmen, suggesting that congresswomen use credit claiming as a strategy dispel gender stereotypes. However, a key component of credit claiming is the credibility of claims in the eyes of constituents (Mayhew 1974). I argue that the stereotypes congresswomen aim to dispel through credit claiming plausibly reduce the impact of their credit claims. Women plausibly face a credit-claiming bind in which gender stereotypes limit the effectiveness of messages designed in part to counter such stereotypes.

Constituents potentially attribute credit for federal funding to congressmen at a higher rate than congresswomen because of gender stereotypes, preventing women in Congress from capitalizing

[14]Source: https://gomez.house.gov/news/documentsingle.aspx?DocumentID=2728
[15]Source: https://peltola.house.gov/news/documentsingle.aspx?DocumentID=116

on their district funding advantage. This proposed mechanism results in my first hypothesis.

**Hypothesis 1:** *The public attributes less credit to congresswomen compared to congressmen for the same federal spending projects.*

Second, the potential downside of claiming credit for federal spending projects might be more severe for congresswomen than congressmen. Credit claiming can become a political liability when the spending is framed as irresponsible or wasteful (Barron and McLaughlin 2024; Grimmer, Westwood and Messing 2014), and congresswomen may be more vulnerable to such attacks than congressmen. Attacks on congressional spending are common, particularly since the return of earmarks in 2021. Women politicians are perceived as being more liberal than men politicians (Dolan and Kropf 2004; Koch 2002; McDermott 1997), and wasteful spending critiques of congresswomen's credit claims may tap into this stereotype.

Additionally, women candidates are particularly vulnerable to stereotype-based attacks on the campaign trail (Bauer 2015; Cassese and Holman 2018), meaning that wasteful spending attacks may be especially harmful to congresswomen's electorally motivated credit claims. A gendered backlash against credit claiming would be critical from a congressional elections standpoint. Swing-district MCs, representing politically heterogeneous districts, tend to use a credit-claiming-focused homestyle more than MCs in safe districts (Ashworth and Mesquita 2006; Grimmer 2013). If women claiming credit makes them seem more liberal than men claiming credit, then credit claiming fails to achieve its strategic purpose for congresswomen in swing districts. Therefore, credit claiming potentially carries more downside risk for congresswomen than for congressmen.

**Hypothesis 2:** *Congresswomen's credit claims are more vulnerable to attacks on the spending as fiscally irresponsible than congressmen's credit claims.*

In sum, I ask whether congresswomen face a tougher challenge gleaning the benefits from federal spending projects than congressmen, and I posit two mechanisms through which a gender bias might influence the public's reaction to congressional credit claiming. First, people may attribute less credit to congresswomen compared to congressmen for the same spending projects. Second, congresswomen's credit claims may be particularly vulnerable to attacks on spending as wasteful

and fiscally irresponsible. Both proposed mechanisms would hurt women legislators' ability to counter gendered vulnerability with a distributive politics-based reelection strategy.

## Research Design

I test the theory outlined earlier through an online survey. Grimmer, Westwood and Messing (2014) lay the foundation for testing the effects of credit claiming with multiple experimental designs.[16] The general theme of such designs is to randomly assign respondents to a either credit-claiming treatment, in which a legislator emphasizes their role in bringing home a beneficial project, or a non-credit-claiming control treatment. Additionally, various aspects of the credit claim can be randomly assigned, such as the amount of funding claimed, the type of project claimed, or whether the credit claim is adjoined by a critique of the spending as wasteful. Following treatment, respondents are asked to evaluate the legislator on general characteristics as well as characteristics specifically related to credit claiming.

After fielding an online survey sample of roughly 2,000 respondents through the Lucid Theorem sampling platform, I build on the experimental design outlined earlier.[17] My experimental treatment features a newspaper excerpt about a fictitious member of Congress. The length and structure of the excerpt remains constant across treatment conditions, but I randomly assign respondents to one of three content treatments: a standard advertising message (control), a credit-claiming message for a spending project in a recently passed bill, and a credit-claiming message adjoined by a critique of the bill as wasteful by the nonpartisan Committee for a Responsible Federal Budget. Critically, I add a gender dimension to the design by randomly varying the gender of the legislator making the credit claim. This is accomplished using stereotypical male and female names—Matthew Anderson and Madeline Anderson—and varying the gender pronouns used in

[16] See Gerber, Patashnik and Tucker (2022) for an extension of this experimental design.

[17] Lucid samples are nationally representative across a number of demographics variables including gender, race, age, and party affiliation (Coppock and McClellan 2019). The demographic breakdown of my sample is displayed in the Appendix. I only include respondents who correctly answered a simple attention check question (see Appendix for wording).

the newspaper except.[18] Randomly varying legislator gender allows for an examination of whether gender conditions the effects of credit-claiming messages.[19] Additionally, I randomize the party of the legislator, as party likely shapes how people interpret credit claims and party stereotypes may now limit the effects of gender stereotypes (Dolan 2014; Hayes 2011).

After reading the newspaper excerpt, respondents were asked to answer a number of questions aimed at measuring the effects of the experimental treatments on legislator evaluations. First, respondents gave their general evaluation of the legislator by answering the following question: "How pleased would you be if MC NAME was your representative in Congress?" Responses were given on 7-point Likert scale, ranging from "very displeased" to "very pleased." Second, respondents evaluated the legislator's ability to pass legislation that helps their community, offering a measure of perceived effectiveness in Congress. Third, respondents rated the legislator's level of fiscal responsibility, highlighting a potential downside of credit claiming. These questions were asked with a similar 7-point Likert scale structure as the general evaluation. Together, these measures clarify the various effects of congressional credit claiming.

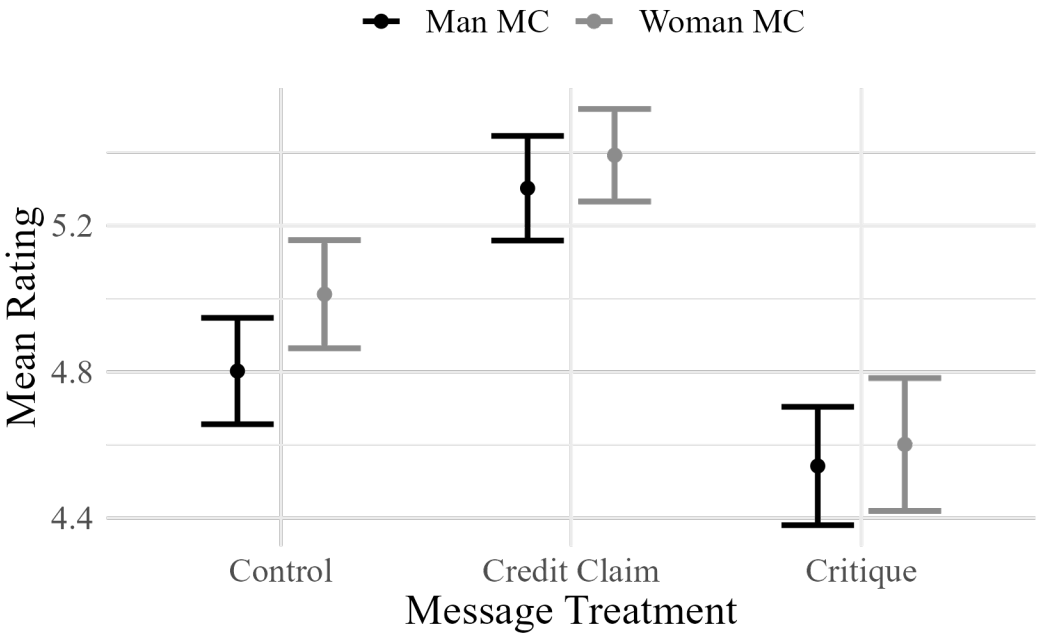
# Results

For an initial analysis of the general support measure, I plot mean support for the MC (measured on a 1–7 scale) with 95% confidence intervals across the six treatment conditions in Figure 1. Two things stand out in Figure 1. First, credit claiming for spending projects increases support for MCs, while framing the spending as wasteful eliminates the increase in MC support. These findings replicate the results of previous credit-claiming experiments (Grimmer, Westwood and Messing 2014). Second, women MCs do not appear to underperform men MCs in any of the three substantive treatment conditions. If anything, women MCs appear to slightly outperform men MCs on the general support measure.

[18]The names were chosen from a list of names analyzed by Butler and Homola (2017). Both are stereotypical white names that signal candidate gender. Further research should explore the effects of MC race on credit claiming, as the use of stereotypical white names limits the ecological validity of this research.

[19]Tables A2 and A3 in the Appendix show that the randomization achieved balance on covariates of interest.

**Figure 1: Mean MC Support Across Treatment Group**




NOTE: Figure displays mean MC support (1-7 scale) across treatment conditions. 95% confidence intervals surround point estimates.

Statistical comparisons shed further light on the findings in Figure 1. Credit claiming increases MC support relative to the control message by an average of 0.38 points (7-point scale) for the woman MC and 0.50 points for the man MC. Conversely, credit claiming with a critique of the spending decreases MC support relative to the normal credit-claiming message by an average of 0.79 points for the woman MC and 0.76 points for the man MC. Using analysis of variance (ANOVA) comparisons with Tukey's HSD (honest significant difference) corrections to adjust for multiple comparisons, all four of these effects reach statistical significance ($p$-value $< 0.05$; see Online Appendix Tables A5 and A6).

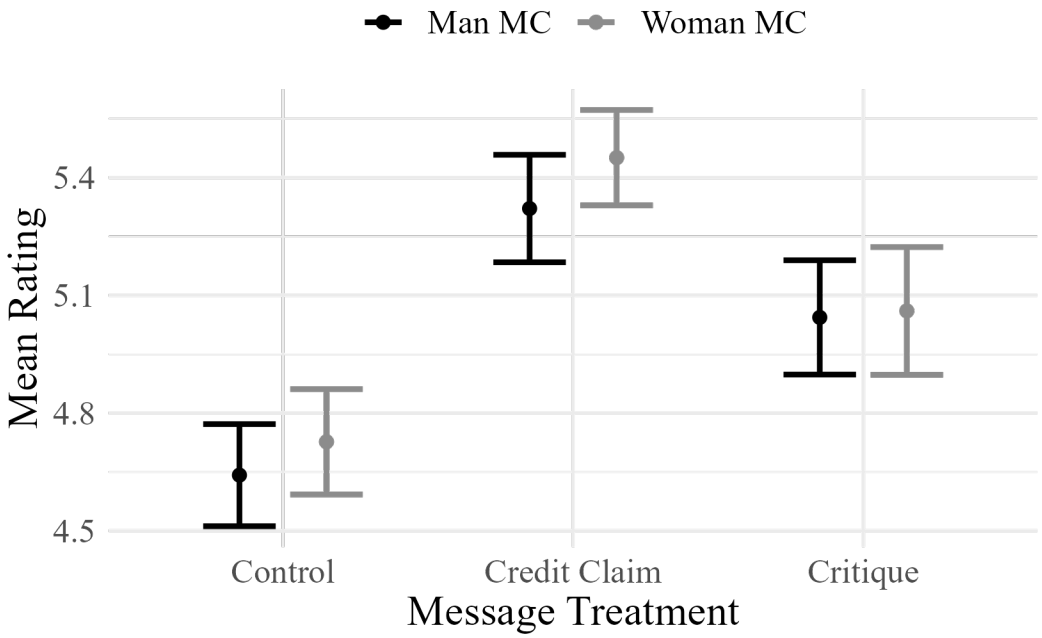
However, the man MC condition does not outperform the woman MC condition within any of the substantive treatment conditions. The only difference based on gender appears to be a slight overperformance by women in the control message condition.[20] ANOVA comparisons with Tukey corrections reveal no statistically significant MC gender-based difference in general support in any

[20] A difference of means test yields a 0.21 advantage for women over men in the control condition ($p$-value $< 0.05$) in Table A4 in the Appendix, though this effect looses statistical significance when adjusting for multiple comparisons (see Table A7 in the Appendix). I further flesh out this finding in the partisanship section of the results.

of the three substantive conditions (see Table A7 in the Appendix). The substantive treatments have a substantial impact on MC support, but legislator gender does not play a meaningful role in respondents' reactions to the newspaper except. This pattern of results holds across alternative measures of MC support—binary approve/disapprove and hypothetical vote intention (see Tables A24–A30 in the Appendix). Regardless of measure, I find no evidence of gender effects favoring men over women.

Next, I evaluate the measure of the MC's perceived ability to pass legislation that helps their community. Credit claiming is designed to increase perceptions of MCs' influence and effectiveness in Congress, so this measure represents a core element of credit-claiming success. Figure 2 displays the mean of this measure with 95% confidence intervals across gender and the two substantive treatment conditions relevant to **H1**—the control condition and the credit claim condition.

**Figure 2: Mean MC Effectiveness Across Treatment Group**




NOTE: Figure displays mean effectiveness ratings (1-7 scale) across treatment conditions. 95% confidence intervals surround point estimates.

Figure 2 shows that credit claiming substantially boosts public perceptions of MC effectiveness at passing legislation that helps their community. The woman MC's perceived effectiveness rating increases from an average of 4.73 (on a 1–7 scale) in the control condition to 5.45 in the credit

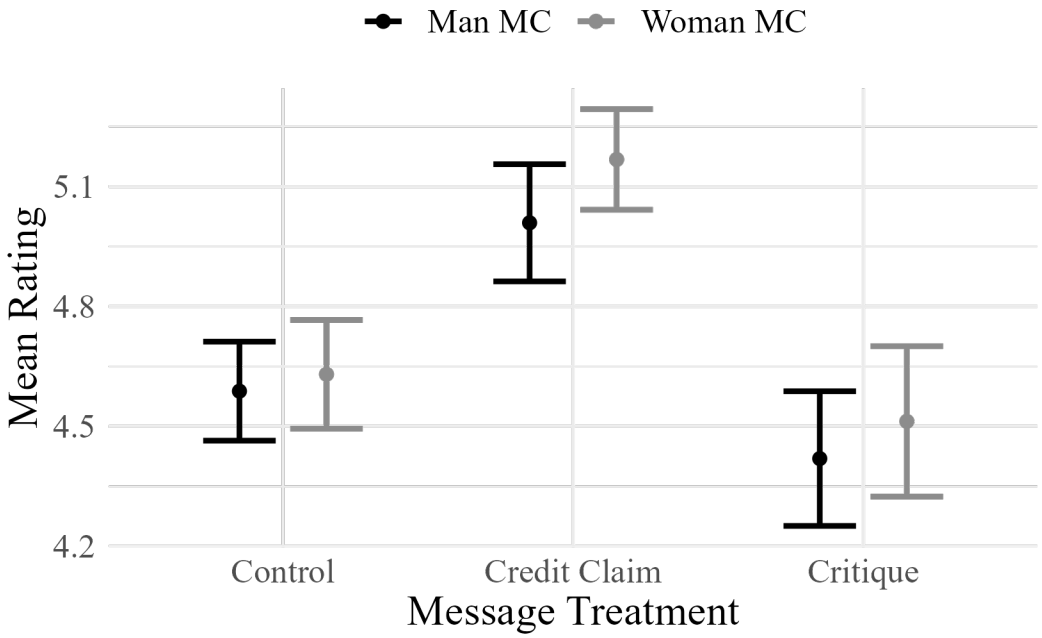
claim condition. Similarly, the man MC's perceived effectiveness rises from an average of 4.64 in the control condition to 5.32 in the credit claim condition. Thus, the credit claim boost is 0.72 points for the woman MC and 0.68 for the man MC (see Table A12 in the Appendix). Both of these differences are statistically significant ($p$-value $< 0.01$). However, legislator gender does not appear to play a meaningful role in the effects of credit claiming. The woman MC receives slightly higher effectiveness ratings, on average, than the man MC in both the control condition and the credit claim condition, though neither of these comparisons reaches statistical significance in a difference of means test (see Table A9 in the Appendix). Therefore, results stand in contrast to the expectations of **H1**.

I explore whether women legislators are particularly vulnerable to attacks on their distributive politics efforts as wasteful by analyzing respondents' perceptions of the legislator's fiscal responsibility. Figure 3 displays the mean ratings of MC fiscal responsibility across legislator gender for the credit claim condition and credit claim with a spending critique condition. The spending critique does decrease perceptions of MC fiscal responsibility. The woman MC's average fiscal responsibility rating decreases from 5.17 in the credit claim condition to 4.51 in the credit claim with a critique condition. Similarly, the man MC's average fiscal responsibility rating drops from 5.01 in the credit claim condition to 4.42 in the credit claim with a critique condition. Both of these comparisons reach statistical significance ($p$-value $< 0.01$) in an ANOVA analysis with Tukey corrections (see Table A17 in the Appendix). Spending critiques effectively dampen the benefits of credit claiming by portraying the credit-claiming MC as fiscally irresponsible.

However, contrary to **H2**, congresswomen do not appear to face a greater downside risk of credit claiming for spending projects compared to congressmen. The difference in mean fiscal responsibility ratings between the credit claim condition and the credit claim with a spending critique condition for the woman MC and the man MC—0.66 and 0.59, respectively—are very similar. Figure 3 indicates that there is not a substantively meaningful legislator gender-based difference in fiscal responsibility ratings within either the credit claim or credit claim with a spending critique condition. Further, difference of means tests confirm the lack of a statistically significant gender

effect within each of the substantive conditions (see Table A14 in the Appendix). In both the credit claim and the credit claim with a spending critique conditions, respondents did not rate the woman MC and the man MC differently on fiscal responsibility.[21] Therefore, I report no support for **H2**.

**Figure 3: Mean MC Fiscal Responsibility Across Treatment Group**




NOTE: Figure displays mean responsibility ratings (1-7 scale) across treatment conditions. 95% confidence intervals surround point estimates.

Rather than indicate the rejection of **H1** and **H2**, the null results on MC gender could instead emerge through research design or implementation issues such as lack of power, problematic measurement of concepts, or respondent inattentiveness. However, I argue that the foregoing results do, in fact, indicate a rejection of the hypotheses. While I did not conduct a pre-experiment power analysis, there are enough observations per message group (approximately 700 per message group) to distinguish even a small gender difference through difference of means testing.[22] Further, the woman MC technically outperforms the man MC in every comparison shown here, making it unlikely that a lack of power is hiding gender differences favoring the man MC.

[21] I observe the same pattern of results for the perceived MC ideology measure (see Tables A19–A23 in the Appendix). The critique message increase perception of MC liberalism, but there is no gender effect.

[22] Assuming a small effect (0.30 gender difference) for the general support measure, the difference of means tests are well powered (0.89) to distinguish this effect.

On the issue of concept measurement, the measures used to test **H1** and **H2** are established in the literature (Grimmer, Messing and Westwood 2012; Grimmer, Westwood and Messing 2014). Additionally, that I observe meaningful message treatment effects in the expected direction in all three figures helps validate the study from both a concept measurement and a respondent inattentiveness standpoint. Rather than observe null findings across the board, I replicate previous findings on message treatments. Therefore, I argue that the null findings on MC gender reflect a null relationship rather than a design or implementation issue. The message treatments lead to substantial variation in the dependent variables in the expected directions, while the gender treatment does not lead to variation in the dependent variables.

Next, I estimate ordinary least squares regressions with a *Substantive Content Treatment * MC Gender Treatment* interaction to provide a more direct test of the hypotheses. This modeling strategy tests whether substantive treatment effects are conditioned by legislator gender. In other words, the interaction examines whether the effects of the substantive treatments are different for the woman MC and the man MC. If legislator gender conditions how the public reacts to congressional credit claiming, I should observe a meaningful interaction effect.

Table 1 displays the results of this regression analysis for all three of the dependent variables discussed, with controls for respondent gender and partisan relationship to the MC.[23] In all three models, I find no evidence of an interaction effect between the substantive treatment and MC gender treatment. The interaction term coefficients are substantively small and do not come close to statistical significance. Credit claiming boosts general MC support and perceptions of the MC's ability to pass legislation that helps their community, and it does so similarly for men and women legislators. A critique of the spending as wasteful reverses the general support effects of credit claiming and decreases perceptions of MC fiscal responsibility, and there is no gender-based difference in this effect.[24]

[23]The Partisan Match variable follows the methodological strategy used in Grimmer, Westwood, and Messing (2015) and Gerber, Patashnik, and Tucker (2022) to classify a respondent/MC party match as 1, mismatch as –1, and independent respondents as 0.

[24]It is worth noting the low R2 measure of model fit in Table 1 (0.08, 0.08, and 0.07). Such measures are similar to existing credit claiming experiments—Gerber, Patashnik, and Tucker (2022) employ a similar experimental design and report an R2 of 0.04 and 0.07 in their models.

**Table 1: Regression with MC gender and message treatment interaction**

| | General Support | Effectiveness | Fiscal Responsibility |
|---|---|---|---|
| Message: Claim | 0.517*** | 0.706*** | 0.438*** |
| | (0.107) | (0.098) | (0.106) |
| Message: Critique | -0.228** | 0.438*** | -0.135 |
| | (0.104) | (0.095) | (0.102) |
| MC Gender: Woman | 0.199* | 0.087 | 0.035 |
| | (0.107) | (0.098) | (0.106) |
| Partisan Match | 0.191*** | 0.149*** | 0.146*** |
| | (0.038) | (0.034) | (0.037) |
| Respondent Gender: Woman | -0.007 | -0.046 | -0.002 |
| | (0.062) | (0.057) | (0.061) |
| Respondent Party: Ind | -0.498*** | -0.460*** | -0.538*** |
| | (0.076) | (0.069) | (0.074) |
| Respondent Party: Rep | -0.358*** | -0.179*** | -0.400*** |
| | (0.075) | (0.069) | (0.074) |
| Credit Claim X Woman MC | -0.118 | 0.025 | 0.115 |
| | (0.152) | (0.140) | (0.150) |
| Critique X Woman MC | -0.174 | -0.110 | 0.030 |
| | (0.153) | (0.140) | (0.150) |
| Constant | 5.062*** | 4.851*** | 4.871*** |
| | (0.090) | (0.082) | (0.088) |
| Num.Obs. | 2064 | 2064 | 2064 |
| R2 | 0.083 | 0.077 | 0.071 |
| RMSE | 1.41 | 1.29 | 1.38 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

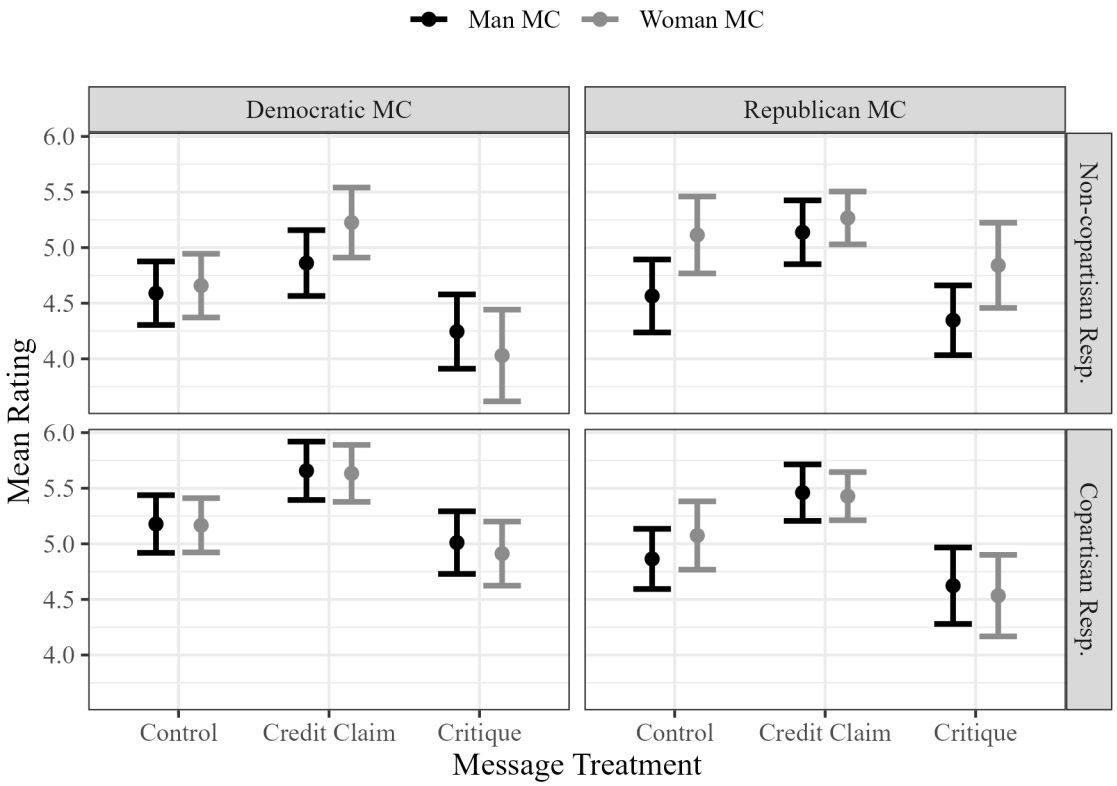
Note. Ordinary least squares models. Standard errors in parentheses.

Finally, I explore how partisanship impacts the experimental findings by subsetting the analysis by MC party (randomly assigned) and MC copartisan status with the respondent. Distributive politics are interpreted through a partisan lens (Sidman 2019), so subsetting the data by respondent and MC partisanship may yield additional insights. Figure 4 displays general support for the MC across treatment groups for each of the four possible partisan relationships: copartisan Republican MC, non-copartisan Republican MC, copartisan Democratic MC, and non-copartisan Democratic MC. Generally, subsetting by partisanship does not change the result. The message content treatment influences MC support, but MC gender does not. This is particularly true when the MC and respondent are copartisans—MC gender has no influence over copartisan MC support.

However, Figure 4 suggests a few additional findings. First, the general support advantage of women MCs in the control group observed in Figure 1 appears to stem from the upper-right quadrant of Figure 4. Among Democratic respondents who were assigned a Republican MC and the control message condition, the woman MC condition yielded substantially higher general support ratings than the man MC condition. Second, also in the upper-right quadrant, the woman MC condition yielded substantially higher general support ratings than the man MC condition for the credit claim with a critique message. Subsetting the data by partisanship is non-preregistered exploratory analysis, but these finding offer an interesting pathway for future research. In the messaging environment offered by this research design, Democrats appear to favor Republican congresswomen to Republican congressmen.

In sum, my findings are robust across analyses. The message content treatments influenced general support as well as perceptions of legislator effectiveness and fiscal responsibility, but the legislator gender treatment did not. These findings stand in contrast to the expectations of **H1** and **H2**. Congresswomen face unique challenges in retaining their offices, but their ability to claim credit for federal spending projects appears to equal that of congressmen.

**Figure 4: Mean MC Support Across Treatment Group and Party Affiliation**




NOTE: Figure displays mean MC support (1-7 scale) across treatment conditions and partisanship. 95% confidence intervals surround point estimates.

# Conclusion

This study advances the congressional representation and gender and politics literatures by clarifying the role of legislator gender in credit claiming for distributive benefits. I develop a theory for how legislator gender might influence public reactions to congressional credit claiming. I design and field a survey experiment to test this theory, and I find that legislator gender does not meaningfully impact the effectiveness of credit claiming for distributive benefits.

Facing gendered vulnerability in elections, women legislators tend to outperform their male colleagues on representational tasks, such as securing district funding projects (Lazarus and Steigerwalt 2018). My findings suggest that women legislators can effectively use a distributive politics strategy for ameliorating gendered vulnerability in congressional elections. Women MCs can boost their public support through credit claiming at a similar rate as men MCs, and spending critiques

do not have a particularly damaging effect on congresswomen. My findings comport with a recent study in a different political landscape. Using a conjoint experiment to study the effect of legislator gender in perceptions of member of Parliament (MP) productivity in Britain, Hargrave and Smith (2023) find that voters prefer productive MPs, but "unproductive men do not receive more positive evaluations than unproductive women, nor are productive men rewarded for their efforts any more than productive women."[25] For voters in the United States and United Kingdom, the positive public opinion benefits of effective representation are not conditioned by legislator gender. Therefore, the findings of this study plausibly generalize to other countries: effective representation is a useful tool for both women and men legislators to build public support.

The rejection of both hypotheses was unexpected, though it does follow a recent trend in the literature. Individual experiments expecting to find gender differences favoring men have uncovered similar findings to those reported above (Teele, Kalla and Rosenbluth 2018), and a meta-analysis of 67 conjoint and factorial candidate-choice experiments concludes that "the average effect of being a woman (vs. a man) is an approximately 2 percentage point increase in support" (Schwarz and Coppock 2022, 657). Gender stereotypes do harm women candidates on specific trait evaluations such as competence and leadership (Bauer 2020; Dittmar 2015; Meeks 2012; Schneider and Bos 2014), but women candidates do not appear to face an overall gender bias in elections. This study fits into the literature by showing that legislative effectiveness, fiscal responsibility, and credit-claiming ability do not fall into the gender stereotype category of findings. Instead these results align more closely with the findings from candidate-choice experiments. Congresswomen's credit claims are just as effective as those of congressmen and do not carry more downside risk of spending critics.

One explanation for these findings is that women legislators' representational advantage has been internalized by the public. A recent study uses a field experiment to find constituents expect women legislators to do more work than men legislators (Butler, Naurin and Öhberg 2022). Increased expectations for women legislators could result from or lead to women legislators' over-

[25] Hargrave and Smith (2023) was published after this experiment was fielded and analyzed. Therefore, their findings played no role in the theory or research design presented here.

performance on representational tasks. In either case, constituents appear to both expect and receive greater effective representation from women legislators. The high standard set for women legislators may counter gender stereotypes on trait evaluations, leading to the null findings displayed above. Further research might explore whether this is the case and, if so, the consequences of a higher representational standard for women legislators.

By exploring both the potential upside and downside of congressional credit claiming through the lens of legislator gender, this study sheds new light on the dynamics of gender, legislative representation, and elections. Extant research shows that congresswomen outperform congressmen on representational tasks (Anzia and Berry 2011; Lazarus and Steigerwalt 2018; Volden, Wiseman and Wittmer 2013). Their overperformance on representational tasks leads to additional credit-claiming opportunities, and congresswomen do, in fact, claim credit more than congressmen (Dolan and Kropf 2004). This study extends this line of research by showing that women legislators do not face gender stereotypes while credit claiming—the upside and downside of credit claiming is not conditioned by legislator gender. The implication of this study, therefore, is that congresswomen can turn their representational advantage into an electoral advantage. However, additional research is needed to further flesh out this claim. To clarify how these independent findings actually influence the ballot box, further research must explore how credit claiming aggregates over time to shape legislator evaluations and influence electoral outcomes.

This study advances the credit-claiming literature by adding a gender dimension to previous experimental designs, but it hold constant aspects of credit claiming that deserve further attention. For instance, the topic of the federal grant being claimed, transportation, is less likely to evoke stereotypes than more gendered policy issues like military bases. If the results obtained here hold regardless of the topic of spending, women may be able to mitigate gender stereotypes on certain policy issues through credit claiming. Alternatively, women may face a credit-claiming barrier when claiming credit for a more gendered type of spending. By varying the type of project being claimed, future research can test this question. Finally, an important limitation of this research is the use of only stereotypically white legislators' names in the experimental vignettes. The role of

race and ethnicity in credit claiming is an important venue for further research. Credit claiming for distributive benefits remains a core strategy that legislators use to build public support, and the ways in which this strategy interacts with identity deserve further investigation.

# *Conclusion*

Decisions on the geographic allocation of federal funds affect everyone in the United States. For states, cities, towns, and people, federal spending projects bring jobs, infrastructure, and economic development. For members of Congress, federal spending projects offer opportunities for electorally valuable credit claiming. This dissertation weaves together research on different aspects of congressional distributive politics, from the nature of policy change to the effectiveness of credit claiming, to offer insights on congressional policymaking, representation, and electoral strategy.

The insights gleaned from this research range from theoretical to practical. For instance, the primary takeaway from Chapter 1 – that geographic spending change follows punctuated equilibrium patterns – contributes to a theoretically meaningful stream of research on the nature of policy change. Additionally, Chapter 3 develops a new theoretical perspective on credit claiming and congressional representation. However, Chapter 3 also includes concrete guidance for members of Congress: public support is generated by securing the *right* distributive benefits rather than securing the *most* distributive benefits. The main finding from Chapter 2 – that transportation earmarks were institutionalized into the funding formula and increased in policy significance during the earmark moratorium – offers the practical insight that eliminating particularistic spending is far more complex than banning congressional earmarks. Finally, the evidence presented in Chapter 4 suggests congresswomen can use credit claiming to turn their over-performance, relative to congressmen, on distributive politics into electoral capital.

In sum, this dissertation clarifies clarifies multiple components of the geographic allocation of federal funds by the US Congress. I approach this topic from different perspectives, fusing disparate literature and methodological approaches. As a result, the findings carry meaningful implications for research on legislative studies, public policy, political communication, and public opinion.

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*Appendix*

# Chapter 1 Appendix

**Figure A1: Distribution of district spending changes by party control of the House**

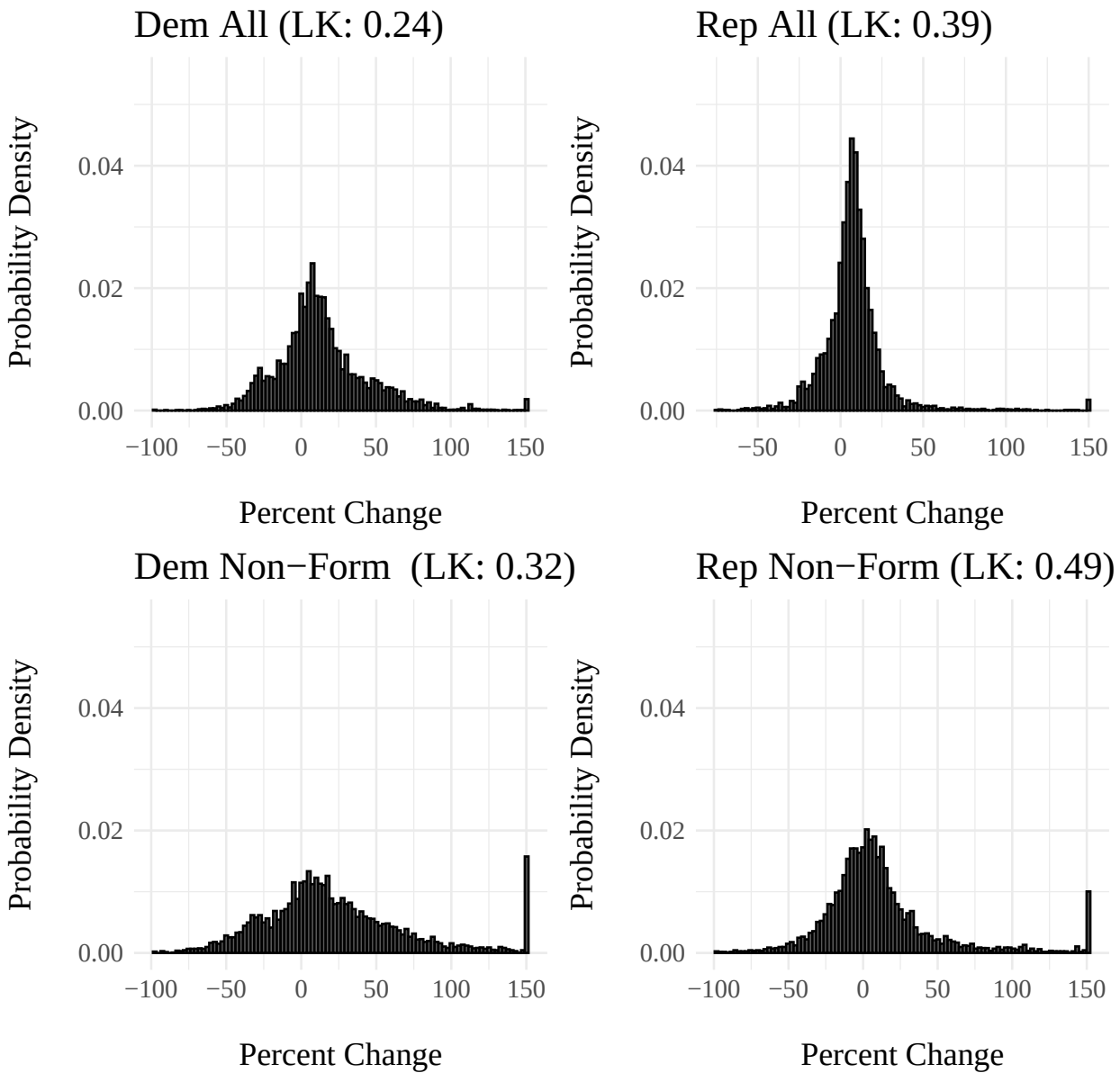


**Table A1: District Spending Instability Regression Results (Standardized DV)**

| | Non-formula spending | | All spending | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Appropriations Member | -0.09*** | -0.08*** | -0.05* | -0.03 |
| | (0.03) | (0.03) | (0.03) | (0.03) |
| Majority Party | 0.00 | 0.30*** | 0.04* | 0.59*** |
| | (0.02) | (0.03) | (0.02) | (0.03) |
| MC Party: R | 0.07*** | 0.42*** | -0.03 | 0.61*** |
| | (0.02) | (0.03) | (0.02) | (0.03) |
| President's Party | -0.04 | -0.04* | -0.01 | -0.01 |
| | (0.02) | (0.02) | (0.02) | (0.02) |
| Close District | 0.00 | -0.01 | 0.02 | -0.01 |
| | (0.04) | (0.04) | (0.04) | (0.04) |
| Tenure in Congress | 0.01*** | 0.01*** | 0.01*** | 0.01*** |
| | (0.00) | (0.00) | (0.00) | (0.00) |
| Majority * MC Party: R | | -0.63*** | | -1.16*** |
| | | (0.04) | | (0.04) |
| Constant | -0.05* | -0.22*** | -0.07*** | -0.39*** |
| | (0.03) | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 9757 | 9757 | 9770 | 9770 |
| RMSE | 1.00 | 0.99 | 1.00 | 0.96 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
DV is standardized to have a mean of 0 and SD of 1

# Chapter 2 Appendix

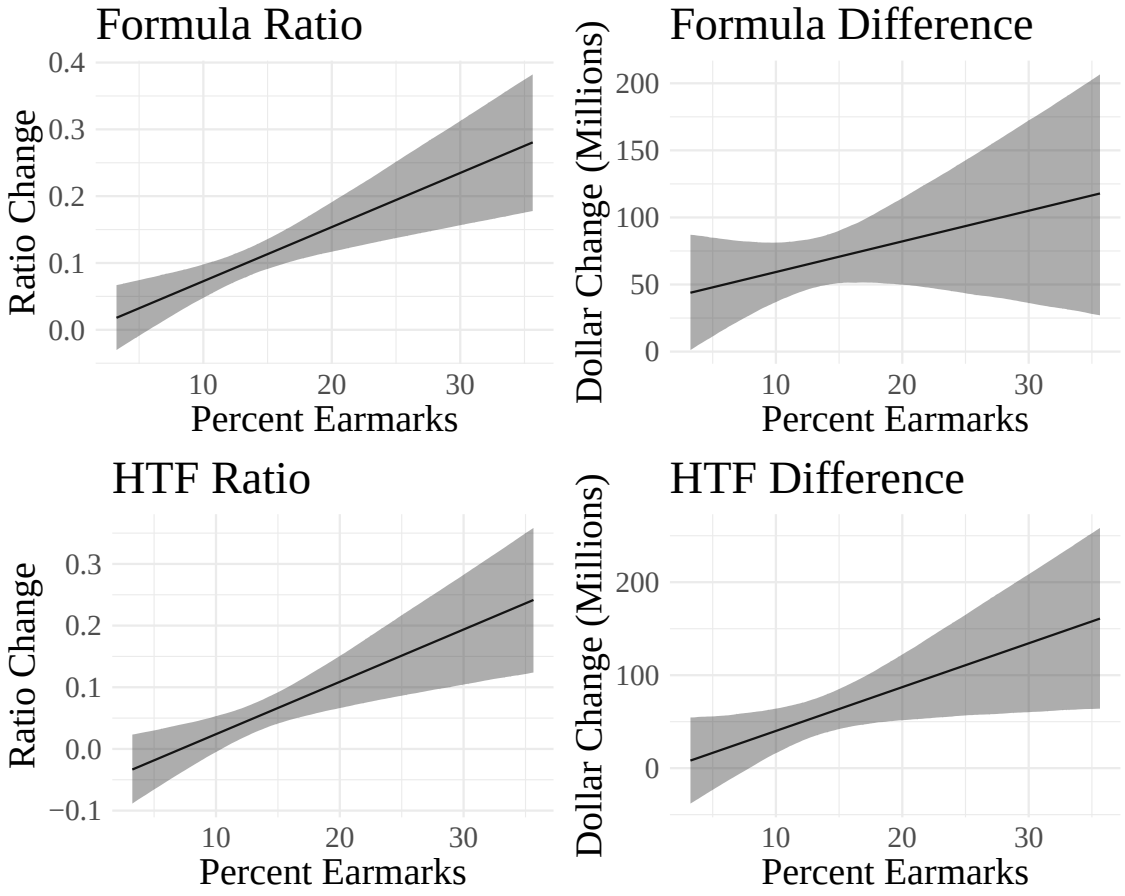
## Online Appendix A: OLS Regression Models

I replicate the main analysis with OLS estimators rather than robust regression. Findings from Table 1 and Figure 3 in the manuscript do not meaningfully change.

**Table A1: Earmarks and Highway Funding Malapportionment Drift: 2010-2020**

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Formula Ratio | Formula Difference (In Millions) | HTF Ratio (Logged) | HTF Difference (In Millions) |
| | (1) | (2) | (3) | (4) |
| Percent Earmarks | 0.008*** | 2.275 | 0.008*** | 4.729** |
| | (0.002) | (1.989) | (0.003) | (2.197) |
| Population (Logged) | 0.086 | 49.065 | 0.137 | −8.640 |
| | (0.076) | (66.345) | (0.087) | (73.292) |
| Federal Aid Highway Miles (Logged) | 0.034 | 18.070 | −0.096*** | −30.714 |
| | (0.024) | (20.489) | (0.027) | (22.635) |
| Vehicle Miles Traveled (Logged) | −0.118 | −72.996 | −0.059 | 16.884 |
| | (0.091) | (78.873) | (0.104) | (87.131) |
| Constant | 0.474 | 358.903 | 0.968 | 95.856 |
| | (0.532) | (462.424) | (0.607) | (510.844) |
| Observations | 51 | 51 | 51 | 51 |
| $R^2$ | 0.422 | 0.145 | 0.543 | 0.283 |
| Adjusted $R^2$ | 0.371 | 0.070 | 0.503 | 0.220 |
| Residual Std. Error (df = 46) | 0.076 | 66.382 | 0.087 | 73.333 |
| F Statistic (df = 4; 46) | 8.382*** | 1.948 | 13.673*** | 4.531*** |
| *Note:* OLS Regression | | | | *p<0.1; **p<0.05; ***p<0.01 |

**Figure A1: OLS Regression Simulation**



I replicate the placebo analysis with OLS estimators rather than robust regression. Findings from Table 2 in the manuscript do not meaningfully change.

**Table A2: Earmarks and Highway Funding Malapportionment Drift: 2005-2009**

| | *Dependent variable:* | |
|---|---|---|
| | HTF Ratio (Logged) | HTF Difference (In Millions) |
| | (1) | (2) |
| Percent Earmarks | $-0.005^{**}$ | $-1.506^{*}$ |
| | (0.002) | (0.753) |
| Population (Logged) | $-0.098$ | $-26.211$ |
| | (0.075) | (26.121) |
| Federal Aid Highway Miles (Logged) | $-0.066^{***}$ | $-5.962$ |
| | (0.022) | (7.731) |
| Vehicle Miles Traveled (Logged) | 0.146 | $-2.701$ |
| | (0.089) | (30.898) |
| Constant | $0.665^{*}$ | $471.560^{***}$ |
| | (0.390) | (135.986) |
| Observations | 51 | 51 |
| $R^2$ | 0.294 | 0.602 |
| Adjusted $R^2$ | 0.233 | 0.567 |
| Residual Std. Error (df = 46) | 0.073 | 25.624 |
| F Statistic (df = 4; 46) | $4.790^{***}$ | $17.401^{***}$ |
| *Note:* OLS Regression | $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01 | |

## Online Appendix B: Models with Region of the Country Control

I replicate the analysis, controlling for region of the country. Findings from Table 1 in the manuscript do not meaningfully change.

**Table B1:Earmarks and Highway Funding Malapportionment Drift: 2010-2020**

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Formula Ratio | Formula Difference (In Millions) | HTF Ratio (Logged) | HTF Difference (In Millions) |
| | (1) | (2) | (3) | (4) |
| Percent Earmarks | 0.004*** | 2.000 | 0.007*** | 3.609*** |
| | (0.002) | (1.336) | (0.002) | (1.124) |
| Population (Logged) | −0.015 | 25.810 | 0.006 | −19.114 |
| | (0.056) | (47.470) | (0.054) | (39.929) |
| Federal Aid Highway Miles (Logged) | 0.018 | 14.258 | −0.113*** | −26.052* |
| | (0.020) | (17.193) | (0.019) | (14.462) |
| Vehicle Miles Traveled (Logged) | −0.001 | −34.451 | 0.090 | 35.580 |
| | (0.068) | (58.088) | (0.066) | (48.860) |
| Region: Northeast | 0.044 | 28.952 | −0.014 | 1.716 |
| | (0.028) | (24.217) | (0.027) | (20.369) |
| Region: South | −0.003 | 0.206 | −0.040* | −31.922* |
| | (0.023) | (19.849) | (0.022) | (16.696) |
| Region: West | 0.010 | 0.506 | −0.043** | −3.583 |
| | (0.023) | (19.346) | (0.022) | (16.273) |
| Constant | −0.014 | −96.885 | −0.095 | 11.672 |
| | (0.141) | (120.374) | (0.136) | (101.251) |
| Observations | 51 | 51 | 51 | 51 |
| Residual Std. Error (df = 43) | 0.049 | 36.542 | 0.042 | 34.425 |

*Note:* Robust Linear Regression *p<0.1; **p<0.05; ***p<0.01

I replicate the analysis, controlling for region of the country. Findings from Table 2 in the manuscript do not meaningfully change.

**Table B2:Earmarks and Highway Funding Malapportionment Drift: 2005-2009**

| | *Dependent variable:* | |
|---|---|---|
| | HTF Ratio (Logged) | HTF Difference (In Millions) |
| | (1) | (2) |
| Percent Earmarks | −0.001 | −1.166*** |
| | (0.001) | (0.430) |
| Population (Logged) | −0.002 | −26.688* |
| | (0.049) | (15.909) |
| Federal Aid Highway Miles (Logged) | −0.038** | −2.494 |
| | (0.017) | (5.471) |
| Vehicle Miles Traveled (Logged) | 0.026 | 0.610 |
| | (0.060) | (19.291) |
| Region: Northeast | −0.002 | 11.037 |
| | (0.024) | (7.879) |
| Region: South | 0.006 | 5.837 |
| | (0.020) | (6.538) |
| Region: West | −0.018 | 7.320 |
| | (0.020) | (6.345) |
| Constant | −0.018 | 76.162* |
| | (0.123) | (39.849) |
| Observations | 51 | 51 |
| Residual Std. Error (df = 43) | 0.028 | 10.520 |
| *Note:* Robust Linear Regression | *p<0.1; **p<0.05; ***p<0.01 | |

## Online Appendix C: Models with Controls for Covariate Change Over Time

I replicate the 2010-2020 malapportionment drift analysis, controlling for the change in population, federal aid highway miles, and vehicle miles traveled on federal aid highways between 2010 and 2020. Findings from Table 1 in the manuscript do not meaningfully change when including these variables.

**Table C1: Earmarks and Highway Funding Malapportionment Drift: 2010-2020**

| | *Dependent variable:* | | | |
|---|---|---|---|---|
| | Formula Ratio | Formula Difference (In Millions) | HTF Ratio (Logged) | HTF Difference (In Millions) |
| | (1) | (2) | (3) | (4) |
| Percent Earmarks | 0.004*** | 1.575 | 0.007*** | 2.469** |
| | (0.001) | (1.120) | (0.002) | (1.007) |
| Population (Logged) | 0.018 | 49.943 | 0.043 | 22.577 |
| | (0.041) | (37.012) | (0.055) | (33.264) |
| Federal Aid Highway Miles (Logged) | 0.008 | 4.852 | −0.100*** | −17.080* |
| | (0.013) | (11.375) | (0.017) | (10.223) |
| Vehicle Miles Traveled (Logged) | −0.016 | −33.901 | 0.048 | −1.374 |
| | (0.049) | (43.811) | (0.065) | (39.375) |
| Change in Population (per 100,000 people) | −0.002 | −6.025*** | −0.002 | −2.704** |
| | (0.002) | (1.511) | (0.002) | (1.358) |
| Change in FAH Miles (per 10,000 Miles) | 0.070** | 50.543* | 0.016 | −57.718** |
| | (0.033) | (29.253) | (0.044) | (26.291) |
| Change in VMT (per 10,000 VMT) | −0.047** | −41.740** | 0.003 | −18.764 |
| | (0.019) | (16.889) | (0.025) | (15.179) |
| Constant | −0.082 | −138.132 | −0.215 | −78.546 |
| | (0.101) | (90.549) | (0.135) | (81.381) |
| Observations | 51 | 51 | 51 | 51 |
| Residual Std. Error (df = 43) | 0.027 | 22.150 | 0.046 | 26.876 |

*Note:* Robust Linear Regression *p<0.1; **p<0.05; ***p<0.01

I replicate the 2005-2009 malapportionment drift analysis, controlling for the change in population, federal aid highway miles, and vehicle miles traveled on federal aid highways between 2005 and 2009. Findings from Table 2 in the manuscript do not meaningfully change when including these variables.

**Table C2: Earmarks and Highway Funding Malapportionment Drift: 2005-2009**

| | *Dependent variable:* | |
|---|---|---|
| | HTF Ratio (Logged) | HTF Difference (In Millions) |
| | (1) | (2) |
| Percent Earmarks | −0.002 | −1.137** |
| | (0.001) | (0.502) |
| Population (Logged) | −0.020 | −29.542* |
| | (0.042) | (17.340) |
| Federal Aid Highway Miles (Logged) | −0.044*** | −7.431 |
| | (0.012) | (5.149) |
| Vehicle Miles Traveled (Logged) | 0.043 | 8.282 |
| | (0.049) | (20.584) |
| Change in Population (per 100,000 people) | 0.003 | −1.074 |
| | (0.004) | (1.491) |
| Change in FAH Miles (per 10,000 Miles) | 0.023 | 6.233 |
| | (0.050) | (20.987) |
| Change in VMT (per 10,000 VMT) | −0.016 | −6.600 |
| | (0.036) | (14.987) |
| Constant | 0.033 | 91.342** |
| | (0.100) | (41.872) |
| Observations | 51 | 51 |
| Residual Std. Error (df = 43) | 0.033 | 13.729 |
| *Note:* Robust Linear Regression | *p<0.1; **p<0.05; ***p<0.01 | |

## Online Appendix D: Dollars Stemming from SAFETEA-LU Earmarks

Figure 1 displays funding directly attributable to SAFETEA-LU earmarks for six similarly sized states. I include the full list states in the figure below:

| STATE | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 | 2019 | 2020 | Total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ALABAMA | 67 | 67 | 63 | 63 | 62 | 62 | 66 | 67 | 68 | 70 | 71 | 726 |
| ALASKA | 161 | 186 | 173 | 173 | 173 | 173 | 181 | 184 | 188 | 193 | 197 | 1982 |
| ARIZONA | 24 | 25 | 23 | 23 | 23 | 23 | 24 | 24 | 25 | 26 | 26 | 266 |
| ARKANSAS | 81 | 81 | 76 | 76 | 76 | 76 | 79 | 80 | 82 | 85 | 86 | 878 |
| CALIFORNIA | 491 | 498 | 465 | 464 | 463 | 463 | 487 | 494 | 505 | 518 | 529 | 5377 |
| COLORADO | 66 | 67 | 62 | 62 | 62 | 62 | 65 | 66 | 67 | 69 | 71 | 719 |
| CONNECTICUT | 55 | 55 | 51 | 51 | 51 | 51 | 54 | 55 | 56 | 57 | 59 | 596 |
| DELAWARE | 32 | 32 | 30 | 30 | 30 | 30 | 31 | 32 | 32 | 33 | 34 | 344 |
| DIST. OF COL. | 29 | 30 | 28 | 27 | 27 | 27 | 29 | 29 | 30 | 31 | 31 | 319 |
| FLORIDA | 113 | 113 | 105 | 105 | 105 | 105 | 110 | 112 | 115 | 118 | 120 | 1221 |
| GEORGIA | 88 | 88 | 82 | 82 | 82 | 82 | 86 | 87 | 89 | 92 | 94 | 953 |
| HAWAII | 29 | 29 | 27 | 27 | 27 | 27 | 28 | 29 | 30 | 30 | 31 | 315 |
| IDAHO | 29 | 29 | 27 | 27 | 27 | 27 | 29 | 29 | 30 | 31 | 31 | 318 |
| ILLINOIS | 254 | 256 | 239 | 238 | 238 | 238 | 250 | 253 | 259 | 266 | 271 | 2762 |
| INDIANA | 55 | 55 | 52 | 51 | 51 | 51 | 54 | 55 | 56 | 57 | 59 | 597 |
| IOWA | 84 | 84 | 78 | 78 | 78 | 78 | 82 | 83 | 85 | 88 | 89 | 909 |
| KANSAS | 40 | 40 | 37 | 37 | 37 | 37 | 39 | 40 | 40 | 41 | 42 | 431 |
| KENTUCKY | 72 | 72 | 67 | 67 | 67 | 67 | 70 | 71 | 73 | 75 | 76 | 775 |
| LOUISIANA | 106 | 106 | 99 | 99 | 99 | 99 | 104 | 105 | 108 | 110 | 113 | 1146 |
| MAINE | 40 | 40 | 37 | 37 | 37 | 37 | 39 | 40 | 40 | 42 | 42 | 431 |
| MARYLAND | 62 | 62 | 58 | 57 | 57 | 57 | 60 | 61 | 63 | 64 | 65 | 666 |
| MASSACHUSETTS | 58 | 58 | 54 | 54 | 54 | 54 | 57 | 57 | 59 | 60 | 61 | 625 |
| MICHIGAN | 87 | 87 | 81 | 81 | 81 | 81 | 85 | 86 | 88 | 91 | 92 | 940 |
| MINNESOTA | 88 | 95 | 89 | 89 | 89 | 89 | 93 | 94 | 97 | 99 | 101 | 1022 |
| MISSISSIPPI | 71 | 71 | 67 | 67 | 66 | 66 | 70 | 71 | 72 | 74 | 76 | 772 |
| MISSOURI | 130 | 136 | 127 | 127 | 127 | 127 | 133 | 135 | 138 | 142 | 145 | 1465 |
| MONTANA | 64 | 80 | 75 | 75 | 75 | 75 | 79 | 80 | 82 | 84 | 85 | 852 |
| NEBRASKA | 36 | 36 | 34 | 34 | 33 | 33 | 35 | 36 | 37 | 37 | 38 | 389 |
| NEVADA | 98 | 98 | 92 | 91 | 91 | 91 | 96 | 97 | 99 | 102 | 104 | 1060 |
| NEW HAMPSHIRE | 13 | 13 | 12 | 12 | 12 | 12 | 13 | 13 | 13 | 14 | 14 | 143 |
| NEW JERSEY | 96 | 101 | 94 | 94 | 94 | 94 | 99 | 100 | 103 | 105 | 107 | 1089 |
| NEW MEXICO | 36 | 36 | 33 | 33 | 33 | 33 | 35 | 35 | 36 | 37 | 38 | 387 |
| NEW YORK | 175 | 175 | 163 | 163 | 163 | 163 | 171 | 174 | 178 | 182 | 186 | 1893 |
| NORTH CAROLINA | 66 | 66 | 62 | 62 | 62 | 62 | 65 | 66 | 67 | 69 | 71 | 719 |
| NORTH DAKOTA | 34 | 34 | 32 | 32 | 31 | 31 | 33 | 34 | 34 | 35 | 36 | 366 |
| OHIO | 119 | 119 | 111 | 111 | 111 | 111 | 116 | 118 | 121 | 124 | 126 | 1287 |
| OKLAHOMA | 111 | 111 | 104 | 103 | 103 | 103 | 109 | 110 | 113 | 116 | 118 | 1200 |
| OREGON | 108 | 108 | 101 | 101 | 101 | 101 | 106 | 108 | 110 | 113 | 115 | 1172 |
| PENNSYLVANIA | 141 | 141 | 132 | 131 | 131 | 131 | 138 | 140 | 143 | 147 | 150 | 1525 |
| RHODE ISLAND | 49 | 49 | 45 | 45 | 45 | 45 | 48 | 48 | 49 | 51 | 52 | 527 |
| SOUTH CAROLINA | 53 | 53 | 49 | 49 | 49 | 49 | 51 | 52 | 53 | 55 | 56 | 569 |
| SOUTH DAKOTA | 51 | 51 | 48 | 48 | 48 | 48 | 50 | 51 | 52 | 53 | 54 | 553 |
| TENNESSEE | 93 | 93 | 87 | 87 | 87 | 87 | 91 | 93 | 95 | 97 | 99 | 1010 |
| TEXAS | 137 | 138 | 129 | 128 | 129 | 129 | 136 | 139 | 142 | 145 | 148 | 1499 |
| UTAH | 53 | 53 | 49 | 49 | 49 | 49 | 52 | 52 | 53 | 55 | 56 | 570 |
| VERMONT | 64 | 64 | 60 | 60 | 60 | 60 | 63 | 64 | 65 | 67 | 68 | 693 |
| VIRGINIA | 102 | 102 | 95 | 95 | 95 | 95 | 100 | 101 | 104 | 106 | 109 | 1105 |
| WASHINGTON | 99 | 104 | 97 | 97 | 97 | 97 | 102 | 104 | 106 | 109 | 111 | 1124 |
| WEST VIRGINIA | 65 | 65 | 61 | 60 | 60 | 60 | 63 | 64 | 66 | 68 | 69 | 702 |
| WISCONSIN | 67 | 78 | 72 | 72 | 72 | 72 | 76 | 77 | 79 | 81 | 82 | 828 |
| WYOMING | 21 | 21 | 19 | 19 | 19 | 19 | 20 | 20 | 21 | 21 | 22 | 223 |
| Total | 4362 | 4451 | 4155 | 4144 | 4141 | 4141 | 4352 | 4414 | 4517 | 4634 | 4727 | 48039 |

## Online Appendix E: Explanation of the SAFETEA-LU Funding Formula

The *Formula Ratio* and *Formula Difference* measures of malapportionment drift in the manuscript rely on the re-creation of the SAFETEA-LU funding formula with 2010 and 2010 data. As noted in the manuscript, I rely on the Eno Center for Transportation's publicly available SAFETEA-LU funding formula spreadsheets. Eno researchers developed this tool to re-create the SAFETEA-LU formula for 2018, and they note the assumptions and limitations of their formula re-creation in a published research report (Lewis, Davis and Grossman 2019, 30-31). I argue that the formula results in a useful measure of "correct" funding for the purpose of the manuscript (to detect policy drift over time), but it is important to highlight the formula's assumptions.

- First, an assumption is made about the process of apportionment: "Scenario 2 applies the apportionment formulas that were used in SAFETEALU to the programs that exist under FAST (with NHFP combined with NHPP). This scenario assumes that funding would be apportioned to the five programs separately and not in a lump sum (as it was in MAP-21)" (Lewis, Davis and Grossman 2019, 30).

- Second, some features of SAFETEA-LU do not map cleanly onto the structure of the funding formula since it has been frozen: "Because the Scenario 2 formulas are from SAFETEA-LU, they have some aspects that may not translate into MAP-21's programs and program goals. For example, in SAFETEA-LU, the NHPP program excluded the Interstate System from its calculation of lane miles. While this had some logic under SAFETEA-LU because a separate Interstate program existed, the same logic does not apply with a FAST-based program structure. However, since this scenario is meant to be illustrative of SAFETEA-LU era programs as they were, almost none of the formula distributions were adjusted"(Lewis, Davis and Grossman 2019, 31).

- Third, the formula re-creation does not include SAFETEA-LU earmarks or equity bonus funding: "The one key adjustment from SAFETEA-LU era formulas is that this apportionment does not consider SAFETEA-LU's Equity Bonus program. Because an increasing por-

tion of the HTF is revenue from the general fund of the U.S. Treasury, and because the aim of this exercise is exploring potential apportionments that could help states achieve policy goals (while not ignoring politics), it is assumed that SAFETEA-LU's apportionments would have more relevance if Equity Bonus was not included as a consideration" (Lewis, Davis and Grossman 2019, 31). My aim is to use the formula to generate "correct" measures of apportionment, and I consider this adjustment to result in more accurate measures.

# Chapter 3 Appendix

# Appendix A: Survey #1 Sample Demographics and Balance Tests:

**Figure A1: Sample Demographics**

| Variable | N = 3,291[1] |
|---|---|
| Party | |
| *dem* | 1,204 (37%) |
| *ind* | 1,087 (33%) |
| *rep* | 992 (30%) |
| Gender | |
| *man* | 1,600 (49%) |
| *woman* | 1,691 (51%) |
| Age | 44 (31, 60) |
| Race | |
| *Asian American* | 153 (4.6%) |
| *Black* | 382 (12%) |
| *Latino* | 199 (6.0%) |
| *Multi-racial* | 69 (2.1%) |
| *Other* | 192 (5.8%) |
| *White* | 2,296 (70%) |
| Education | |
| *Bachelors_or_higher* | 1,372 (42%) |
| *Less_than_Bachelors* | 1,900 (58%) |
| Income | 5.00 (3.00, 8.00) |
| Parent | 1,001 (30%) |

[1] n (%); Median (IQR)

## Figure A2: Project Priority Assignment Balance

| Variable | **1**, N = 366[1] | **2**, N = 358[1] | **3**, N = 376[1] | **4**, N = 366[1] | **5**, N = 365[1] | **6**, N = 354[1] | **7**, N = 390[1] | **8**, N = 350[1] | **9**, N = 352[1] | **p-value**[2] |
|---|---|---|---|---|---|---|---|---|---|---|
| Party | | | | | | | | | | 0.094 |
| *dem* | 126 (35%) | 125 (35%) | 133 (35%) | 149 (41%) | 139 (38%) | 119 (34%) | 146 (38%) | 134 (38%) | 129 (37%) | |
| *ind* | 134 (37%) | 145 (41%) | 124 (33%) | 112 (31%) | 108 (30%) | 118 (33%) | 133 (34%) | 111 (32%) | 102 (29%) | |
| *rep* | 105 (29%) | 88 (25%) | 119 (32%) | 104 (28%) | 117 (32%) | 117 (33%) | 110 (28%) | 105 (30%) | 120 (34%) | |
| Gender | | | | | | | | | | 0.13 |
| *man* | 190 (52%) | 179 (50%) | 183 (49%) | 172 (47%) | 191 (52%) | 176 (50%) | 193 (49%) | 146 (42%) | 161 (46%) | |
| *woman* | 176 (48%) | 179 (50%) | 193 (51%) | 194 (53%) | 174 (48%) | 178 (50%) | 197 (51%) | 204 (58%) | 191 (54%) | |
| Age | 44 (33, 60) | 44 (32, 61) | 43 (29, 58) | 44 (32, 60) | 42 (31, 60) | 44 (32, 60) | 43 (31, 57) | 47 (32, 63) | 45 (31, 59) | 0.2 |
| Race | | | | | | | | | | 0.7 |
| *Asian American* | 25 (6.8%) | 15 (4.2%) | 19 (5.1%) | 18 (4.9%) | 17 (4.7%) | 9 (2.5%) | 21 (5.4%) | 12 (3.4%) | 17 (4.8%) | |
| *Black* | 41 (11%) | 38 (11%) | 45 (12%) | 41 (11%) | 38 (10%) | 40 (11%) | 55 (14%) | 49 (14%) | 32 (9.1%) | |
| *Latino* | 20 (5.5%) | 17 (4.7%) | 31 (8.2%) | 30 (8.2%) | 19 (5.2%) | 23 (6.5%) | 21 (5.4%) | 19 (5.4%) | 18 (5.1%) | |
| *Multi-racial* | 8 (2.2%) | 10 (2.8%) | 7 (1.9%) | 3 (0.8%) | 8 (2.2%) | 9 (2.5%) | 8 (2.1%) | 6 (1.7%) | 9 (2.6%) | |
| *Other* | 22 (6.0%) | 26 (7.3%) | 18 (4.8%) | 18 (4.9%) | 18 (4.9%) | 25 (7.1%) | 19 (4.9%) | 23 (6.6%) | 20 (5.7%) | |
| *White* | 250 (68%) | 252 (70%) | 256 (68%) | 256 (70%) | 265 (73%) | 248 (70%) | 266 (68%) | 241 (69%) | 256 (73%) | |
| Education | | | | | | | | | | 0.6 |
| *Bachelors_or_higher* | 149 (41%) | 134 (37%) | 154 (41%) | 162 (44%) | 161 (44%) | 148 (42%) | 164 (43%) | 141 (41%) | 154 (44%) | |
| *Less_than_Bachelors* | 216 (59%) | 224 (63%) | 221 (59%) | 203 (56%) | 202 (56%) | 204 (58%) | 221 (57%) | 207 (59%) | 194 (56%) | |
| Income | 5.00 (3.00, 7.00) | 6.00 (3.00, 8.00) | 5.00 (3.00, 8.00) | 6.00 (3.00, 8.00) | 5.00 (3.00, 8.00) | 5.00 (3.00, 8.00) | 5.00 (3.00, 8.00) | 6.00 (3.00, 8.00) | 5.00 (3.00, 7.25) | 0.4 |
| Parent | 104 (28%) | 106 (30%) | 111 (30%) | 112 (31%) | 115 (32%) | 115 (32%) | 131 (34%) | 102 (29%) | 99 (28%) | 0.8 |

[1] n (%); Median (IQR)

[2] Pearson's Chi-squared test; Kruskal-Wallis rank sum test

# Appendix B: Survey #2 Sample Demographics and Balance Tests:

**Figure B1: Sample Demographics**

| Variable | N = 1,533[1] |
|---|---|
| Party | |
| *dem* | 535 (42%) |
| *ind* | 254 (20%) |
| *rep* | 496 (39%) |
| Gender | |
| *man* | 740 (48%) |
| *woman* | 793 (52%) |
| Age | 47 (34, 61) |
| Race | |
| *Asian American* | 71 (4.6%) |
| *Black* | 164 (11%) |
| *Latino* | 87 (5.7%) |
| *Multi-racial* | 26 (1.7%) |
| *Other* | 86 (5.6%) |
| *White* | 1,098 (72%) |
| Education | |
| *Bachelors_or_higher* | 621 (41%) |
| *Less_than_Bachelors* | 904 (59%) |
| Income | 6.0 (3.0, 8.0) |
| Parent | 398 (26%) |

[1] n (%); Median (IQR)

**Figure B2: Project Priority Assignment Balance**

| Variable | **Six**, N = 771[1] | **Top**, N = 751[1] | **p-value**[2] |
|---|---|---|---|
| Party | | | 0.5 |
| *dem* | 261 (40%) | 272 (43%) | |
| *ind* | 127 (20%) | 127 (20%) | |
| *rep* | 258 (40%) | 234 (37%) | |
| Gender | | | >0.9 |
| *man* | 370 (48%) | 362 (48%) | |
| *woman* | 401 (52%) | 389 (52%) | |
| Age | 47 (35, 62) | 48 (33, 61) | >0.9 |
| Race | | | 0.7 |
| *Asian American* | 41 (5.3%) | 30 (4.0%) | |
| *Black* | 86 (11%) | 76 (10%) | |
| *Latino* | 45 (5.8%) | 40 (5.3%) | |
| *Multi-racial* | 15 (1.9%) | 11 (1.5%) | |
| *Other* | 40 (5.2%) | 45 (6.0%) | |
| *White* | 544 (71%) | 548 (73%) | |
| Education | | | 0.7 |
| *Bachelors_or_higher* | 316 (41%) | 300 (40%) | |
| *Less_than_Bachelors* | 452 (59%) | 446 (60%) | |
| Income | 6.0 (3.0, 8.0) | 6.0 (3.0, 8.0) | 0.8 |
| Parent | 199 (26%) | 197 (26%) | 0.9 |

[1] n (%); Median (IQR)

[2] Pearson's Chi-squared test; Wilcoxon rank sum test

**Figure B3: Senate Influence Assignment Balance**

| **Variable** | **Low**, N = 721[1] | **High**, N = 801[1] | **p-value**[2] |
|---|---|---|---|
| Party | | | 0.3 |
| *dem* | 257 (43%) | 276 (40%) | |
| *ind* | 120 (20%) | 134 (20%) | |
| *rep* | 215 (36%) | 277 (40%) | |
| Gender | | | >0.9 |
| *man* | 346 (48%) | 386 (48%) | |
| *woman* | 375 (52%) | 415 (52%) | |
| Age | 49 (34, 61) | 46 (34, 62) | 0.9 |
| Race | | | 0.3 |
| *Asian American* | 36 (5.0%) | 35 (4.4%) | |
| *Black* | 76 (11%) | 86 (11%) | |
| *Latino* | 36 (5.0%) | 49 (6.1%) | |
| *Multi-racial* | 14 (1.9%) | 12 (1.5%) | |
| *Other* | 31 (4.3%) | 54 (6.7%) | |
| *White* | 527 (73%) | 565 (71%) | |
| Education | | | 0.7 |
| *Bachelors_or_higher* | 296 (41%) | 320 (40%) | |
| *Less_than_Bachelors* | 422 (59%) | 476 (60%) | |
| Income | 6.0 (3.8, 8.0) | 5.0 (3.0, 8.0) | 0.2 |
| Parent | 186 (26%) | 210 (26%) | 0.9 |

[1] n (%); Median (IQR)

[2] Pearson's Chi-squared test; Wilcoxon rank sum test

# Appendix C: Survey #1 Tabular Description of Spending Demand

Table C1: Full Sample – Mean and 95% CI for Spending Category Ranking (Figure 3)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 4.056454 | 3.964206 | 4.148702 |
| Local Economy | 5.451022 | 5.365987 | 5.536058 |
| Education | 5.762588 | 5.681357 | 5.843818 |
| Employment | 5.730546 | 5.649918 | 5.811175 |
| Environment | 4.363137 | 4.272357 | 4.453917 |
| Healthcare | 6.274031 | 6.195191 | 6.352871 |
| Public Lands | 3.415624 | 3.340909 | 3.490339 |
| Public Safety | 5.544706 | 5.461850 | 5.627561 |
| Transportation | 4.403723 | 4.326699 | 4.480746 |

Table C2: Democrats – Mean and 95% CI for Spending Category Ranking (Figure 4)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 3.378333 | 3.239238 | 3.517428 |
| Local Economy | 5.215000 | 5.076402 | 5.353598 |
| Education | 5.930833 | 5.797011 | 6.064656 |
| Employment | 5.693333 | 5.562928 | 5.823739 |
| Environment | 4.945000 | 4.800758 | 5.089242 |
| Healthcare | 6.659167 | 6.532179 | 6.786154 |
| Public Lands | 3.427500 | 3.302333 | 3.552667 |
| Public Safety | 5.309167 | 5.171441 | 5.446893 |
| Transportation | 4.441667 | 4.314172 | 4.569161 |

Table C3: Republicans – Mean and 95% CI for Spending Category Ranking (Figure 4)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 5.033502 | 4.860654 | 5.206351 |
| Local Economy | 5.719797 | 5.565218 | 5.874376 |
| Education | 5.503553 | 5.352849 | 5.654258 |
| Employment | 5.895431 | 5.750464 | 6.040399 |
| Environment | 3.430457 | 3.276664 | 3.584250 |
| Healthcare | 5.762436 | 5.618427 | 5.906446 |
| Public Lands | 3.299492 | 3.167815 | 3.431170 |
| Public Safety | 5.954315 | 5.809420 | 6.099210 |
| Transportation | 4.407107 | 4.266673 | 4.547540 |

Table C4: Independents – Mean and 95% CI for Spending Category Ranking (Figure 4)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 3.918123 | 3.762462 | 4.073784 |
| Local Economy | 5.470101 | 5.321116 | 5.619086 |
| Education | 5.814167 | 5.675809 | 5.952526 |
| Employment | 5.620055 | 5.475355 | 5.764756 |
| Environment | 4.563017 | 4.402643 | 4.723392 |
| Healthcare | 6.315547 | 6.180253 | 6.450841 |
| Public Lands | 3.512420 | 3.380971 | 3.643868 |
| Public Safety | 5.430543 | 5.284941 | 5.576145 |
| Transportation | 4.356026 | 4.222396 | 4.489655 |

Table C5: Full Sample – Mean and 95% CI for Spending Category Rating (0-100)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 53.67196 | 52.64208 | 54.70184 |
| Local Economy | 65.62099 | 64.79266 | 66.44933 |
| Education | 70.70796 | 69.84177 | 71.57416 |
| Employment | 66.66036 | 65.82444 | 67.49628 |
| Environment | 58.67104 | 57.68445 | 59.65763 |
| Healthcare | 72.00824 | 71.18140 | 72.83508 |
| Public Lands | 54.71712 | 53.84759 | 55.58665 |
| Public Safety | 66.48093 | 65.58867 | 67.37319 |
| Transportation | 63.19835 | 62.35412 | 64.04259 |

Table C6: Democrats – Mean and 95% CI for Spending Category Rating (Figure 5)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 51.31917 | 49.60544 | 53.03289 |
| Local Economy | 67.54417 | 66.23543 | 68.85291 |
| Education | 75.53750 | 74.25398 | 76.82102 |
| Employment | 70.22417 | 68.95081 | 71.49752 |
| Environment | 69.55333 | 68.18363 | 70.92303 |
| Healthcare | 77.22167 | 76.00061 | 78.44273 |
| Public Lands | 59.99000 | 58.61995 | 61.36005 |
| Public Safety | 67.22667 | 65.76218 | 68.69116 |
| Transportation | 67.28083 | 65.95720 | 68.60447 |

Table C7: Republicans – Mean and 95% CI for Spending Category Rating (Figure 5)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 61.39391 | 59.57904 | 63.20877 |
| Local Economy | 66.96345 | 65.46171 | 68.46520 |
| Education | 68.28426 | 66.65511 | 69.91341 |
| Employment | 65.48223 | 63.90723 | 67.05724 |
| Environment | 48.07614 | 46.21908 | 49.93321 |
| Healthcare | 69.33503 | 67.82365 | 70.84641 |
| Public Lands | 51.93299 | 50.32129 | 53.54470 |
| Public Safety | 70.51472 | 69.00504 | 72.02440 |
| Transportation | 62.31777 | 60.77343 | 63.86211 |

Table C8: Independents – Mean and 95% CI for Spending Category Rating (Figure 5)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 49.35051 | 47.60080 | 51.10021 |
| Local Economy | 62.36431 | 60.86952 | 63.85909 |
| Education | 67.65409 | 66.08017 | 69.22802 |
| Employment | 63.85373 | 62.35210 | 65.35535 |
| Environment | 56.34499 | 54.65034 | 58.03963 |
| Healthcare | 68.72125 | 67.18839 | 70.25411 |
| Public Lands | 51.48942 | 49.98154 | 52.99730 |
| Public Safety | 62.03864 | 60.41947 | 63.65781 |
| Transportation | 59.55014 | 58.04714 | 61.05314 |

Table C9: Full Sample – Mean and 95% CI for Spending Category Allocation (0-10)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 0.8818431 | 0.8374475 | 0.9262388 |
| Local Economy | 1.1747940 | 1.1284090 | 1.2211790 |
| Education | 1.4556301 | 1.4072146 | 1.5040457 |
| Employment | 1.2192554 | 1.1735255 | 1.2649854 |
| Environment | 0.9583460 | 0.9137258 | 1.0029663 |
| Healthcare | 1.5277388 | 1.4770314 | 1.5784462 |
| Public Lands | 0.6180348 | 0.5880081 | 0.6480614 |
| Public Safety | 1.2155935 | 1.1702840 | 1.2609031 |
| Transportation | 0.9492829 | 0.9100783 | 0.9884875 |

Table C10: Democrats – Mean and 95% CI for Spending Category Allocation

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 0.6833333 | 0.6238403 | 0.7428264 |
| Local Economy | 1.0418333 | 0.9727101 | 1.1109566 |
| Education | 1.5315000 | 1.4531905 | 1.6098095 |
| Employment | 1.2351667 | 1.1582795 | 1.3120539 |
| Environment | 1.1933333 | 1.1130264 | 1.2736402 |
| Healthcare | 1.6515833 | 1.5697224 | 1.7334443 |
| Public Lands | 0.6358333 | 0.5884284 | 0.6832383 |
| Public Safety | 1.0647500 | 0.9971283 | 1.1323717 |
| Transportation | 0.9626667 | 0.8997215 | 1.0256118 |

Table C11: Republicans – Mean and 95% CI for Spending Category Allocation

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 1.2042640 | 1.1083620 | 1.3001660 |
| Local Economy | 1.2860914 | 1.1973634 | 1.3748193 |
| Education | 1.3698477 | 1.2825619 | 1.4571335 |
| Employment | 1.2356345 | 1.1531092 | 1.3181598 |
| Environment | 0.6204061 | 0.5595281 | 0.6812841 |
| Healthcare | 1.3815228 | 1.2915752 | 1.4714705 |
| Public Lands | 0.5590863 | 0.5047347 | 0.6134379 |
| Public Safety | 1.3865990 | 1.2982981 | 1.4748999 |
| Transportation | 0.9582741 | 0.8814808 | 1.0350675 |

Table C12: Independents – Mean and 95% CI for Spending Category Allocation

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 0.8096596 | 0.7352060 | 0.8841132 |
| Local Economy | 1.2214351 | 1.1368280 | 1.3060423 |
| Education | 1.4517019 | 1.3648827 | 1.5385211 |
| Employment | 1.1862006 | 1.1073828 | 1.2650183 |
| Environment | 1.0057038 | 0.9244102 | 1.0869973 |
| Healthcare | 1.5224471 | 1.4306192 | 1.6142750 |
| Public Lands | 0.6515179 | 0.5966072 | 0.7064287 |
| Public Safety | 1.2263109 | 1.1456333 | 1.3069886 |
| Transportation | 0.9250230 | 0.8595341 | 0.9905119 |

# Appendix D: Survey #2 Spending Demand Descriptives:

**Figure D1: Mean Spending Category Ranking Scores**

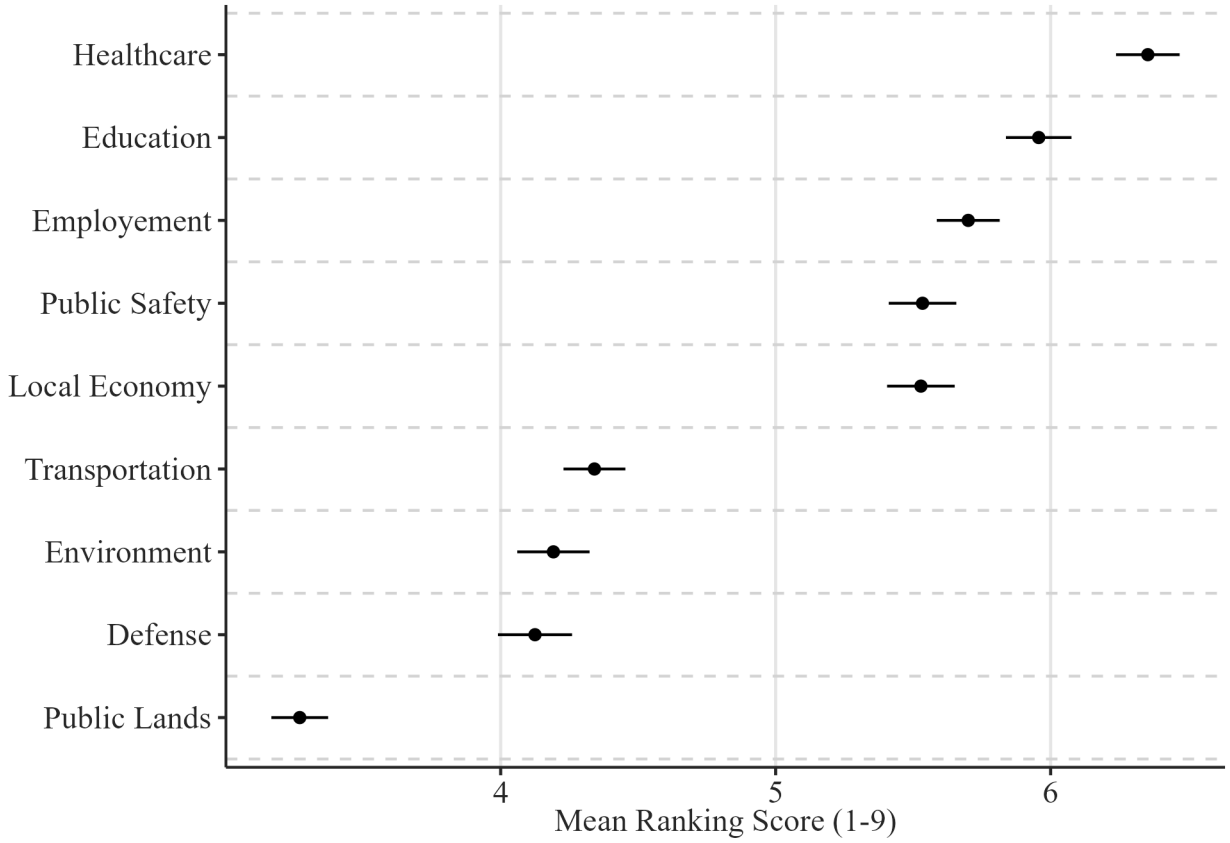


**Figure D2: Mean Spending Category Ranking Scores by Party Affiliation**

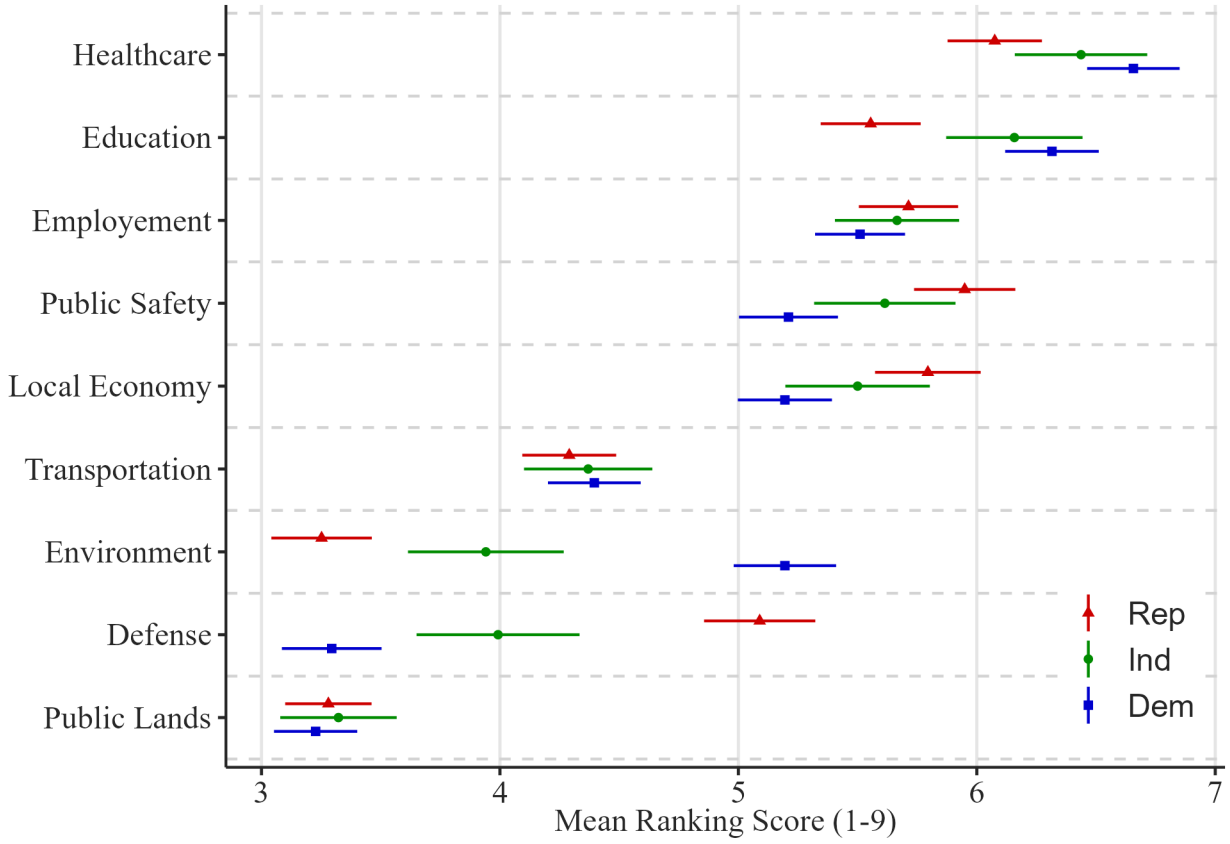


**Figure D3: Mean Spending Category Rating (0-100) Scores by Party Affiliation**

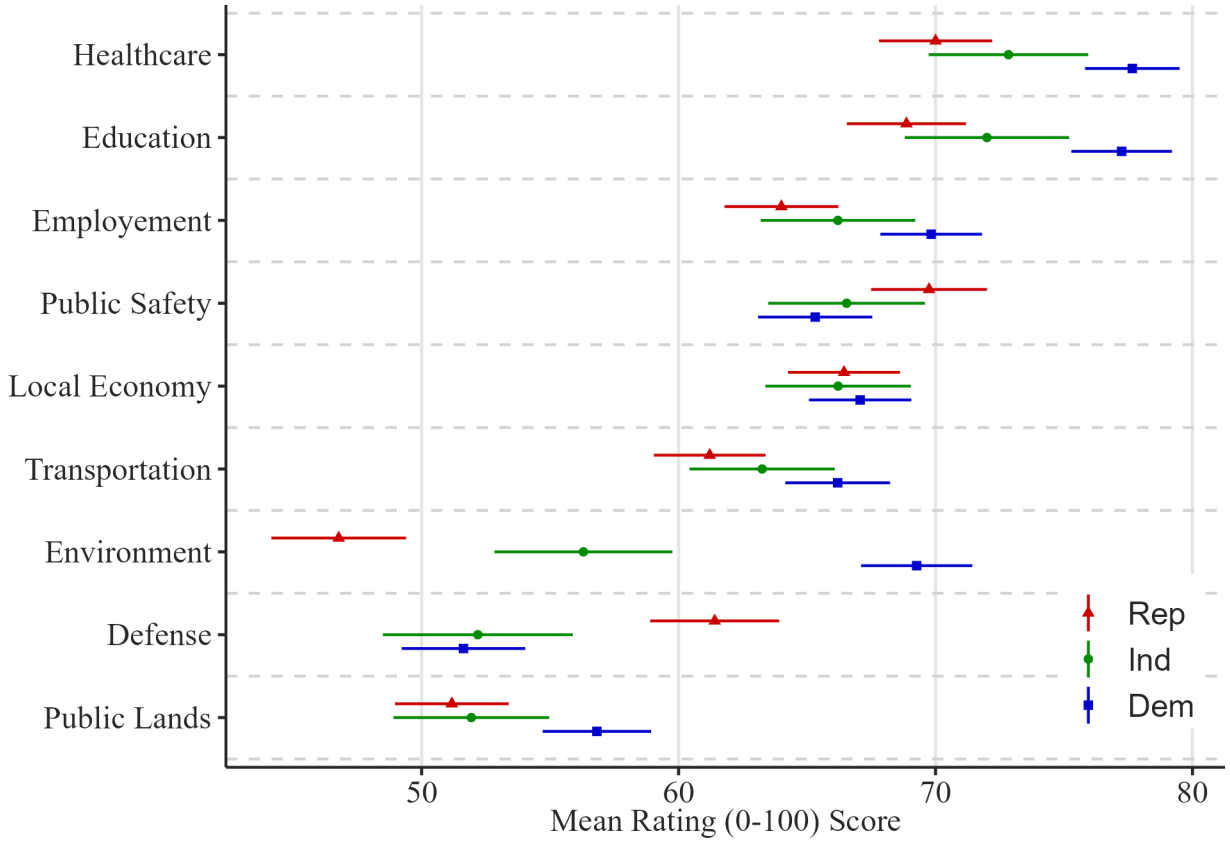


**Figure D4: Mean Spending Category Allocation Scores by Party Affiliation**

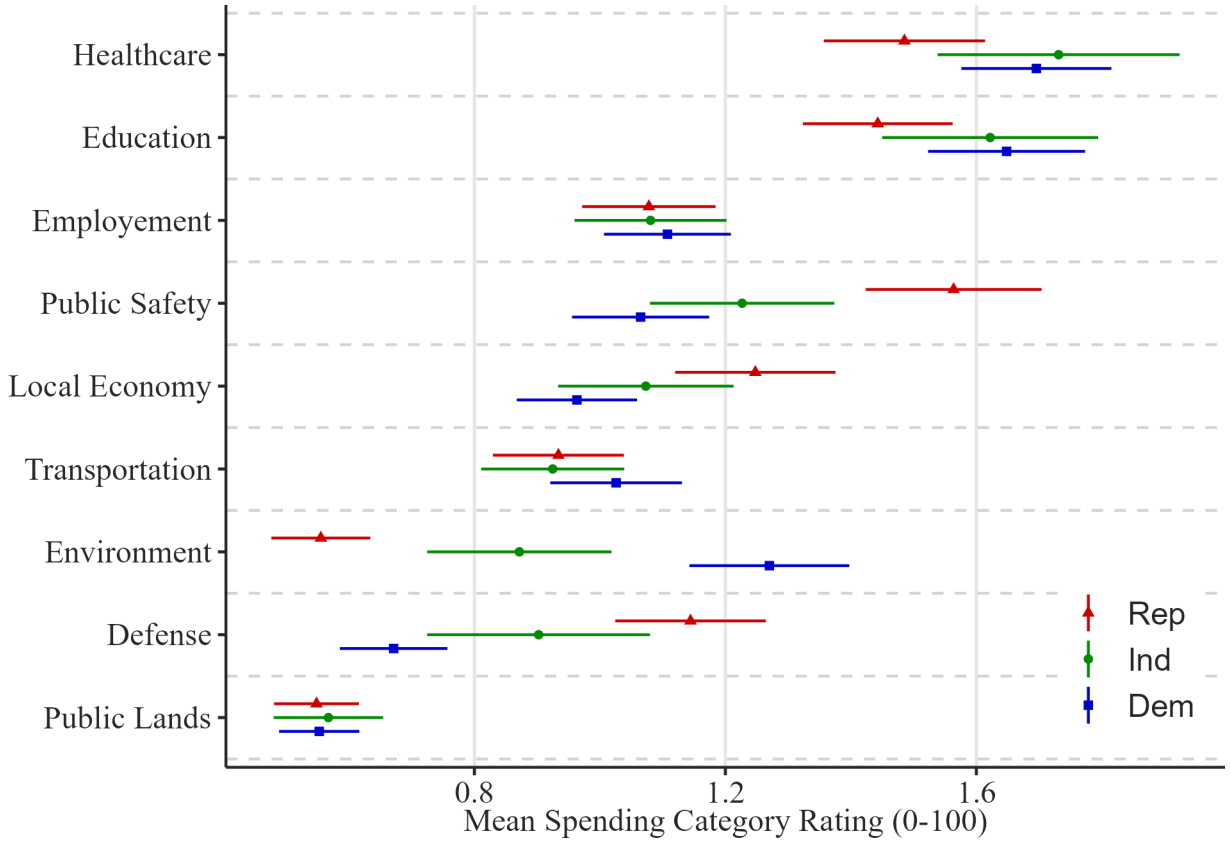


Table D1: Full Sample – Mean and 95% CI for Spending Category Ranking

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 4.124836 | 3.990046 | 4.259626 |
| Local Economy | 5.528252 | 5.405303 | 5.651202 |
| Education | 5.956636 | 5.837402 | 6.075870 |
| Employment | 5.700394 | 5.585988 | 5.814801 |
| Environment | 4.191853 | 4.060318 | 4.323388 |
| Healthcare | 6.353482 | 6.237846 | 6.469118 |
| Public Lands | 3.269382 | 3.166137 | 3.372628 |
| Public Safety | 5.534166 | 5.411274 | 5.657057 |
| Transportation | 4.340999 | 4.228445 | 4.453553 |

Table D2: Democrats – Mean and 95% CI for Spending Category Ranking

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 3.294559 | 3.085541 | 3.503578 |
| Local Economy | 5.195122 | 4.997808 | 5.392436 |
| Education | 6.315197 | 6.118938 | 6.511456 |
| Employment | 5.510319 | 5.321658 | 5.698980 |
| Environment | 5.195122 | 4.980319 | 5.409925 |
| Healthcare | 6.656660 | 6.462724 | 6.850597 |
| Public Lands | 3.227017 | 3.052442 | 3.401592 |
| Public Safety | 5.210131 | 5.002618 | 5.417644 |
| Transportation | 4.395872 | 4.201333 | 4.590412 |

Table D3: Republicans – Mean and 95% CI for Spending Category Ranking

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 5.089431 | 4.856151 | 5.322711 |
| Local Economy | 5.794715 | 5.573249 | 6.016182 |
| Education | 5.554878 | 5.345167 | 5.764589 |
| Employment | 5.713415 | 5.505261 | 5.921568 |
| Environment | 3.252032 | 3.041367 | 3.462698 |
| Healthcare | 6.075203 | 5.877326 | 6.273081 |
| Public Lands | 3.280488 | 3.099348 | 3.461627 |
| Public Safety | 5.949187 | 5.736831 | 6.161543 |
| Transportation | 4.290650 | 4.093771 | 4.487529 |

Table D4: Independents – Mean and 95% CI for Spending Category Ranking

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 3.992126 | 3.650352 | 4.333900 |
| Local Economy | 5.500000 | 5.197027 | 5.802973 |
| Education | 6.157480 | 5.871541 | 6.443420 |
| Employment | 5.665354 | 5.404924 | 5.925785 |
| Environment | 3.940945 | 3.614364 | 4.267525 |
| Healthcare | 6.437008 | 6.159256 | 6.714760 |
| Public Lands | 3.322835 | 3.078637 | 3.567032 |
| Public Safety | 5.614173 | 5.317714 | 5.910633 |
| Transportation | 4.370079 | 4.100994 | 4.639164 |

Table D5: Full Sample – Mean and 95% CI for Spending Category Rating (0-100)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 54.83114 | 53.37355 | 56.28873 |
| Economy | 66.09067 | 64.86326 | 67.31808 |
| Education | 72.30026 | 71.00596 | 73.59456 |
| Employment | 66.27070 | 65.01812 | 67.52327 |
| Environment | 57.45992 | 55.98963 | 58.93021 |
| Healthcare | 73.10972 | 71.87287 | 74.34658 |
| Public Lands | 53.40013 | 52.12944 | 54.67082 |
| Public Safety | 66.18463 | 64.86544 | 67.50381 |
| Transportation | 62.65703 | 61.41214 | 63.90192 |

Table D6: Democrats – Mean and 95% CI for Spending Category Rating

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 51.62852 | 49.22301 | 54.03403 |
| Economy | 67.06567 | 65.07473 | 69.05661 |
| Education | 77.24390 | 75.28505 | 79.20276 |
| Employment | 69.82927 | 67.85008 | 71.80846 |
| Environment | 69.26642 | 67.10260 | 71.43024 |
| Healthcare | 77.65854 | 75.81704 | 79.50003 |
| Public Lands | 56.82176 | 54.71150 | 58.93203 |
| Public Safety | 65.31707 | 63.09453 | 67.53962 |
| Transportation | 66.19137 | 64.14950 | 68.23324 |

Table D7: Republicans – Mean and 95% CI for Spending Category Rating

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 61.40447 | 58.89324 | 63.91570 |
| Economy | 66.43902 | 64.26092 | 68.61713 |
| Education | 68.86789 | 66.54735 | 71.18842 |
| Employment | 64.00203 | 61.78789 | 66.21617 |
| Environment | 46.77236 | 44.15132 | 49.39339 |
| Healthcare | 70.00203 | 67.80076 | 72.20331 |
| Public Lands | 51.17480 | 48.96029 | 53.38931 |
| Public Safety | 69.74797 | 67.49208 | 72.00386 |
| Transportation | 61.21138 | 59.03715 | 63.38561 |

Table D8: Independents – Mean and 95% CI for Spending Category Rating

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 52.18898 | 48.49404 | 55.88391 |
| Economy | 66.20866 | 63.37721 | 69.04011 |
| Education | 72.00000 | 68.80206 | 75.19794 |
| Employment | 66.20079 | 63.19456 | 69.20701 |
| Environment | 56.29528 | 52.83195 | 59.75860 |
| Healthcare | 72.83858 | 69.73254 | 75.94462 |
| Public Lands | 51.93307 | 48.90253 | 54.96361 |
| Public Safety | 66.53937 | 63.48975 | 69.58899 |
| Transportation | 63.25197 | 60.42658 | 66.07736 |

Table D9: Full Sample – Mean and 95% CI for Spending Category Allocation (0-10)

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 0.8937582 | 0.8313811 | 0.9561353 |
| Economy | 1.1183311 | 1.0540851 | 1.1825772 |
| Education | 1.5433640 | 1.4726341 | 1.6140939 |
| Employment | 1.1396846 | 1.0779847 | 1.2013846 |
| Environment | 0.8962549 | 0.8336959 | 0.9588140 |
| Healthcare | 1.6449409 | 1.5698277 | 1.7200541 |
| Public Lands | 0.5576216 | 0.5186497 | 0.5965934 |
| Public Safety | 1.2630749 | 1.1938920 | 1.3322578 |
| Transportation | 0.9429698 | 0.8865317 | 0.9994079 |

Table D10: Democrats – Mean and 95% CI for Spending Category Allocation

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 0.6712946 | 0.5855567 | 0.7570325 |
| Economy | 0.9634146 | 0.8675427 | 1.0592865 |
| Education | 1.6482176 | 1.5230693 | 1.7733660 |
| Employment | 1.1076923 | 1.0065567 | 1.2088279 |
| Environment | 1.2701689 | 1.1427289 | 1.3976088 |
| Healthcare | 1.6956848 | 1.5760937 | 1.8152759 |
| Public Lands | 0.5527205 | 0.4887803 | 0.6166606 |
| Public Safety | 1.0649156 | 0.9556900 | 1.1741412 |
| Transportation | 1.0258912 | 0.9208534 | 1.1309290 |

Table D11: Republicans – Mean and 95% CI for Spending Category Allocation

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 1.1445122 | 1.0245853 | 1.2644391 |
| Economy | 1.2477642 | 1.1199837 | 1.3755447 |
| Education | 1.4428862 | 1.3235461 | 1.5622262 |
| Employment | 1.0780488 | 0.9717336 | 1.1843639 |
| Environment | 0.5552846 | 0.4764274 | 0.6341417 |
| Healthcare | 1.4853659 | 1.3568379 | 1.6138938 |
| Public Lands | 0.5483740 | 0.4808084 | 0.6159396 |
| Public Safety | 1.5638211 | 1.4237163 | 1.7039259 |
| Transportation | 0.9339431 | 0.8295372 | 1.0383489 |

Table D12: Independents – Mean and 95% CI for Spending Category Allocation

| Type | Mean | Lower %95 CI | Upper %95 CI |
|---|---|---|---|
| Defense | 0.9023622 | 0.7247941 | 1.0799303 |
| Economy | 1.0732283 | 0.9334740 | 1.2129827 |
| Education | 1.6220472 | 1.4498705 | 1.7942240 |
| Employment | 1.0807087 | 0.9596440 | 1.2017733 |
| Environment | 0.8716535 | 0.7248077 | 1.0184994 |
| Healthcare | 1.7311024 | 1.5382378 | 1.9239669 |
| Public Lands | 0.5673228 | 0.4800398 | 0.6546059 |
| Public Safety | 1.2267717 | 1.0798115 | 1.3737318 |
| Transportation | 0.9248031 | 0.8107566 | 1.0388497 |

# Appendix E: Conjoint Experiment Results

To further validate the spending demand measurement strategy, I included a conjoint experiment at the end of the second survey. Participants were assigned to 10 conjoint tasks with two projects per task (20 total projects). In each task, respondents chose between two potential federal grants for their community. The projects randomly varied by the following four attributes: dollar amount (7 levels), project type ranking (9 levels, based on the earlier ranking measure of spending demand), grant recipient (3 levels), and project timeline (5 levels). Below, I estimate average marginal component effects (AMCE) using OLS regression, regressing the project choice variable on the four categorical attributes. I cluster standard errors on respondents.

**Figure E1: Conjoint Experiment Results**

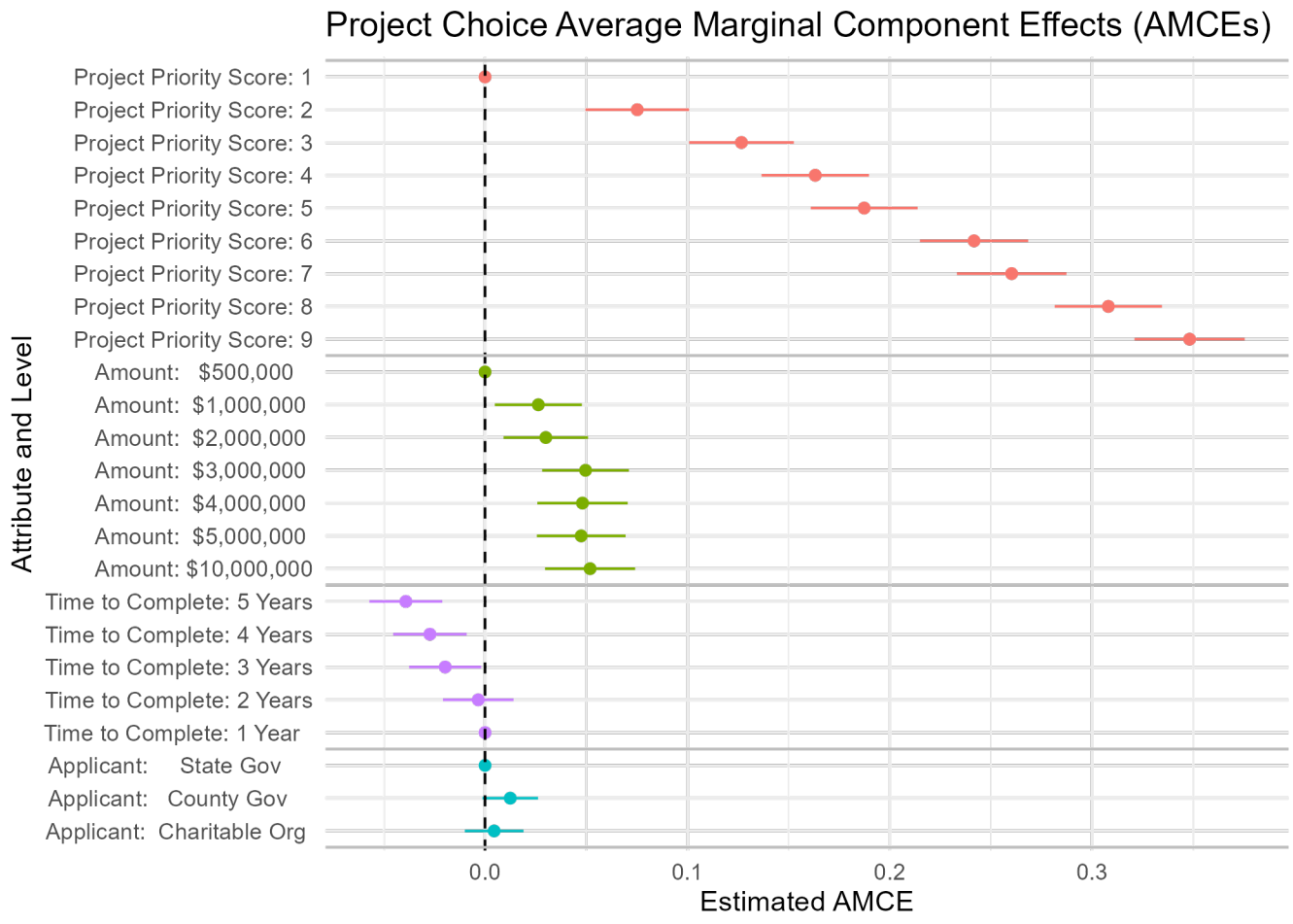



Figure E1 shows a clearly positive relationship between respondents' earlier project ranking

and their likelihood of choosing a project in a conjoint experiment. A medium priority project (5th) is 19% more likely to be chosen than the lowest priority project, and a top priority project is 35% more likely to be chosen than the lowest priority project. These results validate the ranking measure of project demand employed in the manuscript, as project raking scores drive results in the conjoint experiment.

# Appendix F: Experiment #1 Tabular Regression Results:

Table F1: DV – Feeling Therm. (Full Sample)

| | | | |
|---|---|---|---|
| Rating | 0.15*** | | |
| | (0.02) | | |
| Allocation | | 0.09*** | |
| | | (0.02) | |
| Ranking | | | 0.11*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 3277 | 3277 | 3277 |
| R2 | 0.023 | 0.008 | 0.012 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F2: DV – Effectiveness (Full Sample)

| | | | |
|---|---|---|---|
| Rating | 0.14*** | | |
| | (0.02) | | |
| Allocation | | 0.07*** | |
| | | (0.02) | |
| Ranking | | | 0.10*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 3277 | 3277 | 3277 |
| R2 | 0.021 | 0.005 | 0.010 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F3: DV – Representation (Full Sample)

| | | | |
|---|---|---|---|
| Rating | 0.15*** | | |
| | (0.02) | | |
| Allocation | | 0.10*** | |
| | | (0.02) | |
| Ranking | | | 0.12*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 3277 | 3277 | 3277 |
| R2 | 0.022 | 0.011 | 0.013 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F4: DV – Fiscal Responsibility (Full Sample)

| | | | |
|---|---|---|---|
| Rating | 0.13*** | | |
| | (0.02) | | |
| Allocation | | 0.09*** | |
| | | (0.02) | |
| Ranking | | | 0.10*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 3277 | 3277 | 3277 |
| R2 | 0.018 | 0.008 | 0.011 |

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

Table F5: DV – Feeling Therm. (Democratic Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.15*** | | |
| | (0.02) | | |
| Allocation | | 0.09*** | |
| | | (0.02) | |
| Ranking | | | 0.11*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 1759 | 1759 | 1759 |
| R2 | 0.022 | 0.008 | 0.012 |

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

Table F6: DV – Effectiveness (Democratic Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.12*** | | |
| | (0.02) | | |
| Allocation | | 0.07*** | |
| | | (0.02) | |
| Ranking | | | 0.10*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 1759 | 1759 | 1759 |
| R2 | 0.015 | 0.004 | 0.010 |

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

# Appendix G: Experiment #2 Tabular Regression Results:

Table F7: DV – Representation (Democratic Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.14*** | | |
| | (0.02) | | |
| Allocation | | 0.09*** | |
| | | (0.02) | |
| Ranking | | | 0.11*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 1759 | 1759 | 1759 |
| R2 | 0.020 | 0.008 | 0.012 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F8: DV – Fiscal Responsibility (Democratic Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.13*** | | |
| | (0.02) | | |
| Allocation | | 0.08*** | |
| | | (0.02) | |
| Ranking | | | 0.12*** |
| | | | (0.02) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.02) | (0.02) | (0.02) |
| Num.Obs. | 1759 | 1759 | 1759 |
| R2 | 0.017 | 0.006 | 0.015 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F9: DV – Feeling Therm. (Republican Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.16*** | | |
| | (0.03) | | |
| Allocation | | 0.09*** | |
| | | (0.03) | |
| Ranking | | | 0.11*** |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1513 | 1513 | 1513 |
| R2 | 0.026 | 0.007 | 0.012 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F10: DV – Effectiveness (Republican Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.16*** | | |
| | (0.03) | | |
| Allocation | | 0.07*** | |
| | | (0.03) | |
| Ranking | | | 0.10*** |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1513 | 1513 | 1513 |
| R2 | 0.027 | 0.005 | 0.010 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F11: DV – Representation (Republican Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.15*** | | |
| | (0.03) | | |
| Allocation | | 0.12*** | |
| | | (0.03) | |
| Ranking | | | 0.12*** |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1513 | 1513 | 1513 |
| R2 | 0.024 | 0.015 | 0.015 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F12: DV – Fiscal Responsibility (Republican Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.14*** | | |
| | (0.03) | | |
| Allocation | | 0.09*** | |
| | | (0.03) | |
| Ranking | | | 0.08*** |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1513 | 1513 | 1513 |
| R2 | 0.019 | 0.009 | 0.007 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F13: DV – Feeling Therm. (Independent Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.10*** | | |
| | (0.03) | | |
| Allocation | | 0.12*** | |
| | | (0.03) | |
| Ranking | | | 0.11*** |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1087 | 1087 | 1087 |
| R2 | 0.010 | 0.014 | 0.013 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F14: DV – Effectiveness (Independent Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.08** | | |
| | (0.03) | | |
| Allocation | | 0.07** | |
| | | (0.03) | |
| Ranking | | | 0.07** |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1087 | 1087 | 1087 |
| R2 | 0.006 | 0.005 | 0.005 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F15: DV – Representation (Independent Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.07** | | |
| | (0.03) | | |
| Allocation | | 0.10*** | |
| | | (0.03) | |
| Ranking | | | 0.08*** |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1087 | 1087 | 1087 |
| R2 | 0.005 | 0.010 | 0.006 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table F16: DV – Fiscal Responsibility (Independent Respondents)

| | | | |
|---|---|---|---|
| Rating | 0.06** | | |
| | (0.03) | | |
| Allocation | | 0.08*** | |
| | | (0.03) | |
| Ranking | | | 0.06* |
| | | | (0.03) |
| Constant | 0.00 | 0.00 | 0.00 |
| | (0.03) | (0.03) | (0.03) |
| Num.Obs. | 1087 | 1087 | 1087 |
| R2 | 0.004 | 0.006 | 0.003 |

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

Table F17: Treatment X Party Interaction (DV – Feeling Therm.)

| | | | |
|---|---|---|---|
| Project Priority: Rating | 0.13*** | | |
| | (0.02) | | |
| Project Priority: Allocation | | 1.76*** | |
| | | (0.46) | |
| Project Priority: Ranking | | | 1.04*** |
| | | | (0.22) |
| Republican Resp. | -0.56 | -0.30 | -0.54 |
| | (2.11) | (1.10) | (1.82) |
| Project Priority X Republican Resp. | 0.01 | -0.25 | -0.01 |
| | (0.03) | (0.65) | (0.32) |
| Constant | -0.82 | 5.86*** | 2.66** |
| | (1.49) | (0.76) | (1.23) |
| Num.Obs. | 3272 | 3272 | 3272 |
| R2 | 0.024 | 0.008 | 0.012 |

* p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01

Table F18: Treatment X Party Interaction (DV – Effectiveness)

| | | | |
|---|---|---|---|
| Project Priority: Rating | 0.01*** | | |
| | (0.00) | | |
| Project Priority: Allocation | | 0.12*** | |
| | | (0.04) | |
| Project Priority: Ranking | | | 0.09*** |
| | | | (0.02) |
| Republican Resp. | -0.23 | -0.08 | -0.07 |
| | (0.19) | (0.10) | (0.17) |
| Project Priority X Republican Resp. | 0.00 | 0.00 | 0.00 |
| | (0.00) | (0.06) | (0.03) |
| Constant | 0.01 | 0.54*** | 0.24** |
| | (0.14) | (0.07) | (0.11) |
| Num.Obs. | 3272 | 3272 | 3272 |
| R2 | 0.021 | 0.005 | 0.010 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table F19: Treatment X Party Interaction (DV – Representation)

| | | | |
|---|---|---|---|
| Project Priority: Rating | 0.01*** | | |
| | (0.00) | | |
| Project Priority: Allocation | | 0.15*** | |
| | | (0.04) | |
| Project Priority: Ranking | | | 0.09*** |
| | | | (0.02) |
| Republican Resp. | -0.06 | -0.09 | -0.10 |
| | (0.19) | (0.10) | (0.16) |
| Project Priority X Republican Resp. | 0.00 | 0.04 | 0.01 |
| | (0.00) | (0.06) | (0.03) |
| Constant | -0.32** | 0.23*** | -0.05 |
| | (0.13) | (0.07) | (0.11) |
| Num.Obs. | 3272 | 3272 | 3272 |
| R2 | 0.022 | 0.011 | 0.013 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table F20: Treatment X Party Interaction (DV – Fiscal Responsibility)

| | | | |
|---|---|---|---|
| Project Priority: Rating | 0.01*** | | |
| | (0.00) | | |
| Project Priority: Allocation | | 0.13*** | |
| | | (0.04) | |
| Project Priority: Ranking | | | 0.10*** |
| | | | (0.02) |
| Republican Resp. | -0.09 | -0.10 | 0.07 |
| | (0.18) | (0.09) | (0.16) |
| Project Priority X Republican Resp. | 0.00 | 0.01 | -0.03 |
| | (0.00) | (0.06) | (0.03) |
| Constant | -0.26** | 0.24*** | -0.09 |
| | (0.13) | (0.07) | (0.11) |
| Num.Obs. | 3272 | 3272 | 3272 |
| R2 | 0.018 | 0.008 | 0.011 |

* p < 0.1, ** p < 0.05, *** p < 0.01

Table G1: Experiment 2 Regression Results

| | Thermometer (0-100) | Effectiveness (0-10) | Representation (0-10) | Responsibility (0-10) |
|---|---|---|---|---|
| Project Priority | 3.87*** | 0.34*** | 0.39*** | 0.27** |
| | (1.14) | (0.11) | (0.10) | (0.11) |
| Senator Influence | 0.39 | 0.24** | 0.11 | 0.14 |
| | (1.14) | (0.11) | (0.10) | (0.11) |
| Constant | 7.63*** | 0.55*** | 0.36*** | 0.35*** |
| | (1.00) | (0.10) | (0.09) | (0.10) |
| Num.Obs. | 1522 | 1522 | 1522 | 1522 |
| R2 | 0.008 | 0.009 | 0.010 | 0.005 |

* p < 0.1, ** p < 0.05, *** p < 0.01

# Chapter 4 Appendix

**Table A1: Demographics of Survey Sample**

| Gender | Race/Ethnicity | Ideology |
|---|---|---|
| 53% Women | 69% White | 10% Very liberal |
| 47% Men | 11% Black | 13% Liberal |
| | 7% Latino | 9% Slightly liberal |
| | 6% Mixed | 37% Moderate |
| | 4% Asian | 9% Slightly conservative |
| | 2% Other | 13% Conservative |
| | | 9% Very conservative |
| **Party** | **Region** | **Religious Attendance** |
| 37% Democrats | 37% South | 40% Never |
| 31% Republicans | 23% West | 26% Few times a year |
| 31% Independents | 20% Northeast | 8% Once or twice a month |
| | 19% Midwest | 8% Almost every week |
| | | 17% Every week |
| **Income** | **Age** | **Education** |
| Median: $40,000-$49,999 | Median: 45 | 3% Less than high school diploma |
| | | 27% High school graduate |
| | | 22% Some college |
| | | 7% 2-year degree |
| | | 24% 4-year degree |
| | | 16% Post-graduate education |

**Table A2: MC Gender Treatment Balance**

| Variable | **Man MC**, N = 1,086[1] | **Woman MC**, N = 1,023[1] | p-value[2] |
|---|---|---|---|
| Respondent Gender | | | 0.7 |
| *man* | 504 (46%) | 485 (47%) | |
| *woman* | 580 (54%) | 538 (53%) | |
| Race | | | 0.5 |
| *White* | 749 (69%) | 711 (70%) | |
| *Asian American* | 43 (4.0%) | 48 (4.7%) | |
| *Black* | 116 (11%) | 122 (12%) | |
| *Latino* | 83 (7.6%) | 60 (5.9%) | |
| *Mixed* | 69 (6.4%) | 62 (6.1%) | |
| *Other* | 26 (2.4%) | 19 (1.9%) | |
| Party | | | 0.3 |
| *dem* | 404 (37%) | 383 (38%) | |
| *ind* | 352 (32%) | 303 (30%) | |
| *rep* | 330 (30%) | 335 (33%) | |
| Education | | | 0.3 |
| *1* | 27 (2.5%) | 30 (2.9%) | |
| *2* | 287 (27%) | 285 (28%) | |
| *3* | 244 (23%) | 226 (22%) | |
| *4* | 71 (6.6%) | 67 (6.6%) | |
| *5* | 252 (23%) | 262 (26%) | |
| *6* | 198 (18%) | 149 (15%) | |
| Age | 45 (32, 61) | 45 (32, 60) | >0.9 |
| Income | 4 (2, 7) | 4 (2, 7) | >0.9 |

[1] n (%); Median (IQR)

[2] Pearson's Chi-squared test; Wilcoxon rank sum test

**Table A3: Message Treatment Balance**

| Variable | control, N = 710[1] | claim, N = 702[1] | critique, N = 696[1] | p-value[2] |
|---|---|---|---|---|
| Gender | | | | 0.3 |
| *man* | 324 (46%) | 347 (50%) | 318 (46%) | |
| *woman* | 386 (54%) | 354 (50%) | 378 (54%) | |
| Race | | | | 0.8 |
| *White* | 489 (69%) | 479 (68%) | 492 (71%) | |
| *Asian American* | 34 (4.8%) | 30 (4.3%) | 27 (3.9%) | |
| *Black* | 82 (12%) | 85 (12%) | 71 (10%) | |
| *Latino* | 48 (6.8%) | 44 (6.3%) | 50 (7.2%) | |
| *Mixed* | 39 (5.5%) | 51 (7.3%) | 41 (5.9%) | |
| *Other* | 18 (2.5%) | 12 (1.7%) | 15 (2.2%) | |
| Party | | | | 0.2 |
| *dem* | 280 (39%) | 264 (38%) | 242 (35%) | |
| *ind* | 203 (29%) | 233 (33%) | 219 (32%) | |
| *rep* | 226 (32%) | 205 (29%) | 234 (34%) | |
| Education | | | | 0.7 |
| *1* | 20 (2.8%) | 20 (2.9%) | 17 (2.5%) | |
| *2* | 195 (28%) | 200 (29%) | 177 (26%) | |
| *3* | 167 (24%) | 138 (20%) | 165 (24%) | |
| *4* | 39 (5.5%) | 51 (7.3%) | 48 (7.0%) | |
| *5* | 170 (24%) | 179 (26%) | 165 (24%) | |
| *6* | 118 (17%) | 111 (16%) | 118 (17%) | |
| Age | 45 (32, 60) | 45 (32, 60) | 45 (31, 62) | 0.7 |
| Income | 4 (2, 7) | 4 (2, 7) | 4 (2, 7) | 0.4 |

[1] n (%); Median (IQR)

[2] Pearson's Chi-squared test; Kruskal-Wallis rank sum test

## Treatment Vignette Language

**Control**

*Please read the following news* excerpt *about a member of Congress. After which, you will be asked to evaluate the member.*

**{Congresswoman/Congresswoman} Anderson Announces Winner of Art Competition**

Today, Congresswoman/Congressman Madeline/Matthew Anderson (D/R | MN) announced that 17-year-old Nate Beyer won first place in the annual congressional district art competition. Nate's winning entry, "Sunsets," was created using colored pencils and will now be displayed in the U.S. Capitol with other winning entries. More than 30 students competed is this year's art competition.

**Credit Claim**

*Please read the following news* excerpt *about a member of Congress. After which, you will be asked to evaluate the member.*

**{Congresswoman/Congresswoman} Anderson Secures $44 Million for Local Road and Highway Projects**

Today, Congresswoman/Congressman Madeline/Matthew Anderson (D/R | MN)) released the following statement after securing more than $44 million in funding within the Fiscal Year 2023 appropriations bill for local road and highway projects in her district.

"I am proud of our efforts to secure this funding, which will play a critical role in uplifting our people and unlocking our region's full potential," said Congresswoman Anderson. "The millions of dollars coming home will support good-paying jobs, revitalize neighborhoods, and drive our economy forward."

**Credit Claim with Spending Critique**

*Please read the following news* excerpt *about a member of Congress. After which, you will be asked to evaluate the member.*

**{Congresswoman/Congresswoman} Anderson Secures $44 Million for Local Road and Highway Projects**

Today, Congresswoman/Congressman Madeline/Matthew Anderson (D/R | MN) released the following statement after securing more than $44 million in funding within the Fiscal Year 2023 appropriations bill for local road and highway projects in her district.
"I am proud of our efforts to secure this funding, which will play a critical role in uplifting our people and unlocking our region's full potential," said Congresswoman Anderson. "The millions of dollars coming home will support good-paying jobs, revitalize neighborhoods, and drive our economy forward."

The nonpartisan Committee for a Responsible Federal Budget reported that the appropriations bill is wasteful and contributes to the growing federal deficit. "Given that the national debt has grown by $1.1 trillion since January, lawmakers' decision to add more spending and borrowing in this appropriations package is a tremendous mistake. The full spending package is estimated to cost $1.6 trillion and makes our debt and inflationary problems worse."

## Table A4: Difference of Means Tests of MC Gender: General Support

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| Control | 4.802 | 5.012 | -0.210 | 0.047** |
| Credit Claim | 5.302 | 5.393 | -0.090 | 0.354 |
| Credit Claim with Critique | 4.542 | 4.601 | -0.059 | 0.635 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC General Support (1-7).

**Table A5: ANOVA with Tukey Comparisons: General Support (Woman MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Woman - Claim / Woman | -0.380 | 0.001*** |
| Critique / Woman - Claim / Woman | -0.792 | 0.000*** |
| Critique / Woman - Control / Woman | -0.411 | 0.001*** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC General Support (1-7).

**Table A6: ANOVA with Tukey Comparisons: General Support (Man MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Man - Claim / Man | -0.500 | 0.000*** |
| Critique / Man - Claim / Man | -0.760 | 0.000*** |
| Critique / Man - Control / Man | -0.260 | 0.040** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC General Support (1-7).

**Table A7: ANOVA with Tukey Comparisons: General Support**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Woman - Control / Man | 0.210 | 0.385 |
| Claim / Woman - Claim / Man | 0.090 | 0.963 |
| Critique / Woman - Critique / Man | 0.059 | 0.995 |
| Control / Woman - Claim / Woman | -0.380 | 0.007*** |
| Critique / Woman - Control / Woman | -0.411 | 0.004*** |
| Critique / Woman - Claim / Woman | -0.792 | 0.000*** |
| Control / Man - Claim / Man | -0.500 | 0.000*** |
| Critique / Man - Control / Man | -0.260 | 0.135 |
| Critique / Man - Claim / Man | -0.760 | 0.000*** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Comparisons that vary on both substance condition and MC gender were included in the analysis but removed from the table for interpretation clarity. Dependent variable = MC General Support (1-7).

**Table A8: Difference of Means Tests of MC Gender: General Support (Partisan Subsets)**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| **Republican MC / Copartisan** | | | | |
| Control | 4.864 | 5.075 | -0.210 | 0.315 |
| Credit Claim | 5.460 | 5.429 | 0.032 | 0.852 |
| Credit Claim with Critique | 4.623 | 4.534 | 0.089 | 0.728 |
| **Republican MC / Non-copartisan** | | | | |
| Control | 4.565 | 5.114 | -0.549 | 0.025** |
| Credit Claim | 5.138 | 5.267 | -0.129 | 0.499 |
| Credit Claim with Critique | 4.347 | 4.841 | -0.494 | 0.052* |
| **Democratic MC / Copartisan** | | | | |
| Control | 5.178 | 5.167 | 0.012 | 0.949 |
| Credit Claim | 5.657 | 5.633 | 0.024 | 0.900 |
| Credit Claim with Critique | 5.011 | 4.912 | 0.099 | 0.631 |
| **Democratic MC / Non-copartisan** | | | | |
| Control | 4.590 | 4.658 | -0.068 | 0.741 |
| Credit Claim | 4.861 | 5.225 | -0.364 | 0.101 |
| Credit Claim with Critique | 4.245 | 4.030 | 0.215 | 0.429 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC General Support (1-7).

**Table A9: Difference of Means Tests of MC Gender: Effectiveness**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| Control | 4.642 | 4.727 | -0.085 | 0.373 |
| Credit Claim | 5.321 | 5.451 | -0.130 | 0.166 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Effectiveness (1-7).

**Table A10: Difference of Means Test of Message: Effectiveness (Woman MC Subset)**

| Credit Claim | Control | Diff. of Means | P-value |
|---|---|---|---|
| 5.451 | 4.727 | 0.724 | 0.000*** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Effectiveness (1-7).

**Table A11: Difference of Means Test of Message: Effectiveness (Man MC Subset)**

| Credit Claim | Control | Diff. of Means | P-value |
|---|---|---|---|
| 5.321 | 4.642 | 0.679 | 0.000*** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Effectiveness (1-7).

**Table A12: ANOVA with Tukey Comparisons: MC Effectiveness**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Claim / Woman - Claim / Man | 0.130 | 0.519 |
| Control / Woman - Control / Man | 0.085 | 0.803 |
| Control / Woman - Claim / Woman | -0.724 | 0.000*** |
| Control / Man - Claim / Man | -0.679 | 0.000*** |

* p < 0.1, ** p < 0.05, *** p < 0.01

NOTE: Comparisons that vary on both substance condition and MC gender were included in the analysis but removed from the table for interpretation clarity. Dependent variable = MC Effectiveness (1-7).

**Figure A1: Mean MC Effectiveness Across Treatment Group and Party Affiliation**

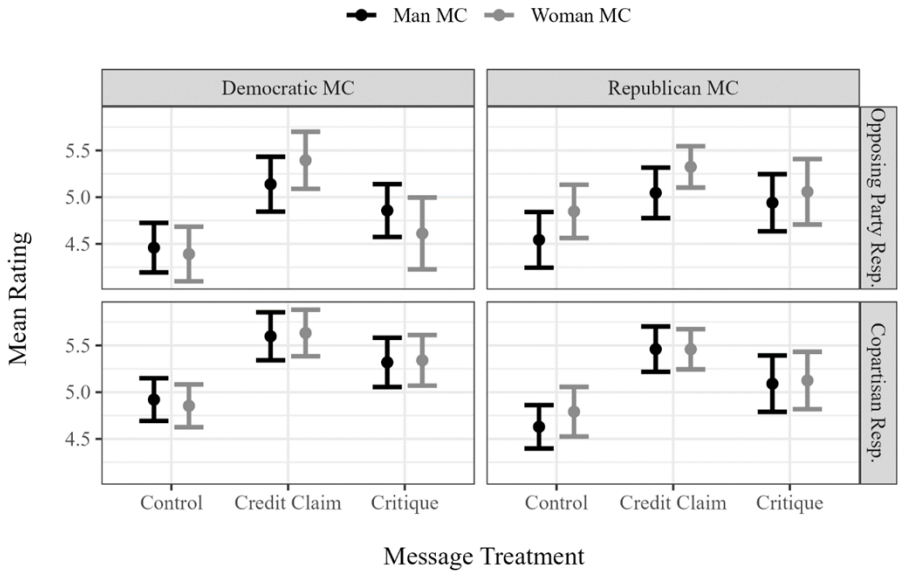


**Table A13: Difference of Means Tests of MC Gender: MC Effectiveness (Partisan Subsets)**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| **Republican MC / Copartisan** | | | | |
| Control | 4.630 | 4.791 | -0.161 | 0.371 |
| Credit Claim | 5.460 | 5.459 | 0.001 | 0.995 |
| Credit Claim with Critique | 5.091 | 5.125 | -0.034 | 0.877 |
| **Republican MC / Non-copartisan** | | | | |
| Control | 4.543 | 4.848 | -0.305 | 0.149 |
| Credit Claim | 5.046 | 5.324 | -0.278 | 0.120 |
| Credit Claim with Critique | 4.941 | 5.058 | -0.117 | 0.621 |
| **Democratic MC / Copartisan** | | | | |
| Control | 4.921 | 4.854 | 0.067 | 0.687 |
| Credit Claim | 5.598 | 5.633 | -0.035 | 0.847 |
| Credit Claim with Critique | 5.319 | 5.341 | -0.022 | 0.909 |
| **Democratic MC / Non-copartisan** | | | | |
| Control | 4.460 | 4.392 | 0.068 | 0.737 |
| Credit Claim | 5.139 | 5.394 | -0.255 | 0.239 |
| Credit Claim with Critique | 4.857 | 4.612 | 0.245 | 0.316 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Effectiveness (1-7).

**Table A14: Difference of Means Tests of MC Gender: Fiscal Responsibility**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| Credit Claim | 5.009 | 5.168 | -0.158 | 0.109 |
| Credit Claim with Critique | 4.420 | 4.513 | -0.093 | 0.470 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Fiscal Responsibility (1-7).

**Table A15: Difference of Means Test of Message: Responsibility (Woman MC Subset)**

| Credit Claim | Credit Claim with Critique | Diff. of Means | P-value |
|---|---|---|---|
| 5.168 | 4.513 | 0.655 | 0.000*** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Fiscal Responsibility (1-7).

**Table A16: Difference of Means Test of Message: Responsibility (Man MC Subset)**

| Credit Claim | Credit Claim with Critique | Diff. of Means | P-value |
|---|---|---|---|
| 5.009 | 4.420 | 0.590 | 0.000*** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Fiscal Responsibility (1-7).

**Table A17: ANOVA with Tukey Comparisons: MC Fiscal Responsibility**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Critique Woman - Critique Man | 0.093 | 0.848 |
| Claim Woman - Claim Man | 0.158 | 0.506 |
| Critique Woman - Claim Woman | -0.655 | 0.000*** |
| Critique Man - Claim Man | -0.590 | 0.000*** |

* p < 0.1, ** p < 0.05, *** p < 0.01

NOTE: Comparisons that vary on both substance condition and MC gender were included in the analysis but removed from the table for interpretation clarity. Dependent variable = MC Fiscal Responsibility (1-7).

**Figure A2: Mean MC Fiscal Responsibility Across Treatment Group and Party Affiliation**

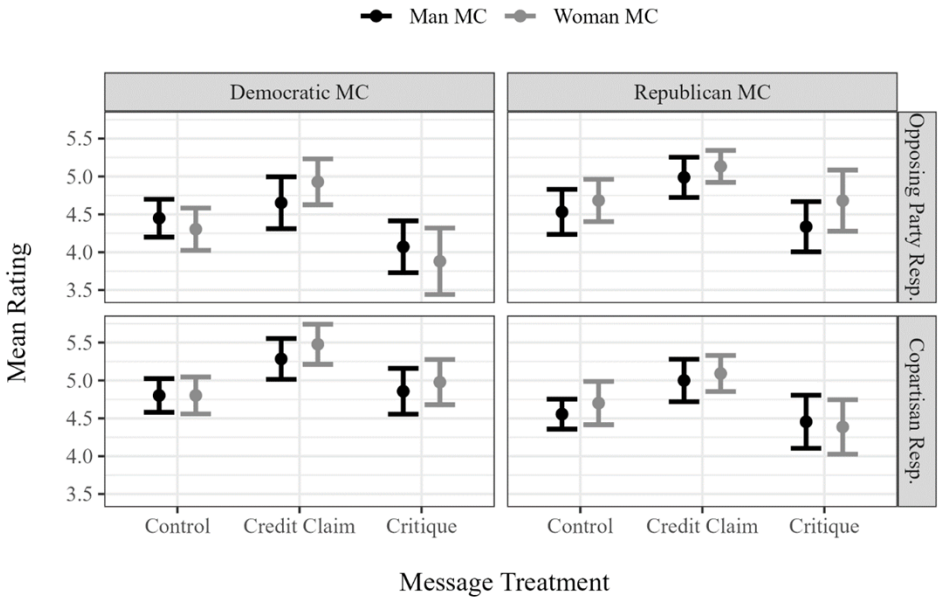


**Table A18: Difference of Means Tests of MC Gender: Responsibility (Partisan Subsets)**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| **Republican MC / Copartisan** | | | | |
| Control | 4.556 | 4.701 | -0.146 | 0.412 |
| Credit Claim | 5.000 | 5.092 | -0.092 | 0.625 |
| Credit Claim with Critique | 4.455 | 4.386 | 0.068 | 0.790 |
| **Republican MC / Non-copartisan** | | | | |
| Control | 4.533 | 4.684 | -0.151 | 0.469 |
| Credit Claim | 4.989 | 5.133 | -0.145 | 0.403 |
| Credit Claim with Critique | 4.337 | 4.681 | -0.345 | 0.198 |
| **Democratic MC / Copartisan** | | | | |
| Control | 4.802 | 4.802 | -0.000 | 1.000 |
| Credit Claim | 5.284 | 5.478 | -0.193 | 0.317 |
| Credit Claim with Critique | 4.857 | 4.978 | -0.121 | 0.578 |
| **Democratic MC / Non-copartisan** | | | | |
| Control | 4.450 | 4.304 | 0.146 | 0.445 |
| Credit Claim | 4.653 | 4.930 | -0.277 | 0.238 |
| Credit Claim with Critique | 4.071 | 3.881 | 0.191 | 0.502 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Fiscal Responsibility (1-7).

**Figure A3: Mean MC Effectiveness Ratings**


**Table A19: Difference of Means Tests of MC Gender: Perceived Ideology**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| Control | 4.316 | 4.307 | 0.008 | 0.935 |
| Credit Claim | 4.269 | 4.302 | -0.034 | 0.731 |
| Credit Claim with Critique | 4.497 | 4.668 | -0.170 | 0.107 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Ideology (1-7).

**Table A20: ANOVA with Tukey Comparisons: Perceived Ideology (Woman MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Woman - Claim / Woman | 0.005 | 0.998 |
| Critique / Woman - Claim / Woman | 0.366 | 0.001*** |
| Critique / Woman - Control / Woman | 0.360 | 0.002*** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Ideology (1-7).

**Table A21: ANOVA with Tukey Comparisons: Perceived Ideology (Man MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Man - Claim / Man | 0.047 | 0.886 |
| Critique / Man - Claim / Man | 0.229 | 0.060* |
| Critique / Man - Control / Man | 0.182 | 0.146 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Ideology (1-7).

**Figure A4: Mean MC Perceived Ideology Across Treatment Group and Party Affiliation**

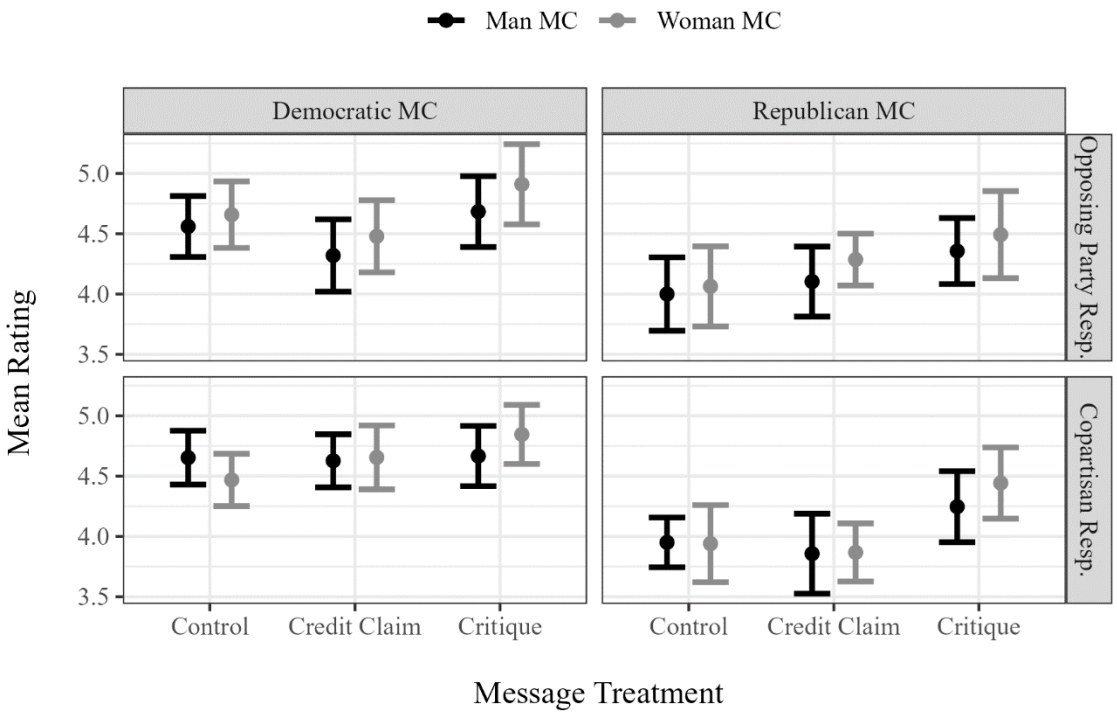


**Table A22: Difference of Means Tests of MC Gender: Perceived Ideology (Partisan Subsets)**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| **Republican MC / Copartisan** | | | | |
| Control | 3.951 | 3.940 | 0.010 | 0.958 |
| Credit Claim | 3.857 | 3.867 | -0.010 | 0.961 |
| Credit Claim with Critique | 4.247 | 4.443 | -0.196 | 0.357 |
| **Republican MC / Non-copartisan** | | | | |
| Control | 4.000 | 4.063 | -0.063 | 0.783 |
| Credit Claim | 4.103 | 4.286 | -0.182 | 0.324 |
| Credit Claim with Critique | 4.356 | 4.493 | -0.136 | 0.556 |
| **Democratic MC / Copartisan** | | | | |
| Control | 4.653 | 4.469 | 0.185 | 0.246 |
| Credit Claim | 4.627 | 4.656 | -0.028 | 0.873 |
| Credit Claim with Critique | 4.667 | 4.846 | -0.179 | 0.316 |
| **Democratic MC / Non-copartisan** | | | | |
| Control | 4.560 | 4.658 | -0.098 | 0.607 |
| Credit Claim | 4.319 | 4.479 | -0.159 | 0.462 |
| Credit Claim with Critique | 4.684 | 4.910 | -0.227 | 0.318 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Ideology (1-7).

**Table A23: Perceived Ideology Mediation Analysis**

| | Message Mediation | MC Gender Mediation |
|---|---|---|
| ACME | 0.004 | 0.000 |
| | [-0.013, 0.022] | [-0.007, 0.007] |
| ADE | -0.335*** | 0.138 |
| | [-0.495, -0.176] | [-0.023, 0.296] |
| Total Effect | -0.331*** | 0.138 |
| | [-0.486, -0.170] | [-0.023, 0.297] |
| Prop. Mediated | -0.010 | 0.000 |
| | [-0.074, 0.043] | [-0.090, 0.139] |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC General Support (1-7).

**Figure A5: Mean MC Vote Intention Ratings**

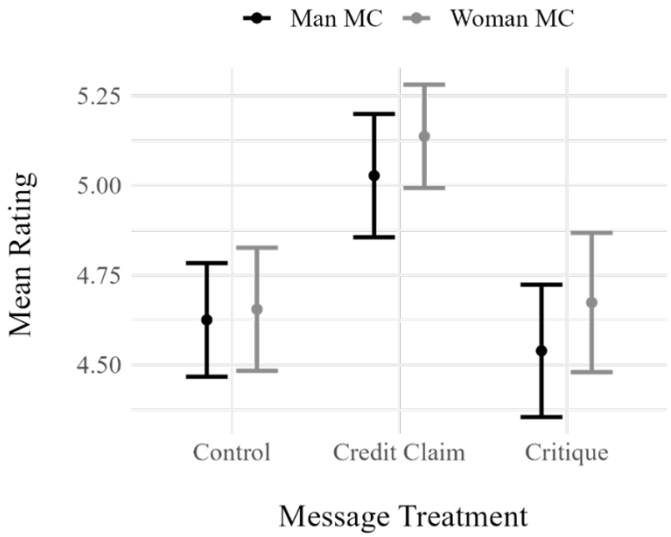



**Table A24: Difference of Means Tests of MC Gender: Vote Intention**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| Control | 4.626 | 4.655 | -0.030 | 0.804 |
| Credit Claim | 5.028 | 5.137 | -0.110 | 0.338 |
| Credit Claim with Critique | 4.540 | 4.674 | -0.135 | 0.325 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Vote Intention (1-7).

**Table A25: ANOVA with Tukey Comparisons: Vote Intention (Woman MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Woman - Claim / Woman | -0.482 | 0.000*** |
| Critique / Woman - Claim / Woman | -0.463 | 0.000*** |
| Critique / Woman - Control / Woman | 0.019 | 0.988 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Vote Intention (1-7).

**Table A26: ANOVA with Tukey Comparisons: Vote Intention (Man MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Man - Claim / Man | -0.402 | 0.004*** |
| Critique / Man - Claim / Man | -0.488 | 0.000*** |
| Critique / Man - Control / Man | -0.086 | 0.758 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Vote Intention (1-7).

**Figure A6: Mean Vote Intention Across Treatment Group and Party Affiliation**

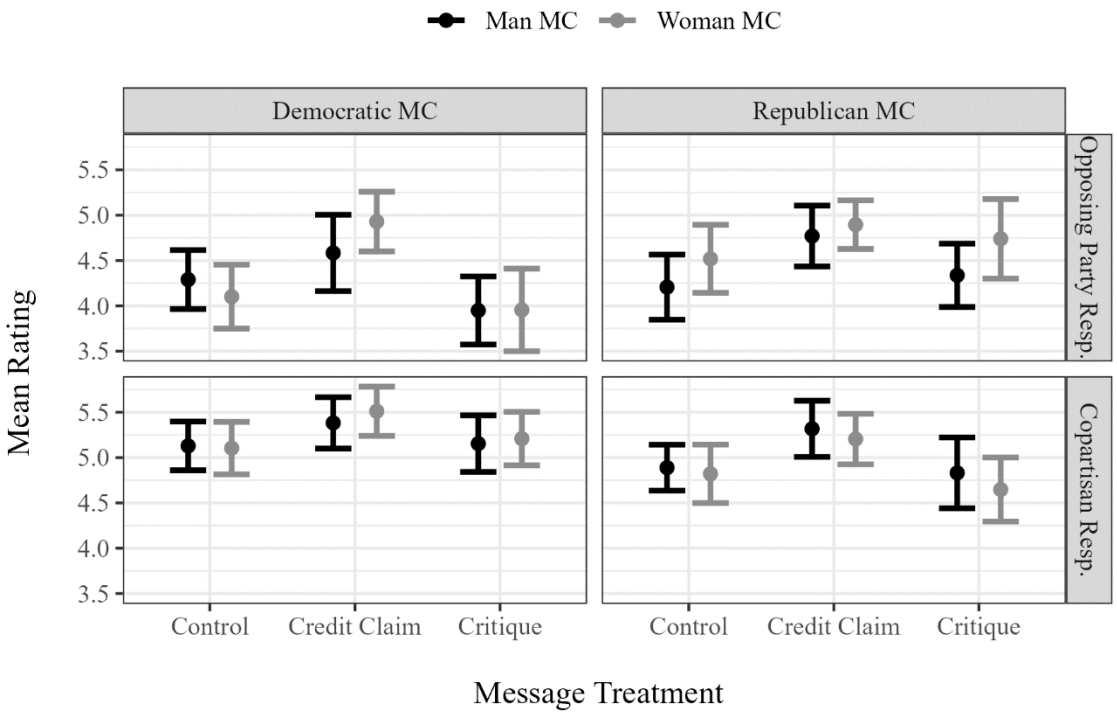



**Table A27: Difference of Means Tests of MC Gender: Vote Intention (Partisan Subsets)**

| Substantive Treatment | Man MC | Woman MC | Diff. of Means | P-value |
|---|---|---|---|---|
| **Republican MC / Copartisan** | | | | |
| Control | 4.889 | 4.821 | 0.068 | 0.746 |
| Credit Claim | 5.317 | 5.204 | 0.113 | 0.595 |
| Credit Claim with Critique | 4.831 | 4.648 | 0.183 | 0.496 |
| **Republican MC / Non-copartisan** | | | | |
| Control | 4.207 | 4.519 | -0.312 | 0.240 |
| Credit Claim | 4.770 | 4.895 | -0.125 | 0.569 |
| Credit Claim with Critique | 4.337 | 4.739 | -0.402 | 0.162 |
| **Democratic MC / Copartisan** | | | | |
| Control | 5.129 | 5.104 | 0.025 | 0.903 |
| Credit Claim | 5.382 | 5.511 | -0.129 | 0.521 |
| Credit Claim with Critique | 5.154 | 5.209 | -0.055 | 0.802 |
| **Democratic MC / Non-copartisan** | | | | |
| Control | 4.290 | 4.101 | 0.189 | 0.442 |
| Credit Claim | 4.583 | 4.930 | -0.346 | 0.206 |
| Credit Claim with Critique | 3.949 | 3.955 | -0.006 | 0.983 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
NOTE: Dependent variable = MC Vote Intention (1-7).

**Figure A7: Mean MC Approval Ratings**

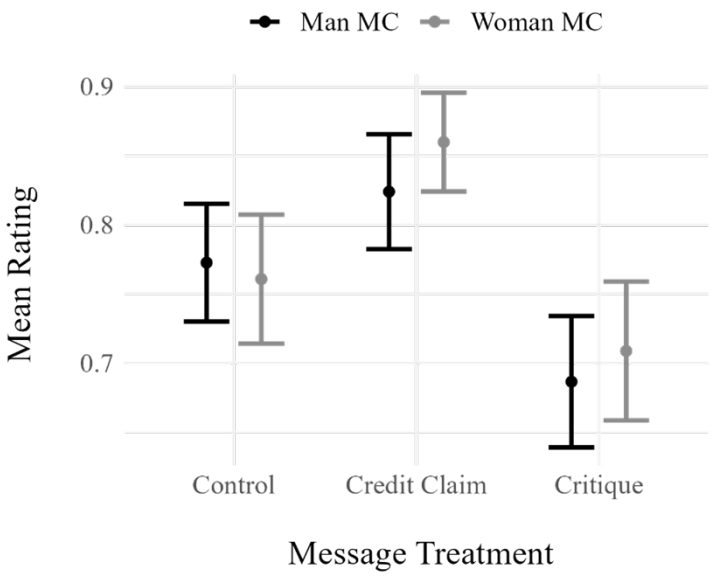



**Table A28: Difference of Proportions Tests of MC Gender: Approval**

| Substantive Treatment | Man MC | Woman MC | P-value |
|---|---|---|---|
| Control | 0.773 | 0.761 | 0.780 |
| Credit Claim | 0.824 | 0.860 | 0.236 |
| Credit Claim with Critique | 0.687 | 0.709 | 0.585 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Approval (0/1).

**Table A29: ANOVA with Tukey Comparisons: Approval (Woman MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Woman - Claim / Woman | -0.099 | 0.005*** |
| Critique / Woman - Claim / Woman | -0.151 | 0.000*** |
| Critique / Woman - Control / Woman | -0.052 | 0.245 |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Approval (0/1).

**Table A30: ANOVA with Tukey Comparisons: Approval (Man MC Subset)**

| Contrast | Diff. of Means | Adjusted P-value |
|---|---|---|
| Control / Man - Claim / Man | -0.051 | 0.249 |
| Critique / Man - Claim / Man | -0.137 | 0.000*** |
| Critique / Man - Control / Man | -0.086 | 0.016** |

* p < 0.1, ** p < 0.05, *** p < 0.01
NOTE: Dependent variable = MC Approval (0/1).

**Figure A8: Mean Approval Across Treatment Group and Party Affiliation**

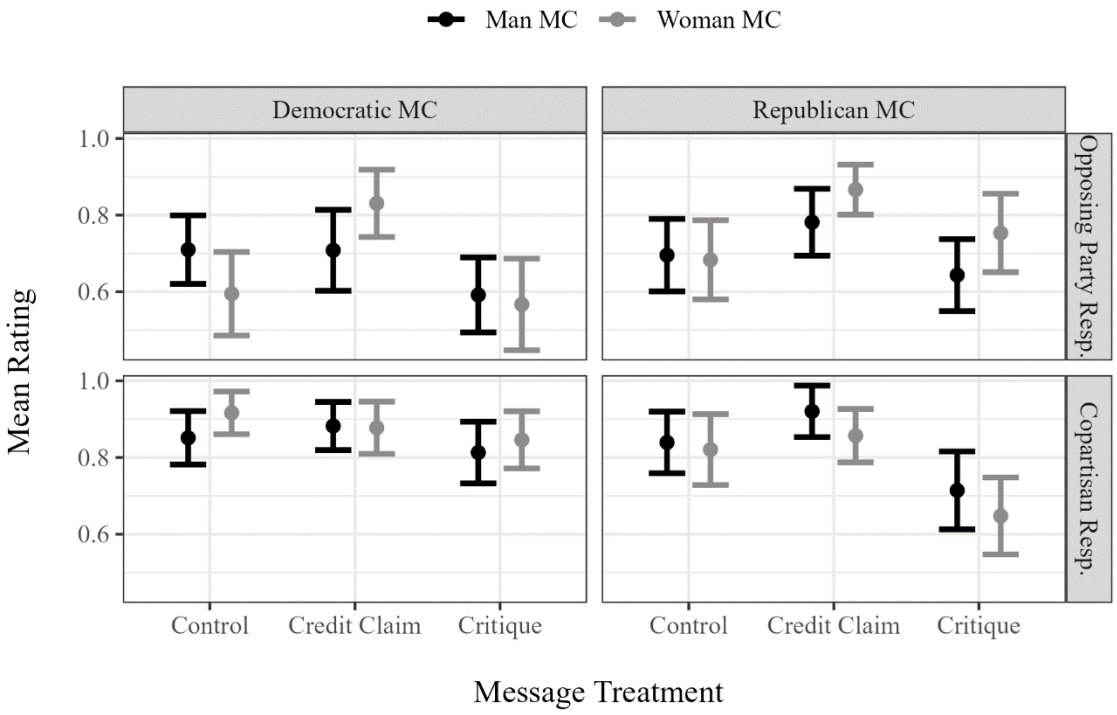


**Table A31: Regression with MC gender and message treatment interaction**

| | General Support | Effectiveness | Fiscal Responsibility |
|---|---|---|---|
| Message: Claim | 0.517*** | 0.718*** | |
| | (0.107) | (0.093) | |
| Message: Critique | -0.228** | | -0.565*** |
| | (0.104) | | (0.112) |
| MC Gender: Woman | 0.199* | 0.089 | 0.154 |
| | (0.107) | (0.093) | (0.112) |
| Partisan Match | 0.191*** | 0.131*** | 0.184*** |
| | (0.038) | (0.039) | (0.048) |
| Resp. Gender: Woman | -0.007 | 0.040 | -0.031 |
| | (0.062) | (0.066) | (0.080) |
| Resp. Party: Ind | -0.498*** | -0.512*** | -0.581*** |
| | (0.076) | (0.079) | (0.096) |
| Resp. Party: Rep | -0.358*** | -0.113 | -0.514*** |
| | (0.075) | (0.079) | (0.097) |
| Credit Claim X Woman MC | -0.118 | 0.014 | |
| | (0.152) | (0.132) | |
| Critique X Woman MC | -0.174 | | -0.092 |
| | (0.153) | | (0.159) |
| Constant | 5.062*** | 4.798*** | 5.368*** |
| | (0.090) | (0.083) | (0.106) |
| Num.Obs. | 2064 | 1382 | 1369 |
| R2 | 0.083 | 0.114 | 0.085 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

NOTE: Ordinary least squares models. Dependent variable = MC ratings. Standard errors in parentheses.

**Table A32: Regression with Demographic Variables**

| | General Support | Effectiveness | Fiscal Responsibility |
|---|---|---|---|
| Message: Claim | 0.524*** | 0.720*** | 0.458*** |
| | (0.107) | (0.098) | (0.105) |
| Message: Critique | -0.238** | 0.431*** | -0.124 |
| | (0.104) | (0.095) | (0.101) |
| MC Gender: Woman | 0.194* | 0.101 | 0.049 |
| | (0.107) | (0.098) | (0.105) |
| Partisan Match | 0.194*** | 0.144*** | 0.144*** |
| | (0.038) | (0.034) | (0.037) |
| Respondent Gender: Woman | 0.015 | -0.006 | 0.017 |
| | (0.063) | (0.058) | (0.062) |
| Respondent Party: Ind | -0.458*** | -0.412*** | -0.508*** |
| | (0.078) | (0.071) | (0.076) |
| Respondent Party: Rep | -0.339*** | -0.157** | -0.310*** |
| | (0.078) | (0.071) | (0.076) |
| Credit Claim X Woman MC | -0.134 | -0.009 | 0.084 |
| | (0.153) | (0.139) | (0.149) |
| Critique X Woman MC | -0.156 | -0.102 | 0.019 |
| | (0.154) | (0.140) | (0.149) |
| Education | 0.018 | 0.024 | 0.023 |
| | (0.023) | (0.021) | (0.022) |
| Age | 0.001 | 0.002 | -0.007*** |
| | (0.002) | (0.002) | (0.002) |
| Respondent Race: Asian | -0.014 | 0.101 | 0.206 |
| | (0.158) | (0.144) | (0.153) |
| Respondent Race: Black | 0.205* | 0.252*** | 0.444*** |
| | (0.106) | (0.097) | (0.103) |
| Respondent Race: Latino | -0.118 | 0.023 | 0.255** |
| | (0.132) | (0.120) | (0.128) |
| Respondent Race: Mixed | 0.045 | -0.033 | 0.146 |
| | (0.132) | (0.120) | (0.128) |
| Respondent Race: Other | -0.321 | -0.260 | -0.155 |
| | (0.217) | (0.198) | (0.211) |
| Income | 0.022* | 0.033*** | 0.014 |
| | (0.011) | (0.010) | (0.011) |
| Constant | 4.789*** | 4.464*** | 4.879*** |
| | (0.156) | (0.143) | (0.152) |
| Num.Obs. | 2053 | 2053 | 2053 |
| R2 | 0.091 | 0.089 | 0.093 |

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

NOTE: Ordinary least squares models. Dependent variable = MC ratings. Standard errors in parentheses.