How the Supply of Economic Policy Proposals Shapes Public Demand for Redistribution

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Preface and Acknowledgements

I began thinking about the ideas developed in this dissertation in 2010 while enrolled in an introduction to research methods course with Lynn Sanders and a political behavior course with Nick Winter. Both provided me with a steady stream of feedback, advice, recommendations, and reactions to my ideas over the following six years, shaping my thinking and writing in countless ways. I entered graduate school more interested in film than political science – indeed, I had never enrolled in a public opinion course before coming to Virginia. Lynn and Nick showed me that the study of opinion and behavior is important, interesting, and difficult, and I'm thankful for their guidance. In particular, Lynn's sustained attention to normative political theory within the project of positive social science helped give my work meaning: focusing on the broader, value-laden stakes of individual-level actions and opinions enriches empirical results. She also read many drafts of these chapters, providing both broad and specific suggestions that have improved my arguments and clarified my contributions. Nick taught me how to collect, analyze and evaluate empirical evidence, clarified my conceptual arguments, and built a survey platform (which I use in Chapters 3 and 4). Without his help, I would not have been able obtain or understand the original survey data I present here. His broad scholarly interests led me to engage with research on psychology, linguistics, and information processing that I otherwise would have missed.

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Introduction

"We do not know how men would behave in response to the facts of the Great Society. All that we really know is how they behave in response to what can fairly be called a most inadequate picture of the Great Society." – Walter Lippmann (1922, 18)

Despite the fact that many Americans experience privation and economic insecurity, public support for redistribution is muted. Many Americans do not possess accurate factual information about the economy and inequality, and most do not understand estate taxes and other redistributive policies.[1] At the same time, American political culture does not prioritize economic equality. Scholars focused on American exceptionalism suggest that American political culture is fundamentally egalitarian, while its economic culture is not (Hochschild 1981; Sombart 1906; Tocqueville 1840). Others argue that that the political coalitions that established economic equality as a norm and created social welfare states in Europe were more unified, partisan, and effective than their counterparts in the U.S. (Lipset and Marks 2001). Scholars of political behavior find that Americans favor inequality in the economic sphere (Hochschild 1981), and conclude that "Americans are most definitely not economic egalitarians" (Schlozman, Verba, and Brady 2012, 60). Even among principled proponents of economic egalitarianism, support for redistributive policies is not assured. Bartels (2008) argues that egalitarians fail to connect their principled beliefs with concrete policy decisions. Cramer (2016) finds that those who suffer material hardship are often suspicious of elite-sponsored redistributive policy proposals.

In this dissertation, I argue that American opposition to redistribution is rooted in a failure of political imagination: Americans are unlikely to support redistributive policies when elected officials rarely discuss them, when the issues are difficult to interpret, and when proposed policies appear implausible. I define redistributive policy as any state action that changes the distribution of

[1] See Norton and Ariely (2011), Gimpelson and Treisman (2015), Sides (2016), Johnston and Wronski (2015), and Bartels (2008), among others.

economic resources between the rich and poor. I further distinguish direct redistributive policies, such as the minimum wage, means-tested social spending, and the estate tax, from indirect forms of redistribution, such as job creation incentives and business-friendly corporate tax policy. Using public opinion surveys, survey experiments, and observational data, I examine the relationship between political representations of economic issues, cognitive representations of economic policies, and economic reality. I show that redistributive opinion is especially malleable, but also that political actors rarely propose direct redistributive policies in the first place. When elected officials and political candidates do propose indirect, market-oriented economic interventions, individuals faced with high levels of economic insecurity in their local communities are unlikely to participate in elections or support the interventions.

This argument expands upon existing accounts of opinion formation by highlighting the role of external political stimuli in anchoring reported beliefs and preferences. When individuals encounter political stimuli, they consciously consider how the stimuli relate to preexisting considerations and construct an evaluation based on accessible ideas (Zaller 1992). At the same time, people experience unconscious affective responses to political information (Lodge and Taber 2014). These processes lead individuals to form a belief, attitude, or preference: an idea about the political stimuli. But both models of opinion formation mentioned above locate opinion formation in unobservable psychological processes, rather than in the observable exchange between a political subject and political representations of policy. My theory considers the dynamic relationship between external stimuli and response in order to model the effects of political representations that invite and initiate political action. By focusing on the strategic selection and use of external political stimuli, rather than individual-level responses in isolation, my account of opinion formation better explains mass political behavior. This refocusing of public opinion scholarship – from fixed opinions within the respondent to the dynamic relationship between opinions and external political

stimuli – is a necessary change in perspective given recent research on opinion formation (Sanders 1999; Lenz 2013; Druckman 2014).

Studying preferences and political behavior separately from the political stimuli that invite both lacks external validity. The "constructivist turn" in public opinion scholarship instead suggests that "preferences are constituted in the communication that occurs during decision making" (Disch 2011, 102). In this account, public opinions are endogenous to the political messages that inspire them, rather than bedrock preferences that exist absent any political framing, information, context, or provocation (Lenz 2013). All expressed opinions are based on external stimuli, whether survey questions, news coverage of political events, or deliberative conversation. Instead of attempting to discern some form of baseline or "true" opinion, I am interested in how different kinds of political messages (both political and environmental) influence individuals' expressed preferences (Druckman 2014).

I begin by developing a descriptive overview of economic policy-focused political communication in the wake of the 2008 Financial Crisis. I create a measure of the aggregate supply side of public opinion using political advertising data and congressional press releases. At a time in which material insecurity dramatically increased for most Americans, the public actually became less supportive of redistributive policy and government intervention (Brooks and Manza 2013; Bartels 2013). By focusing on the representation of redistributive policy in political communications, I identify one mechanism that explains this counterintuitive pattern. Political actors rarely proposed direct, state-supported interventions meant to address new economic problems. Instead, communications supplied indirect, market-centered economic policy responses to the recession: promoting business and job growth rather than direct government investment and means-tested subsidies. As a result, campaigns that studiously avoid direct redistributive policy interventions are unlikely to engender broad public support for them.

In chapter 2, I develop a theory of political – rather than public – opinion, clarifying the claim that public sentiment is created in response to political stimuli, rather than separately existing as a fixed and stable set of considerations in the minds of voters. By focusing on the dyadic relationship between stimuli and response, I highlight the conditional nature of expressed opinions and suggest how political messages that anchor evaluations to specific policy decisions might encourage individuals to articulate more meaningful preferences. Existing evidence of public support for redistribution is often biased by survey respondents' incomparable interpretations of what policies mean (Huber and Paris 2013). By designing questions that reflect real world political communication, opinion analysts can assess redistributive policy support in more valid and generalizable ways.

Political information is not supplied in a vacuum: rather, political actors strategically package that information for public consumption, while the individuals who receive information operate within diverse economic contexts. In chapter 3, I show that the strategic representation of economic information shapes individuals' reported preferences separately from the information itself. Individuals are more likely to support redistributive policies when the visual presentation of economic information conveys why they ought to support them. For example, Republicans become much more accepting of government intervention against inequality when inequality looks to be increasing dramatically, but less accepting when the issue appears relatively constant over time. Democrats and Republicans also are more likely to accept information about identical progressive tax policy proposals when they are supplied by ideologically congruent sources.

Environmental context further conditions information acceptance and opinion formation. In chapter 4, I show that the effects of information and facts are conditional on individuals' experience with economic reality. I conduct a survey experiment that tests how local economic mobility information interacts with local economic context to shape support for national economic policy.

My results show that those living in areas with high levels of economic immobility are especially unlikely to support either direct and indirect redistributive interventions, while the opposite is true for those living in places with high levels of mobility. This result reveals inherent limitations in public support for market-oriented redistributive policy proposals, which appear less effective to those living in immobile economic contexts.

Experimental results might not generalize to real-world political messages and observable behavior: no single survey experiment can approximate the barrage of political advertising targeted at battleground states, while responses to survey questions do not necessarily predict formal political behavior. It is also impossible to randomly assign environmental economic context. Thus I also conduct observational research that examines the effects of aggregate political communications on mass political behavior. In chapter 5, I test the mobilizing and persuasive effects of redistributive issue-focused political advertising by modeling the moderating effects of issue-relevant economic context. I show that the aggregate supply of market-focused job creation proposals interacts with local economic reality to shape voter behavior. Campaign efforts to politicize economic problems like unemployment and immobility are most likely to succeed at changing minds and encouraging political participation in places experiencing economic growth. But in areas with high levels of unemployment and inequality, efforts to politicize economic challenges with market-based solutions actually discourage political engagement.

This project makes two central contributions. First, I show that American support for redistributive policy depends on the supply of redistributive policy proposals. When political actors avoid talking about redistribution or downplay the importance of inequality, public support for state-sponsored policy interventions is low. Second, I show that the relationship between public opinion and political communication is conditional on environmental context. Individuals who live in more unequal and insecure economic contexts are less receptive to proposed policy interventions.

The implication of this research is that individuals are less likely to perceive proposed market-based redistributive policy interventions to be viable when they perceive their own communities' economic prospects to be limited. By conceptualizing redistributive policy demand as contingent upon the political supply of policy proposals, scholars can better understand Americans' economic and political culture. As economic inequality continues to rise, this research explains how political interventions against it might win or lose public support.

1 How Elected Officials Talk About Redistribution

Abstract

Elected officials avoid making redistributive policy proposals in the face of growing economic inequality and insecurity. Though direct redistributive policy initiatives have the potential to decrease material inequities, political actors prefer to propose indirect, market-oriented programs to increase opportunity. I examine the content of political campaign advertisements aired by gubernatorial and congressional candidates alongside press releases issued by U.S. House Representatives in the wake of the 2008 financial crisis and global recession. I show that federal and state elected officials are reluctant to propose direct economic interventions, including means-tested social spending and minimum wage policy. Instead, political actors talk about market-centered redistribution, focusing on policies that indirectly affect the material conditions of constituents. I suggest that this pattern helps explain the lack of popular support for major redistributive interventions throughout the Great Recession.

"People accept the facts which come to them through existing channels. They like to hear new things in accustomed ways. They have neither the time nor the inclination to search for facts that are not readily available to them." – Edward Bernays (1923, 137-138).

In the wake of the 2008 financial crisis, unemployment increased by 5 percentage points, while retirement savings accounts decreased in value by about 30% (Hurd and Rohwedder 2010). The poverty rate increased from 12.5% in 2007 to 15.1% in 2010 (Danzinger et al 2012). At the same time, economic inequality continued to rise: pre-tax average income for the top 1% of earners increased between 2009 and 2012 by $96,900, while those in the lower 99% experienced a $100 decrease, on average (Saez 2015). However, political campaigns during and after the crisis focused almost entirely on market-based policy solutions, rather than direct government interventions meant to curb the worst material consequences of the crisis. In 2010, 43% of all political advertisements in congressional campaigns mentioned jobs and employment, while just 0.04% mentioned poverty, 0.3% mentioned unions, and 1% mentioned the minimum wage. In the face of increasing economic inequality and widespread economic insecurity, political actors proposed relatively few explicitly redistributive responses. As a result, public support for redistribution was muted: while firsthand experience with job loss caused a temporary increase in support for social spending (Margalit 2013; Newman 2014), both experimental and observational evidence suggests that the Great Recession did not increase egalitarian sentiment (Fisman et al 2015; Bartels 2013). Instead, most Americans actually became more likely to express opposition to government intervention meant to redistribute income and improve material outcomes for the poor (Brooks and Manza 2013).

In this chapter, I provide a descriptive overview of redistributive political communications issued by state and national leaders during the Great Recession. I find evidence that political campaigns overwhelmingly focus on two economic issues: jobs and taxes. At the same time, campaigns studiously avoid issues like poverty, the minimum wage, and means-tested social spending. I find no evidence that electoral candidates target market-oriented redistributive appeals to

the places suffering most from poverty; indeed, candidates are likely to avoid talking about taxation in places with very high poverty rates. I examine the text content of all published U.S. House press releases from 2005-2010, and find that Members of Congress discuss job creation and taxation in market-oriented ways, downplaying direct intervention, social policy, and labor market regulation. This account sets the stage for the empirical chapters that follow, which each examine how individuals form redistributive preferences and decide to engage in political action in response to economic political messages.

Defining Redistributive Policy

During and after the Great Recession, President Obama signed into law several major policies meant to help address economic insecurity and redistribute income. These included the American Recovery and Reinvestment Act of 2009, which included nearly $800 billion dollars of federal stimulus spending, the Patient Protection and Affordable Care Act of 2010, which subsidized health insurance for low-income Americans, and the American Taxpayer Relief Act of 2012, which increased the top marginal income tax, capital gains tax, and dividend tax rates by about 5%. Yet these programs were rarely defined as redistributive by the members of Congress who supported them. Each was also the product of compromise, reflecting fiscally conservative impulses within both Congress and the Obama administration (Lizza 2012). Before and after each policy was successfully enacted, elected officials increasingly described most economic policy proposals as a means of "increasing opportunity" rather than redistributing income (Rucker and Balz 2015; Calmes 2014; Bouie 2015). This rhetorical shift – from economic outcomes to economic prospects – underscores a broader emphasis on indirect redistributive policy proposals across political communications.

In contemporary American politics, policies that change the distribution of economic resources are unlikely to transparently reveal that fact, whether in the realm of tax policy (Mettler

2011) or social policy more generally (Hacker 2002; Faricy 2015). Accordingly, political actors have a large degree of rhetorical leeway when talking about economic policy. Since all of the topics mentioned above are complex and abstract, most Americans have little knowledge about economic inequality (Norton and Ariely 2011; Gimpelson and Treisman 2015) or what redistribution actually entails (Sides 2016; Johnston and Wronski 2014). They also lack the motivation to seek detailed information about specific policies (Lupia 2015). The issues that political actors publicize, then, set the agenda for economic policy more broadly. When elected officials ignore or downplay government's ability to affect economic outcomes, major economic interventions are unlikely to find broad public interest or support.

I define redistributive policies as laws that affect the distribution of economic resources between the rich and poor. Hatch and Rigby (2014) identify three general kinds of redistributive policy: taxes on the wealthy and poor, spending on the poor, and labor market policy. Of course, a much larger set of policies redistribute wealth to various groups that are not defined by material status. Education spending, social security, healthcare spending, and various tax incentives confer benefits on particular constituencies at the expense of others. But I am most interested in policies targeted at those on either end of the income distribution.

I make a conceptual distinction between two kinds of redistributive policy. The first category includes targeted state spending (stimulus spending, anti-poverty programs) as well as regulatory policy (minimum wage rules) that directly redistribute wealth among groups. The second category includes broad, market-oriented policies aimed at increasing economic growth by encouraging private sector investment (decreasing corporate and income taxes, job creation incentives). Direct redistributive policies include Great Society programs meant to increase the standard of living for the impoverished through means-tested social spending to and reduce inequality through direct economic regulation, while the latter group of policies include those meant to encourage business

growth and private investment, indirectly affecting the distribution of resources between rich and poor. Parts of tax policy belong in both categories: while estate taxes and increased income taxes are a means of directly redistributing income (and thus belong in the first category), cutting taxes and providing tax incentives are a means of encouraging private sector investment and job creation, goals that align with the second category. In the analysis that follows, I cannot always cleanly distinguish between specific tax policies, though I show that tax references in congressional press releases are almost never concerned with direct redistribution.

Setting the Redistributive Agenda: Campaign Ads

I begin my analysis with campaign advertisements because ads are a central part of contemporary democratic elections (Fowler, Franz, and Ridout 2016; Freedman, Franz, and Goldstein 2004). Candidates for the U.S. Congress spent almost three billion dollars on political advertising between 2010 and 2012 (Fowler and Ridout 2013), and ads provide a relatively direct means of communication between candidates and potential supporters. I expect that candidates' emphasis on public policies in televised ads reflects a sincere effort to convey issue positions to potential supporters; as such, measures of issue-priority that are based on ads suggest what candidates intend to enact if elected. Campaigns devote the majority of their financial resources to televised advertising (Fowler, Franz, and Ridout 2016, 2); as a result, ads constitute the primary means of political communication between candidates and voters.

As the U.S. economy slowly recovered from the Great Recession, voters experienced economic hardship directly. I expect that strategic campaigns, seeking to prioritize issues that voters are likely to care about, should focus on proposing government-led solutions to economic problems. Thus ads broadcast in the wake of the Great Recession should highlight issues like poverty, unemployment, government stimulus, and other economic policies meant to improve individuals' standard of living. By doing so, campaigns might mobilize voters and persuade potential supporters

by linking improved economic outcomes with political participation (Jones 2015; Hart 2013). However, there is consistent evidence that campaigns did not choose to highlight those issues. Figure 1.1 shows the average proportion, by week, of all Congressional and Gubernatorial ads broadcast in 2010 and 2012 that focus on each of six economic policies.

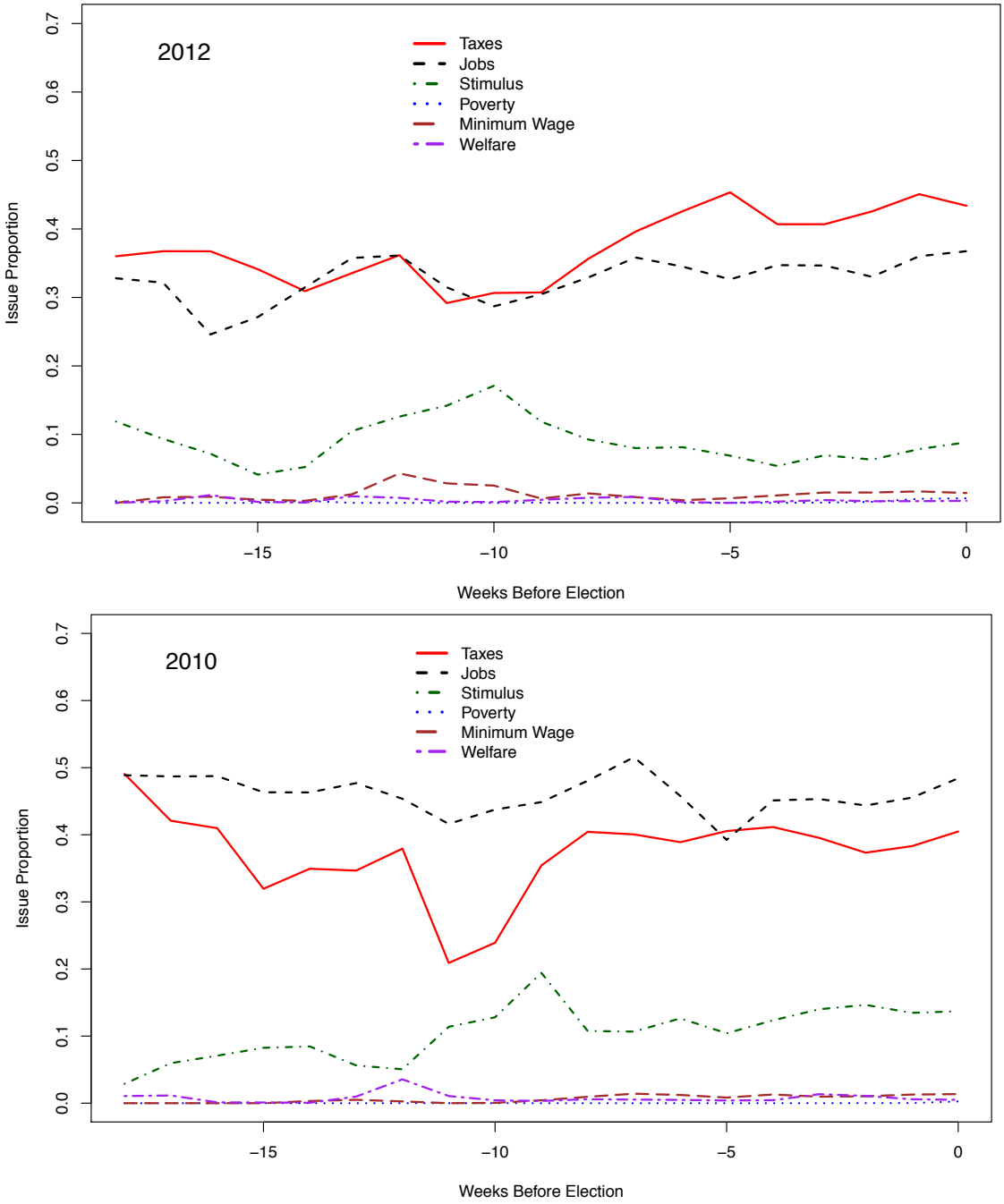


Figure 1.1: Aggregate Economic Issue Political Advertising

The policies I selected include those directly concerned with redistribution (welfare, minimum wage) and those focused on economic growth in general (jobs, stimulus) (Fowler, Franz, and Ridout 2014; 2015). As both figures show, means-tested social spending, poverty, and the minimum wage are all deeply unpopular topics in congressional and gubernatorial political campaigns. Stimulus spending is slightly more popular, but less than half as frequently discussed as taxes or jobs. While taxes are discussed often, many references to the issue refer to lowering federal income and corporate tax rates in order to stimulate economic growth, rather than as a means of redistributing income.

The overall issue-focus of campaigns is relatively stable over time. Both taxes and jobs are consistently popular throughout the time series. In total, 45.6% of ad airings in 2010 and 33.8% of airings in 2012 discuss jobs, suggesting that campaigns were very eager to talk about that aspect of the economy. But the combined proportion of all ads than mention welfare, minimum wage, poverty, or stimulus policy is just 9.7% in 2012 and 14.0% in 2010. When candidates advertise economic policy positions, they do so by talking about taxes and jobs instead of more direct redistributive policies.

It could be the case that only those running for office in places that experienced very high levels of poverty were likely to devote advertising airtime to direct redistributive policy proposals. To evaluate that expectation, I estimate a simple OLS regression equation that examines the relationship between the total number of ads that mention each policy, in each of 200 media markets (observed in both elections), as a function of the media market's average poverty rate. I include a control for the total number of ads broadcast in each market. The results appear in Table 1.1: contrary to the expectation sketched above, the table shows that media market-level poverty is not correlated with five of the six redistributive policy areas. Only tax issue mentions are related to poverty, and the relationship between the two is negative and statistically significant. In other words, as the poverty rate increases by one percentage point, campaigns broadcast about 71 fewer ads that focus on tax

policy. The poverty rate has no relationship with poverty issue mentions, indicating that campaigns did not respond to actual increases in poverty by broadcasting more ads focused on the topic.

	Taxes	Jobs	Stimulus	Poverty	Minimum Wage	Welfare
Poverty Rate	-70.817**	-23.710	1.617	-0.401	0.861	2.354
	(15.873)	(19.975)	(9.489)	(0.562)	(2.963)	(1.728)
Total Ads	0.353**	0.369**	0.095**	0.000	0.011**	0.008**
	(0.008)	(0.010)	(0.005)	(0.000)	(0.002)	(0.001)
2010	-110.259	1,162.080**	411.832**	-9.976*	-30.143	33.292*
	(139.675)	(175.771)	(83.498)	(4.942)	(26.074)	(15.205)
Constant	1,618.172**	173.890	-144.829	16.862	-0.136	-76.448*
	(318.384)	(400.662)	(190.331)	(11.265)	(59.434)	(34.660)
N	400	400	400	400	400	400
R^2	0.847	0.801	0.547	0.014	0.122	0.190

Standard errors in parentheses. ** p<0.01, * p<0.05

Table 1.1: Issue Focus by Media Market Poverty Rate

Overall, the evidence presented in this section shows that political campaigns did not broadcast direct redistributive policy proposals. While indirect forms of intervention, including job creation, were frequently mentioned, those proposals that conjure images of an active government directly shaping the distribution of income were not. In the next section, I expand the analysis beyond campaigns and consider the content of legislative press releases in the period 2005-2010. I find that the same patterns described above generalize outside of the electoral environment, and develop more nuanced measures of how campaigns talk about taxes and jobs.

Redistributive Policy in Press Releases

To evaluate the economic issue-focus of non-electoral political communications, I examine the content of legislative press releases issued by members of the U.S. House of Representatives. The data include 181,462 press releases from a total of 561 U.S. House representatives, issued between 2005 and 2010 (Grimmer 2014), a time frame that includes the entirety of the Great Recession (December 2007 – June 2009). The average representative issued 75 releases per year,

with a minimum of 1 and a maximum of 1274. The median number of releases per member, each year, is 36. Members of the U.S. House use press releases to convey policy and pork accomplishments alongside legislative priorities to constituents in order to cultivate future electoral support (Grimmer et al 2014). As a result, the policies that representatives discuss in press releases should reflect the same strategic logic that guides electoral advertising.

I begin my analysis by examining the relative frequency of specific words related to the redistributive policies discussed above; the results appear in Figure 1.2. The frequency of each word neatly parallels the results of the advertising analysis: taxes and jobs are the most popular issues that representatives discuss in press releases (appearing 109,372 and 79,938 times, respectively), while minimum [wage], poverty, stimulus, and welfare are almost never mentioned. Even though poverty increased dramatically between 2005 and 2010, representatives used the word "poverty" just 1,537 times in press releases. By comparison, "campaign" appears 162,960 times, "war" 69,669 times, "school" 131,313 times, and "health" 105,353 times. The infrequency of terms like "welfare" and "poverty" suggests that both policy areas were rarely discussed by sitting representatives.

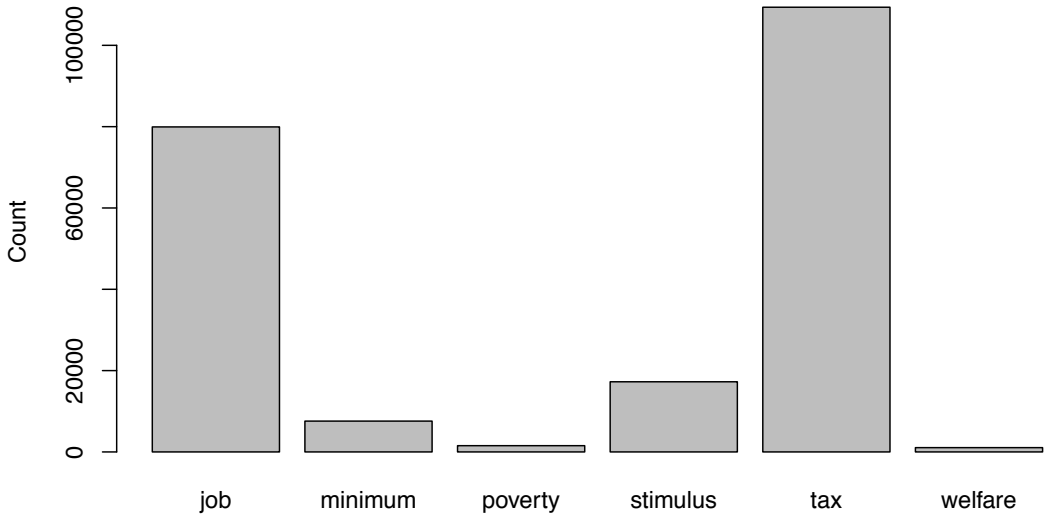


Figure 1.2: Aggregate Economic Issue Congressional Communication

Next I examine words that capture more direct forms of redistribution and the distribution of resources between rich and poor. Policy-specific abbreviations like TANF and SNAP do not appear in 99.99% of press releases, but representatives do occasionally use general redistributive terms. Figure 1.3 shows the frequency of explicitly redistributive words, including "rich," "poor," "redistribut[e]/[ion]," "progressive," "equal," and "wealth." However, due to the scarcity of each word, the sum of cumulative totals is less than 10% of the total presented in Figure 1.2. The most frequently used term, "poor," appears 13,588 times, while the word stem of "redistribution" and "redistribute" appears 371 times in total. This result provides even clearer evidence that House Representatives chose not to advertise direct redistributive policies when communicating with constituents.

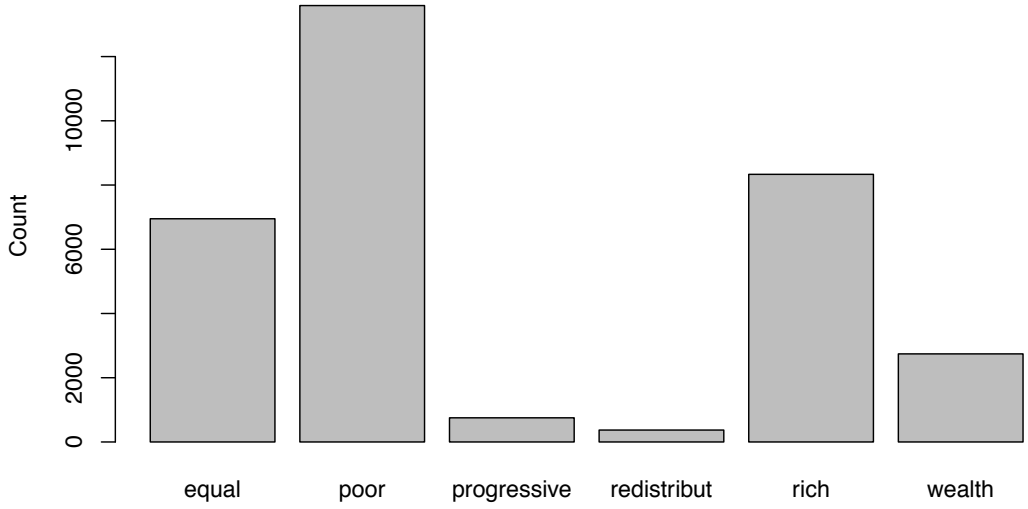


Figure 1.3: Aggregate Redistributive Policy Congressional Communication

While both figures above suggest that representatives rarely talked about redistributive policies, the results presented so far are limited by the specificity of the words I chose to examine. While I think the selected words represent unambiguous referents for the policies of interest, is possible that elected officials talk about redistribution indirectly or use a different vocabulary to discuss the issues. To address that concern, I use computational methods to estimate the latent

topics or issues contained within the corpus of press releases. Using R's stm package (Roberts et al 2014), I processed the press releases, removing stop words, punctuation, and numbers. I stemmed words and converted all text to lower case. I also removed the least frequently-used words.[2]

Next, using Lee and Mimno's (2014) algorithm, I estimate the number of latent topics within the remaining text. Of the 60 estimated topics, only two focus on economic policy (rather than district service, patriotism, war, education, healthcare, or general references to lawmaking). No topic directly refers to redistribution or poverty, suggesting that both issues are conspicuously absent from the rhetorical agenda of House press releases. Figure 1.4 shows the most frequently used words in the two economic policy topics I identified (which clearly correspond with taxes and jobs). In each case, press releases focus on market based economic policy proposals: words like "government," "federal," and "state" are absent from the topics, while more market-oriented terms like "invest," "privat[e]/[ize]," "earn," and "corpor[ate]/[ation]" do appear.

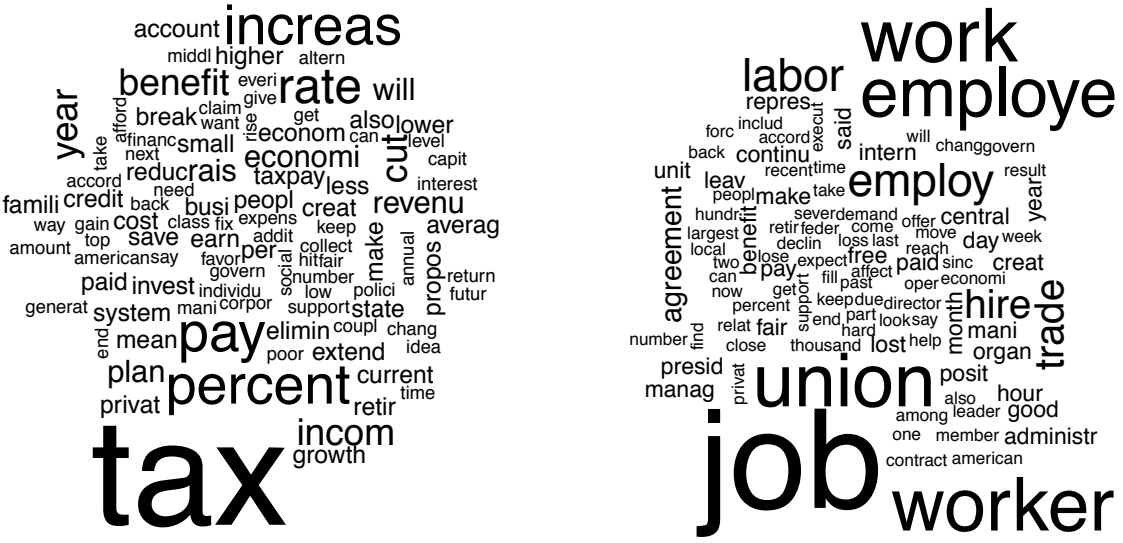


Figure 1.4: Congressional Communication on Economic Policy

Within the tax topic, the words "rich," "redistribute[e]," and "progressive" are missing entirely; representatives discuss tax benefits, rates, cuts, and increases, but do not also frame taxation as a

[2] I use a relatively conservative sparsity level of 0.95 in this analysis.

means of redistribution. Similarly, there are no references to "training" or "programs" within the jobs topic. In both cases, there is no evidence that representatives frame economic policy in terms of an active government, working to directly shape the distribution of resources between rich and poor. Instead, political actors rely fully on market-based language to address economic problems. From 2005 to 2010, elected officials downplayed the possibility that government could directly shape more equal economic outcomes.

Conclusion: The Supply Side of Redistribution

On the whole, the analysis presented in this chapter provides consistent evidence that the supply of redistributive policy proposals is consistently constrained to two market-oriented topics, despite the increased prevalence of economic hardship during and after Great Recession. When elected officials talk about redistribution, they overwhelmingly focus on indirect, market-based policy interventions. Indirectly redistributive references to taxes and jobs are popular, while means-tested social policy and labor market regulation remain off the agenda entirely. In places that suffered most from increased poverty, political campaigns were no more likely to talk about the issue, and were actually less likely to discuss tax policy as a means of shaping economic outcomes. Overall, political actors' disregard for direct redistributive interventions helps explain why Americans expressed greater opposition to government intervention during and after the Great Recession. When political actors do not supply redistributive policy proposals, we should not expect to observe broad support for redistribution. This account is fully consistent with other research on the centrality of market-based public policy in contemporary American governance (Hacker and Pierson 2016), but it is still surprising in light of the severity of the Great Recession.

Scholars have articulated several alternative explanations for public opposition to redistribution after the Great Recession. For example, Bartels (2013) suggests that opinion moves like a "thermostat," reliably shifting rightward after the election of Democratic presidents, while

Brooks and Manza (2013) argue that increased partisan attachment among Republicans led to greater opposition to government interventions that were associated with a Democratic presidential administration. In other words, "individuals who more strongly identified with the Republican Party moved away from government faster than Democratic Party identifiers moved toward government" (741). Both of these accounts are plausible, but each fails to isolate the mechanisms that link individual-level political opinion formation with expressed preferences and behaviors. By ignoring the ways that elected officials and candidates politicize economic issues – directly linking economic circumstances to political responses – existing research provides an incomplete account of political responses to economic events. The descriptive statistics provided here fill that gap, showing that elected officials and candidates shaped public opposition to direct redistribution by simply ignoring a wide range of policy initiatives.

In the next chapter, I develop a theory of redistributive opinion formation that reveals the mutually constitutive nature of political communication and political opinions. By conceptualizing opinions as conditional on political stimuli, I explain why measuring the supply of redistributive policy proposals is a necessary precursor to measuring opinions. I argue that political communications *anchor* political ideas, creating opinions where they otherwise might not exist. If political actors responded to the Great Recession by calling for direct redistribution and an active government, individual Americans would likely follow their leaders.

2 Political Opinion: Anchoring Redistributive Preferences

Abstract

While Americans appear to oppose many redistributive policies, reported opinions are not always meaningful indications of true beliefs. Existing theoretical accounts of opinion formation and expression blame respondents for unconsidered economic policy opinions, ignoring the ways that survey questions and political communications represent policy alternatives in advance of individual-level opinion formation. I argue that question wording is a strategic, political choice, rather than a scientific endeavor: there is no unbiased or perfectly balanced way to represent policies with winners and losers. As a result, expressed opinions should be conceptualized as conditional on political stimuli, provided by survey researchers and other political actors. I develop a model of opinion formation that explicitly incorporates the political content of economic information and show how the model challenges existing accounts of redistributive policy opinion. I also extend the model beyond the survey context, arguing that scholars should conceptualize all political behavior as responsive phenomena.

"A lot of times, people don't know what they want until you show it to them."
- Steve Jobs (Reinhardt 1988)

In the previous chapter, I provided descriptive evidence of a surprising rhetorical gap in political communication. Despite the fact that the U.S. economy suffered a severe recession, unemployment increased dramatically, and economic insecurity increased for millions of Americans, political actors avoided the topic of direct redistribution. In this chapter, I develop a theoretical account that explains how the political communications described in chapter 1 relate to individual-level opinion formation, the subject of chapters 3-4. I argue that political opinions are formed in response to the political supply of policy proposals. When individuals are asked about income inequality, the unemployment rate, progressive taxation, and other abstract economic issues, their answers do not necessarily reflect fundamental political principles of a consistent set of policy preferences. Instead, individuals rely on contextual cues, forming opinions that are constitutively shaped by observable stimuli. By conceptualizing survey response as a reactive process in which politicized question words anchor reported evaluations, I develop a model of opinion formation that resolves existing theoretical tensions in the study of political communication and political behavior and suggests how to improve opinion measurement.

For many Americans, politics is a "sideshow in the great circus of life" (Dahl 1961, 305). Few invest time and attention in the details of proposed policies because the benefits of obtaining political information rarely outweigh the costs (Lupia 2015). As a result, people don't necessarily know what political outcomes they want. Despite this informational deficit, most opinion questions provide only vague or nonspecific descriptions of public policies when soliciting evaluations. Facing such questions, individuals provide answers that are not directly comparable (Huber and Paris 2013; Hopkins and King 2010). At the same time, however, public opinion research gains democratic stature by treating all answers equally, regardless of whether different answers mean different things (Verba 1996). This tension within the project of opinion measurement leads some to conclude that

"most of what gets measured as public opinion does not exist except in the presence of a pollster" (Zaller 1992, 265).[1] That is, individuals may express answers to survey questions that they would not express or act upon in their broader political life. Opinions based on vague representations of public policies are less meaningful than those based on representations that reflect campaign rhetoric. In order to measure more meaningful opinions, I argue that scholars should politicize question wording such that survey words match actual (or counterfactual) political contexts. By redefining the study of public opinion as a necessarily political project – rather than a supposedly neutral scientific enterprise – scholars can assess more meaningful opinions.

The validity of expressed opinions depends upon the content of survey questions and political stimuli more generally. Assessing the content of political communication and survey question wording allows researchers to measure the effects of specific political context on opinion formation and political action. I argue that structural features within the content of political communication systematically shape individual-level responses: when communications focused on abstract or difficult to understand political issues explicitly cue social groups (Nelson and Kinder 1996) or provide comparative reference points (Tversky and Kahneman 1974), they encourage group-centric reasoning and anchor reported evaluations toward the provided stimuli. Encouraging respondents to engage in these cognitive processes is no more democratic than discouraging them, but the decision to incorporate representations of political rhetoric into opinion solicitation does improve the internal and external validity of responses. By anchoring survey responses with more specific and political language, opinion analysts may be able to reduce the informational deficits and systematic biases that lead to random responses, acquiescence bias, and nonresponse.

[1] For a more complete elaboration of this point, see Krippendorf (2005), Bishop (2004), Bourdieu (1979), Tilly (1983), Converse (1964), and Blumer (1948).

Political Opinion

Public opinion is called "public" because it is "about current public affairs, not in being created through a public process of deliberation and discussion" (Peters 1995, 14). Yet most political opinions are expressed in response to publicly expressed stimuli, whether a survey's question wording, news reports, campaign solicitations, or interpersonal conversation. The exchange between an interviewer and respondent is itself a public exchange, though one that is tightly controlled via interview protocol (Sanders 1999). Regardless of how researchers assess opinions, they must represent policies and politics for respondents.

Representations are not inferior to reality, but rather a necessary part of mass communication. As Edelman (1964) explains, "politics is for most of us a passing parade of abstract symbols:" few actually witness legislative activity or issue-based advocacy firsthand (5). Symbolic political stimuli convey information by emphasizing attributes of political objects and encouraging individuals to evaluate those objects from a particular point of view. They are symbolic because they are meaningful words and images – representations of real events and ideas. I call responses to political representations "political opinion" to emphasize the fact that the selection of policy representations is just as political as observed responses to them. The particular words, symbols, and signs included in political communications – including survey questions – influence the preferences that people express. As Druckman (2014) explains, "citizens may not have the innate capacity to form preferences on their own, at least not without the messages provided by strategic political communications" (468). By measuring the content of external political stimuli, the concept of political opinion locates preference formation in the interaction between a known political representation and unobserved predispositions, affective responses, group identities, principles, and beliefs. Opinions are conditional on communication.

Survey questions emphasize particular aspects of political issues (framing) and use more or less informative representations of those issues (information) in order to elicit public evaluations. The process of framing "suggest[s] how politics should be thought about, encouraging citizens to understand events and issues in particular ways" (Kinder 2003, 359). Frames are political in the sense that they must condense the world of relevant information and narrative into a smaller subset of emphasized or deemphasized messages about who gets what, when, and how (Lasswell 1936). This selective process is not necessarily misleading or incomplete; rather, it reflects the strategic interests of those who seek to learn from the public or influence their reactions (Bernays 1923; Ginsberg 1988; Disch 2011; Barabas 2015). At the same time, frames do not have a deterministic effect on expressed opinions; the presence of competing frames and individuals' ability to reconcile frames with policy-specific knowledge, countervailing frames, and other pre-existing considerations limits the persuasiveness of emphasis frames (Druckman 2004). Indeed, the provision of information within a frame – rather than the frame's emphasis, narrowly defined – has the most dramatic effects on the opinions that individuals report (Leeper and Slothuus 2015). Information leads people to understand political issues and imagine how they might affect others. So while the provision of different information might lead to changing preferences, each expressed preference is still meaningful with respect to the emphasis and information contained within the question that provokes an answer.

I focus my argument on the content and context of redistributive policy opinions. While it is likely that many individuals possess stable and considered preferences on "easy" social issues (Carmines and Stimson 1980), economic policy decisions are usually conceptualized as "hard" issues (Johnston and Wronski 2015). Questions of redistributive public policy, which usually involve numeric representations of abstract quantities and center on the means of redistribution rather than the ends, refer to ideas that are difficult for many individuals to conceptualize (Sides 2016; Johnston

and Wronski 2015; Wojcieszak 2014). Since economic issues, including class mobility and inequality, are fundamentally abstract concepts, their political existence as ideas cannot be separated from the emphasis frames and selective information that political actors marshal when describing them. As a result, I focus on the representation of redistributive policy in survey questions, identifying the attributes of questions that limit their interpretability. I also advocate for particular representational strategies: using concrete examples to anchor respondent ideas about the policy in question and providing group-centric cues to convey who the policy affects make survey questions more political by specifying distributive consequences directly.

Political Stimuli and Preference Formation

Scholars of public opinion implicitly conceptualize the public as "little deciders:" individuals who think about politics as a series of potential policy choices and possess fundamental preferences about those choices. In a democratic society, these judgments are meant to guide the behavior of elected officials (Dahl 1956; 1989; Pitkin 1967; Pennock 1979). However, the idea of a well-informed citizenry with consistent policy preferences is a progressive era invention: before mass communication, ruling elites did not presume that individuals would be competent or interested in governance (Schudson 1998). As Althaus (2008) explains, "while capital-P Progressivism has gone out of favor in much of the academy, it seems to thrive as a set of background assumptions in the mainstream of public-opinion research" (91). If survey respondents are provided with only general statements about complex policy choices, only the most well informed "little deciders" will provide meaningful answers, as most people do not have the motivation to learn what potential policies entail. Instead of implicitly requiring that individuals already possess this knowledge – what Lupia (2006) calls an "elitist move" (219) – opinion researchers should provide information about different policy choices, suggest how relevant groups will be affected by the choice, and allow individuals to articulate preferences in response to that information. In practical terms, this means providing

accessible anchors and cues – examples of the consequences of political choices – within survey items.

Individuals possess a range of potentially conflicting considerations – and not universal and fixed preferences – when it comes to most political issues (Zaller and Feldman 1992; Zaller 1992). Since people sample across accessible considerations when articulating opinions, attitudes reported in surveys lack ideological consistency and reliability, while also being subject to question wording effects. When drafting survey questions, researchers are instructed to use "simple, familiar words" but also be "specific and concrete;" they should "avoid leading or loaded questions" but discourage non-response by avoiding "don't know" answer options (Krosnick and Presser 2010, 264). Of course, simple and familiar words are rarely specific and concrete when it comes to redistributive policy. The term "welfare" does not mean the same thing as "assistance to the poor" for most individuals (Huber and Paris 2013). And although the choice to omit "don't know" as a response option is meant to "encourage respondents to do the work necessary to retrieve relevant information from memory," that information frequently doesn't exist, especially in complicated policy arenas (Krosnick and Presser 2010, 384).

In general, survey questions do not aim to make relevant political information or group-centric cues immediately salient – for fear of appearing leading or loaded – and so expressed opinions reflect subjective and potentially incomparable preexisting ideas about political reality. In light of this difficulty, some recommend aggregation – either across respondents (Page and Shapiro 1992; Gilens 2012) or across items (Ansolabehere et al 2008; Wlezien 1995) – as a means of estimating more reliable opinions. But the notion that individuals hold some underlying true opinion that can be measured with more or less error assumes away the changing contextual determinants of real world political expressions. By being explicit about the political choices involved in the

provision of stimuli within question wordings, the concept of political opinion better captures the mutually constitutive nature of those stimuli and reported opinions.

Political actors, including candidates, representatives, and the media, send political messages that are more concrete and group-centric than the information provided within the wording of many opinion surveys. As a result, existing opinion questions may not in fact measure the opinions that "governments find prudent to heed" (Key 1964, 14). For example, if elected officials learn that their constituents think that government "should take more responsibility to ensure that everyone is provided for," it is unclear what they should actually do in response (Alesina and Giuliano 2009). Instead, political actors frequently provide individuals with very specific political information in order to encourage political behavior. The news media, including ideologically-oriented sources, impart political knowledge directly to individuals (Schroeder and Stone 2015). Likewise, electoral campaigns choose to highlight particular policies as totemic "symbols" in order to "build public acceptance" for them (Sheingate 2015; Sheingate 2013, 153, citing Bernays 1923). The project of defining who should get what is a foundational aspect of politics (Lasswell 1936); when questions obscure the "who" and "what," reported answers are less politically valid.

A longstanding debate in opinion research concerns the status of unconsidered opinions. Converse (1964) famously argued that when individuals lack opinions about particular issues, they answer randomly, creating "non-attitudes" that lack both validity and reliability. Others (Achen 1975; Erikson 1978; Page and Shapiro 1992; Alvarez and Brehm 2002) instead claim that attitudes and preferences are "a distribution of points around some central position," introducing measurement error as an explanation for response instability (Achen 1975, 1220). In response to this debate, recent work advocates the use of multiple measures to assess underlying preferences. Ansolabehere, Rodden, and Snyder (2008) combine related policy questions and find strong over-time correlations in public attitudes when aggregated into additive indices. Yet this approach also

obscures the most political part of a preference: since individual survey questions often include comparative evaluations of particular policies, it is important to know what people think about those policies. When it comes to understanding redistributive opinion, each of these "solutions" to the problem of non-attitudes shifts scrutiny away from pollsters and toward the public. They assume that survey questions represent political reality with perfect fidelity, and that the public responds to those representations either with perfect accuracy, randomly, or probabilistically. By contrast, I argue that unconsidered opinions are structurally determined by uninformative survey questions. Consider the two models of opinion formation shown in Figure 2.1:

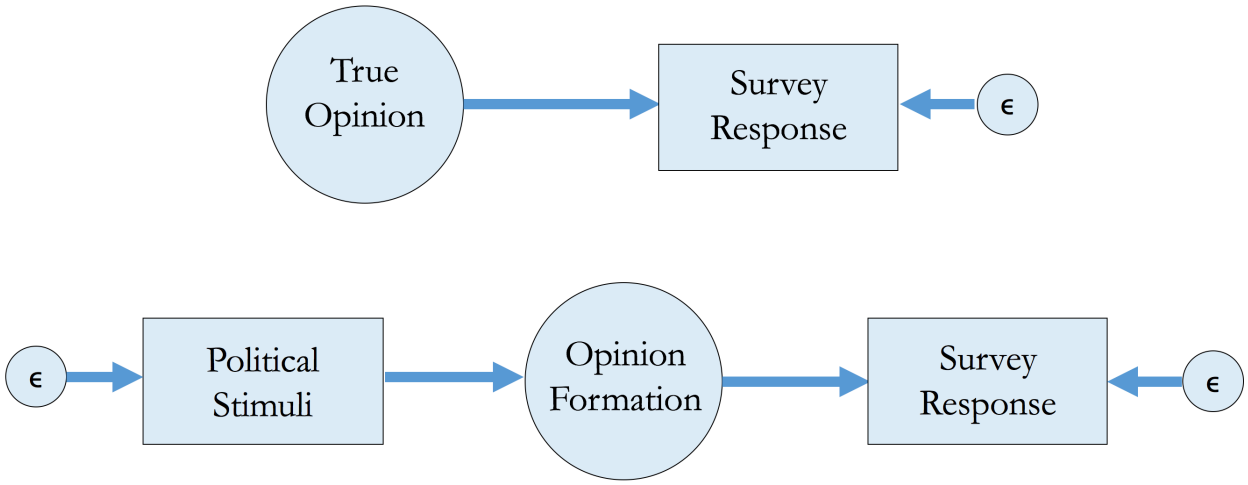


Figure 2.1: Two Models of Opinion Formation

The canonical model of survey response is depicted at the top of the figure (Ansolabehere et al 2008). As is standard in structural equation modeling, circles refer to unobserved concepts, while boxes refer to directly measurable concepts. In the canonical model, a person samples across salient considerations when reporting an unobserved true opinion. That opinion either exists as-is (in which case it is reported), doesn't exist (in which case the respondent choses at random), or exists as a distribution of possible responses from which one is probabilistically reported (Achen 1975). Thus the survey response is a measure of true opinion, though an imperfect one; it includes error. Lee

(2015) summarizes a central assumption of this model: "polls properly mirror public opinion only when they ask about the concerns that are actually on people's minds, concerns about which held views are firm and compelled to find voice." By contrast, the lower model explicitly includes observed political stimuli as a precursor to opinion formation, which leads to an observed survey response, also measured with error. In both models, individuals articulate an opinion (whether latent and true or contextually dependent) by answering a question. The crucial difference between these two models is that only the latter explicitly models the type of political stimuli as a direct cause of opinion formation. In the first model, the existence of question-wording effects presents a "catalogue of horrors" that threatens to undermine the democratic validity of public opinion (Zaller 1992, 29; Bartels 2003). In the second model, opinion is always directly affected by question wording, and the democratic validity of expressed opinions depends on the political aim of the questions asked.

Like Lodge and Taber (2014), I take a "constructionist approach" toward opinion formation that allows for the possibility of strong environmental cues and affective reactions (28). But while Lodge and Taber's model of political information processing focuses on the automatic and affective internal forces that shape expressed opinions, I focus on the observable external stimuli that inform preference articulation. In my account, preference measurement is more "architecture" than "archaeology:" the design and structure of questions should increase the salience of affected groups and policy implications, thus building an expressed opinion, rather than simply "uncovering existing values" (Payne et al 1999, 243). I reject the claim that "preferences among government policy options on specific issues, presidential approval, and many more such variables reside in citizens' heads" (Pasek and Krosnick 2010, 28). Instead, preferences are created when individuals encounter political questions that represent issues and outcomes in order to solicit a new evaluation.

Asking About Redistribution

Widely used redistributive policy survey questions provide respondents with only vague representations of the policies and groups they concern. For example, Alesina and Giuliano (2009) rely upon both a standard GSS item that asks if the government "should do everything it can to improve the standard of living of all poor Americans" and an item from the World Values Survey that ask if individuals agree that "'the government should take more responsibility to ensure that everyone is provided for'" in order to measure redistributive preferences (11-12). The lack of specificity in each of these items limits their ability to capture preferences, narrowly defined as a comparative decision in favor of one policy over another, since they do not provide consistent representations of policy consequences. Who counts as a poor American? What specific responsibilities should the government take on? McCall and Kenworthy (2009) and McCall (2013) assess progressive tax preferences with GSS items that refer to three (undefined) income groups. For those with high, middle, and low incomes, the items ask if taxes are much too high, too high, about right, too low, or much too low. Since individuals have different ideas about "high" and "low," interpersonal incomparability in response threatens the validity of reported responses (Hopkins and King 2010). Bartels (2008) adopts an ANES item that asks if respondents "feel that the government in Washington should see to it that every person has a job and a good standard of living… [or] just let each person get ahead on his own" (79). Missing from this item are any concrete policy choices: it is unclear if the government would create jobs through the nationalization of industry, tax subsidies, or an expansion of Americorps, among countless other options. Individuals also differ in their interpretation of a "good standard of living" and likely feel that some disadvantaged groups are more deserving of government benefits than others (Jensen and Petersen 2016; Kinder and Sanders 1996).

An alternative, but similarly limited analytic approach uses aggregation across redistributive policy questions to improve preference measurement. Kelly and Enns (2010), Luttig (2013), and Johnston and Newman (2016) estimate the effects of economic inequality on public mood liberalism, an index of hundreds of different opinion questions, most of which provide no policy specificity or contextual information. Cavaillé and Trump (2015) use forty-seven general statements about both the distribution of resources and specific preferences for government intervention to distinguish between support for redistribution *from* the rich and *to* the poor. But in each of these cases, individuals are not provided with anchoring information that convey the political consequences of what might be unfamiliar policies. Group cues (rich, poor) appear at times, but the criteria for group membership are not defined at the outset. Rather, these survey questions provide a simple statement about either general government policies or descriptions of how amorphous groups of rich and poor people might be affected by government policies, and then prod respondents for general evaluations.

While the research discussed above (and much related work) provides descriptive evidence about the public's attitudes and general orientations toward inequality and redistribution, little research uses reliable and valid measures of redistributive preferences per se: comparative evaluations of potential policy choices. Survey questions can only assess preferences by including political information and allowing individuals to choose between distinct alternatives. Page, Bartels, and Seawright (2013) provide an example of how to assess preferences by anchoring responses: they ask whether "the minimum wage should be high enough so that no family with a full-time worker falls below the poverty line." By specifically framing the "blooming, buzzing confusion" of redistributive policy reality (James 1890, 462) with reference to a specific policy, defined group, and political decisions, this item provides a better staging ground for the formation of political opinions.

Of course, the decision of what information (and which groups) to include in such a question is political – which is precisely the point.

Given that there is no authoritative or scientific method for determining what political information belongs in survey questions, I rely upon empirical results from social and political psychology to develop general recommendations meant to increase questions' ability to reliably assess preferences. I focus on two strategies for increasing the interpretability of questions: providing anchoring information and group-centric cues. By anchoring information, I mean that questions should initially describe specific policy consequences in concrete and intelligible terms before soliciting a preference. Anchoring leads respondents to form decisions that are biased toward the provided information (Tversky and Kahneman 1974). As long as that information is an accurate representation of the political decision in question, anchoring should improve response reliability in a politically defensible way. But information must also be easy to understand: textual descriptions should facilitate accurate imaginary conceptions of a policy's effects among respondents. For example, questions about government debt should be specific about the individual-level consequences of increasing debt, while questions that ask about support for redistributive policies should actually name specific programmatic outcomes that the policies would initiate or sustain. Rather than asking if the government should "do all it can to improve the standard of living of all poor Americans," questions should ask if government should provide food and monthly payments to households who earn less than a specified minimum income. Questions that are not so precise capture redistributive attitudes or egalitarian sentiment: distinct concepts that might prefigure preferences, rather than preferences themselves. But by clearly stating information about a policy's effects or costs, questions encourage respondents to make more politically informed choices.

A long line of research shows that individuals rely on social groups as a heuristic when interpreting and evaluating political choices (Tajfel 1981; Tajfel and Turner 1987; Kinder and

Sanders 1996; Nelson and Kinder 1996; Winter 2008; Kinder and Dale-Riddle 2012; Piston 2014). Attitudes about groups provide individuals with a cognitive shortcut: if the policy helps a favored group, the policy is worth supporting. Social groups are especially likely to be a relevant heuristic when it comes to "sites of persistent inequality" because the political decisions that create inequality often target specific social groups (Kinder and Dale-Riddle 2012, 8). Racial group evaluations are a powerful determinant of opposition to some forms of redistributive social spending, especially Temporary Assistance for Needy Families benefits (Kinder and Sanders 1997; Gilens 1999) and social security (Winter 2006). However, race loses its predictive power in other realms, including support for an increased minimum wage, the earned income tax credit, and the estate tax. In those policy realms, attitudes toward class-based groups, including sympathy for the poor and resentment against the rich, are more powerful correlates of redistributive opinion (Piston 2014). By clearly identifying social groups, survey questions help respondents link their evaluations of those groups with policies that affect them, improving the quality of reported policy evaluations. For example, a question that asks whether government should reduce income inequality in the abstract is less group-centric than one that asks whether government should reduce income differences between the rich and the poor. And since the words "rich" and "poor" mean different things to different people, anchoring vignettes that provide a specific definition of each group (for example, information about each group's monthly take-home income) helps address problems of inter-personal incomparability (Hopkins and King 2010; King et al 2004). This decision decreases the chance that respondents' ideas about who a policy affects will be biased by incomparable baselines.

Discussion and Conclusion

In this chapter, I argue that a foundational epistemological premise of public opinion research is not tenable: individuals do not always possess pre-existing opinions or considerations that are meaningfully distinct from the stimuli that initiate their expression. When it comes to

complex questions of redistributive policy, the premise that they do is especially untenable. Measures of opinion should not be judged by their "accuracy," but rather, their validity, with reference to changing political realities. By showing people what they do or do not want by providing anchoring examples within question wording, opinion analysts can improve individuals' ability to articulate meaningful political decisions.

Critics might raise the following objection: "political opinion" could be a euphemism for push-polling, in which the provision of political information biases responses toward one particular decision, while less informative stimuli would instead promote more balanced and conscientious reflection. However, this perspective is elitist (Lupia 2006): it assumes that individuals ought to obtain information about abstruse policy proposals independently, so that they can reflect on all possible courses of action just in case they are randomly selected by a pollster. In a representative democracy, individuals do not need to seek out information for all possible policy decisions; they elect officials to do that on their behalf. But some opinion researchers implicitly assume the opposite: that all respondents are equally able to understand and imagine the specific political consequences of often vague and non-specific question words. As existing research (Huber and Paris 2013; King et al 2004; Hopkins and King 2010) demonstrates, that assumption results in less valid opinion measurement. It also leads analysts to adopt a variety of post-hoc measurement models (aggregation across respondents and items) that depoliticize opinion by severing the link between majority approval and specific policy decisions.

My alternative recommendation – that survey items contain concrete political information and group cues – highlights the political choices involved in question wording. There is no scientific, unbiased choice of words that transparently conveys political consequences or policy choices. The decision to define up front who counts as "rich" or "poor" will never please everyone. But by admitting that question words are political and then measuring responses to those words, opinion

scholars can be more transparent and scientific about what they are measuring. Political preferences exist, but assessing them requires the provision of explicitly political provocations within surveys. Without those provocations, elected officials can only be responsive to vague and uninformative expressions of will. The "miracle of aggregation" and other means of extracting meaning from vague surveys questions each constrains the political power of public opinion by reducing affirmative decisions to latent values. By accepting the fact that opinion measurement is both scientific and political, analysts can better assess public preferences and strengthen the link between preferences and policies, ultimately improving scholars' ability to assess substantive democratic representation.

3 Visualizing the Economy[1]

Abstract

Existing claims that Americans oppose redistribution rely upon uninformative survey questions and vague representations of proposed policies. I examine the effects of informative cues and emphasis frames embedded within visual representations of policies on support for redistribution using nationally-representative samples. Since individuals process visual information more efficiently than text, I argue that visual source cues and graphical frames should be an especially powerful means of establishing credibility and emphasizing particular evaluations of information. I conduct two experiments in which individuals encounter the same information depicted alongside randomly assigned visual context. My results reveal that Americans are broadly supportive of direct redistribution, and that identity-consistent and emphatic visual representations of economic information shape reported policy preferences, even when holding information constant. By examining visual representations of political information, communication scholars can increase the external validity of framing and source cue experiments.

[1] Study 2 in this chapter was previously published in *Research and Politics* (Hughes 2015).

"Pictures have always been the surest way of conveying an idea, and next in order, words that call up pictures in memory." – Walter Lippmann (1922, 162-163).

In the previous chapter, I argued that public opinion on questions of redistributive policy are structurally determined by the way that those policies are described. In this chapter, I provide a more stringent test of this claim: I examine whether visual representations of policies shape reported opinions, holding textual information constant. Existing research suggests that individuals' evaluations of economic reality are politically consequential. Bartels (2008) argues that narrow economic retrospection among voters explains Republican presidential election victories, while Hochschild (1981) argues that perceptions of general distributional equity – rather than equality – encourage ambivalence toward redistribution and ultimately help explain the limited American social welfare state. These consequential evaluations do not emerge randomly, nor do they reflect unmediated perceptions of economic reality. Instead, political representations of economic reality shape the perceptions and evaluations that individuals articulate and act upon. In this chapter, I focus on the ways that different visual representations of information – rather than changes in the information itself – affect opinion. I conduct two original survey experiments to test how visual cues and graphical emphases shape expressed redistributive preferences. I provide evidence that visual cues and frames operate similarly to textual cues and frames, especially among those predisposed to accept the arguments they convey.

While a growing body of evidence suggests that the provision of economic information shapes redistributive policy preferences (Kuzimienko et al 2015; Cruces et al 2013), researchers have not isolated the effects of different representations of the same economic information. Visual or formal elements within an informational stimulus shape readers' interpretations of the information separately from the effect of the information itself, just as frames and cues can shape opinion without actually conveying any additional issue-specific information. For example, an advocacy group that wants to highlight recent gun violence might state an empirical fact (the number of mass

shootings is rising) and then depict the same information in a long time-series graph, emphasizing the broad historical trend in order to heighten the dramatic stakes of the issue frame. By visualizing abstract and complex public policies, advocacy groups and other political actors provide individuals with a point of view from which to understand an issue. And since people process information illustrated with visual representations more efficiently than verbal content (Mayer 2005), visual political communications have the capacity to deliver emphases more powerfully then text alone.

In this chapter, I test whether visual frames and cues shape opinion. To do so, I examine both the effects of visual political stimuli relative to no information (Study 1) and the effect of visual political stimuli relative to the same stimuli presented textually (Study 2). By randomly assigning individuals to different informational stimuli, I estimate the causal effect of visual political communication. I increase the external validity of estimated effects by adopting political stimuli that appear to originate from ideological sources. In sum, my findings suggest that visual frames and cues are powerful determinants of opinion, but that their effects are limited: only those who are predisposed to accept the argument conveyed by the frame or cue are likely to report opinions consistent with that argument.

Processing Frames and Cues

Existing research on political communication is sometimes unclear about the conceptual status of framing, failing to distinguish between framing as a means of rhetorical emphasis and framing as a means of providing substantive content or information (Cacciatore et al 2016; Leeper and Slothuus 2015). I define issue framing as a process of selective communication, in which a political actor emphasizes a particular interpretation of some information to convey "the essence of the issue," (Gamson and Modigliani 1987, 143). A long line of research on framing effects (Winter 2008; Chong and Druckman 2007; Kinder and Sanders 1996; Nelson et al 1997) definitively shows that the way policies are framed affects reported opinions. Frames shape opinion because they

"organize—or better, reorganize—information that citizens already have in mind" (Kinder 2003, 359). For example, anti-affirmative action communication might frame the issue by highlighting "undeserved advantages" enjoyed by beneficiaries or "reverse discrimination" against majority groups (Kinder and Sanders 1990). This process is distinct from political persuasion, which involves the provision of "arguments and evidence" that leads people to "change their minds" (Kinder 2003, 367). Frames convey meaning by suggesting how an audience should think about an issue, rather than explaining why one particular interpretation is accurate or correct. In order to isolate the effects of rhetorical emphasis within a fixed informational stimulus, the studies reported here randomly assign representations of the same information, rather than providing respondents with two distinct (but comparable) kinds of information.

Like frames, source cues affect evaluations without providing new information about an issue. Cues are "pieces of information that enable people to form evaluations of an attitude object without in depth knowledge" of the object (Nicholson 2011, 1166; Eagly and Chaiken 1993; Kelman 1958; Zaller 1992; Hartman and Weber 2009). Source cues, which suggest what source some information originates from, serve as a heuristic, allowing individuals to decide whether the information is credible or persuasive separately from the content of the information itself. For example, Druckman (2001a) compared the persuasiveness of articles that were described as originating from either *The New York Times* or *The National Inquirer*, under the expectation that the former source cue would make the story appear to be more credible. Like issue framing, the provision of source cues does not involve the addition or subtraction of information. Rather, different source cues lead individuals to view the same information in different ways.

Visual cues and frames have the capacity to shape opinion in a similar way as rhetorical cues and frames. However, the fact that individuals process visual information separately from verbal information suggests that visual emphases could be more powerful than words alone. Visual political

communications engage both verbal and visual processing systems (Paivio 1986), increasing individuals' abilities to organize relevant information into coherent mental representations (Mayer 2005; Prior 2014). As a result, individuals retain and recognize visual information more efficiently and for longer periods of time than if they were exposed to verbal information (Grabe and Bucy 2009, 17). When describing the distributional consequences of economic policies, words alone are especially unlikely to be effective frames, as numeric information is difficult to process. Individuals vary in their ability to process quantitative information; some are more numerate than others (Peters et al 2006; Merola and Hitt 2016). For those who are less numerate, verbal frames that emphasize that just 0.14% of households pay the estate tax should be less powerful than visual representations of the same information. Similarly, visual source cues (for example, a sponsorship logo) provide individuals with a simple heuristic for evaluating the credibility of more complex economic information.

Existing research on media source cues typically involves the random assignment of textual source information (Druckman 2001a) or verbal statements by reporters who identify as members of ideologically oriented media outlets (Turner 2007). In these cases, participants are informed that the information they read originates from a particular media source, but the medium of the source cue is the same as the information itself. The choice to explicitly label the source of political information could overstate source cue effects, since many cues are not embedded within the text of a graph, report, or video. Similarly, research on framing effects overwhelmingly focuses on rhetorical, rather than visual, emphasis (Chong and Druckman 2007). While some framing studies involve the provision of images meant to illustrate the frame of interest (Druckman 2001a), no existing work examines variation within images that both represent the same information. In order to identify the effect of visual framing, I randomly assign variation within the same representation, so that the separate effects of information and emphasis are not confounded.

While observational research on visual politics provides evidence for how political actors represent policies via pictures and graphs (Grabe and Bucy 2009), experimental research on visual politics is more limited. Developed lines of research examine the effects of visual elements in surveys (Fuchs 2009; Tourangeau, Couper, and Conrad 2013) and the effects of political images more generally (Petersen 2011; Swigger 2012; Prior 2014; Gadarian 2014; Sullivan and Masters 1988), but this scholarship does not distinguish between visual information, visual cues, or visual frames.

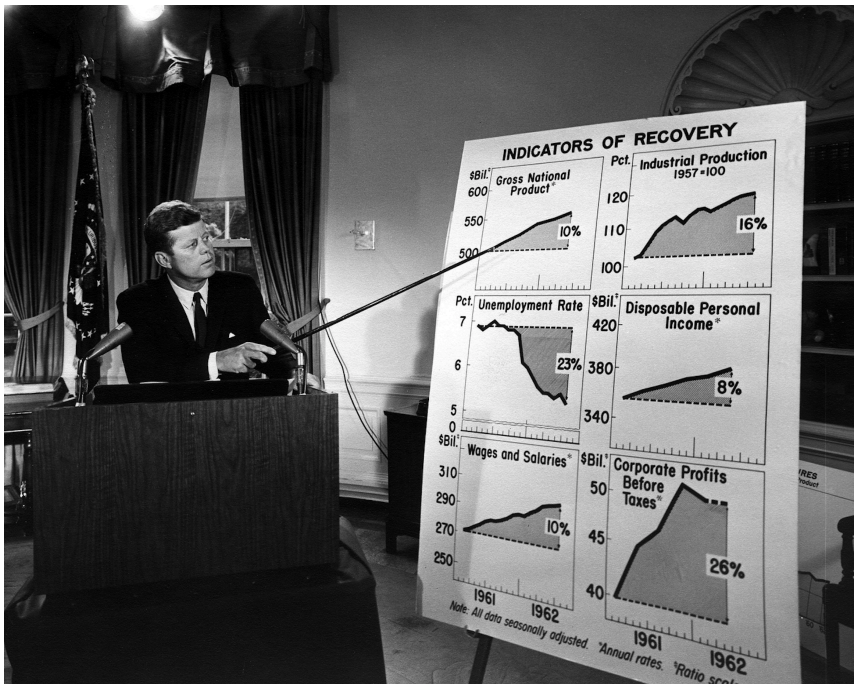


Figure 3.1: John F. Kennedy Address to the Nation on the state of the U.S. Economy, August 13, 1962

Figure 3.1 provides an example of visual frames in political communication: it shows John F. Kennedy advocating for a tax cut. To do so, Kennedy uses an array of visual representations of economic policies, each suggesting that American is recovering. Of course, each representation uses truncated X- and Y-axes in order to frame the information, suggesting that the recovery is relatively more impressive than an unconstrained graph would indicate.

Existing scholarship on graphical representation focus almost entirely on scientific graphs (Schwabish 2014; Tufte 2001; Cleveland and McGill 1985), ignoring graphical communications that approximate those used by campaigns and news media. Data visualization experts often recommend complexity and precision, but political campaigns and advocates often strive for simplicity and persuasion. These shortcomings in existing research limit the generalizability of empirical evidence for framing and source cue effects. To address these limitations, I adopt more externally valid visual stimuli based on real-world visual communications, in order to better understand how cues and frames affect real-world opinion formation.

Study 1: Credible Cues and Estate Tax Opinion

Individuals learn about political economy via communications from elected officials, campaigns, and the mass media (Zaller 1999; Druckman 2001b; Hartman and Weber 2009). When these sources describe unemployment, inflation, tax policy, and social policy, they use necessarily simplified representations of complex realities in order to convey information efficiently. As the preceding discussion suggests, stylistic variation within visual representations might affect individuals' interpretations of the information, separately from the effect of the information itself. I focus first on the representation and politics of the estate tax. An ongoing debate in American political economy concerns public support and opposition to inheritance taxes (Krupnikov et al 2006; Bartels 2008; Sides 2016). Very few Americans are subject to the tax, yet large proportions of Americans vigorously oppose it (Bartels 2008, 199). I expect that information about the tax will increase policy-specific knowledge and support for the tax, as previous research has shows. But I also expect that visual source cues embedded within estate tax information will increase these effects, conditional on partisan alignment between the individual and the source cue.

A consistent finding in research on estate tax opinion (Sides 2016; Bartels 2008) is that the provision of information about the estate tax increases support for it, especially among Democrats.

Following this line of research, I expect that exposure to accurate information about the tax will increase public support for the tax. Building on this research, I also expect that individuals will retain policy-specific knowledge if they are exposed to that information. Thus my first hypothesis concerns the provision of information, rather than the effect of visual cues:

H1: Information about the estate tax will increase policy-relevant knowledge and support for increasing the estate tax.

However, different visual representations of identical information should have distinct effects on opinion. When an individual perceives some representation to originate from a trusted source, she may be more likely to accept the information conveyed in the representation. I test this argument by embedding visual source cues within my representation of estate tax policy. I expect that individuals who are predisposed to accepting the depicted sources as credible will be more likely to accept information conveyed with those cues and will be more likely to change their attitudes than those who do not find a source credible.

H2: Information about the estate tax will have its largest effects on policy-relevant knowledge and support for increasing the estate tax when visual cues are consistent with viewers' political identities.

To test these hypotheses, I created informational representations of the estate tax that were meant to approximate real world media communications. The representations I used are shown in Figure 3.2. As the figure shows, each image contains a media source cue: the top image is ostensibly a screen-capture from Fox News' "The O'Reilly Factor," while the lower image appears to originate from MSNBC's "Rachael Maddow Show." I chose these sources because they are popularly associated with Republicans and Democrats, respectively (Schroeder and Stone 2015; Turner 2007). The visual cues I adopt consist of background graphics and logos obtained from actual televised segments. I modified each image to contain the same information and graphical representation of tax policy, while randomly assigning only the background and source logos.

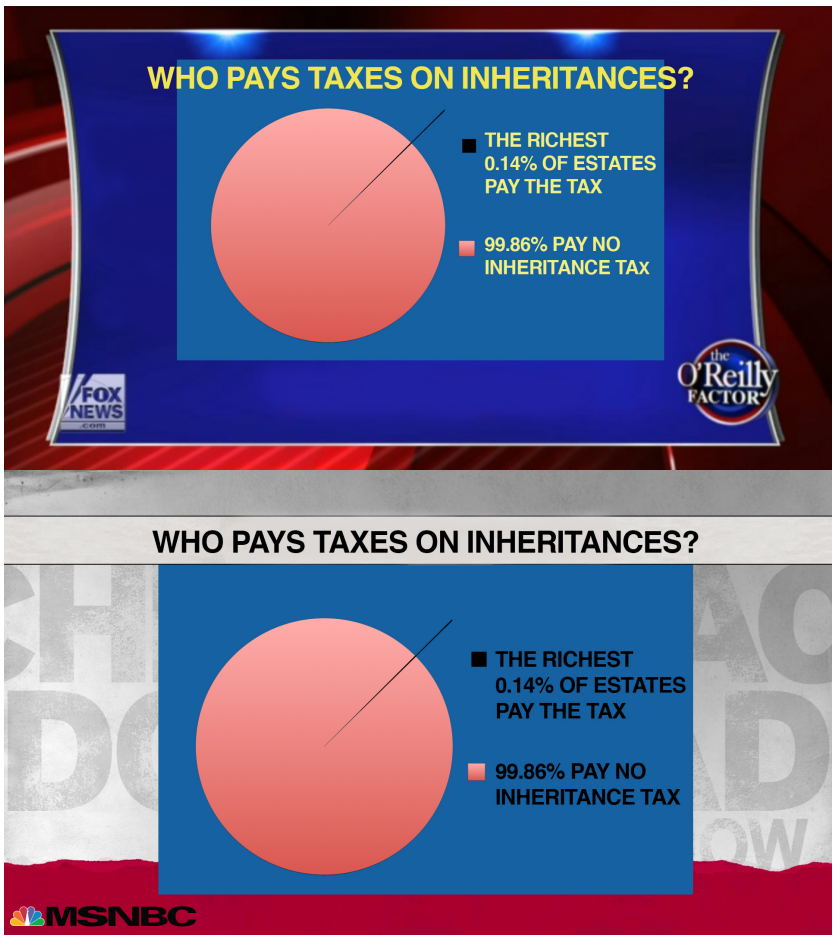


Figure 3.2: Informational Representations of Estate Tax Policy

From November 19 – 24, 2014, I contracted with Survey Sampling International to draw a sample of 715 individuals, meant to be representative of the U.S. as a whole by gender, ethnicity, age, and education. Of that sample, 624 respondents completed the entire survey and 642 completed the module used in this study. This sample is more ideologically representative of the US public than convenience samples of students or online labor market samples. However, it is slightly less conservative and Republican than the US public: 28% of the sample identified as conservative, comparable to the 40% of conservative identifiers in the 2012 ANES (weighted). 30% of the SSI sample identified as Republican, compared with 39% of 2012 ANES respondents. Subjects were randomly assigned to three groups: a control that saw no information, and groups that saw the information in Figure 3.2 (either the Fox or MSNBC version). After exposure to the images,

respondents completed several unrelated survey modules (focused on executive actions and political compromise in healthcare), and then were asked a factual question about the estate tax.

To test policy-relevant knowledge, I asked all subjects to indicate what percent of all households are subject to the estate tax using a multiple choice item. Answers included: the richest 41% / 14% / 1.4% / 0.14% / 0.0014% of all households, and a Don't Know option. Of 642 subjects, 23% chose "don't know." I code as "accurate" all responses that were within two percentage points of the true value (0.14%). By this standard, 50% of subjects answered correctly. Just 22% of subjects were exactly right. I assess support for increasing the estate tax rate with a seven-point scale that ranges from "oppose strongly" to "strongly support." 30% of respondents opposed increasing the estate tax rate, 44% supported increasing the rate, and 26% chose the midpoint on the scale ("neither support nor oppose"). I recode the scale from 0 to 1 in the analysis that follows.

To evaluate hypothesis 1, I first compare the percent correct among those who either were or were not exposed to any information. As expected, learning about the estate tax increases knowledge substantially: 56% of subjects exposed to the information correctly answered the question, while just 25% of those in the control group answered correctly. This difference is statistically significant at $p < 0.001$. Likewise, receipt of information about the estate tax increases support for increasing the tax rate, given that so few are actually subject to the tax. Support among those exposed to information about the tax averages 0.56, while support among those in the control condition is 0.45, a difference of 0.11 that is also statistically significant at $p < 0.001$. These results provide unambiguous evidence in favor of hypothesis 1: the provision of information shapes both knowledge of a policy issue area and opinion about the issue area. A majority of Americans supports the estate tax when they know how many households it affects. Figure 3.3 shows these results graphically. While my findings here are unsurprising in light of previous research (Kuzimienko et al

2015; Sides 2016), they provide additional evidence that public preferences are profoundly conditional upon the receipt of political information.

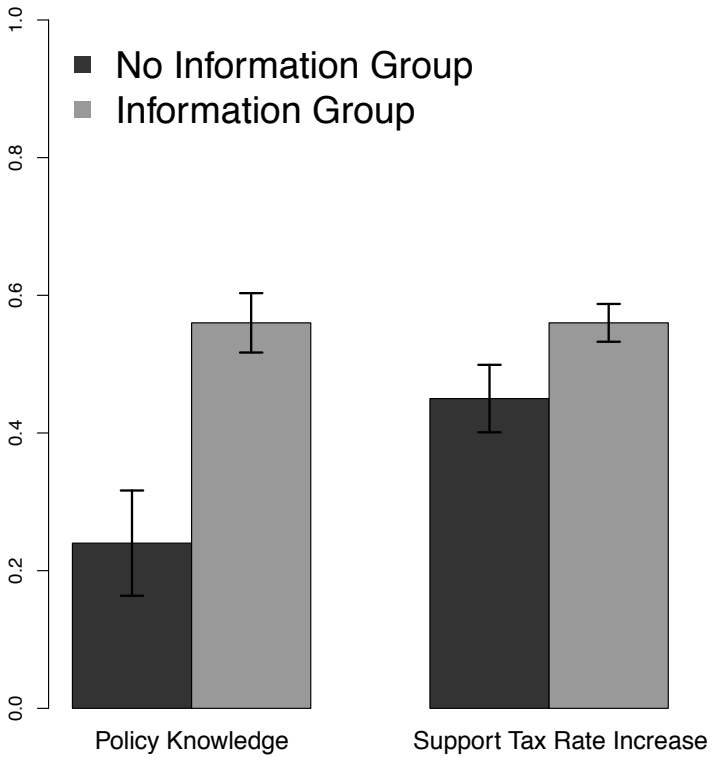


Figure 3.3: Average Correct Responses, by Party and Source Cue

The figure shows average correct responses and support for increasing the estate tax across all respondents in the the sample by treatment condition with 95% confidence intervals. Both differences are statistically significant at $p < 0.001$.

However, the informational treatments I deployed also contained source cues that should affect the credibility of each treatment. I expect that exposure to the Fox News-cuing treatment will lead Republicans to recall information about the estate tax more accurately, relative to Democrats and independents exposed to the same treatment. At the same time, the MSNBC-cuing treatment will have larger informational effects on Democrats. To test these expectations, I restrict my sample to just those subjects who received information about the estate tax. This allows me to isolate the treatment effect of the source cue, as information is held constant across both conditions.

First I compare correct responses among Republicans exposed to each cue. On average, 51% of Republicans in the MSNBC treatment accurately identified the proportion of households

who pay inheritance taxes, compared with 66% of Republicans in the Fox News treatment. This 15 percentage-point difference is statistically significant at $p < 0.05$ (one-sided test), despite a decrease in sample size to 151.

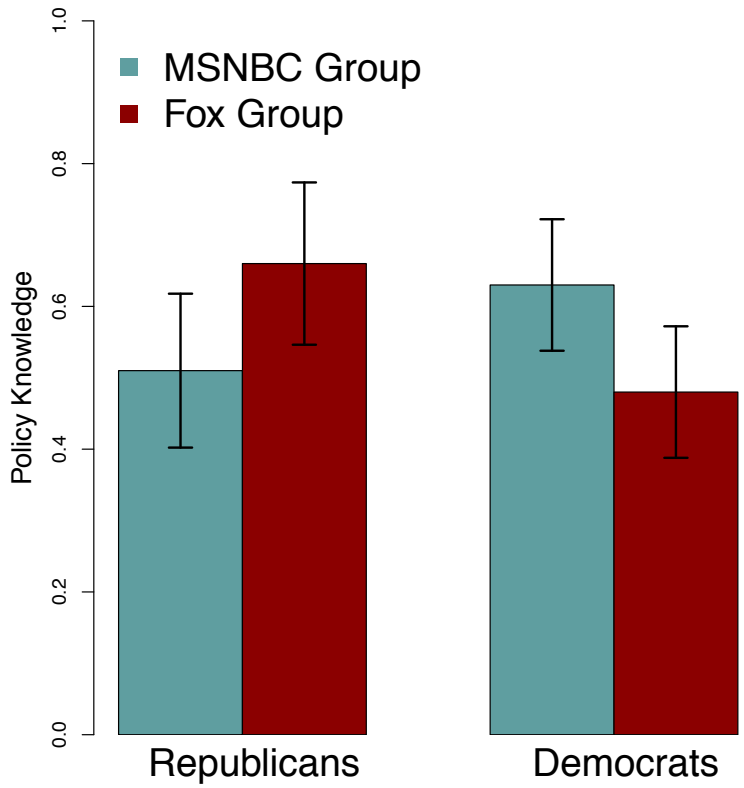


Figure 3.4: Average Correct Responses, by Party and Source Cue

The figure shows average correct responses across partisan subsets of the sample by treatment condition with 95% confidence intervals. Both differences are statistically significant at $p < 0.05$.

Republicans who ostensibly learned about the estate tax from Bill O'Reilly were much more likely to state correct policy-specific information than those who received Maddow-cuing information. I observe similar results for Democrats. 48% of Democrats in the Fox News condition correctly answered the knowledge item, compared with 64% of Democrats in the MSNBC condition, a difference of 15 percentage points that is also significant at $p < 0.05$ (with a sample size of 217). These results provide strong initial evidence in favor of H2: partisans who receive information from seemingly more credible sources are more likely to accurately report that information.

While these results provide initial evidence in favor of H2, I find mixed results when examining the effect of credible cues on policy support. For Republicans, exposure to the Fox News cue modestly increased support for increasing the estate tax rate as predicted (6 percentage points), but the effect is not statistically significant. For Democrats, I observe a positive shift of 7 percentage points after exposure to the Fox News cue, contrary to hypothesis 2. That is, Democrats who saw the MSNBC cue were actually six percentage points less likely to support increasing the rate than those in the Fox News condition. However, this effect is also not statistically significant, and both groups of Democrats were likely to support increasing the number of households who pay the estate tax. Overall these results suggest that credible cues have larger effects on information acceptance, rather than opinion change. However, further research using a wider variety of source cues and economic policy topics is necessary to determine how cue effects vary across policies.

Overall, Study 1 provides evidence that both the visual representation of political information shapes preferences, and that the provision of accurate information increases support for direct redistributive policies. Echoing related research, I demonstrated that informing individuals about estate tax policy with visual political representations increased their policy specific knowledge and led them to be supportive of increasing the tax rate. But more importantly, I showed that information retention depends on the credibility of visual source cues embedded in the information display. By comparing policy knowledge among subjects who saw the same information but from distinct sources, I isolated the effects of those cues on information recall. My results suggest that cues are an important precursor to opinion formation, even if they do not have an immediate effect on policy evaluations.

Study 2: Visualizing Income Inequality

Next I examine the effect of visual frames on reported opinions, holding information constant. To do so, I provide fixed textual information about increasing income inequality to a

group of experimental subjects and randomly assign subsets of the sample to view different graphical representations of the same information. I then assess redistributive preferences, identifying the effect of graphical emphasis on redistributive opinion formation. My results indicate the graphical frames have large effects: for Republicans and conservatives, the inclusion of a graph that de-emphasizes the information presented in text results in a 40% decrease in expressed support for intervention against inequality.

All graphs lead the viewer to perceive a spatial relationship from a particular point of view: there are no universal criteria for determining whether a graph is leading, misleading, unfair, or biased. The very act of illustrating a complex policy issue with a particular line or shape circumscribes alternative interpretations and excludes competing representations. By emphasizing or deemphasizing the contrast between quantities, graphs provide visual emphasis frames that guide opinion formation. Such graphs should also encourage particular kinds of issue evaluations. When a graph deemphasizes information conveyed in text, I expect that individuals will be less likely to think that the information is politically important. Thus, their evaluations of the issue should change.

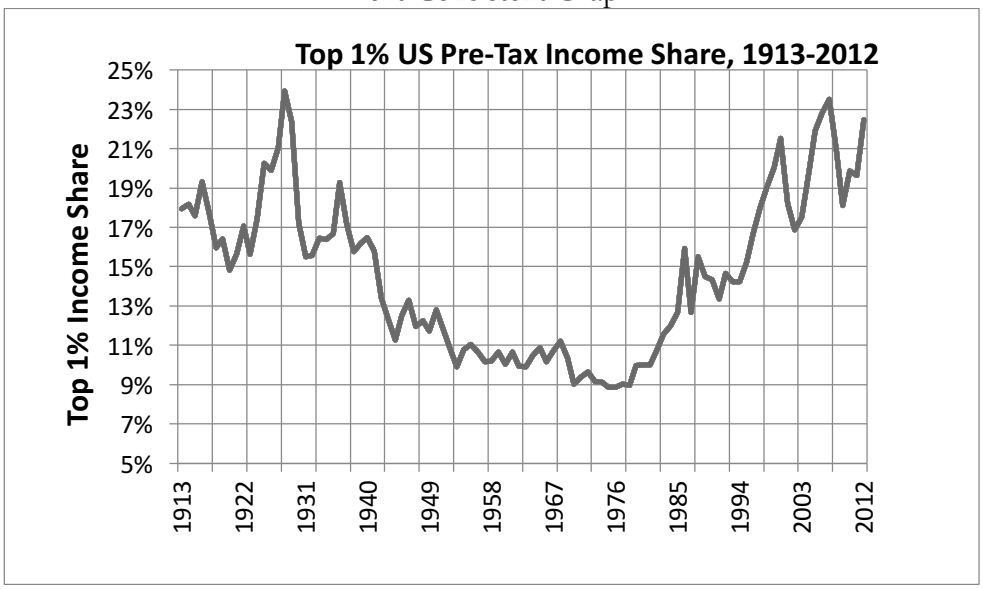
H3: Individuals exposed to graphs that deemphasize textual information will form opinions consistent with the graphical representation.

At the same time, individuals engage in motivated reasoning: they tend to evaluate new sources of information in a way that is consistent with preexisting beliefs (Taber and Lodge 2006), especially when they perceive information to align with partisan and ideological commitments (Slothuus and de Vreese 2010). If graphical emphasis is more consistent with partisan or ideological priors than textual information, graphs should have a larger influence on reported opinions. I expect that when graphical emphasis coincides with an individual's preexisting partisan and ideological considerations about an issue, the individual will be more likely to engage in motivated reasoning, discounting textual information in favor of the graphical representation.

H4: Graphs that deemphasize textual information with have stronger effects on individuals who are predisposed against the textual information.

In order to test the effects of graphical emphasis, I provide the same textual information about rising inequality to a sample of experimental participants, and then randomly assign subjects to view graphs that depict the textual information in different ways.

Text-Consistent Graph



De-Emphasis Graph

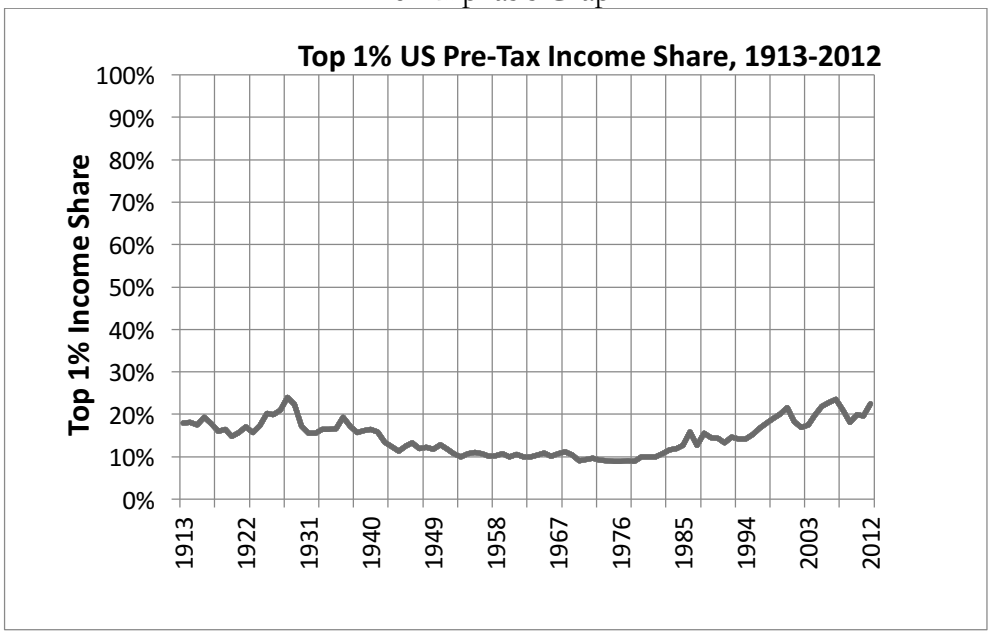


Figure 3.5 Graphs of Income Inequality of Time

The first graph, appearing in the top panel of Figure 3.5, shows the pre-tax income share for the top 1% of earners from 1913 to 2012 with a truncated Y-axis of [5, 25]. The graph is based on Piketty and Saez's (2014) research, and is available to the public on Saez's website. The graph in the lower panel of Figure 3.5 depicts identical data, but with a Y-axis ranging from [0, 100]. As a result, the large contrast between the postwar years and the present is deemphasized: the overall pattern appears more like a straight line. The top graph emphasizes a change over time by restricting the Y-axis to [5, 25], while the bottom graph de-emphasizes that change by including the complete Y-axis. Of course, much of the space represented in the lower graph is empty - the top graph conveys subtle changes over time much more precisely.

From April 28 – May 6, 2014, I contracted with Survey Sampling International to draw a sample of 540 individuals, meant to be representative of the U.S. as a whole by gender, ethnicity, age, and education.[2] This sample is more ideologically representative of the US public than convenience samples of students or online labor market samples. However, it is slightly less conservative and Republican than the US public: 30% of the sample identified as conservative, comparable to the 40% of conservative identifiers in the 2012 ANES (weighted). 27% of the SSI sample identified as Republican, compared with 39% of 2012 ANES respondents. Subjects were randomly assigned to three groups: a control that saw no graphs and two treatment groups that each saw one of the graphs depicted in Figure 3.5.

At the beginning of the survey, all three groups read the following text: "Some say that income inequality has been increasing in America. In the 1920's, the top 1% of the income distribution earned about 20% of all income. From the 1940's through the early 1980's that number

[2] See Chapter 3 Appendix for descriptive statistics. Experimental randomization was successful: using t-tests, I checked to ensure that exposure to the experimental treatments was balanced.

decreased to about 10%, but now the top 1% earns a little more than 20% of all income." Group 2 viewed the text-consistent graph (top panel, Figure 3.5), which emphasizes the change over time described in the text by focusing on the area of the graph between 5% and 25%. Group 3 viewed the de-emphasis graph (bottom panel, Figure 3.5). Group 1 received only the textual information. After exposure to the stimuli, all respondents were asked: "Do you favor, oppose, or neither favor nor oppose the government trying to decrease income inequality?" Respondents indicated favor or opposition on a seven-item Likert scale which I recode 0-1. 54% of respondents expressed support for decreasing income inequality.[3]

I expect that exposure to the graph that de-emphasizes the information conveyed in the text will increase the number of respondents who oppose intervention. Table 3.1 shows results for individuals in each of the three groups.

Government Should Reduce Inequality (0-1)	Group 1: Text Only	Group 2: Text-Consistent Graph	Group 3: De-Emphasis Graph
Average	0.62	0.60	0.51**
(Standard Deviation)	(0.33)	(0.34)	(0.35)
Observations	269 (50%)	146 (27%)	125 (23%)

Table 3.1: Average Inequality Intervention Opinion by Treatment Group

Across the board, most Americans support government action against inequality after reading about the topic. However, the table shows that exposure to the de-emphasis graph had a statistically significant negative effect on support for government intervention against inequality, relative to the text only group ($p < 0.01$, two-sided test) and the unchanged graph group ($p < 0.05$ two-sided test). The effect of the unchanged graph relative to the text only group is not statistically significant ($p = 0.56$). On average, individuals who were exposed to the de-emphasis graph were about 10

[3] This result closely corresponds with a January 2014 poll in which 51% of respondents said that they wanted the government more involved or as involved as it is when it comes to reducing income inequality (NBC News/Wall Street Journal 2014).

percentage points less supportive of intervention against income inequality, plus or minus 3.6 points, confirming hypothesis 1. Support for intervention among those who saw the text-consistent graph is not statistically distinguishable from support among those that read the text alone. By minimizing the change depicted in the information-consistent graph (and conveyed in the text), the de-emphasis graph decreases support for intervention against income inequality.

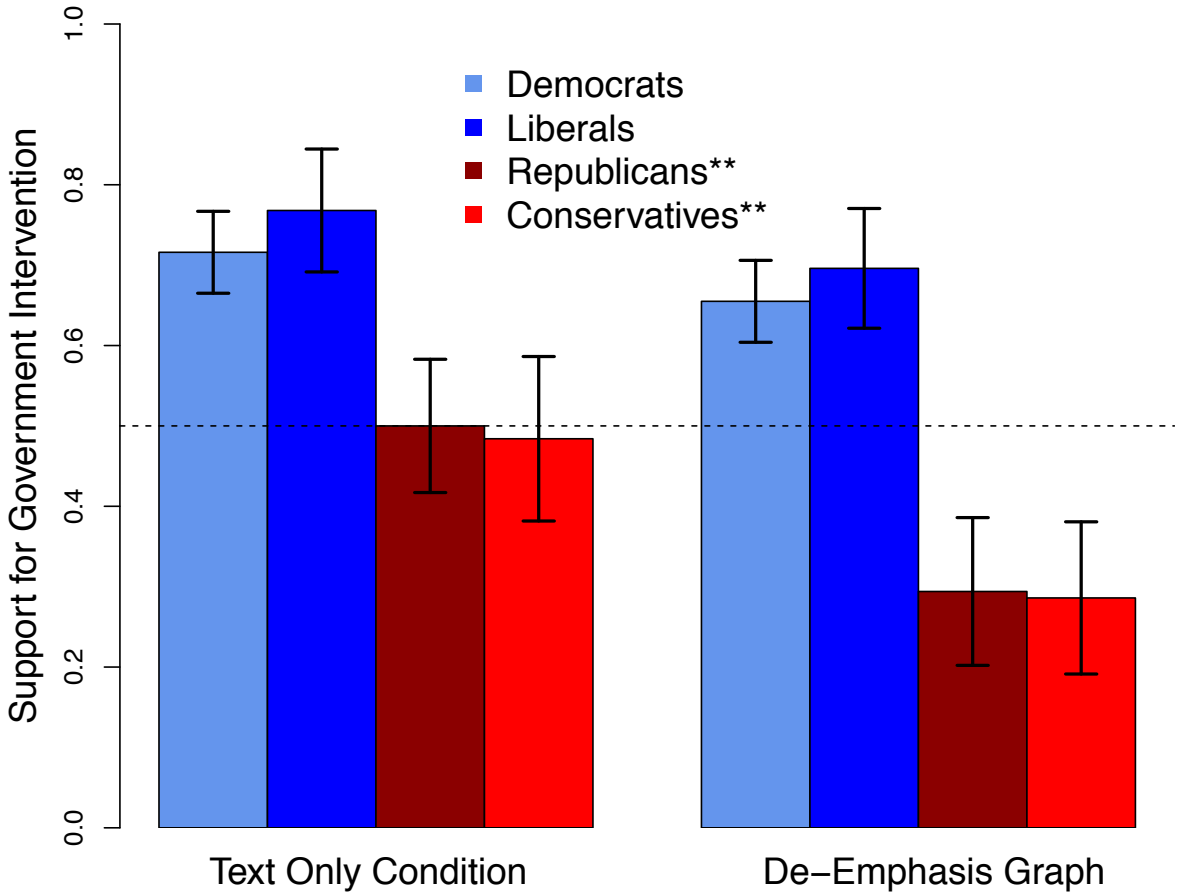


Figure 3.6: Average Inequality Intervention Support by De-Emphasis Graph Exposure

The figure shows average support for intervention across ideological and partisan subsets of the sample by treatment condition with 95% confidence intervals. T-tests indicate that exposure to the de-emphasis graph led all respondents to become less supportive of intervention relative to those who only read the textual information, but the effect is only statistically significant for Republicans and conservatives ($p = 0.003$ and 0.003, respectively). Results are similar when the text-consistent graph is used as the comparison group (see Appendix A3).

As hypothesis 2 suggests, I expect that this effect will be more pronounced for subsets of the sample. Specifically, I expect that the de-emphasis graph will have larger effects on intervention support among Republicans and conservatives than among Democrats and liberals. In order to

evaluate this claim, I examined average responses across subsets of the data. As Figure 3.6 shows, all respondents became less supportive of intervention after seeing the graph with the full Y-axis. But the provision of the graph only has a significant effect for Republicans and conservatives: among those subjects, support drops by 20.6 and 19.9 percentage points, respectively. This large and statistically significant effect dwarfs the modest shift observed for Democrats and liberals; it represents a decrease in overall support of 40% in each case. To understand how the strength of Republican and conservative identity affects these results, I estimate OLS regression equations, which appear in Table 3.2.

Dependent Variable: Government Should Reduce Inequality (0-1)			
De-emphasis Graph	-0.096 (0.034)**	-0.060 (0.038)	-0.053 (0.037)
Strength of Republican Identity (0-3)		-0.062 (0.016)**	
De-emphasis Graph × Strength of Republican Identity		-0.067 (0.034)*	
Strength of Conservative Identity (0-3)			-0.117 (0.019)**
De-emphasis Graph × Strength of Conservative Identity			-0.080 (0.038)*
Constant	0.609 (0.017)**	0.643 (0.018)**	0.664 (0.018)**
R^2	0.01	0.07	0.13
N	540	540	540

Standard errors in parentheses. ** p<0.01, * p<0.05

Table 3.2: Inequality Intervention Opinion Among Those Predisposed Against Information

As the Table shows, exposure to the de-emphasis graph had a modest but significant effect on all respondents' opinions, as discussed above. But for strong Republicans and strong conservatives, exposure to the de-emphasis graph has much more dramatic effects: the average treatment effect for strong Republicans is a 0.26 decrease in support for intervention, while for Republican leaners, the

effect is a more modest decrease of 0.13. For extreme and slight conservatives, the comparable effects are -0.29 and -0.14, respectively. As Republican and conservative identification increases, subjects appear increasingly likely to engage in motivated reasoning and accept the de-emphasized graphical representation of inequality information rather than the textual account.

Discussion and Conclusion

Walter Lippmann first described public opinion using visual terms: "The pictures inside the heads of these human beings, the pictures of themselves, of others, of their needs, purposes, and relationship, are their public opinions" (1922, 18). Yet research on public opinion and political communication almost uniformly focuses on verbal representations of political issues. Graphs and visual cues shape the pictures in people's heads even though they contain no additional information about the issue in question. As Study 1 shows, visual source cues determine whether individuals recall policy-specific information. Study 2 shows that graphical representations of economic reality shape policy preferences, especially when they coincide with individuals' political identities. Since almost all contemporary political communication has pronounced visual components (Grabe and Bucy 2009), the reported cuing and framing effects likely occur across a wide range of different media.

In the studies reported here, I am careful to distinguish between the effects of providing information and emphasizing fixed information via visual framing and cuing. Since individuals often misjudge the extent of income inequality (Norton and Ariely 2011; Cruces et al 2013; Kuzimienko et al 2015), economic mobility (Gimpelson and Treisman 2015; Kraus and Tan 2015), and their location in the income distribution (Fernández-Albertos and Kuo 2015), survey questions that do not provide objective economic information instead measure attitudes that are based on respondents' subjective perceptions of known quantities. If political preferences are interpreted with

respect to the actual political world, the project of soliciting preferences should also involve the provision of information.

In the context of an academic survey, the simple statement of an economic fact includes an implicit source cue: the imprimatur of the institution conducting the research. This source cue could increase the proportion of respondents who accept the information relative to the proportion that would accept it if provided by ideologically incongruent news organizations. Accordingly, this feature of academic survey research limits the external validity of estimated informational effects: individuals may not encounter reliable, accurate, or authoritative factual statements in real-world political discourse. To begin to address this concern, the informational stimuli in Study 1 contained mass-media source cues. Future research should examine a broader range of visual source cues and randomly assign the prominence of those cues. If respondents view scholarly graphs and images as more reliable than mass media visual communications, the information-focused research cited above potentially overstates the effects of economic information.

The results of the two studies reported here provide initial evidence in favor of my larger argument: the representations of economic issues in political communications condition individuals' reactions to those communications, ultimately shaping opinions. As the proliferation of political images continues via new media and electronic political communications, scholars must take a closer look at graphs and how they affect public opinion. At the same time, individuals also possess subjective evaluations of economic reality separate from their exposure to particular political messages. Indeed, individuals perceive local unemployment rates (Newman et al 2015) and other changes in the local and national economy via personal experience and evaluations of how other groups fare (Ansolabehere et al 2014). These perceptions cannot be randomly assigned, but still have the potential to shape economic policy preferences, either on their own or jointly with political stimuli. In the next chapter, I explore the interaction between political information and perceptions

of economic reality directly, arguing that the framing and cuing effects reported here are also conditional on the economic context within which they are articulated.

4 Opportunity is Relative

Abstract

The provision of economic mobility information shapes public support for redistributive policies meant to affect equality of opportunity, but different representations of economic facts convey distinct political messages. To test how variation in the geographic specificity of mobility information and local economic context jointly shape public preferences, I conduct a nationally representative survey experiment using local mobility estimates from Chetty et al (2014). In the experiment, I randomly assign respondents to receive no information, information about average mobility across U.S. commuting zones, or both national and local mobility estimates. Both treatments change attitudes toward a range of direct and indirect opportunity-increasing redistributive policies but local economic mobility context moderates observed effects: subjects in low mobility areas who receive the national treatment become more supportive of government intervention, while those in low mobility areas who receive the national and local treatment become less supportive of intervention. I argue that individuals' relative perceptions of how information applies to their own local context help explain these counterintuitive findings. These results suggest that campaign appeals that focus on low mobility rates might undermine support for intervention, depending on the geographic specificity of the appeal.

"In local affairs the cost of a policy is more easily visible." – Walter Lippmann (1922, 241).

The U.S. may be a land of opportunity for some, but recent evidence suggests that intergenerational economic mobility varies dramatically across the country (Putnam 2015; Chetty et al 2014; Duncan and Murnane 2011). In some parts of the U.S., dreams of upward mobility are likely to come true; in others, the chance that someone born poor will later become wealthy is extremely low. Economic mobility is a politically salient issue: both Democrats and Republicans increasingly frame economic policy appeals in terms of increasing opportunity rather than directly redistributing income (Rucker and Balz 2015; Calmes 2014; Bouie 2015). Since many Americans do endorse increased equality of opportunity, focusing political appeals on increasing economic mobility could marshal popular support for candidates who advocate for market-oriented redistribution. However, scholars have not examined the relationship between information about economic mobility and support for redistributive policy. It is also unclear how individuals interpret national and local-level economic information, especially when provided with both. In this chapter, I demonstrate that the supply of information about the rate of economic mobility shapes opinion, but that its effects are dependent on both the geographic specificity of the information and individuals' subjective perceptions of local economic context. I conduct an experimental analysis designed to test the relationship between different kinds of information about economic mobility and support for redistributive programs. My results show that the provision of distinct geographical reference points shape expressed policy evaluations, especially when those references points are more psychologically provocative.

National-level economic information makes no regional or local distinctions, and when it is the only information that individuals encounter, I expect that they will assume that the average rate applies to their local context. That is, when an individual in a low mobility area is told that the national mobility rate is 10%, she will perceive the rate to reflect relatively low levels of mobility and

support government intervention meant to improve mobility across the U.S. But when an individual lives in a very high mobility area, he will perceive the national rate with reference to his own, relatively mobile area, and be reluctant to support costly interventions: in his daily experience, immobility is not a formidable barrier to equality of opportunity.

However, the provision of both national and local information is more psychologically provocative than national-level information alone; it suggests that a respondent's local area is relatively worse or better than the rest of the country. If the respondent's area is more mobile than the national average, I expect that individuals will want the government to improve the rate for the rest of the country, while feeling economically secure and empowered to support policy change. If someone in a high mobility area is told that his area is more mobile than the country as a whole, he might think that the government should intervene in order to improve the average rate. But if an individual is told that he lives in a less mobile area than the national average, he might feel both economically insecure and unwilling to support intervention: the knowledge that his area lacks opportunity undermines support for costly political interventions. That person might ask why he should pay more to support mobility across the country, given his area's relative lack of mobility.

My analysis proceeds in three sections. First, I define economic mobility, articulate theoretical expectations and describe my empirical strategy. Second, I describe my experimental results and key findings. I show that individuals appear to perceive national information through a local lens. At the same time, providing both local and national information increases redistributive policy support among only those who are relatively more mobile than average. Finally, I discuss the implications of these findings for theories of opinion formation and political communication. In sum, I argue that perceptions of economic mobility and other economic policies are always relative: the political reference points used to represent economic information affects how individuals apply that information. In other words, drawing contrasts between relatively more or less mobile places

has the potential to increase or undermine policy support, underscoring the political nature of policy representation.

Economic Mobility and Redistributive Opinion

Intergenerational economic mobility refers here to the earnings of children relative to their parents. Economic mobility is distinct from social, occupational, or labor mobility in that it is centrally concerned with changes in position determined by wages (Iversen and Armstrong 2006, 4). In an area with high levels of economic mobility, the chance that children will earn more than their parents is high, while an immobile area would feature relatively fixed expected incomes across generations. Both political parties express support for increased economic mobility in principle because a high level of mobility implies equality of opportunity, a widely endorsed democratic norm. And as Egan (2014) points out, "opinion polling shows that support for efforts to reduce the inequality of opportunity in the United States is much stronger than the notion that government should intervene ex post to reduce income differences between the rich and poor" (2). However, existing research on mobility and opinion has been limited by the lack of accurate information about economic mobility and the difficulty of providing meaningful and precise estimates of mobility to individuals who live in more or less mobile contexts.

Existing evidence of economic mobility's effects on political attitudes relies on survey data to estimate mobility rates (Egan 2014; Harding et al 2005).[1] But survey self-reports, especially reports of past family income, suffer from error: it is possible that individuals misremember numerical information from 30 years prior or that social desirability effects lead them to under- or over-report sensitive information, including income. In order to resolve these measurement problems, Chetty et al (2014) use administrative tax data to measure economic mobility. The authors sample the entire

[1] This research also examines a more expansive conception of economic mobility, including measures of socioeconomic status other than income.

population of children born in the U.S. between 1980 and 1982 and use tax filings to identity each child's average household income from 1996-2000 (when the children are about age 18). Then they compare household earnings at that time with mean household income in 2011-2012, when the same children are between 30 and 32 years old. Using this data, the authors estimate the probability that a child reaches the top quintile of the national income distribution, starting from the bottom quintile, for each of 741 commuting zones. Commuting zones (CZ's) are based on aggregated counties: thus they are similar in size to metropolitan statistical areas but include the entirety of the U.S. (Chetty et al 2014, 2). The average CZ includes four counties and has a total population of about 380,000 (18). About 38% of individuals move from the CZ where they grew up to a different commuting zone, but the authors permanently assign location based on place of birth.

Since the probability of upward mobility is based on the national income distribution, this measurement strategy captures absolute upward mobility, permitting direct comparisons across commuting zones. At the same time, "CZ-level mobility statistics are robust to adjusting for differences in the local cost-of-living, shocks to local growth, and using alternative measures of income," increasing the validity of the between zone comparisons included here (2). The population-based average rate of intergenerational mobility is 7.5%, but the average rate across all commuting zones is 10%. I use the 10% rate as the national average within my experimental design in order to facilitate easier comparisons between local rates and a baseline of 10%. I assume that individuals can correctly identify their own commuting zones: the names of each zone are not abstruse or technical, but usually reflect the most populous area in each group of counties.

Existing research suggests that local economic context, including inequality and unemployment rates, shapes beliefs about meritocracy (Newman et al 2014), assessments of overall economic performance (Newman et al 2015; Sønderskov et al 2016), and social welfare preferences (Cutler 2007). However, existing accounts of how local context shapes preference formation are less

clear about the mechanisms by which that context becomes salient. When political campaigns and the news media increase the salience of local economic circumstances, it might be the case that preexisting ideas about that context become less persuasive or accessible in light of new representations. Individuals also encounter economic information in different ways: they may learn about the nation as a whole, about their own local area, or about both. At he same time, bad news about the economy increases feelings of economic insecurity, which serves as a powerful constraint on redistributive policy support. Levine (2015) shows that as political communications highlight issues like inequality and unemployment, individuals receiving those communications become less motivated to devote time, energy, and resources to political action against the status quo. While individuals are likely to be aware of local economic circumstances, messages prompting them to support government intervention within those circumstances are sometimes self-undermining.

Since equality of opportunity is a widely endorsed political norm, information about very low economic mobility should affect the formation of policy preferences. Yet some opinion scholars observe that the provision of information about inequality makes people more likely to evaluate inequality, but not necessarily more likely to support actual changes in redistributive policy (Kuzimienko et al 2015; McCall and Chin 2013). I suggest that this disconnect might stem from a failure of imagination; it is hard to connect the very abstract language of growing inequality with concrete personal experiences, especially when political actors do not supply relevant redistributive policy proposals. In contrast, information about economic opportunity has greater imaginative potential: it is easy to imagine (or remember) that you are a child born into a poor family, faced with limited resources, and restricted in your ability to improve your economic status relative to that of your parents. Indeed, prominent Republicans in 2015 expressed a willingness to "move away from Romney's more abstract message of job growth and to focus more specifically on social mobility and solutions for those at or near the bottom" (Rucker and Balz 2015). By providing individuals with

both a clear definition of economic mobility and linking the actual mobility rate to geographic entities, I illustrate economic information in a way that is both easy for individuals to understand and externally valid to contemporary political communications.

As in the previous chapter, the test I conduct is designed to capture the dynamic relationship between different kinds of information provision and subjects' information-conditional responses. The specific stimuli I use consist of different representations of information about economic mobility derived from estimates provided by Chetty et al. (2014). I first asked for all respondents to provide information about where they live. Then I randomly assign respondents to receive no information, information about national-level economic mobility, or both national and local information. I define mobility by providing the following text to the two treatment groups:

> Economic mobility is the chance someone who was born poor becomes rich later in life. We'll refer to people whose incomes are in the top fifth of all incomes as high-income and those whose incomes are in the bottom fifth as low-income. Across the United States, individuals born in low-income families have a 10% chance of entering the high-income group.

For those assigned to the national and local treatment condition, a final sentence appears, which states: "In [Respondent's location], people who were born in low-income families have a [#]% chance of becoming high-income earners," where the information in brackets is based on local estimates for each respondent. This information captures a basic tenet of the American dream: the idea that an individual can be born poor but achieve wealth through hard work. After exposure to this information, respondents were asked a series of questions about redistributive preferences, including a general question about government intervention in favor of mobility, a question about increasing the minimum wage, and two questions focused on job training and education spending.

When individuals encounter information about economic mobility at the national and local level, their response depends on the kind of information provided and their own interpretation of what that information means. Overall, economic mobility rates are lower than individuals might

expect: the average of all commuting zones is 10%, while local rates vary from 2% to 47%. The ten highest mobility CZ's are all located in North Dakota, South Dakota, and Montana. The ten lowest mobility CZ's are all located within Mississippi, Georgia, Tennessee, South Carolina, and South Dakota.

Most of the U.S. has generally low levels of intergenerational economic mobility, as defined above. I expect that the provision of this information will, on average, lead individuals to be more likely to support opportunity-increasing programs, because it suggests that only a small number of individuals are able to dramatically improve their earnings relative to their parents. However, the respondent's interpretation of the 10% national rate also depends on her local economic context, which individuals may be capable of discerning (Newman et al 2015). If an individual lives in an area with high levels of economic mobility, she might assume that the 10% rate represents an acceptable amount of mobility and resist increased government intervention. At the same time, someone who lives in a low mobility area might interpret the 10% rate as unacceptably low - given her awareness of local context - and thus support government intervention. Crucially, in both cases, I assume that individuals in the national information treatment interpret the national-level information as an accurate estimate for their own local context.

I also assign one group of individuals to receive both national and local information. I argue that this treatment is more psychologically provocative: by explicitly drawing a contrast between national-level mobility and the respondent's local area, I encourage respondents to make comparisons between their local area and the nation as a whole. I expect that those who are informed that their local area is much less mobile than average will feel both economically insecure and become unwilling to support intervention: self-undermining rhetoric should decrease policy support. People who are told that their areas are much less mobile that the country as a whole may support interventions that directly benefit their own less mobile area, but they will be less likely to

support government intervention on the whole, especially because I prime the costs of intervention when assessing redistributive preferences. However, an individual who is told that he lives in an area that is more mobile than the country as a whole should feel relatively more secure. For that individual, government intervention might appear more appealing: he has firsthand knowledge of a relatively mobile area and wants the rest of the country to also enjoy a relatively high mobility rate.

While some redistributive government programs enjoy broad public support (Piston 2014), I think that priming the cost of such programs allows for a more politically generalizable way to estimate opinion, as most political discussions of government programs involve some consideration of their cost. As a result, three of the four redistributive issues I focus on (intervention in general, job training spending, and education spending) are framed as potential policy changes that might result in increased taxes. While government could reallocate resources in order to achieve these policies without any additional taxation, priming taxes leads individuals to consider the potential costs of policy change. This form of rhetorical emphasis should help reveal the relationship between economic insecurity and policy support. By adopting this question wording, I also hope to avoid ceiling effects, in which individuals uniformly express support for increasing government intervention, job training spending, and education spending. The policies I select also include both direct and indirect kinds of redistributive policy.

My hypotheses are:

H1: Subjective perceptions of local economic context determine the meaning of national-level information about economic mobility: in low mobility areas, national-level information about economic mobility will increase support for mobility increasing policy. In high mobility areas, national-level information about economic mobility will decrease support for mobility increasing policy.

H2: Drawing contrasts between local and national mobility rates will encourage feelings of relative insecurity and opposition to intervention among those in less mobile places, but will encourage feelings of relative security and support for intervention among those in more mobile places.

To flesh out these hypotheses, imagine an individual who lives in a low mobility area (2%) and another who lives in a high mobility area (16%). Assuming both individuals live in the same place they grew up, the person who lives in the low mobility area likely knows few people who have earned more than their parents, while the individual in the high mobility area likely knows many. When the individual in the low mobility area is assigned no information about mobility rates, he will be somewhat resistant to support costly government interventions because he might feel relatively insecure and feel uncertain about policy efficacy. When the same person receives information about the national mobility rate (of 10%), he might be more satisfied with his area's mobility: if one in ten Americans is able to go from the bottom 20% of the income distribution to the top 20%, perhaps mobility is higher than he might have imagined. However, when the same individual is told that his area features much lower levels of economic mobility than the country as a whole, he might perceive his area to be quite immobile and less economically secure. Thus he will be more reluctant to endorse redistributive policies that are framed as costly, especially since those policies benefit everyone else, who already enjoy higher mobility rates. For an individual in a high mobility area, exposure to the national rate should have the opposite effect: while that person might think that mobility is high in her area, exposure to the relatively lower rate should decrease perceived security and decrease support for intervention. But when she is informed that her area is in fact much more mobile than the country as a whole, she might be more willing to support redistributive policies because the benefits outweigh the costs. When a person in a high mobility area gets no information, I expect that she will be modestly more supportive of intervention than someone in a low mobility area because she might perceive her local context to be relatively more mobile and seek the same for other areas.

As Figure 4.1 shows, exposure to information about economic mobility will have different effects according to both the kind of information presented and individuals' actual mobility context.

Those exposed to no information will be moderately more supportive of intervention when they live in more mobile areas, but the difference will be slight. Those in low mobility areas (the left side of the figure) will increase support for redistributive policy when told that the national average rate is 10% because they will identify with the national group and assume that the reported rate applies to them. If individuals in low mobility areas think that a 10% rate reflects their own experience of scarce opportunity, they will perceive a need for general government intervention: for that person, 10% means low mobility.

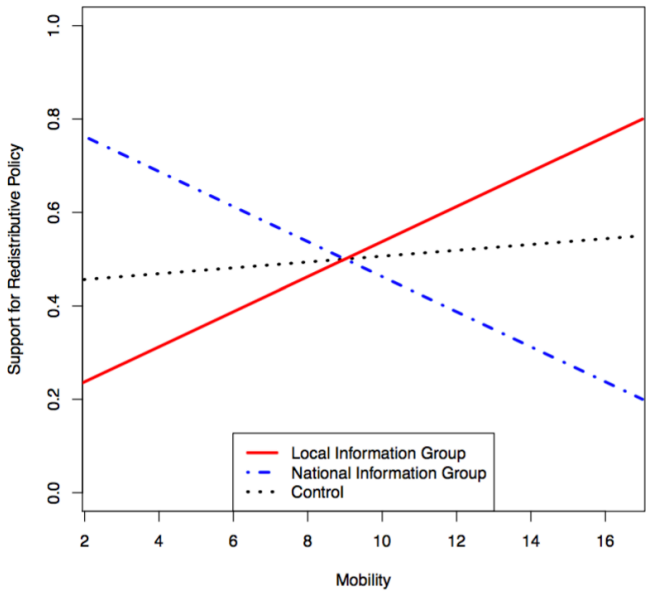


Figure 4.1: Theoretical Expectations

When those who live in places with low levels of mobility (the left side of the figure) are told that their own local areas have below average mobility, I expect that they will not support redistribution: the perceived costs of intervention, combined with feelings of economic insecurity (since they are explicitly told that their areas are less mobile that the country as a whole), should forestall support for policy change, especially since there is no guarantee that intervention will benefit their area in particular. For individuals whose communities enjoy higher than average mobility rates (the right side of the figure), the opposite pattern will occur: those who are told that they live in areas that are more mobile than the nation as a whole will feel relatively more

economically secure and support further intervention. Those on the right side of Figure 4.1 who are only informed that the national mobility rate is 10% will be more likely to oppose intervention, since their daily experience might suggest that a 10% rate is associated with an acceptable amount of opportunity.

Data and Analysis

From November 19-24, 2014, I contracted with Survey Sampling International (SSI) to draw a sample of 715 individuals, meant to be representative of the U.S. as a whole by gender, ethnicity, age, and education.[1] SSI uses quotas defined by Census population estimates to restrict participation to a demographically representative sample. 624 respondents completed the entire survey, but because the mobility items were among the first questions asked, my analysis sample includes between 644 and 647 respondents. This sample is more ideologically representative of the U.S. public than convenience samples of students or online labor market samples, but it is still less conservative and Republican than the U.S. public. The sample is also diverse in terms of actual mobility: its average mobility rate is 8%, with a minimum of 2% and a maximum of 18%. As described above, participants were randomly assigned to the three treatment conditions. Randomization is balanced across local mobility rates, demographics, and partisanship, except for the oldest age group, which was significantly more likely to be assigned to the local information treatment. As a result, I control for age in all empirical analyses.

As described above, the four key dependent variables in the analysis that follows are opinion questions that refer to the following: support for general government intervention to increase economic mobility, support for an increased minimum wage, support for job training programs, and support for increased education spending. I view the government intervention item as the most

[1] This experiment was conducted in the first module in a survey that also included the estate tax experiment in Chapter 3. See Appendix 4 for details.

important dependent variable because it directly corresponds with the informational treatments, while the other three policies are less clearly related to increased mobility. Three of four items include explicit references to policy costs, suggesting that the policy change might mean that taxes would be raised. I expect that this emphasis will also prime insecurity: by specifically mentioning the cost of intervention, those who feel that their local area is less economically secure will be more reluctant to express support for costly programs.

To assess treatment effects, I estimate four OLS regression equations that include interactions between the respondent's actual local mobility level and either the national information or the combination of national and local information. The results of these models appear in Table 1. I also control for a set of demographic characteristics, including age, education, race, gender, and number of children. The table shows how each of the informational treatments affects support for each of the four redistributive policy dependent variables.

	Govt. Intervention	Increase Minimum Wage	Increase Job Training	Increase Education Spending
National Info.	0.346	0.273	0.132	0.073
	(0.096)**	(0.099)**	(0.088)	(0.088)
National Info. × Mobility Rate	-0.037	-0.028	-0.014	-0.009
	(0.012)**	(0.011)*	(0.010)	(0.010)
Local Info.	-0.280	-0.101	-0.217	-0.157
	(0.086)**	(0.106)	(0.085)*	(0.090)^
Local Info. × Mobility Rate	0.035	0.015	0.029	0.024
	(0.010)**	(0.012)	(0.010)**	(0.010)*
Mobility	0.016	0.022	0.004	-0.001
	(0.010)	(0.006)**	(0.008)	(0.008)
R^2	0.12	0.11	0.07	0.12
N	658	656	658	659

OLS Regressions; standard errors in parentheses. Models include controls for poverty, percent white, percent high school graduate, and median household income at the county-level. Full results shown in the appendix; results robust to the exclusion of control variables. SE's clustered by treatment group. ** p<0.01, * p<0.05, ^ p<0.1

Table 4.1: Information Treatments' Effects on Redistributive Preferences

On the whole, exposure to the national information is associated with increased support for direct forms of redistribution (intervention and minimum wage increases), while exposure to the national and local information is associated with decreased support for both direct and indirect policies. The coefficient for the interaction between the national information and the respondent's mobility rate is consistently negative across the models and is statistically significant in two of four models, while the coefficient for the interaction between local information and the rate is consistently positive and significant in three of four models. Exposure to national information is associated with a positive coefficient for support for both government intervention and an increased minimum wage. But as the interaction between national information and mobility reveals, this effect diminishes dramatically for individuals who live in high mobility contexts.

Since the model contains two interaction terms, estimating predicted effects is the only way to understand the total effect of the treatments. For those exposed to national information, moving from the minimum to the maximum level of mobility in the sample is associated with a cumulative 0.54 point total decrease in support for intervention and a 0.43 point decrease in support for increasing the minimum wage. When individuals who live in high mobility areas are exposed to the 10% national mobility rate, they become much less supportive of redistributive policies. This pattern is consistent with hypothesis 1: individuals appear to interpret the meaning of the national rate in different ways, depending on their local context, and as a result, their opinions about necessary government responses also vary.

For those exposed to both national and localized information, the information coefficient is negative and statistically significant for government intervention and job spending. However, individuals in high mobility contexts (who were told their local rates) appear more supportive of redistribution. Moving from the lowest to the highest value of local mobility is associated with relatively increased policy support: 0.54 scale points for government intervention, 0.45 points for job

spending, and 0.35 points for education spending. Those who live in high mobility areas and who are informed of that fact, relative to the national average rate, become more supportive of intervention than those in low mobility areas. This result provides strong evidence in favor of hypotheses 2.

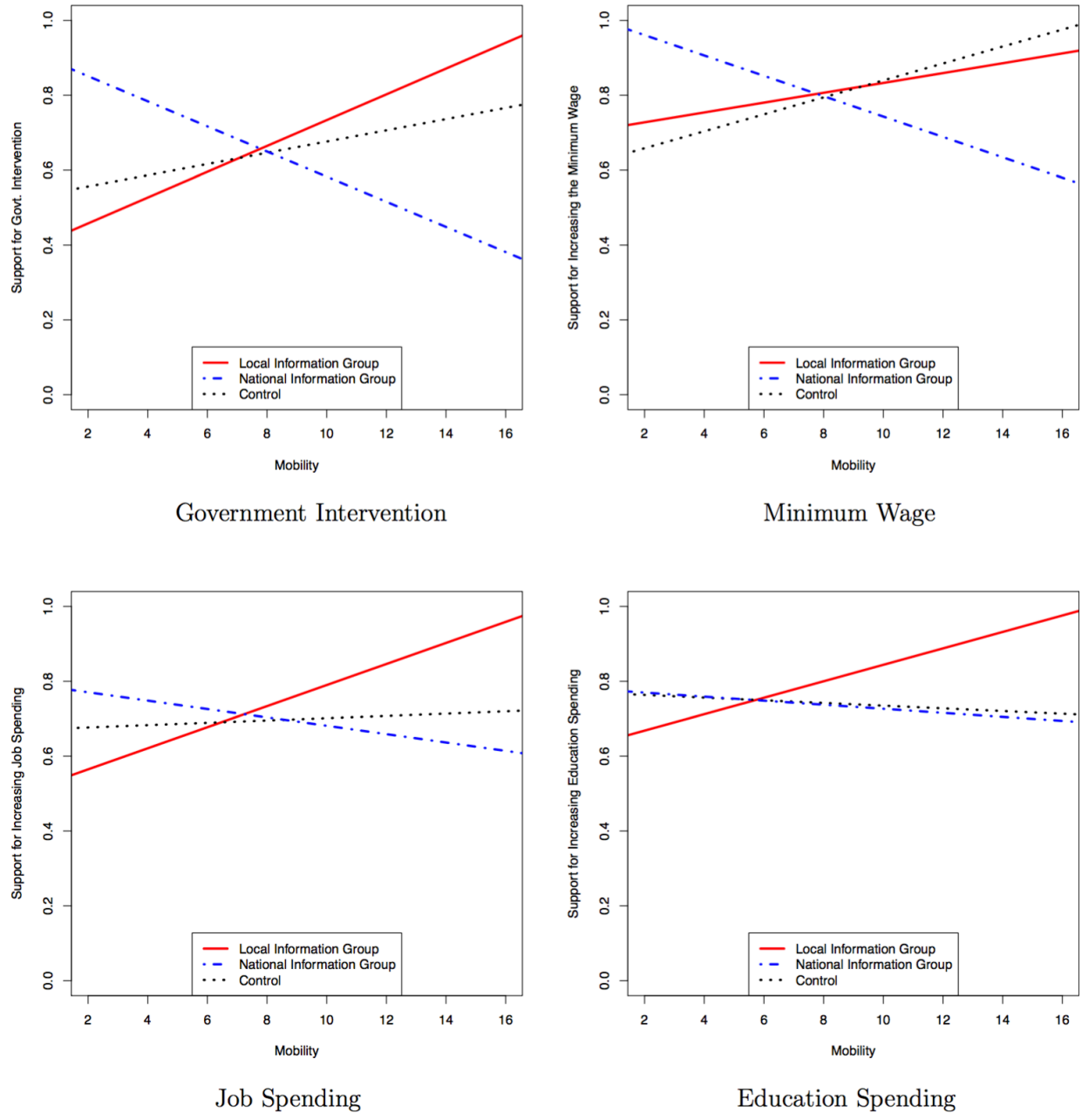


Figure 4.2: Predicted Probabilities

Figure 4.2 shows predicted values for each of the three information groups and each of the four models described in Table 1. The predictions are based on the median respondent in my sample: a white female, between 45 and 54 years old, with some college education and more than one child. The predicted values for government intervention most closely match the theoretical predictions shown in Figure 4.1: for those who live in low mobility areas, exposure to national information causes significantly more support for intervention, while those who also received local information became less supportive of intervention. In higher mobility areas, the opposite pattern occurs: those who were told that their areas were more mobile than average became significantly more supportive of intervention.

The pattern for those exposed to national-level information is strongest for the two most direct policy proposals: government intervention and minimum wage increases. For the more indirect policies, job training programs and education spending, the effect is statistically insignificant. However, the provision of local and national information has the opposite effect: those in high mobility places who learn that their areas are more mobile than the average become more supportive of policy change. This basic pattern is evident in three of the four graphs: for intervention in general, job spending, and education spending, the provision of local information is strongly associated with increased policy support (relative to the no information group), as long as the information confirms that an individual lives in a more mobile area than the country as a whole. At the same time, the provision of local information is associated with decreased support for policy change in low mobility areas. This consistent pattern suggests that relative economic mobility is a powerful determinant of redistributive policy support.

In order to more clearly identify the effect of local information, I also estimate similar regression models using just the subset of the sample that was exposed to either local or national information. The results are reported in Table 2. Here, I assess a more precise treatment effect: that

of the sentence: "In [Respondent's location], people who were born in low-income families have a [#]% chance of becoming high- income earners." As Table 2 shows, the specific effect of local information interacted with the mobility rate is statistically distinguishable from the baseline attitudes of those exposed to national information across three of the four policy areas.

	Govt. Intervention	Increase Minimum Wage	Increase Job Training	Increase Education Spending
Local Info.	-0.265	-0.082	-0.193	-0.154^
	(0.086)**	(0.104)	(0.085)*	(0.091)
Local Info. × Mobility Rate	0.032	0.013	0.026	0.022
	(0.010)**	(0.012)	(0.010)**	(0.010)*
Mobility	-0.019	-0.005	-0.007	-0.009
	(0.007)**	(0.009)	(0.007)	(0.007)
R^2	0.13	0.11	0.06	0.15
N	430	427	428	430

OLS Regressions; standard errors in parentheses. This model excludes all respondents in the no information condition; all respondents received information about the national mobility rate. Models include controls for poverty, percent white, percent high school graduate, and median household income at the county-level. Full results shown in the appendix. SE's clustered by treatment group. ** p<0.01, * p<0.05, ^ p<0.1

Table 4.2: Local Information Treatment's Effect on Redistributive Preferences

The provision of local information is associated with a large and significant decrease in support for government intervention and job spending. But that large effect is offset for those living in an area with relatively high local mobility: when an individual learns that he lives in an area that is much more mobile than the national average, he actually becomes more supportive of government intervention, job spending, and education spending than someone who is told that their local area is less mobile than the country as a whole. These results reveal the dramatic contextual effects of providing a local reference point to individuals when conveying economic information.

Discussion and Conclusion

As the analysis above suggests, information about economic mobility does shape redistributive preferences, but its effects are context dependent. While recent research on the

formation of economic preferences highlights the role of local economic context in structuring public opinion (Sønderskov et al 2016; Johnston and Newman 2016; Newman et al 2015), different messages about that context have the potential to dramatically shape how individuals connect their lived experience with government interventions. In this chapter, I demonstrate that context matters when national and local economic area definitions make it more salient. I find that national mobility information increases redistributive policy support in low mobility areas, while decreasing support in high mobility areas. I suggest that individuals' subjective interpretations of the national rate explain this difference. At the same time, the additional provision of localized information about economic mobility leads individuals in low mobility contexts to express less support redistributive policies, while the same form of information increases redistributive policy support among those who live in high mobility areas. By drawing a contrast between local areas and the nation as a whole, I prime the costs of intervention for those in low mobility areas and the benefits of increased opportunity for those in high mobility areas.

As a whole, this study explores the contextual construction of political opinion, as defined in chapter 2: I focus on the ways that preferences change in light of both local environmental factors and randomly assigned political stimuli. This method of assessing opinions is both externally valid to actual political and media messages and explicit about the endogeneity of expressed opinions. By showing how individuals react after exposure to information at different geographic levels, this study expands on research that how group identities shape sociotropic economic evaluations (Mutz and Mondak 1997; Ansolabehere et al 2014). I show that area definitions might serve as reference points in the same way as information about the fortunes of particular groups of people. My novel experimental design allows me to directly compare the effects of national and local geographic group definitions for a diverse national sample. A research design that does not randomly assign the salience of these identities would obscure the relationship of interest. And indeed, the

counterintuitive findings reported here reveal the importance of geographic specificity when providing representations of economic reality to the public.

America is unequal on average and inequality is unequally distributed across the country. As a result, political campaigns focused on opportunity should be strategic about the kinds of messages that they send. Appeals that highlight the relative lack of mobility for some areas risk alienating potential supporters by providing what Levine (2015) calls self-undermining rhetoric. I provide evidence that those who live in areas with low mobility rates will be less inclined to favor government intervention, unless they are provided with less precise (national) information that makes their local area appear to be more mobile than it actually is. At the same time, local appeals directed toward those who live in very mobile areas might increase support for intervention, as long as those individuals are also provided with a less mobile reference point. Indeed, the results suggest that those who are already economically mobile are also most likely to support redistributive policy changes in the face of information that reaffirms their relative advantages. This finding shows that information about mobility shapes both economic outcomes and also public opinion in predictable ways, and that political leaders and campaigns face a strategic challenge when attempting to politicize the issue. As campaigns and the media increasingly focus on inequality, those members of the public who are most negatively affected by a lack of opportunity might not seek political responses to the problem. This result is consistent with recent research on the political challenges of talking about redistribution, and might explain why so few political campaigns do so.

New lines of research on economic mobility and public opinion remain open. My definition of economic mobility (and its operationalization within the study) is very narrow: it is possible that a broader account of economic and social mobility would provide a more nuanced account of how information about mobility affects opinion. Egan (2014) shows that the material effects of economic mobility - not just information about it - are profound and wide-ranging. I also omit consideration

of more explicitly behavioral consequences of information about mobility, including political activism, voter turnout, and other forms of political activity beyond opinion formation. Finally, the temporal scope of this study is limited: it may be the case that the treatment effects observed here decay quickly, especially as individuals encounter new information about their local economy.

In the next chapter, I examine a more externally valid measure of economic political communication: campaigns' cumulative focus on labor market policy. As the results reported in this chapter suggest, the same economic issue representations mean different things in different places. Building on that result, I show that market-based policy proposals for increasing employment discourage voter mobilization in places with weak local labor markets, while increasing turnout in places with strong labor markets. As long as the supply of economic political information focuses primarily on indirect policy initiatives, individuals' relative perceptions of economic reality will remain a central determinant of policy support.

5 Politicizing Unemployment

Abstract

Campaign advertising politicizes issues, representing economic and social problems as reasons to engage in political action. But efforts to politicize economic issues are conditional on economic reality, which shapes voter perceptions of proposed policy interventions. I use observational data to examine the relationship between job-focused political advertising, local labor market conditions, and voter turnout. Since campaigns cannot control which counties within a media market view economic issue-focused ads, individuals in very different economic contexts are exposed to the same level of aggregate issue-focused advertising. Using both difference-in-difference and cross-sectional analyses, I show that when campaigns broadcast more job-focused ads, voter turnout increases in areas with low levels of unemployment and decreases in areas with high levels of unemployment. These results suggest that issue-focused ads are an imperfect means of encouraging political participation: individuals who might benefit most from policy interventions are demobilized by ads that focus on those policies.

"When the United States was made up of small social units with common traditions and a small geographic and social area, it was comparatively simple for the proponent of a point of view to address his public directly… Today the greatest superman as yet developed by humanity could not accomplish the same result." – Edward Bernays (1923, 135).

Elections provide individuals with a means of holding officials accountable for the state of the economy, but voters do not necessarily perceive a connection between political participation and economic outcomes. Campaigns and political advertisements seek to make this connection explicit by directly linking the decision to vote with promises of improved economic outcomes (Hart 2013; Sides and Karch 2008; Sides 2006). Both incumbents and challengers seek to politicize economic issues in order to persuade and mobilize supporters, suggesting that voting will directly improve the state of the economy (Jones 2015). However, the persuasiveness of issue-focused advertisements depends upon fixed characteristics of their audience. I argue that individuals' preexisting perceptions of economic reality affect the credibility of issue-focused political advertisements. When campaign ads increase the salience of indirect redistributive policies in areas experiencing economic stagnation, they risk discouraging political participation.

Campaign advertisements that focus on jobs politicize the issue: they suggest that politicians can create or protect jobs, and that voting is a means of improving local labor markets. But campaigns broadcast the same job-focused ads in both strong and weak local labor markets simultaneously. My central claim is that labor market context moderates the effects of job-focused political advertising. Raising the salience of jobs in stronger labor markets areas should mobilize voters by providing them with a plausible narrative of improved economic outcomes; one that comports with their lived experience. Ads that politicize jobs in these contexts provide a credible reason to vote. But talking about the labor market in high unemployment contexts remind voters of local economic stagnation, their own economic insecurity (Levine 2015), and failed political interventions. As a result, I expect that job-focused ads will demobilize voters in high unemployment contexts. I test these claims using both cross-sectional and difference-in-difference

regression models that estimate the intent to treat effect of job-focused ads at the county level. My empirical strategy provides a model for examining similar contextual effects across a range of different political issues.

Two structural factors limit the ability of any one candidate to control the issue agenda in an election. First, campaigns broadcast appeals at the media market (also called Designated Market Areas, or DMA) level. Media markets are geographic units comprised of many different counties with varying economic conditions. As a result, individual candidates can not strategically send distinct televised appeals to counties located in the same media market, even if those areas have very different labor markets. At the same time, candidates who strategically focus on issues at the media market-level have little control over the issue content of advertisements aired for other races in the same media markets. These constraints introduce plausibly exogenous variation in the content of political advertisements: while a strategic incumbent candidate might want to focus on the issue of jobs in areas with strong job growth, the same ads will be seen by viewers in weaker labor markets. In the analysis that follows, I examine the effects of the aggregate campaign issue-environment across county-level economic context, as measured by the unemployment rate, on voter turnout. The results of my analysis indicate that the issues raised in ads have substantial effects on voter turnout, but that those effects are contingent upon local labor market context.

Campaign Ads and Economic Concerns

In the four years after the Great Recession of 2008, economic issues became increasingly prominent in political advertisements. From 2004 to 2012, the proportion of all federal and gubernatorial advertisements that discussed the economy increased from 51% to 72% (Jones 2015; Fowler, Franz, and Ridout 2015). Campaigns purchase political advertising because ads target a broad range of potential supporters, providing information and attempting to mobilize individuals to engage in political action (Freedman, Franz, and Goldstein 2004; Hart 2013). Existing empirical

research investigates whether a particular campaign's advertising is effective at mobilizing voters or persuading them to choose a candidate,[2] but little research examines advertising's mobilizing effects within the context of other races and in the context of local, environmental circumstances. Sides and Karch (2008) and Claibourn and Martin (2012) consider whether the issues raised in ads might have heterogeneous effects across different individuals, but no existing work examines how local economic, political, and social context interacts with issue ads.

Advertising focused on the economy has the potential to directly shape voters' perceptions of the economy as a political issue. A long line of scholarship suggests that economic evaluations are central to electoral behavior (Key 1966; Fiorina 1978; Mackuen et al 1992; Nadeau and Lewis Beck 2001; Hibbs 2006). However, there is little consensus about the effects of local economic context on individuals' propensity to vote. Some studies suggest that worsening economic conditions lead individuals to withdraw from politics, due to the costs of political participation and the salience of economic insecurity (Lacy and Grant 1999; Rosenstone and Hansen 1993; Radcliff 1992; Rosenstone 1982: Sniderman and Brody 1977). Others propose that voters are more acutely aware of poor economic performance, and as a result, more willing to turnout and punish incumbents (Incantalupo 2015; Burden and Wichowsky 2014; Gomez and Hansford 2010; Arceneaux 2003). However, both sides of this debate ignore the effects of economic issue-focused political communications, which explicitly link economic circumstances with political action.

Advertisements seek to directly shape individuals' perceptions of economic reality: they describe the economy as either good or bad, and then suggest that political action is necessary to maintain or change the status quo (Jones 2015). But most individuals do not seek out information about the economy on their own (Ansolabehere et al. 2014; Popkin 1991). As a result, economic

[2] Among many others, see Goldstein and Freedman 2002, Huber and Arceneaux 2007, Gerber et al 2011, Krasno and Green 2008, Shaw 1999, and Krupnikov 2014 for examples of observational empirical tests of political advertising's effects.

issue-focused political advertisements raise the political salience of preexisting economic perceptions. By ignoring campaign advertisements, a central communicative mechanism that suggests how voting might improve the economic status quo, existing accounts of economic context's effects on turnout offer only an incomplete understanding of how and when the economy matters. My results clarify this debate by directly measuring the economic issue content of political appeals, revealing how communications can either mobilize or demobilize those who are dissatisfied with local economic conditions.

From 2008-2012, the majority of Americans expressed more concern about the economy than any other aspect of political and social life, while the public became more likely to describe unemployment as the most important problem facing America than any other issue (Gallup 2015).[3] Over the same time period, political advertisements increasingly focused on jobs: the percent of all congressional and gubernatorial ads mentioning the issue rose from 27% in 2008 to 41% in 2012.

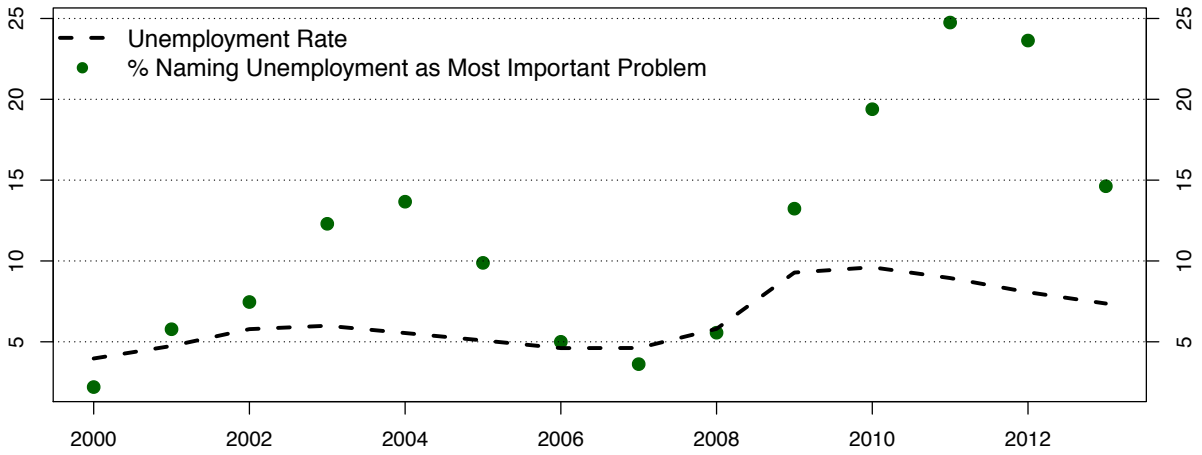


Figure 5.1: Concern about Unemployment and the Unemployment Rate 2000-2013

I argue that political advertisements that raise the salience of jobs should be associated with increased turnout because they explicitly link political participation with solving the problem of unemployment. Unlike economic political communication from mass media sources (Mutz 1992),

[3] Note that increased concern for unemployment does not indicate broad support for redistributive interventions against it (Pew Research Center 2014).

political ads directly discuss the importance of voting as a personal response to economic trends. However, I expect that efforts to politicize jobs will be conditional on the local economic context that individuals experience in their day-to-day lives. When candidates promise to create new jobs, those in strong labor markets will be most likely to perceive such promises as plausible and worth supporting. But in areas with high levels of unemployment, raising the political salience of jobs could demobilize individuals: those appeals also raise the salience of worsening economic circumstances and highlight the costs of political action, ultimately undermining it (Levine 2015). Promises of thousands of new jobs might ring hollow in places that experience long term unemployment, especially when those ads express enthusiasm and pride for labor markets that are in fact failing. When individuals are reminded of their own precarious economic circumstances and the reality of economic stagnation, they become less willing to engage in collective action focused on solutions. As a result, I expect that ads that raise the political salience of employment will only increase turnout in areas that already enjoy low levels of unemployment.

Theory: Context Shapes Political Communication

In general terms, political advertising attempts to influence political action by making issues and considerations more salient to voters, and then proposing political action as a necessary response to those issues (Sides and Karch 2008). Since campaigns usually focus on the same issues across different forms of political communication (Huber and Arceneaux 2007), I view the issue topics raised in advertisements as a proxy measure of campaigns' overall issue-focus. In this case, economic issue-focused advertisements provide a narrative that connects the decision to vote with the prospect of altered economic outcomes (Jones 2015; Hart 2013). Job-focused ads either suggest that unemployment has been rising, and so a change is needed, or that job growth is strong, and thus voters should turn out in support of the incumbent. In both cases, ads politicize unemployment: they suggest that political actors can address the issue, and thus that political action (voting) is

necessary. However, it could also be the case that within these issue-focused ads, "there is no hook, no story, no narrative that connects people's insecurity with a realistic sense of political change," especially in areas with high levels of unemployment (Levine 2015, 13). When individuals live in contexts of economic stagnation, raising the political salience of jobs might result in demobilization: those individuals might be less optimistic about the prospects of political interventions given their longstanding, personal experience with failed policy and a depressed labor market.

Individuals have preexisting perceptions of their local economic context, including local unemployment rates (Newman et al. 2015, 128-129; Sønderskov et al 2016). Political advertisements focused on the economy attempt to politicize those perceptions by depicting elected officials as capable of shaping the economy for the better. Issue-focused ads raise the salience of particular issues, increasing the cognitive accessibility of issue-relevant perceptions. But ads that promise political solutions to economic problems suggest that voters ought to perceive the economy in a particular way: as a political issue that elected officials can control and improve. When ads convey a world of successful policy interventions and competent candidates, those ads may be sharply at odds with individuals' own perceptions of economic and political reality. While those who live in strong economic contexts might not perceive any disconnect between optimistic issue-focused advertisements and their own perceptions of the local economy, those who live in stagnant economic contexts will be less likely to be persuaded.

At the same time, unemployment rates have a strong negative correlation with median income (-0.23 in 2010, -0.29 in 2012), which is a powerful predictor of political participation (Leighley and Nagler 2014; Brady, Verba, and Schlozman 1995). Individuals who live in strong labor markets are more likely to have the resources to engage in political action. Thus job-focused ads should be increasingly effective at mobilizing voters living in strong labor markets, who possess an instrumental motivation to maintain a strong labor market and may also possess ample resources

necessary to vote. As a result, an increased focus on economic issues in strong economic contexts should increase voter mobilization.

By contrast, when political advertisements promise jobs in areas where unemployment is high, I expect to observe a demobilizing effect. In high unemployment contexts, the political narrative advanced by job-focused ads is less credible; since past policy interventions have failed to improve the local labor market, there is little reason for prospective voters to invest resources in participation. It is possible that, as Burden and Wichowsky (2014) suggest, potential voters may be especially attentive to bad economic news and more willing to vote in order to punish incumbents. But political ads that focus on jobs lack credibility: the disconnect between the promises in such ads and voters' perceptions of economic reality make it more likely that individuals perceive political action to be futile. Candidates' labor market promises may be especially likely to ring hollow in places that feature high levels of structural unemployment, and individuals living in weak labor markets might also lack participatory resources. Ads that implicitly highlight a local lack of jobs might further demobilize voters. These lines of argument lead to my first hypothesis:

H1: Job-focused political ads will mobilize voters in areas with low unemployment and demobilize voters in areas with high unemployment.

The discussion so far ignores the emotional content of political advertising, focusing instead on the informational content of issue mentions. But emotion is a central communicative strategy in political advertising (Brader 2005), and the theory of contextual effects described above implies that affective content might also not resonate with voters as intended. I expect that ads which rely on emotional cues when politicizing unemployment will be more effective at mobilizing voters in areas with low levels of unemployment: such ads provide a more affective and potentially persuasive representation of political participation's role in maintaining strong labor markets, while also reminding voters of favorable local economic conditions. Experimental research has found that exposure to enthusiastic ads increase self-reported vote intention by up to ten percentage points

(Brader 2005, 91). And indeed, job-focused ads in my sample frequently include enthusiastic content. For example, an ad broadcast in 2010 by U.S. House candidate David Wu (Oregon) depicts the candidate visiting technology factories and riding in a go-cart while upbeat music plays in the background. The candidate says that one of the best parts of his job is "helping entrepreneurs start high tech businesses and create jobs in Oregon," encouraging voters to reelect Wu in order to enjoy a continued record of job creation. For voters that perceive the economy to be performing well, Wu's ad should increase interest in the election campaign and provide stronger encouragement to vote (Brader 2005, 70). But this enthusiastic ad was also broadcast in counties with unemployment rates almost twice the national average. For individuals living in areas with a 15% unemployment rate, enthusiastic messages amplify the disconnect between subjective perceptions of the local labor market and the vision articulated in political advertisements. When ads are enthusiastic about jobs in ways that are contrary to economic reality, individuals who live in areas with high levels of unemployment will be even less likely to perceive those positive appeals as credible, and thus less motivated to engage in political action.[4]

H2: Enthusiastic job-focused political ads will increase the magnitude of contextual effects.

Finally, I conduct a placebo test in order to evaluate whether political advertisements that focus on issues other than jobs are as likely as job-focused to influence voter turnout across unemployment contexts. If my account of contextual effects is correct, ads that do not focus on jobs should not shape turnout conditional on labor market context. To conduct the test, I examine the effects of campaign finance-focused advertisements in areas with high and low levels of unemployment.

[4] Both enthusiastic and fearful political appeals are likely to encourage voter mobilization, but fearful appeals can sometimes encourage withdrawal from politics (Brader 2005, 70). Because the effects of fearful ads depend on individual-level characteristics of viewers, which are unobserved in my analysis, I exclude them from the primary analysis. In Appendix A3 I show that fearful job-focused ads have no separate, statistically significant effect on turnout across unemployment contexts.

H3: Campaign finance-focused political ads will not systematically mobilize or demobilize voters across unemployment contexts.

Research Design and Data

I conduct an observational analysis of the relationship between advertising, local economic context, and voter turnout in order to assess the effects of ads across large numbers of distinct races and in heterogeneous economic contexts. This choice maximizes external validity: the effects I observe have measureable, real world political consequences and generalize across the U.S. While some observational studies of advertising effects use individual-level data to understand turnout (Sides and Karch 2008; Claibourn and Martin 2012; Freedman, Franz, and Goldstein 2004), I estimate county-level intent-to-treat effects instead of predicted turnout probabilities for individuals. Since it is difficult to accurately gauge individual-level advertising exposure (Arceneaux 2010) and individuals may be likely to over-report socially desirable behavior, including voting, I reduce measurement error by using actual broadcast data and actual election returns at the county level. My measure of job-focus in the electoral advertising environment includes the entire universe of congressional and gubernatorial ads, broadcast across all television networks in the six months before each election.

I use two sources of plausibly exogenous variation to help identify the effects of ads. First, I aggregate all political advertising broadcast across contemporaneous Congressional and Gubernatorial campaigns to measure the total number of ads that discuss jobs in a given media market. Campaigns might strategically choose to focus on issues when they expect that those issues will help persuade potential supporters (Arceneaux 2010). This results in selection biases, where individuals living in areas featuring worsening economic conditions could be systematically more (or less) likely to view economic appeals. However, while candidates control the content of their own advertising, they have less agency when it comes to the issues raised by opponents or in races further up and down the ballot. Indeed, it is possible that a gubernatorial candidate, who broadcasts

thousands of ads that raise the salience of jobs to mobilize voters around his job-creation record, might find his message lost in a sea of ads from a gubernatorial challenger who ignores jobs completely or Congressional candidates that raise the salience of national security or other unrelated issues. As a result, analyzing the broader marketplace of advertising in the aggregate helps avoid the selection problems inherent to empirical strategies focused on single races, while also capturing the more comprehensive set of issue mentions that potential voters are actually exposed to. Thus my explanatory variable of interest is the electoral issue environment: the total number of broadcast advertisements that focus on the labor market across all campaigns.

Second, I take advantage of the fact that all ads are purchased and broadcast at the media market level, an aggregation of counties that includes substantial heterogeneity in underlying economic conditions. The average media market contains just over 25 counties (with a standard deviation of 16), while the number of counties in a media market ranges from 1 to 65. When a campaign chooses to take credit for decreasing unemployment in an ad, the counties that get the message differ dramatically in actual unemployment. In 2010, the average unemployment rate across media markets is 9.37% and the average standard deviation of that rate (within markets) is 2.54. So, for example, county level-unemployment in the San Antonio, Texas media market ranges from 4.3% in McMullen County to 15.2% in Maverick County, but both counties experience the same volume of job -focused political advertising.

To test the effects of job mentions in ads on voter turnout, I conduct both a cross-sectional analysis (using data from 2010 and 2012) and a difference-in-difference analysis (using data from 2012 and 2008.)[5] Since individual campaigns might strategically choose to highlight jobs where they expect the greatest payoff in terms of increased voter turnout, the relationship between job-

[5] Since comparable advertising data is not available for the 2006 midterm election, I am unable to use a difference-in-difference design for 2010.

mentions and turnout is endogenous within each media market-race. However, by examining turnout within media markets and across campaigns and races, I examine the cumulative effects of issue mentions across counties exposed to ads broadcast by multiple strategic actors who may or may not actually seek to increase overall turnout (Citrin et al 2003). In the cross-sectional analysis for 2010, I control for turnout in the 2006 midterm election. I also control for poverty, racial demographics, education, and household income, since those characteristics of counties are likely related to voter turnout separately from the effects of ads and local unemployment (Leighley and Nagler 2014).

For 2012, I adopt a difference-in-difference empirical strategy, using the 2008 Presidential election as a baseline in order to cleanly estimate the causal effect of additional job-focused ads. This design controls for any time-invariant unobserved heterogeneity in underlying political and social characteristics at the county level that could be potentially correlated with voter turnout, including the demographic attributes of counties that I control for in the cross-sectional model. For example, if some unobserved factor makes voters in a given county very likely to turnout regardless of unemployment rate or number of ads focused on jobs, taking the difference between two years helps to ensure that I do not detect a spurious relationship between either unemployment rate or issue salience and observed turnout. Similarly, some counties could always be exposed to large numbers of jobs ads, or have relatively constant high or low unemployment rates. By taking the difference between observations of the same county across two elections, I can more cleanly attribute the difference in turnout between 2008 and 2012 to changes in the key explanatory variables of interest. For this model specification, I control for differences in each of the same control variables as in the cross-sectional analysis.

I use an observational empirical strategy rather than a lab experiment in order to assess the cumulative effects of issue-focused campaigns. While experimental research on political advertising

provides internally valid estimates of ads' political consequences (Ansolabehere et al. 1994; Valentino, Hutchings, and Williams 2004; Brader 2005), only observational data allow scholars to test the effects of cumulative advertising issue mentions and local economic context. There is no way to randomly assign subjects to experience hundreds of ads over time in a way that parallels their exposure during actual, contemporaneous campaigns. Likewise, economic context, including the economic insecurity that permeates areas with very high unemployment rates, is impossible to replicate in the lab. Even field experiments in political advertising (Gerber et al 2011) do not involve the random assignment of issue mentions topics across multiple, contemporaneous campaigns. Researchers also cannot randomly assign economic context, such as local labor market prosperity. As a result, the observational approach I adopt here provides the best possible test of the theory outlined above. However, future research should use experiments in order to tease apart the individual-level psychological mechanisms that explain the aggregate patterns I reveal.

To conduct my analysis, I obtained political advertising data for 2008 – 2012 from the Wisconsin Advertising Project (WiscAds) and the Wesleyan Media Project (Fowler, Franz, and Ridout 2014; 2015). These data include broadcast advertisements at the congressional and gubernatorial level for all 210 media markets in the U.S. My analysis includes all ads aired after June 1 for each year: including a large number of ads in my sample increases the reliability of my measure of campaign issue-focus. The advertising data include an indicator variable that takes the value 1 if an ad includes a direct reference to employment or jobs. I sum the total number of individual airings of all job-focused ads broadcast in each media market and election season as well as the total number of political ads broadcast in each media market and year, following the approach adopted by Sides and Karch (2008). This method of operationalizing my key explanatory variable provides a measure of issue exposure that is directly comparable across states: it preserves differences in total advertising due to the incidence of contemporaneous gubernatorial and senatorial races in some

states.[6] In my analysis, I include model specifications that control for the total number of ads broadcast in order to capture the competitiveness of campaigns, and to help control for potentially diminishing effects of additional ads in areas with a high volume of political advertising. Table 5.1 provides information about the total number of ads in my sample:

		Mean	StDev	Min	Max	N
2010	Jobs Ads (1,000s)	5.66	4.46	0.00	26.91	3,103
	All Ads (1,000s)	12.89	9.28	0.00	51.48	3,103
	Enthusiastic Ads (1,000s)	2.60	2.08	0.00	9.72	3,103
2012	Jobs Ads (1,000s)	3.02	2.89	0.00	12.33	3,104
	All Ads (1,000s)	8.72	8.20	0.00	42.42	3,104
	Enthusiastic Ads (1,000s)	1.09	1.27	0.00	6.39	3,104

Table 5.1: Summary Statistics, Total Number of Ads (Across Counties)

The average number of gubernatorial and congressional jobs ads broadcast in each county is lower in 2012 than in 2010; part of this difference is due to the fact that congressional and gubernatorial candidates air less ads in Presidential election years, and part is due to the fact that states that do not hold Senate and Gubernatorial campaigns in presidential election years decrease the county-level average.[7] Within this sample of ads, I identify positive job-focused ads by summing all airings of ads that are coded as somewhat or strongly enthusiastic by the Wesleyan Media Project. These characteristics of ads are only coded in the 2010 and 2012 data, so I cannot test hypothesis 2 using the difference-in-difference specification. Since my sample of all advertisements includes only gubernatorial and congressional ads, I do not include presidential campaigns' job-focused ads in the

[6] An alternative measure of job issue focus that involves dividing the total number of jobs ads by all political ads would obscure that difference, and ignore the added effects of contemporaneous races.
[7] In 2010, congressional and gubernatorial candidates aired 2,858,442 total ads. In 2012, the total is 2,062,922.

2012 analyses.[8] I include state fixed effects in all model specifications to help account for additional presidential campaign ads that might be broadcast in battleground states in 2012.

I obtained county-level unemployment data from the Bureau of Labor Statistics, poverty data from the U.S. Census Small Area Income and Poverty Estimates website, and voting age population (VAP) estimates from the U.S. Census Redistricting Data website. I obtained education and median income data from the Census American Community Survey. Finally, I use county-level election returns from David Leip's Election Atlas to create county-level VAP turnout measures. Figure 5.2 shows the densities of county-level turnout measures across the three years in my sample.

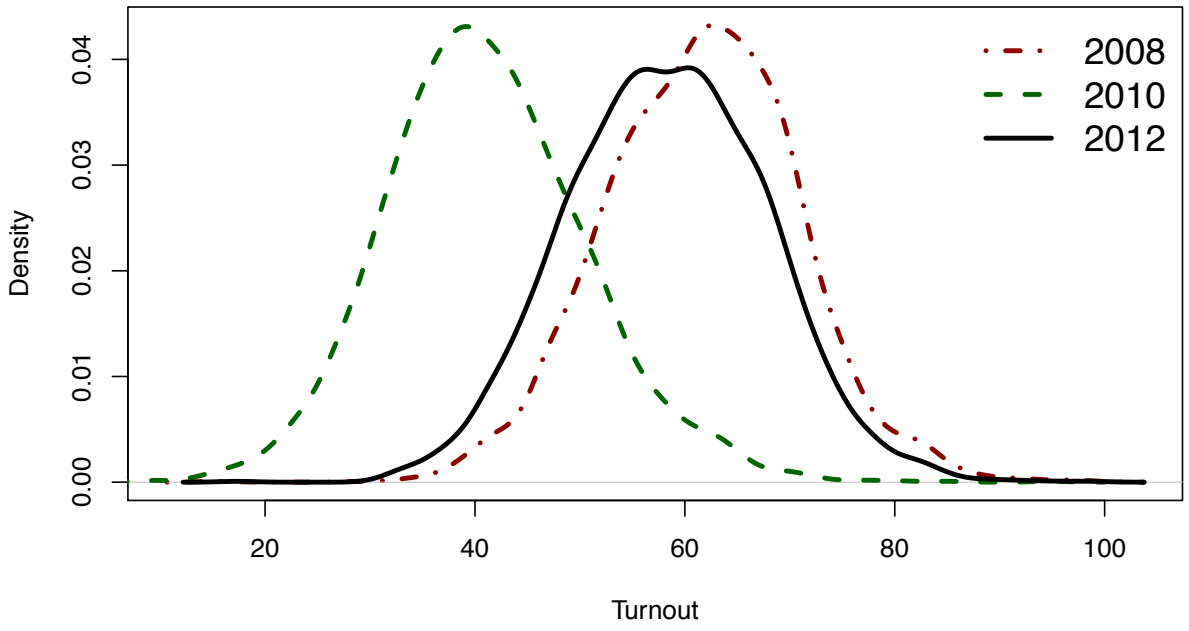


Figure 5.2: Voter Turnout Densities, by Year

As the figure shows, turnout was much higher in both presidential elections than in the midterm election. As a result, I analyze the effects of job mentions in ads for presidential and midterm elections separately. Turnout also declined substantially between 2008 and 2012; the difference in

[8] While presidential ad data are available for 2008, the 2012 data are embargoed against public release until November 2016. I plan to expand the sample of ads used in this analysis when 2012 data become available.

VAP turnout (at the county-average level) is 3.4 percentage points. This difference is almost identical to the difference observed when estimating turnout at the state level with the voting eligible population instead of the voting age population (McDonald 2015). Summary statistics for all other variables are reported in Appendix 5A2.

Analysis and Results

I begin my analysis with a cross-sectional analysis of the relationship between job-focused congressional and gubernatorial advertising and voter turnout in the 2010 midterm election and the 2012 presidential election. I estimate the effects of job-focused ads broadcast in each media market *d* for each county *c*:

$$\begin{aligned}\text{Turnout}_c = {} & \beta_0 + \beta_1 \text{Jobs Ads}_d \times \text{Unemployment}_c + \beta_2 \text{Jobs Ads}_d + \beta_3 \text{Unemployment}_c + \\ & \beta_4 \text{Total Ads}_d + \beta_5 \text{Percent Poverty}_c + \beta_6 \text{Percent White}_c + \beta_7 \text{Percent H.S. Grad}_c + \\ & \beta_8 \text{Log(Median Household Income)} + \beta_9 \text{LagTurnout}_c + \text{State}_c + \varepsilon_c\end{aligned}$$

In the equation above, Turnout_c is the percent of the voting age population who voted for each county *c*. At the media market (*d*) level, Jobs Ads_d is the total number of all congressional and gubernatorial political advertisements that focused on jobs, while Total Ads_d is the total number of all congressional and gubernatorial political advertisements that aired in a given media market. At the county (*c*) level, Unemployment_c is the average unemployment rate, Percent Poverty_c is the percent of the population living in poverty, Percent White_c is the percent of a county's population who are white, $\text{Percent H.S. Grad}_c$ is the percent of high school graduates in each county, income is operationalized as the log of Median Household Income, TurnoutLag_c is turnout from the 2006 midterm election and State_c is a vector of state level fixed effects.

Based on the hypotheses outlined above, I expect that the coefficient for Jobs Ads_d will be positive and significant; this would suggest that a greater focus on jobs increases voter turnout. The interaction between Unemployment_c and Jobs Ads_d should be negative, indicating that when the unemployment rate in a county is high, an increased focus on jobs in that county's media market will

have a demobilizing effect on voter turnout. I am agnostic about the effect of Unemployment_c separately from its interactive effect with job advertisements, since there is both evidence that unemployment drives turnout by encouraging voters to punish incumbents (Burden and Wichowsky 2014) and evidence that it increases that chance that individuals will withdraw from politics (Rosenstone 1982). My theory focuses on the political salience of unemployment, rather than unemployment per se.

The model above assumes that there is no unobserved determinant of turnout that is part of the error term which is also correlated with the unemployment rate. For example, it could be the case that low unemployment rates are correlated with migration patterns, which might increase the number of individuals moving to areas with low unemployment. These individuals might be less likely to register to vote in their new locations. I lack county-level data that might address this concern. However, I do control for percent poverty, which is highly correlated with unemployment, percent white, which is also (negatively) correlated with unemployment and plausibly related to turnout, median income and education. The results appear in Table 5.2.

As expected, the number of job-focused ads broadcast in each county is a strong and significant predictor of increased turnout in both models, though the coefficient for Jobs Ads_d is larger in 2010 than 2012. The interaction coefficients for Jobs Ads_d (1000s) × Unemployment_c are very similar across both years and provide support for hypotheses 1: both are negative and statistically significant. This suggests that as the unemployment rate increases to its highest values, political communications that focus more on jobs have a negative effect on overall turnout, while the opposite pattern occurs in low unemployment areas. In 2010, the predicted turnout rate for the average county in the bottom tenth percentile of the unemployment rate that experiences one standard deviation more than the average number of jobs ads is 42.1%, plus or minus 0.5%. If that same county was exposed to one standard deviation less job-focused ads, predicted turnout is

38.3%, plus or minus 0.5%. This is a dramatic difference, much larger than those observed in field experimental get out the vote interventions (Rooij, Green, and Gerber 2009). In high unemployment contexts, jobs-focused ads do not have the same mobilizing effect. There is no statistically significant difference in predicted turnout for counties in the top tenth percentile of unemployment exposed to relatively more or less jobs ads.

Dependent Variable: Voter Turnout	2010	2012
Jobs Ads_d (1000s) × $Unemployment_c$	-0.039**	-0.035**
	(0.009)	(0.010)
Jobs Ads_d (1000s)	0.642**	0.330**
	(0.134)	(0.104)
$Unemployment_c$	0.377**	0.318**
	(0.076)	(0.046)
Total Ads_d (1000s)	-0.082^	-0.010
	(0.047)	(0.026)
Lagged $Turnout_c$	0.592**	0.830**
	(0.015)	(0.009)
N	3,103	3,104
R^2	0.443	0.785
State Fixed Effects	√	√

Standard errors in parentheses. Models include controls for poverty, percent white, percent high school graduate, and median household income at the county-level. Full results shown in appendix A1. The estimates of interest are robust to the inclusion of media-market clustered standard errors, excluding all counties on the borders of media markets, and using a logged measure of ads (see Appendix A4 – A6). ** p<0.01, * p<0.05, ^ p<0.1

Table 5.2: Effects of Job-Focused Ads Across Unemployment Contexts, 2010 and 2012

For 2012, I observe a demobilizing effect in high unemployment contexts (top 10^{th} percentile) of about 0.5 percentage points. At the same time, the coefficient on unemployment rate is positive and significant for both years, consistent with Burden and Wichowsky's (2014) account of unemployment as a mobilizing force. When voters in weak labor markets see very few job-focused ads, they are much more likely to vote than if they see a large number of ads that promise more jobs, a striking and counterintuitive result. Figure 5.3 shows the predicted turnout rate for the median

county (for poverty, percent white, education, income, and lagged turnout) across a range of actual unemployment rates.

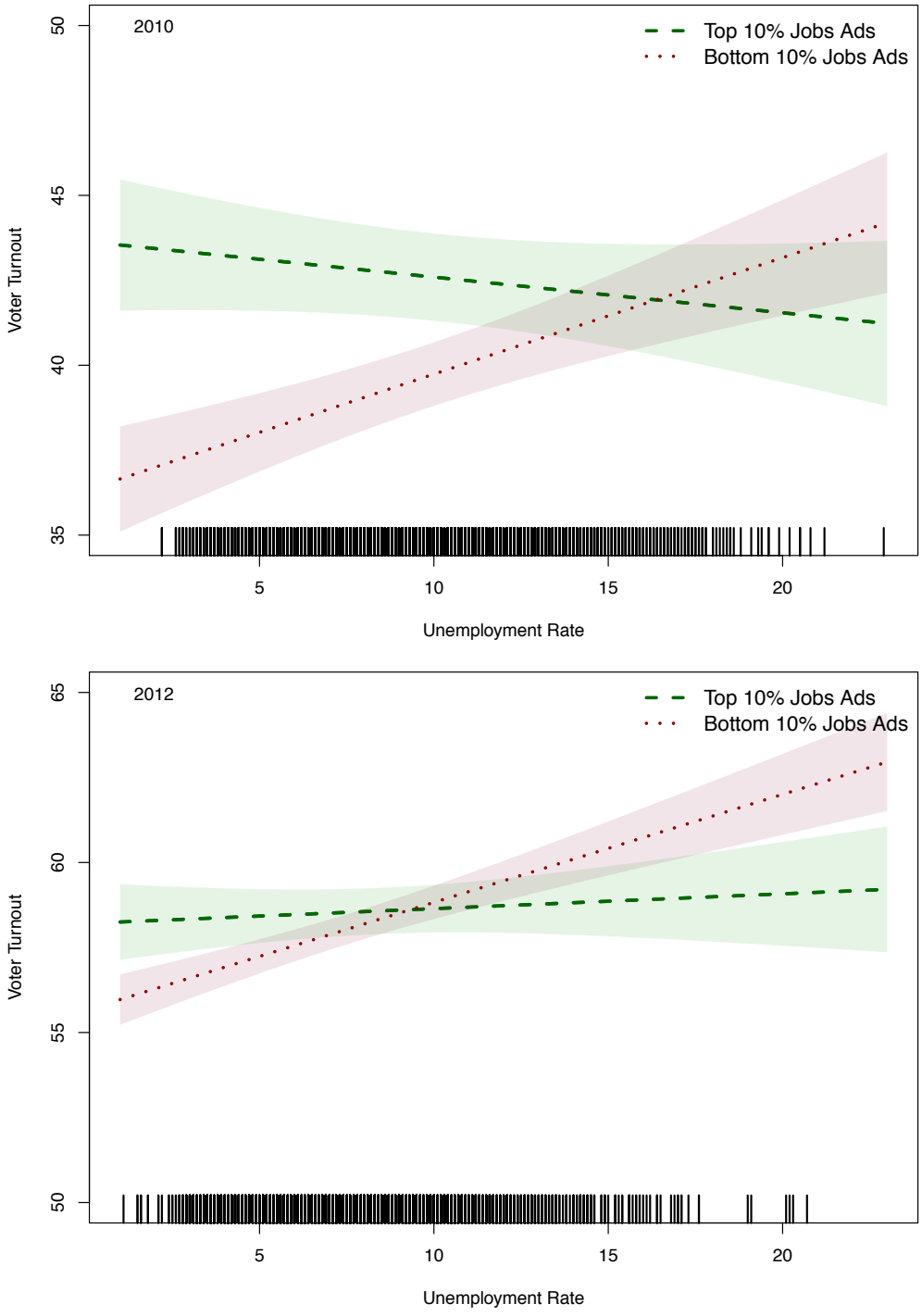


Figure 5.3: Predicted Turnout Across Unemployment Contexts, by Ad Issue Focus

The dashed line shows predicted turnout for counties that experienced much more job advertisements than average, while the dotted line shows predicted turnout for counties that experienced far fewer jobs advertisements than average. As the figure shows, there is clear separation in predicted turnout for areas that were exposed to high and low numbers of job-focused advertisements across both years. The patterns are very similar: in low unemployment contexts, voters are much more likely to turn out when ads focus on jobs. However, there is only evidence of a strong demobilizing effect (relative to no job-focused ads) in 2012 across the unemployment rates in my sample of counties.

While these cross-sectional results provide strong initial evidence in favor of hypothesis 1, it is possible that the relationship is spurious or that unobserved characteristics of counties that are correlated with both the unemployment rate and the number of jobs-focused ads those counties are exposed to are actually driving the results above. To address this possibility, I conduct a similar analysis using data from two consecutive presidential elections. By adopting a difference-in-difference empirical strategy, I explicitly control for time-invariant unobserved heterogeneity in underlying characteristics of counties that could be potentially correlated with voter turnout. If some unobserved and consistent over time feature of individual counties drives turnout separately from the effects of my explanatory variables, this specification controls for it by examining only observed differences across the same counties in two sequential presidential elections. I estimate the following model:

$$\begin{aligned}\Delta\text{Turnout}_c = {} & \beta_0 + \beta_1 \Delta\text{Jobs Ads}_d \times \Delta\text{Unemployment}_c + \beta_2 \Delta\text{Jobs Ads}_d + \beta_3 \Delta\text{Unemployment}_c + \\ & \beta_4 \Delta\text{Total Ads}_d + \beta_5 \Delta\text{Percent Poverty}_c + \beta_6 \Delta\text{Percent White}_c + \beta_7 \Delta\text{Percent H.S. Grad}_c + \\ & \beta_8 \Delta\text{Log(Median Household Income)} + \text{State}_c + \varepsilon_c\end{aligned}$$

Where the same variables used above now take the values of the difference in turnout, unemployment, and the number of ads focused on jobs taken between the years 2012 and 2008. Thus a positive value for $\Delta\text{Turnout}_c$ indicates that a county experienced increased turnout in 2012

relative to 2008, while negative values indicate that turnout decreased. On average, turnout decreased by about 3.4 percentage points between the two years. Again, I include state-level fixed effects to control for unmeasured characteristics of states (including the coincidence of senate or gubernatorial races, a state's battleground status, and any attendant mobilization efforts).

Δ = 2012 - 2008	Δ Turnout (1)	Δ Turnout (2)
Δ Jobs Ads$_d$ (1000s) ×	-0.028*	-0.028*
Δ Unemployment$_c$	(0.014)	(0.014)
Δ Jobs Ads$_d$ (1000s)	0.076^	0.119*
	(0.046)	(0.059)
Δ Unemployment$_c$	0.294**	0.305**
	(0.058)	(0.058)
Δ Total Ads$_d$ (1000s)		-0.012
		(0.015)
Δ Percent Poverty$_c$		-0.123**
		(0.034)
Δ Percent White$_c$		0.148**
		(0.032)
Δ Percent H.S. Grad$_c$		-0.010
		(0.007)
Δ Log(Household Income)$_c$		0.000^
		(0.000)
Constant	-3.954**	-3.804**
	(0.140)	(0.212)
N	3,104	3,104
R^2	0.009	0.025
State Fixed Effects	√	√

Standard errors in parentheses. The model on the right includes controls for change in percent white, percent high school graduate, and median household income at the county-level between 2012 and 2008. Results are robust to both logging the ad count and excluding media market border counties ($p < 0.1$), but the coefficient of interest is not statistically significant when using market clustered standard errors (see Appendix A4 – A6).
** p<0.01, * p<0.05, ^ p<0.1

Table 5.3: Effects of Change in Job-Focused Ads Across Unemployment Contexts, 2012

Table 5.3 shows the results of these models. Overall, there is strong support for the theory, with and without controls. While the effect of job-focused ads is only significant at $p < .10$, the key interaction term that provides support for hypothesis one is substantively large and significant across

the two model specifications. In areas that experienced much worse unemployment in 2012 as in 2008 (the top 10% of Δ Unemployment$_c$), a standard deviation increase in the change in job-focused ads is associated with an overall decline in turnout of 1.9%, plus or minus 0.6%. But in those same areas, a standard deviation decrease in job-focused ads is associated with a more modest decrease in turnout of 0.7%, plus or minus 0.5%. This result provides strong confirmatory evidence that focusing on jobs in places that were hard hit by the recession demobilizes voters. Since the difference-in-difference specification controls for unobserved county-level characteristics that might separately determine turnout, we can be more confident that hypothesis 1 finds support in the data.

So far, I have provided evidence that raising the salience of jobs increases turnout in strong economic contexts and depresses turnout in weak labor markets, confirming my first hypothesis. But not all job-focused ads use the same emotional cues when raising the salience of issues. In order to evaluate hypothesis 2, I separately test the effects of enthusiastic jobs ads. Hypothesis 2 suggests that these ads will be especially likely to engender demobilization in areas with high levels of unemployment, as they make the issue salient in enthusiastic emotional terms that may be even more at odds with local labor market context. If an individual living in a low unemployment area views ads that discuss the availability of jobs in an enthusiastic way, I expect that those ads will be especially likely to discourage engagement, as they amplify the disconnect between perceived labor market context and political advertising. To test hypothesis 2, I estimate the same model as above, but replace Jobs Ads$_d$ with Enthusiastic Jobs Ads$_d$ in the regression equation, where Enthusiastic Jobs Ads$_d$ is the total number of job-focused ads that contained an appeal to enthusiasm as coded by the Wesleyan Media Project.[9]

[9] In an ongoing research project I use trained human coders recruited from an online labor market to evaluate the validity of this measure.

The results in Table 5.4 are similar to those in Table 2, but the coefficients for both Enthusiastic Jobs Ads_d and the interaction term are larger than their corresponding coefficients in the non-valence model. The interaction coefficient in each year is more than twice as large as the effect of all job-focused ads. In 2010, exposure to one standard deviation of additional enthusiastic jobs ads increases turnout by 2.6 percentage points in areas with a 5% unemployment rate (compared with places experiencing one standard deviation fewer ads) while the same change in ads decreases turnout by 0.3 percentage points in areas with a 15% unemployment rate.

Dependent Variable: Voter Turnout	2010	2012
Enthusiastic Jobs Ads_d (1000s) × $Unemployment_c$	-0.093**	-0.098**
	(0.020)	(0.023)
Enthusiastic Jobs Ads_d (1000s)	1.133**	0.771**
	(0.251)	(0.215)
$Unemployment_c$	0.385**	0.316**
	(0.077)	(0.044)
Total Ads_d (1000s)	-0.010	0.003
	(0.030)	(0.017)
Lagged $Turnout_c$	0.593**	0.830**
	(0.015)	(0.009)
N	3,103	3,104
R^2	0.443	0.785
State Fixed Effects	√	√

Standard errors in parentheses. Models include controls for percent white, percent high school graduate, and median household income at the county-level. Full results shown in the appendix.
** p<0.01, * p<0.05, ^ p<0.1

Table 5.4: Effects of Enthusiastic Job-focused Ads Across Unemployment Contexts, 2010 and 2012

I observe very similar results for 2012: exposure to 2,370 enthusiastic jobs ads (one standard deviation above the mean) is associated with a decrease in turnout of 1.1 percentage points in counties with unemployment rates of 15%, relative to the same county with no exposure to enthusiastic jobs ads. This effect is about twice that observed for all job-focused ads, enthusiastic or not. Overall, these results show that in low unemployment areas, enthusiastic ads do increase voter

turnout, though at a smaller magnitude than that observed in experimental studies with turnout intention as the dependent variable of interest (Brader 2005, 91). Overall, issue-focused enthusiastic ads make individuals more likely to participate as long as they perceive campaign appeals to be credible. But in areas where unemployment is high, those enthusiastic ads are less likely to conform with perceptions of economic reality, and enthusiastic ads have a stronger demobilizing effect.

Finally, I conduct a placebo analysis meant to test whether the effects observed in the analyses above are unique to job focused advertisements, or if they generalize to issue areas where there is no reason to expect an interactive effect with local economic conditions. To do so, I examine the effects of advertisements that discuss campaign finance. If my theory is correct, we should not expect that campaign finance focused ads will have any systematic effect on turnout across different unemployment contexts. The results of both cross-sectional and difference-in-difference models are reported in tables 5.5 and 5.6.

Dependent Variable: Voter Turnout	2010	2012
Campaign Finance Ads_d (1000s) ×	-0.026	-0.697
$Unemployment_c$	(0.234)	(0.672)
Campaign Finance Ads_d (1000s)	2.342	5.484
	(1.916)	(5.374)
$Unemployment_c$	0.158**	0.231**
	(0.059)	(0.040)
Total Ads_d (1000s)	-0.000	0.007
	(0.017)	(0.012)
Lagged Turnout	0.593**	0.832**
	(0.015)	(0.009)
N	3,103	3,104
R^2	0.443	0.785
State Fixed Effects	√	√

Standard errors in parentheses. Models include controls for percent white, percent high school graduate, and median household income at the county-level. Full results shown in the appendix.
** p<0.01, * p<0.05, ^ p<0.1

Table 5.5: Placebo Test 1 – Effects of Campaign Finance-focused Ads Across Unemployment Contexts, 2010 and 2012

As the tables show, there is no significant effect for Campaign Finance Ads_d in either specification, and the interaction coefficients are also insignificant. In table 5.6, these coefficients are also incorrectly signed. This result confirms hypothesis 3, and suggests that the effects observed in the preceding analysis are confined to issues that are theoretically linked with underlying economic context. Future work should consider other economic policy issue areas, which may be more likely to interact with local economic context in a similar way as the job-focused ads that I examine here.

Δ = 2012 - 2008	Δ Turnout (1)	Δ Turnout (2)
Δ Campaign Finance Ads_d (1000s) × Δ $Unemployment_c$	0.019	0.020
	(0.029)	(0.029)
Δ Campaign Finance Ads_d (1000s)	-0.084	-0.078
	(0.098)	(0.103)
Δ $Unemployment_c$	0.289**	0.302**
	(0.060)	(0.059)
Δ Total Ads_d (1000s)		0.001
		(0.012)
Δ $Percent\ Poverty_c$		-0.125**
		(0.034)
Δ $Percent\ White_c$		0.146**
		(0.033)
Δ $Percent\ H.S.\ Grad_c$		-0.009
		(0.007)
Δ $Log(Household\ Income)_c$		0.000
		(0.000)
Constant	-3.929**	-3.732**
	(0.139)	(0.215)
N	3,104	3,104
R^2	0.008	0.023
State Fixed Effects	√	√

Standard errors in parentheses. Models include controls for percent white, percent high school graduate, and median household income at the county-level. Full results shown in the appendix. ** p<0.01, * p<0.05, ^ p<0.1

Table 5.6: Placebo Test 2 - Effects of Campaign Finance-focused Ads Across Unemployment Contexts, 2012

Discussion and Conclusion

The intuition that guides this chapter is that the effects of political communication are contextually dependent: talking about jobs in high and low employment contexts makes different economic realities salient to potential voters. Advertisements that link jobs with political participation also remind voters of their own local economic contexts, some of which feature high levels of unemployment and economic stagnation. As a result, I found that the same issue focus can have dramatically different consequences in terms of voter mobilization. Analyses of ad effects that fail to take into account the interaction of issue focus and local economic context would not detect these effects. In this section, I discuss the implications of these findings for the quality of democratic representation, campaign strategy, and political behavior more broadly. I conclude by suggesting how this empirical strategy might be adopted and expanded to test the effects of different kind of political appeals across diverse environmental contexts.

As Levine (2015) points out, voting is a "blunt communicative tool" (26). Using the decision to vote as my key dependent variable might understate the full extent that advertisements shape economic evaluations and political behavior. It is likely that raising the salience of high levels of unemployment makes individuals less willing to devote time and money to campaigns, or perhaps to withdraw from other kinds of informal political behavior. Future research could test the effects of exposure to issue-focused ads on individuals' donation behavior, an important form of formal political participation that is powerfully shaped by televised appeals (Urban and Niebler 2014). However, voting is my primary focus here because compositional shifts in the electorate have the potential to dramatically affect election outcomes (Hansford and Gomez 2010; Burden and Wichowsky 2014), and close election results have long-lasting and wide-ranging effects on public policy (Fowler and Hall 2015). If political communications systematically decrease turnout among those who live in high unemployment contexts, those voters might not enjoy the same level of

substantive representation that voters in high employment contexts obtain. While I do not examine the effects of job-focused ads on candidate vote share in this chapter, it is likely that the large shifts in turnout I observe decreased support for candidates who would otherwise gain strength in areas with high levels of unemployment. Thus I view this research as a starting point, rather than a final word, on the effects of issue-focused advertising in context.

I focus on political advertisements that discuss jobs, an important but broad category within all possible issue appeals. Jobs were frequently discussed in 2008, 2010, and 2012: there is also a high degree of variation in the number of job-focused ads broadcast in each market (relative to the very small total number and low level of variation for more direct redistributive economic issues described in chapter 1). I expect that the effects I observe here should generalize to other issue areas and environmental contexts, but it is also possible that the issue of jobs has an unusually clear link to local context, as individuals are likely to be aware of their local labor market's performance (Newman et al 2015). Improving and expanding measures of local context will help validate and extend these results. For example, it is possible that appeals to poverty have similar effects by local poverty rate, or that appeals focused on the housing crisis affect areas with high levels of home vacancies more than areas with a lower vacancy rate.

At the same time, future research should refine the measurement of an advertisement's issue focus by further exploring the emotional and symbolic cues that campaigns use when making references to political issues (Brader 2005). While I tested the effects of enthusiastic issue-focused appeals, I do not develop a more systematic psychological theory of emotional cues and issue salience. A more detailed account of the structure of communications would help map the psychological process by which individuals decide to engage in political action on as a result of some issue. My theory suggests that all issue ads are equally effective at making relevant environmental context more salient, but it could be the case that particular imagery, music, or other emotional cues

moderate the relationship between ads and reality more powerfully than others. Experimental research designs that directly assess individual-level responses to the issues raised in advertisements will help clarify these processes. At the same time, the observational measurement of issue focus could be improved by incorporating ads' gross rating points (Spenkuch and Toniatti 2015), ad cost, or other indications of an ads prominence (Prior 2001).

My empirical approach and results also provide an important lesson about environmental mediation for scholars of political communications and campaign strategy. As I have argued, structural features of television broadcasting create geographical discontinuities that limit campaigns' ability to directly target ads, and as a result, campaigns risk alienating potential supporters while attempting to mobilize others by focusing on local context. While talking about jobs was an effective mobilization strategy in areas that feature strong labor markets, there is no way to exclude at least some counties that feature high unemployment rates from seeing the same appeals as long as counties differ in underlying labor market conditions. As a result, scholars of political communication can leverage this regulation-induced discontinuity as a natural experiment, studying the effects of appeals that might not actually be targeted at those who receive them. Recent research that examines only counties located on media market boundaries (Spenkuch and Toniatti 2015) provide even cleaner causal identification of ad effects, though that empirical strategy may not generalize to urban areas at the center of most media markets. In appendix A6, I show that my results are robust to excluding border counties on the edges of media markets, but future work might focus on exactly those cases.

My measure of campaign issue focus should be expanded, in order to include a wider range of political appeals. While I exclusively examine the effects of televised appeals, the most prominent and universal communication strategy for contemporary campaigns, campaigns increasing rely upon new media and electronic communications to deliver issue-focused political appeals (Issenberg 2012;

Hersh and Schaffner 2013). As campaigns gain the ability to more narrowly target issue appeals to those most likely to be affected by those appeals, the demobilizing effects I observe here could be minimized. However, campaigns are unlikely to predict in advance the heterogeneous effects of political communications without rigorous field experimentation. As I have shown here, appeals that might be expected to encourage political participation broadly actually discourage it for those that might actually benefit most from engaging in political action. The extent to which campaigns can minimize these demobilizing effects remains to be seen, especially since some candidates might strategically seek to demobilize particular groups when it might improve their electoral prospects.

The relationship between job-focused appeals and local labor markets is just one example of potentially important interactions between political communication and context. Related research might explore how the effects of advertisements focused on crime vary by local crime rates (Gray and Hughes 2015), immigration-focused ads by actual immigrant population, or education-focused ads by the quality of public schools. In each of these cases, campaigns seek to highlight an important social and political issue and convince individuals to engage in politics to held address that issue. But because social and political reality is diverse, individuals bring their own perspectives and opinions to bear on those issues when raised in electoral competition. The disconnect between relatively general issue appeals and particular individuals' experiences of political reality is an important tension in democratic campaigning and representation.

Candidates cannot perfectly represent or respond to the opinions, hopes, and fears of potential supporters, which are in turn shaped by political messages and environmental context. Instead, campaigns set the agenda, by choosing which issues are worth discussing. Since that decision may be guided by campaign consultants and structural features of different electoral contests (Nyhan and Montgomery 2015; Minozzi, Volden, and Darmofal 2014) rather than by local environmental context and the opinions of potential voters, it is possible that campaigns shape the

electorate just as individuals shape policy by voting for candidates. To better understand the reasons that people engage in democratic politics, it is crucial to first examine the issues that political actors prioritize. Politics does not occur in a vacuum: by understanding the ways that political actors solicit public support across different audiences, scholars can better understand the contextual determinants of political opinion and action.

6 Conclusion

"Leaders often pretend that they have merely uncovered a program which existed in the minds of their public. When they believe it, they are usually deceiving themselves. Programs do not invent themselves synchronously in a multitude of minds." – Walter Lippmann (1922, 243)

The arguments and evidence provided so far challenge existing accounts of Americans' opposition to government sponsored redistribution. Instead of claiming that Americans are "most definitely not economic egalitarians" (Schlozman, Verba, and Brady 2012, 60), I argue that Americans' expressed egalitarianism depends on the policy agenda supplied by elected officials and on firsthand experience with local economic conditions. When elected officials avoid talking about state-sponsored redistributive policies, and when economic stagnation is omnipresent, Americans are unlikely to support redistributive policy change. It is difficult for individuals to imagine a different reality without leaders articulating a vision of widespread economic prosperity and proposing new policies to get there. When leaders make no effort to define or defend policy alternatives, there is no reason to expect the public to support those alternatives.

All policies are socially constructed: powerful groups reinforce their status when setting the policy agenda and making proposals (Schneider and Ingram 1993). Central to that process of social construction is political communication: a means for candidates and elected officials to define deserving groups and advocate for political decisions. As Barabas (2015) points out, the issues that political actors choose to talk about, alongside the issues that polling firms ask questions about, create a "democratic denominator," defining the realm of possible political action long before individuals are able to express evaluations of those issues or decide to advocate for and against them. Existing accounts of redistributive policy opinion pay too little attention to this democratic denominator, asking questions about vague policies that respondents cannot always reconcile with other sources of political information, if they even have the motivation to seek out that information.

During and after the great recession, political actors advocated for market-based economic policies rather than direct redistributive programs. As chapter 1 shows, campaigns generally avoided redistributive policy discussions, especially in impoverished areas, while congressional press releases used predominantly market-oriented language to talk about economic goals. Political actors' response to the Great Recession focused on job growth and tax incentives, rather than programmatic social policy. As a result, American ambivalence toward direct redistribution should not be surprising: when the supply of direct redistributive policies is constrained, public demand is too.

By reframing opinion research to highlight the conditionality of expressed preferences relative to political stimuli, I provide a more complete model of opinion formation. Chapter 2 argues that political actors and survey researchers often fail to anchor opinion by providing specific representations of alternative policy choices or cuing relevant groups. This leads to less informative and unreliable expressed opinions and encourages only tepid support for major policy interventions (Brooks and Manza 2013). However, in chapter 3, I show that individuals are surprisingly supportive of some redistributive policies, as long as they are provided with understandable and identity-consistent representations of the economic issues and policy proposals in question.

Local environmental context mediates the relationship between political communication and opinion formation because individuals reconcile political messages with their own experience of economic prospects. In chapter 4, I show that economic mobility information means different things to those who live in places with very high or low levels of mobility, leading only those in strong labor markets to support direct and indirect policy interventions. In chapter 5, I find that job-focused political advertising only incentivizes political behavior when individuals have first-hand experience with strong labor markets. The decision to vote depends upon both the supply of economic policy proposals and individuals' experience with economic reality; talking about indirect,

market-oriented redistribution only increases voter mobilization as long as local labor markets are strong. These chapters underscore the importance of measuring political communication within broader political and economic contexts. They also provide more externally valid evidence of the argument developed in survey experiments.

Most individuals rely on social groups in order to interpret and assign meaning to political messages about policy (Achen and Bartels 2016; Nelson and Kinder 1996). The most important of these identities is partisanship. Individuals develop stable and long-lasting memberships with political parties (Green et al 2003), and partisans are likely to systematically misperceive economic reality, a phenomenon called "partisan perceptual bias" (Green and Gerber 1999; Jerit and Barabas 2012). Democrats and Republicans are each likely to assign blame for economic stagnation to the other party and to rely upon party cues – rather than policy-specific information – to evaluate proposed policies. While much of the research presented here does not explicitly consider the role of party cues in opinion formation, the estate tax experiment in chapter 3 does suggest that both Democrats and Republicans are responsive to ideologically incongruent sources of information. Since the inclusion or exclusion of party cues within policy representations is a political decision, the model I develop in chapter 2 is well-suited to a more complete examination of partisanship's unique role in shaping issue evaluations.

Future work should also focus on individuals' subjective class or economic group identity, and how those identities condition their responses to economic information. However, economic group identification may be weaker than racial, gender, or partisan identification. For example, Americans are less likely to strongly identify with others in terms of economic position or class than those in other countries (Hochschild 1981). Similarly, an individual may not visibly appear economically insecure in the same way that he or she might appear to be a member of race- or gender-specific groups. Finally, Americans dislike talking about money (Shane 2012), and the

concept of economic identity requires greater rhetorical abstraction than more easily imagined social groups. Yet evidence suggests Americans do develop contingent and subjective economic identities on an ad hoc basis: by making comparisons with others (Brown-Iannuzzi et al 2015). They also express sympathy for the poor and resentment toward the rich (Piston 2014). Since there is abundant evidence that evaluations of social groups shape support for policies that affect those groups, future work should focus on how redistributive political communications define or cue economic social identities. Increasing specificity within the supply of policy proposals necessarily defines groups of winners and losers.

An important and related research question concerns the efficacy of redistributive programs: do policies meant to decrease economic insecurity and reduce income disparities actually succeed in doing so? While Bartels (2008) argues that economic performance during post-WWII Democratic presidential administrations (which were more supportive of redistribution than Republican administrations) exceeds performance during Republican presidential terms, there is little evidence that particular policies are responsible for the historical pattern (Blinder and Watson 2016). However, there is a wide array of evidence that shows how direct redistributive policies improve economic outcomes for the poor. For example, Chetty et al. (2016) find that means-tested public housing programs increase intergenerational mobility, while Hoyes et al. (2016) show that the supplemental nutrition assistance program improved women's economic self-sufficiency and children's health. By understanding which policies work, and by providing that information to individuals, both political actors and opinion analysts can elicit more meaningful and informed political opinions.

A related topic concerns the supply of policy-specific political communications focused on redistributive policies that have already been enacted. While many direct redistributive policy interventions are effective, robust rhetorical advocacy and empowered coalitions of supporters are

necessary to sustain policies after enactment (Patashnik 2008; Campbell 2005; Mettler 2007). If the groups affected by redistributive policies are not cohesive and unable to invest political capital in policy change, the change is likely to be reversed (Patashnik 2008, 32). Future work should examine the supply of policy-specific political communication throughout the lifespan of enacted economic policies, in order to better understand the mechanisms that lead social programs and tax policies to persevere after enactment.

In the 2016 presidential campaign, the progressive Democratic candidate Bernie Sanders advocated for a range of redistributive policies that were ignored completely in the wake of the Great Recession (Talbot 2015). These include direct redistributive programs, such as a more progressive estate tax and a youth-targeted, government-funded jobs program, alongside indirect policies, such as free public secondary education, universal childcare and pre-kindergarten education, and a stronger guarantee of collective bargaining rights. Regardless of the outcome of the 2016 presidential election, this shift in political communication, combined with continued economic recovery, should increase public support for redistributive policy. By supplying policy proposals that have been left out of most political actors' rhetorical agendas, Sanders' campaign will present an important test of the theory developed here. Initial evidence indicates that Americans are indeed much more favorable toward redistribution than they were during the Great Recession. For example, a poll conducted in January 2016 found that 73% of Americans favored increasing taxes on the wealthy, 66% favored increased taxes on corporations, 55% supported single payer healthcare, and 59% supported free college (Morning Consult 2016). These results are starkly at odds with the prevailing scholarly account of a public that rejects economic egalitarianism and redistributive policy, and provide very tentative initial evidence that an increased supply of redistributive policy proposals is related to increased public support for those policies.

In 1922, Walter Lippmann wrote that "the facts of modern life do not spontaneously take shape in which they can be known. They must be given a shape by somebody" (345). This project focuses on the process by which political actors shape economic facts for mass consumption. While the claim that elected officials and media sources politicize economic information is uncontroversial, academic researchers often seek to elide their own role in shaping representations of political reality and constraining individual responses. However, the actions of opinion researchers are just as political as elected officials' attempts to set the policy agenda. An important implication of this research is that the study of opinion necessary politicizes the issues under consideration; choosing how to talk about redistribution determines how people think about it. As Lippmann argues, representative government depends upon political actors "making the unseen facts intelligible to those who have to make the decisions" (19). Obscuring economic facts and avoiding the topic of direct redistributive policy depoliticizes public opinion and circumscribes democratic action. By representing reality in more specific and political ways, both elected officials and opinion analysts can empower the American public to make democratic demands.

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Appendix

3A: Chapter 3

	SSI 2014a (%)	ANES 2012 (weighted %)
Gender		
Male	49	48
Female	51	52
Education		
High School or Less	36	40
Some College	31	30
College Degree	21	19
Graduate Degree	12	11
Race		
White Non-Hispanic	67	71
Black Non-Hispanic	12	12
Other	8	6
Hispanic	13	11
Party		
Democrat	52	35
Independent	18	14
Republican	30	39
Ideology		
Liberal	33	25
Moderate	39	35
Conservative	28	40
Age		
Under 35	32	33
35-44	17	14
45-54	17	17
Over 55	34	26

Table 3A1. Sample Demographics for Estate Tax Study

	SSI 2014b (%)	ANES 2012 (weighted %)
Gender		
Male	47	48
Female	53	52
Education		
High School or Less	29	40
Some College	21	30
College Degree	39	19
Graduate Degree	11	11
Race		
White Non-Hispanic	69	71
Black Non-Hispanic	12	12
Other	6	6
Hispanic	13	11
Party		
Democrat	53	35
Independent	20	14
Republican	27	39
Ideology		
Liberal	35	25
Moderate	25	35
Conservative	30	40
Age		
Under 35	31	33
35-44	17	14
45-54	18	17
Over 55	24	26

Table 3A2. Sample Demographics for Income Inequality Study

3A2. Dependent Variable Measurement

Question Text: Do you favor, oppose, or neither favor nor oppose the government trying to decrease income inequality?
Answer options: Favor a great deal / Favor moderately / Favor a little / Neither favor nor oppose / Oppose a little / Oppose moderately / Oppose a great deal
Population: Members of SSI online panel selected to approximate a simple random sample of the adult U.S. population (N = 540).

	All Respondents	Democrats	Independents	Republicans
Favor a great deal	120 (22%)	90 (33%)	13 (12%)	13 (9%)
Favor moderately	96 (18%)	57 (21%)	15 (14%)	21 (15%)
Favor a little	74 (14%)	41 (15%)	13 (12%)	18 (13%)
Neither favor/nor oppose	86 (16%)	38 (14%)	25 (23%)	22 (15%)
Oppose a little	55 (10%)	12 (4%)	18 (16%)	21 (15%)
Oppose moderately	37 (7%)	13 (5%)	5 (5%)	17 (12%)
Oppose a great deal	72 (13%)	21 (8%)	18 (17%)	31 (22%)
N	540 (100%)	272 (100%)	107 (100%)	143 (100%)
Average Support	0.59	0.70	0.50	0.44

Figure 3A3. Hypothesis 2 Results with Text-Consistent Condition Comparison

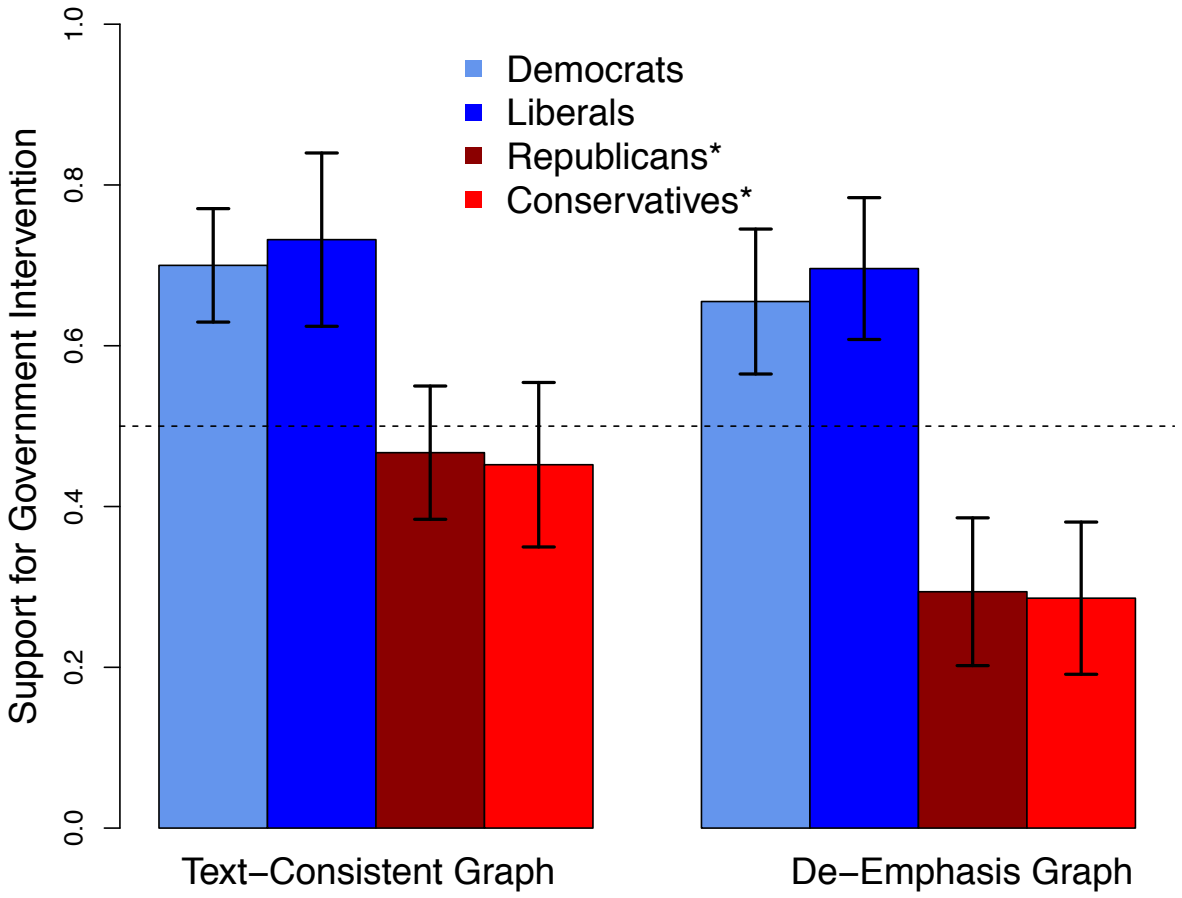
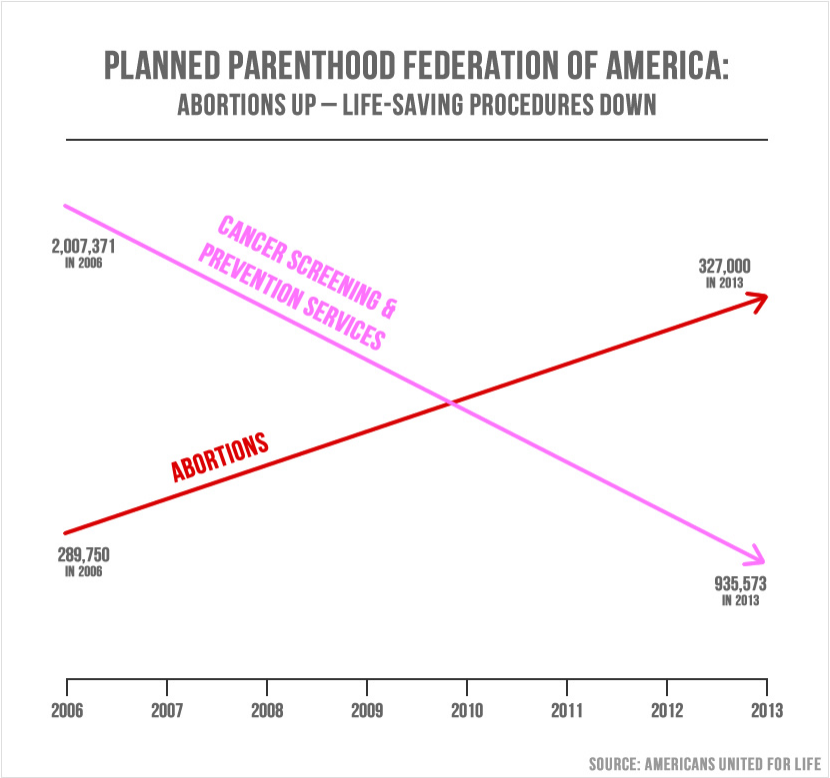


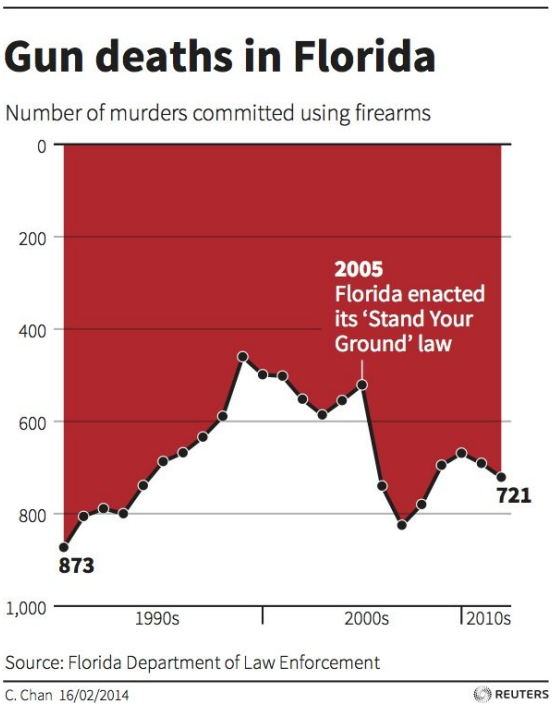
Figure 3A1: Average Inequality Intervention Support by De-Emphasis Graph Exposure

The figure shows average support for intervention across ideological and partisan subsets of the sample by treatment condition with 95% confidence intervals, with the text-consistent graph as the base category. T-tests indicate that exposure to the de-emphasis graph led all respondents to become less supportive of intervention relative to those who only saw the text-consistent graph, but the effect is only statistically significant for Republicans and conservatives ($p = 0.026$ and 0.018, respectively).

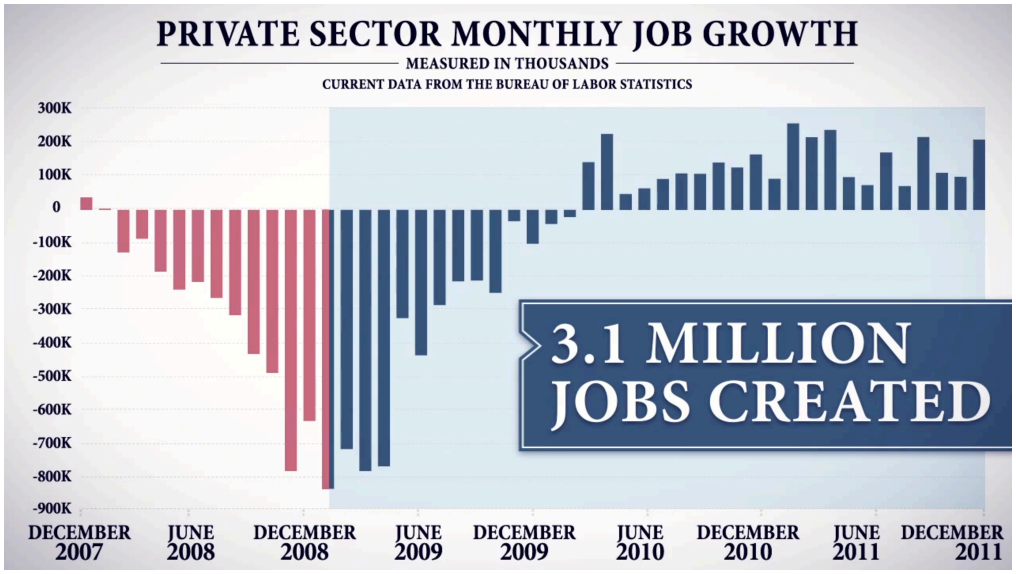
3A4. Examples of Emphasis Graphs in Political Communication



2015 Jason Chaffetz (R-UT) Slide, Congressional Testimony



2014 Reuters, Gun Deaths in Florida



2012 Obama for America Campaign Ad, National

2012 Scott Walker Recall Ad, Wisconsin

4A: Chapter 4

The results presented in Chapter 4 are based on the first module in an omnibus survey that also included the experiment on estate tax information presented in Chapter 3. Random assignment to different treatments in the economic mobility information study is unrelated to random assignment on the estate tax portion of the survey (which was the last module). Reported estate tax opinions do not vary systematically by assignment to mobility information; this is likely due to the presence of three unrelated blocks of questions (on executive actions, health policy, and citizenship) in the intervening modules.

	SSI 2014a (%)	ANES 2012 (weighted %)
Gender		
Male	49	48
Female	51	52
Education		
High School or Less	36	40
Some College	31	30
College Degree	21	19
Graduate Degree	12	11
Race		
White Non-Hispanic	67	71
Black Non-Hispanic	12	12
Other	8	6
Hispanic	13	11
Party		
Democrat	52	35
Independent	18	14
Republican	30	39
Ideology		
Liberal	33	25
Moderate	39	35
Conservative	28	40
Age		
Under 35	32	33
35-44	17	14
45-54	17	17
Over 55	34	26

Table 4A1. Sample Demographics for Economic Mobility Study

	Government Intervention	Minimum Wage	Job Training	Education Spending
National Info.	0.346	0.273	0.132	0.073
	(0.096)**	(0.099)**	(0.088)	(0.088)
National Info. × Mobility Rate	-0.037	-0.028	-0.014	-0.009
	(0.012)**	(0.011)*	(0.010)	(0.010)
Local Info.	-0.280	-0.101	-0.217	-0.157
	(0.086)**	(0.106)	(0.085)*	(0.090)
Local Info. × Mobility Rate	0.035	0.015	0.029	0.024
	(0.010)**	(0.012)	(0.010)**	(0.010)*
Mobility	0.016	0.022	0.004	-0.001
	(0.010)	(0.006)**	(0.008)	(0.008)
Male	-0.018	-0.052	-0.010	-0.049
	(0.024)	(0.025)*	(0.022)	(0.024)*
Age: 25-34	0.051	0.056	-0.015	-0.024
	(0.040)	(0.042)	(0.037)	(0.039)
Age: 35-44	0.039	0.021	0.001	-0.019
	(0.044)	(0.050)	(0.039)	(0.039)
Age: 45-54	-0.035	0.050	-0.064	-0.093
	(0.045)	(0.045)	(0.037)	(0.041)*
Age: 55-64	-0.034	0.029	-0.042	-0.119
	(0.047)	(0.047)	(0.039)	(0.039)**
Age: 65-74	-0.090	-0.076	-0.067	-0.141
	(0.050)	(0.055)	(0.044)	(0.050)**
Age: 75+	-0.178	-0.070	-0.068	-0.197
	(0.060)**	(0.063)	(0.051)	(0.053)**
Black	-0.009	-0.038	-0.019	0.014
	(0.048)	(0.065)	(0.047)	(0.044)
Hispanic	0.106	0.168	0.064	0.064
	(0.037)**	(0.028)**	(0.031)*	(0.034)
Asian	0.052	0.051	0.029	0.040
	(0.035)	(0.039)	(0.033)	(0.030)
Other Race	0.015	0.116	0.007	0.070
	(0.079)	(0.065)	(0.087)	(0.075)
High School Degree	-0.020	0.003	-0.011	-0.037
	(0.044)	(0.046)	(0.047)	(0.049)
Some College	-0.008	-0.006	0.021	0.007
	(0.046)	(0.045)	(0.047)	(0.048)
Bachelor's Degree	0.052	-0.017	0.060	0.082
	(0.053)	(0.051)	(0.049)	(0.052)
Graduate Degree	0.108	0.035	0.128	0.143
	(0.053)*	(0.055)	(0.049)**	(0.053)**
One Child	-0.002	0.002	0.014	-0.000
	(0.031)	(0.031)	(0.030)	(0.030)
More Than One Child	-0.034	-0.042	-0.006	0.015
	(0.028)	(0.029)	(0.027)	(0.025)
Constant	0.450	0.528	0.581	0.713
	(0.093)**	(0.075)**	(0.084)**	(0.082)**
R^2	0.12	0.11	0.07	0.12
N	658	656	658	659

* $p<0.05$; ** $p<0.01$

Table 4A2: Full Model Results for Table 4.1

	Government Intervention	Minimum Wage	Job Training	Education Spending
Local Info.	-0.265	-0.082	-0.193	-0.154
	(0.086)**	(0.104)	(0.085)*	(0.091)
Local Info. × Mobility Rate	0.032	0.013	0.026	0.022
	(0.010)**	(0.012)	(0.010)**	(0.010)*
Mobility	-0.019	-0.005	-0.007	-0.009
	(0.007)**	(0.009)	(0.007)	(0.007)
Male	-0.020	-0.028	-0.037	-0.077
	(0.028)	(0.032)	(0.024)	(0.028)**
Age: 25-34	0.060	0.044	-0.029	-0.041
	(0.046)	(0.052)	(0.042)	(0.045)
Age: 35-44	0.020	-0.017	-0.002	-0.038
	(0.055)	(0.065)	(0.048)	(0.049)
Age: 45-54	0.003	0.088	-0.046	-0.115
	(0.052)	(0.054)	(0.043)	(0.049)*
Age: 55-64	-0.005	0.019	-0.028	-0.081
	(0.059)	(0.061)	(0.048)	(0.047)
Age: 65-74	-0.102	-0.070	-0.100	-0.211
	(0.067)	(0.070)	(0.054)	(0.064)**
Age: 75+	-0.132	-0.064	-0.033	-0.195
	(0.071)	(0.071)	(0.057)	(0.061)**
Black	0.077	0.101	0.046	0.081
	(0.051)	(0.033)**	(0.053)	(0.041)
Hispanic	0.146	0.179	0.048	0.075
	(0.039)**	(0.031)**	(0.034)	(0.039)
Asian	0.074	0.044	-0.008	0.031
	(0.046)	(0.050)	(0.044)	(0.041)
Other Race	0.010	0.089	-0.010	0.068
	(0.091)	(0.072)	(0.105)	(0.087)
High School Degree	-0.064	-0.052	-0.044	-0.065
	(0.052)	(0.047)	(0.057)	(0.060)
Some College	-0.064	-0.042	0.015	-0.031
	(0.055)	(0.047)	(0.059)	(0.062)
Bachelor's Degree	-0.008	-0.047	0.034	0.047
	(0.065)	(0.054)	(0.062)	(0.067)
Graduate Degree	0.017	0.010	0.042	0.091
	(0.061)	(0.062)	(0.058)	(0.065)
One Child	0.039	0.025	0.044	0.031
	(0.037)	(0.038)	(0.037)	(0.039)
More Than One Child	-0.008	-0.019	0.027	0.034
	(0.033)	(0.035)	(0.031)	(0.030)
Constant	0.793	0.799	0.715	0.818
	(0.082)**	(0.096)**	(0.085)**	(0.090)**
R^2	0.13	0.11	0.06	0.15
N	430	427	428	430

* $p<0.05$; ** $p<0.01$

Table 4A3: Full Model Results for Table 4.2

4A2. Dependent Variable Measurement

Government Intervention: Do you support the government doing more to increase economic mobility for low-income Americans, even if it means taxes would be raised?

Minimum wage: Do you support increasing the federal minimum wage?

Job Spending: Do you support increased spending on job training, even if it means taxes would be raised?

Education Spending: Do you support increased spending on education, even if it means taxes would be raised?

The minimum wage question included a five-point response scale, while the other three items included a seven-point scale. I recode all items to 0-1 in order to aid across item comparability.

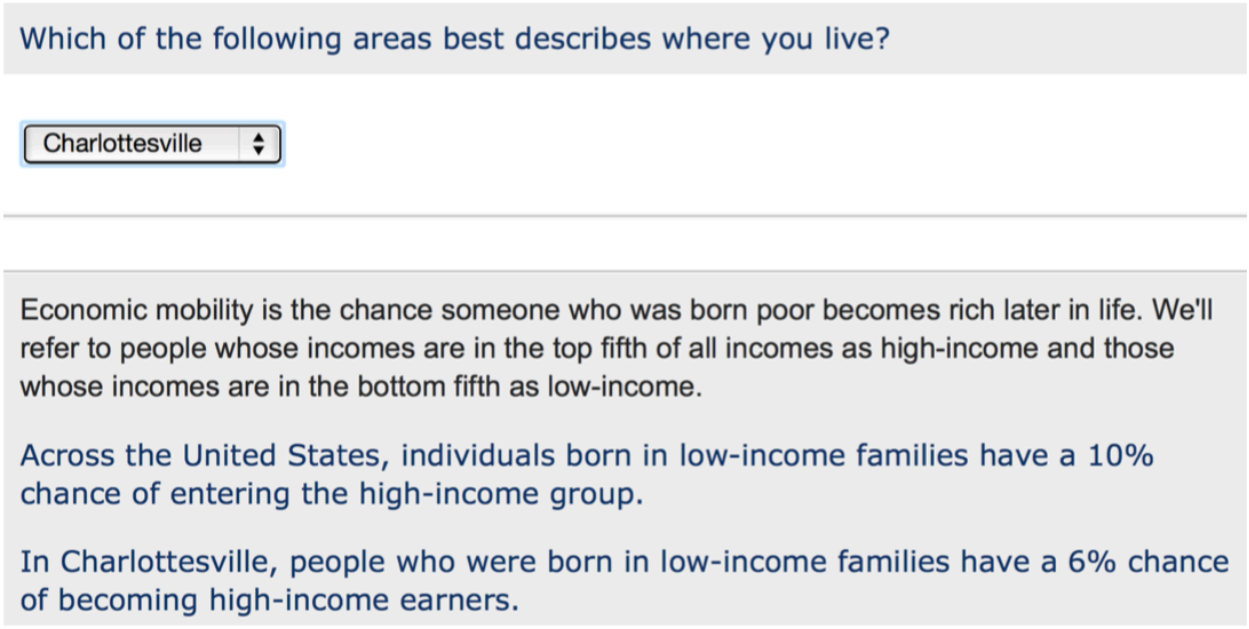


Figure 4A1: Experimental Treatment

5A: Chapter 5

5A1: Full Model Results

Dependent Variable: Voter Turnout	2010	2012
Jobs Ads_d (1000s) ×	-0.039**	-0.035**
$Unemployment_c$	(0.009)	(0.010)
Jobs Ads_d (1000s)	0.642**	0.330**
	(0.134)	(0.104)
$Unemployment_c$	0.377**	0.318**
	(0.076)	(0.046)
Total Ads_d (1000s)	-0.082^	-0.010
	(0.047)	(0.026)
Percent $Poverty_c$	-0.362**	-0.128**
	(0.035)	(0.020)
Percent $White_c$	-0.020^	-0.154
	(0.010)	(0.504)
Percent H.S. $Grad_c$	0.006	-0.002
	(0.012)	(0.007)
Log(Household $Income)_c$	-6.096**	-0.426
	(1.039)	(0.587)
Lagged $Turnout_c$	0.592**	0.830**
	(0.015)	(0.009)
Constant	82.205**	11.380^
	(11.703)	(6.500)
N	3,103	3,104
R^2	0.443	0.785
State Fixed Effects	√	√

Standard errors in parentheses.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A1.1: Job-focused Appeals and Voter Turnout: 2010 and 2012

Dependent Variable: Voter Turnout	2010	2012
Enthusiastic Jobs Ads_d (1000s) × $Unemployment_c$	-0.093**	-0.098**
	(0.020)	(0.023)
Enthusiastic Jobs Ads_d (1000s)	1.133**	0.771**
	(0.251)	(0.215)
$Unemployment_c$	0.385**	0.316**
	(0.077)	(0.044)
Total Ads_d (1000s)	-0.010	0.003
	(0.030)	(0.017)
Percent $Poverty_c$	-0.358**	-0.129**
	(0.035)	(0.020)
Percent $White_c$	-0.020*	-0.154
	(0.010)	(0.503)
Percent H.S. $Grad_c$	0.006	-0.002
	(0.012)	(0.007)
$Log(Household\ Income)_c$	-6.101**	-0.447
	(1.039)	(0.589)
Lagged $Turnout_c$	0.593**	0.830**
	(0.015)	(0.009)
Constant	82.040**	11.663^
	(11.710)	(6.514)
N	3,103	3,104
R^2	0.443	0.785
State Fixed Effects	√	√

Standard errors in parentheses.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A1.2: Enthusiastic Job-focused Appeals and Voter Turnout: 2010 and 2012

Appendix 5A2: Summary Statistics

2010	**Mean**	**St. Dev.**	**Min**	**Max**	**N**
2010 Turnout$_c$ (VAP)	40.74	10.14	8.91	94.93	3,103
2006 Turnout$_c$ (VAP)	41.87	11.73	2.64	91.42	3,103
Unemployment Rate$_c$	9.36	3.15	2.20	29.10	3,103
Poverty Rate$_c$	16.79	6.23	3.10	50.10	3,103
Percent White	83.19	16.43	2.92	99.22	3,103
Percent High School Graduate	83.67	9.49	4.50	100.00	3,103
Log(Median Household Income)	10.28	0.18	9.63	11.14	3,103
2012					
2012 Turnout$_c$ (VAP)	58.28	9.49	17.32	98.64	3,104
2008 Turnout$_c$ (VAP)	61.60	9.12	15.59	96.43	3,104
Unemployment Rate$_c$	7.83	2.75	1.10	27.40	3,104
Poverty Rate$_c$	17.23	6.57	3.10	51.20	3,104
Percent White	78.27	19.56	1.25	100.00	3,104
Percent High School Graduate	81.19	9.21	8.70	100.00	3,104
Log(Median Household Income)	10.31	0.17	9.69	11.20	3,104

Table 5A2: Summary Statistics, County-Level

Appendix 5A3: Fearful Job-Focused Ads

Dependent Variable: Voter Turnout	2010	2012
Enthusiastic Jobs Ads_d (1000s) × $Unemployment_c$	-0.085**	-0.112**
	(0.022)	(0.028)
Fearful Jobs Ads_d (1000s) × $Unemployment_c$	-0.059	0.042
	(0.044)	(0.051)
Enthusiastic Jobs Ads_d (1000s)	1.091**	0.857**
	(0.278)	(0.254)
Fearful Jobs Ads_d (1000s)	0.821	-0.004
	(0.521)	(0.448)
$Unemployment_c$	0.414**	0.315**
	(0.079)	(0.045)
Total Ads_d (1000s)	-0.031	-0.008
	(0.041)	(0.018)
Percent $Poverty_c$	-0.363**	-0.127**
	(0.035)	(0.020)
Percent $White_c$	-0.020^	-0.023
	(0.010)	(0.508)
Percent H.S. $Grad_c$	0.006	-0.002
	(0.012)	(0.007)
Log(Household Income)$_c$	-6.281**	-0.417
	(1.046)	(0.589)
Lagged $Turnout_c$	0.592**	0.829**
	(0.015)	(0.009)
Constant	83.702**	11.264^
	(11.757)	(6.516)
N	3,103	3,104
R^2	0.444	0.785
State Fixed Effects	√	√

Standard errors in parentheses.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A3: Job-focused Appeals by Valence and Voter Turnout: 2010 and 2012

These results show that the effect of enthusiastic ads is robust to controlling for fearful ads, and that fearful ads have no effect separately from that of enthusiastic ads. Due to the competing theoretical predictions associated with the use of fear cues (Brader 2005, 71), it could be the case that heterogeneous response types are counterbalancing each other.

Appendix 5A4: Logged Ad Count

It is plausible that the effects of job-focused ads have diminishing returns as the number of ads increases. I use a natural log transformed version of the dependent variable to model this alternative expectation and find no important differences compared to my preferred specification.

Dependent Variable: Voter Turnout	2010	2012
Log(Jobs Ads_d) × $Unemployment_c$	-0.087**	-0.023**
	(0.029)	(0.009)
Log(Jobs Ads_d)	1.980**	0.112
	(0.436)	(0.098)
$Unemployment_c$	0.854**	0.366**
	(0.245)	(0.065)
Log(Total Ads_d)	-0.722*	0.022
	(0.356)	(0.069)
Percent $Poverty_c$	-0.356**	-0.124**
	(0.035)	(0.020)
Percent $White_c$	-0.018^	-0.180
	(0.010)	(0.504)
Percent H.S. $Grad_c$	0.007	-0.002
	(0.012)	(0.007)
Log(Household $Income)_c$	-5.995**	-0.187
	(1.036)	(0.584)
Lagged $Turnout_c$	0.590**	0.831**
	(0.015)	(0.009)
Constant	74.024**	8.742
	(11.837)	(6.471)
N	3,103	3,104
R^2	0.445	0.784
State Fixed Effects	√	√

Standard errors in parentheses.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A4.1: Logged Job-focused Appeals and Voter Turnout: 2010 and 2012

Δ = 2012 - 2008	Δ Turnout (1)	Δ Turnout (2)
Δ Log(Jobs Ads$_d$) × Δ Unemployment$_c$	-0.071*	-0.074*
	(0.033)	(0.033)
Δ Log(Jobs Ads$_d$)	0.242*	0.305**
	(0.104)	(0.109)
Δ Unemployment$_c$	0.345**	0.358**
	(0.065)	(0.065)
Δ Log(Total Ads$_d$)		-0.044
		(0.028)
Δ Percent Poverty$_c$		-0.121**
		(0.034)
Δ Percent White$_c$		0.149**
		(0.032)
Δ Percent H.S. Grad$_c$		-0.010
		(0.007)
Δ Log(Household Income)$_c$		0.000
		(0.000)
Constant	-4.139**	-3.769**
	(0.170)	(0.259)
N	3,104	3,104
R^2	0.009	0.025
State Fixed Effects	√	√

Standard errors in parentheses. The model on the right includes controls for change in percent white, percent high school graduate, and median household income at the county-level between 2012 and 2008.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A4.2: Effects of Change in Log(Job-Focused Ads) Across Unemployment Contexts, 2012

Appendix 5A5: Media Market Clustering

Although ads are broadcast at the media market level, my treatment of interest is the combined effect of county level unemployment and job-focused ads. However, for those who prefer to conceptualize the treatment at the media market level, I present results that show that the cross-sectional results are robust to using media market clustered standard errors in the regression equation. The difference-in-difference estimates for change in the number of job focused ads have the correct sign, but are insignificant.

Dependent Variable: Voter Turnout	2010	2012
Jobs Ads_d (1000s) × $Unemployment_c$	-0.039**	-0.035*
	(0.012)	(0.014)
Jobs Ads_d (1000s)	0.642**	0.330*
	(0.218)	(0.148)
$Unemployment_c$	0.377**	0.318**
	(0.123)	(0.090)
Total Ads_d (1000s)	-0.082	-0.010
	(0.081)	(0.035)
Percent $Poverty_c$	-0.362**	-0.128**
	(0.050)	(0.027)
Percent $White_c$	-0.020	-0.154
	(0.019)	(0.882)
Percent H.S. $Grad_c$	0.006	-0.002
	(0.016)	(0.012)
$Log(Household\ Income)_c$	-6.096**	-0.426
	(1.348)	(0.944)
Lagged $Turnout_c$	0.592**	0.830**
	(0.039)	(0.022)
Constant	86.151**	12.904
	(15.780)	(10.607)
N	3,103	3,104
R^2	0.443	0.785
State Fixed Effects	√	√

Media Market-level cluster robust standard errors in parentheses.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A5.1: Job-focused Appeals and Voter Turnout: 2010 and 2012

Δ = 2012 - 2008	Δ Turnout	Δ Turnout
Δ Jobs Ads_d (1000s) ×	-0.071	-0.074
Δ $Unemployment_c$	(0.045)	(0.046)
Δ Jobs Ads_d (1000s)	0.242**	0.305**
	(0.122)	(0.130)
Δ $Unemployment_c$	0.345***	0.358***
	(0.098)	(0.100)
Δ Total Ads_d (1000s)		-0.044
		(0.033)
Δ Percent $Poverty_c$		-0.121***
		(0.045)
Δ Percent $White_c$		0.149***
		(0.046)
Δ Percent H.S. $Grad_c$		-0.010
		(0.011)
Δ Log(Household $Income)_c$		0.000
		(0.000)
Constant	-2.682***	-2.261***
	(0.411)	(0.460)
N	3,104	3,104
R^2	0.209	0.222
State Fixed Effects	√	√

Media Market-level cluster robust standard errors in parentheses. The model on the right includes controls for change in percent white, percent high school graduate, and median household income at the county-level between 2012 and 2008.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A5.2: Effects of Change in Job-Focused Ads in 2012

Appendix 5A6: Excluding Media Market Border Counties

It is possible that households living in counties on media market borders are not 'receiving the treatment,' insofar as they view television content broadcast in neighboring DMA's. In this section, I show that my key results are robust to excluding these counties completely, despite a precipitous drop in the number of observations.

Dependent Variable: Voter Turnout	2010	2012
Jobs Ads_d (1000s) × $Unemployment_c$	-0.054**	-0.068**
	(0.020)	(0.021)
Jobs Ads_d (1000s)	0.593*	0.648**
	(0.259)	(0.225)
$Unemployment_c$	0.822**	0.327**
	(0.171)	(0.110)
Total Ads_d (1000s)	-0.030	-0.087
	(0.088)	(0.060)
Percent $Poverty_c$	-0.464**	-0.126**
	(0.060)	(0.039)
Percent $White_c$	-0.018	-1.282
	(0.016)	(0.930)
Percent H.S. $Grad_c$	-0.025	-0.024^
	(0.020)	(0.014)
Log(Household $Income)_c$	-8.332**	-1.918^
	(1.673)	(1.119)
Lagged $Turnout_c$	0.682**	0.850**
	(0.024)	(0.018)
Constant	100.398**	27.504*
	(19.003)	(12.340)
N	850	850
R^2	0.612	0.820
State Fixed Effects	√	√

Standard errors in parentheses.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A6.1: Job-focused Appeals and Voter Turnout: 2010 and 2012 (Excluding Media Market Border Counties)

Δ = 2012 - 2008	Δ Turnout	Δ Turnout
Δ Jobs Ads_d (1000s) ×	-0.150^	-0.153^
Δ $Unemployment_c$	(0.088)	(0.089)
Δ Jobs Ads_d (1000s)	0.122	0.239
	(0.241)	(0.253)
Δ $Unemployment_c$	-0.049	-0.048
	(0.146)	(0.147)
Δ Total Ads_d (1000s)		-0.089
		(0.062)
Δ Percent $Poverty_c$		-0.043
		(0.070)
Δ Percent $White_c$		0.067
		(0.060)
Δ Percent H.S. $Grad_c$		-0.021
		(0.014)
Δ Log(Household $Income)_c$		-0.000
		(0.000)
Constant	-3.114***	-2.832***
	(0.350)	(0.521)
N	850	850
R^2	0.009	0.017
State Fixed Effects	√	√

Standard errors in parentheses. The model on the right includes controls for change in percent white, percent high school graduate, and median household income at the county-level between 2012 and 2008.
** p<0.01, * p<0.05, ^ p<0.1

Table 5A6.2: Effects of Change in Job-Focused Ads in 2012 (Excluding Media Market Border Counties)