

Foreign Policy as Pork-barrel Spending: Incentives for Legislator Credit Claiming on Foreign Aid

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Abstract

Foreign policy often creates geographically-concentrated domestic benefits. A prominent example is the tying of development aid to purchases from the donor country. This feature of aid highlights the utility in examining foreign policy as an instance of pork-barrel politics. Considering tied aid in terms of legislators' incentives to provide constituent benefits, we argue that people will support an increase in foreign aid spending more when it would promote local economic activity, while opposing aid cuts more when reduced local economic output would result. Crucially, we also expect that people will support their state's US senator more when informed that the senator attempted to secure (or retain) locally beneficial funds. We find support for our expectations in a novel survey experiment of US citizens. Our results suggest that legislators' electoral incentives, and consequential local spending, can help explain the adoption of foreign policies despite national-level public disapproval.

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1 Introduction

Foreign policy often has domestic consequences that in turn incentivize its use. For example, contrary to the popular conception of foreign aid as a handout to recipient states, a considerable proportion of aid spending is tied to purchases from firms and non-governmental organizations (NGOs) located in the donor state. However, the practice of aid tying faces criticism (Easterly and Williamson 2011). Development-minded activists routinely apply pressure on donors to end what they see as wasted spending, as it would be more efficient to purchase goods and services from within the region benefiting from aid rather than importing them from a geographically distant donor (Oxfam 2009).

Common attention to the effects and efficiency of tied aid implies an interest in policy prescription (Easterly and Pfütze 2008). However, research on the politics underlying tied aid is nearly absent.¹ In this paper, we contend that scholars can better understand foreign policy in general, and tied aid specifically, through the lens of distributive politics (Milner and Tingley 2015). Specifically, we direct focus to the fact that a large proportion of foreign aid is composed of locally-targeted spending in the donor country—i.e., pork-barrel spending. We argue that citizens will demonstrate a greater appreciation of government spending on aid when they expect it to have a positive local economic impact. We further contend that politicians will pay attention to these citizen preferences and claim credit accordingly. When local politicians communicate that they managed to secure funds for a specific area, we expect residents of this area to view that politician more favorably (Grimmer 2013; Grimmer, Westwood and Messing 2014; Fenno Jr. 1978). Reelection-minded politicians desire more favorable evaluations, and thus face an electoral incentive to secure local tied aid spending. The localized consequences of this policy create a direct link between public opinion and the motivation of politicians (Gottfried and Trager 2016).

To test whether legislators face an incentive to secure local tied aid spending, we design a survey experiment that examines how communications regarding local foreign aid spending

¹For a notable exception, see McLean (2015).

affect individual evaluations of their legislators. We present respondents with a vignette announcing a change to foreign aid outlays that results in either an increase or a reduction in spending within a specific locality. Randomization determines whether this change affects the nearest large city within the respondent’s home state, or a populous city elsewhere in the United States. We also randomly vary whether a member of the US Congress (specifically, a US senator) is mentioned as working to secure the increased funds or trying (and failing) to stop funding cuts. We then examine consequential variation in individual support for aid policy and approval of local US senators.

Our results show evidence for both aspects of our arguments. Individual support for additional aid spending is higher when such spending would have a beneficial local impact; and opposition to aid cuts increases by a comparable amount when these cuts entail local losses. Respondents also express warmer feeling thermometer scores toward senators who are shown to have worked (successfully) to secure funds or (unsuccessfully) to prevent cuts in locally-targeted funds. To explore the electoral implications of our findings, we use a rough, back-of-the-envelope calculation on observational data, finding that improvement in a US senator’s evaluation in line with our experimental results translates to a modest increase in the proportion of people who vote for the sitting senator.

Our argument links public opinion to the practice of tied aid via focus on legislator incentives. Considering foreign policy attitudes and the associated evaluation of legislators in the context of pork-barrel politics has important implications for theory and policy. Scholars typically consider the domestic determinants of foreign policy instruments—including trade policy, sanctions, and military intervention—in terms of national-level ideological or class cleavages. However, these policies all have locally-salient effects to which individuals, and their elected representatives, prudently respond (Milner and Tingley 2015). To our knowledge, these local implications have been studied only with respect to casualties from armed conflict (Kriner and Shen 2014), terrorism (Avdan and Webb 2019), defense spending (Thorpe 2014), democracy promotion (Christiansen, Heinrich and Peterson 2019), and

immigration (Hangartner et al. 2019; Ferwerda, Flynn and Horiuchi 2017).

We propose that a focus on the domestic political geography of foreign policy would improve the study of domestic and foreign policy processes. Our approach can help explain why tied aid, as well as foreign aid more broadly, remains a popular foreign policy tool despite aggregate public disapproval; while the aid policy process remains fundamentally democratic, the role of legislators in designing policy hints that aggregated, national public opinion obscures a more locally-focused process. The causal mechanism we develop also complements recent work on the role of identity in foreign policy preferences (Mansfield and Mutz 2009; Mutz and Kim 2017), as we consider a bounded sociotropic process in which people favor gains for those “like them,” but where recognition of likeness is a function of common residence in a sub-national locality.

A better understanding of political incentives in foreign policy also holds implications for the academic debate on the efficiency of tied aid as well as the activist movement to end it. Our argument suggests that the election-minded impetus behind (tied) aid renders immaterial the consideration of whether the welfare of nominally targeted poor recipients is improved. While we do not claim that legislators, constituents, and US contractors prefer inefficient aid *per se*, the institutional incentives facing legislators in (at least some) donor countries explain why we see tied aid irrespective of its likelihood of achieving stated goals.² Activists wish to lower—or eliminate—the share of aid that is tied. By showing that people appreciate local spending on foreign aid and that they view a politician securing such funds more favorably, our results provide an explanation for why activists’ vocal critiques are unlikely to succeed. Even the most ethically-minded and empirically-supported arguments are likely to fail when they clash with the re-election motives of key players in foreign aid policy (Acemoglu and Robinson 2013).

Our analysis also suggests that tied aid might constitute a necessary side-payment to legislators that actually sustains a greater aggregate commitment of (untied) aid. As aid

²Of course, this does not preclude other incentives to make legislators care about effectiveness (Bush 2015).

is often nationally unpopular in the US but relatively popular among key constituents in some US states, effective logrolling can result. If not for local contracts and the credit claiming opportunities they afford legislators, perhaps a legislative constituency for aid would break down. In more general contexts, we know that executive agencies understand these dynamics and actively work to keep their own political, legislative base supplied (Lazarus 2009; Thorpe 2014). While the legislative coalition for US foreign aid appears stable and systematic (Milner and Tingley 2010), part of the stability may be rooted in the provision of tied aid.

However, our entire analysis has a scope condition that becomes relevant when looking beyond the United States. The incentives to secure local aid exist only when legislators must listen to their local constituents and seek credit for local achievement. Across major democracies, which provide the bulk of foreign aid, there is wide variation in national vs. local legislator orientation (Carey and Shugart 1995), with implications for foreign economic policies (Crisp et al. 2010). When national parties are strong, for example, legislators will cater (more) toward national level priorities, which have been discussed at length in the aid literature (Heinrich 2013). As such, activism against the use of tied aid might be most effective where legislators do not face individual credit claiming incentives.

2 Distributive Politics and Foreign Policy

Given that every policy creates winners and losers, distributional conflict is a ubiquitous feature of politics. While this phenomenon has long been recognized within the realm of domestic policies such as health care or taxes, scholars have only recently begun viewing foreign policy in this manner. For example, trade and migration policy affects returns to skills and education investments; and governments' pursuit of war kills soldiers and rattles families at home.³ For large donors of foreign aid such as the United States, a similar

³See, among many, Rogowski (1987); Hiscox (2001); Hainmueller and Hiscox (2006, 2010); Mansfield and Mutz (2009); Milner and Tingley (2011); Kriner and Shen (2014); Paxton and Knack (2011); Fordham and Kleinberg (2012); Rho and Tomz (2017); Milner and Tingley (2015); Mutz and Kim (2017).

attention to domestic politics is warranted. Indeed, the stated aim of US foreign aid is not only to facilitate development and democracy around the world, but also to promote American security and prosperity.⁴ Attention to which donor citizens benefit personally from policies ostensibly aimed at increasing recipients' prosperity is likely to improve our understanding of how such policies work.

Distributive politics is evident in the common focus on individual interests as understood via membership in a variety of groups. For example, divisions in education, skills, and income shape policy attitudes on trade policy (Fordham and Kleinberg 2012) and aid policy (Milner and Tingley 2011; Paxton and Knack 2011, but see Rho and Tomz 2017). Political economists view this behavior in line with the Stolper-Samuelson model, wherein competition exists between holders of different factors of production—typically capital versus labor, or the Ricardo-Viner model, where competition falls between industry sectors. Yet, despite some evidence for this form of unorganized interest group representation, the link between economic self-interest and trade policy preferences has been questioned (Hainmueller and Hiscox 2006; Mansfield and Mutz 2009). More recently, studies suggest that this apparent economic self-interest could be a function of sociotropic views on the economy and broader views of foreigners (Mansfield and Mutz 2009; Hainmueller and Hiscox 2010; Mutz and Kim 2017). While less-educated and low-skilled citizens generally resist imports of foreign goods and oppose immigration, recent work suggests that a broader negative view of outsiders and a desire for (national) compatriots to “win,” could be responsible (Mansfield and Mutz 2009; Mutz and Kim 2017). These studies could imply a utility in applying social identity theory to the study of distributive politics (Tajfel and Turner 1979), as homophily with fellow in-group members could shape an individual's beliefs more-so than economic incentives. Yet homophily could extend, perhaps even more strongly, to more localized areas. Indeed, research finds that legislators, who are typically aware of group identities, will vote for policy

⁴The USAID website states that the agency “advances US national security and economic prosperity, demonstrates American generosity, and promotes a path to recipient self-reliance and resilience” and that its “efforts are both from and for the American people” (United States Agency for International Development 2018*b*).

they expect their local constituents to prefer (Broz and Hawes 2006; Milner and Tingley 2010, 2015).

Unfortunately, this emerging line of study has largely ignored a seminal aspect of distributive politics: pork-barrel spending,⁵ the allocation of localized public goods (Stokes et al. 2013). Dixit and Londregan (1996) classify pork-barrel spending as form of tactical allocation of resources, distinct from programatic allocation that relies on ideologically-based and (typically) long-term strategic redistribution. Pork-barrel spending is used to secure a “personal vote” for legislators (Carey and Shugart 1995), as constituents approve of—and reelect—legislators who use government spending to provide development and services to their district (e.g., Mayhew 1974; Ferejohn 1974). Reward does not necessarily depend on the legislator actually or actively securing the spending; clever credit-claiming in communications to constituents suffices (Grimmer 2013; Grimmer, Westwood and Messing 2014).⁶ Pork-barrel spending is particularly attractive for more vulnerable legislators—i.e., those whose districts are more competitive, with a greater number of opposition partisans, as in these cases localized spending provides a means of securing support among citizens who disagree with the legislator ideologically (Stein and Bickers 1994; Sellers 1997; Lazarus 2009).

Considering pork-barrel politics with respect to foreign policy is perhaps initially counter-intuitive due to the often national scope of foreign policy debates. However, pork-barrel politics is concerned fundamentally with the (local) distribution of material benefits; and scholars have long understood that economic interests are a primary driver foreign policy (Baldwin 1985). Economic interests influence the use of a wide variety of foreign policy tools including military interventions (Fordham 2009), military spending (Thorpe 2014), sanctions (McLean and Whang 2014), and foreign aid (Alesina and Dollar 2000; Fleck and Kilby 2006;

⁵At least in the case of the United States, one might assume that absence of attention to pork-barrel spending is a consequence of the fact that earmarks have been officially banned in the US Congress. However, the ability of legislators to secure local spending remains; and, indeed, localized spending from policies such as foreign aid could be even more important now that traditional earmarks are not an option.

⁶These studies note that repetition of communications increases a legislator’s personal vote.

Heinrich, Kobayashi and Bryant 2016; McLean 2015). As discussed above, there is evidence that (anticipation of) the economic impact of foreign policy influences individual attitudes. Thus, when material costs or benefits are geographically localized, legislators would face an incentive to engage in tactics associated with pork-barrel politics.

Despite the straightforward idea that the distribution of material benefits to specific localities helps cultivate a personal vote for incumbents, few studies consider it. The closest relevant literature examines the “not in my back yard” (NIMBY) effect, which describes a scenario in which citizens oppose a policy that would (potentially) provide a public good in the aggregate, while imposing costs locally. Several studies have applied this concept to the study of citizen support for armed conflict: opposition to war tends to be highest among citizens who experienced greater casualties in their home towns (e.g., Gartner, Segura and Wilkening 1997; Gartner and Segura 2000). These attitudes spur legislators to act as Kriner and Shen (2014) show. A recent study finds that individuals demonstrates greater opposition to sanctions that would affect a local city, while showing greater support for aid that benefits a city near them (Christiansen, Heinrich and Peterson 2019). However, no study has yet considered legislative incentives to provide—and claim credit for—localized economic benefits associated with foreign policy.

3 Foreign Aid as Pork

Foreign aid is a classic example of a foreign policy tool that persists despite public disapproval (Milner and Tingley 2013). Though classic work suggested that citizen ignorance on foreign aid explains why public opinion on the policy is irrelevant (Almond 1950; Lippmann 1955), we suggest that the politics of foreign aid can be better understood by moving away from a national-level theoretical orientation. Foreign aid—particularly tied aid—provides an important case of a foreign policy tool that can be understood better in the context of pork-barrel politics. Foreign aid is often made contingent on the provision of goods and services sourced within the donor state; even funds disbursed directly to the recipient country

often are stipulated only for purchases from the donor. Despite calls by activists and international organizations to eliminate this practice, which is considered costly and ill-suited to achieve the nominal goal of development aid (e.g., Oxfam 2009; Danish Institute for International Studies 2009), considerable variation remains in the use of aid tying. While the United Kingdom has untied all of its aid, others, and most notably the United States, have not followed suit (Danish Institute for International Studies 2009). In 2016, 13% of OECD aid was formally tied. The same share stands at 34% for the United States.⁷ However, as McLean (2015) demonstrates, the share of *de facto* tied aid can be considerably larger than the share of formally tied aid.

Given the persistent debate regarding whether tied aid improves welfare in the recipient state, it is important to consider why donors would employ this policy tool. After all, without understanding what leads to tied aid in the first place, activists may target the wrong levers to effect change (Acemoglu and Robinson 2013). While studies focus specifically on the political or economic interests underlying donors' decisions to allocate foreign aid (Alesina and Dollar 2000; Palmer, Wohlander and Morgan 2002; Hook 1995; Boutton and Carter 2014; Peterson and Scott 2018), the decision by donors specifically to tie aid rather than utilize untied aid generally is ignored or relegated to assumption. A naive first attempt might simply assume that donors tie aid because they favor enriching their firms. However, such an assumption is reductionist given that a minuscule proportion of donor firms benefit from tied aid contracts; and this simplifying assumption ignores large cross-national variation in the share of aid that is tied.

We center our inquiry on the political geography of aid contracts and view it under the well-established lens of pork-barrel spending. For the domestic location of foreign aid spending to affect foreign policy, people must be aware of spending in their home area, credit and then reward the elected official who is responsible for delivering the funds. In the context of our study, these are legislators who have the “power of the purse” (Fenno Jr.

⁷Calculated from DAC7b at <https://stats.oecd.org>.

1966). As noted above, studies on credit-claiming provide a means by which this mechanism could operate (Grimmer 2013; Grimmer, Westwood and Messing 2014).

One possible explanation for the lack of studies considering foreign policy—particularly foreign aid—with respect to pork-barrel politics is that early observational work found no systematic evidence that the accrual of associated benefits in a legislative district influence the legislator’s likelihood of voting in favor of the policy (Fleck and Kilby 2001). However, this prior examination was cursory, ignoring the potential for logrolling to occur—despite the fact that logrolling is central to some theories of pork-barrel politics (Mayhew 1974; Fenno Jr. 1966; Weingast and Marshall 1988).

Furthermore, while Fleck and Kilby (2001) are correct that some foreign aid spending benefits nearly every state in the United States, their analysis could suffer from lack of attention to the fact that the vast majority of aid-related domestic spending occurs in relatively few states. For example, in 2015, over 90% of foreign aid-related projects occurred in nine states plus the District of Columbia (as shown in the supplemental appendix). Finally, in their analysis of Congressional voting, Fleck and Kilby (2001) acknowledge that they do not test whether legislators face an incentive to seek aid spending in their districts, leaving open the question of whether aid spending could improve constituent evaluation of their legislators and facilitate the cultivation of a personal vote. More recent research suggests that legislators will vote for foreign aid projects expected to benefit their constituents (Broz and Hawes 2006; Milner and Tingley 2010, 2015), though these studies do not consider the specific location of domestic foreign aid spending as a potential cause.

4 Local aid, credit claiming, and public opinion

Commitments of foreign aid result from the actions of legislators who write and subsequently vote on spending bills; aid agencies subsequently manage the details. Assuming that reelection is the primary driver of legislative behavior, we contend that foreign aid bills will be

written in order to advance this goal for a subset of legislators.⁸ Although this end could be served by writing aid bills to satisfy (potential) campaign donors, we focus on legislator behavior in anticipation of constituent response to the policy (upon becoming informed via credit-claiming activities).

Writing bills with specific instructions is only one way through which a legislator may secure localized spending. In the US case, many legislative appropriations are vague, with spending details to be decided by the agency. Legislators continue to seek funds by contacting agencies (Lowande 2018). While legislative voting requires the management of larger coalitions, a single legislator may request funds from the United States Agency for International Development (USAID) easily and without drawing the opposition party's ire. Legislators also can support the efforts of interest groups, which continue to lobby after the legislative vote (Haeder and Yackee 2015; You 2017). Lowande (2018) gives the example of a legislator from Wisconsin convincing USAID to provide additional funding to go to a local university.⁹

In principle, legislators might consider writing aid bills and/or voting for aid if constituents are composed primarily of more educated and wealthier individuals perceived to benefit from the policy, and/or more likely to support common aid missions including development or democracy promotion (Milner and Tingley 2010). Yet, it is unclear that a strong incentive exists to reward legislators for securing such programmatic aid spending because, particularly in competitive districts, disagreement often exists. We contend that the provision of constituency-specific benefits via local sourcing of aid is likely to secure wide constituent support, with only the staunchest (and most principled) opponents to government spending disapproving (Grimmer, Westwood and Messing 2014). A legislator then can credit claim for the benefits that accrue to the district. We expect appreciation of these local interests to be so strong that *individuals will espouse greater support for aid as a policy when informed of the local benefits of this policy (or the local costs of its termination)*.

⁸The bills in question need not benefit even a majority of legislators given the potential for logrolling across issues (Mayhew 1974; Fenno Jr. 1966; Weingast and Marshall 1988).

⁹Honig (2018) demonstrates how USAID has structured its inner workings to comply with congressional demands. See also Bush (2015).

Although some district residents might benefit directly from local spending on foreign aid, we contend that legislative support follows not (exclusively) from economic considerations but rather from a sociotropic—yet geographically localized—preference to benefit one’s community over other domestic locations. While individuals might vary in preexisting attitudes on a variety of foreign policy tools including foreign aid, we contend that they will be more supportive of the policy on average upon learning that it will result in local economic benefits. Given the sociotropic nature of this mechanism, it is critical to consider the means by which constituents become informed of pork-barrel aid spending. Press releases, often sent by legislators for credit claiming purposes, provide a venue for individuals to learn of local spending and reward their legislator for it (Fenno Jr. 1978; Grimmer 2013; Grimmer, Westwood and Messing 2014). Ultimately, however, the source of the information might be less important than the information itself. For example, USAID regularly issues press releases to advertise the domestic benefits—including local spending—associated with foreign aid commitments.¹⁰ Indeed, USAID faces incentives to help legislators with credit claiming in order to secure sufficient support in Congress (Fleck and Kilby 2001; Bertelli and Grose 2009). Press releases by USAID frequently advertise domestic locations that benefit from foreign aid contracts (for a recent example, see United States Agency for International Development 2018*a*), and often mention legislators by name (for example, see United States Agency for International Development 2011).

We argue that legislators experience an incentive to engage in credit-claiming on foreign aid spending.¹¹ An individual’s evaluation of her legislator will increase when she is presented with information that the legislator secured localized foreign aid spending. However, there are actually two specific mechanisms to consider. First, an individual could increase support for all of her local elected officials upon learning of local economic benefits; i.e., the individual

¹⁰USAID press releases are comparable in frequency to those by US senators. For example, Grimmer (2013, 627) notes that the average senator releases 212 press releases per year. In 2017 and 2018, the two most recent full years, USAID released 154 and 248 press releases, respectively.

¹¹Our arguments apply to most donors, minimally if they are democratic and more-so when electoral institutions favor locally-oriented distributed politics. This is the case when states have single-member districts. However, other conditions affect this incentive as well. See Carey and Shugart (1995).

rewards success. Second, an individual could increase support for a legislator upon learning that the legislator expended effort to aid the district. We consider both of these mechanisms simultaneously. Specifically, we expect that *an individual will rate a legislator more favorably when informed of local material benefits (as opposed to material benefits that accrue elsewhere in the United States) from new foreign aid contracts—but that the magnitude of this effect will be greater when the individual is presented with additional information that the legislator worked to secure these benefits.* Extending this logic, we also consider the case in which foreign aid contracts are cut. We argue that *an individual will decrease support for her legislator when informed of aid cuts that will result in local costs (as opposed to costs incurred elsewhere in the United States), but that this effect will be weaker when the citizen is informed that her legislator tried—but failed—to stop the aid cuts.*

5 Research design

We test these expectations with an original survey experiment. Our vignette informs respondents of changes to foreign aid outlays, which affect some domestic contractor who provides goods or services to a foreign country as part of a foreign aid project. We seek to isolate the degree to which the locality of the affected US contractor, in conjunction with the presence or absence of information regarding senator intervention, shapes respondent support for the foreign aid policy and affects feelings toward their local senators. In order to test our hypotheses, we create experimental conditions that could, but need not necessarily, involve mention of legislator intervention. Accordingly, we design a scenario in which respondents read a fictional press release by USAID.¹² As noted above, USAID issues nearly as many press releases as the average senator; and knowing that its funding depends on legislative support (Fleck and Kilby 2001), USAID often mentions legislators who worked to secure

¹²The hypothetical, fictitious nature of the press release is emphasized before participants see the vignette as well as in the debriefing. Specifically, we state at the end that the press release was “fictional” and “designed to help us understand how citizen attitudes regarding foreign aid depend on whether the policy has a home-state impact, as well as how citizen support for elected officials depends on whether that official obtains funding for the home state.

aid spending, as well as the domestic locations that benefit from aid projects. While mock press releases from legislators are likely more common in the credit claiming literature, the information that is presented to respondents ultimately is more important than its source.¹³ Furthermore, rational legislators would not inform their constituents about beneficial spending outside their jurisdiction, and would likely avoid mention of cuts to local spending. Our focus on agency press releases thus maximizes the realism of the vignettes.

5.1 Vignette

Two examples in Figure 1 depict two realizations of our vignettes. The left side presents a case in which aid is increased, touting the award of new contracts to a contractor who will execute a project sponsored by USAID. Specifically, the story focuses on contracts to “purchase equipment and consulting services” for “implementing infrastructure development projects” in two randomly drawn sub-Saharan Africa nations.

In this realization, additional aid spending benefits St. Petersburg, FL. With a probability of one half (0.5), a respondent sees a press release relating to his or her self-identified home state. If the home state is selected, the city closest to the respondent-provided ZIP code is assigned.¹⁴ If the case receives a non-local treatment (i.e., the state discussed in the vignette is not the respondent’s home state), then a large city from some other state is randomly drawn and used. With this approach, we generate our first treatment, namely whether a press release refers to a local or non-local impact of foreign aid.

The second treatment is whether new aid contracts are awarded or existing ones are cut. Complementing the left-side example of an aid increase, the right side of Figure 1 presents

¹³As such, we think that the use of a newspaper article format would not change the results we obtain. However, we use the press release format to increase similarity with recent work (e.g., Grimmer, Westwood and Messing 2014), while changing the source of the press release to USAID in order to improve validity (as, in reality, local members of congress are unlikely to distribute press releases about distant locations and legislators).

¹⁴We use a list of cities that features each state’s largest city and all cities with populations greater than a population of 400,000. The full list is in the appendix, p. A.4. We pre-calculated the geographic distance between many ZIP codes and each city so that we could assign the nearest large city (in the home state) to each person (in the local treatment condition). If a person’s ZIP was not in our dataset (in the local treatment case), then we approximated the nearest city by locating the “closest” ZIP code in the dataset and using that ZIP code’s nearest city. We had to use this approximation in only seven of 841 local cases.

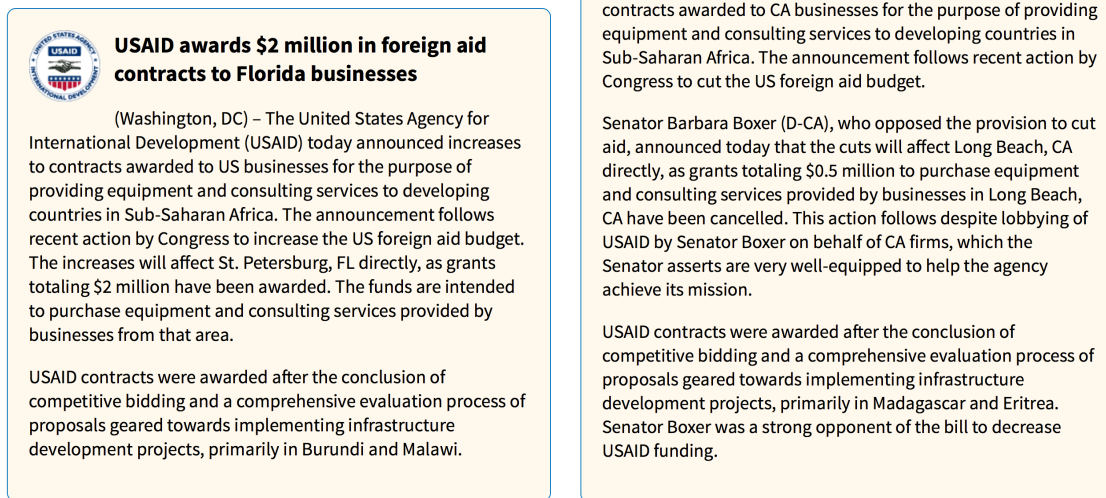


Figure 1: **Examples of vignettes.** The left vignette presents a case in which new funds for aid are allocated, which a contractor in St. Petersburg, FL won. No politician is mentioned as being active in this process. In the right vignette, existing contracts are canceled (which initially had gone to Long Beach, CA), and a senator is mentioned as having intervening (but failing) to prevent this from happening.

an example in which existing aid is cut. In either case, the changes emanate from general action in US Congress.¹⁵

The third randomization of interest is whether a senator is mentioned to have taken action either to secure aid spending or to prevent cancellation of an existing contract (depending on which realization of the second treatment the respondent receives). We view these actions as examples of senator behavior to secure funds for the home state (Grimmer, Westwood and Messing 2014). The left vignette in Figure 1 presents a case without senator intervention, whereas the right vignette presents a case where a senator tried (in vain) to prevent the cancellation of contract.¹⁶ If a senator mention occurs, we randomly assign one of the two US senators serving in the state assigned in our first treatment. While wording naturally

¹⁵We note that “competitive bidding” occurred over the contract, alleviating potential for cases that mention a senator to be perceived as being rigged.

¹⁶We randomize the volume of the contract between \$0.5m, \$1.0m, and \$2m.

various slightly across cases of aid increases versus aid cuts, the respondent learns in both cases that a senator attempted either to secure, or to retain, aid spending.

Below the vignette, we ask respondents to supply information on our first outcome of interest: “What do you think about the [cut/ increase] to USAID contracts? Please choose the option below that best reflects your view.” Respondents could choose from 1–9 scale with 1 labeled as “strong opposition” and 9 as “strong support.” The middle category is indicated at “neutral.” This question lets us capture the effects of our main treatments on respondent attitudes regarding aid projects.

Our second outcome of interest is whether a senator’s intervention affects voter feelings towards him or her. On the page following the vignette and the question about the project evaluation, we ask respondents to report a feeling thermometer score for both senators from their self-identified home state.¹⁷ Respondents are asked to enter the value; and we apply a check to confirm that an integer in the 0-100 range is entered. We ask for home-state senator evaluations irrespective of whether a senator is mentioned in the vignette, and irrespective of whether a vignette-mentioned senator serves the respondent’s home state or a distant state.

5.2 Sample

In Fall 2016, we posted a job on Amazon’s MechanicalTurk (MTurk) platform, seeking participants in a short study on attitudes about US foreign policy. 1,680 respondents participated. In order to improve external validity of our estimates, we restricted the availability of the job to people living in US states with substantial foreign aid contracts. Using USAID contracts for the 2015 fiscal year, the top ten recipients of USAID contracts (by count) are: District of Columbia (D.C., 599 projects), Virginia (192), Maryland (108), Massachusetts (30), New York (23), California (20), Florida (20), North Carolina (18), New Jersey (12),

¹⁷The wording is: “We would like to know your feelings towards some political leaders using something we call the feeling thermometer. Please choose a number between 0 and 100 where 50 indicates that you are neutral (not warm or cold) towards the individual, while numbers closer to 100 indicate more warmth, and numbers closer to 0 indicate more coldness.”

and Texas (11). However, we excluded the District of Columbia because it does not have senatorial representation in the US Congress. Cumulatively, the other nine states account for 85% of aid projects in the United States that are not going to Washington, DC.¹⁸ Figure A.4 in the appendix presents summary graphs regarding the cumulative distribution of USAID projects across states.¹⁹

It is well-known that survey takers from MTurk differ from the US population (Berinsky, Huber and Lenz 2012; Huff and Tingley 2015). Our demographic statistics suggest that our sample is no exception, considering our nine US states of interest. In Section A in the appendix, we see that our survey takers are more likely to be male, younger, more ideologically liberal, and more educated than the broader population of the nine states. We discuss below how we address this form of bias.

5.3 Statistical analysis

When examining support for tied aid, we conduct separate analyses for two subsets of our sample, which we split into cases of aid increases and cuts (i.e., our second treatment). The main predictor of interest is a dummy variable capturing the presence of a local (rather than distant) impact of changed aid spending.²⁰ We also include random intercepts for each of the nine states in our sample.

To improve estimation precision as well as to allow for post-stratification (see below), we also include a set of covariates capturing demographic characteristics. Specifically, we control for age, gender, a dummy indicating low education (below 2-year college), a dummy for high education (4-year college and above), and a seven point liberal-conservative scale.²¹ We

¹⁸By contract volume, we are capturing 94% of non-D.C. 2015 USAID funds. The number and volume of contracts per state (including Washington, D.C.) correlate at 0.68.

¹⁹We checked whether the self-identified state of residence is actually the home state with which respondents identify. We asked respondents to report how long (in years) they had been living in the current state and whether they consider their current state of residence to be “home.” Respondents on average lived 72% of their previous years of life in their current state; and 93% of respondents called their current state of residence “home.”

²⁰In the appendix, we show that interacting the local dummy with an indicator of whether any senator was shown as part of the vignette does not modify the treatment.

²¹The question wordings for this indicator are taken from the Cooperative Election Survey 2016 (Vavreck and Rivers 2008).

also include additional features of the vignette as predictors: dummy variables identifying the cost of the project in question, a dummy variable indicating whether a Democratic or Republican senator was shown to have intervened,²² and the interactions between ideology and each of the senator dummies. The latter capture ubiquitous partisan cue-taking effects and should improve model fit considerably.

We model support for aid projects using an ordered probit, and calculate changes in predicted probabilities that people choose each of the nine outcome levels. The feeling thermometer outcomes are integers between zero and 100. Since 8% and 3% of the feeling ratings are the minimum and maximum, respectively, some modest censoring might be at play. Therefore, we use a doubly-censored regression (ie. tobit) model. The analysis of senators feeling thermometer scores requires a slight modification given that treatments are slightly more complicated. Specifically, though we have feeling thermometer scores for both local senators, at most, only one senator intervenes in favor of tied aid spending. Accordingly, when estimating support for a given senator, we include three key dummy variables: local project without senatorial intervention; local project with intervention from *this* senator; and local project with intervention from the *other* senator in the state. Distant projects serve as the omitted category.²³

Whereas the location of the aid project is expected to matter differently depending on whether aid is cut or increased for the support of the project, this directional distinction need not be at play when considering feelings toward senators. In our vignette, senator interventions in cut and increase cases are portrayed as something beneficial for the people in the particular state: the person helped secure new funds or worked to prevent the cut of existing contracts. Thus, we consider a pooled analysis for the feeling thermometer scores, incorporating cases of aid increase as well as cuts, while controlling for the direction of the aid change; failed attempts to prevent local cuts might be rewarded less than successful

²²These do not perfectly predict each other as no senator may be depicted.

²³We also include an additional random effect for each survey taker (who evaluates both senators), capturing heterogeneity across respondents.

attempts to secure local funding.

The non-representativeness of our MTurk sample likely affects the magnitudes of the project and feeling thermometer estimates. While studies show that samples from MTurk replicate standard experimental results at least qualitatively (Berinsky, Huber and Lenz 2012; Mullinix et al. 2015), we use post-stratification to match our sample’s moments to the nine state sub-populations. This correction based on pre-treatment and other experimental variables should help bring the magnitudes of treatment effects in line with those that would be observed in the population (Hainmueller, Hangartner and Yamamoto 2015). We use data from the 2016 Congressional Cooperative Election Survey (Vavreck and Rivers 2008) to post-stratify.²⁴

We estimate all models using a Bayesian approach to inference, specifically relying on the implementations in MCMCglmm (Hadfield 2010). Details can be found in our replication package; there were no signs of non-convergence for the 40,000 iterations after a burn-in period of 20,000 draws. The Bayesian approach is advantageous here as it allows us to calculate naturally the posterior distribution of all functions of the parameter estimates, such as the post-stratified substantive estimates.²⁵

6 Results

First, we discuss results of our analysis for aid project evaluations. Table 1 presents the estimated model parameters, which reflect the results prior to post-stratification. The first column presents results for all cases in which aid was increased, while the second column gives the cases where aid was cut. We present the posterior mean with the corresponding 95% central credible interval in brackets. Though Table 1 presents coefficients for all included variables, we focus our interpretation on the treatment variables as the additional covariates

²⁴See Wang et al. (2015); Gelman and Little (1997). We marginalize over other non-demographic, vignette-related predictors as well as over the respondent and state random effects.

²⁵The appendix presents an examination of whether our randomization was successful. In short, we find strong evidence of balance across treatment realizations. Further, the distance from respondent to the city mentioned in the vignette varies as expected. The appendix also presents maps visualizing our sample (Kahle and Wickham 2013).

have no substantive interpretation.

	Increase aid	Cut aid
Local project	0.46 [0.27; 0.66]	-0.48 [-0.68; -0.28]
Education: high	0.22 [0.00; 0.43]	0.02 [-0.19; 0.23]
Education: low	-0.13 [-0.49; 0.23]	-0.17 [-0.55; 0.20]
Age	0.09 [-0.70; 0.91]	1.10 [0.32; 1.88]
Gender: female	0.14 [-0.07; 0.34]	0.02 [-0.18; 0.22]
Ideology	-1.19 [-1.65; -0.73]	1.02 [0.56; 1.49]
Shown R Senator	-0.33 [-0.87; 0.20]	-0.18 [-0.65; 0.31]
Shown D Senator	0.09 [-0.27; 0.43]	-0.50 [-0.84; -0.17]
Ideology x Shown R Senator	0.54 [-0.54; 1.59]	-0.06 [-0.96; 0.85]
Ideology x Shown D Senator	0.14 [-0.56; 0.86]	0.34 [-0.35; 1.03]
Costs: 1.5m	0.01 [-0.23; 0.25]	0.02 [-0.22; 0.26]
Costs: 2.0m	-0.11 [-0.36; 0.13]	0.14 [-0.10; 0.37]
Intercept	2.56 [2.09; 3.02]	1.87 [1.42; 2.32]
Cut point 1	0.58 [0.44; 0.75]	0.86 [0.71; 1.01]
Cut point 2	1.07 [0.90; 1.27]	1.59 [1.42; 1.76]
Cut point 3	1.55 [1.37; 1.77]	2.22 [2.05; 2.41]
Cut point 4	2.37 [2.18; 2.58]	3.19 [3.03; 3.42]
Cut point 5	2.97 [2.78; 3.17]	3.58 [3.40; 3.79]
Cut point 6	3.82 [3.61; 4.02]	4.15 [3.94; 4.35]
Cut point 7	4.69 [4.41; 4.88]	4.79 [4.57; 5.05]
Observations	832	848

Table 1: **Support for project; model parameters for ordered probits. Basic specification.** First number gives the point estimate, the range below the 95% confidence interval. MCMCglmm was used to estimate the models.

The coefficient for the dummy variable indicating that an aid project is local (i.e., matched to the respondent’s nearest large city) is positive for aid increase cases (0.46 [0.26, 0.66]). That is, on the latent scale of the ordered probit, respondents show greater support for projects with funds going to locations (on average) 47 miles from the respondents home. Conversely, as expected, aid cuts that would affect a company located in the respondent’s

home state leads to a decrease in support (-0.48 [$-0.68, -0.29$]) on the latent scale.

Though we can evaluate the posterior density of the coefficients, it is more difficult to discern substantive (post-stratified) effects from a glance at a table. These effects depend upon the realization of all other covariates, their respective coefficient estimates, and the cut-points, all of which are evaluated within two non-linear functions (ie. ordered probit), and then differenced. Further, addressing the non-representative of the MTurk data, we apply post-stratification. Thus, we demonstrate substantive effects by simulating treatment effects that marginalize over the distribution of covariates from the CCES (as well as over the other variables relating to the experimental design).

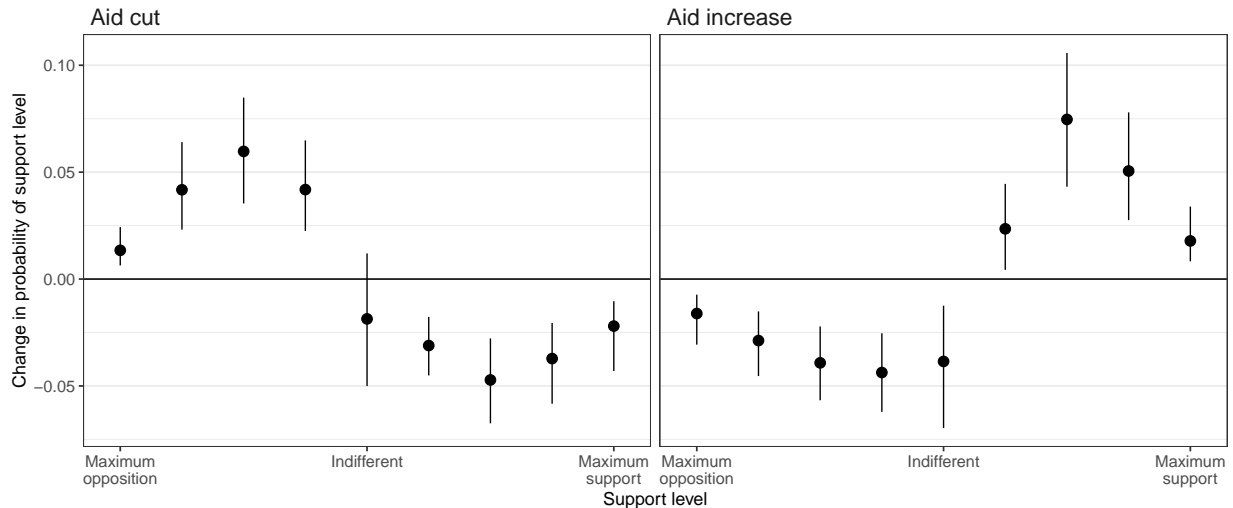


Figure 2: **Substantive effects for local effects on project evaluations.** Each panel shows possible levels of support for the USAID aid policy; and the y-axis gives the change in probability when going from a non-local to a local case. The left panel shows the effects for aid cut vignettes, the right panel for increases in aid.

Figure 2 provides a graph of these substantive effects. Each panel denotes along the x-axis the nine possible values of respondent support for the aid policy, from “maximum opposition” to “maximum support.” The y-axis indicates the change in probability of each value given a change in the treatment from non-local to local. Each dot illustrates the median estimate, while the line plots the 95% central credible interval. The left-hand panel presents results for the case of an aid cut; the right-hand illustrates results in the case of an

aid increase.

Support for cuts to aid declines when the project has local ramifications. The probabilities for the highest four levels of opposition increase by a total of 0.16 [0.09, 0.23]; and the four highest expressions of support decline by -0.14 [-0.29, -0.09]. Turning to cases of aid increases, we see the opposite pattern: opposition to an aid increase declines (-0.13 [-0.18, -0.07]), while support increases (0.17 [0.10, 0.24]). Here, even the middle, “indifferent” category becomes less likely when the aid project provides local economic benefits.

The results presented above control for senator intervention, but do not account for the possibility that respondents presented with information that a senator intervened to secure aid grants, or attempted to prevent aid cuts, might condition the effect of a local policy impact. As a robustness check, we interact the locality treatment with a dichotomous indicator of whether a senator is mentioned as having intervened. While the magnitudes of substantive effects change slightly in these models, the qualitative effect is consistent.

Next, we examine how respondents’ feelings toward their senators are affected by the location of aid changes and the involvement of senators. As the model is again non-linear (tobit) and we have differences between different dummies, the coefficients themselves are not easy to evaluate. We relegate the full set of coefficients to the appendix (Table A.2). Rather, we present substantive effects. Table 2 presents the estimates of three comparisons. We show the expected feeling thermometer score for a senator who has intervened when project changes affect the respondent’s local area to three other cases: 1) where the project has a non-local impact and no senator is mentioned, 2) when the project is local but no senator intervened, and 3) when the project changes have a local impact and the other senator intervened. As the results of these comparisons are not readily gleanable from coefficient estimates, we present the full set of results in the appendix (Table A.2).

Intervening on behalf of constituents pays for a senator; respondents have a more favorable (mean) feeling thermometer score of senators when informed of that senator’s intervention amid changes in aid projects affecting their home state. We find a consistently positive

	Any change	Increase aid	Cut aid
Intervened versus non-local	2.66 [-0.25; 5.53] 0.96	4.02 [-0.15; 8.29] 0.97	3.21 [-0.71; 7.18] 0.95
Intervened versus local, no senator	3.98 [0.62; 7.33] 0.99	7.28 [2.39; 12.37] 1.00	3.15 [-1.33; 7.70] 0.92
Intervened versus local, other senator	0.31 [-1.79; 2.40] 0.61	0.05 [-3.03; 3.15] 0.51	1.50 [-1.22; 4.16] 0.86
Observations	3360	1664	1696

Table 2: **Feeling thermometer scores for senators; treatment effects.** First number gives the mean estimate of the comparison stated on the left, the range below the 95% credible interval. The number below the credible interval gives the posterior probability of the effect being greater than zero. All parameter estimates of the statistical models are shown in Table A.2. MCMCglmm was used to estimate the models.

effect relative to two comparison cases: when an aid project change affects some distant location in another state and where aid changes are local but there is no senatorial action. These results are illustrated in the first two rows in Table 2. In each case, the posterior probabilities of a positive effect is 0.92 or greater. The magnitudes range from 2.66 to 7.28 points (mean), or about 9–25% of the standard deviation of the feeling thermometer scores.

As the table shows, the only scenario in which we do not find a “significant” increase in senator thermometer evaluation following from local intervention is when comparing this case to one in which the other senator intervened. Posterior probabilities drop to 0.65 and 0.78. When the colleague secured funds, intervening increases the feeling thermometer noisily. However, this result also suggests that a senator (noisily) gains even when the other senator exerted the effort to intervene on behalf of the state. This finding could imply that individuals view their state’s US senators as a team.²⁶

²⁶We also examine the heterogeneity of our effects by the state as the emphasis of political geography is the key innovation of our study. To this end, we place random slopes the local projects dummy in Table 1 and on the three treatments in the model underlying the results in Table 2 such that the effects of the variables may differ depending on the state. We find no evidence of any state departing from average in a significant way. Our treatment effects are not affected by the state in which they occur. The specific results can be obtained from the replication package.

7 Illustrating the electoral implications

Our results suggest that voters will rate their senator more highly when informed that the senator intervened in favor of a local foreign aid funds. While feeling thermometer ratings may be useful as a leading indicator for electoral success, we are able to provide an (admittedly crude) assessment of how these ratings might translate to the odds of constituents voting for the incumbent. Votes, after all, are the currency of interest for politicians.

We obtain data from the American National Election Surveys from 2012 and 2016 and code for each observation in the survey the feeling thermometer score for the incumbent senator as well as whether the person said he/she voted for the person. We dropped all people from state-years in which no incumbent was running. This leaves us with 2,743 usable observations. For this rough illustration, we use a simple probit (with random intercepts for states). The outcome is whether someone voted for the incumbent senator given the associated feeling thermometer. Using the results from the probit model, we calculate the first-difference in voting for the incumbent when comparing the expected value of the feeling thermometer score when the senator intervened against the three other cases from before. The probability of a vote increases by 0.005 to 0.03 (medians), depending on the comparisons.

Of course, this is a very crude, back-of-the-envelope illustration and a long list of caveats clearly apply. For example, Grimmer, Westwood and Messing (2014) stress that repeated messages make a difference, and we would have to consider the probability that a given voter actually learns about the senator's USAID-related intervention. With all these caveats in mind, this cursory examination suggests that there are potential electoral benefits associated with higher support that stems from intervention to secure local spending on foreign aid. Future research should revisit the connection between tied USAID funds and incumbents' reelection odds.

8 Conclusion

In order to understand whether the commitment of foreign aid may follow from pork-

barrel politics, we must first establish that members of congress would want to obtain geographically-tied budget allocations. Our results suggest that such an incentive exists: individuals rate foreign aid projects and a senator who secures tied foreign aid funds for their state more favorably. And as our preliminary, rough observational analysis shows, these increased feeling thermometer scores should translate to better reelection odds. Our findings contribute to the understanding of foreign policy by highlighting incentives faced by legislators who play an important role in the foreign policy process. The power of the purse—and the fundamentally electorally-minded purpose to which legislators wield it— affects foreign policy.

Our experimental approach provides evidence that individuals respond favorably when presented with information that their community stands to benefit from local consequences associated with foreign policy. We must acknowledge, however, that like all survey experiments, our approach deviates from reality in providing respondents with explicit and uncomplicated information about the single issue we examine. Reality is messier, as individuals receive a variety of possibly conflicting, or at least distracting, information almost continuously. To improve the external validity of our results, future researchers might invest in a more costly field experiment, working with multiple legislators to distribute press releases regarding foreign aid, randomly assigning whether the release mentions a local economic benefit. For example, Grimmer, Westwood and Messing (2014) invested in a customized internet application to conduct their credit claiming experiment. A similar field experiment with an aid-granting agency could release a number of press releases regarding multiple projects, again varying mention of the local targeting. Of course, these projects would require the willing participation of relevant public officials and agencies, possibly adding to the difficulty in their realization. However, similar studies have been carried out on credit claiming on behalf of the recipient government (Dietrich, Mahmud and Winters 2018; Cruz and Schneider 2017).

Our finding that the incentives of legislators to secure or retain local spending drives them

to advocate for locally-tied foreign aid might be discouraging to activists seeking untying of aid more to promote recipient development more efficiently. However, it should be noted that our vignettes disregard the question of aid efficiency given our focus on press releases intended to advertise localized benefits. While our approach represents a prudent first step, future work could introduce further treatment conditions in which activist groups criticize what they view to be inefficient or wasteful use of resources. As such, these followup studies would begin to model the complex, often conflictual information with which citizens are provided.

There is commonly a disconnect between the study of foreign policy *attitudes* and the study of foreign policy *outcomes* given the complex and potentially indirect process by which citizen views influence the behavior of political leaders. Our findings hold implications for foreign policy outcomes following from a simple yet powerful model of distributive politics. Specifically, when considered in terms of the logrolling process that secures pork across local legislative districts (e.g., Mayhew 1974), our findings suggest that we could see higher total levels of foreign aid (or other foreign policy instruments) employed than would be expected given surveys that consider citizen views in a national context. Future research could consider whether log-rolling occurs specifically across different aid projects that benefit different locations, or, as we suspect is more likely, that legislators log-roll across broader issues, with legislators in states not often benefiting from aid giving away aid-related spoils in exchange for e.g., agricultural subsidies or some other locally-salient benefit. Models that consider how economic or ideological group membership affects individual foreign policy attitudes are considerably more difficult to translate into outcomes, requiring scholars to understand the presence and strength of various groups across legislative districts and states. Future research could benefit from examining whether foreign policy outcomes reflect a distribution of benefits to a wide variety of local communities rather than zero-sum competition between opposing economic or ideological groups.

Future research also could benefit from extending our model beyond the United States

context. For example, given our reliance on the local electoral connection, we expect that our findings might apply only within democracies where electoral incentives steer legislators to claim credit and cultivate a personal vote (Carey and Shugart 1995). In the context of international investment, Crisp et al. (2010) demonstrate the usefulness in considering incentives induced by the electoral system. An immediate hypothesis is that the proportion of tied aid should be larger in systems that incentivize cultivating a personal vote. Also, we might expect to see relatively more foreign policy following from national political cleavages (whether based in economic class or ideology) in countries where legislators do not cultivate a personal vote, leading to more programmatic goods (Milesi-Ferretti, Perotti and Rostagno 2002). We view these as exciting venues for future studies.

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Foreign Policy as Pork-barrel Spending: Incentives for Legislator
Credit Claiming on Foreign Aid
Supporting Information
Not for print publication

Contents

- A Checking randomization** **A.2**

- B Large cities in vignette** **A.4**

- C USAID projects across states** **A.5**

- D Additional figures** **A.6**
 - D.1 Map of the location of all respondents A.6
 - D.2 Map of respondent–city pairings A.7
 - D.3 Maps of respondent–city pairings, detailed A.8
 - D.4 Voting for incumbent by feeling thermometer scores A.9

- E Tables for statistical models** **A.10**

A Checking randomization

In this section, we examine whether our randomization was successful for local versus non-local impact, aid increase versus cut, and mention of senator intervention; we also scrutinize whether the assignment of the closest local cities led to the desired result. First, Figures A and A compare the conditional covariate distributions. These marginal distributions look very similar. Only the “education high” dummy in the aid-cut case exhibits some greater divergence across two treatment realizations.

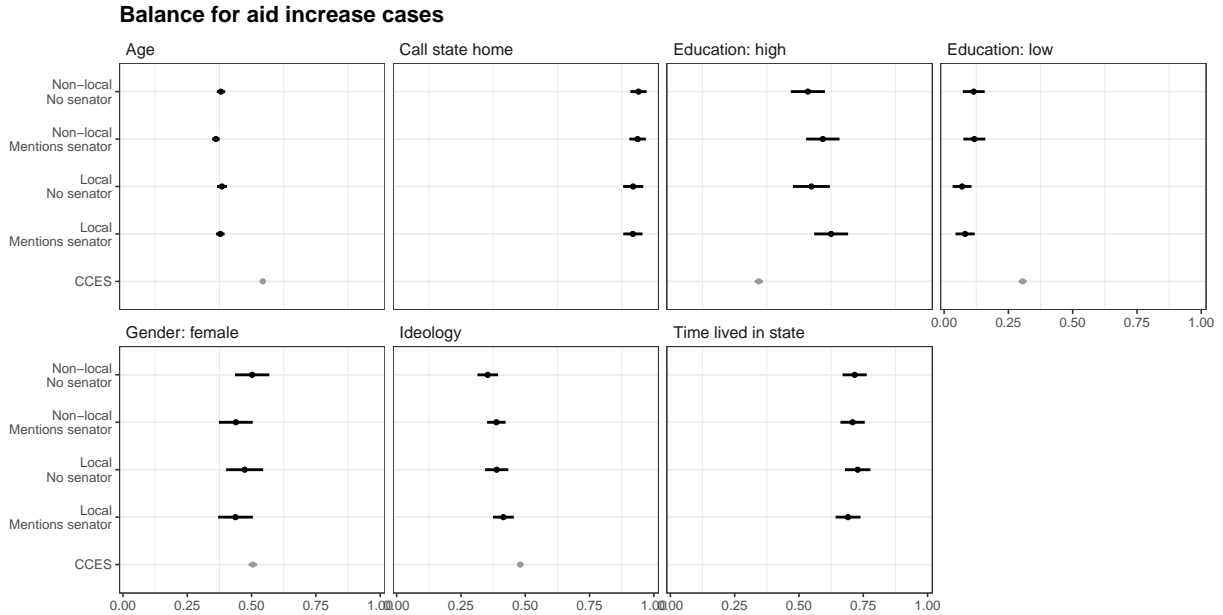


Figure A.1: **Descriptives statistics of covariates and comparison to CCES for cases in which was increased.** Each figure denotes on the y-axis the combination of local/ non-local and whether a senator was shown or not; the x-axis shows the percent of cases (for binary variables) or a rescaled realization (age, time lived in the state, ideology). The realization of the variables from CCES are shown in grey. Dots denote the mean, the line segment the 95% confidence interval.

Second, the location of cities where changes in aid take place were non-randomly assigned if the respondent was assigned a local treatment. The goal is to find the nearest city that could plausibly experience such aid contracts. Figure A.3 shows the distances between the self-reported ZIP code and shown city, for local cases on the left side, and for non-local on the right. It is clear that the non-local cases are considerably farther away from the respondent. For local cases, the mean and median distances are 47 and 20 miles, whereas these are 27 and 56 times farther away in the non-local cases.²⁷

²⁷Figure D.1 in the appendix shows the locations of the self-reported ZIP codes of respondents. Essentially all are actually located in the states of interest to us. Further, Figure D.2 depicts all respondent–city pairings, and Figure D.3 a more specific, zoomed-in illustration.

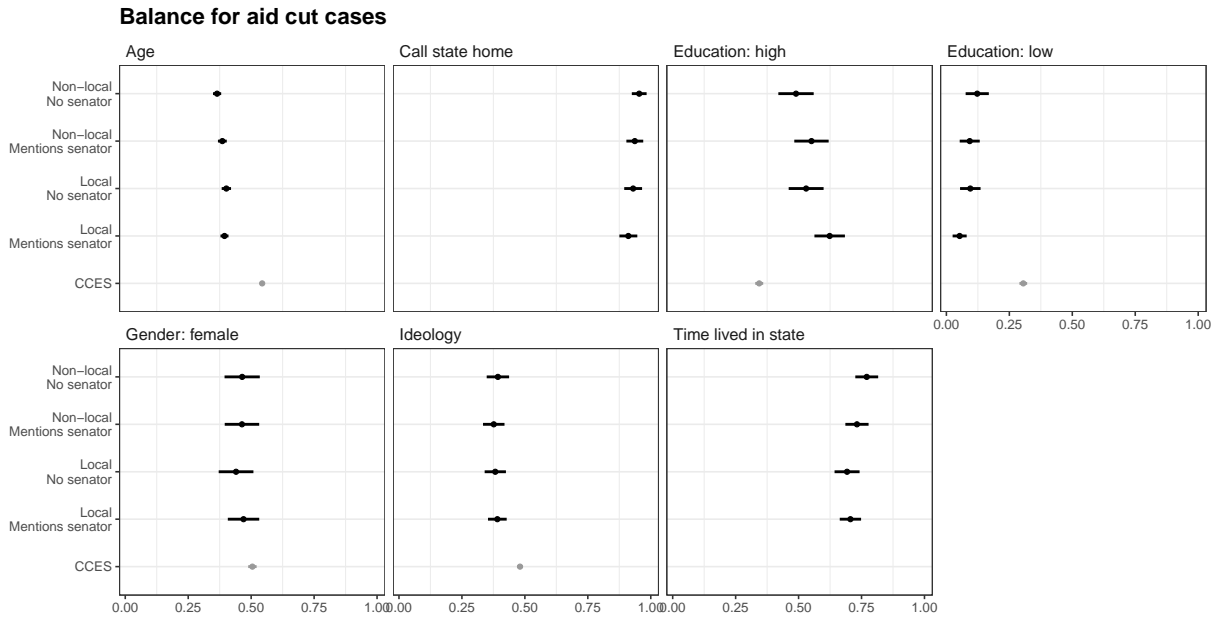


Figure A.2: Descriptives statistics of covariates and comparison to CCES for cases in which aid was cut. The figure is constructed analogously to Figure A.

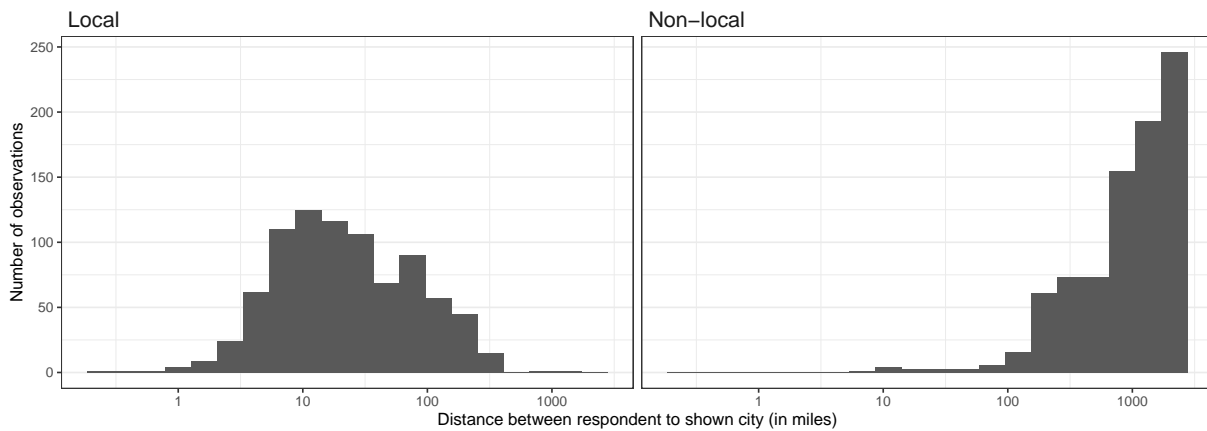


Figure A.3: Histograms of distance from respondent to shown city by local and non-local treatments. Note that the x-axes are scaled logarithmically.

B Large cities in vignette

The following cities are the cities to which a respondent could be matched if the treatment is local. We use the city closest to the respondent's ZIP code (within the self-identified state).

San Diego, CA; San Jose, CA; San Francisco, CA; Los Angeles, CA; Sacramento, CA; Long Beach, CA; Oakland, CA; Fresno, CA; Anaheim, CA; Santa Ana, CA; Riverside, CA; Bakersfield, CA; Chula Vista, CA; Irvine, CA; Stockton, CA; Jacksonville, FL; Miami, FL; Orlando, FL; St. Petersburg, FL; Tampa, FL; Baltimore, MD; Boston, MA; Newark, NJ; Jersey City, NJ; New York, NY; Buffalo, NY; Charlotte, NC; Raleigh, NC; Greensboro, NC; Durham, NC; Houston, TX; San Antonio, TX; Dallas, TX; Austin, TX; Fort Worth, TX; El Paso, TX; Arlington, TX; Corpus Christi, TX; Plano, TX; Laredo, TX; Virginia Beach, VA.

C USAID projects across states

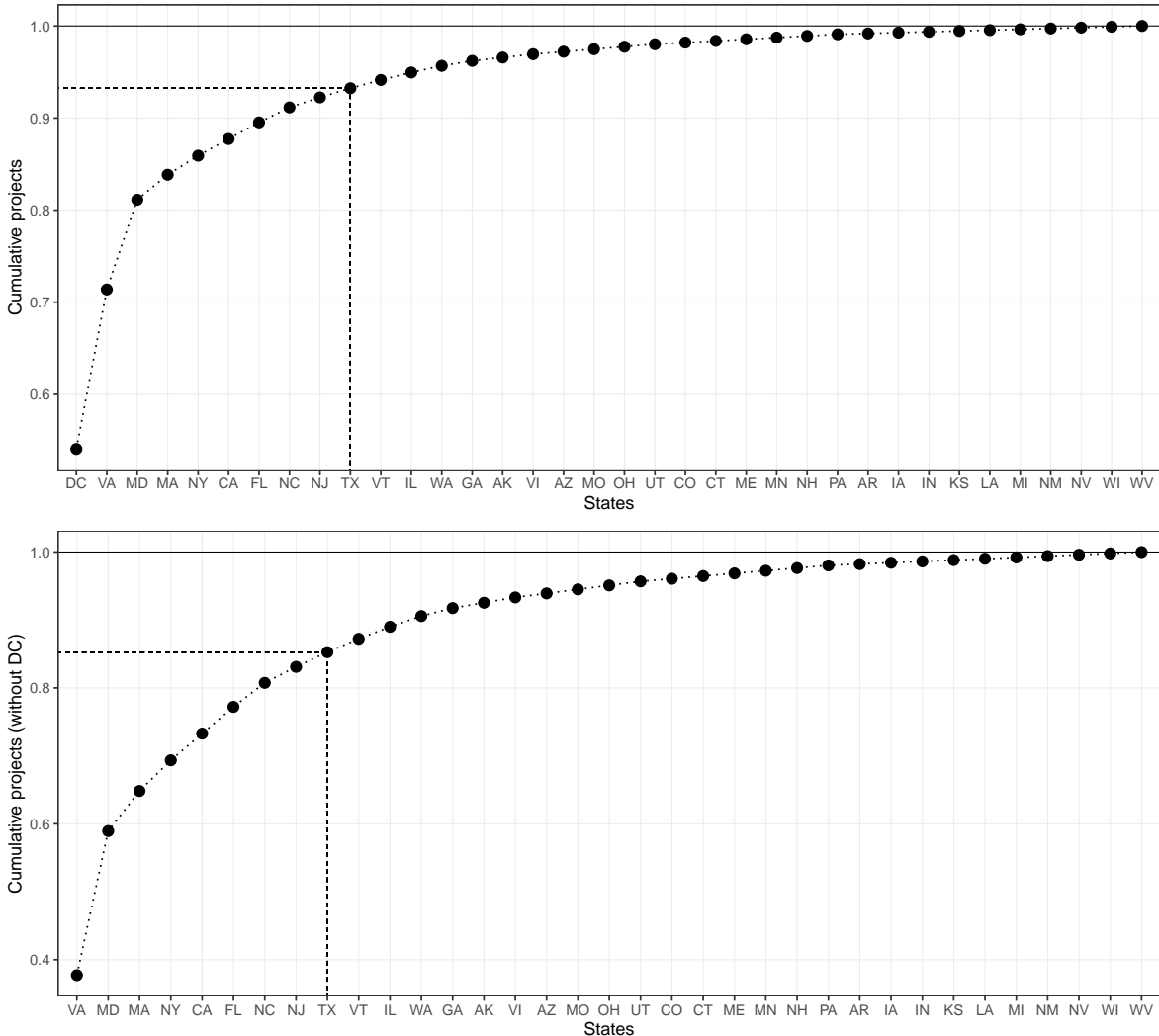


Figure A.4: **Distribution of USAID projects across states.** Each panel gives the cumulative percentage of USAID projects in 2015 (on the y-axis), sorted by each state’s number of projects (along the x-axis). The bottom panel omits Washington, D.C. from the calculation. The dashed lines signify the last state included in our survey experiment (Texas) and the cumulative share of projects captured about our nine states. Note that the y-axes are scaled differently.

D Additional figures

D.1 Map of the location of all respondents



Figure A.5: **Map of the location of all respondents.** Each semi-transparent large dot gives the location of one survey taker. The location was determined by the self-reported ZIP code. Seven of 1680 ZIP could not be located.

D.2 Map of respondent–city pairings

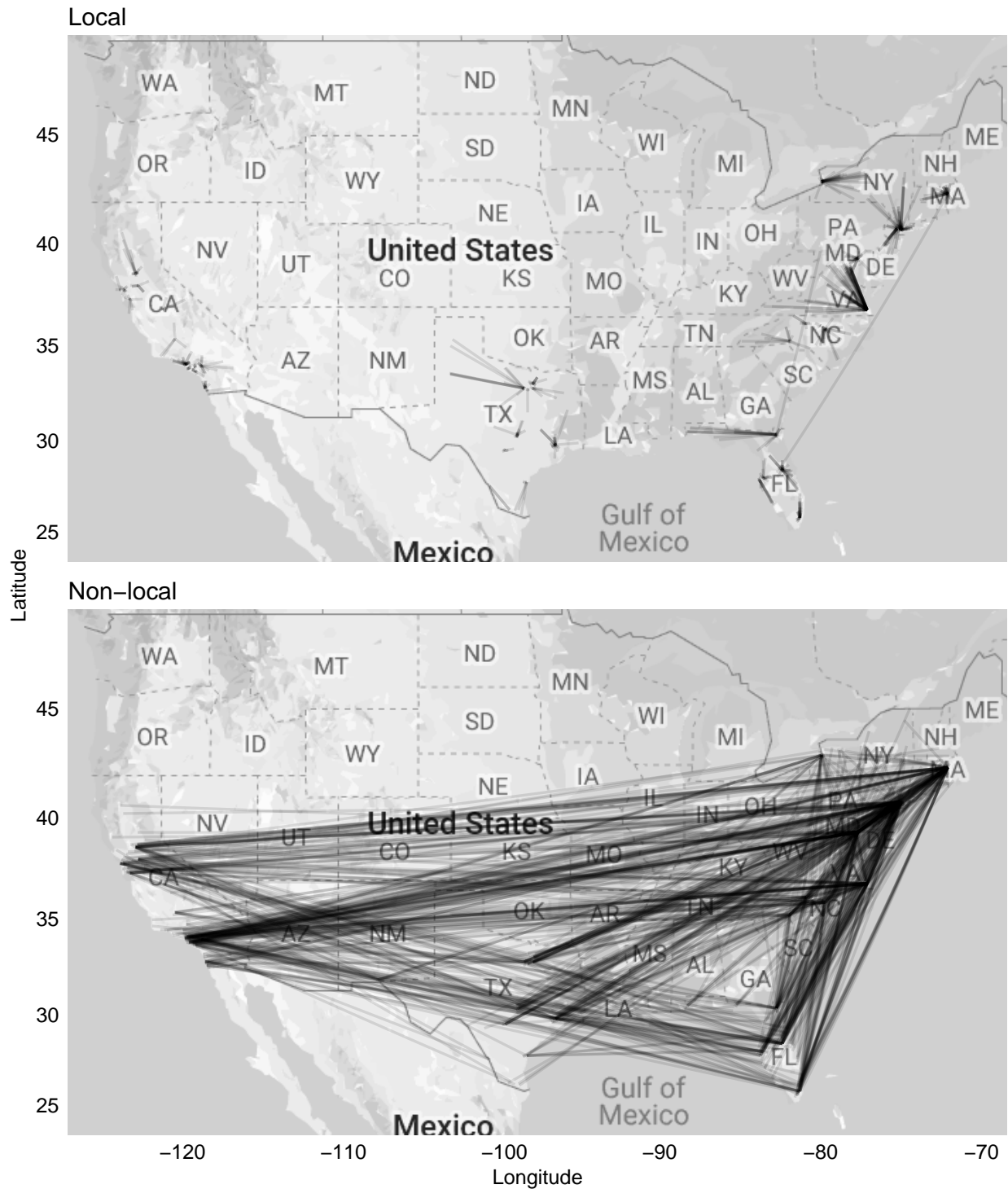


Figure A.6: **Map of respondent–city pairings.** Each arrow goes from the respondent’s location to the shown city. The upper panel gives the pairs for the local cases, the bottom for the non-local cases. There are almost exactly the same number of arrows in each panel (841 and 839, respectively).

D.3 Maps of respondent–city pairings, detailed

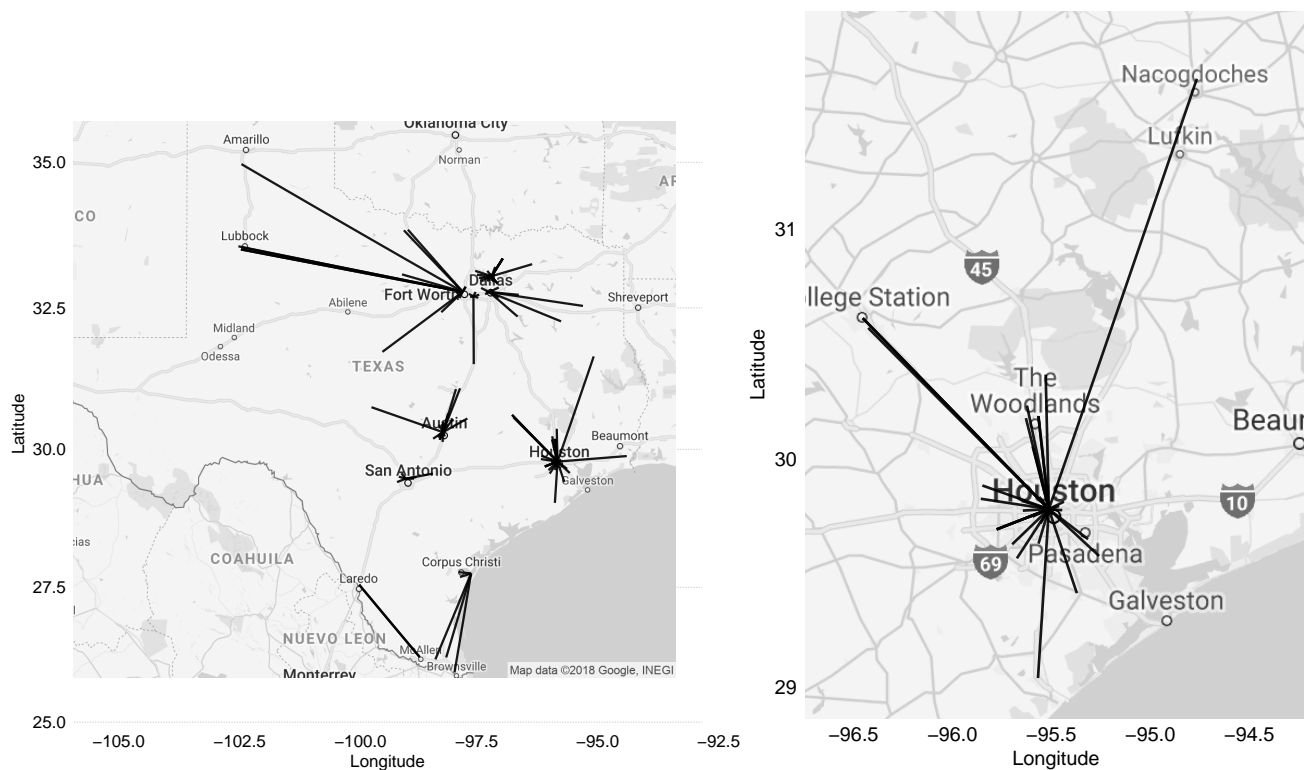


Figure A.7: **Map of respondent–city pairings for local cases only; detailed example.** Each arrow goes from the respondent’s location to the shown city. The left panel gives the pairs of local cases restricted to Texas (129 observations), the right panel restricted to Houston, TX (33 observations).

D.4 Voting for incumbent by feeling thermometer scores

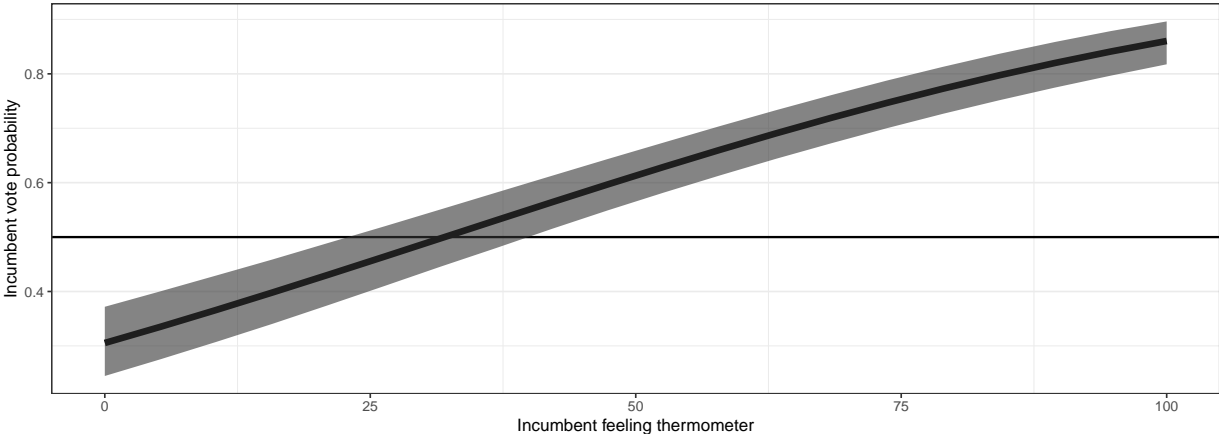


Figure A.8: **Probability of people voting for the incumbent by feeling thermometer score.** The x-axis gives the feeling thermometer score and the y-axis the probability of people voting for the incumbent senator. The light-gray polygon denotes the 95% credible interval. Data from ANES 2012 and 2016. Results from a random-effects probit.

E Tables for statistical models

	Increase aid	Cut aid
Local project	0.59 [0.31; 0.88]	-0.42 [-0.71; -0.13]
Local project x Senator	-0.24 [-0.64; 0.15]	-0.09 [-0.49; 0.29]
Education: high	0.22 [0.01; 0.44]	0.02 [-0.20; 0.24]
Education: low	-0.12 [-0.48; 0.23]	-0.17 [-0.55; 0.19]
Age	0.10 [-0.71; 0.89]	1.05 [0.27; 1.83]
Gender: female	0.14 [-0.06; 0.35]	0.02 [-0.18; 0.21]
Ideology	-1.21 [-1.68; -0.74]	1.02 [0.55; 1.49]
Shown R Senator	-0.23 [-0.79; 0.32]	-0.11 [-0.62; 0.40]
Shown D Senator	0.18 [-0.22; 0.59]	-0.45 [-0.85; -0.04]
Ideology x Shown R Senator	0.57 [-0.56; 1.65]	-0.10 [-1.02; 0.84]
Ideology x Shown D Senator	0.17 [-0.55; 0.92]	0.33 [-0.39; 1.04]
Costs: 1.5m	0.01 [-0.23; 0.24]	0.02 [-0.21; 0.27]
Costs: 2.0m	-0.11 [-0.36; 0.13]	0.14 [-0.10; 0.37]
Intercept	2.52 [2.02; 3.02]	1.83 [1.35; 2.30]
Cut point 1	0.59 [0.43; 0.76]	0.84 [0.67; 1.01]
Cut point 2	1.09 [0.89; 1.28]	1.55 [1.35; 1.74]
Cut point 3	1.57 [1.36; 1.79]	2.18 [1.96; 2.39]
Cut point 4	2.39 [2.17; 2.60]	3.15 [2.92; 3.38]
Cut point 5	2.99 [2.76; 3.20]	3.53 [3.29; 3.77]
Cut point 6	3.84 [3.58; 4.05]	4.10 [3.83; 4.35]
Cut point 7	4.70 [4.39; 4.94]	4.73 [4.44; 5.05]
Observations	832	848

Table A.1: **Support for project; model parameters for ordered probits. Interaction specification.** First number gives the point estimate, the range below the 95% confidence interval. MCMCglmm was used to estimate the models.

	Any change	Increase aid	Cut aid
Local, no Senator	-1.03 [-3.91; 1.87]	-2.34 [-6.44; 1.72]	0.21 [-3.75; 4.26]
	0.24	0.13	0.54
Local, other Senator	2.47 [-0.44; 5.38]	3.69 [-0.52; 8.00]	1.65 [-2.30; 5.58]
	0.95	0.96	0.79
Local, Senator intervened	3.36 [0.46; 6.22]	3.70 [-0.46; 7.98]	3.25 [-0.68; 7.22]
	0.99	0.96	0.95
Education: high	1.98 [-0.48; 4.43]	2.49 [-0.93; 6.05]	1.47 [-1.89; 4.93]
Education: low	0.05 [-4.05; 4.19]	-0.71 [-6.62; 5.07]	0.63 [-5.35; 6.54]
Age	4.83 [-4.43; 13.79]	10.22 [-3.05; 23.34]	0.19 [-12.28; 12.72]
Gender: female	2.15 [-0.15; 4.45]	3.37 [0.09; 6.72]	0.96 [-2.25; 4.17]
Ideology	-51.63 [-55.89; -47.45]	-51.75 [-57.80; -45.74]	-51.72 [-57.43; -45.97]
Senator R	-53.70 [-57.22; -49.88]	-54.58 [-59.50; -49.34]	-53.74 [-58.18; -49.22]
Ideology x R Senator	98.65 [92.40; 104.83]	100.14 [91.09; 109.37]	97.36 [88.87; 105.88]
Costs: 1.5m	1.19 [-1.52; 3.90]	0.43 [-3.49; 4.19]	1.93 [-1.88; 5.80]
Costs: 2.0m	-1.38 [-4.08; 1.36]	-1.21 [-5.25; 2.80]	-1.31 [-5.01; 2.47]
Aid increased	-0.15 [-2.36; 2.10]		
Intercept	65.63 [60.70; 70.88]	62.91 [56.05; 69.89]	67.95 [61.43; 74.48]
Residual SE	15.59 [15.03; 16.15]	16.00 [15.22; 16.82]	15.21 [14.46; 16.01]
Respondent RE SD	20.40 [19.42; 21.40]	20.47 [19.08; 21.92]	20.38 [19.08; 21.78]
State RE SD	1.75 [0.07; 5.29]	1.17 [0.04; 5.80]	0.92 [0.03; 4.47]
Observations	3360	1664	1696

Table A.2: **Feeling thermometer for Senator; censored regressions.** First number gives the point estimate, the range below the 95% confidence interval. The number below the credible interval for the coefficients gives the posterior probability of the coefficient being greater than zero. MCMCglmm was used to estimate the models.

