Backyard effects in democracy promotion: evidence from a survey experiment

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Abstract

In this paper, we design an original survey experiment to examine whether citizen support for foreign policy tools intended to promote democracy abroad depends on whether the policy has a domestic impact local to the citizen. Specifically, we examine whether US citizens support grants of foreign aid more when such aid would promote local firms and organizations, and whether they support aid sanctions less when the policy would endanger local jobs. Using causal mediation analysis, we consider several possible reasons behind the so-called “backyard” effects associated with these foreign policy tools, including moral considerations, cost-benefit analysis, and preference for alternate policies. We find strong evidence that a local impact of aid grants and sanctions affect support for the policies; and mediators explain only a small portion of this effect.

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1 Introduction

Few US citizens place high priority on promoting democracy abroad when this objective is listed among more salient foreign policy goals such as fighting terrorism and protecting American jobs (Pew Research Center for the People & the Press 2013). Yet the United States, like many governments in the developed world, lists democracy promotion as among the primary aims of its foreign policy (e.g., United States White House 2016), and backs this commitment with a variety of foreign policy tools, ranging from grants of democracy assistance to military interventions and economic sanctions against states that move towards authoritarianism (e.g., Rose 2000-2001; Finkel, Pérez-Liñán and Seligson 2007; Von Soest and Wahman 2015; Meernik 1996). One might surmise that citizens who support democracy promotion abroad are cosmopolitans or those who see the spread of democracy as beneficial to domestic security and economic growth; however recent research casts doubt on this idea in general (Brancati 2014). Perhaps variation in support for democracy promotion does not lie with such broad, sociotropic perspectives on national benefits, but rather with more local effects. A large literature examines such “backyard effects,” most commonly the “not in my back yard” (NIMBY) phenomenon in which people oppose policies or developments that impose local costs (e.g., Deer 1992). However, few studies have examined whether and how this mechanism applies to foreign policy.

In this paper, we examine how support for the use of two democracy promotion tools is influenced by the locus of domestic economic impact—specifically a person’s proximity to a city that benefits from grants of democracy aid (that often funds US firms and nonprofit

1http://www.pewresearch.org/fact-tank/2013/12/04/americans-put-low-priority-on-promoting-democracy-abroad/
2https://www.whitehouse.gov/issues/foreign-policy
3Studies that do so typically explore the influence of casualties on support for armed conflicts, or politicians in power during conflicts (e.g., Gartner, Segura and Wilkening 1997; Gartner and Segura 2000; Kriner and Shen 2007, 2013; Gartner 2008). Pelc (2013) reports that the World Trade Organization cases raise interest among people living close to the affected industry.
organizations) or experiences harm from aid sanctions (given that aid that is tied to purchases in the United States). In an original survey experiment of US citizens conducted via Amazon’s Mechanical Turk (MTurk), respondents are presented with a scenario in which the (randomly assigned) outcome of a close election suggests potential either for the strengthening of democracy or for movement towards authoritarianism in some foreign country. We then ask respondents to rate support for a corresponding foreign policy tool: a grant of democracy aid (if the democratic candidate wins), or a sanction that cuts tied aid (if the authoritarian candidate wins). Using respondent self-reported home-state, we randomly assign the locus of domestic impact of the proposed policy tool, either to a city in the respondent’s home state or to a city in a different US state.

Although it is plausible that such a local impact influences citizen support following from a straightforward home-town allegiance, it is also possible that understanding of the local consequences of foreign policy could lead citizens to perceive the policy as more salient, leading to enhanced or outright biased consideration of the underlying issue. Accordingly, we ask a series of followup questions in order to understand the causal mechanisms behind the backyard effects (Imai et al. 2011; Tomz and Weeks 2013). Specifically, we consider whether exposure to a local impact leads citizens to ponder moral imperatives of democracy promotion in general, to evaluate cost-effectiveness differently, or to gauge other policy options they might not otherwise consider to be superior.

Our first main finding is that there are backyard effects in democracy promotion. When benefits accrue locally, support is higher; when there are local cost, support is lower. These effects are approximately five percentage-points in absolute value. Second, we find that there are clear differences in how such effects operate through aid increases and decreases. Treatment effects are transmitted approximately 40% each through two channels. Roughly half of the backyard effect in the provision of democracy aid goes through affecting the cost/benefit evaluation and the attitude about the normative importance of democracy promotion in
general. For the aid sanctions, the cost/benefit calculus is affected while the normative assessment is not. In contrast, the NIMBY effect of sanctions makes helping the losing, pro-democratic side more appealing over punishing the winning, pro-autocratic side.

In the next section, we review the broader literature on public opinion. In particular, we emphasize the backyard effects that studies on conflict involvement and casualties consider and develop our expectations for opinions on democracy promotion. Then, we introduce the design of our survey experiment and present the results of our statistical analysis.

2 Public opinion and foreign policy

Before examining how the location of domestic impact affects support for foreign policy, we begin with a brief discussion of a critical, potentially antecedent question: does public opinion influence foreign policy? The skeptical reader might conclude that a study of foreign policy attitudes is fruitless if public opinion regarding foreign affairs is inconsequential. Although this view has been stated famously (e.g., Almond 1950; Lippmann 1955), recent research suggests that citizen views regarding foreign policy do matter (e.g., Holsti 1992, 2004; Baum and Potter 2008; Canes-Wrone 2015). Stern (1998) and Milner (2006) provide evidence that foreign aid budgets and its disbursement details correlate with public support for aid, respectively, though it is unclear whether this association reflects a causal relationship. Perhaps the strongest evidence for a causal effect comes from Eisensee and Strömbärg (2007) who rely on an instrumental variable approach to demonstrate that natural disasters elsewhere only draw US humanitarian aid if mass news coverage is affected.

Along similar lines, evidence that public opinion affects the use of sanctions has increased in recent years. For example, research demonstrates that sanctions are used to placate demand for action amid an international incident or crisis (Whang 2011; McLean and Whang
while more recent work expands on this explanation by uncovering specific perceived effects of sanctions that citizens favor (Heinrich, Kobayashi and Peterson 2016). Other work echoes the connection between mass media coverage and foreign policy choices, showing that sanctions over human rights violations are more likely as media attention to abuse increases (Nielsen 2013; Peksen, Peterson and Drury 2014). These results suggest that people favor action amid international crises, and that public opinion matters to the extent that they are informed that crises have occurred (see also Baum and Potter 2008, 2015).

Public support for foreign policy could depend on the distributional consequences of the policy (Milner and Tingley 2015). With respect to aid and sanctions, previous work has shown that citizens favor policies that are likely to benefit them directly. Regarding aid, research finds that donor country citizens with more education, skills, and income are more likely to support aid (Milner and Tingley 2011; Paxton and Knack 2011; Chong and Gradstein 2008) whereas those whose employment status have declined are less likely to do so (Heinrich, Kobayashi and Bryant 2016), and that legislators will vote for aid presumed to benefit their constituents (Broz and Hawes 2006; Milner and Tingley 2010, 2015). Such a relationship could stem from material considerations following in accordance with the Stolper-Samuelson theorem: to the extent that wealth and education suggest an individual to be a holder of capital, this individual stands to gain from increased interaction with capital poor nations such as aid recipients; and aid provides a means to increasing such interactions. Similarly, research suggests that the use of sanctions is motivated by lobbying from domestic firms or industries in the sender state that would benefit from economic restrictions as a form of protectionism (Kaempfer and Lowenberg 1988; McLean and Whang 2014; Milner and Tingley 2015). Below, we consider the locus of domestic impact as a means through

\[\text{\footnotesize\(^4\) However, empirical evidence for this contention is indirect. Whang (2011) shows that the use of sanctions is associated with a subsequently higher level of popularity for presidents, while McLean and Whang (2014) operationalize voter awareness of international disputes using an indicator of social globalization.\footnotesize}\]

\[\text{\footnotesize\(^5\) Indeed, recent research suggests that domestic interests in the target could behave similarly (Lektzian and Patterson 2015).\footnotesize}\]
which citizens could view foreign policy as more relevant to their lives.

3 Local effects and attitudes on democracy promotion

Following an international event that captures public attention, for example, the outcome of an important and highly contentious election, citizens in wealthy democratic states could be open to supporting a policy of democracy promotion. Our main argument is that the level of support for democracy promotion via increases in democracy aid or sanctions of tied aid depends on whether the proposed policy would affect a citizen’s local economy. The ability to assess these associations hinges on the fact that foreign aid is often not distributed to recipient states directly. Rather, aid often goes to domestic third parties such as NGOs and contractors, which carry out development tasks (Dietrich 2013). These funds are distributed within the donor country such that jobs and revenue benefit (some) donor citizens. Similarly, aid is often (and particularly in the United States) tied; even funds that go directly to the recipient country may be required to be used for purchases from the donor country. For example in 2007, roughly 37% of Official Development Assistance (ODA) was tied to US purchases. Indeed, this percentage reflects only formally tied aid. In the same year, 68% of aid that was officially untied nonetheless went to US domestic contractors.

While it is relatively unlikely that any given person would be employed by an organization that is affected by aid changes, we nonetheless expect a local impact of such changes to affect the support for the democracy promotion policy. Pelc (2013) provides evidence that people’s interests are indeed roused when international outcomes affect people even at the granularity of the US state level. De jure and de facto tying of aid allow donor politicians

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6 Crucially, this is also the case in democracy promotion via foreign aid as Bush (2015) documents.
to claim credit for delivering benefits to their district, which bolsters a general perception of influence (Grimmer, Westwood and Messing 2014).

### 3.1 The NIMBY Phenomenon

The original work on the “not in my backyard,” or NIMBY phenomenon is far removed from the realm of foreign policy. The NIMBY phenomenon occurs when citizens voice opposition to some development or policy that presumably would provide some useful service in general, the local site of which they perceive to have a detrimental effect on their quality of life. Classic examples include opposition to local storage of hazardous waste or nuclear power plants. However, the NIMBY phenomenon also occurs with respect to transportation infrastructure such as rail lines or airports, prisons, services for the poor, controversial businesses, etc. With respect to government actions, the NIMBY effect in general 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. For example, research demonstrates that 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). A recent study shows that even potential casualties close to home lead people to increase levels of political participation. Specifically, Davenport (2015) finds that turnout in the 1972 elections is higher among parents whose sons face a higher risk of conscription.

Backyard effects beyond the classic NIMBY phenomenon exist. Indeed, a driving force behind pork barrel spending relies on the fact that constituents approve of—and reelect—legislators who use government spending to provide development and services to their district (e.g., Mayhew 1974; Ferejohn 1974), even if legislators had little to do in securing them (Grimmer, Westwood and Messing 2014). Complementing the NIMBY acronym, this phe-
nomenon has been deemed “yes, in my backyard,” or YIMBY. For example, people could prefer to locate renewable energy generation infrastructure locally in order to improve air quality, or attempt to attract firms to their region in order to facilitate local employment. Henceforth, when referring to NIMBY and YIMBY jointly, we make use of “*IMBY” as a term.

3.2 *IMBY effects in democracy promotion

In theorizing about backyard effects of democracy promotion policies, we begin by conceptualizing two scenarios. First, we consider a case in which the prospects for democracy are favorable, for example after the election of a pro-democracy candidate. We contend that this type of case is one in which allocations of democracy aid could be effective, whereas aid sanctions would not be expected. All else equal, citizen support for aid should be informed by their ideology, particularly whether they are internationalists or isolationists (Paxton and Knack 2011). However, if citizens are informed that aid will be distributed to organizations in their area, potentially supporting employment and benefitting their local economy, we expect them to increase their support for the policy in accordance with the YIMBY effect. While this effect could stem solely from desire to bolster their local economy, it is also possible that citizens would oppose policies that would benefit others outside their home state or region, particularly given that aid allocations entail a cost. Evidence for this “not in their backyard” effect has seen little academic study, but has been reported by US media sources over domestic policy issues. For example, some US Senators opposed disaster relief for New York in the aftermath of Hurricane Sandy, despite previously supporting similar efforts for their home states.

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8Other “backyard” effects, with associated acronyms, have been coined. However this study is concerned primarily with these two phenomena.


Applying the logic of YIMBY to the issue of democracy promotion, our first hypothesis addresses a scenario in which citizens receive information that prospects for democracy are favorable:

**Hypothesis 1** *Citizens are likely to support increased allocations of democracy assistance when the economic impact is local as opposed to more distant.*

For our second scenario, we conceptualize a country that appears to be moving towards increased authoritarianism, perhaps following an election in which voter intimidation and violence were widespread. In such a case, we expect that people might favor the use of sanctions to cut existing aid,\(^\text{11}\) while grants of additional democracy aid likely would be opposed. However, if a person were informed that aid sanctions would result in the loss of contracts among organizations in her area, she might rethink support for the policy, in this case following from a classic NIMBY effect. Conversely, citizens might be less opposed to sanctions that harm a more distant domestic location, despite the fact that jobs could be at risk generally. In the latter case, people should favor the potential to promote democracy abroad—either directly or via normative signaling (Heinrich, Kobayashi and Peterson 2016)—specifically because they perceive that other citizens will be paying the costs.

Accordingly, our second hypothesis, applying the NIMBY phenomenon to a scenario in which citizens receive information that prospects for democracy are unfavorable, follows:

**Hypothesis 2** *Citizens are less likely to support the imposition of sanctions by cutting existing assistance when the economic impact is more local as opposed to more distant.*

Finally, we consider the reasoning that lie behind potentially parochial citizen attitudes. Given the low probability that a randomly-selected person would benefit directly from aid-related contracts or grants disbursed to local organizations, any IMBY effect likely follows primarily from other ways in which a local impact affects perceptions of salience or the cost-benefit tradeoff associated with a proposed policy. A direct effect could exist if a citizen

\(^{11}\)Indeed, a democratic backslide could constitute a crisis that leads to higher citizen attention, at least temporarily, to the issue.
simply prefers promoting and protecting the local economy. However, we consider three
classes of potential mediators. First, although morality appears to play a role in citizens
views on foreign policy (e.g., Steel 1999; Barratt 2007; Kertzer et al. 2014), morality might
be reinforced (blunted) when a local impact provides a local benefit (cost). Second, *IMBY
effects may skew the perception of the overall costs and benefits from the change in foreign
aid. Finally, alternate policy options (e.g., seeking action from the United Nations) might
appear more or less attractive depending on how sanctions or grants of aid affect a citizen’s
local area. Each of these three mechanisms suggest some self-serving, confirmation, and
identifiable victim biases in the evaluation of the policy. The hypotheses are thus:

**Hypothesis 3**  A citizen’s response to a local impact of democracy promotion policies
will be mediated by the citizen’s views on the morality of the policy.

**Hypothesis 4**  A citizen’s response to a local impact of democracy promotion policies
will be mediated by the citizen’s views on the cost-effectiveness of the
policy.

**Hypothesis 5**  A citizen’s response to a local impact of democracy promotion policies
will be mediated by the citizen’s perceptions of alternate options to pro-
mote democracy.

4 Survey experiment

In this section, we introduce the survey experiment that allows us to study the existence of
and mechanisms behind *IMBY effects as discussed above. Broadly, we follow the design
used by Tomz and Weeks (2013), relying on the theoretical results of Imai et al. (2011) to
assess treatment and mediation effects. As in our arguments, we study the role of *IMBY
effects in US democracy promotion in a fragile democracy by asking survey-takers to consider
a vignette. In our scenario, an election pits a candidate from a pro-democracy party against a
member from a violent, anti-democratic movement. We consider the case of either candidate
winning and explain that US policy could respond with a change of foreign aid outlays.
We first introduce the common background for both cases, and then discuss our *IMBY
treatments as well as the potentially mediating variables.

4.1 Design

At the outset, each survey-taker sees the same introductory background information about a “small African” country that has a fragile democratic government. The story is centered on an African country because (Subsaharan) Africa has long been the main focus of development efforts (Easterly 2009). The use of “small” as an adjective puts distance between survey-takers and the locus of political action. Had we invoked a “large” or geographically nearby country, people might perceive there to be immediate repercussions from political changes in that country (Schumpeter 1942). Through the distance and the smallness of the country, we set the baseline more clearly with democracy promotion for its own sake. In a similar spirit, the vignette states that US relations with that country have been friendly.

The vignette explains that the country’s political direction is up for grabs: democratic practice is described as fragile, held together by a leader who has died recently of natural causes. Following this death, an election took place in which two candidates competed. One is known to be committed to democratic principles, while the other is affiliated with a violent movement. The latter is said to want to move the country in a more centralized, authoritarian direction.

The first randomization is over the outcome of this election. One half of the survey-takers learn that “despite intimidation at the polls,” the democracy-leaning candidate wins; the other half learn that “because of the intimidation at the polls,” the autocracy-leaning

\footnote{Similarly, research shows that European states give aid to stabilize its nearby areas (see Schneider and Tobin 2013).}

\footnote{Specifically, the introduction reads: “A small African country that enjoys friendly relations with the United States has maintained a fragile democracy in large part due to the efforts of its president. After the death of this president from natural causes, the country held elections in which two candidates competed. Candidate A has been associated with a pro-democracy party and has been known to be committed to promoting rule of law, protecting minorities, and holding free and fair elections. Candidate B has been associated with a violent movement and has been known to be determined to centralize authority in the executive branch by undermining legislative and electoral processes.”}
person emerges victorious. The universal background information and mirrored electoral outcomes reflect a common underlying case where an election should be seen as pivotal for the future of a fragile democracy. In particular, we emphasize the role of violence surrounding this narrowly decided elections; either outcome could have easily gone the other way. We model this scenario in the spirit of a natural experiment in that just small perturbations caused there to be a victory of either candidate. This entails that we establish a common distribution of beliefs about the attributes of either scenario within survey-takers (Dafoe, Zhang and Caughey 2015).

Each survey-taker sees only one realization of the election outcome. Within each case, we randomize the details about the chosen US response and the locus of domestic impact. After reading the presented scenario, the respondent is asked to evaluate the policy on scale from 1 ("strong opposition") through 9 ("strong support"), which dichotomize into "support" for answers of seven and above to ease interpretation. This answer serves as our main outcome variable.

4.1.1 *IMBY cases

In the first case, the pro-democracy leader wins the election “despite intimidation at the polls.” Respondents are told that the new leader seeks to strengthen democratic institutions: “The political system remains contested and efforts to stabilize democratic practices are just beginning.” This is consistent with the background information that this candidate was committed to democratic principles. Our interest lies in how citizens evaluate US policy toward this weakly democratic country. The vignette explains that the US government provides an increase in foreign aid for “specific projects to promote citizen participation and build a stronger civil society” in the recipient country.14 This type of “democracy aid” is a common tool of foreign policy and that has been shown to help establish and solidify

14The amount is randomized: $25, $50, or $75 million USD.
democratic systems (Gibson, Hoffman and Jablonski 2015; Scott and Steele 2005, 2011; Finkel, Pérez-Liñán and Seligson 2007).

In the aid grant scenario, US companies compete over specific contracts “to assist local projects toward these ends” (ie., fostering democracy). Specifically, bidding among US contractors “is for building a web platform that connects political parties, citizens, and newspapers.” In this scenario, the US government hires a US company to carry out the assistance for the pro-democracy efforts put forth by the newly elected government. The volume of funds for the project that is highlighted in our vignette are of non-trivial size.\textsuperscript{15} The major treatment of interest is the location of the company winning the largest contract. With probability of one half, the survey-taker is paired with a company headquartered in a city from the self-identified home-state, and with complementary probability from elsewhere in the United States.\textsuperscript{16} If the respondent-city pair is randomized to be in the same state, we have a potential YIMBY scenario as part of the aid would accrue to the local economy.

If the candidate associated with the violent movement wins “because of intimidation at the polls,” the scenario is broadly albeit not exactly inverted. In the aid sanction scenario, the success of the violent autocracy-leaning candidate is punished as the US government proposes to cut existing aid.\textsuperscript{17} Either one half or one third (randomized) of the cut will harm a specific US manufacturer of specialized goods (plastics, metals). The scenario explains that this aid had gone to the recipient government to buy these goods, which are vital to its economy; thus, sanctions that reduce “tied aid” are proposed. The location of this affected company is randomized following the same scheme from the aid grant scenario. If the home-state of the respondent and the company’s city match, we have a potential NIMBY scenario.

We want to emphasize that the two cases are not symmetrical. In the YIMBY case,\textsuperscript{15} We randomize whether the contract under focus is “half” or “a third” of the total aid volume.\textsuperscript{16} Our list of potential cities comprises the largest city in each state and all cities with populations of 500,000 and more. This preserves some realism as contractors with such volume are less to reside in small cities. The full list is given in the appendix.\textsuperscript{17} Again, the amount is randomized: $25, $50, or $75 million dollars.
we envision an aid increase that aims to help democracy whereas classical tied aid is cut in the NIMBY scenario. These scenarios share a design in which a specific US company is helped or harmed, but the content of foreign aid is different. In the former case, new aid to fund democracy-building projects is provided whereas in the other case aid that is crucial to Country B’s industry is cut. Symmetry and thus close comparability eludes us as we did not want to model a case in which existing democracy aid gets cut when the autocrat wins. Therefore, we should only cautiously compare the cases to each other.\textsuperscript{18}

4.1.2 Mediators

In order to understand how *IMBY effects change people’s evaluation of US democracy promotion efforts, we ask the survey-takers three follow-up questions. If answers to these are affected by *IMBY considerations and if these attitudes relate to the overall evaluation of the policy, then some of the total effect is transmitted through the mediators. Our mediation questions concern the normative assessment of democracy promotion in general, the specifics of costs and benefits of the case, and the relative evaluation of alternative policies.

First, a mediator tries to gauge whether local ramifications change the normative dimension of democracy promotion. We ask whether “the United States [should] generally help fragile democracies abroad.” The answer options are “Yes” and “No.” The intuition is one of self-serving bias. As the question asks about the principle of democracy promotion as indicated by the word “generally”, the particularities of the scenario—whether *IMBY or not—should not matter. However, if it did, then we can conclude there to be self-serving bias in the assessments.

Second, we also want to assess whether the location of domestic impact affect the overall cost-benefit considerations of the policy. We ask: “Relative to the expected benefits of the aid

\textsuperscript{18}We also randomize how confident US government officials are over the success of the policy. The change in aid is said to “will help greatly” or just “will help” the prospects of democracy in the small country. We view this randomization as more as a measurement strategy (Tingley 2014).
increase, how do you evaluate the costs?” Such cost/benefit considerations are fundamental to much analysis of policy. In this case, it is of interest whether backyard effects change the perception of the costs relative to the benefits. If sociotropic evaluations were to guide all evaluations, then the *IMBY treatments should not matter as only change the geography where costs and benefits occur. If egotropism and pocketbook effects play a role (Heinrich, Kobayashi and Bryant 2016), then such geographic changes ought to matter.

Third and continuing the theme from the cost/benefits, the backyard effects may make other policies appear attractive relative to the aid increase or cut. In the case of a pro-democracy victory at the polls and an aid increase, we ask whether any of the following policies is preferred over the aid increase: “Seek support for new government at United Nations,” “Work to undercut influence of the violent movement” (which had lost), “Sign a trade agreement,” or none of these. In the case of aid cuts, the second option is replaced by “Work to improve influence of the pro-democracy party.” The options cover ideas about prioritizing international influence, targeting the losing side, and focusing on economics broadly. If any of these are preferred over the proposed US policy, the *IMBY ramifications would be weaker for the survey-taker. Again, if policies are evaluated on an impersonal, nation-level perspective, then there should be no difference between any of these responses based on the *IMBY treatment.\(^\text{19}\)

### 4.2 Subjects

In February 2016, we posted a short job on Amazon’s Mechanical Turk and asked for people to participate in a two-round survey on foreign policy preferences. The second round differed from the first round in exactly one way, namely that the *IMBY treatment was reversed. This design allows us to “observe” the counterfactual support and answers to the mediation

\(^{19}\)We also ask about the employment consequences of the aid increase or decrease for the randomly drawn company. We view this as a measurement strategy to check whether consequences about jobs differ between the scenarios (Dafoe, Zhang and Caughey 2015).
questions (Tomz and Weeks 2013). 711 people completed the initial job. Three weeks later, we contacted them again for a follow-up survey that would more than double their pay. After up to four reminders were sent, 71% of first-round respondents participated in the second round. Whether a first-round participant completed the second round was independent from the substantive variable of interest. In the appendix, we examine how demographics, whether one saw the backyard treatment, and which case was drawn, as predictors to model participation in the second round. Females as well as older people were more likely to participate the second time. Crucially, the realization of the vignette (democrat wins vs. autocrat wins, with associated variation in aid grant vs. aid sanction) did not matter, as coefficient point estimates were close to zero; therefore, we assume that there were no carryover effects. Accordingly, we are confident that multiple imputation is appropriate to fill in the mediators and support answers in cases of non-response.

Unsurprisingly, the descriptive statistics of respondents differ somewhat from the US population. The following comparisons rely on the weighted realizations from the Cooperative Congressional Election Survey from which we took several questions (Vavreck and Rivers 2008; Ansolabehere and Rivers 2013). The age is lower by about eight years; the percentage of females is 41% as opposed to 56%; and there are noticeably more people with 4-year college degrees and beyond (52% versus 19%). Somewhat surprisingly, the mean ideology on the 7-point scale is substantively similar (3.2 versus 3.6). However, the difference of 0.4 is statistically significant (standard error is 0.07). While differences exist, recent work shows that even unweighted samples from MTurk often replicate the magnitudes of known effects and generally the qualitative result (Berinsky, Huber and Lenz 2012; Mullinix et al. 2015).

20 The exact wording of the questions is in the appendix.
4.3 Inference

As explained above, the survey was run in two rounds and that people saw an identical scenario with only *IMBY treatment status switched the second time. Given a wash-out period of about three weeks, we assume that we can treat the second round as an observation of the counterfactual. That is, as we have data on the evaluation and the mediators under two different treatment states, we can thus study the causal mediation effects. We obtain these in steps. First, we examine the effects of the *IMBY treatment on the outcome and on the mediators, respectively, and then of the mediators on the outcome. Subsequently, we invoke the decomposition results by Imai et al. (2011) to calculate the causal mediation effects. We introduce each in turn.

Beginning with observed variables, we define $Y_{ij}$ as a dichotomous variable capturing whether the policy receives support of seven or higher (on the one-to-nine scale) by respondent $i$ in his/her $j^{th}$ survey round ($1 \leq j \leq 2$); $T_{ij}$ is the *IMBY indicator; the vector $X_{ij}$ contains the intercept, all other randomized variables, and demographic covariates; $M_{ij}$ is the vector of all five observed mediators, while $M_{ij,k}$ ($1 \leq k \leq 5$) is its constituent parts. The statistical models are run separately for each scenario (democrat wins and autocrat wins). As the procedures are identical across the two cases, we do not denote them separately in the notation.

The total backyard effect is found by running a regression of the form

$$Y_{ij} = \alpha_0 T_{ij} + X_{ij}\alpha_1$$

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21 To reduce residual noise in all our regression estimates, we include the following demographics: a dummy for gender; age; a linear version of a 7-point ideology measure; a dummy for high education (4-year college and above); and a proxy of wealth by using the logarithm of the median home value per square foot associated with the respondent’s ZIP code (data come from Zillow Data). Each of them was collected in the first round of our survey.

22 Although we ask only one question about preferred alternative policies, we split responses into three by dichotomizing support for each alternative.

23 Further, we use multiple imputations to fill the missing values in and account for intra-respondent correlation by using cluster-bootstrapping (Harden 2011).
and examining the coefficient $\alpha_0$. Similarly, by regressing the mediators on the treatment dummy,

$$M_{ij,k} = \beta_0kT_{ij} + X_{ij}\beta_{1k}$$

gives us the backyard effect ($\beta_{0k}$) on the mediation responses. That is, how much do local interests affect attitudes that may themselves affect the support. Finally, the effect of the mediators on the support comes from augmenting the first regression with the mediator variables,

$$Y_{ij} = \sum_{k=1}^{5} \gamma_{0k}M_{ij,k} + \gamma_1T_{ij} + X_{ij}\gamma_2.$$ 

Each $\gamma_{0k}$ provides this effect. As the mediators are observed, these effects are not experimentally identified.

These results are of interest in themselves. However, our main goal is to understand how much of the total effect is transmitted via each mediator; this is the domain of causal mediation analysis. Following Imai et al. (2011), we distinguish between the direct and indirect effects for which we first introduce the counterfactual notation and then show how we can estimate them. Let $Y_i(T_i = t, M_{i,k}(T_i = t), M_{i,-k}(T_i = t))$ be the support by person $i$ for the policy when the treatment $T_i$ is equal to $t$ and the mediators $(M_{i,k}(t), M_{i,-k}(t))$ take on their value under the observed treatment, $T_i = t$. $k$ refers to the $k^{th}$ mediator, and $-k$ to all the others. The realized treatment $t$ is 1 if the city was is drawn from the self-reported home-state, and 0 otherwise.

Using this notation, the total effect of the treatment is simply $Y_i(1, M_{i,k}(1), M_{i,-k}(1)) - Y_i(0, M_{i,k}(0), M_{i,-k}(0))$, which we obtain from the first regression above. The notation makes it clear that if the mediators matter for the evaluation of the policy (eg. $Y_i(1, M_{i,k}(1), M_{i,-k}(1)) \neq Y_i(1, M_{i,k}(0), M_{i,-k}(1)))$ and if $M_{i,k}(1) \neq M_{i,k}(0)$, then the $k^{th}$ mediator would transmit...
some of the treatment effect. This is the case as all *IMBY effects must arise through affect-
ing mediators that change the outcome, influence the outcome directly, or through some other, unspecified mediator.

Specifically, we work with the direct effects of the treatment when the mediators take on their levels under the *IMBY treatment condition. That is,

$$Y_i(1, M_{i,k}(1), M_{i,-k}(1)) - Y_i(0, M_{i,k}(1), M_{i,-k}(1)),$$

which gives the backyard effect while holding mediators at their backyard-treatment realization. The minuend is observed, but the subtrahend is a counterfactual quantity that we need to simulate (Tomz and Weeks 2013). We use the observed realizations of the mediators under the *IMBY treatment, but set the treatment to zero, and then predict the counterfactual outcome for each person by using the third regression model as presented above. By averaging across all observations, we have an estimate of the direct effect in the data. By repeating this for each of the cluster-adjusted bootstrap draws and across all imputations, we gain the entire distribution of this mediation quantity.

The indirect effects are separate for each mediator $k$, capturing how much each transmits the total *IMBY effect. Specifically, we have

$$Y_i(1, M_{i,k}(1), M_{i,-k}(1)) - Y_i(1, M_{i,k}(0), M_{i,-k}(1))$$

which captures the change in the support by moving the $k^{th}$ mediator from untreated to the treated realization while the treatment and the other mediators ($-k$) stay at their *IMBY level. Once again, the subtrahend is an unobserved quantity that we will proceed analogously to above.
5 Results

Before discussing the results, we want to examine the realizations of *IMBY treatments. As people were online as they took the survey, we performed an IP address lookup to get the longitude/latitude of the connecting server’s location.\footnote{We did not retain the IP address itself for privacy reasons.} The median distance between a person’s internet connection and the *IMBY city is 175km, whereas it is almost eleven times that distance for non-*IMBY city. We show the histograms of all distances in Figure A.3 in the appendix. Of course, the location of one’s IP address need not reflect one’s home-state allegiance and ties as one could take the survey on MTurk while traveling, for example. However, in general, our approach seems to work even though there are large outliers.\footnote{In one case, the IMBY case is 3,000km further away than the distant case. In future analysis and with additional data, we want to restrict our analysis to cases in which IP addresses are very close to the IMBY city. For example, about 1/3 of IMBY matches are within 100km whereas only a 1/3 of a percent is for the non-IMBY randomizations. Figure A.4 in the appendix shows all IMBY/non-IMBY city pairs on the map of the United States.}

![Figure 1: Backyard effects on support for policy.](image)

We discuss the results in the same order as we introduced the estimators. First, we turn to the total backyard effects for each scenario. We obtain this by graphing the coefficient $\alpha_0$ from the regression. Figure 1 shows these results for both of our cases. Along the
x-axis is the effect size in percentage-points, and the y-axis denotes the respective cases (NIMBY atop, YIMBY on the bottom). Each dot is the median estimate, the horizontal lines give the 90% central confidence interval. Consider first the case of the autocrats’ victory and the US cut in existing aid. When the city where the mentioned aid cut occurs is in the respondent’s home-state, then support is $5.0 \ [3.1, 7.0]$ percentage-points lower than otherwise. Similarly, if the benefits from an aid increase to fund democratic practices after the pro-democracy candidate wins accrue to the respondent’s economy, support increases by $4.4 \ [2.6, 6.0]$ percentage-points. Therefore, we have evidence for NIMBY and YIMBY effects in democracy promotion.

![Figure 2: Backyard effect on mediators. Each panel shows on the x-axis again the percentage-point change in support of the mediator question which is depicted on the y-axis. The left hand panel gives the results for the YIMBY case, the right hand for the NIMBY case. The effects are $\beta_{0k}$ for each mediator and each case.](image)

Individuals’ evaluations of democracy promotion are presumably multidimensional. To assess how backyard ramifications change how people assess the policies, we look to the second statistical model. We regress each of the five mediators on the *IMBY dummy and the
other covariates. Figure 2 shows each corresponding coefficient $\beta_{0k}$. The x-axes are analogous to the figure from before; the y-axis give the separate mediators which are regressed on the treatment variables; and the panels show the YIMBY (left) and NIMBY (right) scenarios. The results for the democrat-wins case follow expectations: if benefits are captured locally, then the self-serving bias assesses that helping fragile democracies is important in general and that the cost/benefits of the present case are particularly good. Also, targeting the losing side or signing a trade deal are less likely to be a preferred alternative. All in all, these attitudes are more protective and supportive of the policy when benefits occur locally.

Turning to the NIMBY effect in which costs would affect the respondent due to a cut in aid, we see that some things are similar in that they are flipped in direction. The NIMBY effect leads to a worse cost/benefit assessment as well as helping the losing democrat (13.4 [10.7, 16.1]) and by seeking out a trade deal (3.6 [1.5, 5.7]) instead of the aid sanction becomes more appealing. Odd, however barely not statistically significant, is the NIMBY effect on whether it is important to help: even though the policy is more costly to the survey-taker, the principle to help fragile democracies is heightened (largely).

The next step in the causal process is the effect of mediators on the support of the policy. It is important to note that as the mediators are actually survey responses so that the results do not enjoy experimental identification. We work with the third regression model that we introduced above. Figure 3 shows the effect of each mediator listed on the y-axis on the support for the policy; the effects ($\gamma_{0k}$) are shown on the x-axis. Once again, the YIMBY case is on the left, NIMBY to the right.

The results for YIMBY again are clearer. If a survey-taker thinks it is important to help fragile democracies in general and judges the cost/benefits acceptable, support for the policy goes up sizably. Further, as any of the three alternative policies is judged to be preferable, support declines. In the autocrat-wins scenario, things are muted as the general normative valuation as well as the appreciation of the UN and trade alternatives do not matter. In
each case, the median coefficient is close to zero. The cost/benefits and the targeting of the losing side are consistent with the YIMBY case.

Last, we turn to the causal mediation effects which are our main quantities of interest. Figure 4 provides the results for each case. As we are decomposing the total effect of the treatments on the support, the figure shows the total effect as the reference point at the bottom of the panels.

Let’s start with the indirect effects. Recall that these are the effects of the backyard repercussions of the aid change that are transmitted by changing other attitudes that are in themselves relevant for supporting the policy. Therefore, each indirect effect is the difference between the support with treatment and mediators set to the backyard treatment ($Y_i(1, M_{i,k}(1), M_{i,-k}(1))$) and the support with the mediator of interest set to the not-*IMBY level while the treatment and the other mediators at *IMBY status ($Y_i(1, M_{i,k}(0), M_{i,-k}(1))$).

Consider the first item on the y-axis from the top in the top panel of Figure 4. Under the
Figure 4: Mediation effects. Each panel shows causal mediation effects under either the YIMBY (top) or the NIMBY case (bottom). The x-axis is the same as before. The y-axes show the estimands.

YIMBY treatment if the democrat wins in the vignette, 0.8 [0.6, 1.1] percentage-points of the total effect of 4.4 [2.6, 6.0] is due to changes in belief whether it is generally important to help fragile democracies; that is 20% [12, 35] of the total effect. The cost/benefit consideration transmits similarly much of the YIMBY effect (16% [9, 30]). In contrast, all three alternative policy approaches mediate barely any of the YIMBY effect; their median estimates sum to 0.3 percentage-points or just 7% of the total effect.

The medians of all indirect effects account for 43% of the total effect when the contractor of the aid increase resides in a respondent’s home state. The remainder, accordingly, are the direct effects. They make up 2.5 [0.7, 4.2] percentage-points of the total effect of 4.3

\[27\]Figure A.2 in the appendix shows all effects relative to the total effect.
percentage-points.

When the autocrat wins and an aid cut is proposed, results are somewhat symmetric to the just-discussed YIMBY case. The cost/benefit evaluation shifts negatively and transmits the NIMBY effect (19% [10, 33] of the total effect), and the alternative to target the losing, pro-democratic side mediates 19.7% [11.2, 35.4] of the total effect. In contrast, the general normative assessment plays no role in the transmission of the NIMBY affect as it itself does not matter for the evaluation of the policy. In line with our YIMBY effects, the direct effects carry most of the total effect. Of the total effect of -5.0 [-7.0, -3.1] percentage-points, the direct effects account for -3.1 [-5.1, -1.1] percentage-points.

6 Conclusion

By cueing respondents to the domestic locus of (one of two) foreign policy tools and then asking them a series of followup questions, we seek to uncover not only whether citizens have parochial interests, but how they justify such preferences. We find strong evidence of *IMBY effects; respondents are more supportive of aid that benefits organizations in their area, and are less supportive of sanctions that harm local firms. Relatively little of these treatment effects are mediated by the respondents’ moral considerations, evaluations of relative benefits of the policy (broadly), or preferences for alternate policies. Accordingly, we provide initial evidence that citizens simply prefer the pursuit of foreign policy that promotes and protects their local economy, irrespective of the stated, international aims of the policy in question.

Future research would benefit from considering a wider area of potential mediators that might explain the *IMBY effects we observe. For example, perhaps a local impact of foreign policy cues citizens to consider the national security implications of the policy. It is possible generally that policies intended to influence democracy thousands of miles away are not deemed salient to most citizens, but that a local impact alerts citizens to the fact that
foreign policy is meaningful. Future research also should address the piece of the puzzle about which we still know relatively little: how does public opinion on aid and sanctions—particularly with respect to geographic considerations—affect actual policy. One implication of our findings is that citizens support democracy aid grants less, and support aid sanctions more, when these policies benefit the economy of a more distant US state. Particularly for the case of democracy sanctions, future research could benefit from examining whether the use of such policy is more common when the negative domestic economic consequences are more concentrated; in this case, fewer citizens would oppose sanctions in accordance with the NIMBY effect. Indeed, we might find that sanctions that are more costly to the US are nonetheless easy to implement if the negative impact is concentrated to fewer geographic areas.
References


Schumpeter, Joseph A. 1942. “Capitalism, Socialism and Democracy.”.


**URL:** https://www.whitehouse.gov/issues/foreign-policy


Backyard effects in democracy promotion: Evidence from a survey experiment

Web Appendix
A  Details about survey experiment design

A.1  Subject recruitment

In February 2016, we posted a job on Amazon’s Mechanical Turk to seek participants for a study on foreign policy preferences. Pay varied between $0.25-0.40 for a survey taking about 3–4 minutes. We restricted the visibility of the ad to our target population (adults living in the United States). Subjects were directed to one of the author’s website to take the survey; once completed, they were shown a unique lengthy alpha-numeric code which served as proof on MTurk that they took the survey. Once we confirmed the code, subjects were paid.

Crucially, we emphasized on several occasions that there will be a second round of the survey which will be shorter and would double the pay. We used a short part of participants’ MTurk IDs and something random (one’s mother’s month of birth) in order to link responses across rounds. The second round was conducted three weeks later. In total, we managed to recruit 711 individuals and 71% of these came back for the second round. In Section A.5 we show to which extend it was random that people chose to participate for a second round.

A.2  Debriefing statement

After taking the survey in the first round, people say this debriefing statement:

“Thank you for agreeing to participate in this study! Your participation was very valuable to us. However, even more valuable would be if you were to respond to the second wave as well. We will invite you in 2–3 weeks to participate again for bonus pay. The purpose of this study is to investigate how people evaluate complex foreign policy decisions. A full debriefing statement will be shown after the second wave, or by contacting the Principal Investigators in about a month.”

A.3  Demographic variables and descriptives

We collected demographic data as well for which relied (mostly) on the wording of the Cooperative Congressional Election Survey (Vavreck and Rivers 2008; Ansolabehere and Rivers 2013).

- **Age.** We asked, “In what year were you born?” and then calculated the age.
- **Gender.** “Are you male or female?”
- **State.** “Which state do you live in?” Participants could choose from 50 states plus District of Columbia.
- **ZIP code.** “In what ZIP code do you currently reside?”
• **Education.** “What is the highest level of education you have completed?” The possible answers were: “No high school”, “high school”, “some college”, “2-year college degree (associates)”, “4-year college degree (BA, BS)”, and “post graduate”.

• **Retrospective economic evaluation.** Specifically, the question was: “Would you say that OVER THE PAST YEAR the nation’s economy has...?” with answer options: “gotten much better”, “gotten better”, “stayed about the same”, “gotten worse”, and “gotten much worse.”

A.4 Source list of cities used for *IMBY treatments*

Albuquerque, NM; Anchorage, AK; Atlanta, GA; Austin, TX; Baltimore, MD; Billings, MT; Birmingham, AL; Boise, ID; Boston, MA; Bridgeport, CT; Burlington, VT; Charleston, WV; Charlotte, NC; Cheyenne, WY; Chicago, IL; Cleveland, OH; Columbia, SC; Columbus, OH; Dallas, TX; Denver, CO; Des Moines, IA; Detroit, MI; El Paso, TX; Fargo, ND; Fort Worth, TX; Fresno, CA; Honolulu, HI; Houston, TX; Indianapolis, IN; Jackson, MS; Jacksonville, FL; Kansas City, MO; Las Vegas, NV; Little Rock, AR; Los Angeles, CA; Louisville, KY; Manchester, NH; Memphis, TN; Miami, FL; Milwaukee, WI; Minneapolis, MN; Nashville, TN; New Orleans, LA; New York, NY; Newark, NJ; Oklahoma City, OK; Omaha, NE; Philadelphia, PA; Phoenix, AZ; Portland, ME; Portland, OR; Providence, RI; Salt Lake City, UT; San Antonio, TX; San Diego, CA; San Francisco, CA; Seattle, WA; Sioux Falls, SD; Tucson, AZ; Virginia Beach, VA; Washington, DC; Wichita, KS; Wilmington, DE
### Participation in Round 2

Table A.1: **Participation in round 2.** This table shows the results from a linear-probability model which regresses a dummy of whether one did not participate in round 2 of our survey on demographics as well as on the experienced case (‘Democrat wins’) and whether one saw the *IMBY case or not.

<table>
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<tr>
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<tr>
<td></td>
<td>(−0.04, −0.01)</td>
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<tr>
<td>Gender</td>
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</tr>
<tr>
<td></td>
<td>(−0.44, −0.02)</td>
</tr>
<tr>
<td>Ideology</td>
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<tr>
<td></td>
<td>(−0.09, 0.04)</td>
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<tr>
<td>‘Life got worse’</td>
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</tr>
<tr>
<td></td>
<td>(−0.34, 0.09)</td>
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<tr>
<td>‘Democrat wins’ in R1</td>
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</tr>
<tr>
<td></td>
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</tr>
<tr>
<td>*IMBY in R1</td>
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<tr>
<td></td>
<td>(−0.23, 0.17)</td>
</tr>
<tr>
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<td></td>
<td>(0.06, 0.99)</td>
</tr>
<tr>
<td>N</td>
<td>711</td>
</tr>
</tbody>
</table>
A.6 Sample screens

Figure A.1: Examples of survey screens. The top screen shows a case in which the pro-democracy leader wins and the U.S. government provides an increase in aid. The bottom image shows the opposite case. Everything that is underlined is subject to randomized as explained in the text.
A.7 Additional figures

**Figure A.2:** Indirect effects as a percentage of the total effect. Constructed analogously to Figure 4 except that the x-axis depicts the percentage of the total effect that the indirect effect accounts for.
Figure A.3: Distance from respondent to randomized city of aid project. Each panel shows a density plot of the geographic distance in kilometers from the longitude/latitude of the survey-taker’s IP address to the location of the city that he/she was randomly paired with. The left hand panel shows the distances for the non-NIMBY/YIMBY cases, the right hand for the home-state cities. Notice that the x-axes are log-scaled.
Figure A.4: Map with all realized pairs of cities. Each line connecting two cities is a pair of cities that was affected by a change in aid over the two survey rounds. The map does not show which was the in-state and which the out-state city.