Experience Open to Interpretation: US Presidents and Economic Coercion

Dawid Walentek

dawid.walentek@ugent.be

Ghent University

Ghent Institute for International and European Studies

Abstract

Research offers a conflicting account of the effect of economic sanctions on the popularity of political leaders, suggesting that the public either rewards an active foreign policy or punishes for the use of coercion. What is more, scholarship assumes that political leaders do not observe ambiguous signals from the public and read the changes of public opinion in an unbiased fashion. Yet, literature on beliefs formation and on difficulties of political leaders to recognise public preferences signal that policy leaders may be susceptible to confirmation bias in respect to audience effects of sanctions. In this article, I develop a formal model of behaviour of political leaders in respect to sanctions in an environment where signals from the public are open to interpretation and predict that US presidents are likely to anticipate a benefit resulting from sanctions imposition and engage in coercion when approval ratings are decreasing. I test the theory using an event study design and data on approval ratings of US presidents. The empirical analysis identifies an anticipation effect for US presidents, with lower approval ratings up to seven months prior to imposition of economic sanctions. Yet, I do not observe an audience effect of the imposition of sanctions.

Introduction

A few weeks before the Russian invasion of Ukraine the US President Joe Biden publicly warned that Russia will pay "a heavy price" if it enters Ukrainian territory.[1] The word price was not used only in a figurative sense, as the US President referred to a package of economic sanctions crafted against Russia. After the introduction of sanctions by Biden against Russia a puzzling message emerged — news headlines ranging from claims that the approval rating of the President is raising thanks to imposition of sanctions, to insights on why the approval rating is not changing and discussion on whether we should expect it to change at all.[2] It seems that while the dynamics of the presidential approval ratings following sanctions is a salient part of the public discourse, their effect on approval are open to interpretation. Current empirical research, analogously, argues that US presidents observe an increase in approval ratings when using economic sanctions, regardless of the outcome, as the public rewards active foreign policy (Whang, 2011) or that US presidents observe a decline in approval ratings following imposition of sanctions, driven by a public dislike for costly foreign policy decisions (Webb, 2018).

Political leaders seeking electoral success think about how their actions resonate with the public. High public approval rating is a 'desirable commodity' pursued by political leaders, as it increases prospects of reelection, legitimises policy choices, offers a positive media appearance and helps political associates competing in elections (Donovan et al., 2020), and foreign policy decisions appear to influence public opinion (Whang, 2011; Webb, 2018; Holsti, 1992; Kertzer and Zeitzoff, 2017). Consequently, we may expect that foreign policy choice of political leaders may be partially motivated by the prospect of domestic audience costs or benefits. However, political leaders operate in an environment with approval rating driven by, for example, the economic or the electoral cycle (Hardie et al., 2020; Hibbs, 2000; Cuzán, 2022; Guntermann et al., 2021; Wlezien, 2015, 2017; Stimson, 1976; Fauvelle-Aymar and Stegmaier, 2013), while also undertaking a number of policy decisions relevant for the public across many policy areas at the same time. This may influence the ability of political leaders to disentangle the effect of an individual policy on the sentiments of the public, and undermine learning from the past experiences of other leaders. Thus, cycles and overlapping events may introduce uncertainty into the interpretation of changes in proxies of sentiments of the public, for example approval rating polls. This, in turn, may reduce the ability of political leaders to assess how voters respond to policy decisions.

Yet, current theory predominantly assumes that political leaders are neither biased in their estimates of the domestic audience costs or benefits of foreign policy choices, nor that they are faced with approval ratings movements open to interpretation. At the same time, the literature offers

[1] https://www.theguardian.com/us-news/live/2022/jan/20/biden-presidency-democrats-senate-voting-rights-trump-live-latestHyperlink to article in The Guardian

[2] Respectively, https://edition.cnn.com/2022/03/13/politics/biden-approval-gas-prices-ukraine/index.html, https://edition.cnn.com/2022/03/31/politics/biden-polling-inflation-russia-ukraine/index.html, and https://edition.cnn.com/2022/03/13/politics/biden-approval-gas-prices-ukraine/index.html

very few insights on the presence and size of the effect that foreign policy decisions have on public opinion — both at the threat stage (Kertzer and Brutger, 2016; Levendusky and Horowitz, 2012; Tomz, 2007; Davies and Johns, 2013) and when engaging in coercion (Whang, 2011; Webb, 2018), and the insights are predominantly drawn from survey experiments, not actual approval ratings. In fact, scholarship frequently assumes presence of the audience cost for issuing empty threats or audience benefit for engaging in coercion based on the behaviour of the agents, inferring it indirectly from the choices of the political leaders (Walentek et al., 2021; Goenner, 2007; Wallace, 2013; Eyerman and Roberta, 1996; Gelpi and Griesdorf, 2001; Partell and Palmer, 1999; Drezner, 2003). This stands in contrast to a prospect that "human belief formation is often very different from that posited in the standard rational choice model" (Minozzi, 2013) and rests on an assumption that political leaders systematically correctly estimate audience benefits and costs, while researchers themselves mark these phenomena as "difficult to study with observational data" (Kertzer and Brutger, 2016), the only type of data available to the political leaders.

Given the literature on confirmation bias in the context of politics (Donovan et al., 2020; Evans and Pickup, 2010; Leeper and Slothuus, 2014), challenges in identifying the drivers of approval ratings (Wlezien, 2015, 2017; Cuzán, 2022) and difficulties of policy leaders to recognise the preferences of the public (Walgrave et al., 2023), we may ask whether political leaders are susceptible to confirmation bias too. If yes, we may observe political leaders to behave as if audience benefits or costs are present — while, in fact, the public may be indifferent to particular policy choices of a political leader. As a result, we may observe indirect evidence for audience costs and benefits, derived from the (rational) actions of a political leader, while the change of sentiments among the public is not an actual phenomena and there is no benefit or cost to the exercise of coercion in foreign policy or both effect are present at the same time, balancing out one another.

The objective of this article is two-fold. First, it sets out to offer a theoretical contribution on the behaviour of political leaders in an environment where information on the dynamics of public opinion is open to interpretation and leaders have prior assumptions about the public in respect to coercion in international relations. Then, the article offers an empirical contribution and assesses whether US presidents anticipate a boost in popularity following imposition of sanctions and are more likely to engage in economic coercion if they observe a decrease in approval ratings.

I use the data on monthly approval ratings for US presidents from the Truman to the Bush presidency — a total of 731 presidency-months based on Gallup data — and take advantage of the updated TIES data set (Morgan et al., 2014) that offers detailed information on the imposition of economic sanctions by the US presidents and also includes observations on threats-only of sanctions. I employ an event study design where months that observed an imposition of sanctions serve as 'treated', while months before a sanction allow us to establish the presence of an anticipation effect and months after a sanction is imposed allow to examine a potential decay of the audience effect.

I find that US president behave as if they are likely to observe an audience effect for the use of economic coercion. US presidents systematically observe lower approval ratings up to seven months prior to the decision to engage in economic sanctions — signalling an anticipation effect. In addition, I observe suggestive evidence that US presidents do not experience higher or lower levels of popularity following imposition of economic sanctions — in contrast to the past work on the effects of imposition of economic sanctions on approval ratings. This may signal indifference of the public to economic coercion; alternatively it mat be driven by balancing of the audience benefits and costs for imposition of sanctions — bridging the existing literature and offering scope for future research (Whang, 2011; Webb, 2018; Kertzer and Brutger, 2016).

Consequently, the findings of this article support the argument that foreign policy decisions are, for a part, driven by audience effects; US presidents do appear to consider public opinion when making foreign policy choices. Yet, the data signals that the expected benefits do not materialise or do not outweigh the costs. Thus, this work shows the importance to reflect on beliefs formation among decision-makers and highlights that we ought to be careful assuming that political leaders' beliefs overlap with the true sentiments of the public.

Information open to interpretation

Scholars of conflict in international relations traditionally depict the choices of the policy-maker as a sequence of decisions taken by a rational and utility-maximising agent (Schultz, 1999, 2001; Fearon, 1997, 1994; Bas and Schub, 2018). At first an agent is faced with a policy issue, resulting from harmful actions of another state (e.g., abuse of human rights or violation of trade rules), and has to decide whether to threaten coercion and demand a return to the status quo ante or accept the status quo. If the agent chooses to accept the status quo the game finishes and if the agent chooses to threaten coercion the game moves to the next stage, where the target of the threats can either back down and withdraw the harmful policy or stand firm. In case of the latter, the agent has to decide whether to realise the threat of coercion or not. If the agent decided to exercise coercion (e.g., introduce economic sanctions) they observe an audience benefit for taking action (e.g, higher approval rating) and a material/economic cost (e.g., financial costs of sanctions). However, if the agent stops at an empty threat and does not engage in coercion, they observe an audience cost, as voters punish inconsistency, but does not bear the material/economic cost.

Yet, there is no consensus in the scholarship on the size and the mere presence of the audience costs and benefits. Current research indicate a number of possibilities; namely — that the true state of public opinion is that (i) voters punish for empty threats (Levendusky and Horowitz, 2012; Davies and Johns, 2013; Nomikos and Sambanis, 2019; Tomz, 2007), (ii) punish for the use of threats (Kertzer and Brutger, 2016), (iii) reward coercive action (Whang, 2011; McLean and Whang,

2014) or (iv) penalise coercive action (Webb, 2018). While the theoretical work assumes that the policy makers are able to clearly identify the direction of change among voters' sentiments resulting from their (in)action in case of conflict, supporting empirical evidence is conflicting. In addition, current theoretical models do not allow for any information on the movement in the approval rating to be open to interpretation by political leaders, a potential source of (confirmation) bias in the assessment of the effects of a policy. This assumption appears remote from the existing evidence about belief-formation among individuals (Minozzi, 2013) and, given the lack of consensus and limited evidence among the research community for or against the presence or absence of audience cost and benefits, leads us to question whether political leaders are able to identify the true values of audience costs and benefits themselves. Given the work on confirmation bias (Donovan et al., 2020; Evans and Pickup, 2010; Leeper and Slothuus, 2014), we may further develop this question and ask whether political leaders do not read information open to interpretation in a biased way, based on varying and imperfect beliefs about the voters preferences (Walgrave et al., 2023). To address this, I propose a model of belief updating in a setting open to interpretation and randomly distributed prior assumptions, based on the work of Fryer et al. (2019).

Simple model of beliefs updating

An agent seeking electoral success is making choices over time, wanting to maximise its approval rating; the agent is updating its beliefs about the true state of public opinion accordingly. Economic sanctions matter in an economic and political sense and generate impact on approval ratings of leaders through audience costs and benefits. It is important for an agent seeking electoral success to identify the true audience cost of not following up on a threat of sanctions (A) and the audience benefits for imposing sanctions (B). While there is also a prospect that the audience is indifferent about not following up on threats (A') or about imposing sanctions (B') or both. Thus, there are four possible states of public opinion in respect to economic coercion, depicted by the following pairs $\omega \in A, B; A, B'; A', B; A', B'$. An agent is exposed to a series of signals of public opinion — approval rating data from polls — described by s_t and with one signal at a given date, where $t \in 1, 2, 3, ...$ Signals belong to a set a, b, a', b', aa', bb', where a indicates to the agent that A is the true state of public opinion, b indicates that B is the true state, a' indicates that A' is the true state, b' indicate that B' is the true state, aa' is a signal open to interpretation about the audience cost for empty threats, and bb' is an ambiguous signal about audience benefit for sanction imposition. An ambiguous signal is a movement (or its lack) in the opinion polls that does not offer a clear indication of the sentiments of public opinion towards a policy choice of the agent.

We assume that with probability equal to q, the signal sequence starts as a or a' or b or b' and matches the true public opinion with a probability $p > 1/2$ for both audience costs and benefits. Yet, the agent faces an exogenous probability equal to π that the signal becomes ambiguous (i.e.,

aa' or bb'). Let us also assume that the agent's prior belief about the state of public opinion is $\omega = AB$, following a probability $\lambda_0 \in (0, 1)$. From this simple model we can conclude that a Bayesian-updating agent will converge to place a probability equal to 1 on the true state of public opinion in respect to domestic audience costs and benefits if we allow for a sufficient sequence of signals, and signals open to interpretation can be comfortably ignored by the agent.

Convergence on coercion

The simple model, leading to convergence of agent's beliefs on the true state of public opinion, assumes a long-term perspective for the agent. Decision-makers may not be granted such a long-term perspective, as their time in the office may be constrained and foreign policy crises appear independent of their tenure in the office. In addition, given the multiple channels that influence public opinion decision-makers are likely to relatively frequently observe signals open to interpretation, undermining the prospects of ignoring them in the short run. Thus, we model an agent that operates in a short-term environment, where ambiguous signals $s_t = aa'$ or $s_t = bb'$ are interpreted based on past experience t.

Let us look at an example. The true state of public opinion is $\omega = A, B$ (i.e., the public punishes for empty threats and rewards the use of coercion) and the share of signals open to interpretation is $\pi = 1/2$ and the probability that a signal represents the true sentiments of the public is $p > 2/3$. We assume that with a probability equal to $1/4$ the signal sequences begins with a or a' or b or b'. For this specification, a sequence of signals may be $b, aa', bb', b, a, bb', a', aa', b', aa', ...$

In our model, the agent interprets $s_t = aa'$ $s_t = bb'$ using the current belief $\lambda_t - 1$, consequently $s_t = aa'$ is interpreted as a if $\lambda_t - 1 > 1/2$ and as a' if $\lambda_t - 1 < 1/2$. By the same token, $s_t = bb'$ is interpreted as b if $\lambda_t - 1 > 1/2$ and as b' if $\lambda_t - 1 < 1/2$.

Let us assume that an agent observes a belief where $\lambda_0 = 3/4$ both for audience costs and benefits. After observing the first signal of public opinion $s_1 = b$ from our series the agent updates the belief about audience benefits to $\lambda_{1,b} = 6/7$, while beliefs about costs remain at $\lambda_{1,a} = 3/4$. Then, when observing the following reading of public opinion, the agent interpreters $s_2 = aa'$ as a and belief about audience cost becomes $\lambda_{2,a} = 6/7$. At $s_3 = bb'$ the agent interprets the ambiguous signal as b and updates the belief about audience benefits to $\lambda_{3,b} = 12/13$. Our example sequence would eventually look as $b, a, b, b, a, b, a', a, b', a, ...$, and the beliefs about audience costs and benefits would converge towards 1.

We can also assume an agent with another set of prior beliefs about audience costs and benefits — where $\lambda_0 = 1/4$ for both — and assume that they observe the same sequence of signals. After observing the first signal $s_1 = b$ the agent updates the belief about audience benefits to $\lambda_{1,b} = 2/5$, while beliefs about costs remain at $\lambda_{1,a} = 1/4$. Next, the agent interpreters $s_2 = aa'$ as a' and belief about audience cost updates to $\lambda_{2,a} = 1/7$. At $s_3 = bb'$ the agent interprets the ambiguous signal as b' resulting in belief about audience benefits equal to $\lambda_{3,b} = 1/4$. Our example sequence would eventually look as $b, a', b', b, a, b', a', a', b', a', ...$, and the beliefs about audience costs and benefits would converge towards 0.

Most importantly, the two examples show that if the prior beliefs do not follow the same parameter values, for example $\lambda_a = 3/4$ for audience costs and $\lambda_b = 1/4$ for audience benefits, we will observe that the agent update the beliefs in opposing directions — where one belief converges towards 1 and the other towards 0.

The conclusion from the simple updating model is that two agents in a short-term setting with different, yet not extreme, prior beliefs about the public preferences in respect to economic sanctions will arrive at different conclusion when observing the same sequence of approval rating data.

Thus, if we assume a distribution of prior beliefs about the domestic audience costs and domestic audience benefits that is not extreme and random among a population of agents, we arrive at an outcome where agents will hold an equal share of the beliefs that set AB; A'B; AB' and A'B' represent the true state of public opinion. This leads to a population of agents predominantly, by a factor of *three to one*, inclined impose sanction — either because of the belief about the role of the audience benefit (A'B) or audience cost (A,B'), or both types of costs (AB). Consequently, we may expect that political leaders, on average, are likely to engage in economic coercion, if experiencing lower approval ratings — in order to avoid a further deterioration of the ratings, to boost it, or both. This allows us to form the following:

H: A political leaders is likely to impose economic sanctions if experiencing a period of lower approval ratings.

Data & Methods

In this study, I match the updated TIES data set on economic sanctions (Morgan et al., 2014) with monthly data on approval ratings of US presidents based on the Gallup polls and monthly economic data from the US Bureau of Labor Statistics.[3] In the article, I focus on presidency months, rather than individual sanctions, and employ an event study model for the statistical analysis of the dynamics of approval ratings of US presidents resulting from the use of economic coercion.

[3]The TIES data set is available at: http://sanctions.web.unc.edu. Gallup is the only provider of approval data that has consistently asked the same question over time, namely: "Do you approve or disapprove of the way [president name] is handling his job as President?" Approval data has been drawn for the website of the Presidency Project at: https://www.presidency.ucsb.edu.

Unit of analysis in a complex setting

The data on economic sanctions and approval ratings of US presidents poses a challenge in its unit of analysis. In the data set, I observe 731 presidency-months from year 1945 to year 2005. In this timeframe, I observe 694 policy actions — either imposition of economic sanctions or threats-only. However, the foreign policy actions are not limited to one per month — the 694 decisions took place in 282 months that are either scatted across the presidency terms or form spells of consecutive months of sanctions imposition. As a result more than half of the months with policy decisions related to economic coercion encompass both trade and security-related sanctions and combine imposition with threats only, while leaving 449 months with no policy action related to economic coercion at all.

For example, in July 1995 the Clinton administration took four policy decision, where three were to impose sanctions and one was a threat only, and three decisions were related to security concerns while one was related to trade considerations. A month earlier, in June 1995, President Clinton took seven decisions on economic coercion and July 1995 marks an end of spell of eight months of consecutive imposition of economic sanctions — in all cases, bar two months, with multiple sanctions in each month and a total of 35 policy decisions taken between April and July 1995. Consequently, if our unit of analysis is only an imposed sanction and we look at the change in the approval rating following an imposition for many observations we run into three problems. First, an identification problem arises, as the same approval rating change may be assigned to many policy choices. Second, when sanctions are imposed in a spell, we run the risk of auto-correlation in the error term and biased inferences, if spells are used as a benchmark. Third, omitted variable bias sets in, as a number of months have a substantial share of threats-only and, due to potential audience costs, we may observe an actual decrease in the approval ratings.

In Figure 1, I offer a visual representation of the data used in this study. Each rectangle represents one of the 731 presidency months and the information is structured following the time spent in office by each president. Not all US presidents served full 96 months, hence a white tail for a number of presidents. Red rectangles indicate a month with at least one economic sanction imposed, while blue rectangles are months without imposition of economic sanctions.

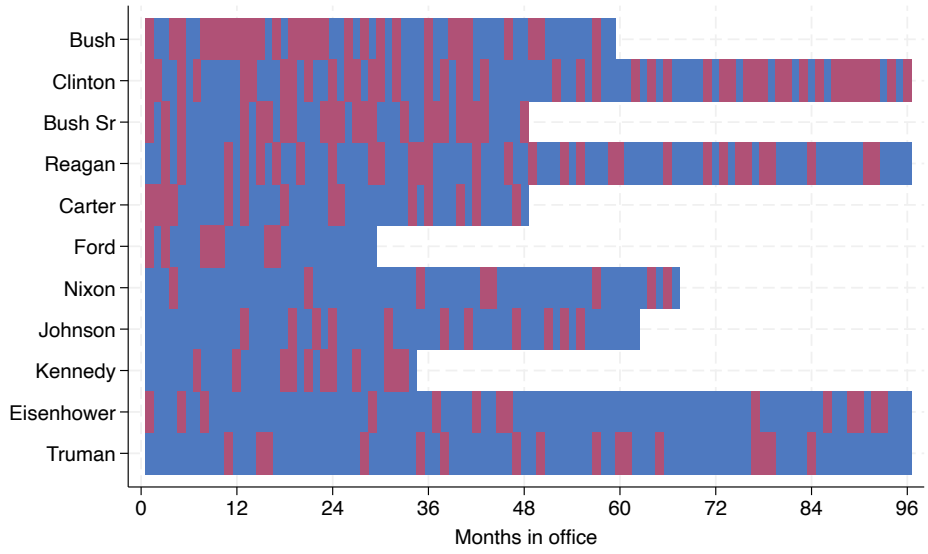


Figure 1: Timeline of sanctions imposition. Each rectangle represents one month. Red indicates months with at least one sanction imposed, blue indicates no imposition.

In this study, I divide the sample into three categories. First, treatment months (N=207), defined as months with imposition of at least one economic sanction. Second, the time before the treatment (N=266), where I identify up to ten months before the imposition of sanctions as leads (i.e., months leading up to treatment). Third, the time after the imposition of sanctions (N=226), where I assign a lag status to up to two months following an imposition. Each presidency month can observe only one of these three categories to avoid double-counting (e.g., a presidency month being a treatment month and a lead for another treatment month at the same time) and the resulting biased inference. If there is a spell of consecutive treatment months, leads are generated only up to the first month of a spell and lags are generated from the last month of a spell. What is more, lags take priority over leads — in other words months that follow an imposition are always coded as lags, in order to avoid introduction into the data presidency months coded as leads with approval rating values potentially inflated by recent imposition of sanctions.

Event study design

In this article, I employ a timing-based event study design (Miller, 2023; Clarke and Schythe, 2020), given that in the data I do not identify US presidents that have never issued economic sanctions (i.e., never-treated units), the timing of the foreign policy challenges that may be addressed with economic coercion is to a degree random and I expect the duration of the audience effect of sanctions imposition to be relatively short-lived.[4] The even study model used in this study is shown in Equation 1 and has the following specification:

[4]The assumption of random appearance of foreign policy challenges implies that US presidents do not have a clear foresight of the timing of crises that they will have to address during their presidency.

$$y_{(i,t)} = \sum_{j=2}^{J} \beta_j (Lag\ j)_{i,t} + \sum_{k=1}^{K} \beta_j (Lead\ k)_{i,t} + \lambda_i + \delta_t + \gamma_{i,t} + \epsilon \quad (1)$$

where y is the outcome variable, *monthly mean approval* based on Gallup's data, for a set of sanctions i at time t. Lags and leads are dummy variables that indicate the distance in months to the treatment presidency month. In the model the baseline is set at one month before the imposition of sanctions and I look for and anticipation effect, treatment effect and its potential decay over time. Thus, I am interested in the β coefficients for the leads and the lags. In order to identify an anticipation effect, I would expect the periods prior to the treatment to observe a negative and statistically significant coefficient. To establish presence of audience benefits resulting from sanctions imposition, I would expect the treatment period to observe a positive and statistically significant coefficient; while for audience costs for sanctions imposition the opposite would hold.[5] I also expect the treatment effect to decrease over time, what should be visible in the coefficient size of the lags — currently, I investigate up to two months after the sanctions are imposed. Next, λ and δ are, respectively, sanctions (*id*) and time (*election proximity*) specific fixed effects. The former accounts for time invariant characteristics of each imposition and covers a sanction or a set of sanctions from the first lead to the last lag. Given that the variable *id* is embedded within presidency it also addresses potential time invariant aspects of approval rating specific to each US president. The latter fixed effect — *election proximity* — accounts for time-specific variation in the approval rating of US presidents driven by the electoral cycle that holds for all US presidents. Finally, γ is a vector of time-varying economic control variables — *unemployment* and *inflation* — drawn from the US Bureau of Labor Statistics — that correlate with presidential approval rates, and ϵ represents the unobserved error term.

To account for potential heterogeneity in the data — driven by the motivation for sanctions — I separate the presidency months into treatment with majority of policy actions related to security consideration (N=80) and treatment where majority of cases are trade related (N=54). Security issues are traditionally the domain of US presidents (Whang, 2011); thus, I would expect that periods related to presidency months where majority of sanctions concern security to show a different pattern to trade-centred treatment months.[6] Another source of heterogeneity in the effect of sanction on presidents' popularity may stem from empty threats and the associated audience costs (Levendusky and Horowitz, 2012; Davies and Johns, 2013; Nomikos and Sambanis, 2019; Tomz, 2007) — a dynamic not addressed by previous research on economic sanctions and approval ratings (Whang, 2011; Webb, 2018). In the heterogeneity test, I account for it by altering the sample to assign treatment month status only if majority of the policy action in a given month are imposed sanctions (N=170), while

[5] One could also imagine the two effects being present simultaneously and balancing out — a dynamic we cannot uncover with the existing survey data on US presidential approval rating.

[6] Following sanctions objectives are treated as security-related: "Contain Political Influence"; "Contain Military Behavior"; "Destabilize Regime"; "Release Citizens, Property, or Material"; "Solve Territorial Dispute"; "Deny Strategic Materials"; "Retaliate for Alliance or Alignment Choice"; "End Weapons/Materials Proliferation" and "Terminate Support of Non-State Actors". Trade objectives concern state aid, tariffs, devaluation, protectionist measures, trade restriction and market liberalisation reforms.

in the main study a weaker condition is used and at least one imposition is sufficient to assign a treatment status (N=207).

Finally, I offer a robustness test and assign a placebo treatment (N=209) with a number of month comparable to the main study at random observing imposition of economic sanctions. Assignment of the treatment months can be replicated using the analysis do-file, the seed for generating random treatment months is set to my ORCID number. I expect to observe a flat line, with none of the leads and lags being significantly different from the baseline month and point estimates showing limited and random movement around the mean (i.e., variation being close to zero).

Results

In this section, I discuss the results of a timing-based event study model based on Equation 1 and examine variation in approval rating of US presidents in the months before and after imposition of economic sanctions. I set out to identify an anticipation effect (prior to imposition) or treatment effect (following imposition) and its decline, and define treatment as a month with at least one sanction imposed by a US president. Results shown in the main text employ sanctions (*id*) and time (*election proximity*) specific fixed effects and include time-varying economic control variables (monthly US inflation and unemployment data). I illustrate the findings with appropriate figures in the main text and list the regression results in the Appendix in Table X, where I also offer a restricted model — excluding fixed effects or economic control variables (or both); there are no meaningful difference between the different regression specifications. Next, I discuss heterogeneity of the findings driven by the objective for economic coercion (security or trade-related sanctions) or resulting from presence of audience cost introducing downward bias into the main analysis. Finally, I offer a robustness test using a placebo treatment.

Identifying anticipation

In Figure 2, I illustrate the results from the event study model. All coefficients are estimated in relation to the baseline category — one month before imposition of economic coercion — depicted with a dotted vertical line. Regression coefficients to the left of the baseline are the monthly leads (negative values on the timeline), while coefficients to the right depict the treatment effect (zero on the timeline) and its decay with the use of monthly lags (positive value on the timeline). The furthest leads — indicating 10, 9 and 8 months prior to imposition — look like an ideal pre-trend, being close to zero and not statistically significantly different from the baseline. Yet, closer to imposition we see the leads to drop — indicating that periods closer to the decision to impose sanctions observe a decrease in the approval ratings of the US presidents. Lead 7 ($\beta = -6.7, p < 0.01$), Lead 6 ($\beta = -3.3, p < 0.05$) and Lead 4 ($\beta = -2.4, p < 0.09$) are statistically significantly lower relative to

the baseline, while the remaining leads' coefficients point in the same direction. A joint significance test of the leads indicates that they are negative and statistically significantly different from zero ($p < 0.05$). This pattern of approval ratings prior to the decision to impose economic sanctions — up to seven months prior to the policy decisions — signals an anticipation effect on the side of the US presidents.

This finding supports the hypothesis derived from the formal model of behaviour in a setting with information open to interpretation; it shows anticipation and signals that US presidents act as if imposition of economic sanctions will address the decline in approval ratings. Interestingly, the data provides suggestive evidence that imposition of sanctions does not actually result in the anticipated effect. Neither the coefficient for imposition nor for any of the lags is statistically different from the baseline. Also, the joint significance test for the treatment and lags yields a result that is not statistically significantly different from zero ($p < 0.81$). It is possible that the public is indifferent about economic sanctions or that the signal sent by the public is to weak to appear in monthly survey data on approval ratings. Alternatively, we may observe a balancing effect of audience benefits and and a belligerence cost for the use of economic coercion.

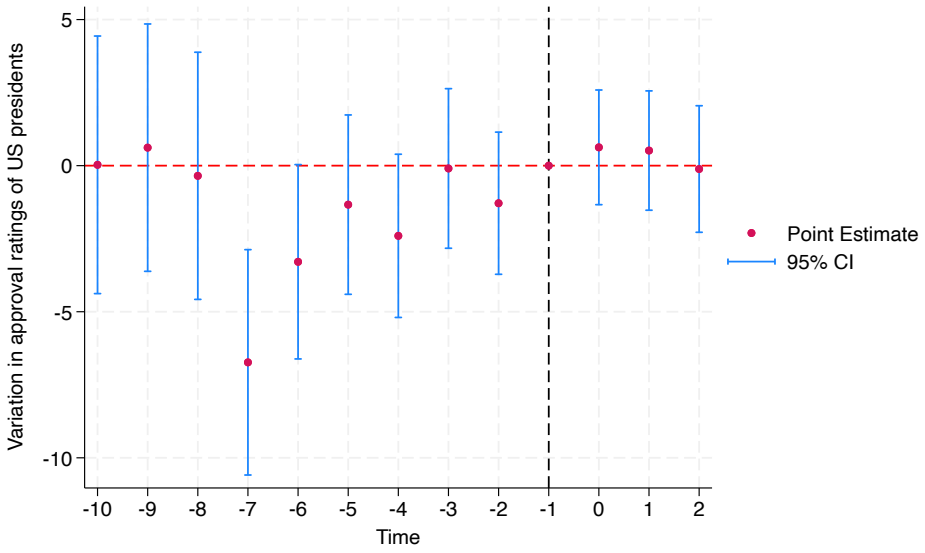


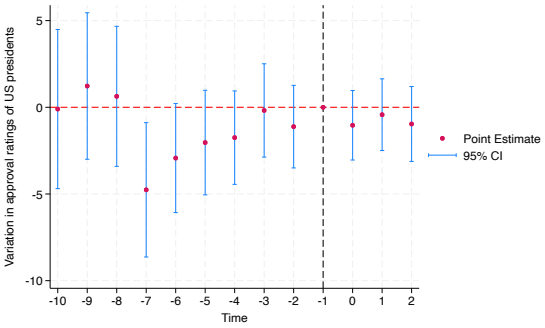
Figure 2: Variation in approval rating of US presidents before and after imposition of economic sanctions (in percentage points). Negative values represent months leading up to imposition (leads), positive values represent months following imposition (lags), zero is the month of imposition and month before imposition serves as reference (depicted with a dotted vertical line). Regression details are listed in the Appendix in Table A.1.

Heterogeneity

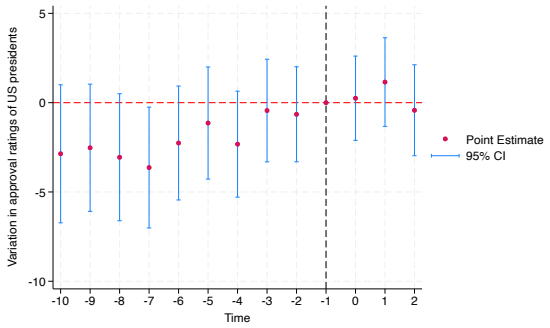
Next, I turn to investigate potential heterogeneity in the data and depict the results of these tests in Figure 3. To begin, I asses the effect of limiting the treatment months to those observing majority of policy actions being imposed sanctions. Given the potential audience cost for empty threats the

treatment effect in the main analysis may be biased downwards. Panel (a), in Figure 3, offers a visualisation of the results. In general, I do not observe a meaningful difference relative to the main model depicted in Figure 2. Here the leads again point to an anticipation effect, the treatment effect and the lags are in fact lower than in the main analysis — a dynamic opposite to our expectations. While neither the treatment nor the lags observe statistical significance, the change of the coefficients offers a signal of belligerence cost for the use of economic coercion.

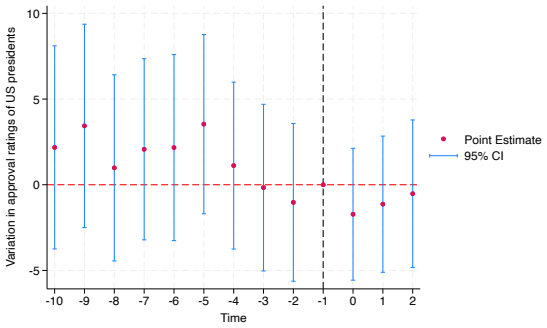
Another potential source of heterogeneity in the results may stem from the objective for the use of economic sanctions. US presidents are perceived to play a pronounced role in security-related foreign policy decisions (e.g., area of non-proliferation and authoritarian regimes seeking nuclear capacities), while trade-related sanctions (e.g., protectionist measures and subsidies to an aircraft producer) may resonate less with the public. To address this, I list sanction months as treated only if majority of the policy decisions concern security issues and depict the results from this analysis in panel (b) of Figure 3, while in panel (c), I show the findings for treatment months limited to majority of policy actions being trade-related. Security-related sanctions show a pattern of approval rating in line with the main findings, with a full set of negative leads signalling an anticipation effect on the side of US presidents, while treatment month and lags are close to zero. At the same time, for trade-related sanction we neither observe a trend in leads nor in lags and the coefficients are all near zero. Given the low power of this test, we can only draw very careful conclusions, yet the data does point towards US presidents anticipating an effect on approval ratings resulting from imposition only in the area of security-related sanctions, while trade related imposition of sanctions appears agnostic to previous movement in approval ratings.



(a) Majority imposition



(b) Majority security



(c) Majority trade

Figure 3: Heterogeneity in variation in approval rating of US presidents before and after imposition of economic sanctions (in percentage points). Negative values represent months leading up to imposition (leads), positive values represent months following imposition (lags), zero is the month of imposition and month before imposition serves as reference (depicted with a dotted vertical line). Panels show treatment limited to months with: (a) imposition as majority of policy decisions (relative to threats only), (b) security as motivation for majority of policy decisions, and (c) trade as motivation for majority of policy decisions. Regression results are available in the Appendix in Table A.2.

It is important to note that the mode of the use of sanctions by the US presidents places limits on our ability to track heterogeneity in the response of the public to different types of sanctions and the decision to impose or back dawn from a threat of sanctions. Very few months offer only a specific type of policy choice and underpinning motivation and the coarse measure of majority of policy actions pertaining to a particular domain still suffers from a relatively low number of cases.

Robustness

Finally, I look at the robustness of the findings of this article. To start, I assign at random treatment months throughout the data on presidency months and generate leads and lags accordingly. I show the results of the placebo test in the Figure 4. As expected, in the placebo analysis none of the results are statistically significant and the leads and lags follow a random pattern around zero variation in the approval ratings of US presidents.

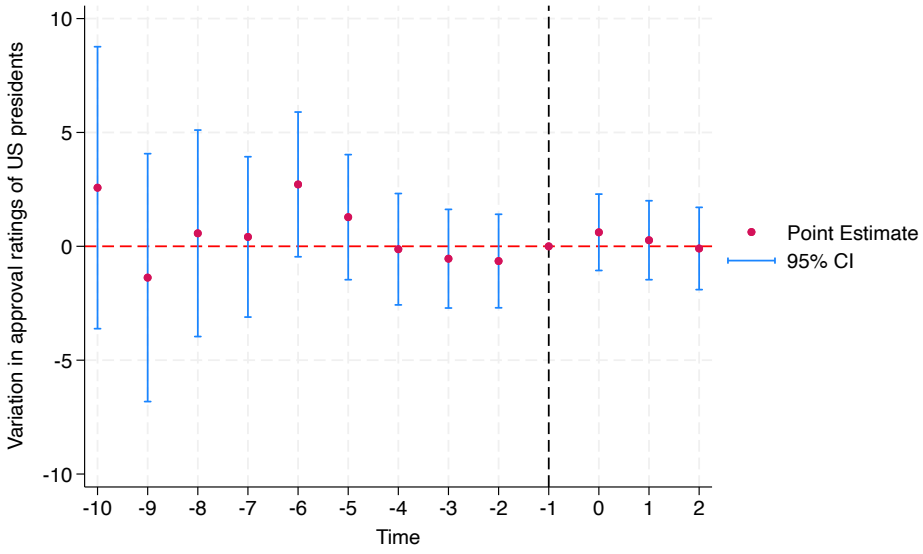


Figure 4: Placebo test and variation in approval rating of US presidents before and after imposition of economic sanctions (in percentage points). Negative values represent months leading up to imposition (leads), positive values represent months following imposition (lags), zero is the month of imposition and month before imposition serves as reference (depicted with a dotted vertical line). Treatment months are generated at random. Regression details are listed in the Appendix in Table A.2.

Conclusion

The objective of the research presented in this article is to study the motivation for imposition of economic sanctions and prospects for confirmation bias among political leaders. To this end, I have investigated, using an event study design, whether US presidents experience lower levels of popularity prior to imposition of economic sanctions and experience a boost in voters' approval following imposition. I have given particular attention to the complex structure of the data and address the issue of the unit of analysis by focusing on presidency month, rather than individual episodes of imposition or threats. The theoretical framework of domestic audience benefits and costs resulting from foreign policy decisions of political leaders underpinned the expectations put forward in this study. I have developed a formal model of Bayesian updating to strengthen the theoretical insights about the role of confirmation bias in a foreign policy context. As a result this article speaks both to the literature on economic sanctions and on the role of public opinion in international conflict, and meaningfully contributes to both, intertwined, scholarships.

I have identified an anticipation effect in the data on approval ratings and sanctions and observe a spell of lower popularity up to seven months prior to imposition of economic coercion. This finding provides suggestive evidence that US presidents act as if domestic audience benefits or domestic audience costs, or both, are present. Hoping to benefit from imposition, or fearing a cost of issuing empty threats, US presidents appear to impose sanctions when approval ratings are in decline. A theoretical model of beliefs updating, developed in this article, clarifies how this dynamic arises

when policy-makers operate in a setting with information open to interpretation. It appears that US presidents are likely to hold beliefs about electoral effects of imposition of economic coercion and sustain biased beliefs about the preferences of the public in the course of their presidency. This work shows that US presidents are, to a part, motivated by their beliefs about the public opinion in their foreign policy choices. Consequently, we observe the empirical implication of domestic audience costs and benefits in foreign policy choices, regardless of the actual effect on the public.

In addition, I have not found evidence for the presence of a domestic audience benefit or cost for imposing economic sanctions on the approval ratings of US presidents; however, there are weak signals of belligerence cost resulting from imposition of sanctions. We may interpret lack of audience effects as an indifference of the public or potential balancing of public dislike for threatening or engaging in costly and coercive foreign policy decisions (Webb, 2018; Kertzer and Brutger, 2016) and reward for active foreign policy (Levendusky and Horowitz, 2012; Tomz, 2007; Whang, 2011; Davies and Johns, 2013). The potential balancing of audience benefits and costs offers an interesting avenue for future research. At the same time, the existence of these costs does not rule out the selection bias underpinning the decisions of US presidents in respect to economic coercion, as they remain likely to systematically experience information open to interpretation on the sentiments of public opinion and their foreign policy. Consequently, the research community ought to be careful in projecting the results of studies on public opinion onto the behaviour of political leaders and reflect on the assumptions of perfect rationality and information among the political leaders as 'human belief formation" substantially differs from what standard rational choice models assume (Minozzi, 2013).

To summarise, this work contributes to three important aspects of the study of coercion in international relations. First, the article shows that US presidents may act as if the audience cost or benefit, or both, are present. This supports the fact that research does observe the empirical implications of the audience costs and benefits in international relations, yet shows the prospect for confirmation bias among political leaders. Second, it highlights the role of beliefs formation among decision-makers, while the relation between political leaders' beliefs may not overlap with the actual public. Third, it signals that there may be no domestic audience benefit or cost for the use of coercion in foreign policy. Yet, this dynamic may be driven be balancing of audience benefits and belligerence costs — opening an avenue for future research.

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Appendix

Table A.1: Variation in approval rating of US presidents and imposition of economic sanction. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

Variables	Model (1)	Model (2)	Model (3)
Lead 10	-1.294	0.213	0.0288
	(4.506)	(2.251)	(2.244)
Lead 9	-2.763	0.880	0.617
	(4.299)	(2.165)	(2.155)
Lead 8	-6.929*	-1.386	-0.348
	(4.123)	(2.080)	(2.153)
Lead 7	-9.783**	-6.819***	-6.732***
	(3.969)	(1.977)	(1.964)
Lead 6	-5.488*	-3.377**	-3.290*
	(3.210)	(1.643)	(1.693)
Lead 5	-3.331	-1.958	-1.334
	(3.039)	(1.542)	(1.562)
Lead 4	-4.020	-2.525*	-2.402*
	(2.753)	(1.407)	(1.422)
Lead 3	-1.077	-0.192	-0.0957
	(2.721)	(1.373)	(1.390)
Lead 2	-4.552*	-0.958	-1.286
	(2.400)	(1.220)	(1.239)
Treatment	-1.423	0.608	0.627
	(1.718)	(0.986)	(0.999)
Lag 1	-2.363	0.287	0.517
	(1.834)	(1.018)	(1.040)
Lag 2	-3.143	-0.143	-0.115
	(2.030)	(1.084)	(1.103)
Unemployment			0.385
			(0.655)
Inflation			-0.368
			(1.155)
Constant	57.38***	82.91***	55.79***
	(1.458)	(3.418)	(6.903)
Observations	680	680	662
R-squared	0.019	0.817	0.821
Treatment	at least one imposed sanction	at least one imposed sanction	at least one imposed sanction
Baseline	month before imposition	month before imposition	month before imposition
Cycle FE	NO	YES	YES
Case FE	NO	YES	YES
Economic controls	NO	NO	YES

Table A.2: Heterogeneity analysis of variation in approval rating of US presidents and imposition of economic sanction. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

Variables	Model (1)	Model (2)	Model (3)
Lead 10	-2.863	2.179	-0.101
	(1.967)	(3.017)	(2.336)
Lead 9	-2.525	3.436	1.227
	(1.813)	(3.020)	(2.150)
Lead 8	-3.057*	0.986	0.632
	(1.810)	(2.764)	(2.056)
Lead 7	-3.634**	2.070	-4.760**
	(1.722)	(2.691)	(1.973)
Lead 6	-2.259	2.169	-2.927*
	(1.624)	(2.764)	(1.601)
Lead 5	-1.140	3.536	-2.034
	(1.597)	(2.663)	(1.535)
Lead 4	-2.323	1.117	-1.749
	(1.511)	(2.480)	(1.372)
Lead 3	-0.441	-0.168	-0.180
	(1.462)	(2.476)	(1.370)
Lead 2	-0.651	-1.029	-1.115
	(1.355)	(2.344)	(1.212)
Treatment	0.249	-1.721	-1.036
	(1.201)	(1.959)	(1.020)
Lag 1	1.153	-1.134	-0.425
	(1.263)	(2.023)	(1.052)
Lag 2	-0.425	-0.524	-0.962
	(1.296)	(2.192)	(1.100)
Unemployment	0.680	2.072***	0.641
	(0.607)	(0.458)	(0.676)
Inflation	-3.271**	-11.41***	-1.345
	(1.386)	(1.572)	(1.194)
Constant	54.05***	47.16***	57.04***
	(8.440)	(4.481)	(7.184)
Observations	662	662	662
R-squared	0.698	0.433	0.808
Treatment	majority security	majority trade	majority imposed
Baseline	month before imposition	month before imposition	month before imposition
Cycle FE	YES	YES	YES
Case FE	YES	YES	YES
Economic controls	YES	YES	YES

Table A.3: Placebo test for analysis of variation in approval rating of US presidents and imposition of economic sanction. Robust standard errors are displayed in parentheses: *** indicates $p < 0.01$, ** indicates $p < 0.05$ and * indicate $p < 0.1$.

Variables	Model (1)
Lead 10	2.574
	(3.151)
Lead 9	-1.375
	(2.771)
Lead 8	0.571
	(2.308)
Lead 7	0.413
	(1.792)
Lead 6	2.719*
	(1.618)
Lead 5	1.280
	(1.398)
Lead 4	-0.127
	(1.245)
Lead 3	-0.544
	(1.102)
Lead 2	-0.646
	(1.045)
Treatment	0.615
	(0.853)
Lag 1	0.267
	(0.882)
Lag 2	-0.0955
	(0.919)
Unemployment	-1.937***
	(0.675)
Inflation	-0.0812
	(1.101)
Constant	55.12***
	(4.067)
Observations	662
R-squared	0.874
Treatment	placebo
Baseline	month before imposition
Cycle FE	YES
Case FE	YES
Economic controls	YES