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NO KIN IN THE GAME:
MORAL HAZARD AND WAR IN THE U.S. CONGRESS

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ABSTRACT

Why do wars occur? We exploit a natural experiment to test the longstanding hypothesis that leaders declare war because they fail to internalize the associated costs. We test this moral hazard theory of conflict by compiling data on 9,210 children of 3,693 U.S. legislators who served in Congress during the four conscription-era wars of the 20th century: World War I, World War II, the Korean War, and the Vietnam War. We test for agency problems by comparing the voting behavior of legislators with draft-age sons versus draft-age daughters. We estimate that (i) having a draft-age son reduces legislator support for pro-conscription bills by 10-17%; (ii) support for conscription increases by a quarter as a legislator's son crosses the upper age threshold; and (iii) legislators with draft-age sons are more likely to win reelection when the draft is less popular. These results are consistent with a political agency model in which voters update their beliefs about politicians' motives when they make unpopular legislative decisions. Our findings provide new evidence that agency problems contribute to political violence, and that elected officials can be influenced by changing private incentives.

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1 Introduction

Violent conflict undermines state capacity, economic growth, public health and human capital formation (Besley and Persson, 2010; Abadie and Gardeazabal, 2003; Collier et al., 2003; Ghobarah et al., 2003). The severity of these costs raises a fundamental question: why do destructive wars occur at all? Credible theories must allow for the failure of bargained settlements to ensure peace (Fearon, 1995). One such explanation reflects a classic political agency problem: if the leaders who order war stand to gain from the benefits without internalizing the costs, then war will be oversupplied (Jackson and Morelli, 2007).

We test this theory using data on roll call votes in the United States Congress during the four conscription-era wars of the 20th Century: World War I, World War II, the Korean War, and the Vietnam War. By observing an exogenous change in the exposure of some legislators to the costs of conflict but not of others, we can detect moral hazard in the decision to wage war. If leaders fully internalize the social cost of conflict in their polity, then both groups should vote identically after the change; if not, then those with higher private costs will reflect this in their voting.

We exploit a natural experiment that is permitted by the nature of conscription-era warfare in the United States. Legislators who had sons within the age boundaries of the draft were more likely to be exposed to the direct costs of conflict than legislators who had only daughters of the same age. Our main identifying assumption is that these two groups would otherwise vote identically—in other words, the gender of a given draft-age child is as good as random. Our identification strategy is also bolstered by the fact that the proposed draft age boundaries often shift from vote to vote. This allows us to include legislator fixed effects in our main specification, meaning that all time-invariant characteristics of legislators are flexibly controlled for.

We find that legislators with sons of draft age are between 10% and 17% less likely to vote in favor of conscription than comparable legislators with only daughters of draft age. To place this magnitude into perspective, it is equivalent to around 50-65% of the “party line” effect of having a sitting president from the opposing party. We also find suggestive evidence that, during the Cold War era when conscription was relatively unpopular, (i)

legislators are less likely to vote in favor of conscription during election years; and (ii) legislators with draft age sons are more likely to win reelection to the following Congress. These findings are consistent with a model of political agency in which voters reward politicians who vote against conscription and punish politicians who do not internalize its social costs.

Our results imply that legislators can be influenced by private motives that are external to political or ideological concerns. One challenge to this interpretation is the possibility that legislators with draft-eligible sons develop empathy for others in the same predicament, and that the change in behavior that we observe is due to concerns for the electorate rather than selfish motives. To explore this, we examine the behavior of politicians with sons around the upper age eligibility cutoff. We interpret this cutoff as a discontinuous determinant of draft exposure, as politicians are “treated” when their son is beneath the cutoff, and not treated when they are above it.¹ Using a regression discontinuity design with legislator fixed effects, we find that a given politician raises her support for conscription by 26% when her son crosses the upper age cutoff. We argue that this is unlikely to be caused by a sudden change in empathy. Instead, we interpret it as evidence that policy choices are manipulable by private motives orthogonal to both career concerns and individual ideology.

To arrive at these results, we undertake two main data collection exercises. In the first, we gather information on the number and gender of children of 3,693 U.S. senators and representatives from a combination of census records and a variety of biographical sources. In the second, we identify 248 roll-call votes relating to conscription from 1917 to 1974, and code the direction of pro- or anti-conscription policy preferences based on contemporaneous newspaper reports, where applicable. This process produces a main estimation sample of more than 26,000 observations at the level of a legislator-vote, combining information on 140 unambiguous roll-call votes, 2,287 legislators, and 5,421 children.

In order to validate our vote-coding procedure, we eschew the task of assigning pro- or anti-conscription codes to roll call votes ourselves and develop instead a method that relies

¹This is not true of the lower cutoff, as a politician with a son who is, say, two years younger than the lower boundary is plausibly exposed to the treatment.

on the behavior of well-known foreign policy “hawks” (pro-war legislators) and “doves” (anti-war legislators) during each era. If a legislator votes in line with the hawks and against the doves on a given measure, it is determined as a hawkish vote. Applying this approach, we find that legislators with draft-eligible sons are again around 10% less likely to vote with hawks on draft-related measures, but are not less likely to vote with hawks on measures unrelated to the draft.

To rationalize these main findings, we turn to a model of political agency that combines elements of moral hazard and adverse selection (Besley, 2006). ‘Good’ politicians pursue measures that are in the voters’ interest, and voters respond by reelecting them. ‘Bad’ politicians decide either to mimic good types in order to win reelection, or to vote against citizens’ interests and lose reelection. This decision is determined in part by the value of private rents that accrue to the politician if she votes against the electorate’s wishes. Typically, researchers do not observe exogenous variation in private rents that politicians can capture through legislative voting. This presents a barrier to empirically testing this type of model. However, in our setting we do observe an exogenous ‘wedge’ between the private benefits of conscription for legislators with draft-eligible sons versus those with daughters of comparable age. This gives us testable implications of the theory that we bring to the data. We find suggestive evidence that those with draft-eligible sons are more likely to win reelection when the draft is unpopular. We also provide evidence that pressure to toe the national party line is likely to be one motive that impels politicians to vote in favor of conscription despite its lack of broad support among voters.

The paper contributes principally to two distinct bodies of research. The first is an emergent empirical literature that connects credible identification strategies to theoretical work on the origins of violent conflict. These foundations are based on contest models in which two sides fight to control total resources. Each side allocates their own resources between production and appropriation, and the probability of victory is determined by the relative effectiveness of fighting technology (Haavelmo, 1954; Hirshleifer, 1988; Garfinkel, 1990; Skaperdas, 1992). One limitation of contest models is that they fail to account for bargained settlements. Wars are risky and destructive, and so it is necessary to understand

why they are avoided in some cases but not in others.²

Two sets of explanations in particular endure for why lengthy wars can occur between rational actors. The first broadly relates to incomplete contracting whereby the inability of each group to credibly commit to a negotiated settlement inhibits peace (Garfinkel and Skaperdas, 2000; Powell, 2006, 2012). For example, Chassang and Padro i Miquel (2009) develop a model in which transient economic shocks reduce the opportunity cost of fighting without altering the present discounted value of victory. In a perfect information environment with an offensive advantage and no third party contract enforcement, groups may not be able to commit credibly to peace, and war can ensue in equilibrium. Empirical papers that exploit plausibly exogenous variation to identify the link from economic conditions to conflict include Miguel et al. (2004), Dube and Vargas (2013), Bazzi and Blattman (2014), Berman and Couttenier (2015), Berman et al. (2017), Harari and Ferrara (2018) and McGuirk and Burke (2020). The second explanation has received less attention in the empirical literature: that wars can occur because the leaders who order violence do not fully internalize the costs (Jackson and Morelli, 2007). This moral hazard theory of conflict relaxes the assumption that groups are unitary actors.³ To the best of our knowledge, we are the first to corroborate it empirically using plausibly exogenous variation.⁴

The second literature broadly relates to the political economy of legislative decision-making. The prevailing view is that a legislator's vote is motivated by reelection concerns, promotion to higher office, and the politician's own ideological beliefs (de Figueiredo and Richter, 2014; Ansolabehere et al., 2003; Levitt, 1996; Washington, 2008). However, this model of policy formation leaves no room for the possibility that legislators are additionally influenced by other private payoffs. While this may be difficult to reconcile with the growing share of campaign contributions emanating from the extreme top of the wealth

²For example, Acemoglu and Robinson (2005) describe how elites expand the franchise to the poor in order to preclude violent revolt.

³'Moral hazard' in the political economy literature broadly describes legislators (agents) pursuing private ends in office at the expense of voters (as principals) who do not observe their motives.

⁴Other papers that relax the assumption of unitary actors by modeling the behavior of political leaders in conflict include De Mesquita and Siverson (1995), Tarar (2006), and Smith (1996). Information asymmetries are also posited as a rational explanation for conflict, although this is limited in particular as a driver of lengthy wars given that the true strength of each armed actor ought to reveal itself quickly in battle (Fearon, 1995; Blattman and Miguel, 2010).

distribution in the United States (Bonica et al., 2013), there exists nonetheless an argument that politicians are largely immune from such influences (Ansolabehere et al., 2003; Levitt, 1994; Tullock, 1972). An alternative explanation for this absence of evidence is the empirical challenge inherent in its detection. An ideal identification strategy would require estimating the effect on legislative voting of a change in private motives that is independent of both political and ideological concerns. By exploiting plausibly exogenous variation in the gender of draft-age children, our study overcomes this problem and finds evidence that legislators respond to private incentives.

We proceed with a brief discussion on the political economy of legislative voting in Section 2. In Section 3 we introduce our data. In Sections 4 and 5 we present our estimation strategy and main results. In Section 6 we examine the empathy versus self-interest interpretation of the main results, and in Section 7 we endogenize the behavior of voters in response to legislators' decisions in a political agency model and empirically test its implications. In Section 8 we conclude.

2 Political Economy of Legislative Voting in a Democracy

There is a broad consensus in the empirical literature that a politician's legislative vote is determined by reelection concerns, promotion to higher office, and their own ideological beliefs (de Figueiredo and Richter, 2014; Ansolabehere et al., 2003; Levitt, 1996). This implies that the politician takes into account four sets of preferences in determining her optimal legislative vote (Higgs, 1989; Levitt, 1996). Reelection concerns are represented both by the preferences of voters in her electorate, and by the preferences of her supporters within that group; promotional concerns are represented by the national party line; and ideological beliefs are exogenously determined fixed preferences.

Assuming that preferences are single peaked, the politician's objective is to select a vote that minimizes the weighted average of the squared distances from the four 'ideal

points’ that correspond to each preference as follows:

$$\begin{aligned} \max_{V_{it} \in \{0,1\}} U_{it} = & - [\alpha_1(V_{it} - M_{it})^2 + \alpha_2(V_{it} - C_{it})^2 + \alpha_3(V_{it} - P_{it})^2 \\ & + \alpha_4(V_{it} - F_i)^2], \end{aligned} \tag{1}$$

where $V_{it} \in \{0, 1\}$ is the legislator i ’s vote at time t ; $M_{it} \in [0, 1]$ is the ideal point in a given issue space of the voters in the legislator’s electorate; $C_{it} \in [0, 1]$ is the equivalent ideal point among the legislator’s supporters; $P_{it} \in [0, 1]$ is the ideal point of the legislator’s national party; and $F_i \in [0, 1]$ is the legislator’s ideological bliss point, which is assumed to be fixed over time. The α parameters represent weights, and all weights sum to 1.

There exists at least some empirical evidence in support of each element in (1). The first, general voter preferences, is derived from the canonical model of Downsian competition in which politicians converge on the preferences of the median voter. Empirical support for this model can be shown by detecting an impact of exogenous changes to the composition of an electorate on subsequent policy outcomes.⁵ However, there also exists evidence that is not compatible with the purest interpretation of the model. For example, US senators from the same constituency vote differently, and an exogenous change in local representation (but not in the electorate) led to important policy changes in India.⁶

The second element, supporter group preferences, is derived from the “duel constituency” hypothesis (Fiorina, 1974), which states that legislators apply additional weight to the preferences of their own supporters within their electorate. This might be due to the existence of primary elections, or because supporters are inclined to volunteer or contribute in other ways to a candidate’s campaign.⁷

⁵For example, Cascio and Washington (2014) show that a plausibly exogenous expansion of black voting rights across southern U.S. states led to greater increases in voter turnout and state transfers in counties with higher black population. Similarly, Miller (2008) shows that the introduction of suffrage rights for American women immediately shifted legislative behavior toward women’s policy preferences.

⁶Poole and Rosenthal (1984) show that Democratic and Republican U.S. senators representing the same state, and therefore the same electorate, exhibit significantly different legislative voting patterns. In India, Chattopadhyay and Duflo (2004) exploit a randomized policy experiment in which certain village council head positions were reserved for women. Despite the electorate remaining unchanged, the reservation policy significantly altered the provision of public goods in a manner consistent with gender-specific preferences. Both of these results violate the median voter theorem, implying that while it has some predictive power, there must exist additional determinants of policy.

⁷Levitt (1996) finds that U.S. senators assign three times more weight to the preferences of their own supporters relative to other voters in their electorate. Brunner et al. (2013) and Mian et al. (2010) also find evidence that is consistent with this effect.

The third element, national party preferences, reflects the fact that politicians have an incentive to vote in line with the national party, who in return can provide promotions to various committee positions.⁸

The final element, a legislator's fixed ideology, is estimated by Levitt (1996) to carry a weight of around 0.60, more than α_1 , α_2 , and α_3 combined. Causal evidence for the existence of this idiosyncratic ideological influence is provided by Washington (2008), who finds that U.S. legislators with more daughters have a higher propensity to vote in favor of liberal measures, particularly ones connected to expanding reproductive rights. Her findings are consistent with sociological theories that parenting daughters increases feminist sympathies.⁹

Incorporating private influences

A notable feature of this model is the absence of a private motive that is distinct from a legislator's fixed ideology and political career concerns. To wit, the model either assumes that there are no other private costs and benefits associated with legislative voting, or that, if there are, legislators are immune to their influence. This appears to be at odds with the apparently large sums of private money that are spent on lobbying and campaign contributions. However, Ansolabehere et al. (2003), echoing Tullock (1972), argue that if campaign contributions were indeed worthwhile investments, they ought to be of substantially higher value in each election cycle given the trillions of dollars of government outlays potentially at stake. They note that campaign spending limits are not binding; that the majority value of contributions come from individual donors rather than special interest Political Action Committees (PACs); and that these individuals give the marginal dollar. They also run fixed effects regressions that uncover no relationship between pro-corporate legislative voting and corporate donations. They conclude that campaign contributions

⁸Evidence from, inter-alia, Bonica (2013), Snyder and Groseclose (2000), and McCarty et al. (2001) supports this view in the context of U.S. congressional voting.

⁹One line of argument is that voters' preferences are represented in government not through α_1 or α_2 , but rather through this channel. This is the "citizen candidate" notion of representation, which states that candidates are unable to make binding commitments to voters, and so voters support candidates whose (known) fixed ideology is most closely aligned to their own (Besley and Coate, 1997; Osborne and Slivinski, 1996). In contrast to median voter theorem, voters elect rather than affect policies.

are largely made for their consumption value, rather than returns on investment.¹⁰

In this paper, we address an alternative potential explanation for the absence of evidence on the role of private influences in legislative voting: the significant empirical challenge inherent in detecting such an effect (as noted by de Figueiredo and Richter, 2014). A causal identification strategy would involve estimating the effect of an exogenous change in the private net benefits of voting on the legislative choices of a politician, conditioning on politician fixed effects to hold ideology constant. While there exists persuasive evidence that, for example, campaign contributions can “buy” time with a legislator (Kalla and Brookman, 2016), that the market value of firms can be affected by exogenous changes in the political power of connected politicians (Jayachandran, 2006; Fisman, 2001), and that exogenous differences in ideology between politicians can affect voting (Washington, 2008), to our knowledge there is no evidence that individual legislators respond to changes to their private net benefits of voting on a given issue. Yet, such a view would be consistent with more recent evidence that the richest individuals in the U.S. are contributing a higher share of contributions to politicians than before (Bonica et al., 2013), and that the pattern of contributions by firm CEOs and economic PACs suggest that they are investing rather than consuming (Gordon et al., 2007).

To incorporate this viewpoint, we propose a modification of equation (1) above in which self-interested legislators are additionally concerned with their own private returns to voting, as follows:

$$\begin{aligned} \max_{V_{it} \in \{0,1\}} U_{it} = & - [\alpha_1(V_{it} - M_{it})^2 + \alpha_2(V_{it} - C_{it})^2 + \alpha_3(V_{it} - P_{it})^2 \\ & + \alpha_4(V_{it} - F_i)^2 + \theta(V_{it} - R_{it})^2], \end{aligned} \tag{2}$$

where $R_{it} \in [0, 1]$ is the ideal point that optimizes legislator i 's time-varying private

¹⁰While the model above is consistent with this view, it can also accommodate a form of effective campaign spending whereby contributions can help to elect a certain politician with sympathetic ideological preferences, as distinct from affecting a politician's policy preferences in a quid pro quo arrangement. However, even this possibility has been challenged empirically, most notably by Levitt (1994). Similarly, the fact that three times more is spent on lobbying in the U.S. than campaign contributions does not imply that legislators are susceptible to private concerns beyond those laid out above. Lobbying is the transfer of information in private meetings from organized groups to politicians or their staffs (de Figueiredo and Richter, 2014). If these activities were shown to have an impact on policy, the possibility would still remain that their impact operates through any of the elements in the model rather than through a private quid pro quo channel.

net benefit, θ is the weight that the politician assigns to this motive, and $\sum_{j=1}^4 \alpha_j + \theta = 1$. The solution to the legislator’s problem becomes:

$$V_{it}^* = \underbrace{\alpha_1 S_{it} + \alpha_2 C_{it} + \alpha_3 P_{it}}_{\text{political motives}} + \underbrace{\alpha_4 F_i + \theta R_{it}}_{\text{private motives}}. \quad (3)$$

We define political motives as those derived from the preferences of voters and political parties, and private motives as those derived from ideological preferences and other time-varying costs and benefits.

Implications for Conflict

Much of the theoretical literature on violent conflict treats actors as unitary decision-makers.¹¹ Implicit in this approach is the assumption that the costs and benefits of conflict are shared among members of each group. The politician’s solution in (3) relaxes this assumption. If, on a given vote, a shock to R_{it} is sufficiently large, then it is possible a leader may vote to enter conflicts in which the expected social costs exceed the benefits, or to avoid conflicts in which the expected social benefits exceed the costs. The critical condition in either case is that the private payoff through θ offsets the influences that operate through the other channels, or $V_{it}^*(. | \theta > 0) = (1 - V_{it}^*(. | \theta = 0))$.¹² This is raised by Fearon (1995) as one explanation for violent conflict between rational groups that cannot be solved necessarily through a negotiated settlement. Jackson and Morelli (2007) develop the concept formally, showing that “political bias”—or the extent to which the pivotal policy maker benefits from conflict relative to the rest of the population—can cause war even in the presence of enforceable transfers between potential belligerents.

Other papers that relax the assumption of unitary actors do so by modeling the politics of conflict from the perspective of leaders (De Mesquita and Siverson, 1995; Smith, 1996; Goemans, 2000; Tarar, 2006), or by addressing a different type of agency issue, whereby

¹¹See Blattman and Miguel (2010) and Garfinkel and Skaperdas (2007) for in-depth reviews of this literature.

¹²The same could be said about changes to C_{it} , P_{it} and F_i , assuming that M_{it} approximates the social optimum. An interesting difference is that those motives are plausibly known to the electorate, whereas R_{it} is plausibly not. We examine this condition in more detail when we endogenize voter behavior in Section 7.

politicians must provide sufficient incentives to solve the collective action problem of raising an army (Grossman, 1999; Beber and Blattman, 2013; Gates, 2002). In our setting, this is achieved by the threat of penalties for draft evasion.

The specific role of moral hazard in conflict has been applied to the case of rebel activity in the presence of external humanitarian interventions—for example, Kuperman (2008) and Crawford (2005) argue that the insurance provided by external groups protects rebel groups from the risks of rebellion, which ultimately leads to more violence—and to the case of states acting more aggressively when they have powerful allies (Christensen and Snyder, 1990; Benson et al., 2014; Narang and Mehta, 2019). In the present paper, we make the related argument that politicians who are protected from the risks of conflict are more likely to support it.

Testing Implications

The central challenge for the researcher in determining whether or not private payoffs influence policy decisions (i.e., $\theta > 0$) is to identify exogenous variation in R_{it} . Otherwise, any estimate of θ could be biased due to positive covariance between R_{it} and any of the other elements in the model. For example, a senator who receives contributions from a weapons producer and favors voting for war in Congress may appear to be malleable through this channel. However, the possibility exists that (i) a large share of her electorate is employed by the firm, in which case M_{it} or C_{it} is measured incorrectly as R_{it} ; or (ii) that she is ideologically predisposed to war and the firm optimally contributed to her campaign, in which case F_i is measured incorrectly as R_{it} .

We overcome this problem by exploiting variation in the age and gender of politicians' children to determine whether or not having a draft-eligible son affects legislative voting on conscription, holding F_i constant. Legislators with draft-eligible sons stand to lose more from the passage of conscription than do legislators with daughters of comparable age, all else equal. This implies that, on a vote to determine whether or not to impel citizens to go to war, legislators exhibited measurable, exogenous variation in R_{it} .

3 Data and Background

Structure Data in our main analysis is at the level of a legislator-vote. Each observation contains information on how the legislator voted and on a range of legislator characteristics, including the number and gender of their children at the time of voting. In our full dataset, which includes votes analyzed for robustness and auxiliary exercises, there are 3,693 legislators, 9,210 children, and around 700,000 legislator-votes spread between the House of Representatives and the Senate from the 64th Congress in 1916 to the 93rd Congress in 1974.¹³ In our core analysis of conscription voting there are 2,287 legislators, 5,420 children, and 26,373 legislator-votes. We describe below our principal data sources and the construction of our main variables.

Vote data Our dependent variable of interest is whether or not a given legislator voted in favor of conscription. Our main sample of interest is the universe of draft-related roll call votes cast in the United States Congress during the 20th Century. We create this sample by first gathering voting records from the Voteview project.¹⁴ We then retain the union of votes that are assigned the “Selective Service” issue code by Voteview (the main conscription legislation in the United States is named the Selective Service Act) and votes that we determine to be relevant. This is aided by short descriptions of each roll-call vote provided by the Gov Track project.¹⁵ This gives a total of 248 votes; 195 determined by Voteview and a further 53 determined by the authors. An example of a measure that is assigned the issue code is: “S.1 Act to provide for the common defense and security of the US and to permit the more effective utilization of man-power resources of the the US by authorizing universal military training and service,” which was passed in the House and Senate in 1951. An example of a vote that was not assigned an issue code by Voteview but was assigned a code by the authors is: “To amend S.1871, by raising the minimum age limit to be selected into the military from 21 to 28 years. (P. 1463, Col. 2),” which was rejected in the Senate in 1917. It was not assigned the “Selective Service Act” issue code most likely because the act itself had not yet passed.

¹³This includes only Congresses that contain roll call votes of interest regarding conscription and warfare.

¹⁴See <https://voteview.com/>.

¹⁵See www.govtrack.org, a project of Civil Impulse, LLC.

Next, in order to examine legislators’ motives for voting, it is necessary for us to assign a ‘direction’ to each roll call vote. For example, in the first example above, it is clear that an “aye” vote implied support for the draft, whereas in the second example it seems less likely to be the case. Raising the lower cutoff could plausibly reflect an anti-draft preference. At the same time, however, it is possible that the passage of that amendment could have raised the likelihood that the main draft bill to which it was attached ultimately passed too. In such a case, there is a danger of misclassifying a pro-draft measure as an anti-draft one.

For each of the 248 votes, therefore, we turned to archival records to determine the implications of an aye versus a nae. This mostly took the form of newspaper articles from the week in which a bill was debated in the *New York Times* and the *Chicago (Daily) Tribune*. In some cases, this research reversed our priors on the direction of a certain vote. For example, an amendment to authorize “the president to conscript 500,000 men if the number is not secured by voluntary enlistment within 90 days” (Senate Vote 51 in the 65th Congress, 1917), might initially appear to be a pro-draft amendment. However, articles in both papers make it clear that this was viewed as a success by the isolationists at the time, as the original Army bill provided for selective draft *without* a call to volunteers.

Several votes were too ambiguous to be coded in either direction. For example, it is not clear *a priori* whether or not a vote to allow exemptions for certain groups is welcomed by a congressperson with a draft-eligible son; on the one hand, the son may be eligible, but on the other, exemptions for other eligible men may increase the probability of being drafted into combat conditional on being eligible.

The results of this data collection exercise can be seen in Table 1, where we document draft-related votes only in Congresses in which we found relevant votes that we could determine as pro- or anti-draft. In total, we code the direction of 140 votes—106 in the Senate and 34 in the House (Column 1). In the second column we present our main dependent variable: *Pro Draft* is equal to 1 if a legislator voted in favor of conscription (*aye* if it was a pro-draft vote, or *emphnae* if it was an anti-draft vote), and 0 otherwise. This exhibits a large amount of variation; the sample average is 0.58. In the third column we present the average absolute margin between aye and other votes (nae or abstentions).

For example, there is one vote in the 89th Senate; Pro Draft is 0.93, which means the margin is $0.93 - (1 - 0.93) = 0.86$, the gap between the winning vote and the losing vote. Column (4) contains the number of draft-related votes in total—i.e., successfully coded or otherwise. The overall number is 232, as the remaining 16 were in other Congresses in which we did not successfully code any votes. We cannot present the same information for the outcome variable, but we do present the average margin to facilitate a comparison between Columns (2) and (3). The respective mean margins are 0.18 and 0.17, suggesting that there is no obvious difference between votes that we could and could not code.

Legislator data The main independent variables are constructed from data on legislators’ family compositions. We first take basic data on legislators themselves from the *Biographical Directory of the United States Congress 1774 - 2005* (Dodge et al., 2005). We then use this information to locate richer household data from alternative sources. Most of this data is acquired from decennial U.S. Census records dating from 1840 to 1940, which we access through *Ancestry*, a company that provides digitized and searchable Census records up to 1940.¹⁶ These records contain information on the name, gender and age of each household member. We cross-check household data across multiple Census records and ensure that the full set of children are accounted for. For those congresspeople too young to have household information contained in the 1940 Census, we rely instead on a broad range of sources that include obituaries in national newspapers; biographies on official federal and local government websites; local media profiles; university archives; and other online repositories.¹⁷

In Table 2, we present this information only for the 2,287 legislators who voted on our main sample of conscription measures in Column (1) of Table 1. Of these, 85% had children at the time of voting, and the average number of children per legislator was 2.37; 68% had at least one son and 65% had at least one daughter. In the second to last column, we present the percentage of legislator-votes in which a legislator’s son was within the draft-eligibility window pertaining to the given roll call vote. For example, on a vote that proposes to enact the draft for all men between 20-25, a legislator with a 26

¹⁶See www.ancestry.com.

¹⁷These include *Notable Names Database*, *Legacy.com*, *Biography.com*, *Newspapers.com*, and *Legacy.com*.

year old son is coded as a 0. However, if the following roll call vote proposed to raise the upper cutoff so that the window runs from 20-30, the same legislator is coded as a 1. This is our main ‘treatment’ variable in the analysis. The House and Senate sample means are 0.23 and 0.21; meaning that over one fifth of legislator-votes on draft bills are cast by legislators with sons in the draft window. Reassuringly, the equivalent figures for daughters are the same.

Data on the age cutoffs are presented in Figure 1 and Table 3. There is more variation in the upper age cutoff than in the lower one. There is also considerably more variation in the proposed cutoffs during the two World Wars than in the two Cold War conflicts.

Taken together, the data show that draft-related measures in Congress were relatively contentious, and that around one-fifth of legislators had sons of draft-eligible age during the relevant votes. Less clear are (i) the perceived costs and benefits of conscription that were postulated during debates on the floor (or in committee) at the time; and (ii) the potential additional costs to a treated legislator of a draft measure passing.

On the first issue, we can learn much from archived newspaper reports about the nature of the debate surrounding conscription. For example, on the first day of the World War I draft bill debate on April 23, 1917, Representative Julius Kahn, who led the Administration’s fight for conscription, invited Captain Percy Benson of the Somerset Yeomanry, a regiment of the British Army, to speak to the House Committee on Military Affairs about “England’s mistakes.” Benson listed five main reasons why the US should pursue conscription. First, he believed that the obligation to defend a democracy ought to be equal; second, he argued that the draft secured “infinitely greater efficiency”, insofar as the government, through the selective process, could ensure that a sufficient number of men could remain in essential industries such as coal mining, shipbuilding, and farming during the war; third was the “economy” of conscription, which allowed the government to call up single men rather than married ones with dependents and potentially expensive allowances and pensions; the fourth point was “continuity of effort”, or the direct efficiency of securing a sufficient number of soldiers with maximum certainty in order to win the war; and the fifth point was to ensure that “slackers” pull their weight and, just as importantly,

that those who “were called slackers who were not slackers at all” would be protected from such terms of opprobrium.¹⁸

In one form or another, many of these points were repeated over the course of draft-era warfare in the US Congress, although, as we discuss in detail in Section 7, the necessity of the draft as a means of securing a sufficient number of soldiers waned as conflict technology became more capital intensive over time (Fordham, 2016). Other arguments against the draft were varied. Unions were consistently opposed to conscription as they viewed it as a form of class exploitation, most likely because the alternative—raising military pay—would increase unions members’ bargaining power and wages.¹⁹ Relatedly, others viewed the draft as an opportunity for special interests to profit,²⁰ while isolationists and pacifists opposed conscription as part of their general opposition to interventionist foreign policy.²¹

One consistent argument against the draft relates to agency frictions of the type that we seek to test in our main analysis. Perhaps the most notable example of this concerns an amendment proposed by Congressman Hubert Stephens of Mississippi to make members of Congress themselves subject to the draft during World War I. Speaking in favor of the amendment, Congressman Frank Clark of Florida argued that “[i]t would be a shame, a cowardly thing [...] for Congress to declare war and then send young boys to do the fighting, while our precious hides are exempt.” Mr. Stephens insisted that there were a “a number of vigorous men on this floor who are fit for service at the front.” The amendment

¹⁸“Draft Bill Debate is to Begin Today,” *The New York Times*, April 23 1917. The New York Times also ran an opinion piece quoting Abraham Lincoln’s defense of the draft during the Civil War, in which he argued in favor of distributing the burden of warfare widely (“A Conscriptor,” *The New York Times*, April 26 1917)

¹⁹“Unions Oppose the Draft – Resolution Adopted Unanimously by Central Federation,” *The New York Times*, April 1 1917

²⁰During the World War I draft debate, The New York Times reported that “Mrs. W.I. Thomas of Chicago, Executive Secretary of the Woman’s International Peace Party, characterized the war as an alliance between Lombard and Wall Streets. Grant Hamilton of the American Federation said labor stood solidly against conscription.” (“Senate Takes Up Draft for Debate,” *The New York Times*, April 22 1917.) This line of argument continued into the World War II era (“Draft Bill Action is Demanded Now,” *The New York Times*, August 22, 1940):

“Senators Holt, Wheeler and Walsh again bitterly attacked the principle of peace-time conscription. Senator Holt asserted that “international bankers” and “wealthy attorneys” were promoting the selective service measure. He said he saw something sinister in what he said was the fact that most of them were “Harvard men.”

²¹Speaking during the debate to enact conscription prior to U.S. involvement in World War II, Senator Ernest Lundeen, a Minnesota Farmer-Labor isolationist, told the Senate that he “did not care whether Germany or England won the war.” (“Draft Bill Upheld in First Test Vote in Senate,” *The New York Times*, April 28 1940.)

was defeated, 130 to 86.²²

On the second issue, we compile in Table 4 data on U.S. draft registrants, draft deployments, total service-members, and total fatalities for each war in our dataset. In total, just over 109 million men were registered for the draft over the four conflicts. Of those, 16.3 million (or 15%) were inducted. This is just under one half of the 35.3 million total service members. Total U.S. fatalities are estimated at 426,132, implying a 1.2% probability of death conditional on serving. Assuming that draftees were killed at the same rate as regular service members, a draft *registrant* had a 0.2% probability of being killed in battle.²³ Note also that this does not take into account other long-run mental, physical and labor-market costs of conflict such as those identified in Angrist (1990) and many others. One fifth of legislators, therefore, had a non-trivial role in determining the risks faced by their own sons in battle.

4 Estimation

Our main specification is as follows:

$$V_{isvcj} = \alpha_i + v_{vcj} + k_{iv} + \beta_1 son_{iv} + \beta_2 draft\ child_{iv} + \beta_3 draft\ son_{iv} + \zeta \mathbb{X}_{iv} + \epsilon_{isvcj}, \quad (4)$$

where V_{isvcj} is an indicator equal to one if the legislator i from state s votes to enact or expand conscription in vote v during Congress c in congressional chamber j ; α_i are legislator fixed effects; v_{vcj} are vote fixed effects; k_{iv} are fixed effects for number of children at the time of vote v ; son_{iv} is an indicator equal to one if a legislator has a son at the time of vote v ; $draft\ child_{iv}$ is an indicator variable equal to one if a legislator has any child of draft-eligible age as determined by the cutoffs in vote v ; $draft\ son_{iv}$ indicates that a legislator has a son of draft-eligible age in vote v ; \mathbb{X}_{iv} is a vector of time varying controls, comprising the legislator’s age, age squared, and terms in office. In regressions without legislator and vote fixed effects, we include controls for party, state and chamber fixed effects. Standard errors are two-way clustered by legislator and vote. We estimate

²²“Amendments Flood House,” *The New York Times*, April 29 1917.

²³This is around 17 times greater than the probability of dying in a traffic accident in the United States in 2019 (see: www.nsc.org/road-safety/safety-topics/fatality-estimates, accessed 3/12/2020.)

the specification with a linear probability model (LPM) and show that the results are qualitatively unchanged when estimated in a conditional (fixed effects) logit model (CL).

Our main identifying assumption is that $draft\ son_{iv}$ is independent of the error term. This is violated if having a draft age son is related to any of the other determinants of optimal voting in equation (3)—voter preferences, party preferences, and ideology. The inclusion of legislator fixed effects, vote fixed effects (the most granular time fixed effects possible), $draft\ child_{iv}$, and fixed effects for total number of children are particularly reassuring in that regard. Conditional on these covariates, we argue that variation in $draft\ son_{iv}$ is as good as random.

Finally, it is necessary to determine the appropriate number of lead years for the lower cutoff in the treatment variable. If, say, the lower cutoff is at 20, then it is likely that a member of congress with a 19 year old son is effectively treated. Failing to account for this will bias the estimated treatment effect $\hat{\beta}_3$ toward zero, as treated legislators will contaminate the comparison group. While the decision is somewhat arbitrary, what should be clear is that $\hat{\beta}_3$ initially rises as we reduce the lower cutoff and add more treated legislators to the treatment group, before smoothly decreasing again as more untreated legislators with younger children are added.

5 Main Results

Table 5 presents the main results with the lower bound set at 4 years below the proposed cutoff. This means that a legislator with a 16 year old son is treated if the proposed lower cutoff is 20 years of age. In Column (1), we show that having a draft-eligible son reduces the probability of voting for conscription by 6.7 percentage points, from a mean of 0.605. Adding state fixed effects reduces the magnitude of the coefficient and removes its statistical significance (although the coefficient on $draft\ child_{iv}$ is marginally significant). In Column (3) we add legislator fixed effects, finding a treatment effect of -0.102 ($p < 0.01$), or 17% of the mean. Finally, we add vote fixed effects and estimate the full model from equation (4) in Column (4), finding again a large and significant negative treatment effect in the region of 10% of the mean.

In Figure 1, we plot the sensitivity of each empirical model in Table 5 to different lower cutoff ages. In all four models, point estimates smoothly rise from the one-year lead to the four-year lead, before falling off slightly at five years. This pattern aligns well with theory: with few leads there are treated legislators in the control group, which biases β_3 downward. The treatment effect is maximized with a four-year lead, as the inclusion of legislators with a five year lead reduces the point estimate. Up until the Vietnam War, the mean duration of the draft per war was 3.3 years. Returning to the example above, it is reasonable that legislators with a 16 year old son are more concerned about conscription on average than those with a 15 year old son.

In Table 6, we examine whether or not the treatment variable is larger in a sample of close votes. Legislators' decisions are more likely to be pivotal in closer votes. We would therefore expect those with draft-eligible sons to be more likely to oppose the draft in narrow votes relative to landslide votes. Defining close votes as those in which the margin was less than 60-40, we find that the draft-age son \times close vote interaction effect in the first three columns ranges from -20 to -15 percentage points ($p < 0.01$), easily enough to convert the average legislator from pro- to anti-draft. Adding vote fixed effects reduces the interaction effect to -1.8 percentage points ($p > 0.10$).

Hawks and Doves In Section 3 we described the process by which we coded 140 votes as either pro- or anti- draft. This is a subset of the 248 draft-related votes in total. The remaining 109 were too ambiguous for us to code with confidence.²⁴

Two drawbacks of this approach are (i) the loss of coverage owing to the ambiguity of certain votes; and (ii) the level of discretion that we were required to exercise in determining the direction of each vote. In order to test the robustness of the main results to sample selection and the authors' discretion, we develop an alternative method of measuring pro- or anti-draft preferences among legislators. Drawing on a variety of sources, including historical accounts and archival newspaper articles, we identify at least two well-known

²⁴These include bills that add exemptions which could potentially help or hinder a legislator depending on the exemption; and bills that were too ambiguous to interpret for other reasons, e.g., a House amendment in 1951 that proposed to prevent draftees from being sent to Europe, which some viewed as limiting the scale of the draft while others viewed it as increasing the likelihood that draftees would be sent to Korea, which was potentially more dangerous.

foreign policy “hawks” and two well-known foreign policy “doves” during each Congress in both the House and the Senate.²⁵ We use this information to create a new variable, *Hawk Vote*, which is equal to 1 if the modal vote among the hawks in a given legislator’s congress-chamber is in favor of a measure and the modal vote among doves is against it. Similarly, it is equal to 0 if the model dove vote is in favor of a measure and the model hawk vote is against it. The variable is only defined in cases where there is a unique mode among hawks and a unique mode among doves that is not the same. The correlation coefficient between *Hawk Vote* and our main *Pro Draft* outcome variable is 0.92.

In Table 7, we repeat the same four specifications as in Table 5 using *Hawk Vote* as the dependent variable. The sample is drawn from all 248 draft-related votes in our dataset, rather than the 140 that we were able to code. On the other hand, votes for which the variable is not defined are omitted. In assigning legislators to treatment or control groups, we use the draft age thresholds assigned to individual votes where possible. Otherwise, we use the thresholds that were most recently passed in a given chamber. The results are almost identical; interpreting Column (4), we see that legislators with sons of draft-eligible age are around 5.5 percentage points (or 9% of the mean) less like to vote for conscription than those with daughters of comparable age.

In Table 8, we restrict the hawks and doves sample to draft-related votes that are *not* included in our main estimation in Table 5, and again find similar results.

In Table 9, we repeat the exercise on the universe of votes in draft-era Congresses that are *unrelated* to the draft. This gives a sample of almost 176,000 legislator-votes. In Column (2) where we control for state fixed effects there is a marginally significant positive effect. In our preferred specifications with legislator fixed effects and added vote fixed effects (Columns 3 and 4 respectively), the treatment effect is not significantly different to zero.

This exercise suggests that our main results are not an artifact of the authors’ vote-coding procedure, and that legislators with sons of draft age do not vote differently to those with daughters of draft age on issues unrelated to the draft.

²⁵The exception is the 82nd House during the Korean War, in which we were only able to find one dove (Robert Crosser, D-Ohio).

Additional robustness In Appendix Table A1, we repeat the exercise with added controls for second order polynomials in the age of every child of each legislator. If a legislator does not have a k th order child, we enter a zero for the corresponding age. These zeros are then flexibly captured by k_{iv} in the regression. These age controls ensure that the treatment effect is not picking up nonlinear effects of a child’s age on legislators’ voting preferences. The main results are robust to their inclusion.

In Appendix Table A2, we show that the main results are qualitatively robust to estimating an equivalent Conditional Logit model.

In Appendix Table A3, we run the same four empirical models as in Table 5 with an alternative set of draft-related votes. While our main votes concern the enactment, extension, or reduction of universal military service (e.g., passage of the Selective Service Act, its extension over time, increasing or decreasing the number of draftees, etc.), the votes that we study in Table A3 instead include what we call “window votes,” which are votes to change the existing upper or lower cutoffs only. We treat these separately because they require an alternative coding procedure. To understand why, say that legislators vote on a measure to change the draft window from 20-30 to 20-35, i.e. raising the upper cutoff from 30 to 35. A legislator with a 32 year old son is clearly negatively impacted, and would be assigned to the treatment group. We denote these legislators as “marginal.” However, it is not obvious to see how a legislator with a 22 year old son is affected by this. On the one hand, their son faces a longer duration of eligibility. On the other, the probability that their son is drafted could be reduced. This was an issue much debated in Congress at the time. Reporting on one such debate in 1940, the New York Times writes:

“The difference in age brackets between the two bills could have a profound effect on the selection results, it was asserted during the debate in the two houses. To raise the 800,000 men it is planned to train during the first year of the program would involve the selection of only one in every twenty-three registrants in the age group of 21 to 45 and one out of every thirteen under the Senate bill’s age range of 21 to 31.”²⁶

²⁶ “House Votes Conscription,” *The New York Times*, September 8th, 1940.

To sidestep this problem, we drop these infra-marginal legislators from the sample, leaving only the marginal group as treated and the extra-marginal legislators as the control along the age dimension. This leaves a sample of around 7,000 legislator-votes. The results can be seen in Figure A1, where we allow the lead years to vary. The treatment effect is maximized with either a one-year or a two-year lead rather than a four year lead as in the main model, perhaps reflecting the fact that window votes tended to occur closer to the ends of wars than the main votes. We present these models with a two year lead in Table A3. Another point to note is that the treatment effect is significant with vote fixed effects but not with legislator fixed effects, which is consistent with the sharply reduced sample size.

Finally, in Table A4, we present results in which the treatment variable is not coded with respect to the proposed cutoffs as determined by vote in question, but rather to the existing cutoffs determined by the most recently approved measure in a given chamber. This introduces measurement error in the treatment variable. The results, although still significant in our preferred specification, highlight the importance of examining the implications of each specific roll call vote rather than basing the treatment status on the prevailing cutoffs.

6 Empathy vs. Self-Interest: Regression Discontinuity

While the main results are consistent with the hypothesis that leaders have selfish motives beyond politics or ideology (i.e., that $\theta > 0$, from Section 2), it is nevertheless possible that the estimated $\hat{\beta}_3$ is consistent with the classic model of legislative voting presented in equation (1). For example, it could be the case that legislators receive information from their draft-eligible sons that makes the social cost of conscription more salient to them for a certain period of time. In that case, we might be observing a change in the legislator's perception of voter preferences, or even a change in the legislator's own ideology, rather than a change in her private returns to voting.

One way to test this empathy vs self-interest interpretation is to examine the behavior of legislators who have sons around the upper age cutoff. Under the self-interest interpre-

tation, those who have sons immediately below the cutoff will behave as if they are treated, whereas those who have sons immediately above the upper cutoff will not. Under the empathy interpretation, one would assume that the legislator’s concern for draft-eligible sons and their families would remain intact—or at least decline more gradually—as their own son crosses the upper threshold.

This test lends itself to a regression discontinuity design around the upper boundary of the draft age eligibility cutoff. We create a running variable defined as the legislator’s son’s age minus the upper cutoff. It is negative when a legislator’s son is below the upper cutoff age on a given vote, and positive when he is above it. If a legislator has more than one son, we select the age of the son closest to the cutoff. We discard all observations for legislators who have sons beneath the lower age cutoff to aid our interpretation. If a legislator has one or more sons within the draft age window and another above it, we use the age of the draft-eligible (i.e., within-window) son closest to the cutoff, as it is more relevant to the legislator’s behavior.

Formally, we estimate the following model, for $RV_{iv} \in (-h, h)$:

$$V_{iv} = \alpha + \phi \mathbf{1}\{RV_{iv} > 0\} + \delta_1 RV_{iv} + \delta_2 RV_{iv} \times \mathbf{1}\{RV_{iv} > 0\} + \varepsilon_{iv}, \quad (5)$$

where RV_{iv} is the running variable (son’s age minus upper cutoff); $\mathbf{1}\{RV_{iv} > 0\}$ is an indicator equal to 1 if RV_{iv} is positive (i.e., if the son’s age is above the upper cutoff); and h is a bandwidth determined by the procedure developed in Calonico et al. (2014). The parameter ϕ measures the effect of having a son exit the draft eligibility window on a legislator’s vote for conscription. A significant and positive ϕ indicates some support for the self-interest motive; a null effect indicates support for the empathy motive alone.

Results Estimates of ϕ are presented in Table 10. In the first column, we see that a legislator with a son slightly above the upper cutoff is 15 percentage points (or about 25% of the mean) more likely to vote for the draft than one with a son slightly below the upper cutoff. In the second column, we add controls for legislator fixed effects. The results imply that a given legislator is 15.7 percentage points more likely to support the draft after his

or her son crosses the upper cutoff relative to before. In columns (3) and (4) we repeat the exercise focusing only on close votes, or those in which the margin of victory was a maximum of 20% of the votes cast. This reduces the sample size by around 70%, and gives equivalent estimates of ϕ at 36 and 38 percentage points respectively—over half of the overall mean.

In Table 11, we test for similar discontinuities with two placebo outcomes: whether the legislator is in the Senate or the House, and whether the legislator is a Democrat or not. Whether in the close vote sample or in the full sample, there is no discontinuous association with the running variable at the upper cutoff.

In Table 12, we repeat the exercise from Table 10 only with the daughter’s age replacing the son’s age in the running variable.²⁷ In the full sample of votes there is a *negative* jump at the cutoff, and in the sample of close votes there is no significant effect. One possible explanation for this negative effect is that there is no underlying relationship between the running variable and the outcome, and so the discontinuity that we observe is one of many along the distribution. We can interrogate this by examining RD estimates at a variety of placebo cutoff points to either side of the true cutoff. In Table A5 we present 10 placebo tests for the son’s age effect. These begin at -15 and increase in intervals of three years. The true estimate is the only one that is positive and significant. There is one negative and significant estimate at the cutoff + 12 years. In Table A6, we replace the son’s age with the daughter’s age. Six of these RD estimates are significant, and of those two are positive and four are negative. These coefficients suggest there is no clear relationship between a legislator’s daughter’s age and the probability that they vote in favor of conscription.

Figures Figure A2 in the Appendix presents the visual analogue of these RD results. As the data-driven bandwidths are rarely above 5 years, we additionally present in Figure 3 a local linear regression discontinuity (RD) plot that mimics the first result in Table 10 without any bandwidth restrictions. On the right hand side is the equivalent plot where the running variable is the legislator’s daughter’s age minus the upper cutoff rather than

²⁷The running variable is generated according to the same procedure outlined above, only substituting daughters for sons in each step.

the son’s age. The discontinuity is clear with respect to the son’s age, but not with respect to the daughter’s. Figure 4 repeats the exercise using a second order polynomial on each side of the cutoffs. Again, there is a positive discontinuity on the left hand side, but not on the right. In Figure 5, we show RD plots with both placebo outcomes—Senator and Democrat—and the son’s age running variable. Neither exhibit a significant jump. Finally, in Figure 6, we plot the density of the son’s age running variable, finding no significant evidence of bunching on either side of the cutoff.

Taken together, evidence from legislators’ voting behavior around the upper cutoff strongly suggests that self-interest is at least part of the motive behind the main results rather than a sense of empathy for the electorate or an enlightened form of ideology. On average, a legislator is around 15 percentage points more likely to vote in favor of conscription when her son crosses the upper age eligibility threshold.

7 Political Agency and Voter Behavior

We postulate in Section 2 that a sufficiently large shock to R_{it} could cause political leaders to vote to enter conflicts in which the expected social costs exceed the benefits, or to avoid conflicts in which the expected social benefits exceed the costs, provided that $\theta > 0$. So far, we have shown that an exogenous increase in the private costs of conscription for some legislators reduces the likelihood that they vote in favor of enacting it. What is still unclear is whether, on average, these treated legislators with draft-eligible sons better represent their constituents’ preferences over conscription than similar control legislators with daughters of comparable age. If the treated group better reflects voters’ concerns, then it is the control group politicians that deviate from the social optimum by failing to internalize the costs of their decision. If the control group better reflects voters’ concerns, then it is the treatment group politicians that deviate from the social optimum by failing to internalize the benefits of their decision.²⁸

In this section, we endogenize the behavior of the electorate in order to better un-

²⁸We interpret the term ‘social optimum’ loosely as a reflection of the median voter’s preference; this is violated if the average preference is different to the median preference, or if voters do not have sufficient information to determine the socially optimal position.

derstand the dynamics of politicians' decisions. It is useful to anchor our analysis of voter behavior within the framework of established political agency models in which the electorate is the principal and elected officials are agents who enact legislation on their behalf.²⁹ Informational problems can arise if politicians can hide effort or motives. We consider two general types of models that align with our main results: (i) 'pure' moral hazard in which all politicians maximize private rents; and (ii) moral hazard with adverse selection, in which bad politicians maximize private rents and good politicians enact voters' preferred policies. We propose an empirical test of theoretical implications to determine which model more closely fits the data.

7.1 Pure moral hazard

The 'first generation' political agency models focus on moral hazard as the defining agency problem (Barro, 1973; Ferejohn, 1986). In these, self-serving politicians seek to maximize rents. Voters know that all politicians are self-serving, but they do not perfectly observe the rents that accrue to politicians in office nor the actions that they take. In equilibrium, elections partially constrain rent-seeking in the period before an election as politicians value future rent-seeking opportunities.³⁰

The implications of this approach in our setting are straightforward. First, it is consistent with our main result that politicians vote in their self-interest. Second, as voters are aware that all politicians are self-serving, they are therefore indifferent between incumbents and challengers. This implies that a politician's equilibrium legislative record will not affect her reelection probability.

7.2 Moral hazard with politician types

The modern workhorse political agency model presented in Besley (2006) combines elements of moral hazard from first generation political agency models with elements of models that allow for different politician types. In these, elections serve the twin purposes of restraining politician behavior, as above, and selecting 'good' politicians who care more

²⁹See Besley (2006) for an in-depth account of these models.

³⁰Elections only partially constrain politicians as voters must permit a level of rent-seeking that prevents politicians from plundering all public resources immediately.

about voter welfare. In chasing private rents, ‘bad’ politicians can also mimic good ones in order to disguise their type to the electorate.³¹ Below, we briefly describe a basic version of the model in order to consider its implications in our setting.

Environment

Consider two time periods $t \in \{1, 2\}$ in which N politicians of type $i \in \{g, b\}$ either vote against or in favor of conscription. This decision is represented by $V_{jt} \in \{0, 1\}$, where $j \in \{1, 2, \dots, N\}$ denotes an individual politician. We define type g as ‘good’ politicians for whom the weight placed on private (non-ideological) returns to legislative voting is zero, and type b as ‘bad’ politicians for whom this weight is strictly positive. Voters do not observe these types.

The state of the world $S_t \in \{0, 1\}$ determines which policy is preferred by voters. If $\sum_{j=1}^N \frac{A_{jt}}{N} > 0.5$, where

$$A_{jt} = \begin{cases} 1 & \text{if } V_{jt} = S_t \\ 0 & \text{otherwise,} \end{cases}$$

then voters receive a payoff Δ ; otherwise their payoff is zero. All politicians get a payoff E from being in office—this could reflect ‘ego rents’ (Rogoff, 1990) or other material gains from office. Good politicians receive a payoff of $E + (\Delta \mid \sum_{j=1}^N \frac{A_{jt}}{N} > 0.5)$, and always choose $V_{jt} = S_t$. Bad politicians receive a private benefit of $r_t \in (0, \bar{R})$ from choosing a policy $V_{jt} = (1 - S_t)$, where r_t is drawn independently from a distribution with a conditional density function $G(r)$. The mean value of r_t is μ , and we have shown in our main results above that $\bar{R} > \beta(\mu + E)$, where β is a discount factor. In other words, r_t can be sufficiently large that bad politicians choose policies that do not align with voter preferences.

The timing of the game is as follows. Nature determines the type of politician and the state of the world at the beginning. Once in office, politicians observe the draw r_1 and select $V_{jt}(S_t, i)$. Voters observe only V_{jt} and their own payoff, and then decide whether or not to reelect the incumbent. Following the election, politicians receive a new draw r_2 ,

³¹This is not possible in pure adverse selection models.

and Period 2 decisions are made. The game ends once Period 2 payoffs are realized.

Equilibrium

We solve for a perfect Bayesian equilibrium in which politicians behave optimally in each period given the reelection rule that voters put in place. Voters update their beliefs using Bayes rule.

In Period 2, every type of politician chooses her short term optimal decision without considering the electoral implications, i.e., $V_{j2}(S_2, g) = S_2$ and $V_{j2}(S_2, b) = (1 - S_2)$. In Period 1, good politicians choose $V_{j1}(S_1, g) = S_1$. The more interesting problem concerns the bad politician, who must weight the value of her private benefit against the present value of mimicking a good politician in order to receive rents in the second period. Let λ represent the probability that a bad politician mimics a good one in Period 1. Voters' belief that a politician is good conditional on observing $V_{j1}(S_1, i) = S_1$ is:

$$\Pi = \frac{\pi}{\pi + (1 - \pi)\lambda} \geq \pi.$$

This implies that a politician can always improve their reputation Π by voting as would a good type. If voters are retrospective—that is, if they observe and learn from legislative voting—then politicians who choose $V_{jt} = S_1$ are reelected, and those who choose $V_{jt} = (1 - S_1)$ are not reelected as they are bad types for certain and will yield voters a zero payoff in Period 2.

The optimal Period 1 decision for a bad politician is determined by the relative value of the private rent r_1 against the value of disguising her type and winning reelection, which is $\beta(\mu + E)$. Thus, the probability that a bad politician takes the action preferred by voters is

$$\lambda = G(\beta(\mu + E)).$$

Proposition 1.

- (i) Good politicians always choose $V_{jt}(S, g) = S_t$.
- (ii) Bad politicians always choose $V_{j2}(S_2, b) = (1 - S_2)$ in Period 2.

- (iii) *Bad politicians will choose $V_{j1}(S_1, b) = S_1$ in Period 1 if they earn sufficiently small private rents $r_1 < r^* \equiv \beta(\mu + E)$ from voting against the electorate's preferred policy.*
- (iv) *All politicians who choose $V_{j1}(S_1, i) = S_1$ in Period 1 are reelected.*

Bad politicians will therefore select $V_{jt} = (1 - S_t)$ in Period 1 if they earn sufficiently large private rents $r_1 > r^*$; otherwise they will mimic good politicians in order to survive to the second period. Elections can therefore discipline politicians to an extent, but they are still an imperfect mechanism as bad politicians can take actions to disguise their type.

Conscription and heterogeneous rent shocks Allow the private rent shock to be characterized as follows: one subgroup of politicians receive r_1^h and another receives r_1^l , where $r_1^h > r_1^l$. This implies that there is an exogenous difference in r_1 between politicians that cuts across both good and bad types. Good types select $V_{j1}(S_1, g) = S_1$ irrespective of r_1 , and are reelected. If $r_1^h > r_1^l > \beta(\mu + E)$, then all bad types choose $V_{j1}(S_1, b) = (1 - S_1)$ and are not reelected. Similarly, if $\beta(\mu + E) > r_1^h > r_1^l$, then all bad types choose $V_{j1}(S_1, b) = S_1$ and are reelected. However, if $r_1^h > \beta(\mu + E) > r_1^l$, then bad types with r_1^h select $V_{j1}(S_1, b | r_1^h) = (1 - S_1)$ and are not reelected, while bad types with r_1^l select $V_{j1}(S_1, b | r_1^l) = S_1$ and are reelected. With heterogeneous rent shocks, therefore, there exists an equilibrium in which some bad types pursue private rents and are voted out of office, while other bad types mimic good types and survive to Period 2 because their rent shock r_1^l is worth less than the present value of the second period returns.

Applying this logic to the case of conscription votes, we can interpret having a draft-eligible son as a source of heterogeneity in the private rent shock. For example, consider the case in which conscription is broadly unpopular with voters, i.e., $S_1 = 0$. Bad types without draft-eligible sons observe $r_1^h > \beta(\mu + E)$, meaning that their private benefit of voting in favor of conscription exceeds the present value of survival to Period 2.³² However, bad types with draft-eligible sons observe $r_1^l < \beta(\mu + E)$, and instead mimic good types by choosing $V_{j1}(0, b | r_1^l) = 0$ and winning reelection. The draft eligibility 'shock' introduces an exogenous wedge between r_1^l and r_1^h . Provided that $r_1^h > \beta(\mu + E) > r_1^l$, bad politicians

³²Private benefits in this case could stem from an ideological disposition, from national party pressure, or from lobby group or special interest pressure. In effect, any motive that is distinct from voters' preferences.

with draft-age sons will oppose the draft, improve their reputation with voters, and survive to Period 2. Conversely, if conscription is popular with voters, i.e., $S_1 = 1$, then the draft eligibility shock implies that politicians with draft age sons face r_1^h and vote against constituents' wishes, thereby revealing their true type and losing reelection.

Corollary 1. *If $S_1 = 0$ and $r_1^h > \beta(\mu + E) > r_1^l$, then politicians with draft-age sons will vote against conscription and win reelection. If $S_1 = 1$ and $r_1^h > \beta(\mu + E) > r_1^l$, then politicians with draft-age sons will vote against conscription and lose reelection.*

7.3 Testing implications

To determine which of these interpretations aligns better with the data, we examine the electoral outcomes of legislators following Congresses in which they voted on draft-related measures. In our main test, this amounts to replacing the outcome variable in equation (4) with an indicator equal to 1 if the legislator wins their next election. Our main identification assumption is that the observed relationship between having a draft age son and electoral outcomes operates through the channel of conscription votes. In the nomenclature of instrumental variables, we have observed in the main empirical section that the first stage is significant. What we cannot declare with as much confidence is that the exclusion restriction is valid. Politicians with draft-eligible sons may be more (or less) electable after congresses that contained draft votes for reasons other than their voting behavior if, for example, draft-eligible sons had a distinct influence in campaigning. We proceed with this caveat in mind.

Draft popularity Before conducting this analysis, we must first establish how aggregate public support for the draft trends over the duration of our sample. Nationally representative data on public support for the draft is available from surveys administered by the Roper Center's Public Opinion Archive in 1945, 1952, 1969, 1980, 1981, 1985, and 2003. These are analyzed by Fordham (2016), who documents a steady, steep decline in support for the draft from around 70% in 1945 to around 20% at the outset of the Iraq War in

2003.³³ The author’s principal explanation for the decline in support relates to military technology: public support for the draft is a function of its necessity to win the war. As military conflict became more capital intensive over time, the importance of conscription as means of ensuring victory waned. This suggests that public support for conscription is also likely to be declining from World War I to World War II, although we can not verify this in the absence of polling data. A second factor relates to the salience of military casualties. Karol and Miguel (2007) provide evidence that home-state casualties in the Iraq war reduced the vote share for George W. Bush between the 2000 and 2004 presidential elections. It is thus plausible that the more fatalities reported during draft-era wars, the more unpopular is the draft itself. Turning to data presented in Table 4, we can calculate U.S. fatalities per draftee for each conflict: 0.018 in World War 1, 0.029 in World War II, and 0.024 in the Cold War theaters. Given the proliferation of mass media over this period, it is reasonable to assign a higher weight to latter figures, as information on those fatalities is more likely to proliferate among voters.

Taken together, these facts indicate that voter support for the draft exhibits a downward secular trend from World War I to the Cold War conflicts. We can shed light on this interpretation by examining how senators vote in election years over time. Senators serve six-year terms with staggered elections every two years. This feature allows us to control for vote fixed effects by exploiting the fact that only one third of senators can face an election year at any moment in time. In Table 13, we present evidence that senators who are up for election are (weakly) more likely to vote in favor of the draft during World War I, and less likely to vote for the draft during World War II and the Cold War conflicts. The evidence is similar whether we use our main outcome of pro-draft votes or the hawks and doves outcome. The results are significant only in the regression using hawks and doves votes and including legislator fixed effects (Column 4).

Election Outcomes To test the implications of the model, we examine whether or not legislators with draft-eligible sons are more likely to be reelected over time in our sample.

³³Clifford and Spencer (1986) note that support for conscription was substantially lower at the beginning of World War II than in 1945. In March 1940, 20 months prior to the attack on Pearl Harbor, 98.4% of Americans opposed going to war against Germany, reflecting a “general desire not to repeat the mistakes of 1917/18” (pp. 8). This suggests that the trend is not strictly declining at all points in time.

To do this, we simply run the following specification for each of the main periods (World War I, World War II and the Cold War) as follows:

$$E_{iscj} = \alpha_i + v_{vcj} + k_{ivcj} + \gamma_1 son_{ivcj} + \gamma_2 draft_{ivcj} + \gamma_3 draft\ son_{ivcj} + \psi \mathbb{X}_{ivcj} + \epsilon_{isvcj}, \quad (6)$$

where E_{iscj} measures election outcomes for legislator i in state s following congressional session c in chamber j , and $c \in \{64, 65, 66\}$ denotes World War I; $c \in \{76, 77, 78, 79\}$ denotes World War II; and $c \in \{82, 88, 89, 90, 91, 92, 93\}$ denotes the Cold War. The variable $draft\ son_{ivcj}$ indicates that the legislator has a draft-eligible son in vote v , which is contained in congressional session c in chamber j . The vector \mathbb{X}_{ivcj} represents controls for party, house or senate, terms in office, age, and age squared. The election outcome E_{iscj} is either a binary variable indicating that legislator i was reelected or a variable representing their margin of victory in the following election (which ranges from -1 to 1). Our main specification does not include legislator fixed effects, as the outcome variables now vary at the level of a legislator-term rather than a legislator-vote.

We test the following implications from the political-agency models described above:

1. **Pure moral hazard.** In the pure moral hazard model with only bad types, voters do not learn from legislative behavior. The *draft son* variables will have no effect on a legislator's subsequent election performance, i.e., $\gamma_3 = 0, \forall c$.
2. **Moral hazard with politician types.** In the moral hazard model with politician types, voters do learn from legislative behavior. The *draft son* variable will have a positive impact on election performance if $S_t = 0$ is state of the world and a negative impact if $S_t = 1$ is the state of the world. If S_t is declining over time, the empirical implication becomes $(\hat{\gamma}_3 \mid 1\{World\ War\ I\}) < (\hat{\gamma}_3 \mid 1\{World\ War\ II\}) < (\hat{\gamma}_3 \mid 1\{Cold\ War\})$

In the second implication, we treat the *draft son_{iv}* variable as an exogenous wedge between r_1^l and r_1^h . All bad politicians observe a private rent shock that we do not observe, but those with draft-eligible sons receive a different *net* rent shock once they take into

account their additional private costs of conscription. This difference between r_1^l and r_1^h is observed as *draft son_{iv}*.

7.4 Results

We present the results of this exercise in Figure 7 and in Appendix Tables A7 to A9. The outcome variable is equal to 1 if the legislator won reelection to the following congressional session and equal to 0 if the legislator lost reelection to the following congressional session. It is not defined for legislators who do not contest the following election. Having a draft-age son is negatively associated with reelection during World War I and World War II. All six point estimates (that is, for models that cumulatively include fixed effects for number of children, state, and roll call vote) are negative. In the first model using the WWI sample, the point estimate is -0.1676 ($p < 0.05$). In the other five models, the point estimate is around -6 to -7 percentage points, and is marginally significant ($p < 0.10$) in two cases, and not significant in three. The mean probability of reelection conditional on contesting the election is 0.79 in WWI and 0.84 in WWII.

By contrast, all three point estimates on the Cold War sample are in the region of $+1.5$ percentage points, although none are statistically distinguishable from zero. The mean probability of reelection conditional on contesting is 0.89.³⁴ We show in Appendix Tables A13 to A15 that these differences are not driven by differences in the “first stage” relationship between having a draft-age son and pro-draft voting, which is negative and significant during both World War I and the Cold War.

7.5 Discussion

These findings suggest that conscription became more unpopular with voters over time; and that legislators with draft-age sons became increasingly more likely to win reelection. Taken together, they provide suggestive evidence in support of the model’s implications that bad politicians pool their legislative votes with good types when the payoff from

³⁴In Appendix Figure A3 and Tables A10 to A12 we present analogous results where the outcome variable is the next election margin of victory, ranging from -1 to 1 . The results follow a similar pattern, with all point estimates in the Cold War sample greater than the point estimates in the WWI sample. Model 1 in the WWII sample is something of an anomaly, where the point estimate is $+0.05$ ($p > 0.10$).

unpopular votes is (exogenously) lower.

An interesting question to consider is why some politicians still vote in favor of conscription when it is broadly unpopular. Our model and results imply that the control group of otherwise identical legislators who voted in favor of conscription are deriving utility from their vote through channels other than voter preferences. Linking back to Section 2, these could be party career concerns (P_{it}), ideology (F_i), or other unobserved private benefits (R_{it}). In Table 14, we provide some evidence in support of party career concerns as a partial explanation. We add to our baseline specification an indicator that is equal to 1 if the president is from the same party as a given legislator. With legislator fixed effects, this is identified off the switch from Lyndon B. Johnson to Richard Nixon in 1968. With or without fixed effects, this variable has a strong positive association with pro-draft voting.

This finding is supported by narrative accounts of Nixon’s approach to conscription before and after his election as president. Fordham (2016, p. 29) notes that while there were Republicans and Democrats on both sides of the debate over ending the draft, Nixon “campaign[ed] on a promise to put a stop to it, but repeatedly asked for its extension as president.”³⁵ Nixon’s turn was particularly evident at the time of the Hatfield-Goldwater amendment to raise the pay of the military in 1970. The measure was an explicit attempt to end the draft by attracting a sufficient number of volunteers to render it obsolete within a year. The New York Times wrote at the time:

President Nixon campaigned in favor of a volunteer army in 1968 and has supported the concept time and again since he became President. But he opposed the Hatfield-Goldwater amendment on the grounds that it would be too expensive and that the draft was essential as long as the United States maintained a sizable force in Southeast Asia.³⁶

That legislative voting reflects this pattern is reassuring. More evidence on this motive can be gleaned from newspaper reports at the other end of our sample period. Under

³⁵Similarly, Fordham notes that “Ronald Reagan criticized Jimmy Carter’s decision to restore draft registration during his 1980 presidential campaign, but then decided to continue registration after he became president.”

³⁶“Senate Bars Plan Designed To Bring Volunteer Army,” *The New York Times*, Aug. 26, 1970

the heading “Ban Two Draft Opponents — Democrats in Cleveland Declare Gordon and Crosser ‘Done’,” The New York Times reported the following in April 1917:

Indignant at the spectacle of two Congressman from Cleveland openly opposing President Wilson’s war policies, the leaders of the local Democratic organization today declared William Gordon and Robert Crosser “done.” The two Congressmen were practically read out of the Democratic Party by the declaration that the political organization of which Secretary of War Baker is head will never again support either man for nomination or election.³⁷

These accounts, coupled with our findings above, suggest that pro-draft voting in the U.S. Congress appears to stem at least in part from national party edicts. This does not rule out important roles for other motives noted above: other common arguments that repeatedly appear in archival reports relate to the technological efficiency of conscription relative to volunteer armies, and also to a distributional motive whereby high-income special interests favor conscription ahead of a war tax. This latter motive aligns well with the consistent opposition to conscription demonstrated by organized labor groups throughout the 20th century.

In summary, we find evidence supporting a model of political agency that combines aspects of moral hazard and adverse selection. When conscription is relatively popular, voters punish legislators with draft-eligible sons; when it is relatively unpopular, voters are more likely to reward them.

8 Conclusion

In this paper, we test the hypothesis that political agency problems contribute to violent conflict: political leaders who do not internalize the costs of war are more likely to vote in favor of it. We demonstrate this by compiling data on the voting behavior and family compositions of over 3,300 legislators who served in the U.S. Congress during the four conscription era wars of the 20th century. We find that, (i) relative to those with daughters

³⁷“Ban Two Draft Opponents — Democrats in Cleveland Declare Gordon and Crosser ‘Done’,” *The New York Times*, Apr. 28, 1917

of comparable age, legislators with sons who are eligible to be drafted are around 10-17% less likely to vote for conscription; (ii) legislators increase their support for the draft by a quarter when their sons cross the upper age threshold; and (iii) legislators with draft-eligible sons are more likely to win reelection when the draft is less popular.

We interpret these results within the framework of a political agency model that combines aspects of moral hazard and adverse selection. Good politicians reflect voters' concerns and are reelected; bad politicians can choose between pooling with good ones in order to win reelection, or voting against the electorate's preference in order to pursue private rents. In our set up, having a draft-eligible son introduces exogenous variation in the private benefits of conscription for bad politicians. Consistent with this model, we show that politicians with draft-eligible sons are more likely to be reelected when the draft is broadly unpopular. This indicates that some bad politicians pool their votes with good ones in order to win reelection.

To the best of our knowledge, this is the first paper to identify the impact of changing private incentives on legislative voting with individual fixed effects. This implies that politicians are malleable, which itself has potentially interesting implications beyond the issue of conscription. Exploring private incentives of legislators in other policy domains remains a fruitful avenue for future research. Our results also suggest that representative democracy may better enhance social welfare when voters are aware of legislators' private incentives, and when they vote often enough to impose accountability on important legislative decisions.

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Figures

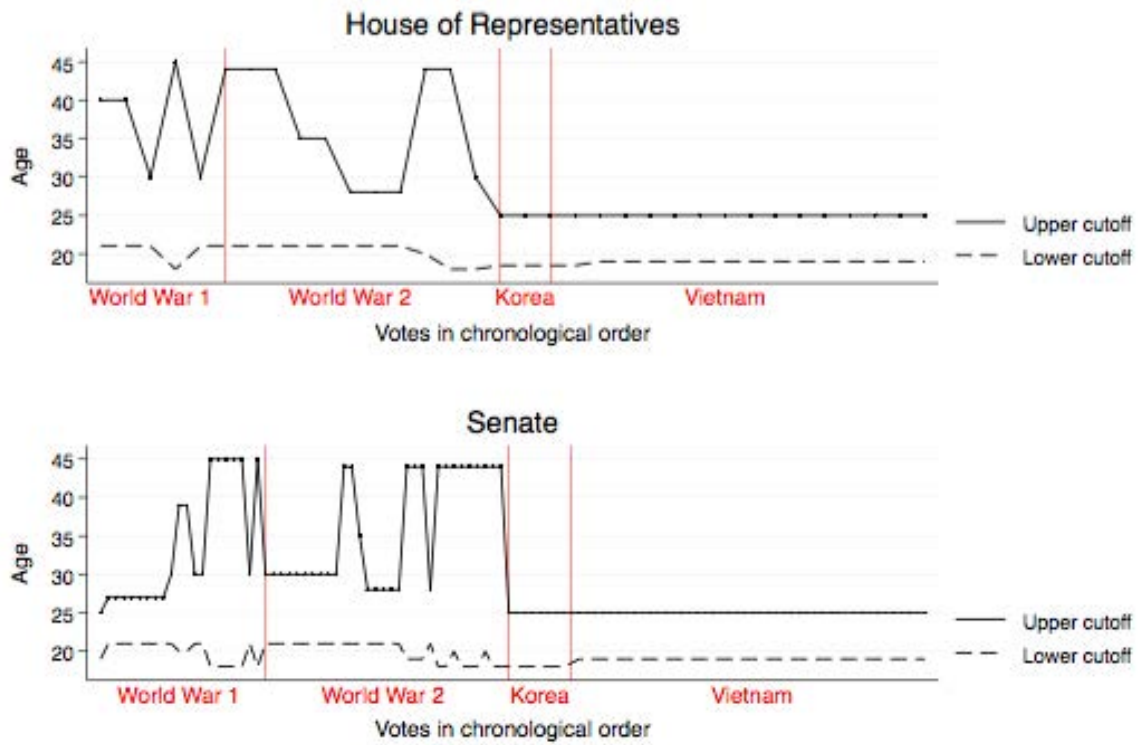


Figure 1: Proposed draft age cutoffs by roll call vote in the House and Senate.

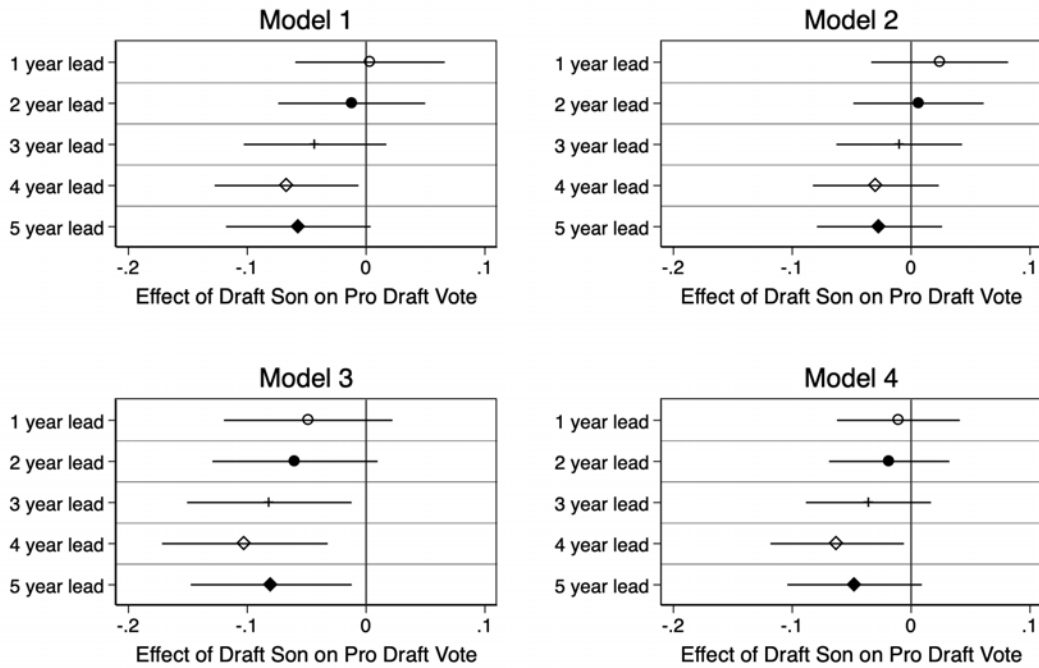


Figure 2: **Impact of having draft-eligible son on pro-draft votes at various lower thresholds.** The models correspond to those shown in Table 5. Leads refer to the number of years below the lower draft cutoff used to calculate the age boundary for the treatment variable. Error bars represent 95 percent confidence intervals.

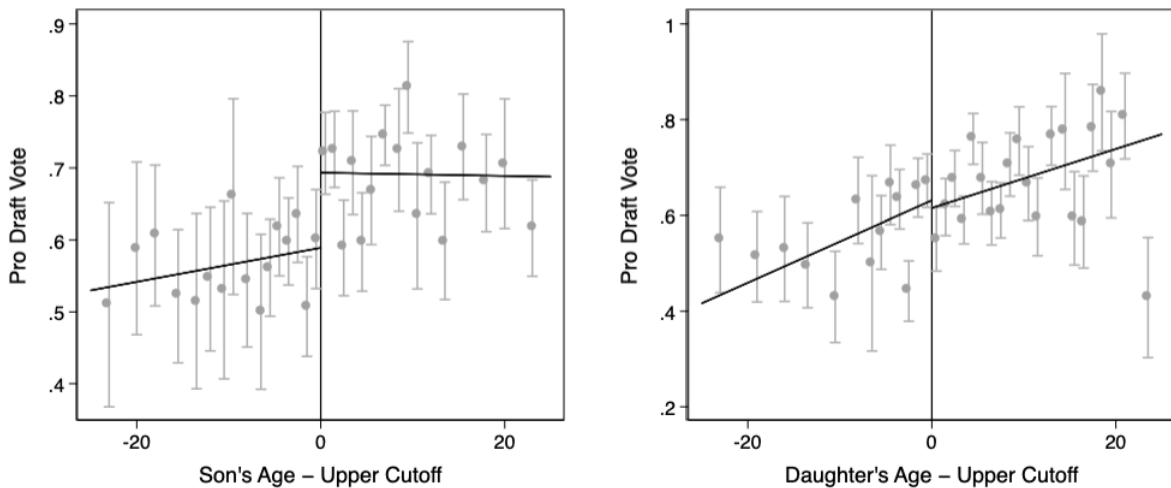


Figure 3: **Regression Discontinuity at the Upper Cutoff.** Son age coefficient: 0.1379 (SE: 0.06); Daughter age coefficient -0.016 (SE 0.02). Bandwidth: 25.

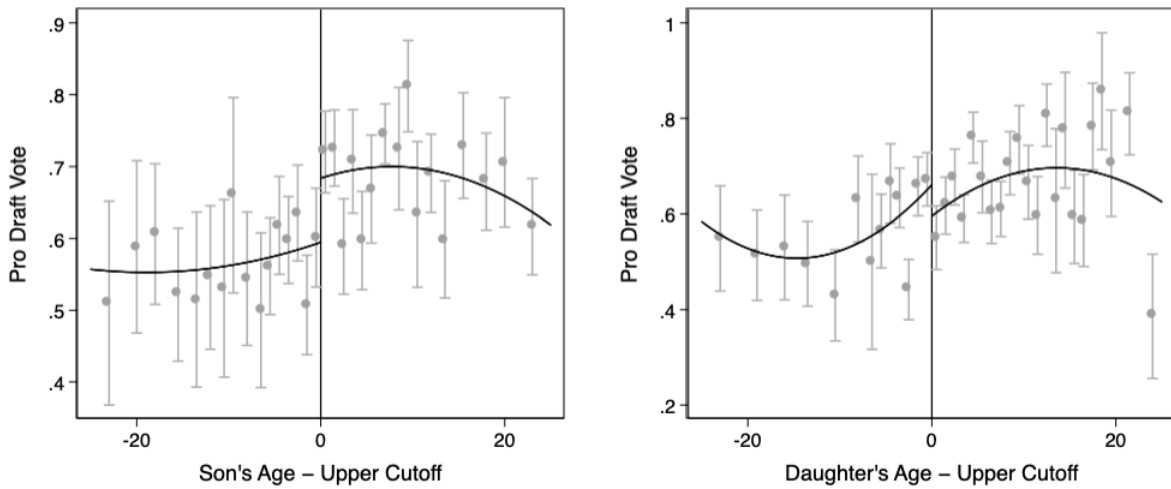


Figure 4: **Regression Discontinuity at the Upper Cutoff: Quadratic fits.** Son age coefficient: 0.1915 (SE: 0.06); Daughter age coefficient -0.066 (SE 0.03). Bandwidth: 25.

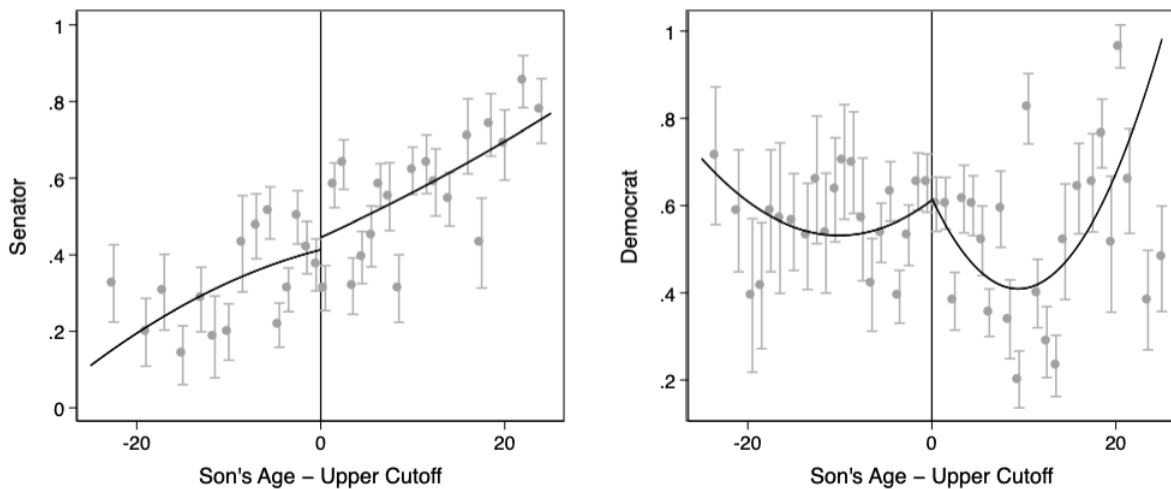


Figure 5: **Regression Discontinuity at the Upper Cutoff: Placebo Outcomes.** Son age coefficient in Senator regression: 0.0103 (SE: 0.05); Son age coefficient in Democrat regression -0.0229 (SE 0.0811). Bandwidth: 25.

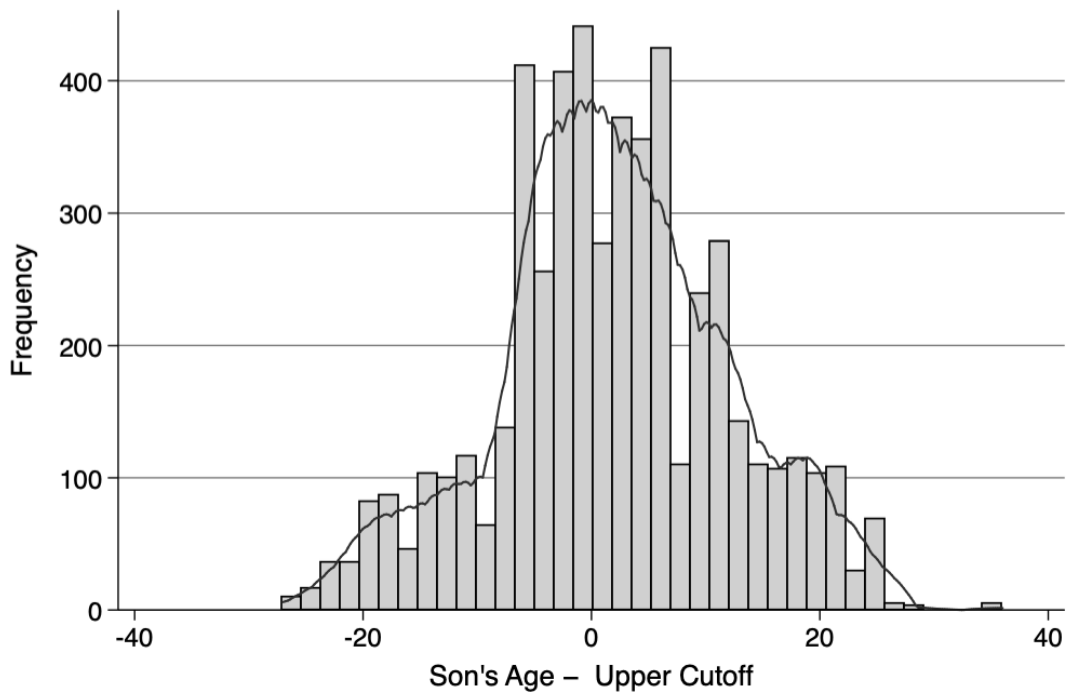


Figure 6: Running Variable Density

Impact of Draft Son on Pr(reelection) by War

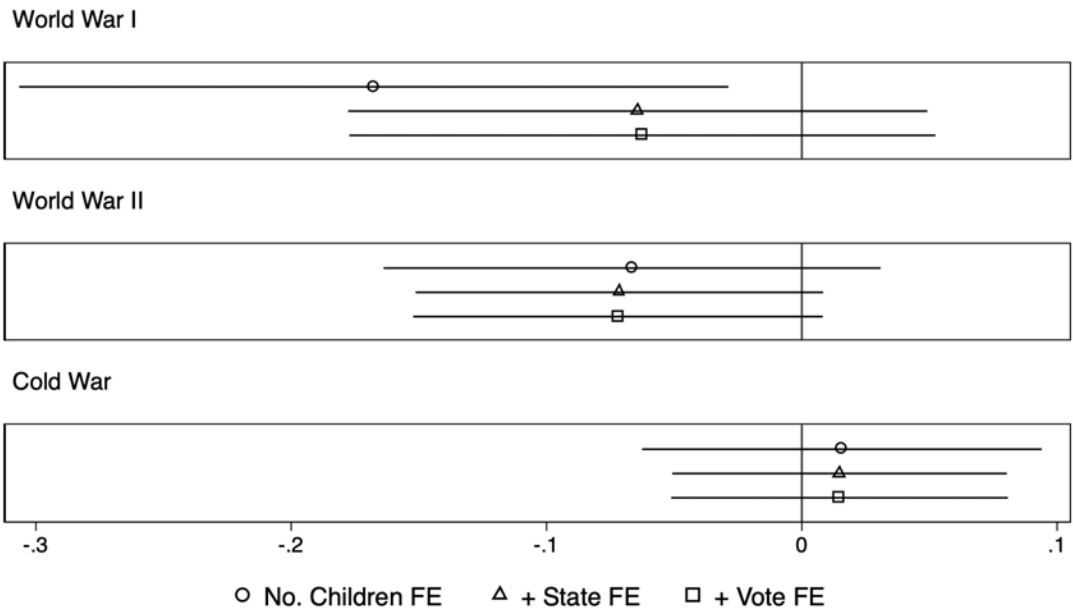


Figure 7: **Impact of having draft-eligible son on reelection probability.** The models correspond to those shown in Appendix Tables A7 to A9. Error bars represent 95 percent confidence intervals.

Tables

Table 1: Roll-Call Vote Summary Statistics

Congress	Draft Votes (Main Sample)			Draft Votes (All)	
	Votes	Pro Draft	Margin	Votes	Margin
<i>Senate</i>					
93	2	0.61	0.32	2	0.32
92	34	0.49	0.35	58	0.39
91	2	0.63	0.40	3	0.35
90	7	0.79	0.77	9	0.79
89	1	0.93	0.86	3	0.55
82	8	0.61	0.38	12	0.46
79	6	0.55	0.46	13	0.39
77	12	0.52	0.30	21	0.31
76	13	0.50	0.39	22	0.35
66	1	0.11	0.01	2	0.06
65	20	0.43	0.53	33	0.46
Total	106			178	
Mean	9.64	0.56	0.44	16.18	0.40
Std. Dev.	10.03	0.21	0.23	17.08	0.18
<i>House</i>					
92	10	0.60	0.34	11	0.35
91	2	0.74	0.47	2	0.47
90	2	0.85	0.70	2	0.70
89	1	0.88	0.77	1	0.77
88	1	0.88	0.77	1	0.77
82	2	0.80	0.60	3	0.55
79	2	0.42	0.03	9	0.15
77	5	0.47	0.28	8	0.18
76	4	0.53	0.15	5	0.21
65	5	0.70	0.49	12	0.37
Total	34			54	
Mean	3.4	0.69	0.46	5.4	0.45
SD	2.76	0.17	0.26	4.25	0.24
<i>Combined</i>					
Total	140	0.58	0.18	232	0.17
Std. Dev.		0.49	0.12		0.12

Note: Data on vote records is from the Voteview project. Data on pro-draft voting is calculated by the authors based on contemporaneous newspaper reports. *Main Sample* refers to these votes. *All* refers to these votes plus other draft-related votes that took place in the same congressional sessions.

Table 2: Family Composition Summary Statistics

Congress	Legislators	Children		Sons		Daughters		Draft Age	
		Any	No.	Any	No.	Any	No.	Any Son	Any Dtr.
<i>Senate</i>									
93	102	0.97	2.94	0.79	1.49	0.80	1.45	0.31	0.32
92	103	0.95	2.75	0.78	1.44	0.77	1.31	0.23	0.27
91	102	0.93	2.73	0.76	1.40	0.75	1.32	0.22	0.24
90	101	0.91	2.61	0.74	1.38	0.72	1.24	0.19	0.18
89	103	0.89	2.58	0.74	1.37	0.70	1.21	0.17	0.15
82	99	0.89	2.34	0.66	1.18	0.68	1.16	0.20	0.22
79	109	0.86	2.49	0.75	1.34	0.61	1.15	0.23	0.22
77	109	0.87	2.48	0.74	1.30	0.60	1.18	0.24	0.17
76	104	0.85	2.57	0.73	1.39	0.58	1.17	0.33	0.26
66	101	0.75	2.14	0.57	1.04	0.55	1.10	0.43	0.42
65	111	0.74	1.98	0.56	0.96	0.56	1.02	0.15	0.17
Mean	105.30	0.87	2.48	0.71	1.29	0.66	1.20	0.23	0.23
Std. Dev.	3.92	0.33	1.69	0.45	1.12	0.47	1.13	0.42	0.42
<i>House</i>									
92	442	0.88	2.63	0.71	1.34	0.71	1.29	0.26	0.28
91	447	0.88	2.58	0.71	1.29	0.71	1.29	0.06	0.04
90	438	0.89	2.53	0.71	1.27	0.73	1.25	0.22	0.21
89	443	0.87	2.37	0.68	1.22	0.68	1.15	0.20	0.19
88	443	0.89	2.34	0.69	1.20	0.69	1.14	0.17	0.20
82	447	0.81	1.88	0.60	0.95	0.60	0.93	0.18	0.19
79	444	0.81	1.98	0.58	0.99	0.61	0.99	0.24	0.22
77	452	0.79	1.92	0.61	1	0.55	0.92	0.12	0.10
76	457	0.80	2.02	0.62	1.06	0.59	0.96	0.31	0.25
65	456	0.79	2.28	0.63	1.21	0.61	1.07	0.21	0.19
Mean	447.91	0.84	2.29	0.66	1.18	0.64	1.11	0.21	0.21
Std. Dev.	6.36	0.37	1.79	0.47	1.21	0.48	1.15	0.41	0.40
<i>Combined</i>									
Total	2287	0.85	2.37	0.68	1.23	0.65	1.15	0.22	0.21
Std. Dev.		0.35	1.75	0.47	1.17	0.48	1.14	0.41	0.41

Note: Data on the family composition of legislators comes from census records (1840-1940) where possible and a variety of other biographical records where not. See main text for more details.

Table 3: Age Thresholds

Lower cutoff	Freq.	Percent
18	3,555	13.53
18.5	1,883	7.17
19	11,142	42.41
20	892	3.40
21	8,798	33.49

Upper cutoff	Freq.	Percent
25	13,490	51.35
27	888	3.38
28	2,010	7.65
30	2,840	10.81
35	1,013	3.86
39	222	0.85
40	912	3.47
44	3,783	14.40
45	1,112	4.23

Note: These are proposed draft age thresholds based on roll-call votes. The unit of analysis is the legislator-vote.

Table 4: Registration, Deployment and Fatalities

	Total in Service	Draft Inductions	Draft Registered	Battle Deaths
World War 1	4,734,991	2,810,296	24,000,000	53,402
World War 2	16,112,566	10,110,104	45,000,000	291,557
Korea	5,720,000	1,529,539	13,200,000	33,739
Vietnam	8,744,000	1,857,304	27,000,000	47,434
Total	35,311,557	16,307,243	109,200,000	426,132

Note: Data on total U.S. Servicemembers and Battle Deaths are from the “America’s Wars’ fact sheet compiled by the U.S. Department of Veteran’s Affairs, accessed at https://www.va.gov/opa/publications/factsheets/fs_americas_wars.pdf. Data on Draft Inductions are from U.S. Selective Service System, accessed at <https://www.sss.gov/About/History-And-Records/Induction-Statistics>. Data on total number of men registered for the draft come from multiple sources: the WW1 figure is from <http://www.history.com/this-day-in-history/u-s-congress-passes-selective-service-act>; the WW2 figure is from <https://www.cbo.gov/sites/default/files/cbofiles/ftpdocs/83xx/doc8313/07-19-militaryvol.pdf>; the Korean War figure is from Flynn (2002, p. 73).; and the Vietnam War figure are from Morris (2006, p. 15). All website were accessed on 10/29/2017.

Table 5: Impact of Having Draft-Age Son on Pro-Draft Vote (Main Votes)

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0669** (0.0305)	-0.0297 (0.0267)	-0.1021*** (0.0351)	-0.0622** (0.0283)
Draft age child	-0.0211 (0.0271)	-0.0411* (0.0231)	0.0125 (0.0307)	0.0132 (0.0277)
Any son	0.0121 (0.0308)	-0.0069 (0.0256)	0.1444 (0.1023)	-0.0298 (0.0853)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.605	0.605	0.605	0.605
Observations	19159	19159	18995	18995

Note: Standard errors are doubled clustered by legislator and vote.
 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote.

Table 6: Impact of Having Draft-Age Son on Pro-Draft Vote (Close Votes)

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son \times close vote	-0.2006*** (0.0259)	-0.1924*** (0.0254)	-0.1497*** (0.0264)	-0.0179 (0.0186)
Draft age son	-0.0033 (0.0327)	0.0310 (0.0296)	-0.0564 (0.0364)	-0.0571** (0.0284)
Draft age child	-0.0203 (0.0271)	-0.0402* (0.0230)	0.0140 (0.0305)	0.0133 (0.0277)
Any son	0.0132 (0.0308)	-0.0057 (0.0257)	0.1325 (0.1001)	-0.0301 (0.0854)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.605	0.605	0.605	0.605
Observations	19159	19159	18995	18995

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. Close roll-call votes are those in which the margin of victory is within 20 percentage points.

Table 7: Hawks and Doves Method with Draft Category Votes

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0651** (0.0327)	-0.0181 (0.0263)	-0.0746** (0.0295)	-0.0552** (0.0241)
Draft age child	0.0007 (0.0288)	-0.0224 (0.0234)	0.0422 (0.0271)	0.0340 (0.0228)
Any son	0.0428 (0.0313)	0.0245 (0.0245)	0.1070 (0.0936)	0.0261 (0.0825)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.625	0.625	0.627	0.627
Observations	20786	20786	20578	20577

Note: Standard errors are doubled clustered by legislator and vote.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. A hawkish vote is one that aligns with the modal vote cast by hawks, and against the modal vote cast by doves.

Table 8: Hawks and Doves Method with Draft Category Votes Excluding Main Votes

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0694 (0.0455)	-0.0191 (0.0364)	-0.1219** (0.0567)	-0.0827* (0.0419)
Draft age child	0.0028 (0.0366)	-0.0293 (0.0311)	0.0253 (0.0537)	0.0392 (0.0370)
Any son	0.0910** (0.0346)	0.0720** (0.0286)	0.0756 (0.1680)	0.1040 (0.1200)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.568	0.568	0.565	0.565
Observations	6632	6632	5946	5946

Note: Standard errors are doubled clustered by legislator and vote.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. A hawkish vote is one that aligns with the modal vote cast by hawks, and against the modal vote cast by doves.

Table 9: Hawks and Doves Method with Non-Draft Category Votes

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Draft age son	0.0114 (0.0181)	0.0298* (0.0153)	-0.0097 (0.0143)	-0.0146 (0.0120)
Draft age child	-0.0209 (0.0167)	-0.0263* (0.0142)	0.0129 (0.0144)	0.0114 (0.0118)
Any son	0.0099 (0.0195)	-0.0022 (0.0151)	0.0559 (0.0523)	0.0746 (0.0460)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.514	0.514	0.514	0.514
Observations	175591	175591	175591	175556

Note: Standard errors are doubled clustered by legislator and vote.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. A hawkish vote is one that aligns with the modal vote cast by hawks, and against the modal vote cast by doves.

Table 10: Regression Discontinuity at the Upper Cutoff

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
RD estimate at upper cutoff	0.1520*** (0.0426)	0.1569*** (0.0456)	0.3572*** (0.1268)	0.3792*** (0.1307)
Bandwidth	CCT	CCT	CCT	CCT
Running variable	Son age	Son age	Son age	Son age
Legislator FE	No	Yes	No	Yes
Vote Sample	All	All	Close	Close
Observations	5185	5098	1577	1534

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The running variable is the legislator's son's age minus the upper draft age threshold. Close votes are those in which the margin was within 20 percentage points

Table 11: Regression Discontinuity with Placebo Outcomes

	Senator		Democrat	
	(1)	(2)	(3)	(4)
RD estimate at upper cutoff	0.0094 (0.0422)	0.0963 (0.0935)	-0.0229 (0.0811)	-0.1073 (0.0822)
Bandwidth	CCT	CCT	CCT	CCT
Running variable	Son age	Son age	Son age	Son age
Vote Sample	All	Close	All	Close
Observations	5185	1577	5155	1565

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The running variable is the legislator's son's age minus the upper draft age threshold.

Table 12: Regression Discontinuity with Daughter Age

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
RD estimate at upper cutoff	-0.1225*** (0.0420)	-0.1241*** (0.0422)	-0.1608 (0.1089)	-0.1602 (0.1090)
Bandwidth	CCT	CCT	CCT	CCT
Running variable	Dtr age	Dtr age	Dtr age	Dtr age
Legislator FE	No	Yes	No	Yes
Vote Sample	All	All	Close	Close
Observations	5348	5272	1604	1565

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The running variable is the legislator's daughter's age minus the upper draft age threshold.

Table 13: Election Proximity by War

	Pro Draft Vote		Hawkish Vote	
	(1)	(2)	(3)	(4)
Election Year	0.0249 (0.0480)	0.0610 (0.0659)	0.0334 (0.0674)	0.1467** (0.0583)
Election Year \times WWII	0.0037 (0.0850)	-0.0241 (0.0970)	-0.0197 (0.0875)	-0.1441** (0.0720)
Election Year \times Cold War	-0.0616 (0.0539)	-0.1207 (0.0859)	-0.0365 (0.0821)	-0.1517* (0.0883)
Vote FE	Yes	Yes	Yes	Yes
Legislator FE	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.622	0.622	0.627	0.629
Observations	15426	15284	16476	16241

Note: Standard errors are doubled clustered by legislator and vote.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. A hawkish vote is one that aligns with the modal vote cast by hawks, and against the modal vote cast by doves.

Table 14: President Party

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0577** (0.0286)	-0.0276 (0.0249)	-0.0900*** (0.0330)	-0.0544** (0.0263)
Draft age child	-0.0190 (0.0258)	-0.0393* (0.0222)	0.0037 (0.0296)	0.0072 (0.0262)
Any son	0.0168 (0.0293)	-0.0012 (0.0247)	0.1669 (0.1016)	-0.0064 (0.0854)
President party	0.2346*** (0.0285)	0.2053*** (0.0261)	0.1359*** (0.0248)	0.1124*** (0.0217)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.605	0.605	0.605	0.605
Observations	19159	19159	18995	18995

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. *President party* indicates that the sitting president represents the same party as the given legislator.

Appendix

Appendix Figures

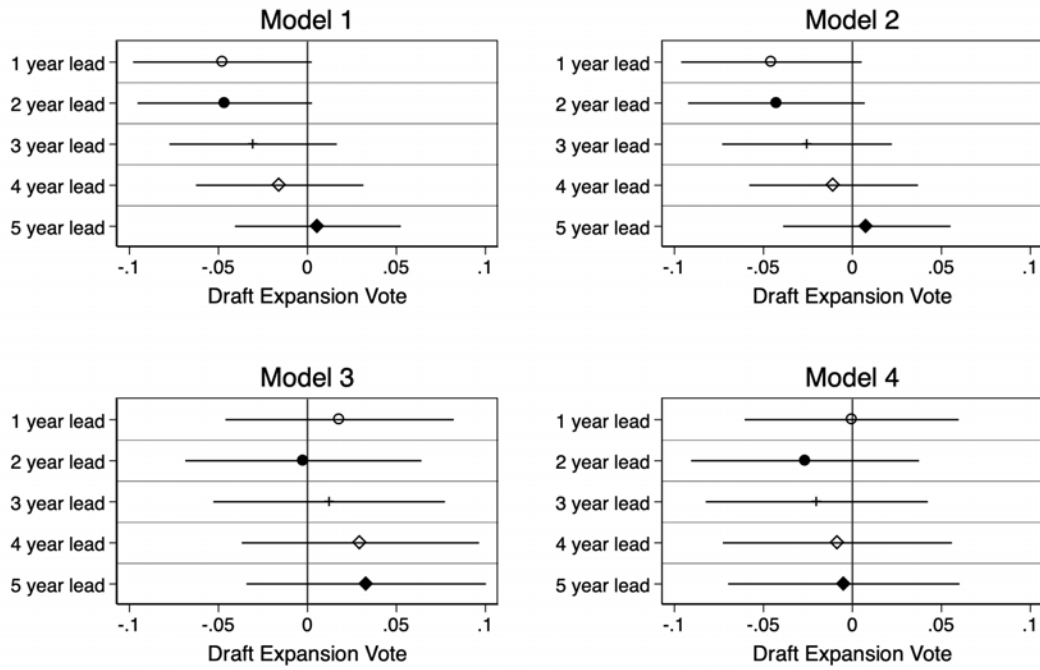


Figure A1: Impact of having draft-eligible son on window votes at various lower thresholds. Error bars represent 95 percent confidence intervals.

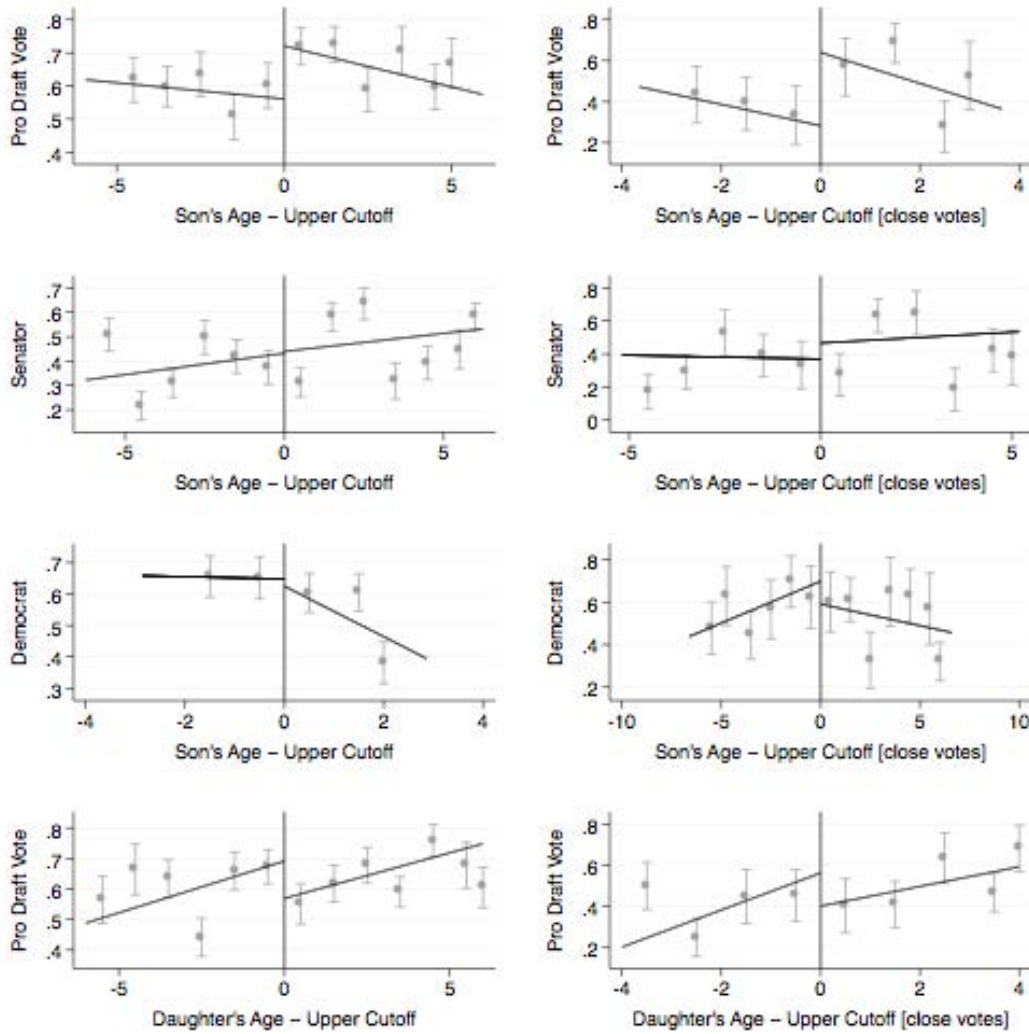


Figure A2: Corresponding RD plots for results presented in Tables 10, 11, and 12. Specifications with fixed effects are omitted. The top panel presents our main results with a full sample and with close votes. The second and third panels show RD results with placebo outcomes. The bottom panel shows RD results using the age of the legislator's daughter rather than son.

Impact of Draft Son on Next Election Margin by War

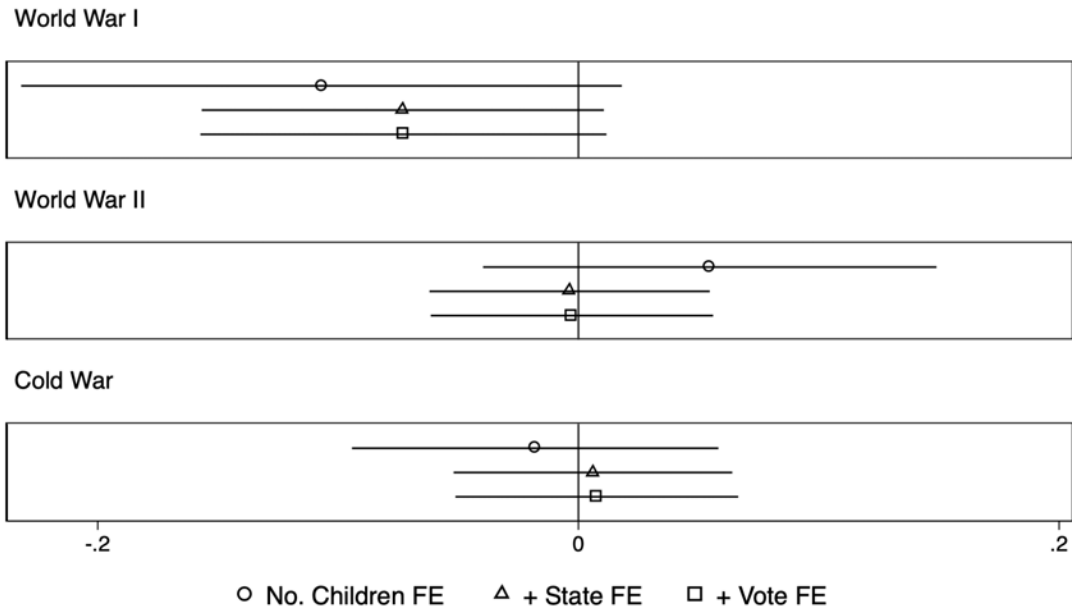


Figure A3: **Impact of having a draft-eligible son on next election margin, by war.** The models correspond to those shown in Appendix Tables A10 to A12. Error bars represent 95 percent confidence intervals.

Impact of Draft Son on Pro Draft Votes by War

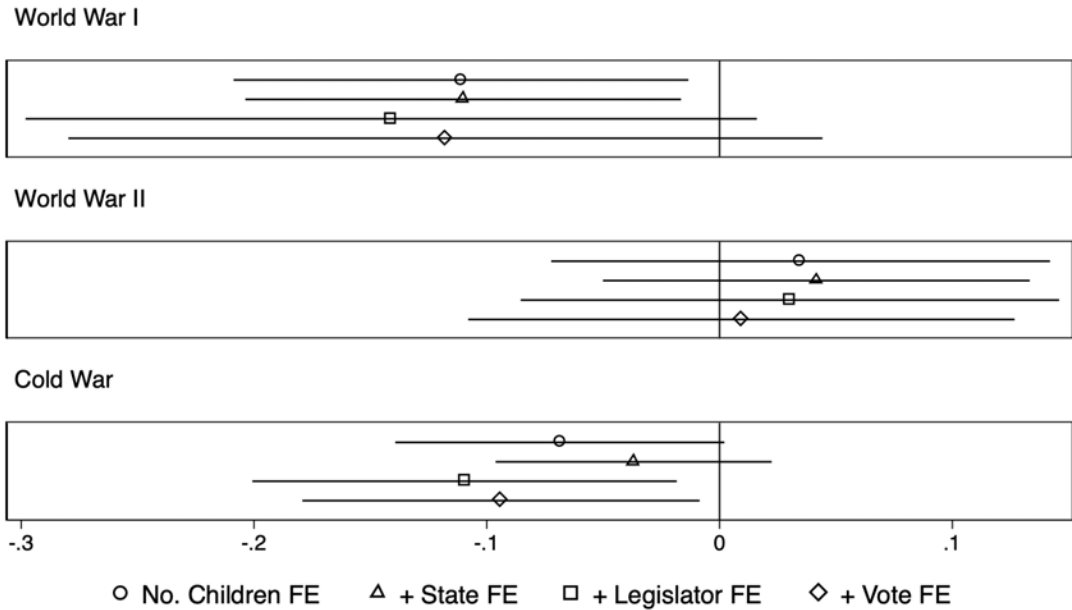


Figure A4: **Impact of having a draft-eligible son on pro draft voting, by war.** The models correspond to those shown in Appendix Tables A13 to A15. Error bars represent 95 percent confidence intervals.

Appendix Tables

Table A1: Main Model with Added Controls for 2nd Order Polynomial in Each Child's Age

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0641** (0.0308)	-0.0222 (0.0265)	-0.0826** (0.0345)	-0.0471* (0.0273)
Draft age child	-0.0310 (0.0298)	-0.0457* (0.0255)	0.0056 (0.0307)	0.0156 (0.0281)
Any son	0.0134 (0.0306)	-0.0103 (0.0253)	0.1097 (0.1041)	-0.0537 (0.0912)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.605	0.605	0.605	0.605
Observations	19159	19159	18995	18995

Note: Standard errors are doubled clustered by legislator and vote.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote.

Table A2: Conditional Logit (Main Votes)

	Pro Draft Vote		
	(1)	(2)	(3)
Draft age son	-0.2847** (0.1301)	-0.1287 (0.1234)	-0.6764*** (0.2481)
Draft age child	-0.0939 (0.1242)	-0.1981* (0.1152)	0.0609 (0.2427)
Any son	0.0513 (0.1325)	-0.0309 (0.1176)	0.6144 (0.5110)
Legislator FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.605	0.605	0.573
Observations	19145	19145	14277

Note: Standard errors are clustered by legislator. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Impact of Having Draft-Age Son on Pro-Draft Vote (Window Votes)

	Pro Draft Expansion Vote			
	(1)	(2)	(3)	(4)
Draft age son (window)	-0.0478 (0.0334)	-0.0455 (0.0343)	-0.0574* (0.0306)	-0.0004 (0.0441)
Draft age child (window)	0.0877 (0.0559)	0.0836 (0.0564)	0.0238 (0.0286)	-0.0006 (0.0348)
Any son	0.0017 (0.0269)	-0.0143 (0.0227)	-0.0196 (0.0235)	0.0625 (0.1683)
Legislator FE	No	No	No	Yes
Vote FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.512	0.512	0.512	0.519
Observations	7109	7109	7109	6884

Note: Standard errors are doubled clustered by legislator and vote.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote.

Table A4: Impact of Having Draft-Age Son on Pro-Draft Vote Using Existing Rather than Proposed Draft Cutoffs

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0567** (0.0281)	-0.0224 (0.0253)	-0.0057 (0.0246)	-0.0396* (0.0238)
Draft age child	0.0408 (0.0366)	0.0221 (0.0359)	-0.0249 (0.0225)	0.0138 (0.0222)
Any son	0.0053 (0.0237)	-0.0116 (0.0208)	-0.0188 (0.0203)	0.0301 (0.0886)
Vote FE	No	No	Yes	Yes
Legislator FE	No	No	No	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.580	0.580	0.580	0.579
Observations	26371	26371	26371	26278

Note: Standard errors are double clustered by legislator and vote.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Regression Discontinuity at the Upper Cutoff – Placebo Cutoffs with Son Age

	Pro Draft Vote										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
RD estimate at upper cutoff	-0.0138 (0.1355)	-0.1331 (0.1114)	-0.1408 (0.1025)	0.0981 (0.0650)	-0.0482 (0.0527)	0.1520*** (0.0426)	0.0769 (0.0538)	0.0852 (0.0528)	0.0845 (0.0684)	-0.2630*** (0.0576)	0.1030 (0.0904)
Cutoff	-15	-12	-9	-6	-3	0	3	6	9	12	15
Bandwidth	3.80	4.48	4.62	6.59	4.61	6.23	4.43	4.74	4.01	4.82	4.26
Running variable	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age
Observations	5185	5185	5185	5185	5185	5185	5185	5185	5185	5185	5185

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The running variable is the legislator's son's age minus the upper draft age threshold.

Table A6: Regression Discontinuity at the Upper Cutoff – Placebo Cutoffs with Daughter Age

	Pro Draft Vote										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
RD estimate at upper cutoff	-0.0062 (0.1454)	-0.0469 (0.1297)	0.4113*** (0.1154)	0.0498 (0.0933)	-0.1632*** (0.0556)	-0.1240*** (0.0421)	-0.0666 (0.0456)	-0.1499*** (0.0525)	-0.0306 (0.0609)	0.2393*** (0.0602)	-0.1908** (0.0891)
Cutoff	-15	-12	-9	-6	-3	0	3	6	9	12	15
Bandwidth	3.73	4.60	4.54	6.25	4.56	5.92	4.42	4.74	4.01	4.82	4.26
Running variable	Dtr age	Dtr age	Dtr age	Dtr age	Dtr age	Dtr age	Dtr age	Dtr age	Dtr age	Dtr age	Dtr age
Observations	5348	5348	5348	5348	5348	5348	5348	5348	5348	5348	5348

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The running variable is the legislator's daughter's age minus the upper draft age threshold.

Table A7: Next Election Victory: WWI

	1(Reelected)		
	(1)	(2)	(3)
Draft age son	-0.1676** (0.0707)	-0.0643 (0.0577)	-0.0625 (0.0584)
Draft age child	0.1106 (0.0676)	0.0729 (0.0520)	0.0740 (0.0524)
Any son	0.0292 (0.0717)	-0.0845 (0.0568)	-0.0856 (0.0571)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.790	0.790	0.790
Observations	1335	1335	1335

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election victory is equal to 1 if the incumbent wins the next election.

Table A8: Next Election Victory: WWII

	1(Reelected)		
	(1)	(2)	(3)
Draft age son	-0.0665 (0.0496)	-0.0715* (0.0406)	-0.0720* (0.0409)
Draft age child	0.0093 (0.0489)	0.0267 (0.0360)	0.0265 (0.0364)
Any son	0.0568 (0.0395)	0.0478 (0.0373)	0.0463 (0.0375)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.844	0.844	0.844
Observations	4138	4138	4138

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election victory is equal to 1 if the incumbent wins the next election.

Table A9: Next Election Victory: Cold War

	1(Reelected)		
	(1)	(2)	(3)
Draft age son	0.0157 (0.0399)	0.0148 (0.0334)	0.0148 (0.0336)
Draft age child	-0.0159 (0.0383)	-0.0092 (0.0324)	-0.0109 (0.0326)
Any son	-0.0048 (0.0430)	0.0425 (0.0315)	0.0419 (0.0316)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.885	0.885	0.885
Observations	9935	9935	9935

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election victory is equal to 1 if the incumbent wins the next election.

Table A10: Next Election Margin: WWI

	Margin		
	(1)	(2)	(3)
Draft age son	-0.1069*	-0.0731*	-0.0728*
	(0.0636)	(0.0426)	(0.0430)
Draft age child	0.0462	0.0572	0.0574
	(0.0614)	(0.0372)	(0.0375)
Any son	0.0693	-0.0134	-0.0136
	(0.0599)	(0.0358)	(0.0360)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.307	0.307	0.307
Observations	1335	1335	1335

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election victory is equal to 1 if the incumbent wins the next election.

Table A11: Next Election Margin: WWII

	Margin		
	(1)	(2)	(3)
Draft age son	0.0546 (0.0480)	-0.0037 (0.0297)	-0.0027 (0.0299)
Draft age child	-0.0760 (0.0484)	-0.0499* (0.0288)	-0.0484 (0.0295)
Any son	-0.0255 (0.0491)	-0.0048 (0.0276)	-0.0045 (0.0283)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.302	0.302	0.302
Observations	4138	4138	4138

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election victory is equal to 1 if the incumbent wins the next election.

Table A12: Next Election Margin: Cold War

	Margin		
	(1)	(2)	(3)
Draft age son	-0.0180 (0.0389)	0.0060 (0.0295)	0.0076 (0.0300)
Draft age child	-0.0429 (0.0382)	-0.0376 (0.0284)	-0.0412 (0.0285)
Any son	0.0122 (0.0445)	0.0659* (0.0339)	0.0651* (0.0341)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.309	0.309	0.309
Observations	9935	9935	9935

Note: Standard errors are doubled clustered by legislator and vote. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election victory is equal to 1 if the incumbent wins the next election.

Table A13: Impact of Having Draft-Age Son on Pro-Draft Vote: WWI

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.1111** (0.0497)	-0.1101** (0.0476)	-0.1410* (0.0800)	-0.1178 (0.0825)
Draft age child	0.0158 (0.0456)	0.0129 (0.0403)	0.1234* (0.0739)	0.1708** (0.0721)
Any son	0.0325 (0.0528)	0.0289 (0.0486)	0.0000 (.)	0.0000 (.)
Legislator FE	No	No	Yes	Yes
Vote FE	No	No	No	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.644	0.644	0.648	0.648
Observations	1790	1790	1776	1776

Note: Standard errors are double clustered by legislator and vote.

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

Table A14: Impact of Having Draft-Age Son on Pro-Draft Vote: WWII

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	0.0348 (0.0546)	0.0416 (0.0467)	0.0302 (0.0589)	0.0094 (0.0598)
Draft age child	0.0395 (0.0494)	0.0307 (0.0423)	-0.0171 (0.0591)	-0.0101 (0.0606)
Any son	-0.0064 (0.0465)	-0.0509 (0.0393)	0.3857 (0.2456)	0.3803 (0.2447)
Legislator FE	No	No	Yes	Yes
Vote FE	No	No	No	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.510	0.510	0.512	0.512
Observations	5187	5187	5029	5029

Note: Standard errors are double clustered by legislator and vote.

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

Table A15: Impact of Having Draft-Age Son on Pro-Draft Vote: Cold War

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0686* (0.0360)	-0.0369 (0.0302)	-0.1095** (0.0465)	-0.0939** (0.0435)
Draft age child	-0.0252 (0.0348)	-0.0404 (0.0287)	-0.0002 (0.0436)	0.0068 (0.0429)
Any son	-0.0018 (0.0377)	-0.0276 (0.0307)	-0.0510 (0.1033)	-0.1070 (0.0983)
Legislator FE	No	No	Yes	Yes
Vote FE	No	No	No	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.640	0.640	0.638	0.638
Observations	12182	12182	12100	12100

Note: Standard errors are double clustered by legislator and vote.

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