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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 examine the longstanding hypothesis that political elites engage in conflict because they fail to internalize the associated costs. We exploit a natural experiment by comparing the congressional voting behavior of U.S. legislators with draft-age sons versus those with draft-age daughters during the four conscription-era wars of the 20th century, when only men could be drafted. Using panel data, we estimate that having a draft-age son reduces a legislator's support for pro-conscription bills by 10-17% relative to having a draft-age daughter. Then, using a regression discontinuity design, we estimate that a legislator's support for conscription increases by a fifth when their son crosses the upper age threshold. We also find that legislators with draft-age sons are more likely to win reelection when the draft is less popular. This is consistent with a political agency model in which voters update their beliefs about politicians' motives when they make unpopular legislative decisions. Our findings establish that agency problems contribute to political conflict. More generally, we provide new evidence that politicians are influenced by private incentives that are orthogonal to political concerns or ideological preferences.

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1. Introduction

Violent conflict undermines state capacity, economic growth, public health and human capital formation.[1] 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 (Coase, 1960, 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 support war. If leaders fully internalize the social cost of conflict in their polity, then both groups should vote identically in expectation after the change. If not, then those with higher private costs will be less likely to vote in favor of conflict.

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 bolstered by the fact that the proposed draft age boundaries frequently 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—including their ideological preferences and those of their constituents—are flexibly controlled for.

We estimate 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 half of the "party line" effect of having a sitting president from the opposing party. Our findings imply that if every member of Congress had a draft-eligible son, the Senate would have voted to end the draft in 1970 and withdraw entirely from southeast Asia in 1971, two years ahead of the Paris Peace Accords. Out of the 88 roll-call votes that went in favor of compelling soldiers to battle during these wars, we estimate that 30 would have been reversed.

We also find evidence that legislators with draft-age sons were more likely to win reelection during the Cold War era, when conscription was less popular, relative to the World War I era, when conscription was more popular. This finding is consistent with a model of political agency in which voters reward politicians who vote against unpopular policies and punish politicians who do not.

Our results imply that legislators can be influenced by private incentives that are independent of political or ideological concerns. One challenge to this interpretation is the possibility that

[1]See Dell and Querubin (2017), Besley and Persson (2010), Abadie and Gardeazabal (2003), León (2012), Prem, Vargas and Namen (2021), Collier, Elliott, Hegre, Hoeffler, Reynal-Querol and Sambanis (2003) and Ghobarah, Huth and Russett (2003).

legislators with draft-eligible sons invest more effort in learning about the social costs and benefits of conscription, and that the change in behavior that we observe is due to concerns for the electorate rather than pure self-interest. According to this interpretation, it is new information rather than changing private incentives that is driving the main result.

To distinguish between these mechanisms, 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.[2] Applying this logic, we use a regression discontinuity design with legislator fixed effects to determine whether or not private incentives can partly explain our main finding. If we detect no change in voting behavior at this cutoff, then it is likely that the information mechanism explains the main result, as it ought to persist long after the politician's own son ages out of eligibility. However, if we do observe a significant effect at this cutoff, then it is likely that private incentives play some role, as this mechanism implies a sharp change in voting behavior as soon as the politician's son becomes ineligible.

We find evidence in support of this latter interpretation. We find that a politician's support for conscription increases by 10.7 percentage points (or 19% of the mean) on average when their son crosses the upper age cutoff. We argue that this is unlikely to be caused by a sudden change in preferences or electoral motives. Instead, we interpret it as evidence that policy choices can be influenced by private incentives that are orthogonal to both career concerns and individual ideology. When we omit legislator fixed effects, the estimate is larger still, implying that the information mechanism is also important.

To rationalize these findings, we turn to a workhorse 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 they vote 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 provides testable implications of the theory that we can bring to the data. The first is that legislators with draft-age sons will be more likely to vote against conscription, as we show in our main analysis. The second is that, as a result, legislators with draft-age sons will be less likely to win reelection when conscription is relatively popular and more likely to win reelection when conscription is relatively unpopular.

To test this, we first confirm evidence from historical accounts that conscription was more popular in the earlier period of the 20th century and became much less popular during the Cold War conflicts. This is likely due to the declining labor-intensity of war over time (Fordham, 2016) as well as a successful effort by the U.S. government to use propaganda and censorship during

[2]This 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.

World War I (Hamilton, 2020, Axelrod, 2009). We check this fact by estimating the effect of election proximity on legislative voting in the Senate. If the draft is unpopular, then senators who are up for reelection will be less likely to vote in favor of it. We focus on our subsample of senators because, unlike the House of Representatives, elections for the Senate are staggered across three groups over six years. This allows us to control for time fixed effects and harness plausibly exogenous variation in election proximity between politicians over time. Consistent with the case literature, we indeed find that senators who were up for reelection were more likely than other senators to vote in favor of conscription during World War I and less likely to vote in favor of conscription during the Cold War. Next, we examine the effect of having a draft-age son on the probability of being reelected. In line with the theory, we find that politicians with draft-age sons were significantly less likely to win reelection during World War I. This effect dissipates entirely by the Cold War, where the point estimate is positive but not statistically distinguishable from zero.

Finally, we turn our attention to another question that is raised by this model: why do some legislators still vote in favor of conscription when it is unpopular? We focus on one potential explanation that is informed by the political economy literature, namely pressure to toe the national party line. Throughout the period that we study, conscription is supported by the White House administration of the time, and legislators from the same party as the President are expected to follow suit. We examine this relationship by focusing on the switch from President Lyndon B. Johnson's administration to that of President Richard Nixon following the latter's election in 1968. This allows us to estimate the effect of party alignment with the sitting president on legislative voting while controlling for individual fixed effects. We find that politicians increase their support for conscription by 11.3 percentage points (or 19% of the mean) when their political party is the same as that of the president in power. We conclude that pressure to toe the national party line is likely to be one motive that impels politicians to vote in favor of conscription in spite of its lack of broad support among voters. Taken together, these empirical exercises align well with the workhorse model of political agency that combines elements of both moral hazard and adverse selection.

To arrive at these results, we undertake two main data collection exercises. In the first, we gather information on the age and sex of 9,210 children of 3,693 U.S. senators and representatives from a combination of census records and 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 measures based in part on contemporaneous newspaper reports. 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 this vote-coding procedure, we eschew the task of assigning pro- or anti-conscription codes to roll call votes ourselves and develop instead an alternative method that relies 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.

The paper links two bodies of research. The first relates to the political economy of legislative decision making. The prevailing view is that a legislator's decision is motivated by a combination of political concerns and purely private concerns (Ansolabehere, de Figueiredo and Snyder, 2003, Levitt, 1994). Political concerns derive from the preferences of the legislator's constituents, who determine reelection, and the legislator's party, who can otherwise influence career outcomes. Private concerns derive from the legislator's own ideological preferences. However, this model of policy formation leaves no room for the possibility that legislators are influenced by additional private incentives that are independent of ideological preferences, such as quid-pro-quo transfers from special interests. While there exists an argument that politicians are largely immune from such influences (Ansolabehere et al., 2003, Tullock, 1972), it is difficult to reconcile with the growing share of campaign contributions emanating from the top of the wealth distribution in the United States (Bertrand, Bombardini, Fisman and Trebbi, 2020, Bonica, McCarty, Poole and Rosenthal, 2013).

One potential reason for the absence of evidence on this question is the substantial empirical challenge that is poses. Consider the example of a politician who votes in favor of war after receiving a campaign contribution from a weapons manufacturer. It is possible that the contribution caused the politician to vote for war. However, it is also possible that the manufacturer contributed to the campaign precisely because it knew that the politician would vote for war. In this case it is the politician's ideological preference that jointly determines the contribution and the vote. Thus, in order to determine whether or not politicians are truly malleable, the econometrician must observe an exogenous change in private incentives *holding ideological preferences constant*. By exploiting within-legislator variation in exposure to the private costs of conscription, we overcome this selection bias problem in our empirical approach. In so doing, we provide quantitative evidence that democracy alone does not resolve the fundamental political agency problem of misaligned interests between citizens and their political representatives.

Our study complements the important work of Washington (2008), who finds that legislators with daughters are more likely than other legislators to vote liberally due to 'female socialization,' which is a change in preferences that one experiences after having a daughter. Washington's result provides novel causal evidence that a legislator's individual preferences can influence congressional decision making.[3] Just as that study exploits exogenous variation *between* legislators to show that ideological preferences affect legislative voting, our study exploits variation *within* legislators to show that private incentives also affect legislative voting. In this regard, we too identify an important explanatory variable that has been previously omitted in the literature. Moreover, our finding has implications for the broader literature on special interests and quid-pro-quo politics, as we show that legislators respond sharply to changing private incentives,

[3]Other papers that examine the connection between a policy-maker's background and their policy choices include Teso and Carreri (2021) and Gelpi and Feaver (2002). More directly, Dube and Harish (2020) find that European polities ruled by queens were more likely to experience conflict than those ruled by kings, and Benzell and Cooke (forthcoming) show that kinship ties between monarchs contributed to the decline in European war frequency.

which is an important assumption underlying many of these studies (Grossman and Helpman, 2001, Bertrand et al., 2020).

The second body of research 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.[4] 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.[5]

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). This can help to explain why transient economic shocks may lead to violence.[6] Empirical papers that exploit plausibly exogenous variation to identify the link from economic conditions to conflict include Miguel, Satyanath and Sergenti (2004), Dube and Vargas (2013), Bazzi and Blattman (2014), Berman, Couttenier, Rohner and Thoenig (2017), Harari and La 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. The idea is formalized in Jackson and Morelli (2007) and can trace its roots at least as far back as Kant (1795). This moral hazard theory of conflict relaxes the assumption that groups are unitary actors.[7] To the best of our knowledge, we are the first to corroborate it empirically using plausibly exogenous variation.[8]

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 information 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. We provide a counterfactual analysis in Section 8 and we conclude in Section 9.

[4]See Haavelmo (1954), Hirshleifer (1988), Garfinkel (1990) and Skaperdas (1992).

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

[6]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.

[7]'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.

[8]Other 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).

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 ideological preferences (de Figueiredo and Richter, 2014, Ansolabehere et al., 2003, Levitt, 1996). This implies that a politician takes into account four sets of preferences in determining their optimal legislative vote (Higgs, 1989, Levitt, 1996). Reelection concerns are represented both by the preferences of voters in their electorate, and by the preferences of their 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:

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

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.[9] 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.[10]

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

[9]For 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.

[10]Poole 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.

elections, or because supporters are inclined to volunteer or contribute in other ways to a candidate's campaign.[11]

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.[12]

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 in support 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.[13]

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. 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 conclude that campaign contributions are largely made for their consumption value, rather than returns on investment.[14]

In this paper, we consider an alternative 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 (de Figueiredo and Richter, 2014). A causal identification strategy would require that we observe exogenous variation in the politician's private returns to voting on a legislative issue while conditioning on fixed effects to hold preferences constant. While there exists persua-

[11] Levitt (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, Ross and Washington (2013) and Mian, Sufi and Trebbi (2010) also find evidence that is consistent with this effect.

[12] Evidence from, inter-alia, Bonica (2013), Snyder and Groseclose (2000), and McCarty, Poole and Rosenthal (2001) supports this view in the context of U.S. congressional voting.

[13] One 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.

[14] While 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 choices 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.

sive evidence that, for example, campaign contributions can buy time with a legislator (Kalla and Broockman, 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 existing evidence that individual legislators respond to changes in the private rents that they would obtain by voting on a given issue in Congress. Yet, such a view would be consistent with more recent evidence on the patterns of political contributions in the United States (Bertrand et al., 2020, Bonica et al., 2013, Gordon, Hafer and Landa, 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:

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

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

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

We define political motives as those derived from the preferences of voters and political parties, and private motives as those derived from the legislator's own ideological preferences and other time-varying costs and benefits (i.e., *rents*).

Implications for Conflict Much of the theoretical literature on violent conflict treats actors as unitary decision-makers.[15] 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 $V_{it}^{*}(. \mid \theta > 0) = (1 - V_{it}^{*}(. \mid \theta = 0))$.[16] 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

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

[16] The 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.

of the population—can cause war even in the presence of enforceable transfers between potential belligerents.[17]

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 nonzero 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 stems not only from the fact that draft-eligible sons are susceptible to the dangers of combat deployment, but also from the costs that derive from avoiding the draft by, for example, joining the National Guard or expending political capital to otherwise escape deployment. 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

A. Data

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.[18] In our core analysis of conscription

[17]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 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, Meirowitz and Ramsay, 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.

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

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.[19] 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.[20] This gives a total of 248 votes; 195 determined by Voteview and a further 53 determined by the authors.

An example of a vote that was assigned an issue code by Voteview is vote number 52 in the 65th Senate in 1917, which authorizes the president to "to raise a regular army and to draft into military service as many men as are needed to meet existing emergencies." Another is vote number 304 in the same session, which amends the draft legislation by eliminating exemptions for special occupations. An example from World War II is vote number 63 in the 77th House in 1941, which extends the term of service by 18 months to 30 months and removes a limit on the number of draftees. An example from the Korean War is vote number 37 in the 82nd House, "to provide for the common defense and security of the U.S. and to permit the more effective utilization of man-power resources of the the U.S. by authorizing universal military training and service," which extended conscription by 4 years and extended the term of service by 3 months. Finally, an example from the Vietnam War is vote number 78 in the 92nd Senate, which aimed to reduce the maximum number of persons to be inducted into the armed forces to 100,000 in 1972 and 60,000 in 1973.

An example of a vote that was not assigned an issue code by Voteview but was assigned a code by the authors is vote number 9 in the 65th Senate in 1917, "to resume consideration of S. 1871, a bill authorizing the president to increase, temporarily, the military establishment of the U.S.'. It was not assigned the "Selective Service Act" issue code most likely because the act itself had not yet passed.

Next, in order to examine legislators' motives for voting, it is necessary for us to assign a 'direction' to each roll call vote. In the first example above (vote 52 in the 65th Senate), it is clear that an *aye* vote implies support for the draft. For vote 78 in the 92nd Senate, it is clear that *nae* implies support for the draft.[21] However, in many cases the assignment is not obvious. Thus, there is a danger of misclassifying a pro-draft measure as an anti-draft one and vice versa.

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

[19] See `https://voteview.com/`.

[20] See `www.govtrack.org`, a project of Civil Impulse, LLC.

[21] Where necessary, we use the term *aye* in place of *yea* and the term *nae* in place of *no*.

"the president to conscript 500,000 men if the number is not secured by voluntary enlistment within 90 days" (vote 21in the 65th Senate) might initially appear to be a pro-draft amendment. However, articles in both papers make it clear that this was favored by 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 sessions 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. In column (1), we list the number of draft-related votes included in our main sample. In column (2), we present our main dependent variable: *Pro Draft* is equal to 1 if a legislator voted in favor of conscription (e.g., *aye* if it was a pro-draft vote, or *nae* if it was an anti-draft vote), and 0 otherwise. This exhibits a large amount of variation: the sample average is 0.58. In column (3) 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. The final two columns present all draft-related votes—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 with our main sample. The average margins are 0.18 in our main sample and 0.17 in the total sample, suggesting that there is no major 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 and Koed, 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.[22] These records contain information on the name, gender and age of each household member. We cross-check household data by hand 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 (mainly the *New York Times* and *Washington Post*); biographies on official federal and local government websites; local media profiles; university archives; and other online repositories.[23]

[22]See `www.ancestry.com`.

[23]These include *Notable Names Database* (www.nndb.com), *Legacy.com*, *Biography.com*, *Newspapers.com*, and *Find a Grave* (www.findagrave.com).

Table 1: Summary of Roll Call Votes

	Main Sample Votes			All Draft Votes	
Congress	Votes (1)	Pro Draft (2)	Margin (3)	Votes (4)	Margin (5)
			Senate		
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)
			House		
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
Std. Dev.	(2.76)	(0.17)	(0.26)	(4.25)	(0.24)
			Combined		
Total	140			232	
Mean		0.58	0.18		0.17
Std. Dev.		(0.49)	(0.12)		(0.12)

Note: The 65th and 66th Congresses are during the World War I era; the 76th-79th are during the World War II era; the 82nd is during the Korean War era; and the 89th-93rd are during the Vietnam War era. The *Main Sample Votes* are those for which a value of *Pro Draft* exists. *All Draft Votes* include all votes that relate to conscription. The data are from the Voteview project.

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-year-old son is coded as a 0. However, if the proposed window runs from 20-30 for a different roll call vote, the same legislator is then 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. The equivalent figures for daughters are the same. This is particularly reassuring, as it is consistent with our main identifying assumption that the gender of a given draft-age child is as good as random.

Data on the age cutoffs are presented in Figure 1 and Appendix Table A2. 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. The Vietnam War contains more roll call votes than any other war.

Taken together, the raw data show that (i) draft-related measures in Congress were relatively contentious, with a mean pro-draft vote of 0.58; (ii) around one-fifth of legislators had sons of draft-eligible age during the relevant votes; (iii) the equivalent statistic for daughters is the same.

Assigning treatment For most of the 140 votes, we assign treatment status according to the proposed draft age window that is associated with the bill or amendment under debate. Continuing an earlier example, for vote number 63 in the 77th House on removing a limit on the number of draftees and extending the term of service, the draft age window is 21-28. This clearly defines the treatment status of legislators.

However, for the 37 roll call votes that propose to alter the draft window itself, treatment status is less well defined. 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 straightforward to understand how an infra-marginal legislator with a 22-year-old son is affected by this. On the one hand, the son faces a longer duration of eligibility. On the other, the probability that he is drafted is reduced because of the larger pool of eligible draftees.[24]

To address this problem, we drop the infra-marginal legislators, leaving only the marginal group as treated and the extra-marginal legislators as the control along the age dimension.

[24]This was an issue debated in Congress at the time ("House Votes Conscription," *The New York Times*, September 8th, 1940):

> 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.

Table 2: Family Composition Summary Statistics

		Children		Sons		Daughters		Draft Age	
Congress	Legislators (1)	Any (2)	No. (3)	Any (4)	No. (5)	Any (6)	No. (7)	Any Son (8)	Any Dtr. (9)
					Senate				
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)
					House				
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)
					Combined				
Total	2287								
Mean		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 possible. See main text for more details.

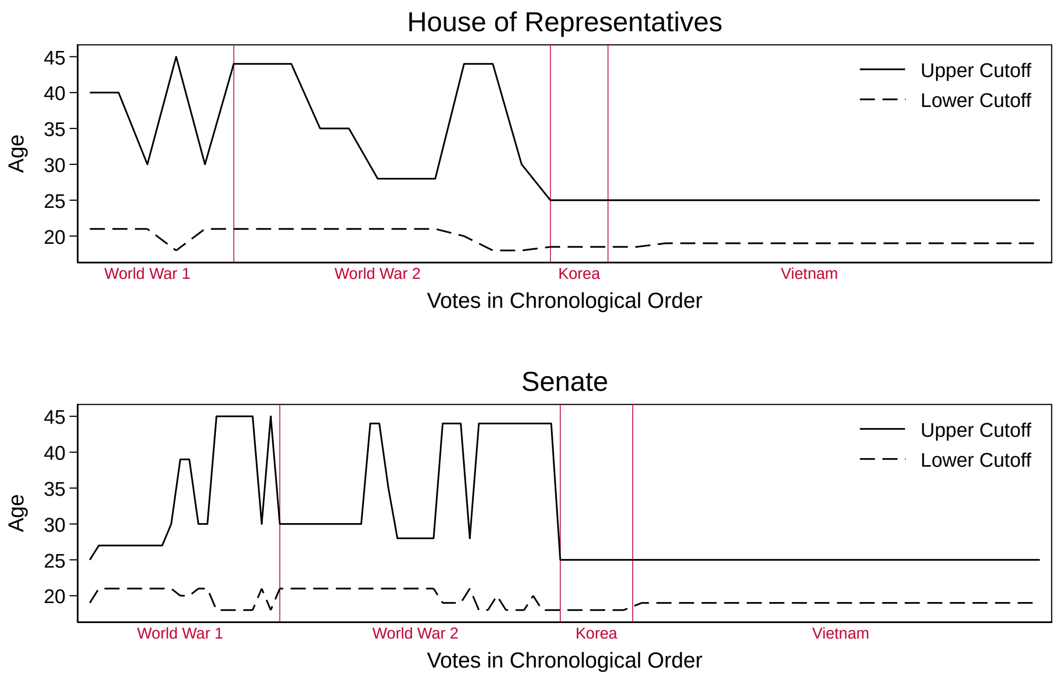


Figure 1: **Age Cutoffs Over Time in the House and Senate**

Note: This figure depicts variation over time in the proposed upper and lower age cutoffs for draft eligibility at the level of a roll call vote. Votes are arranged in chronological order. The red lines demarcate eras. There are 106 votes in the Senate and 34 in the House. See Appendix Table A2 for summary statistics.

Because this necessitates a different coding procedure to assign treatment status across legislators, we separate these 37 'window' votes (7,109 legislator-votes) from our main analysis, leaving 103 votes (19,262 legislator-votes) in our baseline sample. We present our analysis of these window votes in the appendix.

Hawks and Doves As described above, the process by which we code votes as either pro- or anti-draft reduces our sample to 140 votes out of the 232 draft-related votes that take place in the congressional sessions that we study. The remaining 92 are too ambiguous for us to code with confidence.[25] Two drawbacks of this approach are (i) the loss of coverage due 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 foreign policy "hawks" and two well-known foreign policy "doves" during each Congress in both the House and the Senate. We use this information to create a new variable, *Hawkish 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 modal 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 (different) unique mode among doves. The correlation coefficient between *Hawkish Vote* and our main *Pro Draft* outcome variable is 0.92.

Supplemental Materials We include in the Data Appendix (Appendix A) a list of the roll call votes studied in our analysis and also a list of the hawks and doves used to construct our *Hawkish Vote* alternative outcome variable.

In Appendix B, we provide more background on the legislative decisions that we study. We discuss the costs and benefits of conscription that were postulated during debates on the floor (or in committee) at the time, and we consider the additional private costs incurred by treated legislator. We estimate that the probability of a soldier dying conditional on serving during our study period is 1.2%, which implies that a draft *registrant* had a 0.2% probability of being killed in battle. This is around 17 times greater than the probability of dying in a traffic accident in the U.S. in 2019. When one includes other long run mental, physical and labor-market costs of combat such as those identified in Angrist (1990) and others, it is evident that around one-fifth of

[25]For example, 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.

legislators had a non-trivial role in determining the risks of battle faced by their own sons.[26]

4. Estimation

Our main specification is as follows:

$$V_{iv} = \alpha_i + v_v + k_{iv} + s_{iv} + \beta_1 draft\ son_{iv} + \beta_2 draft\ child_{iv} + \mathbb{X}'_{iv}\zeta + \epsilon_{iv}, \tag{4}$$

where V_{iv} is an indictor equal to one if the legislator i votes in favor of conscription in vote v, which is a unique roll call vote that takes place either in the House or in the Senate; α_i denotes legislator fixed effects; v_v denotes vote fixed effects; k_{iv} denotes fixed effects for number of children at the time of vote v; s_{iv} denotes fixed effects for number of sons at the time of vote v; $draft\ son_{iv}$ is an indicator variable equal to one if a legislator has a son of draft-eligible age as determined by the cutoffs in vote v; $draft\ child_{iv}$ indicates that a legislator has a child of draft-eligible age in vote v; $\mathbb{X}_{iv}$ is a vector of time-varying controls, comprising the legislator's age, age squared, terms in office, as well as party and chamber (i.e., house or senate) fixed effects, which are absorbed in regressions with individual and vote fixed effects respectively. Standard errors are two-way clustered by legislator and vote. We estimate the specification using a linear probability model.

The parameter β_1 represents the additional impact of having at least one draft-age son relative to having at least one draft-age daughter. It is estimated off the variation generated by having a son age into/out of the draft window relative to having a daughter age into/out of the draft window. Our 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—conditional on the other covariates. 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 and total number of sons are reassuring in that regard. In particular, the inclusion of legislator fixed effects represents an important departure from the literature that allows us to control for the fixed ideological preferences of legislators and their constituents. Conditional on these covariates, we assume that variation in $draft\ son_{iv}$ is as good as random. Legislators who exhibit within-variation account for 31.5% of observations in the main sample.

It is important to note that the comparison group in this setup—legislators with daughters of draft-age—may themselves be affected by the passing of conscription. Most obviously, these legislators may take into account the fate of their sons-in-law who fall within the draft-age

[26] While we do not observe the children in our dataset as adults, there are several accounts of sons of legislators serving as draftees or as volunteers, often incurring serious injury or death. For example, during World War I, John M. Nelson's (R-WI) son was arrested for attempting to avoid induction (Walker, 2008, pp. 206), while Edward Pou's (D-NC) son was killed while serving in France. During World War II, two of J. Parnell Thomas' (R-NJ) sons served, one was drafted and one volunteered ("Jersey congressman's son joins paratroops," *The Atlanta Constitution*, Nov 13 1942), while John R. Murdock's (D-AR) son was killed in action in 1943 ("Congressman Murdock Advised of Son's Death," *The Atlanta Constitution*, Sep 09 1943). In the Korean War, John V. Beamer's (R-IN) son was drafted ("Withdraw Draft Deferment for Son of Congressman," *Chicago Daily Tribune*, 18 July 1953). One source estimates that 26 sons of legislators served during the Vietnam War (Bryan, 1976).

boundaries. A cursory comparison of means suggests that support for the draft is 6.42 percentage points lower in this group relative to those with daughters outside of the draft window.[27] This suggests that our estimate of β_1 will be conservative relative to the treatment effect that we estimate with a regression discontinuity design in Section 6.[28]

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 $\hat{\beta}_1$ toward zero, as treated legislators will contaminate the comparison group along the age dimension. While the decision is somewhat arbitrary, what should be clear is that $\hat{\beta}_1$ initially rises as we reduce the lower cutoff and add more treated legislators to the treatment group, before smoothly decreasing again as we add more untreated legislators with younger children.

5. Results

Main results We present the main results in Table 3 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 7.4 percentage points ($p < 0.05$), from a mean of 0.605. Adding vote fixed effects reduces the magnitude of the coefficient to -0.0532 ($p < 0.10$). In Column (3) we add legislator fixed effects to the baseline model, which gives a treatment effect of -0.1013 ($p < 0.01$), almost 17% of the mean. Finally, we include both vote and legislator 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 6.2 percentage points, or just over 10% of the mean ($p < 0.05$).

These regression results align closely with the crude difference-in-means estimate: on average, legislators with at least one draft-age son vote in favor of conscription in 56% of votes and those with at least one draft-age daughter (and no draft-age sons) vote in favor of conscription in 63% of votes, implying a 7 percentage-point treatment effect.[29]

[27] Among legislators with at least one daughter and no sons, those with daughters within the age boundaries vote in favor of conscription in 60.74% of votes, while those with daughters outside of the age boundaries vote in favor of conscription in 67.16% of votes.

[28] We do not have data on sons-in-law due to the familiar problem of matching census records over time for women who adopt their husbands' names. We also forgo analyzing data on grandchildren, which poses a similar obstacle. However this is not likely to affect our estimate, as grandchildren ought to be distributed equally in expectation (by sex and by age) across the treatment and comparison groups. Finally, Washington (2008) shows that legislators with daughters have higher preferences for defense spending. If these preferences are greater when a legislator's daughter is within the draft age window, then our estimate of β_1 may be biased away from zero. The comparison of means above suggests that this is not a realistic concern. In any case, our regression discontinuity design circumvents this issue.

[29] Again, the lower age boundary allows for a 4 year lead. This was also applied for the difference-in-means reported in the previous section between legislators with daughters inside versus outside of the draft-age window.

Table 3: Main Results

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.0740**	-0.0532*	-0.1013***	-0.0622**
	(0.0310)	(0.0312)	(0.0349)	(0.0282)
Draft Age Child	-0.0149	-0.0024	0.0127	0.0135
	(0.0279)	(0.0282)	(0.0306)	(0.0277)
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Legislators	2284	2284	2120	2120
Votes	103	103	103	103
Mean Dep. Var.	0.605	0.605	0.605	0.605
Observations	19262	19262	19098	19098

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in favor of conscription on a given vote. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Additional analysis In Appendix Figure A1, we plot the sensitivity of each empirical model in Table 3 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 is consistent with theory: with few leads there are treated legislators in the control group, which biases $\hat{\beta}_1$ toward zero. The treatment effect is maximized (in absolute terms) with a four-year lead, as the inclusion of legislators with a five year lead reduces the magnitude of point estimate. The mean duration of the draft per conflict was 3.3 years up until the Vietnam War, suggesting that politicians' revealed expectations are reasonably accurate.

In the Appendix Tables A3 to A6 we present balance tests in which the outcome variables are (i) an indicator for Democrat (versus other): (ii) an indicator for Senator (versus House Representative); (iii) terms in office; and (iv) age. We present the same four specifications for each outcome, omitting the variable itself from the battery of controls. Out of 16 estimates, none are significant at the 10% level, suggesting again that the the treated and control observations are statistically very similar. In Table A7, we repeat the main exercise with added controls for the age and age-squared of every child of each legislator. These age controls ensure that the treatment effect is not picking up a quadratic effect of a child's age on legislators' voting preferences. The results are robust to their inclusion.[30]

In Table A8, we allow our main estimate to vary by the the final margin in a given roll call vote. It is plausible that the treatment effect is greater for votes in which a given legislator is more likely to be pivotal, i.e., close votes relative to landslide votes. To investigate this, we create four

[30] If a legislator does not have a child (or a second child, or a third child, and so on), we enter a zero for the corresponding age of that child number. These zeros are then flexibly captured by k_{iv} in the regression.

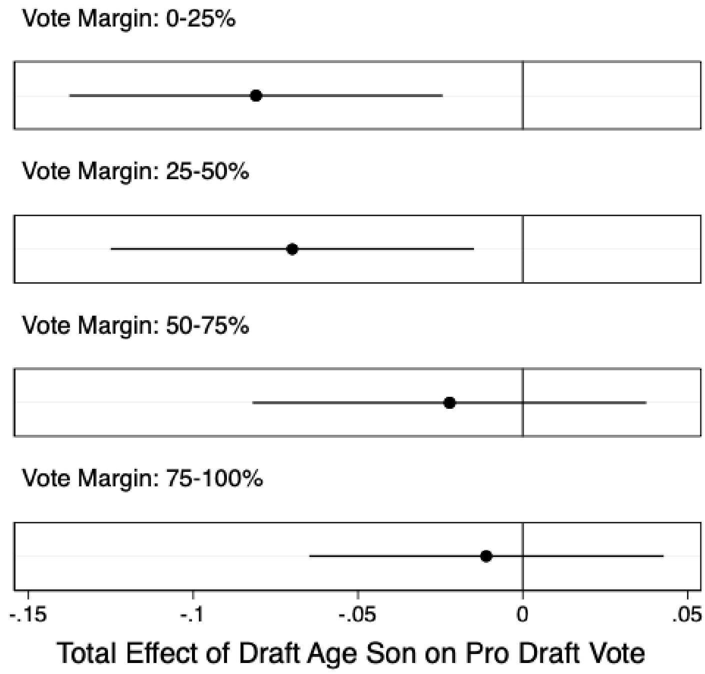


Figure 2: **Heterogeneity by Roll Call Vote Margin.** This figure shows the total effect of having a draft age son on pro-draft votes at different roll call vote margins. See Table A8 for the underlying estimates.

vote-level dummy variables to indicate whether the margin is between 0-25%, 25-50%, 50-75% or 75-100% of the total votes cast. We then interact these with our $draft\ son_{iv}$ and $draft\ child_{iv}$ variables. We find some evidence that the differential effect of having a draft age son approaches zero for votes that were less contentious, although the magnitudes of the interaction effects are not very large (e.g., +0.0274 for votes with a 75-100% margin relative to the 0-25% category) and the estimates are not significant. We see a stronger pattern when we examine heterogeneity by $draft\ child_{iv}$, perhaps indicating that legislators are also concerned about sons-in-law. In Figure 2, we present the total effect of having a draft age son (i.e., the sum of the coefficients for $draft\ son_{iv}$ and $draft\ child_{iv}$) for each vote margin category. The total effect is negative and significant for the closest votes and it tends toward zero as votes become less contentious.

In Table A9, we analyze window votes. In Panel A, we estimate the same four specifications using only the sample of 37 window votes. We begin with a two-year lead in how we define the draft window. This implies that our treatment group (along the age dimension) contains only the 'marginal' observations while the control group contains only the extra-marginal legislators who have children that are either older than the upper cutoff and/or at least two years younger than the lower cutoff.[31] The point estimates are again negative. The treatment effect is significant with vote fixed effects but not with legislator fixed effects, which is consistent with the sharply reduced sample size. In Panel B, we combine the window votes (with a two-year lead) with our

[31]This is the optimal lead structure, perhaps reflecting the fact that window votes tended to occur closer to the ends of wars, when they were seen as an attempt to accelerate the completion of combat operations.

main sample votes (with a four-year lead) for a total of 140 roll call votes. We estimate treatment effects that are consistent with our main results. In Panel C, we allow for a four-year lead for both the window votes and the main sample of votes, again finding similar results.

Hawks and Doves In Panel A of Table 4, we estimate the same four specifications as in Table 3, only now using *Hawkish Vote* as the dependent variable rather than *Pro Draft Vote*. The sample is larger as it is drawn from 193 draft-related votes in our dataset rather than the 140 that we were able to code.[32] 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 draft-age sons are around 5.5 percentage points (just under 9% of the mean) less like to vote for conscription than those with daughters of comparable age.

In Panel B, we restrict the hawks and doves sample to draft-related votes that are *not* included in our main sample in Table 3. Using this non-overlapping sample, we again find very similar results (allowing for slightly larger standard errors due to the restricted sample size).

In Panel C, 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. The point estimates are all close to zero and none of them are statistically significant.

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

6. Establishing Causal Mechanisms

Our main results are consistent with two casual mechanisms, broadly defined. The first is that leaders have private incentives that are independent of political or ideological motives (i.e., that $\theta > 0$, from Section 2). The second is that leaders with draft-age sons seek out information that makes the social costs and benefits of conscription more salient. In this case, we might be observing the effects of a subsequent change in the legislator's perception of voter preferences—or even a change in the legislator's own preferences—rather than a change in his or her private rents from voting. With this interpretation it is possible that our estimated $\hat{\beta}_1$ is consistent with the classic model of legislative voting presented in equation (1). Pure self-interest may have spurred the legislator to learn more about the consequences of the vote, but thereafter the legislator may be motivated by reelection or ideological preferences.

One way to test this incentives-versus-information interpretation is to examine the behavior of legislators who have sons around the upper age cutoff. In both cases, a legislator will oppose the draft as long as their son is below this threshold. Under the incentives interpretation, a legislator will change their voting behavior immediately as their son ages out of the eligibility window.

[32]Votes for which the *Hawkish Vote* variable is not defined are omitted, which is why there are not 232 votes in this sample.

Table 4: Hawks and Doves

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Panel A: Draft Category Votes				
Draft Age Son	-0.0714**	-0.0598*	-0.0741**	-0.0549**
	(0.0328)	(0.0321)	(0.0292)	(0.0240)
Draft Age Child	0.0066	0.0076	0.0422	0.0340
	(0.0289)	(0.0284)	(0.0271)	(0.0229)
Legislators	2189	2189	1981	1981
Votes	193	192	193	192
Mean Dep. Var.	0.625	0.625	0.627	0.627
Observations	20786	20785	20578	20577
Panel B: Excluding Main Votes				
Draft Age Son	-0.0717	-0.0557	-0.1228**	-0.0828*
	(0.0454)	(0.0402)	(0.0563)	(0.0416)
Draft Age Child	0.0060	0.0162	0.0245	0.0388
	(0.0365)	(0.0330)	(0.0537)	(0.0368)
Legislators	1921	1921	1235	1235
Votes	72	72	72	72
Mean Dep. Var.	0.568	0.568	0.565	0.565
Observations	6632	6632	5946	5946
Panel C: Non-Draft Votes				
Draft Age Son	0.0090	-0.0054	-0.0099	-0.0147
	(0.0182)	(0.0172)	(0.0143)	(0.0120)
Draft Age Child	-0.0189	-0.0266	0.0127	0.0111
	(0.0169)	(0.0162)	(0.0144)	(0.0118)
Legislators	2620	2620	2620	2620
Votes	1952	1917	1952	1917
Mean Dep. Var.	0.514	0.514	0.514	0.514
Observations	175591	175556	175591	175556
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in line with the modal hawk vote and against the modal dove vote. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$.

Under the information interpretation, however, a legislator will maintain their opposition to the draft as their son ages out of the eligibility window. Their concern for other draft-eligible men and their families ought to remain intact—or at least decline more gradually—even as their own son is no longer at risk.

This test lends itself to a regression discontinuity (RD) design. 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. 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.

In addition to shedding light on causal mechanisms, this approach also generates a treatment effect that is not suppressed by the son-in-law effect that is likely present when we use legislators with draft-age daughters as a comparison group. Thus, we consider this RD analysis both as a validation exercise and as a means to shed light on causal mechanisms.

Following Gelman and Imbens (2019), we estimate the following local linear model as our baseline specification, for $RV_{iv} \in (-h, h)$:

$$V_{iv} = \alpha_i + \phi \mathbf{I}\{RV_{iv} > 0\} + \delta_1 RV_{iv} + \delta_2 RV_{iv} \times \mathbf{I}\{RV_{iv} > 0\} + \mathbb{C}'_{iv}\eta + \varepsilon_{iv}, \tag{5}$$

where RV_{iv} is the running variable (son's age minus upper cutoff); $\mathbf{I}\{RV_{iv} > 0\}$ is an indicator equal to 1 if RV_{iv} is positive (i.e., if the son's age is above the upper cutoff); α_i are legislator fixed effects; the vector $\mathbb{C}'$ represents time-varying controls for age, age squared, terms in office and a variable indicating that the legislator has a son who is beneath the lower cutoff; and, finally, h is the bandwidth. The parameter ϕ captures the effect of having a son exit the draft eligibility window on a legislator's probability of voting in favor of conscription. A null estimate indicates support for the information mechanism alone. A significant and positive ϕ indicates that the incentive mechanism is playing an important role in explaining our main finding.

To estimate this model, we rely on the procedure developed by Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo, Farrell and Titiunik (2019). This has a number of advantages. First, it automates the choice of many 'tuning parameters' that are usually left to the discretion of researchers. Thus, we adopt the default selection of the data-driven MSE-optimal bandwidth h, the choice of kernel (triangular), and the procedure used to compute standard errors (nearest neighbor, allowing for clusters at the level of a legislator and adjusting for mass points due to our discrete running variable). Second, it computes both conventional estimates and estimates that are corrected for leading bias by using information from a higher-order estimate. Third, it allows for the inclusion of control variables, which is particularly important for our mechanism tests. Finally, it accommodates discrete running variables.

Validation tests We first present results from our validation exercise in Table 5. In the top panel we present conventional regression discontinuity estimates; in the lower panel we present bias-corrected estimates (Calonico et al., 2014). We do not include legislator fixed effects in any specification. In column (1), we estimate a treatment effect of 23.46 percentage points using the

Table 5: Regression Discontinuity: Validation Tests

	Pro Draft Vote						Senator	Democrat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Conventional Regression Discontinuity Estimates							
Son's Age > Upper Cutoff	0.2346**	0.3031**					-0.0215	-0.0786
	(0.0992)	(0.1251)					(0.0879)	(0.0901)
Daughter's Age > Upper Cutoff			-0.0568	-0.1292	-0.1658	-0.1552		
			(0.0679)	(0.0914)	(0.1180)	(0.1125)		
	Bias-Corrected Regression Discontinuity Estimates							
Son's Age > Upper Cutoff	0.2803**	0.3243**					-0.0234	-0.1298
	(0.1268)	(0.1455)					(0.1049)	(0.1106)
Daughter's Age > Upper Cutoff			-0.0448	-0.1646	-0.2106	-0.1923		
			(0.0814)	(0.1134)	(0.1392)	(0.1283)		
Local Polynomial Order	1	2	1	1	2	2	1	1
Bandwidth	4.72	8.12	9.02	4.72	7.22	8.12	10.33	7.30
Mean Dep. Var.	0.58	0.58	0.58	0.58	0.58	0.58	0.43	0.58
Observations	8094	8094	8061	8061	8061	8061	9039	8964

Note: The unit of analysis is the legislator-vote. In columns (1) to (6), The outcome variable is an indicator equal to one if the legislator votes in favor of conscription on a given vote. In column (7), the outcome variable is an indicator equal to 1 if the legislator is a senator. In column (8), the outcome variable is an indicator equal to 1 if the legislator is a Democrat. We use the data-driven bandwidth selection procedure developed by Calonico et al. (2014) and Calonico et al. (2019) to compute all RD estimates, except for those presented in columns (4) and (6), where we select the bandwidths manually. The upper panel presents conventional RD estimates; the lower panel presents bias-corrected RD estimates. Standard errors are two-way clustered by legislator and vote.$^{***}p < 0.01$,$^{**}\ p < 0.05$,$^{*}\ p < 0.1$.

conventional estimation procedure and 28.03 using the bias-corrected estimation procedure. Both estimates are significant at the 5% level. These estimates are larger in magnitude than those estimated in our main fixed effects regression, where the comparison group is comprised of legislators with daughters within the draft age boundaries. This suggests that the son-in-law effect is non-trivial.[33]

In column (2), we estimate a local second-order polynomial model, finding a larger effect still. In column (3), we use a placebo running variable, defined as a legislator's daughter's age minus the upper age cutoff. Even in the presence of a son-in-law effect, we would not expect a similar estimate in this case unless the legislator's son-in-law is precisely the same as as his or her daughter. We find no significant effect. In column (4), we replace the automated bandwidth selection method and instead declare the same bandwidth used in column (1) in order to ensure that the null effect is not simply due to this difference. Again, we find no significant effect. In columns (5) and (6), we repeat the exercise using a second-order polynomial, and again we fail to reject the null of no effect. Finally, in columns (7) and (8), we use placebo outcome variables—respectively *Senator* and *Democrat*—and find no significant effect.

In Figure 3, we plot the conventional estimates for columns (1), (2), (3), (7) and (8), as well

[33]These estimates are very similar to the effect of political party alignment with the president, which we discuss in the following section.

Table 6: Regression Discontinuity: Mechanism Tests

	Pro Draft Vote					
	(1)	(2)	(3)	(4)	(5)	(6)
	Conventional Regression Discontinuity Estimates					
Son's Age > Upper Cutoff	0.2346**	0.0290**	0.0270*	0.3031**	0.1042***	0.1139***
	(0.0992)	(0.0143)	(0.0143)	(0.1251)	(0.0225)	(0.0224)
	Bias-Corrected Regression Discontinuity Estimates					
Son's Age > Upper Cutoff	0.2803**	0.1073***	0.1056***	0.3243**	0.1289***	0.1390***
	(0.1268)	(0.0217)	(0.0218)	(0.1455)	(0.0270)	(0.0268)
Legislator FE	No	Yes	Yes	No	Yes	Yes
Local Polynomial Order	1	1	1	2	2	2
Bandwidth	4.72	4.84	4.72	8.12	7.44	8.12
Mean Dep. Var.	0.58	0.58	0.58	0.58	0.58	0.58
Observations	8094	8094	8094	8094	8094	8094

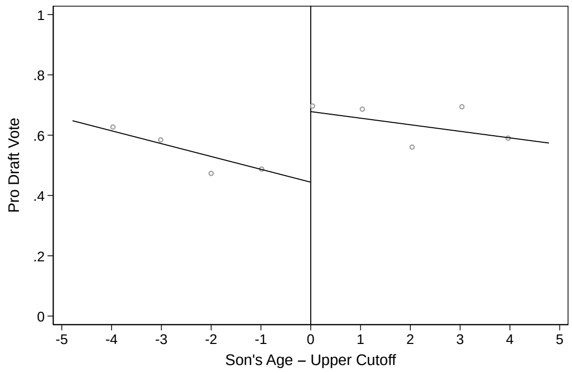
Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if the legislator votes in favor of conscription on a given vote. We use the data-driven bandwidth selection procedure developed by Calonico et al. (2014) and Calonico et al. (2019) to compute all RD estimates, except for those presented in columns (3) and (6), where we select the bandwidths manually. The upper panel presents conventional RD estimates; the lower panel presents bias-corrected RD estimates. Standard errors are two-way clustered by legislator and vote.$^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$.

as the running variable density around the cutoff, which indicates that there is no significant difference in the distribution either side.

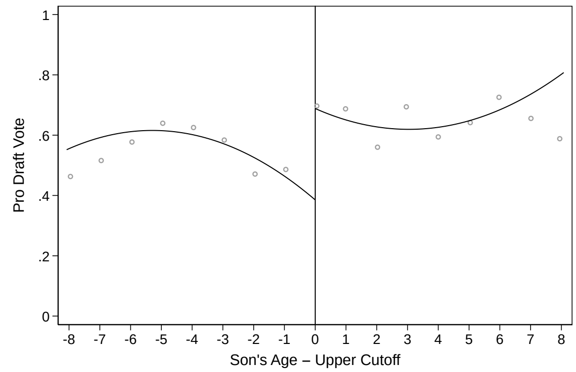
Overall, this exercise provides strong evidence in support of our main finding, suggesting that—if anything—those effects are conservatively estimated.

Mechanism tests In Table 6, we present the results of our mechanism tests. Our object of interest is the difference between the RD estimate without legislator fixed effects, which combines between- and within-legislator variation, and the RD estimate with legislator fixed effects, which exploits only within-legislator variation. If the latter estimate is significant, it indicates that private incentives play a role in explaining the main results. If not, it indicates that the main results are explained by the information mechanism.

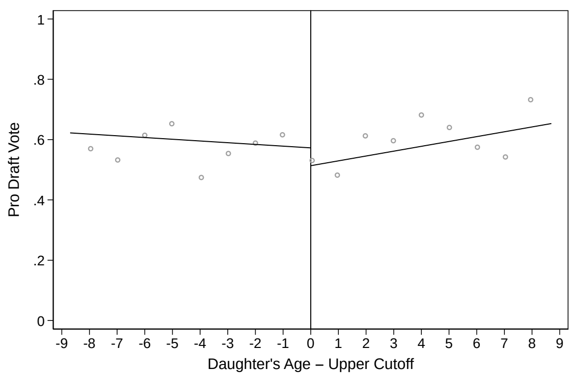
For ease of comparison, we present again in column (1) the baseline RD estimate without fixed effects. In column (2), we present the corresponding estimate with fixed effects. Focusing first on the bias-corrected estimates, we find that legislators increase their support for conscription by 10.7 percentage points when their son ages out of the draft-eligibility window (p<0.001). This implies that private incentives can explain congressional decisions. The fixed effects estimate is smaller in magnitude than the estimate without fixed effects, suggesting that information also plays an important role. The estimate declines somewhat when we use the conventional estimation procedure rather than the bias-corrected procedure, but it is still positive (+0.029) and significant (p<0.05). In column (3), we adjust the bandwidth selection to match column (1). This does not make a notable difference.



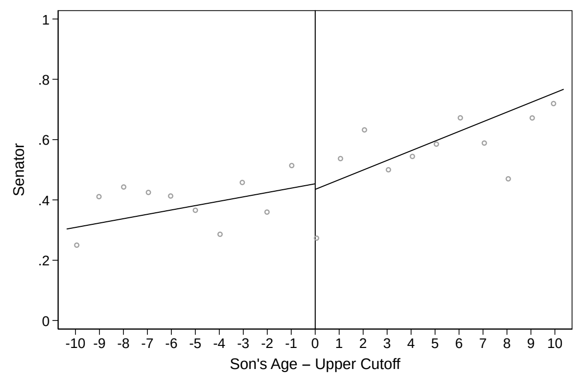
(a) Plot Corresponding to Table 5, Column (1)



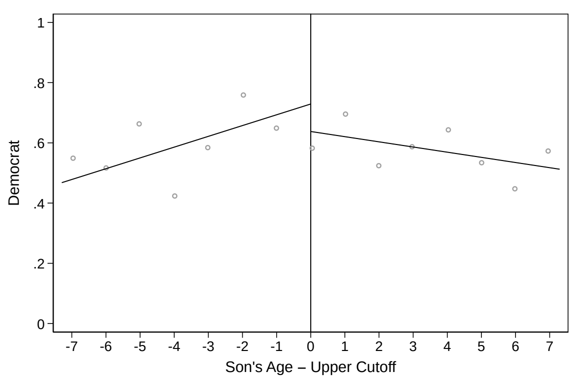
(b) Plot Corresponding to Table 5, Column (2)



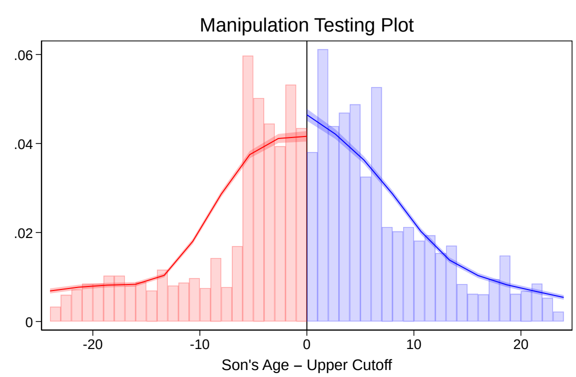
(c) Plot Corresponding to Table 5, Column (3)



(d) Plot Corresponding to Table 5, Column (7)



(e) Plot Corresponding to Table 5, Column (8)



(f) McCrary Density Test (p = 0.84)

Figure 3: **Regression Discontinuity Figures**

In columns (4)-(6), we follow the same process using local second-order polynomial models. Again, we estimate positive and significant effects in the presence of fixed effects: 12.89 percentage points using the bias-corrected method and 10.42 percentage points using the conventional method ($p<0.01$ in both cases). The estimates are similar when we set the bandwidth to match the baseline model without fixed effects. As in the linear case, the within-legislator effects are smaller in magnitude than the between-legislator effects, implying a role for both information and incentives in explaining the main results.

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 for 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 $\theta > 0$. It is unclear whether, on average, treated legislators with draft-eligible sons better represent their constituents' preferences than similar legislators with daughters of comparable age. If the treated group better reflects voters' concerns, then it is the comparison group politicians that deviate from the social optimum by failing to internalize the costs of their decision. If the comparison 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.[34]

In this section, we endogenize the behavior of the electorate in order to better understand 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.[35] 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 empirical tests to determine which model more closely fits the data.

A. 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.[36]

[34] We 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.

[35] See Besley (2006) for an in-depth account of these models.

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

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 similarly self-serving, they are therefore indifferent between incumbents and challengers. This implies that a politician's legislative record will not affect their reelection probability.

B. 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 about voter welfare. In chasing private rents, 'bad' politicians can also mimic good ones in order to disguise their type to the electorate.[37] 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 $j \in \{1, 2, \ldots N\}$ denotes an individual politician. We define type g as 'good' politicians for whom the weight placed on the private returns to legislative voting is zero, and type b as 'bad' politicians for whom this weight is strictly positive. Let π represent the probability that a randomly picked politician from the pool is a good type. 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 such 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, and period 2 decisions are made. The game ends once period 2 payoffs are realized.

[37] This is not possible in pure adverse selection models.

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 their 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, denoted by 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 their 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 low 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 some 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 $V_{j1}(S_1, b \mid r_1^h) = (1 - S_1)$ and are not reelected, while bad types with r_1^l select $V_{j1}(S_1, b \mid r_1^l) = S_1$ and are reelected. With heterogeneous rent shocks, therefore, there exists a set of payoffs 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 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.[38] However, bad types with draft-eligible sons observe $r_1^l < \beta(\mu + E)$, and instead mimic good types by choosing $V_{j1}(0, b \mid r_1^l) = 0$ 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 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.

C. 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. The pure moral hazard model predicts that having a draft-eligible son will not impact a politician's reelection probability. The moral hazard model that allows for different types predicts that having a draft-eligible son will decrease the average politician's reelection probability when the draft is popular among voters, and increase the average politician's reelection probability when it is not (Corollory 1).

We thus proceed in two steps. First, we identify the periods during which the draft was more popular among voters. We do this by testing for changes in the voting behavior of politicians during the period immediately before elections. This also serves as an alternative test to distinguish between the two models. In the pure moral hazard setup, politicians should not change their behavior in an election year. In the model with adverse selection, by contrast, bad politicians will have an incentive to change their behavior during an election year in an attempt to pool with good types and win reelection.

In the second step, we use the information garnered from this exercise to determine whether or not politicians with draft-age sons are more likely to be elected during those periods.

[38] Private 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.

Draft popularity 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.[39] These and related surveys are analyzed by Fordham (2016), who documents a collapse in support for the draft from around 70% in 1945 to around 20% at the outset of the Iraq War in 2003.[40] The main figure from that study is reproduced in Figure A2.

The prevailing 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 a means of ensuring victory waned. For example, conscription was viewed as essential to raise an army capable of entering World War I; by contrast, the U.S. fought the wars in Iraq and Afghanistan this century without appreciably enlarging the military at all, let alone through conscription (Fordham, 2016).

A second explanation 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 therefore plausible that fatalities reported during draft-era wars undermined support for the draft itself. Turning to data presented in Table A1, 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 rise of mass media over this period, it is reasonable to assign a higher weight to the Cold War figure in particular, as information on those fatalities is more likely to have proliferated among voters (Flynn, 1993).

One limitation of the existing research is the absence of polling data from earlier in the 20th century. While both explanations are consistent with the idea that public support for the draft has been falling over time, we do not have clear evidence to support this assessment for World War I. In order to investigate this, we examine how legislators vote on issues related to conscription during election years relative to other years for each era in our sample. If public support for the draft is high (i.e., if $S_t = 1$), then legislators will be more inclined to support the draft when they are up for reelection, all else equal. By contrast, if public support is low, then legislators will be less inclined to vote in favor of the draft when they are up for reelection.

We propose an empirical test that harnesses quasi-experimental variation. Senators serve six-year terms with staggered elections every two years. Thus, only one third of senators can face an election year at any moment in time. Because of this, we can compare the voting behavior of senators who are up for reelection versus those who are not for every Congress in our sample. This feature allows us to control for vote fixed effects—which flexibly capture common time effects—and recover a plausibly unbiased estimate of the effect of election years on legislative voting related to conscription for each era.[41]

[39]Experimental evidence from Horowitz and Levendusky (2011) shows that the specter of conscription reduces support for war in the United States. Exploiting variation from Vietnam draft lottery, Erikson and Stoker (2011) and Bergan (2009) show that survey respondents who were more exposed to conscription sharply reduced their support for the war. While these findings are informative, the first-order concern in our setting is how aggregate public support for the draft trends over the duration of our sample.

[40]Clifford 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.

[41]For this analysis, we define election years as the period *before* the November election date.

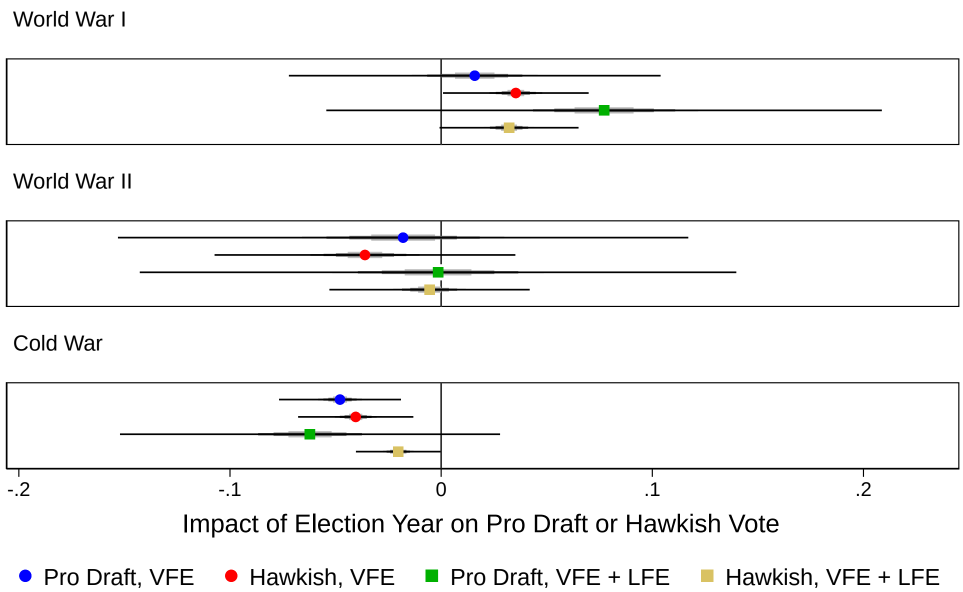


Figure 4: **Effect of Election Proximity on Pro Draft Votes and Hawkish Votes over Time**. These estimates correspond to those presented in Tables A10 and A11.

The results of this exercise are presented in Figure 4 and in Tables A10 and A11. We estimate the impact of election years on both *Pro Draft Vote* and *Hawkish Vote* separately for three periods, World War I, World War II, and the Cold War.[42] In each specification we include the baseline control variables and vote fixed effects ("VFE"). For each period, we also estimate specifications that additionally include legislator fixed effects ("LFE").

The estimates present a clear pattern. During World War I, legislators who were up for reelection were more likely to vote in favor of conscription than other legislators. The point estimates are positive whether we examine *Pro Draft Vote* or *Hawkish Vote* and whether we include legislator fixed effects or not. The estimates are marginally significant when we use the hawks and doves sample (which contains 27,809 observations) and they are less precisely estimated when we use the main sample (1,335 observations). By contrast, we see the opposite effect during the Cold War conflicts: all four estimates are negative and three are statistically significant.

These results align closely with the existing literature outlined above, and also with narrative accounts that stress the effectiveness of the U.S. government's propaganda and censorship efforts during World War I—best characterized by George Creel's 'Committee on Public Information' (Axelrod, 2009, Hamilton, 2020)—and of the mass anti-conscription protest movement during the Vietnam War (Flynn, 1993, 2002).

Taken together, these facts indicate that voter support for the draft declined substantially between World War I and the Cold War conflicts, and that politicians on average modified their legislative voting behavior to reflect this in advance of elections.

[42]We combine the Korean War and Vietnam War samples as there are only 8 votes in the former. This has no meaningful effect of the results.

Election Outcomes Using these findings, we now examine whether or not legislators with draft-eligible sons are more likely to be reelected over time in our sample. To do this, we simply run the following specification for each era as follows:

$$E_{ic(v)} = v_v + k_{iv} + s_{iv} + \gamma_1 draft\ son_{iv} + \gamma_2 draft\ child_{iv} + \mathbb{X}'_{iv}\psi + \epsilon_{iv}, \tag{6}$$

where $E_{ic(v)}$ is a binary variable indicating that legislator i was reelected following congressional session c, 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 $draft\ son_{iv}$ indicates that the legislator has a draft-eligible son for vote v, which is contained in congressional session c. The vector $\mathbb{X}_{iv}$ represents the baseline controls for party, house or senate, terms in office, age, and age squared. Our main specification does not include legislator fixed effects, as the outcome varies at the level of a legislator-term rather than a legislator-vote. We only include legislator-votes for legislators who contested their next election.

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 $draft\ son$ variable will have no effect on a legislator's subsequent election performance, i.e., $\gamma_1 = 0, \forall\ c$.

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 $draft\ son_{iv}$.

D. Results

We present the results of this exercise in Figure 5 and in Table A12. For each era, we estimate the specification with and without vote fixed effects. The results present a mirror image of our previous results on draft popularity over time. During World War I, when the draft was most popular, legislators with draft age sons were less likely to win reelection (p < 0.10). The point estimates approach zero for World War II, and the sign flips for the Cold War, when the draft became unpopular.

We show in Tables A13 to A14 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 for both World War I and the Cold War. This pattern is also inconsistent with random noise, suggesting that the pure moral hazard model is again not supported by the data.

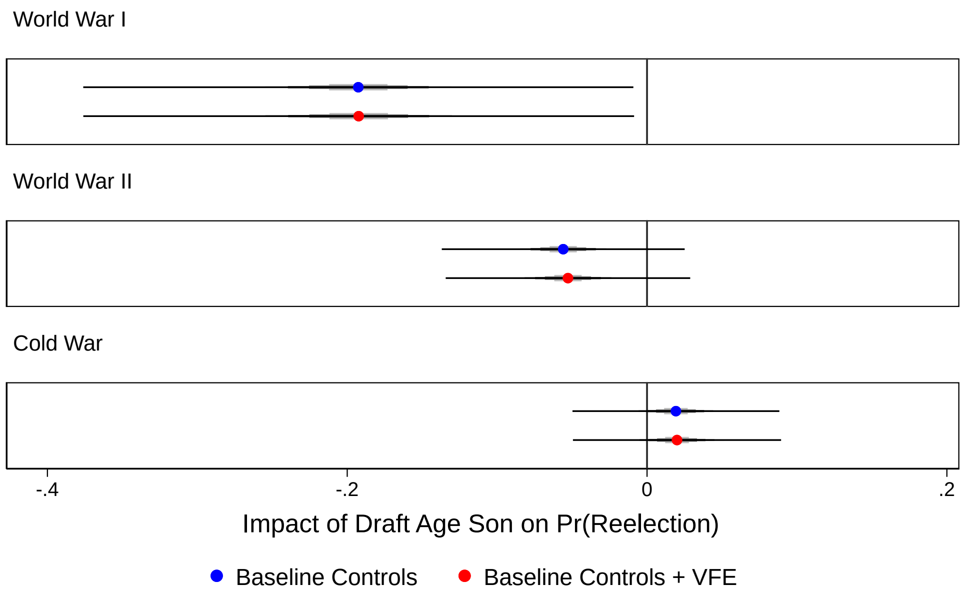


Figure 5: **Effect of Draft Age Son on Pr(Reelection) over Time**. These estimates correspond to those presented in Table A12.

Rather, the findings suggest that, as conscription became less popular with voters over time, legislators with draft-age sons became increasingly more likely to win reelection. The most obvious explanation is that these legislators are less likely to vote in favor of conscription, as we have shown in our main analysis, and are more likely to be reelected as a result. Taken together with the previous exercise on the effects of election proximity, this interpretation provides support for the model of political agency that allows for different politician types. Bad politicians pool their legislative votes with good types when their payoff from private rent-seeking is exogenously lower.

E. Other Motives

The previous analysis raises the important question of why some politicians still vote in favor of conscription when it is broadly unpopular, as was the case during the Cold War era. 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 equation 3, these could be party career concerns (P_{it}), ideology (F_i), or other unobserved private benefits (R_{it}).

Here, we explore the hypothesis that pressure to comply with the national party line played a significant role. To test this, 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 estimate is identified off the switch from President Lyndon B. Johnson (a Democrat) to President Richard Nixon (a Republican) in 1969. This specification allows us to flexibly control for the party identification of the given legislator, as well as all other time invariant characteristics such as ideological preferences.

This hypothesis is motivated 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 "campaigned on a promise to put a stop to it, but repeatedly asked for its extension as president."[43] Nixon's reverse 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.[44]

In Table 7, we provide strong evidence in support of party career concerns as a partial explanation for pro-draft legislative voting. Political alignment with the sitting president significantly increases the probability that a given legislator votes in favor of conscription. Controlling for individual fixed effects, we estimate that legislators are 11 percentage points more likely to vote in favor of conscription when the party of the president aligns with their own (column 4). This indicates that pro-draft voting in the U.S. Congress appears to stem at least in part from national party edicts.

This exercise also provides some quantitative context for interpreting the main estimate of interest in this paper. Again focusing on column (4), we note that the impact of having a son of draft age (relative to a daughter of similar age) is almost half the magnitude of this party political effect. Note also that the regression discontinuity estimates from Section 6 (+0.1073 with fixed effects and +0.2803 without, using the bias-corrected estimates) are almost identical to the party political effect estimated here (+0.1131 and +0.2350).

[43]Similarly, 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."

[44]"Senate Bars Plan Designed To Bring Volunteer Army," *The New York Times*, Aug. 26, 1970. More evidence on this motive can be gleaned from newspaper reports at the other end of our sample period. Under 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.[45]

Table 7: President Party Alignment

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.0642**	-0.0417	-0.0894***	-0.0545**
	(0.0290)	(0.0293)	(0.0328)	(0.0262)
Draft Age Child	-0.0135	-0.0028	0.0038	0.0075
	(0.0266)	(0.0267)	(0.0295)	(0.0263)
President's Party	0.2350***	0.2283***	0.1363***	0.1131***
	(0.0286)	(0.0303)	(0.0247)	(0.0217)
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Legislators	2284	2284	2120	2120
Votes	103	103	103	103
Mean Dep. Var.	0.605	0.605	0.605	0.605
Observations	19262	19262	19098	19098

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

8. Counterfactual Analysis

In this section we use our estimates to quantify the historical differences that may have emerged if all members of Congress had draft-age sons. This counterfactual analysis comes with the usual caveat that it is a partial equilibrium exercise—we are holding constant all other factors, including the content of the roll call votes themselves and the expectations that members of Congress have regarding their passage. Nevertheless, it is informative to determine whether or not our estimates are quantifiably meaningful.

Of the 103 roll call votes that we study in our main analysis, 88 went in favor of conscription—that is, either pro-draft measures were adopted or anti-draft measures were defeated. Of these 88 votes, 17 passed by margins of less than 6.22 percentage points (our estimated treatment effect from Section 5); 24 passed by less than 10.73 percentage points (our RD estimate using within-legislator variation); and 50 passed by less than 28.03 percentage points (our RD estimate using within- and between-legislator variation.)

An alternative approach is to identify the roll call votes in which legislators with draft-age sons were opposed to the majority decision. Of the 88 votes in which the majority favored conscription, 30 would have been reversed if all members of Congress had a draft-age son. These votes include failed attempts in the Senate to end the draft in 1970 (the aforementioned Hatfield-Goldwater amendment) and to withdraw entirely from southeast Asia in 1971, two years before the Paris Peace Accords that signaled the end of US involvement in Vietnam.

This counterfactual exercise highlights how the course of history could have changed if leaders internalized the consequences of their military decisions. It also helps to crystalize the link between the theoretical conflict literature (e.g., Jackson and Morelli, 2007, Fearon, 1995) and our empirical application. Most of decisions that we analyze involve impelling citizens to fight in conflicts that already exist. These can be viewed as decisions to engage in warfare on the intensive margin. However, as the example above shows, some decisions involve withdrawing entirely from combat arenas, which can be viewed as decisions to engage in warfare on the extensive margin (via duration rather than onset).

9. Conclusion

We test the longstanding hypothesis that political agency problems can lead to violent conflict between groups. Political elites who do not internalize the costs of war are more likely to be in favor of it. We demonstrate this by compiling data on the voting behavior and family composition of 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 of comparable age, legislators with sons who are eligible to be drafted are around 10-17% less likely to vote for conscription; and (ii) legislators increase their support for the draft by almost one fifth when their sons cross the upper age threshold.

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 application, having a draft-eligible son introduces exogenous variation in the private rents that bad politicians derive from voting on conscription. Consistent with this model, we show that politicians with draft-eligible sons are more likely to be reelected when the draft is broadly unpopular.

Our analysis provides new evidence that helps to explain the puzzle of why violent conflict can occur between groups despite being costly. Agency frictions within groups can lead to conflict between groups precisely because these costs of warfare are not internalized by political elites.

From a more general perspective, we identify a large and significant effect of private incentives on congressional decision-making conditional on individual fixed effects. This implies that politicians are malleable, which has important implications beyond the issue of conscription. Identifying the effect of private incentives of legislators in other policy domains represents a fruitful avenue for future research. Our results suggest that representative democracy may better enhance social welfare when citizens are aware of legislators' private incentives and when they vote often enough to impose accountability on important legislative decisions, including those related to war.

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For Online Publication: Appendix

Appendix A. Data Appendix

In the accompanying file APPENDIX_votes, we list the 248 pro-draft votes in our dataset. This is contains descriptions from Voteview and Govtrack for each roll call vote. The variable *Pro Conscript* is equal to 1 if an aye vote indicates support for conscription. It is equal to 0 if an aye vote indicates opposition to conscription. Higher values indicate that the vote is ambiguous. The variable *Confident* is an indicator for whether or not we include the vote in our main sample. The variables *Proposed Lower* and *Proposed Upper* indicate the upper and lower cutoffs associated with a given vote. These variables are used to construct a legislator's treatment status on a given vote. The variables *Actual Lower* and *Actual Upper* indicate the current legal cutoffs at the time of the vote. The variable *Window Vote* indicates that the vote aims to change either the upper or lower cutoff. The *Notes* column contains the authors' notes for each vote, where applicable. The authors' 'scrapbook' of newspaper articles, which contains additional sources that they used to code *Pro Conscript* and *Confident*, is available on request.

In the accompanying file APPENDIX_hawksdoves, we list the hawks and doves for each conflict period, together with sources.

Appendix B. Background on Draft Voting in Congress

In this section we consider (i) the social costs and benefits of conscription that were postulated during debates on the floor (or in committee) at the time; and (ii) the additional private 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, House Representative Julius Kahn, who led the Administration's fight for conscription, invited a British Army captain, Percy Benson, 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.[A1]

[A1] "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 Conscriptionist," *The New York Times*, April 26 1917)

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 because 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.[A2] Relatedly, others viewed the draft as an opportunity for special interests to profit,[A3] while isolationists and pacifists opposed conscription as part of their general opposition to interventionist foreign policy.[A4]

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 Hubert Stephens of Mississippi to make members of Congress themselves subject to the draft during World War I. Speaking in favor of the amendment, 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 was defeated, 130 to 86.[A5]

To understand the additional private costs incurred by treated legislators, we compile in Table A1 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.[A6] 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. Around one fifth of legislators, therefore, had a non-trivial role in determining the risks faced by their own sons in battle.

[A2]"Unions Oppose the Draft – Resolution Adopted Unanimously by Central Federation," *The New York Times*, April 1 1917

[A3]During 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."

[A4]Speaking 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.)

[A5]"Amendments Flood House," *The New York Times*, April 29 1917.

[A6]This 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.)

Table A1: 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.

Because we observe the sons of legislators at a young age in our dataset, we are prevented from knowing their military status as adults. Zillman (1997, 2006) documents that the use of deferments and other tactics by privileged groups to avoid military service became contentious during the Vietnam War. The perceived injustice arising from this was perhaps best captured by the song "Fortunate One" by Creedence Clearwater Revival in 1969, which contains the line "It ain't me, it ain't me, I ain't no senator's son. It ain't me, it ain't me, I ain't no fortunate one." Its author, John Fogerty, wrote in his memoir that the song was inspired in part by David Eisenhower, the grandson of President Dwight D. Eisenhower, and his wife Julie Nixon, daughter of President Richard Nixon, who were "symbolic in the sense that they weren't being touched by what their parents were doing" (Fogerty, 2015, pp. 160).

Nevertheless, Zillman (1997) estimates that around 25% of eligible men in privileged groups—future judges and future members of Congress—served during the Vietnam War either as draftees or as volunteers motivated by the draft. Adding to this the opportunity cost of serving in the Reserve or National Guard, plus the reputational cost of avoiding combat through this or other means (per the quote above), it is reasonable to assume that legislators with draft-age sons had more to lose with the passing of conscription than other legislators had.

Appendix C. Additional Appendix Figures

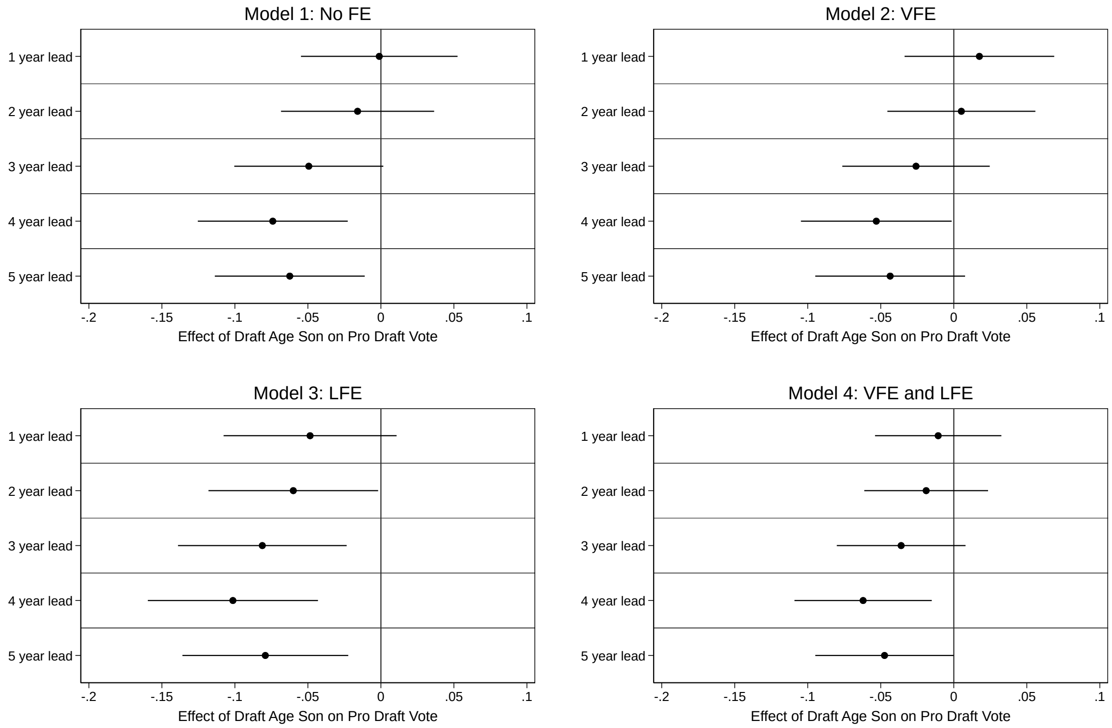

Figure A1: **Impact of Draft Age Son Pro Draft Votes with Varying Leads**

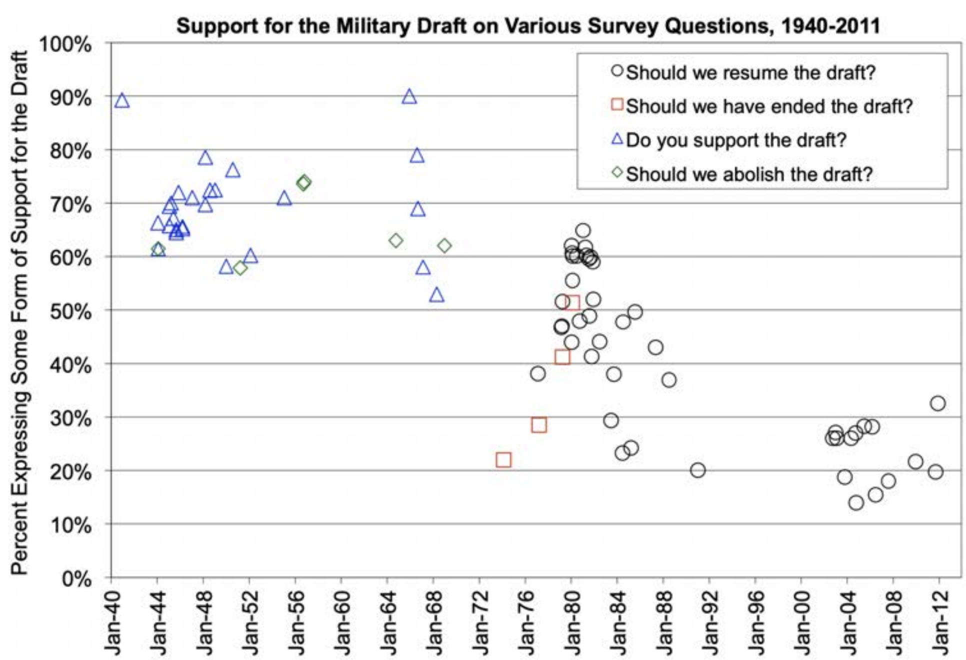

Figure A2: **Support for Conscription Over Time, from Fordham (2016)**

Appendix D. Additional Appendix Tables

Table A2: 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 A3: Balance: Democrat

	Democrat			
	(1)	(2)	(3)	(4)
Draft Age Son	0.0636	0.0506	0.0062	0.0062
	(0.0455)	(0.0455)	(0.0062)	(0.0061)
Draft Age Child	0.0067	0.0129	-0.0076	-0.0070
	(0.0441)	(0.0439)	(0.0079)	(0.0072)
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Legislators	2271	2271	2107	2107
Votes	103	103	103	103
Mean Dep. Var.	0.589	0.589	0.589	0.589
Observations	19088	19088	18924	18924

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator is a Democrat. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A4: Balance: Senator

	Senator			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.0195	-0.0000	-0.0112	-0.0000
	(0.0513)	(0.0000)	(0.0175)	(0.0000)
Draft Age Child	-0.0311	0.0000	0.0041	-0.0000
	(0.0484)	(0.0000)	(0.0187)	(0.0000)
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Legislators	2284	2284	2120	2120
Votes	103	103	103	103
Mean Dep. Var.	0.421	0.421	0.423	0.423
Observations	19262	19262	19098	19098

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator is a senator. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A5: Balance: Terms

	Terms			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.0679	0.1227	0.0465	-0.0109
	(0.1859)	(0.1368)	(0.0942)	(0.0167)
Draft Age Child	-0.4943***	-0.4020***	-0.0308	-0.0111
	(0.1863)	(0.1418)	(0.0936)	(0.0127)
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Legislators	2284	2284	2120	2120
Votes	103	103	103	103
Mean Dep. Var.	3.175	3.175	3.192	3.192
Observations	19262	19262	19098	19098

Note: The unit of analysis is the legislator-vote. The outcome variable is the number of congressional terms served by the legislator at the time of the vote. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A6: Balance: Age

	Age			
	(1)	(2)	(3)	(4)
Draft Age Son	1.1780	0.4572	-0.2078	0.0029
	(0.8806)	(0.7754)	(0.3045)	(0.0029)
Draft Age Child	0.1765	0.9015	-0.3819	0.0012
	(0.8641)	(0.8251)	(0.3201)	(0.0012)
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Legislators	2284	2284	2120	2120
Votes	103	103	103	103
Mean Dep. Var.	54.617	54.617	54.647	54.647
Observations	19262	19262	19098	19098

Note: The unit of analysis is the legislator-vote. The outcome variable is the age of the legislator at the time of the vote. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A7: Control for Non-Linear Effect of Child's Age

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.0714**	-0.0534*	-0.0816**	-0.0470*
	(0.0311)	(0.0311)	(0.0344)	(0.0273)
Draft Age Child	-0.0268	-0.0085	0.0055	0.0156
	(0.0304)	(0.0305)	(0.0305)	(0.0280)
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes
Legislators	2284	2284	2120	2120
Votes	103	103	103	103
Mean Dep. Var.	0.605	0.605	0.605	0.605
Observations	19262	19262	19098	19098

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in favor of conscription. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A8: Allowing for Heterogenous Effects by Vote Margin:

	Pro Draft Vote	
	(1)	(2)
Draft Age Son	-0.0598	-0.0722*
	(0.0438)	(0.0414)
× Roll Call Vote Margin 25-50%	0.0175	0.0049
	(0.0261)	(0.0229)
× Roll Call Vote Margin 50-75%	-0.0040	0.0224
	(0.0473)	(0.0383)
× Roll Call Vote Margin 75-100%	0.0115	0.0274
	(0.0439)	(0.0428)
Draft Age Child	-0.0241	-0.0087
	(0.0416)	(0.0411)
× Roll Call Vote Margin 25-50%	0.0175	0.0061
	(0.0254)	(0.0234)
× Roll Call Vote Margin 50-75%	0.0413	0.0363
	(0.0375)	(0.0423)
× Roll Call Vote Margin 75-100%	0.0450	0.0425
	(0.0410)	(0.0411)
Legislator FE	No	Yes
Vote FE	Yes	Yes
Number of Sons FE	Yes	Yes
Number of Children FE	Yes	Yes
Other Controls	Yes	Yes
Legislators	2284	2120
Votes	103	103
Mean Dep. Var.	0.605	0.605
Observations	19262	19098

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in favor of conscription. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A9: Window Votes

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Panel A: Window Votes with 2-Year Lead				
Draft Age Son	-0.0323	-0.0524*	-0.0060	-0.0296
	(0.0347)	(0.0307)	(0.0524)	(0.0463)
Draft Age Child	0.0902	0.0278	0.1312*	0.0171
	(0.0554)	(0.0255)	(0.0676)	(0.0365)
Legislators	1825	1825	1600	1600
Votes	37	37	37	37
Mean Dep. Var.	0.512	0.512	0.519	0.519
Observations	7109	7109	6884	6884
Panel B: Window Votes with 2-Year Lead and Main Votes with 4-Year Lead				
Draft Age Son	-0.0576**	-0.0521**	-0.0483*	-0.0392*
	(0.0247)	(0.0246)	(0.0247)	(0.0208)
Draft Age Child	0.0337	0.0052	0.0614*	0.0130
	(0.0307)	(0.0216)	(0.0342)	(0.0199)
Legislators	2287	2287	2194	2194
Votes	140	140	140	140
Mean Dep. Var.	0.580	0.580	0.579	0.579
Observations	26371	26371	26278	26278
Panel C: Window Votes and Main Votes with 4-Year Lead				
Draft Age Son	-0.0496**	-0.0454*	-0.0453*	-0.0399*
	(0.0246)	(0.0245)	(0.0242)	(0.0203)
Draft Age Child	0.0287	0.0014	0.0563*	0.0123
	(0.0309)	(0.0215)	(0.0338)	(0.0190)
Legislators	2287	2287	2194	2194
Votes	140	140	140	140
Mean Dep. Var.	0.580	0.580	0.579	0.579
Observations	26371	26371	26278	26278
Legislator FE	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in favor of conscription (i.e., votes in favor of expanding the draft age eligibility window or against narrowing the window). Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A10: Effect of Election Year on Pro Draft Votes, By Wars

	Dependent Variable: Pro Draft Vote					
	WW1		WW2		Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)
Election Year	0.0159	0.0772	-0.0180	-0.0015	-0.0478***	-0.0618
	(0.0480)	(0.0718)	(0.0791)	(0.0827)	(0.0174)	(0.0531)
Legislator FE	No	Yes	No	Yes	No	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	462	454	729	592	1062	988
Votes	10	10	26	26	66	66
Mean Dep. Var.	0.710	0.714	0.534	0.539	0.647	0.646
Observations	1335	1327	4152	4015	9941	9867

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in favor of conscription. Election Year is an indicator equal to 1 if the roll call vote occurs during the year of an election for the given legislator and if it occurs before the election takes place. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table A11: Effect of Election Year on Hawkish Votes, By Wars

	Dependent Variable: Hawkish Vote					
	WW1		WW2		Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)
Election Year	0.0354*	0.0321	-0.0361	-0.0055	-0.0405**	-0.0203*
	(0.0209)	(0.0200)	(0.0432)	(0.0288)	(0.0166)	(0.0122)
Legislator FE	No	Yes	No	Yes	No	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	681	681	762	762	1142	1142
Votes	407	407	316	316	1017	1017
Mean Dep. Var.	0.521	0.521	0.541	0.541	0.504	0.504
Observations	27809	27809	25859	25859	104569	104569

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in line with the modal vote among hawks and against the modal votes among doves. Election Year is an indicator equal to 1 if the roll call vote occurs during the year of an election for the given legislator and if it occurs before the election takes place. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

Table A12: Effect of Draft Age Son on Next Election Victory

	Dependent Variable: Reelected in Next Election					
	WW1		WW2		Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)
Draft Age Son	-0.1926*	-0.1924*	-0.0559	-0.0528	0.0193	0.0200
	(0.1001)	(0.1002)	(0.0474)	(0.0477)	(0.0414)	(0.0416)
Draft Age Child	0.1285	0.1286	0.0017	0.0013	-0.0167	-0.0181
	(0.0833)	(0.0836)	(0.0494)	(0.0505)	(0.0390)	(0.0391)
Vote FE	No	Yes	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	462	462	726	726	1061	1061
Votes	10	10	26	26	67	67
Mean Dep. Var.	0.790	0.790	0.844	0.844	0.884	0.884
Observations	1335	1335	4138	4138	10007	10007

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if the legislator wins their next election after the given roll call vote. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$.

Table A13: Main results by War

	Dependent Variable: Pro Draft Vote											
	WW1				WW2				Cold War			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Draft Age Son	-0.0953**	-0.0642	-0.1350***	-0.1178**	0.0202	0.0145	0.0239	0.0093	-0.0736*	-0.0694*	-0.1090**	-0.0924**
	(0.0329)	(0.0381)	(0.0300)	(0.0503)	(0.0505)	(0.0503)	(0.0533)	(0.0564)	(0.0383)	(0.0391)	(0.0444)	(0.0405)
Draft Age Child	0.0051	0.0491	0.1263**	0.1708***	0.0498	0.0431	-0.0101	-0.0102	-0.0200	-0.0027	-0.0007	0.0064
	(0.0484)	(0.0420)	(0.0543)	(0.0493)	(0.0467)	(0.0471)	(0.0592)	(0.0628)	(0.0356)	(0.0361)	(0.0427)	(0.0402)
Legislator FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	581	581	567	567	863	863	706	706	1196	1196	1114	1114
Votes	10	10	10	10	26	26	26	26	67	67	67	67
Mean Dep. Var.	0.644	0.644	0.648	0.648	0.510	0.510	0.512	0.512	0.640	0.640	0.638	0.638
Observations	1790	1790	1776	1776	5186	5186	5029	5029	12285	12285	12203	12203

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in favor of conscription. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A14: HD: Main results by War

	Dependent Variable: Hawkish Vote											
	WW1				WW2				Cold War			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Draft Age Son	0.0148	0.0053	-0.0461	-0.0509	-0.0190	-0.0207	-0.0299	-0.0345	-0.0853*	-0.0784	-0.1164*	-0.0789
	(0.0329)	(0.0307)	(0.0394)	(0.0367)	(0.0487)	(0.0477)	(0.0478)	(0.0377)	(0.0476)	(0.0484)	(0.0643)	(0.0581)
Draft Age Child	-0.0178	-0.0163	0.0481	0.0414	0.0512	0.0529	0.0551	0.0400	-0.0119	0.0104	0.0009	0.0272
	(0.0313)	(0.0205)	(0.0437)	(0.0274)	(0.0417)	(0.0405)	(0.0414)	(0.0393)	(0.0437)	(0.0440)	(0.0604)	(0.0573)
Legislator FE	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Vote FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	665	665	568	568	769	769	686	686	1038	1038	965	965
Votes	38	38	38	38	61	61	61	61	94	93	94	93
Mean Dep. Var.	0.587	0.587	0.592	0.592	0.539	0.539	0.545	0.545	0.693	0.693	0.693	0.693
Observations	4288	4288	4191	4191	6243	6243	6160	6160	10255	10254	10182	10181

Note: The unit of analysis is the legislator-vote. The outcome variable is an indicator equal to one if a legislator votes in line with the modal vote among hawks and against the modal votes among doves. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.