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

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ABSTRACT

Why do wars occur? We exploit a natural experiment to test the longstanding hypothesis that leaders declare war because they fail to internalize the associated costs. We test this moral hazard theory of conflict by compiling data on the family composition of 3,693 US legislators who served in the U.S. Congress during the four conscription-era wars of the 20th century: World War I, World War II, the Korean War, and the Vietnam War. We test for agency problems by comparing the voting behavior of congressmen with draft-age sons versus draft-age daughters. We estimate that having a draft-age son reduces legislator support for pro-conscription bills by 10-17%. Legislators with draft-age sons are more likely to be reelected subsequently, suggesting that support for conscription is punished by voters. Our results provide new evidence that agency problems contribute to political violence, and that elected officials can be influenced by changing private incentives.

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1 Introduction

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

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

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

We find that legislators with sons of draft age are between 10% and 17% less likely to vote in favor of conscription than comparable legislators with only daughters of draft age. To place this magnitude into perspective, it is equivalent to 50-70% of the "party line" effect of having a sitting president from the opposing party. Moreover, we also find that these legislators are more likely to win reelection subsequently, and that legislators on average are less likely to vote in favor of conscription during election years. These findings suggest that voters reward politicians who vote against conscription, and that other politicians do not internalize its social costs.

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

To arrive at these results, we undertake two main data collection exercises. In the first, we gather information on the number and gender of children of 3.693 U.S. senators and representatives from a combination of census records and and a variety of biographical sources. In the second, we identify 249 roll-call votes relating to conscription from 1917 to 1974, and code the direction of pro- or anti-conscription policy preferences based on contemporaneous newspaper reports. Our main estimation sample contains 26,000 observations at the level of a legislator-vote.

In order to validate our vote-coding procedure, we eschew the task of assigning pro- or anti-conscription codes to roll call votes ourselves and develop instead a method that relies 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 pro-draft vote. This approach expands our sample to around 800,000 observations. Applying it, 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 contributes to two distinct bodies of research. The first is an emergent empirical literature that connects credible identification strategies to theoretical work on the origins of violent conflict. These foundations are based on contest models in which two sides fight to control total resources. Each side allocates their own resources between production and appropriation, and the probability of victory is determined by the relative effectiveness of fighting technology (Haavelmo, 1954; Hirshleifer, 1988; Garfinkel, 1990; Skaperdas, 1992). One limitation of contest models is that they fail to account for bargained settlements. Wars are risky and destructive, and so it is necessary to understand why they are avoided in some cases but not in others.[2]

Two sets of explanations in particular endure for why lengthy wars can occur between rational actors. The first broadly relates to incomplete contracting, whereby the inability of each group to credibly commit to a negotiated settlement inhibits peace (Garfinkel and Skaperdas, 2000; Powell, 2006, 2012). For example, Chassang and Padro i Miquel (2009) develop a model in which transient economic shocks reduce the opportunity cost of

[1]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.

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

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 so war can ensue in equilibrium. Empirical papers that exploit plausibly exogenous variation to identify the link from economic conditions to conflict include McGuirk and Burke (2016), Miguel et al. (2004), Dube and Vargas (2013), Bazzi and Blattman (2014), Berman and Couttenier (2015), Berman et al. (2017), and Harari and La Ferrara (2014). The second explanation has received less attention in the empirical literature: that costly wars occur because the leaders who order it do not fully internalize the costs. This "moral hazard" theory of conflict relaxes the assumption that groups are unitary actors.[3] To the best of our knowledge, we are the first to corroborate it empirically.[4]

The second literature broadly relates to the political economy of legislative decision-making. The prevailing view is that a legislator's vote is motivated by reelection concerns, promotion to higher office, and the politician's own ideological beliefs (de Figueiredo and Richter, 2014; Ansolabehere et al., 2003; Levitt, 1996; Washington, 2008). However, this model of policy formation leaves no room for the possibility that legislators are additionally influenced by other private payoffs. While this may be difficult to reconcile with the growing share of campaign contributions emanating from the extreme top of the wealth distribution in the United States (Bonica et al., 2013), there exists nonetheless an argument that politicians are largely immune from such influences (Ansolabehere et al., 2003; Levitt, 1994; Tullock, 1972). An alternative explanation for this absence of evidence is the empirical challenge inherent in its detection. An ideal identification strategy would require estimating the effect on legislative voting of a change in private motives that is orthogonal to both political and ideological concerns. By exploiting plausibly exogenous variation in the gender of draft-age children, our study overcomes this problem and finds that legislators respond to private incentives.

We proceed with a brief discussion on the political economy of legislative voting in Section 2. In Section 3 we introduce our data; In Sections 4 and 5 we present our estimation strategy and main results; and in Sections 6 and 7 we further investigate the motives of legislators in light of the main results. In Section 8 we conclude.

2 Political Economy of Legislative Voting in a Democracy

There is a broad consensus in the political economy literature that a politician's legislative vote is determined by reelection concerns, promotion to higher office, and their own ideological beliefs (de Figueiredo and Richter, 2014; Ansolabehere et al., 2003; Levitt, 1996).

[3]Other papers that relax the assumption by modeling the behavior of political leaders in conflict include De Mesquita and Siverson (1995), Tarar (2006), and Smith (1996).

[4]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).

This implies that the politician takes into account four sets of preferences in determining her optimal legislative vote (Higgs, 1989; Levitt, 1996). Reelection concerns are represented both by the preferences of voters in her electorate, and by the preferences of her supporters within that group; promotional concerns are represented by the national party line; and ideological beliefs are exogenously determined fixed preferences.

Assuming that preferences are single peaked, the politician's objective is to select a vote that minimizes the weighted average of the squared distances from the four "ideal points" that correspond to each preference as follows:

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

where $V_{it} \in \{0,1\}$ is the legislator i's vote at time t; $S_{it} \in [0,1]$ 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 the fixed over time. The α parameters represent weights, and all weights sum to 1.

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

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

[5] 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.

[6] 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.

[7] 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 et al. (2013) and Mian et al. (2010) also

The third element, national party preferences, reflect the fact that politicians are incentivized to vote in line with the national party, who in return can provide promotions to various committee positions.[8]

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

Incorporating private influences

A notable feature of this model is the absence of a private motive that is distinct from a legislator's fixed ideology and political career concerns. To wit, the model either assumes that there are no other private costs and benefits associated with legislative voting, or that, if there are, legislators are immune to their influence. This appears to be at odds with the apparently large sums of private money that are spent on lobbying and campaign contributions. However, Ansolabehere et al. (2003), echoing Tullock (1972), argue that if campaign contributions were indeed worthwhile investments, they ought to be of substantially higher value in each election cycle given the trillions of dollars of government outlays potentially at stake. They note that campaign spending limits are not binding; that the majority value of contributions come from individual donors rather than special interest Political Action Committees (PACs); and that these individuals give the marginal dollar. They also run fixed effects regressions that uncover no relationship between pro-corporate legislative voting and corporate donations. They conclude that campaign contributions are largely made for their consumption value, rather than returns on investment.[10]

find evidence that is consistent with this effect.

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

[9] 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.

[10] 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 preferences in a quid pro quo arrangement. However, even this possibility has been challenged empirically, most notably by Levitt (1994). Similarly, the fact that three times more is spent on lobbying in the U.S. than campaign contributions does not imply that legislators are susceptible to private concerns beyond those laid out above. Lobbying is the transfer of information in private meetings from organized groups to politicians or their staffs (de Figueiredo and Richter, 2014). If these activities were shown to have an impact on policy, the possibility would still remain that their impact operates through any of the elements in the model rather than through a private quid pro quo channel.

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

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

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

where $M_{it} \in [0, 1]$ 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 is:

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

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

Implications for Conflict

Much of the theoretical literature on violent conflict treats actors as unitary decision-makers.[11] Implicit in this approach is the assumption that the costs and benefits of conflict are shared among members of each group. The politician's solution in (3) relaxes

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

this assumption. If, on a given vote, a shock to M_{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 $V_{it}^{*}(S_{it}, C_{it}, P_{it}, F_i, M_{it}) \neq V_{it}^{*}(S_{it}, C_{it}, P_{it}, F_i)$.[12] This is raised by Fearon (1995) as one explanation for violent conflict between rational groups that cannot be solved necessarily through a negotiated settlement.

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), Tarar (2006), and Smith (1996)), 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 the present setting this is achieved by the threat of penalties for draft evasion.

The specific role of moral hazard in conflict has been applied usually to the case of rebel activity in the presence of external humanitarian interventions. 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. In our paper, we make the related argument that politicians who are protected from the risks of conflict are more likely to support it.

Testing Implications

The central challenge for the researcher in determining whether or not private payoffs influence policy decisions (i.e., $\theta > 0$) is to identify exogenous variation in M_{it}. Otherwise, any estimate $\hat{\theta}$ could be biased due to positive covariance between M_{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 S_{it} or C_{it} is measured incorrectly as M_{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 M_{it}.

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

[12] The same could be said about changes to C_{it}, P_{it} and F_i, assuming that S_{it} approximates the social optimum. An interesting difference is that those motives are plausibly known to the electorate, whereas M_{it} is plausibly not.

3 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 800,000 legislator-votes spread between the House of Representatives and the Senate from the 45th Congress in 1877 to the 107th Congress in 2003.[13] In our core analysis of conscription voting there are 2,287 legislators, 5,420 children, and 26,373 legislator-votes starting in the 65th Congress and ending in the 93rd Congress. We describe below our principal data sources and the construction of our main variables.

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

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

[13]This includes only congresses that contain roll call votes of interest regarding conscription and warfare.
[14]See https://voteview.com/.
[15]See www.govtrack.org, a project of Civil Impulse, LLC.

anti-draft one.

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

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

The results of this data collection exercise can be seen in Table 1, we where document draft-related votes only in congresses in which we found relevant votes that we could determine as pro- or anti-draft. In total, we coded the direction of 140 votes—106 in the Senate and 34 in the House (Column 1). In the second column we present our main dependent variable: *Pro Draft* is equal to 1 if a legislator voted in favor of conscription (aye if it was a pro draft vote, or nae if it was an anti draft vote), and 0 otherwise. This exhibits a large amount of variation; the overall mean is 0.58. In the third column we present the average absolute margin between aye and other votes (nae or abstentions). For example, there is one vote in the 89th Senate; Pro Draft is 0.93, which means the margin is $0.93 - (1 - 0.93) = 0.86$, the gap between the winning vote and the losing vote. Column (4) contains the number of draft-related votes in total—i.e., successfully coded or otherwise. The overall number is 232, as the remaining 16 were in other congresses. We cannot present the same information for the outcome variable, but we do present the average margin to facilitate a comparison between Columns (2) and (3). The respective mean margins are 0.18 and 0.17, suggesting that there is no obvious difference between votes that we could and could not code. In Columns (6) and (7), we repeat the exercise for all votes that were assigned war-related codes in Voteview. There are 2,874 in total in these congresses, and the average margin is not significantly different to those of the two draft vote samples.

Legislator data The main independent variables are constructed from data on legislators' family compositions. We first take basic data on legislators themselves from the *Biographical Directory of the United States Congress 1774 - 2005* (Dodge et al., 2005). We then use this information to locate richer household data from alternative sources. Most of this data is acquired from decennial U.S. Census records dating from 1840 to 1940, which we access through *Ancestry*– a company that provides digitized and searchable Census records up to 1940.[16] These records contain information on the name, gender and birth date of each household member. We cross-check household data across multiple Census records and ensure that the full set of children are accounted for. For those congresspeople too young to have household information contained in the 1940 Census, we rely instead on a broad range of sources that include obituaries in national newspapers; biographies on official federal and local government websites; local media profiles; university archives; and online repositories such as the *Notable Names Database*, *Legacy.com*, and *Biography.com*.[17]

In Table 2, we present this information only for the 2,287 legislators who voted on our main sample of conscription measures in Column (1) of Table 1. Of these, 85% had children at the time of voting, and the average number of children per legislator was 2.37; 68% had at least one son and 65% had at least one daughter. In the second to last column, we present the percentage of legislator-votes in which a legislator's son was within the draft-eligibility window pertaining to the given roll call vote. For example, on a vote that proposes to enact the draft for all men between 20-25, a legislator with a 26 year old son is coded as a 0. However, if the following roll call vote proposed to raise the upper cutoff so that the window runs from 20-30, the same legislator is coded as a 1. This is our main "treatment" variable in the analysis. The House and Senate sample means are 0.23 and 0.21; meaning that over one fifth of legislator-votes on draft bills are cast by legislators with sons in the draft window. Reassuringly, the equivalent figure for daughters are the same.

4 Estimation

Our main specification is as follows:

$$V_{isvcj} = \alpha_i + v_{vcj} + k_{iv} + \beta_1 son_i + \beta_2 draft_{iv} + \beta_3 son \times draft_{iv} + \zeta \mathbb{X}_{iv} + \epsilon_{isvcj}, \qquad (4)$$

where V_{isvcj} is an indictor equal to one if the legislator i from state s votes to enact or expand conscription in vote v during congress c in congressional chamber j; α_i are legislator fixed effects; v_{vcj} are vote fixed effects; k_{iv} are fixed effects for number of children at the time of vote v; son_{iv} is an indictor equal to one if a legislator has a son at the time of vote v; $draft_{iv}$ is an indicator variable equal to one if a legislator has any child of draft-eligible age as determined by the cutoffs in vote v; $son_i \times draft_{iv}$ indicates that a legislator has a son of draft-eligible age in v; $\mathbb{X}_{iv}$ is a vector of time varying controls, including the legislator's age, age squared, and terms in office. In regressions without legislator and

[16] See www.ancestry.com

[17] The authors will provide a full list of sources in an online appendix.

vote fixed effects, we include controls for party, state and chamber fixed effects. Standard errors are two-way clustered by legislator and vote. We estimate the specification with a linear probability model (LPM) and a conditional (fixed effect) logit model (CL).

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

Finally, it is necessary to determine the appropriate number of lead years for the lower cutoff in the treatment variable. If, say, the lower cutoff is at 20, then it is likely that a congressperson with a 19 year old son is effectively treated. Failing to account for this will bias the treatment variable β_3 toward zero, as treated legislators will contaminate the control group. While the decision is somewhat arbitrary, what should be clear is that β_3 initially rises as we reduce the lower cutoff and add more treated legislators to the treatment group, before smoothly decreasing again as more untreated legislators with younger children are added.

5 Main Results

Table 3 presents the main results with the lower bound set at 4 years below the proposed cutoff. This means that a legislator with a 16 year old son is treated if the proposed lower cutoff is 20 years of age. In Column (1), we show that having a draft-eligible son reduces the probability of voting for conscription by over 6 percentage points, from a mean of 0.6. Adding state fixed effects reduces the size of the coefficient and removes its statistical significance. In Column (3) we add legislator fixed effects, finding a treatment effect of -0.104 ($p < 0.01$), or 17% of the mean. Finally, we add vote fixed effects and estimate the full model from equation (4) in Column (4), finding again a large and significant negative treatment effect in the region of 10% of the mean.

In Figure 1, we plot the sensitivity of each empirical model in Table 3 to different lower cutoff ages. In all four models, point estimates smoothly rise from the 1 year lead to the 4 year lead, before falling off slightly at 5 years. This pattern aligns well with theory: with few leads there are treated legislators in the control group, which biases β_3 downward. The treatment effect is maximized with a 4 year lead, as the inclusion of legislators with a 5 year lead reduces the point estimate. Up until the Vietnam War, the mean duration of the draft per war was 3.3 years, so, returning to the example above, it is reasonable that a legislators with a 16 year old son are more concerned about conscription on average than those with a 15 year old son.

In Table 4, we repeat the exercise with added controls for second order polynomials in the age of every child of each legislator. If a legislator does not have a kth order child, we enter a zero for the corresponding age. These zeros are then flexibly captured by k_{iv} in the regression. These age controls ensure that the treatment effect is not picking up nonlinear effects of childrens' age on legislators' voting preferences. The main results are robust to their inclusion, and to the further inclusion of cubic and quartic age controls.[18]

Hawks and Doves In Section 3 we described the process by which we coded 140 votes as either pro- or anti- draft. This is a subset of the 248 draft-related votes in total. The remaining 109 were too ambiguous for us to code with confidence.[19]

Two drawbacks of this approach are (i) the loss of coverage owing to the ambiguity of certain votes; and (ii) the level of discretion that we were required to exercise in determining the direction of each vote. In order to test the robustness of the main results to sample selection and the authors' discretion, we develop an alternative method of measuring pro- or anti-draft preferences among legislators. Drawing on a variety of sources, including historical accounts and archival newspaper articles, we identify at least two well-known foreign policy "hawks" and two well-known foreign policy "doves" during each Congress in both the House and the Senate.[20] We use this information to create a new variable, *Hawk Vote*, which is equal to 1 if the modal vote among the hawks in a given legislator's congress-chamber is in favor of a measure and the modal vote among doves is against it. Similarly, it is equal to 0 if the model dove vote is in favor of a measure and the model hawk vote is against it. The variable is not defined in cases where there is neither a unique mode among hawks nor doves.

In Table 5, we repeat the same four specifications as in Table 3 using *Hawk Vote* as the dependent variable. The sample is drawn from all 248 draft-related votes in our dataset, rather than the 140 that we were able to code. On the other hand, votes for which the variable is not defined are omitted. The results are almost identical; interpreting Column (4), we see that legislators with sons of draft-eligible age are around 9% less like to vote for conscription than those with daughters of comparable age.

In Table 6, 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 778,000 legislator-votes. In assigning legislators to treatment or control groups, we use the draft age cutoffs that were most recently passed in a given chamber. Only in Column (2) where we control for state fixed effects is there a significant treatment effect. In our preferred specifications with

[18]Available on request.

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

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

legislator fixed effects and added vote fixed effects (Columns 4 and 5 respectively), the treatment effect is a precisely estimated zero.

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

Additional robustness In the Appendix Table A1, we show that the main results are qualitatively robust to estimating an equivalent Conditional Logit model.

In Table A2, we run the same four empirical models as in Table 3 on an alternative set of draft-related votes. While our main votes concern the enactment, extension, or reduction of universal military service (e.g., passage of the Selective Service Act, its extension over time, increasing or decreasing the number of draftees, etc.), the votes that we study in Table A2 pertain exclusively to what we call "window votes," which are votes to change the existing upper or lower cutoffs only. We treat these separately because they require an alternative coding procedure. To understand why, say that legislators vote on a measure to change the draft window from 20-30 to 20-35, i.e. raising the upper cutoff from 30 to 35. A legislator with a 32 year old son is clearly negatively impacted, and would be assigned to the treatment group. We denote these legislators as "marginal". However, it is not obvious to see how a legislator with a 22 year old son is affected by this. On the one hand, their son faces a longer duration of eligibility. On the other, the probability that their son is drafted could be reduced. To sidestep this problem, we drop these infra-marginal legislators from the sample, leaving only the marginal group as treated and the extra-marginal legislators as the age control. This leaves a sample of 7,000 legislator-votes only. The results can be seen in Figure A1, where we allow the lead years to vary. The treatment effect is maximized with a two year lead rather than a four year lead as in the main model, perhaps reflecting the fact that window votes tended to occur closer to the ends of wars than the main votes. We present these models with a two year lead in Table A2. Another point to note is that the treatment effect is significant with vote fixed effects but not with legislator fixed effects, which is consistent with the sharply reduced sample size.

Finally, in Table A3, we present results in which the treatment variable is not coded with respect to the proposed cutoffs as determined by vote in question, but rather to the existing cutoffs determined by the most recently approved measure in a given chamber. This introduces measurement error in the treatment variable. The results, although still significant in our preferred specification, highlight the importance of examining the implications of each specific bill rather than basing the treatment status on the prevailing cutoffs.

6 Empathy vs. Self-Interest: Regression Discontinuity

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

One way to test this empathy vs self-interest interpretation is to examine the behavior of legislators who have sons around the upper age cutoff. Under the self-interest interpretation, those who have sons immediately below the cutoff will behave as if they are treated, whereas those who have sons immediately above the upper cutoff will not. Under the empathy interpretation, one would assume that the legislator's concern for draft-eligible sons and their families would remain intact—or at least decline more gradually—as their own son crosses the upper threshold.

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

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

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

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

Results Estimates of ϕ are presented in Table 7. In the first column, we see that a legislator with a son slightly above the upper cutoff is 16 percentage points (or about 26% of the mean) more likely to vote for the draft than one with a son slightly below the upper cutoff. In the second column, we add controls for legislator fixed effects. The results imply that a given legislator is 15.7 percentage points more likely to support the draft after his or her son crosses the upper cutoff relative to before. In columns (3) and (4) we repeat the exercise focusing only on close votes, or those in which the margin of victory was a maximum of 20% of the votes cast. This reduces the sample size by around 70%, and gives equivalent estimates of ϕ at 36 and 38 percentage points respectively—over half of the overall mean.

In Table 8, we test for similar discontinuities with two placebo outcomes: whether the congressman is in the Senate or the House, and whether the congressman is a Democrat or not. Whether in the close vote sample or in the full sample, there is no discontinuous association with the running variable at the upper cutoff.

In Table 9, we repeat the exercise from Table 7 only with the daughter's age replacing the son's age in the running variable.[21] In the full sample of votes there is a negative jump at the cutoff, and in the sample of close votes there is no significant effect. What could be causing this negative jump in the full sample? One possibility is that there is no underlying relationship between the running variable and the outcome, and so the discontinuity that we observe is one of many along the distribution. We can interrogate this by examining RD estimates at a variety of placebo cutoff points to either side of the true cutoff.

In Table 10 we present 10 placebo tests for the son's age effect. These begin at -15 and increase in intervals of three years. The true estimate is the only one that is positive and significant. There is one negative and significant estimate at the cutoff + 12 years. In Table 11, we replace the son's age with the daughter's age. Six of these RD estimates are significant, and of those two are positive and four are negative. These coefficients suggest the there is no clear relationship between a legislator's daughter's age and the probability the they vote in favor of conscription.

Figures Figure 2 presents a visual accompaniment to these results. On the left hand side is a local linear regression discontinuity plot (RD plot) that mimics the first result in Table 7 without any bandwidth restrictions. On the right hand side is the equivalent plot where the running variable is the legislator's daughter's age minus the upper cutoff rather than the son's age. The discontinuity is clear with respect to the son's age, but not with respect to the daughter's. Figure 3 repeats the exercise using a second order polynomial on each side of the cutoffs. Again, there is a positive discontinuity on the left hand side, but not on the right. In Figure 4 we display RD plots that are estimated with controls for legislator fixed effects, again finding a similar picture. In Figure 5, we show RD plots with both placebo outcomes—Senator and Democrat—and the son's age running variable. Neither exhibit a significant jump. Finally, in Figure 6, we plot the density of the son's

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

age running variable, finding no significant evidence of bunching on either side of the cutoff.

Taken together, evidence from legislators' voting behavior around the upper cutoff strongly suggests that self-interest is the motive behind the main results rather than a sense of empathy for the electorate or an enlightened form of ideology. A given legislator is around 16 percentage points more likely to vote in favor of conscription when their crosses the upper age eligibility threshold.

7 Electoral Implications

We state in Section 2 that a sufficiently large shock to θ could cause political leaders to vote to enter conflicts in which the expected social costs exceed the benefits, or to avoid conflicts in which the expected social benefits exceed the costs. So far, we have shown that an exogenous increase in the private costs of conscription for some legislators reduces the likelihood that they vote in favor of enacting it.

What is still unclear is whether, on average, these "treated" legislators with draft-eligible sons better represent their constituents' preferences over conscription than similar "control" legislators with daughters of comparable age. If the treated group better reflects voters' concerns, then it is the control group that exhibits moral hazard in the decision to enact conscription, as they do not internalize the expected costs of their decision. If the treated group does not better reflect voters' concerns, then they do not internalize the net social benefits of conscription.

To determine which of these interpretations are more likely, we examine the electoral outcomes of each group following congresses in which they voted on draft-related measures. In effect, this amounts to replacing the outcome variable in equation (4) with (i) the legislator's next election margin; and (ii) an indicator equal to 1 if the legislator wins their next election.

We present the results of these regressions in Tables 12 and 13. In Table 12, we see that having a draft-eligible son increases the average margin of electoral victory by 8.7 percentage points in the specification with time fixed effects. In Table 13, we see that this translates to an increased likelihood of electoral victory in the region of 12 percentage points (or 17.5% of the mean).[22] Moreover, we show in Table 14 that senators are less likely to vote for conscription in election years, although the estimate is not significant in the presence of vote fixed effects, which exploits the fact that election years are staggered across three groups within the Senate during a given period. In Table 15, we exploit our Hawks and Doves sample and run the same analysis on a larger group of 222,919 senator-votes. We find that senators who are up for reelection are around 1.29 percentage

[22] As the outcome varies at the level of a legislator-term, the inclusion of legislator fixed effects severely limits the power of the tests.

points less likely to vote in favor of conscription than those who are not, although in these specifications the coefficient is not significant in models (1) to (3).

These findings echo recent evidence from Horowitz and Levendusky (2011) that the specter of conscription reduces support for war in the United States. This implies 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 (S_{it} and C_{it}); namely, party career concerns (P_{it}), ideology (F_i), or other unobserved private benefits (M_{it}). In Table 16, we provide some evidence in support of party career concerns as a partial explanation. Each panel represents four separate regressions. In the top panel, we show that the national 'party line'—measured as the share of pro draft votes cast by a given legislator's party—is strongly correlated with voting in favor of the draft. A ten percentage point increase in the party line measure roughly equates to the impact of having a draft-eligible son. In the second column we add an indicator that is equal to 1 if the president is from the same party as a legislator. With legislator fixed effects, this is identified off the switch from Lyndon B. Johnson to Richard Nixon in 1968. With or without fixed effects, this variable has a strong positive association with pro-draft voting.

Taken together, these exercises suggest that, on average, voters have rewarded anti-conscription legislators historically in the U.S., and that control group legislators do not fully internalize the social costs of conflict.

8 Conclusion

In this paper, we test the longstanding hypothesis that wars can occur due to political agency problems: political leaders who do not internalize the costs of conflict are more likely to vote in favor of it. We demonstrate this by compiling data on the voting behavior and family compositions of over 3,300 legislators who served in the U.S. Congress during the four conscription era wars of the 20th century. We find that, 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. We also find that these legislators are 17% more likely to win subsequent elections, suggesting that political representatives on average do not internalize the social costs of declaring war, and that other non-electoral motives play a role in this decision.

Our results provide new evidence that legislators are motivated by private concerns that are external to political or ideological influence. To illustrate this, we exploit a regression discontinuity design to show that a given legislator will increase their support for conscription by around a quarter when their son passes the upper eligibility threshold. We interpret this as evidence against an empathy explanation, in which these legislators develop a concern for the electorate when the costs of conscription become more salient to them.

Overall the results suggest that representative democracy may better enhance social welfare when voters are aware of legislators' private incentives, and vote often enough to impose accountability on important legislator decisions. Exploring private incentives of legislators in other policy domains remains a fruitful avenue for future research.

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Figures

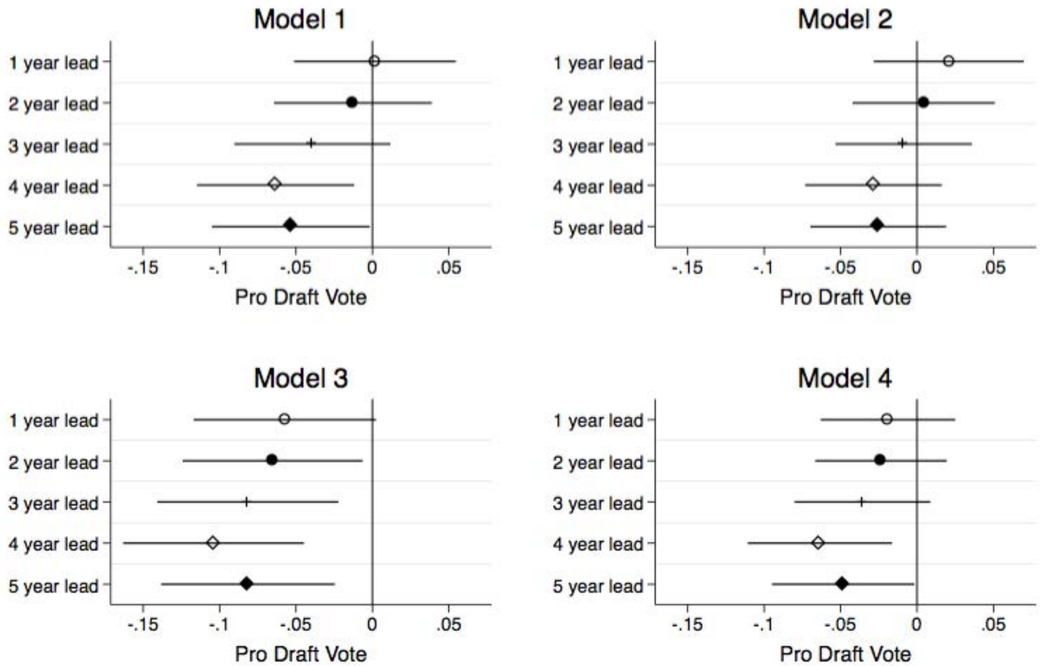

Figure 1: Impact of having draft-eligible son on voting for draft at various lower thresholds

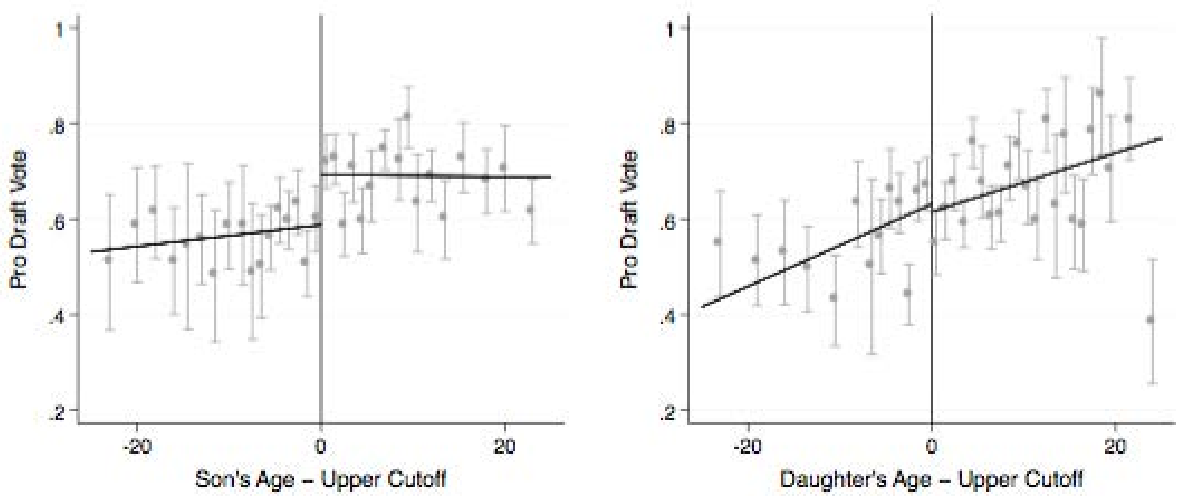

Figure 2: Regression Discontinuity at the Upper Cutoff

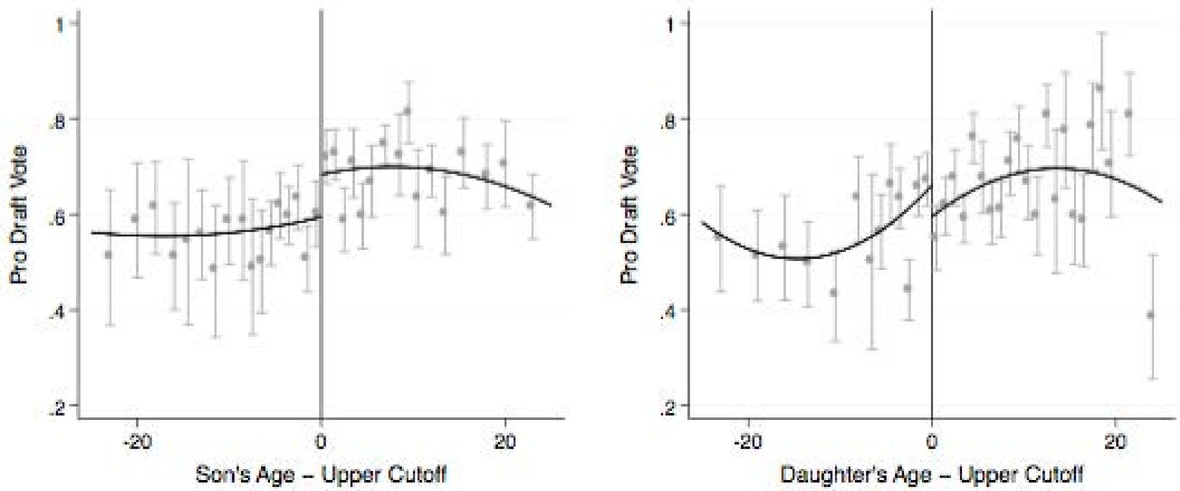

Figure 3: Regression Discontinuity at the Upper Cutoff: Quadratic

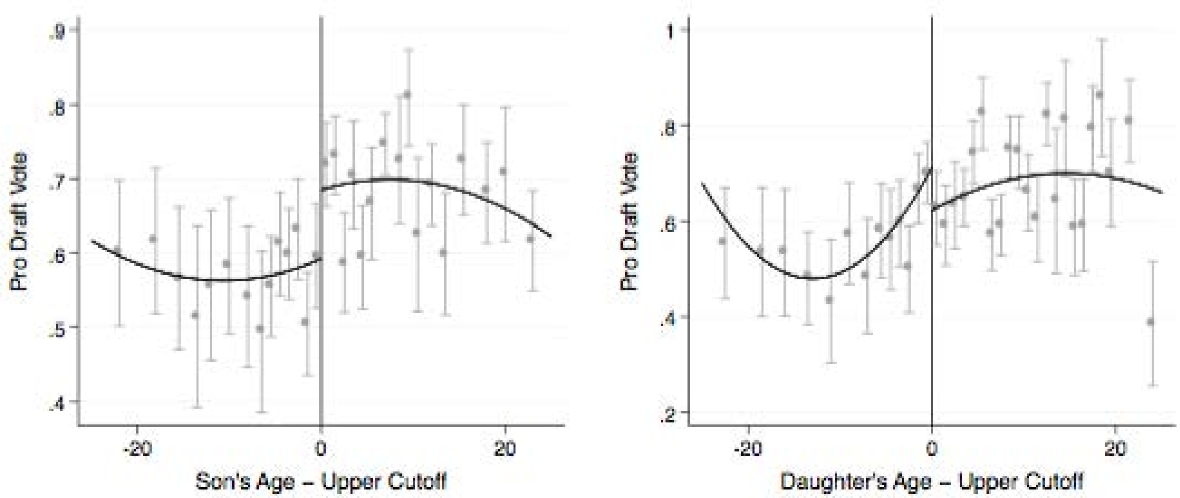

Figure 4: Regression Discontinuity at the Upper Cutoff: Within Legislator Variation

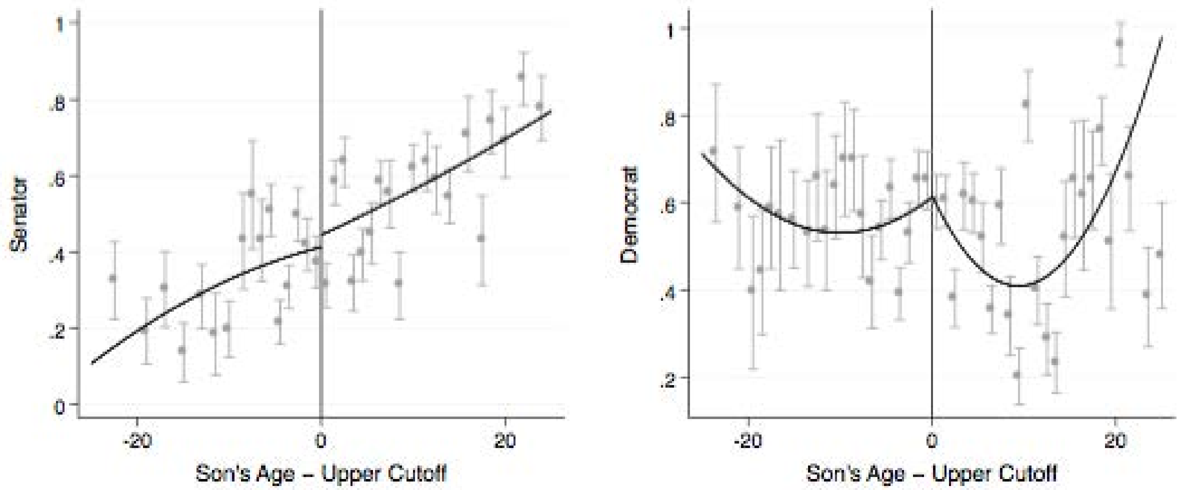

Figure 5: Regression Discontinuity at the Upper Cutoff: Placebo Outcomes

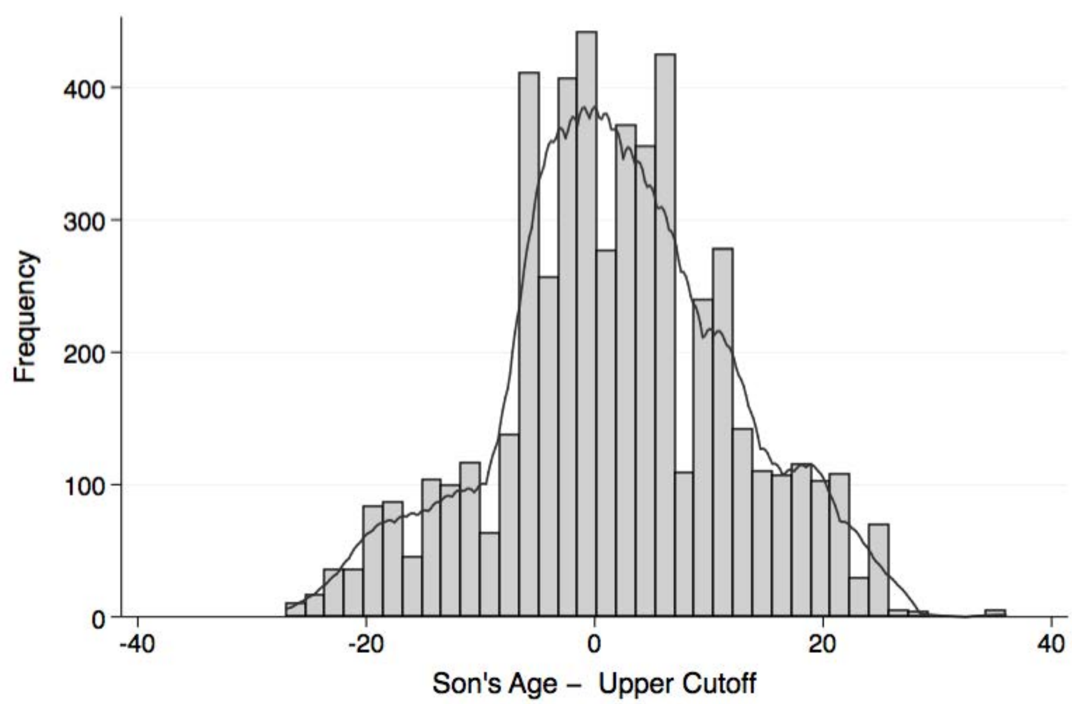

Figure 6: Regression Discontinuity Plots: Running Variable Density

Tables

Table 1: Summary Statistics: Votes

	Draft Votes (Sample)			Draft Votes (All)		War Votes	
Congress	Votes	Pro Draft	Margin	Votes	Margin	Votes	Margin
Senate	(1)	(2)	(3)	(4)	(5)	(6)	(7)
93	2	0.61	0.32	2	0.32	262	0.39
92	34	0.49	0.35	58	0.39	288	0.39
91	2	0.63	0.40	3	0.35	161	0.46
90	7	0.79	0.77	9	0.79	148	0.55
89	1	0.93	0.86	3	0.55	132	0.52
88	.	.	.	.	.	128	0.47
82	8	0.61	0.38	12	0.46	151	0.36
79	6	0.55	0.46	13	0.39	67	0.47
77	12	0.52	0.30	21	0.31	93	0.40
76	13	0.50	0.39	22	0.35	90	0.39
66	1	0.11	0.01	2	0.06	242	0.33
65	20	0.43	0.53	33	0.46	211	0.45
Sum	106			178		1973	
Mean	9.64	0.56	0.44	16.18	0.40	164.42	0.43
SD	10.03	0.21	0.23	17.08	0.18	71.32	0.07
House							
93	.	.	.	.	.	176	0.36
92	10	0.60	0.34	11	0.35	115	0.38
91	2	0.74	0.47	2	0.47	78	0.32
90	2	0.85	0.70	2	0.70	77	0.39
89	1	0.88	0.77	1	0.77	75	0.40
88	1	0.88	0.77	1	0.77	44	0.36
82	2	0.80	0.60	3	0.55	46	0.31
79	2	0.42	0.03	9	0.15	41	0.30
77	5	0.47	0.28	8	0.18	60	0.39
76	4	0.53	0.15	5	0.21	45	0.28
66	.	.	.	.	.	77	0.34
65	5	0.70	0.49	12	0.37	67	0.31
Sum	34			54		901	
Mean	3.4	0.69	0.46	5.4	0.45	75.08	0.35
SD	2.76	0.17	0.26	4.25	0.24	38.10	0.04
Combined							
Total	140	0.58	0.18	232	0.17	2874	0.21
SD		0.49	0.12		0.12		0.17

Table 2: Summary Statistics: Family Composition

		Children		Sons		Daughters		Draft Age	
Congress	Legislators	Any	N.	Any	N.	Any	N.	Any Son	Any Dtr.
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
SD	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
SD	6.36	0.37	1.79	0.47	1.21	0.48	1.15	0.41	0.40
Combined									
Total	2287	0.85	2.37	0.68	1.23	0.65	1.15	0.22	0.21
SD		0.35	1.75	0.47	1.17	0.48	1.14	0.41	0.41

Table 3: Impact of having draft-eligible son on pro-draft vote; main votes

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Any son × draft age	-0.0634**	-0.0285	-0.1040***	-0.0635**
	(0.0310)	(0.0269)	(0.0356)	(0.0284)
Any children × draft age	-0.0204	-0.0427*	0.0141	0.0150
	(0.0279)	(0.0238)	(0.0310)	(0.0278)
Any son	0.0125	-0.0080	0.1470	-0.0224
	(0.0309)	(0.0256)	(0.1016)	(0.0842)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.604	0.604	0.604	0.604
Observations	18823	18823	18658	18658

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 4: Main result with added controls for 2nd order polynomial in each child's age

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Any son × draft age	-0.0614*	-0.0225	-0.0863**	-0.0508*
	(0.0313)	(0.0272)	(0.0353)	(0.0277)
Any children × draft age	-0.0290	-0.0477*	0.0092	0.0188
	(0.0311)	(0.0281)	(0.0314)	(0.0289)
Any son	0.0139	-0.0104	0.1153	-0.0458
	(0.0308)	(0.0257)	(0.1023)	(0.0893)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.604	0.604	0.604	0.604
Observations	18823	18825	18660	18660

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 5: Hawks and Doves method with draft category votes

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Any son × draft age	-0.0594*	-0.0167	-0.0734**	-0.0566***
	(0.0330)	(0.0268)	(0.0301)	(0.0082)
Any children of draft age	-0.0010	-0.0229	0.0410	0.0345
	(0.0289)	(0.0235)	(0.0280)	(0.0247)
Any son	0.0385	0.0229	0.1099	0.0274
	(0.0309)	(0.0243)	(0.0930)	(0.0823)
Vote FE	No	No	No	Yes
Congressman FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.623	0.623	0.626	0.626
Observations	20175	20175	19970	19969

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 6: Hawks and Doves method with non-draft category votes

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Any son × draft age	0.0233	0.0339***	0.0054	-0.0008
	(0.0148)	(0.0117)	(0.0111)	(0.0074)
Any children of draft age	-0.0336**	-0.0377***	-0.0063	-0.0032
	(0.0137)	(0.0106)	(0.0099)	(0.0074)
Any son	0.0028	-0.0115	0.0128	0.0319
	(0.0175)	(0.0120)	(0.0338)	(0.0341)
Vote FE	No	No	No	Yes
Congressman FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.455	0.455	0.455	0.455
Observations	777911	777911	777911	777829

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 7: Regression discontinuity at the upper cutoff

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
RD estimate at upper cutoff	0.1601***	0.1570***	0.3572***	0.3792***
	(0.0446)	(0.0467)	(0.1268)	(0.1307)
Bandwidth	CCT	CCT	CCT	CCT
Running variable	Son age	Son age	Son age	Son age
Legislator FE	No	Yes	No	Yes
Vote Sample	All	All	Close	Close
Observations	5187	5100	1577	1534

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

Table 8: Regression discontinuity with placebo outcomes

	Senator		Democrat	
	(1)	(2)	(3)	(4)
RD estimate at upper cutoff	0.0078	0.0963	-0.0229	-0.1073
	(0.0426)	(0.0935)	(0.0811)	(0.0822)
Bandwidth	CCT	CCT	CCT	CCT
Running variable	Son age	Son age	Son age	Son age
Vote Sample	All	Close	All	Close
Observations	5187	1577	5157	1565

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

Table 9: Regression discontinuity with daughter age

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

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

Table 10: Regression discontinuity at the upper cutoff; placebo cutoffs with son age

	Pro Draft Vote										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
RD estimate	-0.0163	-0.1273	-0.1441	0.1088	-0.0472	0.1601***	0.0776	0.0852	0.0845	-0.2630***	0.1028
	(0.1360)	(0.1097)	(0.1033)	(0.0670)	(0.0529)	(0.0446)	(0.0538)	(0.0528)	(0.0684)	(0.0576)	(0.0904)
Cutoff	-15	-12	-9	-6	-3	0	3	6	9	12	15
Bandwidth	3.73	4.60	4.54	6.25	4.56	5.92	4.42	4.74	4.01	4.82	4.26
Running variable	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age	Son age
Observations	5187	5187	5187	5187	5187	5187	5187	5187	5187	5187	5187

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

Table 11: Regression discontinuity at the upper cutoff; placebo cutoffs with daughter age

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

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

Table 12: Impact of draft-eligible son on next election margin

	Next Election Margin		
	(1)	(2)	(3)
Any son × draft age	0.0839**	0.0550*	0.0866***
	(0.0318)	(0.0302)	(0.0303)
Any children × draft age	-0.0791**	-0.0619*	-0.0951***
	(0.0326)	(0.0319)	(0.0315)
Any son	-0.1069***	-0.0643*	-0.1094***
	(0.0343)	(0.0325)	(0.0334)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.170	0.170	0.170
Observations	6436	6436	6436

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 13: Impact of draft-eligible son on next election victory

	1(Reelected)		
	(1)	(2)	(3)
Any son × draft age	0.1200*	0.0936*	0.1248**
	(0.0652)	(0.0487)	(0.0610)
Any children × draft age	-0.0379	-0.0328	-0.0542
	(0.0635)	(0.0472)	(0.0607)
Any son	-0.1380**	-0.0942**	-0.1395**
	(0.0634)	(0.0467)	(0.0619)
Legislator FE	No	No	No
Vote FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.712	0.712	0.712
Observations	6436	6436	6436

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$.

Table 14: Senate election proximity and pro-draft vote; main and threshold votes

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Election year	-0.0084	-0.0176	-0.0561**	-0.0274
	(0.0334)	(0.0314)	(0.0256)	(0.0290)
Any son × draft age	-0.1020**	-0.0478	-0.0811**	-0.0613*
	(0.0432)	(0.0412)	(0.0389)	(0.0366)
Any children × draft age	0.0402	0.0059	0.0567	0.0285
	(0.0396)	(0.0361)	(0.0354)	(0.0361)
Any son	0.0846*	0.0489	0.5659***	0.1789
	(0.0448)	(0.0400)	(0.1980)	(0.1747)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.514	0.514	0.514	0.514
Observations	10762	10762	10743	10743

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 15: Senate election proximity on hawkish voting; full HD sample

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Election year	-0.0127	-0.0032	0.0034	-0.0129**
	(0.0104)	(0.0089)	(0.0080)	(0.0062)
Any son × draft age	-0.0543	0.0073	-0.0091	-0.0226
	(0.0339)	(0.0241)	(0.0175)	(0.0145)
Any children of draft age	0.0058	-0.0265	-0.0080	-0.0050
	(0.0332)	(0.0238)	(0.0191)	(0.0166)
Any son	0.0339	0.0231	0.0119	0.0959**
	(0.0437)	(0.0276)	(0.0785)	(0.0388)
Vote FE	No	No	No	Yes
Congressman FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.514	0.514	0.514	0.514
R squared	0.090	0.158	0.253	0.389
Observations	222998	222998	222998	222919

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 16: Party influence

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Any son × draft age	-0.0638**	-0.0342	-0.0955***	-0.0554**
	(0.0295)	(0.0256)	(0.0339)	(0.0261)
Party line	0.0073***	0.0067***	0.0056***	0.0060***
	(0.0007)	(0.0007)	(0.0009)	(0.0010)
Any son × draft age	-0.0546*	-0.0257	-0.0939***	-0.0575**
	(0.0290)	(0.0251)	(0.0337)	(0.0267)
President party	0.2390***	0.2110***	0.1373***	0.1114***
	(0.0283)	(0.0259)	(0.0256)	(0.0224)
Any son × draft age	-0.0598**	-0.0319	-0.0920***	-0.0546**
	(0.0294)	(0.0253)	(0.0333)	(0.0259)
Party line	0.0054***	0.0052***	0.0048***	0.0057***
	(0.0008)	(0.0008)	(0.0010)	(0.0011)
President party	0.1081***	0.0848***	0.0620***	0.0222
	(0.0202)	(0.0190)	(0.0214)	(0.0205)
Vote FE	No	No	No	Yes
Legislator FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
N. of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.607	0.607	0.607	0.607
Observations	18728	18728	18563	18563

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. Party line is the share of pro-draft votes cast by members of a given legislator's party. President party indicates that the sitting president represents the same party as the given legislator.

Appendix

Appendix Figures

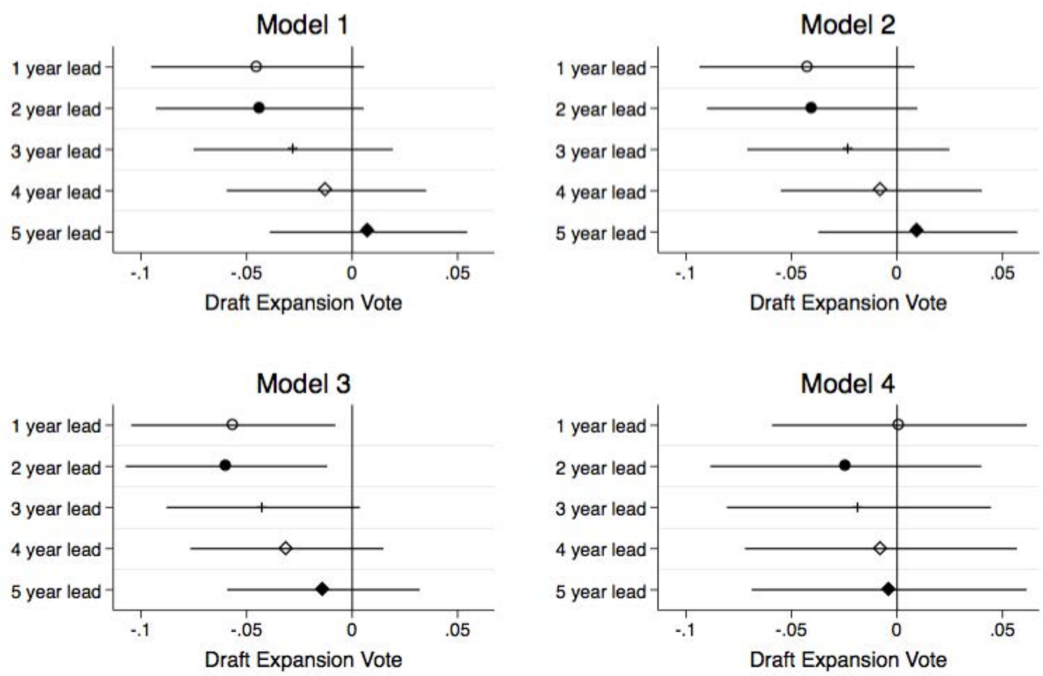

Figure A1: Impact of having draft-eligible son on window votes at various lower thresholds

Appendix Tables

Table A1: Conditional Logit; main votes

	Pro Draft Vote		
	(1)	(2)	(3)
Any son × draft age	-0.2698**	-0.1235	-0.6447***
	(0.1301)	(0.1227)	(0.2497)
Any children × draft age	-0.0908	-0.2051*	0.0231
	(0.1247)	(0.1157)	(0.2436)
Any son	0.0528	-0.0353	0.6371
	(0.1328)	(0.1167)	(0.5181)
Legislator FE	No	No	Yes
State FE	No	Yes	Yes
Number of children FE	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Mean dep. var.	0.604	0.604	0.569
Observations	18809	18809	13910

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

Table A2: Impact of having draft-eligible son on pro-draft vote; window votes

	Pro Draft Expansion Vote			
	(1)	(2)	(3)	(4)
Any son × draft window	-0.0438	-0.0402	-0.0596**	-0.0243
	(0.0299)	(0.0303)	(0.0290)	(0.0390)
Any children × draft window	0.0972***	0.0934***	0.0280	0.0142
	(0.0250)	(0.0255)	(0.0246)	(0.0332)
Any son	0.0039	-0.0127	-0.0172	0.2495**
	(0.0240)	(0.0231)	(0.0214)	(0.1103)
Legislator FE	No	No	No	Yes
Vote FE	No	No	Yes	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.512	0.512	0.512	0.519
Observations	7088	7088	7088	6868

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

Table A3: Impact of having draft-eligible son on pro-draft vote; existing rather than proposed draft cutoffs

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Any son × draft age	-0.0532*	-0.0210	-0.0080	-0.0407*
	(0.0287)	(0.0258)	(0.0260)	(0.0238)
Any children × draft age	0.0401	0.0213	-0.0247	0.0168
	(0.0417)	(0.0411)	(0.0234)	(0.0223)
Any son	0.0054	-0.0123	-0.0182	0.0388
	(0.0241)	(0.0221)	(0.0206)	(0.0857)
Vote FE	No	No	Yes	Yes
Legislator FE	No	No	No	Yes
State FE	No	Yes	Yes	Yes
Number of children FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Mean dep. var.	0.579	0.579	0.579	0.578
Observations	26006	26008	26008	25914

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