No Kin In The Game: Moral Hazard and War in the U.S. Congress

Eoin McGuirk[*] Nathaniel Hilger[†] Nicholas Miller[‡]
Yale University Stripe, Inc. Dartmouth College

January 14, 2018

For an updated draft, please click here

Abstract

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

---

[*]Corresponding author; email: eoin.mcguirk@yale.edu; Department of Economics, Yale University. [†]email: nhilger@stripe.com. [‡]email: nicholas.l.miller@dartmouth.edu; Department of Government, Dartmouth College. We thank Motaz Al-Chanati, Jenna Anders, Victor Brechenmacher, Kian Ivey, Vanessa Labrador, Andrew Li, and 36 students from the Fall 2016 Econ 115 class at Yale for their excellent research assistance. We also thank Jesse Shapiro, Mushfiq Mobarak, Fabrizio Zilibotti, Daniel Posner, Nico Voigtlaender, Kelsey Jack, Ray Fisman, Michael Kremer, Nicholas Ryan, Daniel Keniston, Ebonya Washington, Greg Huber, Ceren Baysan, James Thomas, and participants at BIARI (Brown University), NEUDC (Tufts University), Yale University's Leitner Political Economy seminar, and Yale's Development Economics workshop for helpful comments and conversations. All errors are ours.

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 the benefits without internalizing the costs, then war will be oversupplied (Jackson and Morelli, 2007).

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

We exploit a natural experiment that is permitted by the nature of conscription-era warfare in the United States. Legislators who had sons within the age boundaries of the draft were more likely to be exposed to the direct costs of conflict than legislators who had only daughters of the same age. Our main identifying assumption is that these two groups would otherwise vote identically—in other words, the gender of a given draft-age child is as good as random. Our identification strategy is also bolstered by the fact that the proposed draft age boundaries 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. We also find that these legislators are more likely to win reelection to the following Congress, and that legislators on average are less likely to vote in favor of conscription during election years. These findings are consistent with a model of political agency in which voters reward politicians who vote against conscription and punish politicians who do not internalize its social costs.

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

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

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

[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.

against the doves on a given measure, it is determined as a hawkish 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.

To rationalize these main findings, we turn to a model of political agency that combines elements of moral hazard and adverse selection (Besley, 2006). 'Good' politicians pursue measures that are in the voters' interest, and voters respond by reelecting them. 'Bad' politicians decide either to mimic good types in order to win reelection, or to vote against citizens' interests and lose reelection. This decision is determined in part by the value of private rents that accrue to the politician if she votes against the electorate's wishes. Typically, researchers do not observe exogenous variation in private rents that politicians can capture through legislative voting. This presents a barrier to empirically testing this type of model. However, in our setting we do observe an exogenous 'wedge' between the private benefits of conscription for legislators with draft-eligible sons versus those with daughters of comparable age. This gives us testable implications of the theory that we bring to the data. We find that those with draft-eligible sons are around 10 percentage points more likely to win reelection, suggesting that the draft was broadly unpopular on average. Moreover, we show that this effect is reversed when the draft is more popular during the earliest period in our sample. We also provide evidence that pressure to tow the national party line is likely to be one motive that impels politicians to vote in favor of conscription despite its lack of broad support among voters.

The paper contributes principally to two distinct bodies of research. The first is an emergent empirical literature that connects credible identification strategies to theoretical work on the origins of violent conflict. These foundations are based on contest models in which two sides fight to control total resources. Each side allocates their own resources between production and appropriation, and the probability of victory is determined by the relative effectiveness of fighting technology (Haavelmo, 1954; Hirshleifer, 1988; Garfinkel, 1990; Skaperdas, 1992). One limitation of contest models is that they fail to account for bargained settlements. Wars are risky and destructive, and so it is necessary to understand why they are avoided in some cases but not in others.[2]

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

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

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

[4]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).

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

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

2 Political Economy of Legislative Voting in a Democracy

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

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

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

where $V_{it} \in \{0, 1\}$ is the legislator i's vote at time t; $M_{it} \in [0, 1]$ is the ideal point in a given issue space of the voters in the legislator's electorate; $C_{it} \in [0, 1]$ is the equivalent ideal point among the legislator's supporters; $P_{it} \in [0, 1]$ is the ideal point of the legislator's national party; and $F_i \in [0, 1]$ is the legislator's ideological bliss point, which is assumed to 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]

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

[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 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.

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]

[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

In this paper, we address an alternative potential explanation for the absence of evidence on the role of private influences in legislative voting: the significant empirical challenge inherent in detecting such an effect (as noted by de Figueiredo and Richter, 2014). A causal identification strategy would involve estimating the effect of an exogenous change in the private net benefits of voting on the legislative choices of a politician, conditioning on politician fixed effects to hold ideology constant. While there exists persuasive evidence that, for example, campaign contributions can "buy" time with a legislator (Kalla and 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 evidence that individual legislators respond to changes to their private net benefits of voting on a given issue. Yet, such a view would be consistent with 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:

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

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 is:

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

that their impact operates through any of the elements in the model rather than through a private quid pro quo channel.

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

Implications for Conflict

Much of the theoretical literature on violent conflict treats actors as unitary decision-makers.[11] Implicit in this approach is the assumption that the costs and benefits of conflict are shared among members of each group. The politician's solution in (3) relaxes this assumption. If, on a given vote, a shock to R_{it} is sufficiently large, then it is possible a leader may vote to enter conflicts in which the expected social costs exceed the benefits, or to avoid conflicts in which the expected social benefits exceed the costs. The critical condition in either case is that the private payoff through θ offsets the influences that operate through the other channels, or $V^*_{it}(M_{it}, C_{it}, P_{it}, F_i, R_{it}) = (1 - V^*_{it}(M_{it}, C_{it}, P_{it}, F_i))$.[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. Jackson and Morelli (2007) develop the concept formally, showing that "political bias"—or the extent to which the pivotal policy maker benefits from conflict relative to the rest of the population—can cause war even in the presence of enforceable transfers between potential belligerents.

Other papers that relax the assumption of unitary actors do so by modeling the politics of conflict from the perspective of leaders (De Mesquita and Siverson, 1995; Tarar, 2006; 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. For example, Kuperman

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

[12] 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.

(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 the present paper, we make the related argument that politicians who are protected from the risks of conflict are more likely to support it.

Testing Implications

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

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

3 Data and Background

Structure Data in our main analysis is at the level of a legislator-vote. Each observation contains information on how the legislator voted and on a range of legislator characteristics, including the number and gender of their children at the time of voting. In our full dataset, which includes votes analyzed for robustness and auxiliary exercises, there are 3,693 legislators, 9,210 children, and around 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

[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.

passed too. In such a case, there is a danger of misclassifying a pro-draft measure as an anti-draft one.

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

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

The results of this data collection exercise can be seen in Table 1, where we document draft-related votes only in Congresses in which we found relevant votes that we could determine as pro- or anti-draft. In total, we code the direction of 140 votes—106 in the Senate and 34 in the House (Column 1). In the second column we present our main dependent variable: *Pro Draft* is equal to 1 if a legislator voted in favor of conscription (aye if it was a pro draft vote, or 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 in which we did not successfully code any votes. We cannot present the same information for the outcome variable, but we do present the average margin to facilitate a comparison between Columns (2) and (3). The respective mean margins are 0.18 and 0.17, suggesting that there is no obvious difference between votes that we could and could not code. 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*.

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

[16] See www.ancestry.com.

means are 0.23 and 0.21; meaning that over one fifth of legislator-votes on draft bills are cast by legislators with sons in the draft window. Reassuringly, the equivalent figures for daughters are the same.

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

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

On the first issue, we can learn much from archived newspaper reports about the nature of the debate surrounding conscription. For example, on the first day of the World War I draft bill debate on April 23, 1917, Representative Julius Kahn, who lead the Administration's fight for conscription, invited Captain Percy Benson of the Somerset Yeomanry, a regiment of the British Army, to speak to the House Committee on Military Affairs about "England's mistakes." Benson listed five main reasons why the US should pursue conscription. First, he believed that the obligation to defend a democracy ought to be equal; second, he argued that the draft secured "infinitely greater efficiency", insofar as the government, through the selective process, could ensure that a sufficient number of men could remain in essential industries such as coal mining, shipbuilding, and farming during the war; third was the "economy" of conscription, which allowed the government to call up single men rather than married ones with dependents and potentially expensive allowances and pensions; the fourth point was "continuity of effort", or the direct efficiency of securing a sufficient number of soldiers with maximum certainty in order to win the war; and the fifth point was to ensure that "slackers" pull their weight and, just as importantly, that those who "were called slackers who were not slackers at all" would be protected from such terms of opprobrium.[17]

[17] "Draft Bill Debate is to Begin Today," *The New York Times*, April 23 1917. The New York Times

In one form or another, many of these points were repeated over the course of draft-era warfare in the US Congress, although, as we discuss in detail in Section 7, the necessity of the draft as a means of securing a sufficient number of soldiers waned as conflict technology became more capital intensive over time (Fordham, 2016). Other arguments against the draft were varied. Unions were consistently opposed to conscription as they viewed it as a form of class exploitation, most likely because the alternative—raising military pay—would increase unions members' bargaining power and wages.[18] Relatedly, others viewed the draft as an opportunity for special interests to profit,[19] while isolationists and pacifists opposed conscription as part of their general opposition to interventionist foreign policy.[20] One consistent argument agains the draft relates to agency frictions of the type that we seek to detect in our main analysis. Perhaps the most notable example of this concerns an amendment proposed by Congressman Hubert Stephens of Mississippi to make members of Congress themselves subject to the draft during World War I. Speaking in favor of the amendment, Congressman Frank Clark of Florida argued that "[i]t would be a shame, a cowardly thing [...] for Congress to declare war and then send young boys to do the fighting, while our precious hides are exempt." Mr. Stephens insisted that there were a "a number of vigorous men on this floor who are fit for service at the front." The amendment was defeated, 130 to 86.[21]

On the second issue, we compile in Table 4 data on U.S. draft registrants, draft de-

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)

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

[19] 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."

[20] 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.)

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

ployments, 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. One fifth of legislators, therefore, had a non-trivial role in determining the risks faced by their own sons in battle.

4 Estimation

Our main specification is as follows:

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

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;[22] $\mathbb{X}_{iv}$ is a vector of time varying controls, including the legislator's age, age squared, and terms in office. In regressions without legislator and 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 show that the results are qualitatively unchanged when estimated in a conditional (fixed effects) 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

[22]Note that this is not an interaction variable in practice, as it is possible to have a son and a child of draft age without having a son of draft age.

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 5 presents the main results with the lower bound set at 4 years below the proposed cutoff. This means that a legislator with a 16 year old son is treated if the proposed lower cutoff is 20 years of age. In Column (1), we show that having a draft-eligible son reduces the probability of voting for conscription by 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 5 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. 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 6, 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.[23]

In Table 7, we examine whether or not the treatment variable is larger in a sample of close votes. Legislators' decisions are more likely to be pivotal in closer votes. We would therefore expect those with draft-eligible sons to be more likely to oppose the draft in narrow votes relative to landslide votes. Defining close votes as those in which the margin was less than 60-40, we that the treatment effect in the specification with legislator fixed effects increases from around 10 percentage points to 15 percentage points—enough to convert the average legislator from pro- to anti-draft.

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

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

[23] Available on request.

[24] 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.

foreign policy "hawks" and two well-known foreign policy "doves" during each Congress in both the House and the Senate.[25] We use this information to create a new variable, *Hawk Vote*, which is equal to 1 if the modal vote among the hawks in a given legislator's Congress-chamber is in favor of a measure and the modal vote among doves is against it. Similarly, it is equal to 0 if the model dove vote is in favor of a measure and the model hawk vote is against it. The variable is not defined in cases where there is neither a unique mode among hawks nor doves.

In Table 8, we repeat the same four specifications as in Table 5 using *Hawk Vote* as the dependent variable. The sample is drawn from all 248 draft-related votes in our dataset, rather than the 140 that we were able to code. On the other hand, votes for which the variable is not defined are omitted. 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 9, we repeat the exercise on the universe of votes in draft-era Congresses that are unrelated to the draft. This gives a sample of almost 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 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 5 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

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

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. This was an issue much debated in Congress at the time. Reporting on one such debate in 1940, the New York Times writes:

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

To sidestep this problem, we drop these infra-marginal legislators from the sample, leaving only the marginal group as treated and the extra-marginal legislators as the control along the age dimension. This leaves a sample of 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

[26] "House Votes Conscription," *The New York Times*, September 8th, 1940.

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 returns to voting.

One way to test this empathy vs self-interest interpretation is to examine the behavior of legislators who have sons around the upper age cutoff. Under the self-interest 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 (i.e., within-window) son closest to the cutoff, as it is more relevant to the legislator's behavior.

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

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

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 10. 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 11, we test for similar discontinuities with two placebo outcomes: whether the legislator is in the Senate or the House, and whether the legislator is a Democrat or not. Whether in the close vote sample or in the full sample, there is no discontinuous association with the running variable at the upper cutoff.

In Table 12, we repeat the exercise from Table 10 only with the daughter's age replacing the son's age in the running variable.[27] In the full sample of votes there is a *negative* jump

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

at the cutoff, and in the sample of close votes there is no significant effect. One possible explanation for this negative effect is that there is no underlying relationship between the running variable and the outcome, and so the discontinuity that we observe is one of many along the distribution. We can interrogate this by examining RD estimates at a variety of placebo cutoff points to either side of the true cutoff.

In Table 13 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 14, 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 that they vote in favor of conscription.

Figures Figure A2 in the Appendix presents the visual analogue of these results. As the data-driven bandwidths are rarely above 5 years, we additionally present in Figure 3 a local linear regression discontinuity plot (RD plot) that mimics the first result in Table 10 without any bandwidth restrictions. On the right hand side is the equivalent plot where the running variable is the legislator's daughter's age minus the upper cutoff rather than the son's age. The discontinuity is clear with respect to the son's age, but not with respect to the daughter's. Figure 4 repeats the exercise using a second order polynomial on each side of the cutoffs. Again, there is a positive discontinuity on the left hand side, but not on the right. In Figure 5, we show RD plots with both placebo outcomes—Senator and Democrat—and the son's age running variable. Neither exhibit a significant jump. Finally, in Figure 6, we plot the density of the son's age running variable, finding no significant evidence of bunching on either side of the cutoff.

Taken together, evidence from legislators' voting behavior around the upper cutoff strongly suggests that self-interest is 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 her son crosses the upper age eligibility threshold.

7 Political Agency and Voter Behavior

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

In this section, we endogenize the behavior of the electorate in order to better 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.[29] Informational problems can arise if politicians can hide effort or motives. We consider two general types of models that align with our main results: (i) 'pure' moral hazard in which all politicians maximize private rents; and (ii) moral hazard with adverse selection, in which bad politicians maximize private rents and good politicians enact voters' preferred policies. We propose an empirical test of theoretical implications to determine which model more closely fits the data.

[28] 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.

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

7.1 Pure moral hazard

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

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

7.2 Moral hazard with politician types

The modern workhorse political agency model presented in Besley (2006) combines elements of moral hazard from first generation political agency models with elements of models that allow for different politician types. In these, elections serve the twin purposes of restraining politician behavior, as above, and selecting 'good' politicians who care more about voter welfare. In chasing private rents, 'bad' politicians can also mimic good ones in order to disguise their type to the electorate.[31] Below, we briefly describe a basic version of the model in order to consider its implications in our setting.

Environment

Consider two time periods denoted by $t \in \{1, 2\}$. In period t, N politicians vote against or in favor of conscription: $V_t \in \{0, 1\}$. The state of the world $S_t \in \{0, 1\}$ determines which

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

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

policy is preferred by voters. If $\sum_i^N \frac{A_i}{N} > 0.5$, where

$$A_i = \begin{cases} 1 & \text{if } V_t = S_t \\ 0 & \text{otherwise,} \end{cases}$$

then voters receive a payoff Δ; otherwise their payoff is zero. There are two types of politician $i \in \{g, b\}$. We define type g as a good politician for whom the weight placed on private (non-ideological) returns to voting is zero ($\theta_g = 0$) and b as a bad politician for whom this weight is strictly positive ($\theta_b > 0$). Voters do not observe these types. 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 $E + (\Delta \mid \sum_i^N \frac{A_i}{N} > 0.5)$, and always choose $V_t = S_t$. Bad politicians receive a private benefit of $r \in (0, R)$ from choosing a policy $V_t = (1 - S_t)$, where r is drawn independently from a distribution whose CDF is $G(r)$. The mean value of r is μ, and we have shown in our main results above that $R > \beta(\mu + E)$, where β is a discount factor. In other words, R can be sufficiently large that bad politicians choose policies that do not align with voter preferences.

The timing of the game is as follows. Nature determines the type of politician and the state of the world at the beginning. Once in office, politicians observe the draw r_1 and select $V_t(S, i)$. Voters observe only V_t 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.

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., $V_2(S, g) = S_2$ and $V_2(S, b) = (1 - S_2)$. In Period 1, good politicians choose $V_1(S, g) = S_1$. The more interesting problem concerns the bad politician, who must weight the value of her private benefit against the present value of mimicking a good politician in order to receive rents in the second period. Let λ represent the probability that a bad politician mimics a good one in Period 1. Voters' belief that a politician is good conditional on observing $V_1(S,i) = S_1$ is:

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

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

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

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

Proposition 1.

(i) Good politicians always choose $V_t(S,g) = (S_t)$.

(ii) Bad politicians always choose $V_2(S,b) = (1 - S_2)$ in Period 2.

(iii) Bad politicians will choose $V_1(S,b) = (S_1)$ in Period 1 if they earn sufficiently small private rents $r_1 < r* \equiv \beta(\mu + E)$ from voting against the electorate's preferred policy.

(iv) All politicians who choose $V_1(S,i) = (S_1)$ in Period 1 are reelected.

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

Conscription and heterogeneous rent shocks Allow the private rent shock to be characterized as follows: one subgroup of politicians receive r_1^h and another receives r_1^l, where $r_1^h > r_1^l$. This implies that there is an exogenous difference in r_1 between politicians that cuts across both good and bad types. Good types select $V_1(S, g) = S_1$ irrespective of r_1, and are reelected. If $r_1^h > r_1^l > \beta(\mu + E)$, then all bad types choose $V_1(S, b) = (1 - S_1)$ and are not reelected. Similarly, if $\beta(\mu + E) > r_1^h > r_1^l$, then all bad types choose $V_1(S, b) = S_1$ and are reelected. However, if $r_1^h > \beta(\mu + E) > r_1^l$, then bad types with r_1^h select $V_1(S, b \mid r_1^h) = (1 - S_1)$ and are not reelected, while bad types with r_1^l select $V_1(S, b \mid r_1^l) = S_1$ and are reelected. With heterogeneous rent shocks, therefore, there exists an equilibrium in which some bad types pursue private rents and are voted out of office, while other bad types mimic good types and survive to Period 2 because their rent shock r_1^l is worth less than than the present value of the second period returns.

Applying this logic to the case of conscription votes, we can interpret having a draft-eligible son as a source of heterogeneity in the private rent shock. For example, consider the case in which conscription is broadly unpopular with voters, i.e., $S_1 = 0$. Bad types without draft-eligible sons observe $r_1^h > \beta(\mu + E)$, meaning that their private benefit of voting in favor of conscription exceeds the present value of survival to Period 2.[32] However, bad types with draft-eligible sons observe $r_1^l < \beta(\mu+E)$, and instead mimic good types by choosing $V_1(0, b \mid r_1^l) = 0$ 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.*

[32] 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.

7.3 Testing implications

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

$$E_{iscj} = v_{vcj} + k_{ivcj} + \beta_1 son_{ivcj} + \beta_2 draft_{ivcj} + \beta_3 son \times draft_{ivcj} + \zeta \mathbb{X}_{ivcj} + \epsilon_{isvcj}, \quad (6)$$

where E_{iscj} measures election outcomes for legislator i in state s following congressional session c in chamber j. The treatment variable $son \times draft_{ivcj}$ indicates that the legislator has a draft-eligible son in vote v, which is contained in congressional session c in chamber j. The election outcomes E_{iscj} are (i) a binary variable indicating that the legislator was reelected; and (ii) the margin of victory for the legislator in the next election. The vector X_{ivcj} represents 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 variables now vary at the level of a legislator-term rather than a legislator-vote.[33]

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 $son \times draft$ variable will have no effect on a legislator's subsequent election performance, i.e., $\beta_3 = 0$.

[33] We nonetheless present results with legislator fixed effects below.

2. **Moral hazard with politician types**. In the moral hazard model with politician types, voters do learn from legislative behavior. The $son \times draft$ variable will have a positive impact on election performance if $S_t = 0$ is state of the world ($\beta_3 > 0$), and a negative impact if $S_t = 1$ is the state of the world ($\beta_3 < 0$).

In the second implication, we treat the $son \times draft_{iv}$ 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 $son \times draft_{ivcj}$.

7.4 Results

We present the results of these regressions in Tables 15 and 16. In Table 15, we show that having a draft-eligible son significantly increases the average margin of electoral victory in the first three specifications. The magnitude is 9.5 percentage points in the specification with time fixed effects, which is over half of the mean margin of victory in the sample. This suggests that conscription is unpopular, and that bad types who voted in favor of it were subsequently punished by voters. In Column (4), we see that the effect is much smaller and statistically insignificant in the presence of legislator fixed effects. This is most likely due to the limited power of the test given that the outcome now varies at the level of a legislator-term.[34] In Table 16, 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) in the specification with time fixed effects. Again, the effect is consistently large and significant when we cumulatively add fixed effects for number of children, state, and vote, but not in the presence of legislator fixed effects.

To further aid our interpretation, we show in Table 17 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 18, we employ

[34]It is also possible that legislators who changed their vote in response to the shock were revealed as bad types

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 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). This findings provide additional support for the interpretation that, on average $S_t = 0$ better reflects the state of the world throughout our sample than $S_t = 1$.

Two-stage bivariate probit The specification in (6) can be read as a reduced form equation, where $son \times draft_{ivcj}$ is the exogenous instrument and V_{isvcj} is the omitted endogenous regressor. Our preferred interpretation of the above findings rests on the assumption that $son \times draft_{ivcj}$ is independent of the second stage error term, i.e., that it relates to E_{iscj} only through V_{isvcj}. This is an assumption that we can not verify conclusively. However, we can check to see that the second stage is at least consistent with our interpretation by explicitly modeling both equations. With binary dependent, endogenous and exogenous variables, we estimate a two-stage bivariate probit specification (Heckman, 1978) as follows:

$$\begin{aligned} V_{isvcj} =& 1(v_{vcj} + k_{ivcj} + \alpha_1 son \times draft_{ivcj} + \alpha_2 son_{ivcj} + \alpha_3 draft_{ivcj} + \alpha_4 \mathbb{X}_{ivcj} + \mu_{isvcj} > 0) \\ E_{iscj} =& 1(v_{vcj} + k_{ivcj} + \zeta V_{isvcj} + \omega_1 son_{ivcj} + \omega_2 draft_{ivcj} + \omega_3 \mathbb{X}_{ivcj} + e_{isvcj} > 0). \end{aligned} \tag{7}$$

Our interpretation of the reduced form result implies that $\zeta < 0$; i.e., pro-draft voting negatively affects reelection probability.

The results of this exercise are shown in Table 19. The first stage α_1 coefficients are negative and significant in the models with fixed effects for number of children and state (Columns (1) and (2)), but they are not significant in the models with higher dimensional fixed effects for roll call votes and legislators. The second stage results support our interpretation that pro-draft legislative voting negatively affects the probability that a legislator wins reelection. The average marginal effects are large: as shown in Column (2), a pro-draft vote is associated with a 35 percentage points reduction in the likelihood of reelection on average—about 49% of the mean. While we cannot rule out the possibility that some of this effect is attributable to other ways in which having a draft-eligible son helps to win reelection, the findings are highly consistent with our model of model hazard with politician types and retrospective voting.[35]

7.5 Discussion

Taken together, these findings suggest that, on average, conscription was unpopular with voters. This raises two important questions: (i) Is this result consistent other accounts of public opinion relating to the draft? (ii) If the draft is unpopular on average, why do politicians vote in favor of it most of the time?

On the first issue, 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. Nationally representative data on public support for the draft is available from surveys administered by the Roper Center's Public Opinion Archive in 1945, 1952, 1969, 1980, 1981, 1985, and 2003. These are analyzed by Fordham (2016), who documents a steady, steep decline in support for the draft from around 70% in 1945 to around 20% at the outset of the Iraq War in 2003.[36] The author's principal explanation for the decline in support relates to military technology: public support for the draft is a function of its necessity to win the war. As military conflict became more capital intensive over time, the importance of conscription as means of ensuring victory waned. This suggests that public support for conscription is also likely to be declining from World War I to World War II, although we can not verify this in the absence of polling data. A second factor relates to

[35] The equivalent exercise using IV2SLS rather than bivariate probit produces implausible point estimates that are outside the (0,1) interval, although none are statistically significant. See Appendix Table A4 for IV2SLS results with 1(Reelected) as the outcome variable and Appendix Table A5 for the IV2SLS results with next election margin as the outcome variable.

[36] 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. This reflected a "general desire not to repeat the mistakes of 1917/18" (pp. 8).

the salience of military casualties. Karol and Miguel (2007) provide evidence that home-state casualties in the Iraq war reduced the vote share for George W. Bush between the 2000 and 2004 presidential elections. It is thus plausible that the more fatalities reported during draft-era wars, the more unpopular is the draft itself. Turning to data presented in Table 4, we can calculate U.S. fatalities per draftee for each conflict: 0.018 in World War 1, 0.029 in World War II, and 0.024 in the Cold War theaters. Given the proliferation of mass media over this period, it is reasonable to assign a higher weight to latter figures, as information on those fatalities is more likely to proliferate among voters.

Taken together, these facts indicate that voter support for the draft trends downwards from World War I to the Cold War conflicts. An important verification test therefore is to examine whether or not legislators with draft-eligible sons are more likely to be reelected over time in our sample. To test this, we simply interact the $son \times draft_{ivcj}$ variable with indicators for the World War II and Cold War periods in a specification otherwise identical to Equation (6).

The results of this exercise are presented in Table 20. We present three specifications in order to parse the results from Table 16. The omitted category in each one is World War I. In all three specifications, legislators with draft age sons during World War I are less likely to win reelection. The point estimates are not significantly different from zero in Columns (1) and (2). The point estimate is -0.53 ($p < 0.01$) with vote fixed effects. Legislators with draft age sons during World War II are significantly *more* likely to win reelection in all three specifications. The point estimate is 0.62 ($p < 0.01$) relative to World War I with vote fixed effects. Finally, legislators with draft age sons during the Cold War are more likely still to win reelection. The point estimate is 0.71 ($p < 0.01$) relative to World War I with vote fixed effects. In sum, we see evidence that legislators with draft age sons are increasingly likely to win reelection as we move from World War I to World War II to the Cold War. This is consistent with an application of our model in which conscription is initially popular in World War I and increasingly unpopular thereafter.

On the second issue, our model and results imply that the control group of otherwise identical legislators who voted in favor of conscription are deriving utility from their vote through channels other than voter preferences. Linking back to Section 2, these could be party career concerns (P_{it}), ideology (F_i), or other unobserved private benefits (R_{it}). In Table 21, 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 panel we add an indicator that is equal to 1 if the president is from the same party as a given legislator. With legislator fixed effects, this is identified off the switch from Lyndon B. Johnson to Richard Nixon in 1968. With or without fixed effects, this variable has a strong positive association with pro-draft voting.

This finding is supported by narrative accounts of Nixon's approach to conscription before and after his election as president. Fordham (2016, p. 29) notes that while there were Republicans and Democrats on both sides of the debate over ending the draft, Nixon "campaigned on a promise to put a stop to it, but repeatedly asked for its extension as president."[37] Nixon's turn was particularly evident at the time of the Hatfield-Goldwater amendment to raise the pay of the military in 1970. The measure was an explicit attempt to end the draft by attracting a sufficient number of volunteers to render it obsolete within a year. The New York Times wrote at the time:

> President Nixon campaigned in favor of a volunteer army in 1968 and has supported the concept time and again since he became President. But he opposed the Hatfield-Goldwater amendment on the grounds that it would be too expensive and that the draft was essential as long as the United States maintained a sizable force in Southeast Asia.[38]

That legislative voting reflects this pattern is reassuring. More evidence on this motive can be gleaned from newspaper reports at the other end of our sample period. Under the heading "Ban Two Draft Opponents — Democrats in Cleveland Declare Gordon and Crosser 'Done'," The New York Times reported the following in April 1917:

[37] 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."

[38] "Senate Bars Plan Designed To Bring Volunteer Army," *The New York Times*, Aug. 26, 1970

> 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.[39]

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

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

8 Conclusion

In this paper, we test the hypothesis that political agency problems contribute to violent conflict: political leaders who do not internalize the costs of war are more likely to vote in favor of it. We demonstrate this by compiling data on the voting behavior and family compositions of over 3,300 legislators who served in the U.S. Congress during the four conscription era wars of the 20th century. We find that, (i) relative to those with daughters of comparable age, legislators with sons who are eligible to be drafted are around 10-17%

[39] "Ban Two Draft Opponents — Democrats in Cleveland Declare Gordon and Crosser 'Done'," *The New York Times*, Apr. 28, 1917

less likely to vote for conscription; (ii) legislators increase their support for the draft by a quarter when their sons cross the upper age threshold; and (iii) on average, legislators with draft-eligible sons are more likely to win reelection.

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

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

References

Abadie, A. and Gardeazabal, J. (2003). The economic costs of conflict: A case study of the basque country. *American Economic Review*, 93(1):113–132.

Acemoglu, D. and Robinson, J. A. (2005). *Economic origins of dictatorship and democracy*. Cambridge University Press.

Ansolabehere, S., de Figueiredo, J. M., and Snyder, James M., J. (2003). Why is there so little money in u.s. politics? *Journal of Economic Perspectives*, 17(1):105–130.

Barro, R. J. (1973). The control of politicians: an economic model. *Public choice*, 14(1):19–42.

Bazzi, S. and Blattman, C. (2014). Economic shocks and conflict: Evidence from commodity prices. *American Economic Journal: Macroeconomics*, 6(4):1–38.

Beber, B. and Blattman, C. (2013). The logic of child soldiering and coercion. *International Organization*, 67(1):65–104.

Bergan, D. E. (2009). The draft lottery and attitudes towards the vietnam war. *Public Opinion Quarterly*, 73(2):379–384.

Berman, N. and Couttenier, M. (2015). External shocks, internal shots: The geography of civil conflicts. *The Review of Economics and Statistics*, 97(4):758–776.

Berman, N., Couttenier, M., Rohner, D., and Thoenig, M. (2017). This mine is mine! how minerals fuel conflicts in africa. *American Economic Review*, 107(6):1564–1610.

Besley, T. (2006). *Principled agents?: The political economy of good government*. Oxford University Press on Demand.

Besley, T. and Coate, S. (1997). An economic model of representative democracy. *The Quarterly Journal of Economics*, 112(1):85–114.

Besley, T. and Persson, T. (2010). State capacity, conflict, and development. *Econometrica*, 78(1):1–34.

Blattman, C. and Miguel, E. (2010). Civil war. *Journal of Economic Literature*, 48(1):3–57.

Bonica, A. (2013). Ideology and interests in the political marketplace. *American Journal of Political Science*, 57(2):294–311.

Bonica, A., McCarty, N., Poole, K. T., and Rosenthal, H. (2013). Why hasn't democracy slowed rising inequality? *Journal of Economic Perspectives*, 27(3):103–24.

Brunner, E., Ross, S. L., and Washington, E. (2013). Does less income mean less representation? *American Economic Journal: Economic Policy*, 5(2):53–76.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Cascio, E. U. and Washington, E. (2014). Valuing the vote: The redistribution of voting rights and state funds following the voting rights act of 1965 *. *The Quarterly Journal of Economics*, 129(1):379–433.

Chassang, S. and Padro i Miquel, G. (2009). Economic shocks and civil war. *Quarterly Journal of Political Science*, 4(3):211–228.

Chattopadhyay, R. and Duflo, E. (2004). Women as policy makers: Evidence from a randomized policy experiment in india. *Econometrica*, 72(5):1409–1443.

Clifford, J. G. and Spencer, S. R. (1986). *The First Peacetime Draft*.

Collier, P., Elliott, V. L., Hegre, H., Hoeffler, A., Reynal-Querol, M., and Sambanis, N. (2003). *Breaking the Conflict Trap : Civil War and Development Policy*. Number 13938 in World Bank Publications. The World Bank.

Crawford, T. W. (2005). Moral hazard, intervention and internal war: A conceptual analysis. *Ethnopolitics*, 4(2):175–193.

de Figueiredo, J. M. and Richter, B. K. (2014). Advancing the empirical research on lobbying. *Annual Review of Political Science*, 17(1):163–185.

De Mesquita, B. B. and Siverson, R. M. (1995). War and the survival of political leaders: A comparative study of regime types and political accountability. *American Political Science Review*, 89(4):841–855.

Dodge, A. R., Koed, B. K., et al. (2005). *Biographical Directory of the United States Congress, 1774-2005: The Continental Congress, September 5, 1774, to October 21, 1788, and the Congress of the United States, from the First Through the One Hundred Eighth Congresses, March 4, 1789, to January 3, 2005, Inclusive*, volume 108. Government Printing Office.

Dube, O. and Vargas, J. (2013). Commodity price shocks and civil conflict: Evidence from colombia*. *The Review of Economic Studies*.

Erikson, R. S. and Stoker, L. (2011). Caught in the draft: The effects of vietnam draft lottery status on political attitudes. *American Political Science Review*, 105(2):221?237.

Fearon, J. D. (1995). Rationalist explanations for war. *International Organization*, 49:379–414.

Ferejohn, J. (1986). Incumbent performance and electoral control. *Public choice*, 50(1):5–25.

Fiorina, M. P. (1974). *Representatives, roll calls, and constituencies*. Lexington Books Lexington, MA.

Fisman, R. (2001). Estimating the value of political connections. *American Economic Review*, 91(4):1095–1102.

Flynn, G. Q. (2002). Conscription and democracy: The draft in france, great britain, and the united states. *Armed Forces & Society*, 29(1):158–161.

Fordham, B. O. (2016). Historical perspective on public support for the draft: War costs and military service. *Journal of Global Security Studies*, 1(4):303–322.

Garfinkel, M. R. (1990). Arming as a strategic investment in a cooperative equilibrium. *The American Economic Review*, pages 50–68.

Garfinkel, M. R. and Skaperdas, S. (2000). Conflict without misperceptions or incomplete information how the future matters. *Journal of conflict resolution*, 44(6):793–807.

Garfinkel, M. R. and Skaperdas, S. (2007). *Economics of Conflict: An Overview*, volume 2 of *Handbook of Defense Economics*, chapter 22, pages 649–709. Elsevier.

Gates, S. (2002). Recruitment and allegiance: The microfoundations of rebellion. *Journal of Conflict resolution*, 46(1):111–130.

Ghobarah, H. A., Huth, P., and Russett, B. (2003). *American Political Science Review*, 97(02):189–202.

Gordon, S. C., Hafer, C., and Landa, D. (2007). Consumption or investment? on motivations for political giving. *Journal of Politics*, 69(4):1057–1072.

Grossman, H. I. (1999). Kleptocracy and revolutions. *Oxford Economic Papers*, 51(2):267–283.

Haavelmo, T. (1954). *A study in the theory of economic evolution*. North-Holland Amsterdam.

Harari, M. and La Ferrara, E. (2014). Conflict, climate and cells: A disaggregated analysis.

Heckman, J. (1978). Dummy endogenous variables in a simultaneous equation system. *Econometrica*, 46(4):931–59.

Higgs, R. (1989). Do legislators' votes reflect constituency preference? a simple way to evaluate the senate. *Public Choice*, 63(2):175–181.

Hirshleifer, J. (1988). The analytics of continuing conflict. *Synthese*, 76(2):201–233.

Horowitz, M. C. and Levendusky, M. S. (2011). Drafting support for war: Conscription and mass support for warfare. *The Journal of Politics*, 73(2):524–534.

Jackson, M. O. and Morelli, M. (2007). Political bias and war. *American Economic Review*, 97(4):1353–1373.

Jayachandran, S. (2006). The jeffords effect. *Journal of Law and Economics*, 49(2):397–425.

Kalla, J. L. and Broockman, D. E. (2016). Campaign contributions facilitate access to congressional officials: A randomized field experiment. *American Journal of Political Science*, 60(3):545–558.

Karol, D. and Miguel, E. (2007). The electoral cost of war: Iraq casualties and the 2004 us presidential election. *Journal of Politics*, 69(3):633–648.

Kuperman, A. J. (2008). The moral hazard of humanitarian intervention: Lessons from the balkans. *International Studies Quarterly*, 52(1):49–80.

Levitt, S. D. (1994). Using repeat challengers to estimate the effect of campaign spending on election outcomes in the us house. *Journal of Political Economy*, 102(4):777–798.

Levitt, S. D. (1996). How Do Senators Vote? Disentangling the Role of Voter Preferences, Party Affiliation, and Senate Ideology. *American Economic Review*, 86(3):425–41.

McCarty, N., Poole, K. T., and Rosenthal, H. (2001). The hunt for party discipline in congress. *The American Political Science Review*, 95(3):673–687.

McGuirk, E. and Burke, M. (2017). The economic origins of conflict in africa. Technical report, National Bureau of Economic Research.

Mian, A., Sufi, A., and Trebbi, F. (2010). The political economy of the us mortgage default crisis. *American Economic Review*, 100(5):1967–98.

Miguel, E., Satyanath, S., and Sergenti, E. (2004). Economic shocks and civil conflict: An instrumental variables approach. *Journal of Political Economy*, 112(4):725–753.

Miller, G. (2008). Women's suffrage, political responsiveness, and child survival in american history*. *The Quarterly Journal of Economics*, 123(3):1287–1327.

Morris, B. E. (2006). The effects of the draft on us presidential approval ratings during the vietnam war, 1954-1975. Technical report, AIR FORCE INST OF TECH WRIGHT-PATTERSON AFB OH.

Osborne, M. J. and Slivinski, A. (1996). A model of political competition with citizen-candidates. *The Quarterly Journal of Economics*, 111(1):65–96.

Poole, K. T. and Rosenthal, H. (1984). The polarization of american politics. *The Journal of Politics*, 46(4):1061–1079.

Powell, R. (2006). War as a commitment problem. *International Organization*, 60:169–203.

Powell, R. (2012). Persistent fighting and shifting power. *American Journal of Political Science*, 56(3):620–637.

Rogoff, K. (1990). Equilibrium political budget cycles. *American Economic Review*, 80(1):21–36.

Skaperdas, S. (1992). Cooperation, Conflict, and Power in the Absence of Property Rights. *American Economic Review*, 82(4):720–39.

Smith, A. (1996). Diversionary foreign policy in democratic systems. *International Studies Quarterly*, 40(1):133–153.

Snyder, J. M. and Groseclose, T. (2000). Estimating party influence in congressional roll-call voting. *American Journal of Political Science*, 44(2):193–211.

Tarar, A. (2006). Diversionary incentives and the bargaining approach to war. *International Studies Quarterly*, 50(1):169–188.

Tullock, G. (1972). The purchase of politicians. *Western Economic Journal*, 10(3):354–55.

Washington, E. L. (2008). Female socialization: How daughters affect their legislator fathers. *American Economic Review*, 98(1):311–32.

Figures

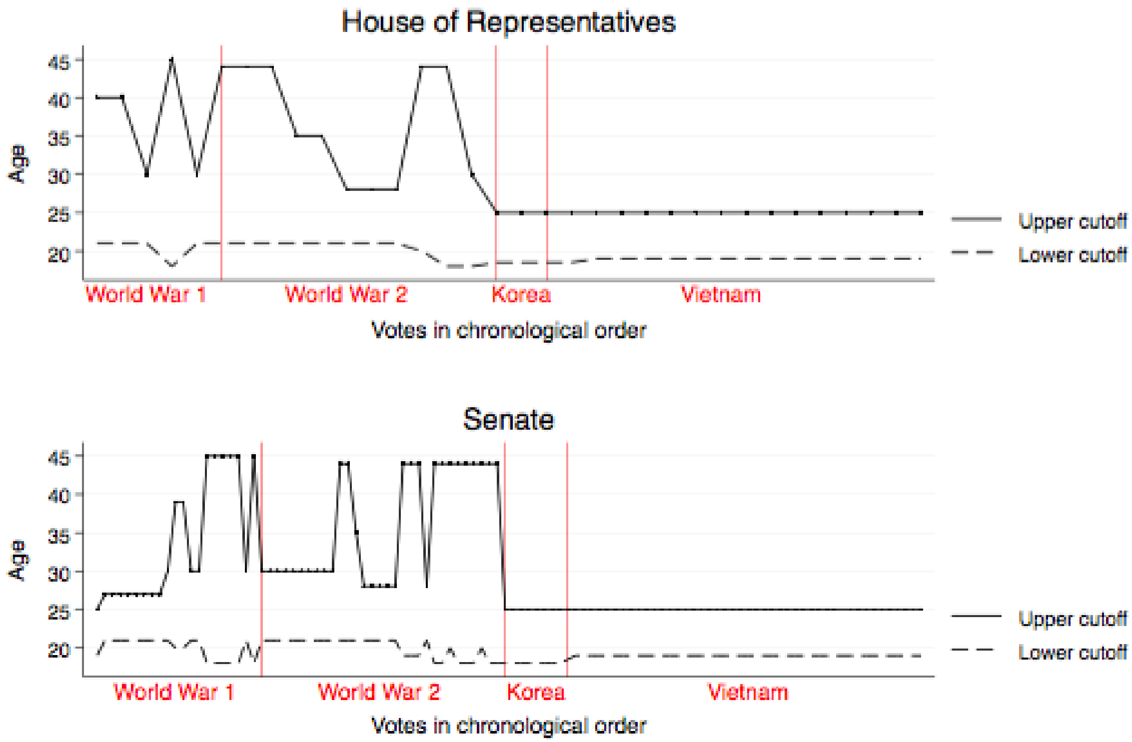

Figure 1: Proposed draft age cutoffs by roll call vote.

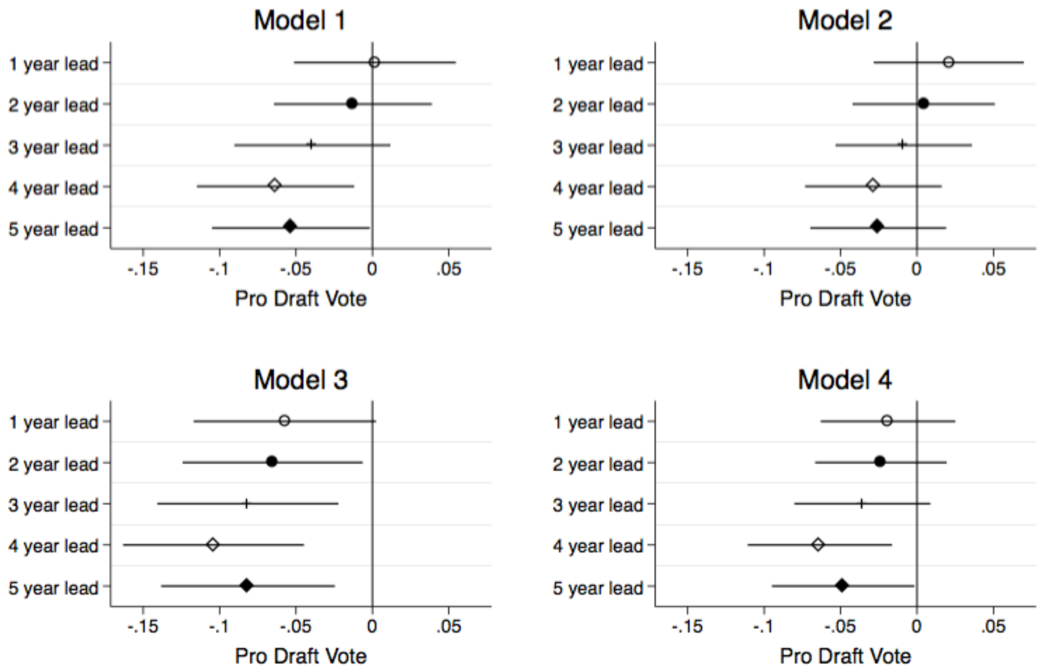

Figure 2: **Impact of having draft-eligible son on pro-draft votes at various lower thresholds**. The models correspond to those shown in Table 5. Leads refer to the number of years below the lower draft cutoff used to calculate the age boundary for the treatment variable.

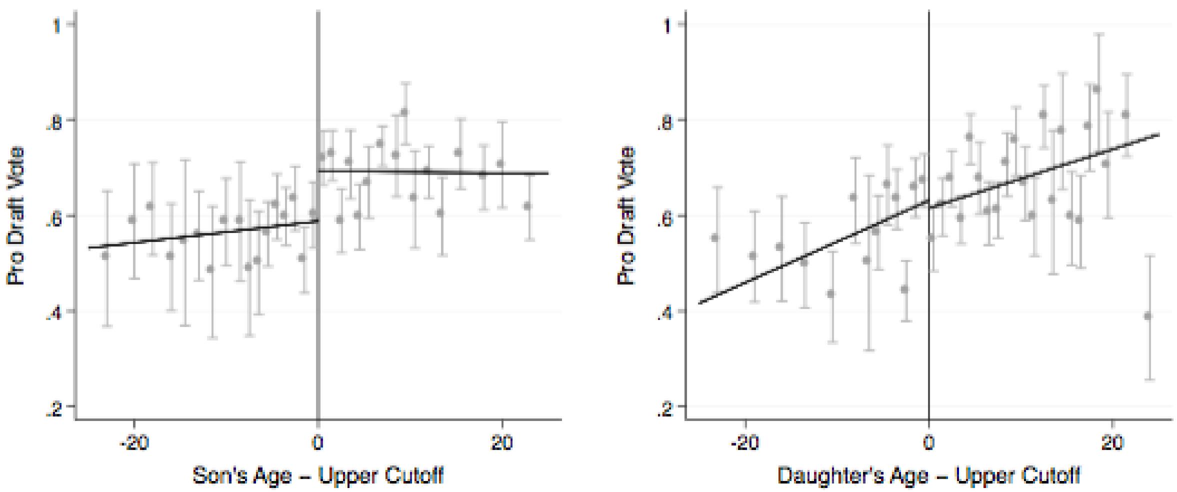

Figure 3: **Regression Discontinuity at the Upper Cutoff**. Son age coefficient: 0.1379 (SE: 0.06); Daughter age coefficient -0.016 (SE 0.02). Bandwidth: 25.

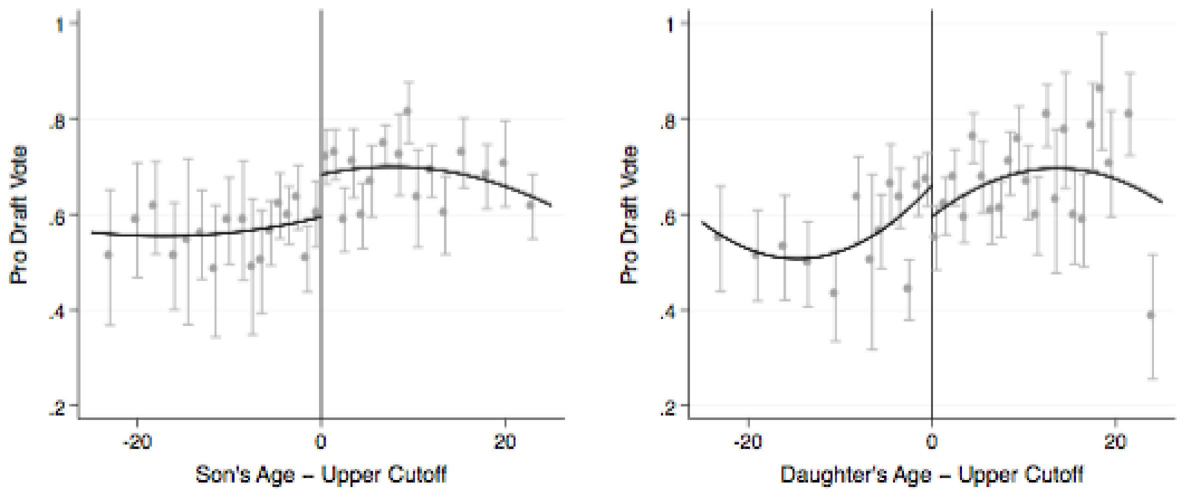

Figure 4: **Regression Discontinuity at the Upper Cutoff: Quadratic**. Son age coefficient: 0.1915 (SE: 0.06); Daughter age coefficient -0.066 (SE 0.03). Bandwidth: 25.

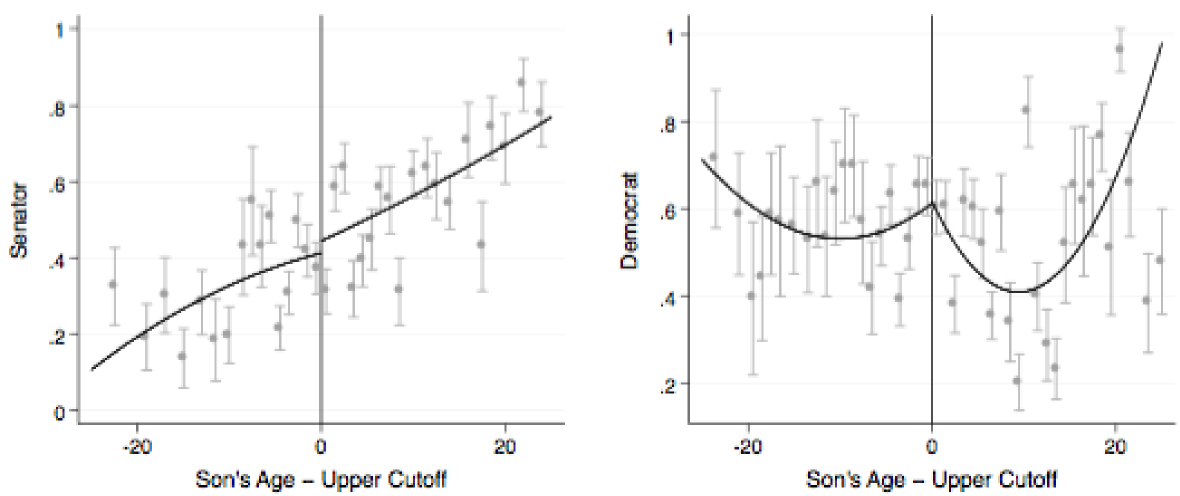

Figure 5: **Regression Discontinuity at the Upper Cutoff: Placebo Outcomes**. Son age coefficient in Senator regression: 0.0103 (SE: 0.05); Son age coefficient in Democrat regression -0.0229 (SE 0.0811). Bandwidth: 25.

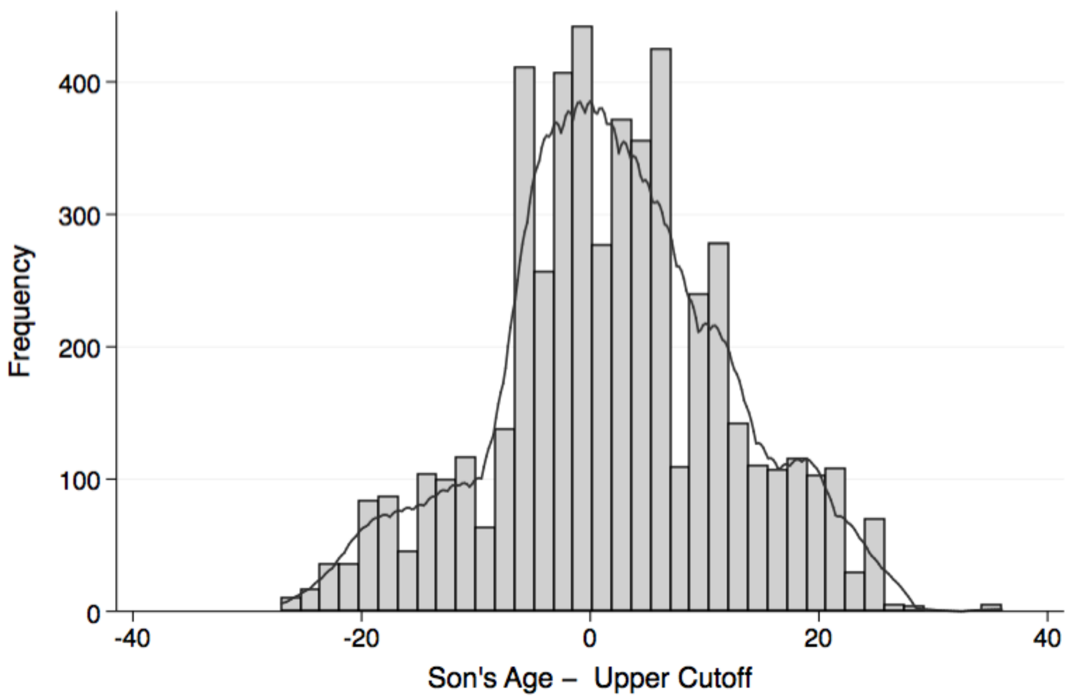

Figure 6: Regression Discontinuity Plots: Running Variable Density

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

Note: Data on vote records is from the Voteview project. Data on pro-draft voting is calculated by the authors based on contemporaneous newspaper reports.

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

Note: Data on the family composition of legislators comes from census records (1840-1940) where possible, and a variety of biographical records. See main text for more details.

Table 3: Age thresholds

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

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

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

Table 4: Registration, deployment and fatalities

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

Note: Data on total U.S. Servicemembers and Battle Deaths are from the "America's Wars' fact sheet compiled by the U.S. Department of Veteran's Affairs, accessed at https://www.va.gov/opa/publications/factsheets/fs_americas_wars.pdf. Data on Draft Inductions are from U.S. Selective Service System, accessed at https://www.sss.gov/About/History-And-Records/Induction-Statistics. Data on total number of men registered for the draft come from multiple sources: the WW1 figure is from http://www.history.com/this-day-in-history/u-s-congress-passes-selective-service-act; the WW2 figure is from https://www.cbo.gov/sites/default/files/cbofiles/ftpdocs/83xx/doc8313/07-19-militaryvol.pdf; the Korean War figure is from Flynn (2002, p. 73).; and the Vietnam War figure are from Morris (2006, p. 15). The total number of draftees killed in Vietnam is around 17,000 (see http://history-world.org/vietnam_war_statistics.htm). All website were accessed on 10/29/2017.

Table 5: Impact of having draft-eligible son on pro-draft vote; main votes

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0634**	-0.0285	-0.1040***	-0.0635**
	(0.0310)	(0.0269)	(0.0356)	(0.0284)
Draft age child	-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. $^{***}p < 0.01, ^{**} p < 0.05, ^{*} p < 0.1$. The unit of analysis is the legislator-vote.

Table 6: Main result with added controls for 2nd order polynomial in each child's age

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0614*	-0.0225	-0.0863**	-0.0508*
	(0.0313)	(0.0272)	(0.0353)	(0.0277)
Draft age child	-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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The unit of analysis is the legislator-vote.

Table 7: Impact of having draft-eligible son on pro-draft vote; close votes

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son × close vote	-0.2069***	-0.1980***	-0.1547***	-0.0170
	(0.0274)	(0.0260)	(0.0259)	(0.0185)
Draft age son	0.0020	0.0337	-0.0567	-0.0586**
	(0.0328)	(0.0296)	(0.0368)	(0.0286)
Draft age child	-0.0195	-0.0416*	0.0156	0.0151
	(0.0275)	(0.0235)	(0.0308)	(0.0278)
Any son	0.0136	-0.0067	0.1349	-0.0228
	(0.0310)	(0.0257)	(0.0994)	(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. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$. The unit of analysis is the legislator-vote. Close roll-call votes are those in which the margin of victory is within 20 percentage points.

Table 8: Hawks and Doves method with draft category votes

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Draft age son	-0.0594*	-0.0167	-0.0734**	-0.0566***
	(0.0330)	(0.0268)	(0.0301)	(0.0082)
Draft age child	-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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The unit of analysis is the legislator-vote. A hawkish vote is one that aligns with the modal vote cast by hawks, and against the modal vote cast by doves.

Table 9: Hawks and Doves method with non-draft category votes

	Hawkish Vote			
	(1)	(2)	(3)	(4)
Draft age son	0.0233	0.0339***	0.0054	-0.0008
	(0.0148)	(0.0117)	(0.0111)	(0.0074)
Draft age child	-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. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$. The unit of analysis is the legislator-vote. A hawkish vote is one that aligns with the modal vote cast by hawks, and against the modal vote cast by doves.

Table 10: Regression discontinuity at the upper cutoff

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
RD estimate at upper cutoff	0.1601***	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 11: 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 12: 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 13: 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 14: 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 15: Impact of draft-eligible son on next election margin

	Next Election Margin			
	(1)	(2)	(3)	(4)
Draft age son	0.0921***	0.0637**	0.0950***	-0.0314
	(0.0327)	(0.0304)	(0.0312)	(0.0295)
Draft age child	-0.0818**	-0.0633*	-0.0984***	-0.0329
	(0.0335)	(0.0326)	(0.0320)	(0.0272)
Any son	-0.1144***	-0.0697**	-0.1171***	-0.2559*
	(0.0346)	(0.0331)	(0.0336)	(0.1423)
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.	0.170	0.170	0.170	0.173
Observations	6436	6436	6436	6360

Note: Standard errors are doubled clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election margin is the percentage point difference between the incumbent and the challenger in the next election.

Table 16: Impact of draft-eligible son on next election victory

	1(Reelected)			
	(1)	(2)	(3)	(4)
Draft age son	0.1193*	0.0979*	0.1240**	-0.0240
	(0.0655)	(0.0501)	(0.0616)	(0.0305)
Draft age child	-0.0377	-0.0335	-0.0539	-0.0261
	(0.0635)	(0.0481)	(0.0608)	(0.0363)
Any son	-0.1373**	-0.0969**	-0.1387**	0.1167
	(0.0630)	(0.0469)	(0.0616)	(0.1475)
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.	0.712	0.712	0.712	0.721
Observations	6436	6436	6436	6360

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

Table 17: 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)
Draft age son	-0.1020**	-0.0478	-0.0811**	-0.0613*
	(0.0432)	(0.0412)	(0.0389)	(0.0366)
Draft age child	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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The unit of analysis is the legislator-vote.

Table 18: Senate election proximity on hawkish voting; full Hawks and Doves 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)
Draft age son	-0.0543	0.0073	-0.0091	-0.0226
	(0.0339)	(0.0241)	(0.0175)	(0.0145)
Draft age child	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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The unit of analysis is the legislator-vote.

Table 19: Two-stage bivariate probit estimate of average treatment effects of pro-draft vote on reelection

	1(Reelected)			
	(1)	(2)	(3)	(4)
Pro Draft Vote	-1.3913***	-1.2115***	0.1698	0.7820
	(0.0768)	(0.3196)	(0.5796)	(41.6458)
Average marginal effect	-0.4258***	-0.3466***	0.0430	0.0146
	(0.022)	(0.102)	(0.146)	(0.962)
First stage	Pro Draft Vote			
Draft age son	-0.2641**	-0.2182*	0.0767	-0.1754
	(0.1060)	(0.1172)	(0.1553)	(1.0703)
Congressman 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 1(Reelected)	0.712	0.712	0.712	0.712
Observations	6438	6438	6438	6438

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

Table 20: Heterogeneous impact of draft-eligible son on next election victory by war

	1(Reelected)		
	(1)	(2)	(3)
Draft age son	-0.1644	-0.1361	-0.5307***
	(0.2009)	(0.2114)	(0.1938)
Draft age son × WWII	0.2834	0.2380	0.6241***
	(0.1944)	(0.2095)	(0.1922)
Draft age son × Cold War	0.2957	0.2393	0.7086***
	(0.1974)	(0.2086)	(0.1961)
Draft age child	-0.0376	-0.0328	-0.0551
	(0.0635)	(0.0479)	(0.0604)
Any son	-0.1371**	-0.0962**	-0.1347**
	(0.0630)	(0.0466)	(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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$. The unit of analysis is the legislator-vote. The outcome variable varies at the level of the legislator-term. Next election victory is equal to 1 if the incumbent wins the next election.

Table 21: Party influence

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft age son	-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)
Draft age son	-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)
Draft age son	-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

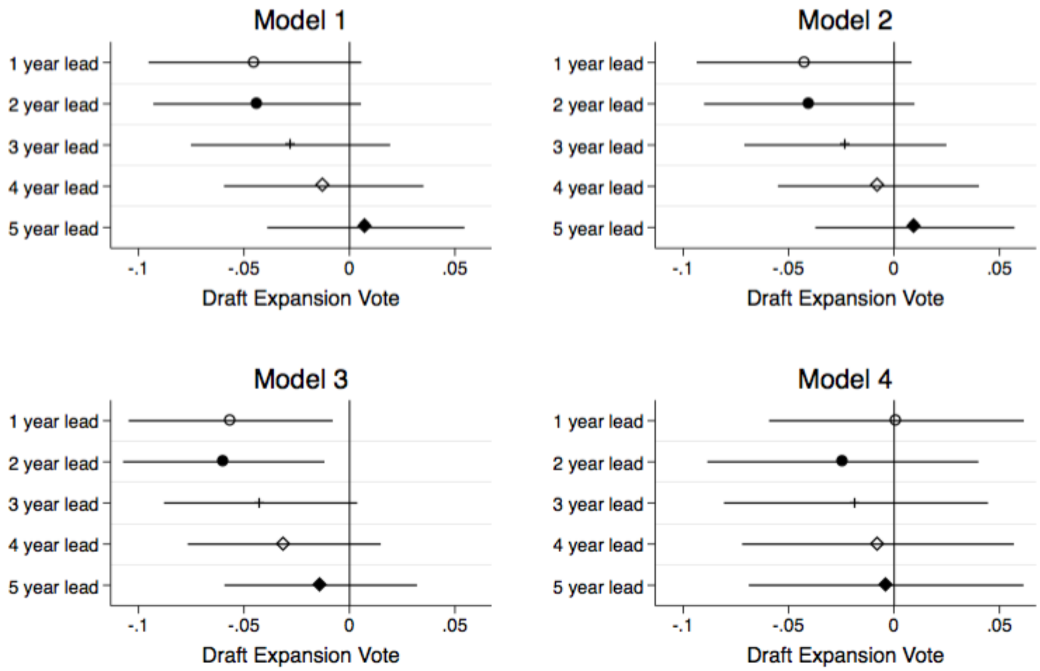

Figure A1: Impact of having draft-eligible son on window votes at various lower thresholds

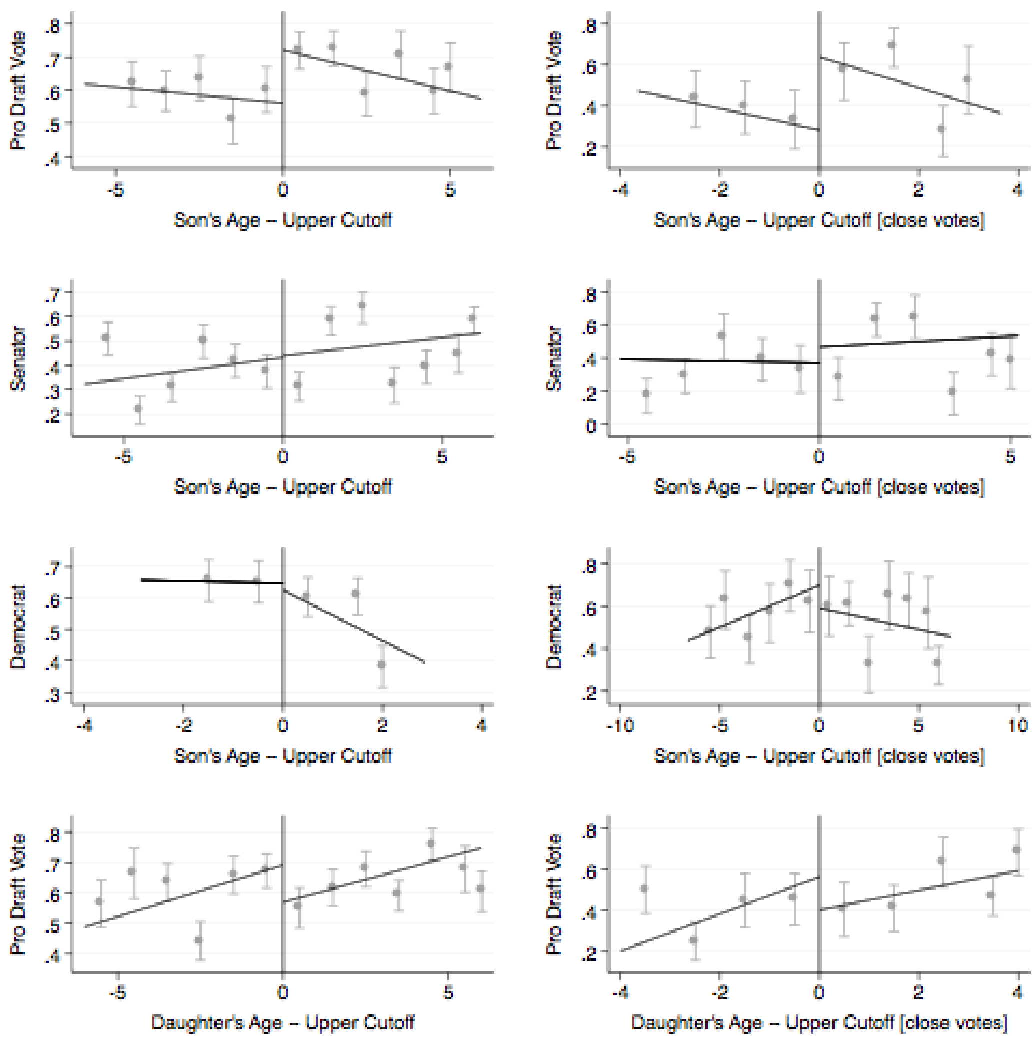

Figure A2: Corresponding RD plots for results presented in Tables 10, 11, and 12. Specifications with fixed effects are omitted. The top panel presents our main results with a full sample and with close votes. The second and third panels show RD results with placebo outcomes. The bottom panel show

Appendix Tables

Table A1: Conditional Logit; main votes

	Pro Draft Vote		
	(1)	(2)	(3)
Draft age son	-0.2698**	-0.1235	-0.6447***
	(0.1301)	(0.1227)	(0.2497)
Draft age child	-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)
Draft age son (window)	-0.0438	-0.0402	-0.0596**	-0.0243
	(0.0299)	(0.0303)	(0.0290)	(0.0390)
Draft age child (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)
Draft age son	-0.0532*	-0.0210	-0.0080	-0.0407*
	(0.0287)	(0.0258)	(0.0260)	(0.0238)
Draft age child	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. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A4: IV2SLS effect of pro-draft vote on reelection

	1(Reelected)			
	(1)	(2)	(3)	(4)
Pro Draft Vote	-2.4011	-3.8693	4.4394	0.4394
	(2.621)	(6.373)	(7.048)	(0.633)
First stage K-P test p-value	0.021	0.239	0.249	0.103
Legislator FE	No	No	No	Yes
Term 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.	0.712	0.712	0.712	0.721
Observations	6436	6436	6436	6360

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

Table A5: IV2SLS effect of pro-draft vote on next election margin

	Next Election Margin			
	(1)	(2)	(3)	(4)
Pro Draft Vote	-1.8543	-2.5199	2.8398	0.5739
	(1.819)	(4.039)	(4.693)	(0.664)
First stage K-P test p-value	0.021	0.239	0.249	0.103
Legislator FE	No	No	No	Yes
Term 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.	0.170	0.170	0.170	0.173
Observations	6436	6436	6436	6360

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