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

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ABSTRACT

We study agency frictions in the United States Congress. We examine the longstanding hypothesis that political elites engage in conflict because they fail to internalize the associated costs. We compare the voting behavior of legislators with draft age sons versus draft age daughters during the conscription-era wars of the 20th century. We estimate that having a draft age son reduces pro-conscription voting by 7-11 percentage points. Support for conscription recovers when a legislator's son ages out of eligibility. We establish that agency problems contribute to political conflict and that politicians are influenced by private incentives orthogonal to political concerns or ideological preferences.

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1. Introduction

Political agency problems can arise when legislative behavior is shaped by private incentives. If leaders are influenced by personal interests when making legislative decisions, then public policy is less likely to represent the will of voters (Grossman and Helpman, 2001, Besley, 2006). Identifying these frictions empirically has proven difficult, since one needs to observe variation in legislators' private incentives that is independent of variation in their political incentives. In this article, we address this challenge by studying political agency problems in the context of legislative voting on issues of war and peace in the United States Congress.

The presence of such agency frictions implies that private incentives affect policy decisions relating to the conduct of war. We test for this relationship using data on roll call votes in Congress during the four conscription-era wars of the 20th Century—World Wars I and II, the Korean War, and the Vietnam War—when legislative votes on conscription played a fundamental role in determining the number of troops sent to battle. By observing exogenous variation in the exposure of some legislators to the private costs of conscription relative to others, we can detect moral hazard in an important policy decision affecting war. If legislators fully internalize the social costs of conflict, then both groups will be equally supportive of conscription. If not, then those with higher private costs will be less supportive.

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

We employ three empirical approaches to estimate the effect of private incentives on legislative decisions relating to conscription. In our first approach, we exploit cross-sectional variation in the gender of a legislator's draft age child. We estimate that legislators with exposed sons are between 7-11 percentage points less likely to vote in favor of conscription than comparable legislators with daughters of the same age. This is 12-19% of the dependent variable mean of 0.58. This difference is robust to the inclusion of fixed effects for the legislator's number of children and number of sons, implying that it is not due to a more general effect of having children of either sex on legislative voting.

In our second empirical approach, we compare the voting behavior of legislators with sons either side 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] Applying this logic, we employ a regression discontinuity design 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.

estimate an 18.8 percentage point increase in the probability of voting in favor of conscription for those with sons above the cutoff. This estimate indicates that our main result is not due to a more general effect of a child's gender at a specific age on legislative voting. We find no significant effect in a placebo test using the age of a legislator's only daughter.

In our third empirical approach, we exploit panel variation in the sample of legislators with sons or daughters switching into and/or out of the age eligibility window. We confirm that the panel evidence supports the cross-sectional evidence: controlling for legislator fixed effects, having a draft age son reduces support for conscription by 6-11 percentage points relative to having a draft age daughter. This finding definitively rules out the role of time-invariant confounds.

Having established a connection between private incentives and legislative voting, we then harness the panel variation to better understand the underlying mechanisms at play. One straightforward explanation is that pure self-interest is driving the results. Politicians are less likely to support conscription when they are exposed to its costs and more likely to support it when they are not. However, it is also possible that private incentives may have spurred the politician to invest more effort in learning about the *social* costs of conscription, after which the change in behavior that we observe is due to political concerns or ideological preferences rather than pure self-interest. With this interpretation, it is new information that is driving the result.

To distinguish between these mechanisms, we examine the behavior of legislators when their treatment status changes from one to zero. This occurs as the youngest son of a legislator ages out of draft eligibility at the upper cutoff. If the information mechanism is at play, we should not detect a change in voting behavior, since this motive ought to persist long after the politician's own son ages out of eligibility. If the self-interest mechanism is at play, however, we should detect a change in voting behavior, since the politician's son becomes ineligible and thus the politician is sheltered from the associated costs.

This test lends itself to an event study design that combines within-legislator variation at this cutoff together with between-legislator variation in the gender of a youngest child. We estimate that the average legislator is 12.7 percentage points more likely to vote in favor of conscription one year after their son ages out of eligibility relative to one year before. This is unlikely to be caused by a sudden change in preferences or electoral motives. Instead, we interpret it as evidence that policy choices can be influenced by private incentives that are orthogonal to both political concerns and individual ideology.

To rationalize these findings, we turn to a workhorse model of political agency that combines elements of moral hazard and adverse selection (Besley, 2006). 'Good' politicians pursue measures that are in the voters' interest, and voters respond by reelecting them. 'Bad' politicians decide either to mimic good types in order to win reelection, or to vote against citizens' interests and lose reelection. This decision is determined in part by the value of private rents that accrue to the politician if they vote against the electorate's wishes. Typically, researchers do not observe exogenous variation in private rents that politicians can capture through legislative voting. This presents a barrier to empirically testing this type of model. However, in our setting we do observe an exogenous 'wedge' between the private benefits of conscription for legislators with draft-eligible sons versus those with daughters of comparable age. This provides testable implications of the theory that we can bring to the data. The first is that legislators with draft age sons will be more likely to vote against conscription, as we show in our main analysis. The second is that, as a result, legislators with draft age sons will be less likely to win reelection when conscription is relatively popular and more likely to win reelection when conscription is relatively unpopular.

To test this, we first confirm evidence from historical accounts that conscription was more popular in the earlier period of the 20th century and became much less popular during the Cold War conflicts. This is likely due to the declining labor-intensity of war over time (Fordham, 2016) as well as a successful effort by the U.S. government to use propaganda and censorship during World War I (Hamilton, 2020, Axelrod, 2009). We check this fact by estimating the effect of election proximity on legislative voting in the Senate. If the draft is unpopular, then senators who are up for reelection will be less likely to vote in favor of it.[2] Consistent with the case literature, we indeed find that senators who were up for reelection were more likely than other senators to vote in favor of conscription during World War I and less likely to vote in favor of conscription during the Cold War. Next, we examine the effect of having a draft age son on the probability of being reelected. In line with the theory, we find that politicians with draft age sons were significantly less likely to win reelection during World War I. This effect dissipates entirely by the Cold War, where the point estimate is positive but not statistically distinguishable from zero. Together, these descriptive exercises align well with political agency models that combine moral hazard and adverse selection.

To arrive at our results, we undertake two main data collection exercises. In the first, we identify 248 roll-call votes relating to conscription in the House and Senate from 1917 to 1974. We code the 'direction' of pro- or anti-conscription measures based in part on contemporaneous newspaper reports. In the second, we gather biographical information on the families of the U.S. senators and representatives who voted on these measures. For this we use a combination of census records (for those present in years up to 1940) and a variety of other biographical sources (for those who are not). This process produces a main estimation sample of 26,373 observations at the level of a legislator-vote, combining information on 140 unambiguous roll-call votes, 2,287 legislators, and 5,737 children.

In order to validate our vote-coding procedure, we additionally develop an alternative method that relies on the behavior of well-known foreign policy "hawks" (pro-war legislators) and "doves" (anti-war legislators) during each era. If a legislator votes in line with the hawks and against the doves on a given measure, it is determined as a hawkish vote. Applying this approach, we find that legislators with exposed sons are again around 7-11 percentage points less likely to vote with hawks on draft-related measures, but no less likely to vote with hawks on measures unrelated to the draft.

This paper links two bodies of research. Our principal contribution is to the political economy of legislative decision making. The prevailing view is that a legislator's decision is motivated by a combination of political concerns and purely private concerns (Ansolabehere, de Figueiredo

[2] We focus on our subsample of senators because, unlike the House of Representatives, elections for the Senate are staggered across three groups over six years. This allows us to control for time fixed effects and harness plausibly exogenous variation in election proximity between politicians over time.

and Snyder, 2003, Levitt, 1996). Political concerns derive from the preferences of the legislator's constituents, who determine reelection, and the legislator's party, who can otherwise influence career outcomes. Private concerns derive from the legislator's own ideological preferences. However, this model of policy formation leaves no room for the possibility that legislators are influenced by additional private incentives that are independent of ideological preferences, such as quid-pro-quo transfers from special interests. While there exists an argument that politicians are largely immune from such influences (Ansolabehere et al., 2003, Tullock, 1972), it is difficult to reconcile with the growing share of campaign contributions emanating from the top of the wealth distribution in the United States (Bertrand, Bombardini, Fisman and Trebbi, 2020, Bonica, McCarty, Poole and Rosenthal, 2013).

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

Our study complements the important work of Washington (2008), who finds that legislators with daughters are more likely than other legislators to vote liberally due to 'female socialization,' which is a change in preferences that one experiences after having a daughter. Washington's result provides novel causal evidence that a legislator's individual preferences can influence congressional decision making.[3] Just as that study exploits exogenous variation *between* legislators to show that ideological preferences affect legislative voting, our study additionally exploits variation *within* legislators to show that private incentives also affect legislative voting. In this regard, we identify another important explanatory variable that has been previously omitted in the literature. Our finding has implications for the broader literature on special interests and quid-pro-quo politics, as we show that legislators respond sharply to changing private incentives, which is an important assumption underlying many of these studies (Grossman and Helpman, 2001, Bertrand et al., 2020).

The second body of research connects credible identification strategies to theoretical work on the origins of violent conflict. These foundations are based on contest models in which two sides fight to control total resources. 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

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

they are avoided in some cases but not in others (Coase, 1960, Fearon, 1995).[4] One explanation is that wars can occur because the leaders who order violence do not fully internalize the costs. This idea is formalized in Jackson and Morelli (2007), where its roots are traced at least as far back as Kant (1795). This moral hazard theory of conflict relaxes the assumption that groups are unitary actors.[5] To the best of our knowledge, we are the first to corroborate it using quasi-experimental variation.

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 we present our estimation strategy and main results. In Section 5 we examine the information versus self-interest interpretation of the main results, and in Section 6 we endogenize the behavior of voters in response to legislators' decisions in a political agency model and empirically test its implications. We conclude in Section 7.

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 private ideological concerns (de Figueiredo and Richter, 2014, Ansolabehere et al., 2003, Levitt, 1996). This implies that a politician weights three sets of preferences in determining their optimal legislative vote. Reelection concerns are derived from the preferences of voters; promotional concerns are derived from the national party edict; and ideological concerns are derived from exogenous preferences.

There exists at least some empirical evidence in support of each motive.[6] The first, voter preferences, is derived from the canonical model of Downsian competition in which politicians converge on the preferences of the median voter. The second, 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. The third element, a legislator's fixed ideology, is estimated by Levitt (1996) to carry a weight of around 0.60, more than the others combined. Evidence in support of this idiosyncratic ideological influence is provided by Washington (2008), who finds that U.S. legislators with more daughters have a higher propensity to vote in favor of liberal measures, particularly ones connected to expanding reproductive rights. Those findings are consistent with sociological theories that parenting daughters increases feminist sympathies.[7]

[4] On the various costs of war, see Abadie and Gardeazabal (2003), Ghobarah, Huth and Russett (2003), Besley and Persson (2010), Besley and Mueller (2012), León (2012), Dell and Querubin (2017) and Prem, Vargas and Namen (2021). Besley and Persson (2009), building on Tilly (1993), make the distinction between internal conflicts, which undermine state capacity, and external conflicts, which can be conducive to building state capacity.

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

[6] See Appendix A for a more comprehensive account of this literature.

[7] One argument is that voters' preferences are represented in government not through Downsian competition, 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.

Incorporating private rents 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. It is assumed either that there are no other private costs and benefits associated with legislative voting, or that, if there are, legislators are immune to their influence. This appears to be at odds with the apparently large sums of private money that are spent on lobbying and campaign contributions. However, Ansolabehere et al. (2003), echoing Tullock (1972), argue that if campaign contributions were indeed worthwhile investments, they ought to be of substantially higher value in each election cycle given the trillions of dollars of government outlays potentially at stake. They conclude that campaign contributions are largely made for their consumption value.[8]

In this paper, we propose an alternative explanation for the absence of evidence on the role of private influences in legislative voting: the significant empirical challenge in detecting such an effect (de Figueiredo and Richter, 2014). A clean identification strategy would require that we observe exogenous variation in the politician's private returns to voting on a legislative issue while holding preferences 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 little evidence that individual legislators respond to changes in private rents that are tied to voting in a specific manner in Congress. Yet, such a view would be consistent with more recent evidence on the patterns of political contributions in the United States (Bertrand et al., 2020, Bonica et al., 2013, Gordon, Hafer and Landa, 2007).

To incorporate this motive, we propose a model of legislative behavior in which legislators are concerned with their own private returns to voting in addition to the elements above. Assuming that preferences are single peaked, the politician's objective is to select the vote that minimizes the weighted average of the squared distances from four 'ideal points' that correspond to each preference, as follows:

$$\max_{V_{it}=\{0,1\}} U_{it} = -\left[\alpha_1(V_{it}-M_{it})^2 + \alpha_2(V_{it}-P_{it})^2 + \alpha_3(V_{it}-F_i)^2 + \theta(V_{it}-R_{it})^2\right], \tag{1}$$

where $V_{it} \in \{0,1\}$ is legislator i's vote at time t; $M_{it} \in [0,1]$ is the ideal point in a given issue space of the median voter in the legislator's electorate; $P_{it} \in [0,1]$ is the ideal point of the legislator's national party; $F_i \in [0,1]$ is the legislator's fixed ideological bliss point; $R_{it} \in [0,1]$ is the ideal point that optimizes the legislator's time-varying private benefit; and $\sum_{j=1}^{3} \alpha_j + \theta = 1$. The

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

solution to the legislator's problem is:

$$V_{it}^* = \underbrace{\alpha_1 M_{it} + \alpha_2 P_{it}}_{\text{political motives}} + \underbrace{\alpha_3 F_i + \theta R_{it}}_{\text{private motives}}. \tag{2}$$

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

Application to conflict Much of the theoretical literature on violent conflict treats actors as unitary decision-makers.[9] 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 (2) relaxes this assumption. If, on a given vote, a shock to R_{it} is sufficiently large, then it is possible a leader may vote to enter conflicts in which the expected social costs exceed the benefits, or to avoid conflicts in which the expected social benefits exceed the costs. The critical condition in either case is that the private payoff through θ offsets the influences that operate through the other channels, or $V_{it}^*(. \mid \theta > 0) = (1 - V_{it}^*(. \mid \theta = 0))$.[10] This is raised by Fearon (1995) as one explanation for violent conflict between groups of rational agents. 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.

Testing implications The central challenge for the researcher in determining whether or not private rents influence policy decisions (i.e., $\theta > 0$) is to observe exogenous variation in R_{it}. Otherwise, any estimate of θ could be biased due to 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 a large share of her electorate is employed by the firm, in which case M_{it} is measured incorrectly as R_{it}; or 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-exposed son affects legislative voting on conscription, holding F_i constant. Legislators with exposed sons stand to lose more from the passage of conscription than legislators with daughters of comparable age, all else equal. This stems not only from the fact that exposed sons are susceptible to the dangers of combat deployment, but also from the costs that derive from avoiding the draft by, for example, joining the National Guard or expending political capital to otherwise escape deployment. This implies that, on a vote to determine whether or not to impel citizens to go to war, legislators exhibited measurable, exogenous variation in R_{it}.

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

[10]The same could be said about changes to 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, and are thus 'contracted', whereas R_{it} is plausibly not. We examine this condition in more detail when we endogenize voter behavior in Section 6.

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 other characteristics related to the vote and to the legislator, including biographical information on their children at the time of voting. In our core sample there are 2,287 legislators, 5,737 children, and 26,373 legislator-votes spread between the House of Representatives and the Senate from the 65th Congress in 1917 to the 93rd Congress in 1974.[11] We describe below our principal data sources and the construction of our main variables.[12]

Vote data Our main 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 conscription-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. We then retain the union of votes that are assigned the "Selective Service" issue code by *Voteview* (the main conscription legislation in the United States is named the Selective Service Act) and votes that we determine to be relevant. This is aided by short descriptions of each roll-call vote provided by the *Gov Track* project.[13] This gives a total of 248 votes; 195 determined by *Voteview* and a further 53 determined by the authors.

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

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

Next, in order to examine legislators' motives for voting, it is necessary for us to assign a 'direction' to each roll call vote. In the first example above (vote 52 in the 65th Senate), it is clear that an *aye* vote implies support for the draft. For vote 78 in the 92nd Senate, it is clear that *nae*

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

[12]For more detailed information, see our Data Appendix (Appendix B).

[13]For *Voteview*, see `https://voteview.com/`. For *Gov Track*, a project of Civil Impulse, LLC, see `www.govtrack.org`.

implies support for the draft.[14] However, in many cases the assignment is not obvious. Thus, there is a danger of misclassifying a pro-draft measure as an anti-draft one and vice versa.

For each of the 248 votes, therefore, we turned to archival records to determine the implications of an aye versus a nae. This mostly took the form of newspaper reports from the week in which a bill was debated.[15] 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" (vote 21in the 65th Senate) may appear to be a pro-draft amendment. However, reports confirm that this was favored by isolationists at the time, as the original 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 a vote to allow exemptions for certain groups is welcomed by a legislator with a draft age son. On the one hand, the son may be exempted; on the other, exemptions for other 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 Appendix Table A1, where we document draft-related votes only in sessions in which we found relevant votes that could be determined as pro- or anti-draft. In total, we code the direction of 140 votes—106 in the Senate and 34 in the House. Our main dependent variable, *Pro Draft*, is equal to 1 if a legislator voted in favor of conscription (e.g., *aye* if it was a pro-draft vote, or *nae* if it was an anti-draft vote), and 0 otherwise. The sample average is 0.58.[16]

The proposed age cutoffs attached to these votes are presented in Figure A1 and Appendix Table A2. There is more variation in the upper age cutoff than in the lower one. There is also considerably more variation in the proposed cutoffs during the two World Wars than in the two Cold War conflicts. The Vietnam War contains more roll call votes than any other war.

Biographical data The main independent variables are constructed from a combination of vote data and biographical data on legislators' family compositions. We first take basic data on legislators themselves from the *Biographical Directory of the United States Congress 1774 - 2005* (Dodge and Koed, 2005). We then use this information to locate richer household data from alternative sources. Most of this data is acquired from decennial U.S. Census records dating from 1870 to 1940.[17] These records contain information on the name, gender and age of each household member. We cross-check household data across as many Census records as possible to account for the maximum number of children that can feasibly be located. For legislators too young to have household information contained in the 1940 Census, we rely instead on a broad range of sources that include obituaries in national newspapers (mainly the *New York Times* and *Washington Post*);

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

[15]The *New York Times* was a particularly useful resource due to its consistent and detailed coverage of these bills and their amendments during the entire sampling period.

[16]Overall, there are 232 draft-related votes—successfully coded or otherwise—in these congressional sessions. The remaining 16 votes were in other congresses in which we did not successfully code any votes.

[17]We access this through *Ancestry*, a company that provides digitized and searchable Census records up to 1940 at the time of writing.

biographies on official federal and local government websites; local media profiles; university archives; and other online repositories.

We summarize the main characteristics of this data in the upper panel of Table A3 in Appendix B. Of the 2,287 legislators in our main sample of conscription votes, we have data on the number of children of 2,267 legislators (99%). Within this group, 87% have at least one child.[18] Overall, we identify 5,737 children, or 2.53 per legislator with child data. Of these children, we are missing age data for 174 (0.08 per legislator) and gender data for 10 (<0.00).[19]

In the lower panel, we summarize data at the level of a legislator-vote. On average, 26% of legislators have a son within the draft age boundaries and 25% have a daughter within the draft age boundaries. The average upper cutoff is 30.42 and the average lower cutoff is 19.53.

Vote types For most of the 140 votes, we assign treatment status according to the proposed draft age window that is associated with the bill or amendment under debate.[20]

However, for the 37 roll call votes that propose to alter the draft window itself, treatment status is less well defined. To understand why, say that legislators vote on a measure to change the draft window from 20-30 to 20-35, i.e., raising the upper cutoff from 30 to 35. A legislator with a 32-year-old son is clearly negatively impacted, and would be assigned to the treatment group. We denote these legislators as marginal. However, it is not straightforward to understand how an infra-marginal legislator with a 22-year-old son is affected by this. On the one hand, the son faces a longer duration of eligibility. On the other, the probability that he is drafted is reduced because of the larger pool of eligible draftees.[21]

To address this problem, we drop the infra-marginal legislators, leaving only the marginal group as treated. Because this necessitates a different coding procedure to assign treatment status across legislators, we separate these 37 'window' votes (7,109 legislator-vote observations) from our main analysis, leaving 103 votes (19,262 legislator-vote observations) in our main baseline sample. We present our analysis of these window votes in the appendix, where we compare legislators with sons versus those with daughters within this marginal group.

Hawks and Doves As described above, the process by which we code votes as either pro- or anti-draft reduces our sample to 140 votes out of the 232 draft-related votes that take place in the congressional sessions that we study. The remaining 92 are too ambiguous to be coded with

[18]The equivalent figure in Washington (2008) is 86% for the 105th Congress.

[19]This imbalance can arise due to obituaries, which often state the names of surviving children only.

[20]Continuing an earlier example, for vote number 63 in the 77th House on removing a limit on the number of draftees and extending the term of service, the draft age window is 21-28. This window determines a legislator's treatment status.

[21]This was an issue debated in Congress at the time ("House Votes Conscription," *New York Times*, September 8th, 1940): "The difference in age brackets between the two bills could have a profound effect on the selection results, it was asserted during the debate in the two houses. To raise the 800,000 men it is planned to train during the first year of the program would involve the selection of only one in every twenty-three registrants in the age group of 21 to 45 and one out of every thirteen under the Senate bill's age range of 21 to 31."

confidence.[22] Two drawbacks of this approach are (i) the loss of coverage due to the ambiguity of certain votes; and (ii) the level of discretion that we were required to exercise in determining the direction of each vote.

In order to test the robustness of the main results to sample selection and the authors' discretion, we develop an alternative method of measuring pro- or anti-draft preferences among legislators. Drawing on a variety of narrative sources, including historical accounts and archival newspaper articles, we identify at least two well-known foreign policy "hawks" and two well-known foreign policy "doves" during each Congress in both the House and the Senate. We use this information to create a new variable, *Hawkish Vote (Narrative)*, which is equal to 1 if the modal vote among the hawks in a given legislator's congress-chamber is in favor of a measure and the modal vote among doves is against it. Similarly, it is equal to 0 if the modal dove vote is in favor of a measure and the model hawk vote is against it. The variable is only defined in cases where there is a unique mode among hawks and a (different) unique mode among doves. The correlation coefficient between *Hawkish Vote (Narrative)* and our main *Pro Draft* outcome variable is 0.92.

To supplement this 'narrative' approach to identifying hawks and doves, we additionally employ an 'extrapolation' approach based on our *Pro Draft* variable. Here, we identify hawks as those who vote in favor of conscription in at least 75% of votes and doves as those who vote against conscription in at least 75% of votes. We create a new variable, *Hawkish Vote (Extrapolation)*, by following the same process as above. The correlation coefficient between *Hawkish Vote (Extrapolation)* and our main *Pro Draft* outcome variable is 0.96, and the correlation between both hawkish vote variables is 0.90.

Supplemental materials We include in the Data Appendix (Appendix B) a more detailed description of the biographical data, the roll call vote data, and the hawks and doves data. In Appendix C, we provide more background on the legislative decisions that we study. We discuss the costs and benefits of conscription that were postulated during debates on the floor (or in committee) at the time, and we consider the additional private costs incurred by treated legislator. We estimate that the probability of a soldier dying conditional on serving during our study period is 1.2%, which implies that a draft *registrant* had a 0.2% probability of being killed in battle.[23] When one includes other long run mental, physical and labor-market costs of combat such as those identified in Angrist (1990) and others, it is evident that around a quarter

[22]For example, a House amendment in 1951 that proposed to prevent draftees from being sent to Europe, which some viewed as limiting the scale of the draft while others viewed it as increasing the likelihood that draftees would be sent to Korea, which was potentially more dangerous.

[23]This is around 17 times greater than the probability of dying in a traffic accident in the U.S. in 2019.

of legislators had a non-trivial role in determining the risks of battle faced by their own sons.[24]

4. Estimation

We employ three empirical approaches to determine if additional exposure to the private costs of conscription influences a legislator's vote. Each approach harnesses a different source of variation. Our main approach exploits cross-sectional variation in the gender of a legislator's draft age child. Our second approach exploits cross-sectional variation in the age of a legislator's son around the upper cutoff. Our third approach exploits panel variation along both dimensions.

A. Cross-sectional variation in sex composition of draft age children

Estimating equation We restrict the sample to legislators who have at least one draft age child of any sex for vote v. This represents 52% of the legislator-votes for which the *Pro Draft* outcome variable is nonmissing. Our main specification is:

$$V_{iv} = \alpha_v + \kappa_{iv} + \sigma_{iv} + \beta_1 \textit{draft son}_{iv} + \mathbb{X}'_{iv}\zeta + \epsilon_{iv}, \tag{3}$$

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

The parameter β_1 represents the additional impact of having at least one draft age son relative to having at least one draft age daughter. Our identifying assumption is that $\textit{draft son}_{iv}$ is independent of the error term. This is violated if having a draft age son is related to any of the other determinants of optimal voting in equation (2)—voter preferences, party preferences, and ideology—conditional on the other covariates. The inclusion of fixed effects for total number of children and total number of sons is important in this regard, as the number and sex composition of one's children will affect the likelihood that one has a draft age son while also potentially influencing one's ideological preferences or even voter preferences. Including these fixed effects is made possible by the age dimension of the treatment. Essentially, we are comparing the effect

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

of having a draft age son versus having a draft age daughter among legislators with the same total number of sons and daughters. Vote fixed effects ensure that we are holding constant the content of the measure on the floor, while also absorbing chamber fixed effects and the most granular time fixed effects possible. Conditional on these covariates, we assume that variation in *draft son*$_{iv}$ is as good as random.

It is important to note that the comparison group in this setup—legislators with draft age daughters—may themselves be affected by the passing of conscription if they have a son-in-law who is exposed. A cursory comparison of means suggests that support for conscription is 6.42 percentage points lower in this group relative to those with daughters outside of the draft window.[25] This suggests that our estimate of β_1 will be conservative relative to the treatment effect that we estimate with a regression discontinuity design below.[26]

Finally, in defining the treatment variable it is necessary to determine the appropriate number of lead years for the lower cutoff. If, say, the lower cutoff is at its median value of 19, then a legislator with an 18-year-old son is effectively treated, since they are potentially exposed to the draft for the full duration of the window. Furthermore, the comparison legislators for these leading cohorts are less likely to be exposed through a son-in-law, as teenage daughters are less likely to be married than older daughters. Since determining the optimal number of lead years is somewhat arbitrary, we present treatment effects for various interpretations of the effective lower boundary. This exercise is discussed in more detail in Appendix D. It indicates that we use a definition of *draft son*$_{iv}$ that includes four leading years, which means that the sample includes legislators with 15-year-old children when the proposed lower cutoff is 19, for example.

Balance on observables In Appendix Table A6, we present tests for balance between our treatment and comparison groups across five variables: an indicator for whether a legislator is a member of the Democratic Party; an indicator for whether a legislator is a senator; age in years; number of terms in congress; and a measure of 1939 income in dollars per year that we gather from the 1940 Census, which is therefore incomplete but ought to be balanced in any case. In Panel A, we regress each variable on *draft son*$_{iv}$ and fixed effects for number of children and number of sons. In Panel B we present an unconditional balance test in which those fixed effects are omitted. This panel also includes the constant, which is the sample average for the comparison group. The sample average for the treatment and comparison combined is presented

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

[26] We do not have data on sons-in-law due to the familiar problem of matching census records over time for women who adopt their husbands' names. We also forgo analyzing data on grandchildren, which poses a similar obstacle. However this is not likely to affect our estimate, as grandchildren ought to be distributed equally in expectation (by sex and by age) across the treatment and comparison groups. Separately, Washington (2008) shows that legislators with daughters support higher defense spending. If these preferences are greater when a legislator's daughter is within the draft age window, then our estimate of β_1 may be biased away from zero. The comparison of means above suggests that this is not a realistic concern. In any case, our regression discontinuity design circumvents this issue. Finally, Oswald and Powdthavee (2010) find that having a daughter increases left-wing tendencies. If left wing tendencies extend to opposing conscription in this sample, this would further attenuate our estimate of β_1 (although this is likely absorbed by our fixed effects).

Table 1: Effect of Draft Age Son vs. Draft Age Daughter

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.1102***	-0.1104***	-0.0707*	-0.0756**
	(0.0375)	(0.0367)	(0.0369)	(0.0371)
Controls	No	Yes	No	Yes
Vote FE	No	No	Yes	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Legislators	1427	1427	1427	1427
Votes	103	103	103	103
Mean Dep. Var.	0.58	0.58	0.58	0.58
Observations	9920	9920	9920	9920

Note: The unit of analysis is the legislator-vote. The sample contains all legislator-votes for which the legislator has at least one draft age child. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

in the second-to-last row of the table. In no specification do we detect a significant difference between our treatment and our comparison group along any of the five dimensions.

Results We present the main results in Table 1. In column (1), we control only for fixed effects for number of children and number of sons. We find that having a draft age son reduces the probability of voting for conscription by 11.02 percentage points (p < 0.01), from a mean of 0.58. Adding controls for party, chamber, age, age squared and terms in office makes very little difference to the estimate (column 2). In column (3) we add vote fixed effects to the baseline model, which yields a treatment effect of –7.07 percentage points (p < 0.10). Finally, in column (4), we again add controls in addition to vote fixed effects, yielding a treatment effect of –7.56 percentage points (p < 0.05), or 13.03% of the mean.

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

Additional analysis in the Online Appendix In Appendix D, we plot estimates of β_1 using different values of the effective lower cutoff age for each of the models in Table 1 (Appendix Figure A2). In all four models, point estimates smoothly rise from the one-year lead to the four-year lead, before falling off at five years.[27] This pattern is likely due to leading cohorts—i.e., legislators with sons within 4 years of the lower cutoff—being more intensely treated *relative to their comparison cohorts* because their exposure is less likely to be offset by a countervailing son-in-law effect (Appendix Table A5).

[27]The mean duration of wartime conscription per conflict is 4.6 years, suggesting that politicians' revealed expectations are reasonably accurate. Before the Vietnam War, the mean duration is 3.3 years.

In Appendix Table A7, we show that the results are robust to the omission of fixed effects for number of sons and number of children. The estimates range from around 4.9 to 7 percentage points. In Appendix Table A8, we analyze window votes. In Panel A, we estimate the same four specifications using only the sample of 37 window votes. We begin with a two-year lead in how we define the draft window, which is the optimal lead structure suggested in Appendix Figure A3.[28] This implies that our sample contains legislators with 'marginal' children plus those with children within two years of the proposed lower cutoff. The estimates are negative and significant across all specifications. In the most comprehensive specification (column 4), we estimate a treatment effect of –11.08 percentage points ($p<0.05$). In Panel B, we combine the window votes (with a two-year lead) with our main sample votes (with a four-year lead) for a total of 140 roll call votes. In Panel C, we combine both sets of votes using a four-year lead. All estimates are again negative and statistically significant.

Hawks and Doves In Panel A of Table A9, we estimate the same four specifications as in Table 1, only now using *Hawkish Vote (Narrative)* (columns 1–4) and *Hawkish Vote (Extrapolation)* (columns 5–8) as the dependent variables rather than *Pro Draft Vote*. Here, the sample is no longer restricted to votes that were amenable to manual coding. In assigning legislators to treatment or control groups, we use the draft age thresholds relevant to individual roll call votes where possible. Otherwise, we use the thresholds that were most recently passed in a given chamber. In all eight specifications, the treatment effect is negative and statistically significant, ranging from –6.7 ($p<0.10$) to –11.5 percentage points ($p<0.01$).

In Panel B, we restrict the hawks and doves sample to draft-related votes that are *not* included in our main sample in Table 1. This is to check that the results in Panel A are not driven entirely by the manually coded votes. Using this non-overlapping sample, we again find negative effects across all eight specifications. The coefficients range from –3.7 to –8.7 percentage points, although, due in part to the restricted sample size, the estimates are not statistically significant.

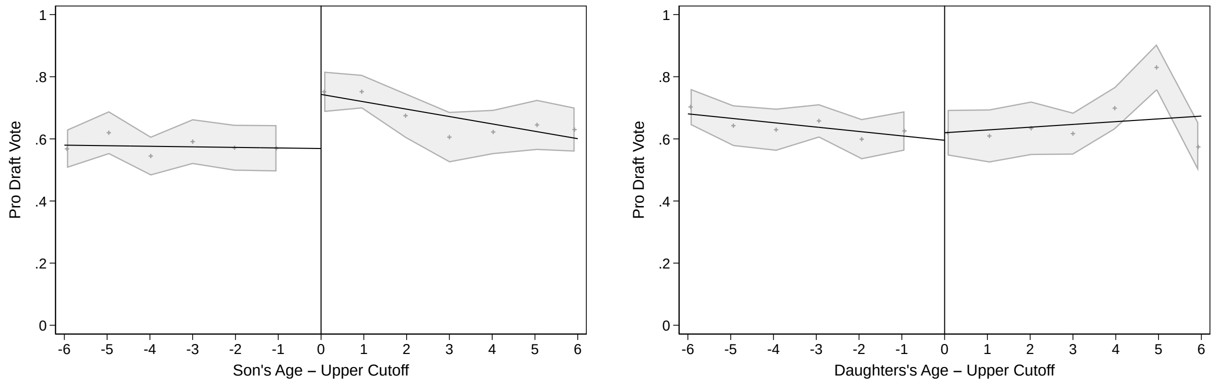
In Panel C, we repeat the exercise on the universe of votes in draft-era congresses that are *unrelated* to the draft. The point estimates are all very close to zero and none of them are statistically significant.[29]

B. Cross-sectional variation in age of sons around upper cutoff

Estimating equation For our second approach, we rely on variation in the age of sons rather than variation in the gender of a child at a given age. Because variation in age is not exogenous, we employ a regression discontinuity design around the upper cutoff. The logic is straightforward: legislators with sons marginally beneath the cutoff are exposed to conscription while legislators with sons marginally above the cutoff are not. Otherwise, they are comparable.

[28]This perhaps reflects the fact that window votes tended to occur closer to the ends of wars, when they were seen as an attempt to accelerate the completion of combat operations. On average, window votes occurred around 1.8 years before the end of battle operations, whereas the equivalent figure for our main sample votes is 3.1 years.

[29]For a graphical presentation of all 24 estimates in this table, please see Appendix Figure A6.



(a) RD Effect of Son's Age at Upper Cutoff

(b) RD Effect of Daughter's Age at Upper Cutoff

Figure 1: **Regression Discontinuity Plots.** These plots correspond to estimates in Table A10. The estimate for ρ in part (a) is 0.1879 ($p<0.05$). The placebo estimate in part (b) is -0.0044 ($p>0.10$).

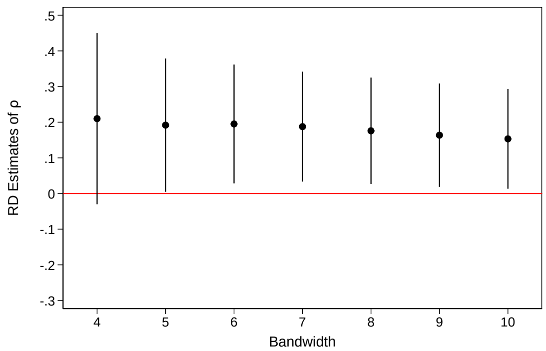
We restrict the sample to legislators who have one son at the time of vote v. This implies that the treated and control groups ought to be unconditionally balanced on observables, obviating the need for covariates in our main estimating equation. Following Gelman and Imbens (2019), we estimate the following local linear model as our baseline specification, for $RV_{iv} \in (-h, h)$:

$$V_{iv} = \delta_0 + \rho \mathbf{I}(RV_{iv} > 0) + \delta_1 RV_{iv} + \delta_2 RV_{iv} \times \mathbf{I}(RV_{iv} > 0) + \varepsilon_{iv}, \tag{4}$$

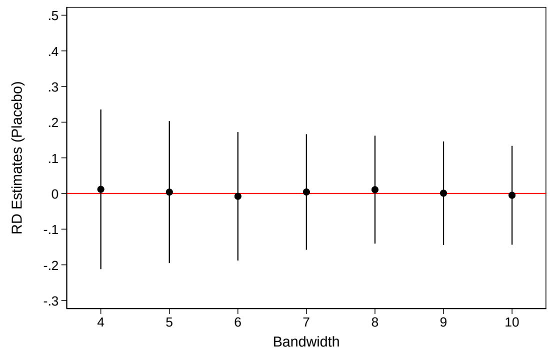
where RV_{iv} is the running variable (son's age minus the upper cutoff); $\mathbf{I}(RV_{iv} > 0)$ is an indicator equal to 1 if RV_{iv} is positive (i.e., if the son's age is above the upper cutoff); δ_0 is a constant; and h is the bandwidth. The parameter ρ captures the effect of having a son outside of draft eligibility relative to having a son exposed to the draft. A positive estimate indicates that legislators take into account their private incentives when voting, supporting the first analysis. In this case, however, the estimate is not attenuated by the son-in-law effect that is likely present when we use legislators with draft age daughters as a comparison group.

To estimate this model, we rely on the procedure developed in Calonico, Cattaneo and Titiunik (2014). This has a number of advantages: it computes both conventional estimates and estimates that are corrected for leading bias; it accommodates discrete running variables; and it automates the choice of many 'tuning parameters' that are usually left to the discretion of researchers. Thus, we adopt the default selection of the data-driven MSE-optimal bandwidth h, the choice of kernel (triangular), and the procedure used to compute standard errors (nearest neighbor, allowing for legislator clusters and adjusting for mass points due to our discrete running variable).

Results We present our estimates in Appendix Table A10 and Figure 1. Conventional regression discontinuity estimates are presented in the upper panel of Table A10, while bias-corrected estimates are presented in the lower panel alongside conventional standard errors as well as robust standard errors from Calonico et al. (2014) (CCT). In column (1), we show that the conventional RD estimate is 18.8 percentage points ($p<0.05$) and the bias-corrected estimate is 21.95 percentage points ($p<0.01$ and $p<0.05$ using conventional and CCT robust standard errors respectively). In



(a) RD Effect of Son's Age at Upper Cutoff



(b) RD Effect of Daughter's Age at Upper Cutoff

Figure 2: **Sensitivity to Bandwidth Choice**

column (2), we show the equivalent RD estimate using a placebo running variable based on the age of a legislator's only daughter relative to the upper cutoff. In clear contrast to the first column, the estimates are close to zero and statistically insignificant.

We present graphical evidence of these effects in Figure 1. In part (a), on the left hand side, we show the plot corresponding to the conventional estimate in column (1) of Table A10, together with 95% confidence intervals and binned means for each year. The relationship is linear up to a stark discontinuity at the upper cutoff. In part (b), on the right hand side, we present the placebo estimate based on the age of a legislator's daughter. Here, we see a tightly estimated zero at the cutoff, implying a large 'difference-in-discontinuities' estimate.

For these estimates, the data-driven bandwidth is 6.93 and 6.17 years respectively. We examine sensitivity to this bandwidth selection in Figure 2. We present RD estimates using bandwidths ranging from 4 to 10 years. In part (a), we see that the RD estimate of a son's age at the upper cutoff remains large, significant and markedly stable. Similarly, the placebo RD estimate in part (b) is consistently at or very close to zero.

In columns (3) to (5) of Appendix Table A10 we test for balance using the same set of observable variables as in the previous section: an indicator for whether the legislator is from the Democratic Party, age, an indicator for whether they are a senator, and a measure of income (in dollars per year) from the 1940 Census. In no case do we detect a significant RD estimate. The corresponding figures for these balance tests are presented in Appendix Figure A7.

Finally, in Appendix Table A11, we show that our main results are robust to the inclusion of controls, vote fixed effects, and the combination of both. In all three specifications, the RD estimate is large and significant, ranging from 16.64 percentage points ($p < 0.05$) to 23.18 percentage points ($p < 0.01$), while the equivalent placebo estimates are not significant.

C. Panel variation in age and sex of children

Estimating equation For our third approach, we combine within-legislator variation in exposure along the age dimension with between legislator variation in the sex composition of children. This allows us to control for legislator fixed effects, which implies that we are holding constant

Table 2: Effect of Draft Age Son with Legislator FE

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.1060***	-0.1006***	-0.0642**	-0.0600**
	(0.0372)	(0.0353)	(0.0297)	(0.0292)
Draft Age Child	0.0242	0.0092	0.0251	0.0158
	(0.0317)	(0.0310)	(0.0267)	(0.0279)
Controls	No	Yes	No	No
Vote FE	No	No	Yes	Yes
Legislator FE	Yes	Yes	Yes	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Legislators	711	711	711	711
Votes	103	103	103	103
Mean Dep. Var.	0.62	0.62	0.62	0.62
Observations	9249	9249	9249	9249

Note: The unit of analysis is the legislator-vote. The sample contains all legislators who exhibit variation in treatment or control status. Standard errors are two-way clustered by legislator and vote. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

time-invariant factors such as legislators' ideological preferences and fixed characteristics related to their electorate. We restrict the sample to 'switchers'—i.e., those who exhibit variation in having any sons or daughters within the effective draft age boundaries. This represents 48% of the legislator-votes for which the *Pro Draft* outcome variable is nonmissing. The specification is:

$$V_{iv} = \alpha_i + \alpha_v + \kappa_{iv} + \sigma_{iv} + \beta_1^{FE} \textit{draft son}_{iv} + \beta_2^{FE} \textit{draft child}_{iv} + \mathbb{X}'_{iv}\zeta^{FE} + \epsilon_{iv}^{FE}, \tag{5}$$

where α_i represents legislator fixed effects and $\textit{draft child}_{iv}$ is an indicator for whether legislator i has a draft age child of any sex for vote v.

Results The baseline specification is presented in column (1) of Table 2. The estimate of β_1^{FE} is –10.6 percentage points (p<0.01). Adding the time varying controls makes little difference (column 2). We include vote fixed effects in column (3), which reduces the magnitude of the estimate to –6.4 percentage points (p < 0.05). Finally, in column (4), we again add the time varying controls, yielding an estimate of –6 percentage points (p < 0.05). These estimates are broadly in line with the cross-sectional results in Table 1.

We present the full set of 'Hawks and Doves' results in Appendix Table A12. In Panel A, we estimate negative and significant effects across all specifications and for both the *Hawkish Vote (Narrative Method)* and *Hawkish Vote (Extrapolation Method)* outcome variables. In Panel B, we again find negative and significant estimates across all eight specifications in the sample of draft-related votes that are *not* included in our main sample. In Panel C, we show that treated legislators do not vote differently on votes that are unrelated to the draft.[30]

[30]These estimates are presented graphically in Appendix Figure A8.

D. Interpreting Magnitudes

Our estimates range in magnitude from around 6 to 18.8 percentage points, depending on the specification.[31] To put these figures into context, in Appendix F we explore the hypothesis (discussed in Section 2) that pressure to comply with the party line also played a significant role. We present a fixed effects regression indicating that party alignment with the White House Administration is associated with an 11 percentage point increase in the probability that a legislator votes for conscription. This effect is in line with our estimates.

In Appendix G, we discuss counterfactual exercises. In one, we estimate that if every legislator were exposed to the draft, 30 of the 88 votes in which the majority favored conscription would have been reversed. These votes include failed attempts in the Senate to effectively end the draft in 1970 (the Hatfield-Goldwater amendment) and to withdraw entirely from southeast Asia in 1971, two years before the Paris Peace Accords that signaled the end of U.S. involvement in Vietnam.

5. Establishing Causal Mechanisms

We interpret our results as evidence that political agents are motivated directly by pure self-interest: legislators are less likely to vote in favor of conscription when they are personally exposed to its costs; otherwise their behavior is no different to that of other legislators. According to this mechanism, the change in voting behavior that we observe is due only to variation in private rents, i.e., R_{it} from equation 1.

An alternative mechanism is due primarily to information. Legislators who are initially exposed to the costs of conscription may invest more effort in learning about its consequences. As a result, they ultimately vote against conscription because of updated ideological preferences (which determines F_i) or changes in how they perceive political concerns (i.e., M_{it}). With this interpretation it is possible that our estimates are consistent with the classic model of legislative voting. Pure self-interest may have spurred the legislator to learn more about the consequences of the vote, but thereafter the legislator may be motivated by reelection or ideological preferences.

To disentangle the *private rents* channel from this alternative *information* channel, we examine the behavior of legislators as they exit treatment status. This occurs when a legislator's youngest son ages out of draft eligibility at the upper threshold. Under the private rents interpretation, a legislator will change their voting behavior immediately as their son ages out of eligibility. Under the information interpretation, however, a legislator will maintain their opposition to the draft as their son ages out of eligibility. Their concern for other families ought to remain intact—or at least decline more gradually—even as their own son is no longer at risk.

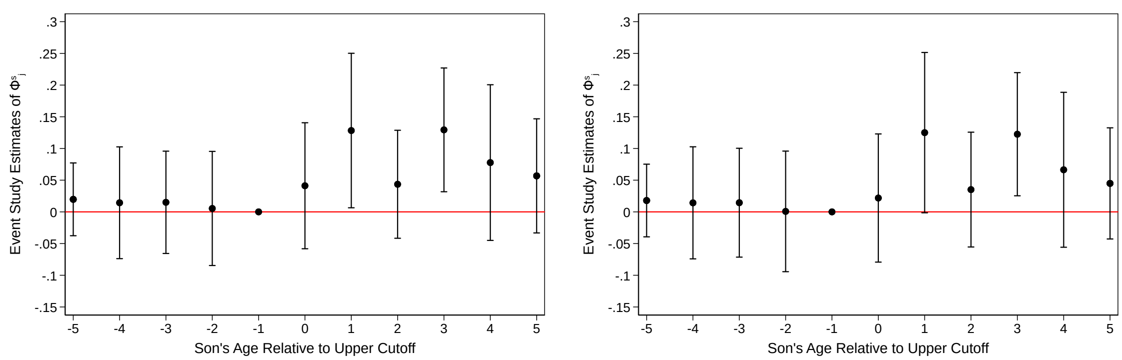
[31]The RD effect is largest, due possibly to the additional sensitivity to the draft of legislators who have only one son at the cutoff, in addition to the absence of a countervailing son in law effect.

This test lends itself to an event study design. We estimate the following equation:

$$\begin{aligned} V_{iv} =& \alpha_i + \alpha_v + \kappa_{iv} + \sigma_{iv} + \sum_{j=-27, j\neq -1}^{36} \Phi_j^s \cdot \mathbf{I}(\textit{son relative age}_{iv} = j) \\ &+ \sum_{j=-27, j\neq -1}^{36} \Phi_j^c \cdot \mathbf{I}(\textit{child relative age}_{iv} = j) + \mathbb{X}'_{iv}\zeta^{ES} + \epsilon_{iv}^{ES}, \end{aligned} \tag{6}$$

where *son relative age*$_{iv}$ is the age of legislator i's youngest son relative to the upper cutoff for vote v, and *child relative age*$_{iv}$ is the age of legislator i's youngest child of any sex relative to the upper cutoff for vote v. Negative values are defined only if the child is within the draft age boundaries, implying that legislators are treated if *son relative age*$_{iv} < 0$ and untreated if *son relative age*$_{iv} \geq 0$. Thus, the indicator function $\mathbf{I}(.)$ is positive for vote v only if legislator i's youngest child is either exposed to the draft or is older than the upper cutoff. The variables range from –27 to 36.[32]

Positive estimates of Φ_j^s for $j \geq 0$ indicate that legislators increase support for conscription when their sons age out of eligibility relative to when their daughters age out. This is consistent with the *private rents* mechanism. Estimates of Φ_j^s that are equal to zero for $j \geq 0$ indicate that legislators do not change their behavior when their personal exposure to conscription ends. This is consistent with the *information* mechanism.



(a) Estimates from Eq. 6 without Controls

(b) Estimates from Eq. 6 with Controls

Figure 3: **Event Study Plots: Change in Pro Draft Vote as Sons Age out of Draft Eligibility**. The corresponding estimates are presented in Table A13.

Results In Figure 3, we present the event study plot associated with equation 6. We present estimates of Φ_j^s for $j = -4, -3, -2 \ldots 4$ together with the cumulative estimates for $j \leq -5$ and $j \geq 5$. The additional controls in $\mathbb{X}'_{iv}$ are omitted in panel (a) and included in panel (b).

The estimates are very close to zero for all negative values of *son relative age*$_{iv}$. As a legislator exits treatment status, however, we see a clear and significant increase in the probability that they vote in favor of conscription. This is evident in both figures. Focusing on the more comprehensive specification, one year after exiting treatment status, legislators are 12.5 percentage points more likely to support conscription; three years after exiting, the difference is 12.25 percentage points

[32]The draft age window was from 18 to 45 in 1918 and 1920.

($p < 0.10$ and $p < 0.05$, respectively); thereafter, the effect diminishes but stays positive. The corresponding estimates are presented in Appendix Table A13. The combined estimates are positive across all four specifications, and are significant for specifications that include either vote fixed effects (2), controls (3), or both (4).

These results provide evidence in favor of the *private rents* mechanism, indicating that politicians are influenced by private incentives that are independent of ideological or political concerns.

6. Political Agency and Voter Behavior

In Appendix E, we endogenize the behavior of the electorate in order to better understand the dynamics of politicians' decisions. We summarize the main insights below.

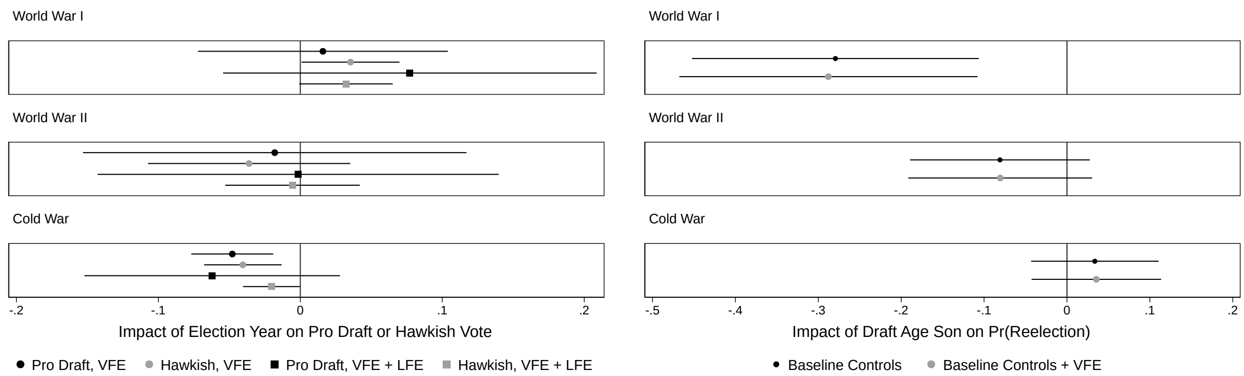
Conceptual framework We consider a model that combines moral hazard with adverse selection (Besley, 2006). The electorate is the principal and politicians are agents who enact legislation on its behalf. Informational problems can arise if politicians can hide effort or motives. There are two politician types. 'Good' politicians enjoy political power and always choose to enact voters' preferred policies. 'Bad' politicians additionally enjoy private rents that can be obtained by deviating from the popular policy choice. Elections serve the twin purposes of restraining politician behavior—as in 'pure' moral hazard models (Barro, 1973, Ferejohn, 1986)—and selecting good politicians who care about voter welfare. In chasing private rents, therefore, bad politicians can mimic good ones in order to disguise their type to the electorate and win reelection.[33]

By definition, good politicians place no weight on private rents. They select the popular policy choice in a given period t and are consequently reelected to serve in $t + 1$. By contrast, bad politicians first observe the draw of private rents that is available to them in period t. They can then either vote *for* the popular policy choice, thereby mimicking a good type and winning reelection; or, they can vote *against* it, thereby gaining the private rents and losing reelection. This decision is determined by the relative value of these paths.

There are two central propositions arising from this model. The first is that politicians (on average) respond to private rents, as we have already shown. The second is that a shock to private rents will affect the probability of reelection. If the shock induces the politician to select the popular policy choice, then they will be more likely to win. If the shock induces the politician to eschew the popular policy choice, then they will be more likely to lose.

Testing implications Applying this model to our setting generates the following prediction: draft exposure *decreases* the probability of reelection when conscription is popular and *increases* the probability of reelection when it is unpopular. This prediction arises when one interprets draft exposure as an exogenous shock that alters the net private rents available to bad politicians voting on conscription. When conscription is popular, exposure increases the private rents available to politicians who vote against it. When the conscription is unpopular, exposure decreases the private rents available to politicians who vote in favor of it.

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



(a) Effect of Election Proximity on Pro Draft Voting

(b) Effect of Draft Exposure on Reelection.

Figure 4: **Draft Popularity and Reelection of Exposed Legislators over Time.** The estimating equations are presented in Appendix E. See Tables A14 and A15 (part a) and Table A17 (part b).

To complete our set of empirical predictions, it is necessary to determine the popularity of conscription over time. We take two approaches. First, we turn to nationally representative data on public support for conscription, which shows a sharp decline from around 70% in 1945 to around 20% in 2003 (Fordham, 2016).[34] Second, we estimate the impact of election proximity on legislative voting behavior over time. Legislators facing reelection ought to be more likely to select popular policies relative to other legislators. Since senators serve six-year terms with staggered elections every two years, we can compare the voting behavior of those facing reelection versus those who are not by controlling for vote fixed effects.[35]

The results of this exercise are presented in Figure 4a and in Appendix Tables A14 and A15. We find that legislators facing reelection were more likely to vote in favor of conscription during World War I, and less likely to do so during the Cold War conflicts.[36] This is consistent with the survey evidence and also with narrative accounts that stress the effectiveness of the U.S. government's propaganda and censorship efforts during World War I—best characterized by George Creel's 'Committee on Public Information' (Axelrod, 2009, Hamilton, 2020)—and of the mass anti-conscription protest movement during the Vietnam War (Flynn, 1993, 2002).

Empirical patterns To test this model's prediction, we estimate the effect of draft exposure on the probability of reelection in each of the three eras. This amounts to replacing the outcome variable in equation 3 with an indicator for winning reelection to the next term.[37]

[34] This is likely due to technological change: as warfare became less labor intensive, the importance of conscription waned Fordham (2016). A second explanation relates to the salience of military casualties, which is politically costly to the incumbent (Karol and Miguel, 2007) and was a defining feature of the war in Vietnam (Flynn, 1993).

[35] For this analysis, we define election proximity as the calendar year before the November election date.

[36] Estimates using the hawks and doves extrapolation measure (Table A16) are very similar to those using the narrative measure (Table A15). In all specifications, we combine the Korean War and Vietnam War samples as there are only 8 votes in the former.

[37] The sample is restricted to legislators who competed for reelection. The data on election outcomes is from the Inter-University Consortium for Political and Social Research (1995).

We present the results of this exercise in Figure 4b and in Appendix Table A17. For each era, we estimate the specification with and without vote fixed effects. The results present a mirror image of our previous results on draft popularity over time in Figure 4a. During World War I, when the draft was most popular, legislators with draft age sons were less likely to win reelection ($p < 0.05$). The point estimates approach zero for World War II, and the sign flips for the Cold War, when conscription became significantly unpopular.

We show in Appendix Figure A10 that these differences are not driven by differences in the "first stage" relationship between draft exposure and pro-draft voting, which is negative for all three eras.[38] While we cannot rule out the influence of other trends during the 20th century, these findings are nonetheless consistent with the model's prediction that, as conscription became less popular with voters over time, legislators with draft age sons—who are more likely to oppose conscription—became increasingly more likely to win reelection.

7. Conclusion

We provide new evidence that politicians are influenced by private incentives that are independent of political or ideological motives. We demonstrate this by studying voting behavior in the U.S. Congress during the four conscription-era wars of the 20th century, when legislators frequently voted on measures that affected the number of soldiers sent to battle overseas. We find that legislators with draft age sons are significantly less likely to vote for conscription than comparable legislators. We conclude that political elites who do not internalize the costs of conflict are more likely to support it.

We interpret these results within the framework of a political agency model that combines aspects of moral hazard and adverse selection. In our application, having an exposed son introduces exogenous variation in the private rents that bad politicians can derive from voting on conscription. Consistent with this model, we show that politicians with exposed sons are more likely to be reelected when conscription is broadly unpopular.

Our analysis provides new evidence that helps to explain the puzzle of why violent conflict can occur between groups despite being costly. Agency frictions can lead to conflict precisely because these costs are not internalized by political elites. This logic can be extended to explain the persistence other seemingly inefficient policies.

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

[38] This pattern is also inconsistent with a pure moral hazard model where voters are indifferent between candidates.

8. Data Availability

The data and code needed to replicate the tables and figures in this article can be found in McGuirk, Hilger and Miller (2023) in the Harvard Dataverse, `https://doi.org/10.7910/DVN/SAYY1S`.

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

McGuirk, Hilger and Miller

For Online Publication: Appendix

Appendix A. Additional Background on Literature

Legislative Voting Motives There exists at least some empirical evidence in support of the first three elements 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. 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.

However, there also exists evidence that is not compatible with the purest interpretation of the model. For example, 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.

One could additionally include a separate motive based on supporter group preferences. This 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. Levitt (1996) finds that U.S. senators assign three times more weight to the preferences of their own supporters relative to other voters in their electorate. Brunner, Ross and Washington (2013) and Mian, Sufi and Trebbi (2010) also find evidence that is consistent with this effect.

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

As noted in the main text, evidence in favor of the third element, ideology, is demonstrated in Washington (2008).

Conflict and Agency Frictions Other papers that relax the assumption of unitary actors do so by modeling the politics of conflict from the perspective of leaders (De Mesquita and Siverson, 1995, Smith, 1996, Goemans, 2000, Tarar, 2006), or by addressing a different type of agency issue, whereby politicians must provide sufficient incentives to solve the collective action problem of raising an army (Grossman, 1999, Beber and Blattman, 2013, Gates, 2002). In our setting, this is achieved by the threat of penalties for draft evasion. The specific role of moral hazard in conflict has been applied to the case of rebel activity in the presence of external humanitarian interventions—for example, Kuperman (2008) and Crawford (2005) argue that the insurance provided by external groups protects rebel groups from the risks of rebellion, which ultimately leads to more violence—and to the case of states acting more aggressively when they have powerful allies (Christensen and Snyder, 1990, Benson, Meirowitz and Ramsay, 2014, Narang and Mehta, 2019). In the present paper, we make the related argument that politicians who are protected from the risks of conflict are more likely to support it.

Appendix B. Data Appendix

Online Replication Materials and Data Sources We include data, code, and sources in our online replication folder in the Harvard Dataverse (McGuirk et al., 2023). The URL is: `https://doi.org/10.7910/DVN/SAYY1S`.

Vote Data We summarize the roll call vote data in Table A1, where we document draft-related votes only in sessions in which we found relevant votes that we could determine as pro- or anti-draft. In total, we code the direction of 140 votes—106 in the Senate and 34 in the House. In column (1), we list the number of draft-related votes included in our main sample. In column (2), we present our main dependent variable: *Pro Draft* is equal to 1 if a legislator voted in favor of conscription (e.g., *aye* if it was a pro-draft vote, or *nae* if it was an anti-draft vote), and 0 otherwise. This exhibits a large amount of variation: the sample average is 0.58. In column (3) we present the average absolute margin between aye and other votes (nae or abstentions). For example, there is one vote in the 89th Senate; Pro Draft is 0.93, which means the margin is $0.93 - (1 - 0.93) = 0.86$, the gap between the winning vote and the losing vote. The final two columns present all draft-related votes—i.e., successfully coded or otherwise. The overall number is 232, as the remaining 16 were in other congresses in which we did not successfully code any votes. We cannot present the same information for the outcome variable, but we do present the average margin to facilitate a comparison with our main sample. The average margins are 0.18 in our main sample and 0.17 in the total sample, suggesting that there is no major difference between votes that we could and could not code along this dimension.

We present variation in the upper and lower proposed draft cutoffs over time at the level of a vote in Figure A1. The corresponding data at the level of a legislator-vote is presented in Table A2.

Table A1: Summary of Roll Call Votes by Congress

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

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

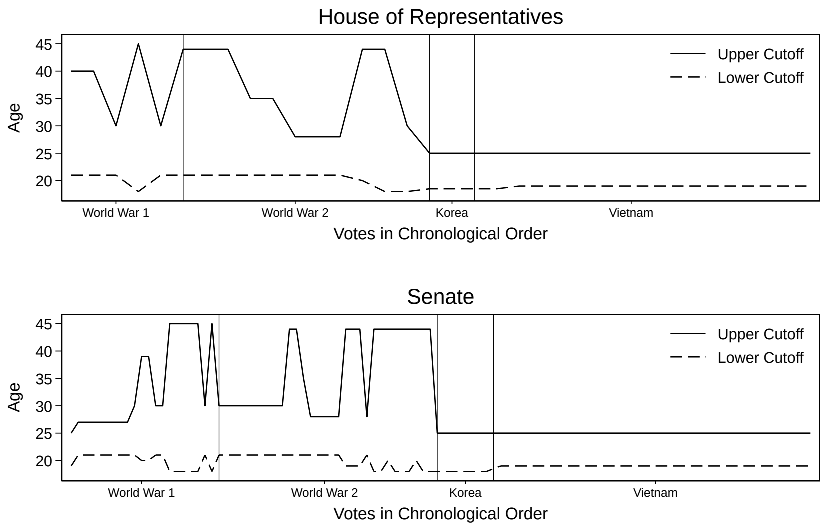


Figure A1: **Age Cutoffs Over Time in the House and Senate**. This figure depicts variation over time in the proposed upper and lower age cutoffs for draft eligibility at the level of a roll call vote. Votes are arranged in chronological order. The red lines demarcate eras. There are 106 votes in the Senate and 34 in the House. See Appendix Table A2 for summary statistics.

Table A2: Age Thresholds

Lower Cutoff	Freq.	Percent
18	3,555	13.48
18.5	1,883	7.140
19	11,245	42.64
20	892	3.382
21	8,798	33.36
Total	26,373	100

Upper Cutoff	Freq.	Percent
25	13,593	51.54
27	888	3.367
28	2,010	7.621
30	2,840	10.77
35	1,013	3.841
39	222	0.842
40	912	3.458
44	3,783	14.34
45	1,112	4.216
Total	26,373	100

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

Biographical Data Our research team gathered by hand the biographical data on legislators and their children. The first step is to locate legislators' families in as many Census records as possible from 11 congresses in the Senate and 10 in the House running from 1917 to 1974. We use decennial Census data from 1870 to 1940 for this task. We use multiple Census records in order to document the age and gender of legislators' children before they exit the household.

If there is no Census record containing this data, we use a variety of alternative sources. One approach is to use obituaries in national newspapers, such as the *Los Angeles Times*, the *New York Times* and the *Washington Post*, and in local newspapers, such as the *Tampa Bay Times* and the *Hartford Courant*. In many cases, this provided the number and gender of children who survived the legislator, but not the age. Other sources include biographies on government websites (e.g., `history.house.gov`), online university archives (e.g., `https://finding-aids.lib.unc.edu/05111/` for Lunsford Richardson Preyer and `https://quod.lib.umich.edu/b/bhlead/umich-bhl-2013102?rgn=main;view=text` for Donald W. Riegle, Jr.) and miscellaneous websites such as *Wikipedia*, the *Notable Names Database* (`https://www.nndb.com/`), *Biography.com* and *Legacy.com*. The full list of biographical sources is in the online replication folder (McGuirk et al., 2023).

We summarize the main characteristics of this data in the upper panel of Appendix Table A3. There are 2,287 legislators in our main sample of conscription votes. Of these, we have data on the number of children of 2,267 legislators (99%). Within this group, 87% have at least one child. The equivalent figure in Washington (2008) is 86% for the 105th Congress. Overall, we identify 5,737 children, or 2.53 per legislator with child data. Of these children, we are missing age data for 174 (0.08 per legislator) and gender data for 10.[A1]

In the lower panel, we summarize data at the level of a legislator-vote. There are 26,373 observations in the 140 votes for which the *Pro Draft* outcome variable is coded. The sample average of that variable is 0.58. The average upper cutoff is 30.42 years of age and the average lower cutoff is 19.53. We have 103 main votes and 37 window votes. Casting the main votes, on average, 26% of legislators have a son within the draft age boundaries and 25% have a daughter within the draft age boundaries. Expanding this to include a four-year lead period as we do in the main analysis, the figures are 37% and 35%. Casting the window votes, the equivalent figures are 12% and 11% (no lead) and 16% and 15% (two-year lead).

Hawks and Doves We also list the hawks and doves for each conflict period in the online replication folder (McGuirk et al., 2023). We compile these lists from background reading materials, including books on conscription in the U.S. and newspaper reports on various conscription debates in Congress. The sources are included in the data file.

[A1] This imbalance can arise due to obituaries, as mentioned above.

Table A3: Descriptive Statistics for Analysis Sample

	Mean	SD	Count	Min	Median	Max
	Legislator Level Variables					
Have Data on Number of Children	0.99	0.09	2,287	0	1	1
Num. Children Missing Age Data	0.08	0.38	2,287	0	0	4
Num. Children Missing Gender Data	0.00	0.08	2,287	0	0	2
Any Children	0.87	0.33	2,267	0	1	1
Number of Children	2.53	1.88	2,267	0	2	14
Any Sons	0.68	0.47	2,259	0	1	1
Number of Sons	1.23	1.22	2,259	0	1	9
Any Daughters	0.65	0.48	2,259	0	1	1
Number of Daughters	1.15	1.16	2,259	0	1	7
Income in 1939 (From 1940 Census)	2978.07	2385.95	991	0	3,600	17,000
	Legislator-Vote Level Variables, 140 Votes					
Pro Draft Vote	0.58	0.49	26,373	0	1	1
Proposed Upper Draft Cutoff	30.42	7.48	26,373	25	25	45
Proposed Lower Draft Cutoff	19.53	1.11	26,373	18	19	21
Age	54.59	10.72	26,373	25	54	90
Terms	2.96	1.97	26,373	1	2	11
Democrat	0.58	0.49	26,373	0	1	1
Senator	0.42	0.49	26,373	0	0	1
Main Votes	0.73	0.44	26,373	0	1	1
Draft Age Son	0.26	0.44	19,264	0	0	1
Draft Age Daughter	0.25	0.44	19,264	0	0	1
Draft Age Child	0.39	0.49	19,264	0	0	1
Draft Age Son (4 Year Lead)	0.37	0.48	19,264	0	0	1
Draft Age Daughter (4 Year Lead)	0.35	0.48	19,264	0	0	1
Draft Age Child (4 Year Lead)	0.52	0.50	19,264	0	1	1
Window Votes	0.27	0.44	26,373	0	0	1
Draft Age Son	0.12	0.32	7,109	0	0	1
Draft Age Daughter	0.11	0.31	7,109	0	0	1
Draft Age Child	0.19	0.39	7,109	0	0	1
Draft Age Son (2 Year Lead)	0.16	0.37	7,109	0	0	1
Draft Age Daughter (2 Year Lead)	0.15	0.36	7,109	0	0	1
Draft Age Child (2 Year Lead)	0.26	0.44	7,109	0	0	1

Note: The legislator-level biographical data in the top panel is predominantly from Census records from 1870-1940. Where Census records were not located, a variety of other sources were used. The legislator-vote data combines biographical data with data on the substance of roll call votes relating to conscription in the U.S. Congress during the 20th century. See Section 3 and Appendix B for information on how this was assembled.

Appendix C. Background on Draft Voting in Congress

In this section we consider (i) the social costs and benefits of conscription that were postulated during debates on the floor (or in committee) at the time; and (ii) the additional private costs to a treated legislator of a draft measure passing.

On the first issue, we can learn much from archived newspaper reports about the nature of the debate surrounding conscription. For example, on the first day of the World War I draft bill debate on April 23, 1917, House Representative Julius Kahn, who led the Administration's fight for conscription, invited a British Army captain, Percy Benson, to speak to the House Committee on Military Affairs about "England's mistakes." Benson listed five main reasons why the U.S. 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.[A2]

In one form or another, many of these points were repeated over the course of draft-era warfare in the U.S. Congress, although, as we discuss in Section 6, the necessity of the draft as a means of securing a sufficient number of soldiers waned as conflict technology became more capital intensive over time (Fordham, 2016). Other arguments against the draft were varied. Unions were consistently opposed to conscription because they viewed it as a form of class exploitation, most likely because the alternative—raising military pay—would increase unions members' bargaining power and wages.[A3] Relatedly, others viewed the draft as an opportunity for special interests to profit,[A4] while isolationists and pacifists opposed conscription as part of their general opposition to interventionist foreign policy.[A5]

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

[A2]"Draft Bill Debate is to Begin Today," *The New York Times*, April 23 1917. The New York Times also ran an opinion piece quoting Abraham Lincoln's defense of the draft during the Civil War, in which he argued in favor of distributing the burden of warfare widely ("A Conscriptionist," *The New York Times*, April 26 1917)

[A3]"Unions Oppose the Draft – Resolution Adopted Unanimously by Central Federation," *The New York Times*, April 1 1917

[A4]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."

[A5]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.)

The amendment was defeated, 130 to 86.[A6]

To understand the additional private costs incurred by treated legislators, we compile in Table A4 data on U.S. draft registrants, draft deployments, total service-members, and total fatalities for each war in our dataset. In total, just over 109 million men were registered for the draft over the four conflicts. Of those, 16.3 million (or 15%) were inducted. This is just under one half of the 35.3 million total service members. Total U.S. fatalities are estimated at 426,132, implying a 1.2% probability of death conditional on serving. Assuming that draftees were killed at the same rate as regular service members, a draft *registrant* had a 0.2% probability of being killed in battle.[A7] Note also that this does not take into account other long-run mental, physical and labor-market costs of conflict such as those identified in Angrist (1990) and many others. Many legislators, therefore, had a non-trivial role in determining the risks faced by their own sons in battle.

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

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

[A6]"Amendments Flood House," *The New York Times*, April 29 1917.

[A7]This is around 17 times greater than the probability of dying in a traffic accident in the United States in 2019 (see: `www.nsc.org/road-safety/safety-topics/fatality-estimates`, accessed 3/12/2020.)

Table A4: Registration, Deployment and Fatalities

	Total in Service	Draft Inductions	Draft Registered	Battle Deaths
World War I	4,734,991	2,810,296	24,000,000	53,402
World War II	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 figures are from Morris (2006, p. 15). All website were accessed on 10/29/2017.

Appendix D. Lead Years

In Figure A2, we plot estimates of β_1 using different interpretations of the effective lower cutoff age for each of the models in Table 1. In all four models, point estimates smoothly rise from the one-year lead to the four-year lead, before falling off at five years. The mean duration of wartime conscription per conflict is 4.6 years, suggesting that politicians' revealed expectations are reasonably accurate.[A8] This pattern is consistent with two interpretations. The first is that the leading cohorts—i.e., legislators with sons within 4 years of the proposed lower cutoff—are more intensely treated than currently exposed cohorts because they expect to face a longer duration of wartime exposure. The second is that leading cohorts are more intensely treated *relative to their comparison cohorts* because their exposure is less likely to be offset by a countervailing son-in-law effect.

For example, say the proposed draft age window is 19-25, which is the sample mode. A legislator with a 22-year-old son is exposed currently, while a legislator with a 15-year-old son is exposed in expectation. Thus, legislators with sons in either the leading or currently exposed cohort are treated. However, a symmetrical characteristic is not present in the comparison group, since a legislator with a 22-year-old daughter is likely to be exposed currently through marriage, while a legislator with a 15-year-old daughter is not. This asymmetry implies that the estimated treatment effect initially increases (in absolute magnitude) as we include the leading cohorts, before fading as we add irrelevant cohorts.

We find clear support for this second interpretation in Appendix Table A5, where we decompose the main estimate by including separate indicators for sons in the leading and current cohorts, as well as an indicator for having one child of any sex in the leading cohort. This leaves legislators with children in the current cohort as the reference group. We find that (i)

[A8] Before the Vietnam War, the mean duration is 3.3 years.

legislators with sons are always less likely to vote for conscription than their comparison group with daughters; (ii) these differences are greater in the leading cohort relative to the current cohort; (iii) this is because legislators with daughters in the leading cohort are more likely to vote for conscription than those with daughters in the current cohort.

In the middle panel, we show that, relative to having a daughter in the leading cohort, the effect of having a son in the current cohort is statistically indistinguishable from the effect of having a son in the leading cohort.

Together, these findings indicate that the estimated treatment effects are greater in the leading cohort because of a weaker son-in-law attenuation effect.

Other figures In Figure A3, we show the equivalent figures for window votes. Here, the optimal lead structure is two years. This aligns remarkably well with the fact that, on average there are 1.8 remaining years of wartime conscription when window votes are cast. The equivalent number for our main votes is 3.1 years. In Figures A4 and A5, we show the equivalent figures for the hawks and doves regressions. The optimal leads are the same as those for the main vote sample in Figure A2.

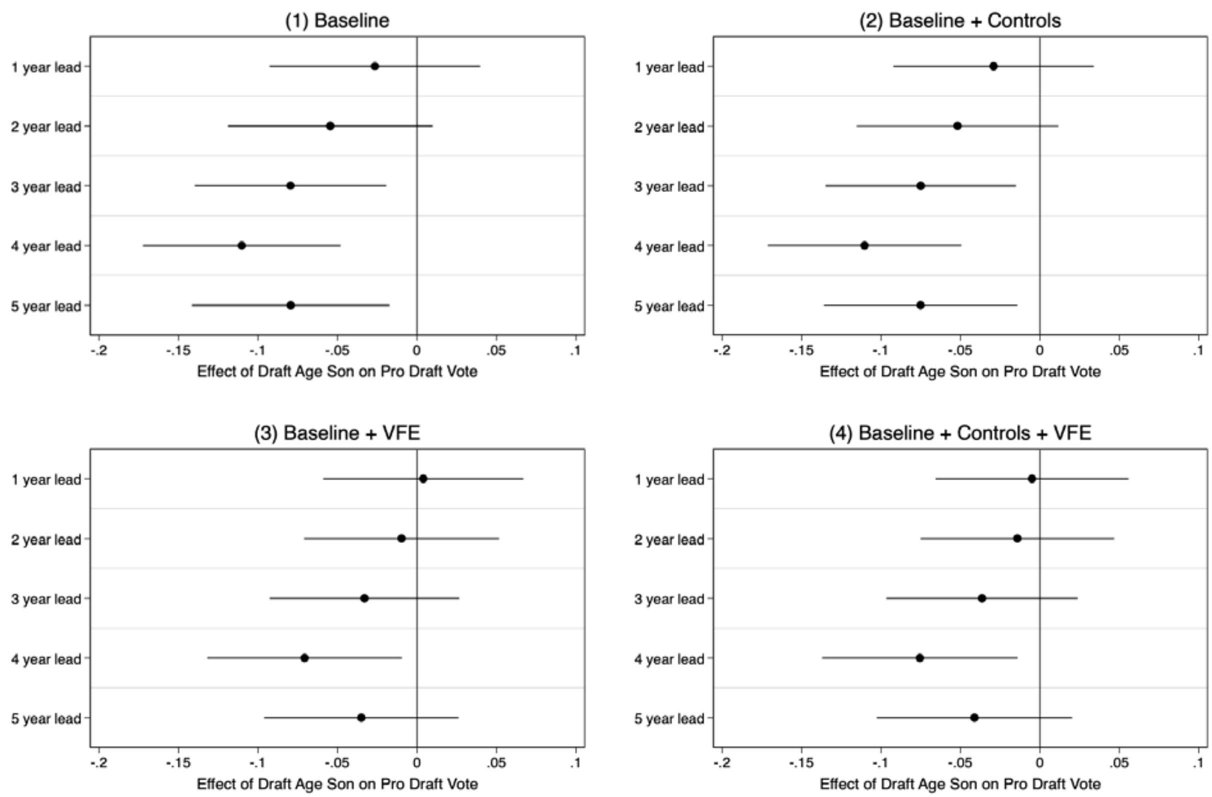


Figure A2: **Impact of Draft Age Son on Pro Draft Votes with Varying Leads**. These are estimates of β_1 in Equation 3 using various leads for *draft son*$_{iv}$. The four 4-year specifications correspond to those in Table 1.

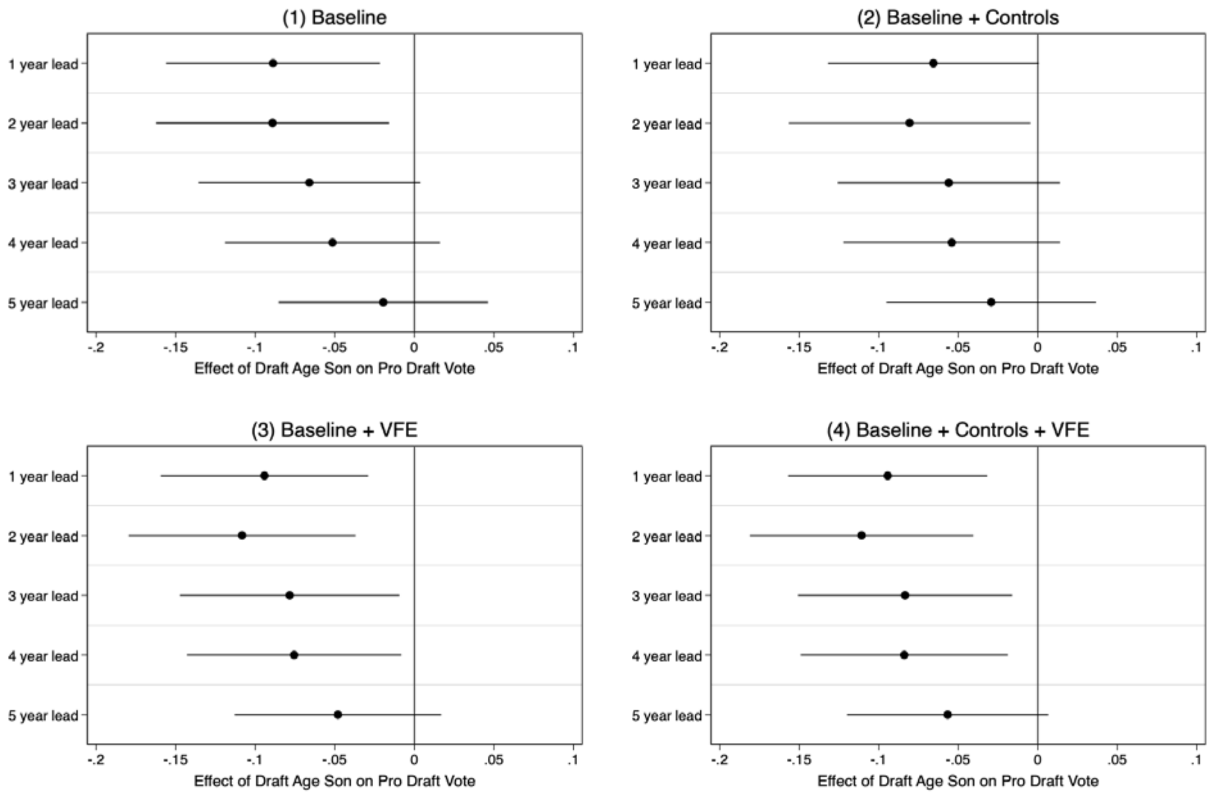


Figure A3: **Impact of Draft Age Son on Pro Draft Votes with Varying Leads (Window Votes)**. The four 2-year specifications correspond to those in Panel A of Appendix Table A8.

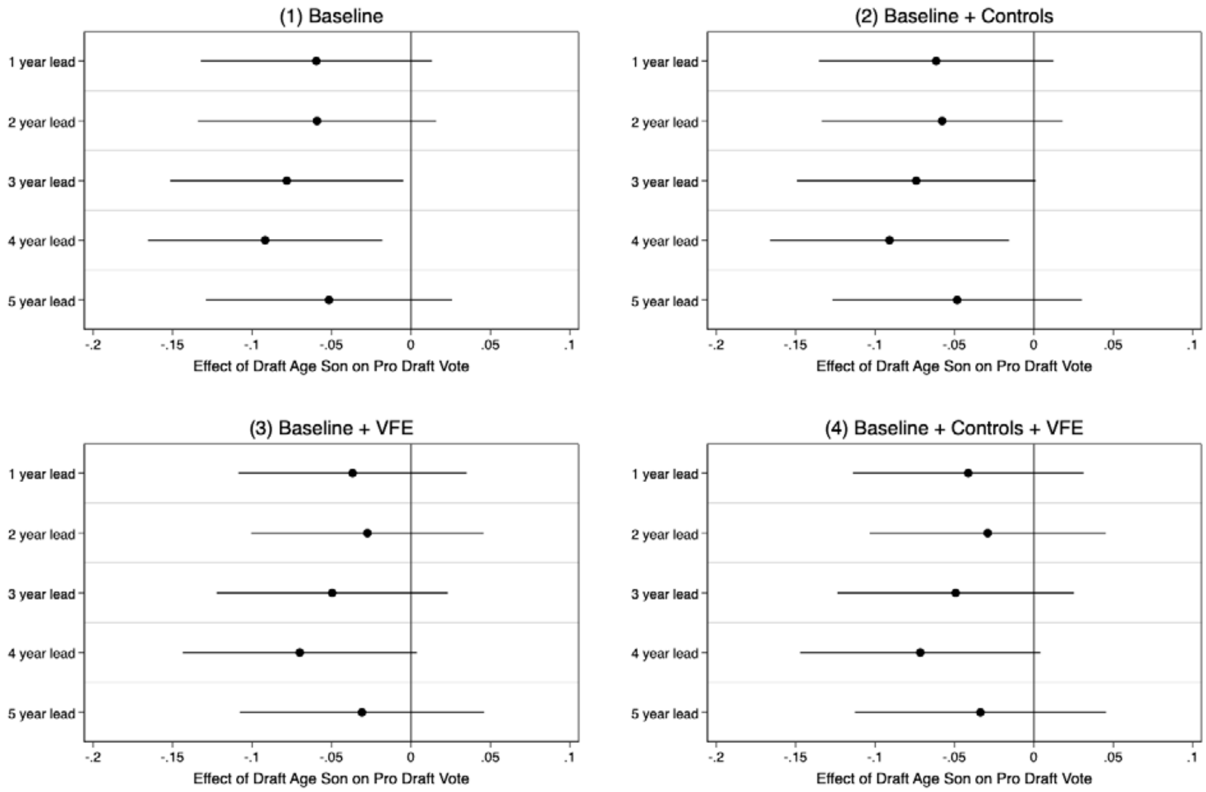


Figure A4: **Impact of Draft Age Son on Hawkish Votes with Varying Leads (Narrative Method)**. The four 4-year specifications correspond to those in columns 1-4 in Panel A of Appendix Table A9.

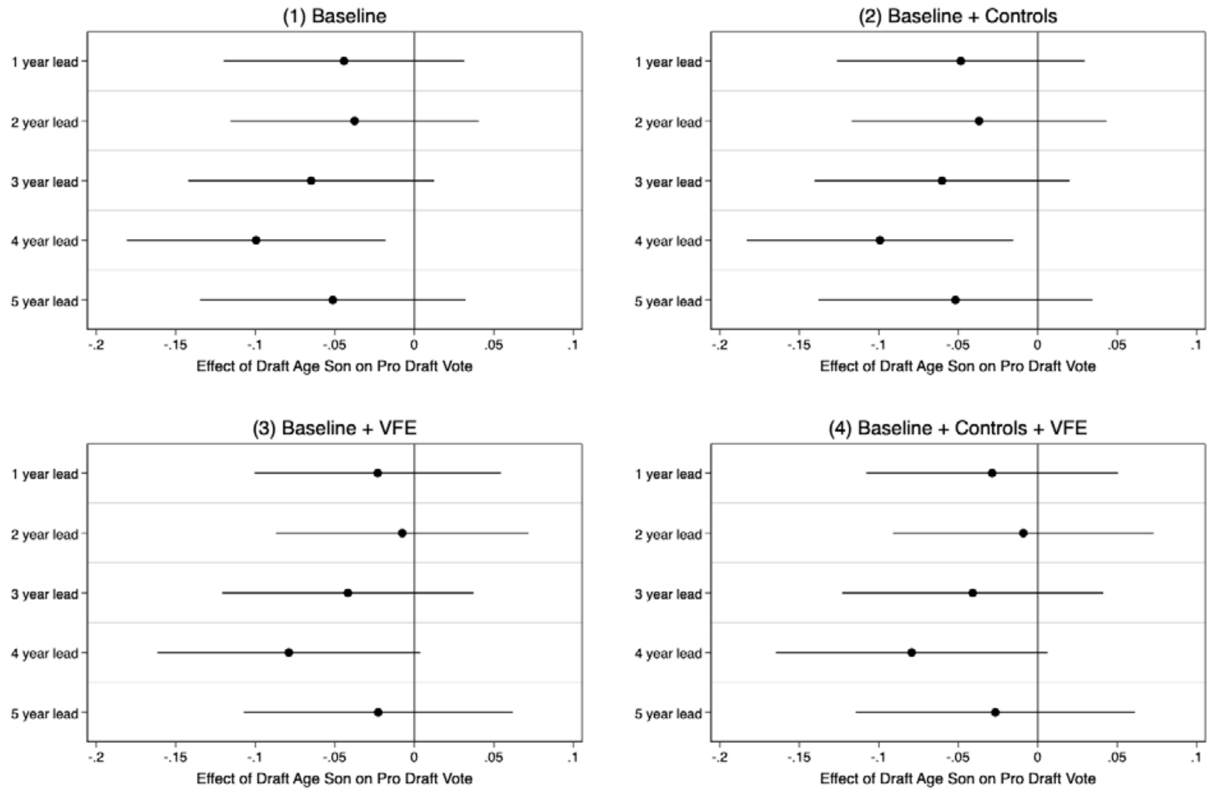


Figure A5: **Impact of Draft Age Son on Hawkish Votes with Varying Leads (Extrapolation Method)**. The four 4-year specifications correspond to those in columns 5-8 in Panel A of Appendix Table A9.

Table A5: Separating Effect of Leading Cohort and Exposed Cohort

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Leading Cohort				
Son	-0.1127***	-0.1106***	-0.0911**	-0.0907**
	(0.0371)	(0.0362)	(0.0357)	(0.0352)
Child	0.0780**	0.0698**	0.0662**	0.0731**
	(0.0323)	(0.0330)	(0.0306)	(0.0319)
Current Cohort				
Son	-0.0349	-0.0452	-0.0176	-0.0279
	(0.0305)	(0.0305)	(0.0287)	(0.0299)
Additional Calculations				
Current Son - Leading Child	-0.1129	-0.1150	-0.0838	-0.1010
p-value	[0.00]	[0.01]	[0.03]	[0.02]
Test: [Current Son - Leading Child] = [Leading Son]				
p-value	[1.00]	[0.90]	[0.81]	[0.76]
Controls	No	Yes	No	Yes
Vote FE	No	No	Yes	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Legislators	1427	1427	1427	1427
Votes	103	103	103	103
Mean Dep. Var.	0.58	0.58	0.58	0.58
Observations	9920	9920	9920	9920

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

Appendix E. Political Agency Theory

In this section, we provide a more comprehensive description of our theoretical and empirical exercises reported in Section 6 of the main manuscript.

We postulate in Section 2 that a sufficiently large shock to R_{it} could cause political leaders to vote for conflicts in which the expected social costs exceed the benefits, or to avoid conflicts in which the expected social benefits exceed the costs, provided that $\theta > 0$. It is unclear whether, on average, treated legislators with draft-eligible sons better represent their constituents' preferences than similar legislators with daughters of comparable age. If the treated group better reflects voters' concerns, then it is the comparison group politicians that deviate from the social optimum by failing to internalize the costs of their decision. If the comparison group better reflects voters' concerns, then it is the treatment group politicians that deviate from the social optimum by failing to internalize the benefits of their decision.[A9]

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.[A10] Informational problems can arise if politicians can hide effort or motives. We consider two general types of models that align with our main results: (i) 'pure' moral hazard in which all politicians maximize private rents; and (ii) moral hazard with adverse selection, in which 'bad' politicians maximize private rents and 'good' politicians enact voters' preferred policies. We propose empirical tests to determine which model more closely fits the data.

A. Pure moral hazard

The 'first generation' political agency models focus on moral hazard as the defining agency problem (Barro, 1973, Ferejohn, 1986). In these, self-serving politicians seek to maximize rents. Voters know that all politicians are self-serving, but they do not perfectly observe the rents that accrue to politicians in office nor the actions that they take. In equilibrium, elections partially constrain rent-seeking in the period before an election as politicians value future rent-seeking opportunities.[A11]

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

B. Moral hazard with politician types

The modern workhorse political agency model presented in Besley (2006) combines elements of moral hazard from first generation political agency models with elements of models that allow for different politician types. In these, elections serve the twin purposes of restraining politician behavior, as above, and selecting 'good' politicians who care more about voter welfare. In chasing private rents, 'bad' politicians can also mimic good ones in order to disguise their type to the electorate.[A12] Below, we briefly describe a basic version of the model in order to consider its implications in our setting.

Environment Consider two time periods $t \in \{1,2\}$ in which N politicians of type $i \in \{g,b\}$ either vote against or in favor of conscription. This decision is represented by $V_{jt} \in \{0,1\}$, where

[A9] 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.

[A10] See Besley (2006) for an in-depth account of these models.

[A11] Elections only partially constrain politicians as voters must permit a level of rent-seeking that prevents politicians from plundering all public resources immediately.

[A12] This is not possible in pure adverse selection models.

$j \in \{1, 2, \ldots N\}$ denotes an individual politician. We define type g as 'good' politicians for whom the weight placed on the private returns to legislative voting is zero, and type b as 'bad' politicians for whom this weight is strictly positive. Let π represent the probability that a randomly picked politician from the pool is a good type. Voters do not observe these types.

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

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

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

The timing of the game is as follows. Nature determines the type of politician and the state of the world at the beginning. Once in office, politicians observe the draw r_1 and select $V_{jt}(S_t, i)$. Voters observe only V_{jt} and their own payoff, and then decide whether or not to reelect the incumbent. Following the election, politicians receive a new draw r_2, and period 2 decisions are made. The game ends once period 2 payoffs are realized.

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

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

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

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

The optimal period 1 decision for a bad politician is determined by the relative value of the private rent r_1 against the value of disguising their type and winning reelection, which is $\beta(\mu +$

E). Thus, the probability that a bad politician takes the action preferred by voters is

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

Proposition 1
(i) Good politicians always choose $V_{jt}(S, g) = S_t$.
(ii) Bad politicians always choose $V_{j2}(S_2, b) = (1 - S_2)$ *in period 2.*
(iii) Bad politicians will choose $V_{j1}(S_1, b) = S_1$ *in period 1 if they earn sufficiently low private rents* $r_1 < r* \equiv \beta(\mu + E)$ *from voting against the electorate's preferred policy.*
(iv) All politicians who choose $V_{j1}(S_1, i) = S_1$ *in period 1 are reelected.*

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

Conscription and heterogeneous rent shocks Allow the private rent shock to be characterized as follows: one subgroup of politicians receive r_1^h and another receives r_1^l, where $r_1^h > r_1^l$. This implies that there is an exogenous difference in r_1 between politicians that cuts across both good and bad types. Good types select $V_{j1}(S_1, g) = S_1$ irrespective of r_1, and are reelected. If $r_1^h > r_1^l > \beta(\mu + E)$, then all bad types choose $V_{j1}(S_1, b) = (1 - S_1)$ and are not reelected. Similarly, if $\beta(\mu + E) > r_1^h > r_1^l$, then all bad types choose $V_{j1}(S_1, b) = S_1$ and are reelected. However, if $r_1^h > \beta(\mu + E) > r_1^l$, then bad types with r_1^h select $V_{j1}(S_1, b \mid r_1^h) = (1 - S_1)$ and are not reelected, while bad types with r_1^l select $V_{j1}(S_1, b \mid r_1^l) = S_1$ and are reelected. With heterogeneous rent shocks, therefore, there exists a set of payoffs in which some bad types pursue private rents and are voted out of office, while other bad types mimic good types and survive to period 2 because their rent shock r_1^l is worth less than 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.[A13] However, bad types with draft-eligible sons observe $r_1^l < \beta(\mu + E)$, and instead mimic good types by choosing $V_{j1}(0, b \mid r_1^l) = 0$ and winning reelection. The draft eligibility 'shock' introduces an exogenous wedge between r_1^l and r_1^h. Provided that $r_1^h > \beta(\mu + E) > r_1^l$, bad politicians with draft age sons will oppose the draft, improve their reputation with voters, and survive to period 2. Conversely, if conscription is popular with voters, i.e., $S_1 = 1$, then the draft eligibility shock implies that politicians with draft age sons face r_1^h and vote against constituents' wishes, thereby revealing their true type and losing reelection.

[A13] 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.

Corollary 1

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

C. Testing implications

To determine which of these interpretations aligns better with the data, we examine the electoral outcomes of legislators following Congresses in which they voted on draft-related measures. In our main test, this amounts to replacing the outcome variable in equation (3) with an indicator equal to 1 if the legislator wins their next election. The pure moral hazard model predicts that having a draft-eligible son will not impact a politician's reelection probability. The moral hazard model that allows for different types predicts that having a draft-eligible son will decrease the average politician's reelection probability when the draft is popular among voters, and increase the average politician's reelection probability when it is not (Corollory 1).

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

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

Draft popularity Nationally representative data on public support for the draft is available from surveys administered by the Roper Center's Public Opinion Archive in 1945, 1952, 1969, 1980, 1981, 1985, and 2003.[A14] These and related surveys are analyzed by Fordham (2016), who documents a collapse in support for the draft from around 70% in 1945 to around 20% at the outset of the Iraq War in 2003.[A15] The main figure from that study is reproduced in Figure A9.

The prevailing explanation for the decline in support relates to military technology: public support for the draft is a function of its necessity to win the war. As military conflict became more capital intensive over time, the importance of conscription as a means of ensuring victory waned. For example, conscription was viewed as essential to raise an army capable of entering

[A14]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.

[A15]Clifford and Spencer (1986) note that support for conscription was substantially lower at the beginning of World War II than in 1945. In March 1940, 20 months prior to the attack on Pearl Harbor, 98.4% of Americans opposed going to war against Germany, reflecting a "general desire not to repeat the mistakes of 1917/18" (pp. 8). This suggests that the trend is not strictly declining at all points in time.

World War I; by contrast, the U.S. fought the wars in Iraq and Afghanistan this century without appreciably enlarging the military at all, let alone through conscription (Fordham, 2016).

A second explanation relates to the salience of military casualties. Karol and Miguel (2007) provide evidence that home-state casualties in the Iraq war reduced the vote share for George W. Bush between the 2000 and 2004 presidential elections. It is therefore plausible that fatalities reported during draft-era wars undermined support for the draft itself. Turning to data presented in Table A4, we can calculate U.S. fatalities per draftee for each conflict: 0.018 in World War I, 0.029 in World War II, and 0.024 in the Cold War theaters. Given the rise of mass media over this period, it is reasonable to assign a higher weight to the Cold War figure in particular, as information on those fatalities is more likely to have proliferated among voters (Flynn, 1993).

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

We propose an empirical test that harnesses quasi-experimental variation. Senators serve six-year terms with staggered elections every two years. Thus, only one third of senators can face an election year at any moment in time. Because of this, we can compare the voting behavior of senators who are up for reelection versus those who are not for every Congress in our sample. This feature allows us to control for vote fixed effects—which flexibly capture common time effects—and recover a plausibly unbiased estimate of the effect of election years on legislative voting related to conscription for each era.[A16]

The results of this exercise are presented in Figure 4a and in Tables A14 and A15, using the *Pro Draft Vote* and *Hawkish Vote (Narrative)* outcomes respectively. We additionally present the results for *Hawkish Vote (Extrapolation)* in Table A16. We estimate the impact of election years on both these outcomes separately for three periods, World War I, World War II, and the Cold War.[A17] In each specification we include the baseline control variables and vote fixed effects ("VFE" in the figure). For each period, we also estimate specifications that additionally include legislator fixed effects ("LFE").

The estimates present a clear pattern. During World War I, legislators who were up for reelection were more likely to vote in favor of conscription than other legislators. The estimate is significant only in the specifications without LFE using the *Hawkish Vote* outcomes. However, the point estimates are positive using all three outcomes and in all specifications. By contrast, we see the opposite effect during the Cold War conflicts: all six estimates are negative and four are statistically significant.

[A16]For this analysis, we define election years as the period *before* the November election date.

[A17]We combine the Korean War and Vietnam War samples as there are only 8 votes in the former. This has no meaningful effect of the results.

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

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

Election Outcomes Using these findings, we now examine whether or not legislators with draft-eligible sons are more likely to be reelected over time in our sample. To do this, we simply run the following specification for each era as follows, again using the cross-sectional sample from equation 3:

$$E_{ic(v)} = v_v + \kappa_{iv} + \sigma_{iv} + \gamma_1 draft\ son_{iv} + \gamma_2 draft\ child_{iv} + \mathbb{X}'_{iv}\psi + \epsilon_{iv}, \tag{A1}$$

where $E_{ic(v)}$ is a binary variable indicating that legislator i was reelected following congressional session c, and $c \in \{64, 65, 66\}$ denotes World War I; $c \in \{76, 77, 78, 79\}$ denotes World War II; and $c \in \{82, 88, 89, 90, 91, 92, 93\}$ denotes the Cold War. The variable $draft\ son_{iv}$ indicates that the legislator has a draft-eligible son for vote v, which is contained in congressional session c. The vector $\mathbb{X}_{iv}$ represents the baseline controls for party, house or senate, terms in office, age, and age squared. Our main specification does not include legislator fixed effects, as the outcome varies at the level of a legislator-term rather than a legislator-vote. We only include legislator-votes for legislators who contested their next election.

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

1. **Pure moral hazard**. In the pure moral hazard model with only bad types, voters do not learn from legislative behavior. The $draft\ son$ variable will have no effect on a legislator's subsequent election performance, i.e., $\gamma_1 = 0, \forall\ c$.

2. **Moral hazard with politician types**. In the moral hazard model with politician types, voters do learn from legislative behavior. The $draft\ son$ variable will have a positive impact on election performance if $S_t = 0$ is state of the world and a negative impact if $S_t = 1$ is the state of the world. If S_t is declining over time, the empirical implication becomes $(\hat{\gamma}_1 \mid 1\{World\ War\ I\}) < (\hat{\gamma}_1 \mid 1\{World\ War\ II\}) < (\hat{\gamma}_1 \mid 1\{Cold\ War\})$

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

D. Results

We present the results of this exercise in Figure 4b and Table A17 (with controls) and Table A18 (without controls). For each era, we estimate the specification with and without vote fixed effects.

The results present a mirror image of our previous results on draft popularity over time. During World War I, when the draft was most popular, legislators with draft age sons were less likely to win reelection ($p < 0.05$). The point estimates approach zero for World War II, and the sign flips for the Cold War, when the draft became unpopular.

We show in Figure A10 that these differences are not driven by differences in the "first stage" relationship between having a draft age son and pro-draft voting, which is negative for all three eras. This pattern is also inconsistent with random noise, suggesting that the pure moral hazard model is again not supported by the data.

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

Appendix F. Comparing Other Motives: Political Alignment

The previous analysis raises the important question of why some politicians still vote in favor of conscription when it is broadly unpopular, as was the case during the Cold War era. Our model and results imply that the control group of otherwise identical legislators who voted in favor of conscription are deriving utility from their vote through channels other than voter preferences. Linking back to equation 2, these could be party career concerns (P_{it}), ideology (F_i), or other unobserved private benefits (R_{it}).

Here, we explore the hypothesis that pressure to comply with the national party line played a significant role. To test this, we add to our baseline specification an indicator that is equal to 1 if the president is from the same party as a given legislator. With legislator fixed effects, this estimate is identified off the switch from President Lyndon B. Johnson (a Democrat) to President Richard Nixon (a Republican) in 1969. This specification allows us to flexibly control for the party identification of the given legislator, as well as all other time invariant characteristics such as ideological preferences.

This hypothesis is motivated by narrative accounts of Nixon's approach to conscription before and after his election as president. Fordham (2016, p. 29) notes that while there were Republicans and Democrats on both sides of the debate over ending the draft, Nixon "campaigned on a promise to put a stop to it, but repeatedly asked for its extension as president."[A18] Nixon's reverse was particularly evident at the time of the Hatfield-Goldwater amendment to raise the pay of the military in 1970. The measure was an explicit attempt to end the draft by attracting a

[A18]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."

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.[A19]

In Table A19, we provide strong evidence in support of party career concerns as a partial explanation for pro-draft legislative voting. Political alignment with the sitting president significantly increases the probability that a given legislator votes in favor of conscription. Controlling for individual fixed effects, we estimate that legislators are 11 percentage points more likely to vote in favor of conscription when the party of the president aligns with their own (column 4). This indicates that pro-draft voting in the U.S. Congress appears to stem at least in part from national party edicts.

Importantly, this exercise provides some quantitative context for interpreting the main estimate of interest in this paper. This 11 percentage point effect is within the range of our estimates for having an exposed son: the median estimate among those presented in Tables 1 and 2 is around 9 percentage points, while the main RD estimate in Table A10 is 18.79 percentage points. The weight assigned to party preferences in Levitt (1996) is around 0.13, suggesting that the weight that legislators assign to private rents (i.e. θ) may be similar. Using our 9 percentage point estimate, for example, we can scale this as $0.13 \times \frac{0.9}{0.11} = 10.6\%$. While this is a somewhat coarse calculation, it provides an informative approximation of the weight that legislators assign to private rents.[A21]

Appendix G. Counterfactual Analysis

In this section we use our estimates to quantify the historical differences that may have emerged if all members of Congress were exposed to the draft. This counterfactual analysis comes with the usual caveat that it is a partial equilibrium exercise—we are holding constant all other factors, including the content of the roll call votes themselves and the expectations that members of

[A19]"Senate Bars Plan Designed To Bring Volunteer Army," *The New York Times*, Aug. 26, 1970. More evidence on this motive can be gleaned from newspaper reports at the other end of our sample period. Under the heading "Ban Two Draft Opponents — Democrats in Cleveland Declare Gordon and Crosser 'Done'," The *New York Times* reported the following in April 1917:

> Indignant at the spectacle of two Congressman from Cleveland openly opposing President Wilson's war policies, the leaders of the local Democratic organization today declared William Gordon and Robert Crosser "done." The two Congressmen were practically read out of the Democratic Party by the declaration that the political organization of which Secretary of War Baker is head will never again support either man for nomination or election.[A20]

[A21]The estimate for party weights in Levitt (1996) ranges from 0.02 to 0.25 depending on the specification. A reasonable weight for our application is 0.13 from Table 3, column 7, which is based on the preferences of elites within the party rather than rank-and-file members).

Congress have regarding their passage. Nevertheless, it is informative to determine whether or not our estimates are quantifiably meaningful.

Of the 103 roll call votes that we study in our main analysis, 88 went in favor of conscription—that is, either pro-draft measures were adopted or anti-draft measures were defeated. Of these 88 votes, 17 passed by margins of less than 6 percentage points (our panel estimate); 19 passed by margins of less than 7.56 percentage points (our main cross-sectional estimate) and 43 passed by less than 18.79 percentage points (our RD estimate).

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

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

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Appendix H. Additional Appendix Figures

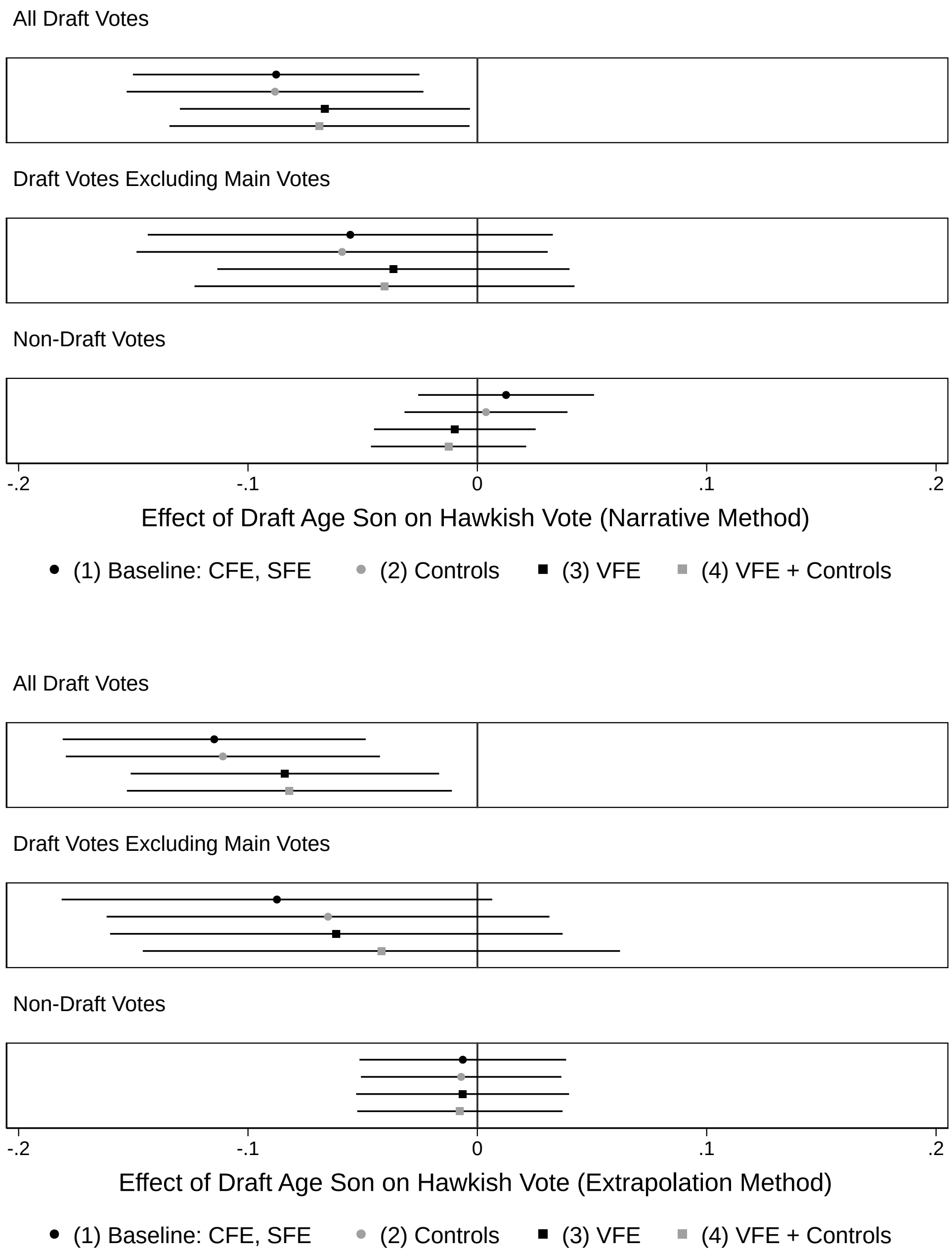
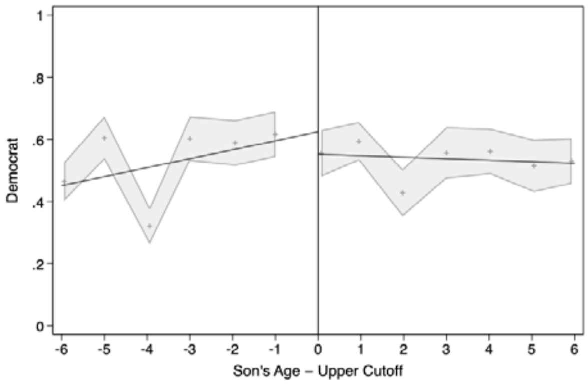
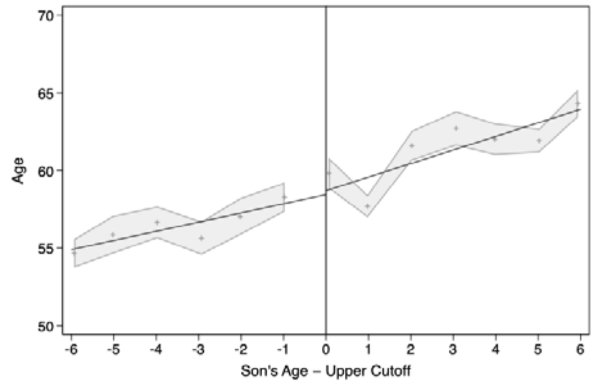


Figure A6: **Hawks and Doves: Cross-Sectional Estimates**. These estimates correspond to those presented in Table A9.



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



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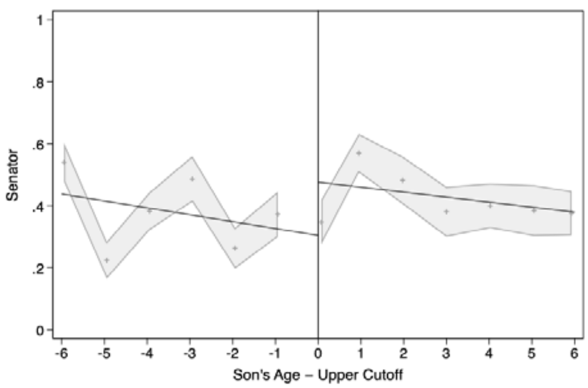
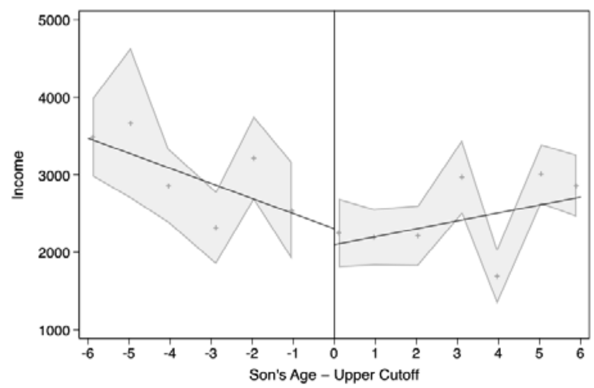


(c) Plot Corresponding to Table 5, Column (5)



(d) Plot Corresponding to Table 5, Column (6)

Figure A7: **Regression Discontinuity Plots: Balance**

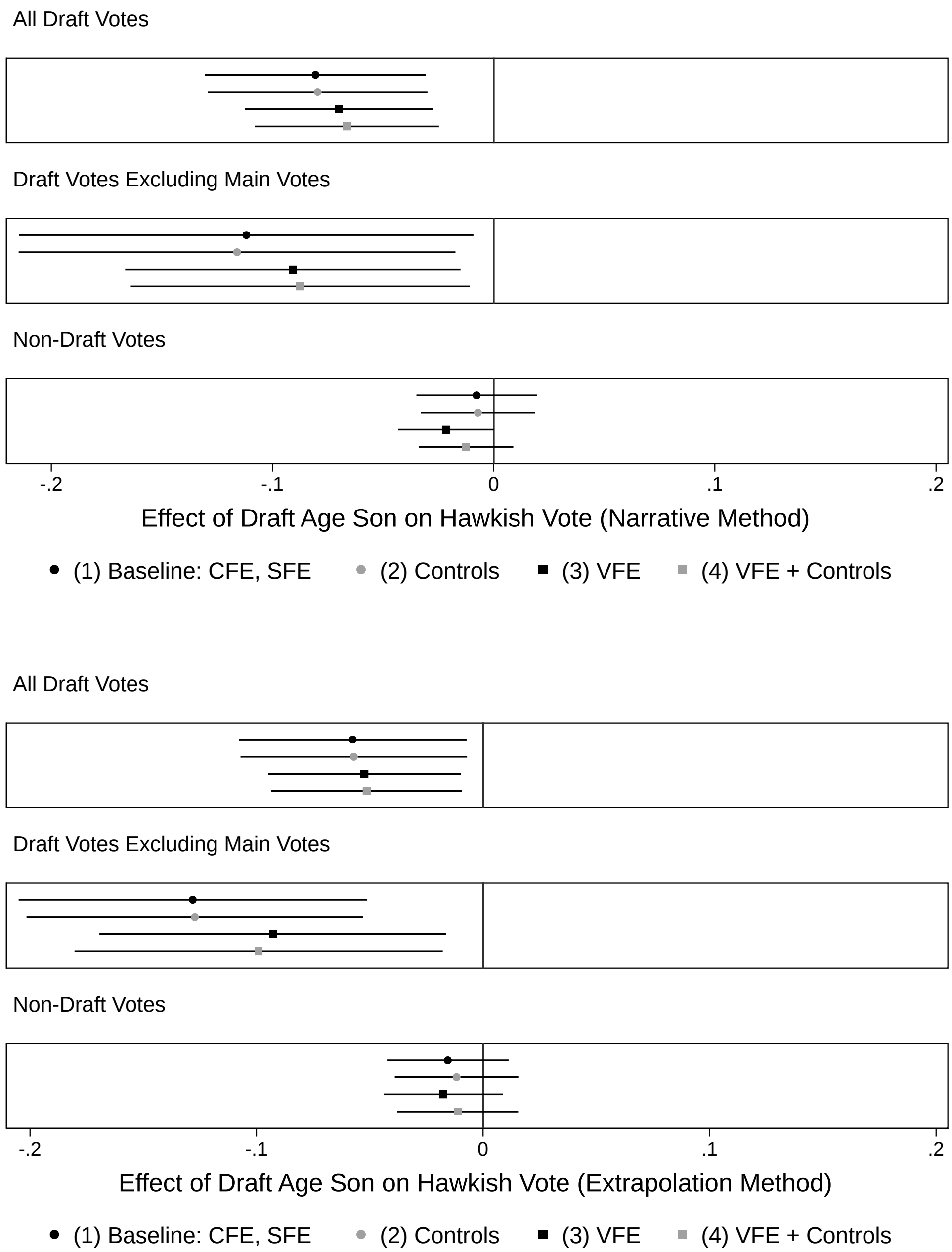


Figure A8: **Hawks and Doves: Fixed Effects Estimates**. These estimates correspond to those presented in Table A12.

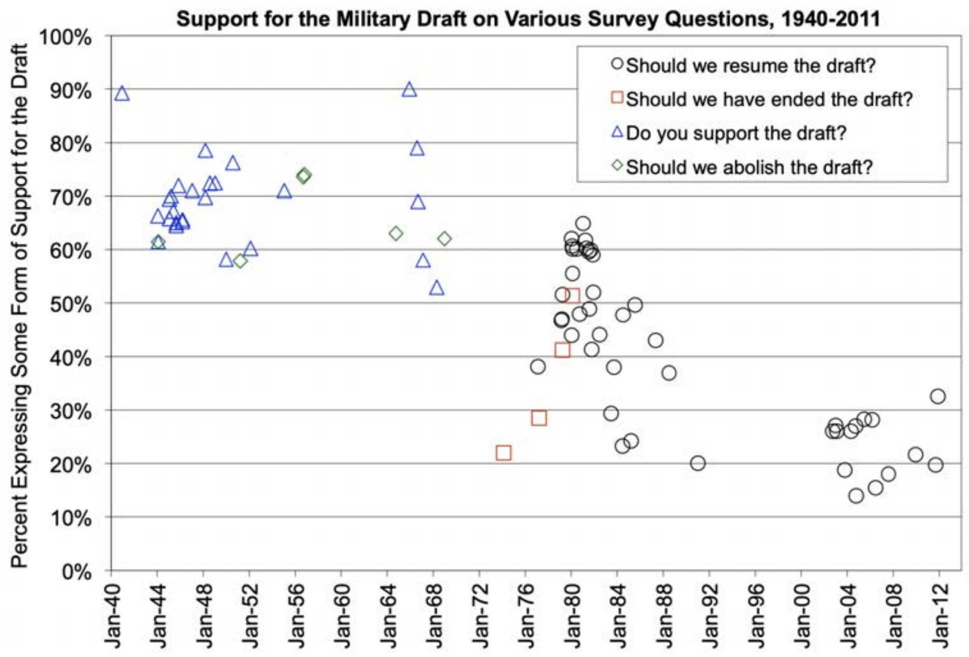


Figure A9: **Support for Conscription Over Time, from Fordham (2016)**

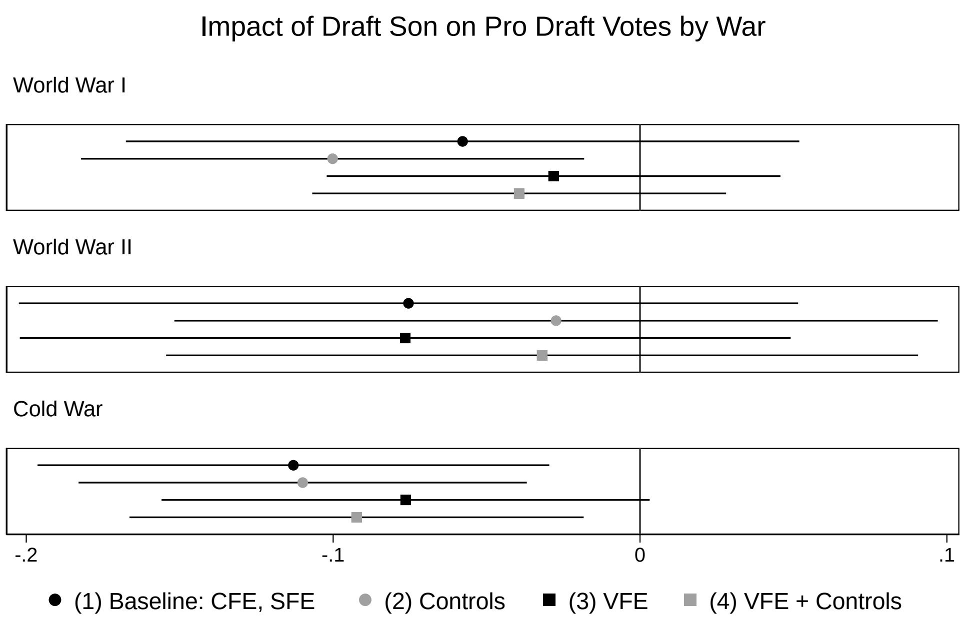


Figure A10: **Main Effects by War**

Appendix I. Additional Appendix Tables

Table A6: Balance on Observables

	(1) Democrat	(2) Senator	(3) Age	(4) Terms	(5) Income
	Panel A: Conditional Balance Test				
Draft Age Son	-0.040	0.037	1.558	-0.064	282.261
	(0.053)	(0.062)	(1.048)	(0.205)	(449.776)
Number of Sons FE	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes
	Panel B: Unconditional Balance Test				
Draft Age Son	0.057	0.042	0.957	-0.143	79.893
	(0.041)	(0.047)	(0.653)	(0.144)	(269.374)
Constant	0.546***	0.389***	53.363***	2.994***	2917.996***
	(0.036)	(0.068)	(0.555)	(0.178)	(313.244)
Number of Sons FE	No	No	No	No	No
Number of Children FE	No	No	No	No	No
Legislators	1427	1427	1427	1427	677
Votes	103	103	103	103	103
Mean Dep. Var.	0.587	0.420	54.059	2.890	2976.622
Observations	9922	9922	9922	9922	4463

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

Table A7: Omitting Fixed Effects for Number of Sons and Number of Children

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Draft Age Son	-0.0698**	-0.0669**	-0.0486*	-0.0518*
	(0.0274)	(0.0263)	(0.0267)	(0.0264)
Controls	No	Yes	No	Yes
Vote FE	No	No	Yes	Yes
Number of Sons FE	No	No	No	No
Number of Children FE	No	No	No	No
Legislators	1427	1427	1427	1427
Votes	103	103	103	103
Mean Dep. Var.	0.58	0.58	0.58	0.58
Observations	9922	9922	9922	9922

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

Table A8: Window Votes

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Panel A: Window Votes with 2-Year Lead				
Draft Age Son	-0.0891**	-0.0806*	-0.1083**	-0.1108**
	(0.0434)	(0.0450)	(0.0422)	(0.0416)
Legislators	811	811	811	811
Votes	37	37	37	37
Mean Dep. Var.	0.55	0.55	0.55	0.55
Observations	1880	1880	1880	1880
Panel B: Combination of Window Votes (2-Year Lead) and Main Votes (4-Year Lead)				
Draft Age Son	-0.1024***	-0.1005***	-0.0848***	-0.0892***
	(0.0302)	(0.0300)	(0.0303)	(0.0301)
Legislators	1479	1479	1479	1479
Votes	140	140	140	140
Mean Dep. Var.	0.58	0.58	0.58	0.58
Observations	11800	11800	11800	11800
Panel C: Combination of Window Votes and Main Votes (All 4-Year Lead)				
Draft Age Son	-0.0922***	-0.0922***	-0.0756**	-0.0803***
	(0.0295)	(0.0294)	(0.0298)	(0.0297)
Legislators	1510	1510	1510	1510
Votes	140	140	140	140
Mean Dep. Var.	0.58	0.58	0.58	0.58
Observations	12107	12107	12107	12107
Controls	No	Yes	No	Yes
Vote FE	No	No	Yes	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes

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

Table A9: Hawks and Doves

	Hawkish Vote (Narrative Method)				Hawkish Vote (Extrapolation Method)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: All Draft Votes				
Draft Age Son	-0.0877**	-0.0882**	-0.0665*	-0.0688*	-0.1147***	-0.1109***	-0.0839**	-0.0820*
	(0.0378)	(0.0391)	(0.0383)	(0.0396)	(0.0399)	(0.0414)	(0.0407)	(0.0428)
Legislators	1349	1349	1348	1348	1232	1232	1231	1231
Votes	187	187	185	185	168	168	165	165
Mean Dep. Var.	0.60	0.60	0.60	0.60	0.59	0.59	0.59	0.59
Observations	9972	9971	9970	9969	11739	11739	11736	11736
				Panel B: All Draft Votes Excluding Main Votes				
Draft Age Son	-0.0554	-0.0590	-0.0366	-0.0405	-0.0874	-0.0651	-0.0615	-0.0418
	(0.0530)	(0.0538)	(0.0460)	(0.0497)	(0.0561)	(0.0577)	(0.0590)	(0.0622)
Legislators	1090	1090	1090	1090	1001	1001	1000	1000
Votes	69	69	69	69	58	58	57	57
Mean Dep. Var.	0.55	0.55	0.55	0.55	0.55	0.55	0.55	0.55
Observations	3301	3300	3301	3300	3881	3881	3880	3880
				Panel C: Non-Draft Votes				
Draft Age Son	0.0125	0.0038	-0.0098	-0.0125	-0.0063	-0.0071	-0.0064	-0.0076
	(0.0233)	(0.0216)	(0.0214)	(0.0206)	(0.0274)	(0.0266)	(0.0282)	(0.0272)
Legislators	1730	1726	1730	1726	1720	1716	1720	1716
Votes	1825	1825	1737	1737	1414	1414	1403	1403
Mean Dep. Var.	0.50	0.50	0.50	0.50	0.54	0.53	0.54	0.53
Observations	90302	89815	90214	89727	113038	112049	113027	112038
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Vote FE	No	No	Yes	Yes	No	No	Yes	Yes
Number of Sons FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Table A10: Regression Discontinuity Estimates

	Pro Draft Vote		Democrat	Age	Senator	Income
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Conventional Regression Discontinuity Estimates					
I(Age > Upper Cutoff)	0.1879	-0.0044	-0.1128	-0.2250	0.1517	-319.6216
	(0.0787)**	(0.0888)	(0.1234)	(1.5793)	(0.1208)	(749.9420)
	Panel B: CCT Bias-Corrected Regression Discontinuity Estimates					
I(Age > Upper Cutoff)	0.2195	0.0190	-0.1802	-0.9120	0.1793	-715.0617
Conventional SE	(0.0787)***	(0.0888)	(0.1234)	(1.5793)	(0.1208)	(749.9420)
CCT Robust SE	(0.0954)**	(0.1118)	(0.1463)	(2.0610)	(0.1427)	(886.5030)
Running Variable: Age of	Son	Daughter	Son	Son	Son	Son
Bandwidth	6.93	6.17	6.07	4.78	8.76	5.97
Observations	4574	4669	4574	4574	4574	2589

Note: The unit of analysis is the legislator-vote. In columns (1) and (2), the outcome variable is an indicator equal to one if the legislator votes in favor of conscription on a given vote. In column (3), the outcome variable is an indicator equal to 1 if the legislator is a Democrat. In column (4), the outcome variable is the legislator's age. In column (5), the outcome variable is an indicator equal to 1 if the legislator is a senator. In column (6), the outcome variable is the legislator's 1939 income, report in the 1940 Census (where applicable). We use the data-driven bandwidth selection procedure developed by Calonico et al. (2014) and Calonico, Cattaneo, Farrell and Titiunik (2019) to compute all RD estimates. The upper panel presents conventional RD estimates; the lower panel presents bias-corrected RD estimates. Standard errors are two-way clustered by legislator and vote.$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table A11: Regression Discontinuity Estimates with Controls

	Pro Draft Vote					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Conventional Regression Discontinuity Estimates					
I(Age > Upper Cutoff)	0.1664**	0.0128	0.2182***	0.0959	0.2196***	0.0769
	(0.0703)	(0.0856)	(0.0780)	(0.0793)	(0.0717)	(0.0749)
	Panel B: CCT Bias-Corrected Regression Discontinuity Estimates					
I(Age > Upper Cutoff)	0.1719	0.0351	0.2176	0.0899	0.2318	0.0869
Conventional SE	(0.0703)**	(0.0856)	(0.0780)***	(0.0793)	(0.0717)***	(0.0749)
CCT Robust SE	(0.0843)**	(0.1064)	(0.0969)**	(0.0924)	(0.0877)***	(0.0874)
Running Variable: Age of	Son	Daughter	Son	Daughter	Son	Daughter
Bandwidth	9.53	6.08	7.09	8.19	6.91	8.85
Controls	Yes	Yes	No	No	Yes	Yes
Vote FE	No	No	Yes	Yes	Yes	Yes
Observations	4574	4669	4574	3676	4574	3676

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

Table A12: Panel Variation: Hawks and Doves

	Hawkish Vote (Narrative Method)				Hawkish Vote (Extrapolation method)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: All Draft Votes				
Draft Age Son	-0.0805***	-0.0796***	-0.0700***	-0.0663***	-0.0575*	-0.0570*	-0.0523**	-0.0514**
	(0.0302)	(0.0301)	(0.0257)	(0.0252)	(0.0304)	(0.0303)	(0.0257)	(0.0254)
Draft Age Child	0.0443	0.0470*	0.0355	0.0416*	0.0271	0.0265	0.0240	0.0146
	(0.0272)	(0.0277)	(0.0230)	(0.0231)	(0.0257)	(0.0262)	(0.0243)	(0.0242)
Legislators	1214	1214	1214	1214	1170	1170	1170	1170
Votes	192	192	190	190	170	170	168	168
Mean Dep. Var.	0.63	0.63	0.63	0.63	0.63	0.63	0.63	0.63
Observations	14176	14175	14174	14173	15816	15816	15814	15814
				Panel B: All Draft Votes Excluding Main Votes				
Draft Age Son	-0.1118*	-0.1160*	-0.0908**	-0.0875*	-0.1282***	-0.1272***	-0.0928**	-0.0991**
	(0.0616)	(0.0592)	(0.0455)	(0.0460)	(0.0460)	(0.0444)	(0.0458)	(0.0486)
Draft Age Child	0.0190	0.0246	0.0395	0.0544	0.0591	0.0357	0.0283	0.0187
	(0.0535)	(0.0558)	(0.0388)	(0.0415)	(0.0721)	(0.0625)	(0.0513)	(0.0528)
Legislators	789	789	789	789	509	509	509	509
Votes	72	72	71	71	58	58	57	57
Mean Dep. Var.	0.58	0.58	0.58	0.58	0.57	0.57	0.57	0.57
Observations	4090	4089	4089	4088	4246	4246	4245	4245
				Panel C: Non-Draft Votes				
Draft Age Son	-0.0077	-0.0071	-0.0215	-0.0125	-0.0156	-0.0117	-0.0175	-0.0112
	(0.0165)	(0.0156)	(0.0131)	(0.0130)	(0.0163)	(0.0166)	(0.0160)	(0.0162)
Draft Age Child	0.0108	0.0191	0.0135	0.0199	0.0170	0.0319*	0.0136	0.0262
	(0.0171)	(0.0163)	(0.0140)	(0.0138)	(0.0181)	(0.0180)	(0.0181)	(0.0185)
Legislators	1365	1365	1365	1365	1361	1361	1361	1361
Votes	1904	1904	1790	1790	1461	1461	1426	1426
Mean Dep. Var.	0.52	0.52	0.52	0.52	0.56	0.55	0.56	0.56
Observations	116265	115564	116151	115450	140938	139638	140903	139603
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Vote FE	No	No	Yes	Yes	No	No	Yes	Yes
Legislator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Sons FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

Table A13: Event Study Estimates

	Pro Draft Vote			
	(1)	(2)	(3)	(4)
Son Relative Age <= - 5	-0.0003	0.0417	0.0198	0.0179
	(0.0412)	(0.0373)	(0.0290)	(0.0290)
Son Relative Age = - 4	-0.0011	0.0472	0.0144	0.0143
	(0.0522)	(0.0495)	(0.0446)	(0.0447)
Son Relative Age = - 3	0.0035	0.0278	0.0151	0.0145
	(0.0534)	(0.0515)	(0.0408)	(0.0434)
Son Relative Age = - 2	-0.0434	0.0132	0.0054	0.0008
	(0.0593)	(0.0537)	(0.0455)	(0.0481)
Son Relative Age = 0	-0.0116	0.0412	0.0412	0.0219
	(0.0546)	(0.0568)	(0.0503)	(0.0511)
Son Relative Age = +1	0.0776	0.1389**	0.1283**	0.1250*
	(0.0635)	(0.0619)	(0.0616)	(0.0640)
Son Relative Age = +2	0.0319	0.0803	0.0435	0.0352
	(0.0527)	(0.0526)	(0.0431)	(0.0458)
Son Relative Age = +3	0.0608	0.1181**	0.1294***	0.1225**
	(0.0609)	(0.0569)	(0.0494)	(0.0491)
Son Relative Age = +4	0.0276	0.0783	0.0778	0.0664
	(0.0751)	(0.0739)	(0.0621)	(0.0618)
Son Relative Age >= +5	0.0401	0.0764	0.0568	0.0449
	(0.0596)	(0.0553)	(0.0455)	(0.0443)
Sum of Event Study Estimates				
$\sum_{j=0}^{5} \phi_j^s$	0.2264	0.5332	0.4769	0.4158
p-value	0.387	0.037	0.028	0.058
Controls	No	Yes	No	Yes
Vote FE	No	No	Yes	Yes
Legislator FE	Yes	Yes	Yes	Yes
Number of Sons FE	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes
Legislators	1249	1249	1249	1249
Votes	140	140	140	140
Mean Dep. Var.	0.58	0.58	0.58	0.58
Observations	14705	14705	14705	14705

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

Table A14: Effect of Election Year on Pro Draft Votes, By Wars

	WW1		WW2		Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)
Election Year	0.0159	0.0772	-0.0180	-0.0015	-0.0479***	-0.0621
	(0.0480)	(0.0718)	(0.0791)	(0.0827)	(0.0173)	(0.0540)
Legislator FE	No	Yes	No	Yes	No	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Sons FE	No	No	No	No	No	No
Number of Children FE	No	No	No	No	No	No
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	462	454	729	592	1062	988
Votes	10	10	26	26	67	67
Mean Dep. Var.	0.710	0.714	0.534	0.539	0.647	0.646
Observations	1335	1327	4152	4015	10013	9939

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

Table A15: Effect of Election Year on Hawkish Votes (Narrative Method)

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

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

Table A16: Effect of Election Year on Hawkish Votes (Extrapolation Method)

	WW1		WW2		Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)
Election Year	0.0808**	0.0123	-0.0251	-0.0103	-0.0441*	-0.0175
	(0.0411)	(0.0364)	(0.0374)	(0.0302)	(0.0234)	(0.0125)
Legislator FE	No	Yes	No	Yes	No	Yes
Vote FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Sons FE	No	No	No	No	No	No
Number of Children FE	No	No	No	No	No	No
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	681	680	760	760	1140	1140
Votes	347	347	287	287	904	904
Mean Dep. Var.	0.572	0.572	0.550	0.550	0.553	0.553
Observations	22545	22544	38603	38603	135008	135008

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

Table A17: Effect of Draft Age Son on Next Election Victory

	WW1		WW2		Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)
Draft Age Son	-0.2793**	-0.2878**	-0.0808	-0.0805	0.0336	0.0354
	(0.0944)	(0.0981)	(0.0635)	(0.0649)	(0.0461)	(0.0468)
Vote FE	No	Yes	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	Yes	Yes	Yes	Yes	Yes	Yes
Legislators	229	229	445	445	635	635
Votes	10	10	26	26	67	67
Mean Dep. Var.	0.788	0.788	0.824	0.824	0.885	0.885
Observations	626	626	2213	2213	5445	5445

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

Table A18: Effect of Draft Age Son on Next Election Victory without Controls

	WW1		WW2		Cold War	
	(1)	(2)	(3)	(4)	(5)	(6)
Draft Age Son	-0.2609**	-0.2756**	-0.0561	-0.0627	0.0113	0.0392
	(0.0915)	(0.0999)	(0.0503)	(0.0571)	(0.0490)	(0.0477)
Vote FE	No	Yes	No	Yes	No	Yes
Number of Sons FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children FE	Yes	Yes	Yes	Yes	Yes	Yes
Other Controls	No	No	No	No	No	No
Legislators	229	229	445	445	635	635
Votes	10	10	26	26	67	67
Mean Dep. Var.	0.788	0.788	0.824	0.824	0.885	0.885
Observations	626	626	2213	2213	5445	5445

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

Table A19: President Party Alignment

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

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