No Kin in the Game: Moral Hazard and War in the US Congress

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We study agency frictions in the US Congress. We examine the long-standing 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 twentieth century. We estimate that having a draft age son reduces proconscription 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.

I. Introduction

Political agency problems can arise when legislative behavior is shaped by private incentives. If leaders are influenced by personal interests when

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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 US 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 twentieth 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

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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 on either side of 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 (RD) design and 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 in to 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

[1] This is not true of the lower cutoff, as a politician with a son who is, say, 2 years younger than the lower boundary is plausibly exposed to the treatment.

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 1 year after their son ages out of eligibility relative to 1 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 twentieth 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 US government to use propaganda and censorship during World War I (Axelrod 2009; Hamilton 2020). 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

[2] We focus on our subsample of senators because, unlike the House of Representatives, elections for the Senate are staggered across three groups over 6 years. This allows us to control for time fixed effects and harness plausibly exogenous variation in election proximity between politicians over time.

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 anticonscription measures based in part on contemporaneous newspaper reports. In the second, we gather biographical information on the families of the US 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 (prowar legislators) and doves (antiwar 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 (Levitt 1996; Ansolabehere, de Figueiredo, and Snyder 2003). 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 (Tullock 1972; Ansolabehere, de Figueiredo, and Snyder 2003), it is difficult to reconcile with the growing share of campaign contributions emanating from the top of the wealth distribution in the United States (Bonica et al. 2013; Bertrand et al. 2020).

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 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 because of 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 they are avoided in some cases but not in others

[3] Other papers that examine the connection between a policy maker's background and their policy choices include Gelpi and Feaver (2002) and Carreri and Teso (2023). 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 (2021) show that kinship ties between monarchs contributed to the decline in European war frequency.

(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 II. In section III, we introduce our data. In section IV, we present our estimation strategy and main results. In section V, we examine the information versus self-interest interpretation of the main results, and in section VI, 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 VII.

II. 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 (Levitt 1996; Ansolabehere, de Figueiredo, and Snyder 2003; de Figueiredo and Richter 2014). 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, which 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.

[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 app. A for a more comprehensive account of this literature.

Evidence in support of this idiosyncratic ideological influence is provided by Washington (2008), who finds that US 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]

A. *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, de Figueiredo, and Snyder (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

[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 (Osborne and Slivinski 1996; Besley and Coate 1997). In contrast to the median voter theorem, voters elect rather than affect policies.

[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 United States 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.

connected politicians (Fisman 2001; Jayachandran 2006), 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 (Gordon, Hafer, and Landa 2007; Bonica et al. 2013; Bertrand et al. 2020).

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:

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

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 $\Sigma_{j=1}^{3}\alpha_j + \theta = 1$. The 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*).

B. *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.

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

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.

C. *Testing Implications*

The central challenge for the researcher in determining whether 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 because of 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 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 to impel citizens to go to war, legislators exhibited measurable exogenous variation in R_{it}.

III. Data and Background

A. *Structure*

Data in our main analysis are 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

[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 sec. VI.

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]

B. Vote Data

Our main dependent variable of interest is whether 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 US Congress during the twentieth 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 GovTrack project.[13] This gives a total of 248 votes, with 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 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 304 in the same session, which amends the draft legislation by eliminating exemptions for special occupations. An example from World War II is vote 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 37 in the 82nd House, "to provide for the common defense and security of the United States and to permit the more effective utilization of man-power resources of the United States 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 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 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

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

[12] For more detailed information, see app. B.

[13] For Voteview, see https://voteview.com/. For GovTrack, a project of Civil Impulse, see https://www.govtrack.us.

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

For each of the 248 votes, we therefore turned to archival records to determine the implications of an aye versus a nay. 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 21 in the 65th Senate) may appear to be a prodraft 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 hand, 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 table A1, where we document draft-related votes only in sessions in which we found relevant votes that could be determined as pro- or antidraft. In total, we code the direction of 140 votes—106 in the Senate and 34 in the House. Our main dependent variable, *Prodraft*, is equal to 1 if a legislator voted in favor of conscription (e.g., aye if it was a prodraft vote or nay if it was an antidraft 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 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.

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

[15] The *New York Times* was a particularly useful resource because of 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.

C. *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 US Congress 1774–2005* (Dodge and Koed 2005). We then use this information to locate richer household data from alternative sources. Most of these data are acquired from decennial US 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 who 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*), biographies on official federal and local government websites, local media profiles, university archives, and other online repositories.

We summarize the main characteristics of these data in the top panel of table A3 (in app. 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 bottom 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.

D. *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 ages 20–30 to

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

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

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

[20] Continuing an earlier example, we find that for vote 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.

ages 20–35, that is, raising the upper cutoff from age 30 to age 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 inframarginal 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 inframarginal 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.

E. *Hawks and Doves*

As described above, the process by which we code votes as either pro- or antidraft 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 confidence.[22] Two drawbacks of this approach are (1) the loss of coverage because of the ambiguity of certain votes and (2) 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 antidraft 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

[21] This was an issue debated in Congress at the time: "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" (Hinton 1940, 1).

[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.

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 *Prodraft* outcome variable is 0.92.

To supplement this narrative approach to identifying hawks and doves, we additionally employ an extrapolation approach based on our *Prodraft* 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 *Prodraft* outcome variable is 0.96, and the correlation between both hawkish vote variables is 0.90.

F. Supplemental Materials

We include in 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 of legislators had a nontrivial role in determining the risks of battle faced by their own sons.[24]

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

[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 (Republican, Wisconsin) son was arrested for attempting to avoid induction (Walker 2008, 206), while Edward Pou's (Democrat, North Carolina) son was killed while serving in France. During World War II, two of J. Parnell Thomas's (Republican, New Jersey) sons served, with one drafted and one volunteering (*Atlanta Constitution* 1942), while John R. Murdock's (Democrat, Arkansas) son was killed in action in 1943 (*Atlanta Constitution* 1943). In the Korean War, John V. Beamer's (Republican, Indiana) son was drafted (*Chicago Daily Tribune* 1953). One source estimates that 26 sons of legislators served during the Vietnam War (Bryan 1976).

IV. Estimation

We employ three empirical approaches to determine whether 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*

1. 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 *Prodraft* outcome variable is nonmissing. Our main specification is

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

where V_{iv} is an indictor equal to 1 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; $draft\ son_{\mathrm{iv}}$ is an indicator variable equal to 1 if a legislator has a draft-exposed son, as determined by the cutoffs in vote v; and $\mathbb{X}_{iv}$ 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 ordinary least squares.

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 $draft\ son_{iv}$ 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 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 an RD design below.[26]

Finally, in order for us to define 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 age 19, for example.

2. Balance on Observables

In 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

[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 because of 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 RD 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).

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 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.

3. 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 ($p < .01$), 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 (col. 2). In column 3, we add vote fixed effects to the baseline model, which yields a treatment effect of −7.07 percentage points ($p < .10$). Finally, in column 4, we again add controls in addition to vote fixed effects, yielding a treatment effect of −7.56 percentage points ($p < .05$), 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.

4. Additional Analysis in the 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 (fig. A2). In all four models, point estimates smoothly rise from the 1-year lead to the 4-year lead before falling off at 5 years.[27] This pattern is likely due to leading cohorts—that is, 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 (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.

TABLE 1
Effect of Draft Age Son versus Draft Age Daughter

	Prodraft Vote			
	(1)	(2)	(3)	(4)
Draft age son	−.1102***	−.1104***	−.0707*	−.0756**
	(.0375)	(.0367)	(.0369)	(.0371)
Controls	No	Yes	No	Yes
Vote fixed effects	No	No	Yes	Yes
Number of sons fixed effects	Yes	Yes	Yes	Yes
Number of children fixed effects	Yes	Yes	Yes	Yes
Legislators	1,427	1,427	1,427	1,427
Votes	103	103	103	103
Mean dependent variable	.58	.58	.58	.58
Observations	9,920	9,920	9,920	9,920

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.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

In 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 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 2-year lead in how we define the draft window, which is the optimal lead structure suggested in figure A3.[28] This implies that our sample contains legislators with marginal children plus those with children within 2 years of the proposed lower cutoff. The estimates are negative and significant across all specifications. In the most comprehensive specification (col. 4), we estimate a treatment effect of –11.08 percentage points ($p < .05$). In panel B, we combine the window votes (with a 2-year lead) with our main sample votes (with a 4-year lead) for a total of 140 roll call votes. In panel C, we combine both sets of votes using a 4-year lead. All estimates are again negative and statistically significant.

5. 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)* (cols. 1–4) and *Hawkish Vote (Extrapolation)* (cols. 5–8) as the dependent variables rather than *Prodraft Vote*. Here, the sample is no longer restricted to votes that were amenable

[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.

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 ($p < .10$) to –11.5 percentage points ($p < .01$).

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 nonoverlapping sample, we again find negative effects across all eight specifications. The coefficients range from –3.7 to –8.7 percentage points, although in part because of 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*

1. 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 an RD 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.

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 $\mathrm{RV}_{iv} \in (-h, h)$:

$$V_{iv} = \delta_0 + \rho \mathbf{I}(\mathrm{RV}_{iv} > 0) + \delta_1 \mathrm{RV}_{iv} + \delta_2 RV_{iv} \times \mathbf{I}(\mathrm{RV}_{iv} > 0) + \epsilon_{iv}, \quad (4)$$

where RV_{iv} is the running variable (son's age minus the upper cutoff), $\mathbf{I}(\mathrm{RV}_{iv} > 0)$ 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

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

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 mean squared error 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).

2. Results

We present our estimates in table A10 and figure 1. Conventional RD estimates are presented in the top panel of table A10, while bias-corrected estimates are presented in the bottom panel alongside conventional standard errors as well as robust standard errors from Calonico, Cattaneo, and Titiunik (2014). In column 1, we show that the conventional RD estimate is 18.8 percentage points ($p < .05$) and the bias-corrected estimate is 21.95 percentage points ($p < .01$ and $p < .05$ using conventional standard errors and robust standard errors from Calonico, Cattaneo, and Titiunik [2014], respectively). In 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 column 1, the estimates are close to zero and statistically insignificant.

We present graphical evidence of these effects in figure 1. In figure 1*A*, we show the plot corresponding to the conventional estimate in column 1 of table A10, together with 95% confidence intervals and binned means

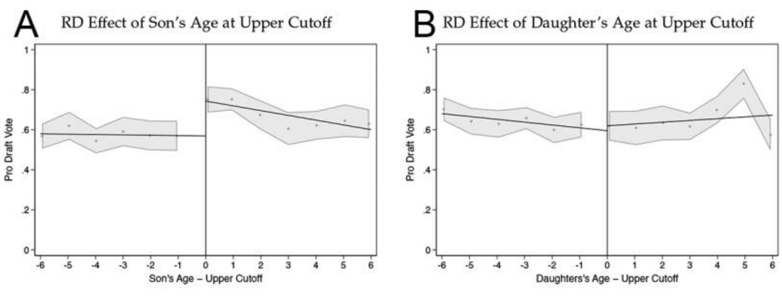

FIG. 1.—RD plots. These plots correspond to estimates in table A10. *A*, The estimate for ρ is 0.1879 ($p < .05$). *B*, The placebo estimate is -0.0044 ($p > .10$).

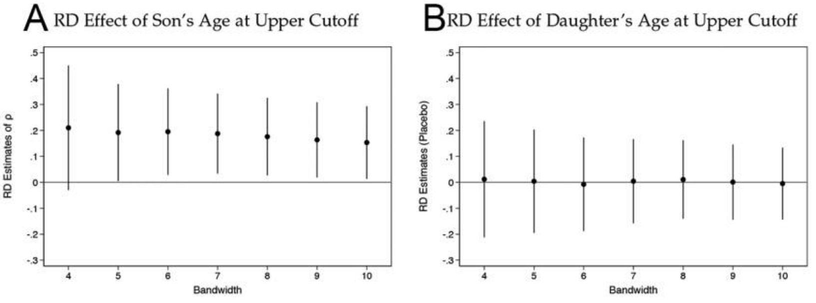

FIG. 2.—Sensitivity to bandwidth choice.

for each year. The relationship is linear up to a stark discontinuity at the upper cutoff. In figure 1*B*, 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 figure 2*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 figure 2*B* is consistently at or very close to zero.

In columns 3–5 of table A10, we test for balance using the same set of observable variables as in section IV.B.1: 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 figure A7.

Finally, in 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 ($p < .05$) to 23.18 percentage points ($p < .01$), while the equivalent placebo estimates are not significant.

C. Panel Variation in Age and Sex of Children

1. 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 time-invariant factors, such as legislators' ideological preferences and fixed characteristics related to their electorate. We restrict the sample to "switchers"—that is, 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 *Prodraft* outcome variable is nonmissing. The specification is

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

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.

2. Results

The baseline specification is presented in column 1 of table 2. The estimate of β_1^{FE} is -10.6 percentage points ($p < .01$). Adding the time-varying controls makes little difference (col. 2). We include vote fixed effects in column 3, which reduces the magnitude of the estimate to -6.4 percentage points ($p < .05$). Finally, in column 4, we again add the time-varying controls, yielding an estimate of -6 percentage points ($p < .05$). 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 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*

TABLE 2

Effect of Draft Age Son with Legislator Fixed Effects

	Prodraft Vote			
	(1)	(2)	(3)	(4)
Draft age son	−.1060***	−.1006***	−.0642**	−.0600**
	(.0372)	(.0353)	(.0297)	(.0292)
Draft age child	.0242	.0092	.0251	.0158
	(.0317)	(.0310)	(.0267)	(.0279)
Controls	No	Yes	No	No
Vote fixed effects	No	No	Yes	Yes
Legislator fixed effects	Yes	Yes	Yes	Yes
Number of sons fixed effects	Yes	Yes	Yes	Yes
Number of children fixed effects	Yes	Yes	Yes	Yes
Legislators	711	711	711	711
Votes	103	103	103	103
Mean dependent variable	.62	.62	.62	.62
Observations	9,249	9,249	9,249	9,249

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.

** $p < .05$.

*** $p < .01$.

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]

D. *Interpreting Magnitudes*

Our estimates range in magnitude from around 6 to 23 percentage points, depending on the specification.[31] To put these figures into context, in appendix F we explore the hypothesis (discussed in sec. II) 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, 2 years before the Paris Peace Accords that signaled the end of US involvement in Vietnam.

V. Establishing Causal Mechanisms

A. *Private Rents versus Information*

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 only due to variation in private rents, that is, 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

[30] These estimates are presented graphically in fig. A8.

[31] The RD effect is largest possibly because of 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.

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.

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.

B. 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.

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

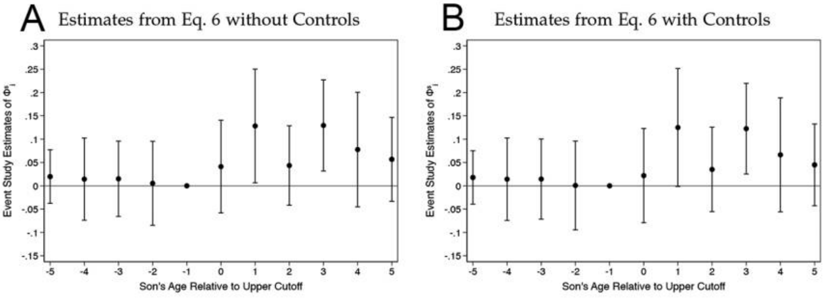

FIG. 3.—Event study plots: change in prodraft vote as sons age out of draft eligibility. The corresponding estimates are presented in table A13.

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, we find that 1 year after exiting treatment status, legislators are 12.5 percentage points more likely to support conscription; 3 years after exiting, the difference is 12.25 percentage points ($p < .10$ and $p < .05$, respectively); thereafter, the effect diminishes but stays positive. The corresponding estimates are presented in 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.

VI. 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.

A. 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.

B. *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.

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 show a sharp decline from around 70% in 1945 to around 20% in 2003 (Fordham 2016).[34] Second, we estimate the impact

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

[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).

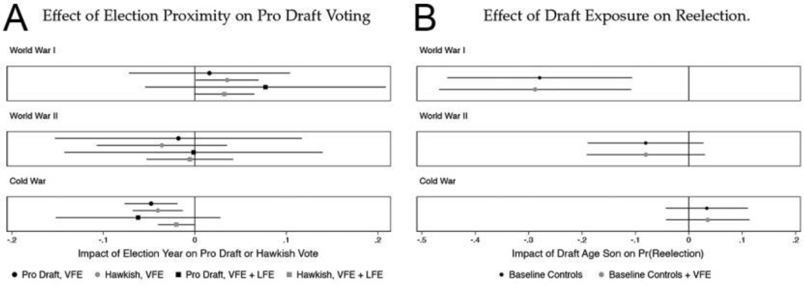

FIG. 4.—Draft popularity and reelection of exposed legislators over time. The estimating equations are presented in appendix E. See tables A14 and A15 (*A*) and table A17 (*B*).

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 6-year terms with staggered elections every 2 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 4*A* and in 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 US 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 anticonscription protest movement during the Vietnam War (Flynn 1993, 2002).

C. *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]

We present the results of this exercise in figure 4*B* and 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

[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 eight votes in the former.

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

popularity over time in figure 4*A*. During World War I, when the draft was most popular, legislators with draft age sons were less likely to win reelection ($p < .05$). 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 figure A10 that these differences are not driven by differences in the first-stage relationship between draft exposure and prodraft voting, which is negative for all three eras.[38] While we cannot rule out the influence of other trends during the twentieth 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.

VII. 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 US Congress during the four conscription-era wars of the twentieth 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 of 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

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

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.

Data Availability

Data and code replicating 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.

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