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Last Post for "The Greatest Generation": The Policy Implications of the Decline of Military Experience in the U.S. Congress

This paper characterizes the behavioral and policy implications of the decline in the number of military veterans in the U.S. Congress, from more than 70% of legislators in the early 1970s to less than 30% in the contemporary House and Senate. Many scholars argue that military service shapes information and beliefs, and that this decline has had negative effects on defense policy. The analysis tests these arguments using voting data from the House and Senate in the 1990s and the House in the 1970s, showing that the impact of veteran status on votes is generally small and has a relatively minor effect on legislative outcomes.

This paper addresses concerns about the generation-long decline of military experience among legislators in the U.S. Congress. Has this decline affected the success or failure of defense-related proposals? Do veteran legislators vote differently than nonveterans—and are these differences related to military service? The value of military experience and the justifiable concern over its decline are virtually articles of faith in the civil-military relations literature, as well as in media accounts. The aim here is to bring data to bear on these matters.

The analysis centers on almost 50 House and Senate defense votes taken in the early 1990s and eight additional House votes from the early 1970s. At the margin of other factors (such as party, incumbent characteristics, and constituent demands), the impact of veteran status is generally quite small. Moreover, veterans' personal policy preferences do not differ from those held by nonveteran colleagues. Thus, although some individuals may be transformed or informed by their military service, these effects do not appear to be systematic and the lack of military experience is not having the dire results that some have postulated.

Veterans, Congress,
and Civil-Military Relations

This paper addresses a central question in the contemporary civil-military relations literature: What are the policy consequences of the generation-long decline in congressional military experience? Figure 1 shows variation in congressional military experience among male House members since 1967, the height of the Vietnam conflict.

As the figure indicates, House military experience has declined by almost a factor of three since the 1960s. Analysis presented by Bianco and Markham (2001) shows that the decline is largely the product of generational replacement, as legislators who were veterans of World War II and the Korean War are replaced by individuals who came of age during the Vietnam conflict or thereafter and who have a much lower probability of military service.

Why is the decline in congressional military experience of interest? The expectation in the civil-military literature is that veteran legislators and other government officials will think and behave differently than their nonveteran colleagues (Holsti 1998, 2001; Kohn 1994; Sarkesian, Williams, and Bryant 1995). As a result, the typical legislator (like other nonveterans in government) is seen as lacking the information or values needed to make good defense policy choices (Cohen 2000; Desch 2001; Morgan 2001). Moreover, the literature warns of the risk of "a chasm developing between the military and civilian worlds, where the civilian world doesn't fully grasp the mission of the military, and the military doesn't understand why the memories of our civilians and civilian policy makers are so short, or why the criticism is so unrelenting" (Former Secretary of Defense Cohen, quoted in Feaver and Kohn 2000).[1]

This scholarly concern mirrors societal expectations. Ambrose (1997) and Brokaw's (1998) accounts of World War II emphasize how military service shaped American society and public policy. Thomas Ricks's oft-cited book, *Making the Corps* (1997), argues that the end of compulsory service has had negative consequences for defense policy making, partly because of the decline of veterans in Congress. Warnings about the drop in congressional veterans also appear in the media[2] and veterans' publications (Calkins 1999; Correll 1995; Davis 1999; Dyhouse 1999; Foster 1997).

Readers familiar with the congressional literature may question concerns about the decline of military experience or the existence of a civil-military gap, believing legislators, regardless of their military experience, will behave in accordance with constituent demands. If so, then any correlation between military experience and legislator behavior is

FIGURE 1
Variation in Military Experience in the U.S. House, 1967–99
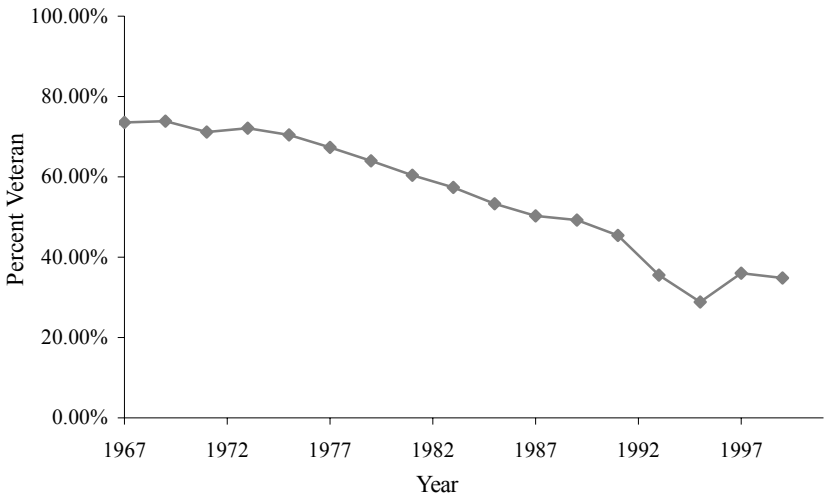
spurious and concerns over the decline of veterans in Congress are misguided. At best, military experience might have an indirect effect, if veterans are better informed about defense-related interests in their districts and use this information when deciding how to vote.

At one level, these concerns are beside the point. Whether or not the reader expects a relationship between veteran status and legislative behavior, the fact is that many scholars in the civil-military literature believe that such a relationship exists—and many decision makers in Washington agree. Given the widespread belief in this proposition and the dire warnings about its implications, and given the important policy questions at stake (for example, the desirability of some form of national service or even the revival of conscription), we can agree that a test of the proposition is appropriate.

The notion that military experience might shape legislator behavior in the defense sphere and elsewhere is reasonably plausible considering what we know about legislators' motivations and decisions. Contemporary theories (see, for example, Arnold 1990, Bianco 1994, Fenno 1978, Jackson and Kingdon 1992, and Mayhew 1974) argue that constituent demands are important constraints when proposals are salient to large numbers of voters. Yet legislators sometimes ignore constituent demands on high-salience proposals and vote according to other criteria. Moreover, high-salience proposals arise only a dozen or so times a

session. In general, vote decisions are shaped by many factors, including personal policy preferences, district pressures, and party affiliation (for reviews, see Jackson and Kingdon 1992 and Uslaner 2000; for analyses of defense votes, see Lindsay 1990, 1991).

How could military experience influence a legislator's behavior? Military service could be a marker for a bundle of attitudes, information, and values that are acquired through service. If so, military experience should be included as a right-hand-side variable in analyses of vote decisions or other behavior.[3] Moreover, even if the electoral connection has a strong impact on legislators' behavior, these mechanisms might cause congressional veterans to vote differently than their nonveteran colleagues on some proposals. This rationale is also consistent with some recent work. Fordham (2001) finds that veteran status is a significant influence on voter opinions regarding military spending and attitudes toward military service, a finding also seen in surveys of civilian and military elites (Holsti 1998, 2001). Eitelberg and Little's (1995) analysis of votes in the 1980s–90s House finds that "non-veterans are somewhat less likely than veterans to be pro-defense." And Feaver and Gelpi (2002) show that the probability that the United States will initiate a militarized dispute is sensitive to the percentage of veterans in the House.[4]

In sum, the civil-military relations literature asks a plausible empirical question about the decline of military experience in Congress. The implications of this question are profound. Will a Congress of nonveterans enact different (or worse) defense policies than a Congress dominated by veterans? Should the United States consider reinstating mandatory service as a means of bridging the civil-military gap? The first step toward addressing these questions is to establish whether or not veteran legislators vote differently than nonveterans—and whether these differences remain after controlling for other factors. That is the focus of this article.

More specifically, my analysis focuses on estimating the impact of military experience on vote decisions involving defense or foreign-policy-related "key votes" in the 102d–104th House and Senate, along with eight additional votes from the 91st–92d House.[5] Most of the works cited thus far assume veterans and nonveterans differ and that these differences will be mirrored in behavior. The goal of this paper is to reorient the debate to focus on actual behavior with measurable policy consequences.[6] Because much of the supposed impact of military service has to do with beliefs and information about military operations, defense and foreign policy votes seem the appropriate venue to determine if an impact exists.[7] I chose the 102d–104th Congresses for analysis because

they contain many important votes on defense policy and are also the nadir of military experience since World War II. I include votes from the 91st–92d House because these sessions have the highest level of veterans in Congress over the history of the United States and are situated at the beginning of the decline of congressional veterans.[8]

I assume that a multivariate analysis is appropriate for assessing the impact of military experience on behavior and outcomes. The goal is to determine whether or not veteran status matters at the margin of other factors that shape vote decisions. If military experience has no influence at the margin, then any correlation between military experience and vote decisions is indeed spurious—and concerns about the decline of military experience in Congress are surely misguided. Of course, other influences on voting such as constituency pressures may mask the true impact of military experience. Either way, a multivariate approach will reveal the true relationship between military experience and vote decisions.

Veterans, Votes, and Policy Outcomes

This analysis of the impact of military experience on vote decisions and outcomes centers on a series of multivariate logistic regression equations, each explaining votes on a particular defense proposal. Exogenous variables follow the conventional wisdom (see, for example, Fenno 1978, Jackson and Kingdon 1992, and Uslaner 2000), describing a legislator's personal ideology, gender, and military experience, as well as district demands.

The analysis is agnostic in its expectations about the signs of the parameters that relate military experience to vote decisions. Without detailed information about the nature of each proposal, it is difficult to specify how a veteran or nonveteran legislator should vote—or what the impact of military experience might be at the margin of other factors. In light of these concerns, I use three indicators to capture the impact of military experience. The first is the statistical significance (p values) of the military experience variables—that is, does military experience have a discernable impact on how a legislator votes on a particular proposal? If the civil-military literature is correct in its expectations, then this variable should be statistically significant for a substantial fraction of the proposals under study. The second measure is change in probability estimates (delta p) for military experience—at the margin, what is the difference in the likelihood that a veteran legislator would vote yea compared to a nonveteran? I report delta ps for each proposal, as well as absolute averages across all House and Senate votes. Large

delta *p*s imply that veteran status had a substantial impact on vote decisions, suggesting that the decline in military experience has had important implications for policy outcomes. Conversely, small delta *p*s, or low absolute average effects, imply that veteran status was not an important factor in a legislator's vote decision or decisions. The final measure uses the parameters for each proposal to assess how support would change if every legislator were a veteran—or if no legislator had military experience.[9] Large changes imply that the decline has had a substantial impact on defense outcomes; small differences imply that the decline has not affected defense policy, or that military experience is not an important factor in vote decisions.

The reader may complain that the analysis described here aims at proving a null hypothesis, which is of course impossible.[10] The difficulty arises because the starting point of this paper, the conventional wisdom that veteran status matters, specifies a hypothesis of nonzero effect. The alternate hypothesis is therefore one of no effect—no relationship between veteran status and behavior. It is not obvious which of these predictions should be considered the null hypothesis. More importantly, the nature of these hypotheses creates significant difficulties in the interpretation of the results. Consider the question of significance levels for regression parameters. In general, the fact that parameters satisfy higher significance levels—.01 rather than .05— usually increases confidence in a hypothesis of interest. But in this analysis, increased significance levels bias the analysis toward the no-effect hypothesis and away from the conventional wisdom. Put another way, given high enough significance levels, we will see that the no-effect hypothesis is trivially true.

This situation is impossible to eradicate, as it follows from the nature of the conventional wisdom about veterans in Congress and the goal of testing this conventional wisdom. To remove the potential for interpretation bias, however, I use three strategies, all of which are standard accommodations to the situation faced here. Rather than using one of the usual criteria for statistical significance (that is, .05 or .01), I consider statistical significance using an extremely loose .25 level. Second, I use the logistic change-in-probability (delta *p*) results to provide an alternate measure of influence that is not affected by levels of significance. Finally, I report significance levels for all of the parameters that relate veteran status to roll-call behavior. Thus, should the reader like to determine if selecting different significance levels would lead to different conclusions about the impact of veteran status on behavior, all the necessary information is available.

Model Specification

The unit of analysis is an individual legislator. I use the following model to measure the impact of military experience on votes:

$$Y_{ik} = \beta_1 + \beta_2(\text{Veteran}) + \beta_3(\text{Young Veteran}) + \beta_4(\text{Female})$$
$$+ \beta_5(\text{Party}) + \beta_6(\text{Party} * \text{Female}) + \beta_7(\text{Ideology Dim. 1})$$
$$+ \beta_8(\text{Ideology Dim. 2}) + \beta_9(\text{Dem. Vote}) + \beta_{10}(\text{Ind. Vote})$$
$$+ \beta_{11}(\text{Pct. Military}) + \beta_{12}(\text{Bases}) + \upsilon_I$$

The dependent variable, Y_{ik}, is legislator i's vote on proposal k. The model is estimated fifty-five times: twenty-one 102d–104th House votes, twenty-six 102d–104th Senate votes, and eight 91st–92d House votes. The first parameter, β_1, is the intercept. β_2 and β_3 capture the impact of military experience on voting. The first variable, *Veteran*, equals 1 for all veterans and 0 for all other legislators. *Young Veteran* equals 1 for all veterans born in 1945 or thereafter, and 0 for all others. (Veteran data come from McKibbin 1997, augmented by the *Congressional Biographical Dictionary*.) The cohorts divide legislators who faced general conscription from those for whom service was, to some extent, a matter of choice. (The Senate and early House estimates omit the second veteran variable because few legislators have the requisite birth dates.) If military service shapes attitudes and information, then veterans should vote differently than nonveterans—that is, β_2 and β_3 should be statistically and substantively significant.

Party (Democrat = 1, Republican = 0) captures ideological differences across the caucuses, as well as any party pressures that arose when proposal k was voted on. *Female* equals 1 for all female legislators and 0 for all male legislators, and it captures any gender-related behavioral differences, which is crucial because no female legislators are veterans. The Female-Democrat interaction is included for the same reason. (Senate and early House regressions omit the gender variables because of the small number of female legislators.) *Ideology Dim. 1* and *Dim. 2* are legislator i's NOMINATE scores for the session that proposal k was voted on (Poole and Rosenthal 1997), and they measure personal policy preferences.[11] *Dem. Vote* (vote for the Democratic presidential candidate in last election) and *Ind. Vote* (vote for the principal independent candidate in the last presidential election—not used for the 102d House and Senate) capture the overall partisan balance of legislator i's district. *Pct. Military* (percentage of active-duty personnel in district workforce) and *Bases* (number of major military installations) account for unique demands expressed by

individuals on active military service, as well as economic or pork-barrel pressures from constituents who have a direct or indirect involvement with base operations (Adler and Lapinski 1997).

Results

Given the number of estimations in this analysis, I do not report parameter estimates for each proposal, focusing instead on aggregate measures and transformations. (The estimate results are available on request.) Table 1 lists delta p values for several exogenous variables. For the military experience variable, the delta p is simply the difference in the probability of a yea vote if other variables are held at their sample means while the veteran variable moves from 0 to 1. For continuous variables, the increase is one standard deviation; for instrumental variables, 0 to 1. Table 1 reports the mean of the absolute values of the delta ps for each proposal.[12] Delta p estimates for gender are reported separately for each party. The results show that in the 102d–104th House, the absolute average impact of being an old veteran is .07—transforming the typical member born before 1945 from a nonveteran to a veteran changes (increases or decreases) the probability of voting yea by an average of .07. The absolute average delta p for young veterans is .09. Senate and early House results are similar.

These results suggest that military experience has a nonzero but not decisive impact on vote decisions. The average impact of veteran status is the smallest of all the results given in Table 1; the change reported for veterans pales in comparison to the impact of party affiliation (.48) or of a one-standard-deviation change in ideology (.21). At one level, this finding is no surprise: the proposals being analyzed often split legislators along partisan and ideological lines, so it makes sense that party affiliation and personal preferences shaped legislators' decisions. But the results for military experience are comparable to those reported for gender—itself not usually seen as an important influence on defense or foreign policy votes.

Table 2 reports the statistical significance (p value) and change in probability estimates (delta p) for the military experience variable in each House regression. P values less than .25 are indicated by an asterisk (smaller values = less support for the no-effect null hypothesis). The three left-hand columns report the year, roll-call number, and a brief description of each proposal. Table 2 also clarifies the impact of military experience on outcomes by allowing for comparison of the actual percentage of yea votes for each proposal to two hypothetical measures. *All-Vet % Yea* reports how many legislators would have

TABLE 1
Absolute Average Change in Probability Estimates
for Military Experience and Other Variables,
102d–104th House and Senate, 91st–92d House

Variable	Absolute Average Change in Probability Estimates for		
	102d–104th House	102d–104th Senate	91st–92d House
Old Veteran	.08	.10	.09
Young Veteran	.10	—	—
Female (R)	.16	—	—
Female (D)	.13	—	—
Party (Dem.)	.49	.52	.37
Ideology	.21	.31	.19
Democratic Vote	.10	.10	.05

voted yea if all were veterans, a percentage calculated by using the logit parameters to generate the probability of a yea vote by each legislator while assuming the veteran variable equals 1 for all legislators and the young veteran variable equals 1 for everyone born after 1945. These probabilities are then summed across all legislators to get the expected percentage of yea votes. *No-Vet % Yea* is the expected percentage with the veteran variables set to 0.

Consider the vote on HR 2491, the vote to table a resolution aiming to end draft registration. The *p* values for the military experience and young veteran variables are .13 and .12, indicating that military experience had a statistically discernable impact. For older legislators, being a veteran increased their probability of voting yea by .12— substantially greater than the typical impact of .07 reported in Table 1. For young veterans, the delta *p* is substantially higher, .32. The hypothetical measures show that when HR 2491 was voted on, 49.6% of legislators voted yea. If there were an all-veteran legislature, then the expected yea vote would be 60.0%, enough to table the proposal. If no legislators were veterans, then the expected percentage of yea votes would be 46.9%, not much different from the actual number.[13] Thus, for HR 2491, the relatively small number of House veterans—or the decline over the last generation—made a sure winner into a close loser.

TABLE 2
The Impact of Veteran Status on Defense-Related Votes
in the U.S. House of Representatives, 102d–104th House

Year	Roll Call	Bill Number (Description)	Veteran Status		Young Veteran		Actual % Yea	All-Vet % Yea	No-Vet % Yea
			P Value	Delta P	P Value	Delta P			
1991	9	HJ RES 77 (P: Gulf War Auth.)	.56	.03	.97	.03	57.9	59.0	56.9
1991	144	HR 2521 (A: MX Missile)	.09*	−.13	.41	−.23	40.4	34.4	43.0
1991	261	HR 2100 (P: Vets Benefits)	.82	−.01	.90	−.03	60.3	59.7	60.5
1991	354	HR 2508 (C: Foreign Aid)	.67	−.03	.06*	.25	37.8	41.1	37.8
1992	164	HR 5006 (A: Nuclear Testing)	.03*	−.17	.97	−.17	56.0	52.3	59.0
1993	183	SJ RES 45 (P: Somalia Auth.)	.57	−.10	.46	.18	58.0	58.9	58.4
1993	222	HR 2401 (A: Burden Sharing)	.33	−.06	.24*	.09	46.6	47.5	47.5
1993	223	HR 2401 (A: Close Foreign Bases)	.47	.04	.87	.06	67.4	69.8	66.4
1993	417	HR 2401 (A: Gays in Military)	.30	−.05	.04*	.05	69.4	69.6	72.9
1993	418	HR 2491 (A: Prevent Draft Term.)	.13*	.12	.12*	.35	49.6	60.0*	46.9
1993	462	HR 4301 (A: Arms for Bosnia)	.08*	−.11	.33	.02	57.8	54.9	60.1
1994	278	HR 4301 (A: Bosnia Bombing)	.01*	.17	.09*	−.06	42.4	45.6	38.8
1995	359	HR 1561 (A: Repeal War Powers)	.93	−.001	.59	−.08	48.2	46.5	48.7
1995	370	HR 1530 (A: B-2 Funding)	.36	−.07	.41	.03	48.6	48.3	49.3
1995	378	HR 1530 (A: Prison Labor)	.51	.06	.89	.08	49.5	51.4*	48.6
1995	396	HR 1817 (A: Tenn. Firing Range)	.06*	−.16	.61	−.09	49.5	46.2	52.1*
1995	401	HR 1817 (P: Military Construction)	.31	.04	.67	.06	75.0	79.2	73.6
1995	639	HR 2126 (A: Fund B-2 Bombers)	.58	−.04	.64	.01	49.6	49.4	50.4*
1995	856	HR 2770 (P: Bosnia Deployment)	.84	.02	.88	−.001	48.8	49.1	48.6
1996	174	HR 3230 (P: Defense Authorization)	.58	−.04	.64	−.07	64.6	62.5	65.6
1996	216	HR 3540 (A: Intl. Military Education)	.11*	−.11	.81	−.08	56.5	52.4	59.1

A = amendment; C = conference report; M = other motion; P = final passage; T = motion to table.
*p < .25.

Table 2 shows that HR 2491 is very much the exception to the rule. Of the 21 proposals, military experience is statistically significant at .25 or better in only seven cases. The young veteran variable is statistically significant in only five cases. Moreover, military experience is significant at .05 or better in only two cases; the young veteran variable meets the criterion in only one case. The delta p estimates are generally in the single digits. Finally, the all-veteran outcome differs from the actual outcome in only two cases: HR 2491 and HR 1530 (Prison Labor Amendment). In two other cases, HR 1817 (Tennessee Firing Range Amendment) and HR 2126, the no-vet outcome (enactment) differs from the actual outcome (defeat). Since all four votes were close, however, these effects do not support the idea that military experience generally has a large impact on legislative outcomes. Thus, although Table 2 contains many measures on which veterans might reasonably see things differently from nonveterans—veterans' benefits; draft registration; gays in the military; the Gulf War, Somalia, and Bosnia authorizations; and repeal of the War Powers Act—once we control for other factors, we discover that veterans vote like nonveterans. If we were trying to predict outcomes or vote decisions, then the results would suggest we focus on constituency, personal policy preferences, and partisan lobbying rather than on military experience.

Table 3 presents similar results for the 26 votes in the 102d–104th Senate. Military experience is statistically significant at .25 or better in only four cases, with only one case of significance above .05. The delta p estimates are usually in the single digits. And the hypothetical outcomes differ from the actual in only three cases.

Looking across the two chambers strengthens these findings about the modest impact of military experience. There are no cases in which military experience produces a statistically significant, substantively similar impact on votes in the House and Senate. In the case of the Gulf War authorization, for example, military experience has no impact in the House but a modest impact in the Senate; in the case of draft registration, the pattern is reversed. Factors other than veteran status are driving vote decisions in both chambers. Note also that across the House and Senate, the percentage of cases in which military experience is significant at .25 or better is less than 20%—less than what we would expect by chance if military experience had no impact on behavior.

Finally, Table 4 gives similar information for the eight votes in the 91st and 92d House. At first glance, the results suggest that the impact of military experience may have been higher a generation ago—the veteran variable is statistically significant in three of eight cases, and

TABLE 3
The Impact of Veteran Status on Defense-Related Votes in the U.S. Senate, 102d–104th Senate

Year	Number	Bill Number (Description)	Military Experience P Value	Military Experience Delta P	Actual % Yea	All-Vet % Yea	No-Vet % Yea
1991	2	SJ Res 2 (P: Gulf War Authorization)	.23*	-.17	52.0	49.0*	56.1
1991	142	HR 2212 (P: China MFN)	.59	.09	55.6	56.6	52.5
1991	174	S 1507 (A: B-2 Bomber)	.37	.11	42.3	45.1	38.0
1992	69	SC Res 106 (A: Defense Spending)	.84	-.04	46.9	46.9	47.9
1992	83	S 2403 (A: Seawolf Funding)	.03*	-.34	48.0	38.8	58.2*
1992	167	HR 5373 (A: Nuclear Test Ban)	.77	-.007	71.6	70.5	71.6
1992	214	S 3114 (A: SDI Funding)	.73	.07	49.0	50.0	48.0
1993	9	HR 2401 (T: Gays in the Military)	.67	-.05	34.3	32.3	37.5
1993	273	HR 2491 (P: End Draft Registration)	.26	-.15	58.6	53.5	62.6
1993	310	HR 3116 (A: Seawolf Funding)	.77	.04	52.5	55.5	50.5
1993	313	HR 3116 (A: Somalia)	.90	.016	61.0	61.0	60.0
1994	5	S 1281 (A: Vietnam Trade Embargo)	.64	-.05	62.6	60.6	64.6
1995	331	S 21 (P: Bosnian Arms Embargo)	.13*	-.23	70.4	59.1	76.5
1995	357	S 1026 (A: Arms Sales Loans)	.55	-.10	41.4	39.3	44.4
1995	359	S 1026 (T: Nuclear Testing)	.91	.004	56.0	51.0	56.0
1995	368	S 1026 (A: Limit Use of Mines)	.90	-.008	71.3	71.3	71.3
1995	391	S 1087 (T: Space-based Lasers)	.56	-.11	58.2	56.1	60.2
1995	603	SJ Res 44 (P: Bosnia Deployment)	.51	.06	69.7	71.7	66.7
1996	113	SC Res 57 (A: Defense Budget)	.60	-.10	42.4	40.4	44.4
1996	5	S 1124 (C: Defense Spending)	.60	.05	62.2	65.5	60.0
1996	160	S 1745 (A: Missile Defense)	.42	-.24	45.4	43.3	48.4
1996	173	S 1745 (A: Defense Spending)	.18*	-.35	45.0	39.0	50.0*
1996	176	S 1745 (T: Nuclear Weapons Tests)	.71	.07	54.1	56.1	53.1
1996	194	S 1894 (T: Defense Contracts)	.28	.03	70.4	75.5	67.3
1996	197	S 1894 (A: F-18 Funding)	.65	-.09	44.0	43.0	46.0
1996	243	HR 3540 (T: Burma policy)	.97	.005	45.5	45.5	45.5

A = amendment; C = conference report; M = other motion; P = final passage; T = motion to table.
*p < .25.

TABLE 4
The Impact of Veteran Status on Defense-Related Votes in the 91st–92d House

Year	Number	Bill Number (Description)	Military Experience P Value	Military Experience Delta P	Actual % Yea	All-Vet % Yea	No-Vet % Yea
1969	148	HR 15149 (P: Foreign Aid)	.33	–.05	51.6	49.9	54.2
1970	117	HR 15628 (T: Foreign Military Sales)	.59	.04	60.7	61.2	59.8
1971	205	HR 8687 (T: Defense Authorization)	.02*	.18	52.5	55.4	46.8*
1971	320	S 2819 (T: Foreign Military Assistance)	.13*	.16	55.9	58.5	52.1
1971	22	HR 6531 (A: Military Draft)	.40	–.06	49.9	48.6	51.4*
1972	223	HR 16029 (A: Foreign Military Aid)	.30	.06	54.3	54.7	49.2*
1972	280	HR 16742 (M: Foreign Travel)	.06*	–.12	60.5	58.4	64.9
1972	105	HR 14989 (A: State, Justice Appro.)	.79	.02	43.7	44.0	43.1

A = amendment; C = conference report; M = other motion; P = final passage; T = motion to table.
*$p < .25$.

subtracting the impact of veteran status changes the result in three cases as well. Yet significance in three out of eight cases is only one more case than we would expect by chance at the .25 level. The delta *p* estimates are similar to those observed for the contemporary House and Senate. Finally, removing the impact of veteran status on votes changes the outcome of voting in three cases, but all these cases were extremely close votes in which small changes could easily alter the result. Thus, the observed shifts are no surprise.

Discussion

This paper places the decline of congressional military experience in context. It focuses on key defense and foreign policy votes—votes on which the lessons, values, and information conveyed by military service might be expected to shape behavior, but also votes that settle questions of national significance. The analysis finds little evidence that the decline in congressional military experience has caused a systematic policy bias. At the margin of other factors, military experience does not typically have a statistically or substantively significant impact on vote decisions or legislative outcomes.

These results do not eliminate the possibility that military experience matters on other kinds of votes, such as proposals that are of interest only to veterans, or on committee proceedings. Nor do they preclude other effects, such as resentment among military officers toward nonveteran legislators or difficulties in explaining military operations to legislators whose only exposure to the military is through movies and history books. Nevertheless, this paper's finding (or nonfinding) addresses a key concern in the civil-military literature. Military veterans are certainly vanishing from the contemporary House and Senate, but this transformation does not appear to be affecting legislator behavior or legislative outcomes.

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NOTES

1. I omit many other academic and mass media citations because of space considerations. For more discussion, including numerous citations to works that arrive at conclusions similar to those described here, see the contributions to Feaver and Kohn 2001.

2. C.J. Chivers, "Military Fights an Imaginary Rift with the Public," *USA Today*, 14 Sept. 1999.

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Scott Shepard, "Number of Ex-Military in Congress Does About-Face," *Austin American-Statesman,* 30 May 1994, A3.

John R. Zillman, "The Troubling Dearth of Military Vets in Congress," *Christian Science Monitor,* 13 January 1999, 13.

3. One referee suggested that military experience might also influence other kinds of legislator behavior or have an indirect effect on roll-call votes through variables such as party affiliation or policy preferences. I performed analyses, omitted here but available upon request, examining these effects. I found that veterans are more likely to be assigned to defense-related committees in the House and Senate, but there is no evidence of a partisan bias: the percentage of veterans among House and Senate Democrats and Republicans is roughly similar in all of the recent Congresses. I also performed a multivariate analysis of legislator's personal policy preferences (proxied by NOMINATE scores), but I found no evidence of a relationship between ideology and military experience. Even so, it is possible that differences in preferences exist on a narrow range of military-related topics, especially pay and other quality-of-life matters. Such a finding would be consistent with the literatures on women and minorities in Congress (e.g., Canon 1999; Carroll 1990; Darcy, Welch, and Clark 1994; and Thomas and Welch 1991), which finds preference-based differences, but only for some issues (for women, social-welfare concerns; for minorities, civil rights).

4. These findings support the direct-effects argument, but they do not confirm it: Fordham analyzes voters not legislators; Eitelbert and Little do not control for other influences, such as constituent demands; and Feaver and Gelpi's aggregate-level analysis does not establish that military experience influences individual vote decisions.

5. "Key votes" are selected by *Congressional Quarterly* and are generally considered important or decisive by insiders. This analysis includes all defense key votes, excluding lopsided votes (greater than 75–25% split) and a few party-line votes

for which estimation was problematic. These exclusions remove votes for which military experience is unlikely to be a significant influence: experience is irrelevant if there is near-unanimity or if legislators divide along party lines (the caucuses contain roughly equal percentages of veterans). The focus on major legislation reflects the concern that the decline has significant consequences for major issues in defense policy. If experience matters only when minor, veteran-specific issues are voted on, then the policy relevance of decline is open to question.

6. The reader may wonder about selection bias due to agenda effects: suppose that having fewer veterans changed the range of enactable defense proposals; knowing this, defense-related committees wrote proposals favored by nonveterans. Both veterans and nonveterans would vote yea—nonveterans because the proposals look good, and veterans because something better is unenactable. But selection bias does not appear to be a problem here. First of all, it is not obvious why veterans would vote for injurious proposals or write them on committee. Particularly in the Senate, veterans dominate defense-related committees—in 1991, 54.5% of House committee members were veterans; in the Senate, 74.1% were veterans. Moreover, many of the proposals in this analysis were not subject to extensive committee markup. Analyzing votes in the 1970s, when more than 70% of members of Congress were veterans, provides a final check, as the scenario outlined here is unlikely to occur in a chamber dominated by veterans. Similar results across the two time periods—shown later in the paper—argue against selection bias in the 1990s data.

7. Another possibility is that military experience shapes votes on other proposals, such as communitarian reforms (for example, national service) or issues of morality and values (such as abortion or welfare). The focus on defense and foreign policy makes sense here given the concerns expressed in the civil-military literature and because of the obvious link between military experience and these policy areas.

8. The fact that there are fewer roll calls to analyze in the 91st and 92d House compared to the 102d–104th House and Senate is due to the introduction of electronic voting in 1975.

9. This strategy is necessary because the fact that military experience has a large impact on vote decisions does not imply that it affects the outcome. For example, even if military experience increases the probability of a yea vote on a proposal, it might not change aggregate totals much if the probability of a yea vote (and the expected number of yea votes) is already high because of other factors. Similarly, even if the impact of military experience is low, it might change the outcome on an extremely close vote if many legislators are veterans.

10. I am grateful to a referee who highlighted this problem for me, and forced me to explain the logic of the analysis.

11. The reader may argue that NOMINATE scores capture a combination of the representative's personal preferences and constituent demands. Substituting this interpretation would not change the analysis or alter its findings. The other notable concern is that the interpretation of NOMINATE scores may vary over time. Because the analysis here is limited to votes in the 1970s and 1990s, such concerns are unlikely to arise. The problem is also moot because the NOMINATEs are included as control variables and are not interpreted in any way.

12. I do not use simple means because the signs of the delta ps vary, so the mean would suppress variation. To save space, I omit exogenous variables that are usually

nonsignificant in the estimations (e.g., independent vote, percent military, and bases) from the table.

13. The change in expected votes is bigger for the all-vet calculation compared to the no-vet calculation because relatively few (~25%) House members are veterans.

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