Warriors Don’t Bowl Alone: Military Service and Civic Participation

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Abstract

Without much evidence, experts on social capital have dismissed the importance of military service on social capital accumulation. We present evidence from the National Survey of Families and Households (Wave 1: 1987-188) that sharply contradicts these claims. Using an index of civic participation calculated from time spent in activities of civic organizations, we find that veterans have significantly higher rates of civic participation than non-veterans. The results are particularly robust for combat veterans. In our estimation based on a theoretical model of time allocation, combat veterans have a 23% higher rate of civic participation than non-veterans, and non-combat veterans have an 11% higher rate. The positive effects for combat service persist across all age groups in the sample, which is defined as men aged 30-64 (a sample that includes men from all three major conflicts in the mid 20th century).

Our analysis of causal pathways rejects the importance of labor supply effects as playing any role in the effect of military service on participation. Additionally, we reject the hypothesis that military service effects are due to the GI Bill or other policies that increase education attainment of veterans, though we find that increased education definitely raises civic participation (as many others have found). The causal pathway for military service appears to be primarily through a transformation of preferences towards civic time. Analysis of participation in specific organizations shows that roughly half of increased participation of combat veterans (excluding participation in veterans groups) is devoted to groups primarily devoted to civic service. Furthermore, the effects of military service on participation in specific groups are negatively correlated with the effects of education, reinforcing the finding that military service has distinct effects from the widely-studied effects of education.
1. Introduction

In the classic civic republican story of Cincinnatus, this citizen-soldier leaves his plow to heroically take up the sword in defense of his community. Once the enemy is vanquished and the war over, Cincinnatus famously returns to his plow, eschewing political power for the private life of the farm. Of course, we know of this long-dead warrior because of his superior virtue, especially in comparison to many others who failed to surrender the sword once it was wielded. Indeed, many famous war heroes, true or fashioned, have succumbed to the temptation of politics. American history, let alone world history, is rife with examples, for good or for bad. Washington, Jackson, Grant, and Eisenhower are among the more well-known; Harrison and Taylor were heroes of their day but mostly forgotten to us.

But what of the not-so-famous warriors? Does the tale of Cincinnatus on the one hand and Washington on the other fully capture what these men – and now also women – have done once they have returned from the battlefield? Do they simply take up the plow – or computer, wrench, or steering wheel – and otherwise bask in the bounty of private life or get involved in the nitty-gritty of local, state, or national politics?

This article seeks to show that neither of these two archetypes and narratives realistically depicts the life of the returned soldier in terms of his participation or lack thereof in community life. Instead, we find that those who perform military service are more likely than their fellow citizens to be civically engaged. Indeed, they participate in the kinds of intermediary institutions that de Tocqueville celebrated at significantly higher rates than others. And even more interesting, those who faced combat are even more civically engaged than other returned soldiers. In short, warriors are not among those who “bowl alone” in contemporary America.

What these soldiers have proven adept at is building social capital upon their return from not only the training grounds but also the battle grounds. Following Robert Putnam’s seminal work on civic engagement, the social capital literature has exploded and is quite broad. Yet while war as one of many types of extreme national challenges has been examined in the context of social capital, there has been little consideration of the specific role military service may play.

We aim to fill that void by looking at a particularly important aspect of social capital: civic

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1 For ease, unless otherwise noted, we use the term soldier to refer to all members of the Armed Forces - soldiers, sailors, airmen, and Marines alike.
2 For evidence of the enormous growth in research on social capital, as well as fine introductions to the subject, see Halpern, 2005: 9; and Field, 2003: 4.
participation. In particular, we argue that military service and combat experience tend to reshape individual preferences towards civic life such that they participate more frequently in it. Moreover, one of the novel aspects of this study is that we explain this phenomenon with reference to the issue of time allocation; we thus make the counterintuitive argument that despite the frequently higher opportunity cost of the veterans’ time, they are more likely to engage in civic life given the increased utility they get out of it relative to the private time they lose in the process.

2. Conceptual Background

The aim of this article is to examine the relationship between military service, combat experience, and civic engagement. In the process, we hope to explicitly contribute to the large literature on social capital given that civic participation is both a means to the formation of social capital and a critical indicator of its existence. In particular, by bringing in neglected military variables, we seek to round out our collective understanding of the determinants of social capital. We also aim to better our knowledge of the specific effects of military service and combat experience on individual soldiers, their families, and the broader community in which they live.

Unfortunately, the link between military service and civic engagement of any kind has been largely ignored by social scientists. Even when it is touched on, military service is relegated to the sidelines, as it is by Robert Putnam who rules it out as an influence on social capital in a mere footnote (Putnam, 2000: 485, FN41). This oversight is surprising given that war has had huge effects on the lives of soldiers and civilians as well as being a crucible for so many aspects of political life and institutions (See Modell and Haggerty, 1991; Kryder, 2000; Karsten, 1978; Ruger, Wilson, and Waddoups, 2002; Higgs, 1987; and Skocpol, 1992). Nonetheless, a few scholars have tried to fill this lacuna; the next section of the paper describes and examines this literature. What this reveals is that more work is needed to understand the relationships between military experience, including combat, and civic engagement.

3 There is some controversy about what social capital is and whether those who study social capital have adequately operationalized it in their research (Halpern, 2005, 9-10). Roughly speaking, social capital refers to social networks and the norms and trust that develop amongst individuals in these complex relationships. Civic participation is a critical indicator for social capital and a key element of the formation and maintenance of the networks, trust, and norms at the heart of the concept. Unfortunately, as many have pointed out, there are many problems with such definitions and operationalizations. However, it is not our task here to deal with these important problems. On social capital, see Hanifan, 1920; Coleman 1988; Putnam, 1995; Fukuyama 1999; Putnam 2000; Field, 2003; Ostrom and Ahn, 2003; Putnam 2004; and Halpern, 2005.
2.1. Military Service and Political Participation

A few studies exist that suggest a link between military service and a more specific form of civic engagement via political participation. Despite these attempts, Christopher Ellison is still correct when he notes (and David Leal more recently confirms), “In sum, there is little solid evidence regarding the effects of military service and/or combat experience on subsequent conventional political participation in the general population” (Ellison, 1992: 364; and see Leal, 1999: 155).

Two of the most prominent studies that have examined this link have focused on black and Latino veteran political participation. Leal focuses on Latino veterans and concludes that the “the null hypothesis, that military experience is unconnected to civic involvement, is clearly incorrect” – at least for this group (Leal, 1999:170). Instead, he finds that “Latino veterans, and particularly draftees, exhibited higher levels of voting and low-intensity nonelectoral political activities. Anglo veterans did not increase their participation to the same extent” (153) Ellison also finds military service to be an important variable. In his study of black veterans, he concludes that “In general, military background variables [of blacks] are unrelated to low-initiative political activity but are strong predictors of high-initiative political involvement” (Ellison, 1992: 369). Duration of service, however, was found to have little effect, though it does reduce the likelihood of voting in state/local elections (371). One of his most important findings is that combat matters in terms of political participation. This will be discussed at more length below.

The most important studies of veteran political participation are Kent Jennings and Gregory Markus’s examination of Vietnam veterans and Jeremy Teigen’s more recent work on veteran voting. Unlike the previously discussed articles, these studies do not focus solely on blacks or Latinos. Jennings and Markus’ find that generalizations are difficult to make given the variation among different groups in the sample. However, their data lead them to argue that “With the possible exception of general political interest, military service per se makes for scant differences in the 1973 profiles of our 1965 high school senior cohort.” In other words, military service had little quantifiable effect on political preference. Teigen finds that nearly all veterans in his sample participate at significantly higher levels than “similarly aged” non-veterans. However, the big exception is Vietnam veterans who “exhibit statistically lower rates of political
participation than nonveteran men of their generation” (Teigan, 2006: 604). This last point challenges the findings of Jennings and Markus’ focused study of Vietnam veterans, and it highlights the need for more research in order to satisfactorily answer the questions at the heart of these studies.

Of course, if those who find that military service has an impact on participation are right, it would be instructive to know why and how it translates into increased political participation. Leal argues that one possibility is that “The military . . . may be a training ground for many of the same politically relevant skills that Verba et al (1995) found can be learned in churches and the workplace.” However, he rules this out because he found differential rates in political participation for draftees and volunteers (see 170). In fact, he speculates that the difference could be attributed to combat. Ellison provides more thoroughgoing speculation, suggesting several possible reasons why military service and/or combat might matter:

1. “The experience of combat duty, and to a lesser extent basic military training, may strengthen collective commitment among individual veterans, instilling a sense of responsibility for (a) contributing to the welfare of others and (b) assuming undesirable but necessary tasks”;
2. “Combat veterans and other military veterans may be viewed as leaders within the black community, and may be accorded particular respect because of their past sacrifices and accomplishments”;
3. “The successful completion of military service, and particularly combat survival, may foster or accentuate certain personality traits conducive to high-initiative political involvement: self-confidence, persistence, self-discipline, and pragmatic problem-solving orientations”;
4. “For black soldiers, military life may confer more specific attitudes” plus “may leave black veterans better prepared than their nonveteran counterparts to deal with government agencies and other bureaucratic institutions” (Ellison, 1992: 373).

But what about civic participation more broadly? Are soldiers interested in more than just politics when they leave the service? Indeed, one might argue that civic engagement in general is a more important way that veterans might participate in communal life, especially if the kind of political participation that veterans engage in is of the rent-seeking rather than community-enriching variety (Mueller, 2003).
2.2. Military Service and Civic Participation

Surprisingly, given its central role in the history of states and societies, the military has been generally ignored in the study of civic participation. Of course, de Tocqueville is a notable exception as he worried about the effects of war on civil society. However, according to Theda Skocpol, this interest was not carried forward in the early classics on civic participation. For example, in Almond and Verba’s classic study *The Civic Culture*, “the effects of war on civic engagement were not explored” (Skocpol et al, 2002, 138-139).

This paucity of emphasis on the possible link between the military and civic participation has carried over into the current period of study on social capital. Skocpol argues that this is because most scholars of civil society “rely on an institutional displacement understanding of the relationship between state activity and voluntarism” (Skocpol et al, 2002, 139). What has been done is either centered around the political participation studies outlined above or on political opinions/attitudes (for examples of the latter, see Feaver and Gelpi, 2004; Feaver and Kohn, eds., 2001; Janowitz and Wesbrook, 1983; Jennings and Markus, 1977; and Schreiber, 1979). However, there are some recent studies that have looked at the broader link. Yet most of these focus on the connection between the war in general – in particular the energy, spirit, and needs sparked by war and the institutions developed to handle them – and civic participation rather than the relationship between wartime service itself and future civic engagement. Indeed, as Modell and Haggerty note, “little has been done to assess what kind of difference it makes to the individual in his civilian capacity, or to the society of which he is a member, when he is a veteran” (Modell and Haggerty, 1991, 220-221).

In these few studies, there is a strong sense that wars do increase civic participation but not necessarily the participation of veterans. Skocpol, for example, argues that “U.S. wars have promoted civic vitality. In a nation whose citizens are famous for their proclivity to organize and join voluntary endeavors, outbreaks of martial conflict have sparked voluntarist upsurges that repeatedly carried over into postwar eras” (Skocpol, 2002, 537). Indeed, “Big wars have been surprisingly good for American civic voluntarism. The Civil War and the twentieth-century world wars spurred the creation of new associations and buoyed the fortunes of preexisting groups willing and able to join victorious wartime mobilizations” (Skocpol, Munson, Karch, and Camp, 2002, 134). Putnam seconds this view, noting that from his study of the matter,
“membership in civic associations has spurted after both major wars in the twentieth century, and political scientist Theda Skocpol has extended this argument to the whole of American history” (Putnam, 2000, 267).

Skocpol and Putnam, however, do not link military service itself with the increased civic engagement that they find war has occasioned. Unsurprisingly, Skocpol, the architect of the “bringing the state back in” movement in political science makes an “institutional synergy” argument for why participation spikes during and following wars (See Skocpol et al, 2002). Putnam, on the other hand, holds that war causes “patriotism and collective solidarity” or “shared adversity” which in turn translates into increased civic participation (see Putnam, 2000, 54 and 270). Moreover, he dismisses the argument that military veterans might be part of the reason for post-war associational enthusiasm. To the contrary, he argues that veterans are not more engaged even as the war generations to which they belong are. He argues: “Veterans are not more engaged civically than other men of their generation. The enduring effects of World War II on the civic habits of those who lived through it were not limited to the battlefield. Or perhaps the brutalizing effects of combat counterbalanced its communitarian effects” (Putnam, 2000, 485, FN 41).

The most important and sustained look at the issue of veteran civic participation is found in Suzanne Mettler’s work on the G.I. Bill (Mettler, 2002; Mettler, 2005). She finds that veteran status does matter but only indirectly through access to the G.I. Bill educational benefits veteran status conferred. Specifically, it is via the use of these benefits that veteran status matters in terms of later civic engagement and not because of what soldiers experienced or gained via military service or battlefield experience. In comparing those veterans that participate (civically and politically) versus veterans that do not, Mettler finds that veteran civic engagement was highly impacted by whether they used the GI Bill for education. Crucially, she claims it is not just education doing the driving, an important point if true given that education is, as Putnam argues (and widely confirmed), “the strongest correlate that I have discovered of civic engagement in all its forms” (Putnam, 1995, 667; Brehm and Rahn, 1997; Hall, 1999; Leal, 1999; Putnam, 2000; Brewer, 2003; and Glaeser, Laibson, and Sacerdote, 2002).\(^4\) Instead, Mettler argues that the GI Bill worked to increase civic participation by causing a feeling of reciprocity among those who used it (as they felt a need to give something back to society) and

\(^4\) SVEN --- ADD FOOTNOTE HERE ABOUT HER FN15 and what it means??????
specifically helped lower socio-economic members feel they owed something back as well as empowering them to do so (Mettler, 2002).

However, a few studies are actually highly skeptical, period, of any link between military service and civic and political engagement. Ronald Krebs in reviewing these works notes that they “have concluded that military service, even during wartime, has left veterans politically unmoved or even alienated” (Krebs, 2004, 112 and 118-119). One of these, by Kent Jennings and Gregory Markus, was touched on above. However, Krebs is right when he concludes that “more focused research is needed before these questions can be answered with confidence . . .” (Krebs, 2004, 119).

As will be developed below, our analysis challenges these skeptics as well as the works of Putnam and Mettler about the non-effect of veteran status itself. Veterans are more engaged civically than other men of their generation. In our findings, World War II vets have significantly higher rates of participation than the non-veterans of their age/generation. Indeed, the real surprise is that the non-veterans of that time are so drastically below everyone else. They are the real outliers. Moreover, combat had the opposite effect that Putnam hypothesizes it did in terms of World War II – it probably accentuates war’s communitarian effects rather than counterbalancing them. Moreover, our finding on Vietnam directly contradict part of Putnam’s argument on Vietnam veterans where he notes that “Long-term research on veterans of these wars suggests that while Vietnam vets have been relatively isolated socially, even decades after the war, vets of the Second World War were more socially integrated” (Putnam, 2002, 272).5

Before moving to our argument and the evidence supporting it, it is worth noting that at least two scholars have speculated that combat might play a role in increasing political participation. Ellison finds that combat increases high-initiative participation among black males: “Black adult males with previous combat experience are approximately 2.37 times (exp[0.864]) more likely than nonveterans to have participated in political campaigns, and roughly 2.05 times more likely than nonveterans to have called or written public officials about a concern or problem” (Ellison, 1992, 369-370). Leal also speculates that the difference in participation between draftees and volunteers could be related to combat (Leal, 1999).

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5 It should be noted that Putnam’s statement here seems to contradict what he said in the earlier quotation cited from page 485 of the same book.
However, the intuitive argument is the one Putnam deploys. Namely, combat would seem to cause individuals to become alienated from their communities. However, one could argue that combat increases one’s willingness to trust since they learn under fire to trust buddies and depend on them. This is one way combat could help build social capital. However, and more relevant to the work here, social capital could be created by increased civic participation since military life, especially combat, may induce a feeling of collective responsibility and larger sense of social solidarity (see Wilson, 2007), what one might call “civic altruism,” that increases associational participation. In contrast, society at large has “hunkered down” in a diverse world where many elements have reduced the scope of one’s feelings as far as social solidarity is concerned (Putnam 2007; Wilson, 2007). Moreover, combat itself may work to radically increase feelings of social responsibility a la Durkheim. Fire, here, forges a particular bond between the combat veteran and the community for which he puts himself into danger.

2.3. Civic Participation as a Time-Allocation Problem

A distinguishing feature of civic participation (as opposed to other aspects of social capital) is that it is time-intensive. Therefore, it is useful to have a conceptual model that is built around the time allocation problem individuals face. We posit a simple utility maximization model that draws on the framework pioneered by Becker (1965). Recent work on social capital investment over the life-cycle (Glaeser et al. 2002) also shares some common ground with our approach. We take as our starting point the generalization of the static labor-leisure tradeoff that is standard in countless studies of the labor supply.

Consider an individual’s allocation of time. Time can be spent either working for pay, $t_w$, for which the individual earns a wage of $w$, or not working for pay. Non-work time is time is divided into two categories: time spent on participation in civic activities, $t_c$, and time spent on private activities, $t_p$, including leisure, housework, child-rearing, and other private activities. The individual’s utility function, specified generally, is:

$$U(x, t_c, t_p, \theta).$$ (3)

In words, the individual’s utility is a function of consumption, civic time, private time, and $\theta$, which is a vector of both exogenous variables that affect utility and parameters that affect the
shape of the utility function. It is through \( \theta \) that variables such as veteran status or education enter the analysis. We assume that marginal utilities associated with consumption and private time are always positive. However, the marginal utility of civic participation may be positive or negative and need not be monotonic.

If \( T \) is the individual’s total time endowment, we can specify the total time constraint as follows:

\[
t_w + t_c + t_p \leq T
\]

Additionally, if we index each of the three time components by \( j \), each component of time must satisfy the following constraint: \( 0 \leq t_j \leq T \). Following convention, if \( t_j = 0 \) we refer to the individual as being at a “corner solution.”

Finally, the individual also faces a standard budget constraint, where consumption \( (x) \) must be less than the sum of assets \( (A) \) plus earned income. Wages are determined by \( \theta \) (the most important part of which is education, \( E \)) and time spent on civic participation. Combining these elements together, we get the simple budget constraint:

\[
x \leq t_w w(\theta, t_c) + A(\theta)
\]
\[
\frac{\partial U}{\partial x} w = \frac{\partial U}{\partial t_p} + \frac{\partial U}{\partial t_c} + \frac{\partial U}{\partial x} \frac{\partial w}{\partial t_c} t_w \tag{4}
\]

The first term is the marginal value (through the marginal utility of consumption) of an additional unit of time spent from working; the second is the marginal value of time spent on private activities; and the third is the marginal value of time spent on civic participation, which is the sum of the direct utility affect from civic time plus the indirect effect through the higher consumption possibilities associated with the wage effect of civic participation. In short, the individual in this model allocates time across activities to equate the marginal values associated with each time use. If the equalities in (4) were out of balance, the individual could increase total utility by reallocating time towards the activities with high marginal values and away from activities with low marginal values. The equalities in equation (4) can be solved to yield a set of optimal allocations, \(t_p^*, t_w^*, t_c^*, x^*\), which are functions of \(\theta\).

Our main variable of interest is military service, \(M\). Here we define it is a continuous variable and assume, for convenience, that the utility function and optimal time allocation functions are differentiable with respect to \(M\). However our empirical implementation uses only three discrete categories of service: those who served in combat, those who served in the military but not in combat, and non-veterans. We use the model above to highlight different ways that military experience may affect the level of participation.

Pathway 1: Labor Market Productivity: Military service may improve marketable skills or it may build social networks, both of which could work to improve wage rates for veterans relative to non-veterans. We call this the wage effect. The effect of higher wages can either raise or lower hours worked, depending on whether the individual is on the positive or negative part of the labor supply curve. At low wages, labor supply is increasing in wages, while at high wages it is decreasing. Empirical evidence for American men in this cohort (Pancavel, 1986) suggests that labor supply is very inelastic, with estimated elasticities near zero. This would suggest that the role of wages in determining time allocation is, on average, small.
In this model a positive wage effect is mitigated by the last term in (4), \( \frac{\partial U}{\partial x} \frac{\partial w}{\partial t_c} t_w \), which represents the pecuniary return to civic participation. Shifting time towards work raises the wage return to \( t_c \) since each dollar in higher wages is multiplied by a higher value of \( t_w \). Thus if \( \frac{\partial w}{\partial t_c} \) is positive, any shift towards higher work time is at least partially offset by this effect. In short, if the individual gets a pecuniary return to investing in civic time, he does not need to work as much to get the same earnings; he will thus invest more in civic time and less in work.

Finally, assets and non-earned income can affect labor supply (and, hence, civic participation time). Increases in assets will cause a reduction in labor supply and hence increase consumption of all normal goods. It is possible that military service will increase non-earned assets through military pensions and medical benefits. However, this effect is also mitigated by the pecuniary return to civic time discussed above. This is because if the effect of civic time on wages is high, the individual can substitute non-earned income for wage income, thereby lowering the incentive to employ civic time.

### Pathway 2—Civic Efficiency

Skills developed in the military can be applied not only to the market but also to civic participation. If a veteran is more effective at civic activities, then he is able to generate more utility for himself with a given unit of time input. In the model, there is a direct and an indirect effect. The direct effect is simply that military service directly raises the utility gained from a unit of civic time; i.e., that \( \frac{\partial^2 U}{\partial t_c \partial M} > 0 \). The most important of these skills that would directly translate into greater civic efficiency would include leadership (and followership or obedience) and teamwork, two skills that the military consistently stresses and rewards.\(^6\) Moreover, these are skills that are not taught or encouraged anywhere near the same extent in standard educational

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\(^6\) On teamwork and obedience, Anni Baker notes that one of the primarily purpose of basic training is to foster these skills: “The only way soldiers will have any hope of surviving the chaos of battle is to act together as a team, unquestioningly obeying the commands of a leader exactly as received” (Baker, 2008: 25). Leadership is primarily a skill emphasized in officer indoctrination and training. However, enlisted personnel – particularly NCO’s - are also taught to take initiative and when called on to exercise leadership skills. This is especially the case in the Navy where Chiefs are often referred to as the “backbone of the Navy” and are heavily relied upon to lead their junior enlisted and even “assist” junior officers.
settings such as universities. Although there are opportunities for students to learn and exercise leadership skills and teamwork in higher ed (such as in fraternities, sororities, and other campus groups), there are not formal avenues of leadership training and individual effort and reward is the nature of the university experience for most students.

The indirect effect is that military service works through another variable, with the most likely suspect being education. Military service can lead to greater education, which was one result of the G.I. Bill for millions of Americans after World War II, or it may increase the productivity of education in terms of civic efficiency.

Pathway 3—Preference Transformation: Even if $M$ has no effect on the productivity of civic time, it may directly affect preferences about time allocation. This could occur by increasing the marginal utility of civic time (as in pathway 3) or in reducing either the marginal utility of private time or the marginal utility of consumption (or by some combination of these three effects). We discussed earlier theoretical reasons why preferences might be changed by military service, though we cannot measure preferences directly.

_____. The crux of this argument is that military life, especially combat, induces a feeling of collective responsibility and larger sense of social solidarity (see Wilson, 2007), what one might call “civic altruism,” that increases associational participation by returned soliders. Specifically, civic altruism is increased by military service and combat since these experiences transform an individual’s preferences from stricter more narrowly self-interested ones to “other-regarding” ones (or more cynically, to preferences that flow from enlightened self-interest). This preference change probably results from a mix of explicit indoctrination as professionals with a corporate responsibility and the act of living and operating with such a responsibility. (It should be noted that this sense of corporate responsibility is strong even among the enlisted, who technically in the Huntingtonian sense are not professionals but tradesmen). As Huntington notes in his classic book, *The Soldier and the State*, “The motivations of the officer [and given that he is too strict about to whom this applies, we can apply it to all of the ranks] are a technical

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7 This phrase may have an older heritage, but the first time I saw this term was in the context of the discussion of national interests in George and Keohane, 1980.
love for his craft and the sense of social obligation to utilize this craft for the benefit of society. The combination of these drives constitutes professional motivation” (Huntington, 1957; 15; and see Sarkesian and Connor, 2006).

In other words, the military man feels a responsibility as a professional that the average businessman or tradesmen does not – a responsibility to the broader society in which he operates and not just to the bottom-line or some other more narrow self-interest. It is this sense of corporate responsibility that sticks with soldiers such that it becomes part of who they are and so they naturally become “civic altruists” – to use our word – when they leave the service. This explains why it is not just sporting groups and veteran groups that veterans join at higher rates but other “other-regarding” groups as well.

Even though we cannot measure preferences directly to explain this civic altruism in practice, we can examine the types of civic participation that veterans perform and compare that distribution of activities to activities of non-veterans. More detail on the types of civic activities undertaken may reveal something about whether the motivation for participation is enjoyment of group activities or, possibly, a greater sense of civic altruism.

The analysis above applies to interior solutions. The mathematics associated with a corner solution are conventional, but since they are more complicated and do not add appreciably to our theoretical intuitions, they are laid out in the Appendix. At a corner, the marginal return to a unit of time in a particular activity is so low that the individual would actually like to consume a negative quantity of time in one activity and re-allocate the time towards another activity; however, time allocations cannot be less than zero. In this situation, the solution is still characterized by the pathways discussed above, but changes in the time allocations for people at the corner are censored at zero until the incentives become strong enough to move behavior from \( t_j = 0 \) to \( t_j > 0 \). In the data we use below, about 30% of individuals report zero hours of civic time. Additionally, some individuals also have zero hours of labor, even though we have restricted the sample to men under age 65. Statistically, this points to the importance of using an estimation method, such as Tobit analysis, which is appropriate when a non-trivial percentage of the data points are censored at zero.
Completely differentiating the theoretical causal pathways discussed above would require data that we do not have in hand. We can make some progress, however. Our main objective is to test the hypothesis that military service increases civic participation in later life against the null hypothesis that it has no effect. We can also test whether military service or civic participation has a significant effect on wages and assets, thus identifying whether pathways 1 matters for this sample of men. We cannot directly estimate the importance of pathways 2 and 3, nor differentiate between them with the data at hand. But through examining the types of civic time that are undertaken by veterans and non-veterans, we can explore whether time allocations that are characterized by civic altruism are relatively more common among veterans than non-veterans.

The static model outlined above is not designed to investigate life-cycle behaviors such as savings or investment (including social capital investment). However, keeping in mind the causal pathways that occur over the life-cycle is important in the empirical estimates to follow. Our primary focus will be on estimating the effect of variables determined by early adulthood, including education and military service, on civic participation in mid-life. Thus we treat both education and military service as pre-determined exogenous variables and we do not include mid-life variables such as assets or marital status as explanatory variables in our main regression equations because they are potentially endogenous (for instance, assets are jointly determined with time allocation decisions). We can, however, explore the relationship between military service and education, and we estimate the effects of the intervening variables such as marital status and assets on our estimates of the effect of military service.

Taking the time to spell out the theoretical model helps us think carefully about how we specify the estimating equations in the next section and helps us make more precise statements about our theoretical conjectures. In the next section we move to empirical estimation of the model.

3. Methods and Data

3.1. Empirical Specification and Estimation Methods

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8 If we had data on the leadership and organizational skills of veterans and non-veterans, we could possibly identify pathway 4, but such data is not available.
In the model detailed above, the individual chooses how to allocate his time towards different ends. Thus the time allocations are endogenous variables. The solution to the allocation problem yields demand functions for each time variable and for consumption as functions of the exogenous variables in the system. The demand function of primary interest here can be approximated linearly as:

\[ t_c = \theta \beta + u_c \]  

(6)

where \( \beta \) is vector of regression coefficients and \( u_c \) an unobserved error term. In a naïve analysis of the demand for civic time, it might be tempting to include such variables as time spent working, \( t_w \), the wage rate or other variables contemporaneously chosen with civic time. However, these variables are endogenous and, according to the model, incorporating these endogenous variables will lead to inconsistent coefficient estimates.

Tobit is a common maximum likelihood estimator for dealing with censored dependent variables that does not need extensive discussion here. In (7) close to 30% of the values for \( t_c \) are censored at zero, meaning a sizable percentage of the sample is at a corner solution. The Tobit model estimates the effect of regression coefficients on \( t_c^* \). considers an underlying latent variable \( t_c^* \) that is continuous and defined on the domain \([-\infty, +\infty]\). The corresponding value of \( t_c \) is define as in (6) if \( t_c^* > 0 \) and as \( t_c = 0 \) if \( t_c^* \leq 0 \). Tobit regression coefficients refer to \( \frac{\partial t_c^*}{\partial \theta} \).

However, it is a simple manner to convert the coefficients to reflect \( \frac{\partial t_c}{\partial \theta} \), instead, which are the values we report.

After investigating the overall effect of military service, we examine evidence related to the causal pathways discussed above. To be specific, we estimate the following equations:

\[ w = \theta \gamma + u_w \]  

(7)

\[ t_w = \theta \alpha + u_h \]  

(8)

\[ A = \theta \delta + u_A \]  

(9)

where \( \gamma, \alpha, \) and \( \delta \) are regression coefficients and \( u_w, u_h, u_A \) are unobserved error terms. Equations and (7) and (8) determine the wage effect and corresponding labor supply response associated with military service. We will estimate (7) using the Heckit procedure which accounts for labor market selection. Although \( t_c \) is endogenous, we also estimate a version of (7) with \( t_c \) as an
explanatory variable to test whether there is a pecuniary return to civic participation. This requires a set of regressors that determine whether an individual works or not but does not affect his wage. We estimate (8) by Tobit, using the fitted wage from (7) as an explanatory variable, which allows us to estimate the overall labor supply response for both workers and non-workers (who are censored at $t_w = 0$). Additionally, we estimate the effect of military service on non-earned income, $A$, in equation (9).

The final estimation we conduct is the impact of military service on participation in individual activities in the civic participation index (discussed below). Each component has a four-value response variable, and we use ordered logit estimates to calculate the probability of participation in each activity to compare the allocation of time across different kinds of activities.

3.2. Data and Analytical Variables

The NSFH is a national random sample of the non-institutionalized adult (19 years of age or older or married) population in the contiguous United States. We use wave I of the survey, which was conducted from March 1987 through May 1988. During this period, one adult in each household was randomly selected as the primary respondent for a total of 13,008 subjects. Our sample includes 3,057 male respondents, 417 of whom were combat veterans, 832 were non-combat veterans, and the remaining 1808 had no military experience. Because we think the time allocation decision may be very different for men past traditional retirement ages and because, we restrict our sample to men ages 30-64. In 1987-88, the oldest men in this sample were young adults in the WW2, so we have good representation of men from WW2, the Korean war and the Vietnam war.

The dependent variable in our analysis is an index of civic participation (ICP), which is constructed from a set of 15 questions related to the time allocated by respondents to activities of organized groups and associations. Survey respondents were asked the following: “Here is a list of various kinds of organizations. How often, if at all, do you participate in each type of organization?” The response options for each question were as follows:

0) Never
1) Several times a year
2) About once a month
3) About once a week
4) Several times a week
Note that the respondents were prompted to give their participation in a “group or organization.” This is important because it captures formal rather than informal participation. Participation in these activities outside of an organized group is not captured in this variable.

Using the point values 0-4, the ICP is a simple sum of responses to each of the 15 questions. The mean value of the index is 3.98, with standard deviation of 4.xx. A significant proportion (27% of the analytical sample reported no participation in any of the questioned activities. Given the question wording, it is not clear how respondents answered the question if they participated less than “several” times a year but more than “never.” Frequencies for specific groups are in Table 1:

Table 1: Frequency of Organizational Participation

<table>
<thead>
<tr>
<th>Type</th>
<th>0</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>a. Fraternal</td>
<td>88.5%</td>
<td>6.2%</td>
<td>3.5%</td>
<td>1.3%</td>
<td>0.4%</td>
</tr>
<tr>
<td>b. Service</td>
<td>87.2%</td>
<td>6.9%</td>
<td>3.2%</td>
<td>2.2%</td>
<td>0.6%</td>
</tr>
<tr>
<td>c. Veterans</td>
<td>92.8%</td>
<td>5.2%</td>
<td>1.4%</td>
<td>0.4%</td>
<td>0.3%</td>
</tr>
<tr>
<td>d. Political</td>
<td>91.6%</td>
<td>6.8%</td>
<td>1.1%</td>
<td>0.1%</td>
<td>0.4%</td>
</tr>
<tr>
<td>e. Labor Union</td>
<td>87.9%</td>
<td>7.0%</td>
<td>4.2%</td>
<td>0.5%</td>
<td>0.4%</td>
</tr>
<tr>
<td>f. Sports</td>
<td>61.4%</td>
<td>19.4%</td>
<td>5.8%</td>
<td>8.1%</td>
<td>5.3%</td>
</tr>
<tr>
<td>g. Youth</td>
<td>79.9%</td>
<td>11.5%</td>
<td>3.6%</td>
<td>3.2%</td>
<td>1.8%</td>
</tr>
<tr>
<td>h. School</td>
<td>73.8%</td>
<td>18.7%</td>
<td>4.5%</td>
<td>1.7%</td>
<td>1.4%</td>
</tr>
<tr>
<td>i. Hobby/garden</td>
<td>83.6%</td>
<td>8.2%</td>
<td>4.0%</td>
<td>2.6%</td>
<td>1.7%</td>
</tr>
<tr>
<td>j. Sororities and Fraternities</td>
<td>95.5%</td>
<td>3.7%</td>
<td>0.6%</td>
<td>0.2%</td>
<td>0.1%</td>
</tr>
<tr>
<td>k. Nationality</td>
<td>95.0%</td>
<td>3.6%</td>
<td>1.0%</td>
<td>0.1%</td>
<td>0.2%</td>
</tr>
<tr>
<td>l. Farm</td>
<td>95.9%</td>
<td>3.1%</td>
<td>0.6%</td>
<td>0.1%</td>
<td>0.2%</td>
</tr>
<tr>
<td>m. Literary/Art</td>
<td>87.6%</td>
<td>8.0%</td>
<td>2.1%</td>
<td>1.8%</td>
<td>0.5%</td>
</tr>
<tr>
<td>n. Professional</td>
<td>78.2%</td>
<td>16.0%</td>
<td>4.4%</td>
<td>1.1%</td>
<td>0.4%</td>
</tr>
<tr>
<td>o. Church</td>
<td>64.2%</td>
<td>18.0%</td>
<td>6.6%</td>
<td>6.9%</td>
<td>4.4%</td>
</tr>
</tbody>
</table>

Question: "How often, if at all, do you participate in each type of organization?

Our primary focus is on the impact of military service on civic participation. Those survey respondents who were veterans were asked if they were ever in combat during their service, though we do not know the time, length or intensity of combat. From this information we create our three military service categories: 1) non-veterans; 2) non-combat veterans; 3) combat veterans. We imagine that self-reported combat includes a wide range of intensity and
length of service ranging from merely traveling briefly through a combat arena to lengthy, intensive involvement in battle. Combing these different levels of experience into one combat variable likely attenuates the estimated effect of combat significantly.

We include several important socioeconomic and demographic control variables in our estimation models of civic participation. We divide these variables into the three categories. First, demographic control variables include age, age-squared, race (black, white, and other) status, and educational attainment. Second, we have a set of family background variables. This is fortunate since in many studies little is known about the individual’s childhood. Childhood variables may have a strong influence on human capital attainment and preferences related to allocation of time. We have the following background measures: whether the respondent was ever on public assistance as a child; the socioeconomic status of the father’s occupation (a continuous measure calculated by NSFH researchers); whether the respondent’s mother worked outside of the home; whether the family had a religious affiliation; and the educational attainment of the respondent’s father and mother. Third, we use the following variables used in estimating labor market behaviors: usual hours worked per week; hourly wage; a measure of non-earned income that includes spousal income and all other income from assets; marital status (married=1, unmarried=0); and health status, which is a five point scale ranging from poor to very good, with increasing health receiving a higher value. For convenience we treat this ordinal measure as a continuous health measures. Table 2 gives descriptive statistics for each variable.

Table 2: Descriptive Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Index of Civic Participation</td>
<td>3.98</td>
<td>4.45</td>
</tr>
<tr>
<td>Main Demographic variables</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Combat-veteran</td>
<td>0.15</td>
<td>0.36</td>
</tr>
<tr>
<td>Non-combat veteran</td>
<td>0.29</td>
<td>0.46</td>
</tr>
<tr>
<td>Age</td>
<td>44.03</td>
<td>10.08</td>
</tr>
<tr>
<td>Race (black)</td>
<td>0.10</td>
<td>0.30</td>
</tr>
<tr>
<td>Race (other)</td>
<td>0.09</td>
<td>0.29</td>
</tr>
<tr>
<td>Education</td>
<td>13.22</td>
<td>3.36</td>
</tr>
<tr>
<td>Childhood background variables</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No religious affiliation</td>
<td>0.05</td>
<td>0.22</td>
</tr>
<tr>
<td>Father's SES</td>
<td>0.30</td>
<td>0.18</td>
</tr>
<tr>
<td>On Public Assistance</td>
<td>0.08</td>
<td>0.27</td>
</tr>
<tr>
<td>Mother worked outside home</td>
<td>0.25</td>
<td>0.43</td>
</tr>
</tbody>
</table>
These control variables are good representation of the hypothesized determinants of civic participation. The empirical literature shows that age is strongly associated with civic participation, with participation rising with age until it reaches its maximum in the late thirties to early forties and then steadily declines (see Putnam, 1995; Glaeser et. al., 2002). Race has also been shown as a predictor of civic participation. Putnam asserts that at least until the 1980s, blacks belonged to more associations on average than whites, partially due to their membership in religious and ethnic organizations (Putnam, 1995). People who consider themselves active members of a religion also generally demonstrate higher levels of civic participation (obviously due in large part to their participation in church-affiliated groups). The current research does not provide evidence showing any particular religion promoting more civic participation than others, thus we limit our religion measure to a dummy variable of whether the respondent professed affiliation with any religion. Studies of civic participation also show that married people belong to about 15-25% more groups than comparable single men and women (Putnam, 1995). Finally, though some researchers have disagreed over whether the income effect or substitution effect is greater when determining the impact of education and income on civic participation, most studies show that both income and education are associated with higher levels of civic participation (Putnam, 1995: 667; Brehm and Rahn, 1997; Hall, 1999; Leal, 1999; Putnam, 2000; Brewer, 2003; and Glaeser, et al, 2002).

3.3. Empirical Limitations.

These models of civic participation have some inherent limitations. First, although we have constructed the models in order to capture the effect of early life variables such as education and military service on later-life time allocations, we can say relatively little about life-cycle dynamics or about trends over time. As noted above, we cannot distinguish between age, period and cohort effects. Furthermore, because our study is retrospective rather than
prospective, we do not have a representative sample of young adults that we would in a prospective study. Our results are subject to survivor bias if mortality is correlated with any of our explanatory variables. Obviously this occurs for men who were killed while in service. Given that education and race are known to affect mortality, we are also likely to over-represent the higher educated in our sample (compared to a prospective study). However, given that overall mortality between ages 20-64 is quite low in the mid-twentieth century, we expect that survivor bias is likely small.

Another limitation is that we are looking only at men at prime working ages. We do not examine the early seeds of civic participation (say, in college or prior to marriage), nor do we examine how military service might affect participation in late life. Furthermore, by ignoring women from the sample, we can only make inferences about the effect of military service on civic participation for men. This is somewhat justifiable, however, since one of the central elements of this paper is to see the effect of combat experience on civic participation, and women rarely engage in combat. Despite these limitations, these models provide important insights into an important aspect of the post-military life for veterans.

4. Results

4.1. Reduced-form estimates

Tobit estimates of equation (6) are given below in Table 3. The first column contains the main estimates, and the second column contain the same specification except the category “veterans groups” is deleted from the construction of the ICP. This is to check whether increased participation by veterans is primarily due to participation in veterans groups (which almost no non-veterans participate in). Though the results are weaker, especially for combat veterans, both classes of veterans have a significantly greater participation in civic activities whether or not we include veterans groups as one of the categories.

In Table 3, we present marginal effects transformed from the Tobit coefficients (which turn out to be qualitatively similar in magnitude to simple OLS estimates, which are not shown). Given mean values for the ICP, a combat veteran has about 23% higher civic participation than non-veterans and 15% higher when eliminating veterans groups from the analysis. Non-combat veterans have 11% higher participation than non-veterans, changing to 9% when veterans groups
are excluded. Although these are relatively modest effects, their magnitude is not trivial. One way to see this is that combat service has an estimate effect roughly equivalent to 3 years of formal education. Given the importance that education plays in the social capital literature, an effect of this size is clearly not trivial.

As expected, education has a robust effect on civic participation. Interestingly there are also important racial differences. Blacks have significantly higher participation that whites, while other non-whites have significantly less. The most striking difference with the existing literature on social capital, however, is the effect of age. Our estimate of the age profile shows a peak of participation at age 38, followed by a gradual decline. A prominent feature of work by Putnam, on the other hand, is an increasing age profile well into old age (Putnam, 1995: 673; Glaeser, et al., 2002). We explore further the interaction between age and military service in the next sub-section.

Some of the family background variables prove to be significant predictors of civic participation as well. Those without a religious affiliation (about 5% of the sample) and those who were on public assistance (about 8%) while children show markedly lower participation. Parental education and work variables, however, do not have significant effects. Though not statistically significant, having a mother in the work force raises participation somewhat. The coefficient on the father’s socioeconomic status, though, is not as large as it might appear. A one standard deviation increases in SES would raise participation by only .1 index points.

With a cross-sectional sample, we cannot distinguish between age, period and cohort effects. This is especially true because most of those who experience combat did so when they were young adults. We can not contribute, therefore, to the discussion in the literature on trends in social capital accumulation. More important, we do not know whether the effect of military service is an age effect or a generational effect.

One important thing to know is whether the military effects persist across the sample or whether they are concentrated among particular cohorts. We replicated the analysis above using age/service interactions. For each 5-year age cohort, we create there dummy variables included: non-veterans, non-combat veterans, and combat veterans. These 21 dummy variables (minus the
reference category of non-veterans aged 30-34) were then used in place of the age and military service variables in the previous specificiation. Tobit regression estimates are given in the appendix. We summarize the age/service effects in Figure 1. 

Across the age profile, combat veterans have consistently higher estimates of civic participation than non-veterans and, generally, higher than non-combat veterans. Although this pattern is consistent, differences between service categories at a given age are not generally statistically significant due to small cell sizes associated with the 21 different age/service categories. Interestingly the highest estimated level of participation is the combat veterans aged 35-39, men who were 19 and younger when Vietnam really started heating up in 1967-68. The smallest effects were for veterans of the Korean war era. We do find an effect of service for the WWII vets, but not as large comparably as the Vietnam effect. In general, we find no “greatest generation” effect for civic participation, especially among those who did not serve. But, again, we warn the reader that that Figure 1 contains no guidance on how to decompose age effects from generation effects.

---

Figure 1: Civic Participation by age and service type

- Non-Veterans
- Non-Combat Veterans
- Combat Veterans
4.2. Exploring Causal Pathways

4.2.a. Labor Productivity Effects

Table 4 below contains estimates for the labor supply responses. The first column of estimates is a Heckit estimate of the wage offer equation. Non-earned income, marital status and health are additional variables used to estimate the first stage equation, which is found in column (2). The first part of the labor market pathway is the wage effect. Here we see that non-combat veterans achieve a modest and marginally significant wage premium equal to about a dollar (which is roughly the same estimated effect as a year of schooling), but that combat service has no wage premium; indeed, the coefficient estimate is slightly negative. Though it is theoretically endogenous, we include ICP as an explanatory variable, which has a trivial effect on the dependent variable. Thus we find no evidence here for a pecuniary return to civic time in terms of wages. Military service also has no effect on asset income (regressions not shown), and neither assets nor wages have an impact on labor supply, as seen in column (3).

--Table 4 Here--

The implications of the estimates in Table 4 is that the time allocation effect of military service does not work through the labor market pathway of increased wages and higher labor supply. Estimates in column (2) show a strong negative effect of military service on the probability of employment and column (3) shows a similar effect on total hours worked for workers and non-workers. In fact combat veterans are employed at a rate 13 percentage points less than non-veterans. That is a very large effect. These estimates indicate a relatively strong impact of combat service on labor supply, but it is not through the pathway of higher wages and a resulting reduction in hours worked, nor is it through higher asset income.

4.2.b. Civic Efficiency

The estimates show an important difference between combat and non-combat effects, though neither works through a labor supply response. Non-combat experience seems to raise productivity, much as education does. Another way to explore the theoretical causal pathways is to add regressors to the model starting with early life variables and proceeding through to the full
structural model, which includes health, marital status and non-earned income. Table 5 summarizes this exercise. Each row represents the marginal effect of combat and non-combat service adding an additional variable (or group of variables).

In this exercise, the difference between combat and non-combat is clear. Combat effects are robust and relatively unchanged, regardless of what variables are added to the specification. Non-combat effects, on the other hand, seem to be partially mediated by education. In a regression [not shown] of educational attainment on military service and the other control variables, combat service by .39 years (p=.027) and non-combat service raises it by .67 years (p<.0001). However, when we re-estimate the model in Table 3 with education interactions [not shown], these interactions are relatively small and statistically significant.

### Table 5: Mediating effects of control variables on military service effects

<table>
<thead>
<tr>
<th>Control Variables</th>
<th>Combat</th>
<th>Non-Combat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>1.09 ***</td>
<td>0.74 ***</td>
</tr>
<tr>
<td></td>
<td>(.29)</td>
<td>(.22)</td>
</tr>
<tr>
<td>+ Early Life</td>
<td>1.02 ***</td>
<td>0.67 ***</td>
</tr>
<tr>
<td></td>
<td>(.29)</td>
<td>(.22)</td>
</tr>
<tr>
<td>+ Education</td>
<td>0.92 ***</td>
<td>0.45 **</td>
</tr>
<tr>
<td></td>
<td>(.29)</td>
<td>(.21)</td>
</tr>
<tr>
<td>+ Marital Status</td>
<td>0.91 ***</td>
<td>0.44 **</td>
</tr>
<tr>
<td></td>
<td>(.28)</td>
<td>(.21)</td>
</tr>
<tr>
<td>+ Non-Earned Income</td>
<td>0.91 ***</td>
<td>0.44 **</td>
</tr>
<tr>
<td></td>
<td>(.28)</td>
<td>(.21)</td>
</tr>
<tr>
<td>+ Health</td>
<td>1.05 ***</td>
<td>0.45 **</td>
</tr>
<tr>
<td></td>
<td>(.29)</td>
<td>(.21)</td>
</tr>
</tbody>
</table>

Notes: Full regression results available in Appendix. Each row represents additional control variables added to the specification. Robust standard errors in parentheses. Reference groups are: Non-veterans, Age: 30-34, Race: White. Sample restricted to men aged 30-64. Significance levels: $\alpha=.01$:***; $\alpha=.05$:**; $\alpha=.1$:*

What are the theoretical implication of these results? Pathway 2 emphasizes that military service may increase an individual’s civic leadership and organizational skills either directly or indirectly. The evidence above suggests little evidence of a significant indirect effect, especially for combat veterans. For non-combat vets, there may be some evidence for a small role for this
pathway: service increases wages and it increases education, both of which will likely raise civic productivity. Unfortunately, we do not have any measures of the direct civic efficiency pathway.

Thus the only way that combat service could operate through Pathway 2 is if the types of civic skills obtained through the military are completely different from the civic skills obtained in schooling. This is plausible, as was noted earlier, but we have no direct evidence for this pathway. Moreover, as Leal explicitly argues, the differential effect of service on political participation for draftees and volunteers infirms the civic skills/civic efficiency argument (Leal, 1999: 170). Given no evidence for Pathway 1 and weak evidence for Pathway 2, we conclude that the most plausible explanation for the strong results is Pathway 3, preference change. We explore this pathway next.

4.2.c. Preference Transformation

Do the types of activities engaged in by veterans and non-veterans reflect evidence for preference transformation? There are different types of transformation that might occur. We emphasize two. First is an increased preference towards civic altruism. This would be reflected in differences in the types of activities performed by veterans as opposed to non-veterans. The second would be a general preference to engage in social rather than private activities. If this occurs we would expect to see a relatively uniform increase in all types of activities for veterans as opposed to non-veterans.

Given that we have data on individual activities, we can estimate the effect of military service on different types of activities. Since most of these activities are undertaken by less than 10% of men in the sample, we do not expect strong regression results due to the statistical difficulty in explaining rare events. Table 6 performs an activity-by-activity analysis of each individual activity (excluding veteran’s groups where the effects of military service are both huge and uninteresting, other than that combat veterans participate at much higher rate than non-combat veterans). We estimate each equation by ordered logit analysis and ordered logit coefficients are shown along with significance levels.

The second set of estimates in Table 6, labeled “Increase in Time Allocation” is an estimate of the actual percentage increase in total time over a year for each activity. Our approach here is admittedly crude, but it is useful to give a sense of the magnitude of the regression coefficients on actual time allocations. For each activity category we assign the following time units:
Never: 0
Several times a year: 6
About once a month: 12
About once a week: 50
Several times a year: 150

Holding other variables constant at their mean values, we can predict the probability in being in each of the five categories using the ordered logit estimates. We then calculate a sum of total time allocations using these probabilities as weights. Using this procedure, we estimate a 13% increase in total annual time for non-combat veterans and a 22% increase for combat veterans. Note that these numbers are very close to the percentage increases in the ICP estimated in Table 3, even though the method here does not use Table 3 estimates in any way. We also estimate the time allocations for each specific activity and how much of an increase is associated with being a veteran.

Though many results show no statistically significant effect, it is reassuring that our reported difference between combat and non-combat veterans also holds true for specific activities. Combat veterans are almost uniformly more likely to participate (and at higher levels) than non-combat veterans. Exceptions are those cases where both combat and non-combat veterans have only small effects on participation. Interestingly, there is a relatively strong (corr=-.4) negative correlation between the education coefficient and the combat coefficient in these regressions. Veterans (particularly combat veterans) are less likely to participate in groups that have the highest level of education—book clubs, garden clubs, political groups, for example.

Table 6: Effects of Service on Specific Activities

<table>
<thead>
<tr>
<th>Ordered Logit Coefficient</th>
<th>Increase in Time Allocation</th>
<th>% of Total Increase</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Non-Combat Veteran</td>
<td>Combat Veteran</td>
</tr>
<tr>
<td>j. Sororities and Fraternities</td>
<td>0.692 ***</td>
<td>1.126 ***</td>
</tr>
<tr>
<td>b. Service</td>
<td>0.312 *</td>
<td>0.595 **</td>
</tr>
<tr>
<td>e. Labor Union</td>
<td>0.396 ***</td>
<td>0.511 **</td>
</tr>
<tr>
<td>f. Sports</td>
<td>0.183 *</td>
<td>0.384 ***</td>
</tr>
<tr>
<td>h. School</td>
<td>0.173</td>
<td>0.337 **</td>
</tr>
<tr>
<td>a. Fraternal</td>
<td>0.187</td>
<td>0.326 *</td>
</tr>
<tr>
<td>g. Youth</td>
<td>0.204</td>
<td>0.313 *</td>
</tr>
<tr>
<td>n. Professional</td>
<td>0.104</td>
<td>0.237</td>
</tr>
<tr>
<td>k. Nationality</td>
<td>0.333</td>
<td>0.103</td>
</tr>
</tbody>
</table>
Is there a pattern in Table 6 that might show a more nuanced preference transformation than reflected in the total increase? It is hard to tell. Service groups and fraternal organizations (which are often service-oriented) are high on Table 6 and can be considered examples of civic altruism, as would schools and youth groups. On the other hand, church groups and political groups might also be considered altruistic activities and they are low on the list. Apparently the difference between veterans and non-veterans in civic participation is primarily of a non-religious and non-political nature. Other groups such as unions, professional groups, nationality groups and sororities and fraternities are much harder to pin down. There may be service aspects to these groups, but certainly there is a strong possibility that their activities are not primarily service-oriented. Additionally, even though a given group might be involved in service, this does not mean that the motive for participation is altruistic, as was discussed earlier.

One possibly troubling result in Table 6 is that over 44% of the total increase in civic time was for combat veterans is due to increased participation in sports groups. This includes, of course, the bowling leagues Putnam remembers so fondly remembered from his childhood. Tocqueville might not be as impressed. However, 43% of the total change can be attributed to service groups, school groups and youth groups, which seem to reflect a strong shift towards civic altruism. Thus the evidence suggests a broad-based increase towards higher engagement in a variety of organizations with a strong part of the shift consistent with a higher level of civic altruism.

5. Discussion

The central result of our analysis is that military service is associated with a significantly higher rate of civic participation in later life among working-age men. This effect is particularly strong for combat veterans. These effects are robust across age groups in the cross-section (with the strongest effects for Vietnam-era service) and for a variety of different types of organizations. We have devoted considerable attention to understanding the causal pathways
through which this effect might work. Because attendance at groups involves time, which has value in other uses, our theoretical frame is a model of the time allocation decision. Our simple model points to three causal pathways that were examined in Section 4.

First, we find relatively strong evidence that increased participation is not due to a labor supply response that would occur if a positive wage effect results in the individual to shifting hours from work time to civic time. Our evidence confirms other studies that the labor supply elasticity of men at this time is near zero. Furthermore, wage effects are relatively small and exist only for non-combat veterans. Also, other factors such as the pecuniary return to civic time and the effect of non-earned assets are also not present in our estimates. In short, the different components of a potential labor supply result simply are not there. The significant shift of hours away from work time must be due to other factors.

Pathway 2 entails the possibility that veterans are civically engaged because they have skills which increase their “civic efficiency.” This is the idea that because of their leadership and organizational skills, they are able to get a higher return for each unit of time invested. This higher return would unambiguously cause a shift towards more civic time. This might result from an indirect effect of higher education. There is a strong positive association between both combat and non-combat status and education, with the effect of non-combat status about twice as high. However, education does not explain the military service effects. The effect of combat is not changed at all by the inclusion of education and the effect of non-combat service is only slightly mediated by education. Thus, even though the GI Bill significantly raised the education level of veterans and, thereby, civic participation, it is not the case that military service had positive effects because of policies such as the G.I. Bill as Mettler prominently claims and which is poised to become the standard line (Krebs, 2004).

This leaves us with the final causal pathway, preference change. Usually, reliance on changing preferences as an explanation for empirical estimates reveals a lack of theoretical imagination. We hope that is not the case here. We resort to preference changes only after eliminating the other causes suggested by the model (though we do not claim our model exhausts all the theoretical possibilities). An examination of specific activities suggests that military service, particularly combat, worked to transform veterans’ preferences in a more civic direction. Not surprising, this direction was not towards literary or garden clubs, politics or religion. The
largest single increase is due to sports organizations, but a relatively large piece of the total change in time allocation can be attributed towards what appears to be civic altruism.

6. Concluding Comments

Scholarly research on civic engagement and the broader realm of social capital is vast, extensive, broad, and (oftentimes) confusing. If Putnam is right about both the importance and decline in civic participation (and thus social capital as well), understanding the determinants of civic engagement should remain high on the research agenda of our discipline. In this essay, we have tried to add a measure of clarity to one aspect of the debate: time spent on participation in organizations. Because the key dimension of participation is time, we develop a theoretical model of time allocation to explore the theory of civic participation. Although our empirical attention is devoted to military service, we think our theoretical model can be applied much more broadly to this more general question of understanding the causes of civic participation.

Among its multiplicity of effects, warfare shakes up the lives of individuals (particularly young men) and leads them to engage in an intense, traumatic and sometimes inspiring set of actions that most of them would otherwise not choose. From a statistical standpoint this shake-up is highly advantageous because we can then examine the lives of these young soldiers as they age and take on conventional life activities. One might even conjecture that if the act of making war does not affect a man’s life, what would?

Strangely, leading scholars in the field have largely ignored this question, assumed it away, or come up with alternative explanations for why veterans look different than non-veterans. That our findings of strong positive effects starkly contradict the conventional wisdom on military service and civic engagement is reason to pay them attention. That this conventional wisdom stands on such a weak foundation of empirical evidence is reason for scholars to devote considerably more work to understanding how civic disruptions such as war will shape the type of society we have in the future. Given the number of young men and women currently returning from war each year, the need could hardly be more pressing.

WORKS CITED
[References incomplete]


