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The Influence of Men's Military Service on Smoking Across the Life Course

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Abstract

The military is described as a social context that contributes to the (re-)initiation or intensification of cigarette smoking. We draw on data from the 1985–2014 National Survey of Drug Use and Health (NSDUH) and the Wisconsin Longitudinal Study (WLS) to conduct complementary sub-studies of the influence of military service on men's smoking outcomes across the life course. Descriptive findings from an age–period–cohort analysis of NSDUH data document higher probabilities of current smoking and heavy smoking among veteran men across a broad range of cohorts and at all observed ages. Findings from sibling fixed-effects Poisson models estimated on the WLS data document longer durations of smoking among men who served in the military and no evidence that selection explains the observed relationship. Together, these results provide novel and potentially generalizable evidence that participation in the military in early adulthood exerts a causal influence on smoking across the life course.

Keywords

military service; veterans; smoking; life course; sibling fixed effects

Introduction

The extant literature on military service and smoking consistently describes the military as a risk environment (Nelson & Pederson, 2008). The military is described as “tobacco friendly” (Offen, Arvey, Smith, & Malone, 2011, p. 404), “pro-tobacco” (Jahnke et al., 2011, p. 1382), and “highly hospitable to smoking” (E. A. Smith & Malone, 2012, p. 1202). It is an institution that has “a long history of having a culture that supports and encourages tobacco use by its members” (Jahnke et al., 2010, p. 88), “institutional norms that promote smoking” (Conway, 1998, p. 219), and, from a policy perspective, “a culture of tobacco ‘exceptionalism’” (E. A. Smith & Malone, 2012, p. 1202). One study found that military newspapers covered tobacco-related issues less than all other health topics, typically focused

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on individual responsibility for the health risks that smoking entails, and included advertisements for tobacco products (Haddock et al., 2005). Moreover, a content analysis of 97 U.S. military tobacco policies indicated that only one-third mentioned the potentially negative impact of smoking on military readiness, while a mere 6.2% noted that smoking was non-normative or suggested that it was incompatible with military service (Hoffman et al., 2011).

This pro-tobacco social context shapes the perceptions and behaviors of active-duty personnel. Individual service members often view smoking as part of military culture and use tobacco for pleasure, comfort, trade, and to boost morale (Nelson & Pederson, 2008). During deployment, junior enlisted personnel report smoking to manage stress, boredom, anxiety, sleep deprivation, and proximity to danger, even though they recognize the health risks associated with smoking (Poston et al., 2008). Research identifies various factors that contribute to tobacco use among active-duty personnel, including smoking breaks offered to them by superiors, the attractiveness of smoking areas, the social dimension of smoking, and the relatively low cost of tobacco products, which are sold tax free through military commissaries and exchanges (Haddock et al., 2008; Hoffman et al., 2008; Nelson & Pederson, 2008; E. A. Smith, Blackman, & Malone, 2007). Available evidence suggests that levels of smoking among active-duty personnel have historically been higher than among civilians (Department of Defense, 1986), and that substantial proportions of contemporary active-duty personnel smoke (Barlas, Higgins, Pflieger, & Diecker, 2013; Bray et al., 2006; B. Smith et al., 2008).

Efforts to reduce smoking in the military have been implemented since the mid-1980s (Arvey & Malone, 2008; Hoffman et al., 2011; Jahnke et al., 2011). However, they have not been particularly successful because of the influence of tobacco companies, the perverse incentive that recreation and well-being activities are funded by tobacco sales, and the belief among some policy makers and administrators that choosing to smoke or not is a right that should not be taken away from military personnel who are serving to protect the rights of all Americans (A. M. Joseph, Muggli, Pearson, & Lando, 2005; Offen et al., 2011). During the Gulf War (1990–1991), tobacco companies distributed free cigarettes to deployed service members, targeted troops with direct advertising and logo-branded items, and promoted themselves as benefactors by helping service members maintain contact with family and sponsoring “welcome home” events (E. A. Smith & Malone, 2009). It is also notable that such practices are not recent developments. As stated in 1997 by Stephen C. Joseph (1997), then Assistant Secretary of Defense for Health Affairs,

Unfortunately, smoking has been a commonly accepted behavior among military personnel for many years. Our past military culture condoned and even encouraged this harmful behavior through subsidized tobacco sales, free cigarette packs in field rations, and the “smoke ‘em if you got ‘em philosophy” (p. 218).

In this article, we argue that the military is a risk environment with respect to smoking, and that exposure to the military early in the life course can have long-term consequences for smoking across the life course. We present findings from two distinct but complementary sub-studies, which together demonstrate consistently higher levels of smoking among male veterans relative to male non-veterans. The first sub-study uses pooled data from the

National Survey of Drug Use and Health (NSDUH) and age–period–cohort analytic methods to demonstrate that male veterans have higher levels of smoking than male non-veterans at all ages within birth cohorts that span most of the 20th century. The second sub-study uses unique data from the Wisconsin Longitudinal Study (WLS) and sibling fixed-effects analytic methods to control for early-life environments and endowments that select individuals into military service and affect smoking over the life course. We find little evidence that selection matters, which suggests that the observed association between military service and duration of smoking through midlife among men is a causal effect. Together, the findings from these two sub-studies provide novel and potentially generalizable evidence linking early-adulthood exposure to the military’s pro-tobacco culture to smoking behavior across the life course.

Background

Military service remains a salient pathway to adulthood for men, and increasingly women, even though overall rates of participation in the military have declined over the past 50 years (Bennett & McDonald, 2013; Kelty, Kleykamp, & Segal, 2010; Kelty & Segal, 2013). Life-course scholars have demonstrated that the pathways individuals take during the demographically dense period of young adulthood (Rindfuss, 1991) shape the subsequent course of their lives, and have extensively examined the influence of family, education, labor markets, and penal institutions on subsequent life-course trajectories and outcomes (Shanahan, Mortimer, & Johnson, 2016). A smaller subset of life-course scholars has examined the effects of military service on individuals across the life course (for recent reviews of this literature, see London & Wilmoth, 2016; MacLean & Elder, 2007; Settersten, 2006; Wilmoth & London, 2013, 2016), with some of the earliest and most-sustained theoretical and empirical contributions coming from Glen H. Elder Jr. and his colleagues (Elder, 1986, 1987; Wilmoth & London, 2013).

Extant life-course research has demonstrated substantial variation in the effect of military service across individual characteristics, the timing of military service in the life course, service experiences, historical periods, and cohort membership (Angrist, 1990; Angrist & Krueger, 1994; Bedard & Deschênes, 2006; Carlson & Andress, 2009; Gimbel & Booth, 1994, 1996; Teachman, 2004, 2005, 2007a, 2007b, 2011; Teachman & Call, 1996; Teachman & Tedrow, 2004, 2007, 2008; Whyman, Lemmon, & Teachman, 2011; Wilmoth, London, & Parker, 2010). Taken together, there is substantial evidence that the U.S. military is a critical social institution that is variably associated with educational, occupational, income, marital/family, health, and other life-course trajectories and outcomes (Wilmoth & London, 2013). There is also evidence that military service is associated variably with substance use (Miech, London, Wilmoth, & Koester, 2013). However, clear identification of the extent to which military service causes variation in a broad range of life-course outcomes has proven to be a vexing problem because of inadequate accounting for factors that jointly influence selection into the military and such outcomes (Wolf, Wing, & Lopoo, 2013).

Recently, life-course researchers have begun to consider the mechanisms by which military policies and culture generate risk and protective environments that influence the health behaviors of active-duty personnel in ways that might carry over into their civilian lives. For

example, military service appears to provide a protective environment that can encourage long-term health promoting behaviors, such as exercise (Mikkola et al., 2009), and curb health-harming behaviors, such as drug use (Miech et al., 2013). At the same time, military service appears to be a risk environment for some health behaviors. For example, recent research suggests that high-calorie dietary habits that are established by men during the high-activity active-duty period carry over into the lower-activity veteran period and contribute to a higher prevalence of overweight and obesity among veterans than among non-veterans across the life course (Teachman & Tedrow, 2013; Wilmoth, London, & Himes, 2015).

Consistent with the prevailing portrait of the military's pro-tobacco culture, levels of smoking among active-duty personnel have historically been high. Smoking levels declined from 51.0% in 1980 to 29.9% in 1998 (Bray et al., 2003; Conway, 1998); however, they increased thereafter and remained above 30% from 1998 until 2005 (Bray et al., 2006). Generally, levels of smoking have been higher among active-duty personnel than among civilians; in 1985, 47% of active-duty personnel smoked compared with 30% of civilians (Department of Defense, 1986). Findings from the Millennium Cohort Study indicate that smoking increased 44% among non-deployed personnel and 57% among those who deployed, with the majority of the increase in both groups due to recidivism among past smokers rather than first-time initiation (B. Smith et al., 2008). As of 2011, 24% of active-duty military personnel, compared with 19% of the civilian population, were active smokers (Barlas et al., 2013).

Nicotine addiction is hard to overcome, and therefore, patterns of smoking (re-)established, maintained, or amplified during the period of active-duty military service may carry over into the veteran period of the life course, which characterizes the vast majority of the adult lives of most people who serve in the military. There is considerable evidence documenting a strong association between veteran status and smoking (Bondurant & Wedge, 2009; Brown, 2010; Collie, Clancy, Yeatts, & Beckham, 2006). The likelihood of ever and current smoking is higher for veterans than for non-veterans (Feigelman, 1994; Klevens et al., 1995; McKinney, McIntire, Carmody, & Joseph, 1997). From 2003 to 2007, age-adjusted rates of current smoking among veterans were higher than among non-veterans (Brown, 2010). Recent veterans continue to have high levels of smoking, with 50% of Operation Iraqi Freedom (OIF)/Operation Enduring Freedom (OEF) veterans indicating that they had ever smoked (Straits-Troster, Calhoun, Kudler, & Jones, 2007). Data from the National Health Interview Study (2007–2010) show that male veterans aged 25 to 64 years were more likely to be current smokers than non-veterans (29% vs. 24%); among men aged 45 to 54 years, 36% of veterans reported being current smokers, compared with 24% of non-veterans (Centers for Disease Control and Prevention, 2012). There is some evidence that smoking during military service is associated with a lifelong increase in cigarette consumption (Bondurant & Wedge, 2009; Feigelman, 1994; Klevens et al., 1995).

Despite the consistency of the associations between current and prior military service, respectively, and smoking that are found in existing research, it remains unclear whether the observed relationships result from the selection of smokers into the military or because the military induces people to start or restart smoking (i.e., a causal effect of military service).

As noted by Conway (1998), “Given that rates of smoking among U.S. military personnel tend to be higher than smoking rates in the civilian sector, a logical question to ask is whether the military ‘attracts’ or ‘creates’ smokers” (p. 219). We know that early-life experiences and endowments, such as parental characteristics, disadvantaged socioeconomic environments, personality, and IQ, influence smoking patterns (Harville et al., 2010; Lacey, Cable, Stafford, Bartley, & Pikhart, 2011; van Loon, Tijhuis, Surtees, & Ormel, 2005), as well as the propensity to join the military (Elder, Wang, Spence, Adkins, & Brown, 2010; Kilburn & Klerman, 1999; Kleykamp, 2006; MacLean & Elder, 2007; Teachman, Call, & Segal, 1993a, 1993b; Teachman & Tedrow, 2014). Genetic factors also predict smoking (Belsky et al., 2013). However, existing studies that focus on active-duty military service or veteran status and smoking are often not comparative, and they generally do not account for the role of these potentially confounding early-life social-environmental influences or genetic endowments. Moreover, while there is considerable interest in the life-course consequences of military service among researchers, policy makers, and practitioners (Wilmoth & London, 2013), few well-controlled, comparative life-course studies have investigated the influence of the military on smoking in mid- or later life (Bedard & Deschênes 2006). In part, this lack of research is due to data constraints and the difficulty of sorting out the selection of smokers into military service from the potential causal effect of military service on smoking. Yet, determining whether the association between military service and smoking is due to selection or is a causal effect is critically important because evidence of a causal effect of military service on smoking can contribute to public health and Department of Defense policy efforts to reduce smoking in the military (Arvey & Malone, 2008; Bondurant & Wedge, 2009; Hoffman et al., 2011; Jahnke et al., 2010) and initiatives that aim to provide compensatory resources to veterans with smoking-related health problems (Offen, Smith, & Malone, 2010).

The Current Investigation

In this article, we present the results of two complementary sub-studies. Sub-Study 1 uses pooled data from the NSDUH to conduct an age–period–cohort analysis of veteran status differences in smoking among men. This broad, national study includes birth cohorts that span much of the 20th century and allows us to document intra-cohort veteran status differences in current smoking and heavy smoking, as well as inter-cohort changes among veterans and non-veterans. Although the findings from Sub-Study 1 can establish, descriptively, whether veterans smoked more than non-veterans throughout much of the 20th century and in the early 21st century, the results leave open the question of whether exposure to the “pro-tobacco” military culture caused the observed higher prevalence of smoking because we are unable to adequately control for selection into military service. Sub-Study 2 addresses this concern by using unique data from the WLS, which includes men who were most likely to serve in the military during the Cold War, between the Korean and Vietnam Wars (see Wilmoth & London, 2016, for a profile of 20th-century veteran cohorts). A distinctive feature of the WLS is that data were collected prospectively from respondents and a randomly selected sibling. Thus, we can use sibling fixed-effects models, combined with the statistical control of some observed factors that vary between siblings, such as age, IQ score, and personality, to account for the role of early-life experiences and endowments

in the relationship between military service and smoking. To the extent that the findings from the two sub-studies are consistent, they can provide novel and potentially generalizable evidence that participation in the military in early adulthood exerts a causal influence on smoking across the life course.

Data and Method

Sub-Study 1: NSDUH

Data.—Sub-Study 1 examines veteran status differences in current smoking and current heavy smoking (defined as a pack a day or more) among 25- to 64-year-old men by age, period, and cohort. Data for Sub-Study 1 come from the NSDUH, a series of annual, nationally representative, cross-sectional surveys of the U.S. civilian, non-institutionalized population. This study uses data from 1985, 1988, and every year from 1990 to 2014 inclusive (no survey was conducted in 1986, 1987, or 1989). The NSDUH was designed to provide estimates of the prevalence of illegal and legal drug use in the household population of the United States. These surveys use a multistage probability sample, with minor variation in the sampling frame from 1985 to the present. The analytic sample includes male respondents who were born between 1920 and 1984 (inclusive) and answered smoking questions, resulting in a sample size of 189,499.

The survey has undergone some methodological improvement over time, which has led to higher, and presumably more accurate, estimates of substance use. The two major improvements were as follows: (a) a shift from paper-and-pencil to computer-assisted surveys in 1999 and (b) the introduction of respondent incentives in 2002 (Kennet & Gfroerer, 2005). These improvements affected respondents of all ages in the specific years that they were implemented. Therefore, in the analyses that follow, their effects will influence the estimates of “historical period” effects and not “cohort” or “age” effects.

Interviews were conducted in the home by trained interviewers. To maximize the validity of responses and to minimize underreporting, respondents answered questions about possibly sensitive issues, such as illegal drug use, using a self-administered format. Response rates were typically 70% or higher. All information is self-reported. More detailed information about the survey is available at the Substance Abuse and Mental Health Services Administration (SAMHSA) web site: <http://www.oas.samhsa.gov/nhsda.htm>.

Measures.—The dependent variables in our analyses measure current cigarette smoking. *Current smoking* is coded 1 for respondents who reported that they smoked a cigarette within the past 30 days and 0 otherwise. *Current heavy smoking* is coded 1 for respondents who reported that they had smoked in the past 30 days and that the average number of cigarettes they had smoked was “16 to 25 cigarettes per day (about 1 pack)” or higher, and 0 otherwise. The NSDUH has provided imputed responses for the small number of cases that have missing data on these questions since 1999, with the level of imputed data at 1% or less in each year. Per the recommendation of the NSDUH, we use these imputed cases in our multivariable analyses; because the number of imputed cases is so small, they have little influence on the study results.

The independent variables measure *military service*, *year of age*, *birth cohort*, and *historical period*. Year of age is provided in the public release NSDUH data in years up to 1998, but, in later years, a variable indicating year of age is not provided. Instead, in the later survey years, the age variables that are provided in the public release of the NSDUH data indicate only whether the respondent's age falls within a broad category, such as 35–49 or 50–64. However, additional information on age is available from the alcohol section of the survey, which asks respondents to report both their age when they first drank alcohol and the calendar year when they first drank alcohol. From this information, it is possible to accurately calculate the respondent's birth year, and, consequently, the age of the respondent at the time of the survey. Overall, in 1999 and subsequently, 91% of the respondents reported ever drinking alcohol and also provided information from which to deduce their age. For people who reported never drinking alcohol, we assumed they also never smoked and randomly assigned them to an age within their broad age category in the public release. Analyses not shown demonstrate that this method of deducing age in the public release data works extremely well, with age, period, and cohort effects estimated using this method as compared with a gold standard when age was known correlating at .99 or higher (see Miech et al., 2013 for details).

For analytic purposes, we used the continuous age variable described above to construct eight 5-year age categories, beginning with 25–29 and ending with 60–64 years. Historical period consists of six 5-year periods starting in 1985–1989 and ending in 2010–2014. Cohort consists of 14 5-year periods starting at 1920. The first five 5-year cohorts (1920–1944) are combined into one group because the small numbers in each 5-year group prevented independent estimation of effects. The combination of these cohorts into one group also aids in the identification of the age–period–cohort model described below.

We include race/ethnicity in the multivariable analysis, with indicator variables for non-Hispanic Blacks and Hispanics of any race.

Method.—Current (heavy) smoking is expressed as the following function:

$$\text{Logit}(Y_{ik}) = \alpha + C_k + P_j + A + e_{ik}, \quad (1)$$

where the effect of the k -th cohort is given by C_k , the effect of the j -th period is given by P_j , and the effect of age is given by A , where α is a constant and e_{ij} is random disturbance. This model was estimated using logistic regression, and the standard errors were adjusted using the svy: commands in STATA (<http://www.stata.com/manuals13/svy.pdf>). Data from all surveys were pooled, and data from each survey were assigned unique strata numbers to adjust the standard errors for design effects (Korn & Graubard, 1999). All analyses of the NSDUH data used person-level weights.

One advantage of age–period–cohort models is their ability to control for year-specific changes in prevalence due to factors such as changes in question wording or changes in the placement of the question within the survey (i.e., a “context effect”). For example, the wording of the “current smoking” question changed in 1995 to “How long has it been since

you last smoked a cigarette?” from “When was the most recent time you had a cigarette?” in previous years. Any change in estimated prevalence attributable to the new wording would affect respondents of all ages and all cohorts, and would therefore be detected as a “period” effect in an age–period–cohort model. These “period” controls allow for better and more accurate estimation of birth cohort effects, which are the main focus of this study.

Sub-Study 2: WLS

Data.—Sub-Study 2 aims to evaluate the potential causal effect of military service on the duration of smoking across the life course by using sibling fixed-effects analytic methods. Data for Sub-Study 2 are drawn from the WLS, which is based on a random one-third sample of all 1957 Wisconsin high school graduates and a sibling of these graduates (Herd, Carr, & Road, 2014; Sewell, Hauser, Springer, & Hauser, 2004). The graduate respondents were originally empaneled with an in-person questionnaire at age 18 (in 1957), which was followed by data collection at ages 25, 36, 54, 65, and 72. As of 2005, approximately 80% of the original sample was still participating in the survey. The paired sibling was randomly selected from a roster of all siblings, except when the graduate was a twin, in which case the twin was selected. Roughly 2,000 siblings were empaneled in 1977. The full sibling sample was implemented in 1994 and has been included in each data collection effort since. Detailed documentation and response rates for all waves of the WLS are accessible at <http://www.ssc.wisc.edu/wlsresearch/>. The sibling sample used in this article includes male-only sibling pairs ($N = 832$). We also ran analyses on all male WLS participants ($N = 3,498$). We did not include females in the analyses because doing so could bias the estimates given how few women served in the military and the disproportionately higher smoking prevalence among men as compared with women.

We draw on data from the first survey when the graduate respondents were originally empaneled at age 18 (1957) and all subsequent waves of data collection in 1964, 1975, 1993, 2005, and 2011. Response rates from 1975 onward range from 75% to 90%.

In addition to unusually high response rates for a longitudinal study with about 60 years of high-quality prospective data, the WLS provides two central advantages for the purposes of this project. First, it is the only existing study of aging in the United States with a sibling sample and a rich array of prospectively measured childhood measures (e.g., IQ score—an important predictor of both military service and later-life health outcomes). Second, the WLS has a life-course occupational record that can establish military service (i.e., veteran) status with a high degree of accuracy.

There are some specificities of the WLS that warrant mention. First, we note that everyone in the *graduate* sample completed high school, while the *sibling* sample includes individuals who did not complete high school (about 7%). We do not think this is a critical issue for our analysis because evidence suggests that, for this cohort, there are no meaningful differences in smoking between those without a high school degree and those with only a high school degree—the variation in smoking patterns is among those with *at least* a high school degree (Centers for Disease Control, 2008).

Second, we note that the propensity to serve in the military was the same for both the graduates and their siblings. Moreover, approximately 90% of both the graduates and siblings who had a history of military service enlisted (i.e., were not drafted). The main difference between the graduates and siblings who served was in their propensity to see combat. While approximately 12% of the graduate sample saw combat, 34% of the sibling sample saw combat—which is a function of the fact that the siblings were more likely to serve in Korea or Vietnam.

A final consideration is the historical and regional specificity of the sample (we address generalizability in Sub-Study 1, described earlier). Partly as a result of the regional and historical specificity of the sample, there are only a handful of African American, Hispanic, or Asian persons in the WLS; the sample composition reflects the very small number of racial/ethnic minorities among Wisconsin high school graduates when the WLS began. Despite this selectivity, there is substantial heterogeneity in other aspects of the participants' social origins. For example, Hauser and Sweeney (1997) estimated that more than 20% of the graduate sample had lived in a household where the 1957–1960 average of parents' income fell below the official poverty line. While we acknowledge that the WLS is homogeneous in some ways, we also note that sample homogeneity can be valuable. It has strengthened many studies by helping to rule out unobserved variable bias as a threat to causal inference. Notable studies include Snowdon's (2002) sample of nuns and the Framingham Study (Levy & Brink, 2005). Indeed, most multivariable statistical analyses are intended to reduce sample heterogeneity.

Measures.—The dependent variable in this analysis is *smoking duration*. Smoking duration measures the number of years that respondents and siblings smoked regularly through 1993. Participants were asked, “Have you ever smoked cigarettes regularly?” If they answered “yes,” they were then asked what year they began smoking regularly and, if they were no longer smoking, what year they stopped. What constituted regular smoking was not specifically defined for participants. Following these questions, they were asked, “For how many years did you smoke regularly?” Our measure of smoking duration is based on responses to this last question; the prior items served to prompt the participant's memory in the context of the survey. This measure includes non-smokers (i.e., those with zero duration), which is why we use Poisson models to obtain estimates. Although there are more recent waves of the WLS, we employed the 1993 outcome data to reduce the potential effects of mortality selection on the study findings; the relationship between military service and smoking may be suppressed by mortality selection at older ages. In 1993, the graduate respondents were approximately 54 years old, and 95% of their paired siblings were 47 to 61 years old (i.e., within 7 years of the graduate respondents).

Military service is the independent variable. It is a dichotomous measure of ever versus never served on active duty in the armed forces of the United States. Data on military service were drawn from surveys conducted in 1964, 1975, and 1993.

We include several control variables in the analysis. Unlike many other studies, we can use prospectively measured variables to account for a range of early-life conditions and endowments. In conjunction with sibling fixed-effects modeling, this allows us to determine

the extent to which observed and unobserved factors influence selection. Individual-level controls include year of birth and IQ score. IQ is measured by the Henmon–Nelson IQ test, which was primarily administered during the respondent and sibling’s freshman year of high school. We also measure the “Big 5” standard measures of personality, which include openness, conscientiousness, extraversion, agreeableness, and neuroticism (McCrae & Costa, 1994; McCrae & Costa, 2003). These assessments were made in 1993. In models that do not include the sibling fixed effects, we also include a set of early-life family background variables that vary between families. These include the following: mother’s and father’s educational attainment, measured in years; average parental income from 1957 to 1960; parent’s socioeconomic index score measured in 1974; and whether the individual lived with both parents in 1957. These family background variables are not included in the models that include the sibling fixed effects because they are shared by the siblings and therefore are accounted for by the sibling fixed effect; however, the individual-level controls are included because these may vary across siblings.

Method.—To the best of our knowledge, this is the first study to use sibling fixed-effects models to test the relationship between military service and a long-term smoking outcome. To examine the potential role of early-life confounders, we estimate Poisson regression models with and without sibling fixed effects, using the sample of all males with observed controls for models without fixed effects and the sample of sibling pairs with discordant military service histories for the models that include the fixed effects. Because some characteristics vary across siblings within families and may predict both military service and smoking duration across the life course, the sibling fixed-effects models also include controls for birth year, IQ, and personality. The analyses focus on establishing whether there is a relationship between military service and life-course smoking duration once observed and unobserved early-life factors are taken into account. Standard errors are adjusted to account for intra-cluster correlations generated as a result of paired siblings in the sample (White, 1980).

Siblings likely experienced similar environments, both in the home and in neighborhoods and communities, and they also have partially shared genetic endowments. A sibling fixed-effects analysis is powerful because it accounts for a range of unobserved environmental factors, especially early-childhood conditions (Hauser, 1984; Hauser & Mossel, 1985; Hauser & Sewell, 1986; Hauser, Sheridan, & Warren, 1999; Warren, Sheridan, & Hauser, 2002), which may confound the relationship between military service and smoking. For example, it accounts for parental and family influences that may predict both military service and smoking (e.g., a parent’s military experience, a propensity to take risky behaviors, lower socioeconomic status, and local community influences). Sibling fixed-effects models also partially account for unobserved genetic factors, which may predict both smoking behavior and military service. Specifically, sibling models help attenuate—though not eliminate—the effects of some genetic confounders (Conley & Rauscher, 2013; Cook & Fletcher, 2014; Fletcher & Lehrer, 2011).

Using sibling fixed-effects models, in addition to models that use observable measures of early-life social-environmental characteristics, allows us the opportunity to gauge how much influence these factors have on the relationship between military service and smoking. If the

inclusion of sibling fixed effects weakens or eliminates the relationship between military service and smoking, then this supports the hypothesis that the relationship between military service and smoking is confounded by early-life endowments and experiences that select individuals into military service and smoking. Results indicating a statistically significant association between military service and smoking obtained from models that use the sibling fixed-effects approach would provide some of the strongest evidence available of a causal relationship between military service and smoking. Such evidence would support a causal interpretation of any positive association between military service and smoking emerging from Sub-Study 1.

Findings

Sub-Study 1: NSDUH

Sample description and smoking prevalence.—Table 1 presents descriptive statistics for males aged 25 to 64 years from 1985 to 2014. Overall, 22.5% of men had served in the military, 30.9% reported current smoking, and 15.6% reported current heavy smoking.

Table 1 also reveals modest variation in current smoking and heavy smoking by military service status, as well as variation by age, period, cohort, and race/ethnicity. Men who have served in the military are more likely to be current smokers than men who have not served in the military (34.1% vs. 30.0%). The military service status difference in heavy smoking is even larger, with 21.4% of men who have served in the armed forces reporting heavy smoking compared with 13.9% of men who have not served in the military. The prevalence of current smoking decreases substantially, and relatively monotonically, with age, from 38.3% among 25- to 29-year-olds to 21.1% among 60- to 64-year-olds. There is more stability in current heavy smoking across age. For all age groups from 25–29 to 55–59, the prevalence of heavy smoking ranges between 14.3% and 17.1%; for the oldest age group, the prevalence drops to 12.7%. Across historical time, the overall prevalence of current smoking declines sharply from 39.4% in 1985–1989 to 32.4% in 1990–1994, pauses two decades, and then again declines in 2010–2014 to 28.1%. Heavy smoking decreases more monotonically over time from 26.5% in 1985–1989 to 10.2% in 2010–2014. Smoking prevalence also varies by birth cohort, but these levels are difficult to interpret directly because more recent cohorts are relatively young, and youth confers a substantially heightened risk of smoking. Interpretation of smoking levels across cohorts requires controls for age and historical period, which is accomplished with the formal age–period–cohort analysis that we present below. With respect to race/ethnicity, current smoking is highest among Blacks (35.1%), with prevalence levels relatively low for Hispanics (28.6%) and non-Black non-Hispanics (31.0%). Levels of current heavy smoking are lowest among Hispanics (5.6%), higher for Blacks (10.0%), and highest for non-Black non-Hispanics (18.7%).

Age–Period–Cohort Analysis.—Table 2 presents the results of the formal age–period–cohort analyses of current smoking and current heavy smoking, respectively. For both outcomes, the coefficient comparing men who served in the military with those who did not is positive and statistically significant, and there are significant interactions between veteran

status and cohort. We also tested interactions between veteran status and age and period, respectively, but these were not statistically significant. To facilitate interpretation of the results from these models, we estimated predicted probabilities by military service status, cohort, and age for non-Black non-Hispanic respondents in 1985–1989. Predicted probabilities of current smoking over the observed age range for a given cohort are presented in Table 3; those for current heavy smoking are presented in Table 4. We also include three descriptive statistics in Tables 3 and 4. The intra-cohort difference represents the difference in the predicted probability between veterans and non-veterans within a given cohort. Based on the results presented in Table 2, these differences are all statistically significant, albeit of variable magnitude. The second and third statistics represent the inter-cohort veteran and inter-cohort non-veteran differences, respectively. These are calculated separately for veterans and non-veterans in relation to the immediately preceding cohort (if there is one). These statistics describe between-cohort increases or decreases in smoking and heavy smoking, respectively, among similar veterans and similar non-veterans. Although we do not provide tests of statistical significance, divergence by veteran status in the direction and/or magnitude of changes between cohorts suggests that there may be different influences on the smoking patterns of veterans and non-veterans across cohorts.

The results presented in Table 3 reveal several important patterns. First, as indicated within each cohort-specific panel in the row labeled “Intra-Cohort Difference,” at all ages, those who served in the military are always more likely than those who did not to be current smokers, although the size of the intra-cohort difference varies across cohorts. As noted above, these differences are all statistically significant (see Table 2). The size of the intra-cohort veteran status differences increases substantially from one cohort to the next from the 1920–1944 through the 1955–1959 cohort, and then declines through the 1970–1974 cohort. The size of the intra-cohort veteran status difference then increases through 1980–1984, but contracts again in the most recent cohort. At the peak of the veteran status difference in current smoking in the 1955–1959 cohort, the predicted probability of current smoking among 25- to 29-year-old veterans is .603 compared with .426 among similar non-veterans. Among 60- to 64-year-old veterans in the same cohort, the probability of current smoking is .444 compared with .280 among similar non-veterans. Notably, in the three most recent cohorts, over the relatively young age ranges observed, which are more proximate to the period of active-duty military service, the probabilities of current smoking are always greater than .5 and approach those observed among similar veterans in the peak 1955–1959 cohort.

Second, a focus on inter-cohort differences by military service status indicates that the long-term trends for men who served in the military and those who did not are different. These results can be seen in the rows labeled “Inter-Cohort Veteran Difference” and “Inter-Cohort Non-Veteran Difference,” respectively. For each, the starting point is the 1920–1944 cohort, and the comparison is always with the cohort that immediately precedes it. For example, compared with the 1920–1944 cohort, veterans in the 1945–1949 cohort have higher probabilities of smoking, while non-veterans in the 1945–1949 cohort have lower probabilities of smoking, net of age, period, and race/ethnicity. The same divergent pattern is observed in several other cohorts as well. Overall, inter-cohort probabilities of smoking continue to increase among veterans until 1955–1959, but then become negative through 1970–1974. For the 1975–1979 and 1980–1984 cohorts, the inter-cohort veteran change is

again positive; it turns negative for the 1985–1989 cohort. For non-veterans, inter-cohort probabilities of current smoking are negative through 1955–1959, but turn positive after that (with the exception of the most recent 1985–1989 cohort).

Table 4 presents the results from the age–period–cohort analysis of heavy smoking. The results indicate that at every age, within every cohort, men who have served in the military are more likely than men who have not to be current heavy smokers (see “Intra-Cohort Difference”). Moreover, as was the case with current smoking, the inter-cohort changes among men who did and did not serve in the military differ. Among those who had served, current heavy smoking increases across cohorts through the cohort born 1955–1959. The inter-cohort change is negative for those born 1960–1964 (relative to those born 1955–1959) and remains negative through 1975–1979. After that, the inter-cohort change is positive for the 1980–1984 cohort and negative for the 1985–1989 cohort. Among men who did not serve in the military, heavy smoking decreases from one cohort to the next across the full range of observed cohorts.

These patterns suggest that different factors were influencing smoking trends among 25- to 64-year-old men who did and did not serve in the military. For current smoking, the divergence seems to be most pronounced among men born prior to 1960. For current heavy smoking, the trends are divergent across a broader range of birth cohorts. Such factors tend to increase smoking and heavy smoking across the life course among men from those cohorts who had served in the military, and decrease smoking and heavy smoking among those who did not serve in the military. These patterns are descriptive, but are consistent with the hypothesis that exposure to the pro-tobacco culture of the military increased smoking and heavy smoking. Sub-Study 2, as presented below, examines smoking over the life course using a research design that allows us to better examine whether military service is a plausible cause for the patterns we observe in the NSDUH.

Sub-Study 2: WLS

Sample description.—Table 5 describes the samples we use for the WLS analyses. Statistics are presented for both the full sample, which includes all males, and the sibling sample, which includes all-male sibling pairs for which we have data on both siblings. In addition to providing an overview of the characteristics of the samples, Table 5 demonstrates that there are no meaningful differences between the full sample and the paired sibling sample on the outcome—years of regular smoking—or the demographic, personality, and family background variables that are included in the analysis. Overall, the mean duration of regular smoking is 13.5 and 13.7 years, respectively, in the full and sibling samples. Supplemental analyses (not shown) also indicate that there are no differences between the two samples in the percentage who ever smoked or mean educational attainment. For the sibling sample, the percentage ever smoked is 62.6, and the mean educational attainment is 14.2 years ($SD = 2.5$), while for the full sample, the percentage ever smoked is 61.9, and the mean educational attainment is 14.2 years ($SD = 2.6$).

Military service and smoking.—Table 6 presents estimates for models without and with sibling fixed effects. The findings support the hypothesis that military service increases

smoking duration across the life course. In addition, these analyses provide little evidence that early-life environments or endowments play a significant role in confounding the relationship between military service and smoking.

Models 1a and 1b in Table 6 present estimates from Poisson regressions that do not include sibling fixed effects, but do include observed early-life factors. Model 1a only includes military service and birth year; Model 1b adds the full set of demographic and early-life controls, including birth year, IQ score, the personality measures, and the family background variables. The incident risk ratio (IRR) in Model 1a is 1.189 (1.107, 1.278) compared with 1.175 (1.094, 1.262) in Model 1b. Accounting for observed early-life factors has little effect on the relationship between military service and smoking duration in the models that exclude sibling fixed effects.

Models 2a and 2b in Table 6 present estimates from Poisson regression models that include the sibling fixed effect and thereby account for unobserved early-life factors that might confound the relationship between military service and smoking duration. Model 2a only includes military service and birth year as observed control variables; Model 2b adds IQ score and personality, which are factors that may vary between paired siblings. The findings are similar to those obtained from Models 1a and 1b; there is almost no difference in the military service coefficient between these models. In both models that include the sibling fixed effects, those with military service experience smoke significantly longer than their siblings who did not serve in the military. The IRR in Model 2a is 1.210 (1.147, 1.276) compared with 1.198 (1.135, 1.264) in Model 2b. Accounting for early-life conditions, either with observed covariates or by using sibling fixed effects to control for unobserved early-life environments and endowments, provides no evidence that selection confounds the relationship between military service and smoking.

Sensitivity analyses.—Supplemental sensitivity analyses (not shown) reinforce the main findings presented above. In 2005, the WLS measured the age at which individuals started smoking with a dichotomous indicator of age at initiation before age 18 (when eligibility for military service commences) and after age 18. Analysis of the smaller sample of 2005 survey participants reduces the effect sizes of the military service coefficients by about 20% as compared with the same models estimated on the full sample in 1993, which is consistent with the effect of mortality selection. However, the effects remained statistically significant and robust. Moreover, the findings were not attenuated in the sibling fixed-effects models when the age started smoking variable was included.

Discussion

We use pooled data from the 1985–2014 NSDUH to conduct an age–period–cohort analysis of the influence of military service on current smoking and current heavy smoking, and data from the WLS to estimate sibling fixed-effects models of the influence of military service on the duration of smoking. The first of these two complementary sub-studies documents intra-cohort military service status differences in current smoking and heavy smoking. Within every cohort, at all observed ages, men who have served in the military are more likely to be current smokers and to be current heavy smokers. In addition, inter-cohort change differs by

military service status. Among men who served in the military, increased probabilities of smoking and heavy smoking are often observed in progressive birth cohorts even as decreases are observed among men from the same cohorts who did not serve. The second sub-study uses data from the WLS to document that men who served in the military have longer durations of smoking than their siblings who did not serve, and that the association between military service and smoking duration is not explained by observed or unobserved early-life characteristics and endowments. Taken together, the findings from these two sub-studies provide evidence of a historically long-term association between serving in the military and smoking that persists as men age. These findings are consistent with the hypothesis that it is exposure to the pro-tobacco military culture that causes the (re-)establishment, maintenance, and/or intensification of smoking early in adulthood, which has consequences for smoking across the life course.

The question of whether the relationship between military service and smoking is a function of selection into the military has been raised in the literature (Conway, 1998; Wolf et al., 2013). To the best of our knowledge, this is the first study of its kind that has directly tested the potential for selection with a combination of rich and prospectively measured variables that predate military service, as well as the use of sibling fixed-effects models. The results presented in this article provide no evidence that early-life environmental factors—or genetic endowments—provide an explanation for the observed relationship between military service and smoking. Importantly, not only did the relationship between military service and smoking remain after accounting for these early-life environments and endowments, but the relationship between military service and smoking was also *unchanged* after accounting for them. Failing to adjust for early-life factors, either with prospectively measured, observed early-life factors or with sibling fixed effects, does not appear to lead to substantial overestimation of the relationship between military service and smoking. This lends support to the argument that it is exposure to the pro-tobacco institutional culture of the military that accounts for observed associations between military service and smoking outcomes. While we cannot discount the influence of selection across all of the cohorts examined in our analysis of the NSDUH data, the WLS findings undermine the hypothesis that selection alone would account for the observed patterns.

Even before we are born, institutions and social structures begin to shape our lives through their variable effects on our parents and communities, as well as on our genetic endowments. Initially, such influences are mediated by our families of origin; however, as we pass through childhood and adolescence, we begin to experience these institutional and structural influences firsthand through the various roles in which we engage. As we transition into adult roles, we may experience opportunities related to family connections, higher education, and professional employment, or we may begin to realize the consequences of growing up in disadvantaged circumstances as they play out in constrained educational, employment, and housing opportunities. For some, the net result of these early-life factors is a voluntary transition into the military. In earlier periods, before the advent of the All-Volunteer Force, a chance draw in a draft lottery might have determined exposure to the military.

Cumulatively, as of September 2014, it is estimated that there were almost 22 million veterans living in the United States, of whom about 75% served during wartime (National Center for Veteran Statistics and Analysis, 2014). Based on data from the 2010–2014 American Community Survey, veterans represent about 9% of the civilian population aged 18 and above (U.S. Census Bureau, 2016). Many more individuals live their lives linked to those who have served, which underscores the prevalence of military service experiences and the potential public health consequences of military-related smoking (i.e., the effects of secondhand smoke). Although the size of the active-duty force has declined over the course of the late-20th and early-21st centuries (see Figure 1.1 in Wilmoth & London, 2013), in 2011, 1,411,424 Americans were on active duty in the armed forces (Department of Defense, 2012). Because the U.S. military remains a primary employer of young and middle-aged adults, what does and does not happen when they enter the military in young adulthood matters a lot, not only for them as individuals, but also for the nation.

The novel and potentially generalizable results presented in this article, and other evidence available in the literature, suggest that for much of the 20th century, the military was a risk environment with respect to smoking. Moreover, our results suggest that exposure to this risk early in the life course shaped mid- and later-life smoking outcomes. Given that smoking is one of the largest contributors to preventable mortality and chronic disease, it is noteworthy that it is linked so strongly to public service in a governmental institution. Evidence exists that the military can institute policies that reduce substance use (Miech et al., 2013). It is important that military policy makers enhance efforts to reduce smoking among those who are currently serving in the armed forces, not only for the sake of service members themselves, but also for their families and the nation. Such efforts might include the types of initiatives that have been tried, and also undermined, in the past (Arvey & Malone, 2008; Hoffman et al., 2011; Jahnke et al., 2011; A. M. Joseph et al., 2005; Offen et al., 2011; E. A. Smith & Malone, 2009). Multipronged policy changes to reduce the influence of tobacco companies and service members' access to subsidized tobacco products in the military are warranted, as are military-focused health campaigns to promote a culture of non-smoking. Subsidizing opportunities to quit smoking may be effective. The military can and should use its considerable institutional power to promote a range of initiatives that aim to change the historically dominant pro-tobacco culture of the military to one in which smoking is considered non-normative, unhealthy, and inconsistent with military preparedness.

As the results presented in this study provide novel and potentially generalizable evidence of a causal effect of military service on smoking across the life course, they should prove useful to policy makers who advocate for changing the pro-tobacco culture of the military (Jahnke et al., 2010; Jahnke et al., 2011; E. A. Smith & Malone, 2012). They may also be useful to those who are advocating for disability pensions for tobacco-disabled veterans (Offen et al., 2010). Such efforts hinge, to some extent, on demonstrating a causal effect of military service on smoking across the life course. Although our results provide consistent, policy-relevant evidence, it is important to note that all studies have limitations. Each of the two sub-studies that we include in this article has limitations. However, to some extent, these are offset by the other study (e.g., lack of ability to assess causality in NSDUH, but wide generalizability, and vice versa in WLS). An additional limitation of both sub-studies is our

inability to measure heterogeneity in military service experiences (e.g., combat exposure, branch, rank, duration of service), which may be related to smoking outcomes. Specification of variation in the causal effects of different service experiences on smoking outcomes is something that future studies should attempt to address.

In conclusion, we find no support for the premise that the correlation between military service and smoking is confounded by the failure to account for early-life family environments or genetic endowments. Instead, results support the hypothesis that military service influences smoking patterns across the life course. Many of the institutional factors that have historically influenced the relationship between military service and smoking remain in place today (Bondurant & Wedge, 2009). This analysis, in conjunction with other recent analyses of veteran status differences in drug use and body mass index (BMI), suggests that military policies, practices, and cultures may have important implications for the long-term health behaviors of those who serve in the military—and thus likely their long-term health outcomes (Miech et al. 2013; Teachman & Tedrow, 2014; Wilmoth et al., 2015). Intensified efforts to change the pro-tobacco culture of the military and orient the military toward short- and long-term smoking prevention are war ranted.

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Table 1.

Sample Description, 1985–2014 National Survey of Drug Use and Health (NSDUH), Unweighted $N=189,499$.

Variable	Total	Current smoking ^a	Current heavy smoking ^b
	% ^c (SE)	% ^c (SE)	% ^c (SE)
Total	100	30.9 (0.19)	15.6 (0.15)
Military service			
Yes	22.5 (0.21)	34.1 (0.48)	21.4 (0.41)
No	77.5 (0.21)	30.0 (0.20)	13.9 (0.16)
Age (years)			
25–29	13.9 (0.11)	38.3 (0.35)	14.3 (0.27)
30–34	14.0 (0.11)	35.0 (0.38)	15.7 (0.30)
35–39	14.6 (0.14)	32.4 (0.44)	16.5 (0.39)
40–44	13.8 (0.14)	31.2 (0.47)	16.4 (0.39)
45–49	13.4 (0.13)	30.3 (0.46)	17.1 (0.40)
50–54	11.8 (0.14)	28.2 (0.61)	15.8 (0.53)
55–59	9.9 (0.15)	25.0 (0.68)	15.2 (0.57)
60–64	8.6 (0.14)	21.1 (0.68)	12.7 (0.61)
Period			
1985–1989	6.1 (0.33)	39.4 (1.16)	26.5 (1.04)
1990–1994	16.4 (0.30)	32.4 (0.60)	20.5 (0.55)
1995–1999	17.4 (0.29)	30.3 (0.62)	17.6 (0.45)
2000–2004	18.9 (0.16)	30.8 (0.34)	14.5 (0.24)
2005–2010	20.3 (0.17)	30.7 (0.24)	13.3 (0.19)
2010–2014	20.9 (0.18)	28.1 (0.30)	10.2 (0.18)
Birth cohort			
1920–1944	13.4 (0.22)	26.9 (0.74)	18.4 (0.66)
1945–1949	11.5 (0.16)	28.4 (0.64)	18.5 (0.55)
1950–1954	15.3 (0.17)	29.4 (0.52)	17.2 (0.41)
1955–1959	17.2 (0.16)	31.3 (0.45)	17.2 (0.37)
1960–1964	17.4 (0.14)	31.2 (0.37)	15.8 (0.29)
1965–1969	13.8 (0.11)	30.3 (0.35)	13.5 (0.26)
1970–1974	10.4 (0.09)	32.3 (0.40)	12.2 (0.27)
1975–1979	7.4 (0.07)	36.9 (0.43)	11.7 (0.28)
1980–1984	4.9 (0.06)	39.0 (0.60)	10.4 (0.29)
1985–1989	1.9 (0.04)	36.9 (0.74)	7.9 (0.46)
Race/ethnicity			
Non-Black, non-Hispanic	71.8 (0.24)	31.0 (0.24)	18.7 (0.20)
Black, non-Hispanic	10.5 (0.14)	35.1 (0.48)	10.0 (0.31)
Hispanic	12.4 (0.15)	28.6 (0.42)	5.6 (0.21)

^aCurrent smoking is defined as smoking a cigarette within the past 30 days.

^b Current heavy smoking is defined as smoking 16 to 25 cigarettes per day (about one pack) or more in the past 30 days.

^c% = weighted percentage.

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Table 2.

Age–Period–Cohort Logistic Regression Analysis of Current Smoking and Current Heavy Smoking, 1985–2014 National Survey of Drug Use and Health (NSDUH), Unweighted $N=189,499$.

Variable (reference)	Current smoking ^a		Current heavy smoking ^b	
	<i>b</i> (SE)	<i>p</i>	<i>b</i> (SE)	<i>p</i>
Veteran status (non-veteran)				
Veteran	0.718 (0.053)	***	0.674 (0.067)	***
Birth cohort (1955–1959)				
1920–1944	0.115 (0.082)		0.281 (0.104)	**
1945–1949	0.046 (0.062)		0.209 (0.077)	**
1950–1954	0.017 (0.042)		0.055 (0.054)	
1960–1964	–0.013 (0.036)		–0.097 (0.046)	*
1965–1969	–0.062 (0.046)		–0.261 (0.060)	***
1970–1974	0.042 (0.060)		–0.296 (0.079)	***
1975–1979	0.199 (0.076)	**	–0.311 (0.097)	***
1980–1984	0.276 (0.094)	**	–0.443 (0.120)	***
1985–1989	0.216 (0.107)	*	–0.669 (0.154)	
Veteran-cohort interactions (veteran*1955–1959)				
Veteran*1920–1944	–0.594 (0.090)	***	–0.452 (0.102)	***
Veteran*1945–1949	–0.491 (0.085)	***	–0.379 (0.100)	***
Veteran*1950–1954	–0.261 (0.074)	***	–0.152 (0.095)	
Veteran*1960–1964	–0.242 (0.070)	***	–0.136 (0.082)	
Veteran*1965–1969	–0.364 (0.074)	***	–0.157 (0.087)	
Veteran*1970–1974	–0.614 (0.075)	***	–0.431 (0.094)	***
Veteran*1975–1979	–0.383 (0.093)	***	–0.429 (0.126)	**
Veteran*1980–1984	–0.314 (0.111)	***	–0.169 (0.160)	
Veteran*1985–1989	–0.562 (0.173)	**	–0.327 (0.221)	
Age (35–39 years)				
25–29	0.211 (0.041)	***	0.104 (0.057)	
30–34	0.100 (0.029)	**	0.073 (0.041)	
40–44	–0.042 (0.034)		–0.086 (0.048)	
45–49	–0.111 (0.047)	*	–0.152 (0.063)	*
50–54	–0.238 (0.064)	**	–0.313 (0.085)	***
55–59	–0.432 (0.084)	***	–0.452 (0.111)	***
60–64	–0.663 (0.102)	***	–0.757 (0.136)	***
Period (1985–1989)				
1990–1994	–0.279 (0.058)	***	–0.249 (0.070)	***
1995–1999	–0.344 (0.065)	***	–0.337 (0.077)	***
2000–2004	–0.310 (0.069)	***	–0.461 (0.087)	***
2005–2010	–0.318 (0.081)	***	–0.440 (0.105)	***
2010–2014	–0.456 (0.097)	***	–0.619 (0.125)	***
Race/ethnicity (non-Black, non-Hispanic)				

Variable (reference)	Current smoking ^a		Current heavy smoking ^b	
	<i>b</i> (SE)	<i>p</i>	<i>b</i> (SE)	<i>p</i>
Black, non-Hispanic	0.169 (0.023)	***	−0.683 (0.037)	***
Hispanic	−0.123 (0.022)	***	−1.201 (0.041)	***
Intercept	−0.510 (0.059)	***	−1.046 (0.072)	***

Note. *b* = logistic regression coefficient; analysis is weighted. Standard errors (SE) are corrected for the complex sampling design.

^aCurrent smoking is defined as smoking a cigarette within the past 30 days.

^bCurrent heavy smoking is defined as smoking 16 to 25 cigarettes per day (about 1 pack) or more in the past 30 days.

*
p < .05.

**
p < .01.

p < .001.

Table 3.

Predicted Probability of Current Smoking by Veteran Status, Cohort, and Age, 1985–2014 National Survey of Drug Use and Health (NSDUH), Unweighted $N = 189,499$.

Cohort	Age (in years)							
	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64
1920–1944								
Veteran				.422	.406	.375	.331	.282
Non-veteran				.392	.376	.347	.304	.258
Intra-cohort difference ^a				+.030	+.030	+.029	+.027	+.024
Inter-cohort veteran difference ^b				—	—	—	—	—
Inter-cohort non-veteran difference ^c				—	—	—	—	—
1945–1949								
Veteran			.441	.431	.414	.383	.339	.289
Non-veteran			.386	.376	.360	.331	.290	.245
Intra-cohort difference ^a			+.055	+.055	+.054	+.052	+.049	+.044
Inter-cohort veteran difference ^b			—	+.008	+.008	+.008	+.008	+.007
Inter-cohort non-veteran difference ^c			—	−.016	−.016	−.015	−.014	−.013
1950–1954								
Veteran		.516	.491	.481	.463	.432	.385	.332
Non-veteran		.403	.379	.369	.353	.325	.284	.239
Intra-cohort difference ^a		+.113	+.112	+.111	+.110	+.107	+.101	+.093
Inter-cohort veteran difference ^b		—	+.050	+.050	+.049	+.048	+.046	+.043
Inter-cohort non-veteran difference ^c		—	−.007	−.007	−.007	−.006	−.006	−.005
1955–1959								
Veteran	.603	.576	.552	.541	.524	.493	.444	
Non-veteran	.426	.399	.375	.365	.350	.321	.280	
Intra-cohort difference ^a	+.177	+.177	+.177	+.176	+.175	+.171	+.164	
Inter-cohort veteran difference ^b	—	+.060	+.061	+.061	+.061	+.061	+.059	
Inter-cohort non-veteran difference ^c	—	−.004	−.004	−.004	−.004	−.004	−.003	
1960–1964								
Veteran	.541	.513	.488	.478	.461	.429		
Non-veteran	.423	.396	.372	.362	.347	.318		
Intra-cohort difference ^a	+.118	+.117	+.116	+.115	+.114	+.111		
Inter-cohort veteran difference ^b	−.062	−.063	−.064	−.064	−.064	−.063		
Inter-cohort non-veteran difference ^c	−.003	−.003	−.003	−.003	−.003	−.003		
1965–1969								
Veteran	.498	.471	.446	.435	.418			

Cohort	Age (in years)							
	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64
Non-veteran	.411	.384	.361	.351	.336			
Intra-cohort difference ^a	+.088	+.086	+.085	+.084	+.083			
Inter-cohort veteran difference ^b	–.043	–.043	–.043	–.042	–.042			
Inter-cohort non-veteran difference ^c	–.012	–.012	–.011	–.011	–.011			
1970–1974								
Veteran	.462	.434	.410	.400				
Non-veteran	.436	.409	.385	.375				
Intra-cohort difference ^a	+.026	+.025	+.025	+.025				
Inter-cohort veteran difference ^b	–.036	–.036	–.035	–.035				
Inter-cohort non-veteran difference ^c	+.025	+.025	+.024	+.024				
1975–1979								
Veteran	.558	.531	.506					
Non-veteran	.475	.447	.423					
Intra-cohort difference ^a	+.083	+.084	+.083					
Inter-cohort veteran difference ^b	+.097	+.097	+.096					
Inter-cohort non-veteran difference ^c	+.039	+.038	+.038					
1980–1984								
Veteran	.594	.567						
Non-veteran	.494	.467						
Intra-cohort difference ^a	+.100	+.101						
Inter-cohort veteran difference ^b	+.036	+.036						
Inter-cohort non-veteran difference ^c	+.019	+.019						
1985–1989								
Veteran	.518							
Non-veteran	.479							
Intra-cohort difference ^a	+.039							
Inter-cohort veteran difference ^b	–.076							
Inter-cohort non-veteran difference ^c	–.015							

Note. Predicted probabilities based on current smoking model estimates in Table 2. Predictions are for non-Black non-Hispanic respondents in the 1985–1989 period. Predicted probabilities are only presented for the age range observed in the data for a given cohort.

^a Intra-cohort differences represent the difference between veterans and non-veterans within a given cohort.

^b Inter-cohort veteran differences represent the difference between veterans in a given cohort relative to veterans in the contiguous earlier cohort; it reflects between-cohort change among veterans.

^c Inter-cohort non-veteran differences represent the difference between non-veterans in a given cohort relative to non-veterans in the contiguous earlier cohort; it reflects between-cohort change among non-veterans.

Table 4.

Predicted Probability of Current Heavy Smoking by Veteran Status, Cohort, and Age, 1985–2014 National Survey of Drug Use and Health (NSDUH), Unweighted $N = 189,499$.

Cohort	Age (in years)							
	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64
1920–1944								
Veteran				.348	.333	.298	.270	.214
Non-veteran				.299	.286	.254	.228	.179
Intra-cohort difference ^a				+.049	+.047	+.044	+.041	+.035
Inter-cohort veteran difference ^b				—	—	—	—	—
Inter-cohort non-veteran difference ^c				—	—	—	—	—
1945–1949								
Veteran			.367	.348	.333	.298	.270	.214
Non-veteran			.318	.299	.286	.254	.228	.179
Intra-cohort difference ^a			+.066	+.064	+.062	+.058	+.054	+.046
Inter-cohort veteran difference ^b			—	0	0	0	0	0
Inter-cohort non-veteran difference ^c			—	−.015	−.014	−.013	−.012	−.010
1950–1954								
Veteran		.402	.385	.365	.350	.314	.285	.227
Non-veteran		.285	.271	.254	.242	.213	.191	.148
Intra-cohort difference ^a		+.117	+.114	+.111	+.108	+.100	+.094	+.079
Inter-cohort veteran difference ^b		—	+.017	+.017	+.016	+.016	+.015	+.013
Inter-cohort non-veteran difference ^c		—	−.031	−.030	−.029	−.027	−.025	−.021
1955–1959								
Veteran	.433	.426	.408	.387	.372	.335	.305	
Non-veteran	.280	.274	.260	.244	.232	.204	.183	
Intra-cohort difference ^a	+.153	+.152	+.148	+.144	+.140	+.131	+.122	
Inter-cohort veteran difference ^b	—	+.024	+.023	+.023	+.022	+.021	+.020	
Inter-cohort non-veteran difference ^c	—	−.011	−.011	−.010	−.010	−.009	−.009	
1960–1964								
Veteran	.377	.370	.353	.334	.319	.285		
Non-veteran	.261	.255	.242	.226	.215	.189		
Intra-cohort difference ^a	+.116	+.115	+.111	+.107	+.104	+.096		
Inter-cohort veteran difference ^b	−.056	−.056	−.055	−.054	−.053	−.050		
Inter-cohort non-veteran difference ^c	−.019	−.019	−.018	−.017	−.017	−.015		
1965–1969								
Veteran	.335	.328	.312	.294	.280			

Cohort	Age (in years)							
	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64
Non-veteran	.231	.225	.213	.199	.189			
Intra-cohort difference ^a	+.104	+.103	+.099	+.095	+.092			
Inter-cohort veteran difference ^b	–.042	–.042	–.041	–.040	–.039			
Inter-cohort non-veteran difference ^c	–.030	–.030	–.029	–.027	–.026			
1970–1974								
Veteran	.270	.264	.250	.234				
Non-veteran	.225	.219	.207	.193				
Intra-cohort difference ^a	+.045	+.044	+.043	+.041				
Inter-cohort veteran difference ^b	–.065	–.064	–.062	–.060				
Inter-cohort non-veteran difference ^c	–.006	–.006	–.006	–.006				
1975–1979								
Veteran	.267	.261	.247					
Non-veteran	.222	.217	.205					
Intra-cohort difference ^a	+.045	+.044	+.043					
Inter-cohort veteran difference ^b	–.003	–.003	–.003					
Inter-cohort non-veteran difference ^c	–.003	–.003	–.002					
1980–1984								
Veteran	.293	.287						
Non-veteran	.200	.195						
Intra-cohort difference ^a	+.093	+.092						
Inter-cohort veteran difference ^b	+.026	+.025						
Inter-cohort non-veteran difference ^c	–.022	–.022						
1985–1989								
Veteran	.220							
Non-veteran	.166							
Intra-cohort difference ^a	+.054							
Inter-cohort veteran difference ^b	–.073							
Inter-cohort non-veteran difference ^c	–.034							

Note. Predicted probabilities based on current heavy smoking model estimates in Table 2. Predictions are for non-Black non-Hispanic respondents in the 1985–1989 period. Predicted probabilities are only presented for the age range observed in the data for a given cohort.

^a Intra-cohort differences represent the difference between veterans and non-veterans within a given cohort.

^b Inter-cohort veteran differences represent the difference between veterans in a given cohort relative to veterans in the contiguous earlier cohort; it reflects between-cohort change among veterans.

^c Inter-cohort non-veteran differences represent the difference between non-veterans in a given cohort relative to non-veterans in the contiguous earlier cohort; it reflects between-cohort change among non-veterans.

Table 5.

Sample Description, Wisconsin Longitudinal Study (WLS), 1957–1993.

Variable	Males in male sibling pairs (<i>n</i> = 832)		All males (<i>n</i> = 3,498)	
	%	<i>M</i> (<i>SD</i>)	%	<i>M</i> (<i>SD</i>)
Smoking duration (0–53 years)		13.7 (14.1)		13.5 (13.7)
Active-duty military service (yes = 1)	57.5		57.6	
Birth year (1922–1964)		1939 (4.6)		1939 (3.7)
IQ score (61–145)		103.2 (15.2)		103.1 (15.3)
Personality measures				
Extroversion (1–36)		15.6 (8.4)		17.9 (8.1)
Agreeableness (2–36)		18.9 (9.6)		21.8 (9.2)
Conscientiousness (2–36)		19.7 (10.3)		22.9 (9.8)
Neuroticism (1–30)		10.7 (6.2)		12.3 (6.2)
Openness (1–35)		16.0 (7.2)		17.8 (6.9)
Family background				
Mother's education (7–18 years)		10.7 (3.0)		10.7 (2.9)
Father's education (7–18 years)		10.4 (3.2)		10.5 (3.2)
Average parental income, 1957–1960 (US\$0-US\$60,910)		6,069 (4,740)		6,483 (6,266)
Parents' socioeconomic index score (1–74)		15.9 (10.9)		17.4 (11.4)
Lived with both parents in 1957 (yes = 1)	90.8		94.0	

Table 6.

Poisson Regression Analysis of Lifetime Smoking Duration by Military Service and Observed Control Variables, Without and With Sibling Fixed Effects, Wisconsin Longitudinal Study, 1957–1993.

Variable (reference category/ range)	All males (<i>n</i> = 3,498)		Male sibling pairs (<i>n</i> = 832)	
	Model 1a	Model 1b	Model 2a	Model 2b
	IRR [95% CI]	IRR [95% CI]	IRR [95% CI]	IRR [95% CI]
Military service (No)	1.189 [1.107, 1.278]	1.175 [1.094, 1.262]	1.210 [1.147, 1.276]	1.198 [1.135, 1.264]
Birth year (1922–1964)	0.982 [0.972, 0.991]	0.982 [0.973, 0.991]	0.969 [0.965, 0.974]	0.972 [0.967, 0.976]
IQ score (61–145)		0.997 [0.994, 0.999]		0.994 [0.992, 0.996]
Personality measures				
Extroversion (1–36)		1.005 [0.997, 1.013]		1.010 [1.004, 1.016]
Agreeableness (2–36)		0.993 [0.985, 1.001]		0.999 [0.992, 1.005]
Conscientiousness (2–36)		0.993 [0.985, 1.001]		0.985 [0.978, 0.992]
Neuroticism (1–30)		1.011 [1.004, 1.018]		1.019 [1.013, 1.024]
Openness (1–35)		1.001 [0.993, 1.010]		0.995 [0.989, 1.002]
Family background				
Mother's education (7–18)		1.006 [0.990, 1.023]		
Father's education (7–18)		1.017 [0.998, 1.037]		
Average parental income (US\$0–US\$60,910)		1.055 [0.983, 1.133]		
Parents' socioeconomic index score (1–74)		0.991 [0.983, 0.999]		
Lived with both parents in 1957 (no)		0.882 [0.775, 1.004]		
Sibling fixed effect included	No	No	Yes	Yes

Note. IRR = incident risk ratio. 95% CI = 95% confidence interval; bolded coefficients are statistically significant at the $p < .05$ level.