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EFFECTS OF SCHOOL SHOOTINGS ON RISKY BEHAVIOR, HEALTH AND HUMAN CAPITAL

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ABSTRACT

We examine the impact of school shootings on health and human capital outcomes of exposed students as adults in their twenties and early thirties. Our data on school shooting incidents is from a database compiled by the Center for Homeland Defense and Security. The analytic dataset contains incidents from 1994-2005 in conjunction with Behavioral Risk Factors Surveillance System survey data from 2003-2012. We find substantial evidence that, relative to their unexposed peers, students exposed to school shootings experience declines in health and well-being, engage in more risky behaviors, and have worse education and labor market outcomes.

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1 Introduction

"Toxic stress results from intense adverse experiences that may be sustained over a long period of time—weeks, months or even years" (Middlebrooks and Audage, 2008). Arguably, school shootings are "intense adverse experiences" and the stress they create may have long lasting effects. Stress during early adulthood has been shown to negatively affect health later in life (Grossman et al., 2018). Therefore, the health and human capital outcomes among those exposed to school shootings may plausibly be worse.

In this paper, we examine the effects of school shootings on the health, health-related behavior and human capital outcomes of exposed middle and high school students as adults in their twenties and early thirties in the contiguous United States. Taking a population perspective, we find substantial evidence of declines in health and well-being, worse health-related behaviors and worse education and labor market outcomes. Our work complements Rossin-Slater et al. (2019); Cabral et al. (2020); Levine and McKnight (2020b). Each of those studies examined the proximate effects of school shootings. In contrast, we use a comprehensive set of measures meant to study longer term effects, 6-18 years after school shooting incidents. The long term effects of school shootings have not been examined in the literature in the U.S. context. A recent working paper by Bharadwaj et al. (2021), examines the effect of a unique mass shooting in Norway on exposed children in both the short and long term; the results reported are similar to our own.

Our data on school shooting incidents is from a recent, comprehensive database, the K-12 School Shooting Database, compiled by the Center for Homeland Defense and Security (CHDS) at the Naval Postgraduate School. According to Riedman and O'Neill (2020), state that the data collection attempted to include "[each and every instance] a gun is brandished, is fired, or a bullet hits on school property for any reason, regardless of the number of victims (including zero), time, day of week, or reason [e.g. planned attack, accidental, domestic violence, gang-related, officer involved shooting]." The incidents in the school shootings dataset span the period from 1970 to 2018 and through 2019 up to the date of publication. Work by Levine and McKnight (2020b), using the same school shooting database used in this paper, examines the effect of shootings that "occurred during school hours on a school day and resulted in [at least one] death"; the authors chose to examine these shootings out of concerns about reporting bias believing that school shooting reporting "patterns may not be an accurate reflection of experience." Levine and McKnight (2020b) analyzed their chosen subset of shootings on the basis of shooting type. Their results demonstrate the different school shooting types affect different student populations. Cai and Patel (2019) reported on a number of characteristics of these shooting incidents. The key indicators in the dataset for our analysis are the year of the shooting, the type of and location of the school and the number of casualties (injured and killed) in that incident. We use incidents from 1994 to 2005 occurring in middle and high schools in most of our analyses.

We use the 2003-2012 years of the Behavioral Risk Factors Surveillance System (BRFSS) surveys as the source for health, human capital and demographic characteristics at the individual-level (Centers for Disease Control and Prevention, 2019). We restrict the sample to individuals 23 to 32 years of age. An individual is defined as being exposed to a school shooting incident if they live in a county where a school shooting occurred and if they were between 11 and 17 years old during the year of the shooting. All other 23 to 32 old BRFSS respondents living in that county are defined as unexposed. In addition, all 23 to 32 old BRFSS respondents who live in counties that did not face school shootings are defined as unexposed. Implicitly, we assume that the survey respondent lived in the county of the shooting at the time of the shooting to the survey date, and no respondent moved to the county after the shooting date. These are prima facie sources of measurement error, and a limitation of our study which we describe in detail below. The treatment variable takes the value of zero if the individual was not exposed and is equal to the number of casualties (injured or killed) in that incident if the individual is exposed to a shooting incident. We examine the effects of exposure to a school shooting on a number of health, health-related behaviors, human capital and labor market outcomes.

We find substantial evidence of declines in health and well-being, worse health-related behaviors and worse education and labor market outcomes. The results remain qualitatively unchanged when plausible, alternative definitions of exposure and intensity of incidents are considered. We find that the effects are stronger among individuals for whom 6-12 years have elapsed since the school shooting. Many effects appear to dissipate among individuals for whom 13-18 years have elapsed since the shooting. Unfortunately, for reasons addressed elsewhere in the literature e.g., Mason and Fienberg (1985); Heckman and Robb (1985), we cannot cleanly identify differences between age effects and elapsed time effects. Younger exposed individuals are also more likely to have been exposed more recently, so we cannot say whether our findings are due to age effects or elapsed time effects. Measurement errors in assignment to treatment are also likely to be greater among the sample with longer durations from the shooting incidents.

Our results are likely to be lower bounds of the true effects. Note that ours is an intent-to-treat study design. There are two main sources of measurement error in assignment to treatment. First, we are only able to match BRFSS respondents to the shooting incidents by county. The median county in the US has four high schools, with larger urban counties having substantially more schools. To the extent that individuals who went to other schools in the same county may not have been affected by a shooting at a different school, the measurement error in our treatment variable leads to estimated effects that are biased toward zero. However, to the extent that the impacts of school shootings are felt by children throughout the affected school districts or county, as the popular press suggests, assuming that all children in the county were exposed may be a reasonable approximation to true assignment. Second, BRFSS provides no indication of whether the person moved into the current county of residence recently or whether they have lived there all their lives. In other words, we cannot identify in- or out-migration in the data. If one of the consequences of being exposed to a school shooting is for families to move away from that county, then the estimated effects would be biased toward zero. However, if truly exposed families (whose high-school age children attend schools in which shootings occur) are less likely to move as a consequence of exposure relative to their neighbors whose high-school age children attend schools that did not experience the shooting, then the estimated effects would be biased away from zero. It is a possibility we cannot rule out. But, given the low rates of migration during the time period under consideration (Frey, 2009; DeWaard et al., 2018), along with the small fractions of exposed relative to unexposed individuals within counties, it seems unlikely that either migration bias would be large.

We estimate effects using a number of alternative specifications to check the sensitivity of our primary findings to issues of measurement of exposure, the composition of counties in our analysis, the time period of data we chose to study, and the possibly confounding effects of overall violent crime and socioeconomic conditions including migration. The results are qualitatively robust to each of the alternatives we considered.

The remainder of the paper is organized as follows. A brief survey of the existing literature can be found in section 2. In section 3, we describe the CHDS K-12 School Shooting Database, the Behavioral Risk Factors Surveillance System and the characteristics of our sample. Next, in section 4, we describe the regression specifications used in this study. Results of the main and alternate specifications are described in section 5, following which we conclude.

2 Background

The peer-reviewed literature on school shootings is augmented by reports from the press with the findings of both often mirroring each other. Gaudiano (2019), writing for *Politico*, reported that federal aid applications made by school districts after shooting incidents describe significant deteriorations in academic performance, mental health and substance abuse among students in those districts. According to Fetters (2019) "death by suicide of someone connected to a school shooting is, unfortunately, a familiar story line."

Beland and Kim (2016) found that ninth grade enrollment decreases in schools where a shooting occurred, while continuing students scored lower on math and English standardized tests. The standardized test scores are lower for continuing students for up to three years after a school shooting; the authors interpreted this finding as evidence that school shooting survivors are subjected to "medium-term trauma" (Beland and Kim, 2016). Levine and McKnight (2020a) showed that school shootings, in addition to negatively affecting test scores, increase chronic absenteeism, and lead to a subsequent increases in suicides and accidental deaths, with boys being more affected by school shootings than girls. The authors also found that high fatality school shootings have significant negative effects on the surviving students, and that shootings with no or a low number of fatalities may also produce negative effects (Levine and McKnight, 2020a).

Complementary work by Cabral et al. (2020) uses the universe of public school shootings in Texas and shows that exposed students have higher rates of absence and higher rates of chronic absenteeism relative to unexposed students for two years following a shooting. The exposed students are also more likely to repeat a grade within the same time period. The authors also found that exposure to a school shooting results in negative educational and employment outcomes. Exposed students are less likely to graduate from high school and less likely to enroll in and complete college; they also have decreased employment and earnings at 24-26 years of age. Mass shootings, of which school shootings are a subset of, also negatively affect the economic outcomes in the areas in which they occur (Soni and Tekin, 2020). Forthcoming work shows that mass shootings negatively affects employment, earnings, and housing prices at the county level (Brodeur and Yousaf, 2020).

A recent working paper by Rossin-Slater et al. (2019) examines the effect of fatal school shootings on youth antidepressant use for two subsequent years; the authors compared the number of antidepressant prescriptions written by providers practicing within five miles of an affected school with those written by providers in reference areas ten to fifteen miles away. They found, within this relatively short time frame, that the number of monthly antidepressant prescriptions written for individuals under the age of twenty is 21.3 percent higher within five miles of an impacted school relative to the reference areas and that fatal school shootings have no effect on antidepressant use among adults.

More broadly, in a review of the clinical psychology literature, Orcutt et al. (2014) concluded that "the psychological consequences of directly experiencing or witnessing a mass shooting are often serious." Individuals subject to such incidents are at a higher risk for a variety of consequences of distress. The health of adults and children is negatively affected by stressful, adverse events (Deb et al., 2011; Flaherty et al., 2013; Dursun, 2019; Soni and Tekin, 2020). Adults who engage in unhealthy behaviors to a problematic degree are more likely to increase their engagement in these behaviors after being laid off (Deb et al., 2011). Soni and Tekin (2020) find that the negative effects of mass shootings spill over into the communities in which they occur and subsequently result in decreases in community wellbeing and emotional health, with greater reductions among parents with children under the age of 18. However, the negative impact of an adverse event on health is more pronounced in children. In-utero exposure to mass shootings increases the occurrence of very low birth weight and very premature births (Dursun, 2019). Additionally, childhood adversity, such as mental or physical abuse, or witnessing the abuse of a caregiver, begins to affect health outcomes by early adolescence (Flaherty et al., 2013). Similarly, violence can also attribute to an "underlying risk profile" in children that remains dormant until adulthood and may result in the early onset of chronic health conditions (Taylor, 2010).

The Washington Post reports that 228,000 students (as of May 8, 2019) have lived through a school shooting since the 1999 shooting at Columbine High School (Cox et al., 2019). However, the effects are likely to extend beyond those directly affected. "Much of the cost is not directly linked to actual victims; it is the anticipation of victimization that engenders widespread anxiety, disinvestment in impacted communities, and costly efforts to avoid and mitigate attacks[;] ... shootings in schools ... are far more important than they appear in victimization statistics" (Cook, 2020). Orcutt et al. (2014) conclude that, "although higher levels of exposure are associated with greater distress, the extant research suggests that even low-level exposure results in widespread significant distress after a shooting." Low level exposure is typically experienced by children in nearby schools, plausibly throughout the school district of the directly affected school. Gaudiano (2019) reports that grant applications describe how the impacts of the shootings spread through school districts. "Across the Broward County School district, where Parkland is located, incidents of drug use or possession grew from 511 incidents the previous year to 637 that school year. "Other major offenses" grew from 317 to 367. Physical attacks grew from 34 the previous year to 128. Threats and intimidation grew to 368 incidents compared to 337 the previous year and tobacco offenses to 439 from 127." (Gaudiano, 2019)

3 Data

3.1 Datasets

We use the K-12 School Shooting Database (K12SSD) to identify school shooting incidents (Riedman and O'Neill, 2020). This dataset, created by the Naval Postgraduate School's Center for Homeland Defense and Security, is intended to document "[each and every instance] a gun is brandished, is fired, or a bullet hits school property for any reason, regardless of the number of victims (including zero), time, day of the week, or reason [e.g. planned attack, accidental, domestic violence, gang-related, officer involved shooting]" (Riedman and O'Neill, 2020). The database includes shootings that range from incidents with no casualties (injuries or death) to mass shootings with large numbers of casualties. It documents over 1,400 shootings from 1970 to mid-2019 when we retrieved the database.

Our analysis considers only school shootings in the contiguous United States (the 48 adjoining U.S. states plus the District of Columbia). We only consider incidents when the location is known to be "inside the school building" or "outside on school property". We do not consider shooting incidents when the type of school is "unknown", "other" or if the school is an elementary school. In most analyses, we eliminate shooting incidents in which there were no casualties (injured or killed victims). Following arguments by Levine and McKnight (2020b), who demonstrate the different school shooting types affect different student populations, we estimate models using data on exposure limited to shootings that occurred only during school activities and also shootings that resulted in at least one death. We also expand the definition of exposure to include shooting incidents with no casualties.

We use data on school shootings from 1994 through 2005. The K12SSD documents 238 incidents with one or more casualties. Of these, 19 schools have 2 incidents in the same year and 4 schools have 3 incidents in the same year. When more than one incident occurs in the same school in a year, we sum up the number of casualties and treat those incidents as one incident. Figure 1 shows the time series properties of school shootings. The number of incidents per year has a u-shape, rising at both ends of the analysis period. In some years, both incidents and casualties are high, but in other years casualties are high even when the number of incidents is low. Figure 2 shows the geographic dispersion of these school shootings. No region of the country is spared such incidents; 103 counties experienced 1 shooting incident, 24 counties experienced 2-3 incidents and 13 counties experienced 4 or more incidents.

We use data from the 2003 to 2012 Behavioral Risk Factor Surveillance System (BRFSS) surveys to measure health, health-related behaviors, human capital and labor market outcomes of individuals 23-32 years old. The BRFSS is a nationwide telephone survey that collects data about US residents' self-perceived health status, health-related behaviors and chronic health conditions. It also includes measures of education, labor market status and income. Sponsored by the Centers for Disease Control and Prevention (CDC) and supported by a number of federal and state government agencies, the BRFSS currently collects data in all 50 states, the District of Columbia, and three US territories (Centers for Disease Control and Prevention, 2019). According to D. Flegel, a technical writer working on behalf of the CDC's public inquiry system, through 2005, the county code is set to missing if there were less than 50 respondents for a county, (personal communication, February 3, 2021). Subsequently, the county code is set to missing if there are fewer than 50 respondents in that county or if the population of the county is less than 10,000 adults (D. Flegel, personal communication, February 3, 2021). Our primary sample has data on individuals living in 1402 counties in the contiguous US. In addition, note that BRFSS stopped reporting county identifiers after 2012.

We merge these data with the K12SSD using the county and year of school shooting incidents, that occur inside the school building or outside on school property, as identifiers. One county with a school shooting incident is not identifiable in the BRFSS data. In addition, 757 counties without shootings are not identifiable in the BRFSS data. Figure 2 shows their locations.

3.2 Exposure

A BRFSS respondent is defined as being exposed to a school shooting incident if that respondent lives in a county where a school shooting occurred and if they were between 11 and 17 years old in the year of the shooting. Individuals in our sample who lived in counties with a school shooting but who were not 11-17 years old during the shooting are defined as being unexposed. In addition, all other 23 to 32 old BRFSS respondents who live in counties without school shootings are defined as unexposed. Note that we treat 18 year old individuals as unexposed because we cannot be sure of their schooling status. Figure 3 shows the range of ages that can be exposed to shootings that occurred between 1994 and 2005 in each year of the BRFSS data. For example, individuals ages 25-31 in 2012 could have been exposed to a shooting in 1998. In 2012, individuals 23-24 and 32 years old could not have been exposed to a 1998 shooting. Figure 3 also shows ranges of possibly exposed ages for shootings that occurred in 1994, the earliest shooting year, and 2005, the latest shooting year. The latest shooting year only contributes exposed individuals in 2011 and 2012. The earliest shooting year, 1994, contributes 23-26 year old individuals exposed to shootings in 2003 through 29-32 year old individuals exposed in 2012.

In our primary analysis, we define the treatment variable as taking the value of zero if the individual was not exposed to a school shooting inside the school building or outside on school property and as taking the value equal to the number of casualties (injured or killed) in the shooting incident inside the school building or outside on school property that the individual was exposed to. Table 1 shows that 8.2% of our sample was exposed to a school shooting. The median number of casualties among the exposed is 1, while the 95th percentile value is 5. In secondary analyses, we define the treatment variable using incidents that occurred only during school activities (during school days or extra-curricular events) and separately using only those incidents that resulted in at least one death. In specification checks, we also expand the definition of exposure to include shooting incidents with no casualties, and use a more narrow definition to include only mass shootings, ie., shootings that resulted in 3 or more casualties.

3.3 Covariates

Our primary sample consists of 197,426 women and 122,519 men between 23 and 32 years old (see table 1). We conduct our analysis separately for men and women. The median age in the primary sample is 28.

Given our choice of ages of BRFSS respondents, the ages at which they were at risk of exposure, 6 to 18 years would have elapsed since the shooting up to the year of the survey. In the primary sample, among those exposed, the median time elapsed from shooting is 12 years. In order to differentiate between individuals who may have shorter time elapsed from the date of the shooting as compared to those with longer elapsed times, we also estimate the models after restricting our sample to those exposed 6-12 years ago, and those exposed between 13-18 years ago. In the shorter elapsed time sample, the median time elapsed since shooting in this sample is 10 years; the median age is 28 years. In the longer elapsed time sample, the median cohort time elapsed is 15 years and the median age is 29 years.

Among respondents not exposed to school shootings, 12% of women and 7% of men identify as Black (see table 2). In contrast, among those exposed to a school shooting, about 24% of women and 15% of men identify as Black. Table 2 also shows that women and men in other minority groups are also overrepresented in the exposed group relative to the unexposed.

3.4 Outcomes

We examine the effects of exposure to school shootings on four measures of risky behavior, four measures of health status and three human capital outcomes. Table 3 lists all the measures and presents summary statistics.

We examine cigarette smoking, alcohol consumption, seatbelt use and physical exercise as measures of risky behaviors. We examine cigarette smoking using a binary indicator *ever smoked* for whether a person smoked at least 100 cigarettes in their lifetime. We then examine current smoking status among those who had ever smoked using a multinomial variable that takes 3 values: *quit*, *smokes daily* and *smokes occasionally*. We use counts of the *number of drinking days*, the *maximum number of drinks on one occasion* and the *number of days* $\geq$ *5 drinks* to measure alcohol consumption. Seatbelt use is measured as a multinomial variable that takes integer values corresponding to *always wears a seatbelt*, *wears a seatbelt nearly always* and *wears a seatbelt sometimes, seldom or never*. We use a binary indicator for *no physical exercise* and a count of *number of days without enough rest*.

We examine a measure of lack of adequate rest, BMI categories, self reported health and mental health status as measures of health. A count of *days with not enough rest* is used to measure rest. We use a multinomial variable with *normal weight*, *underweight*, *overweight* and *obese* as the categories of BMI. Self reported health status is measured as a multinomial variable defined by *excellent or very good health*, *good health* and *fair or poor health*. Mental health is measured as a count of the *number of not good mental health days*.

Education status is measured as a multinomial variable with levels: *high school graduate*, *not a high school graduate*, *attended technical school or college* and *graduated technical school or college*. Income is measured in 8 intervals with lower limits set at \$0, \$10,000, \$15,000, \$20,000, \$25,000, \$35,000, \$50,000 and \$75,000. For employment status, we use a binary indicator for *working* in contrast to *not working* for the sample of men. However, in preliminary analysis on the sample of women, we observed differences between women who were not employed because they were homemakers and women who reported other reasons for not working. Therefore, we chose to model employment status for the sample of women using a multinomial variable with three categories: *employed*, *homemaker* and *not working*.

4 Econometric specification

4.1 Model

Denote individuals by $i = 1, 2, ..., N$, counties in the US by $j = 1, 2, ..., J$ and years of the survey in the study by $t = 1, 2, ..., T$. Denote the number of casualties due to exposure to a school shooting for individual i in county j at time t by c_{ijt}. Let $\boldsymbol{x}_{ijt}$ denote age and race indicators. Let d_{ict} denote the duration from the school shooting to the year of the survey for an exposed individual. Duration is set to zero for unexposed individual.

For each outcome, we estimate a regression model with the following functional form:

$$\mathrm{E}(y_{ijt}) = f(c_{ijt}\beta_c + \boldsymbol{x}_{ijt}\boldsymbol{\beta_x} + \beta_d d_{ijt} + \nu_j + \omega_t) \tag{1}$$

where ν_j denotes county fixed effects and ω_t denotes year fixed effects. Note that we enter duration as a linear, continuous variable to avoid the identification issues introduced if indicators for age and year and duration are all entered together (Mason and Fienberg, 1985; Heckman and Robb, 1985).

In the cases where the outcome, y_{ijt} is binary, we estimate a logistic regression with equation 1 guiding covariates. In the cases where the outcome is multinomial, we estimate a multinomial logit regression. When the outcome is measured as a count variable, we estimate Poisson regressions with covariates specified according to equation 1. Finally, for income, which is reported as an interval measure in the BRFSS, we estimate an interval regression for the intervals of the logarithm of income. Income is substantially skewed while the distribution of the log of income is considerably symmetric.

Note that the regressions all include county and year fixed effects, so the coefficient on the exposure variable, c_{ijt}, is identified using two sources of variation. One source of variation is the comparison of respondents living in a county who were exposed to a shooting (determined by the difference between their current age and the date of the shooting) and respondents in the same county who were not exposed to a shooting. The second source of variation is the comparison between the outcomes of respondents who live in counties without shootings with exposure potential (based on their age) compared to individuals without exposure potential serves to partial out effects of unobserved confounders.

The current literature suggests that school shootings affect boys and girls differently (Levine and McKnight, 2020a). Therefore, we estimate our model for exposed male and female students separately. For each regression, we report exponentiated coefficients on exposure, $\exp(\beta_c)$. These have the virtue of being interpretable as percentage changes in the outcome across all measure types. Standard errors of coefficients are estimated using the sandwich formula that adjusts for clustering of regression errors at the state level. We report p-values of the null hypothesis that the exponentiated coefficient equals 1 along with 95% confidence intervals in the figures and tables described below.

As described above, we define exposure using incidents that occurred inside the school building or outside on school property and that resulted in at least one casualty (injured or killed). We also define exposure using incidents that occurred only during school days or extra-curricular events, and separately using only those incidents that resulted in at least one death.

4.2 Specification checks

The first set of specification checks consider alternatives to our measure of exposure. First, we eliminate counties that had more than 4 shootings out of concern for the possibility that counties with multiple shootings may be different in ways not accountable by use of fixed effects. Second, we conduct two analyses to address a limitation of our data, we can only assign a shooting incident to a county, and because most counties have more than one high school, we cannot be sure that an individual designated as being exposed was actually exposed. In one specification check, we drop counties with more than 20 schools. Such counties, which comprise about 12% of all counties have many more schools than the typical county and much larger populations. In the other specification, we modify our definition of exposure from the number of casualties to the number of casualties divided by the number of schools in the county (the expected number of casualties for a potentially exposed respondent). Finally, we conduct two analyses to check the robustness of our primary results to alternative definitions of incidents. In one analysis, we limit exposure to incidents that occurred only during a school day (excluding events). In a second analysis, we include additional shooting incidents in which there were no casualties. In a third analysis, we limit incidents to those defined as mass shootings only, i.e., those with three or more casualties.

In our second set of specification checks, we first conduct two analyses that change the period of shooting incidents used in the analysis. Relative to the main sample period of 1994 - 2005, we expand the shooting incidents period to be from 1993 - 2006 and we contract the period to be from 1995 - 2004. Second, we address a concern arising from the fact that individuals who were exposed to shootings are, on average, younger and more likely to be of minority race and/or Hispanic ethnicity. We balance the exposed and unexposed samples on these characteristics, by calculating sampling weights for the unexposed sample using an entropy balancing technique developed by Hainmueller (2012). Third, we estimate regression specifications that control for annual county unemployment rates. Data on annual county unemployment rates were obtained from the Bureau of Labor Statistics (2021). Fourth, we estimate regression specifications that control for annual county-level homicide rates. We use the annual county-level homicide rates as a proxy for overall crime. The overall crime that the BRFSS respondents are exposed to may influence their likelihood to engage in risky health behaviors. Homicide rates were obtained from Kaplan (2021). Finally, we estimate regressions after limiting the samples to exclude high migration counties. Data on net migration was obtained from the University of Wisconsin-Madison's Center for Demography and Ecology (Voss et al., 2004). We calculated net migration rates of counties over the 1990-2000 decade for 20-35 year old individuals (in the year 2000). By eliminating counties that were in the top 5th percentile of net in-migration and those in the top 5th percentile of net out-migration (a total of 134 counties), we restrict the sample to individuals who were more likely to have lived in the same county through the period of possible exposure to school shootings to the survey year.

We also estimate two sets of regressions in which assignments to treatment are placebos. In the first set of placebo regressions, a BRFSS respondent is defined as being exposed to a school shooting incident if that respondent lived in a county where a school shooting occurred and if they were between 4 and 10 years old in the year of the shooting. These individuals are 7 years younger in range than the actual treated individuals. For this specification, the sample includes 20-29 year old individuals, who are 3 years younger in range than those chosen for the primary sample. Ideally, we would have preferred to lower the ages of sampled individuals by 7 years to be consistent with the placebo-treated individuals. Unfortunately, using 18 and 19 year olds in the sample would likely contaminate the placebo assignment with actual exposure. In the second set of placebo regressions, respondents who lived in counties where a shooting occurred when they were between 20 and 26 years old were defined as being exposed, 9 years older in range than the actual treated individuals. In keeping with the spirit of the cohorts for the first set of placebo regressions, for this specification, the sample includes 28-37 year old individuals who are 5 years older than those in the primary sample. We should note that these are not strictly placebo regressions because we cannot rule out possible exposure among these older and younger individuals. For example, some of these individuals may have been siblings of exposed high schoolers. Others may attend schools that were geographically contiguous or close to the high schools in which shootings occurred. Nevertheless, we expect to observe statistically insignificant effects in each of these settings.

5 Results

For each outcome, we estimate a regression model specified as in equation 1 above. We estimate a logistic regression when the outcome is a binary indicator, a multinomial logit regression when the outcome is multinomial, Poisson regression when the outcome is integer valued and interval regression when the outcome is continuous but reported only as lying in an interval. For each regression, we report exponentiated coefficients on exposure, $\exp(\beta_c)$.

For the primary sample of women, Figure 4 displays effects reported as exponentiated coefficients with associated 95% confidence intervals associated with the effect of being exposed to a shooting incident on each of the outcomes described above. We find that exposure to school shooting affects drinking behavior. There is weak evidence that the number of drinking days increases (p-value 0.158). There is statistically significant evidence that an additional shooting casualty increases the maximum number of drinks an affected woman drinks on one occasion by 0.5 percent and increases the risk of drinking 5 or more drinks by 1.8 percent.

Exposure to school shootings also affects other types of behavior. Specifically, exposed women are significantly less likely to wear seatbelts. To be precise, we find that one additional shooting casualty increases the likelihood of nearly always wearing a seatbelt relative to always wearing a seatbelt by 3.3 percent. Women exposed to shootings are also less likely to engage in physical activity by 0.4 percent. School shootings affect the Body Mass Index (BMI) of exposed female students years later. Relative to normal weight, we find that an additional shooting casualty increases the likelihood of being underweight by 2.0 percent, decreases the likelihood of being overweight by 0.7 percent and decreases the likelihood of being obese by 0.9 percent. In other words, women exposed to school shootings are of uniformly lower weight later in life than unexposed women, including being underweight.

We find school shootings affect educational attainment, income and labor force participation among women. Relative to being a high school graduate, exposure to an additional shooting casualty increases the likelihood of not being a high school graduate by 1.1 percent and increases the likelihood of attending college or technical school by 0.7 percent. The likelihood of graduating college is not significantly affected by exposure to school shootings. Income among women exposed to school shootings is 0.5 percent lower than those not exposed. As described above, we examine female labor force participation using a multinomial variable with three categories. We find that, relative to the likelihood of being employed, exposure to a school shooting increases the likelihood of being unemployed by 2.2 percent and the likelihood of being a homemaker by 1.2 percent.

In our sample of women, we find that exposure to school shootings has no effects on smoking, both as measured by the likelihood of ever having smoked cigarettes and on the likelihood of being a current smoker or a past smoker (but having quit) among those who had ever smoked. Exposure to school shootings also has no statistically significant effect on the quality of rest women get and no effect on the ways in which women report their own general health. Exposed women also do not report significantly different numbers of not good mental health days compared to the unexposed.

Figure 5 displays the effects of exposure to school shootings for the sample of men. In contrast to the results for women, the risk of smoking among men is modified by exposure. The risk of smoking daily among those who ever smoked is 1.7 percent higher for exposed men relative to men who were not exposed. Analogous to the results for women, we find substantial, statistically significant effects of exposure to school shootings on the alcohol consumption behavior of men. Exposure to shootings increases the number of drinking days by 0.5 percent, increases the maximum number of drinks an affected man drinks on one occasion by 0.3 percent and increases the number of drinking days where five or more drinks are consumed by 1.2 percent.

Exposure to school shootings affects seatbelt use among men in more severe ways than among women. Exposure to a school shooting increases the likelihood of wearing seatbelts only sometimes, seldom, or never by 2.1 percent. Men are also much less likely to engage in physical exercise post-exposure; the likelihood of not engaging in physical exercise increases by 1.5 percent. They also report more days without enough rest (by 0.9 percent).

Exposure to school shootings among men also decreases body mass across categories. Exposed men are 1.1 percent more likely to be underweight (albeit with a p-value of 0.099) and 0.6 percent less likely to be obese. Men who were exposed to school shootings report significantly different levels of health status. Relatively to excellent or very good health, exposure increases the likelihood of being in good reported health by 1.0 percent and of being in fair or poor health by 2.0 percent.

Among men, exposure to school shootings also has human capital effects, although the effects are less noticeable compared to the effects observed in the sample of women. Exposure to a school shooting increases the likelihood of attending college or technical school by 1.4 percent with no changes in the likelihoods of not graduating from high school or graduating from college relative to graduating from high school. Income decreases by 0.5 percent among exposed men as compared to those not exposed. The effect on employment status is small and statistically insignificant.

5.1 Alternative definitions of shooting incidents

Not all shootings are the same. Accuracy of measurement might vary by the nature of the incident. Students and others might be affected in different ways (Levine and McKnight, 2020b). Therefore, we also estimate models using data on exposure limited to shootings that occurred only during school activities and also shootings that resulted in at least one death, both of which are more likely to be reported (a precondition for inclusion in the K12SSD) and reported with less measurement error. Figure 6 displays estimates of effects using these two alternative definitions for the sample of women. Two broad qualitative regularities emerge. First, the results for both alternative definitions are qualitatively very similar to the effects reported using the primary definition. There is one qualitative anomaly in terms of the signs of the effects. When shooting incidents are defined using numbers of people killed only, it appears that women are less likely to be smoking daily. In contrast, the effect of school shootings on smoking status is statistically insignificant when the primary definition and the other alternative definitions are used. In addition, a few estimates that were statistically significant in the main sample become marginally insignificant (at the 5% level of significance) when the alternative definitions are used. Note that both alternative definitions reduce the number of shooting incidents, and therefore reduce the number of people exposed to such incidents. Second, the estimated effects are largest when the definition of incidents is limited to those involving deaths. We believe this finding is plausible for both reasons that motivated this analysis.

Figure 7 displays estimates when the alternative definitions are used for the sample of men. Relative to the results for the sample of men using the primary definition of exposure, the results for both alternative definitions are qualitatively very similar to the effects reported using the primary definition. The most notable difference is that those exposed to shootings using either of the alternative definitions are more likely to be underweight. Consequently, what appeared to be a substantive difference between the effects on women and men may not be so. In addition, as with the effects for the sample of women, the estimated effects for men are largest when the definition of incidents is limited to those involving deaths.

5.2 Stratified by duration since exposure

We estimate separate models after splitting the sample of exposed individuals into two groups. In one group, the duration between exposure to the shooting and the BRFSS survey is 6-12 years. In the other, the duration between exposure and the survey is 13-18 years. To be precise, beginning with the main samples of women and men, we drop exposed individuals if they do not meet the definition of the duration constraint. The sample of unexposed individuals remains the same.

Figure 8 displays the results for the sample of women. In the analysis restricted to those whose exposures occurred 6-12 prior to the survey, we find that the results are consistent with those obtained using the corresponding full sample. The number of drinking days increases by 0.6 percent. The maximum number of drinks consumed is 0.4 percent higher. Exposed women show an increase of 1.6 percent in the number of drinking days where five or more drinks are consumed. Women exposed 6-12 years ago are less likely to wear seatbelts. The effects of exposure to school shootings on body mass among women also echoes those observed in the full sample. Exposure increases the likelihood of being underweight by 2.1 percent, decreases the likelihood of being overweight by 0.8 percent and decreases the likelihood of being obese by 0.9 percent. Women exposed to school shootings 6-12 years prior to the survey are 1 percent less likely to graduate from high school and more likely, by 0.6 percent, to attend college or technical school, but not graduate. Exposure to a school shooting leads to significant declines in income - by 0.6 percent for women. Finally, the evidence shows that, among women, exposure 6-12 years prior to the survey increases the likelihood of not working by 2.7 percent and of being a homemaker by 1.7 percent.

We find that the effects diminish in magnitudes and statistical significance for many outcomes from the sample with shorter duration from exposure to survey to the sample with longer periods. Most of the effects of school shootings are not statistically significant although generally the signs are consistent with the results for the full sample of women.

In the sample of men, the results of the analysis restricted to those whose exposures occurred 6-12 prior to the survey are consistent with those obtained using the corresponding full sample. Figure 9 displays these findings. There is clear evidence of more frequent and greater alcohol consumption. There is also evidence of more risky behavior. Those exposed to school shootings are 1.3 percent more likely to nearly always wear a seatbelt (as compared to always wearing a seatbelt) and 2.3 percent more likely to sometimes, seldom, or never wear a seatbelt. As with the sample of women, we find that the effects diminish in magnitudes and statistical significance for many outcomes from the sample with shorter duration from exposure to survey to the sample with longer periods. Nevertheless, exposure to school shootings 13-18 years ago is significantly associated with higher risk of smoking daily, with greater number of drinking days, with higher risk of no physical exercise and the number of days without enough rest. Exposure also increases the risks of being underweight and of being overweight, and of not graduating from high school.

5.3 Specification checks

We conduct a number of checks to our basic specifications focus on possible errors in the measurement of exposure. The results of these analyses, for the samples of women and men, are shown in Figures 10 and 11 respectively. Note that, because we can only assign a shooting incident to a county, and because most counties have more than one high school, we cannot be sure that an individual designated as being exposed was actually exposed. As a counterargument to this possibility, note that Gaudiano (2019) reports that impacts of school shootings do spread through entire school districts which are often geographically aligned with counties. Nevertheless in one specification check, we drop counties with more than 20 schools. Such counties, which comprise about 12% of all counties have many more schools than the typical county and much larger populations. The results are consistent with those obtained in the primary analysis. We also conduct a robustness check using a “scale” measure of intensity. Instead of assigning the number of casualties as the intensity of exposure, we use the number of casualties divided by the number of schools in the county (or expected intensity for a potentially exposed respondent). The results are quite consistent with the main findings.

Next, we eliminate counties that had more than 4 shootings in one analysis out of concern for the possibility that counties with multiple shootings may be different in ways not accountable by use of the county fixed effects. Finally, we conduct three analyses that change the definition of exposure. In one analysis, we only consider incidents with casualties that occurred during school days. This definition is more stringent than one we considered in our primary analyses that restricted exposure to incidents with casualties that occurred during school days or school extra-curricular events. In another analysis, we consider an expansion of the types of incidents included in the measure of exposure. We include the 12.8% of shooting incidents recorded in the K12SSD that meet our school type and location criteria with no casualties in our definition of exposure. We code exposure equal to one if the shooting incident had no casualties, and exposure equal to the number of casualties plus one for incidents with casualties. In a third analysis, we restrict shooting incidents to include only those defined as mass shootings, i.e., incidents that involved three or more casualties. In all cases, Figures 10 and 11 show that the results are qualitatively similar to the primary analyses.

We conduct two analyses that change the period of shooting incidents used in the analysis. Relative to the main sample period of 1994 - 2005, we expand the period of shooting incidents to be from 1993 - 2006 and we contract the period to be from 1995 - 2004. In both cases, the results, shown in Figures 12 and 13 are consistent with those of the primary analysis. Second, we address a concern arising from the fact that individuals who were exposed to shootings are, on average, younger and more likely to be of minority race and/or Hispanic ethnicity (Table 2). Although controlling for these characteristics in our regressions and including county fixed effects should mitigate any confounding due to these differences, we try to mitigate any such effects further by balancing the exposed and unexposed samples on these characteristics. We do so by calculating sampling weights for the unexposed sample using an entropy balancing technique developed by Hainmueller (2012) that leads to the treated and untreated samples to have very similar weighted sample characteristics. Entropy balance combined with regression is doubly robust (Zhao and Percival, 2017). The results of these analyses for the samples of women and men, shown in Figures 12 and 13 respectively, are consistent with the primary results.

Might changes in local socioeconomic characteristics, not taken into account in the county fixed effects, produce substantial omitted variable biases? To examine such possibilities, we estimate regression specifications that control for annual county unemployment rates. We also estimate regression specifications that control for annual county-level homicide rates. In both cases, we find that results do not change in any substantive ways (Figures 12 and 13). Finally, as noted above, we cannot identify in- or out-migration in the BRFSS data. Therefore, we estimate specifications based on samples that eliminated counties which were in the top 5th percentile of net in-migration and those in the top 5th percentile of net out-migration (a total of 134 counties). The estimates of the effects of school shootings remain virtually unchanged.

The results of two sets of placebo-like regressions are displayed in Figures 14 and 15. In the first set of placebo regressions, individuals who were between 4 and 10 years old in the year (and county) of a shooting are defined as exposed. The results, displayed in Figure 14, show that the effects are overwhelmingly insignificant among women and men. In the case of men, all three statistically significant effects (ever smoked, number of drinking days and no physical exercise), suggest that exposed men have better health and health behaviors. If these are not spurious, these findings would suggest that sources of bias would bias coefficients in the primary regressions, where we find evidence of worse health, toward zero.

In the second set of placebo regressions, individuals who lived in counties where a shooting occurred when they were between 20 and 26 years old were defined as being exposed. Again, the results, displayed in Figure 15, show that the effects are overwhelmingly insignificant for both samples of women and men. Exposed women appear significantly less likely to be not working or homemakers, which are opposite in sign to the results we find in the primary sample. Again, if these are not spurious, these findings would suggest that sources of bias would bias coefficients in the primary regressions toward zero.

6 Conclusion

In this paper, we examine the effects of school shootings on health, health-related behaviors and human capital outcomes of exposed students as adults in their twenties and early thirties. We use data from K12SSD, a comprehensive database of school shootings, and BRFSS, to estimate the effects of exposure to school shootings while in high school.

Among women and men, we find substantial evidence of declines in health and well-being, worse health-related behaviors and worse education and labor market outcomes. The results are robust to alternative definitions of shooting incidents and in a variety of specification checks. We also find that the effects are stronger among individuals for whom 6-12 years have elapsed since the school shooting as compared to individuals for whom 13-18 years have elapsed since the shooting. Because duration since the shooting and age are mechanically related in our design, we cannot cleanly identify differences between age effects and elapsed time effects. We also expect greater measurement errors in assignment to treatment in the sample with longer duration since exposure. Therefore, we do not draw strong conclusions about the possibility of effects truly dissipating over time from these results.

In general, our results are likely to be lower bounds of the true effects because a number of likely sources of measurement error, most notably in assignment to treatment, are likely to attenuate estimates of effects. We estimate effects using a number of alternative specifications to check the sensitivity of our primary findings to issues of measurement of exposure, the composition of counties in our analysis, the time period of data we chose to study, and others. The results are qualitatively robust to each of the alternatives we considered.

Our study has a number of limitations. Notably, because we are only able to match BRFSS respondents to the shooting incidents by county, we cannot identify whether a high school age individual in a county actually attended the school in which a shooting occurred. In addition, because BRFSS provides no indication of whether the person moved into the current county of residence recently or whether they have lived there all their lives, we cannot identify whether a person who was of high school age during a shooting incident in a county actually lived in that county when it occurred or whether they moved out of that county following the school shooting. Nevertheless, our results including a number of specification checks are strongly suggestive of detrimental causal effects of school shootings on health behaviors, health and human capital in the population. Finally, we are not able to test the mechanisms through which exposure to school shootings affect our outcomes of interest. As we alluded to earlier in our paper, the "toxic stress" from these "intense adverse experiences" may be a mechanism worth exploring. We leave this to future research.

7 Declaration of Interests

Declarations of interest: none.

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Table 1: Characteristics of casualties and duration in primary sample and strata of duration since exposure

Sample definition	Sample size		P_{50}	Percent	Casualties		Duration from exposure		
	Female	Male	age	exposed	P_{50}	P_{95}	P_{50}	Min	Max
Full sample	197426	122519	28	8.2	1	5	12	6	18
6-12 years duration	189119	117150	28	4.5	1	4	10	6	12
13-18 years duration	188184	116623	29	4.1	2	5	15	13	18
Incidents with deaths only	197426	122519	28	4.4	1	3	12	6	17
Incidents during school day or events	197426	122519	28	5.5	2	3	12	6	17

Note: The primary sample consists of individuals ages 23-32 living in 1402 counties from 2003-2012 BRFSS. P_{50} is the median among those exposed. P_{95} is the 95^{th} percentile value in the sample among those exposed. Min and Max denote the minimum and maximum sample values among those exposed.

Table 2: Means of covariates in primary samples of women and men

	Sample of women		Sample of men	
	Not exposed	exposed	Not exposed	exposed
Age in years	28.19	26.85	28.10	26.80
Black race	0.12	0.24	0.07	0.15
non-Black Hispanic ethnicity	0.13	0.20	0.12	0.19
Other race	0.06	0.09	0.07	0.11

Note: The primary sample consists of individuals ages 23-32 living in 1402 counties from 2003-2012 BRFSS.

Table 3: Means of outcomes in primary samples of women and men

	Sample of women			Sample of men		
	N	mean		N	mean	
		Not exposed	exposed		Not exposed	exposed
Ever smoked	196,589	0.376	0.302	121,840	0.454	0.409
Among ever smoked: quit	69,464	0.435	0.390	51,030	0.419	0.372
smokes daily		0.175	0.213		0.207	0.259
smokes occasionally		0.390	0.397		0.374	0.369
No. of drinking days	137,926	3.821	4.738	99,132	6.850	7.382
Max drinks on one occasion	78,877	3.284	3.407	63,098	5.978	5.836
No. of days >= 5 drinks	101,618	0.853	1.077	82,769	2.174	2.177
Wears seatbelt: always	74,405	0.823	0.872	52,546	0.711	0.794
nearly always		0.106	0.076		0.148	0.114
sometimes, seldom, never		0.071	0.052		0.141	0.092
No physical exercise	196,195	0.209	0.214	121,567	0.168	0.152
No. of days not enough rest	56,653	11.455	10.543	31,972	9.895	9.117
BMI category: normal	157,380	0.033	0.034	66,364	0.015	0.015
underweight		0.471	0.493		0.352	0.412
overweight		0.262	0.250		0.409	0.386
obese		0.234	0.223		0.224	0.186
Health: excellent or v. good	192,815	0.621	0.614	115,047	0.636	0.643
good		0.287	0.291		0.280	0.274
fair or poor		0.092	0.095		0.084	0.082
No. of not good mental health days	195,394	4.500	4.412	121,332	3.268	3.402
Education level: HS graduate	188,437	0.088	0.099	112,535	0.093	0.083
not HS graduate		0.239	0.206		0.282	0.233
attended college/tech		0.296	0.269		0.266	0.262
graduated college/tech		0.377	0.426		0.359	0.421
Income $0 - $9,999	180,372	0.064	0.087	112,680	0.037	0.054
$0,000 - $14,999		0.056	0.067		0.039	0.051
$15,000 - $19,999		0.087	0.097		0.073	0.077
$20,000 - $24,999		0.109	0.106		0.102	0.105
$25,000 - $34,999		0.142	0.136		0.143	0.128
$35,000 - $49,999		0.174	0.154		0.189	0.158
$50,000 - $74,999		0.177	0.151		0.195	0.174
$75,000 -		0.190	0.202		0.222	0.254
Employment: working	195,211	0.651	0.625	122,188	0.840	0.779
not working		0.167	0.226		0.160	0.221
homemaker		0.183	0.148			

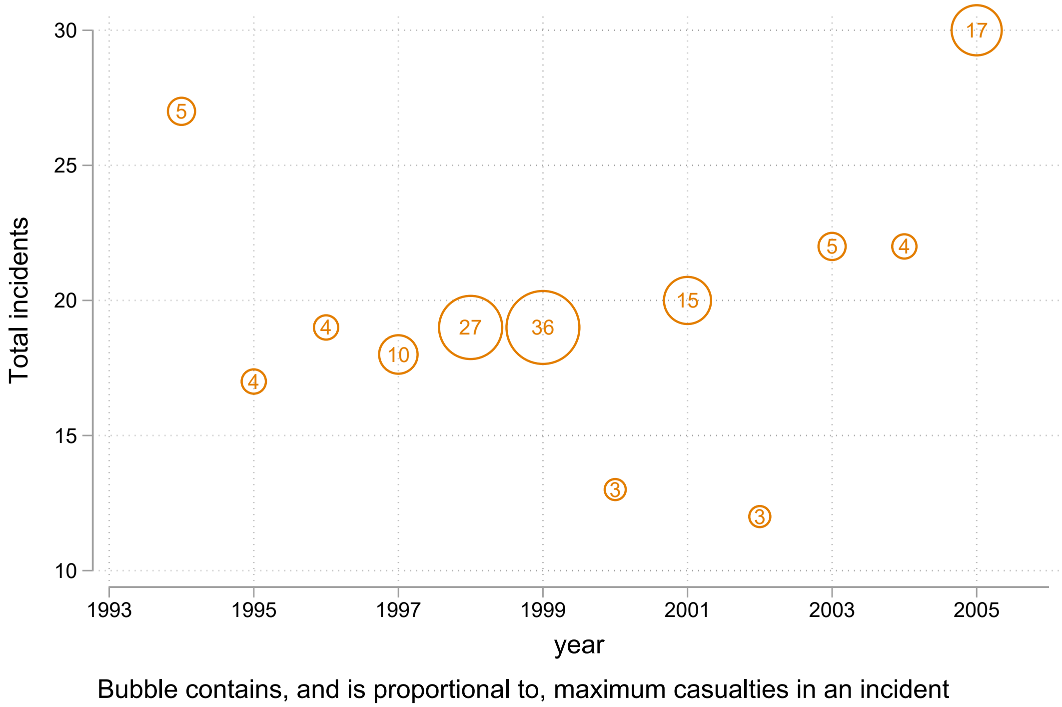
Note: The primary sample consists of individuals ages 23-32 living in 1402 counties from 2003-2012 BRFSS.

Table 4: Characteristics of casualties and duration in samples and definitions used in specification checks

Change from main specification	Sample size		Percent	Intensity		Years since exposure		
	female	male	exposed	P_{50}	P_{95}	P_{50}	Min	Max
Shorten shooting period to 1995-2004	197426	122519	6.3	2	4	12	6	17
Extend shooting period to 1993-2006	197426	122519	10.5	1	5	13	6	19
Covariate balancing weights applied	197426	122519	50.0	1	5	12	6	18
Restrict to counties (1396) with $\leq$ 4 incidents	190682	118325	6.5	1	3	12	6	18
Restrict to counties (1239) with $\leq$ 20 schools	122365	74915	2.4	1	8	12	6	18
Divide intensity by the number of schools	197426	122519	8.2	.033	.158	12	6	18
Incidents during the school day only	197426	122519	5.0	2	3	12	6	17
Incidents include those with 0 casualties	197426	122519	6.8	3	5	12	6	17

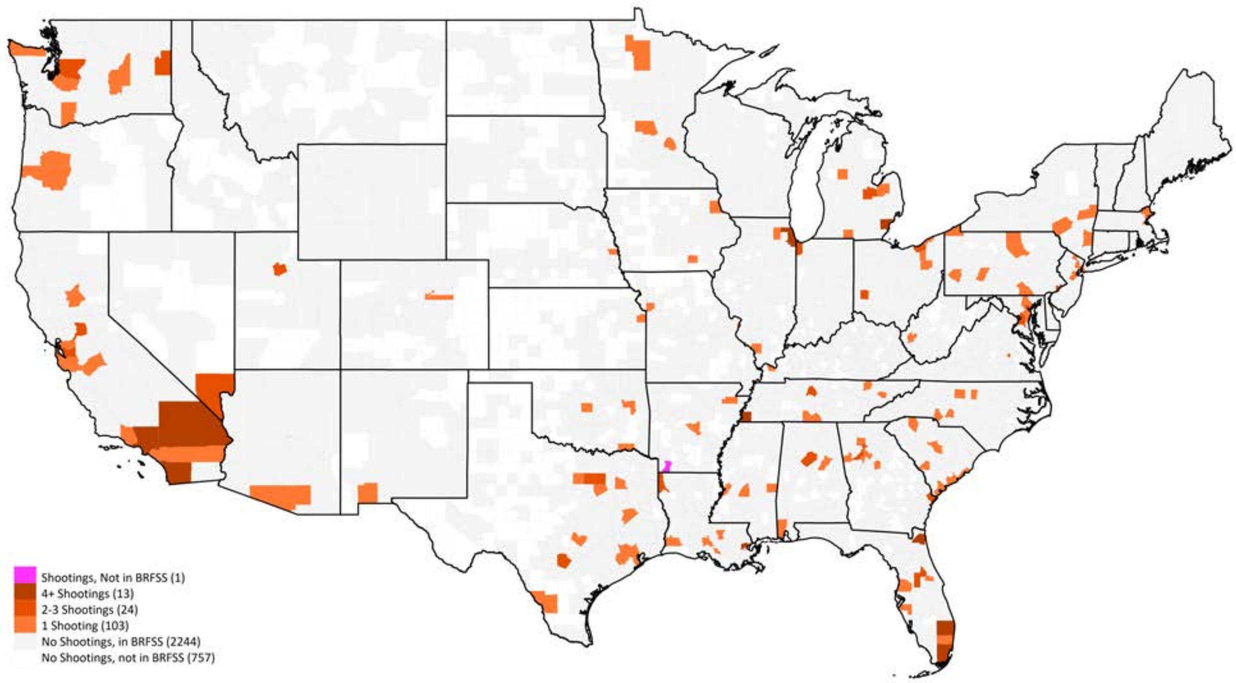
Note: The primary sample consists of individuals ages 23-32 living in 1402 counties from 2003-2012 BRFSS. The samples used in specification checks also span the same counties and years unless otherwise specified. P_{50} is the median among those exposed. P_{95} is the 95^{th} percentile value in the sample among those exposed. Min and Max denote the minimum and maximum sample values among those exposed. The numbers of counties in county-restricted samples are reported in parentheses.

Figure 1: Numbers of school shooting incidents and associated casualties over time



Note: Includes school shootings from 1994-2005.

Figure 2: Counties in which school shooting incidents occurred



Note: Includes school shootings from 1994-2005.

Figure 3: Ages of BRFSS respondents in each survey year who would be potentially exposed to school shootings in three illustrative years

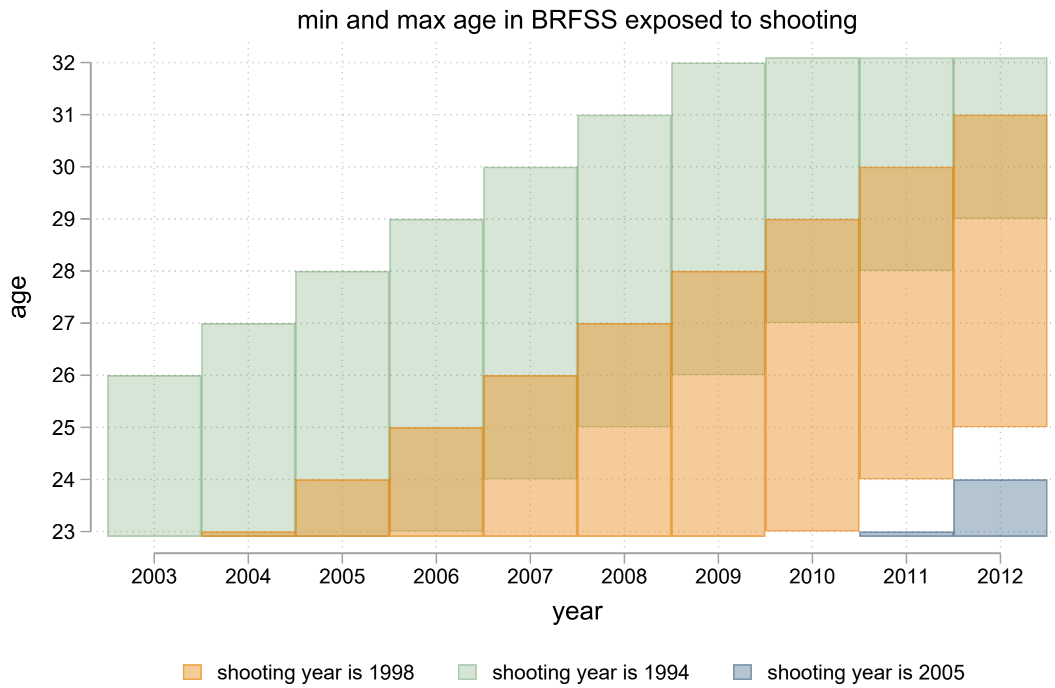
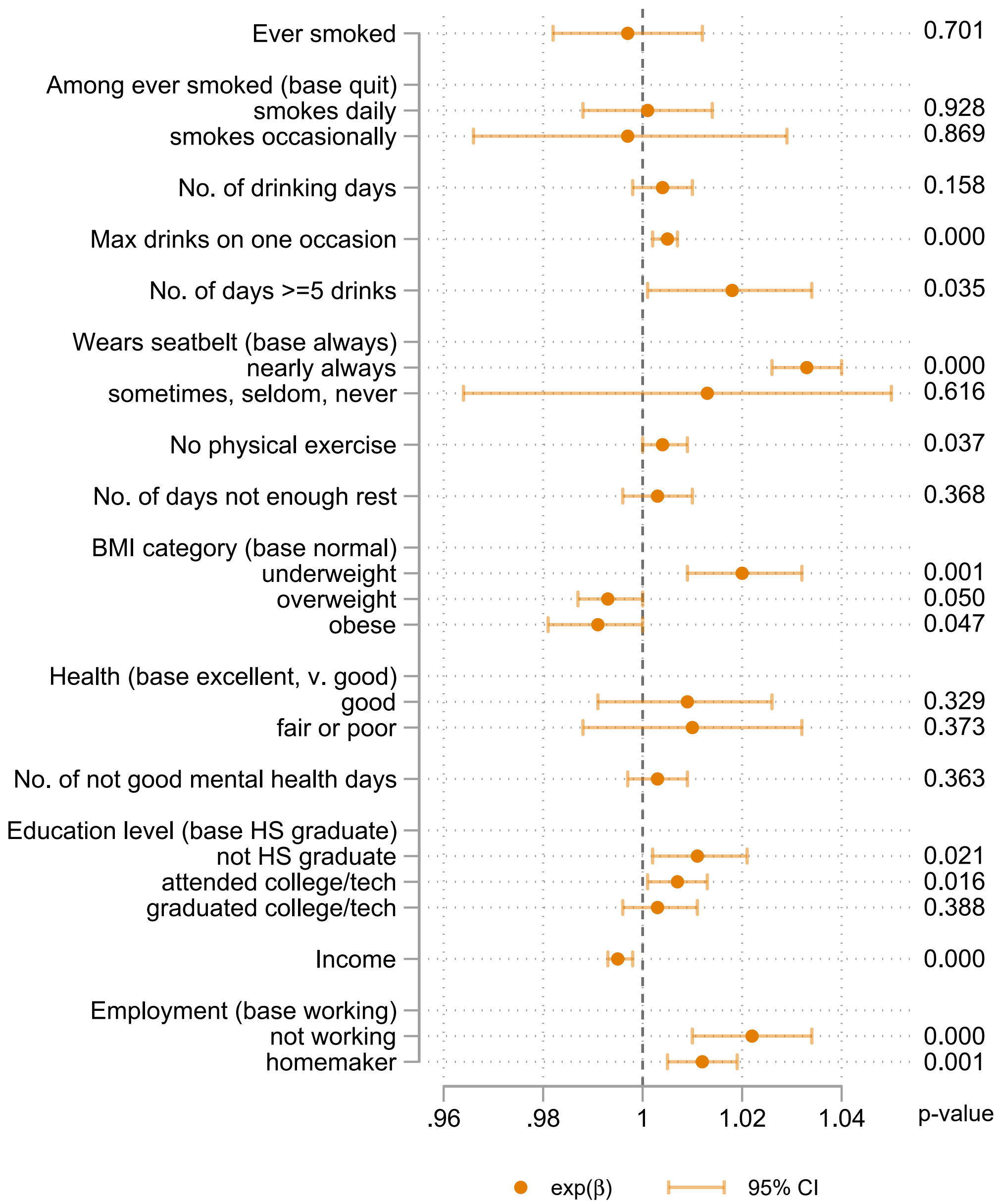
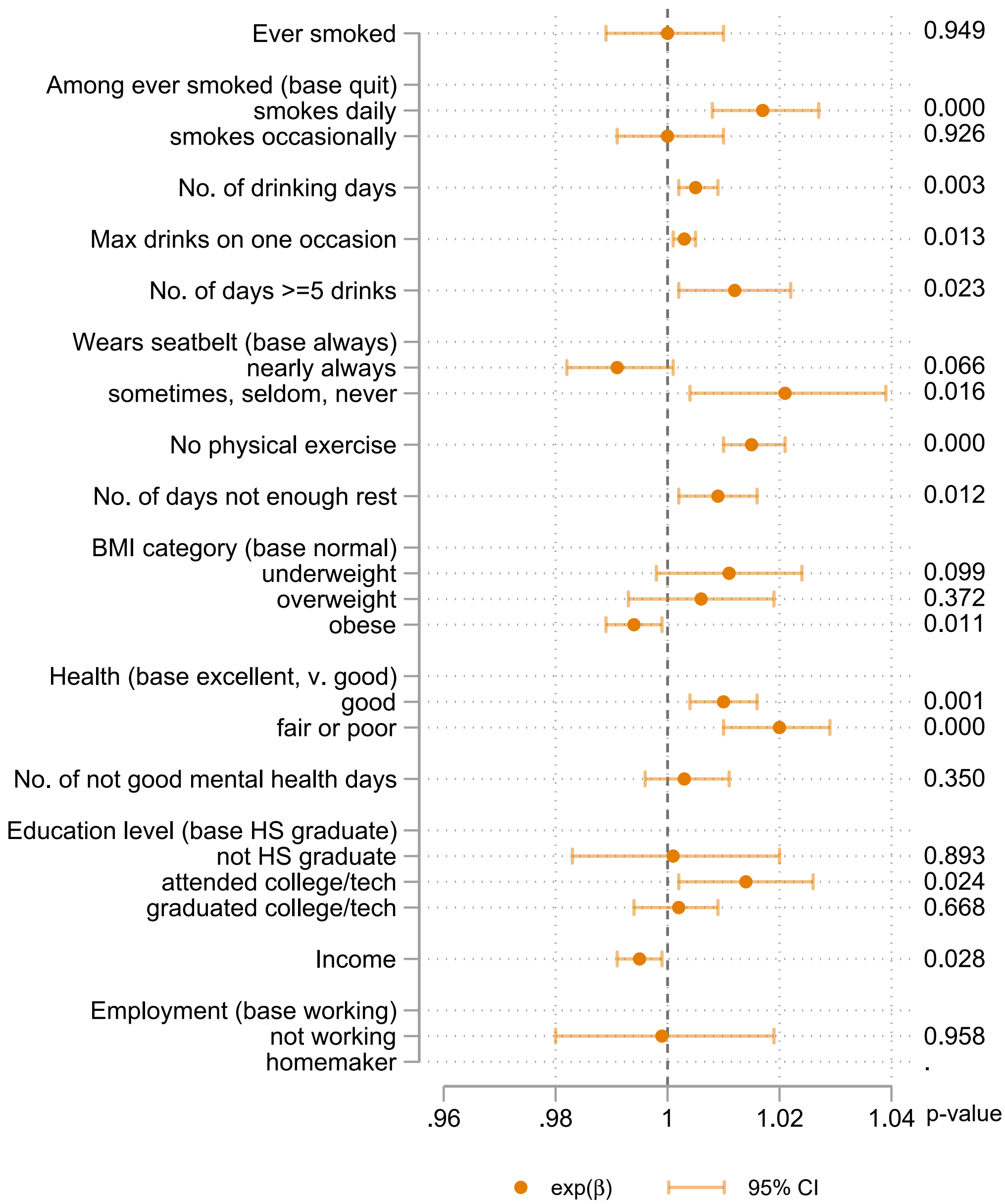


Figure 4: Estimates of effects of exposure to school shootings on outcomes among women



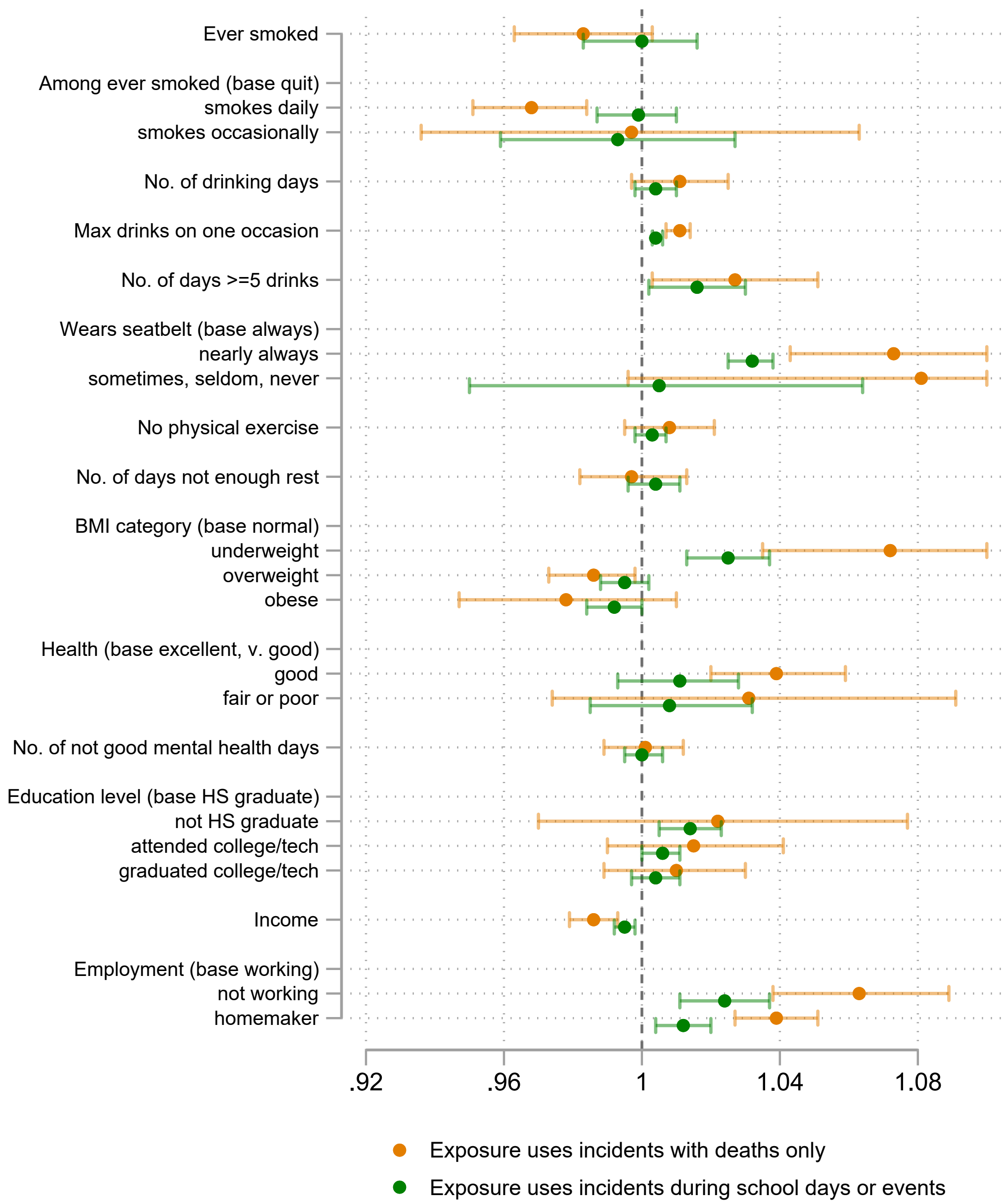
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.95,1.05] to enhance readability.

Figure 5: Estimates of effects of exposure to school shootings on outcomes among men



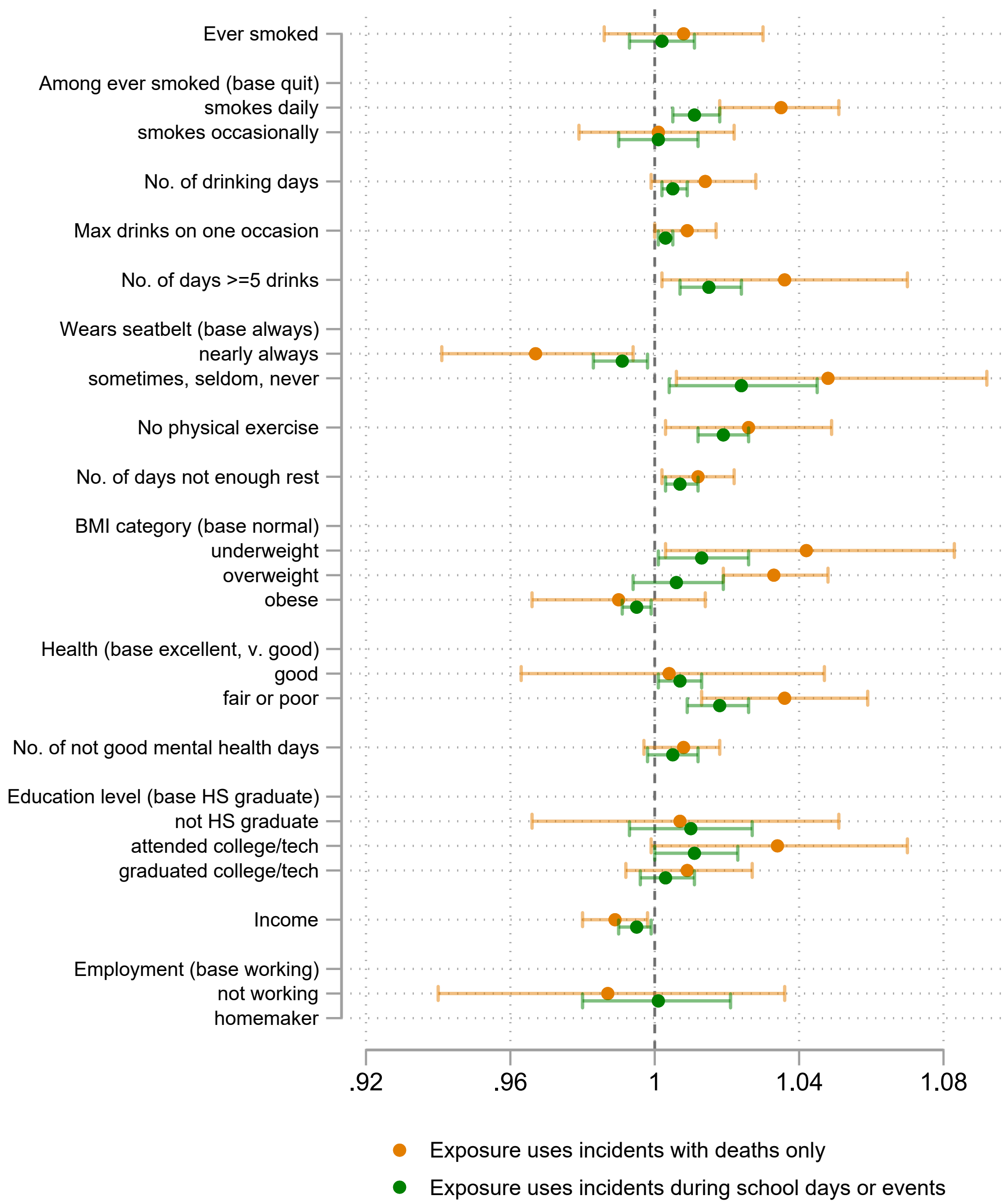
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.95,1.05] to enhance readability.

Figure 6: Estimates of effects of exposure (using alternative definitions) to school shootings on outcomes among women



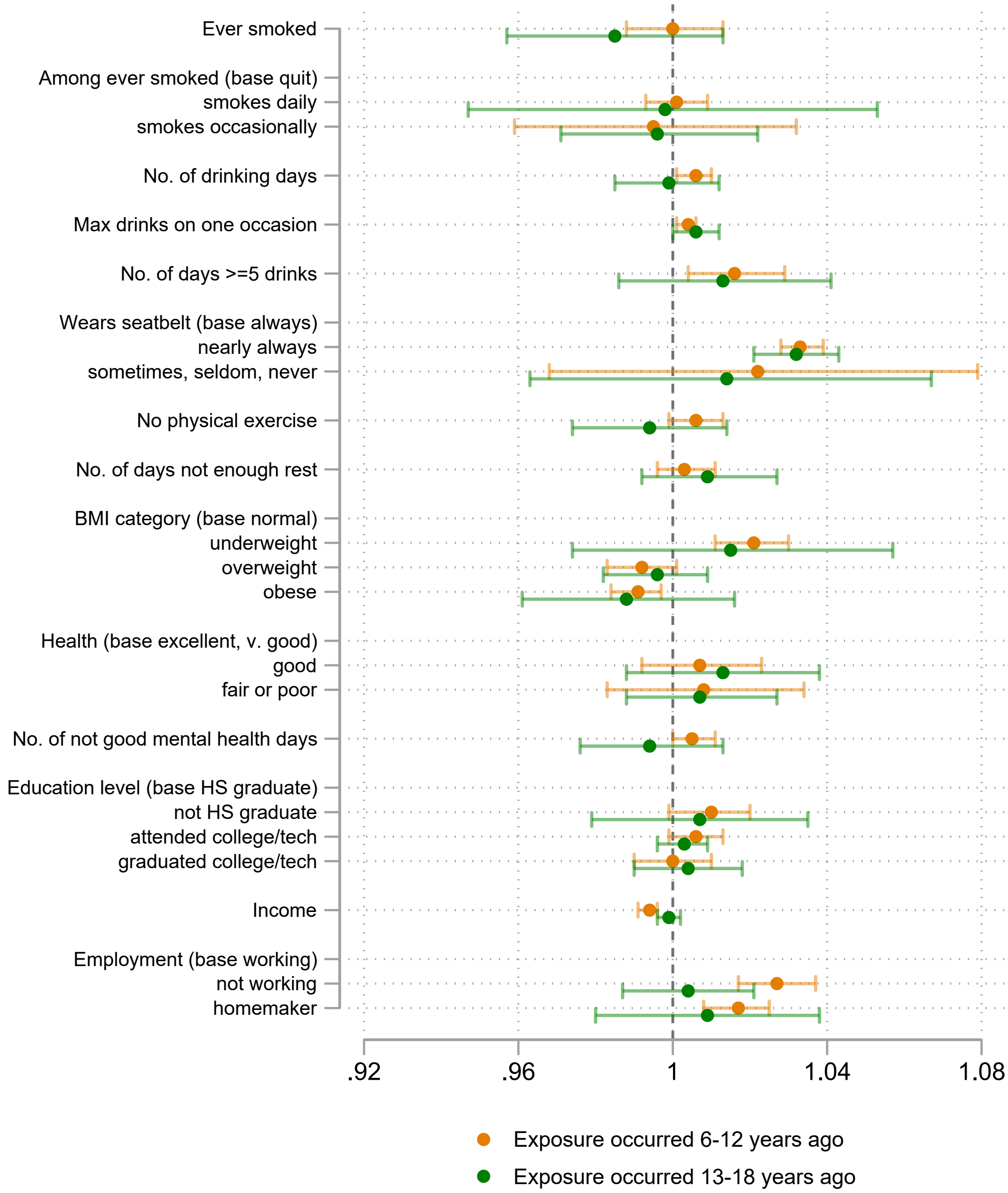
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.9,1.1] to enhance readability. One alternative definition considers incidents only if a death occurs and defines exposure as the total deaths. The other definition considers incidents only if one occurs during a school day or during school extra-curricular events.

Figure 7: Estimates of effects of exposure (using alternative definitions) to school shootings on outcomes among men



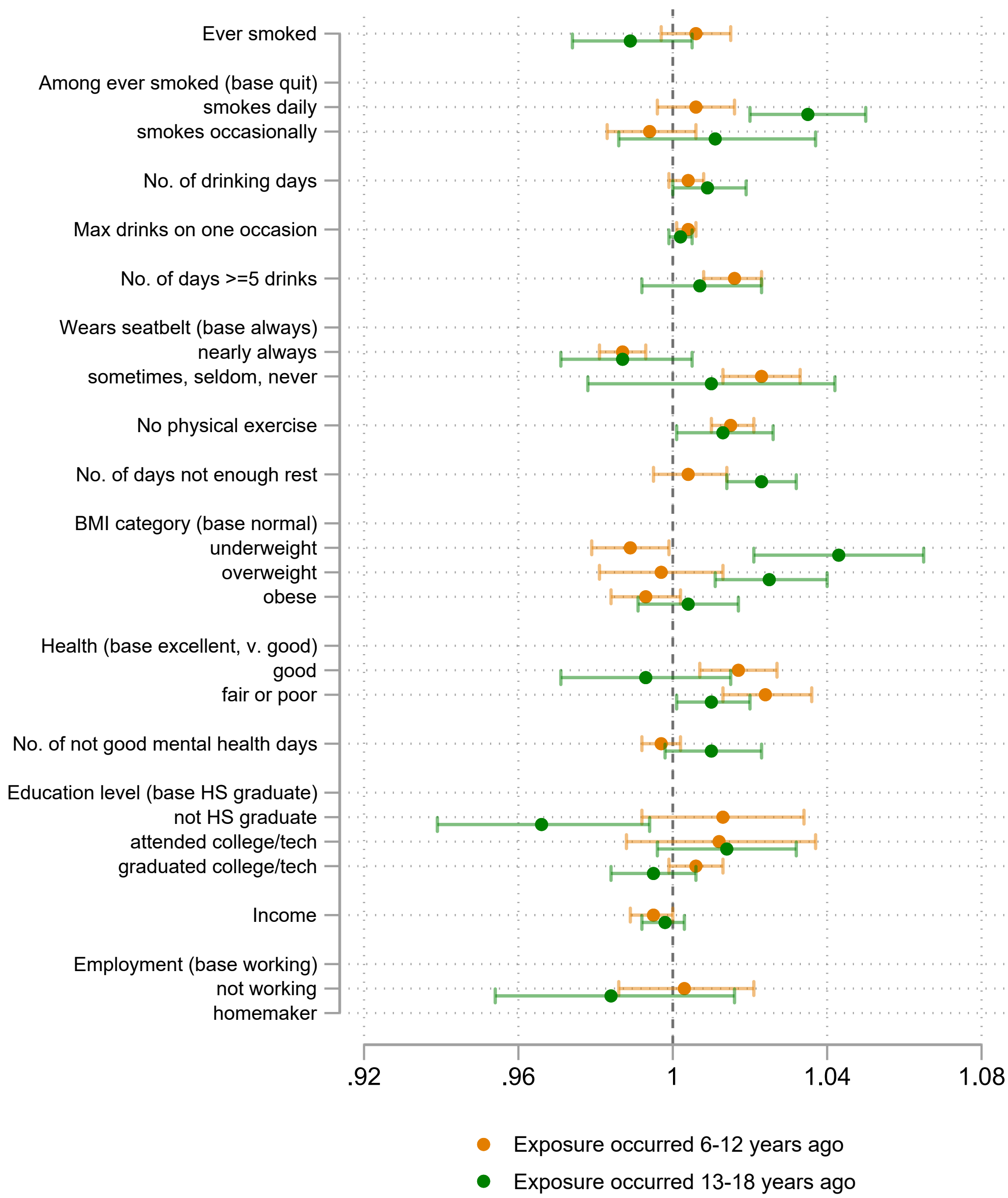
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.9,1.1] to enhance readability. One alternative definition considers incidents only if a death occurs and defines exposure as the total deaths. The other definition considers incidents only if one occurs during a school day or during school extra-curricular events.

Figure 8: Estimates of effects of exposure to school shootings on outcomes among women stratified by duration since exposure



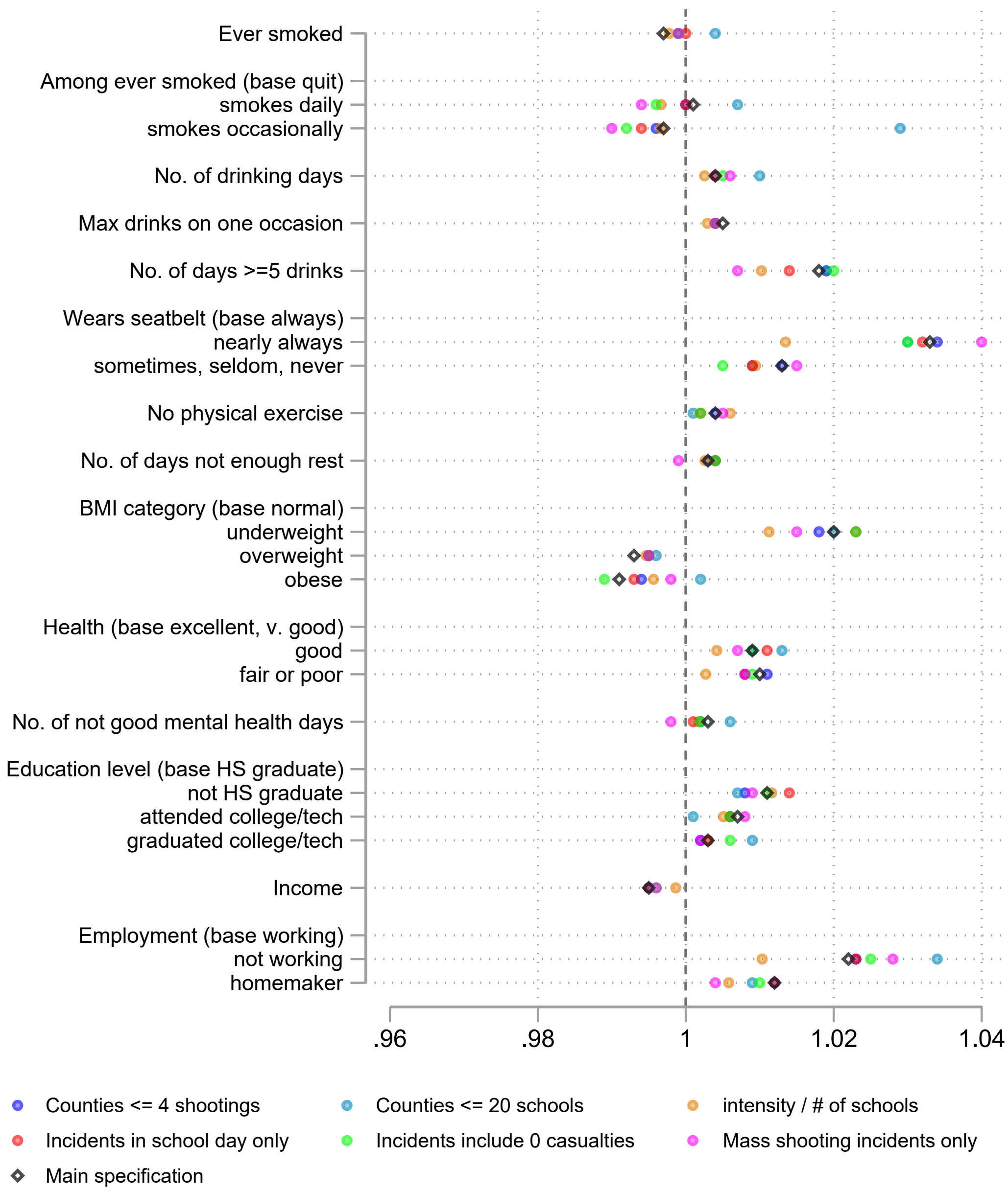
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.9,1.1] to enhance readability. One stratum considers incidents that occurred 6-12 years prior to the survey. The other stratum considers incidents that occurred 13-18 years prior to the survey.

Figure 9: Estimates of effects of exposure to school shootings on outcomes among men stratified by duration since exposure



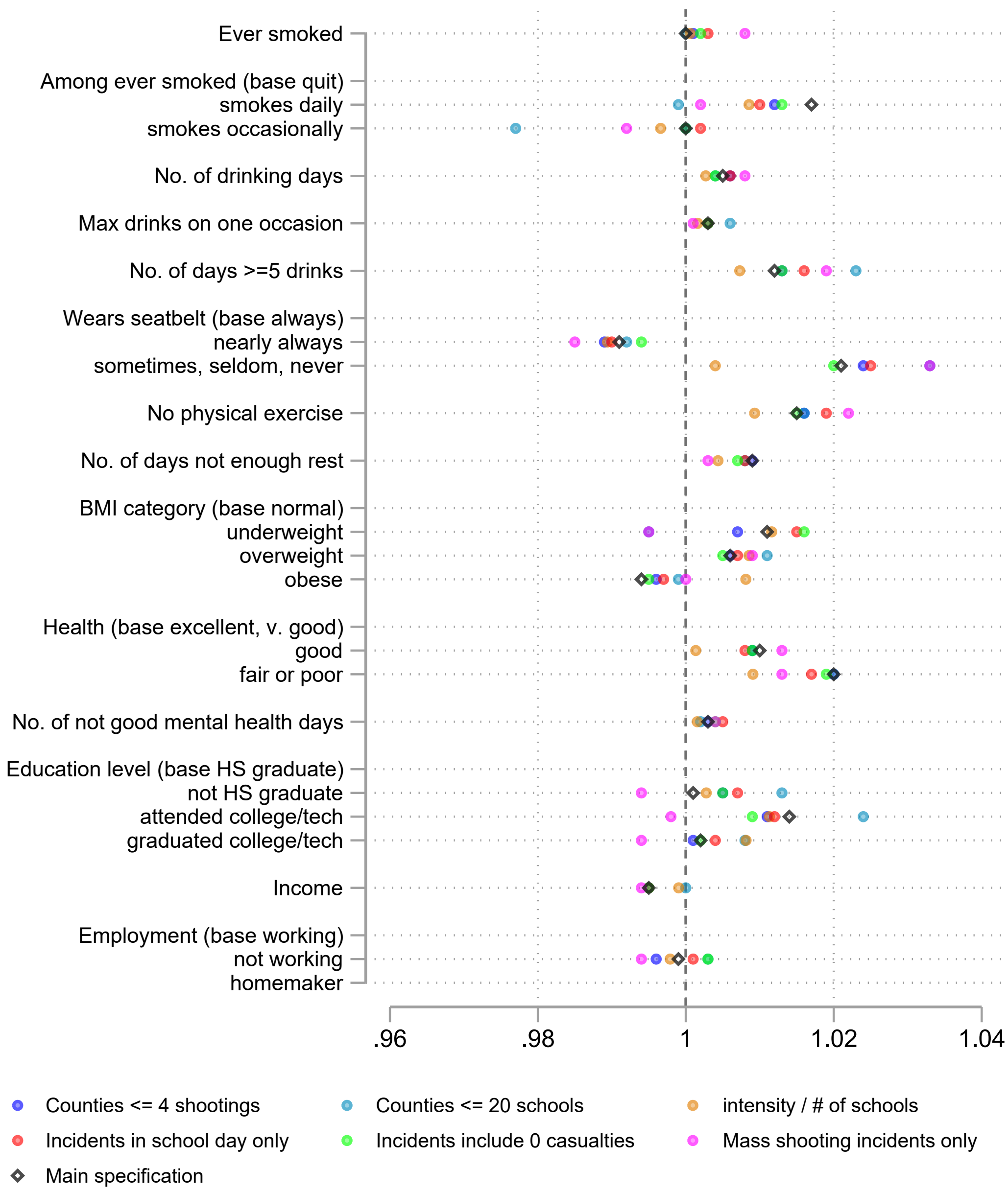
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.9,1.1] to enhance readability. One stratum considers incidents that occurred 6-12 years prior to the survey. The other stratum considers incidents that occurred 13-18 years prior to the survey.

Figure 10: Estimates of effects of exposure to school shootings on outcomes among women in specification check samples and definitions



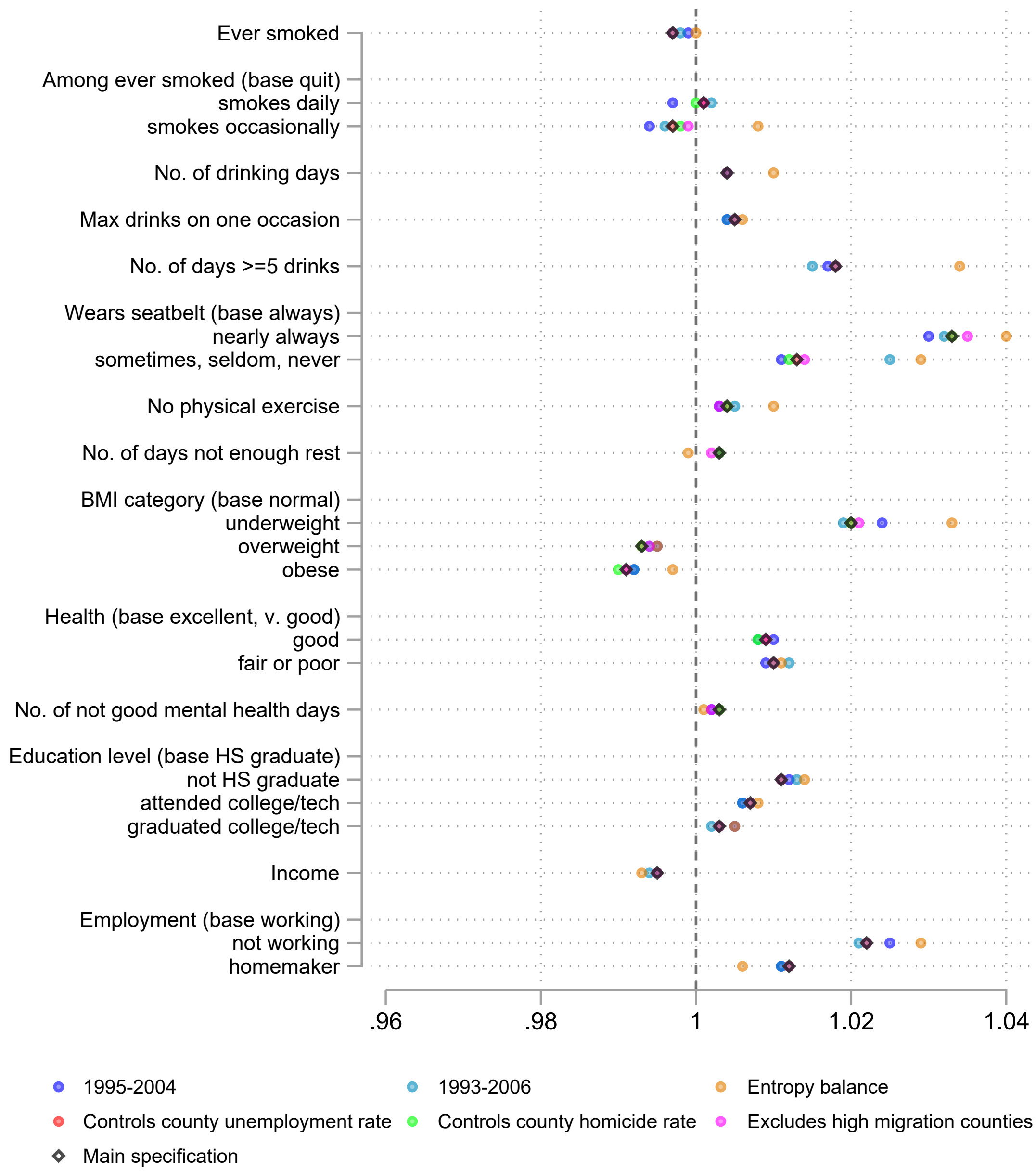
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients are reported.

Figure 11: Estimates of effects of exposure to school shootings on outcomes among men in specification check samples and definitions



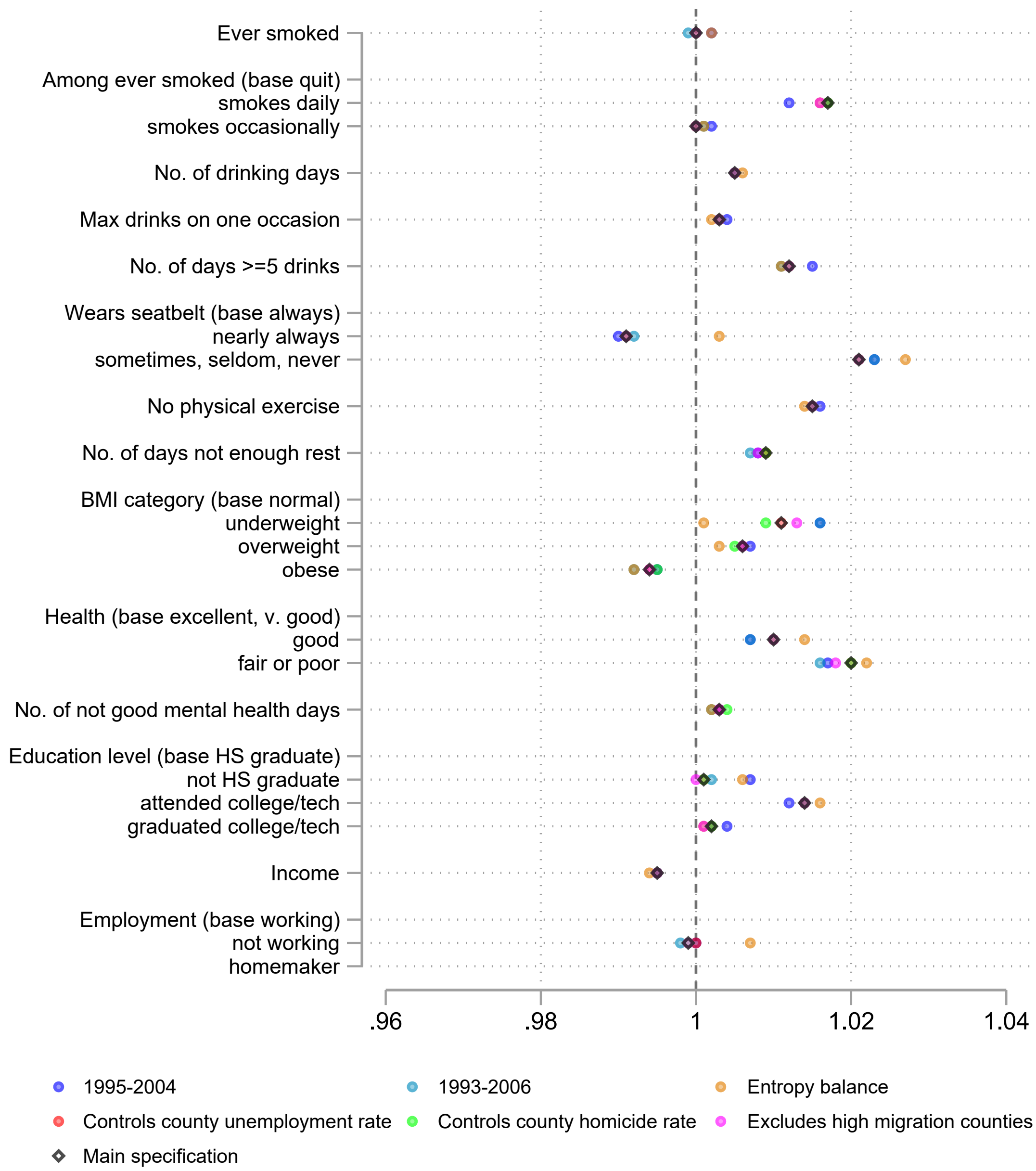
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients are reported.

Figure 12: Estimates of effects of exposure to school shootings on outcomes among women in specification check samples and definitions



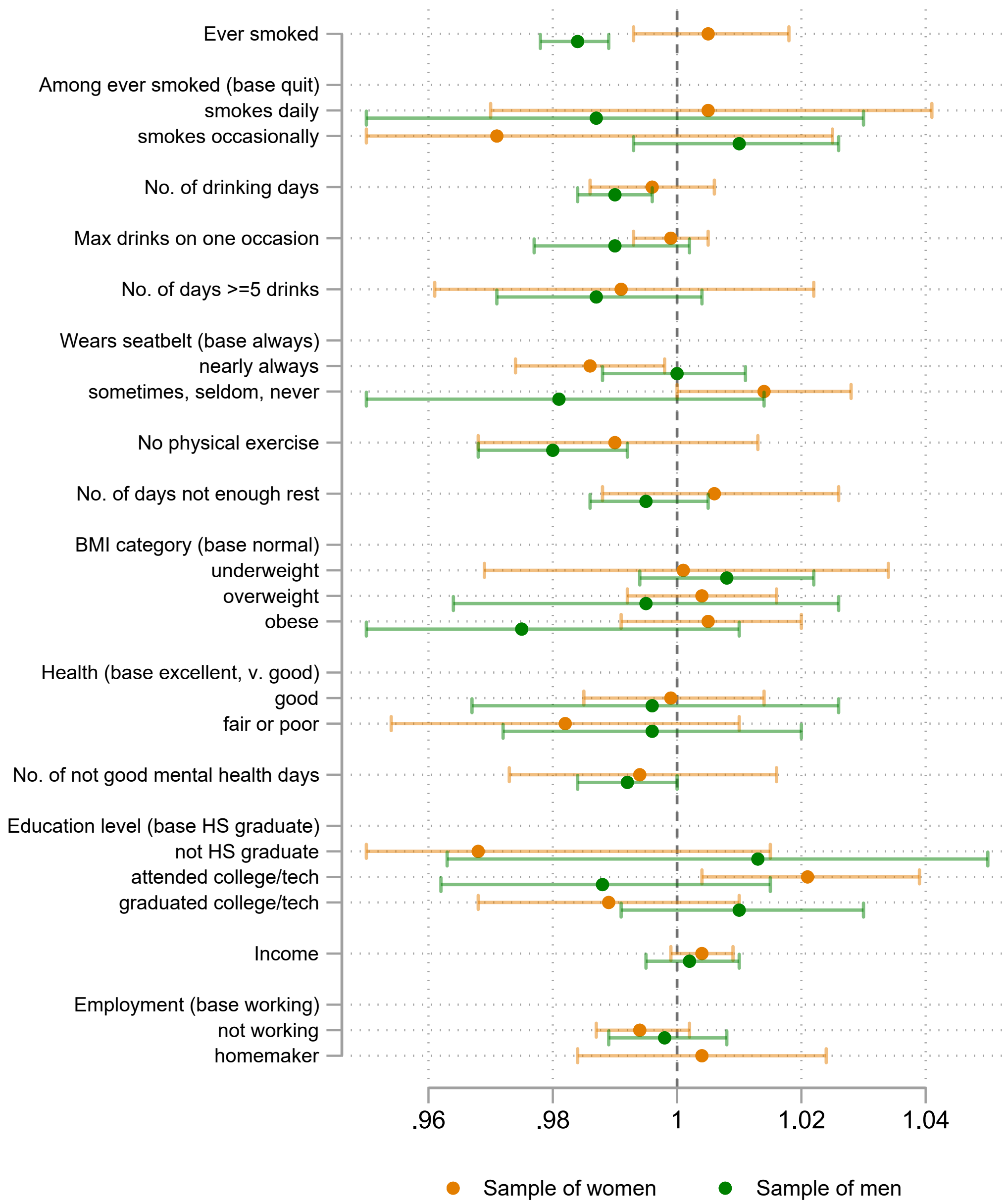
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients are reported.

Figure 13: Estimates of effects of exposure to school shootings on outcomes among men in specification check samples and definitions



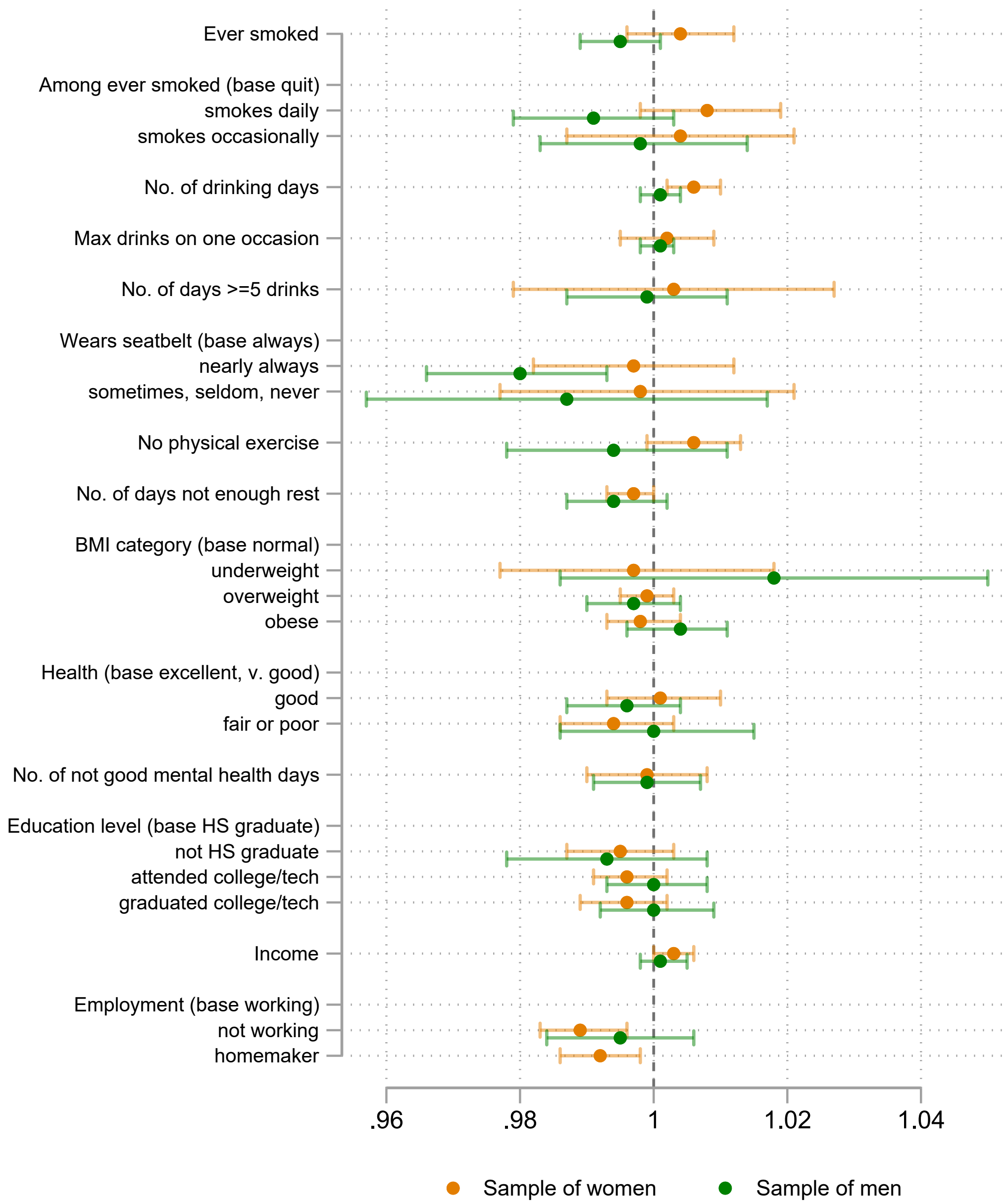
Note: The primary sample consists of individuals living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients are reported.

Figure 14: Estimates of effects of exposure to school shootings on outcomes with placebo treatment assigned to individuals 4-10 years old at the time of the shooting incident



Note: The sample consists of individuals ages 20-29 years old living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.95,1.05] to enhance readability.

Figure 15: Estimates of effects of exposure to school shootings on outcomes with placebo treatment assigned to individuals 20-26 years old at the time of the shooting incident



Note: The sample consists of individuals ages 28-37 years old living in 1402 counties from 2003-2012 BRFSS. Covariates include indicators for age, race and ethnicity, county and year, and duration. Exponentiated coefficients along with cluster-adjusted p-values for statistical significance and associated 95% confidence intervals are reported. Confidence limits are bottom and top coded in [0.95,1.05] to enhance readability.