



Published in final edited form as:

J Epidemiol Community Health. ; 80(6): 393–400. doi:10.1136/jech-2025-225138.

LAYOFFS IN AUTOMOBILE MANUFACTURING AND MORTALITY AMONG REMAINING WORKERS

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Abstract

Background. Layoffs may affect the health of those who lose their jobs as well as those who remain employed. Existing studies have found that remaining employed through layoffs is associated with poorer mental health in the short-term, but the implications for long-term outcomes such as mortality remain unclear.

Methods. We estimated adjusted hazard ratios for all-cause and cause-specific mortality associated with layoff intensity while employed among white men, non-white men, and women in a cohort of 9,761 autoworkers who worked at one of three plants in Michigan between the years 1950 and 1980. We defined layoff intensity as the number of layoff months endured while employed divided by duration of employment. We identified layoff months as those in which the percentage of the workforce leaving employment was 1.96 standard deviations above the predicted value from an autoregressive integrated moving average model.

Results. We found statistically significant associations among non-white men but not women or white men. Relative to layoff intensity below the first quartile, the adjusted hazard ratio associated with layoff intensity between the first and second quartiles was 1.35 (95% CI: 1.05, 1.74) for all-cause mortality among non-white men. The adjusted hazard ratios associated with layoff intensity between the second and third quartiles were 1.85 (95% CI: 1.08, 3.17) and 2.41 (95% CI: 1.00, 5.84) for death due to all cancers and lung cancer, respectively.

Conclusion. Layoffs endured while employed may lead to early mortality among non-white male employees. Reducing workforce instability may reduce racial disparities in health.

INTRODUCTION

Layoffs, workforce reductions, and organizational downsizing are commonly accepted management practices in the United States (US).¹ The annual rate of job losses due to layoffs in the US between 1967 and 2019 ranged from 200 thousand to nearly 3 million

per year. Exceptional years aside, the rate of layoffs has generally remained stable; between 2022 and 2025, the average rate was 1.2 million per year.² Since the nature, conditions, and availability of work constitute important social determinants of health, the potential health effects of job loss have been the focus of research for decades.³ The vast majority of these studies focus on the health of those who lost their jobs, but layoffs may affect the health of the layoff survivors, those who remain employed through the layoff, as well.

Acute decreases in the number of workers employed by an organization constitute structural changes in the workplace social environment. Workforce reductions often mark the adoption of new forms of work and workplace relationships in which employers may expect greater economic output from the remaining employees.⁴ Technological transitions involving mechanization, computerization, and automation can facilitate increases in productivity, but adoption of these innovations often leads to greater employer control over the pace of work and reduced need for skilled labor.⁵ Hence, workers who remain employed following layoff events may have less autonomy and collective bargaining power but greater job strain. While these dynamics were historically more pertinent to “blue-collar” occupations, recent developments in artificial intelligence have rendered “white-collar” workers susceptible to technological displacement and work intensification as well.⁶ In any organization, the size of the workforce, the workplace environment, and the nature of work are necessarily dynamic. However, when changes are acute and frequent, they can harm the long-term health of workers.

Past studies of the potential health effects of layoffs on those who remain employed are few in number but have found that layoff survivors experienced greater rates of depression, anxiety, difficulty sleeping, musculoskeletal symptoms, and substance use compared to those who did not endure layoffs while employed.^{7,8} Reductions in the number of employees of a company or organization have also been linked to increased mortality among those remaining.⁹ Much of the existing research on the health effects of surviving layoffs has been limited to European workers.^{9–11} However, between-country heterogeneity in the association between layoffs and worker health is likely because contextual factors such as labor/social policies, strength of unions, quality of unemployment benefits, and cultural attitudes towards work may be important effect measure modifiers.¹² Within countries, the association likely varies across demographic groups defined by race, ethnicity, and gender as those characteristics often influence or define social position and the psycho/sociocultural mechanisms that link social exposures to health.¹³

The mechanisms underlying the relationship between layoffs and health may be behavioral and biochemical. Existing research shows that enduring layoffs while employed is associated with the adoption of harmful coping strategies such as tobacco smoking and substance use.^{8,11} In addition to affecting behaviors, social exposures can also interact with underlying health status to activate and non-specifically reinforce pathophysiologic pathways. Even in the absence of clinical disease, exposure to stressful events may lead to biochemical changes with effects that may not be apparent for long latent periods. Past studies of organizational instability show that workers who survived precarious employment, downsizing, and reorganizations experienced greater dysregulation of stress response signals and cardiometabolic markers than those in stable employment.^{14,15} Stressful exposures may

have lasting cumulative effects, so individuals in disadvantaged sociodemographic groups may be particularly vulnerable to the potential harms of subsequent exposure.¹⁶

The present study used data from the United Auto Workers-General Motors (UAW-GM) cohort to examine the associations between layoff intensity while employed and the rate of death due to all causes, cardiovascular disease, cancer, and external causes over a follow-up period of over six decades, spanning the years from 1950 to 2015. In addition to these three broadly defined outcomes, we also investigated more specific causes of death including coronary heart disease, acute myocardial infarction, lung cancer, and colorectal cancer. We investigated broadly and specifically defined outcomes because layoff events may affect the health of survivors through various nonspecific causal pathways.¹⁷ To account for effect measure modification by race and gender, we computed subgroup-specific adjusted hazard ratios. In the context of global labor/supply chains, weakening labor unions, and increasing automation, layoffs may occur more frequently and unpredictably in the US. Understanding the long-term health effects of layoff events on those who remain employed through those events can help improve our understanding of how workforce instability may harm worker health and exacerbate health disparities.

METHODS

We used data on a subset of 9,761 employees from the UAW-GM cohort, which included all employees who worked at least three years at one of three plants in Michigan before the early 1980s. We focused our analysis to those employed between 1950 and 1980, the decades following World War 2 during which we had data on all individuals employed at these plants. Follow-up for cause-specific mortality began three years after hire and ended on date of death, the oldest observed age of death, or December 31, 2015, whichever came first. Figure S1 illustrates ten examples of how an individual may contribute person-time to the study. The data included information abstracted from employer and union records. Extensive details on the cohort are published elsewhere.^{18,19} Outcome data were obtained by linkage to the National Death Index.²⁰ Underlying cause of death was distinguished using the ninth and tenth revisions of the International Classification of Diseases (see supplemental materials).^{21,22}

Layoffs

For each plant, we identified months between the years 1950 and 1980 in which layoff events took place by identifying months in which the percentage of the workforce leaving employment was 1.96 standard deviations greater than the predicted value from the best-fitting autoregressive integrated moving average (ARIMA) model.²³ Since the monthly employment data are expressed as percentages, they are bounded between 0 and 1. So before fitting the data to ARIMA models, we first applied the probit transformation. We selected the degree of differencing by inspecting the differenced time series. Differencing and seasonal differencing (12 months) resulted in a time series with no discernible trends or cycles (see Figure S2). We determined the orders of the moving average (MA) and autoregressive (AR) components by fitting all possible ARIMA models with MA and AR orders less than or equal to the maxima implied by the autocorrelation and partial

autocorrelation functions. We used the MA and AR orders which yielded the lowest Akaike Information Criterion (AIC).²⁴

For a particular individual under follow-up, we classified a month of employment as exposed to layoff if the individual did not leave employment in that month. For our calculations, we assumed that all months were 30.4 days in length. Individuals who left work during a layoff month were not classified as having endured that layoff while employed. In this way, we limit bias due to selection out of the workforce during layoff events resulting from health-related factors. We defined time-varying average layoff intensity for each person-year as the number of layoff months endured while employed divided by duration of employment.

Statistical analysis

We examined the association between layoff intensity while employed and mortality due to all causes, cardiovascular disease, coronary heart disease, acute myocardial infarction, all cancers, lung cancer, colorectal cancer, and external causes using Cox proportional hazards models. We handled ties in event times using Efron's method, indexed the risk sets by age, and stratified the baseline hazard by plant and categories of calendar year, date of hire, employment status, and duration of employment formed using the quartiles of the respective variables among those who experienced the outcome of interest. To account for effect measure modification, we fitted separate models for white men, non-white men, and women. There were insufficient outcomes among non-white women for further stratification. We treated race as a binary variable because more than 99% of the non-white individuals in the cohort were Black so distinguishing those who were neither white nor Black was analytically impossible.

We conducted analyses with layoff intensity represented both categorically and as a restricted penalized cubic spline.²⁵ When representing layoff intensity categorically, we used cut points based on quartiles of non-zero exposure among those who experienced the outcome of interest for all outcomes except colorectal cancer and external causes. For colorectal cancer, we used tertiles for white men and the median for non-white men and women to accommodate the smaller number of observed deaths. For external causes among women, we also used the median. We permitted the penalized splines 4 degrees of freedom for all outcomes except colorectal cancer and restricted them to linearity for values less than the minimum observed exposure or greater than the 90th percentile. For colorectal cancer, we selected the degrees of freedom using AIC.²⁶ In the analyses of categorical layoff intensity, the lowest category (quarter, third, or half) was the reference category. In the analyses of continuous layoff intensity, the reference value was 0 layoffs per year.

Sensitivity analyses

Our method of operationalizing exposure to layoffs is novel, so we conducted sensitivity analyses to verify that we would not expect a spurious association *a priori*. In particular, we conducted simulations to assess whether the distribution of studentized log-hazard ratios would be centered on the null value of zero in analyses where layoff intensity is replaced with a similarly defined random pseudo-exposure. We generated the pseudo-exposures by

sampling months randomly from the months spanned by the employment record. The number of random months we sampled for each plant was equal to the number of layoff months we detected using the method described above. For each employee, we computed the average intensity of random months by dividing the number of random months while employed by the duration of employment. Apart from fitting the exposure-response models using categorical intensity of random months rather than layoff intensity, all other aspects of the modeling were identical to those in the main analyses of categorical exposure. We contrasted the observed test statistics to the randomization null distribution approximated using 1000 simulations and computed a randomization *P*-value, that is, how often the randomization procedure produced test statistics that were at least as extreme as the observed test statistic.²⁷

All analyses were performed in R version 4.2.2. ARIMA models were fitted using built-in functions from the 'stats' package.²⁸ Cox proportional hazards models were fitted using the 'survival' package.²⁹

ETHICS APPROVAL STATEMENT

This study was approved by the Committee for the Protection of Human Subjects at the University of California, Berkeley, which waived the requirement for obtaining informed consent for this records-based study.

RESULTS

Table 1 presents summary statistics for the study population of 7,622 white men, 1,236 non-white men, and 903 women. The median number of layoffs endured while employed was equivalent for white and non-white men, but white men tended to work for fewer years than non-white men so median layoff intensity was greater among the former group. Women endured a greater median number of layoffs, layoff intensity, and duration of employment than men. Women also tended to have a greater age at hire and age at death than men. Non-white men had the lowest median age at death. Most women were employed in Plant 2 and most non-white men were employed at Plant 1.

Figure 1 shows the monthly time series of the percentage of the workforce leaving employment by plant. Our method detected a similar number of layoff months in each plant: 11, 12, and 9 in Plants 1, 2, and 3, respectively. The order of the AR component, degree of differencing, and order of the MA component of the ARIMA model for each plant are presented in Table S1. The maximum percent leaving employment in a month was greatest in Plant 1 at 5.0% in September of 1975 followed by Plant 2 at 2.8% in January of 1961. In Plant 3, the maximum was 1.8% in September of 1976. Reports published in the New York Times on ongoing and planned layoffs in the automotive industry in those months partially corroborated these findings.^{30,31}

Figure 2, Figure 3, and Figure 4 show the subgroup-specific adjusted hazard ratio estimates associated with categorical layoff intensity. Table S2, Table S3, and Table S4 present the numeric results. The adjusted hazard ratios among white men were generally close to the null for all of the causes of death we investigated. Among non-white men, the adjusted

hazard ratio for all-cause mortality was elevated and greatest among those with layoff intensity in the second quarter at 1.35 (1.05, 1.74). The adjusted hazard ratios among non-white men for cancer and lung cancer associated with layoff intensity in the third quarter were also elevated at 1.85 (1.08, 3.17) and 2.41 (1.00, 5.84), respectively.

Figure S4, Figure S5, and Figure S6 show the penalized spline fits associated with layoff intensity among white men, non-white men, and women, respectively. As in the analyses of categorical layoff intensity, there were no statistically significant associations between layoff intensity while employed and mortality among white men. Among non-white men, the adjusted hazard ratios for death due to cancer associated with layoff intensity while employed reached a maximum of 3.0 at 5.3 layoffs per 10 years of employment. That for death due to lung cancer reached a maximum of 3.1 at 6.9 layoffs per 10 years of employment. The adjusted hazard ratio for death due to external causes was greater than 1 for values of layoff intensity greater than 3.7 layoffs per 10 years and reached statistical significance at 8.4 layoffs per 10 years. The analyses of splined exposure among women were mixed and subject to large standard errors.

The distributions of the test statistics produced in the randomization tests were approximately normal and centered on the theoretical null value for all of the outcome-exposure associations that we investigated (see Figure S7, Figure S8, and Figure S9). Among non-white men, the observed test statistics were large in magnitude relative to the randomization distributions for death due to all causes, cancer, and lung cancer.

DISCUSSION

Our study adds to the sparse literature on the potential effects of layoffs and workforce reductions on those who remain at work. We found evidence of positive associations between layoff intensity while employed and mortality outcomes among non-white men but not women or white men. Most existing studies on the health of layoff survivors did not investigate mortality but outcomes such as non-fatal injuries, mental health, sick leave taken, and hospitalization.^{8,32} However, other studies that investigated mortality outcomes reported elevated risk among layoff survivors.^{33,34} In one cohort study of approximately 24 thousand municipal employees from 10 Finnish towns, Vahtera and colleagues found that employees in departments that underwent organizational downsizing in the 1990s experienced a two-fold greater hazard of death over a 7.5-year follow-up period than those who worked in departments that did not downsize.⁹ Our estimates may have differed in magnitude from these findings because of differences in country, setting, duration of follow-up, and magnitude of layoffs.

Since we hypothesized that greater layoff intensity while employed would have greater harms on health, we were surprised to find that there was no clear evidence of an association among women or white men and that among non-white men, associations were generally not monotonic. The healthy worker effect may partly explain the lack of monotonicity: individuals who are less susceptible to the potential effects of layoffs may remain employed for a longer period of time.³⁵ This potential source of bias motivated our inclusion of duration of employment in the adjustment set. However, since duration of employment may

be on the causal path between layoff intensity while employed and mortality, conditioning on that variable may also result in bias. Future analyses may use g-methods to better account for this complex source of bias.³⁵

The contrast in the findings by race was consistent with the body of evidence on the disparate harms of stressful social exposures on individuals occupying disadvantaged racial groups.^{36–38} In the US, racial groups are subject to a hierarchy of power that manifests on multiple levels, including personal, interpersonal/cultural, and institutional/structural. If white men in our study population had greater access to protective structural resources and cultural factors, then they may have also experienced a lower susceptibility to the harmful effects of layoffs.

Among non-white men, our estimates of the adjusted hazard ratio associated with layoff intensity while employed was greatest for lung cancer. Although this finding suggests that smoking may have been an important mediator in this study population, we emphasize that the potential causal mechanisms underlying the associations between social exposures and health may vary substantially over time and across different contexts. Social exposures have the capacity to act through various mediating mechanisms and affect diseases of diverse etiologies. Hence, the upstream exposures should be the target of amelioration rather than their intervening mechanisms because the elimination of one mediator could result in its replacement by another.^{17,39}

Since there were far fewer women in this study population than men, and very few non-white women, there were insufficient deaths to permit estimation of adjusted hazard ratios among women with stratification by race. Regrettably, this may obfuscate important differences in the association among white versus non-white women. Future analyses of study populations with larger numbers of women may provide valuable insight into how gender, race, and their intersection may play a role in the relationship between layoffs and the health of those who remain employed.

Among studies of surviving layoffs, our approach to identifying and defining exposure to layoffs while employed is unique. Past studies often treat surviving layoff as a binary variable, but we were able to take advantage of the long study period and longitudinal employment data to investigate the potential effects of layoff intensity on survivors as a categorical time-varying exposure. Our exposure definition captured variation in layoff intensity over the entire course of a worker's employment. Past studies often identified layoffs from employment data using fixed thresholds on the number or percentage of workers leaving employment for any reason over a particular time interval. Months or years in which the number or proportion exceeded that threshold would be identified as layoffs or workforce reductions.^{8,32}

Our approach to identifying layoffs insulated us from the need to specify fixed thresholds directly on the employment data. Instead, we specified a threshold in terms of the spread of the predicted distribution of percent leaving work. Our choice of 1.96 standard deviations means that the percent of workers leaving employment in the identified months would be greater than the upper bound of the 95% prediction interval. This choice of threshold is

comparable to that of another study in which the exposure of interest was defined using the residuals of a time series model.²³ In that study, the investigators used a threshold of 2 standard deviations. By identifying layoff months in this manner, we were able to account for trends, cyclicalities, and autocorrelation.⁴⁰ A small layoff during a period of workforce stability would be detected as well as a large layoff during a period of instability. Furthermore, this method also avoids the need to specify cooling periods to prevent over-counting a single layoff event that spans multiple time periods.

As in other studies of the potential health impact of surviving layoffs, we did not have data on the reason individuals left employment. We identified layoff months based on separations due to all reasons. It is possible that the layoff months we identified were the result of unexpectedly high rates of voluntary separations rather than terminations at the discretion of the employer. However, if these months were the result of unexpectedly high rates of retirement, we would expect the age of leavers to be associated with the layoff months we identified. However, we found no evidence that the age of leavers in those months was different from the age of leavers in other months in the study period (correlation less than 0.01). Furthermore, newspaper reports from the time appear to partially corroborate our layoff identification strategy.^{30,31}

Our analysis found evidence that non-white employees at three automobile manufacturing plants who endured greater layoff intensity while employed experienced a greater hazard of death and in particular, death due to lung cancer. The study population we investigated consisted of “blue-collar” workers who were employed between 1950 and 1980, but our findings may have implications for “white-collar” professions as well. Since the 1960s, assembly line workers have faced technological displacement and work intensification due to automation. Today, the increasing presence of artificial intelligence may have a similar impact on professions that have historically resisted automation.⁶ The extent to which layoffs in different industries and organizations will affect the health of remaining employees and contribute to racial disparities in health will depend on contextual factors such as the general economic milieu, strength of employee protections, availability of unemployment benefits, cultural attitudes towards work, and how these factors operate in a racialized and gendered society.¹² Future research on potentially protective contextual factors and other upstream determinants offers the prospect of improving worker health and reducing disparities during periods of economic and organizational instability.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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KEY MESSAGES**What is already known on this subject**

Few studies have investigated the potential health impact of layoffs on the employees who remain employed. Existing studies have found associations linking surviving layoff and poorer mental health, but the implications of these short-term health effects for long-term mortality remains unclear.

What this study adds

We find evidence of an association between exposure to layoffs while employed and mortality due to all causes and cancer among non-white men over 65 years of follow-up. The associations between layoff intensity while employed and mortality were greater among non-white men than women or white men.

How this study might affect research, practice, or policy

Workers who remain employed following a layoff event may still experience negative health consequences related to that layoff event. Interventions that mitigate workforce instability and its downstream effects may protect the long-term health of individuals after they leave the workforce and reduce racial disparities in health.

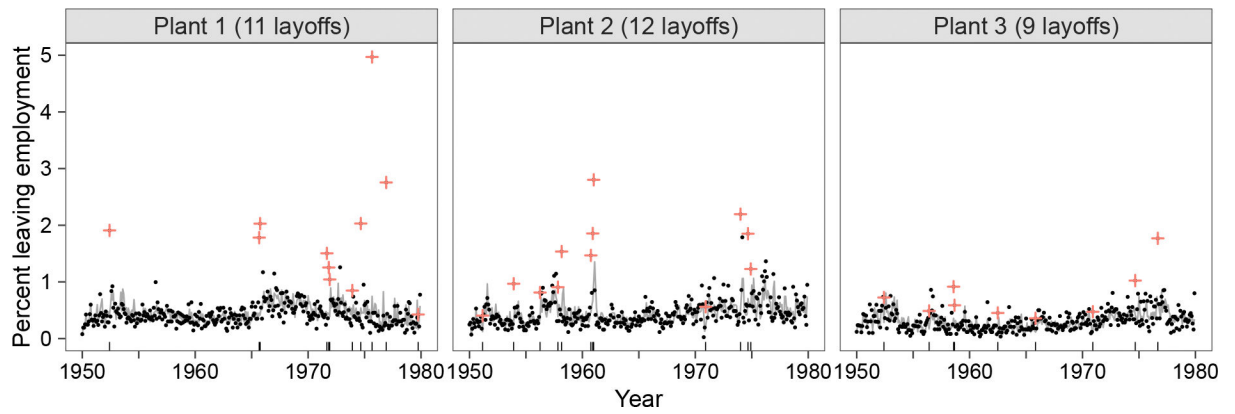


Figure 1.

Percentage of the workforce leaving employment in each month. The black points represent the observed data, the grey line segments show the model-based prediction and the orange crosses indicate months identified as layoff months. Layoff months are also indicated on the horizontal axis by the rug plot.

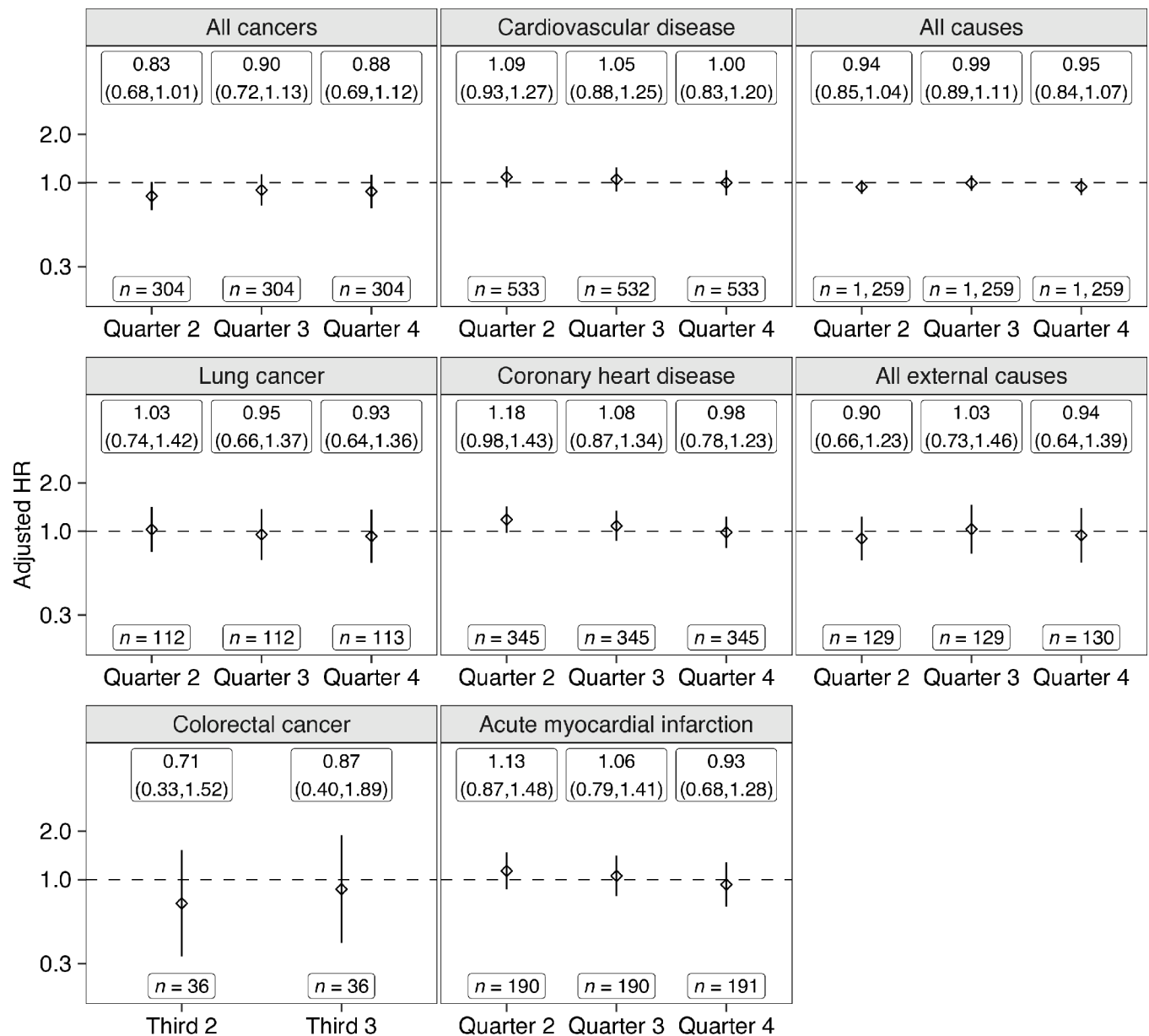


Figure 2.

Adjusted HRs (95% CIs) associated with layoff intensity while employed among white men. Risk sets were indexed by age, and the baseline hazard was stratified by plant, employment status and categories of calendar year, date of hire and duration of employment. For legibility, layoff intensity is expressed per 10 years of employment.

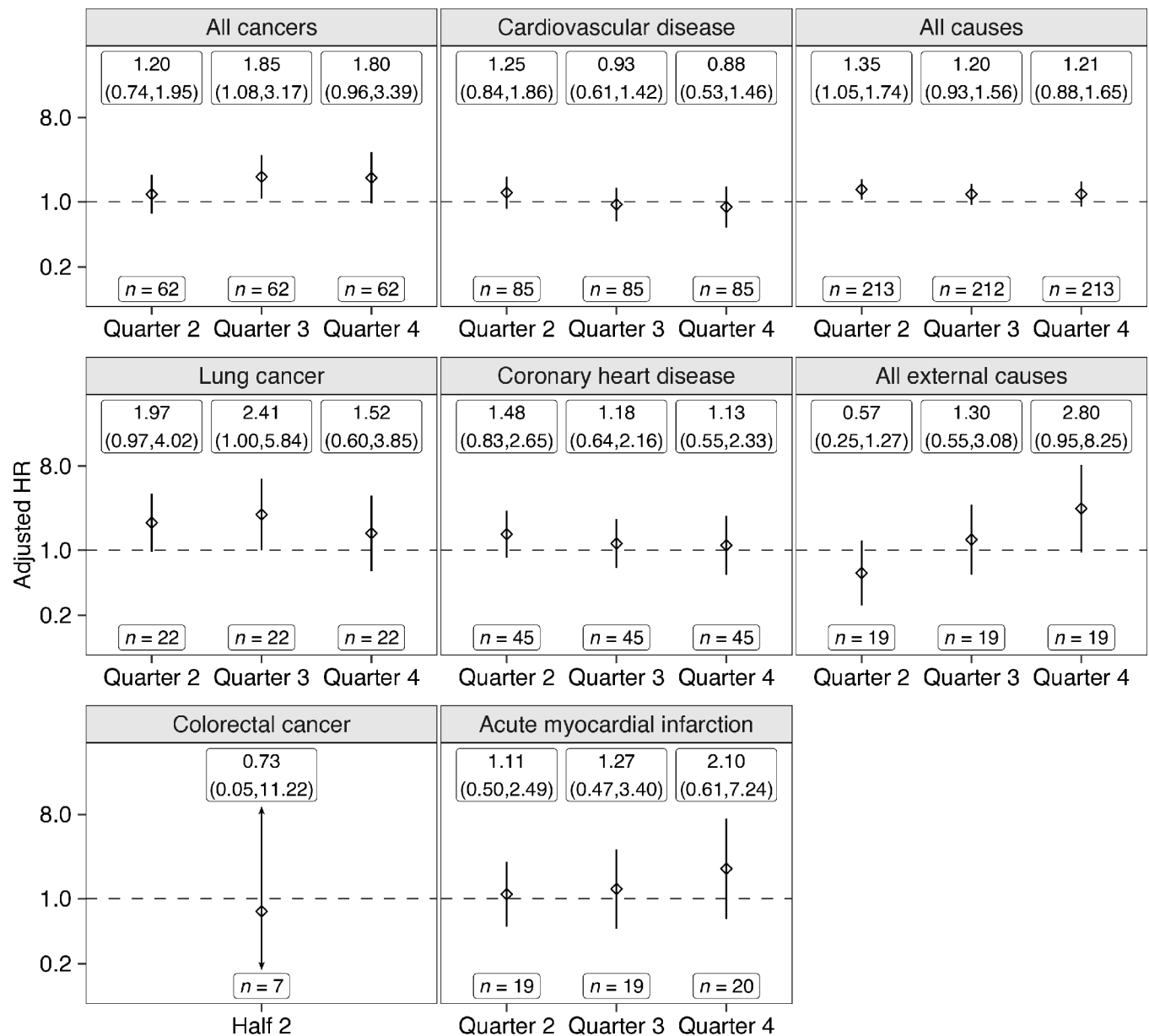
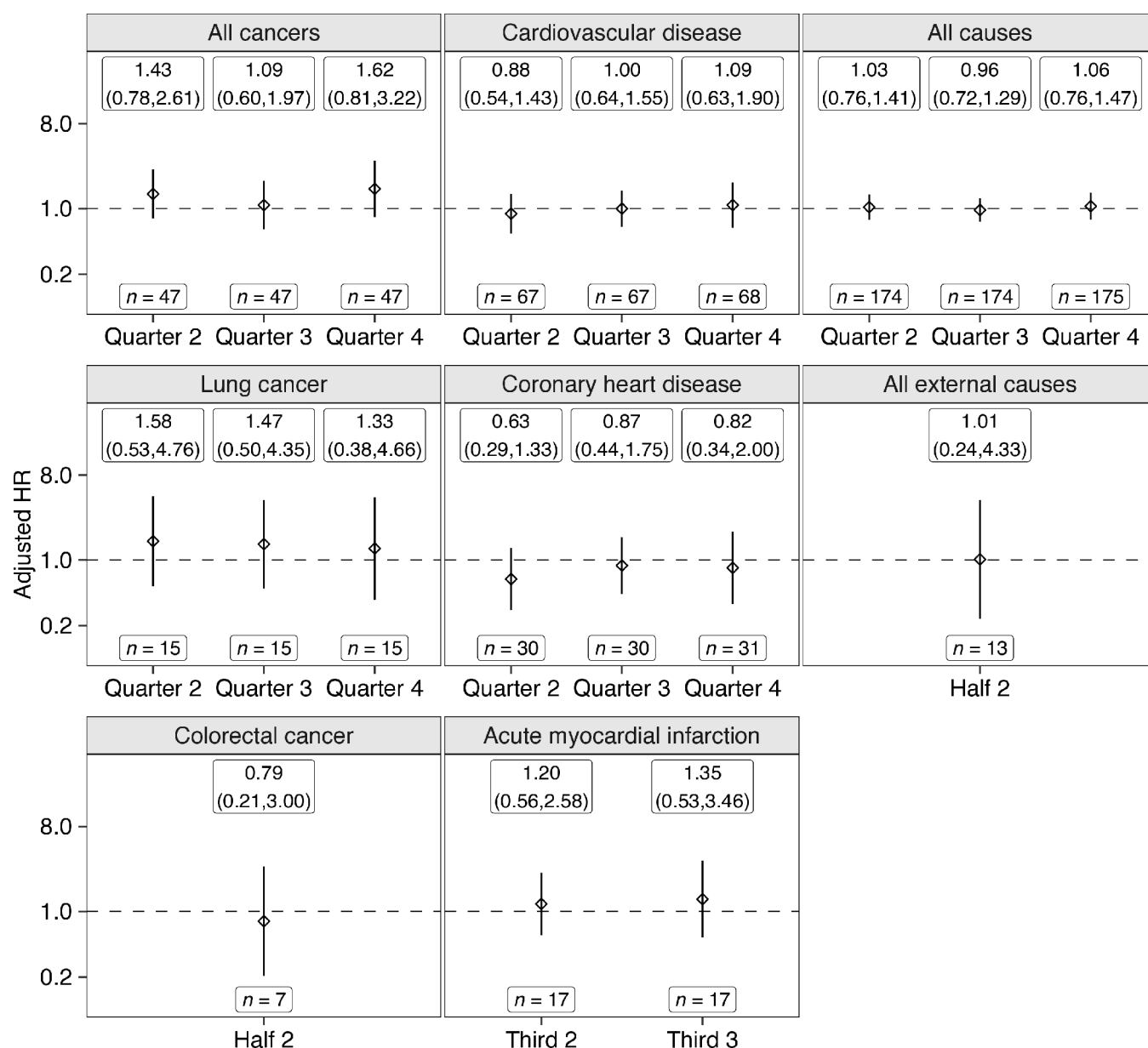


Figure 3.

Adjusted HRs (95% CIs) associated with layoff intensity while employed among non-white men. Risk sets were indexed by age, and the baseline hazard was stratified by plant, employment status and categories of calendar year, date of hire and duration of employment. For legibility, layoff intensity is expressed per 10 years of employment. Upper or lower limits of 95% CIs that extend beyond the plot area are indicated by an arrow.

**Figure 4.**

Adjusted HRs (95% CIs) associated with layoff intensity while employed among women. Risk sets were indexed by age, and the baseline hazard was stratified by plant, employment status and categories of calendar year, date of hire and duration of employment. For legibility, layoff intensity is expressed per 10 years of employment.

Table 1:

Study population characteristics for white men, non-white men, and women.

(a) Continuous covariates: median (first quartile, third quartile).

	Total	White men	Non-white men	Women
<i>N</i>	9,761	7,622	1,236	903
Layoffs experienced	3 (2, 7)	3 (2, 7)	3 (1, 6)	5 (2, 7)
Layoff intensity (per 10 years)	3.8 (2.5, 5.0)	3.8 (2.5, 4.9)	3.0 (1.5, 4.6)	4.4 (3.7, 5.6)
Year of birth	1927 (1917, 1940)	1928 (1917, 1940)	1929 (1920, 1940)	1921 (1916, 1929)
Year of hire	1954 (1952, 1965)	1955 (1952, 1964)	1959 (1952, 1966)	1953 (1952, 1955)
Age at hire	27.4 (21.1, 37.3)	26.5 (20.5, 37.4)	27.1 (22.1, 36.2)	33.3 (27.5, 37.7)
Duration of employment	7.8 (4.9, 14.8)	7.5 (4.8, 14.1)	8.6 (5.2, 16.0)	9.3 (5.1, 18.2)
Age left work	36.8 (28.2, 52.6)	35.6 (27.6, 52.5)	38.0 (29.4, 50.8)	44.1 (35.2, 54.5)
Years of follow-up	39.4 (25.4, 47.0)	39.9 (25.1, 47.6)	34.8 (22.9, 44.0)	41.1 (33.0, 49.3)
Age of death	69.3 (58.2, 78.9)	68.9 (58.2, 78.2)	65.5 (54.8, 75.0)	77.6 (66.8, 85.9)
Year of death	1992 (1979, 2002)	1990 (1978, 2002)	1993 (1980, 2002)	1998 (1989, 2005)