Union Burying Ground

Mortality, Mortality Inequities, and Sinking Labor Union Membership in the United States

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Background: Over the last several decades in the United States, socioeconomic life-expectancy inequities have increased 1–2 years. Declining labor-union density has fueled growing income inequities across classes and exacerbated racial income inequities. Using Panel Study of Income Dynamics (PSID) data, we examined the longitudinal union–mortality relationship and estimated whether declining union density has also exacerbated mortality inequities.

Methods: Our sample included respondents ages 25–66 to the 1979–2015 PSID with mortality follow-up through age 68 and year 2017. To address healthy-worker bias, we used the parametric g-formula. First, we estimated how a scenario setting all (versus none) of respondents' employed-person–years to union-member employed-person–years would have affected mortality incidence. Next, we examined gender, racial, and educational effect modification. Finally, we estimated how racial and educational mortality inequities would have changed if union-membership prevalence had remained at 1979 (vs. 2015) levels throughout follow-up.

Results: In the full sample (respondents = 23,022, observations = 146,681), the union scenario was associated with lower mortality incidence than the nonunion scenario (RR = 0.90, 95% CI = 0.80, 0.99; RD per 1,000 = −19, 95% CI = −37, −1). This protective association generally held across subgroups, although it was stronger among the more-educated. However, we found little evidence mortality inequities would have lessened if union membership had remained at 1979 levels.

Conclusions: To our knowledge, this was the first individual-level US-based study with repeated union-membership measurements to analyze the union–mortality relationship. We estimated a protective union–mortality association, but found little evidence declining union density has exacerbated mortality inequities; importantly, we did not incorporate contextual-level effects. See video abstract at, http://links.lww.com/EDE/B839.

Keywords: G-computation; Health disparities; Health inequities; Healthy-worker bias; Labor movement; Labor unions; Parametric g-formula; Social epidemiology

(*Epidemiology* 2021;32: 721–730)

Submitted June 27, 2020; accepted May 24, 2021

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Partial support for this research came from a Eunice Kennedy Shriver National Institute of Child Health and Human Development research infrastructure grant (P2CHD042828) to the Center for Studies in Demography & Ecology at the University of Washington. J.E.-G.'s and A.H.'s research was also partially supported by a grant from the National Institute on Aging of the National Institutes of Health (R01AG060011). Finally, J.E.-G.'s research was additionally support by a grant from the National Institute of Mental Health of the National Institutes of Health (T32MH013043).

The University of Washington IRB determined this study to be exempt from IRB review because it used publicly available, deidentified data. Nonetheless, the University of Washington IRB reviewed and approved the study because PSID requires such approval to access the restricted-use mortality data.

The Panel Study of Income Dynamics (PSID) is a publicly available survey conducted by the University of Michigan. PSID data can be obtained at https://psidonline.isr. umich.edu/, where readers can also find information about applying for access to the restricted-use mortality data used in our analyses. Code for implementing our parametric g-formula approach is available in eAppendix 3 (http://links.lww.com/EDE/B820).

The authors report no conflicts of interest.

SDC Supplemental digital content is available through direct URL citations in the HTML and PDF versions of this article (www.epidem.com).

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ISSN: 1044-3983/21/325-721
DOI: 10.1097/EDE.0000000000001386

Socioeconomic inequities in United States life expectancy have grown 1–2 years since the 1980s, driven by sinking relative incomes among the working class and a strengthening income–mortality relation caused by factors like rising costs of necessities.[1] Declining life expectancy among the marginalized has fueled the inequities[1] and contributed to overall declines in United States life expectancy annually from 2014 to 2017.[2] Meanwhile, racial life-expectancy inequities remain considerable due to persistent structural racism in the distribution and organization of social determinants of health like property, employment, healthcare, incarceration, and political power.[3,4] In 2017, Black life expectancy was 3.5 years shorter than White life expectancy.[5] Class inequities in income have grown simultaneously. For example, the ratio of the average earnings of those in the top 1% of the income distribution to the average earnings of those in the bottom 50% of the income distribution increased from 27 to 81 from 1980 to 2015.[6] Additionally, racial income inequities have remained largely unchanged since the 1960s. In 1968, the median Black family income was 57% of the median White family income; in 2016, the corresponding figure was 56%.[7]

Declining labor-union density—the proportion of workers belonging to unions—has exacerbated income inequities.[8] From 1983 to 2019, union density decreased from 20% to 10% among workers ages 16 and older, including from 27% to 11% among non-Hispanic Black workers, from 22% to 9% among workers with a high-school (HS) degree or less, and from 28% to 8% among workers in manufacturing industries (eAppendix 1; https://links.lww.com/EDE/B820).[9] Increasing business and political opposition to unionism following the 1970s and 1980s recessions—including a doubling of illegal firings and unfair labor practices by employers[10]—fueled the decline.[10,11] Concomitantly, certain union leaders pursued labor-management partnerships, quelling the direct action that propelled unions' historical successes and exacerbating membership losses[11]; from the 1970s to 2010s, strikes involving 1,000-plus workers decreased from 300 per year to fewer than 20.[12] Nonetheless, despite declining power and troubled organizing models, unions still mitigate inequities. Union-density increases bolster worker power over wages, including those of nonunionized workers, and promote union organizing for redistributive policies.[8,13,14] One study estimated 2007 wage inequities would have been one-fifth lower among women and one-third lower among men if union density had remained had 1973 levels[13]; another estimated 2007 Black-White wage inequity would have been 3% to 10% lower among men and 13% to 30% lower among women if union density had remained at 1973 levels.[15] Recent studies have similar findings.[8,16]

Union membership may also protect workers from premature mortality and reduce mortality inequities. For example, by augmenting workers' power relative to their employers, unionization may allow workers to demand better wages[8] and benefits,[17] as well as stronger protections from occupational hazards,[18–20] layoffs,[21] and discrimination,[18] lowering their risk of fatal chronic diseases, occupational injuries, and mental illnesses.[22] Moreover, by promoting solidarity among workers, unionization may alleviate feelings of alienation and powerlessness, reducing workers' risk of mental illnesses, drug use, and their sequelae, like suicide and fatal overdose.[23,24] Union membership's salutary effects may be greatest for Black and less-educated workers, a finding of prior studies on the union wage and benefit premium.[8]

Unionization's hypothesized greater benefits among Black workers does not mean unions have always organized for racial justice. Indeed, while certain unions in the 19th and 20th centuries played key roles in antisegregation and civil-rights struggles, others excluded racialized (and women) workers and bolstered racial inequities and white supremacy.[10,25–29] Nonetheless, although racist practices persist in some unions, many do provide racialized workers with certain protections against employer discrimination and harassment.[15,18] Such protections may explain Black workers' affinity for unionization and their larger wage and benefit premiums,[15] and suggest that unionization may buffer structural racism's effects on Black workers' health. Thus, given the potentially stronger union–mortality relationship among Black and less-educated workers and their disproportionate union-membership losses, we hypothesized declining union density may have exacerbated racial and educational mortality inequities.

Despite mechanisms linking unionization, mortality, and mortality inequities, few US-based studies have examined the relationship empirically; those that have produced mixed findings. For example, although several ecological studies estimated protective associations between union density and occupational fatalities,[30–32] fatal overdoses,[23,33] and suicides,[33] another found no association between union density, all-cause mortality, and all-cause mortality inequities.[33] Studies measuring union membership and health or mortality at the individual-level have had similarly mixed findings. For example, although a study by Waitzman[34] estimated a protective union–mortality association among a male 1960s–1970s cohort, a recent longitudinal study by Eisenberg-Guyot et al.[35] found no association between union membership and self-rated health or mental illness. Given the limited, contradictory prior research, the individual-level relationships between union membership, mortality, and mortality inequities remain uncertain.

We addressed this gap using longitudinal, individual-level Panel Study of Income Dynamics (PSID) data. Our specific aims were to (1) estimate the cumulative incidence of mortality during follow-up in our sample if all (versus none) of respondents' employed-person–years had been union-member employed-person–years, (2) examine effect modification by gender, race, and education, and (3) estimate how racial and educational mortality inequities in our sample would have changed if union-membership prevalence across racial and educational groups had remained at 1979 levels throughout follow-up rather than at 2015 levels.

METHODS

Data and Sample

The University of Michigan's Survey Research Center runs the PSID, which enrolled a nationally representative probability sample of US families in 1968.[36] PSID interviewed these core families and subsequent split-off families (families formed by persons who left core families to form new, economically-independent families) annually from 1969 to 1997 and biennially thereafter; since 1972, most interviews have been over telephone.[36]

We used data on family reference persons and their partners ages 25–66 from survey waves in odd years from 1979 to 2015 with mortality follow-up through age 68 and year 2017; from 1979 to 1997, we used waves from every other year to align with the survey's 1999–2015 structure. We ended follow-up at age 68 because we hypothesized union membership would have the strongest effects among working-age adults. Reference persons and their partners entered our sample at the first wave that they were employed by someone other than themselves and remained until death or their last wave of follow-up, whichever came first; we censored respondents who missed a wave at their last continuous wave. We excluded nonreference persons and nonpartners because such respondents did not have data on all relevant variables. We also excluded respondents in PSID's 1990–1995 Latino Sample because of their short follow-up and extensive missingness on several relevant variables, as well as respondents ever employed in military occupations or industries (1%) due to their lack of union membership and the sector's unique structure.

Exposure

Each wave from 1979 to 2015, PSID asked non-self-employed respondents whether they were covered by a labor-union contract, and if so, whether they were members of the contract-providing union. Although all union-member respondents were union-contract covered, only 86% of contract-covered respondents were union members, since contract-covered workers can opt out of union membership and thus avoid paying certain union dues or fees while still benefiting from their union contracts.[37] We used union membership as the exposure rather than contract coverage because membership more strongly correlates with health-promoting factors like high wages.[38]

Outcome

Our outcome was all-cause mortality, available for all respondents in PSID's restricted-use file.[36] We assigned deaths occurring within 2 years of a survey wave to that wave (e.g., for respondents interviewed in 1985, we assigned 1985 and 1986 deaths to the 1985 wave, as well as 1987 deaths that occurred before the 1987 wave). In most instances, surviving household members reported death information about decedents at the next wave.[39] For decedents without surviving household members, death information came from several sources, including surviving nonhousehold contact persons, administrators of decedents' estates, or the post office.[39] For 98% of deaths within our years and ages of interest, PSID provided the precise death year. For 1% of deaths, PSID provided a 1- to 2-year range for the death year (e.g., 1982–1983 or 1982–1984); for these deaths, we assigned the death year to the range's latter year. We excluded respondents associated with the remaining deaths, which were only known to have occurred within a range of 3 or more years.

Confounders

Baseline covariates identified as potential confounders included respondents' gender (assigned by the interviewer as female/male), race (self-identified; operationalized as Black/other/White), age, education (<HS/HS/some college/≥college), census region of residence (Midwest/Northeast/South/West), parental wealth when growing up (poor/average/well-off), disability status (whether respondents had a disability that limited the amount/type of work they could do), and year. We did not consider ethnicity because PSID did not collect ethnicity data until 1985.

Time-varying covariates identified as potential confounders included respondents' marital status (married or cohabiting/not married or cohabiting), employment status (employed/not employed), occupation, and industry. We categorized occupation into seven categories after crosswalking the codes to make them consistent temporally[40,41]; we categorized industry into nine categories after crosswalking (Table 1).[42]

Statistical Analyses

Primary and Subgroup Analyses

As discussed in eAppendix 2; http://links.lww.com/EDE/B820, we hypothesized that (1) prior union membership affected current employment status (because union membership may increase employment stability[21,35]), (2) current employment status affected current union membership (because only the employed can be union members), and (3) current employment status affected future mortality (because being employed may improve health[43]), a confounding structure that could cause healthy-worker bias.[44] In such a setting, standard covariate-adjustment approaches cannot consistently estimate mean potential outcomes under various exposure scenarios because (1) employment status both confounds and mediates the union–health relationship, and (2) only the employed are union-membership eligible, creating structural nonpositivity.[45–48] Nonetheless, the parametric g-formula, which generalizes standardization to settings with time-varying exposure–confounder feedback, can consistently estimate mean potential outcomes in settings with potential healthy-worker bias, which is why we used it in our analyses.[46,47,49]

eAppendix 3; http://links.lww.com/EDE/B820, contains code for implementing our approach using R's "gfoRmula" package.[50] Throughout our analyses, follow-up time (years since baseline) was treated as the time scale.[51] First, we fitted pooled parametric models on the observed data for time-varying union membership, time-varying confounders, and mortality, using logistic models for binary variables and multinomial logistic models for categorical variables.[52] We assumed the following ordering of time-varying variables within waves: (1) marital status, (2) employment status, (3) occupation, (4) industry, (5) union membership, and (6) mortality (Figure 1). To predict time-varying variables in wave t_k, the pooled parametric models had predictors of baseline confounders, prior time-varying variables in t_k, time-varying variables in t_{k-1}, year, and follow-up time. We specified categorical covariates as described in the Confounders section and age as a three-knot restricted cubic spline to allow for nonlinear age–outcome relationships.[53] We specified year and follow-up time as five-knot restricted cubic splines in most models, although we specified them differently in several models to improve fit (eAppendix 4; http://links.lww.com/EDE/B820).

Next, we created a Monte Carlo pseudo-sample by randomly drawing 25,000 respondents with replacement from the observed baseline sample[52]; we drew a sample larger than the

TABLE 1. Descriptive Statistics of 1979–2015 Panel Study of Income Dynamics Sample at Baseline Stratified by Union Membership

	Nonunion	Union
N	19,656	3,366
Age (median [1st quartile, 3rd quartile])	29 [26, 36]	31 [26, 41]
Year (median [1st quartile, 3rd quartile])	1995 [1983, 2005]	1987 [1979, 1999]
Male, n (%)	9,391 (48)	2,092 (62)
Race, n (%)		
Black	6,283 (32)	1,318 (39)
Other	1,698 (9)	258 (8)
White	11,675 (59)	1,790 (53)
Education, n (%)		
<HS	3,734 (19)	704 (21)
HS	6,626 (34)	1,381 (41)
Some college	4,857 (25)	726 (22)
College+	4,439 (23)	555 (17)
Married/permanently-cohabiting, n (%)	14,384 (73)	2,630 (78)
Parental wealth, n (%)[a]		
Poor	5,906 (30)	1,211 (36)
Average	8,440 (43)	1,360 (40)
Well-off	5,310 (27)	795 (24)
Occupation, n (%)		
Farming, forestry, and fishing	280 (1)	9 (0)
Managerial	1,721 (9)	64 (2)
Operators, fabricators, and laborers	3,123 (16)	1,170 (35)
Precision production, craft, and repair	1,943 (10)	537 (16)
Professional specialty	3,069 (16)	471 (14)
Services	3,733 (19)	487 (15)
Technical, sales, and admin support	5,787 (29)	628 (19)
Industry, n (%)		
Agriculture, forestry, and fisheries	424 (2)	16 (1)
Construction	1,169 (6)	231 (7)
Finance, insurance, and real estate	1,311 (7)	37 (1)
Manufacturing	3,375 (17)	1,061 (32)
Mining	119 (1)	17 (1)
Public administration	980 (5)	273 (8)
Services	7,134 (36)	894 (27)
Transport, communications, and other public utilities	1,183 (6)	573 (17)
Wholesale and retail trade	3,961 (20)	264 (8)
Region of residence, n (%)		
Midwest	4,509 (23)	1,016 (30)
Northeast	2,595 (13)	737 (22)
South	9,074 (46)	976 (29)
West	3,478 (18)	637 (19)
Work disability, n (%)[b]	1,474 (8)	216 (6)
Family income (median [1st quartile, 3rd quartile])[c]	5.7 [3.5, 8.6]	7.0 [4.6, 9.8]

[a]Parental wealth when respondent was growing up.
[b]Respondent had disability that limited the type or amount of work they could do.
[c]Tens of thousands of family income in 2017 dollars.
HS indicates high school.

observed sample to minimize simulation error.[54] We then predicted values of time-varying variables in respondents' second waves using their baseline pseudosample observations and parameters from the pooled parametric models.[52] We then used predicted values of time-varying variables in respondents' second waves and parameters from the pooled parametric models to predict values of time-varying variables in respondents' third waves, and so on, until mortality or the end of follow-up, whichever came first.[52] In our natural-course scenario,[52] we left union membership as predicted by the pooled parametric models. In our union scenario, we set union membership to "union" whenever respondents were predicted to be employed. Finally, in our nonunion scenario, we set union membership to "nonunion" whenever respondents were predicted to be employed. These scenarios avoided nonpositivity bias by only allowing employed respondents to be union-membership eligible.[49] In all scenarios, we eliminated censoring from administrative causes (e.g., changes in PSID's sampling frame) and loss to follow-up, an unbiased approach if censoring is noninformative, given measured confounders.[52] Prior epidemiologic research in PSID has found little evidence of attrition bias[55]; moreover, in our sample, we found minimal differences in measured confounders between censored and uncensored observations (eAppendix 5; http://links.lww.com/EDE/B820).

Finally, we calculated risk ratios (RRs) and risk differences (RDs) by contrasting the simulated cumulative incidence of mortality per 1,000 respondents through the end of follow-up (38 years) in the union and nonunion scenarios. Although union densities of 100% or 0% are unlikely to occur in the United States, the stark union-versus-nonunion contrast increased our sensitivity to detect a union–mortality association. We calculated confidence intervals for the contrast by repeating the g-formula algorithm on 200 bootstrap samples, with standard errors estimated as the standard deviations of the bootstrap distributions.[52] We probed for model misspecification by comparing the simulated exposure, time-varying confounder, and outcome distributions in the natural course with those in the observed data.[45–47,52] We also examined effect modification by gender (women or men), race (Black or White [those identifying as "Other" excluded due to small cell sizes]), and education (≤HS or >HS) by running our approach in subgroups.

Inequity Analyses

In our inequity analyses, we used the same approach to contrast mortality incidence among racial and educational subgroups in two additional (more realistic[56]) scenarios. In these scenarios, in waves in which respondents were predicted to be employed, we randomly drew their union-membership values from binomial distributions with means equal to the probability of union membership observed for that subgroup in 1979 or 2015. In the 1979 scenario, this corresponded to a union-membership probability of 0.28 for Black respondents, 0.23 for White respondents, 0.28 for ≤HS respondents, and 0.18 for >HS respondents. In the 2015 scenario, this corresponded to a union-membership probability of 0.14 for Black respondents, 0.12 for White respondents, 0.14 for ≤HS respondents, and 0.12 for >HS respondents. A sharper mortality reduction

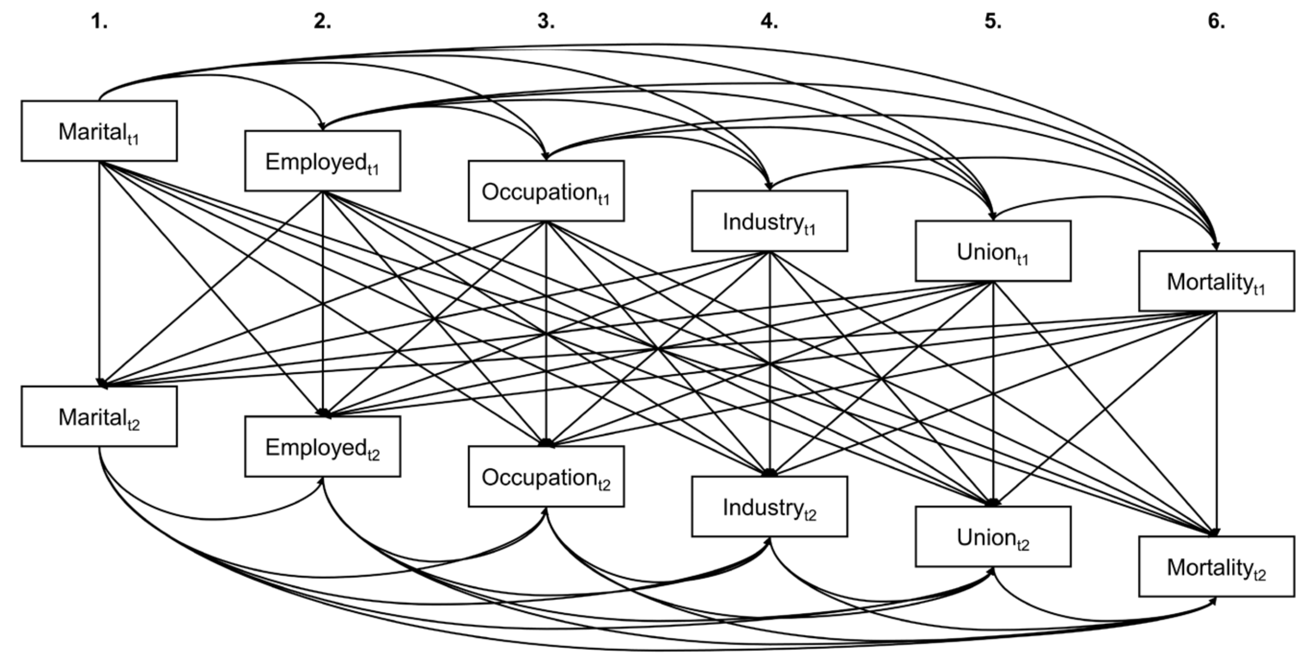

FIGURE 1. Hypothesized temporal ordering of time-varying variables in parametric g-formula analyses. Time-varying variables in wave t_k were functions of baseline confounders, prior time-varying variables in t_k (if any), time-varying variables in t_{k-1}, year, and follow-up time. Time t_k for mortality occurred up to 2 years after time t_k for all other time-varying variables, as we assigned deaths occurring within 2 years of a survey wave to that survey wave.

in THE 1979 scenario relative to the 2015 scenario for Black and less-educated respondents than for White and more-educated respondents would suggest declining union membership exacerbated racial and educational mortality inequities. We excluded those identifying as "Other" from the racial inequity analyses because of small cell sizes.

Sensitivity Analyses

We tested our results' robustness to alternative specifications, including (1) using a 2-year-lagged union-membership exposure, (2) treating occupation and industry as time-invariant confounders, (3) additionally adjusting for respondents' baseline self-rated health, (4) adjusting for respondents' baseline census division of residence, (5) modeling year with five-knot restricted cubic splines in all pooled parametric models, (6) excluding those employed in managerial or farming, forestry, and fishing occupations at baseline, (7) allowing respondents to enter our sample at the wave first wave they were ages 25–66, regardless of whether they were employed by someone other than themselves at that wave, (8) examining differences in union membership's effects in right-to-work versus non-right-to-work states, and (9) running traditional Cox models with a baseline union-membership exposure. eAppendix 7; http://links.lww.com/EDE/B820, contains details.

Missing Data

Our exposure and confounders contained some missingness (≤4%). We addressed missingness in baseline confounders by carrying observed values forwards (or backward if necessary) when possible. We addressed remaining missingness in the confounders and exposure using a single multivariate imputation by chained equations with 50 iterations.[57] As predictors, the imputation models included all baseline confounders, plus time-varying exposure and confounders in t_k and t_{k-1} (or t_{k+1} in respondents' baseline wave). We did not create multiple imputed datasets because doing so in our parametric-g-formula setting was computationally infeasible.

Ethical Review

The University of Washington IRB determined this study to be exempt from IRB review because it used publicly available, deidentified data. Nonetheless, the University of Washington IRB reviewed and approved the study because PSID requires such approval to access the restricted-use mortality data.

RESULTS

Descriptives

Our full sample included 23,022 respondents with 910 deaths, 146,681 observations, and a median and maximum follow-up of 10 years and 38 years, respectively. At baseline, 15% of respondents were unionized (Table 1). Union workers tended to be older and less educated than nonunion workers, and were more likely to be Black, men, married/cohabiting, living outside the South, and to have grown up poor. Moreover, union workers more often had occupations defined as "operator, fabricator, and laborer" and "precision production, craft, and repair," as well as industries defined as "manufacturing" and "transportation, communications, and other public utilities." Finally, union workers had median family incomes 22% higher than nonunion workers. eAppendix 6; http://links.lww.com/EDE/B820, displays sample trends in union membership by demographic, occupation, and industry.

Primary and Subgroup Analyses

Overall, the simulated cumulative incidence of mortality by the end of follow-up in the natural course was 181 per 1,000 (Figure 2). Simulated mortality incidence was greater among male, Black, and less-educated respondents than among female, White, and more-educated respondents (Table 2). In most analyses, the simulated mortality incidence in the natural course aligned with the observed incidence, as did the simulated union-membership probabilities (Figure 2 and eAppendix 18; http://links.lww.com/EDE/B820). However, although the simulated employment-status probabilities generally aligned with the observed probabilities, the simulated distributions of other time-varying confounders tended to differ from the observed distributions more considerably, particularly occupation and industry (eAppendix 18; http://links.lww.com/EDE/B820).

Overall, 20% of person–years in the union scenario were spent not employed, lower than the 23% in the nonunion scenario. Moreover, the union scenario was associated with lower mortality incidence than the nonunion scenario (RR = 0.90, 95% CI = 0.80, 0.99; RD per 1,000 = −19, 95% CI = −37, −1) (Table 2). This protective association generally held across subgroups, although it was stronger among more-educated respondents (RR = 0.72, 95% CI = 0.57, 0.88; RD per 1,000 = −35, 95% CI = −56, −13) than among less-educated respondents (RR = 0.95, 95% CI = 0.85, 1.1; RD per 1,000 = −11, 95% CI = −36, 15).

Inequity Analyses

We found little evidence that mortality inequities would have lessened if union-membership prevalence had remained at 1979 levels throughout follow-up rather than at 2015 levels. Specifically, among Black respondents, the 1979 scenario was not associated with lower mortality incidence than the 2015 scenario (RR = 0.99, 95% CI = 0.96, 1.0; RD per 1,000 = −3, 95% CI = −7, 1), similar to the association among White respondents (RR = 0.99, 95% CI = 0.97, 1.0; RD per 1,000 = −2, 95% CI = −4, 0) (Table 3). Likewise, among less-educated respondents, the 1979 scenario was not associated with lower mortality incidence than the 2015 scenario (RR = 0.99, 95% CI = 0.97, 1.0; RD per 1,000 = −2, 95% CI = −6, 1), similar to the association among more-educated respondents (RR = 0.99, 95% CI = 0.98, 1.0; RD per 1,000 = −1, 95% CI = −3, 1) (Table 3).

Sensitivity Analyses

None of the sensitivity analyses showed meaningfully different results from the main analyses (eAppendices 7–16; http://links.lww.com/EDE/B820).

DISCUSSION

Summary of Results

Using the parametric g-formula, we estimated the cumulative incidence of mortality during follow-up in our cohort if all (versus none) of respondents' employed-person–years had

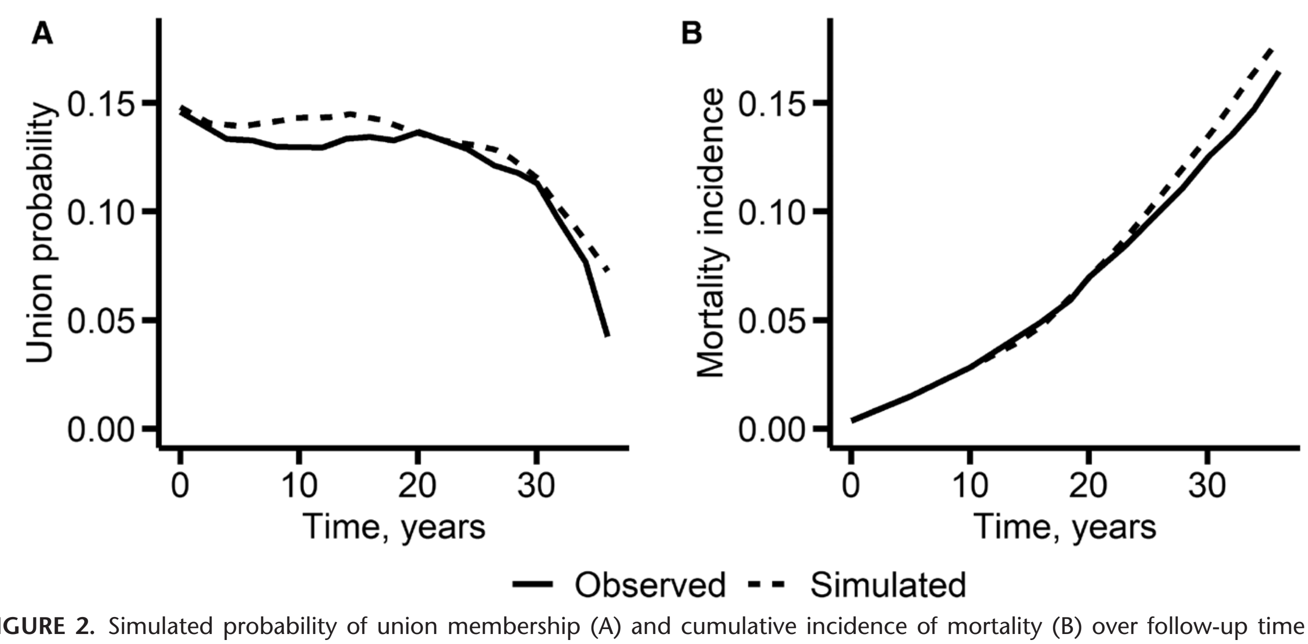

FIGURE 2. Simulated probability of union membership (A) and cumulative incidence of mortality (B) over follow-up time (in years) in the natural course compared with the observed values in 1979–2015 Panel Study of Income Dynamics sample with mortality follow-up through 2017. Time 0 in A occurred up to 2 years before time 0 in B, as we assigned deaths occurring within 2 years of a survey wave to that survey wave.

TABLE 2. Parametric G-formula Estimates of the 38-year Risk of Mortality if All (vs. None) of Respondents' Employed Person–years Had Been Union Member Employed-Person–Years

	Respondents[a]	Observations[a]	Scenario	Risk[b]	RR	95% CI		RD[b]	95% CI	
Overall	23,022	146,681	Union	166	0.90	0.80	0.99	−19	−37	−1
			Nonunion	185						
Gender										
Women	11,539	76,285	Union	128	0.87	0.71	1.0	−19	−44	6
			Nonunion	147						
Men	11,483	70,396	Union	209	0.92	0.83	1.0	−18	−40	4
			Nonunion	227						
Race										
Black	7,601	43,785	Union	184	0.89	0.75	1.0	−22	−51	7
			Nonunion	206						
White	13,465	92,844	Union	140	0.90	0.77	1.0	−16	−36	4
			Nonunion	156						
Education										
≤HS	12,445	72,809	Union	223	0.95	0.85	1.1	−11	−36	15
			Nonunion	233						
>HS	10,577	73,872	Union	90	0.72	0.57	0.88	−35	−56	−13
			Nonunion	125						

RR and RD estimates compare the risk (i.e., cumulative incidence) in the always-union (union) scenario relative to the risk in the never-union (nonunion) scenario. Subgroup estimates produced from stratified models. Confidence intervals calculated from nonparametric bootstrap with 200 repetitions.

[a]Unique respondents and observations in 1979–2015 Panel Study of Income Dynamics sample (with mortality follow-up through 2017) used to fit pooled time-varying exposure, confounder, and outcome models. Monte Carlo pseudo-sample used in simulations had 25,000 respondents.

[b]Per 1,000 respondents.

CI indicates confidence interval; HS, high school.

been union-member employed-person–years, and estimated how racial and educational mortality inequities would have changed if union-membership prevalence across subgroups had remained at 1979 levels throughout follow-up rather than at 2015 levels. We found that the union scenario was associated with lower mortality incidence overall and in most subgroups, particularly among more-educated respondents. Although modest on the relative scale, the estimates on the absolute scale in the full sample were meaningful, corresponding to 19 per 1,000 fewer premature deaths among workers during a typical career. Nonetheless, contrary to expectations, we did not estimate that racial and educational mortality inequities would have lessened if union-membership prevalence had remained at 1979 levels. Rather, the 1979 scenario was not associated with meaningfully lower mortality incidence than the 2015 scenario in any subgroup, likely because the union-membership probabilities contrasted in the two scenarios were too similar to capture the protective associations identified in the starker always-union-versus-never-union contrast. However, declining union density may eventually affect mortality and mortality inequities if current union-density trends continue and contrasts with 1979's union density grow sufficiently stark.

Assuming no unmeasured confounding, no model misspecification, no selection bias, and no information bias (strong assumptions discussed later), we interpret our results as estimating that there was a protective effect of union membership on mortality over follow-up in our sample.[58] We prefer interpreting our results in terms of realized causal effects (the effect union membership did have in the past in our study) rather than intervention effects (the effect union membership would have in the future under a hypothetical intervention, an interpretation more common in g-methods analyses) for two reasons.[58,59] First, in our inequity analyses, we were interested in the effect that declining union density did have on mortality inequities over follow-up—a realized causal effect.[58,59] Second, union membership's mortality effects would likely differ depending on the specific "intervention" used to (de)unionize the sample (e.g., rank-and-file organizing versus labor-law changes) and the type of unionization targeted for intervention (e.g., militant versus conservative unionization, both of which may have been present in our sample).[58,60] Moreover, a unionization intervention would likely have spillover and contextual-level effects that would modify the union–mortality relationship.[10,13,29] These factors would violate the stable unit treatment value assumption required for an intervention-effect interpretation but not for a realized-effect interpretation.[59,60]

Using a 1960s–1970s male-only sample and baseline union-membership exposure, Waitzman[34] also identified protective union–mortality associations. Although Waitzman's[34] protective associations were generally stronger than ours, the studies are not directly comparable given the different time periods, samples, and modeling approaches. For example, union membership's beneficial effects may have weakened, given labor's diminishing power.[11] Although Waitzman[34] did not examine racial and educational effect modification, our weaker than anticipated estimates among Black and

TABLE 3. Parametric G-formula Estimates of the 38-year Risk of Mortality if Respondents' Employed-person–years Had Been Set to Union Member Employed-Person–Years with the Union Membership Probabilities Observed in 1979 (vs. 2015)

	Respondents[a]	Observations[a]	Scenario	Risk[b]	RR	95% CI		RD[b]	95% CI	
Race										
Black	7,601	43,785	1979	200	0.99	0.96	1.0	−3	−7	1
			2015	203						
White	13,465	92,844	1979	152	0.99	0.97	1.0	−2	−4	0
			2015	154						
Education										
≤HS	12,445	72,809	1979	230	0.99	0.97	1.0	−2	−6	1
			2015	233						
>HS	10,577	73,872	1979	119	0.99	0.98	1.0	−1	−3	1
			2015	120						

RR and RD estimates compare the risk (i.e., cumulative incidence) in the 1979 scenario relative to the risk in the 2015 scenario. Subgroup estimates produced from stratified models. Confidence intervals calculated from nonparametric bootstrap with 200 repetitions.

[a]Unique respondents and observations in 1979–2015 Panel Study of Income Dynamics sample (with mortality follow-up through 2017) used to fit pooled time-varying exposure, confounder, and outcome models. Monte Carlo pseudo-sample used in simulations had 25,000 respondents.

[b]Per 1,000 respondents.

CI indicates confidence interval; HS, high school.

less-educated respondents may be due to several factors, including racism in the union movement and the cumulative, multifaceted causes of disproportionate mortality among the marginalized—like centuries of structural racism[4] and class oppression[61]—which union membership alone cannot completely countervail.

Although we believe our results are plausible, several factors may have biased our estimates and compromised our ability to estimate causal effects. The biases' net effects are ambiguous. Regarding bias toward the null, research suggests that unmeasured workplace-level characteristics like hazardous working conditions may cause workers to unionize.[62] This partly explains why some quantitative studies have associated unionization with increased occupational-injury risk, contradicting historical and anecdotal evidence.[62] Because we were unable to adjust for workplace-level confounders, unmeasured confounding by such factors may have caused an underestimate of unionism's protective effects. Unmeasured workplace-level confounding may be especially likely in this study because of the unexpectedly weak union–mortality association among less-educated respondents, the workers most often exposed to hazardous working conditions. Union-membership misclassification may have also biased our results towards the null. For example, Card[63] found that 2.5%–3.0% of 1977 Current Population Survey respondents misreported their union status. This would mean at least 17% of workers classified as union in our analyses were actually nonunion (eAppendix 17; http://links.lww.com/EDE/B820).[13] Unfortunately, to our knowledge, there is no research on the accuracy of PSID's union-membership data.

Regarding bias away from the null, union workers may differ from nonunion workers in unmeasured factors.[38] For example, although contested, some authors argue that unionized workplaces selectively hire more-skilled or more-productive workers than nonunionized workplaces.[37] Such unmeasured factors may also affect mortality. For example, in this study, if healthier workers were selected into union jobs because such jobs required greater physical exertion than nonunion jobs, union membership might spuriously appear to reduce mortality, true effects aside. Although we adjusted for baseline disability status and baseline self-rated health (to address potential health selection into union jobs), as well as occupation and industry (to, among other things, address variation in the physical demands of different jobs), we may not have completely blocked confounding pathways.

Strengths and Limitations

Our study had several strengths. First, it is to our knowledge the only individual-level US-based study to examine the union–mortality association with repeated union-membership measurements. Second, it is one of few individual-level, longitudinal studies to examine how a socioeconomic factor—declining union density—has contributed to changing mortality inequities. Finally, unlike standard covariate-adjustment approaches, our parametric g-formula approach addressed potential healthy-worker bias.

Our study also had additional limitations beyond unmeasured confounding and union-membership misclassification. First, the natural-course distributions of occupation and industry differed somewhat from the observed distributions throughout our analyses, suggesting we may have misspecified our models for those variables. However, two observations mitigate concerns that misspecification biased our findings: (1) we accurately modeled the distributions of other time-varying variables in most analyses, including employment status, and (2) our results were similar in traditional Cox analyses (and in other sensitivity analyses). Although always-union-versus-never-union g-formula estimates cannot be directly compared with Cox estimates, the estimates' similarity reduces the likelihood that quirks in our modeling approach can explain our findings. Second, our sample had few deaths, which precluded stratifying by gender–race or gender–education. Nonetheless, the relationship between union membership, mortality, and mortality inequities may vary by gender within racial and educational groups given gender differences in employment conditions and union-membership prevalence.[15] Third, in the inequity analyses, we assumed setting union-membership prevalence to 1979 or 2015 levels would affect mortality inequities solely through the direct individual-level union–mortality relationship. In reality, drastic union-density changes may have many societal-level effects, given: (1) union-density increases can have spillover effects on health-related factors (e.g., wages) in nonunionized workplaces,[13] (2) union-density increases can shape health-related structural factors, like working-class power and social policies.[10,29] Such contextual-level phenomena could also affect mortality and mortality inequities. Finally, we assumed union membership's effects did not change temporally or vary by sector or region, a strong assumption given changing union power over follow-up and the many union organizing models.[29] Nonetheless, the consistency of the union wage premium suggests temporal changes in union membership's effects may be modest.[8] Moreover, our stratification by gender, race, and education may have proxied for sector and region, as well as gender and racial differences in worker treatment by employers and unions.

CONCLUSIONS

In summary, we found protective union–mortality associations among working-age US adults, but little evidence to support the hypothesis that declining union density has exacerbated racial and educational mortality inequities, although we did not incorporate contextual-level effects into our analyses. In future studies, to address unmeasured confounding, researchers could consider quasiexperimental approaches like regression-discontinuity designs,[20,32] although finding data for such approaches is challenging. Additionally, to incorporate contextual-level effects, researchers could consider multilevel approaches, such as examining how individual-level union membership interacts with area-level union density and other measures of working-class power, like strike rates, to affect health.

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