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# Does the Union Make Us Strong? Labor-Union Membership, Self-Rated Health, and Mental Illness: A Parametric G-Formula Approach

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Union members enjoy better wages and benefits and greater power than nonmembers, which can improve health. However, the longitudinal union-health relationship remains uncertain, partially because of healthy-worker bias, which cannot be addressed without high-quality data and methods that account for exposure-confounder feedback and structural nonpositivity. Applying one such method, the parametric g-formula, to US-based Panel Study of Income Dynamics data, we analyzed the longitudinal relationships between union membership, poor/fair self-rated health (SRH), and moderate mental illness (Kessler 6-item score of ≥5). The SRH analyses included 16,719 respondents followed from 1985–2017, while the mental-illness analyses included 5,813 respondents followed from 2001–2017. Using the parametric g-formula, we contrasted cumulative incidence of the outcomes under 2 scenarios, one in which we set all employed-person-years to union-member employed-person-years (union scenario), and one in which we set no employed-person-years to union-member employed-person-years (nonunion scenario). We also examined whether the contrast varied by sex, sex and race, and sex and education. Overall, the union scenario was not associated with reduced incidence of poor/fair SRH (relative risk = 1.01, 95% confidence interval (CI): 0.95, 1.09; risk difference = 0.01, 95% CI: −0.03, 0.04) or moderate mental illness (relative risk = 1.02, 95% CI: 0.92, 1.12; risk difference = 0.01, 95% CI: −0.04, 0.06) relative to the nonunion scenario. These associations largely did not vary by subgroup.

g-computation; healthy-worker bias; labor movement; labor unions; occupational health; parametric g-formula; social determinants of health

Abbreviations: CI, confidence interval; K6, Kessler Psychological Distress Scale; PSID, Panel Study of Income Dynamics; RR, relative risk; RD, risk difference; SRH, self-rated health.

The labor movement has struggled with worker health and safety since the industrial revolution's inception. In the 19th century, Marx and Engels decried the toxic working and living conditions endured by the burgeoning working class in England and elsewhere, and championed the roles of nascent working-class organizations like labor unions in overcoming the deleterious conditions (1–3). Concurrently, US workers organized unions, political parties, and other formations to address low wages, grueling hours, degraded labor processes, and deadly working conditions (4, 5). In the early to mid-20th century, many of these organizations became legally recognized unions after workers won unionization and collective-bargaining rights (4, 6). Unions have since advanced US occupational health (7), as the American Public Health Association has formally recognized (8, 9). Unions helped pass the Occupational Safety and Health Act of 1970 (OSHA) (4), and despite declining power, they remain critical in exposing occupational hazards (10), enforcing OSHA regulations (11, 12), and protecting workers from workplace harassment and discrimination (13).

In prior US-based ecological studies, researchers have identified protective associations between union density (the proportion of workers belonging to unions) and rates of occupational fatalities (14–16), fatal overdoses (17, 18), and suicides (18), hypothesizing that union density influences structural factors like regulatory regimes, social policies, and working-class power. However, although many individual-level US-based studies have analyzed the relationship between union membership and occupational injuries (19), few have analyzed the relationship between union membership and non–occupational-injury outcomes (20–22). For example, a study by Reynolds et al. (21) identified a protective cross-sectional association between union membership and self-rated health (SRH), while another by Waitzman (22) found a protective association between union-contract coverage and mortality in a 1960–1970s male-only cohort.

Limited prior research aside, mechanisms might link union membership to better SRH and mental health (the outcomes under study). First, by bolstering workers' bargaining power with management, unionization might protect workers from material deprivation (e.g., inadequate wages and benefits) (21, 23, 24), occupational hazards (e.g., chemicals) (7, 13), and stressors (e.g., job instability, poor autonomy, and discrimination) (13, 21), lowering their risk of chronic diseases and occupational injuries (and their sequelae, like poor SRH), as well as their risk of mental illnesses (25–27). Additionally, by building solidarity among workers, unions could lessen feelings of alienation and powerlessness, factors that can worsen SRH and mental health (28, 29). Union membership's health benefits might be strongest for less-educated and racialized workers. For example, studies have found the union wage premium—typically 15%–20%—is largest for less-educated and Black workers, as is the benefit premium (23). Beyond differences in the wage and benefit premium, union membership might disproportionately benefit racialized and women workers by providing them with certain protections against employment-related racism and sexism (13).

One barrier to studying the individual-level union-health relationship has been healthy-worker survivor bias, a bias that occurs when prior exposure (e.g., union membership) affects current employment status (a confounder), current employment status affects exposure, and employment status independently affects the outcome. In this study, we hypothesized: 1) prior union membership affected current employment status (because union membership could increase employment stability), 2) current employment status affected current union membership (because only the employed were eligible for union membership), and 3) current employment status affected current and future health (because being employed could improve health) (Web Figure 1, available at https://doi.org/10.1093/aje/kwaa221). Relationship (2) is a feature of union membership, while relationships (1) and (3) have been identified in prior studies of our data (30, 31); we also identified them in our sample (Web Appendix 1 and Web Table 1). Thus, employment status confounded and mediated the union-health relationship, a setting in which standard covariate-adjustment approaches cannot consistently estimate total exposure effects (32–35). However, the parametric g-formula—a generalization of standardization—can consistently estimate total effects in such settings, as well as avoid bias in settings with structural nonpositivity, essential for this analysis because one cannot be simultaneously unemployed and a union member (32–35).

Applying the parametric g-formula to Panel Study of Income Dynamics (PSID) data, we estimated the longitudinal relationships between union membership, SRH, and mental illness. Specifically, our goals were to: 1) estimate how a scenario setting all (vs. none) of respondents' 2-year-lagged employed-person-years to union-member employed-person-years would affect incidence of poor/fair SRH and moderate mental illness, and 2) examine whether the scenarios' effects varied by sex, sex and race, and sex and education.

## METHODS

### Data

The PSID is a panel survey conducted by the University of Michigan's Survey Research Center (36). In 1968, PSID enrolled a nationally representative probability sample of US families (36). PSID interviewed respondents in these "core" families and subsequent "split-off" families (those who moved out of core families to form economically independent families) annually from 1969–1997 and biennially thereafter. Since 1972, most interviews have been conducted via telephone (36). Overall response rates have averaged 91% and wave-to-wave response rates have averaged 94% since PSID's inception (37). Although attrition is greatest among less-educated and racialized respondents, research has found little evidence of attrition bias in epidemiologic analyses (38). PSID has collected socioeconomic and demographic data since 1968 (36). Beginning in 1984, PSID asked proxy and nonproxy respondents about their SRH (36). From 2001–2017, save 2005, PSID administered to nonproxy respondents the Kessler Psychological Distress Scale (K6) (36), a 6-question scale developed to estimate the prevalence of serious mental illness (39).

Our SRH and mental-illness analyses used data on family heads (or "reference persons") and their spouses/partners (spouses/partners did not have data on all variables of interest) aged 25–64 from survey waves in odd years from 1985–2017 and 2001–2017, respectively; we treated the survey as biennial to align with the survey's post-1997 structure. We excluded respondents in PSID's 1990–1995 "Latino sample" because of their short follow-up and extensive missingness on several variables of interest, as well as respondents ever employed in "military" occupations or industries (1%) due to the sector's lack of union membership.

### Exposure

PSID asks respondents who are employed by someone other than themselves whether they are covered by a labor-union contract and, if so, whether they are members of the union providing the contract (87% of those covered). We used union membership as our exposure rather than contract coverage because membership more strongly correlates with health-promoting factors like high wages (40). We lagged union membership 2 years prior to outcome measurement to mitigate reverse causation and because unionization's health benefits might not accrue immediately.

### Health outcomes

We dichotomized SRH—measured using the standard question ("Would you say your health in general is . . . ")—as poor/fair versus good/very good/excellent to improve reliability (41). We also dichotomized K6 (range 0–24) as $<5/\geq 5$, which reliably distinguishes those with/without moderate mental illness, defined as mental illness requiring treatment and causing impaired functioning (39).

## Confounders

Baseline covariates identified as potential confounders included respondents' age, race (Black/other/White, unless otherwise noted), sex (female/male), education (less than high-school/high-school education/some-college/beyond college, unless otherwise noted), census division of residence (see Table 1), childhood socioeconomic status (poor/average/well-off), disability status (whether respondents had a disability that limited the amount/type of work they could do), and year.

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. Regarding occupations, PSID used 1970 census codes from 1985–2001, 2000 codes from 2003–2015, and 2010 codes in 2017; after crosswalking the codes to create a consistent time series (42, 43), we divided the occupations into 7 categories (see Table 1). Regarding industries, PSID used 1970 census codes from 1977–2001, 2000 codes from 2003–2015, and 2012 codes in 2017; after crosswalking (44), we divided the industries into 9 categories (see Table 1).

## Sample

For analyses of union membership and SRH, respondents entered our sample at the first wave they were employed by someone other than themselves; we excluded those reporting the outcome at that wave, as well as those observed for only 1 wave. Respondents remained in our sample until their first incident outcome or their last follow-up wave, whichever came first. For analyses of union membership and mental illness, respondents had K6 measurements only in certain waves because PSID did not administer the K6 in 2005, while in other waves, PSID administered the K6 only to nonproxy respondents; we assumed 2005 respondents and proxy respondents did not have K6 values of $\geq 5$. Respondents entered our sample at the first wave at which they were employed by someone other than themselves and had a K6 measurement; we excluded those reporting the outcome at that wave. Respondents remained in the sample until their last K6 measurement or their first incident outcome, whichever came first. We excluded respondents with fewer than 2 K6 measurements during follow-up. For both outcomes, we excluded respondents missing any outcome data during follow-up, and we censored respondents who missed a wave of follow-up at their last continuous wave. Web Figures 2–3 display flow diagrams.

## Statistical analyses

*Primary analyses.* We conducted our parametric g-formula analyses using the "gfoRmula" package (45) in R (R Foundation for Statistical Computing, Vienna, Austria); Web Appendix 2 contains code for implementing our approach. The parametric g-formula can consistently estimate the mean potential outcome under an exposure scenario, given assumptions of: 1) no unmeasured confounding, 2) counterfactual consistency (respondents' counterfactual outcomes under their observed exposure histories equal their observed outcomes), 3) no model misspecification, 4) no interference (respondents' potential outcomes do not depend upon other respondents' exposures), and 5) positivity (no exposure scenarios that require respondents be exposed within strata of a confounder in which exposure is impossible) (33, 34, 46). The incidence of the outcome in each scenario is the weighted sum over the various exposure and covariate histories of the probability of the outcome conditional on exposure and covariates (32).

First, using the observed data, we fitted pooled parametric models for time-varying exposure (lagged 2 years), time-varying confounders (lagged 2 years), and the outcome of interest, using logistic models for binary variables and multinomial logistic models for categorical variables (45). Within waves, we assumed the following temporal ordering of time-varying variables: 1) marital status, 2) employment status, 3) occupation, 4) industry, and 5) union membership (Figure 1). 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 $t_{k-1}$, follow-up time (years since baseline), and year. In all models, we specified categorical covariates, as described above in Confounders, and age as a 3-knot restricted cubic spline (via the "rms" package (47)) to allow for nonlinear age-outcome relationships (48). In most models, we specified follow-up time as a fixed effect, although we specified it as a restricted cubic spline in selected analyses to improve fit; year's specification varied more considerably. Web Tables 2–3 contain details.

After fitting the parametric models, we randomly drew respondents with replacement from the observed data to create a Monte Carlo pseudosample of 25,000 (45). We drew a sample larger than the original cohort to minimize simulation error (49); using an even larger sample was not computationally feasible. Using the baseline observations of the pseudosample, we predicted observations in the second wave using parameters from the parametric models described above (45). We then used predicted observations in the second wave to predict observations in the third wave, and so on, until the end of follow-up or the outcome of interest, whichever came first (45). In the "natural course" (45), we left union membership as predicted by the parametric models. In our first scenario, we set 2-year-lagged union membership to "union" whenever respondents were predicted to be employed (union scenario), while in our second scenario, we set 2-year-lagged union membership to "nonunion" whenever respondents were predicted to be employed (nonunion scenario). These dynamic scenarios avoided nonpositivity bias by requiring only that respondents be eligible for union membership when employed (46, 50). The scenarios' dependence upon respondents' time-varying employment status distinguished them from static always-exposed/never-exposed scenarios common in analyses using marginal structural modeling (34, 45). In all

**Table 1.** Descriptive Statistics at Baseline in Self-Rated-Health Analyses Stratified by 2-Year-Lagged Union Membership, Panel Study of Income Dynamics, United States, 1985–2017

| Respondent Characteristic | Nonunion (*n* = 14,459) | | Union (*n* = 2,260) | |
|---|---|---|---|---|
| | No. | % | No. | % |
| Male sex | 6,838 | 47.3 | 1,356 | 60.0 |
| Race | | | | |
| Black | 4,300 | 29.7 | 798 | 35.3 |
| Other | 1,101 | 7.6 | 158 | 7.0 |
| White | 9,058 | 62.6 | 1,304 | 57.7 |
| Education | | | | |
| Less than high school | 2,158 | 14.9 | 349 | 15.4 |
| High school | 4,568 | 31.6 | 882 | 39.0 |
| Some college | 3,900 | 27.0 | 546 | 24.2 |
| College and beyond | 3,833 | 26.5 | 483 | 21.4 |
| Married/permanently cohabiting | 10,484 | 72.5 | 1729 | 76.5 |
| Childhood socioeconomic status[a] | | | | |
| Poor | 3,829 | 26.5 | 726 | 32.1 |
| Average | 6,499 | 44.9 | 965 | 42.7 |
| Well-off | 4,131 | 28.6 | 569 | 25.2 |
| Occupation | | | | |
| Farming, forestry, and fishing | 170 | 1.2 | 3 | 0.1 |
| Managerial | 1,410 | 9.8 | 60 | 2.7 |
| Operators, fabricators, and laborers | 2039 | 14.1 | 669 | 29.6 |
| Precision production, craft, and repair | 1,364 | 9.4 | 350 | 15.5 |
| Professional specialty | 2,537 | 17.5 | 391 | 17.3 |
| Services | 2,529 | 17.5 | 349 | 15.4 |
| Technical, sales, and admin support | 4,410 | 30.5 | 438 | 19.4 |
| Industry | | | | |
| Agriculture, forestry, and fisheries | 299 | 2.1 | 11 | 0.5 |
| Construction | 766 | 5.3 | 132 | 5.8 |
| Finance, insurance, and real estate | 991 | 6.9 | 24 | 1.1 |
| Manufacturing | 2,360 | 16.3 | 571 | 25.3 |
| Mining | 97 | 0.7 | 11 | 0.5 |
| Public administration | 696 | 4.8 | 213 | 9.4 |
| Services | 5,431 | 37.6 | 686 | 30.4 |
| Transport, communications, and other public utilities | 918 | 6.3 | 433 | 19.2 |
| Wholesale and retail trade | 2,901 | 20.1 | 179 | 7.9 |
| Division of residence | | | | |
| East North Central | 2,200 | 15.2 | 488 | 21.6 |
| East South Central | 1,195 | 8.3 | 124 | 5.5 |
| Middle Atlantic | 1,471 | 10.2 | 447 | 19.8 |
| Mountain | 793 | 5.5 | 55 | 2.4 |
| New England | 488 | 3.4 | 83 | 3.7 |
| Pacific | 1795 | 12.4 | 397 | 17.6 |
| South Atlantic | 3,615 | 25.0 | 360 | 15.9 |
| West North Central | 1,257 | 8.7 | 176 | 7.8 |
| West South Central | 1,645 | 11.4 | 130 | 5.8 |

**Table continues**

**Table 1.** Continued

| Respondent Characteristic | Nonunion ($n$ = 14,459) | | Union ($n$ = 2,260) | |
|---|---|---|---|---|
| | No. | % | No. | % |
| Work disability[b] | 819 | 5.7 | 116 | 5.1 |
| Age, years[c] | 29 (26, 37) | | 32 (27, 40) | |
| Year[c] | 1995 (1985, 2005) | | 1991 (1985, 2001) | |
| Family income[c,d] | 6.1 (3.7, 9.2) | | 7.4 (4.9, 10.3) | |

[a] Family's socioeconomic status when respondent was growing up.
[b] Respondent had disability that limited the type or amount of work they could do.
[c] Values are expressed as median (1st quartile, 3rd quartile)
[d] Family income in tens of thousands of 2017 dollars.

scenarios, we eliminated censoring from administrative causes, loss to follow-up, and competing events (death) by simulating respondents' outcomes beyond the waves in which they were observed censored (45). We assumed censoring was random within levels of measured confounders (45).

Finally, using the simulations described above, we estimated the cumulative incidence of each outcome in each scenario through the end of follow-up (32 years for SRH and 16 years for mental illness) (45). We calculated risk ratios and risk differences by comparing the cumulative incidences in the union and nonunion scenarios (45). To calculate confidence intervals, we repeated the g-formula algorithm on 250 bootstrap samples and based the lower and upper bounds on percentiles of the bootstrap distributions (45). We tested for potential model misspecification by comparing the simulated variable distributions at each timepoint in the natural course with those in the observed data (32–34, 45).

*Secondary analyses.* In secondary analyses, we examined whether the relative risks and risk differences varied by sex (female/male), sex and race (person of color/White), and sex and education (up to high-school education/beyond high-school) by running the approach outlined above in each subgroup.

*Sensitivity analyses.* First, to test our results' sensitivity to the exposure lag, we used an unlagged exposure. Second, instead of treating death as a censoring event and estimating cause-specific risks of the outcomes, we allowed for deaths during follow-up and estimated subdistribution risks, which might more closely correspond to risks in real-world scenarios (51); see Web Appendix 3 for details. Third, due to difficulties accurately modeling time-varying occupation and industry, we examined whether treating the variables as time-invariant affected our results. Fourth, we probed for residual geographic confounding by including baseline state of residence rather than baseline division of residence as a covariate in our models. We did not run this specification in subgroups due to small cell sizes. Finally, to compare our g-formula estimates with traditional estimates, we fitted confounder-adjusted Cox models (52). These models had a baseline union-membership exposure, baseline confounders as covariates, and incident SRH or K6 as outcomes. As in the g-formula analyses, all respondents were employed at baseline.

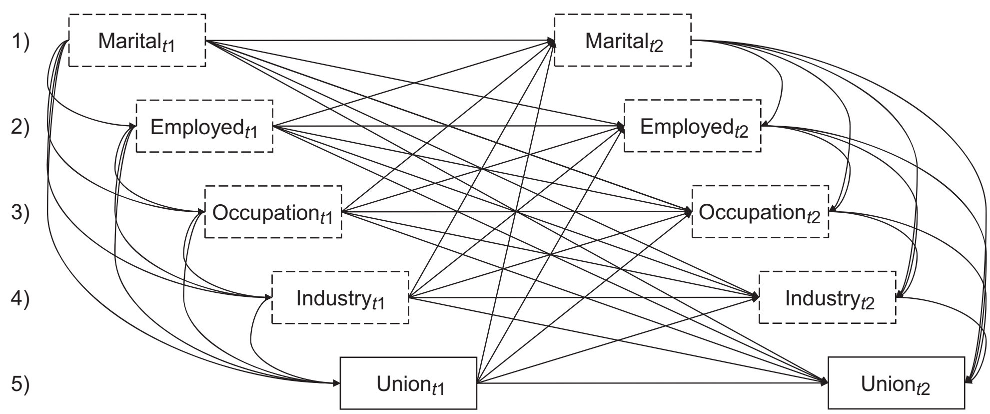


**Figure 1.** Hypothesized temporal ordering of time-varying confounders and exposure in parametric g-formula analyses. Time-varying confounders and exposure in wave $t_k$ were functions of baseline confounders, prior time-varying variables in $t_k$ (if any), time-varying variables in $t_{k-1}$, year, and follow-up time.

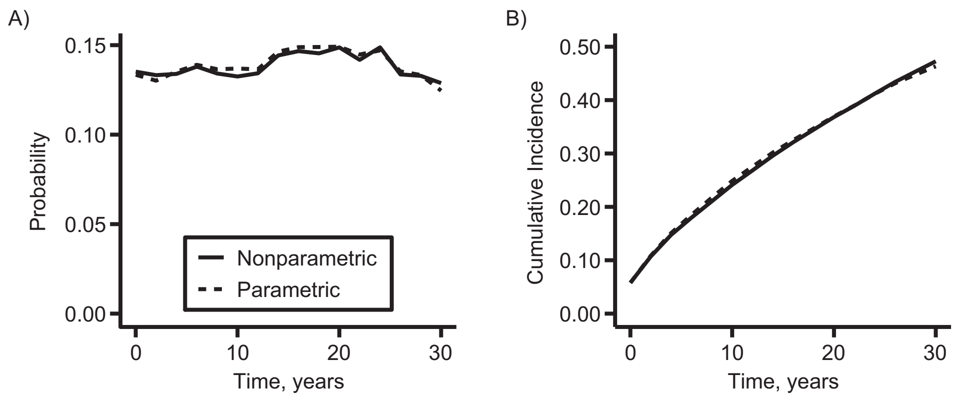


**Figure 2.** Simulated (parametric) probability of 2-year-lagged union membership (A) and cumulative incidence of poor/fair self-rated health (B) during follow-up in the natural course compared with the observed (nonparametric) distributions, Panel Study of Income Dynamics, United States, 1985–2017. Time 0 in panel A occurred 2 years prior to time 0 in panel B, given exposure lag.

*Missing data.* Our exposure, outcome, and confounders contained a small amount of missingness (<4%). To address missingness in time-invariant confounders, we carried respondents' observed values forward (and backward if necessary) when possible. To address remaining missingness in confounders and exposure, we performed a single multivariate imputation by chained equations with 25 iterations using the "mice" package (53) in R (R Foundation for Statistical Computing). The imputation models included as predictors all baseline confounders, time-varying exposure and confounders in $t_k$ and $t_{k-1}$ (or $t_{k+1}$ in respondents' baseline wave), and time-varying SRH in $t_k$ and $t_{k-1}$ (or $t_{k+1}$ in respondents' baseline wave). We did not use imputed SRH or K6 values in our outcome analyses, nor did we create multiple imputed data sets, because doing so in a parametric g-formula setting was not computationally feasible, and methods for pooling estimates after imputation in such a setting have not been well-developed. Estimates from complete-case analyses were similar.

## RESULTS

### Descriptive results

The SRH analyses used data on 16,719 respondents with 3,878 events and 87,422 observations, while the mental-illness analyses used data on 5,813 respondents with 1,981 events and 20,920 observations. At baseline in the SRH analyses, 16% of respondents were union workers (Table 1). Compared with nonunion workers, union workers were more likely to be older, less educated, persons of color, men, married/cohabiting, living outside the South, and to have grown up poor. Moreover, union workers more often worked in "operator, fabricator, and laborer" and "precision production, craft, and repair" occupations, as well as in "manufacturing" and "transportation, communications, and other public utilities" industries. Finally, union workers' median family incomes were 21% higher than nonunion workers' median family incomes. Web Figures 4–6 display trends in union membership over follow-up according to demographic, occupation, and industry.

### Union membership, self-rated health, and mental illness

Overall, the simulated cumulative incidence of poor/fair SRH by the end of follow-up in the natural course was 47% (Figure 2); the corresponding figure for moderate mental illness was 45% (Figure 3). Across subgroups, the incidence of the outcomes was greater among women, people of color, and the less educated than among men, White people, and the more-educated (Tables 2 and 3), although racial inequities were smaller for mental illness than for SRH. In all analyses, the simulated incidence in the natural course aligned with the observed incidence, as did the simulated probability of union membership (Figures 2 and 3 and Web Figures 8–29). However, although the simulated probability of employment status aligned with the observed probability, the simulated distributions of other time-varying confounders tended to differ from the observed distributions more considerably, particularly occupation and industry (Web Figures 8–29).

In the SRH analyses in the full sample, 9% of person-years in the union scenario were spent not employed, lower than the 12% in the nonunion scenario. However, the union scenario was not associated with a lower incidence of poor/fair SRH than the nonunion scenario (relative risk (RR) = 1.01, 95% confidence interval (CI): 0.95, 1.09; risk difference (RD) = 0.01, 95% CI: −0.03, 0.04) (Table 2). This null association largely remained across subgroups, although the union scenario appeared somewhat protective for men (RR = 0.94, 95% CI: 0.87, 1.02; RD = −0.03, 95% CI: −0.06, 0.01), particularly men of color (RR = 0.90, 95% CI: 0.79, 1.00; RD = −0.06, 95% CI: −0.12, 0.00), and somewhat harmful for women (RR = 1.10, 95% CI: 1.00, 1.19; RD = 0.05, 95% CI: 0.00, 0.08), particularly less-educated women (RR = 1.17, 95% CI: 1.05, 1.28; RD = 0.09, 95% CI: 0.03, 0.15).

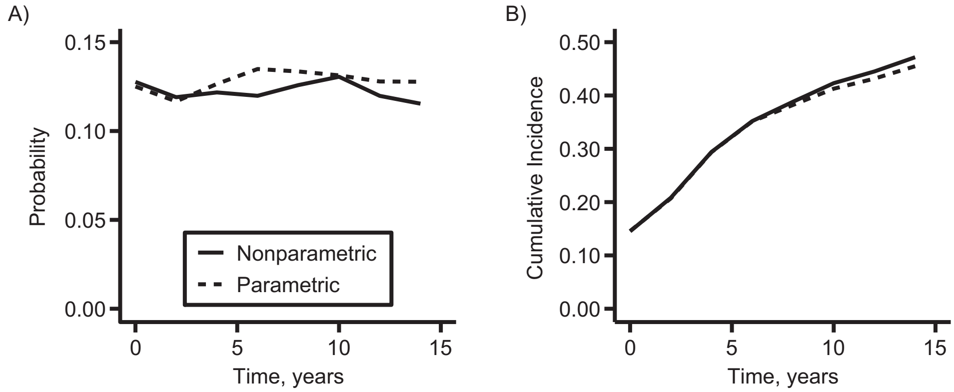


**Figure 3.** Simulated (parametric) probability of 2-year-lagged union membership (A) and cumulative incidence of moderate mental illness (6-item Kessler Psychological Distress Scale score of ≥5) (B) during follow-up in the natural course compared with the observed (nonparametric) distributions, Panel Study of Income Dynamics, United States, 2001–2017. Time 0 in panel A occurred 2 years prior to time 0 in panel B, given exposure lag.

In the mental-illness analyses in the full sample, 7% of person-years in the union scenario were spent not employed, lower than the 8% in the nonunion scenario. However, the union scenario was not associated with a lower incidence of moderate mental illness than the nonunion scenario (RR = 1.02, 95% CI: 0.92, 1.12; RD = 0.01, 95% CI: −0.04, 0.06) (Table 3). This null association largely remained across subgroups, although the union scenario appeared somewhat protective for women of color (RR = 0.90, 95% CI: 0.74, 1.06; RD = −0.05, 95% CI: −0.15, 0.03), men of color (RR = 0.89, 95% CI: 0.66, 1.15; RD = −0.06, 95% CI: −0.17, 0.07), and more-educated men (RR = 0.90, 95% CI: 0.70, 1.11; RD = −0.04, 95% CI: −0.13, 0.05) and was somewhat harmful for White women (RR = 1.17, 95% CI: 1.00, 1.37; RD = 0.08, 95% CI: 0.00, 0.18), less-educated women (RR = 1.10, 95% CI: 0.90, 1.31; RD = 0.05, 95% CI: −0.05, 0.16), and less-educated men (RR = 1.11, 95% CI: 0.89, 1.34; RD = 0.05, 95% CI: −0.05, 0.15).

### Sensitivity analyses

Using an unlagged exposure did not meaningfully affect our estimates (Web Tables 4–5). Estimating subdistribution risks (Web Table 6), treating occupation and industry as baseline variables (Web Tables 7–8), and including state of residence as a covariate (Web Table 9) did not meaningfully affect our estimates either. However, although using Cox models did not meaningfully affect most estimates, union membership appeared more harmful for mental illness in certain subgroups (Web Table 10).

## DISCUSSION

### Summary of results and comparison with prior research

Using a parametric g-formula approach, we estimated how a scenario setting all (versus none) of respondents' 2-year-lagged employed-person-years to union-member employed-person-years would affect incidence of poor/fair SRH and moderate mental illness in a sample of working-age adults with labor-force attachment. Contrary to expectations, the scenario was not associated with reduced incidence of the outcomes in the full sample. Moreover, although we found larger beneficial associations among certain marginalized subgroups, such as with SRH among men of color and with mental health among men and women color, we also had contradictory findings, such as harmful associations with SRH among women of color and the less-educated.

To our knowledge, Reynolds and Brady (21) is the only prior US-based study on union membership and SRH among this age group; there have been none on union membership and mental illness. Although Reynolds and Brady identified a modest protective association between union membership and SRH, particularly among male, less-educated, and lower-income workers, the study is not directly comparable to ours given its cross-sectional design.

Our modest findings might be because union membership's salutary effects on working conditions, wages, and benefits (7, 13, 21, 23) are too weak to measurably improve these health outcomes, particularly given diminishing union power over the study period (54). Although the union wage premium remained unchanged (23), certain union hierarchies grew disconnected from their rank-and-file membership (54), suggesting union membership's solidarity-promoting and alienation-reducing effects might have weakened. Additionally, racism and sexism in the union movement might have undermined union membership's health benefits among marginalized workers. Although many unions have endeavored to protect members against workplace discrimination and harassment, some unions, especially those that remain White- or male-dominated, have not (13).

Our modest findings might also be from bias. First, union membership might be misclassified. For example, Card (55) found that 2.5%–3.0% of 1977 Current Population Survey respondents misreported their union status, true status aside. At the average union-membership prevalence observed in

**Table 2.** Parametric G-Formula Estimates of 32-Year Risk of Poor/Fair Self-Rated Health in Union and Nonunion Scenarios[a] From Simulations Using Data From the Panel Study of Income Dynamics, United States, 1985–2017

| Stratification | No. of Respondents[b] | No. of Observations[b] | Union-Scenario Risk | Nonunion-Scenario Risk | RR[c] | 95% CI[c] | RD[c] | 95% CI[c] |
|---|---|---|---|---|---|---|---|---|
| Overall | 16,719 | 87,422 | 0.47 | 0.46 | 1.01 | 0.95, 1.09 | 0.01 | −0.03, 0.04 |
| Sex | | | | | | | | |
| Women | 8,525 | 45,288 | 0.50 | 0.46 | 1.10 | 1.00, 1.19 | 0.05 | 0.00, 0.08 |
| Men | 8,194 | 42,134 | 0.45 | 0.48 | 0.94 | 0.87, 1.02 | −0.03 | −0.06, 0.01 |
| Sex and race | | | | | | | | |
| Women of color[d] | 3,351 | 15,557 | 0.63 | 0.58 | 1.10 | 0.99, 1.20 | 0.06 | −0.01, 0.12 |
| White women[e] | 5,142 | 29,603 | 0.42 | 0.38 | 1.09 | 0.96, 1.25 | 0.03 | −0.02, 0.09 |
| Men of color | 3,000 | 12,609 | 0.56 | 0.62 | 0.90 | 0.79, 1.00 | −0.06 | −0.12, 0.00 |
| White men | 5,194 | 29,525 | 0.38 | 0.39 | 0.98 | 0.86, 1.11 | −0.01 | −0.05, 0.04 |
| Sex and education | | | | | | | | |
| Women, up to high-school graduation[f] | 3,903 | 19,586 | 0.64 | 0.55 | 1.17 | 1.05, 1.28 | 0.09 | 0.03, 0.15 |
| Women, beyond high school[g] | 4,563 | 25,249 | 0.39 | 0.36 | 1.07 | 0.90, 1.23 | 0.02 | −0.04, 0.08 |
| Men, up to high-school graduation | 4,040 | 18,604 | 0.56 | 0.60 | 0.95 | 0.86, 1.04 | −0.03 | −0.08, 0.02 |
| Men, beyond high school | 4,154 | 23,530 | 0.32 | 0.33 | 0.96 | 0.80, 1.14 | −0.01 | −0.07, 0.04 |

Abbreviations: CI, confidence interval; RD, risk difference; RR, risk ratio.

[a] In the union scenario, all 2-year-lagged employed-person-years set to union-member employed-person-years, while in the nonunion scenario, no 2-year-lagged employed-person-years set to union-member employed-person-years.

[b] Unique respondents and observations in sample used to fit pooled time-varying exposure, confounder, and outcome models. The Monte Carlo pseudosample used in simulations had 25,000 respondents.

[c] Risk ratio and risk difference estimates compare risk (i.e., cumulative incidence) in the union scenario relative to risk in the nonunion scenario. Confidence intervals calculated from nonparametric bootstrap with 250 repetitions. Subgroup estimates produced from stratified models.

[d] Analysis excluded 6 respondents ever employed in “mining” industry due to small counts in that stratum, which produced bootstrap samples with 0 mining-industry respondents.

[e] Analysis excluded 26 respondents ever employed in “mining” industry.

[f] Analysis excluded 14 respondents ever employed in “mining” industry.

[g] Analysis excluded 43 respondents ever employed in “mining” industry or “farming, forestry, and fishing” occupation.

our SRH analyses (14%), a 2.5% misclassification rate would mean 16% of workers classified as union were actually nonunion (Web Appendix 4 and Web Figure 7), causing bias towards the null. To our knowledge, there is no research on the accuracy of PSID’s union-membership data. Second, research suggests preexisting workplace-level characteristics, such as hazardous working conditions, might cause workers to unionize; this could partially explain why certain quantitative studies—conflicting historical and anecdotal evidence—have found that unionization correlates with increased occupational injury risk (19). Although we adjusted for respondent-level occupation and industry, we did not have workplace-level data. Thus, unmeasured confounding by workplace-level factors might have caused us to underestimate union membership’s protective effects. Finally, respondents with similar objective health statuses might have differentially assessed their SRH and mental health depending upon their union status. For example, respondents might compare themselves with peers when evaluating their health (56). In our study, if these peers included respondents’ coworkers (who were likely union-member coworkers for union-member respondents), union membership might not appear to improve health, true effects on objective health status aside. Although this bias is unlikely to be severe, given that prior studies have identified substantial racial and SES disparities in these outcomes, it might have contributed to our findings (57).

### Strengths and limitations

Our study’s strengths included first, a large sample with extensive follow-up and confounder data. To our knowledge,

**Table 3.** Parametric G-Formula Estimates of 16-Year Risk of Moderate Mental Illness (6-Item Kessler Psychological Distress Scale Score of ≥5) in Union and Nonunion Scenarios[a] From Simulations Using Data From the Panel Study of Income Dynamics, United States, 2001–2017

| Stratification | No. of Respondents[b] | No. of Observations[b] | Union-Scenario Risk | Nonunion-Scenario Risk | RR[c] | 95% CI[c] | RD[c] | 95% CI[c] |
|---|---|---|---|---|---|---|---|---|
| Overall | 5,813 | 20,920 | 0.46 | 0.45 | 1.02 | 0.91, 1.12 | 0.01 | −0.04, 0.06 |
| Sex | | | | | | | | |
| Women | 3,376 | 12,185 | 0.54 | 0.51 | 1.04 | 0.91, 1.19 | 0.02 | −0.04, 0.10 |
| Men | 2,437 | 8,735 | 0.42 | 0.42 | 1.01 | 0.86, 1.19 | 0.00 | −0.06, 0.08 |
| Sex and race | | | | | | | | |
| Women of color[d] | 1,528 | 5,415 | 0.49 | 0.55 | 0.90 | 0.74, 1.06 | −0.05 | −0.15, 0.03 |
| White women[e] | 1,832 | 6,711 | 0.58 | 0.49 | 1.17 | 1.00, 1.37 | 0.08 | 0.00, 0.18 |
| Men of color[f] | 881 | 2,892 | 0.46 | 0.52 | 0.89 | 0.66, 1.15 | −0.06 | −0.17, 0.07 |
| White men | 1,553 | 5,835 | 0.44 | 0.41 | 1.07 | 0.85, 1.27 | 0.03 | −0.06, 0.11 |
| Sex and education | | | | | | | | |
| Women, up to high-school graduation | 1,340 | 4,777 | 0.58 | 0.53 | 1.10 | 0.90, 1.31 | 0.05 | −0.05, 0.16 |
| Women, beyond high school[g] | 2,027 | 7,378 | 0.46 | 0.45 | 1.01 | 0.82, 1.19 | 0.00 | −0.08, 0.08 |
| Men, up to high-school graduation | 979 | 3,323 | 0.54 | 0.49 | 1.11 | 0.89, 1.34 | 0.05 | −0.05, 0.15 |
| Men, beyond high school[h] | 1,447 | 5,367 | 0.39 | 0.43 | 0.90 | 0.70, 1.11 | −0.04 | −0.13, 0.05 |

Abbreviations: CI, confidence interval; RD, risk difference; RR, risk ratio.

[a] In the union scenario, all 2-year-lagged employed-person-years set to union-member employed-person-years, while in the nonunion scenario, no 2-year-lagged employed-person-years set to union-member employed-person-years.

[b] Unique respondents and observations in sample used to fit pooled time-varying exposure, confounder, and outcome models. The Monte Carlo pseudosample used in simulations had 25,000 respondents.

[c] Risk ratio and risk difference estimates compare risk (i.e., cumulative incidence) in union scenario relative to risk in the nonunion scenario. Confidence intervals calculated from nonparametric bootstrap with 250 repetitions. Subgroup estimates produced from stratified models.

[d] Analysis excluded 2 respondents ever employed in “mining” industry due to small counts in that stratum, which produced bootstrap samples with 0 mining-industry respondents.

[e] Analysis excluded 14 respondents ever employed in “mining” industry or “farming, forestry, and fishing” occupation.

[f] Analysis excluded 3 respondents ever employed in “mining” industry.

[g] Analysis excluded 9 respondents ever employed in “mining” industry or “farming, forestry, and fishing” occupation.

[h] Analysis excluded 11 respondents ever employed in “farming, forestry, and fishing” occupation.

our study is the first since Waitzman’s 1988 study, which included only men, to analyze the union-health relationship longitudinally among this age group. Second, few social-epidemiologic studies have used parametric g-formula approaches. Our study demonstrated the approach’s benefits, including flexible estimation of scenario contrasts, as well as drawbacks, including computational intensiveness (our SRH analyses ran for 8 days in parallel on a high-performance computing cluster). Finally, our parametric g-formula approach addressed potential healthy-worker survivor bias and other forms of time-varying confounding. Moreover, unlike other approaches often used to address such biases, such as marginal structural modeling, our approach avoided nonpositivity bias by requiring respondents only to be eligible for union membership when employed (46, 50).

In addition to misclassification and firm-level confounding, our study limitations included potential violations of the no-model-misspecification assumption. Although the simulated exposure and outcome distributions in the natural course resembled the observed distributions, the simulated distributions of certain time-varying covariates—particularly occupation and industry—differed from the observed distributions more considerably. Nonetheless, we do not think residual confounding by measured covariates had an undue influence because: 1) we accurately modeled employment status, our most important confounder; and 2) our results were consistent across modeling specifications. Moreover, Cox models yielded estimates similar to our g-formula analyses. Although Cox estimates are not directly comparable to g-formula estimates, the estimates’ similarity suggests that our modest findings were not an artifact of our approach. Additional limitations include potential violations of the consistency and no-interference assumptions. Regarding consistency, we assumed the union-health association did not vary by region, sector, or year, a strong assumption given the variability in union types (e.g., militant/conservative) and declining union power over the period of follow-up (58). This heterogeneity would violate consistency, although our subgroup stratification might have proxied for sector and region. Moreover, the union-wage premium's consistency over the last few decades suggests that temporal changes in the union-health association might be modest (23). Regarding interference, research suggests that unions might improve health-related factors not only among union workers but also among nonunion workers by raising prevailing standards in nonunionized workplaces in similar industries and regions (59). Such spillovers, which would violate no-interference, could be especially likely in this study because PSID recruited respondents through familial networks. Although spillovers could downwardly bias effect estimates, our adjustment for broad industry and region categories might have mitigated such bias, which might be strongest when adjusting for detailed industry and region categories (40). Finally, our study contrasted unlikely always-union-when-employed/never-union-when-employed scenarios. Although the contrast increased our sensitivity to detect a union-health association, such extreme changes in union density—if they occurred in the real world—could have myriad society-wide political and economic repercussions (59). We did not incorporate societal-level effects into our analyses. Moreover, societal-level effects could cause consistency violations if they modified the union-health association.

### Future directions

Given union-membership's roles in wages, benefits, occupational safety, and worker power, our modest findings raise questions that should be pursued in future research. For one, researchers could consider additional outcomes, including mortality, which might be more reliable than SRH and mental illness. Researchers could also consider how area-level union density—which prior research has associated with reduced cause-specific mortality (14–18)—and other area-level labor-related factors, like right-to-work laws and strike rates, interact with individual-level union membership to affect health.

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