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Structural sexism and Women's alcohol use in the United States, 1988–2016

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ABSTRACT

Background: Women's alcohol consumption and binge drinking have increased concurrent with socio-economic gains and may be related to structural sexism.
Methods: We examined associations between structural sexism (state-level sex inequality in political/economic status), and alcohol outcomes among women in Monitoring the Future (N = 20,859) from 1988 to 2016 (ages 27–45 in 2016). We controlled for state and individual confounders and tested three mediators: depressive symptoms, restrictive alcohol norms, and college completion.
Results: Increased structural sexism was associated with decreased alcohol consumption frequency (RR: 0.974, 95% CI: 0.971, 0.976) and binge drinking probability (OR: 0.917, 95% CI: 0.909, 0.926). Norms and education but not depressive symptoms partially mediated these relationships.
Conclusion: Among women in the midlife in recent years, lower levels of state structural sexism were associated with greater alcohol consumption and binge drinking. These findings suggest that as states become more gender-equal—which confer numerous benefits for women's rights and health—additional resources and messaging may be required to prevent harmful alcohol use among women.

1. Introduction

Alcohol use is a cause of heart disease (Roerecke and Rehm, 2014), cancer (Rumgay et al., 2021), vehicle crashes (Bergen et al., 2011), stroke (Patra et al., 2010), kidney disease (Schaeffner and Ritz, 2012), and suicide (Sher, 2005)—six of the ten leading causes of death in the United States (Kochanek et al., 2019)—and contributes to 10% of deaths each year among adults (Stahre et al., 2014). Acutely, binge drinking (drinking at least 4 drinks in a single setting for women, and 5 for men) causes injury, toxicity, and violence; chronically, binge drinking causes cardiovascular disease, cancers, and liver disease (Gmel et al., 2011).

Historically, men on average consume alcohol and binge drink at higher proportions than women (Berkowitz and Perkins, 1987; Perkins, 1992). In recent decades, however, rates of alcohol consumption and binge drinking have increased by approximately 1% every year among adult women, while men's rates have remained largely static (Grucza et al., 2018). Recent trends in women's drinking are largely driven by increases in women in the midlife, i.e. age 30–49, corresponding to those born in the 1970s and 1980s (Keyes et al., 2019b).

This cohort of women entered adulthood during dramatic socio-economic shifts for women. These include more female representation in the labor market, in higher education, and at higher incomes, as well as shifting ideas about women's roles after the Women's Rights Movement in the 1960s–1970s. While gender gaps in US education, income, and employment have on average been narrowing (Bureau of Labor Statistics, 2019), differences remain in women's social, economic, and educational attainments relative to men's that vary across states, leading to different state socioeconomic contexts for women (American Association of University Women, 2020; Goodwin-White, 2018). Such social and economic gender inequalities are indicators of *structural sexism*: structural sexism describes the macro-level—i.e., institutional, systemic, or cultural—systematic ways that societies and institutions cultivate unequal gender-based hierarchies through mutually reinforcing systems across multiple economic and social domains (Homan, 2019; Krieger, 2020). Structural sexism is enacted through policies and practices that disadvantage women—for example, historic laws preventing women from opening a credit card account without their husbands' permission, restrictive reproductive rights laws, failure to ratify the Equal Rights Amendment—as well as through cultural ideologies and normative attitudes regarding gender roles—for example, the cultural belief that

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women who parent should not work. These policies and practices manifest as gendered inequalities in economic, political, and social resources (Homan, 2019; Krieger, 2020), which are hypothesized to drive gender disparities in health.

Over forty years of feminist scholarship has examined the relationship between structural sexism in the United States and women's health outcomes (for reviews, see (King et al., 2020; Milner et al., 2021; Roberts, 2011)) using a variety of indicators of structural sexism. Generally, these indicators capture historical underrepresentation and devaluation of women relative to men (most commonly, in financial, labor, education, and political equality) and therefore indicate both structural sexism and mechanisms through which it may operate. For some health outcomes, reductions in structural sexism have produced benefits to women's health – for example, women in states with lower levels of structural sexism have lower probabilities of chronic illness, poor self-rated health, and depression than women in states with higher levels of structural sexism (Chen et al., 2005; Homan, 2019; Jun et al., 2004; McLaughlin et al., 2011). However, for other women's health outcomes, including violence victimization and harms attributable to alcohol (e.g., vandalization, assault, feeling harassed or threated due to someone's drinking), the relationship is less clear and findings have conflicted (e.g., (Karriker-Jaffe et al., 2019; Martin et al., 2006; Whaley, 2001)).

Competing hypotheses (e.g., (Backhans et al., 2007; Martin et al., 2006; Whaley, 2001; Yllo and Straus, 1984)) posit that reductions in structural sexism will improve women's health (the "amelioration" hypothesis), worsen women's health (the "backlash" hypothesis), or contribute to "convergence" between men's and women's health, i.e., that women's health will become more like men's. In the case of gender gaps that have historically advantaged women—including alcohol use—convergence could have detrimental implications for women's health, as convergence theory implies that rates of women's alcohol use may increase to approach men's when structural environments become more gender equal. Two potential mediators of such convergence may be changes in alcohol norms and women's social positions (Backhans et al., 2007; King et al., 2020; Schmidt, 2014).

Three studies have used cross-national data to examine relationships between structural sexism and alcohol consumption among women and found mixed evidence for convergence or other changes in women's alcohol consumption patterns. The first (Rahav et al., 2006) found that countries with higher levels of economic and political gender equality (i.e., lower sexism) had narrower gender gaps in alcohol consumption. The second found that the gender wage gap was unrelated to women's average alcohol consumption volume among mothers age 24 to 49 (Kuntsche et al., 2011). The third showed that higher levels of gender equality were related to increased odds of consuming alcohol (vs. abstaining) among men and women with children, with stronger effects of gender equality among women (Graham et al., 2020). Within the United States, a single cross-sectional study (Roberts, 2012) tested associations of five separate indicators of women's status (including socioeconomic and political gender equality, reproductive rights, policies regarding violence against women) with women's and men's alcohol consumption. When controlled for other state-level confounders, this study found no meaningful associations of any indicator with alcohol consumption. While this early study was important, it used data from 2005, the beginning of the time when alcohol researchers began to observe increases in women's drinking (Grant et al., 2017a; Grucza et al., 2018) and controlled for individual variables, including education, employment, and income, which may be mediators or moderators, rather than confounders. No U.S. research has examined associations between structural sexism and women's alcohol consumption among the cohort of women born in the 1970s and 1980s who have concerning levels of increased alcohol consumption compared to previous cohorts, nor examined these associations longitudinally, nor used data from the past 15 years.

The present study examines associations between state-level structural sexism and women's alcohol consumption and binge drinking among women born 1970–1987, corresponding to the cohort of women that has most increased alcohol consumption in recent years. Consistent with the convergence hypothesis and some of its potential mediators, we hypothesized that lower levels of structural sexism would be associated with increases in alcohol use and binge drinking among women, and that this relationship would be mediated through greater individual social position, i.e., college attainment, and through less restrictive alcohol norms.

However, the relationship between structural sexism and alcohol consumption patterns may either be the inverse of the hypothesized direction due to a competing mechanistic pathway: the impact of structural sexism on mental health. Women in states with lower levels of structural sexism have lower odds of psychiatric disorders, including depression (Chen et al., 2005; McLaughlin et al., 2011), which is associated with alcohol consumption (Cranford et al., 2011). Therefore, a second, competing hypothesis is that because lower state structural sexism is protective against mood disorders, women in lower structural sexism states may be at lower rather than higher risk of alcohol consumption and binge drinking. We therefore test not only the overall associations but also three potential mediators of this relationship: college completion, restrictive alcohol norms, and depressive symptoms.

2. Methods

2.1. Sample

Monitoring the Future (MTF) includes an ongoing, prospective cohort of high school seniors followed every 2 years until age 29/30 (follow-up waves 1–6), and then at ages 35, 40, and 45 (waves 7–9) (Schulenberg et al., 2020). Baseline surveys were administered within schools, and follow-up surveys were administered via mail. The cohorts of interest for this study were born between 1970 and 1987. The eligible sample was MTF respondents who reported their sex as female, who lived in the United States (excluding Washington, D.C.), and who were high school seniors between 1988 and 2006, surveyed 1988-2016 from approximately age 18 (senior year of high school) through approximately age 45. By 2016, all respondents had received the wave 5 follow up (corresponding to approximately ages 27/28), and the oldest respondents had received the wave 9 follow-up (age 45). 2016 from approximately age 18 (senior year of high school) through approximately age 45. Recruitment is ongoing; therefore, women born later have had fewer opportunities for follow-up. To account for unbalanced follow-up, we included sensitivity analyses of main models in samples through the 5th follow-up only, described in more detail in the Supplement; effect estimates in this sample were ultimately similar in magnitude to effect estimates in the main models. Supplemental Table 1 shows the eligible sample (N = 23,862 unique respondents at baseline).

2.2. Outcomes

Outcomes were assessed for all respondents at each wave. Alcohol consumption frequency was ascertained by asking, "On how many occasions have you had any alcoholic beverage to drink – more than just a few sips – during the last 30 days?" Response options were ordinal, including "0 occasions," "1–2 occasions," "3–5 occasions," "6–9 occasions," "10–19 occasions," "20–39 occasions," and "40 or more occasions."

Binge drinking frequency was ascertained by asking, "Think back over the last two weeks. How many times have you had five or more drinks in a row?" Approximately 70% of responses reported no binge drinking in the past two weeks, therefore, binge drinking was dichotomized as "none" or "at least once."

If respondents reported they had never consumed alcohol or had not consumed alcohol in the past twelve months, they were coded as "0" alcohol consumption occasions and "none" for binge drinking.

2.3. Exposure

We chose among indicators of structural gender-based inequality and used factor analysis to construct a scalar score for structural sexism at the state level for each year. Factor-analytically derived structural sexism scales developed for use across US states have previously employed cross-sectional measures of sex inequalities in income, education, and labor force engagement (Martin et al., 2006; Vieraitis et al., 2008, 2015; Whaley et al., 2013; Whaley and Messner, 2002), and found that factor loadings for state-level indicators of sexism using these economic indicators are generally fairly high in magnitude (e.g., >0.6 for gender inequality in employment, income, managerial/professional occupations, and college completion) (Vieraitis et al., 2008; Whaley and Messner, 2002). However, measures such as gender inequality in political representation and healthcare access may also be important indicators of structural sexism and have been used in other indices (Di Noia, 2002; Griffith and Rose, 2019; Sugarman and Straus, 1988; Yllo, 1983) but have not been empirically evaluated using data reduction techniques.

We selected candidate indicators (N = 11) which were publicly available (i.e., from the US Census or other publicly disseminated data sources) across multiple years, had been used in previous indices, and represented gender inequality across the domains of education, the labor force, income, health access, and political representation. While gender refers to socially prescribed behaviors and dimensions of masculine or feminine experiences, indicators based on publicly available administrative and survey data usually rely on sex differences (Conron et al., 2014; Johnson et al., 2009), i.e., measures comparing male and female representation; we used indicators of sex differences in the present analysis.

Supplemental Table 2 shows the candidate indicators and their data sources. The state-level indicators considered included the following, measured for each of the 50 states every year (N = 11): percentage of male state legislators, male/female ratio for residents living at or above the federal poverty line, male/female ratio for the proportion of adults ages 16 and over in the labor force, male/female ratio for the proportion of working adults in management occupations, male/female ratio for the proportion of working adults who are self-employed, male/female ratio in median income, male/female ratio for the proportion of adults ages 25 and over with at least 4 years of college education, male/female ratio for the proportion of adults with insurance, male/female ratio for proportion of adults who were registered to vote, male/female ratio for proportion of adults who voted, and percent of women living in a county with no abortion provider. We chose not to include specific reproductive policies given that they are often enacted together in legislative packages and highly variable from policy to policy, making them difficult to operationalize in a clear, consistent manner. Instead, as an indicator of reproductive policies, we included a measure of the percentage of women in the state living in a county without an abortion provider, consistent with previous work (Homan, 2019). We derived state-level scores for structural sexism using exploratory and confirmatory factor analysis (described in detail in Appendix A). The exploratory factor analyses indicated that 5 variables best loaded onto a one-factor solution. These were the percentage of male state legislators, male/female ratio for proportion of residents living at or above the federal poverty line, male/female ratio for proportion of adults in the labor force, male/female ratio for proportion of working adults in management occupations, and the male/female ratio for proportion of working adults who are self-employed. Factor loadings for each retained item are shown in Supplemental Table 3. The final confirmatory factor model reflected a two-level (to account for repeated measures by state), one-factor solution, and included a fixed effect for calendar year, as shown in Supplemental Figure 1. Model fit statistics suggested good fit overall (RMSEA = 0.09, TLI = 0.93, CFI = 0.96).

Each state was assigned a model-based factor score for every study year, standardized so that a 1-unit increase represents a 1 standard deviation increase in structural sexism. Descriptive statistics for each state are shown in Supplementary Table 4 and Supplemental Figure 2; scores for each state for each year are available in Supplemental File 1. As a validity check, we compared the factor analytically derived scores in the analytic sample to a previously used indicator of structural sexism (Homan, 2019) in bivariate analyses and found the two to be positively associated ($b = 0.40$, $p < 0.001$ in multilevel linear regression models).

For the purposes of descriptive statistics only (e.g., Table 1), sexism scores were dichotomized into "high" and "low" structural sexism using a median split of values measured across all observations in all study years 1988–2016.

2.4. Confounders

State-level confounders were identified as population density, socioeconomic status, economic inequality, religiosity, and alcohol policy climate. State-level variables were derived from outside sources and linked to individual level MTF data for each respondent based on state of residence at each survey year.

Population density, defined by the US Census as the state-level average population per square mile; these data are available from 1980, 1990, 2000, and 2010. Linear interpolation was used to estimate density between census years. We included this because rates of alcohol consumption are lower in areas with less population density (Dixon and Chartier, 2016).

State-level socioeconomic status was operationalized as the percentage of residents living below the federal poverty level. State-level income inequality was operationalized as the Gini coefficient, a

Table 1
Outcome and covariate distributions among MTF women in eligible sample, 1988–2016, dichotomized by structural sexism level.

	Low sexism[a] (N = 59,931 observations)	High sexism[a] (N = 55,860 observations)	p-value
	N (%) (categorical) Mean (S.D.) (continuous)	N (%) (categorical) Mean (S.D.) (continuous)	
Alcohol outcomes			
Reported any alcohol consumption (dichotomous)	39,379 (66%)	33,701 (60%)	<0.001
Reported any binge drinking (dichotomous)	16,274 (27%)	15,578 (28%)	0.025
State-level confounders			
Percentage of residents who are religious conservatives	17.40 (11.27)	16.73 (11.23)	<0.001
Poverty rate	13.28 (2.83)	13.05 (3.35)	<0.001
GINI	0.61 (0.04)	0.58 (0.03)	<0.001
Population density	207.22 (225.24)	186.62 (212.18)	<0.001
Alcohol policy climate scale	41.63 (7.50)	37.68 (10.06)	<0.001
Individual-level confounders			
Father has college degree	22,353 (39%)	18,941 (35%)	<0.001
White	46,339 (77%)	43,127 (76%)	0.167
Age	26.92 (6.43)	21.29 (3.42)	<0.001
Mediators			
Depressive symptoms	6.48 (2.94)	6.91 (3.20)	<0.001
Endorses restrictive alcohol norms	5890 (16%)	7163 (21%)	<0.001
Completed 4 or more years of college (in follow-up sample)	26,626 (53%)	9502 (35%)	<0.001

[a] For descriptive statistics, high structural sexism refers to states with at or above median level; low structural sexism refers to states below median level.

measure of income inequality ranging from 0 (perfect equality) to 1 (perfect inequality). Both state-level socioeconomic status and income inequality were assessed using measures from the US Census, Current Population Survey, and American Community Survey for all study years. Although area-level socioeconomic status is inconsistently related to higher rates of alcohol consumption (Karriker-Jaffe, 2011), we controlled for both socioeconomic indicators to account for other sources of economic inequality and poverty that are unrelated to sexism.

State-level religious conservatism was operationalized as the percentage of religious conservatives, specifically Evangelical Protestants and Mormons, in each state. These denominations were selected based on traditional views about women's roles and on sanctions on alcohol use, consistent with previous work (Homan, 2019; Roberts, 2012). Higher levels of religiosity are associated with lower levels of alcohol use (Michalak et al., 2007). These data were made available through the Religious Congregation and Membership Surveys, which are assessed every ten years (1980, 1990, 2000, 2010). Linear interpolation was used to estimate percentages in unobserved years.

While the state-level socioeconomic and religious indicators could plausibly operate as mediators rather than confounders—therefore, adjusting for them may attenuate the associations of interest—we made the conservative choice control for them in adjusted models given these pathways were not mediators of interest.

Alcohol policy climate was operationalized using the Alcohol Policy Scale (described in Naimi et al. (2014); briefly, this scale scores state alcohol policy climates based on a composite of efficacy scores of 29 policies shown to reduce alcohol consumption outcomes. These were weighted based on efficacy and strength of implementation, then scored to create a composite scale by the original study authors. Policies included restrictions on days or hours of sale, stringent blood alcohol thresholds for driving, open container laws, and the presence of state alcohol control systems. These data were made available from the scale developers. Scores ranged from 23.2 (laxer policies) to 68.1 (more stringent policies) with a mean of 42.4. States with more stringent policies have lower rates of binge drinking (Naimi et al., 2014). These data were available for 1999–2016, and linear interpolation was used to estimate scale scores in previous years (1988–1998).

All state-level covariates were time-varying, and all covariates estimated with linear interpolation were truncated as needed so that states could not have below-zero values. To confirm that interpolation did not bias results, we performed sensitivity analyses two ways: without control for the variables relying on interpolated data, and examining only the years when data were fully observed.

Regarding individual confounders, while they could not plausibly cause structural sexism, we selected individual measures that are both theoretically and empirically supported causes of alcohol outcomes, and also potential causes of differential *exposure* to sexism—that is, the state they resided in, and the study year(s) that they resided there. These variables included age (time-varying, measured continuously), race/ethnicity (time-invariant) and paternal education (time invariant, dichotomized as whether or not the respondent's father completed college). Paternal, rather than maternal, education was chosen as a confounder because of its influence on childhood socioeconomic status, parental job opportunities during childhood, and adult health (Cohen et al., 2010). In the sample, most respondents (77%) were non-Hispanic White, so race/ethnicity was dichotomized as non-Hispanic White, or not.

2.5. Mediators

Secondary analyses evaluated the potential mediating effects of depressive symptoms, alcohol norms, and education, all measured at the individual level. Among the respondents, approximately 15% were assigned to receive versions of the surveys which queried depressive symptoms during waves 0–6; all respondents were surveyed on depressive symptoms in waves 7–9. Similarly, approximately 80% of respondents received versions of the survey querying alcohol norms during the first 6 study waves.

We used an index of four responses to measure depressive symptoms (Keyes et al., 2019a): "Life often seems meaningless," "The future often seems hopeless," "It feels good to be alive," and "I enjoy life as much as anyone." Responses were Likert-style, ranging from 1 ("Disagree") to 5 ("Agree"). The latter two items were reverse-coded so that higher scores correspond to higher levels of depressive symptoms and all 4 were summed to create a total score, with a possible range between 4 (low depressive symptoms) and 20 (high). These indicators exhibited high reliability (alpha >0.80) across study years and waves (Supplemental Figure 3).

Alcohol norms were assessed via the following question: "Individuals differ in whether or not they disapprove of people doing certain things. Do YOU disapprove of people (who are 18 or older) doing each of the following? Trying one or two drinks of an alcoholic beverage (beer, wine, liquor)." Those who endorsed disapproving or strongly disapproving were coded as having restrictive alcohol norms; those who endorsed approval were not.

Education was time-varying, dichotomized as whether the respondent had completed 4 years of college (or not), and was assessed in all follow-up surveys.

2.6. Confounders of the mediators-outcomes relationships

In mediation models we additionally controlled for two time-varying measures that could confound the mediator-outcome relationships: personal religiosity, operationalized in response to the prompt "How important is religion in your life?" as a binary variable: low ("Not important," "A little important") or high ("Pretty important," "Very Important"); and urbanicity, categorized as living in an urban center (a city with greater than 50,000 residents), suburban (living in a suburb of a city with greater than 50,000 residents), or rural area/small town (living on a farm, in the country, or in a city with fewer than 50,000 residents). Both religiosity and urbanicity are commonly related to alcohol norms, depression, and college completion, as well as alcohol use (Butterfield and Pemberton, 2011; Drabble et al., 2016; Eliassen et al., 2005; Schultz and Neighbors, 2007; Stroope et al., 2015; Wells, 2010). For college completion, three additional mediator-outcome confounders were included to account for baseline predictors of both alcohol use and college completion, all time-invariant and measured at senior year: grade point average (GPA, measured continuously), alcohol use (i.e., baseline measures of alcohol consumption frequency and binge drinking, respectively), and urbanicity.

Regarding exposure-mediator confounders, no new measures were identified beyond those already included as confounders in the main (unmediated) model, described above.

2.7. Missing data

The eligible analytic sample was 118,684 observations, corresponding to 23,862 unique women. Attrition is the modal source of missingness in MTF. The majority of attrition occurs between baseline and the first follow-up (Supplemental Table 1). Respondents were retained in analyses if they responded to at least one survey wave. To account for missingness due to attrition, all analyses were weighted using attrition weights, calculated as the inverse probability of participation at each follow-up based on the following covariates measured at senior year: gender, race/ethnicity, college plans, truancy, high school grades, number of parents in the home, religiosity, maternal and paternal education, alcohol use, cigarette use, marijuana use, other illicit drug use, region, cohort, and sampling weight correcting for oversampling of age 18 substance users. These attrition weights were provided by MTF and were chosen for use to be consistent with previous research using this sample. Though they were calculated using baseline predictors only, previous research on this sample has demonstrated that

both attrition and subsequent adult alcohol outcomes are highly related to senior year alcohol use (Keyes et al., 2020).

A second source of missingness in MTF is item non-response; of the 118,684 observations for sample women between 1988 and 2016, 100,940 (85%) had complete information for all study measures. The primary source of item non-response was for paternal education (6%) and norms (4%) with the remaining measures each having less than 3% of observations missing. To account for potential biases by selective item non-response, multiple imputation by chained equations was used to impute missing values. Ten datasets were imputed, using all study measures and attrition weights as predictors (Seaman et al., 2012), and model estimates were combined using Rubin's Rules (Campion and Rubin, 1989).

2.8. Analyses

Associations between structural sexism and alcohol consumption frequency and binge drinking probability were examined using three-level multilevel models with random intercepts for both individuals and states, with observations at each wave nested within individual respondents, who were nested within states. In the analytic sample, 95% of respondents were in the same state in all waves; however, using nested data for those who did move states could lead to inappropriate estimates of standard error. To account for this potential source of bias, we performed sensitivity analyses examining the study main effects among the sub-sample of respondents who never moved states (N = 113,487 observations, 95% of the sample). Poisson regression models were used for alcohol consumption frequency, and logistic regression models were used for binge drinking probability outcomes. Models are presented unadjusted for confounders, and then sequentially adjusted for state confounders, and then individual confounders. All analyses were weighted using original sampling probabilities to account for complex sample design and also attrition weights.

2.8.1. Mediation analyses

We conducted mediation using traditional mediation analysis (Baron and Kenny, 1986). Prior to analyzing each mediator, we tested the assumptions that the mediator was related to both the outcomes and the exposure, and that there was no mediator-exposure interaction. Candidate mediators that met those assumptions were included as control variables in analytic models to estimate the controlled direct effects and the percentage change in unexponentiated model parameters (i.e., the beta, rather than the odds ratio). Models estimating total effect included control for exposure-outcome confounders, and models estimating controlled direct effects included control for all measured confounders (including mediator-outcome confounders). Given the time-varying structure of the data, this approach does not allow for valid estimates of the indirect effects (Bind et al., 2016; VanderWeele, 2015, 2016; VanderWeele and Tchetgen Tchetgen, 2017); Appendix B includes a detailed description of the limitations of traditional mediation given this data structure, as well as results from the tests of mediation assumptions.

We were unable to assess all mediators in the same analytic model, both because different sub-samples of respondents received questionnaires related to depression and alcohol norms, and because education was invariant at baseline. Therefore, each mediator was tested in a unique model. The first mediation analysis examined whether the relationship between structural sexism and alcohol outcomes was mediated by depressive symptoms. Depressive symptoms were queried on a subset of respondents, beginning in 1989, and for all respondents in follow up 7–9 (N = 29,119 observations). The second mediation analysis examined mediation through restrictive alcohol norms. These were queried on subset for the first 6 study waves only (N = 78,251 observations). The final mediation analysis examined mediation by college completion. All respondents were initially assessed during their senior year of high school, thus education level at their first survey is invariant. College completion was extremely rare in the first follow-up survey, at age 19 or 20 years old. Therefore, for this mediation analysis we subset the analytic sample to only follow-up responses beginning in the wave 2 (N = 73,690 observations).

2.8.2. Secondary analyses

We further tested the extent to which structural sexism influences alcohol outcomes among men in the MTF in the same age group (N = 91,942 observations) as well as by examining interactions between gender and structural sexism in the full MTF eligible sample of both men and women (N = 210,626 observations). Per both convergence theory and observations of national trends in alcohol use by gender, we anticipated that while women's alcohol use would likely increase commensurate with reductions in structural sexism, men's would remain largely stable. However, some previous research suggests that men's health may also be sensitive to changes in structural sexism (King et al., 2020), so we hypothesized that while men's alcohol consumption patterns may be related to structural sexism, any associations would be less pronounced than among women. Finally, we performed six sensitivity analyses to test the robustness of associations to other modeling specifications and with balanced data, all of which are described in detail in Appendix C.

Regression analyses were produced in SAS 9.4, and all other figures were produced using R.

3. Results

Table 1 shows the covariate distribution among observations from sample women, stratified across structural sexism level. For this descriptive table, high structural sexism was defined as at or above the median national level, both alcohol outcomes were dichotomized ("any" vs. "none"), and p-values were calculated using Rao-Scott chi-square tests, which account for non-independence of observations (Rao and Scott, 1987). Observations within states with lower structural sexism evidenced higher state populations densities and less restrictive alcohol policies. Observations from lower structural sexism states compared to higher structural sexism states were related to higher prevalence of any alcohol consumption (66% vs. 60%) and higher education (53% completed college vs. 35%).

Fig. 1 shows the mean score for structural sexism in the analytic sample between 1988 and 2016, which declined by approximately 26% of one standard deviation each year. Trends in indicators of structural sexism, all of which decreased over time (i.e., became more gender equal), are shown in Supplemental Figure 4.

Table 2 shows the estimates examining the relationship between structural sexism, measured continuously, and alcohol outcomes, with and without adjustment for confounders. In fully adjusted models, higher levels of structural sexism were associated with fewer occasions of alcohol consumption (RR: 0.974, 95% CI: 0.971, 0.976) and a lower probability of any binge drinking (OR: 0.917, 95% CI: 0.909, 0.926).

Table 3 shows results for the mediation models. All mediation assumptions were satisfied, with one exception (see Appendix C): in confounder-adjusted models, depressive symptoms were unrelated to alcohol consumption frequency, thus we did not proceed with that specific mediation analysis. For binge drinking, when conditioned on depression, the direct effects were largely unchanged (total effect = 0.884 [95% CI 0.863, 0.906], controlled direct effect = 0.893 [95% CI 0.872, 0.915], 8% change in unexponentiated model parameter). Conditioned on restrictive alcohol norms, the controlled direct effects attenuated both for occasions of alcohol consumption (total effect = 0.966 [95% CI 0.962, 0.969], controlled direct effect = 0.977 [95% CI 0.974, 0.980], 34% change in unexponentiated parameter) and binge drinking (total effect = 0.904 [95% CI 0.893, 0.915], controlled direct effect = 0.926 [95% CI 0.915, 0.938], 24% change in unexponentiated parameter). Conditioned on college completion, the controlled direct effects modestly attenuated for occasions of alcohol consumption (total effect = 0.964 [95% CI 0.961, 0.967], controlled direct effect = 0.973 [0.970, 0.977], 27% change in unexponentiated parameter) but not for

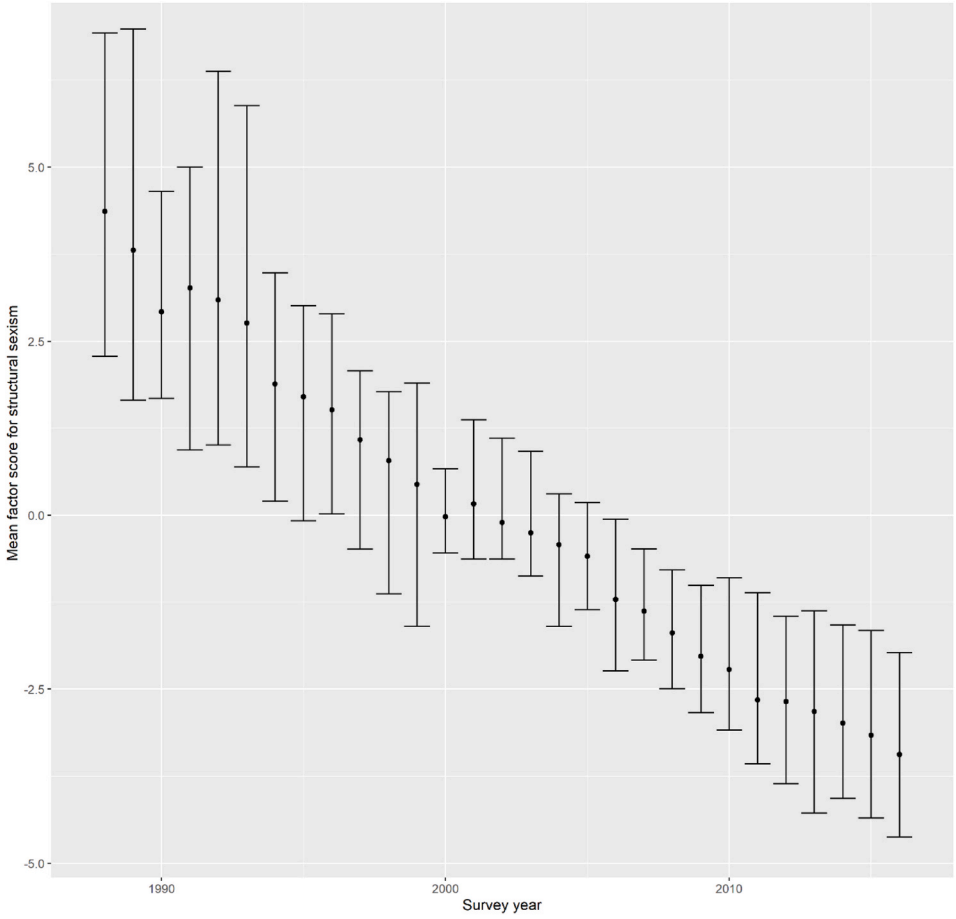

Fig. 1. Average structural sexism scores in MTF sample, 1988–2016.

Table 2
Associations between structural sexism and alcohol consumption outcomes among women in MTF, 1988–2016.

	Alcohol consumption frequency Risk ratio (95% CI)	Binge drinking Odds ratio (95% CI)
Model 1: Structural sexism only	0.980 (0.979, 0.982)	1.041 (1.034, 1.049)
Model 2: Structural sexism, adjusted for state-level[a] confounders	0.977 (0.976, 0.979)	1.035 (1.027, 1.043)
Model 3: Structural sexism, adjusted for state-level[a] and individual[y] confounders	0.974 (0.971, 0.976)	0.917 (0.909, 0.926)

[a] State-level confounders include percentage of residents who are religious conservatives, poverty rate, GINI coefficient, population density, and alcohol policy climate scale.

[y] Individual-level confounders are age, paternal education, and race/ethnicity.

binge drinking (total effect = 0.901 [95% CI 0.889, 0.912], controlled direct effect = 0.903 [95% CI 0.891, 0.916], 3% change in unexponentiated parameter).

In the male sample, as hypothesized, we observed a less pronounced relationship between structural sexism and occasions of alcohol consumption (fully adjusted RR: 0.987, 95% CI: 0.984, 0.990) and binge drinking (fully adjusted OR: 0.976, 95% CI: 0.966, 0.985) than for women. However, only for binge drinking were tests of interaction between structural sexism and gender significant (relative excess risk due to interaction = −0.07, 95% CI: −0.09, −0.05).

Regarding the sensitivity analysis on the sample with balanced data (i.e., restricted to first 5 follow ups), effect estimates were similar in magnitude to those in the main models (RR: 0.978, 95% CI: 0.976, 0.981 for occasions of alcohol consumption; OR: 0.922, 95% CI: 0.913, 0.931 for binge drinking). Results from sensitivity analyses with other model specifications did not meaningfully diverge from the results shown in the main text (Appendix C).

4. Discussion

Consistent with the study hypotheses, both women's alcohol consumption frequencies and binge drinking probabilities were inversely related to structural sexism. Occasions of alcohol consumption declined by 3% and the odds of binge drinking declined by 8% with every 1 S.D. increase in structural sexism. Both associations were partially mediated through restrictive drinking norms but neither relationship was mediated by depression. Alcohol consumption frequency, but not binge drinking, was partially mediated by education. Men's alcohol consumption and binge drinking exhibited inverse relationships to structural sexism as well, but for both outcomes, decreases related to structural sexism were more pronounced among women than among men; this patterning is consistent with findings indicating that reductions in sexism may lead to convergence of women's adverse health behaviors with men's (Backhans et al., 2007; King et al., 2020). Based on this study's findings, reductions in structural sexism may have contributed to national increasing trends in both disproportionate alcohol consumption and disproportionate binge drinking among women.

Neither alcohol frequency nor binge drinking associations were mediated through depressive symptoms. One explanation for the lack of evidence for mediation is that, in the current study, the measure of depressive symptoms is not synonymous with depressive disorder; these

Table 3
Results from mediation analyses examining associations between structural sexism and alcohol consumption frequency among women in MTF, 1988–2016.

Mediator: Depression (N = 29,119 observations)		
	Alcohol consumption frequency	Binge drinking
Total effect of structural sexism in sample	*N/A; depression not related to alcohol consumption frequency in analytic sample*	0.884 (0.863, 0.906)
Controlled[a] direct effect of structural sexism		0.893 (0.872, 0.915)
Percent change in unexponentiated model parameter (beta)		8%
Mediator: Restrictive alcohol norms (N = 78,251 observations)		
Total effect of structural sexism in sample	0.966 (0.962, 0.969)	0.904 (0.893, 0.915)
Controlled[a] direct effect of structural sexism	0.977 (0.974, 0.980)	0.926 (0.915, 0.938)
Percent change in unexponentiated model parameter (beta)	34%	24%
Mediator: College completion (N = 73,690 observations)		
Total effect of structural sexism in sample	0.964 (0.961, 0.967)	0.901 (0.889, 0.912)
Controlled[y] direct effect of structural sexism	0.973 (0.970, 0.977)	0.903 (0.891, 0.916)
Percent change in unexponentiated model parameter (beta)	27%	3%

Note: All models adjusted for percentage of residents who are religious conservatives, poverty rate, GINI coefficient, population density, and alcohol policy climate scale, age, paternal education, and race/ethnicity.
[a] Further controlled for personal religiosity and urbanicity.
[y] Further controlled for personal religiosity, urbanicity, senior year GPA, senior year alcohol use, and senior year urbanicity.

symptoms capture the affective aspects of depression, but do not fully account for the somatic components. Another is that the relationship between depression and alcohol is complex: the causal direction between depression and alcohol use has been debated (i.e., which causes which) and is still unresolved (McHugh and Weiss, 2019). While several studies have examined the impact of structural sexism on depression or depressive symptoms (Chen et al., 2005; McLaughlin et al., 2011), none have examined them in the context of alcohol use. Regardless, the direction of effect overall—i.e., lower risks of alcohol outcomes in high sexism states—suggests that changes in mental health symptomology are not the major operating pathway for these relationships.

Restrictive alcohol norms, however, partially mediated the relationships between structural sexism and both alcohol outcomes. Restrictive norms are related to lower risks of alcohol use (Krieger et al., 2016), and women in high structural sexism states were more likely to endorse restrictive views about alcohol use than those in low structural sexism states. Beyond norms specific to alcohol use, conforming to traditional gender norms is related to alcohol consumption; women who endorse conformity to traditionally masculine norms are at higher risks of alcohol consumption than those who do not (Patro-Hernandez et al., 2020). These shifting norms, and the changing social and economic landscape, have manifested in meaningful changes in women's behavioral health (Heise et al., 2019).

College education partially mediated the relationship between structural sexism and alcohol consumption, but not binge drinking. Disparate findings regarding support for mediation by college completion highlight the different determinants of alcohol consumption frequency and binge drinking; these outcomes exhibit not only different health sequelae but also different predictors, and these may vary across the life-course. While college attendance is frequently characterized by high levels of both alcohol consumption and binge drinking (White and Hingson, 2013)—due to college social contexts and increases in alcohol availability during this time period—after graduation, rates of binge drinking precipitously decline, but alcohol consumption rates remain fairly high (Dawson et al., 2004; Pedersen, 2017). Alcohol consumption is highly normalized among college-educated adults well beyond the college years, in part because higher education confers more opportunities in the labor force and higher-paying jobs, both of which are highly related to alcohol consumption per se but less consistently related to binge drinking (Huckle et al., 2010; Marchand, 2008; Patrick et al., 2021; Prins et al., 2019; Schmidt et al., 2010).

We observed that for both outcomes, the associations with structural sexism were more pronounced among women than among men. While the interaction models for gender were not significant for alcohol consumption frequency, descriptively, the estimates in stratified models sufficiently diverged in magnitude of effect and confidence intervals to suggest that structural sexism disproportionately impacts women's alcohol use at higher rates than men's, and may contribute to converging alcohol risks (Backhans et al., 2007; King et al., 2020; Schmidt, 2014).

To date, only a single study has examined this relationship within the United States, finding no relationship between structural sexism and alcohol consumption frequency or binge drinking (Roberts, 2012). The previous null association was likely due both to sample limitations as well as controlling for individual variables—including education, employment, and income variables—which are likely to be mediators, rather than confounders, of the structural sexism-alcohol relationship. Further, variations in measurement may have contributed to differences in the findings: the previous study made the a priori decision to divide structural sexism into multiple domains (e.g., women's socioeconomic status, political participation, etc.) whereas the empiric model we used indicated that a single factor or domain was sufficient to measure structural sexism, consistent with other previously developed scales measuring the same construct (Vieraitis et al., 2015; Whaley et al., 2013).

A strength of the measure of structural sexism used in the current study is that it spans multiple years and is empirically-derived; however, commonly used indicators (e.g., the wage gap) were not found to be highly correlated with the indicators ultimately retained. This is consistent with previous work showing that the wage gap is not highly correlated with other measures of gender-based inequality in empirically-derived measures (Roberts, 2012; Gillespie and Reckdenwald, 2017). Further, the model-based estimates vary from year to year, so the relative ranks of state sexism scores also varies; we believe for this reason the best use case for these data (which we have made available for other researchers) are in longitudinal studies, given the variation in state scores from year to year. Finally, all indicators composing this measure represent critical economic or political indicators of sexism, but other research has demonstrated different patterning of state sexism values when using alternative measures (e.g., specific reproductive policies, attitudes), and these may indicate different components of structural sexism outside of gender-based economic and political representation disparities which also vary widely across both states and time (Roberts, 2012; Price et al., 2021; McLaughlin et al., 2011).

For both binge drinking and alcohol consumption, observed associations with structural sexism were relatively modest: a 1-unit increase in structural sexism was associated with a reduction in alcohol consumption frequency of approximately 3%, and an 8% reduction in binge drinking probability. Though the associations observed in the current study are small, given both the widespread prevalence of alcohol use and its impact on preventable morbidity and mortality, these increases have substantial implications for population health (Stahre et al., 2014).

Indeed, the magnitudes of observed effects are consistent with the modest but meaningful increases in alcohol consumption and binge drinking observed for women in recent years (Grant et al., 2017b; Hartz et al., 2018; Keyes et al., 2019b). Notably, the association was stronger for binge drinking, consistent with other studies: binge drinking among women has been shown to increase at higher rates than alcohol consumption, up to 6% per year, compared to approximately 1% per year for alcohol consumption per se (Grucza et al., 2018). With regards to clinical impact, the association between structural sexism and binge drinking is more concerning than the association with alcohol consumption frequency, as the magnitude of the effect is higher and binge drinking is a riskier pattern of alcohol intake.

The current research highlights the need to tailor screening and prevention efforts to accommodate the shifting social landscape. Anti-alcohol public health campaigns tailored towards women have not been well-received: consumers have reported frustration after receiving mixed public health messages as to the purported health benefits of alcohol use, and anti-drinking campaigns targeted towards women have received backlash for being sexist and out-of-touch (Constantinou, 2005; Quealy and Sanger-Katz, 2017; Victor, 2016). Focusing on drinking cultures and environments, as well as social roles (e.g., being a worker, being a student) rather than gender alone may be better received, particularly in geographic areas undergoing improvements in gender equality (MacLean et al., 2021). These may include both harm-reduction campaigns (Charlet and Heinz, 2017) and occupational interventions: working adults—and, increasingly, women—spend a large portion of their waking hours at their place of employment (i.e., a captive audience) and employers have a financial interest in keeping them healthy (Ames and Bennett, 2011). Such interventions may play an important role in population health as larger numbers of women enter the workforce.

4.1. Limitations

A limitation of both outcome measures is that they are frequency, rather than volume, measures. Women reporting similar frequencies may consume very heterogeneous quantities of alcohol. However, frequency measures are closely correlated with quantity (volume) measures (Leigh, 2000), and are meaningful predictors of some health complications and mortality, even among those who consume low volumes of alcohol per occasion (Hartz et al., 2017, 2018). Regarding binge drinking, while national guidelines typically measure women's binge drinking as 4 or more drinks in a single setting (NIAAA, 2016), MTF has consistently assessed binge drinking using a threshold of 5 or more drinks; the use of a less sensitive measure may have introduced misclassification of women who binged. However, sensitivity analyses with the subgroup who received a more sensitive measure suggested that the associations were consistent, regardless of the threshold used to determine binge drinking.

Although we controlled for the effects of age, the relationship between structural sexism and alcohol use may vary throughout the life course. Both childhood and adulthood social exposures influence alcohol trajectories (Rahav et al., 2006) and an important area for future investigation is whether the timing and patterning of exposure to structural sexism (i.e., at a young age, or during adulthood) is a meaningful source of variation in women's alcohol use.

5. Conclusion

Decreased structural sexism is a positive social force from both a health and a human rights perspective, leading to numerous societal, health, and personal benefits to women (Heise et al., 2019). Nonetheless, increases in women's equality have conferred some health risks, which need to be countered with public health messaging and evidence-based preventive care. As women have increasingly occupied traditionally male roles and social positions, health risks have changed commensurately because men and women began to share common behaviors and exposures, including alcohol consumption (Rahav et al., 2006; Schmidt, 2014). Promoting greater gender equality for women is not at odds with improving public health surveillance and interventions for women's increases in unhealthy alcohol use—rather, understanding the health-relevant forces in women's lives is essential to tailoring effective prevention and treatment.

These findings illuminate the complexity between health and social systems, and the importance of a critical and thoughtful understanding of the competing social forces guiding individual health behaviors. As US women have become more equal, they may have become more vulnerable to the some of the same adverse social forces that affect men. For example, social scientists have stated concerns regarding "deaths of despair" among Americans in the mid-life; these are inclusive of alcohol-related deaths, and are thought to be associated with macro-level forces including structural violence and inequality, particularly regarding economic forces such as increased income inequality, wage stagnation, and an erosion of labor protections and equitable social policies (Case and Deaton, 2017; Dow et al., 2019). As structural sexism declines, other such well-established sources of inequality may become increasingly more salient to women's health and health behaviors than they have in the past. Investigating and understanding these relationships is essential for surveillance and treatment as the social landscape of gender equality evolves.

Credit author statement

Dr. McKetta contributed to the conceptualization, data curation, formal analysis, methodology, visualization, and writing of this manuscript. Dr. Prins contributed to the methodology, supervision, and writing of this manuscript. Dr. Hasin contributed to the methodology, supervision, and writing of this manuscript. Dr. Patrick contributed to the methodology, supervision, and writing of this manuscript. Dr. Keyes contributed to the conceptualization, investigation, methodology, project administration, resources, software, supervision, and writing of this manuscript.

Declaration of competing interest

None.

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Appendix A. Supplementary data

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