Structural Sexism in the United States and Patterns of Women's Alcohol Use in Recent Decades

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Abstract

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Introduction

Alcohol consumption is a leading cause of morbidity and premature mortality. In the United States, consumption remains highly prevalent, and certain subgroups have been increasing alcohol risks in recent decades. Among these are women in the mid-life, who have increased rates of both alcohol consumption (vs. abstention) and binge drinking (i.e., multiple drinks in a setting). Women's alcohol use has increased concurrent with social and economic gains. These gains in women's social and economic status are indicative of broader declines in structural sexism, a macro-level, systematic source of gender inequality. The objective of this dissertation is to examine the associations between state-level structural sexism (e.g.., social, political, and economic gender inequality) and patterns of women's alcohol consumption. This dissertation is presented in five chapters: first, an introduction; second, a narrative historical review of the relationship between structural sexism in the United States and women's health outcomes, with a lens towards understanding the theoretical and epidemiologic sources of conflicting study findings; third, an empirical study of the relationship between state-level structural sexism and both alcohol consumption and binge drinking among women in the mid-life in recent cohorts;

fourth, an empirical study examining structural sexism as a source of heterogeneity for relationships between women's social positions—namely, their occupational characteristics—and both alcohol consumption and binge drinking; fifth, a discussion of findings and implications for future research.

Materials and methods

The narrative literature review drew from empirical studies in public health, criminology, and sociology (N=43 studies). The two empirical aims used longitudinal data from Monitoring the Future (MTF), a national survey examining substance use throughout young adulthood, using data from cohorts who were high school seniors between 1988-2006. For both empirical aims, I measured structural sexism using a factor-analytically derived score based on state-level social and economic indicators of gender inequality, and assessed occasions of alcohol consumption and probability of binge drinking as study outcomes. Both studies used three-level multilevel models to estimate associations between structural sexism and each alcohol outcome. The first empirical aim included a sample of 23,862 women surveyed between 1988-2016, and beyond the marginal association also tested the role of three mediators: depressive symptoms, college completion, and restrictive alcohol norms. The second empirical aim included a sample of 16,571 women in the MTF follow-up surveys between 1989-2016, and examined whether associations among work status, high-status careers, occupational gender composition, and both alcohol outcomes varied across levels of structural sexism using interaction models between occupational characteristics and state structural sexism.

Results

The review identified the divergent theoretical frameworks and measurement invariance as the most pressing threats to reconciling competing findings. In the review I also observed a dearth of

empirical studies relating structural sexism to any behavioral health outcomes, including alcohol use. In the first empirical study, I demonstrated that women living in states with lower levels of structural sexism evidenced increases in both occasions of alcohol consumption (RR: 0.974, 95% CI: 0.971, 0.976) and probability of binge drinking (OR: 0.917, 95% CI: 0.909, 0.926); I showed that this relationship was specific to women (i.e., it was less pronounced among men) and that mediators of this relationship included increases in college completion and decreases in restrictive alcohol norms. In the second empirical study, I found that working women evidenced higher frequencies of alcohol consumption and higher probabilities of binge drinking than non-working women, and that these differences were most pronounced among women in low-sexism environments. At the lowest level of structural sexism, employed women reported higher occasions of consumption (2.61, 95% CI 2.57, 2.64) then unemployed women (2.32, 95% CI 2.27, 2.37). I also found that women in high-status occupations reported more occasions of alcohol consumption than those in low-status occupations, but only in low-sexism environments.

Conclusions

Lower levels of structural sexism are related to increases in both alcohol consumption and binge drinking among women. In low-sexism environments, working and belonging to a high-status career increases women's alcohol use. Increases in women's equality are positive and important social forces, but have conferred new acceptability of alcohol use that has implications for women's health.

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Dedication

For my dad, who first told me what epidemiologists do.

Chapter 1: Introduction to the Dissertation

Alcohol use is among the most prevalent causes of mortality and morbidity in the United States, contributing to an estimated 9.8% of deaths each year among working-age adults.[1] While alcohol consumption is prevalent and normalized in the United States, it is relevant for health even at low levels.[2,3] Over the past twenty years, the public health understanding of alcohol's toxicity has changed dramatically. In the 1990s and early 2000s, observational research proposed a so-called "j-shaped" curve between alcohol consumption and mortality and morbidity, suggesting that low levels of drinking were beneficial, particularly to heart health.[4,5] While these findings received intense media coverage,[6–10] they have since been empirically disputed by both systematic analyses and Mendelian randomization studies.[4,11,12] They have been further disputed after the discovery that the alcohol industry had funded research promoting the health benefits of low levels of drinking.[13,14] Currently, evidence suggests that no level of alcohol consumption improves health, and that even at low levels alcohol consumption can lead to adverse health outcomes.[15]

The present research examines social causes of women's alcohol consumption in the United States. Historically, men on average consumed alcohol at higher frequencies than women; however, over the past two decades, the gender gap in alcohol consumption in the United States has been narrowing.[16] This narrowing gender gap is primarily due to increases in consumption among women in the midlife (e.g., ages 30s-40s), rather than decreases in consumption among men.[17,18] Two specific consumption patterns in which this gender gap has narrowed are binge drinking, defined as consuming multiple drinks (i.e., 4-5 or more) in a short period, and any alcohol consumption (vs. abstention).

Concurrent with increases in women's alcohol use over the past two decades have been changes in women's status, including higher education and income attainment, labor market participation, and occupational prestige. While on average women still receive lower salaries than men in the same industry and are underrepresented at the highest levels of management, gender gaps in education, income, and employment have been narrowing.[19–22] Women in their 30s and 40s in recent decades entered adulthood after the Women's Rights Movement and are among first cohorts in which large numbers of women—i.e., at prevalences similar to or exceeding men's—have attained higher education, income, and employment.[19–22]

The central hypothesis in the present research is that the shifts in women's social position are relevant to recent alcohol trends. Alcohol consumption is patterned across socio-economic status: while adverse health outcomes and their causes are often linked to lower social positions, alcohol consumption is more complex. Higher-education, higher-income, and working adults are more likely to consume alcohol in general and to engage in any binge drinking than lower-education, lower-income, and non-working adults.[23–25] Along with availability of more expendable resources, workplace and social norms also contribute to this socio-economic heterogeneity in alcohol consumption.[26–28]

A further crucial source of heterogeneity in alcohol consumption is geography. Where people live determines many facets of their health and health behaviors,[29–31] and alcohol consumption is similarly patterned differentially across geographic dimensions, including by US state. This is due to variations in not only alcohol policies,[32] which are often enacted at the state level, but also broader social norms and climate, which vary dramatically by state. In this research, I will show that another important geographic determinant of women's alcohol use is *structural sexism*.

Structural sexism is the "systematic gender inequality in power and resources favoring men within…U.S. state-level political, economic, and cultural institutions."[33] Structural sexism reinforces women's disadvantages relative to men through reproducing gender inequality by systematically denying opportunities to women; these inequalities manifest in health outcomes for women through leading to social/economic deprivations, poor living conditions, inadequate healthcare, and patterns of unhealthy behavior.[33–36] Research measuring the health effects of variations in macro-level women's status or women's equality—both products of structural sexism—has been ongoing since the 1980s.[37] While structural sexism has declined over time nationally,[38] there remains dramatic variation in structural sexism across the US, including by state. This heterogeneity across geography and time can be leveraged to examine how alcohol consumption patterns of women in the mid-life vary across differential levels of exposure structural sexism, which can help us understand its role in contributing to the changing consumption patterns for women.

Very few studies have examined this relationship, so it is poorly understood to what extent, if any, structural sexism has influenced women's drinking. One limitation to extant research is that measurement of structural sexism is highly variable, and there has been no synthesis of the best way to operationalize the construct. Conceptually, the relationship between structural sexism and women's drinking could plausibly be harmful or protective: some research suggests that improvement in women's equality (i.e., reductions in structural sexism) have led convergence of women's health behaviors with men's,[39] including increases in women's drinking to levels that approach men's in bivariate relationships, though with control for women's individual social position those relationships attenuated.[40] Alternatively, lower levels of

structural sexism are protective for stress and psychiatric disorders for women—e.g., depression and anxiety[41,42]—potentially reducing consumption.

Structural sexism may also be an important moderator for more proximal exposures which influence women's alcohol consumption. For example, women in the workforce, particularly those in management positions, are exposed to not only occupational norms surrounding alcohol intake, but also to interpersonal sexism and harassment,[43–46] all of which may influence drinking behaviors.[47,48] Structural sexism may exacerbate or attenuate the associations between work and alcohol use among women in the midlife.

In sum, women in the mid-life are consuming alcohol and binge drinking at higher rates than among previous cohorts. Alcohol use and binge drinking are more common among higher-education, higher-income, and employed adults, where women are increasingly represented. I hypothesize that the changing consumption risks for women in the mid-life are related to these shifts in women's status, and that variations in structural sexism, a determinant of status, has contributed to variation in alcohol consumption patterns for these women. This dissertation will examine the evidence base for structural sexism's impact on women's health, and test associations among structural sexism and alcohol outcomes, in three aims:

Aim 1: Review the literature to examine the evolution and development of research into the relationship between structural sexism and health in the United States through a historical and theoretic lens, with a focus on approaches for measuring structural sexism.

Aim 2: Examine whether state-level differences in structural sexism are associated with binge drinking and alcohol use among women in the mid-life, and test mediating pathways—individual social position, depression, and alcohol norms.

Aim 3: Examine whether structural sexism is a modifier of the relationships among individual-level occupational features and mid-life women's alcohol use.

Chapter 2: An Historical, Narrative Review of Associations Between Structural Sexism in the United States and Women's Health

2.1 Introduction

Sexism exists at multiple levels.[33,36] Structural sexism operates at the *macro*—i.e., institutional, systemic, or cultural—level, reflecting systematic ways that societies and institutions cultivate unjust gender-based hierarchies through mutually reinforcing systems across multiple economic and social domains, including not only norms and beliefs but also policies and laws.[36,49] It is differentiated from interpersonal or *meso*-level sexism, e.g., interactions between people, including discriminatory behaviors directed at women due to their gender; and individual or *micro*-level sexism processes, e.g., feelings of powerlessness or incompetence due to beliefs in gender stereotypes.[36,50] Structural sexism is enacted through policies that target and disadvantage women, as well as cultural ideologies and normative attitudes regarding gender roles. Structural sexism may manifest in explicitly discriminatory ways (e.g., gender-based workplace discrimination), implicitly discriminatory ways (e.g., states' failure to adopt paid family medical leave, which disproportionately impacts women),[51] or "area-based or institutional legacies and indicators of injustice"[36(p45)] (e.g., the gender wage gap).

Structural sexism is hypothesized to lead to gendered inequalities in economic, political, and social resources,[34,36] which in turn drive persistent gender disparities in health. In the United States, gender disparities in health are found across multiple health domains. Women are disproportionately affected by a range of outcomes, including certain mental illnesses (e.g., depression[52]), poorer self-rated health,[53,54] and chronic illnesses (e.g., kidney disease,[55] chronic obstructive pulmonary disorder[56]). A growing body of literature has begun to examine the contribution of structural sexism to these disparities.[39,57,58] While nationally structural sexism has

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declined over time, persistent geographical variations in structural sexism and its consequences lead to vastly different climates for women across places (i.e., state to state).[38,59]

While US research into the relationship between geographical differences in structural sexism and women's health has been ongoing for 40 years, findings are discrepant and measurement of structural sexism is inconsistent. In this narrative, historical review, I both provide an overview of the studies examining the impact of structural sexism on women's health and show evidence that discrepancies in this literature result not only from epidemiologic concerns related to measurement misclassification and confounding, but also from theoretical differences motivating how and why this research has been conducted. These theoretical differences influence both the health outcomes that are considered relevant and how structural sexism is conceptualized. To contextualize these theoretical differences, I draw from not only empirical work examining the health effects of structural sexism, but also seminal, contemporaneous sources in the relevant fields at the time this literature was being produced to demonstrate how the field has been shaped. The intent of this paper is to elucidate the theoretical and methodological issues that have led to discrepancies in the extant literature, and to provide context and future directions for health researchers who wish to both grapple with previous findings and engage in forthcoming research.

This review is organized into three sections, beginning with an overview of the empirical findings. I divide the first section into examining both studies related to violence victimization and studies related to other physical or mental health outcomes. I have categorized outcomes this way both because of the divergent historical and theoretical underpinnings motivating research into these outcomes, and because the majority of research in this area has focused on violence victimization while only a small corpus has focused on other health consequences. In the review

of empirical findings, I discuss the history and theory behind the research and specific concerns relating to measurement and confounding. In the second section, I discuss issues related to measurement of structural sexism as a health-relevant exposure. I argue that measurement problems result in part from inconsistent theoretical backgrounds, and discuss the specific sources of measurement discrepancies, as well as their implications for validity. Finally, the third section will close with a discussion of future directions for this field.

In the past decade, two prior systematic reviews have examined this literature.[39,58] The first[58] was published ten years ago and examined only outcomes related to violence victimization; the second[39] was published recently (2020) but because its scope was limited to health science journals, a large corpus of studies published outside of health science literature were not included. The current review, therefore, expands upon the previous work in several ways: first, in identifying and integrating primary sources not discussed in previous reviews; next, in examining and contextualizing research findings through an in-depth analysis of historical differences in theoretical frameworks underlying this scholarship, which I propose are a critical dimension for understanding discrepant findings; and finally, through an extensive epidemiologic examination and critique of measurement problems, which other reviews have not examined in depth, nor examined through the perspective of internal or external validity.

Study methodology

The studies discussed here examine geographical differences (i.e., state-, city-, and county-level) in structural sexism and women's health outcomes in the United States, though I sometimes reference cross-national and global health research to contextualize domestic studies. I focus the review on the US given that gender-based inequality in the US is measured and manifested in different ways from other nations. Indicators used to represent structural sexism

abroad – e.g., differences in girls' literacy rates – are not particularly applicable in the US, where a relatively higher baseline level of gender equality can mask the other ways (e.g., economically, politically, legally) that women are unequal to men. While a small corpus of international literature examines within-country differences in structural sexism as a contributor to women's health (i.e., studies in Sweden or in Spain), the cultural context of other nations with regard to heterogeneity of legal protections does not directly translate to that of the US, where variation in laws and policies at the state level can result in very different environments for women.[a]

The current study is a purposive, narrative review. I chose this review format, rather than a more quantitative approach (e.g., meta-analysis or meta-regression) because I was more interested in tracing the divergent historical and theoretical origins of this research, and how they contributed to variations in study findings and measurement approaches, than in trying to quantify the precise association between structural sexism and myriad of women's health outcomes. Eligible empirical studies were initially identified from the sources cited in two systematic reviews that have examined relationships between structural sexism and health; the first is a 2011 review of structural sexism and women's violence victimization outcomes,[58] and the second is a 2020 review examining broader health outcomes.[39] Using the eligible studies referenced in these reviews as a starting set (N=25 unique citations), I used the "snowballing" method for identifying subsequent literature:[60] that is, I first searched the articles cited in the starting set (i.e., "backward selection") and then searched articles that subsequently cited the

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[a] This review examines women's health outcomes in relation to structural sexism based on a male-female gender binary, which rightfully has been criticized as exclusionary to the variety of gender expressions and gender non-conformity reflecting individuals' lived experiences.[348] Indeed, gender binarism contributes to systemic sexism, particularly the reductive paradigm situating differences between men and women as biological, rather than cultural or social.[36] Though gender minorities are also victimized by structural sexism, none of the literature identified for this review addressed examined the experiences of this population, which is a major limitation and an important topic for future scholarship.

starting set (i.e., "forward selection"), repeating the process with each new identified empirical study. I additionally examined "related articles" suggested in PubMed and Google Scholar. Through this method I identified 18 additional studies not previously discussed or included in prior reviews. The final sample of included articles comprises 43 empirical studies ranging from 1983 to 2020.

2.2 Review of empirical associations with historical and theoretical contexts

In the US, the impact of structural sexism has been evaluated on mental and behavioral health outcomes, general physical health, and experiences of violence victimization (i.e., family or intimate partner violence, homicide, and sexual assault). Investigation into victimization outcomes is largely motivated by hypotheses related to various feminist traditions, whereas studies of physical and mental health outcomes have generally drawn from frameworks common in the global development and social determinants of health literature. Table 2.1 presents an overview of the health domains, specific outcomes investigated, theoretical frameworks, and the general summary of findings across these domains. Supplemental Table 2.1 shows details regarding the measurement modalities and study findings; a more detailed discussion of measurement and validity appears in the second section of this review.

Table 2.1 Overview of structural sexism and health in the United States

Health domain	Specific outcomes	Hypotheses tested	Theoretical approaches	General findings
Violence victimization	• Family or dating violence[37,61–68] • Sexual assault[62,63,69–76] • Homicide[59,77–93]	Structural sexism is a cause of violence against women. As women become more equal, violence will either decrease (amelioration theory) or increase (backlash)	Liberal feminism, radical feminism, Marxist feminism	Mixed evidence for backlash and amelioration
Mental and behavioral health	• Depressive disorders[41,42,94] • Anxiety disorders[41] • Post-traumatic stress disorder[41] • Alcohol consumption[40]	Structural sexism has caused poor mental and physical health for women. In places where women are more equal, women's health will be better. Alternatively, women's health will become more like men's (convergence), which can lead to both beneficial and adverse changes for women depending on the outcome	1990s-early 2010s: Social determinants of health, global development	Mood disorders: evidence for improvements with more equality Substance use: limited evidence for convergence
General physical health	• Self-rated health[33,95] • Physical functioning[33] • Days of limited activity[59] • Chronic conditions[33] • All-cause and cause-specific mortality[59]		Mid-2010s-present day: Social determinants of health, structural racism, stigma theory	Increased equality improves women's physical health

2.2.1 Violence victimization: Historical and theoretical background

In the 1980s, multiple researchers began exploring the relationship between structural sexism and outcomes involving women's violence victimization.[58] Scholarship focused on these outcomes because experiences of violence victimization, in particular intimate partner violence of married women, were key issues in feminist theories. Violence against women was

conceptualized as a direct consequence of the patriarchy, defined as the overall social system

centered on preserving male dominance and maintenance of control over women.[80,96,97]

Feminist scholarship produced competing hypotheses, the "ameliorative" and "backlash"

hypotheses, theorizing how changing the patriarchy (i.e., reducing structural sexism) would

impact violence victimization of women.[76] Briefly, liberal feminist ameliorative theories of

violence are premised on the idea that women's subordination relative to men is rooted in

exclusion from legal, political, and economic systems[98,99] and that as women became more equal

to men (i.e., the exclusions are reduced), they would become more protected from violence

victimization. The "ameliorative" hypothesis predicts an inverse relationship between gender

equality and violence against women. The backlash theory is premised on the idea that formal

equality was insufficient because social and gender systems were constructed by a corrupt

patriarchy and thus subjugation would be perpetuated until a radical, systemic overhaul.[98,100] The

backlash theory hypothesized that as women became more equal to men, men would increase

perpetration of violence against women in order to maintain the gender hierarchy and thus

predicts a positive relationship between gender equality and violence against women.[b]

2.2.2 Violence victimization: Summary of the evidence

Of the 43 studies identified in this review, 37 included at least one outcome related to

violence victimization. Some studies found evidence for the amelioration hypothesis – that

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[b] A third competing theory arose from Marxist feminism, which hypothesized that economic subjugation, rather than gender inequality, was the source of violence against women; this hypothesis predicts that improving gender *equality* would not impact violence but that improving women's *overall* economic status would. In other words, economic disadvantage per se, rather than gender-relative economic disadvantage, is the structural contributor to violence. While this theoretical framework is not included in the overview of how structural sexism influences health outcomes presented in Table 1 (because it is not explicitly concerned with sexism in terms of *gender equality*), I further reference this theory in the subsequent section, as tests of different feminist hypotheses impacted how measures of structural sexism were operationalized.

geographies with greater structural sexism evidence higher rates of homicide, family or intimate partner violence, and sexual assault than other geographies. The strength of estimates for associations between structural sexism and homicide were fairly modest: for example, higher values of gender inequality (i.e., ratio of men to women) in employment were associated with risks of 0.40 higher deaths by homicide per 100,000 women.[77] Similarly, a 1-unit increase in a factor score for socioeconomic gender inequality was associated with 0.69 increased deaths by homicide per 100,000 women.[79] In terms of relative, rather than absolute, reductions, a 1-unit increase in the United Nations Gender Inequality Index (composed of education, financial, health, labor, and political measures, and adapted for use in the US) was associated with an 8% increase in women's homicides.[87] Regarding family or intimate partner violence, estimates range from an increase in incidence of 0.81 per 100,000 women as the wage gap ratio increases,[61] to increases in the relative risk of violence ranging from 1.6% to 20% (per 1-unit increase in United Nations Gender Inequality Index).[63,65] Regarding sexual assault, the rape rate has been estimated to be 0.23 incidents per 100,000 women higher in areas with higher structural sexism, measured as a composite index of financial, labor, legal, and political measures.[70]

However, other findings support the backlash hypothesis, that geographies with lower levels of structural sexism evidence higher rates of violence victimization for women. Estimates for increases in homicide range from 0.24 deaths per 100,000 women—in areas with more gender equality in state legislatures[79]—to 2.86 excess deaths per 100,000 women in areas with more gender equality in employment.[80] Family or intimate partner violence has been estimated to increase by 12 cases per 100,000 women with every 1-unit decrease in the gender gap in labor force participation.[66] In terms of magnitude, sexual assault increases have evidenced the most heterogenous range of estimated effects, ranging between an increase of 0.13 per 100,000

women in areas where women are more equally represented in managerial/professional occupations, to 138.96 per 100,000 women in areas where women are more equally represented in the labor force.[59,72,73,75–77] Other studies have found no relationship between structural sexism and homicide,[82,83,86,92] family or dating violence,[62] or sexual assault.[62,63,71]

In an attempt to reconcile amelioration and backlash hypotheses, Whaley et al.[76] examined nuances in the structural sexism and victimization relationships to understand if the competing hypotheses exist on an historical continuum, and found evidence that as women become more equal (as measured using gender disparities in economic domains, including the labor force and median income), they *first* experience backlash, but *ultimately* experience amelioration. The authors used lagged models of structural sexism and sexual assault, demonstrating that over time the impact of higher levels of gender equality are protective against sexual assault, despite sometimes resulting in immediate backlash.[76] Cross-sectionally, greater inequality was associated with between a 1.94 and 138.96 unit increase in the rape rate; however, with a 20-year lag, greater gender equality translated into reductions in the rape rate ranging from 16.73 to 25.66 incidents per 100,000 women. No further studies have used lagged exposures to examine these relationships with other outcomes or time periods.

The majority of the studies described here examined outcomes only among women in different geographies, rather than the gender disparity in outcomes. However, several studies examine violence victimization outcomes among men, to show whether structural sexism's impacts are specific to women: for example, one found that more unequal wage gaps resulted in higher rates of intimate partner violence for women, but not for men,[61] and others have similarly documented a null relationship between gender inequality and male homicide.[85] Others, however, have reported that in some samples gender inequality increases rates of male, but not

female, homicide;[84] therefore, the findings remain discrepant not only for women's outcomes but also for men's.

A special consideration for studies of violence victimization is that measurement of the outcome – particularly rates of sexual assault and intimate partner violence—may be biased by different levels of structural sexism. In places with high versus low structural sexism, the same event may be less likely to be classified as a violent victimization. Many researchers utilize crime databases to estimate area-level violence victimization incidence, and these databases depend on women reporting crimes, which itself may be influenced by area-level sexism—i.e., structural sexism may make them feel less empowered or afraid to report, or less likely to identify experiences of violence as criminal. Therefore, an apparent backlash could merely be detection bias, as more women feel empowered to report victimization in lower sexism contexts. Given the discrepancies in both direction and magnitude of effects, in particular for sexual assault, differential misclassification may be heavily biasing results. Studies using surveys rather than crime statistics (e.g., Straus,[64] who found an inverse relationship between increased equality and intimate partner violence using the National Family Violence Survey) may be less vulnerable to this type of measurement bias, as well as studies using proxy measures like hospitalizations for assault, which have been shown to decrease in response to decreased sexism.[61]

Ruling out confounding is another consideration. The quality of studies with regard to confounder control is highly variable, and a likely contributor to the conflicting study findings. For example, Stout[78] found that some measures of structural sexism (e.g., women's representation in managerial professions) were positively correlated with women's homicide, but the authors relied only on Pearson correlations between indicators of structural sexism and rates

of women's homicide, without control for any potential confounding variables. Studies by Yllo[37,67,68] relied primarily on comparing unadjusted proportions of intimate partner violence across states with relatively lower vs. higher levels of sexism, measured using a composite score across several domains of gender inequality. Most studies, however, use multivariable regression models and control for other structural characteristics that may potentially introduce bias: common controls in the violence victimization literature are measures of area socioeconomic status (e.g., the poverty rate), population size, population density, and racial composition, as well as other predictors which are more specific to legal outcomes – such as area deprivation, youth population, and measure of social disorganization. While heterogeneous confounder control has likely contributed to heterogeneity in study findings and conclusions, consistent confounder control has not sufficiently remedied heterogeneity of findings. For example, two studies with nearly identical confounder controls—Baron and Straus (1984) and (1987)—found disparate results; one showed no association between structural sexism[71] and sexual assault, and the other showed an inverse relationship.[70] Rather, the major difference between the studies was the choice of measure of the exposure, described in further detail in Section 2 below.

In summary, studies examining the impact of structural sexism on violence victimization outcomes share a strong theoretical foundation, but are limited by measurement and confounding concerns.

2.2.3 General physical health and mental health: Historical and theoretical background

Studies of the relationship between structural sexism and other physical or mental health outcomes are not rooted in or motivated by evaluating theories of feminism. This may be due to two reasons. First, they are largely siloed from the violence victimization literature. While women's experiences of violence are clearly health outcomes, the scholarship investigating

violence victimization has been primarily conducted by sociologists and criminologists; these papers are generally not indexed in health science databases (e.g., PubMed or PsycInfo) and thus would not appear in systematic reviews querying these databases. For example, a 1999 investigation by Kawachi et al.[59] cited only a single violence victimization paper (Yllo 1983)[37] and stated that theirs (Kawachi's) was only the second study to examine this question in the US—whereas in fact dozens[64,67,90,93,101,68–72,77,78,86] of other studies in the violence victimization literature preceded it. In the subsequent decade, four papers examining health effects of structural sexism were published in health science journals—Jun (2004),[95] Chen (2005),[42] Koenen (2006),[102] McLaughlin (2011)[41]—and they cited only each other, Kawachi (1999),[59] or Yllo (1983)[37] as examples of previous work examining geographic differences in women's status on health outcomes within the US, despite the growing cotemporaneous literature base examining violence victimization outcomes.

The second reason that this literature did not engage with competing feminist theories is that it drew heavily from global development scholarship. Global development is an umbrella term referring to international efforts to promote economic sustainability and reductions in morbidity, particularly in developing nations, frequently through humanitarian aid or public-private partnerships.[103–106] Major stakeholders include the World Bank and the United Nations Development Program. In the 1970s and 1980s, large international consortiums dedicated to global development experienced internal conflict about the role feminism should play in setting an agenda for development and health investment.[107,108] For example, over the course of multiple United Nations conferences on the status of women, heated debate erupted related to whether or not feminism was too divisive a framework, and these tensions were largely left unresolved.[107–109] While these debates resulted in consensus among stakeholders to promote concrete priorities

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related to improving women's status in the developing world—e.g., promoting girls' education, ending child marriage and honor killings, investing in women-owned businesses[110]—these priorities were largely removed from any underlying feminist framework. Rather, the dialogue around gender inequality explicitly disengaged from contemporary feminist scholarship, which was deemed too exclusionary to women outside of the United States.[108,109]

Literature that emerged from global development focused on advancing hypotheses related to the positive benefits of gender equality, not exploring complications like backlash. Further, because this literature was not grounded in explicitly testing competing feminist theories, it considered broader health outcomes. Articles examining gender inequality cross-nationally provided empiric evidence that as women's status increased at the national level, key population health indicators—e.g., maternal mortality, teen pregnancy—also improved (see King 2020[39] and Sen 2007[110] for more comprehensive reviews).

In the US, inquiry into the relationship between structural sexism and mental and physical health outcomes was largely motivated by this global health research and integrating it with the *social determinants of health* framework.[31,111] A premise of this framework is that the structural and contextual features of a person's larger social environment are important contributors to individual health experiences and perpetuate systems of disadvantage that are relevant to health outcomes. In recent years, studies on structural sexism and health have increasingly drawn from related literatures about specific structural exposures, including integrating insights from studies of structural racism[33,49] and stigma theory.[112,113] A common hypothesis across these literatures is that a history of structurally unequal conditions has contributed to health poor health and health inequalities. Corollaries of this hypothesis are that disadvantaged groups living in areas with higher levels of equality have better health, and that

cultivating structural improvements in equality will improve the health of the groups most disadvantaged by these social inequalities. Rooted in these literatures, studies of structural sexism and women's physical and mental health almost exclusively hypothesize this direction of effect.

Specific exceptions to this hypothesis include health outcomes for which the gender gap in health does not disadvantage women; in particular, men are at greater risk for many adverse health behaviors. For outcomes that are more prevalent among men, it may be that as women become more equal to men, women may begin engaging in unhealthy behavioral patterns otherwise more common among men. This hypothesis is known as *convergence*.[39,114] While convergence is consistent with the hypothesis that gender-based disparities narrow as the genders become more equal, convergence does not imply that this narrowing always results in health benefits. One example of convergence is the increase in smoking among women in the 20th century: the male-female gender gap in smoking decreased because more women began to smoke cigarettes at rates approaching men's, and these increases are partly attributed to women's gains in education and the labor force.[115–118] Globally, evidence for convergence has come from findings that countries with lower levels of sexism have narrower gender gaps in alcohol consumption[119] and lower life expectancies[114] for women.

2.2.4 General physical health and mental health: Summary of the evidence

In the US, seven studies have examined structural sexism in relation to physical and mental health outcomes other than violence victimization. These studies generally show that women living in areas with lower levels of sexism evidence better physical and mental health—including better self-rated health,[95] lower risks for chronic conditions,[33] lower mortality risks,[59] better physical functioning,[33] fewer days with limited activity,[59] and lower rates of

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depression[41,42,94] and post-traumatic stress syndrome[41]—than women living in areas with higher

levels of sexism. Kawachi et al (1999)[59] showed that on average, and depending on the sexism

measure used, increased gender equality was associated with between a 5.09 and 54.55 unit

decrease in state-level mortality rates per 100,000 women; in addition, women in more equal

states had between 0.03 and 0.56 fewer days of limited activity. Jun et al (2004)[95] found that,

depending on the sexism measure used, increased gender equality was associated with between a

14% and 30% decrease in poor self-rated health among women in the Behavioral Risk Factor

Surveillance System, with adjustment for individual risk factors. Homan (2019)[33] showed that

higher levels of gender equality (measured using a single composite index) were associated with

0.02 unit decrease in count of chronic conditions and a 0.20 unit improvement in physical

functioning but not with improvements in self-rated health. The fairly large effect sizes for

mortality outcomes warrant further investigation, particularly given the relatively modest to

moderate impact on other health outcomes – i.e., physical functioning, chronic conditions, and

self-rated health—that are intermediaries on the pathway between structural sexism and

mortality.

Regarding mental health outcomes, women living in states with relatively lower levels of

structural sexism evidenced lower risks of depression—with estimates ranging from reductions

of between 0.63 and 0.85 points on the Center for Epidemiologic Studies Depression Scale for

individual women living in more equal states,[42] to a reduction of 5% in the state-level prevalence

of depression[41] and a 25% reduction in the odds of post-partum depression[94]—than women

living in states with relatively higher levels of structural sexism. McLaughlin et al (2011)[41] also

showed that the prevalence of post-traumatic stress disorder was 7% lower in states with more

reproductive autonomy for women, but found no relationship between structural sexism and

anxiety disorders. A single study examined the relationship between state-level structural sexism and death by suicide, and found no relationship.[90]

Regarding behavioral health outcomes, one US study has examined the relationship between state-level structural sexism and alcohol consumption, and found limited evidence for convergence; in unadjusted models, women in states with lower levels of structural sexism on average consumed more alcohol than those in states with higher levels of structural sexism, but this relationship attenuated with control for other state-level attitudes potentially related to sexism, including religiosity.[40]

Some studies have examined health outcomes not only among women, but also among men. Among these, Kawachi (1999) showed that both men and women evidence reductions in premature mortality in states where women's status was higher.[59] Homan (2019) showed that higher levels of structural sexism were associated with poorer self-rated health and physical functioning not only for women, but for men as well.[33] In this study, the relationships among men, however, were less pronounced than among women, suggesting that women may have benefited more from reductions in structural sexism than men. Competing findings have suggested that men's self-rated health declines with increases in gender equality.[120] None of the studies identified examined the gender disparity in health outcomes, only the group-specific estimates.

The lack of engagement with the ongoing debate within feminist literature, particularly with regards to theories of backlash, has been a limitation to these studies. As with violence victimization, women living in states with higher levels of equality may plausibly suffer adverse consequences—for example, stress, which is implicated in multiple morbidities—should backlash occur. By not engaging with the ongoing debate in the violence victimization literature,

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researchers have missed an opportunity to explore nuance in the sexism-health relationship and consider alternative hypotheses or unintended, troubling consequences.

The largest threat to causal inference in studies associating structural sexism with physical or mental health outcomes is confounding. While the violence victimization outcomes evidenced great heterogeneity across study findings and conclusions, research into the general physical and mental health consequences of structural sexism have produced largely consistent results showing protective effects of reductions in sexism. However, to what extent the relationship between structural sexism and physical and mental health outcomes is causal relies on appropriately ruling out alternative explanations, particularly other structural variables that confound the relationship. For studies examining general physical and mental health outcomes, all but one (Lester 1992)[90] have used multivariable regression with some degree of confounder control. Studies examining general physical health have appropriately controlled for area-level economic confounders (i.e., percentage below the poverty line), which are related both to women's inequality and to population physical health outcomes more broadly. Regarding mental health outcomes, however, the approach to residual confounding has not been consistent. As discussed above, Roberts (2012)[40] found an inverse association between structural sexism and alcohol consumption that attenuated with control for other structural exposures—namely, area religiosity—potentially indicating the importance of accounting for other macro-level exposures that are salient not only to structural sexism but also to the outcome. In their study finding an inverse relationship between structural sexism and depressive symptoms, Chen et al (2005)[42] controlled only for the GINI coefficient, an area measure of socioeconomic inequality, which may not sufficiently rule out other structural explanations of depression in particular, e.g., geographic variation in access to mental health providers.[121] For three of the five studies

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measuring mental or behavioral health,[41,90,94] no structural controls were included. Moving forward, demonstrations of robustness of associations to additional sources of confounding, as well as the design choices suited for ruling out residual confounding, will be helpful for better establishing causality.

2.3 Measurement problems

In the previous section, I discussed the historical and theoretical underpinnings that led to two very different approaches to examining health outcomes related to violence victimization compared to general and physical health outcomes. This variation in historical frameworks translated into variation in measurement, namely in the methods used for conceptualizing and operationalizing structural sexism, which likely contributed to heterogeneity of findings across studies.

Of the 43 empirical studies reviewed, 19 of them used some version of four commonly-used scales or indices that were developed to assess gender equality or women's status. These are the "Status of Women Index" (developed by Yllo[37]), the "Gender Equality Index" (developed by Sugarman and Straus,[122] later updated and revised by Di Noia[123]), the "Status of Women in the States" (developed by the Institute for Women's Policy Research[124]), and the "United Nations Gender Inequality Index" (adapted from Griffith and Rose[125] for use in the US). In the remaining 24 studies, authors developed their own measures of sexism. Thus, studies had considerable heterogeneity regarding measures.

Particular sources of variation in measurement relevant to internal validity are the choice of indicators used to capture the construct of structural sexism; whether structural sexism is measured using relative (i.e., women's status relative to men's status) or absolute indicators; and whether to use an aggregated or multi-domain approach to operationalizing sexism, which also

relates to the use of empirical methods and construct validity. A source of variation in measurement which is relevant to external validity is level of geography.[126] Table 2.2 shows individual studies discussed here, the geographic level at which structural sexism was measured; whether the approach used an aggregated, single indicator, or multi-domain approach; which domains were included, and whether the measures were relative or absolute. This table also includes the specific outcomes examined, the health outcome domain (i.e., violence victimization, general physical health, or mental/behavioral health), and a brief summary of the findings. Supplemental Table 2.1 shows more detailed information on the measurement techniques used in these studies, including the individual items utilized, whether the data were reduced into scales or indices, whether reliability or validity was assessed, and the years represented by the sexism measures. Supplemental Table 2.1 is organized by study year, with studies that used the same established measures grouped by measure and studies that developed their own measures listed individually by year.

Table 2.2 Studies examining structural sexism and health outcomes in the United States 1983-2019, measurement details and general findings

Study	Geography	Categories of indicators included	Dimensionality	Absolute or relative	Outcomes examined	Outcome domain	Conclusions*
Homan 2019[33]	State	Attitudinal, financial, labor, political, reproductive	Unidimensional	Relative	Chronic conditions, self-rated health, physical functioning	General physical health	Reduced sexism is protective
Jun 2004[95]	State	Education, financial, labor, political, reproductive	Multidimensional	Mixed relative and absolute	Self-rated health	General physical health	Reduced sexism is protective
Kawachi 1999**[59]	State	Education, financial, labor, political, reproductive	Multidimensional	Mixed relative and absolute	Mortality, days of activity limitations	General physical health	Reduced sexism is protective
Chen 2005[42]	State	Education, financial, labor, political, reproductive	Multidimensional	Mixed relative and absolute	Depression	Mental or behavioral health	Reduced sexism is protective
Lester 1992**[90]	State	Education, financial, labor, legal, political	Unidimensional	Relative	Suicide	Mental or behavioral health	No relationship
McLaughlin 2011[41]	State	Education, financial, labor, political, reproductive	Multidimensional	Mixed relative and absolute	Mood disorders	Mental or behavioral health	Reduced sexism is protective
Mukherjee 2017[94]	State	Education, financial, labor	Multidimensional	Mixed relative and absolute	Post-partum depression	Mental or behavioral health	Reduced sexism is protective

Roberts 2012[40]	State	Education, financial, labor, legal, political, reproductive	Unidimensional	Relative	Alcohol consumption	Mental or behavioral health	No relationship
Aizer 2010[61]	County	Financial	Single measure	Relative	Intimate partner violence	Violence victimization	Reduced sexism is protective
Bailey 1995[77]	City	Financial, education, labor	Multidimensional	Both (tested separately)	Sexual assault	Violence victimization	Reduced sexism is protective
Bailey 1999[69]	City	Financial, education, labor	Multidimensional	Both (tested separately)	Homicide	Violence victimization	Reduced sexism is harmful
Baron 1987[70]	State	Financial, labor, legal, political	Unidimensional	Relative	Sexual assault	Violence victimization	Reduced sexism is protective
Baron 1984[71]	State	Financial, labor, legal, political	Unidimensional	Relative	Sexual assault	Violence victimization	No relationship
Brewer 1995[86]	City	Education, financial, labor	Multidimensional	Relative	Homicide	Violence victimization	No relationship
Campbell 2019[87]	State	Education, financial, health, labor, political	Unidimensional	Relative	Homicide	Violence victimization	Reduced sexism is protective
DeWees 2003[88]	City	Education, financial, labor, marital, political	Multidimensional	Relative	Homicide	Violence victimization	Reduced sexism is harmful
Ellis 1983[72]	City	Education, financial, labor	Multidimensional	Relative	Sexual assault	Violence victimization	Reduced sexism is harmful
Eschholz 2004[73]	City	Education, financial, labor	Multidimensional	Both (tested separately)	Sexual assault	Violence victimization	Reduced sexism is harmful

Gillespie 2017[89]	County	Education, financial, labor	Multidimensional	Relative	Homicide	Violence victimization	Mixed protective and harmful findings
Goodson 2019[62]	County	Education, financial, labor	Multidimensional	Both (tested separately)	Intimate partner violence, sexual assault	Violence victimization	No relationship
Gressard 2015[63]	State	Education, financial, health, labor, political	Unidimensional	Relative	Intimate partner violence	Violence victimization	Reduced sexism is protective
Henke 2020[127]	County	Financial	Single measure	Relative	Intimate partner violence	Violence victimization	Reduced sexism is protective
Kawachi 1999**[59]	State	Education, financial, labor, political, reproductive	Multidimensional	Mixed relative and absolute	Homicide	Violence victimization	Reduced sexism is harmful
Kearns 2020[74]	State	Education, financial, health, labor, political	Unidimensional	Relative	Sexual assault	Violence victimization	Reduced sexism is protective
Lester 1992**[90]	State	Education, financial, labor, legal, political	Unidimensional	Relative	Homicide	Violence victimization	Reduced sexism is protective
Martin 2006[75]	State	Education, financial, labor	Unidimensional	Both (tested separately)	Sexual assault	Violence victimization	Reduced sexism is harmful
Peterson 1992[101]	City	Education, financial, labor	Multidimensional	Relative	Sexual assault	Violence victimization	Mixed protective and harmful findings
Pridemore 2005[91]	State	Attitudinal, financial	Multidimensional	Relative	Homicide	Violence victimization	Reduced sexism is harmful
Reckdenwold 2010[92]	City	Education, financial, labor	Multidimensional	Relative	Homicide	Violence victimization	No relationship

Smith 1995[93]	City	Education, financial labor, marital	Multidimensional	Both (tested separately)	Homicide	Violence victimization	Mixed protective and harmful findings
Stout 1992[78]	State	Financial, labor, legal, political	Multidimensional	Relative	Homicide	Violence victimization	Mixed protective and harmful findings
Straus 1994[64]	State	Education, financial, labor, legal, political	Unidimensional	Relative	Intimate partner violence	Violence victimization	Reduced sexism is protective
Titterington 2006[79]	City	Education, financial, labor, legal, political	Multidimensional	Relative	Homicide	Violence victimization	Mixed protective and harmful findings
Vieraitis 2002[80]	City	Education, financial, labor	Multidimensional	Both (tested separately)	Homicide	Violence victimization	Reduced sexism is harmful
Vieraitis 2007[82]	County	Attitudinal, education, financial, labor	Unidimensional	Both (tested separately)	Homicide	Violence victimization	No relationship
Vieraitis 2008[83]	City	Education, financial, labor	Unidimensional	Both (tested separately)	Homicide	Violence victimization	No relationship
Vieraitis 2015[81]	City	Education, financial, labor	Unidimensional	Both (tested separately)	Homicide	Violence victimization	Reduced sexism is protective
Whaley 2001[76]	City	Education, financial, labor, legal	Multidimensional	Relative	Sexual assault	Violence victimization	Mixed protective and harmful findings
Whaley 2002[84]	City	Education, financial, labor	Unidimensional	Relative	Homicide	Violence victimization	Reduced sexism is harmful
Whaley 2013[85]	City	Education, financial, labor	Unidimensional	Relative	Homicide	Violence victimization	Mixed protective and harmful findings

Willie 2019[65]	State	Education, financial, health, labor, political	Unidimensional	Relative	Intimate partner violence	Violence victimization	Reduced sexism is protective
Xie 2012[66]	City	Education, financial, labor, political	Multidimensional	Both (tested separately)	Intimate partner violence	Violence victimization	Reduced sexism is harmful
Yllo 1983[37]	State	Education, financial, labor, legal, political	Unidimensional	Relative	Intimate partner violence	Violence victimization	Mixed protective and harmful findings
Yllo 1984a[67]	State	Education, financial, labor, legal, political	Unidimensional	Relative	Intimate partner violence	Violence victimization	Mixed protective and harmful findings
Yllo 1984b[68]	State	Education, financial, labor, legal, political	Unidimensional	Relative	Intimate partner violence	Violence victimization	Mixed protective and harmful findings

*Significant ($p<0.05$) results described here. For parsimony, studies that tested multiple relationships and found mixed protective/null results were coded as protective, studies that found mixed harmful/null results were coded as harmful, and studies that found mixed protective/harmful results were coded as mixed; when absolute and relative measures are tested separately (i.e., to adjudicate between Marxist and other feminist theories) findings for only relative measures reported. If outcome was intimate partner homicide, it was categorized here as homicide rather than intimate partner violence.

*Kawachi 1999 and Lester 1992 included twice – both tested homicide outcomes in combination with other general or mental health outcomes.

2.3.1 Indicator choice and content validity

Structural sexism is a latent construct, meaning it is not directly observed, and is generally measured using other, directly observed variables (i.e., indicators). No gold standard measure exists for measures of structural sexism, nor does any definitive test exist to establish its presence, absence, or relative amount. Investigations into the health effects of structural sexism therefore rely on *indicators* of structural sexism, measures that represent the effects of structural sexism. Here I briefly discuss the overall categories that are generally conceived of as indicators of structural sexism, evidence for internal validity and reliability, and concerns related to dimensionality of measures.

The majority of studies used multiple indicators to measure sexism (Supplemental Table 2.1 shows the indicators used in each study), drawing from the following broad categories: financial (e.g., women's income, wages, poverty rates; these are usually obtained from the Census Bureau), labor (e.g., women's labor force participation or percent of women in management occupations; these are usually obtained from the Census Bureau), education (e.g., women's college completion rate; these are usually obtained from the Census Bureau), and political capital (e.g., women representatives in government; these are usually obtained from the Center for American Women and Politics). These are the domains that various liberal feminist discourses have identified as areas where women have been historically underrepresented relative to men, and therefore are both indicators of the patriarchy and mechanisms through which it operates. A category of legal indicators (e.g., the presence or absence of violence against women laws, whether or not a state ratified the Equal Rights Amendment) are sometimes also included, but these are rarer, possibly because there is more variability in the content of the laws, they are less flexible over time (and therefore may be less informative about a state's

changing climate), and they are not routinely tracked by public agencies like the Census Bureau.

Several studies have used indicators related to marital (i.e., the proportion of divorced women)

and health (i.e., the maternal mortality rate, the teen pregnancy rate) dimensions, though using

health indicators of structural sexism to estimate the impacts on other health outcomes may

introduce both bias and reverse-causation concerns.

Because one aspect of sexism is area-level attitudes that manifest as gender inequalities,

some studies have chosen to include an attitudinal measure of structural sexism. However, the

choice of attitudinal measures is debatable: for example, Homan (2019)[33] used the state-level

proportion of religious conservatives as an indicator of sexist attitudes, arguing that religious

conservative ideologies in the US promote traditional gender roles and therefore represent an

indicator of area-level animus against women's equality. However, Roberts (2012)[40] used the

same measure as a control variable—indeed, this measure is the primary reason that the observed

relationship between sexism and alcohol consumption attenuated to the null in their study—

suggesting that that religious conservativism is not an indicator, but a cause, of sexism, and

therefore a structural confounder. Pridemore et al. (2005)[91] used measures of "patriarchal

culture"—here, population density, National Rifle Association membership, and religious

conservatism—but again, any of these measures could plausibly function as a confounder of the

sexism-health relationship, rather than an indicator of structural sexism.

An important distinction for these indicators, particularly the economic ones–e.g., related

to labor, wages, education—is whether they are absolute or relative. Absolute measures are

reflective of women's status in general, whereas relative measures are reflective of women's

status *in relation* to men's. For example, women can have low socioeconomic status (as

represented, for example, by low educational attainment or low median incomes) but not be

disadvantaged relative to men; in this case, women's absolute status would be low, but their relative status would be equal. On the other hand, women can have high socioeconomic status but still be relatively unequal with men; in this example, women's absolute status would be high, but their relative status would be low. Only relative status captures the *gender inequality* dimension of the structural climate.[c]

While the distinction between absolute and relative status has been paramount for scholars examining feminist theories of the patriarchy in relation to violence victimization, this distinction has rarely been articulated by health researchers examining physical or mental health outcomes. The majority of papers examining these relationships utilize a composite relative/absolute measure called the "Status of Women" index, which is updated and disseminated by the Institute for Women's Policy Research every few years (in 1996, 1998, 2000, 2002, 2004, 2006, 2013, and 2015).[41,42,59,95,102] This state-level measure is composed of absolute and gender-relative income, occupation, and education measures, as well as measures of women's political representation (e.g., presence of a female governor, number of female legislators) and a measure for the presence or absence of a variety of restrictive reproductive policies. More recently, health science researchers have begun to move away from combining absolute and relative measures, because they make interpretation difficult.[33,40,58] For measures that combine absolute and relative indicators, it is impossible to adjudicate between the effects that are attributable to sexism from those that are attributable to women's absolute socioeconomic deprivation. For this reason, scholars advocate the use of exclusively *relative* measures of gender inequality to accurately measure sexism.[58,122,123]

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[c]As discussed in the previous section, some feminist scholars have intentionally examined the different impacts of absolute versus relative measures to understand the different contributions of each, as a way to adjudicate among liberal vs. Marxist feminist hypotheses. Appropriately, absolute and relative measures are treated as distinct constructs when these competing hypotheses are tested.

2.3.2 Empirical assessments of internal validity and reliability

Support for validity and reliability of the various measures of structural sexism is highly variable and often extremely limited. The primary method for validating measures of structural sexism and assessing reliability is simply face validity—that is, whether the measure makes sense and seems theoretically aligned with the underlying construct. However, a number of studies have approached measuring structural sexism using empirical methods.

For example, the "Status of Women Index,"[37] which is composed of upwards of 20 measures (minor nuances in specification vary from paper to paper), combines indicators across a variety of conceptually distinct domains (economic, educational, political, and legal) and then sums scores for those domains to create a single index. The Cronbach's alpha reliability statistic for the index is 0.54, which is considered low.[128] No psychometric analysis has been published which evaluates whether reliability would increase with removal of certain items. The "Gender Equality Index"[122] similarly uses an index approach across multiple domains, but the developers made decisions about final included variables based on whether the Cronbach's alpha statistic improved when items were removed; this resulted in an alpha of 0.62 for the overall index. Both of these example indices are composed of 20 or more items and developed to reflect state-level differences in the 1980s. More recent indices using fewer items—for example, Homan's six-item index encompassing economic, attitudinal, and reproductive inequality with a Cronbach's alpha of 0.64[33] —also have demonstrated high reliability, suggesting that more parsimonious scales may perform just as reliably in capturing the underlying construct.

Other studies which do not report reliability statistics instead show correlations across measures or domains of measures, which are highly variable. Using the "Status of Women in the States" measure developed by the Institute for Women's Policy Research, correlations across

domains (named "political participation," "employment and earnings," "economic autonomy," and "reproductive rights") ranged from 0.24 to 0.89, with political measures having the lowest correlations with other domains. The indicators composing "United Nations Gender Equality Index"[125] have correlation coefficients ranging from 0.01 to 0.90,[63] showing great heterogeneity. A measure developed by Brewer & Smith[86] evidenced correlation coefficients among individual items ranging from -0.49 to 0.37, suggesting that some items were correlated in the inverse of hypothesized direction, bringing the validity of the overall measure into question.

The most compelling demonstrations of measurement validity and reliability come from the studies that have employed data reduction techniques to empirically derive measures of sexism and to evaluate goodness of fit. Among the papers discussed here, the earliest to employ one of these techniques was Whaley (2002),[84] who used principal components analysis to reduce 4 measures (ratio of men to women with college degree or higher, ratio of men's to women's median income, ratio of men to women who are unemployed, percentage of women in managerial occupations) into a single measure of sexism. The factor loadings ranged from 0.33 to 0.86, ranging from weak to strong correlations between the individual items and the underlying component, and had an eigenvalue of 2.16, suggesting that a single component explained as much variance as 2.16 individual indicators. Since then, 8 other studies have used empirical data reduction techniques – either principal component or factor analysis – to reduce individual indicators of sexism into either unidimensional or multidimensional measures of structural sexism. In general, the indicators that are frequently included due to their high correlations or factor loadings are measures of relative income, relative education, and relative labor force engagement. A difficulty with comparing goodness of fit comes from inconsistency with how papers report reliability and validity statistics; papers will often describe the range of

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factor loadings, or a reliability coefficient for the overall index, but do not consistently report fit statistics.

2.3.3 Measure dimensionality

How and whether to combine indicators is another major source of study variability. While a small number of studies use a single indicator of sexism (i.e., the wage gap) and thus do not combine or aggregate, the remaining studies have to grapple with the two general ways of conceptualizing structural sexism: as a unidimensional underlying construct or as a multidimensional underlying construct.

The multidimensional approach is premised on structural sexism's occurring and operating over multiple domains, including economic, legal, political, social, and reproductive domains; proponents of this approach argue that a single index of sexism may not sufficiently capture these varied dimensions.[122,123] In this approach, unique dimensions of sexism are tested independently for their unique effects on women's health outcomes, sometimes with control for each other, sometimes not. While this approach appropriately acknowledges that heterogeneous domains of sexism are meaningful because sexism may operate through multiple pathways which may have varying relevance for women's health, it introduces interpretation problems. There is dramatic heterogeneity across not only indicators of measurement, but across which domains should be captured and which are more or less relevant for health. The very limited assessments of measurement validity or reliability mean that is unclear how much credence to give competing results, particularly given that some indicators (e.g., inequality across income, labor, education) have been empirically assessed but others (e.g., political, legal, reproductive, attitudinal indicators) largely have not. As a result, a major disadvantage to the multidimensional approach is that the findings are largely irreconcilable, even within a study.

For example, some studies using a multidimensional approach show mixed protective and harmful findings depending on the domain tested, or a combination of null and protective or null and harmful findings. Interpreting the overall message of these mixed findings is extremely difficult. Across studies, multi-domain approaches create difficulty reconciling the larger corpus of research, leading to conflicting narratives and logical inconsistencies across studies. For example, Chen (2005)[42] found that some indicators of sexism (financial, education, labor, reproductive) were related to depression, but that political indicators were not, and the authors' explanation was that economic and reproductive contexts were highly salient to women's mental health experiences, but that because women were still so politically oppressed there was not yet an impact on women's mental health as the political context improved. When McLaughlin (2011)[41] found, using the same categories, that only reproductive indicators and no others were related to depression, they interpreted these findings by explaining that women's economic and political environments did not truly reflect individual women's experiences, but that reproductive contexts did. In Roberts' (2012)[40] paper which used a similar multi-domain approach, the author found a significant negative association between one domain of sexism (socio-economic status) and alcohol consumption, but concluded that this association was insufficient because all of the other indicators produced null results. Notably, the examples given here come from papers that are also largely under-theorized, as papers examining general and mental health outcomes rarely discuss competing theories. Nevertheless, even within violence victimization scholarship, which is much more theoretically-driven, the problem persists: for example, Titterington (2006)[79] showed that higher socioeconomic gender inequality was associated with an increase in homicide rates, but that higher legislative gender inequality was associated with a decrease in homicide

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rates—making it difficult to adjudicate between competing hypotheses regarding amelioration or backlash.

Fundamentally, the multidimensional approach suggests that different domains of sexism are distinct and meaningful constructs. A unidimensional approach may be more consistent with theoretical descriptions of sexism and how it operates—that is, the various categories of measures are a *result* of a single construct, sexism, rather than different *varieties* of sexism.[d] The unidimensional approach, however, has been better supported by empirical evaluations of the indicators of sexism. Of the 9 studies using empirical techniques to reduce sexism indicators into underlying components, five of them showed evidence for a unidimensional underlying construct,[75,81,83–85] one showed evidence for a two-dimensional underlying construct (in which a single indicator, relative income, did not load and was treated as distinct)[89] and the remaining three did not test dimensionality of all indicators but instead made the a priori decision to divide indicators into multiple domains and run separate factor analyses on each of those domains.[40,79,82] Therefore, empirical tests of dimensionality currently best support a unidimensional approach, but require further investigating.

2.3.4 Level of geography and external validity

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[d]Empirical examinations of structural sexism cannot adjudicate directionality of the relationship between the latent construct (structural sexism) and its indicators. While structural sexism may produce gender inequalities in economic, political, and social capital, there is no empirical test to confirm that the relationship does not go in the other direction. Structural sexism may alternatively be an emergent property – that is, a system-level phenomenon which cannot be approximated as the sum of its constituent parts, but rather is a complex, group-level phenomenon.[349–351] Operationalizing sexism in this way is harder to quantify and may require a complex systems approach, which would be a valuable contribution to this literature.

Most studies of structural sexism are at the state level[e] for both pragmatic and theoretical reasons. First, state-level indicators are generally easy to measure, available for multiple years, and publicly accessible. Next, states are an important political unit, and heterogeneity across state policies—e.g., tax, education, employment, and health care laws—has resulted in vastly different environments for women.[64]

However, inferences from state level findings may not translate to other geographies. Some studies have focused on structural sexism at the city or municipal level, which has benefits and limitations. Among the benefits, city-level exposures may be more proximal to women's daily experiences. For example, the average wage gap in their city of residence may be more influential to women's experiences of stress and deprivation than the average wage gap in their state of residence. Cities may additionally be political enclaves against an otherwise oppressive political state environment. However, limitations include difficulty measuring rarer outcomes—especially in scholarship on violence victimization when researchers consider homicide—leading to influential outliers and missingness skewing the estimated relationship between city-level structural sexism and health. In addition, studies of cities are not generalizable to women who live in rural areas, whereas state-level studies are generalizable to all women who live in the 50 states. Finally, most policies are enacted at the state, rather than the local level, and state policies frequently preempt municipal policies for important topics like reproductive and labor rights. A small number of studies (N=5)[61,62,82,89,127] examined structural sexism at the county level, which in part addresses the first two limitations (size, generalizability) of using city-level measures. While county policies may be more relevant to health than state-level policies, none of

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[e] Washington, D.C, is frequently considered a state in these studies, which limits the types of indicators that can be used to measure sexism. In particular, political measures generally capture the percentage of women either in state government or the number of women senators, which are not applicable indicators for Washington, D.C.

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these studies examined political dimensions of structural sexism. Further, county of residence may not be as meaningful a social unit as city or state, and, as with cities, state policies do frequently preempt more localized political environments.

2.4 Future Directions

In the previous two sections, I discussed the empirical support for the relationship between structural sexism and women's health in the United States. I described how the different historical and theoretical foundations motivating this research have led to differences in which outcomes are examined and which hypotheses are tested. I then discussed problems related to measurement of structural sexism, which are related to these different foundations. In this third section, I discuss future directions for this research. Moving forward, the key elements for reconciling this scholarship and integrating new findings will be reconciling competing theoretical frameworks, unifying and improving measurement approaches, improving causal inference, and examining additional health outcomes.

2.4.1 Reconciling theoretical frameworks

Scholarship on the relationship between structural sexism and women's health has drawn from multiple theoretical backgrounds, but these studies have not been in sufficient dialogue with each other. Studies of violence victimization have been largely siloed from studies of other general or mental health outcomes, and both literatures have been disadvantaged as a result. While the violence victimization literature has remained very grounded in theory, it suffers from more methodologic concerns than the general and mental health research, which has been undertheorized or has not contended with the troubling implications of backlash. Merging the theory-based approach to hypotheses from the violent victimization research with the methodological strengths of the general and mental health research will be a benefit to future

research. Continuing to integrate insights from outside literature—for example, structural stigma and institutional racism—will continue to benefit this line of research, as both fields have developed strong traditions of utilizing relational, theory-based approaches and elucidating complex mechanisms to better inform theory and causal inference.

2.4.2 Unifying measurement

The majority of studies examining the health impacts of structural sexism have focused on violence victimization. After nearly 40 years of scholarship, there remains no consensus on the relationship between structural sexism and violence victimization, due to major heterogeneity across studies, especially regarding measurement. Addressing measurement variability and codifying best practices will be a vital advancement for this literature. In recent years, the field has been moving towards advocating for using exclusively relative measures of structural sexism. It remains to be seen which among the individual measurement approaches, however, are most pertinent for individual health and for the mechanisms that influence diverse health outcomes. Having a standard set of indicators and continuing to evaluate candidate indicators empirically will allow researchers to better understand earlier findings and contextualize future findings.

Regarding misclassification, working towards improved measurement of sensitive outcomes (i.e., intimate partner violence) and examining a variety of data sources (e.g., surveys rather than surveillance) will be an important next step for combating concerns regarding differential reporting in higher- vs. lower-sexism areas.

2.4.3 Improving causal inference

Better measurement alone is insufficient for demonstration of causality between structural sexism and women's health. To make the leap from association to causation, future

research needs to focus on ruling out alternative explanations, particularly ruling out confounding. Statistical adjustment for known confounders is a requirement for inference, but may not be sufficient to rule out concerns regarding residual confounding.

Certain design choices, however, may be employed to reduce the risks of residual confounding. For example, quasi-experimental designs may be helpful for measuring the impacts of specific laws and policies representative of structural sexism. These include difference-in-difference designs, interrupted time series, and synthetic control approaches that can allow researchers to approximate counterfactual conditions in the absence of perfect confounding control. Challenges to these techniques are that they are ill-suited for structural exposures that do not have clear start or end dates. Another alternative design approach would be the use of an instrumental variable, which allows for indirectly measuring the effects of structural sexism on health outcomes while avoiding concerns about residual confounding; the challenge in this scenario is selecting an appropriate instrument. In addition, emergent methods for epidemiology, such as agent-based modeling, accommodate complex, inter-related systems and unintended consequences, and these tools may prove extremely helpful for future research in this area.[129]

Other design choices that have been employed to triangulate inference in studies of other structural exposures (e.g., attitudes, laws and policies) include negative controls or falsification tests.[130] Studies of structural stigma have used both approaches to test the specificity of the exposure-outcome relationship (e.g.,[131,132]). To use a negative control approach, researchers would examine the impact of structural sexism on a sample who would theoretically not be vulnerable to its impact, i.e., men. If structural sexism is shown to impact a health outcome among women, but not men, then the specificity of those results suggest that the effects are indeed being driven by sexism, and not a third confounding variable which would impact women

and men equally. The use of a negative control approach is complicated for structural sexism should both women and men may be influenced by this exposure; indeed, it is unclear whether structural sexism is beneficial, harmful, or neutral for men's health, as discussed above. Therefore, the expectation of a null relationship among a negative control group may not be a realistic assumption. Regarding a falsification test, this approach would examine the effects of structural sexism on a health outcome that is not theoretically related to structural sexism; if structural sexism is shown to impact a theoretically relevant health outcome, but not a theoretically irrelevant one, that provides additional support for a causal association. The challenge with a falsification test is choosing an appropriate outcome that could not be plausibly related to structural sexism.

2.4.4 Examining other health outcomes

The majority of this research in the US has focused on violence victimization outcomes. However, the impact of structural sexism may vary across health domains. Convergence theory, described above, predicts that gender gaps will narrow or even reverse as women become more equal. While advancing more gender equality is largely considered a social good from the perspective of human rights, it is important for researchers to pay attention to how these large social changes may disrupt our understanding of which groups are high risk. In the case of gender gaps that historically advantage women, convergence could have serious implications for population health. Health outcomes such as substance use, injuries, and many cancers tend to be disproportionately higher for men than for women, and understanding the social determinants of these changing risks is essential for surveillance, screening, and treatment.

2.5 Conclusion

In this review, I provided a narrative and historical overview of scholarship on structural sexism and women's health outcomes in the US over the past 40 years. Despite spirited debate and multiple investigations, the consensus is largely unclear on the relationship between structural sexism and women's health. Divergent theoretical underpinnings led to inconsistencies in measurement and discrepant conclusions that have gone largely unresolved. Demonstrating the structural causes of women's health inequalities is a critical step towards improving women's health . To move forward, the field will benefit from solidifying theory-driven approaches and striving for greater consensus regarding measurement modalities. To do so, the various disciplines and scholarships that have explored these associations will need to be in better dialogue, so that they can build from each other's strengths with regards to both theories and techniques. Understanding how these macro-level exposures manifest in women's experiences is important for health and is important for health systems; using an evidence-based approach to setting policy and public health priorities, however, requires reconciling these discrepant findings. Advocating for more gender equality, while also advocating for a deeper understanding of the structural forces that influence women's health, continues to be paramount.

Chapter 3: Structural Sexism and Women's Alcohol Use in the United States, 1988-2016

3.1 Introduction

Alcohol use is a cause of heart disease,[133,134] cancer,[135,136] vehicle crashes,[137–141] stroke,[142,143] kidney disease,[144–146] and suicide[147,148]—six of the ten leading causes of death in the United States—and contributes to 10% of deaths each year among adults.[1,149] Binge drinking (drinking at least 4 drinks in a single setting for women, and 5 for men) is an acute cause of injury, toxicity, and violence and a chronic cause of cardiovascular disease, cancers, and liver disease.[150]

Gender is an important source of variation in alcohol consumption patterns.[f] On average, men consume alcohol and binge drink at higher proportions than women.[151,152] 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.[16,17,153] 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.[17,154–156]

This cohort of women evidencing increased alcohol consumption and binge drinking grew up and entered adulthood during times of 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

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[f] While gender minorities are at an increased risk of alcohol consumption and risky drinking,[352] recent trends studies have focused exclusively on those who identify as either men or women.

the 1960s-1970s. While gender gaps in education, income, and employment have been narrowing,[20,21,157] geographic variation remains in women's social, economic, and educational attainments relative to men's, leading to very different environments for women across US states.[158,159] Social and economic gender inequalities are indicators of structural sexism, defined as the "systematic gender inequality in power and resources favoring men within…U.S. state-level political, economic, and cultural institutions."[33,160] Structural sexism impacts women's health; as discussed in greater detail in Chapter 2, women living in states with lower levels of structural sexism evidence lower probabilities of chronic illness, poor self-rated health, and depression relative to women living in states with higher levels of structural sexism.[33,41,42,95]

Whether structural sexism influences women's alcohol patterns is unknown. Rates of alcohol consumption and binge drinking vary across US states,[161] and structural sexism may be a contributor to this geographic heterogeneity. Geographies with greater gender inequality have more restrictive alcohol norms related to the acceptability of women's alcohol consumption[162] and lower rates of higher education among women,[110] both of which are related to patterns of alcohol use.[162–170] However, geographies also vary in other structural determinants of alcohol consumption: for example, policies regarding alcohol consumption and sales which vary across US states—e.g., limiting alcohol sales on Sundays[171,172] or during certain hours,[173] limiting happy hours,[174] alcohol taxes,[175–179] impaired driving laws[180]—are determinants of heterogeneity in alcohol use, because they limit both the contexts of consumption and alcohol availability. Alcohol policies have been associated with indicators of gender equality, thus may be related to structural sexism; for example, states with restrictive alcohol policies also have restrictive reproductive policies for women.[181] Further determinants of geographical variation in alcohol consumption patterns include demographics (i.e., rurality, poverty) and attitudes (i.e. religious

conservatism), which may additionally influence both structural sexism and the mechanistic pathways through which it operates.[168,170,182–186]

Two studies have used cross-national data to examine elements of the relationship between structural sexism and alcohol consumption among women. The first of these[119] 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 alcohol consumption volume on average among mothers age 24 to 49.[187] In the United States, a single cross-sectional study[40] tested associations among five separate indicators of women's status (socio-economic status, gender equality on socio-economic status, reproductive rights, violence against women policies, political participation) and women's alcohol consumption, and overall concluded there was no association. This study relied on data from 2005, around the very beginning of the time when alcohol researchers began to observe increases in women's drinking.[16,17,188] No domestic research has examined the particular cohort of women (i.e., born in the 1970s and 1980s) evidencing increased alcohol consumption, nor used data from the past 15 years.

In this study I examine 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 increasing alcohol consumption the most in recent years.[17,154,165] I hypothesize that lower levels of structural sexism are associated with increases in alcohol use and binge drinking, which is consistent with national trends in structural sexism, women's alcohol use, and women's economic and social advances. I hypothesize that this relationship is mediated through increases in individual social position, i.e., college attainment, and through declines in restrictive alcohol norms, i.e., how permissive alcohol consumption is deemed.

However, the relationship between structural sexism and alcohol consumption patterns may either be null, or the inverse of the direction I hypothesize, due to a competing mechanistic pathway: the impact of structural sexism on mental health. Women living in states with lower vs. higher levels of structural sexism have reduced odds of psychiatric disorders, including depression.[41,42] Mood disorders, particularly depression, are positively associated with alcohol consumption.[189–195] Therefore, a competing hypothesis is that because residing in a state with relatively lower structural sexism is protective against mood disorders, women in higher structural sexism states may instead be at increased, rather than decreased, risks of alcohol consumption and binge drinking. I will therefore test not only the overall associations but also three potential mediators of this relationship; college completion, restrictive alcohol norms, and depressive symptoms.

3.2 Methods

Sample

Monitoring the Future (MTF) includes an ongoing, prospective cohort study of young people who are followed into adulthood. The present analysis includes surveys of high school seniors followed every 2 years until age 30, and then at ages 35, 40, and 45. Baseline surveys were administered within schools, and follow-up surveys were administered via mail. Recruitment is ongoing; therefore, women born later have had fewer opportunities for follow-up. The cohort of interest for this study was born between 1970-1987. The eligible sample was MTF respondents who identified as women, who lived in the United States (excluding Washington,

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D.C.[g]), and who were high school seniors between 1988 and 2006, surveyed 1988-2016 from approximately age 18 (senior year) through up to approximately age 45. By 2016, all respondents had received the 5th follow up (corresponding to approximately ages 27-28), and the oldest respondents had received up to the 9th follow-up. Supplemental Table 3.1 shows the eligible sample (N=23,862 unique respondents at baseline), organized by cohort (i.e., year in which respondents were seniors) and study wave, with percent retained at each wave relative to the prior wave.

Outcomes

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." Overall, responses were right-skewed (kurtosis = 0.24; Supplemental Figure 3.1) with a median of "1-2 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?" Response options were ordinal, ranging from "None" to "10 or more times"; responses were highly right-skewed (kurtosis = 3.23; Supplemental Figure 3.2) and approximately 70% of responses reported no binge drinking in the past two weeks. Therefore, binge drinking was dichotomized as "none" or "at least once."

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[g] Washington, D.C., was excluded for two reasons: first, it does not have measures of political gender inequality (an indicator of structural sexism used in this analysis) because it does not have the same structure of representation as the other states; next, it is a municipality, rather than a state. Assigning a state-level structural sexism score to residents of a city, and comparing associations where some exposures are at the state level and others are at the city level, could spuriously bias results.

Exposure

Consistent with the recommended practices described in Chapter 2, to measure structural sexism I chose among relative (as opposed to absolute) indicators, and used factor analysis to construct an aggregated (rather than multi-domain) 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 relative income, relative education, and relative labor force engagement,[75,81,83–85] 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).[83,84] 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 (e.g., the Gender Equality Index,[122,123] the Status of Women index,[37] the United Nations Gender Inequality Index[125]), but have not been empirically evaluated using data reduction techniques.

Therefore, to compose a measure of state-level structural sexism, I 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, and political representation. Supplemental Table 3.2 shows the candidate indicators and their data sources. I derived state-level scores for structural sexism using exploratory and confirmatory factor analysis. A description of the modeling and selection process is described in greater detail in Appendix 3.1. Results from the exploratory factor analyses indicated that the 5 variables best loaded onto a one-factor solution. These were the percentage of male state

legislators, the male/female ratio for proportion of residents living at or above the federal poverty line, the male/female ratio for proportion of adults in the labor force, the male/female ratio for proportion of working adults in management occupations, the male/female ratio for proportion of working adults who are self-employed. Factor loadings for each retained item are shown in Supplemental Table 3.3. The final confirmatory factor model was two-level (to account for repeated measures by state), and fitted the one-factor solution with a fixed effect for calendar year, as shown in Supplemental Figure 3.3. 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. Supplemental Figure 4 shows the overall factor scores for all 50 states, with minimum and maximum values, across 1988-2016.

Confounders

Figure 3.1 shows a directed acyclic graph depicting the relationships between the exposure, outcomes, and all confounders and mediators. All confounders were chosen based on their theoretical relationship to the exposure and outcomes; I controlled for common causes of both. State-level confounders were population density,[186] state-level socioeconomic status, defined as percentage of the population living below the poverty line; state-level income inequality, operationalized as the Gini coefficient;[59] state-level religious conservatism, operationalized as the percentage of the population who were Evangelical Christians or Mormons.[182] In addition, I controlled for state-level alcohol policy climate,[196] which is a cause of state-level differences in alcohol consumption patterns, and may reflect other state-level attitudes

that also contribute to state-level sexism. Detailed regarding state-level confounders and data

sources are described in Appendix 3.2.

Figure 3.1: Directed Acyclic Graph of hypothesized relationships
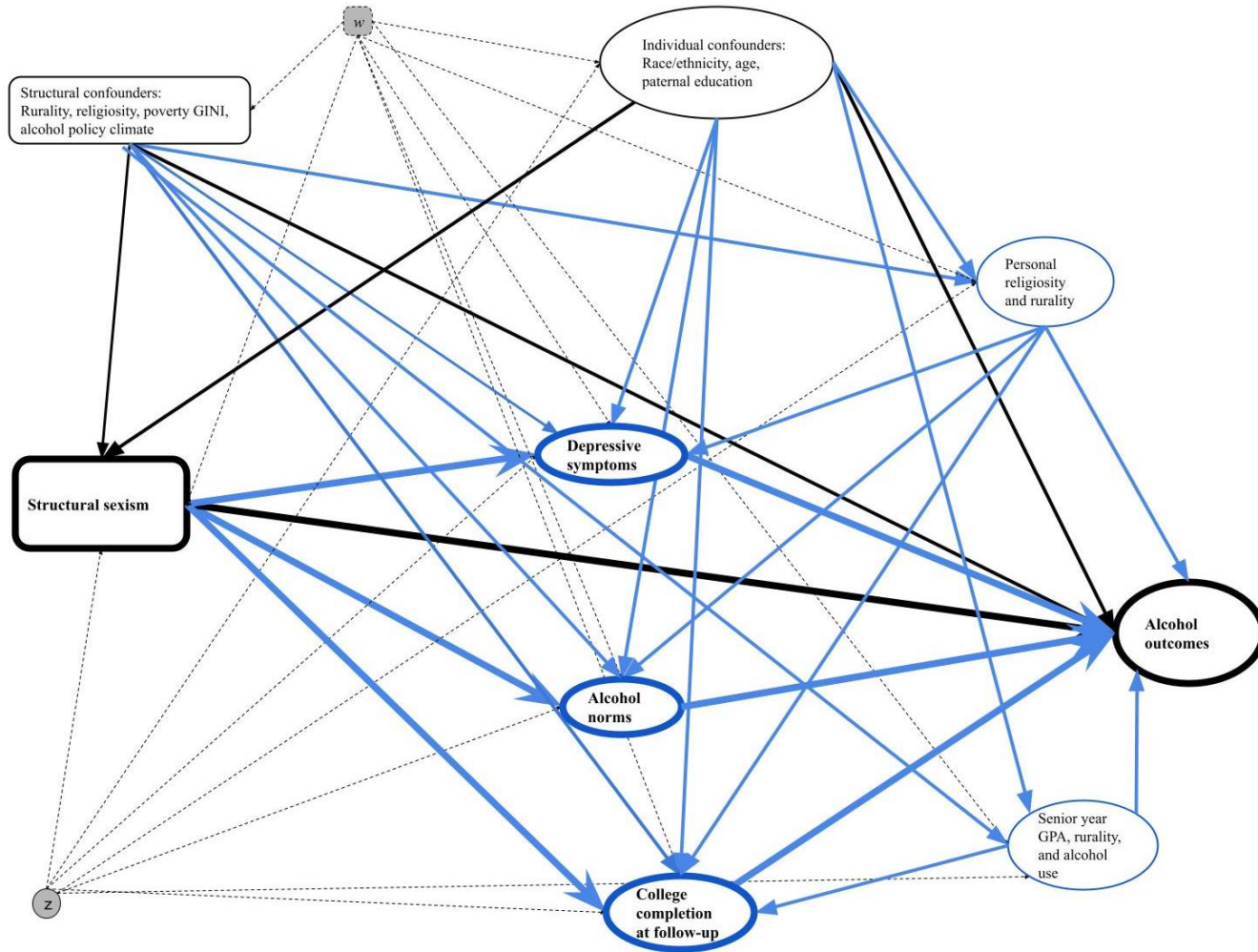
Note: Variables in boxes are measured at the state level. Variables in ovals are measured at the individual level. Black color denotes marginal analyses. Blue color denotes mediation analyses. Grey measures (w, z) denote unknown, unmeasured variables.

Regarding individual confounders, while they could not plausibly cause structural sexism, I selected individual measures that were causes of alcohol outcomes and causes of differential *exposure* to sexism—i.e., state of residence in a particular study year. These were 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 it is a predictor of both childhood socio-economic status and adult health.[197–199] In the eligible sample, the majority of respondents (77%) were non-Hispanic White, so race/ethnicity was dichotomized as non-Hispanic White, or not.[h]

Mediators

Secondary analyses evaluated the potential mediating effects of depressive symptoms, alcohol norms, and education. Among the respondents, approximately 15% were assigned to receive versions of the surveys at each wave 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 (and none at follow-up).

I used an index of four responses to measure depressive symptoms:[200] "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). Supplemental Figure 3.5 shows reliability

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[h] The racial/ethnic composition of the sample was White (77%), Black (9%), Hispanic (8%), Asian (3%) and other race (4%).

statistics for these indicators both other time and by study wave; in general, they exhibit high reliability (alpha > 0.80) consistently across study years and waves.

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.

Confounders of the mediators

In mediation models I additionally controlled for two time-varying measures that confound the mediator-outcome relationships: personal religiosity, operationalized in response to the prompt "How important is religion in your life?" as low ("Not important," "A little important") or high ("Pretty important," "Very Important"); and rurality, categorized as urban (living in a city with greater than 50,000 residents), suburban (living in a suburb of a city with greater than 50,000 residents), or rural (living on a farm, in the country, or in a city with fewer than 50,000 residents). Both religiosity and urbanicity are common causes of alcohol norms, depression, and college completion, as well as alcohol use.[185,201–205] For college completion, three additional mediator-outcome confounders were included to account for baseline predictors of both alcohol use and college completion: senior year grade point average (GPA, measured continuously), senior year alcohol use (i.e., baseline measures of alcohol consumption frequency and binge drinking, respectively), and senior year rurality.

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

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 3.1). 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 age 18: gender, race/ethnicity, college plans, truancy, high school grades, number of parents in the home, religiosity, parental education, alcohol use, cigarette use, marijuana use, other illicit drug use, region, cohort, and sampling weight correcting for over-sampling 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 (rather than time-varying) predictors, previous research on this sample has demonstrated that both attrition and subsequent adult alcohol outcomes are highly related to grade 12 alcohol use.[206]

A second source of missingness in MTF is item non-response; of the 118,684 observations for sample women between 1988-2016, 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 using chained equations was used to impute missing values. Ten datasets were imputed, using all study

measures and attrition weights as predictors, and model estimates were combined using Rubin's Rules.[207]

Analyses

Association between structural sexism and alcohol outcomes

Associations between structural sexism and alcohol consumption frequency and binge drinking frequency were examined using three-level multilevel models with random effects 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, I 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, and then individual confounders. All analyses were weighted using both original sampling probabilities to account for complex sample design and attrition weights.

Mediation analyses

I conducted mediation using traditional mediation analysis, i.e., by conditioning the model on each mediator; given the time-varying structure of the data, this approach does not allow for valid estimates of the indirect effects.[208–211] However, traditional mediation analysis allows for quantifying controlled direct effects conditioned on mediators, assuming all other mediation assumptions are satisfied.[211] Since my study questions are more concerned with the

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plausibility and contribution of specific mediation pathways, rather than precisely quantifying their indirect effects, I show estimates of total effects, the controlled direct effects, and the change in model parameters.

I was 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. Prior to analyzing each mediator, I tested the assumptions that the mediator was related to both the outcomes and the exposure, and that there was no mediator-exposure interaction. Appendix 3.3 includes a detailed description of the limitations of traditional mediation given this data structure, as well as results from the tests of mediation assumptions. Models estimating total effect included control for exposure-outcome confounders, and models estimating controlled direct effect included control for all measured confounders (including mediator-outcome confounders).

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 waves 7-9. For this mediation analysis I subset the respondents to those who received the depression questionnaire (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. For this mediation analysis, I subset the respondents to those who had received the norms questionnaire (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 I subset the analytic sample to only follow-up responses beginning in the 2nd follow up, at age 21 or 22 (N=73,690 observations).

Specificity and sensitivity analyses

The specificity analysis tested to what extent structural sexism influences alcohol outcomes among men in the MTF in the same age group (N=91,942 observations). This is a common test of the specificity of the exposure when examining structural exposures that target select populations:[130] if associations between structurally discriminatory exposures and the outcomes are found in the marginalized group (i.e., women) but not in the non-marginalized group (i.e., men), that provides additional evidence that the structural exposure of interest, and not some residual confounder, is contributing to the observed associations in the marginalized group. Because previous research suggests that men's health may also be sensitive to changes in structural sexism,[39,59] I chose to relax the assumption that the effects would be null among men. Rather, to test the specificity of the relationship, I hypothesized that while men's alcohol consumption patterns would be related to structural sexism, any associations would be less pronounced than among women.

I performed three sensitivity analyses to test the robustness of associations to other modeling specifications: first, by restricting the study sample to those who remained in the same US state during the study period, to confirm that nesting individual observations within states did not contribute to meaningful differences with regard to study estimates or standard errors; next, by restricting observations to the years 1999-2016, to confirm that linear interpolation of the

alcohol policy covariate (described in greater detail in Appendix 3.2) did not spuriously bias results; finally, using a more sensitive measure of binge drinking (the probability of consuming four or more drink in a row, rather than five or more drinks, to be consistent with national guidelines for women's binge drinking specifically[212]). This measure was used in sensitivity analyses, rather than in the main analyses, as it was only asked in recent years to a small subset of respondents (N=3,340 unique respondents, N=5,651 unique observations) and had limited coverage across all 50 states

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

3.3 Results

Table 3.1 shows the covariate distribution among observations from sample women, stratified across structural sexism level. For the purposes of descriptive statistics, high structural sexism was defined as at or above the median national level, and p-values were calculated using Rao-Scott chi-square tests, which account for non-independence of observations.[213] Observations within states with lower structural sexism evidenced higher state populations densities, lower levels of religious conservatives and poverty, and with less restrictive alcohol policies. Observations from lower structural sexism states compared to higher structural sexism states also were related to higher prevalence of any alcohol consumption (66% vs. 60%), higher paternal education (39% with fathers who completed college vs. 35%), higher personal education (53% completed college vs. 35%), and lower prevalence of living in rural area (38% vs. 44%).

Table 3.1 Outcome and covariate distributions among MTF women in eligible sample, 1988-2016, dichotomized by structural sexism level

	Low sexism* (N=59,931 observations)	High sexism* (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 covariates			
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 covariates			
Father has college degree	22,353 (39%)	18,941 (35%)	<0.001
Rural	23,036 (38%)	24,441 (44%)	<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	5,890 (16%)	7,163 (21%)	<0.001
Completed 4 or more years of college (in follow-up sample)	26,626 (53%)	9,502 (35%)	<0.001

*For descriptive statistics, high structural sexism refers to states with at or above median level; low structural sexism refers to states below median level

Figure 3.2 shows the mean score for structural sexism in the analytic sample between 1988-2016, which declined by approximately 26% of one standard deviation each year. Trends in indicators of structural sexism, all of which decreased over time, are shown in Supplemental Figure 3.5. The most pronounced decreases were in the gender ratio of self-employed adults, which fell from 2.9 to 1.7; the gender ratio in managerial occupations, which fell from 2.2 to 1.4; and the percentage of legislators who are male, which fell from an average of 86% to 76%.

Figure 3.2 Average structural sexism scores in MTF sample, 1988-2016
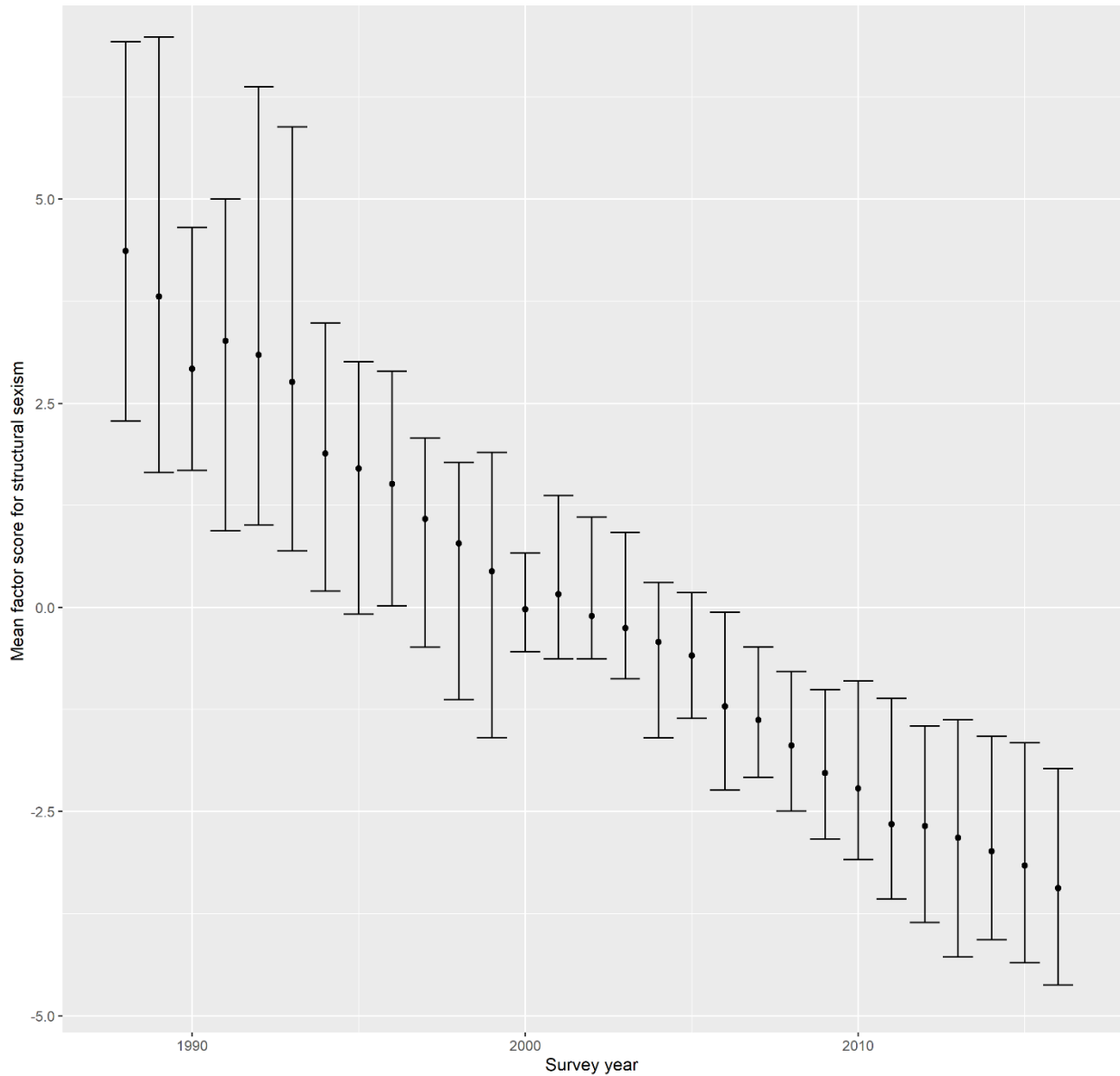
Table 3.2 shows the estimates examining the relationship between structural sexism, measured continuously, and alcohol outcomes. In fully-adjusted models, higher levels of structural sexism remained associated with fewer occasions of alcohol consumption (RR: 0.974, 95% CI: 0.971, 0.976). For binge drinking, individual age was a major confounder biasing the

results in the unadjusted models; in fully-adjusted models, greater structural sexism was associated with lower probability of any binge drinking (OR: 0.917, 95% CI: 0.909, 0.926).

Table 3.2 Associations between structural sexism and alcohol consumption outcomes among women in MTF, 1988-2016

	Alcohol consumption frequency Risk ratio (95% CI) p-value	Binge drinking Odds ratio (95% CI) p-value
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 confounders	0.977 (0.976, 0.979)	1.035 (1.027, 1.043)
Model 3: Structural sexism, adjusted for state-level and individual confounders	0.974 (0.971, 0.976)	0.917 (0.909, 0.926)

Table 3.3 shows results for the mediation models. All mediation assumptions were satisfied, with one exception (see Appendix 3.3): in confounder-adjusted models depressive symptoms were unrelated to alcohol consumption frequency, thus I 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 estimated 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 estimated 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 estimated 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 estimated parameter) but not for 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 estimated parameter).

Table 3.3 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 direct effect of structural sexism		0.893 (0.872, 0.915)
Percent change in unexponentiated 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 direct effect of structural sexism	0.977 (0.974, 0.980)	0.926 (0.915, 0.938)
Percent change in unexponentiated 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 direct effect of structural sexism	0.973 (0.970, 0.977)	0.903 (0.891, 0.916)
Percent change in unexponentiated beta	27%	3%

In the male sample, I 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, though only

for binge drinking were tests of interaction between structural sexism and gender significant

(RERI = -0.07, 95% CI: -0.09, -0.05). Sensitivity analyses for the association between structural

sexism and alcohol outcomes during only study years 1999-2016 (RR: 0.960, 95% CI: 0.956,

0.964 for occasions of alcohol consumption; OR: 0.910, 95% CI: 0.894, 0.926 for binge drinking), among only those who never moved states (RR: 0.971, 95% CI: 0.968, 0.973 for occasions of alcohol consumption; OR: 0.916, 95% CI: 0.908, 0.925 for binge drinking), or among those who were queried using a more sensitive binge drinking measure (OR: 0.921, 95% CI: 0.838, 1.012) did not impact results or interpretation.

3.4 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 (34% for alcohol consumption, 24% for binge drinking) but neither relationship was mediated by depression. Alcohol consumption frequency, but not binge drinking was partially mediated by education. For both outcomes, decreases related to structural sexism were more pronounced among women than among men, providing further evidence that for this outcome changes in structural sexism are disproportionately impacting women. Based on this study's findings, reductions in structural sexism may have contributed to both national increasing trends in both alcohol consumption and binge drinking among women and to geographic patterning of women's alcohol patterns.

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 symptoms capture the affective aspects of depression, but do not fully account for the somatic and motor 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) is still a topic under debate.[194,214–216] While several studies have examined the impact of structural sexism on depression or depressive symptoms,[41,94,217,218] none have examined them in the context of alcohol use, and few have examined other mental health outcomes. However, structural sexism has been implicated in not only internalizing but also externalizing disorders,[217] which are highly related to alcohol abuse and dependence, particularly among women.[219] To what extent externalizing disorders—rates of which have been increasing among women[220]—contribute to associations between structural sexism and alcohol use is beyond the scope of the present study, but warrants further investigation. Regardless, the direction of effect overall—i.e., lower risks of alcohol both outcomes in high sexism states— suggests that changes in mental health symptomology are not the major operating pathway for these relationships, as one would expect to see associations in the opposite direction were that the case.

Restrictive alcohol norms, however, partially mediated the relationships between structural sexism and both alcohol outcomes. Women in high structural sexism states were more likely to endorse restrictive views about alcohol use than those in low structural sexism states, consistent with cross-national research showing that alcohol norms are sensitive to other indicators of gender inequality.[162] Restrictive norms are related to lower risks for both alcohol outcomes.[221] 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.[222] As declines in structural sexism have conferred more gender equality across economic, political, and social

domains, norms have shifted commensurately. These shifting norms, and the changing social and economic landscape, has manifested in meaningful changes in women's behavioral health.[110]

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. While college attendance is frequently characterized by high levels of both alcohol consumption and binge drinking—due to college social contexts (i.e., lack of parental supervision) and increases in alcohol availability during this time period[223–225]—after graduation, rates of binge drinking precipitously decline, but alcohol consumption rates remain fairly high.[226,227] 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.[228–233] The following chapter will further examine relationships among structural sexism, alcohol use, and work-related predictors. To date, a single prior study examined this relationship within the United States, finding no relationship between structural sexism and alcohol consumption frequency or binge drinking.[40] The previous null association was likely due both to the limitations of using a sample in a single study year (2005) as well as controlling for individual variables—including education, employment, and income variables—which are likely to be mediators or moderators, rather than confounders, of the structural sexism-alcohol relationship.

I observed that for both outcomes, the associations with structural sexism were more pronounced among women than among men. This specificity analysis among the male sample

was a way of indirectly assessing the possibility of spurious results due to residual confounding alone. If results were entirely attributable to an unmeasured confounder, one would not expect to see a differential relationship in the exposure-outcome relationship across gender lines, as other structural features would likely impact both men and women similarly. While the interaction models for gender did not reach significance for alcohol consumption frequency, the estimates in stratified models sufficiently diverged, reassuring me that the associations were meaningfully attenuated in the male sample and that residual confounding is unlikely to be the major determinant of the observed associations.

For both binge drinking and alcohol consumption, the observed associations were relatively modest: a 1-unit increase in structural sexism was associated with an approximate 3% reduction in alcohol consumption frequency and 8% reduction in binge drinking probability. This magnitude of observed effects, however, is consistent with the modest but meaningful increases in alcohol consumption and binge drinking observed for women in recent years.[16,17] Notably, the association was stronger for binge drinking, consistent with the patterning in the literature: binge drinking among women has been shown to increase at higher rates than alcohol consumption, at a rate of up to 6% per year, compared to approximately 1% per year for alcohol consumption per se.[16] With regards to clinical impact, the association between structural sexism and binge drinking may be more concerning than the association with alcohol consumption frequency, as the magnitude of the effect is higher and it is a riskier pattern of alcohol intake.

A limitation of both outcome measures is that they are frequency, rather than volume, measures. Women reporting similar frequencies may have consumed very heterogeneous volumes of alcohol. However, frequency measures are closely correlated with volume measures,[234] and have been shown to meaningful predictors of some health complications and

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mortality, even among those who consume low volumes of alcohol per occasion.[2,3] Regarding

binge drinking, while national guidelines typically measure women's binge drinking as 4 or more

drinks in a single setting,[212] 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.

In the present study, I controlled for the effects of age, but the relationship between

structural sexism and alcohol use may vary throughout the life course. Both childhood and

adulthood social exposures influence alcohol trajectories,[119] 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.

Further, to what extent structural sexism moderates other individual-level exposures is an

important question; I further investigate these relationships in the upcoming chapter.

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.

However, increases in women's equality have conferred some health risks, which need to be

combated with counter-messaging and evidence-based preventive care. The patterning of

increases in women's alcohol consumption and binge drinking in relation to decreases in

structural sexism is consistent with findings from other health domains indicating that reductions

in sexism may lead to "convergence" of women's adverse health behaviors with men's.[39,114] As

women have increasingly occupied traditionally male spaces and social positions, health risks

change commensurately because men and women begin to share common behaviors and

exposures, including alcohol consumption.[119] These changes have meaningful implications for both women's health and for health systems, given that alcohol consumption is a contributor to five of the top causes of mortality for US women – heart disease, cancers, respiratory diseases, stroke, and Alzheimer's disease.[235,236] Promoting greater gender equality for women is not at odds with improving public health surveillance and interventions for women's worrisome increases in alcohol use—rather, understanding the health-relevant forces in women's lives is essential to tailoring effective prevention, treatment, and policy.

Chapter 4: Structural Sexism Moderates Work and Occupational Risks for Alcohol Consumption and Binge Drinking Among US Women, 1989-2016

4.1 Introduction

The previous chapter investigated associations between structural sexism and alcohol use among women. The current chapter investigates established predictors of increased alcohol use–i.e., work status and occupational characteristics—and to what extent structural sexism modifies associations between these individual-level work characteristics and alcohol consumption frequency and binge drinking among US women.

Alcohol consumption remains highly prevalent among working-age adults in the United States. Seventy-seven percent of US workers report any past-year alcohol consumption.[237] Patterns of alcohol use vary by labor force engagement: adults who work evidence higher probabilities of both alcohol consumption and binge drinking (i.e., consuming multiple drinks in a setting) than unemployed working-age adults.[18].

Among workers, occupation is a source of heterogeneity for alcohol risks. Approximately 90% of workers in "white-collar" jobs (e.g., clerical, professional occupations) consume alcohol,[238] and their risks of consumption are higher than those in traditional "blue collar" (e.g., manual labor, food service) jobs.[239] Similarly, higher occupational prestige—the social standing conferred by particular occupations—is associated with higher a probability of consuming alcohol.[230,233] While those in higher status careers are more likely to report consuming alcohol than abstaining completely, they may be less likely to endorse binge drinking or other high-intensity alcohol consumption patterns.[229–232] However, recent binge drinking trends suggest

these associations may be changing: workers in higher status careers experienced

disproportionately high increases in binge drinking in the past decade.[240]

Work context and climate are additional occupational characteristics that influence

working adults' alcohol risks.[237,241] One such contextual determinant of alcohol consumption is

occupational gender composition. Workers in male-dominated fields are more likely to report

any alcohol consumption, risky drinking, and drinking with co-workers than those in female-

dominated fields.[242–244]

The patterning of alcohol risks in relation to occupational characteristics may be shifting,

in part because the composition of the labor force has been changing in recent decades. Women

outside the labor force (i.e., homemakers) are historically at lower risks for alcohol consumption

and binge drinking than those in the labor force.[245–247] However, increasing proportions of

women have entered the work force and into high-status, historically male-dominated

occupations.[157,248] As a result, labor-related risks for alcohol use may be more salient for recent

trends in women's drinking than for recent trends in men's drinking. As women increasingly

enter these careers, their alcohol risks may increase commensurately. Indeed, working-age

women have evidenced increases in both alcohol consumption and binge drinking over the past

two decades,[16,17] with increases most concentrated among women in high social positions and in

high prestige occupations.[165,240]

At the population level, shifts in the sex composition of the labor force—i.e., more

representation of women in the workforce and in high-status careers—are reflective of decreases

in structural sexism, and women living in lower structural sexism environments report higher

rates of alcohol consumption and binge drinking than those in high structural sexism

environments, as demonstrated in Chapter 3. More representation of women in the labor force

and in high-status careers at the state level, however, does not imply that every state resident is herself working in a high-status career or even working at all. The previous study examined the average effects of structural sexism on women's alcohol consumption patterns, but population-level exposures (e.g., structural sexism) may exacerbate or attenuate the risks conferred by individual-level exposures (e.g., being employed, occupational characteristics). Women who work or who are in high-status careers in a high structural sexism environment may have very different alcohol risks than those in a low structural sexism environment. To what extent structural sexism, a population-level phenomenon, modifies the effects of women's individual-level work-related alcohol risks remains unknown.

Cross-national data suggest that structural sexism indeed moderates the relationship between employment and women's alcohol risks. In a cross-national comparison of women with children in 16 industrialized countries in the early 2000s, working mothers reported higher volumes of alcohol consumption than non-working mothers, but the strength of the association between work and alcohol consumption varied by country-level gender equality.[187] In countries with lower levels of gender equality (i.e., more sexism), working mothers endorsed higher volumes of consumption than non-working mothers; but this relationship was attenuated in countries with higher levels of gender equality (i.e., less sexism), where working mothers endorsed lower volumes of consumption than non-working mothers.

To date, no study has examined this relationship within the United States, but the evidence within the US suggests that the patterning of the relationships among structural sexism, work, and alcohol use may differ from the cross-national findings. The previous work examined a select sample (mothers) and the data were from before the time period when women's alcohol consumption began to increase in the United States. Further, in Chapter 3, I observed an inverse

relationship between structural sexism and alcohol consumption frequency and binge drinking; that is, women living in areas with the lowest levels of structural sexism reported the highest alcohol use. Combined with insights from national trends in alcohol consumption and binge drinking–that is, that the increases are concentrated among higher status women[165,240]—and the reality that workers in higher status and majority-male occupations consume alcohol at higher frequencies, I hypothesize that increased risks of alcohol use related to work characteristics will be exacerbated, rather than attenuated, in areas characterized by lower levels of sexism.

In sum, there is strong theoretical evidence linking occupational characteristics to alcohol risks; women have increasingly occupied managerial, high-prestige positions and moved into majority-male fields, and these characteristics are important determinants of alcohol use. Structural sexism may be an important modifier of these relationships, and in the current study I examine whether the associations between work characteristics and alcohol outcomes varies across levels of structural sexism. My hypothesis is that employed women will evidence higher frequencies of alcohol consumption and binge drinking than unemployed women, consistent with prior research, but that employed women in low sexism environments will evidence the highest rates of both alcohol outcomes. I anticipate a similar pattern across high- versus low-status occupational characteristics, and comparing women in majority-male occupations to majority-female occupations: that is, the increased risks for alcohol use and binge drinking conferred by higher status occupations and by working in a majority-male occupation will be further exacerbated in low sexism environments.

4.2 Methods

Sample

The current study used data from mid-life women in Monitoring the Future (MTF), an ongoing, prospective cohort study of young people followed from senior year of high school approximately every 1-2 years until age 29/30, and then approximately every five years beginning at 35. Because the majority of workers do not enter the labor force as full time workers until adulthood,[249] the study sample was restricted to the follow-up surveys only, beginning when respondents were 19/20 years old, through the ninth follow-up (corresponding to approximately age 45). Eligible respondents were MTF women who lived in the 50 United States and were high school seniors between 1988 and 2006, followed over the years 1989-2016 (N= 16,571 unique respondents at first follow up). All respondents have had the opportunity to respond to the 5th follow-up survey (at approximately age 27-28) and older respondents (i.e., those who were seniors in 1988-1989) have had the opportunity to respond to the 6-9th follow-up survey (see Supplemental Table 2.1).

Outcomes

The alcohol outcomes were alcohol consumption frequency, and binge drinking frequency, both ascertained using self-report. 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?" Ordinal responses ranged from, "0 occasions," "to "40 or more occasions." Responses were right-skewed a median of "1-2 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 60% of respondents reported no binge drinking. Therefore, binge drinking was further dichotomized as "none" or "any."

Exposures

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The exposures were employment status, professional working in a technical or professional occupation (hereafter, "professional status"), prestige, and occupational gender composition, assessed at each wave.

I examined employment status using two different operationalizations; first, dichotomously, as reporting at least one full time or part time job vs. not; next, as a categorical three-level variable, 1) as working more than one job/a single full-time job, 2) working part-time, or 3) unemployed.

Respondents were asked to choose among options provided by MTF that best described their current or most recent primary job title and these were used to categorize occupational prestige, professional status, and gender ratio. To obtain numerical estimates of prestige and gender composition, I matched MTF occupation categories to US census standard occupation codes. These linkages and a description of how the numeric values were assigned are shown in Appendix 4.1 and Supplemental Table 4.1.

Professional status was evaluated based on classification systems[i] commonly used by the Bureau of Labor Statistics.[250] For the MTF occupation options, managers or administrators, professionals without a doctoral degree, and professional with a doctoral degree were coded as professional status, and the remaining options were coded as not. Respondents were categorized as professional status or not based on their occupation at each observation.

Prestige measures were obtained from the General Social Survey, a nationally-representative opinion survey which in 2012 asked a sub-sample of respondents (N=1001) to

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[i]For the Bureau of Labor Statistics, this category is described as "Management, professional, and related occupations" and corresponds to codes 11-0000–29-0000; examples include management occupations, business occupations, legal occupations, and life science occupations.

rank samples of 9 occupations each by placing them on a "ladder" representing low to high

social standing.[251,252] Each respondent rated batches of 90 occupations total, which overall

represented the possible occupations encoded by census codes (N=820 in 2012). Rankings were

transformed into numerical values, ranging from a possible 0 (lowest prestige) to 100 (highest

prestige) value; these were then fitted to a hierarchical linear model to predict average prestige

score of each occupation with adjustment for inter-rater variability. MTF occupations were

linked to these ratings and an average score was calculated for each. Each of the 14 MTF

occupations was coded as "high prestige" or "low prestige" based on whether they were above or

below the average prestige level (mean score = 46.7). Of note, all of the managerial and

professional occupations were ultimately categorized as high prestige.

Occupational gender composition was calculated by estimating the average proportion of

women in each MTF occupation, using estimates from the Current Population Survey (for years

1989, 1991-1999) and the American Community Survey (for years 1990, 2000-2016).

Occupations where women were fewer than 50% of the workers were classified as "majority

men," and occupations with 50% or greater women workers were classified as not.

Table 4.1 shows each MTF occupation, whether it is classified as professional, high or low

prestige, and majority-men or not.

Table 4.1 MTF occupations, work characteristics, and linkage to SOC major occupation codes

MTF occupation category	Professional status	Prestige, dichotomous	Occupation is majority men
1. Laborer (custodian, material mover, maid, landscape worker, farm worker)	No	Low prestige	Yes
2. Service worker (food preparer or food service worker including fast food, waiter/waitress, call center worker, stock clerk, order filler,	No	Low prestige	No

nursing aide/orderly, teacher assistant, childcare worker)			
3. Operative or semi-skilled worker (bus or truck driver, maintenance or repair worker, assembly line worker)	No	Low prestige	Yes
4. Sales clerk in a retail store or by phone (cashier, supervisor of retail workers)	No	Low prestige	No**
5. Clerical or office worker (secretary, receptionist, bookkeeper, supervisor of office workers, bank teller, postal clerk or carrier)	No	Low prestige	No
6. Protective service (police, firefighter, paramedic)	No	High prestige	Yes
7. Military service	No	High prestige	Yes
8. Craftsman or skilled worker (carpenter, mechanic, machinist, welder)	No	Low prestige	Yes
9. Farm owner, farm manager	No	Low prestige	Yes
10. Owner of a small business	No	High prestige	Yes
11. Sales representative (insurance agent, real estate)	No	High prestige	Yes
12. Manager or administrator (office manager, government official, sales manager)	Yes	High prestige	No
13. Professional without doctoral degree (registered nurse, school teacher, accountant, architect, artist, information technology worker)	Yes	High prestige	No
14. Professional with doctoral degree or equivalent (lawyer, physician, dentist, scientist, college professor)	Yes	High prestige	Yes

**In one survey year (1995), occupation was <50% women (49% women); because it was ≥ 50% women for all other years, this category was coded as not majority men

Moderator

I examined whether associations between employment status, professional status, prestige, occupational gender composition, and alcohol outcomes were moderated by structural sexism. Structural sexism was operationalized using the factor-analytically derived measure

described in Chapter 3. Briefly, a two-level factor model was run on 5 state-level indicators of sexism (the percentage of male state legislators; the male/female ratio for residents living at or above the federal poverty line; the male/female ratio for the proportion of adults ages 16 and over in the labor force; the male/female ratio for the proportion of working adults in management occupations; and the male/female ratio for the proportion of working adults who are self-employed), modeled using repeated measures for state and with historical time modeled as a predictor of the latent variable. A 1-factor solution showed good fit (RMSEA=0.09, TLI=0.93, CFI=0.96). The scores were time-varying, such that each state was assigned a model-based factor score for each year. In the analytic sample, the mean score was -0.42 (range: -4.62 to 6.98).

Covariates

To reduce the potential effect of confounding, I adjusted for both state and individual level covariates that may associations among work characteristics, structural sexism and alcohol patterns. The measures, their data sources, and how they were operationalized are described in greater detail in Chapter 3. The state-level confounders (all time-varying) are state-level alcohol policy climate,[196] state-level rurality,[186] state-level economic status, state-level GINI coefficient, and state-level religious conservatism.[182] The time-varying individual-level covariates are age, personal religiosity, and rurality; the time-invariant individual-level covariates are race/ethnicity, paternal education, and rurality and religiosity at baseline (senior year).

In addition, I added three additional variables which impact both labor force engagement and alcohol consumption patterns: marriage status, dichotomized as currently married or not (i.e., single, engaged, separated, divorced, widowed); highest education attainment (11th grade (minimum), 12th grade, one year of college, two years of college, three years of college, four years of college, or five or more years of college (maximum)); and alcohol consumption and

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binge drinking at baseline (senior year), for alcohol consumption and binge drinking models, respectively. While education was considered a mediator in Chapter 3, for the purposes of the analyses shown here, it was modeled as a control variable to ensure that I was comparing women who otherwise have similar educational backgrounds despite differences in work status. Educational attainment and marriage status were time-varying; alcohol consumption and binge drinking at baseline were time-invariant.

Missing data

The eligible sample for this study is 16,571 unique respondents with 94,822 individual observations through 9 waves of follow-up (for women in the oldest cohorts). The modal source of missingness in MTF is attrition, and all analyses were therefore weighted using attrition weights developed by MTF, which account for baseline substance use and demographic characteristics related to both subsequent alcohol patterns and study retention, described in Chapter 3. Item non-response is the second source of missing in this sample. Of the 94,822 observations for sample women, 71,481 (75%) had complete covariate or outcome information. The primary source of non-response was for paternal education (6%) and rurality at baseline (7%) with the remaining covariates each having less than 3% missing. To account for potential biases by selective item non-response, multiple imputation using chained equations was used to impute missing values into 10 data sets, which were combined using Rubin's Rules to estimate model parameters.[207] Due to computational barriers to performing post-estimation procedures (i.e., pooling model-based predicted probabilities or calculating an F–statistic for interaction test)[253] from pooled models, analyses with complete cases are shown in the main text, and imputed model parameters are shown in the supplement.

Analyses

I first examined whether the associations between employment status and alcohol outcomes were moderated by structural sexism. For these relationships, the full eligible complete case sample was analyzed (N=71,481 observations with 14,985 unique respondents).

I next examined whether the associations among occupational characteristics (professional status, prestige, gender composition) and alcohol outcomes were moderated by structural sexism. For these relationships, only respondents who were in the labor force—defined as observations in which respondents currently worked or who were currently unemployed but had worked, and excluding those who had never worked or who were currently unemployed homemakers—were included in the analyses (N=56,388 observations). In these analyses, I did not adjust for individual educational attainment, because within occupations (particularly among professional occupations) education is frequently invariant and/or hiring is contingent on educational credentials.

All analyses were modeled using multilevel regression with random effects for both individuals and states, with individual respondents nested within states. Each association was examined in two unique regression models, the first with alcohol consumption frequency as the outcome, modeled using Poisson regression, and the second with binge drinking probability, modeled using logistic regression. Models included an interaction term between structural sexism and the occupational characteristic under examination to produce estimates for relative statistics (i.e., risk ratio and odds ratio) for each stratum and to test heterogeneity across strata. Figures were produced based on model-based predictions, fixed at the mean covariate values for the sample. Models are presented with adjustment for all described covariates and weighted using both original sampling probabilities and attrition weights, described above. Analyses and figures were produced in SAS 9.4.

4.3 Results

Table 4.2 shows the covariate distribution across observations in the sample, stratified across levels of state structural sexism, dichotomized using a median split and with p-values derived from Rao-Scott chi-square tests which account for clustering of repeated measures.[213] Overall, a high proportion of women in the sample were employed (80%). Respondents in states with lower levels of structural sexism had higher probabilities of alcohol consumption (69% vs. 65%), post-college education (26% vs. 11%), being in a professional occupation (47% vs. 29%), and being in a high-prestige occupation (55% vs. 33%).

Table 4.2 Outcome and covariate distributions among sample women in MTF follow-up surveys, 1989-2016, dichotomized by structural sexism level

	Low sexism* (N=35,732 observations) N (%) (categorical) Mean (S.D.) (continuous)	High sexism* (N=35,749 observations) N (%) (categorical) Mean (S.D.) (continuous)	p-value
Alcohol outcomes			
Reported any alcohol consumption (dichotomous)	24,609 (69%)	23,388 (65%)	<0.001
Reported any binge drinking (dichotomous)	9,761 (27%)	10,363 (29%)	<0.001
Occupational characteristics			
Currently employed	29,173 (82%)	27,823 (78%)	<0.001
• One full-time job, or multiple jobs	22,915 (64.1%)	18,525 (51.8%)	<0.001
• One part-time job	6,258 (17.5%)	9,298 (26.0%)	
In the labor force	33,071 (93%)	33,933 (95%)	<0.001
• Managerial/professional occupation	14,216 (47%)	7,960 (29%)	<0.001
• High prestige occupation	15,857 (55%)	9,160 (33%)	<0.001
• Majority-men occupations	3,522 (12.3%)	2,743 (9.9%)	<0.001
State-level covariates			
Percentage of residents who are religious conservatives	18.0 (0.1)	16.9 (0.1)	<0.001
Poverty rate	13.5 (2.9)	12.7 (3.2)	<0.001
Population density	207.4 (230.0)	186.7 (212.0)	<0.001
Alcohol policy climate scale	42.6 (7.3)	38.5 (9.3)	<0.001

GINI coefficient	0.61 (0.04)	0.58 (0.03)	<0.001
Individual-level covariates			
Father has college degree	14,011 (39%)	12,994 (36%)	<0.001
Rural	14,147 (40%)	14,620 (41%)	<0.001
Rural at baseline	19,136 (54%)	18,843 (53%)	<0.001
White	29,461 (82%)	29,229 (82%)	0.074
Married	15,417 (43%)	9,608 (27%)	<0.001
More than 5 years of college education	9,365 (26%)	4,090 (11%)	<0.001
Religious	20,709 (58%)	23,037 (64%)	<0.00
Religious at baseline	21,968 (61%)	22,007 (62%)	0.818
Any alcohol consumption at baseline	17,754 (50%)	18,966 (50%)	<0.001
Any binge drinking at baseline	9,243 (26%)	9,330 (26%)	0.604

*For descriptive statistics, high structural sexism refers to states with at or above median level; low structural sexism refers to states below median level

Supplemental Figure 4.1 shows the proportion of women who are employed, employed full-time, in professional occupations, in high prestige occupations, and in majority-men occupations over time. By 2016, the majority of respondents worked full-time and worked in high-prestige, professional occupations.

Table 4.3 shows the associations between work status and occupational characteristics and alcohol consumption frequency (i.e., occasions) and odds of binge drinking. Those who worked (either at all, or full-time or part-time) evidenced relatively higher occasions of alcohol consumption and odds of binge drinking than those who did not (RR for occasions of alcohol consumption for employed women compared to unemployed women = 1.057, 95% CI 1.046, 1.068; OR for binge drinking = 1.191, 95% CI 1.136, 1.250). Professional status was related to increases in occasions of alcohol consumption (RR=1.050, 95% CI 1.040, 1.061), but was inversely associated with binge drinking odds (OR = 0.936, 95% CI 0.894, 0.980 compared to those not in professional status occupations). Working in a higher prestige occupation was unrelated to binge drinking but associated with more occasions of alcohol consumption

(RR=1.056, 95% CI 1.045, 1.066) relative to those working in a lower-prestige occupation.

Working in a majority-men occupation was unrelated to either occasions of alcohol consumption

or binge drinking odds.

Table 4. 3 Associations between work status and occupational characteristics and alcohol consumption outcomes, MTF women 1989-2016, adjusted for individual and state-level covariates

	N observations	Alcohol consumption* RR (95% CI)	Binge drinking* OR (95% CI)
Employed respondents	56,996	1.057 (1.046, 1.068)	1.191 (1.136, 1.250)
Unemployed respondents	14,485	*Ref*	*Ref*
Respondents working full-time or working more than one job	41,440	1.067 (1.056, 1.079)	1.245 (1.184, 1.309)
Respondents working one part-time job	15,556	1.031 (1.017, 1.044)	1.080 (1.018, 1.145)
Unemployed respondents	14,485	*Ref*	*Ref*
Respondents in professional status occupations**	22,176	1.050 (1.040, 1.061)	0.936 (0.894, 0.980)
Respondents not in professional status occupations**	34,212	*Ref*	*Ref*
Respondents in high prestige occupations**	25,017	1.056 (1.045, 1.066)	0.977 (0.934, 1.022)
Respondents in low prestige occupations**	31,371	*Ref*	*Ref*
Respondents in majority-men occupations**	6,265	1.003 (0.990, 1.017)	0.980 (0.919, 1.044)
Respondents in majority-women occupations**	50,123	*Ref*	*Ref*

*Adjusted for alcohol policy climate, state- and individual-level rurality, poverty rate, GINI coefficient, state- and individual-level religiosity, race, age, paternal education, marriage status, highest education completed, rurality at baseline, religiosity at baseline, alcohol outcome at baseline
**Not adjusted for education

I next examined effect modification across these associations by structural sexism.

Structural sexism was inversely associated with both occasions of alcohol consumption (RR:

0.973, 95% CI 0.970, 0.976) and probability of binge drinking (OR: 0.895, 95% CI: 0.882,

0.909). Figures 1-4 show model-based predicted probabilities from all interaction models; these

correspond to statistics reported in Table 4.4. For ease of visualization with interaction models, structural sexism is shown on the x-axis because it is continuous, and work status and occupational characteristics are shown as different strata because they are discrete; Supplemental Figures 4.2 – 4.5 show the same relationships, with work status and occupational characteristics on the x-axis—as they are the main exposures—with point estimates for various cut-offs of structural sexism.

Figure 4.1 shows model-based estimates for the relationship between employment status (employed in blue, unemployed in red) and occasions of alcohol consumption and probability of binge drinking, across levels of structural sexism, corresponding to Table 4.4. I observed heterogeneity in the relationship between employment and both outcomes across levels of structural sexism. As sexism values decreased, the risks for both alcohol outcomes increased, but the risks among employed women increased faster, creating a widening disparity at lower levels of sexism and a convergence of risk at higher levels of sexism. For alcohol consumption frequency, at the lowest level of structural sexism, employed women reported higher occasions of consumption (2.61, 95% CI 2.57, 2.64) then unemployed women (2.32, 95% CI 2.27, 2.37). Similarly, at low levels of sexism, employed women reported binge drinking at higher probabilities than unemployed women (predicted probabilities = 0.32, 95% CI 0.31, 0.33; and 0.23, 95% CI 0.22, 0.25, respectively). Similar trends were observed when employment status was decomposed into three categories (full time, part time, or no job); structural sexism moderated these relationships in an apparent dose-response manner, with women working full-time endorsing both the highest occasions of alcohol consumption and probabilities of binge drinking, and women not working endorsing the lowest (Table 4.4), but only in low structural sexism contexts.

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Figures 4.2 – 4.4 show model-based estimates for the relationships between structural

sexism and professional status, prestige, and occupational gender composition, respectively. For

alcohol consumption, higher status careers (i.e., professional occupations, high prestige

occupations) were associated with higher occasions of consumption relative to lower status

careers in low-sexism contexts, but not in high-sexism contexts. However, the associations

between managerial status, prestige, and binge drinking probability did not vary across levels of

structural sexism; all tests of interaction were non-significant (Table 4.4) and predicted

probabilities did not meaningfully vary at different levels of sexism. For both alcohol outcomes,

associations with working in a majority-male occupation did not meaningfully vary across levels

of sexism.

Figure 4.1 Associations between employment status and alcohol consumption frequency (left) and binge drinking probability (right), across levels of structural sexism, among MTF women 1989-2016
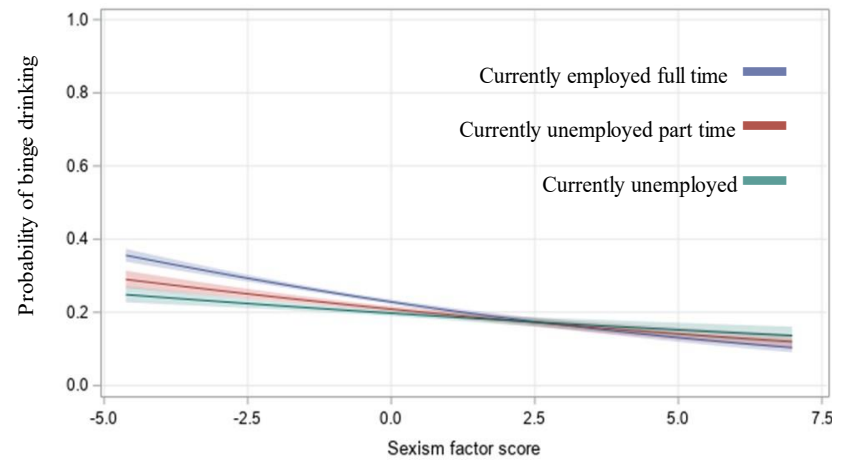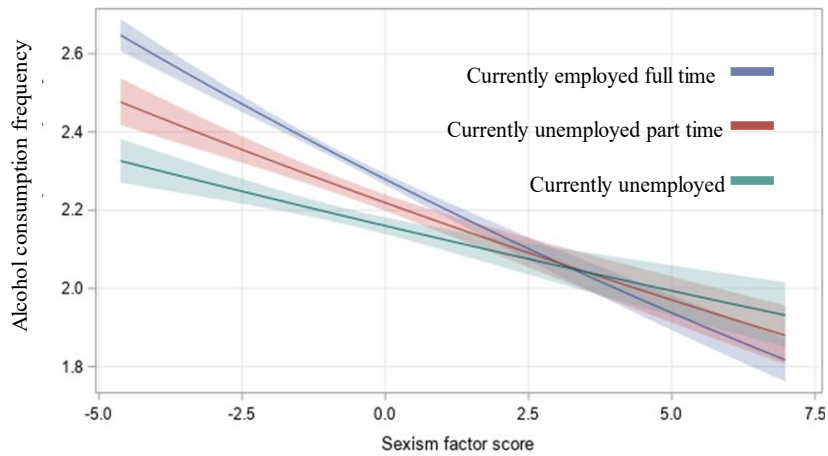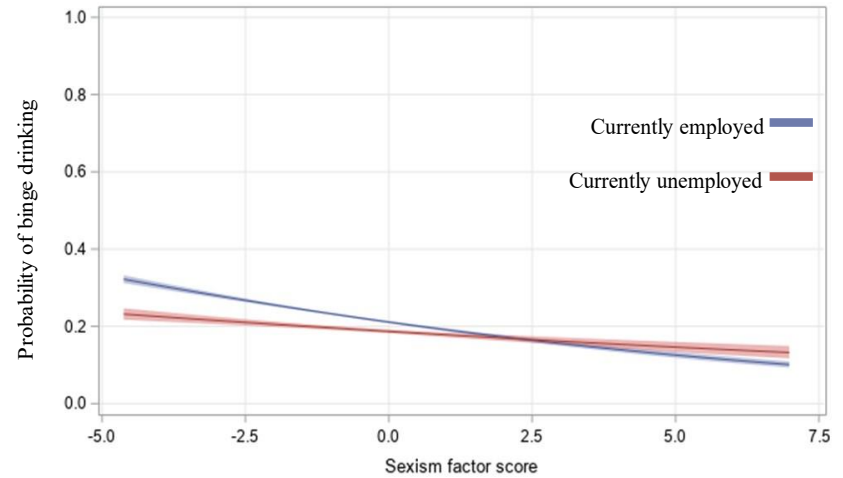
Table 4.4: Associations between work status and occupational characteristics, with effect modification by structural sexism

Stratum	Risk ratio for alcohol consumption frequency with every 1 SD increase in sexism (95% CI)	Model-based predicted alcohol consumption frequency across levels of sexism*			Odds ratio for binge drinking with every 1 SD increase in sexism (95% CI)	Model-based predicted probability of binge drinking across levels of sexism*		
		Min**	Mean	Max		Min**	Mean	Max
Employed respondents	0.970 (0.967, 0.973)	2.61	2.30	1.83	0.883 (0.873, 0.893)	0.32	0.22	0.10
Unemployed respondents	0.985 (0.980, 0.990)	2.32	2.18	1.95	0.943 (0.926, 0.960)	0.23	0.19	0.13
Interaction between structural sexism and dichotomous employment status	*F=37.39, p<0.01*				*F=46.35, p<0.01*			
Full-time	0.968 (0.964, 0.972)	2.65	2.31	1.82	0.874 (0.859, 0.889)	0.32	0.21	0.09
Part-time	0.977 (0.971, 0.982)	2.48	2.24	1.88	0.910 (0.888, 0.932)	0.26	0.19	0.10
Unemployed	0.984 (0.979, 0.990)	2.33	2.18	1.93	0.938 (0.915, 0.962)	0.22	0.18	0.12
Interaction between structural sexism and categorical employment status	*F=16.36, p<0.01*				*F=14.75, p<0.01*			
Respondents in managerial/professional occupations***	0.964 (0.960, 0.969)	2.78	2.91	1.83	0.888 (0.872, 0.904)	0.32	0.22	0.10
Respondents not in managerial/professional occupations***	0.972 (0.969, 0.975)	2.59	2.30	1.86	0.892 (0.881, 0.904)	0.33	0.24	0.12
Interaction between structural sexism and managerial/professional status	*F=10.19, p<0.01*				*F=0.30 p=0.59*			
Respondents in high prestige occupations***	0.962 (0.958, 0.967)	2.81	2.39	1.80	0.886 (0.871, 0.901)	0.32	0.22	0.10
Respondents in low prestige occupations***	0.973 (0.970, 0.977)	2.55	2.29	1.89	0.895 (0.883, 0.907)	0.32	0.23	0.12
Interaction between structural sexism and prestige	*F=24.34, p<0.01*				*F=1.18, p=0.28*			

Respondents in majority-men occupations***	0.962 (0.955, 0.969)	2.75	2.34	1.75	0.879 (0.855, 0.903)	0.33	0.23	0.10
Respondents not in majority-men occupations***	0.970 (0.966, 0.973)	2.66	2.34	1.86	0.894 (0.883, 0.905)	0.32	0.23	0.12
Interaction between structural sexism and occupational gender composition	*F=5.08, p=0.02*				*F=1.48, p=0.22*			

Adjusted for alcohol policy climate, state- and individual-level rurality, poverty rate, GINI coefficient, state- and individual-level religiosity, race, age, paternal education, marriage status, highest education completed, rurality at baseline, religiosity at baseline, alcohol outcome at baseline
**Lowest level of sexism = -4.62; mean = -0.47; highest = 6.98*
***Not adjusted for education*

Figure 4.2: Associations between managerial/professional occupational status and alcohol consumption frequency (left) and binge drinking probability (right), across levels of structural sexism, among MTF women 1989-2016
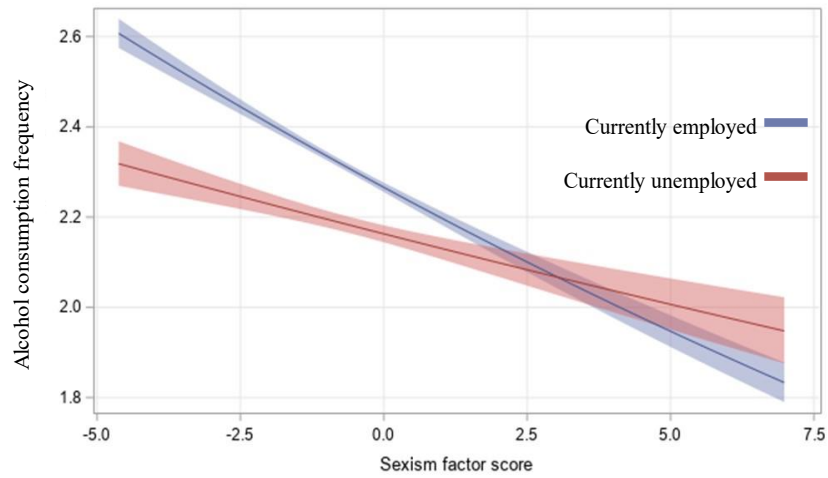
Figure 4.3: Associations between occupational prestige and alcohol consumption frequency (left) and binge drinking probability (right), across levels of structural sexism, among MTF women 1989-2016
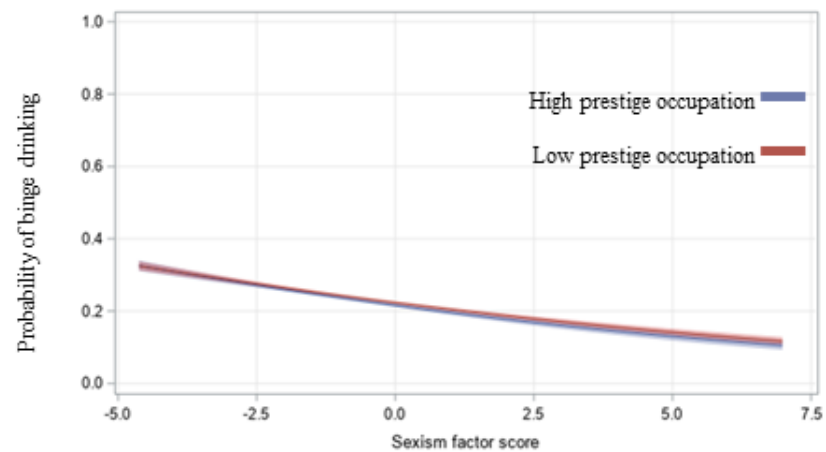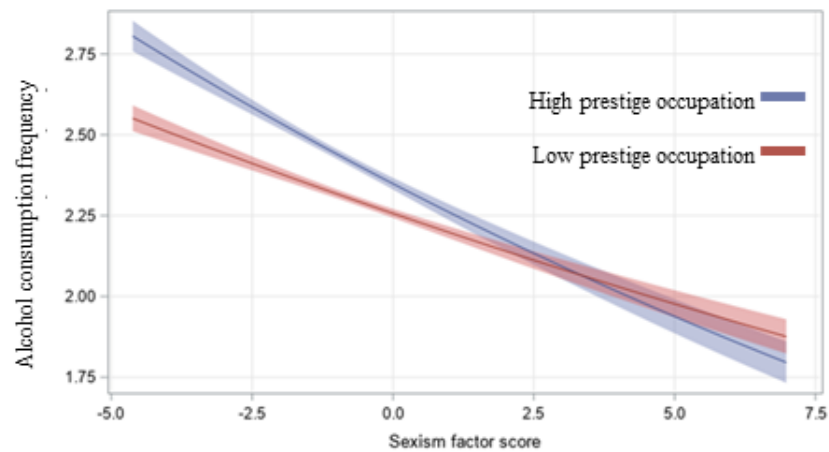
Figure 4.4: Associations between occupational gender composition and alcohol consumption frequency (left) and binge drinking probability (right), across levels of structural sexism, among MTF women 1989-2016
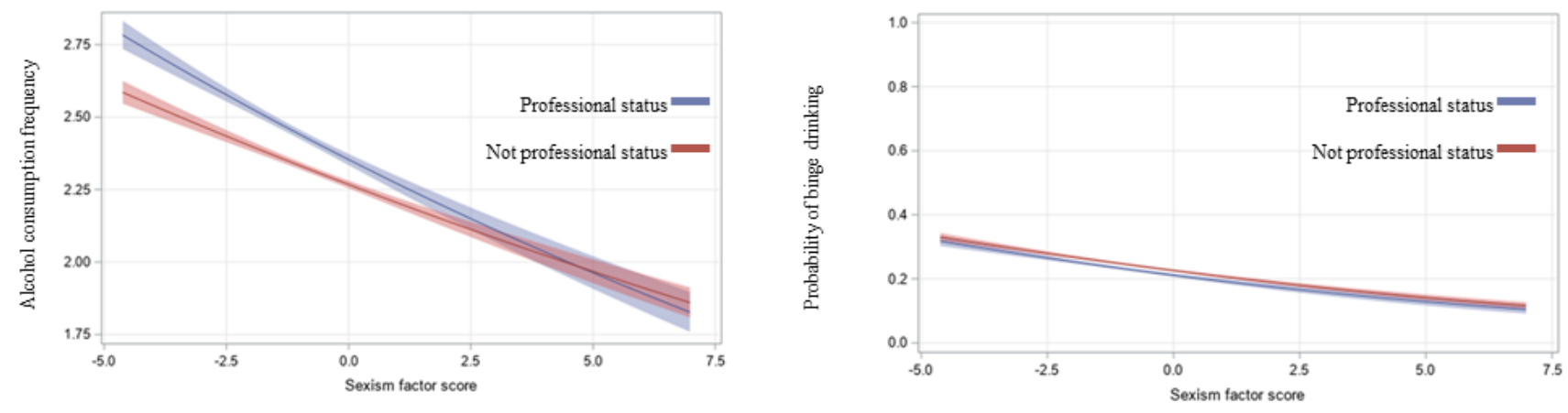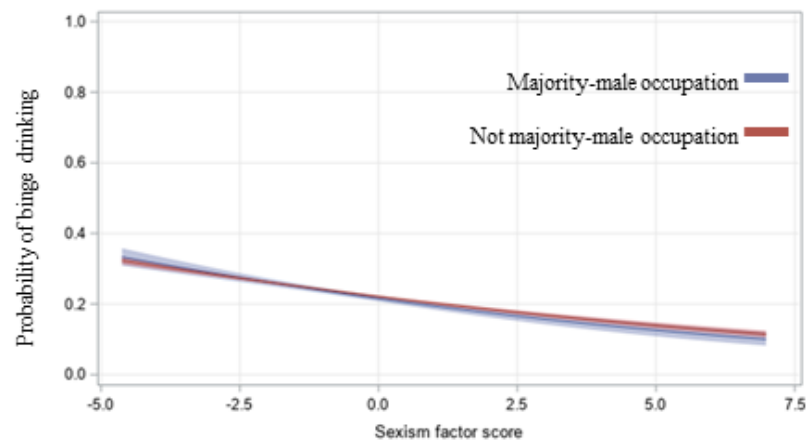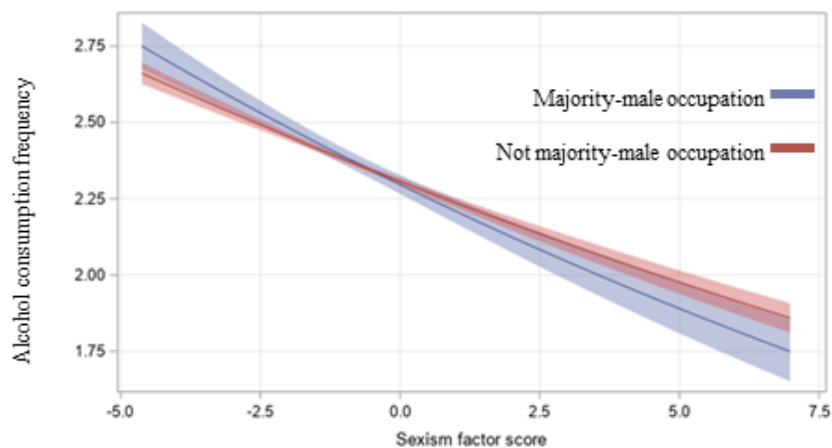
Supplemental Table 4.2 shows estimates for heterogeneity in the relationship between MTF occupation and both alcohol outcomes across levels of structural sexism. The most pronounced decreases in occasions of alcohol consumption as a result of increases in structural sexism were for owners of small businesses (RR=0.937); for binge drinking odds, the most pronounced decreases were for craftsmen or skilled workers (OR=0.699). Supplemental Figure 4.6 shows model-based probabilities of alcohol consumption frequency and binge drinking as a function of interaction between occupation category and sexism. Supplemental Table 4.3 shows pooled parameters from models on imputed data, compared to complete case analysis; imputation of missing data did not meaningfully change results or interpretation.

4.4 Discussion

The present study of women in the MTF between 1989-2016 had four central findings. First, working was associated with increases in both occasions of alcohol consumption and probabilities of binge drinking, and these increases in alcohol use were highest in low sexism environments. Second, at the highest levels of sexism, there were no differential associations between any occupational characteristic and occasions of alcohol consumption nor probability of binge drinking. Third, belonging to a high-status occupation led to increases in occasions of alcohol consumption which were exacerbated in low sexism environments, but belonging to a high-status occupation was not meaningfully related to binge drinking, regardless of structural sexism. Finally, occupational gender composition was unrelated to either outcome, regardless of structural sexism. This is the first study to address heterogeneity in the associations between work status and occupational features and US women's alcohol outcomes by structural sexism. Overall, the totality of the findings suggest that in low sexism environments, working increases

women's alcohol use across multiple drinking patterns and belonging to a high-status career increases occasions of alcohol consumption but not riskier alcohol patterns (i.e., binge drinking).

These findings suggest several possible mechanisms. In general, at low levels of sexism, all subgroups evidenced more occasions of alcohol consumption and higher probabilities of binge drinking. These findings were consistent with the study hypothesis that working women in low sexism environments would evidence higher alcohol risks than both non-working women or women in high sexism environments. Notably, structural sexism did not reduce either alcohol outcome in any occupation group. As demonstrated in Chapter 3, declines in restrictive drinking norms are an important mediator of these increases in alcohol use.[166,167,254] Though Chapter 3 did not investigate differential norms for alcohol consumption per se versus binge drinking (which may var substantially[221,255,256]) norms alone are insufficient to explain the differential slopes across employment status, and the discrepant findings for occasions of alcohol consumption compared to binge drinking. Rather, two mechanisms that are commonly used to explain why employment increases alcohol use are financial resources and occupational drinking cultures.[257–259] Both may play a role in the associations found in the current study.

Relative to non-working women, women who work have more financial resources and may simply be more able to afford to consume alcohol regularly.[260,261] Women who work are also exposed to occupational drinking cultures: in non-pandemic times, many professions and occupations cultivate work-related alcohol consumption (e.g., drinking at or after work, drinking with co-workers) as ways to develop community and camaraderie, as well as to recruit clients.[262,263] Occupational drinking cultures influence worker alcohol consumption behaviors,[264,265] and when employees engage in alcohol consumption patterns that are adherent to the culture at their workplace, they receive meaningful rewards in terms of relationship- and

rapport-building, despite the health risks.[262,263,266–271] Both of these mechanisms may be more salient in contexts with more permissive drinking norms, which would explain not only the differential associations between employment status and both outcomes at low levels of sexism, but also the lack of association between employment and either alcohol outcome at the very highest levels of sexism. When consuming alcohol is more socially appropriate, women who have more resources and more exposure to permissive occupational drinking cultures may engage in alcohol use at higher rates than those who do not; but in less permissive contexts, women's alcohol use may be equally unacceptable regardless of employment or status differences.[162]

At lower levels of sexism, I observed an association between high-status careers and increases in occasions of alcohol consumption, but not binge drinking; financial resources and occupational drinking cultures may also explain this discrepancy across outcomes. Indeed, working in a high-status career is a well-established risk for alcohol consumption (compared to abstention),[230,232,233,272] but it is less consistently related to binge drinking or other patterns of high-intensity consumption, as the current study demonstrated.[232,273,274] Women in high-status careers—i.e., with the most financial resources—frequently have high health literacy;[275,276] they may have internalized public health messaging (which has since been discredited[4,11–15,277,278]) that moderate alcohol consumption is healthy and only excess consumption is risky.[279] Next, workers in higher status careers are exposed to more permissive drinking cultures and more opportunities for work-related drinking than those in low-status careers,[271,280] but only to a certain degree: among higher status careers, moderate alcohol consumption is frequently normative, but excess or risky consumption (e.g., binge drinking, working while intoxicated) is not.[281–283] While it is encouraging that high-status women are not at differentially increased risks

of binge drinking (a more severe consumption pattern), no level of alcohol consumption is beneficial for health.[15]

Finally, in this sample, gender composition was unrelated to either alcohol outcome, regardless of structural sexism. This null association was inconsistent with the study hypotheses. While prior research has demonstrated a positive relationship between working in a male-dominated occupation and alcohol risk, these studies rarely stratified by gender. Studies that have examined the effects of occupational gender composition on women's outcomes—rather than on outcomes with men and women pooled together—show that women working in male-dominated occupations do experience increased risks of adverse mental health and stress,[241,284–290] but there is conflicting evidence for whether alcohol risks increase.[243,291] Therefore, the pathways mediating the relationship between high-status careers and alcohol outcomes may be distinct from the ones mediating the relationships between occupational gender composition and other health outcomes for women. Finally, occupational gender composition may be less influential for women's health behaviors than workplace gender composition; that is, being a majority-male *field* may be less relevant for women's alcohol use than being in a majority-male *firm*.[241,290,292]

The study findings should be evaluated in light of their limitations. First, measures of current employment status mask heterogeneity in temporary vs. permanent unemployment, and MTF does not further probe labor force engagement (e.g., by asking if respondents were looking for a job, or if respondents are retired) aside from asking whether or not the respondent is currently a homemaker (i.e., long-term unemployed). This lack of precision with regard to employment status (i.e., mixing the long-term unemployed with the temporarily unemployed) was not plausibly differential by state and likely any bias would be towards, rather than away

from, a null effect. Similarly, the linkage between MTF job codes and census labor codes was inexact—that is, the MTF job categories do not precisely correspond to established, commonly-used labor codes—which may have resulted in further non-differential misclassification, and a conservative bias towards a null effect. Next, the measurement of prestige relied on a single, time-invariant measure from a national opinion survey. However, prestige is a stable construct with regards to historical time,[293,294] and though individual occupations have changed between 1989 and 2016, the overall occupational categories have not. Finally, these findings do not rule out the possibility of selection; respondents who consume alcohol at higher rates may have selected into the labor force or higher status careers. Further, attrition is the modal source of missingness in this sample, and even with weighting for baseline predictors of attrition, non-differential attrition by alcohol use would further contribute to selection concerns. However, selection alone would not explain the variation across levels of sexism.

While reductions in structural sexism leading to greater gender equality can be viewed as a general social good, such reductions have may have unintended harmful risks for some health behaviors.[39] Similarly, labor force engagement extends positive health benefits to women,[295,296] but it also confers specific risks, which are sensitive to the broader social context.[296] The implications of this research are not that women should disengage from the labor force or avoid pursuing high-status careers because of the risks of alcohol consumption, nor are they that the world should become less equal to protect women's health. Rather, these findings contribute to a growing literature suggesting that alcohol risks are changing in relation to shifting social landscapes, and understanding these changes are essential for identifying higher risk groups for public education, screening and interventions. Indeed, occupational interventions for alcohol use have an extensive research base[297–299] and can be very effective, [297–299] as working adults 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. Measuring and exploring the changing dynamics and determinants of alcohol use, and how these vary across different social dimensions, is paramount to not only alcohol treatment and intervention but also prevention of a multitude of health consequences later in life.

Chapter 5: Conclusion

In this dissertation, I reviewed literature investigating the association between structural sexism and women's health outcomes, examined associations between structural sexism and women's alcohol consumption patterns, and examined to what extent structural sexism moderates associations between occupational characteristics and alcohol use patterns among women.

In my review of the existing literature, I observed a lack of consensus regarding associations between structural sexism and women's health outcomes, due in part to major divergences in theoretical frameworks and measurement modalities across studies. Measurement of structural sexism was highly variable, leading to both internal and external validity concerns. Moving forward, health science researchers will benefit not only from improving measurement, but also from understanding and testing theoretically-informed hypotheses, including the potential for unintended consequences like backlash and convergence. I suggested several design choices that would improve both measurement and inference – the use of negative controls, and empirically-derived exposures—both of which I used in my first empirical study.

In the next chapter, I examined associations between an empirically-derived measure of structural sexism and both alcohol consumption and binge drinking among women in the mid-life. I found that a two-factor model with five indicators of gender inequality (the percentage of male state legislators; the male/female ratio for residents living at or above the federal poverty line; the male/female ratio for the proportion of adults ages 16 and over in the labor force; the male/female ratio for the proportion of working adults in management occupations; and the male/female ratio for the proportion of working adults who are self-employed) and a random effect for historical time showed good fit (RMSEA=0.09, TLI=0.93, CFI=0.96).

Using this measure, I showed that women living in geographies with lower levels of structural sexism had higher risks for both alcohol outcome than women living in geographies with higher levels of sexism. I additionally showed specificity of the relationship to women (using men as a negative control) and provided evidence for two mechanistic pathways that partially mediate the associations: education and restrictive alcohol norms. These findings – that sexism is inversely related to alcohol use among women – highlighted the need to consider troubling consequences like backlash and convergence when examining structural exposures. My findings were consistent with other research suggesting convergence of gendered health disparities as women's social status improves, which is not always beneficial for women's health.[39]

In the next study, I examined how structural sexism may be a moderator for other more proximal, individual characteristics that increase alcohol use—namely, occupational characteristics. I found evidence that structural sexism does modify the relations among working, working in a high-status career, and some alcohol outcomes. At the lowest levels of sexism, women who worked reported higher risks of both alcohol consumption and binge drinking than women do did not work; at the highest levels of sexism, however, work did not differentially influence either alcohol outcome. These findings contribute to a growing body of literature that seek to understand structural sources of heterogeneity for individual risks—in other words, that individual exposures matter for health behaviors, but their salience may be highly dependent on the larger social context.[300,301] These findings also highlight the importance of work and work culture for influencing alcohol use; occupational screening and interventions may play an important role in addressing troubling trends in women's drinking. Finally, these findings emphasize the importance of using a theory-driven approach to think critically about structural

exposures, as well as using an empirically-derived measurement tools—two major gaps identified in the literature review.

An important avenue for future research is explicating mechanisms through which structural sexism operates to influence women's drinking outcomes. For example, individual mediators such as drinking contexts and drinking motivation play a role in both alcohol consumption and binge drinking, vary by gender, and are sensitive to broader social contexts.[304–308] Individual experiences of gender-based discrimination or harassment may also be meaningful mediators, particularly among women who work and work in high status careers: being in higher status positions creates more situations for exposure to discrimination, and those in higher status positions are more aware of subtle forms of discrimination and are more likely to identify these experiences as being discriminatory.[309] Among working women, those in managerial positions, often the highest-prestige positions, have reported experiences of sexism at the highest frequencies.[45] Sexual harassment, a form of interpersonal sexism associated with problem drinking,[47,48,310] is more common among women in supervisory roles than in non-supervisory roles.[46] Therefore, an apparently paradoxical mechanism may be that women living in places characterized by lower levels of structural sexism are more likely to experience increased interpersonal sexism due to higher rates of labor force engagement. Finally, other interpersonal processes, including familial and marital factors, may play an important role in mediating the observed relationships. For example, delaying or forgoing childbearing may contribute meaningfully: women who parent consume alcohol at lower prevalences than women who choose not to parent,[311,312] and women in the mid-life in recent decades have, on average, delayed childbearing relative to previous cohorts.[313] No research has yet examined these processes in relation to structural sexism.

Different alcohol consumption patterns and drinking outcomes have different risk profiles, an established finding that was highlighted in the present research. In general, structural sexism was more impactful on risks of binge drinking, which is a riskier consumption pattern than alcohol consumption per se. Education was an important mediator in the association between structural sexism and alcohol consumption, but not binge drinking. Further, structural sexism moderated the relationship between high-status occupations and alcohol consumption, but not binge drinking. The different relationships across the two alcohol outcomes highlights the need to understand and examine different alcohol use patterns as discrete phenomena that are mediated through different processes. While alcohol consumption per se is frequently sociable or community-building (i.e., to enhance experiences) binge drinking is frequently more maladaptive (i.e., for coping);[302] indeed, other predictors of binge drinking among women are stress, anxiety and depression, and adverse childhood experiences.[303] Better understanding how structural sexism may influence these pathways to differentially impact various alcohol outcomes is a critical next step in this line of research.

Both empirical studies focused on a cohort of women born in the 1970s and 1980s. The rationale for focusing on this cohort was two-fold; first, this is the group among whom alcohol consumption and binge drinking has increased to historically unprecedented rates; next, this age group has witnessed historical variation in structural sexism to a degree that other cohorts like have not. Therefore, if a relationship between structural sexism and alcohol use were to be detectable, it would be so in this cohort. While I do not believe the structural sexism-alcohol associations are exclusively specific to this cohort, it is not clear under what circumstances these relationships are generalizable to older and younger age groups. For example, younger cohorts exhibit different alcohol use trends: among young adults in recent years, alcohol use has been

declining among both men and women.[314,315] To what extent differing structural and individual

processes create heterogeneity in the observed relationships across cohorts remains to be

investigated.

Alcohol use is detrimental to health.[15] Increases among any subgroup are troubling, as

increased use is dangerous for both individual health trajectories and health systems. It remains

to be seen how the shifting landscape of women's alcohol risks will manifest into downstream

health sequelae, and prevention remains a priority. Fortunately, both outpatient and occupational

screening are effective, as is outpatient treatment.[297,298,316] An important consideration, however,

is that while increases in alcohol consumption and binge drinking are associated with decreased

structural sexism, particularly among higher status women, low-income and low-education

women are still more likely to experience adverse health consequences as a result of alcohol use.

The "alcohol harm paradox"[317–319] describes the phenomenon that people at higher status

drinking at higher prevalences, but people at lower status experience higher rates of morbidity

and mortality—for example, in the form of chronic diseases, injuries, and victimization—as a

result of alcohol consumption, likely due to intersecting social forces that perpetrate

socioeconomic and health disadvantage. The current research highlights the need to focus

screening and prevention efforts on all women, including not only higher status women but also

those who are vulnerable to structural disadvantage from other sources.

Structural sexism is one exposure in a constellation of other structural social forces with

which women must increasingly reckon. Among these media and public health messaging

surrounding alcohol use. As women have become stronger market participators, alcohol

advertisers have taken note: the so-called "pinking" of the alcohol market has included

promoting alcohol consumption as self-care for women, and targeting women with gender-

specific products.[320,321] Alcohol advertising is highly visible and well-funded. Globally, the alcoholic beverage industry is the 8th most profitable sector—lower than tobacco, but higher than soft drinks—and its 2018 earnings were nearly $18 billion.[322] Beyond point-of-care screening and treatment, combating advertising will require public health mobilization and effective messaging. Unfortunately, public health has an alcohol messaging problem: consumers have reported frustration after receiving mixed messages[323–325] as to the purported health benefits of moderate drinking and certain beverage types (e.g., red wine), and consumers also evidenced backlash in response to anti-drinking campaigns. For example, a 2016 CDC campaign designed to combat drinking during pregnancy encouraged young women to treat themselves as "pre-pregnant" and to avoid alcohol consumption if they were not currently taking birth control; this campaign was largely panned as sexist and out of touch.[326–328] Increasing evidence around effective public health messaging has delineated the ineffectiveness of stigma-based campaigns,[329–332] which can deter treatment-seeking. Harm-reduction campaigns, however, can be very effective;[333,334] people consume alcohol for social reasons, and campaigns and social trends towards promoting alcohol alternatives (e.g., mocktails) at social events and encouraging adults to be "sober-curious" have been well-received and may be efficacious.[335–337]

While historically men have consumed alcohol at higher rates than women, these risks are converging,[16] and decreases in structural sexism may an important contributor. While increasing gender equality is extraordinarily beneficial for women's economic, political, and social opportunities, as well as for local economies and key health metrics (like unplanned pregnancies and maternal mortality),[110] convergence of health risks may be a troubling, unintended consequence. A superficial interpretation of these dissertation findings is that gender equality harms women – but this is not the implication of the current research. Rather, 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 macro-level social forces shift, population risk profiles shift commensurately. For

example, as women have become more equal, they may have become more vulnerable to the

same adverse social forces to which men are exposed. Over the past decade, social scientists

have vocalized concern 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.[338–341] In particular, the findings described in Chapter 4 are suggestive that once

structural gender inequality attenuates, other well-established sources of inequality (i.e., class)

may become more salient;. such inequalities may have more relevance for women's health and

health behaviors than they have in previous decades. Investigating and understanding these

relationships is essential for surveillance and treatment as the social landscape creeps closer

towards gender equality.

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Supplementary Material

Supplemental Table 2.1: Detailed measurement approaches and findings from empirical studies

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
"Status of women index" (Originally developed by Yllo) **Education:** Ratio of women to men who graduated high school, percentage of women enrolled in post-secondary schools, percentage of female high school interscholastic athletes, percentage of female high school administrators **Financial**: Ratio of women's to men's median income **Labor**: Percentage of women in the labor force, percentage of women in managerial occupations, ratio of men's to women's unemployment rate **Legal:** No occupations barred to women, equal pay laws, fair employment practices act, no maximum hour restrictions for women, proof of resistance not required for rape conviction, corroboration of testimony not required for rape conviction, husband and wife jointly responsible for family support, husband and wife have equal rights to sue for personal injury, wife's property rights unrestricted, wife's right to use maiden name unrestricted, wife's right to maintain separate domicile unrestricted, ratified Equal Rights Amendment, passed a state Equal Rights Amendment **Political:** Percentage of female members in US Congress, percentage of female members of state senate, percentage of female members of state house, percentage of female judges on major appellate and trial courts	Indicators standardized and summed across 4 domains: economic, educational, political, legal. Overall index combines all 4 dimensions	Alpha coefficient for reliability of overall index is 0.54	Not specified	Yllo 1983	Curvilinear relationship between status of women index and violence against women
		None shown		Yllo & Straus 1984	Curvilinear relationship between status of women index and violence against women
		Alpha coefficient for reliability of overall index is 0.54		Yllo 1984	Primarily descriptive findings: prevalence of violence against women in high status states range from 1-16%, prevalence of violence against women in low status states range from 2-10%; heterogeneity by marriage dynamics
		Alpha coefficient for reliability of overall index is 0.54		Baron & Straus 1984	No relationship between status of women index and rape rates

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
"Gender equality index" (Originally composed by Sugarman and Straus[122] and updated by Di Noia[123]) **Financial**: Ratio of women's to men's median income, ratio of women to men receiving small business loans, ratio of proportion of women to men living above the poverty line **Labor**: Ratio of women's to men's labor force participation, ratio of women's to men's representation in managerial and administrative occupations, ratio of percentage of women to men who are unemployed **Legal:** State passed fair employment practices act, women may file lawsuit under fair employment practices act, state passed equal pay laws, women may file lawsuit under equal pay laws, sex discrimination law in public accommodations, sex discrimination law in housing, sex discrimination law in financing, sex discrimination law in education, state provides civil injunction relief for victims of abuse, statutes define family violence as criminal offense, statutes permit warrantless arrests for family violence, statutes requiring data collection for family violence, statutes providing funds for family violence shelters, passage of the Equal Rights Amendment **Political**: Ratio of women's to men's representation in State Senate, ratio of women's to men's representation as mayors, ratio of women's to men's representations in Governing Boards	Summed to make an index for the following domains: *Economic equality, Political equality, Legal equality*. Three domains then standardized and summed to create "overall" index	Correlations run within each sexism domain, Cronbach's alpha shown and if alpha was improved by item deletion then item was removed. Cronbach's alphas ranged from 0.62 to 0.75 across domains. Alpha for overall index = 0.62, correlations across domains ranged from 0.25 to 0.42	1977-1983, 1990	Baron & Straus 1987	*Higher gender equality* (overall index) decreases rate rape by 0.23 units per 100,000 population
				Lester 1992	*Higher gender equality* negatively correlated with homicide rate (-0.40), but no change in suicide rates.
				Straus 1994	Higher gender equality associated with 0.146 unit decrease in rates of "wife assault" (denominator not provided)
	Tested selected items independently	None		Stout 1992	Higher proportions of women in management occupations associated with positively correlated (0.32) with female homicide; higher rates of women's unemployment positively correlated (0.33) with female homicide; higher percentages of female state representatives negatively correlated (-0.29) with female homicide; fair employment practices act associated with 4.5 unit decrease in count of female homicides; equal pay laws associated with 6.1 unit decrease in count of female

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
					homicides; presence of sex discrimination law in the area of public accommodations associated with 3.2 unit decrease in count of female homicides; presence of sex discrimination law in the area of housing associated with 2.5 unit decrease in count of female homicides; states providing for civil injunction relief for victims of abuse associated with 2.3 unit decrease in count of female homicides; the passage of the Equal Rights Amendment associated with 4.8 unit decrease in count of female homicides
	Only socioeconomic and legal equality examined; economic measures examined independently	None		Whaley 2001	Labor force inequality associated with between 1.94 and 138.96 unit decrease in rape rate per 100,000 population depending on the year, legal inequality associated with 0.31 unit decrease in rape rate in 1970; lagged models showed that historical inequality increased 20-year later rate rape by between 16.73 and 25.66 units

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
	Factor analyzed two domains: Gender socioeconomic inequality and gender legislative inequality; the rest measured as indices	Cronbach's alpha ranged from 0.750 to 0.812		Titterington 2006	Higher socioeconomic inequality associated with 0.690 unit increase in female homicide rates per 100,000, higher gender legislative inequality associated with 0.242 unit decrease in female homicide rates
"Status of women in the States" **Education:** Percent of women with a bachelor's degree or higher **Financial:** Women's median annual earnings, ratio of women's to men's earnings, percent of women with health insurance, percent of all firms owned by women, percent of women above the poverty line **Labor:** Percent of women in the labor force, percent of women in managerial and professional occupations **Political**: Percent of women who registered to vote, percent of women who voted, proportion of state senators who are women, proportion of state representatives who are women, percent of elected executive officials and U.S. representatives who are women, proportion of US senators who are women, whether the governor is a woman, presence of a commission for women, presence of a campaign training program for women, presence of a women's political action committee, presence of a state chapters of the National Women's Political Caucus **Reproductive:** Presence of mandatory consent law, presence of mandatory waiting period law, restrictions on public funding of abortion, percent of women living in a county without an abortion provider, presence of pro-choice governor or legislature, acceptance of Medicaid expansion (post-2014), coverage of infertility treatments (post-2014), state recognition of same-sex marriage or second-parent adoption, presence of mandatory sex education in schools Standardized, weighted, and summed to create multi-domain measure of *political participation*, *employment and earnings*, *economic autonomy*, and *reproductive rights*		Correlations across domains ranged from 0.24 to 0.89	1996	Kawachi et al 1999	*Higher political participation* associated with 5.09 unit reduction in female mortality per 100,000, 2.12 unit reduction in female mortality by ischemic heart disease, 0.37 unit reduction in female mortality by CVD, 0.10 unit reduction in female mortality by breast cancer, 0.06 unit reduction in female mortality by cervical cancer, 0.07 unit reduction in female mortality by homicide, 0.03 unit reduction in days of limited activity. *Higher employment and earnings* associated with 36.37 unit reduction in female mortality, 27.13 unit reduction in female mortality by ischemic heart disease, 10.16 unit reduction in female mortality by CVD, 1.32 unit reduction for female mortality by breast cancer, 0.56 unit reduction in female mortality by cervical cancer, 0.51 unit increase in female mortality by

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
					homicide, 0.49 unit decrease in days of limited activity. *Higher economic autonomy* associated with 54.55 unit decrease in female mortality, 17.69 unit decrease in female mortality by ischemic heart disease, 11.63 unit decrease in female mortality by CVD, 0.51 unit decrease in female mortality by cervical cancer, 1.37 unit decrease in mortality by homicide, 0.56 unit decrease in days of limited activity. *Higher reproductive rights* associated with 6.53 unit reduction in female mortality, 1.00 unit reduction in mortality by CVD, 0.18 unit reduction in mortality by breast cancer, 0.07 unit reduction in mortality by cervical cancer, 0.17 unit reduction in mortality by homicide, 0.03 unit reduction days of limited activity
		Correlations across domains ranged from 0.27 to 0.89; political participation has lowest correlations	1996	Chen et al 2005	No association with *political participation*. Higher *employment and earnings* associated with 0.85 unit decrease in depression score. Higher *economic autonomy* associated with 0.88 unit decrease in depression score. *Higher reproductive rights* associated with 0.62 unit decrease in depression score.
		None	2000	Jun et al 2004	*Lower political participation* associated with 14% increased

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
					odds of poor health. *Lower employment and earnings* associated with 29% increased risk of poor health. *Lower economic autonomy* associated with 30% increased risk of poor health.
		None	2002	McLaughlin et al 2011	*Higher political participation* associated with 0.05 increased odds of dysthymia, no other associations. No associations for *employment and earnings, social and economic autonomy*. *Higher reproductive rights* associated with 0.06 decreased odds of any mood disorder, 0.05 decreased odds of depression, 0.07 decreased odds of PTSD
		None	2009-2011	Mukherjee at el 2017	*Higher social and economic equality* associated with 25% decreased odds of post-partum depression
United Nations Gender Inequality Index (Adapted from Griffith and Rose[125]) **Education:** Percentage of men and women with a high school diploma or greater **Financial:** Ratio of women's to men's median incomes **Health:** Chlamydia rate per 100,000 people, annual number of live births to teenage girls, maternal mortality rate **Labor:** Ratio of women to men in labor force, ratio of female-owned businesses to male-owned businesses	Index represents average value of mean-normalized indicators	Correlations examined across individuals indicators and overall index, coefficients ranged from 0.01 to 0.90	2010-2013	Gressard et al 2015	Gender inequality associated with 20% increase in adolescent dating violence among women
	Index development not described in paper	None	2000-2017	Campbell et al 2019	Higher inequity associated with 8% increase in female intimate partner homicide victimization, 7% increase in overall female homicide victimization

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
Political: Percentage of state legislators that are female	Index development not described in this paper	None	2005-2009	Willie & Kershaw 2019	Higher inequality associated with 1.61 percentage point increase in psychological intimate partner violence victimization
	Index represents average value of mean-normalized indicators	None	2010-2012	Kearns et al 2020	Higher inequality correlated with rates of rape (r=0.32)
Education: Difference in male and female mean educational completion **Financial:** Difference between men's and women's median earnings **Labor**: Difference in percentage of employed men and women, difference in percentage of men and women in managerial occupations, percentage of men compared to women employed as judges and lawyers, percentage of men compared to women employed as police	None	None	1972-1976	Ellis & Beattie 1983	Higher sex disparities associated with 38 unit reduction in rate rapes (denominator not provided)
Education: Ratio of men to women who competed high school, ratio of men to women who completed college, ratio of men to women who completed higher education **Financial:** Ratio of men's to women's median income, ratio of men's to women's median incomes among those with high school degree, ratio of men's to women's median incomes among those who completed college, ratio of men to women living below the poverty line **Labor:** Percentage of those in management occupations who are women	None	None	1980	Peterson & Baily 1992	Higher inequality in median income associated with between 0.114 unit increase in rape rates per 100,000 women, higher representation of women in management occupations associated with a 0.126 unit increase in rape rates

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median income **Labor:** Ratio of men to women who are unemployed, percentage of women in managerial occupations	None	None	1980	Bailey & Peterson 1995	Higher inequality in education associated with a 0.235 unit increase in rates of "wife murder" per 100,000 women, higher inequality in unemployment associated with 0.397 unit increase in "wife murder"; higher inequality in median income associated with 0.221 unit increase in murder by acquaintance, higher inequality in managerial occupations associated with 0.083 unit increase in murder by acquaintance
Education: Difference in percentages of men and women who have completed high school, difference in percentage of men and women who have completed 4 years of college **Financial:** Ratio of men's to women's median income, difference in percentage of women and men living below the federal poverty line **Labor:** Difference in percentage of men and women in the labor force, difference in percentage of men and women in managerial occupations	None	Correlation coefficients ranged from -0.49 to 0.36	1983	Brewer & Smith 1995	No relationship between gender inequality and homicide
Education: Percentage of those with college degree or higher who are female, difference in percentages of men and women who have completed high school **Financial:** Ratio of men's to women's median income, percentage of women living below the federal poverty line **Labor:** Percentage of labor force this is female, percentage of professional employees that is female	None	Correlation coefficients between -0.03 and 0.63	1983	Smith & Brewer 1995	More women in professional occupations associated with 0.526 unit decrease in the gender gap in homicide victimization per 100,000 women in low-education sample; more women below the federal poverty line associated with 0.530 unit decrease in the gender gap in

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
Marital: Percentage divorced and separated					homicide victimization in high-education sample
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median income **Labor:** Percentage of men employed	None	None	1980, 1990	Bailey 1999	Higher male/female income gap associated with 4.3 unit reduction in rape rates per 100,000 women; higher male/female education gap associated with 3.3 unit reduction in rape rates. Change in income gap over time (narrowing) associated with 7.4 unit increase in rape rates.
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median income **Labor:** Ratio of men to women who are unemployed, percentage of women in managerial occupations	Principal component analysis performed to reduce items to 1 index	Factor loadings ranged from 0.33 to 0.86, eigenvalue = 2.16	1990	Whaley et al 2002	In the South, gender equality associated with 1.21 unit increase per city population in female homicide victimization perpetrated by men; no impact on female homicide in the non-South
Education: Ratio of women to men with college degree or higher **Financial:** Ratio of women's to men's median income **Labor**: Ratio of women to men in full-time employment, ratio of women to men in managerial occupations, percentage of labor force who is male	None	None	1990	Vieraitis & Williams 2002	Higher equality in full-time employment associated with 2.862 unit increase in female homicide per 100,000 women; higher equality in managerial occupations associated with a 0.849 unit increase in female homicide; higher income equality associated with a 2.606 unit increase in female homicide

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
Education: Ratio of men to women with college degree or higher **Financial**: Ratio of men's to women's median income, ratio of men to women proportion below poverty **Labor**: Ratio of female to male unemployment, ratio of men to women in managerial and professional occupations, ratio of men to women in service occupations, ratio of men to women in manufacturing sector **Marital**: Ratio of single women to single men **Political:** Presence of female mayor	None; each analyzed independently	None	1990	DeWees 2003	Higher male/female college completion associated with 0.880 decrease in homicide victimization per 100,000 women; higher male/female managerial & professional occupation associated with 0.194 unit decrease in homicide victimization; higher male/female median income associated with 0.396 unit decrease in homicide victimization
Education: Ratio of women to men with college degree or higher **Financial**: Ratio of women's to men's median income **Labor**: Ratio of women to men employed full time, ratio of women to men in managerial occupations	None	None	1990	Escholz & Vieraitis 2004	Higher ratio of women to men in managerial occupations associated with 0.981 unit increase in rape rate per 100,000 women
Financial: Ratio of women's to men's median income **Attitudinal**: Population density, National Rife Association Membership, percent religious conservatives	None	None	2000	Pridemore & Freilich 2005	Higher income equality associated with 4.80 unit increase in homicide victimization among non-Hispanic White women (denominator not provided)
Education: Ratio of women to men with college degree or higher **Financial**: Ratio of women's to men's median income **Labor**: Ratio of women to men in civilian labor force, ratio of women to men in managerial occupations	Factor analyzed to create unidimensional measure	One component explained 58% of variance	2000	Martin et al 2006	Increased gender equality associated with 0.117 unit increase in rape rates per 100,000 women

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Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median incomes **Labor:** Ratio of men's to women's employment **Attitudinal:** Percent religious conservatives, proportion voting for conservative president	Principal components analysis used to reduce 3 objective items to 1 index after removing low-loading item; confirmatory factor analysis used to create "patriarchy index" composed of 2 attitudinal measures	Only patriarchy index described; factor loadings = 0.78, single eigenvalue of 1.22	2000	Vieraitis et al 2007	No relationship between gender inequality and female homicide victimization
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median incomes **Labor:** Ratio of men's to women's employment, ratio of men to women in managerial occupations	Factor analysis analysis used to reduce 4 items to 1 factor	Variable loadings all 0.69 or higher, eigenvalue = 2.28	2000	Vieraitis et al 2008	No relationship between gender inequality and female homicide victimization
Financial: Ratio of women's to men's wages	None	None	1990-2003	Aizer 2010	Reduced wage gap associated with 0.81 unit reduction in log number of assaults against women per 100,000
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median incomes **Labor:** Ratio of men's to women's employment	None	None	2000	Reckdenwald & Parker 2010	No relationship between gender inequality and female intimate partner homicide
Education: Ratio of women to men with a bachelor degree or higher, ratio of women to men graduating high school **Financial:** Ratio of women to men living above the poverty line, percent of business female owned, **Labor:** Ratio of women's to men's labor force participation, ratio of women's to men's percentage in managerial and	Standardized and then factor analyzed by domain to make multi-domain index: Gender equality in socioeconomic status (2 factor domain), Reproductive rights, Violence against women	Factor analyses run by domain to determine factor structure with low-loading items removed. Cronbach's alpha shown and if alpha was improved by item deletion then item was removed. Cronbach's alphas	2004	Roberts 2012	Higher socioeconomic equality associated with 0.31 unit decrease in women's past-month alcohol consumption frequency count; higher reproductive rights associated with 0.02 unit decrease in women's past-month binge drinking frequency count; stronger policies for violence

| Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
professional occupations, percent of business female owned, ratio of women **Legal**: State requires domestic violence training for health care providers, laws requiring sexual assault training for police and prosecutors, laws prohibit discrimination against domestic violence victims **Political:** percent of women who voted, presence of a commission for women, presence of a women's legislative caucus **Reproductive:** Presence of mandatory consent law, presence of mandatory waiting period law, restrictions on public funding of abortion, percent of women living in a county without an abortion provider, presence of laws requiring contraceptive coverage, presence of pro-choice governor or legislature, coverage of infertility treatments, state recognition of same-sex marriage or second-parent adoption, presence of mandatory sex education in schools		across domains ranged from 0.48 to 0.81			against women associated with 0.01 unit decrease in women's past-month binge drinking frequency count and 0.04 unit decrease in odds of risky drinking
Education: Percentage of women with college degree minus percentage of men with college degree **Financial:** Female median income minus male median income **Labor:** Rate of female labor force participation minus rate of male labor force participation **Political**: Rate of female voter turnout minus rate of male voter turnout	Income and education standardized and averaged	None	1980, 1985, 1990, 1995, 2000	Xie et al 2012	Higher labor equality associated with 0.12 unit increase in intimate partner violence per 1,000 women

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median incomes **Labor:** Ratio of men to women in labor force	Factor analyses run to reduce items to 1 factor	Loadings ranged from 0.68 to 0.84	1990, 2000	Whaley et al 2013	Curvilinear relationship between status of women index and homicide victimization
Education: Ratio of women to men with college education or higher **Financial:** Ratio of women's to men's median income **Labor:** Ratio of women's to men's employment	Principal components analysis used to reduce 3 items to 1 index by year	Factor loading averages ranged from 0.757-0.876, eigenvalues > 1.60 at each time period	1990, 2000	Vieraitis et al 2015	Gender equality associated with 0.21 unit decrease in intimate homicide for 1 study year only (1990) (denominator not provided)
Education: Ratio of women to men with college education or higher **Financial:** Ratio of women's to men's median income **Labor:** Ratio of women's to men's employment, ratio of women to men in managerial occupations	Principal components analysis used to reduce 3 of 4 items to single index; remaining variable (ratio of female/male median income) did not load and was analyzed independently	Eigenvalue 1.776 for index, explains 59.19% of variance	2000	Gillespie & Reckdenwald 2017	No relationship with intimate partner homicide in marginal models but moderation observed in rural vs. urban areas – IRR in rural areas for higher gender equality = 0.78; IRR for urban areas = 1.21
Education: Ratio of men to women with college degree or higher **Financial:** Ratio of men's to women's median incomes **Labor:** Ratio of men to women in labor force	None	None	2014	Goodson & Bouffard 2019	No associations between gender equality and any violence victimization outcomes

Measure and Indicators	Data reduction technique	Validity/reliability checks	Years	Studies	Findings
Attitudinal: Percent of state population composed of religious conservatives **Financial:** Ratio of men's to women's wages, ratio of men's to women's poverty rate **Labor:** Ratio of men's to women's labor force participation **Political:** Proportion of state legislature seats occupied by men **Reproductive:** Percent of women living in a county without an abortion provider	Standardized and summed to make an index	Cronbach's alpha for index = 0.64	1988-2012	Homan 2019	Higher sexism associated with 0.023 unit increase in count of chronic conditions, 0.169 unit increase on poor physical functioning scale
Financial: Ratio of women's to men's wages	None	None	2006-2011	Henke & Hsu 2020	Higher sexism associated with 0.1 unit decrease in count of intimate partner violence

**Supplemental Table 3.1: Eligible sample N by cohort (year at 12th grade) and study wave**

		Study wave								
Senior year	Baseline N eligible respondents	Follow-up 1 (age 19–20) N, % retained from previous	Follow-up 2 (age 21–22) N, % retained from previous	Follow-up 3 (age 23–24) N, % retained from previous	Follow-up 4 (age 25–26) N, % retained from previous	Follow-up 5 (age 27–28) N, % retained from previous	Follow-up 6 (age 29–30) N, % retained from previous	Follow-up 7 (age 35) N, % retained from previous	Follow-up 8 (age 40) N, % retained from previous	Follow-up 9 (age 45) N, % retained from previous
1988	1270	1010, 80%	963, 95%	896, 93%	812, 91%	782, 96%	736, 94%	693, 94%	637, 92%	576, 90%
1989	1235	964, 78%	898, 93%	821, 91%	749, 91%	691, 92%	681, 99%	613, 90%	553, 90%	532, 96%
1990	1179	905, 77%	816, 90%	732, 90%	691, 94%	667, 97%	610, 91%	547, 90%	521, 95%	
1991	1216	942, 77%	861, 91%	805, 93%	734, 91%	687, 94%	658, 96%	578, 88%	523, 90%	
1992	1242	971, 78%	881, 91%	816, 93%	769, 94%	728, 95%	714, 98%	629, 88%	588, 93%	
1993	1268	944, 74%	874, 93%	813, 93%	736, 91%	704, 96%	704, 100%	604, 86%	512, 85%	
1994	1281	945, 74%	877, 93%	793, 90%	746, 94%	717, 96%	676, 94%	582, 86%	525, 90%	
1995	1275	956, 75%	856, 90%	805, 94%	757, 94%	750, 99%	693, 92%	623, 90%		
1996	1245	902, 72%	817, 91%	756, 93%	733, 97%	704, 96%	672, 95%	582, 87%		
1997	1267	901, 71%	778, 86%	727, 93%	748, 103%	667, 89%	646, 97%	536, 83%		
1998	1187	846, 71%	738, 87%	701, 95%	674, 96%	621, 92%	600, 97%	513, 86%		
1999	1231	794, 65%	730, 92%	748, 102%	646, 86%	631, 98%	601, 95%	503, 84%		
2000	1281	841, 66%	800, 95%	742, 93%	673, 91%	654, 97%	602, 92%			
2001	1268	766, 60%	775, 101%	690, 89%	660, 96%	626, 95%	585, 93%			
2002	1302	864, 66%	804, 93%	736, 92%	706, 96%	692, 98%	639, 92%			
2003	1286	827, 64%	736, 89%	678, 92%	643, 95%	586, 91%	553, 94%			
2004	1262	791, 63%	703, 89%	666, 95%	622, 93%	597, 96%	527, 88%			
2005	1275	720, 56%	712, 99%	663, 93%	584, 88%	553, 95%				
2006	1292	682, 53%	662, 97%	650, 98%	600, 92%	525, 88%				
Total	23862	16571	15281	14238	13283	12582	10897	7003	3859	1108

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Supplemental Figure 3.1: Histogram of alcohol consumption frequency among sample women, 1988-2016
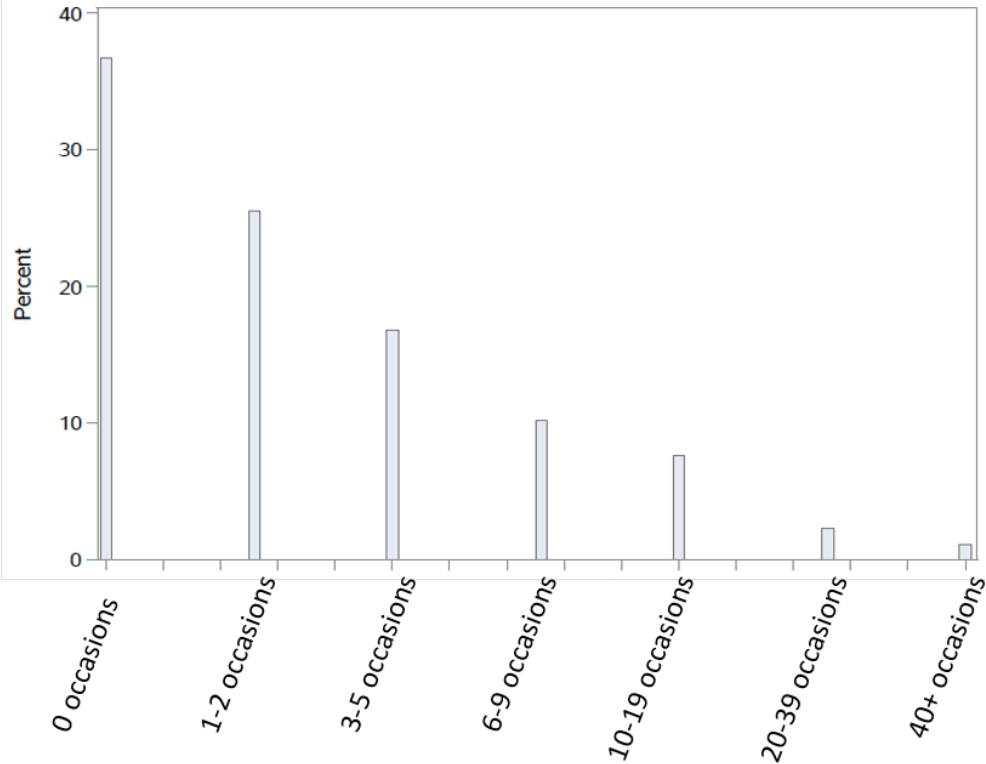
Prompt: On how many occasions have you had any alcoholic beverage to drink – more than just a few sips – during the last 30 days?

Supplemental Figure 3.2: Histogram of binge drinking frequency among sample women, 1988-2016
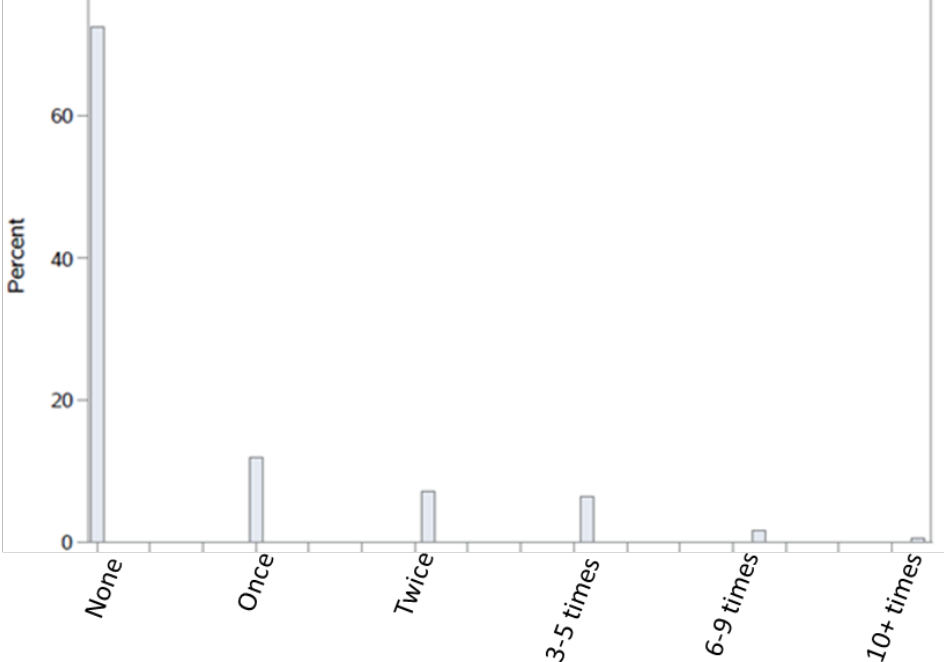
Prompt: Think back over the last two weeks. How many times have you had five or more drinks in a row?

Supplemental Table 3.2: Measures, years of availability, and data sources for structural sexism items

Measure	Data source	Years available
Percentage of state legislators who male	Rutgers University Center for American Women and Politics, disseminated by University of Michigan Institute for Public Policy and Social Research	1988-2016
Percent of adults living above the poverty line, male:female ratio	American Community Survey (ACS) and Current Population Survey (CPS), disseminated by IPUMS USA and IPUMS CPS	1990, 2000-2016 (ACS) 1988, 1989, 1991-1999 (CPS)
Labor force participation among adults ages 16 and over, male:female ratio	American Community Survey (ACS) and Current Population Survey (CPS), disseminated by IPUMS USA and IPUMS CPS	1990, 2000-2016 (ACS) 1988, 1989, 1991-1999 (CPS)
Median income among adults, male:female ratio	American Community Survey (ACS) and Current Population Survey (CPS), disseminated by IPUMS USA and IPUMS CPS	1990, 2000-2016 (ACS) 1988, 1989, 1991-1999 (CPS)
Proportion of working adults in management or professional occupations, male:female ratio	American Community Survey (ACS) and Current Population Survey (CPS), disseminated by IPUMS USA and IPUMS CPS	1990, 2000-2016 (ACS) 1988, 1989, 1991-1999 (CPS)
Percentage of women living in counties without an abortion provider	Guttmacher Institute, disseminated by Dr. Patricia Homan via author correspondence	1988, 1992, 1996, 2000, 2005, 2008, 2011 (linear interpolation used for unobserved years)
Proportion of adults age 25 and over with at least 4 years of college, male:female ratio	American Community Survey (ACS) and Current Population Survey (CPS), disseminated by IPUMS USA and IPUMS CPS	1990, 2000-2016 (ACS) 1988, 1989, 1991-1999 (CPS)
Proportion of adults with insurance, male:female ratio	American Community Survey (ACS) and Current Population Survey (CPS), disseminated by IPUMS USA and IPUMS CPS	1990, 2000-2016 (ACS) 1988, 1989, 1991-1999 (CPS)

Proportion of working adults who are self-employed, male:female ratio	American Community Survey (ACS) and Current Population Survey (CPS), disseminated by IPUMS USA and IPUMS CPS	1990, 2000-2016 (ACS) 1988, 1989, 1991-1999 (CPS)
Proportion of adults who are registered to vote, male:female ratio	Current Population Survey (CPS), disseminated by IPUMS CPS	1988-2016
Proportion of adults who voted, male:female ratio	Current Population Survey (CPS), disseminated by IPUMS CPS	1988-2016

Note: All were measured at the state-level and coded such that higher values represented high levels of inequality (i.e., sexism).

Appendix 3.1: Procedures for building a factor model for the exposure

The structural sexism score for each state in each study year was determined using factor-analytically derived scores from models using the candidate indicators. To build the model, I first assessed dimensionality by examining a scree plot for the 11 candidate indicators, which showed two underlying factors with eigenvalues >1 before the remaining eigenvalues dropped dramatically in size. Therefore, both one- and two-factor solutions were modeled, using a two-level exploratory factor analysis that accounted for repeated measures by state, over the years 1988-2016. The two-level model clustered repeated measure of states over time to account for non-independence of state indicators over time. Item selection for the final model was based on goodness-of-fit, defined at item loading at ≥0.40; those that did not meet criterion were not retained, and models were re-run with high-fitting indicators only, based on commonly accepted guidelines.[342–344] Based on this criterion, 5 indicators were retained, and parallel analysis and scree plot of the final retained indicators suggested a single factor solution appropriately captured the variance among the indicators (i.e., a single eigenvalue >1, all others dropped dramatically and were <1, suggesting a single underlying factor). To confirm the final model fit statistics, confirmatory factor analysis was performed, with a fixed effect for year included as a predictor for the underlying factor(s); confirmatory analysis was performed on the full data set, rather than a held-out subsample, because the confirmatory models were used to produce state-level factor scores for all 50 states across all study years. Factor analyses were performed in MPlus.[345]

As a validity check, I compared the factor analytically-derived scores in the analytic sample to a previously used indicator of structural sexism[33] in bivariate analyses and found the two to be associated (b=0.40, p<0.001 in linear regression models). The comparison indicator was a measure used to quantify state-level structural sexism, over time, representing a composite of the male/female wage gap, the male/female labor force participation ratio, the male/female poverty ratio, the percent of state legislature seats occupied by men, and the percent of state population composed of religious conservatives. I chose not to use the previous measure as the main exposure in this paper for two reasons; first, it was not empirically-derived; second, one item in the previous scale (percent religious conservatives) I believe to be a confounder, rather than an indicator of sexism.

Supplemental Table 3.3: Retained items and factor loadings

Measure	Factor loading
Percentage of state legislators who male	0.57
Proportion of working adults who are self-employed, male:female ratio	0.62
Percent of adults living above the poverty line, male:female ratio	0.68
Proportion of working adults in management or professional occupations, male:female ratio	0.77
Labor force participation among adults ages 16 and over, male:female ratio	0.88

Supplemental Figure 3.3: Directed acyclic graph of confirmatory factor model for structural sexism
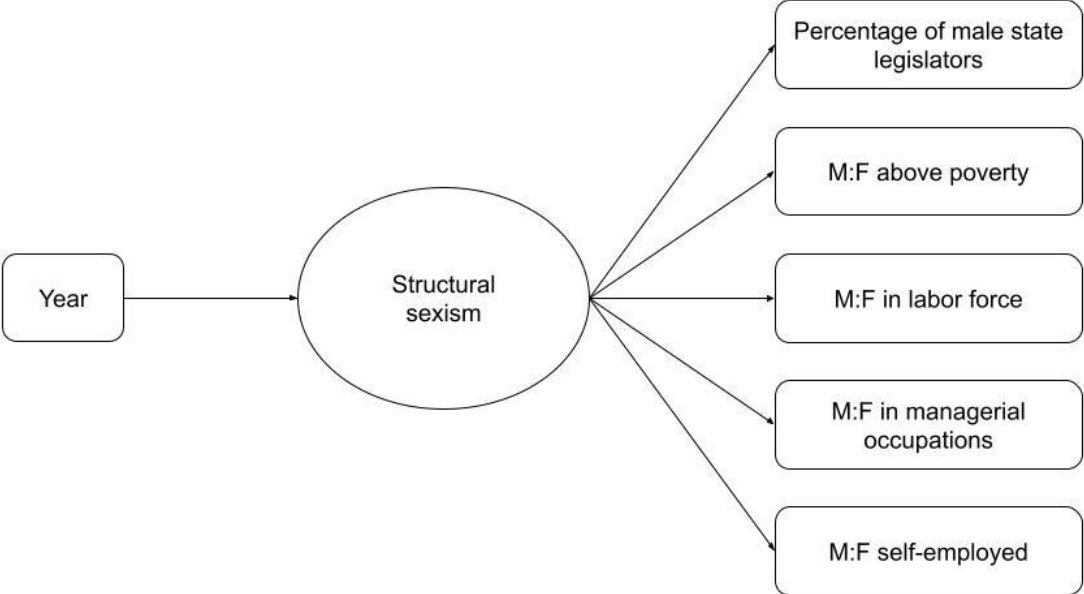
Supplemental Figure 3.4: Factor scores for structural sexism over time, 1988-2016, all 50 US states
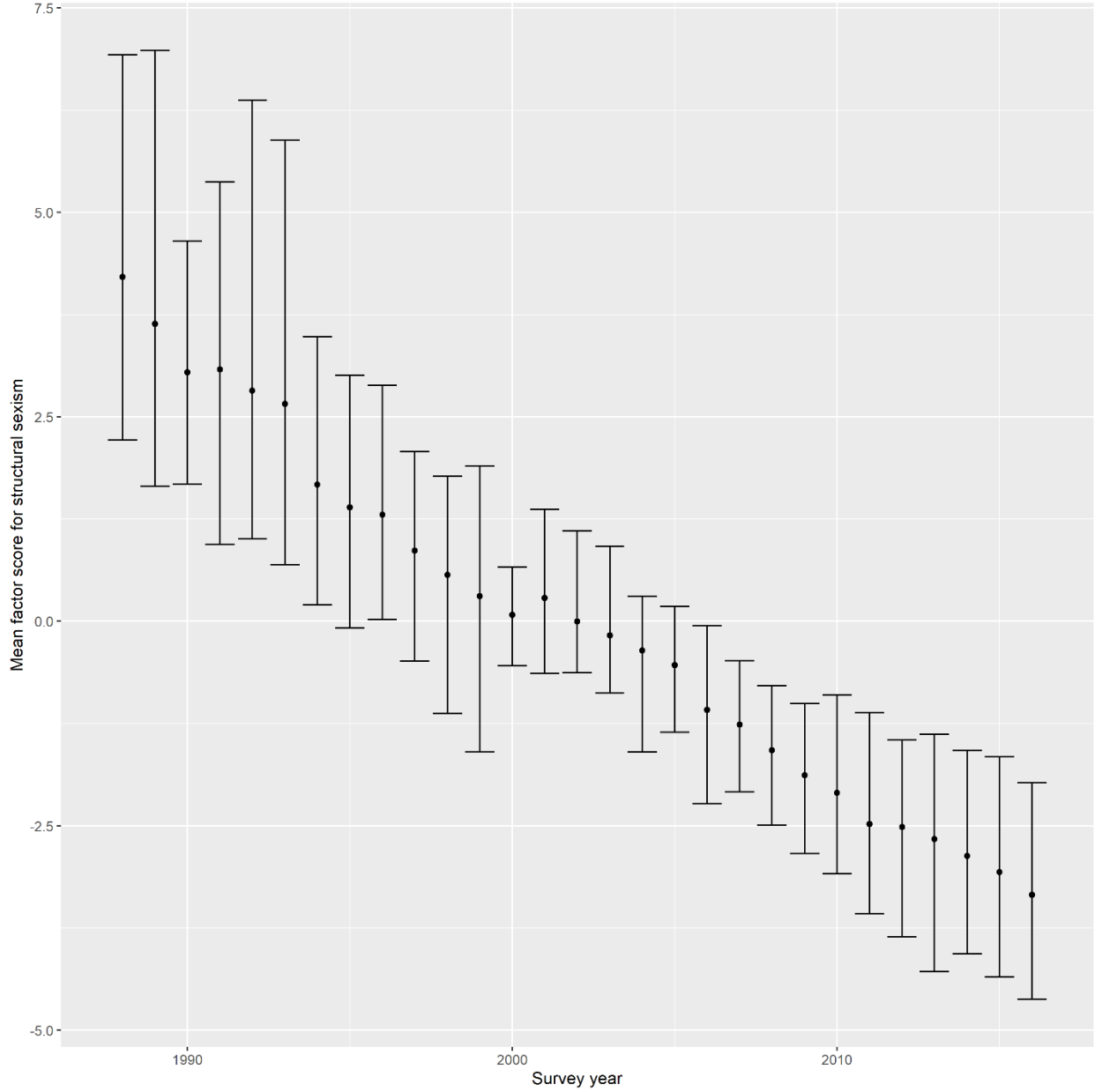
Appendix 3.2: State-level confounders operationalization and data sources

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 and linear interpolation was used to estimate density between census years.

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 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.

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 both on traditional views about both women's roles and sanctions on alcohol use, consistent with previous work.[33,40] These data were made available through the Religious Congregation and Membership Surveys, which are assessed every ten years (1980, 1990, 2000, 2010) and linear interpolation was used to estimate percentages in previous years.[33,40] While other religious groups in the United States also both sanction alcohol use and endorse traditional views about women's roles, in the United States only a small minority of adherents belong to denominations beyond Christianity and Judaism. For example,

data on state-level representation on Muslim adherents—another religious category in which certain denominations both forbid sanction alcohol consumption and subscribe to traditional views about women's roles—were not available for all study years, were not consistently measured across the two waves they were available (2000, 2010), and represented a small minority of respondents (<1%).

Alcohol policy climate was operationalized using the Alcohol Policy Scale (described in Naimi et al[196]); briefly, this scale scores state alcohol policy climates based on a composite of efficacy scores of 29 policies relevant to alcohol consumption (e.g., restrictions on sales, blood alcohol concentration laws). These data were made available from the scale developers. Scores ranged from 23.2 (relatively lax policies) to 68.1 (relatively stringent policies) with a mean of 42.4. These data measured the years 1999-2016 and linear interpolation was used to estimate scale scores in previous years (1988-1998); to confirm that interpolating these values did not bias results, I performed sensitivity analyses restricting analyses to only observed study years for this variable (i.e., 1999-2016).

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.

Supplemental Figure 3.5: Reliability of indicators of depressive symptoms over time and over study wave
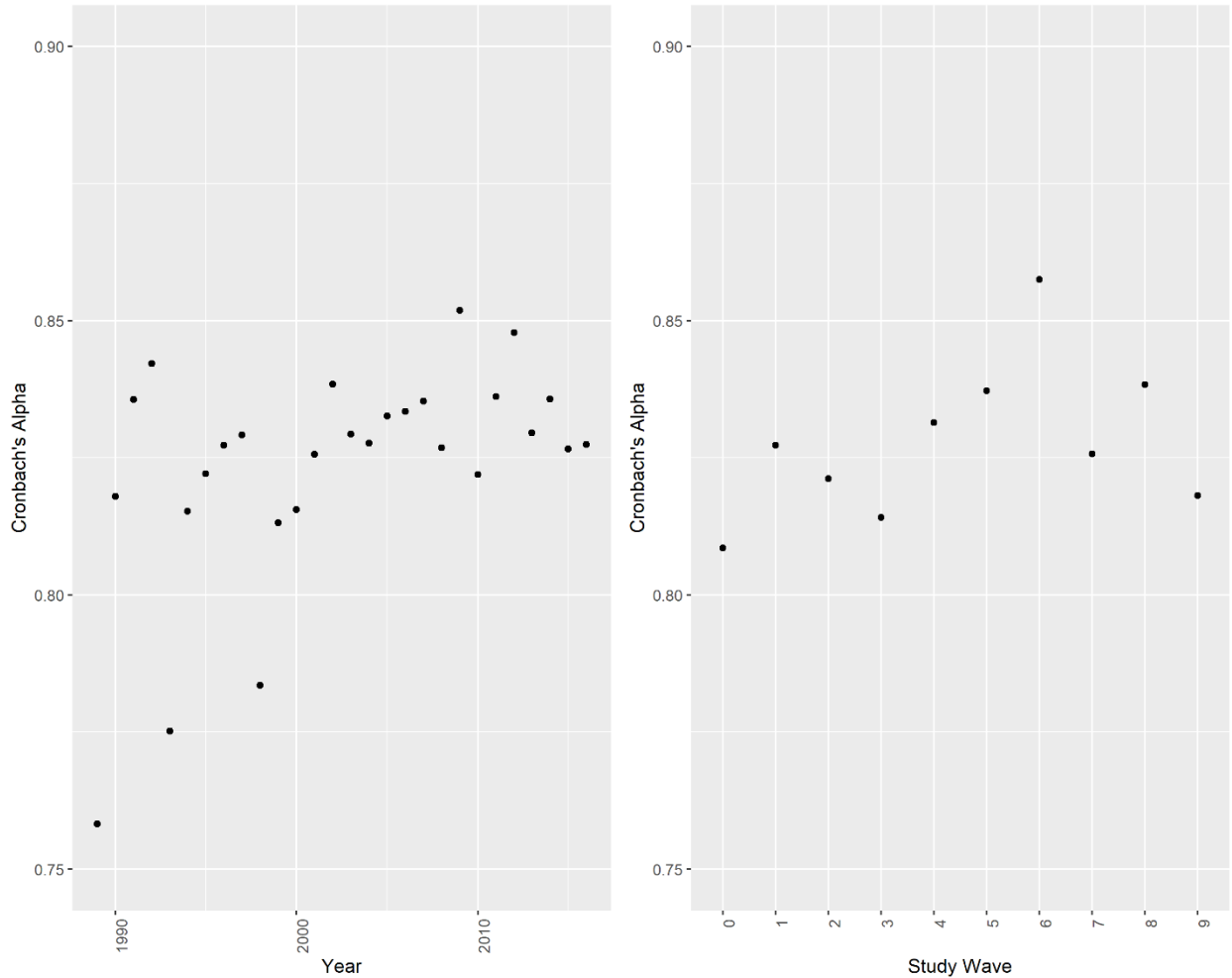
Supplemental Figure 3.6: Trends in structural sexism indicators over time, 1988-2016
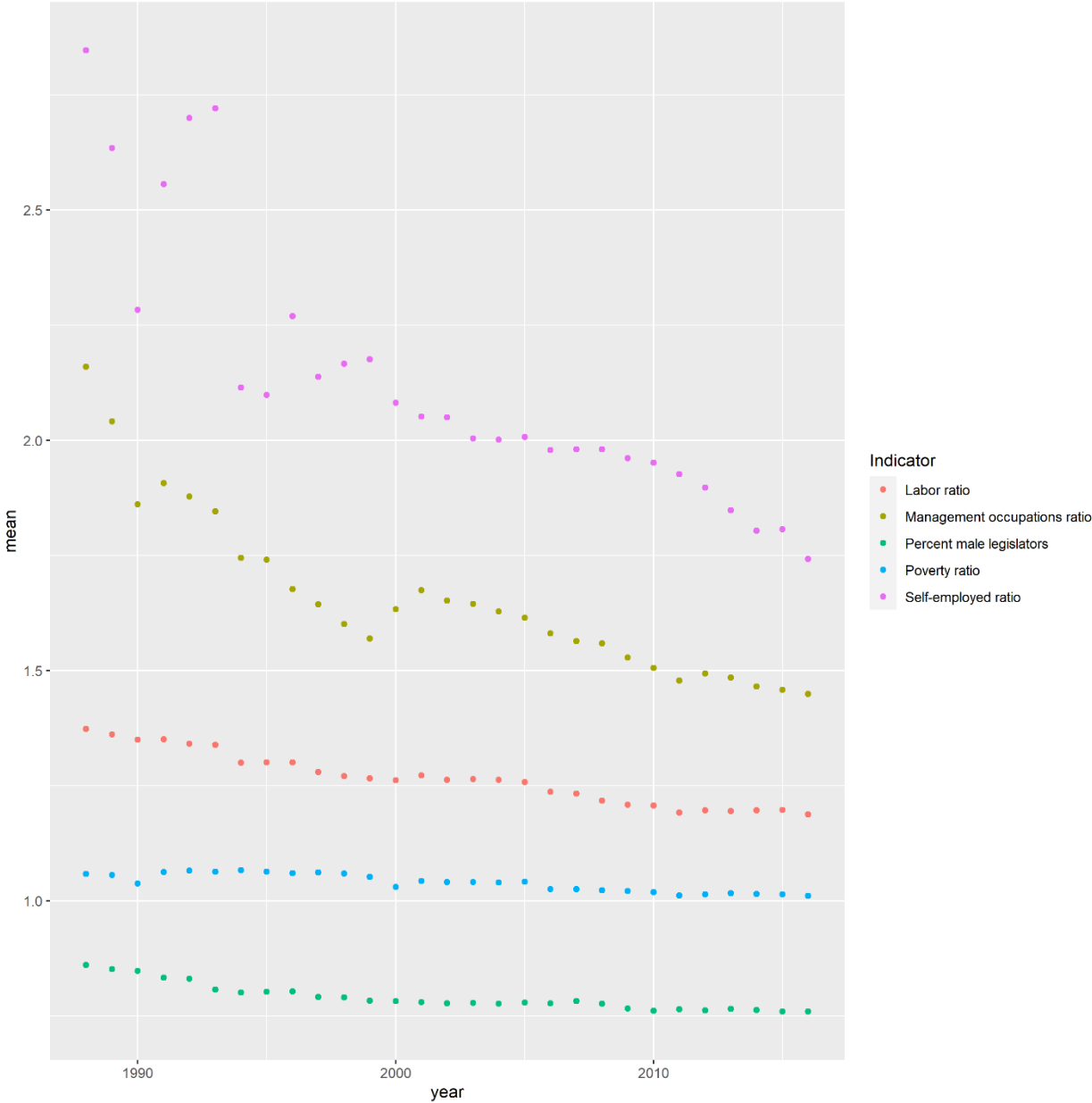
Appendix 3.3: Limitations of traditional mediation and tests of mediation assumptions

For evaluating mediation with time-varying mediators, outcomes and mediators can confound each other (Supplemental Figure 3.7, below, shows a schematic and brief description of the problem). One solution for accounting for this problem—i.e., mediators at one wave acting as confounders at a subsequent wave—is the use of non-parametric G methods.[208,209,346] However, these models are still under development for three-level multi-level models and for non-normal outcomes, which I utilized in the current study. Further, the structure of the MTF data and different subsamples queried for mediation measures precluded the use of a single model to test all mediation pathways using a more advanced causal model. The consequence of using traditional mediation, rather than alternative techniques, is that estimates of the indirect effects may be spurious and incorrectly quantified; however, estimates of the controlled direct effects—i.e., the association between the exposure and the outcome with the mediator included as a control variable—are still interpretable, assuming all other mediation assumptions are met.

In the current study I was more interested in whether and to what extent the associations are mediated through any of the identified pathways—depression, alcohol norms, education—rather than the exact magnitude of the indirect causal pathways of each mediators. Therefore, the controlled direct effect estimates produced by traditional mediation analyses are sufficient to show the plausibility of each mediation pathway and estimate the relative contribution of each mediator to the overall effect. I proceeded with traditional mediation analyses (i.e., including mediators as control variables in regression models and comparing estimated mode betas), but only assessed comparisons between total effects and controlled direct effects, rather than

estimates of indirect effects. Supplemental Figure 3.8 shows a schematic of each mediation model tested, and the relationships among the exposure, confounders, mediators, and outcomes.

Several assumptions were required for the controlled direct effect to be valid: that the exposure was related to the mediator, that the mediator was related to the outcome, that there was no exposure-mediator interaction, that all sources of exposure-outcome confounding were accounted for, that all sources of exposure-mediator confounding were accounted for, that all sources of mediation-outcome confounding were accounted for, and that there was no exposure-induced confounding.[210,211] The first three assumptions are testable, and the test results are shown below: in bivariate relationships I tested relationships between both the exposure and outcome and each mediator, and there was no exposure-mediator interaction, assessed using the relative excess risk due to interaction (RERI), defined as the effect estimate for the interaction between sexism and each mediator less the effect estimate for sexism and the mediator and the intercept.[347] The RERI was calculated using a template provided by Knol and VanderWeele[347] using estimates from interaction models between structural sexism and the mediators. While the latter four model assumptions were untestable, based on the graphical model of the conceptual relationships across study measures (Figure 1, Supplemental Figure 8), I identified no further confounders not any exposure-induced confounding.

Assumption 1: Exposure was related to the mediator

This assumption was tested for each mediator in bivariate models adjusting for all exposure-mediator confounders. Structural sexism was positively related to depression (RR=1.01, 95% CI 1.01, 1.02) and restrictive alcohol norms (OR=1.12, 95% CI 1.10, 1.13), but negatively associated with college completion (OR=0.83, 95% CI 0.82, 0.84). Therefore, the first assumption was satisfied for each potential mediator.

Assumption 2: Mediator was related to the outcome

This assumption was tested for each mediator in bivariate models adjusting for all mediator-outcome confounders, including the exposure variable. Restrictive alcohol norms were negatively associated with both alcohol consumption frequency (RR=0.65, 95% CI 0.64, 0.66) and binge drinking (OR=0.31, 95% CI 0.29, 0.34). College completion was positively associated with both alcohol consumption frequency (RR=1.13, 95% CI 1.12, 1.14) and binge drinking (OR=1.07, 95% CI 1.03, 1.12). Depressive symptoms, however, were unrelated to alcohol consumption frequency (RR=1.00) but were positively related to binge drinking (OR=1.03, 95% CI 1.02, 1.04). Thus, for alcohol consumption, depressive symptoms did not meet this assumption and could therefore not plausibly mediate the structural sexism – alcohol consumption frequency relationship; I did not proceed further with this mediation analysis. For the other 5 relationships, the second assumption was satisfied.

Assumption 3: No exposure/mediator interaction

This assumption was tested for each of the five remaining mediator models adjusting for all sources of confounding. For alcohol consumption, there was no interaction between structural sexism and restrictive norms (RERI=0.00) or college completion (RERI=-0.01, 95% CI 0.00, -0.01). For binge drinking, there was no interaction between structural sexism and restrictive norms (RERI=0.00), depressive symptoms (RERI=0.00) or college completion (RERI=0.02, 95% CI 0.00, 0.04) Therefore, this assumption was satisfied for all models tested.

Supplemental Figure 3.7: Schematic of mediation analysis and time-varying confounding by mediator
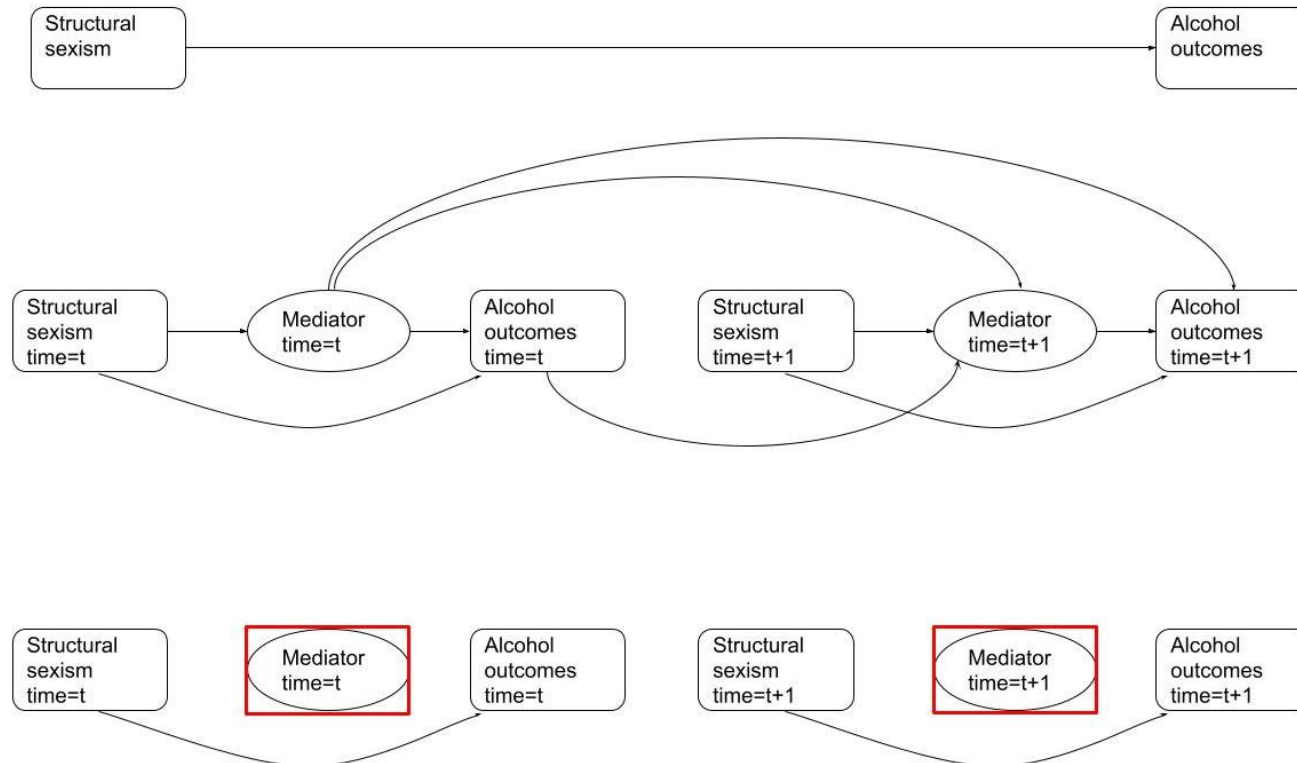
Note: Top image shows total effects (exposure-outcome); middle image shows mediation pathway for total effects, decomposed into direct (exposure-outcome) and indirect effects (exposure-mediator-outcome), with mediator at time T confounding the mediator-outcome relationship at time T+1; bottom image shows mediation pathway conditioned on mediator, with only controlled direct effects remaining

Supplemental Figure 3.8: Mediation models
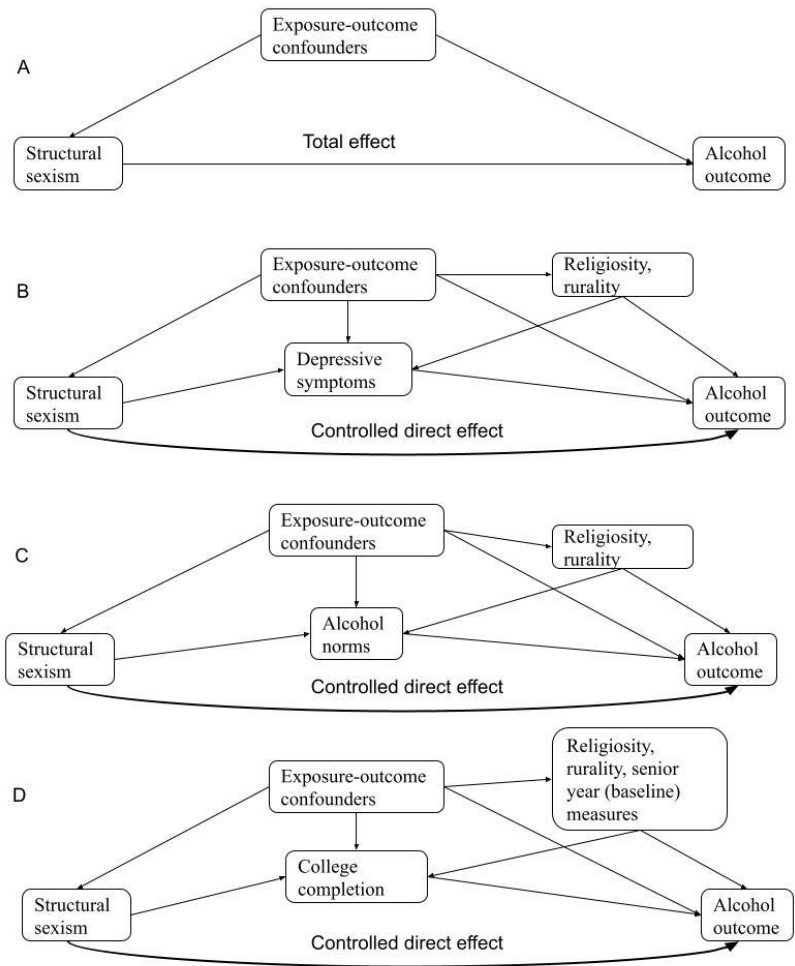
Note: Panel A shows total direct effects, panels B-D show mediation models for depressive symptoms, alcohol norms, and college completion

Appendix 4.1: Crosswalk between MTF occupation codes and US census standard occupation codes

In their original data sources, both occupational prestige and gender composition were linked to US census standard occupation codes (SOC). MTF uses a slightly different taxonomy to classify occupations, so in order to link MTF occupations to prestige and gender composition measures the MTF occupation codes were matched to SOC codes. Supplemental Table 4.1 shows these linkages. Each of the 14 possible MTF occupation codes was then given a prestige score based on the average of the 23 SOC occupations to which they corresponded. Some MTF occupations corresponded to multiple possible SOC categories; in situations where MTF occupation codes corresponded to multiple SOC categories (N=8 MTF occupations), I estimated prestige and gender ratios as the average of the SOC occupation categories to which they corresponded.

Supplemental Table 4.1: Reclassifying SOC major occupation categories to link to MTF occupation categories

| MTF occupation category	SOC major occupation categories corresponding to MTF
Laborer (custodian, material mover, maid, landscape worker, farm worker)	Building and Grounds Cleaning and Maintenance Occupations Farming, Fishing, and Forestry Occupations
Service worker (food preparer or food service worker including fast food, waiter/waitress, call center worker, stock clerk, order filler, nursing aide/orderly, teacher assistant, childcare worker)	Food Preparation and Serving Related Occupations Personal Care and Service Occupations
Operative or semi-skilled worker (bus or truck driver, maintenance or repair worker, assembly line worker)	Production Occupations; Construction and Extraction Occupations Transportation and Material Moving Occupations Installation, Maintenance, and Repair Occupations
Sales clerk in a retail store or by phone (cashier, supervisor of retail workers)	Sales and Related Occupations
Clerical or office worker (secretary, receptionist, bookkeeper, supervisor of office workers, bank teller, postal clerk or carrier)	Office and Administrative Support Occupations;
Protective service (police, firefighter, paramedic)	Protective Service Occupations
Military service	Military Specific Occupations
Craftsman or skilled worker (carpenter, mechanic, machinist, welder)	Production Occupations Arts, Design, Entertainment, Sports, and Media Occupations
Farm owner, farm manager	Farming, Fishing, and Forestry Occupations
Owner of a small business	Production Occupations Sales and Related Occupations Business and Financial Operations Occupations Arts, Design, Entertainment, Sports, and Media Occupations Management occupations
Sales representative (insurance agent, real estate)	Sales and Related Occupations
Manager or administrator (office manager, government official, sales manager	Sales and Related Occupations Community and Social Services Occupations Management occupations
Professional without doctoral degree (registered nurse, school teacher, accountant, architect, artist, information technology worker)	Healthcare Support Occupations

|	
	Business and Financial Operations Occupations Education, Training, and Library Occupations Management occupations Healthcare Practitioners and Technical Occupations Architecture and Engineering Occupations
Professional with doctoral degree or equivalent (lawyer, physician, dentist, scientist, college professor)	Management occupations; Legal Occupations Life, Physical, and Social Science Occupations Computer and Mathematical Occupation; Healthcare Practitioners and Technical Occupations Architecture and Engineering Occupations

Supplemental Figure 4.1: Trends in employment and occupational characteristics among women in the MTF sample, 1989-2016
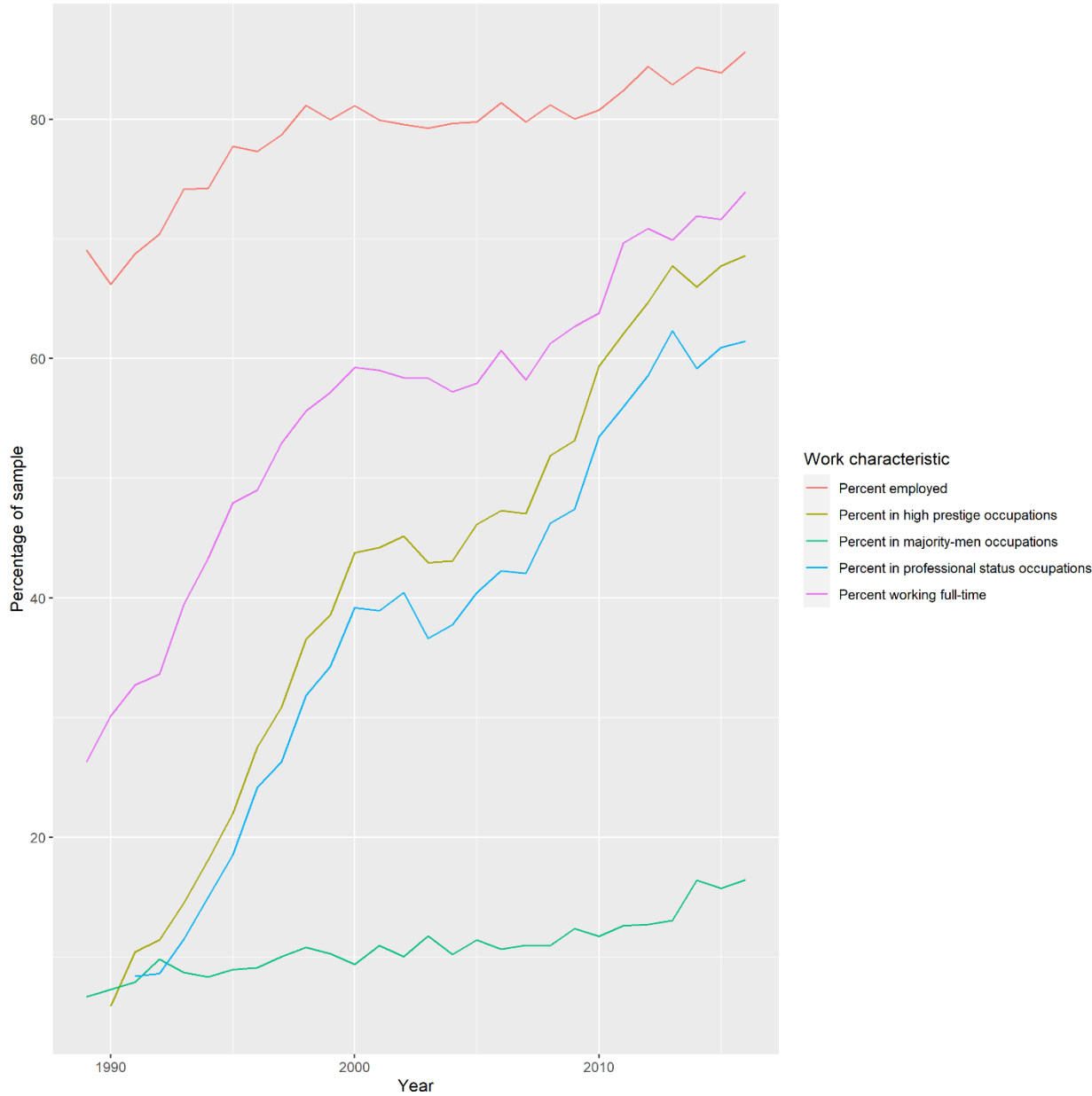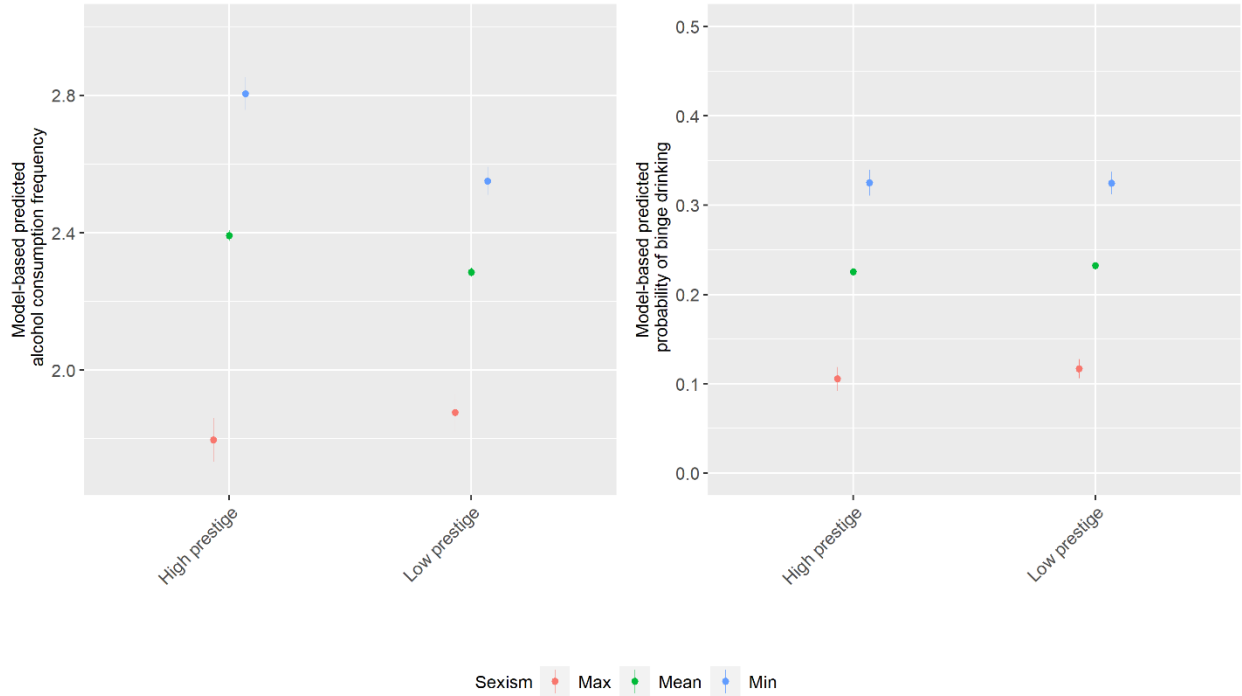
Supplemental Figure 4.2: Associations between employment status and alcohol consumption frequency and binge drinking probability, across different cut-points of structural sexism, among MTF women 1989-2016
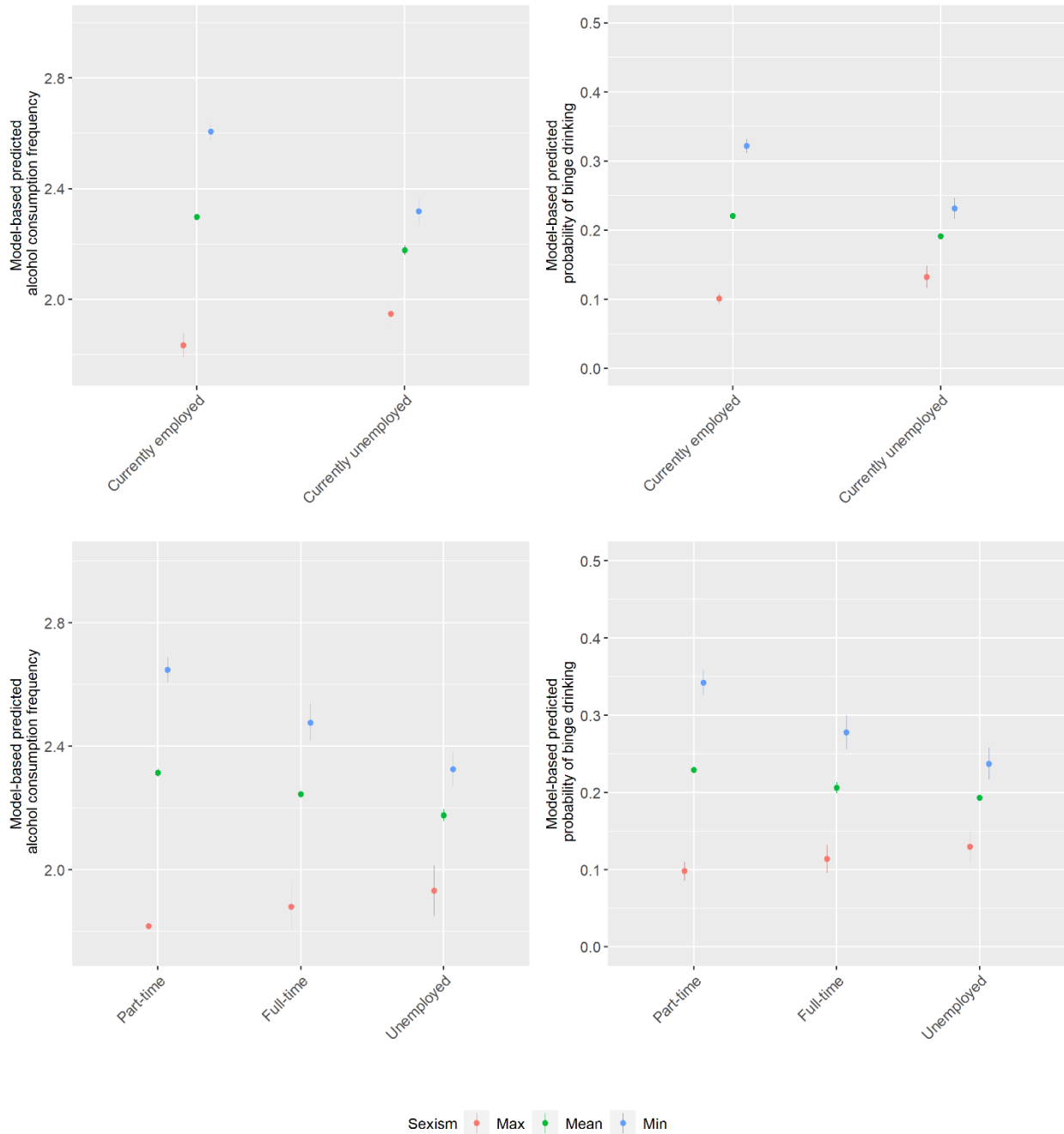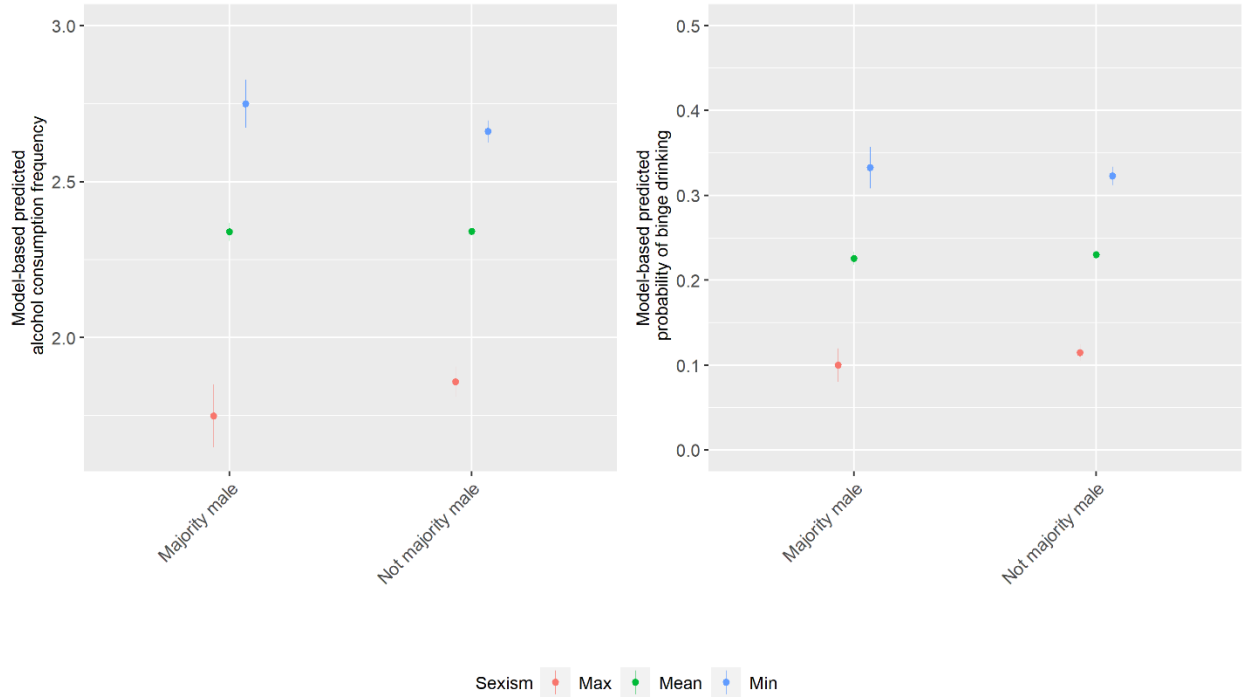
Supplemental Figure 4.3: Associations between professional status and alcohol consumption frequency and binge drinking probability, across different cut-points of structural sexism, among MTF women 1989-2016
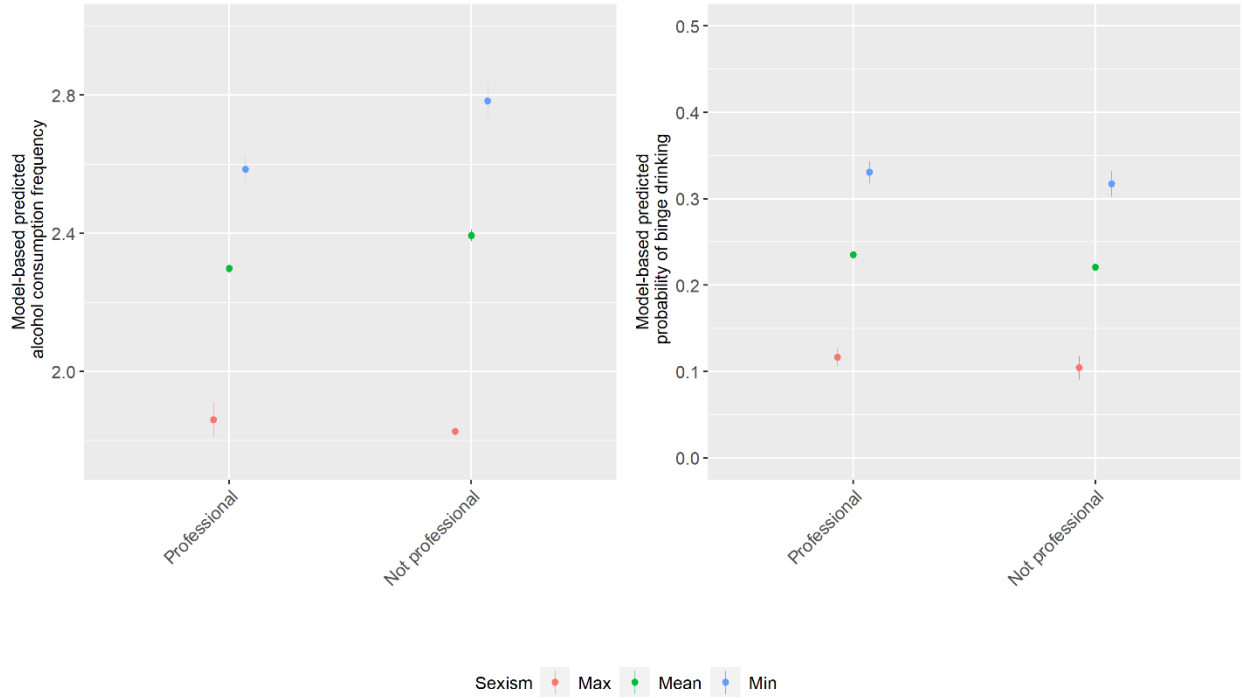
Supplemental Figure 4.4: Associations between occupational prestige and alcohol consumption frequency and binge drinking probability, across different cut-points of structural sexism, among MTF women 1989-2016
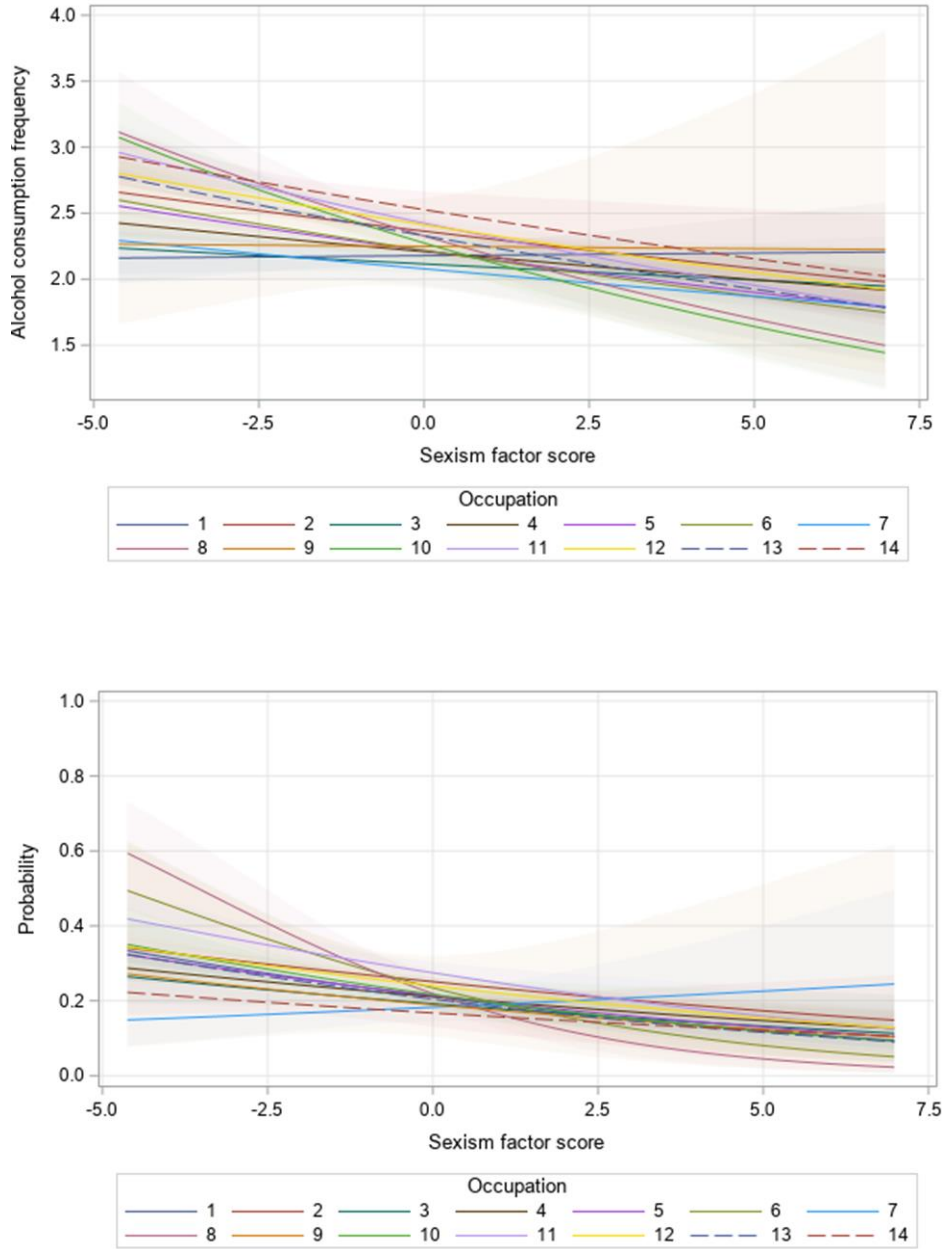
Supplemental Figure 4.5: Associations between occupational gender composition and alcohol consumption frequency and binge drinking probability, across different cut-points of structural sexism, among MTF women 1989-2016

Supplemental Table 4.2: Associations between structural sexism and alcohol outcomes among women in MTF follow-up surveys, 1989-2016, with interaction by occupation category

MTF occupation category	N observations in category	Risk ratio for alcohol consumption frequency with every 1 SD increase in sexism (95% CI)	Odds ratio for binge drinking with every 1 SD increase in sexism (95% CI)
1. Laborer	856	1.002 (0.976, 1.028)	0.874 (0.798, 0.956)
2. Service worker	9,789	0.975 (0.958, 0.992)	0.910 (0.882, 0.940)
3. Operative or semi-skilled worker	995	0.988 (0.964, 1.013)	0.915 (0.838, 0.998)
4. Sales clerk in a retail store or by phone	6,337	0.980 (0.963, 0.998)	0.915 (0.881, 0.951)
5. Clerical or office worker	12,959	0.970 (0.954, 0.986)	0.886 (0.860, 0.913)
6. Protective service	429	0.966 (0.934, 1.000)	0.778 (0.680, 0.89)
7. Military service	331	0.979 (0.943, 1.016)	1.055 (0.909, 1.223)
8. Craftsman or skilled worker	383	0.939 (0.907, 0.972)	0.699 (0.607, 0.806)
9. Farm owner, farm manager	52	0.998 (0.929, 1.073)	0.904 (0.646, 1.265)
10. Owner of a small business	651	0.937 (0.910, 0.964)	0.866 (0.770, 0.973)
11. Sales representative	1,430	0.958 (0.935, 0.981)	0.870 (0.802, 0.944)
12. Manager or administrator	4,669	0.968 (0.950, 0.986)	0.896 (0.855, 0.938)
13. Professional without doctoral degree	16,369	0.963 (0.947, 0.979)	0.874 (0.847, 0.901)
14. Professional with doctoral degree or equivalent	1,138	0.969 (0.947, 0.991)	0.927 (0.817, 1.052)

Supplemental Figure 4.6: Associations between occupation category and alcohol consumption frequency (top) and binge drinking probability (bottom), across levels of structural sexism, among MTF women 1989-2016

Supplemental Table 4.3: Associations between work status and occupational characteristics, with effect modification by structural sexism, imputed estimates

Stratum	Alcohol consumption		Binge drinking	
	Betas estimates from complete case models B (SE)	Beta estimates from imputed models B (SE)	Betas estimates from complete case models B (SE)	Beta estimates from imputed models B (SE)
Intercept	0.572 (0.060)	0.560 (0.053)	1.260 (0.213)	0.758 (0.238)
Sexism	-0.015 (0.002)	-0.012 (0.003)	-0.059 (0.009)	-0.044 (0.012)
Employed	0.046 (0.004)	0.052 (0.005)	0.152 (0.018)	0.158 (0.022)
Employed x Sexism	-0.015 (0.002)	-0.016 (0.003)	-0.065 (0.010)	-0.066 (0.012)
Intercept	0.587 (0.068)	0.568 (0.053)	1.462 (0.293)	0.805 (0.238)
Sexism	-0.016 (0.003)	-0.013 (0.003)	-0.064 (0.013)	-0.046 (0.011)
Full-time	0.054 (0.006)	0.059 (0.005)	0.184 (0.027)	0.185 (0.024)
Part-time	0.027 (0.007)	0.036 (0.006)	0.069 (0.030)	0.097 (0.026)
Full-time x Sexism	-0.016 (0.003)	-0.016 (0.003)	-0.071 (0.014)	-0.072 (0.013)
Part-time x Sexism	-0.008 (0.004)	-0.011 (0.003)	-0.031 (0.016)	-0.042 (0.015)
Intercept	0.758 (0.043)	0.662 (0.041)	1.418 (0.165)	0.802 (0.186)
Sexism	-0.028 (0.002)	-0.027 (0.002)	-0.114 (0.007)	-0.101 (0.007)
Managerial/professional status**	0.037 (0.005)	0.046 (0.005)	-0.086 (0.019)	-0.064 (0.024)
Managerial/professional status x Sexism**	-0.008 (0.002)	-0.007 (0.002)	-0.005 (0.010)	-0.004 (0.012)
Intercept	0.770 (0.043)	0.670 (0.041)	1.450 (0.166)	0.822 (0.187)
Sexism	-0.027 (0.002)	-0.026 (0.002)	-0.111 (0.007)	-0.101 (0.007)
High prestige occupation**	0.040 (0.005)	0.047 (0.005)	-0.045 (0.019)	-0.027 (0.023)
High prestige occupations x Sexism**	-0.012 (0.002)	-0.010 (0.002)	-0.010 (0.009)	-0.004 (0.011)
Intercept	0.729 (0.043)	0.630 (0.041)	1.474 (0.165)	0.833 (0.186)
Sexism	-0.031 (0.002)	-0.030 (0.002)	-0.113 (0.006)	-0.101 (0.007)

Majority-men occupation**	-0.005 (0.007)	0.004 (0.007)	-0.033 (0.027)	0.032 (0.034)
Majority-men occupations x Sexism**	-0.008 (0.004)	-0.005 (0.004)	-0.017 (0.014)	-0.001 (0.016)

Adjusted for alcohol policy climate, state- and individual-level rurality, poverty rate, GINI coefficient, state- and individual-level religiosity, race, age, paternal education, marriage status, highest education completed, rurality at baseline, religiosity at baseline, alcohol outcome at baseline
**Not adjusted for education*