Structural stigma and all-cause mortality in sexual minority populations

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ABSTRACT

Stigma operates at multiple levels, including intrapersonal appraisals (e.g., self-stigma), interpersonal events (e.g., hate crimes), and structural conditions (e.g., community norms, institutional policies). Although prior research has indicated that intrapersonal and interpersonal forms of stigma negatively affect the health of the stigmatized, few studies have addressed the health consequences of exposure to structural forms of stigma. To address this gap, we investigated whether structural stigma—operationalized as living in communities with high levels of anti-gay prejudice—increases risk of premature mortality for sexual minorities. We constructed a measure capturing the average level of anti-gay prejudice at the community level, using data from the General Social Survey, which was then prospectively linked to all-cause mortality data via the National Death Index. Sexual minorities living in communities with high levels of anti-gay prejudice experienced a higher hazard of mortality than those living in low-prejudice communities (Hazard Ratio [HR] = 3.03, 95% Confidence Interval [CI] = 1.50, 6.13), controlling for individual and community-level covariates. This result translates into a shorter life expectancy of approximately 12 years (95% C.I.: 4–20 years) for sexual minorities living in high-prejudice communities. Analysis of specific causes of death revealed that suicide, homicide/violence, and cardiovascular diseases were substantially elevated among sexual minorities in high-prejudice communities. Strikingly, there was an 18-year difference in average age of completed suicide between sexual minorities in the high-prejudice (age 37.5) and low-prejudice (age 55.7) communities. These results highlight the importance of examining structural forms of stigma and prejudice as social determinants of health and longevity among minority populations.

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Introduction

Stigma increases risk for deleterious mental and physical health outcomes across multiple groups, including racial/ethnic minorities (Paradies, 2006; Williams, 1999), sexual minorities (i.e., individuals who identify as lesbian, gay, or bisexual) (Meyer, 1995), individuals who are overweight/obese (Muennig, 2008), and those with mental illness (Link & Phelan, 2006). Stigma serves as a chronic source of psychological stress (Clark, Anderson, Clark, & Williams, 1999; Link

& Phelan, 2006; Major & O'Brien, 2005; Meyer, 2003a; Pachankis, 2007), which in turn contributes to the development of psychopathology (Brown, 1993; Dohrenwend, 2000) and disrupts physiological pathways that increase vulnerability to disease (Cherkas et al., 2006; Epel et al., 2004; McEwen, 1998).

As substantive evidence emerges that stigma represents an important social determinant of health (Hatzenbuehler, Phelan, & Link, 2013), researchers have begun to focus on the appropriate measurement and conceptualization of stigma and related constructs (Clark et al., 1999; Krieger et al., 2010; Lauderdale, 2006; Meyer, 2003b; Quinn & Chaudoir, 2009; Williams, Neighbors, & Jackson, 2008). It is widely recognized that stigma operates at multiple levels, including individual (e.g., self-stigma; Mittal,

Sullivan, Chekuri, Allee, & Corrigan, 2012), interpersonal (e.g., hate crimes; Herek, 2009), and structural, which refers to societal-level conditions, cultural norms, and institutional practices that constrain the opportunities, resources, and wellbeing for stigmatized populations (Corrigan et al., 2005; Link & Phelan, 2001). Although researchers have long theorized that structural stigma may exert deleterious consequences for health, there has been scant empirical attention paid to this topic. Indeed, a comprehensive review article identified only two studies on structural forms of (mental illness) stigma, leading the authors to conclude that "the under-representation of this aspect is a dramatic shortcoming in the literature on stigma, as the processes involved are likely major contributors to unequal outcomes" (Link, Yang, Phelan, & Collins, 2004, p. 515–516).

One reason for this dearth of research is the paucity of available measures of structural stigma. In the absence of such measures, researchers have relied on assessing structural sources of stigma at the individual level of analysis. One problem with individual-level measures, however, is that they cannot capture certain forms or dimensions of stigma, particularly those that exist at the structural level (Meyer, 2003b). An additional barrier to research on the health consequences of structural stigma is methodological. Given the pervasiveness of structural stigma, there is often little or no variation to study, which restricts the kinds of research questions that are possible to pursue. For example, research on structural racism and health has had to focus almost exclusively on racial residential segregation (Williams & Collins, 2001) because neighborhoods offer one of the few areas of analysis with adequate variation in structural discrimination.

In an effort to advance the literature on structural stigma and health, the current study developed a measure of structural stigma with adequate geographic variation and then linked this measure to individual health outcomes. We focused our inquiry on sexual minorities, a stigmatized group that currently and historically has confronted multiple forms of structural stigma. Since the social movements of the 1960s, homosexuality has gained gradual acceptance in mainstream society, as reflected through changes in the public's perception of gays and lesbians as well as in recent policies extending protections to this group. This acceptance, however, has been far from uniform; consequently, there is substantial spatial and temporal variation in environments that are supportive of gays and lesbians. Recent studies conducted by Hatzenbuehler, Keyes, and Hasin (2009); Hatzenbuehler, McLaughlin, Keyes, and Hasin (2010); and Hatzenbuehler (2011) have indicated that sexual minorities who live in areas with greater structural stigma (e.g., states that initiated constitutional amendments banning same-sex marriage) have higher rates of psychiatric disorders and are more likely to attempt suicide than sexual minorities living in low structural stigma areas.

Although these studies have documented some of the negative mental health consequences of exposure to structural stigma, no research to date has examined whether variations in structural stigma influence other health outcomes among sexual minorities, including premature death. Answering this research question not only requires information on structural stigma over time, but also the ability to assess the mortality of sexual minorities who have been differentially exposed to environments characterized by high versus low levels of structural stigma. Such data is extremely difficult to procure and, until this point, has not existed. However, an innovative new dataset—the General Social Survey/National Death Index study—permits a novel test of the impact of structural stigma (operationalized as area-level anti-gay attitudes) on sexual minority mortality. Since 1972, the General Social Survey (GSS) has been the primary source of social indicator data for the social sciences. It contains questions surrounding a wide array of social attitudes—including anti-gay prejudice—as well as measures of sexual orientation. Moreover, Muennig, Johnson, Kim, Smith, and Rosen (2011) recently linked the GSS to mortality data from the National Death Index (NDI) so that information on mortality is now available for participants across multiple waves of the GSS. Our study is the first to leverage the strength of the linked GSS-NDI data to assess whether structural stigma increases the risk of mortality among sexual minorities residing in areas of high stigma.

Methods

Data sources

The GSS is a representative sample of the U.S. non-institutionalized English-speaking population aged 18 and over. Originally an annual survey, the GSS became a biennial survey beginning in 1994. Response rates range from 70 to 82%; further information on the response rate and characteristics of non-response can be obtained from the National Opinion Research Center (NORC), which conducts the GSS. The sampling design has varied over the years, but the majority of years employed full probability sampling.

The General Social Survey/National Death Index (GSS-NDI) is a new, innovative prospective cohort dataset in which participants from 18 waves of the GSS are linked to mortality data by cause of death, which was obtained from the NDI. To link the two datasets, GSS provided identifiable information on the respondents. The linkage methodology that was employed has been well validated in other national surveys, including the National Health Interview Survey and the National Health and Nutrition Examination Survey (National Center for Health Statistics, 2009).

The GSS-NDI covers GSS survey years 1978–2002 linked to NDI data through 2008. The truncated years for this study (1988–2002) were selected given our focus on sexual minority populations, which were not available in the GSS survey until 1988, when questions related to the number and gender(s) of sexual partners were first included. More details on the GSS/NDI study, including the linkage methodology, can be obtained elsewhere (Muennig et al., 2011).

Sample and measures

Sexual minority status

Classification of sexual minority status was based on a behavioral measure of sexual orientation. Since 1988, respondents were asked whether their sexual partners were exclusively male, exclusively female, or both male and female. Gender of sexual partners was assessed over the past 12 months and the past 5 years. Some years also included questions asking the number of sexual partners the respondent had of each gender since age 18. If subjects had any sexual partners of the same sex in the past 12 months, the past 5 years, or since age 18, they were categorized as sexual minorities. We included all three time frames in case there were incongruent responses (e.g., someone who indicated a same-sex relationship in the past year, but not the past five years, which is logically impossible), to capture individuals with past but not current same-sex relationships, and to include respondents from years in which all three questions were not asked simultaneously. Of the 21,045 respondents assessed between 1988 and 2002, 914 (4.34%) engaged in same-sex relationships. These rates of same-sex sexual behaviors are comparable to those observed in other nationally representative surveys (Gilman et al., 2001).

Independent variable: structural stigma

Construction of the measure. In order to capture prejudicial social attitudes against sexual minorities, and to focus on community-level variations in these attitudes, we constructed a

measure capturing the average level of anti-gay prejudice at the community level, which we hereafter refer to as "structural stigma." In the GSS, there are only 5 items that assess attitudes toward homosexuality. One question, "Should homosexual couples have the right to marry one another?," was asked only once (in 1988) and thus was omitted from the sample. The remaining four items that comprised the prejudice scale were: (1) "If some people in your community suggested that a book in favor of homosexuality should be taken out of your public library, would you favor removing this book, or not?" (2) "Should a man who admits that he is a homosexual be allowed to teach in a college or university, or not?" (3) "Suppose a man who admits that he is a homosexual wanted to make a speech in your community. Should he be allowed to speak, or not?" (4) "Do you think that sexual relations between two adults of the same sex is always wrong, almost always wrong, wrong only sometimes, or not wrong at all?"

These four questions were each dichotomized such that a value of one indicated the presence of anti-gay prejudice. Three of the four questions were already written as dichotomous items, but the fourth question, relating to attitudes towards homosexual sexual relations, required conversion to a dichotomous response to ensure consistency across the 4 items. Respondents who indicated that homosexual relations were "not wrong at all" were coded with zeros; the other three responses indicating that the respondent felt that same-sex relations are wrong at least to some degree was coded with a one. The Cronbach's alpha for the four prejudice questions was 0.75, which indicates that this is a reliable measure for our analyses.

Next, we combined the four anti-gay prejudice questions into a single summed value that was averaged at the community level to create the average sum of structural stigma in the community (assessed here as the primary sampling unit). In the GSS, primary sampling units (PSUs) are composed of either metropolitan statistical areas (MSAs) or non-metropolitan counties and serve as an indicator of "life space" where individuals live, work, and play (Gibson, 1995). Analysis of the distribution of scores by PSU indicated that a dichotomized measure of high structural stigma would be preferable for addressing the role of exposure to structural stigma on mortality risk. The top quartile was selected as the cut point for the dichotomized measure because there was a clear separation between the top quartile and the lower levels of prejudice when the distribution of values was examined across PSUs. PSUs in the top quartile of the distribution were coded as one (indicating high structural stigma communities), and the remaining PSUs were given a value of zero (indicating low structural stigma communities). This dichotomized measure therefore serves as an indicator of whether a sexual minority respondent lives in a high or low structural stigma community. The values for the PSU structural stigma variable ranged from a low of 0.81 to a high of 2.67, out of a possible range of 0–4. The cutoff for the dichotomized measure was 1.77, which indicates that, on average, people residing in the high structural stigma PSUs agreed with almost half of the anti-gay prejudice items.

There were 184 PSUs in the GSS from 1988 to 2002. Given our research question, we limited our sample to all persons residing in PSUs with at least one sexual minority individual ($n = 170$ PSUs, or 92.39% of all sampled PSUs). Thus, we dropped 1044 cases from fourteen PSUs with no sexual minority individuals across the years of our analyses, resulting in a total sample of 20,001 individuals (95.0% of all sampled individuals), of whom 914 were sexual minorities (4.57%). The lowest number of individuals sampled within any PSU was 42. Contextual variables require at least 30 cases per PSU to be used (Snjiders & Bosker, 1993). Consequently, we had more than an adequate number of cases per PSU for analyses.

Finally, PSU codes changed in 1993, which allowed us to calculate values for the PSU-level prejudice items bounded from 1988 to 1993 and from 1993 to 2002. People sampled from 1993 affected estimates for PSU values in both groups because in 1993 both types of PSU codes were used in the GSS sample design. These time-specific PSU codes were used to create the area prejudice score. Consequently, respondents' prejudice values were only grouped by 5- and 8- year periods instead of the entire 15 years of the study. As such, the PSU-level stigma values were less likely to be affected by any changes in prejudice that occurred over the study period with this approach.

Missing data and imputation approach. Each of the four prejudice items was asked in all waves that we analyzed and among persons in all PSUs we analyzed. Among those respondents who were asked the prejudice questions in each year, there were few items missing, with missing values ranging from a low of 2.9% on the item regarding public speeches to a high of 6.7% on the item regarding same-sex relations. However, given the structure of the GSS, not all questions were asked among all respondents each year. Each of these measures had greater than five percent missing due to this planned missing design, meaning that not all respondents were given the chance to respond to all questions.

The use of this split-ballot design motivated the use of an imputation strategy. If we relied solely on complete cases through listwise deletion, we would lose a sizable portion of our data resulting in significant loss of power. Multiple imputation avoids this problem (Acock, 2005; Little & Rubin, 2002; Ragunathan, 2004). Due to the importance of the prejudice measures in our analyses, and the fact that none of the questions were asked among all respondents in every year, we used multiple imputation to address the missing data. We used the "ice" command in Stata 11.2 (Royston, 2005) to impute missing values for the prejudice variables with the other independent variables used in the imputation models. After the data were imputed, the datasets were analyzed together using "Rubin's Rules" for combining imputed datasets for analysis (Little & Rubin, 2002). Ten datasets were created using all covariates in the chained imputation models. The imputation command was adjusted to ensure proper estimation of missing values on the covariates (i.e., continuous, dichotomous, or ordinal measurement). When analyzed separately, there were no statistical differences between the estimates of the means and standard errors of the covariates between imputed datasets.

This multiple imputation approach enabled us to predict the missing values for the prejudice questions for respondents in years where the questions were asked but they were not given the opportunity to respond. This was an appropriate solution to handling missing data because the split ballot design of the GSS ensures that participants were distributed randomly across the groups. Further, we used the entire sample and all of our covariates, including the time variable (i.e., year of interview), in the imputation models to ensure that the predicted values of prejudice were the best possible measures for each respondent. It was important to include year of interview in the imputation models in case there was any significance to year of interview in calculating the most likely responses for respondents in a given year.

Outcome variable: all-cause mortality

Information on all-cause mortality was obtained from the NDI, as described above. Of the 914 sexual minorities in our sample, 134 (14.66%) were dead by 2008. In our models, respondents who had died by 2008 were coded as ones and those who survived the study period were coded as zeros.

Covariates

Individual-level covariates. We included an array of covariates to assess the plausibility of alternative explanations for premature mortality among sexual minorities living in communities with high

levels of structural stigma. Specifically, we examined three types of individual-level covariates: health measures; socioeconomic indicators; and sociodemographics.

We address baseline health using self-rated health status, which was assessed via a single item: "Would you say your own health, in general, is excellent, good, fair or poor?" Prior research has demonstrated that self-rated health is a validated indicator of health distress and/or the presence of disease and differentiates heightened mortality risk (Idler & Benyamini, 1997). Self-rated health was dichotomized at fair/poor versus excellent/good.

We included two socioeconomic measures, household income and individual educational attainment, due to the established inverse association between these variables and individual mortality risk (e.g., Sorlie, Backlund, & Keller, 1995). Given the skewed distribution of the income variable, we used the natural logarithm of income in our models. We measured educational attainment with a measure corresponding to the respondents' number of years of formal education.

In addition to health and socioeconomic measures, we also analyzed several demographic controls associated with individual mortality risk, including respondent racial/ethnic identification (White, Black, or other race), sex (male or female), age at interview, and nativity status (indicating whether the respondent was born outside the United States).

Community-level covariates. We included three PSU-level covariates from the GSS dataset to address potential community-level confounders of the relationship between structural stigma and mortality. These measures included the average PSU-level educational attainment, average PSU-level income (natural logarithm transformed), and the proportion of individuals living in the PSU who identified as "slightly" to "extremely" politically conservative. We elected to include the two community-level socioeconomic measures due to the relationship between neighborhood segregation, affluence, and health (Bellatorre, Finch, Do, Bird, & Beck, 2011). Since we did not have additional variables measuring access to resources (e.g., supermarkets, proximity to parks), we used these measures as proxies for area-level resources. We included the proportion of politically conservative individuals in the community because conservatism is associated with our independent variable (i.e., negative attitudes toward homosexuality) (Morrison & Morrison, 2002; Schwartz, 2010; Wood & Bartowski, 2004). These community-level measures were created from the full sample of 20,001 GSS respondents and were appended onto the 914 sexual minority individuals according to their PSU of residence.

Statistical analysis

Our objective was to assess the impact of structural stigma within a given community on accelerating the time to death by 2008 (the last available year of mortality data) for sexual minority populations. To do so, we utilized Cox proportional hazard models. This analytic strategy was selected because we modeled time to death over the study period, which resulted in a censored amount of time at risk. Cox proportional hazard models can be used to analyze time to death, with the variable for death differentiating the deceased from those who were still living in 2008. For the deceased, we created our time variable by subtracting the year of interview from the year of death, which represents the number of years lived by each respondent following the interview. For those who were still alive in 2008, we subtracted year of interview from 2008, which represents the number of years between the time a respondent was interviewed and the final year of our study.

Our focal analyses involve examining the effect of structural stigma—operationalized as living in communities with high levels of anti-gay prejudicial attitudes—on premature mortality risk after

controlling for individual- and community-level predictors of mortality among the sexual minority respondents. In addition to these primary analyses, we conducted three secondary analyses to provide further support for our inferences about the relationship between structural stigma and mortality. The first test, described in the results section below, addressed potential measurement issues. The second test presents descriptive information on specific causes of death in high versus low-stigma PSUs to provide information on potential mechanisms linking structural stigma to health. The third test explored the differential impact of structural stigma on mortality for sexual minorities versus heterosexuals. Recent studies have documented that subordination of low status groups not only harms minority groups, but also majority groups (e.g., Lee, Muennig, & Kawachi, 2012; Stanistreet, Bambra, & Scott-Samuel, 2005), suggesting that structural stigma may also have negative health consequences for heterosexuals. Consequently, we hypothesized that structural stigma would be associated with mortality for heterosexuals, but that these relationships would be stronger among sexual minorities.

Preliminary analyses showed that the interaction between PSU-level prejudice and sex was not statistically significant for sexual minorities, indicating that the association between structural stigma and mortality was similar for men and women. Consequently, analyses combined sexual minority men and women. All analyses were conducted in STATA and weighted to adjust for the complex sampling design. The statistical significance was set at $p < 0.05$.

Hazard ratios (HR) were converted into life expectancy values at age 18 by multiplying the age-specific mortality rates starting at age 18 in an unabridged life table by the HR. The change in life expectancy before and after adjusting age-specific mortality rates is the difference in life expectancy (Muennig & Gold, 2001). The 95% CI was computed using the upper and lower bound of the HR.

Results

Descriptive statistics

Table 1 summarizes the survey design adjusted sociodemographic characteristics of the sexual minority sample. Of the 914 sexual minority respondents, roughly 78% were white, 16% were Black and 7% were members of other racial groups. Approximately 12% of the sexual minority sample was comprised of immigrants. Although there are fewer male respondents in the GSS (and in the US population) overall than females, males were slightly overrepresented in our sample of sexual minority individuals (51%). The mean age of all sexual minority respondents at year of interview was just under 40. It is noteworthy that this sample had a high proportion of individuals who rated their health as being either "good" or "excellent" (81%).

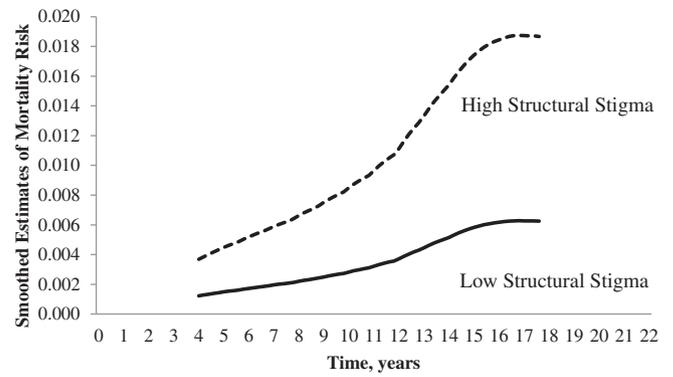*Associations between structural stigma and mortality among sexual minorities*

We ran five models to evaluate associations between structural stigma and mortality among sexual minorities (see Table 2, Models 1−5), which were estimated in a series of progressive models from the baseline model (Model 1). Model 2 included demographic risk factors (sex, age at interview, race/ethnicity and nativity status); Model 3 added controls for socioeconomic status (household income and years of education); Model 4 added controls for self-rated health; and Model 5 included additional controls for community-level confounders (PSU-level income, education, and conservatism). The fit indices indicated that the final model provided the best fit for the data, while also including a comprehensive array of

Table 1
Sample demographics of the sexual minority respondents in the General Social Survey/National Death Index Study (N = 914).

Variable	Weighted mean or Proportion[a]	TSA standard error	95% CI lower boundary	95% CI upper boundary
Respondent died by 2008	0.14	0.01	0.11	0.16
White	0.78	0.02	0.73	0.82
Black	0.16	0.02	0.13	0.19
Other race	0.07	0.01	0.04	0.09
Male	0.51	0.02	0.48	0.55
Female	0.49	0.02	0.45	0.52
Age at interview	39.86	0.54	38.79	40.93
Immigrant	0.12	0.02	0.07	0.17
Income (ln)	10.27	0.04	10.19	10.36
Years of education	13.40	0.12	13.15	13.64
Fair/Poor self rated health	0.18	0.02	0	1
Resides in a high prejudice PSU	0.12	0.02	0	1
PSU Average education	13.28	0.08	10.31	15.14
PSU Average income (ln)	10.40	0.02	9.54	11.03
PSU Proportion conservative	0.34	0.01	0.22	0.54

Notes.
PSU = primary sampling unit. LN = logarithm transformed. TSA = Taylor Series Approximation.
[a] Weighted proportions are values below 1; all other values are weighted means.

Fig. 1. Estimated smoothed mortality hazards by high prejudice residential area, General Social Survey/National Death Index, 1988–2002.

possible covariates associated with alternative explanations for the relationship between time to mortality and sexual minority status. Even after controlling for all individual and community-level risk factors, structural stigma was still strongly associated with premature mortality among sexual minorities (HR = 3.03, 95% C.I.: 1.50–6.13). This translates into a life expectancy difference of roughly 12 years on average (95% C.I.: 4–20 years). These results indicate that sexual minorities living in communities with high levels of structural stigma die sooner than sexual minorities living in low-stigma communities, and that these effects are independent of established risk factors for mortality. Figs. 1 and 2 depict the estimated smoothed mortality hazards and the survival time, respectively, by structural stigma. By 2002, 92.4% of sexual minorities living in low structural stigma areas were still alive;

conversely, only 77.8% of sexual minorities living in high structural stigma areas were still alive in 2002 (Fig. 2).

Tests of specificity and strength

First, we ran analyses (not shown but available upon request) using alternative measures of structural stigma, including predicted factor scores at the PSU level and the average summed prejudice scores at the PSU level (continuous measure). Our results for these other presentations of PSU-level stigma were stronger than the dichotomized measure we used, suggesting that the observed relationships are consistent across alternative measures of structural stigma; we elected to present the dichotomous measure for ease of interpretation.

Second, analyses that examined specific causes of death in the sexual minority sample revealed that suicide and homicide-related deaths were all elevated in the high-stigma PSUs compared to the low-stigma PSUs. Specifically, 6.25% of the high-stigma PSU deaths were due to suicide, compared to 2.94% of low-stigma PSU deaths, which translates into a relative risk (RR) of 2.1. Additionally, 6.25%

Table 2
Cox proportional hazard models predicting hazards of death for sexual minority individuals (N = 914).

	Model 1 HR (95% CI)	Model 2 HR (95% CI)	Model 3 HR (95% CI)	Model 4 HR (95% CI)	Model 5 HR (95% CI)
Structural stigma					
Top quartile PSU-level prejudice score	2.51 (1.58, 4.00)	2.28 (1.42, 3.67)	2.29 (1.40, 3.75)	2.29 (1.40, 3.75)	3.03 (1.50, 6.13)
Demographics					
Age at interview		1.05 (1.04, 1.07)	1.05 (1.04, 1.07)	1.05 (1.04, 1.07)	1.05 (1.04, 1.06)
Black		2.99 (1.94, 4.59)	3.03 (1.90, 4.81)	3.01 (1.86, 4.89)	2.87 (1.76, 4.67)
Other race		2.48 (1.04, 5.93)	2.43 (1.01, 5.84)	2.44 (1.01, 5.88)	2.28 (0.97, 5.37)
Female		0.58 (0.39, 0.86)	0.58 (0.39, 0.87)	0.58 (0.39, 0.87)	0.59 (0.39, 0.88)
Not US born		0.59 (0.27, 1.33)	0.59 (0.26, 1.33)	0.59 (0.26, 1.33)	0.54 (0.25, 1.18)
Socioeconomic factors					
Household income (log transformed)			1.04 (0.86, 1.24)	1.04 (0.87, 1.24)	1.04 (0.86, 1.86)
Years of education			0.99 (0.93, 1.05)	0.99 (0.93, 1.05)	0.99 (0.93, 1.05)
Self-assessed health					
Fair/Poor self-rated heath				1.03 (0.61, 1.73)	1.04 (0.61, 1.78)
Whole sample PSU-level covariates					
PSU-Average years of education					1.70 (0.56, 5.17)
PSU-Average income (log transformed)					0.86 (0.61, 1.19)
PSU-proportion conservative					0.01 (0.00, 0.32)
Model fit indices					
F (df)	15.25 (1, 162)	30.84 (6, 162)	23.72 (8, 161.9)	20.67 (9, 161.3)	17.29 (12, 161.5)
Prob > F	0.0001	0.0000	0.0000	0.0000	0.0000
Model imputations	10	10	10	10	10

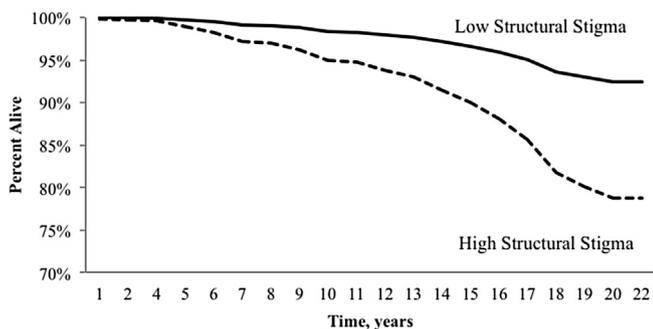Notes. HR = Hazard Ratios. CI = Confidence Intervals.

Fig. 2. Survival time by high prejudice residential area, General Social Survey/National Death Index, 1988–2002.

of the high-stigma PSU deaths were due to violence or murder, compared to 1.96% of low-stigma PSU deaths, which translates into an RR of 3.2. When we examined average age at death by cause of death for these two outcomes, we found large disparities between the high- and low-stigma PSUs. There was an 18-year difference for average age of suicide between the high- and low-stigma PSUs; in particular, those living in high-stigma PSUs died of suicide on average at age 37.5, compared to age 55.7 for those living in low-stigma PSUs. Moreover, violence/murder-related deaths occurred on average 4 years earlier in the high- versus low-stigma PSUs (age 31.5 versus 35.5, respectively).

In addition to suicide and homicide, we also examined causes of death due to cardiovascular disease (CVD) and cancer. Results indicated that 25% of the high-stigma PSU deaths were due to CVD causes, compared to 18.63% of the low-stigma PSU deaths, which translates into an RR of 1.34. In contrast, cancer-related deaths were slightly elevated in the low-stigma PSUs (27.45% of deaths) compared to the high-stigma PSUs (25.01% of deaths). Finally, only 5 sexual minorities died of HIV/AIDS-related causes, and these did not differ between high-stigma (3.13% of deaths) and low-stigma (3.92% of deaths) PSUs. Given the small sample sizes of specific causes of death, we cannot generalize these findings. Nevertheless, taken together, these patterns for specific causes of death lend support for our general hypotheses regarding all-cause mortality risk.

Third, in models including the entire GSS/NDI sample from 1988 to 2002, the interaction between structural stigma and sexual orientation was statistically significant (HR = 1.60, 95% CI: 1.06, 2.41), indicating that the effects of structural stigma on mortality were significantly stronger for the sexual minority population than for heterosexuals. While residing in high structural stigma communities moderately increased the hazard ratio for time to death for heterosexuals by 49% (HR = 1.49, 95% C.I. = 1.19, 1.87), residing in these communities increased the hazard of death by 203% for sexual minorities (HR = 3.03, 95% C.I.: 1.50, 6.13). The effect of structural stigma on mortality was therefore over four times larger for sexual minorities than for heterosexuals (203/49 = 4.14).

Discussion

The central finding of the current study is that sexual minority individuals who live in high structural stigma communities—defined as communities with greater prejudicial attitudes against gays and lesbians—die sooner than those who live in communities with low levels of structural stigma. Structural stigma remained strongly associated with mortality risk among sexual minorities even after controlling for multiple established risk factors at both the individual (age at interview, self-rated health, race/ethnicity, household income, sex, nativity status, and educational

attainment) and community (collective average education level, income, and proportion of individuals identifying as politically conservative) levels. These results were robust across different ways of creating the structural stigma measure (continuous, dichotomous, factor score). The HR for structural stigma translated into a life expectancy difference of roughly 12 years, which is greater than life expectancy differences between high school dropouts and graduates (Meara, Richards, & Cutler, 2008; Muennig, Fiscella, Tancredi, & Franks, 2010). It is important to note, however, that the confidence interval for the life expectancy effect was large, given the small sample size of sexual minorities; consequently, this result should be interpreted with some caution and requires replication in a larger sample.

When we analyzed specific causes of death, the strongest relationships with PSU-level stigma were observed for homicide and suicide, which represent relatively direct pathways linking structural stigma to mortality. Indeed, homicide is one of the most direct links possible between hostile community attitudes and death, and our results indicated that homicide and violence-related deaths were over three times more likely to occur in high-stigma PSUs than in low-stigma PSUs. Similarly, previous studies have indicated that LGB youth are more likely to attempt suicide in counties with greater anti-gay stigma (Hatzenbuehler, 2011). We extend those findings by showing that sexual minorities in our sample were more likely to die by suicide in high-stigma communities than in low-stigma communities, and that these suicide-related deaths occurred at significantly younger ages (18 years earlier, on average) in high- versus low-stigma communities.

Our results also suggest that psychosocial stress may represent an indirect pathway through which structural stigma contributes to mortality. The experience of discrimination, prejudice, and social marginalization creates several unique demands on stigmatized individuals that are stress-inducing (Clark et al., 1999; Link & Phelan, 2006; Major & O'Brien, 2005; Meyer, 2003a; Pachankis, 2007). In turn, psychosocial stressors are strongly linked to CVD risk (e.g., Miller, Chen, & Parker, 2011; Slopen, Koenen, & Kubzansky, 2012; Taylor, Lehman, Kiefe, & Seeman, 2006), and our results indicated that CVD-related causes of death were elevated in high-stigma communities compared to low-stigma communities. In contrast, rates of cancer were slightly higher in low-stigma compared to high-stigma communities. Given that behavioral risk factors (e.g., diet, smoking, and heavy alcohol consumption) are strongly implicated in cancer etiology, this result suggests that behavioral risk factors are unlikely to explain our results.

Finally, HIV/AIDS-related causes of death could, theoretically, contribute to the premature mortality that we observed among sexual minorities living in high structural stigma communities. Indeed, there is a plausible mechanism linking community-level prejudice to HIV/AIDS infection, via unsafe sexual activity. In support of this hypothesis, many sexual minorities living in areas that stigmatize homosexuality internalize these negative societal messages, a process known as internalized stigma (Meyer, 2003a). Internalized stigma, in turn, has been associated with HIV risk behaviors, including unprotected anal intercourse among sexual minority men (Hatzenbuehler, Nolen-Hoeksema, & Erickson, 2008). Despite the plausibility of this hypothesis, there were very few HIV/AIDS-related causes of death in the GSS/NDI sample, and these causes of death did not differ by structural stigma. Thus, we did not find evidence that HIV/AIDS-related causes of death explain the effect of structural stigma on sexual minority mortality in the current analyses. However, there are two limitations regarding the data on HIV/AIDS-related causes of death that prevent us from definitively ruling out this possibility. In particular, the NDI only identifies the leading cause of death; therefore, we do not know

whether the respondents had HIV/AIDS in the context of other causes of death. In addition, reporting practices for HIV/AIDS-related causes of death have changed over time as the disease became more widely documented and understood, which could produce an under-estimate of the number of HIV/AIDS-related causes of death in our sample, especially in earlier years of the epidemic. However, the first death in our sample occurred in 1989, several years into the AIDS epidemic; moreover, only 16% of the deaths in our sample occurred before or during 1996, when AIDS-related causes of death were at their peak (CDC, 2013), suggesting that different reporting practices are unlikely to bias our results. Nevertheless, in a larger sample or with repeated follow-up it could be possible to more thoroughly test if earlier deaths among sexual minorities in high structural stigma environments are due to HIV/AIDS, an important direction for future inquiry.

Our results are broadly consistent with recent research from other stigmatized groups. In one recent study, researchers used Jim Crow legislation in Southern states as a measure of exposure to institutional racism. Comparing mortality among Whites and Blacks in states with and without Jim Crow legislation in the decade between 1960 and 1970, Krieger (2012) documented that the highest mortality rates occurred in Black populations within Jim Crow states. Taken together, this research highlights the role that structural forms of stigma may play in shaping adverse health outcomes among minority group members. At the same time, although the structural stigma-mortality relationship was significantly stronger for sexual minorities than for heterosexuals, we also found that structural stigma moderately accelerated mortality risk for heterosexuals. This finding is consistent with recent evidence that structural stigma and other forms of social inequality may exert harmful effects not only for minorities, but also for majority group members. For instance, Whites living in communities with greater anti-Black prejudice have elevated rates of mortality compared to Whites living in low-prejudice communities (Lee et al., 2012). Similarly, men have higher mortality rates in countries with greater gender inequality (Stanistreet et al., 2005). However, no study, including our own, has established whether the relationships between structural stigma/social inequality and health among majority group members are causal, or merely the consequence of unmeasured confounding. The identification of mechanisms that can explain why structural stigma and social subordination negatively affect the health of majority group members will help to address this unresolved issue.

This study has several limitations. First, despite the fact that we controlled for various established covariates for mortality at both the individual and community levels, there is nevertheless the possibility of unmeasured confounding. In particular, areas with high levels of prejudice against gays and lesbians may present variations in other contextual risk factors that contribute to poor health and premature mortality. Examples include availability and quality of health care, air quality, crime rates, and the built environment, only some of which will be captured by our measures of PSU education and income. In addition, the GSS does not consistently measure behavioral risk factors for mortality, including diet, smoking, and alcohol consumption. Consequently, we were not able to include these factors as covariates in our statistical models. However, stress contributes to overeating (Adam & Epel, 2007), smoking (Piazza & Le Moal, 1998), and heavy drinking (Keyes, Hatzenbuehler, & Hasin, 2011). Because stress represents a likely mediator of the relationship between structural stigma and mortality (as discussed above), it would have been inappropriate to control for these behavioral factors in our analyses.

Second, sexual orientation is based on self-report data regarding the gender of sexual partners, rather than on self-identification as lesbian, gay, or bisexual. Although measures of sexual behavior and

identification are correlated, these dimensions of sexual orientation define different population subgroups (Sell, Wells, & Wypij, 1995), and health outcomes can differ as a function of which dimension of sexual orientation is measured in the study (e.g., Bostwick, Boyd, Hughes, & McCabe, 2010). Consequently, it is unclear whether our results are generalizable to individuals who self-identify as lesbian, gay, or bisexual. In addition, demographic factors, including gender, age, and race/ethnicity, as well as variance in stigma over time and across geographic areas, affect people's willingness to report same-sex sexual behaviors. However, if high stigma leads to under-reporting of same-sex sexual behaviors, then the results we present here are likely an underestimate of the effect of structural stigma on mortality, given that concealment of sexual orientation is associated with multiple physical health problems, including premature mortality (Cole, Kemeny, Taylor, & Visscher, 1996a; Cole, Kemeny, Taylor, Visscher, & Fahey, 1996b).

Third, the GSS is not a longitudinal panel study; thus, respondents are only interviewed once, and their residence is coded as where they were living at the time of interview. It is possible that the respondents who were initially interviewed while living in a high structural stigma PSU later moved to a low-stigma PSU, perhaps in search of more tolerant environments (Laumann, Ellingson, Mahay, Paik, & Youm, 2004), which would result in misclassification. Moreover, if healthier respondents are more likely to move to low-stigma PSUs, differential selection by health status may, in part, be responsible for these results (i.e., healthy sexual minorities are sorting into low stigma environments, leaving unhealthy sexual minorities to reside in high stigma environments). There is one geographic mobility question in the GSS asking whether the respondent lives in the same city/town/county where they lived when they were 16. The correlation between this item and the low PSU-level stigma variable was small ($r = 0.13$) but statistically significant ($p < 0.001$), indicating that sexual minorities who moved were more likely to migrate to low-stigma PSUs. However, mobility was not associated with better self-rated health ($r = 0.02$, $p = 0.16$); thus, healthier respondents were not more likely to move to low-stigma PSUs, suggesting that differential selection by health status is not a plausible alternative explanation for our results. Moreover, there was no association between geographic mobility and mortality among sexual minorities (HR = 1.17, 95% CI: 0.76, 1.78), demonstrating that our results are robust to selection effects regarding mobility.

In addition to these limitations, there are several strengths of the current study, including the prospective design and population-based sampling scheme. An additional strength of the study was the assessment of individual (versus aggregate) mortality data. Whereas many studies using ecological data suffer from the "ecological fallacy" when they try to extrapolate from aggregated data to individuals (Schwartz, 1994), the current study was able to avoid incorrect inference across levels by linking ecological variables (i.e., structural stigma) to individual-level outcomes (i.e., mortality). A final strength was our measurement of the independent variable (i.e., structural stigma). Importantly, the community-level measure of structural stigma does not rely on sexual minorities' perceptions of how stigmatizing their communities are, but rather was based on the prejudicial attitudes of all GSS respondents living in that community. This approach therefore overcomes many of the limitations of individual-level measures of stigma, which have characterized most stigma and health research to date (Meyer, 2003b). An additional strength of our measure was the ability to document associations between structural stigma and mortality at geographic scales below the state level. Most studies examining the health consequences of structural stigma among sexual minorities have been conducted at the state level (Hatzenbuehler et al., 2009, 2010). Because the areal unit of analysis in the GSS/NDI was the PSU

(composed of MSAs and counties), we were able to utilize measures of ecological environments that are more proximal to sexual minorities than states. Indeed, the PSU is small enough in scale that sexual minorities can plausibly be aware of their community's prejudicial attitudes at this level of analysis. At the same time, community-level attitudes likely vary within MSAs and counties, and this variation may be related to differential mortality risk among sexual minorities across these smaller geographic areas. Future research that operationalizes community-level attitudes towards homosexuality across different spatial scales is therefore warranted.

In sum, our results contribute to a growing body of evidence documenting that structural forms of stigma harm the health of sexual minorities (Hatzenbuehler et al., 2009, 2010; Rostosky, Riggle, Horne, & Miller, 2009). Previous studies have used a data strategy that is similar to the one adopted in the current report, in that measures of structural stigma—specifically policies that differentially target gays and lesbians—were linked to individual mental health outcomes (e.g., Hatzenbuehler et al., 2010). None of these existing studies, however, has focused on physical health outcomes. We show for the first time that structural stigma is associated with all-cause mortality among sexual minority populations, suggesting a broadening of the consequences of structural stigma beyond mental health outcomes to include premature death.

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