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"Measurement and analytic vulnerabilities in the study of structural stigma"

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ABSTRACT

Two general concerns remain following this updated and corrected study of structural stigma's effect on the premature mortality of sexual minorities. First, there seem to be better and worse ways to measure structural stigma. Scholars should be invested in best-possible measures. Second, remaining questions about measures and expectations suggest more attention be paid to an optimal modeling approach to predicting health outcomes among sexual minorities, one that neither underspecifies nor overspecifies models, but aims instead at better understanding stigma processes in population-based samples, not just searching for its effects.

While a reading of the original 2014 study that prompted these new analyses could yield merited disbelief at the magnitude of structural stigma's effect on the premature mortality of sexual minorities, several additional waves of data—together with adjustments in analytic approach— now raise only generalist concerns. Two in particular come to mind. First, there seem to be better and worse ways to measure structural stigma. We should hold out for—and invest available funding resources in—better measures. Second, questions about measures and expectations underlie a search for an optimal modeling approach to predicting health outcomes among sexual minorities. Can scholars do better justice to discerning the process by which stigma may harm, without underspecifying or overspecifying their models? I think so, and I suspect the authors agree.

I am convinced of the reality of structural stigma. And yet the concept appears to be a moving target, quite relative, and sensitive to analytic decisions. One notable new international study created a country-specific measure of "legal and policy discrimination and protections spanning six domains (i.e., unequal age of consent for same-sex sexual activity, asylum provisions for sexual minorities, protections against bias-motivated violence, legal protections against discrimination, same-sex partnership and parenting recognitions, freedom of assembly)," a comprehensive variable that nevertheless suffers since it must by definition ignore variation within countries (Pachankis and Bränström, 2019: e0218084).

The international study suggests the United States exhibits low country-level stigma and only modest concealment of sexual orientation. The times have changed. Support for same-sex marriage now well exceeds 60 percent of the American population (according to Gallup polling). Hence solitary attitudinal measures of structural stigma—including the authors' own previous use of a community's simple lack of enthusiasm for same-sex marriage—seem anemic when compared with either policy-oriented dichotomous variables or multi-faceted ones like that described above (Hatzenbuehler et al., 2017). An opinion about marriage, after all, is directed toward an institutional arrangement rather than antagonism toward a person or persons, as is the case with two of the four GSS items utilized in the present study. On the other hand, deciding exactly how to operationalize a constructed GSS structural stigma index is rather subjective and vulnerable to opportunistic tweaking.

Second, the authors do a fine job of avoiding the analytic approach that orders everything into the model that could have anything to do with the outcome variable. But just as overspecification can pose a problem, so too can omitted variable bias. In particular in this study, the question of the place of self-reported health in predicting the mortality of sexual minorities arose, augmented by the (aggravating) absence of self-reported health from a portion of the GSS respondents due to a split-ballot design. The authors claim elsewhere that structural stigma yields direct effects on "stigma processes" such as concealment or rejection sensitivity, but this is quite distinctive from a direct effect on mortality itself (Hatzenbuehler, 2016: 742). The latter makes less sense, particular if self-reported health and health behaviors are not being simultaneously assessed. In the present study, self-reported health appears to mitigate a direct effect of structural stigma on mortality, meaning the former is a plausible pathway for an indirect effect of the latter. It would seem what the present study is missing, due to data limitations, is the ability to map the pathways of stigma's purported influence (e.g., Reif et al., 2019). Together with the authors of this piece, I want to press other scholars to move from a state of asserting the likely presence of structural stigma to documenting how it does and does not work to shape health and mortality (Reif et al., 2019, 747). Many large or population-based studies of health disparities, unfortunately, never get close to this—in part due to data limitations. In

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not a few such studies, stigma remains the unreflective and/or unmeasured theoretical assumption that links sexual minority self-identities to poorer health outcomes (e.g., Wu et al., 2018). As this field matures, scholars analyzing population-based data can and should do better.

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