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Same-sex and Opposite-sex Marriages as a Proxy Measure of Sexual Orientation and its Association with Psychopathology: A Reply to Zietsch and Sanders

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We thank the authors for their thoughtful and constructive commentaries on our paper (Sanders, 2022; Zietsch, 2022). Our recent study published in *Journal of Sex Research* used a large general population-based Swedish birth cohort to examine the extent to which the association between same-sex marriage and hospital-based diagnoses of depression, substance abuse, and completed or attempted suicide from age 18 years was accounted for by shared genetic and familial environmental influences relevant to both sexual orientation and psychopathology (Xu et al., 2022). Only depression, substance abuse, and completed or attempted suicide identified using the Swedish Patient, the Cause of Death, and the Prescribed Drug registers were considered since they are the commonly studied mental health outcomes that have been consistently found to be associated with sexual orientation in prior literature. We are agnostic ourselves on the theoretical explanations for the elevated risk of mental health problems in non-heterosexual compared with heterosexual population. But we recognize that there is a healthy debate between minority stress accounts (and other social adversity or discrimination-related ones) and common cause explanations (e.g., those involving genetic confounding, or third variable confounding by factors such as personality traits and factors affecting self-reports of discrimination and trauma; Bailey, 2020 cf. Meyer et al., 2021; Peel et al., 2022). Minority stress is certainly not a complete explanation for the elevated risk mentioned. Zietsch (2022) and Sanders (2022) raised some concerns regarding the interpretation of this study. Here we briefly respond to the key points from the two invited commentaries in this reply letter. We agree that marital status as a proxy measure of sexual orientation has limitations and then further discuss whether our results were robust to the resulting selection bias. We recalculate the proportion of the increased risk of mental health diagnoses associated with same-sex marriage compared with opposite-sex

marriage explained by familial confounding using the method suggested by Zietsch (2022). We end with some remarks regarding the meaning and implications of the genetic findings.

We acknowledged that marital status as a proxy measure of sexual orientation has limitations, such as introducing possible selection bias in our study. However, we performed several sensitivity analyses and found that our results appeared robust in the original paper. Whether sexual orientation should be conceptualized as discrete categories or a continuum is still under debate (Savin-Williams, 2016), but there is growing evidence supporting the taxonic (categorical) structure of sexual orientation (Norris et al., 2015). Sexual orientation is also a relatively stable trait and less likely to change from adolescence to young adulthood (Xu et al., 2021). It is difficult to believe that the vast majority of people who enter same-sex marriage are not same-sex attracted to a substantial and consistent degree (that is, mostly or exclusive homosexual over a stable life-course, which of course may be less stable for women; Xu et al., 2021). Thus, we believe that same-sex marriage is a reasonable proxy variable characterized by good positive predictive value for same-sex attraction at least (if not always for lesbian, gay or bisexual [LGB] identity labels or same-sex sexual behavior). Of course, the same does not apply to heterosexual marriage (where some LGB people may enter into opposite-sex marriage for social or cultural pressures as mentioned by the commentaries), and so heterosexual or opposite-sex marriage will have lower negative predictive value for opposite-sex attraction. However, given that the majority of the population would identify themselves as heterosexual (over 90%, depending on the definition of heterosexuality; Bailey et al., 2016), the inclusion of any LGB people in the opposite-sex marriage group may have little impact on our estimates of the risk of mental health diagnoses among individuals in opposite-sex marriages. Naturally, any conclusions are limited to that part of the population which enters into same-sex and opposite-sex marriages,

and so we are not generalizing to all LGB people. We agree with Zietsch (2022) that not only a proportion of people who are LGB are in the category of being same-sex married.

Sanders (2022) raised further concerns regarding the selection bias resulting from using marital status as a proxy measure of sexual orientation. Sanders (2022) suggested that individuals enter marriages may be differ from those not in marriages in personality and social factors (e.g., the tendency to seek mental health care), which may lead to different estimates of sexual orientation disparities in psychopathology between analyses using sexual attraction or identity or behavior and analyses using marital status to measure sexual orientation. In our study, individuals in same-sex marriages were compared with those in opposite-sex marriages. Accordingly, those personality and social factors should affect both individuals in same-sex marriages and opposite-sex marriages similarly (e.g., both would be more likely to seek mental health care than their counterparts who were not in marriages), which would reduce such bias. Our sensitivity analysis also found that sexuality disparities in the risks of mental health diagnoses after marriage did not differ statistically significantly from that before marriage, which further alleviates concern that marriage itself is differentially associated with risk.

Sanders (2022) further suggested that gay/lesbian couples may differ from heterosexual couples in the motives to enter into legal unions (Aldén et al., 2015), which would be a confounder for the association between marital status (same-sex or opposite-sex marriages) and mental health diagnoses. Sexual orientation differences in the motives to enter into legal unions may be confounded by cohort and period effects that were not controlled for in Aldén et al (2015) cited in Sanders (2022). The extension of some legal rights (e.g., joint legal parenthood and residence communal property) from heterosexual marriages to legally recognized same-sex partnerships (registered partnership) and marriages from 1995 to 2009 in Sweden may have a

bigger impact on individuals in registered partnerships or same-sex marriages than those in opposite-sex marriages (period effects). Individuals in registered partnerships were also older than individuals in opposite-sex marriages in Aldén et al (2015). Thus, individuals in registered partnerships may be from an older generation and hold more traditional family values than individuals in opposite-sex marriages (e.g., fertility rate declined for the 1970 cohort than that for the 1980 cohort, and the mean age at first birth increased from 1990 to 2018 in Sweden, which were possibly due to the weakening of traditional family value and rising in individual autonomy and self-actualization; Lesthaeghe, 2014; Hellstrand et al., 2021), which may explain the sexual orientation differences in the motives to enter into legal unions found in Aldén et al (2015). Thus, sexual orientation differences in the motives to enter into legal unions found in Aldén et al (2015) may become statistically non-significant when cohort and period effects were controlled for. Nonetheless, if the association between marital status (same-sex or opposite-sex marriages) and mental health diagnoses reported in our study were confounded by sexual orientation differences in the motives for entering into legal unions, the increased risk of mental health diagnoses associated with same-sex marriage reported in our study would be significantly greater than those in studies that measured sexual orientation directly. However, hazard ratios for mental health diagnoses associated with same-sex marriage reported in our study were comparable to those in studies that measured sexual orientation directly (e.g., Innes et al., 2015), which further reduces such concerns.

Finally, the proportion of the increased risk of mental health diagnoses associated with same-sex marriage compared with opposite-sex marriage explained by shared familial confounding derived from the sibling comparison analysis in our study was comparable to that derived from analyses of adoptees (the phenotypic correlation between same-sex marriage and

each mental health diagnosis was small in magnitude, around .1, which indicates that the magnitude of shared familial confounding contributions should be small). We agree with Zietsch (2022) that this method is mostly applicable for linear regression. Nonetheless, this method helps the interpretations across the two designs we used. We have now also calculated the proportion of the increased risk of mental health diagnoses using the method suggested by Zietsch (2022), which was $(\text{Model 2 Hazard ratio} - \text{Model 4 Hazard ratio}) / (\text{Model 2 Hazard ratio} - 1)$. For men, measured and unmeasured shared familial factors explained 23.08%, 36.67%, and 11.49% of the increased risk of being diagnosed with depression, substance abuse, and suicide, respectively. For women, the figures were 39.44%, 3.65%, and 26.36%, respectively. These estimates may be less comparable to the ones derived from analyses of adoptees (the phenotypic correlation between same-sex marriage and each mental health diagnosis as a measure of the amount of shared familial confounding contributions relevant to both traits). However, no matter what method to use, the proportion of this increased risk explained by shared familial confounding that make siblings similar was small or medium in magnitude and depended on type of mental health diagnosis. We agree with the point about our use of shared familial influences, as mentioned in our Discussion (sibling comparisons do not control for all shared familial influences since full siblings only share on average 50% of their segregating genes). The estimates from the adoption analysis do suggest greater overlapping genetic influences but these estimates have substantially low precision because of the small sample of adopted siblings. We agree with Zietsch that our estimates of "common causes" are best understood as lower bounds".

Finally, regarding the meaning and implications of finding genetic (or other) correlations for human behavior, including sexual orientation, we take the standard philosophical viewpoint that the causes of a trait are not logically related to the moral, social or ethical implications

regarding the trait. There is no scientific or formal philosophical reason, that we can see, as to why finding genetic factors are associated with a particular trait would have the potential to re-pathologize a formerly stigmatized trait any more than there would be if specific non-genetic factors were found to be associated with said trait. Even in health terms, the implications of genetic findings are not clear. For example, finding genetic associations would not mean that health inequalities are unmodifiable or that public health interventions to reduce these inequalities would not have an impact. These philosophical issues (essentially, variations on the notion of the naturalistic fallacy) are complex and there may be instances in which facts about causes are related to facts about morals, socio-politics or ethics. But, by and large, empirical facts are not the same as moral, socio-political, or ethical facts.

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