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COMMENTARY

Reasons for Caution About the Fraternal Birth Order Effect

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The etiology of sexual orientation is an intriguing and poorly understood topic. Genes appear to play an important role, which raises the Darwinian paradox as to why genes for homosexuality would be maintained in the population (Zietsch et al., 2008). In terms of non-genetic influences, harsh childhood environments and childhood sexual abuse are associated with adult homosexuality, though the causality needs clarification and the mechanisms are not known (Zietsch et al., 2012). Also in this mix is the fraternal birth order effect (FBOE)—the focus of the target article (Blanchard, 2017). Numerous studies have found that homosexual men have a disproportionate number of older brothers relative to heterosexual men, though, again, the mechanisms by which this variable might relate to development of sexual orientation are unknown (notwithstanding the "maternal immune hypothesis" [Blanchard & Bogaert, 1996], for which I know of no direct evidence).

The target article concludes, based on a meta-analysis of published FBOE findings, that the effect's existence is "almost beyond doubt at this point." In my view, this overstates the case. Below, I discuss sampling and analytic issues with the meta-analysis and the studies it draws on, which call into question the strength of evidence for the FBOE.

First, the meta-analysis only included published findings. Many meta-analyses include unpublished results sourced through mailing lists, dissertation databases, conference abstracts, and so on. Given widespread bias toward publishing positive results, including only published findings raises the possibility of a meta-analytic result biased in the direction of the hypothesized effect. There are methods for testing and accounting for publication bias (e.g., Simonsohn, Nelson, & Simmons, 2014; Thornton & Lee, 2000), but Blanchard does not mention the phenomenon.

Second, among published studies, Blanchard's five criteria for inclusion—none of which were explicitly justified—led to exclusion of some of the largest informative studies testing the FBOE. This is especially problematic given that the bulk of these studies report nonsignificant findings for the FBOE (e.g., Bearman & Brückner, 2002; Bogaert, 2010; Frisch & Hviid, 2006).

Third, Blanchard's five inclusion criteria also led to exclusion of all available probability samples (i.e., samples selected randomly with respect to the independent variable—sexual orientation in this case). This means the meta-analysis only includes samples in which the proportion of homosexuals bears no resemblance to the proportion of homosexuals in the general population (on average, the samples contain 51% homosexuals, compared to around 2–3% in the general population). Such nonrandom sampling risks selection of homosexual and heterosexual groups that differ in ways other than focal variables. This issue is especially salient given that six of the seven probability samples I am aware of report null effects for the FBOE (Bearman & Brückner, 2002; Bogaert, 2005, 2010; Francis, 2008; Frisch & Hviid, 2006; Zietsch et al., 2012).

Further, it can be seen in Table 1 of the target article that most of the samples included in the meta-analysis are drawn from highly non-representative populations, including sex offenders, transsexuals, and patient samples (pedophilia, gender dysphoria, sexually transmitted diseases, psychoanalysis). It is notable that none of these sampling issues precluded inclusion in the meta-analysis, nor were they discussed. In contrast, the inclusion criteria were stringent in excluding large probability studies (which found null effects for the FBOE) because of more minor problems such

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as use of proxy indicators of sexual orientation (e.g., same-sex marriage).

The large and significant heterogeneity in the effect sizes between studies (75% of which is real between-sample variance, as opposed to statistical noise) suggests that how the samples are collected has a large influence on the older brother effect. This raises the possibility that sampling peculiarities are *creating* the fraternal birth order effect, especially given that large probability samples generally do not show a significant effect.

Moving on from sampling issues, another potential problem is that analytic decisions seem to be biased toward finding the desired effect. For example, several researchers have used one-tailed tests of the effect (e.g., Bogaert, 2005; Poasa, Blanchard, & Zucker, 2004; Purcell, Blanchard, & Zucker, 2000) or have reinterpreted other researchers' null effects as significant by arguing that a one-tailed test should have been used (Blanchard & VanderLaan, 2015)—even though one-tailed tests are only appropriate in very rare circumstances that do not apply in this case (Lombardi & Hurlbert, 2009).

In a similar vein, the target article argues that Frisch and Hviid's (2006) study of a probability sample of two million Danes, in which the authors reported a null effect for the FBOE, actually reveals a significant effect "when family size is taken into account." Note that Blanchard offers no theoretical reason why controlling for family size should *create* a significant FBOE where there was otherwise not one. In any case, Frisch and Hviid's analysis already controlled for family size (via number of younger siblings), as well as other potential confounders including age, calendar period, birth place, mother's age, father's age, multiple birth status, and duration of parental marriage. Blanchard presumably did not control for these other variables, which might explain the discrepancy in results. It should also be noted that while they found no significant FBOE for individuals in homosexual marriages, Frisch and Hviid (2006) found that individuals with more older siblings were more likely to be *heterosexually* married, the effect being similar for older brothers and older sisters (p-values $< .001$). This overall pattern of results is not consistent with the FBOE; the fact that Blanchard reinterprets the data as supporting the FBOE raises the question of whether bias in analytical choices might have affected the meta-analysis, given that flexibility in such choices can inflate effect sizes and test statistics (Simmons, Nelson, & Simonsohn, 2011).

In sum, I propose that while there is considerable evidence for the FBOE, there are also enough problems with the literature as a whole that we should not discard all doubt about the robustness, generalizability, or even existence of the effect. In particular, without clear evidence for the FBOE in large probability samples, this fascinating phenomenon remains uncertain.

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