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Science and Public Health as a Tool for Social Justice Requires Methodological Rigor: A Response to Turban et al. "Sex Assigned at Birth Ratio Among Transgender and Gender Diverse Adolescents in the United States"

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We write, as scientists, methodologists, and clinicians working in gender-affirming care, our views to the recent article published in *Pediatrics* by Turban and colleagues, who also argue for better provision of healthcare for transgender and nonbinary (trans) youths.[1] The authors utilized the Youth Risk Behavior Surveillance System (YRBSS) to examine the social contagion hypothesis of the unsubstantiated claim of rapid-onset gender dysphoria. While we agree with the authors' conclusion that social contagion rhetoric should not be used to politically and medically argue against the provision of care for trans adolescents – as currently reflected and weaponized with several anti-trans policies in the US banning gender-affirming medical care[2,3] – we examined cautiously the study's premise and analytical design, and identified critical theoretical and methodological concerns specific to its conceptualization of social contagion and its data analysis.

Foremost, in alignment with several bodies of major medical organizations, we assert that transphobia is a social contagion, and being transgender is not. Turban et al.' sex-assigned ratio analysis draws in on Littman's misconceptualized social contagion hypothesis, which was operationalized as parents' observation of their trans kid having at least one trans friend in their peer group, and occured more among parents with trans kids assigned female at birth (AFAB) than male assigned at birth (AMAB).[4] The premise of social contagion hypothesis is a gross misinterpretation of trans communities' social support, cohesiveness, and connectedness. Indeed, it is well-documented that trans youths, when asked directly, seek out support and knowledge from peers about gender and health as part of their developmental milestones and resiliency[5]. In addition, conducting analysis based on sex-assigned AMAB:AFAB-ratio is not conceptually a direct disconfirmation of the premise in the study by Littman given the significant recruitment bias that yielded a skewed study sample of mostly parents of AFAB trans kids. As such, the very phenomenon that is being interrogated by Turban et al.; the presence of a biased AMAB:AFAB-ratio, is an artifact of a flawed study design, rather than a true, verifiable component of the supposed theory.

Therefore, researchers and policymakers' continued use of social contagion as a lens to account for trans youths' connectedness with each other rests on misguided practice of theorizing, hypothesis testing, and legislating.

To assess the validity of the ROGD hypothesis Turban et al. perform a trend analysis to provide point estimates of AFAB and AMAB trans youth nationally based on a set of samples from a subset of states. Unfortunately, there are serious threats to validity of both the trend analysis and the estimates of AFAB and AMAB youth. For the former, the trend analysis is based only on two time points, which is insufficient to provide robust interpretation, let alone a trend. Also unfortunate, is the study's data reporting error that severely misrepresents the robustness of the sample. Turban et al. state that their estimates of the AMAB:AFAB-ratio are based on 16 states in the abstract, 15 for 2017 and 15 for 2019 (Delaware in 2017 only and New Jersey in 2019 only) as shown in the caption for Table 1. However, only 10 states fielded the SOGI module in 2017,[6] and of them only 9 had publicly available data (Massachusetts does not provide permission for the CDC to share their data).[7] Similarly, only 14 states with publicly available data fielded the SOGI module in 2019. Under these circumstances, the trend analysis is comparing subsets of trans youth from different states and any differences are likely due to sampling bias making the trend analysis invalid.

Beyond issues with the trend analysis are issues with the individual point estimates for the AMAB:AFAB-ratio. For 2017 and 2019 less than one-fifth and one-fourth, respectively, of the 50 states and five territories in the United States are included in the analysis. Table 1 shows the high variability of the AMAB:AFAB-ratio between states and within states across time points. Much of this variability is driven by the sample size; for instance, in the Rhode Island sample between 2017 and 2019 the AMAB:AFAB-ratio inverted from 0.8 to 3.0, based on TGD youth samples of only 39 and 16 persons, respectively. In comparison to Rhode Island, Maryland dominates the sample for both years. Specifically, Maryland comprises 67% of the sample (1547/2302 persons) in 2017, and 40% (711/1790 person) in 2019. To show the sensitivity to state inclusion, we recalculated the AMAB:AFAB-ratio without that state for 2017 and 2019, and found 1.2 and 1.1, respectively compared to the 1.5 and 1.2 in the authors original analysis. Because the authors methodology did not account for oversampling, their analysis provided biased results and shifted the "national" estimate that is driven by a single state. Additionally, the analysis neglects the survey sampling design. YRBSS state surveys are two-stage cluster samples and the data include weights that allow for state-level representative analyses. We have written elsewhere that such analyses are suboptimal for estimating transgender populations when one-step gender identity measurements are used,[8] however; to overlook the sampling schema precludes the possibility of state-level representative estimates, making further extrapolation to the entire US dubious. This critique also applies to the pairwise comparative analyses in Tables 2 and 3 between trans, cisgender sexual minority, and cisgender heterosexual youth which similarly fail to account for survey sampling design and clustering by state. Providing details of any approach regarding accounting of sampling schema, if any were indeed utilized, would have benefitted the analysis. Lastly, the analysis becomes more problematic when one notes that the few states included are not a random sample of the US states and territories, but instead show a concentration of states in the Northeast, and a smaller group in the Midwest with remaining states having no neighbors that also collected data on TGD youth (Figure 1). There are

state-level variations in policy climates for transgender youth in school[9–11] that would facilitate or prohibit their inclusion in the sampling frame for a given state. With the overrepresentation of Northeastern states, and the exclusion of most Southern and Midwestern states, the approach used here cannot reliably provide national estimates that incorporate regional variation in policy climes.

We applaud the authors for their approach to address the deleterious and unsubstantiated rhetoric of the anti-transgender movement. Many of the challenges that we identified are not specific to the authors but represent challenges facing all researchers, trans and cis, working in transgender health who are forced to work with suboptimal data sources that lack inclusion of transgender persons or fail to use best practices for gender identity ascertainment. [12] However, science and public health as a tool of social justice requires methodological rigor in addition to conviction and intent. While this study was admirable, we find that the results were overinterpreted and that the theoretical and methodological shortcomings of the article run the risk of being more harmful than supportive. A more productive pursuit is the continued advocacy of methods development to ascertain gender identity, comprehensive inclusion of transgender individuals in survey design and sampling strategies, and increased funding for prospective and truly representative datasets to answer these questions with high-quality and methodologically sound research studies. In the absence of more and better data, studies like this one create methodological problems for future scientists to correct rather than allowing the science to scaffold toward a more just and equitable future for trans youth, and indeed trana communities generally.

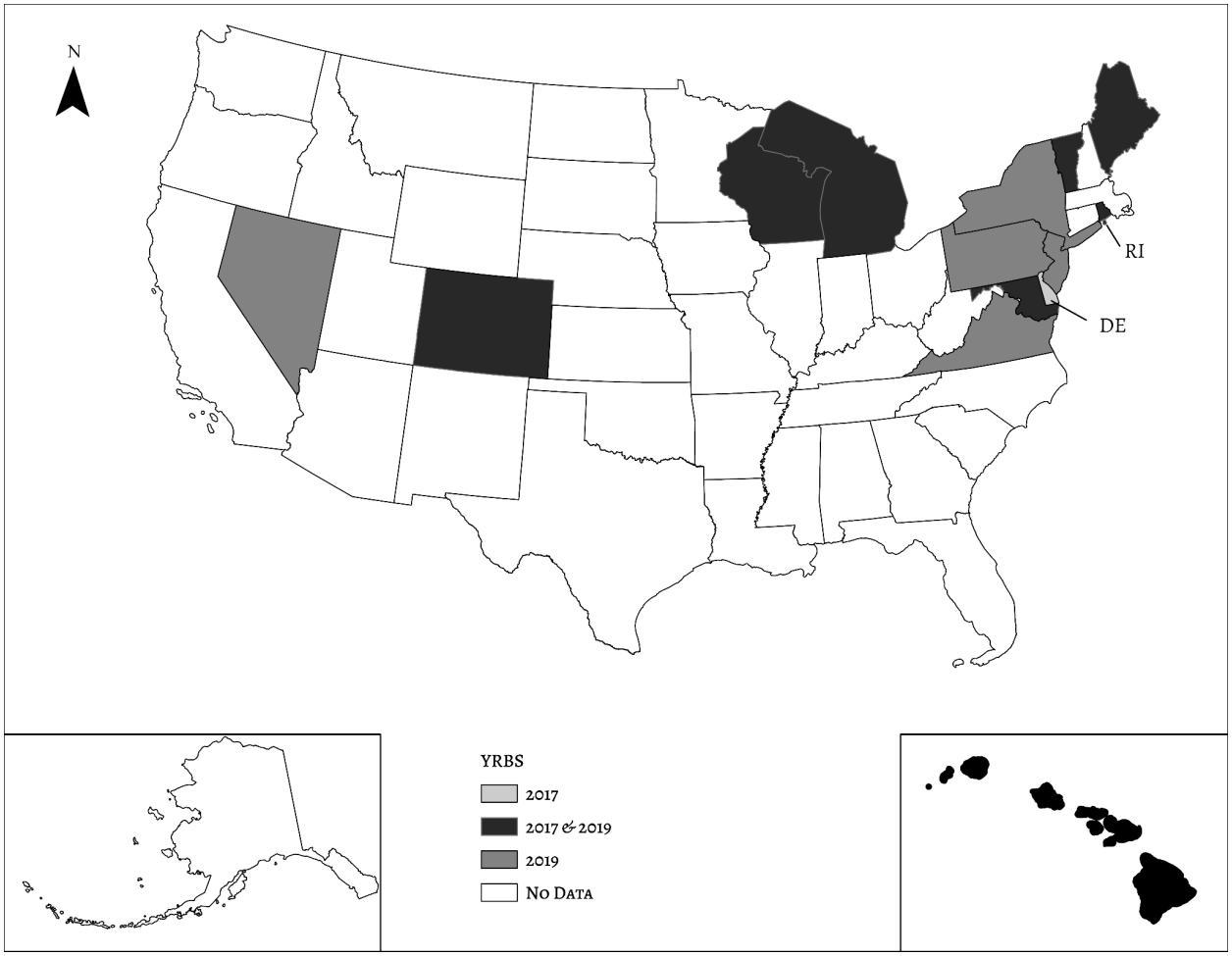
Table 1: State Breakdown of Data Availability and AMAB:AFAB-Ratio among states with publicly available YRBSS Data

State	2017 AMAB:AFAB (ratio) N=2302*	2019 AMAB: AFAB (ratio) N=1790
Colorado (CO)	6:9 (0.7)	10:7 (1.4)
Delaware (DE)	18:12 (1.5)	--
Florida (FL)	--	33:41 (0.8)
Hawaii (HI)	97:47 (2.1)	33:37 (0.9)
Maine (ME)	78:79 (1)	65:64 (1)
Maryland (MD)	881:542 (1.6)	342:278 (1.2)
Michigan (MI)	8:14 (0.6)	30:36 (0.8)
Nevada (NV)	--	9:13 (0.7)
New Jersey (NJ)	--	4:5 (0.8)
New York (NY)	--	113:77 (1.5)
Pennsylvania (PA)	--	8:14 (0.6)
Rhode Island (RI)	17:22 (0.8)	12:4 (3)
Vermont (VT)	124:115 (1.1)	143:138 (1)
Virginia (VA)	--	36:30 (1.2)
Wisconsin (WI)	22:15 (1.5)	7:12 (0.6)
Overall	1251:855 (1.46)	845:756 (1.12)
Excluding Maryland	370:313 (1.18)	503:478 (1.05)

"--" indicates that state did not field the SOGI module during that year

*It should be noted that the sample size of our analysis is slightly larger than Turban et al (despite their claim of including more states). Our results have been verified with two copies of the YRBSS state-level data from different research teams and so it is possible that Turban et al. used unknown inclusion criteria that slightly shrunk their data set. However, the issues of misrepresenting the state data availability and dependence on Maryland remain.

Figure 1: States with Publicly Available Data Fielding SOGI Module in YRBSS by Year



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