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From: **Adam Keiper** <akeiper@thenewatlantis.com>

Date: Fri, Sep 2, 2016 at 3:45 PM

Subject: your Advocate.com article on "Sexuality and Gender" (the 'New Atlantis' report)

To: deanhamer@aol.com

Dear Dr. Hamer –

I am writing to reply to your recent article on the website of *The Advocate* responding to the report "[Sexuality and Gender: Findings from the Biological, Psychological, and Social Sciences](#)" published in *The New Atlantis*, which I edit. I would like to offer a few remarks on some of the points you raise. (You are welcome to publish this e-mail, if you wish.) Let me note up front that I am not here writing on behalf of the authors of the report, nor as a scientist or physician. (I am neither.) Instead, I write as an interested reader of your article and as the editor of *The New Atlantis* who worked closely with the authors of the report over the course of several months. I am grateful to you for taking the time to review the report and write about it, even if in disagreement.

To dive right into the substantive criticisms you make: you write that the report "claims to show sexual orientation is chosen and not fixed, and that gay people are not 'born gay.'" The report written by Drs. Mayer and McHugh does *not* claim to show that sexual orientation is chosen, and in fact explicitly states that sexual orientation is *not* chosen in the very first paragraph of the section on sexual orientation: "We hope to show here that, though sexual orientation is not a choice, neither is there scientific evidence for the view that sexual orientation is a fixed and innate biological property."

You criticize the report for "baldly stating that sexual orientation is an 'ambiguous' concept." The report does not make a bald assertion; to the contrary, it argues that sexual orientation has various facets (e.g., attractions, behaviors, and identity), each of which is enormously complicated, and that it can be operationalized in studies in a variety of ways. I would note that the definition you quote from, taken from an [American Psychological Association online brochure](#) (and quoted in our *New Atlantis* report) goes on, after the part you quoted, to discuss how the concept is more complicated than just an "enduring pattern of ... attractions": the APA also discusses identity and behavior, and complicated social aspects of sexuality. This speaks to the point you make eloquently: that sexual orientation "may be complex — every human characteristic is."

But is it true, as you then claim, that sexual orientation is "far less complicated and ambiguous than many of the facets of personality that psychologists spend their time attempting to measure and study; e.g., 'warmth,' 'self-esteem,' and 'imagination'"? Certainly sexual orientation can be defined and operationalized narrowly, but it can be, and often is, discussed in much broader, more ambiguous, terms. Understanding these complications, ambiguities, and internal tensions is an aim of the first part of the report.

You then criticize the report's selection of studies on the genetics of homosexuality, writing that

Of the six studies using proper probability sampling methods that have been published in the peer-reviewed literature in the past 16 years, [Mayer and McHugh] include only one — and it just so happens to be the one with the lowest estimate of genetic influence of the entire set.

You [linked to a table](#) that lists six twin-registry studies and one probability-sampling study. Drs. Mayer and McHugh discuss two of those seven studies: the [Langstrom et al., 2010](#) twin study from Sweden, and the [Kendler et al., 2000](#) study from the United States. You seem to be implying that Drs. Mayer and McHugh engaged in cherry-picking, listing only a single study that "happens to be the one with the lowest estimate

of genetic influence,” but the Kendler study — their discussion of which you may not have noticed — gives a relatively *high* estimate of the heritability of sexual orientation compared to the other studies listed in the table.

As for the Langstrom *et al.* study, the report highlighted it as an example of particularly good twin research on sexual orientation since it not only used proper probability sampling but also employed one of the more valid indicators of sexual orientation — namely, asking individuals whether they have ever had sexual relations with an individual of the same sex. This was taken by Drs. Mayer and McHugh to be a more objective way to operationalize sexual orientation than the more subjective definitions used in some of the other studies in that table (which included self-reported attractions and a multifaceted self-assessment scale, and in other cases were simply unclear).

You note that a recent review by [J. Michael Bailey \*et al.\* \(2016\)](#) was not discussed in the *New Atlantis* report. Professor Bailey’s paper is very recent, and so our authors were unable to include a discussion of it before our work on “Sexuality and Gender” was completed.

You also assert that Drs. Mayer and McHugh “claim that nobody is ‘born gay.’” While Drs. Mayer and McHugh dispute the notion that people are simply “born gay,” they do not argue that there is no genetic contribution to sexual behaviors, attractions, and identity, writing that “certain genetic profiles probably increase the likelihood the person later identifies as gay or engages in same-sex sexual behavior.”

Turning to your criticism of the report’s discussion of gender identity, you write that Drs. Mayer and McHugh argue that the “‘dysphoria,’ [of transgender children] as [Mayer and McHugh] insist on pathologizing gender fluidity, might be transient.” This is a distortion of what Drs. Mayer and McHugh have written. First, they do not “pathologiz[e] gender fluidity”; indeed, they explicitly draw a distinction between gender dysphoria (as defined in the latest edition of the *DSM* to include clinically significant distress) and cross-gender identification: “[There is no scientific evidence that all transgender people have gender dysphoria, or that they are all struggling with their gender identities.](#)” As to your remark about transience, it is true that the report discusses the low rates of persistence for gender dysphoria, drawing on statistics reported in the most recent edition of the *DSM*’s discussion of gender dysphoria.

You next criticize the report for neglecting “two very important recent studies showing that trans children who are affirmed by their parents are as happy and healthy as their peers, and that allowing them to express their true gender decreases depression and anxiety.” In fact, the report does acknowledge that gender-affirmative approaches may be effective for helping some children, noting that “[some children may have improved psychological well-being if they are encouraged and supported in their cross-gender identification.](#)” As for the two studies you mention: one ([Olson \*et al.\*, 2016](#)) is very recent and so did not make it into the report, but from a quick review, I would note that its findings seem less conclusive than you suggest: it does not find that transgender children affirmed by their parents are “as happy...as their peers,” as you say; rather, it finds that on measures of anxiety and depression, transgender children (ages 3 to 12) affirmed in their identity by their parents have similar levels of depression but elevated levels of anxiety compared to their peers. The other study you allude to was apparently a conference presentation ([Marinkovic and Newfield, 2015](#)), which did not provide much specific information on either its methods or results, and so would not have been suitable for inclusion in the *New Atlantis* report.

Regarding the report’s section on mental health outcomes for LGBT populations, you criticize Drs. Mayer and McHugh for discussing the social stress model but failing to focus “on how such social stressors might be reduced.” To discuss how social stressors might be reduced would be to discuss policy, but the report focuses on science. Drs. Mayer and McHugh felt that this report was not the place to offer, analyze, or endorse policy proposals.

You then write that “the authors jump to the conclusion, with no supporting evidence or calculations whatsoever, that these [social stress] factors are insufficient to fully explain the observed mental health discrepancies.” To say that Drs. Mayer and McHugh jumped to this conclusion is to get things rather backwards. It is for the researchers working on the social stress model to provide evidence that the social

stress model sufficiently accounts for the elevated rates of mental health problems in LGBT populations. The report does not deny that social stress plays a role in the mental health problems experienced by LGBT populations, stating that the social stress model “[probably accounts for some of the poor mental health outcomes experienced by sexual minorities](#).” Rather, Drs. Mayer and McHugh argue that, given the state of the available evidence, it cannot be concluded that social stress entirely explains the elevated rate of mental health problems for LGBT populations.

I will avoid commenting on the non-substantive portions of your article, but would like to thank you, again, for what you wrote on the substance.

Yours,  
Adam Keiper

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