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The myth of persistence: Response to “A critical commentary on follow-up studies and ‘desistance’ theories about transgender and gender non-conforming children” by Temple Newhook et al. (2018)

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ABSTRACT

Temple Newhook et al. (2018) provide a critique of recent follow-up studies of children referred to specialized gender identity clinics, organized around rates of persistence and desistance. The critical gaze of Temple Newhook et al. examined three primary issues: (1) the terms persistence and desistance in their own right; (2) methodology of the follow-up studies and interpretation of the data; and (3) ethical matters. In this response, I interrogate the critique of Temple Newhook et al. (2018).

KEYWORDS

Desistance; developmental psychiatry; DSM-5; gender dysphoria; gender identity disorder; persistence; transgender

Prolegomenon

As Temple Newhook et al. (2018) noted in the Introduction to their critical essay, they focused on “...the four most recent follow-up studies...which are most often cited as evidence for desistance theories” with regard to children variously labeled as “transgender,” “gender non-conforming,” etc.¹

Four thoughts: (1) From an historical perspective, are Temple Newhook et al., in fact, “systematically [not] engaging scholarly literature” by eliding a critical discussion of the earlier studies, particularly the follow-up by Green (1987) (for references to the other “early” studies, see Ristori and Steensma [2016, Table 1])? In my view, this is a form of empirical and intellectual erasure, which should be problematized. (2) With regard to the analysis of persistence vs. desistance rates, I would not have included the study by Steensma, Biemond, de Boer, and Cohen-Kettenis (2011), since it was an intentionally selective sample and not representative of the potential pool of clients seen at the Amsterdam clinic between the years 2000 and 2007. Perhaps even more importantly, some of the participants in Steensma et al. (2011) were part of the earlier study by Wallien and Cohen-Kettenis (2008) (T. D. Steensma, personal communication, March 29, 2018); hence, Temple Newhook et al. in their

Table 1 are doing a partial “duplicate” counting of the follow-up data from the Dutch clinic. (3) I think that Temple Newhook et al. should have included the follow-up study by Singh (2012), since it contains the largest number of children ($n = 139$ birth-assigned males²) among the recent follow-up studies (and, for that matter, any follow-up study). Temple Newhook et al. appear to implicitly devalue the Singh study (which they do not even reference) because it has not yet been published in a peer-reviewed journal.³ However, a doctoral dissertation approved by a decent university with three Ph.D. committee members and an external Ph.D. examiner should not be ignored. (4) Temple Newhook et al. used the phrase “desistance theories,” which is a bit odd. The first step is to summarize the data on persistence and desistance. The second step would be to theorize or hypothesize not only why desistance occurs when it does, but also to theorize or hypothesize why persistence occurs when it does. In my view, this is no trivial matter because, as I will discuss below, there might be very good reasons to predict that, in at least some subgroups of contemporary transgender children (or as Meadow [in press] calls them, “trans kids”), the rate of persistence is going to be much higher than reported in the follow-up studies to date.

On the terms persistence and desistance

Temple Newhook et al. (2018) offer up a brief discussion of the etymology of the word desistance, which I appreciated. Here, I will offer up how I think the terms persistence and desistance became part of the linguistic landscape with regard to children with a diagnosis of gender identity disorder/gender dysphoria.⁴ At the 2003 meeting of the Harry Benjamin International Gender Dysphoria Association (now the World Professional Association for Transgender Health) in Gent, Belgium, I was a Discussant in a symposium and the title of my talk was “Persistence and desistance of gender identity disorder in children” (Zucker, 2003). As far as I can remember, I stumbled across the terms persistence and desistance after reading a paper by August, Realmuto, Joyce, and Hektner (1999), who reported on the rates of persistence and desistance of oppositional defiant disorder in a community sample of children with a diagnosis of attention-deficit hyperactivity disorder. At the time, the terms sounded pretty cool to me and they have been used for a long time now in clinical developmental child and adolescent psychology/psychiatry research (e.g., Farrington, 1991; Simonoff et al., 2013; Verhulst & Althaus, 1988).

For scholars writing about gender dysphoria, the terms caught on. Of course, there is no law that says these terms should be privileged. One could use alternative terms like continuation vs. discontinuation (persistence and desistance have fewer syllables), continuation vs. “in remission,” etc. “In remission” is a bit too medical for my taste, but it is not necessarily an incorrect term. One could even add the term “recurrence” for those children and adolescents where gender dysphoria desists but then recurs. In any case, the “real” objection to the term “desistance” is that some clinicians, some researchers, and some activists simply don’t like the empirical fact that there are some children who received the DSM-5 diagnosis of gender dysphoria (or its predecessor DSM-III/DSM-IV labels gender identity disorder of childhood or gender identity disorder) who do not continue to have “it” when they are older. That is really the crux of the discourse implicit in Temple Newhook et al.’s attempt to “reconstruct” the extant empirical literature, which the most ardent critics like to call “junk science” (e.g., Ford, 2017; Tannehill, 2016).

The data

Temple Newhook et al. (2018) use an 80% desistance rate “from a prior transgender identity” as the launching pad for their critique of the literature. I have two problems with this: one that is perhaps semantics; the other is empirical.

The term “transgender identity” is hardly an objective label for a child’s gendered subjectivity. It is a label imposed by Temple Newhook et al. Of course, there may have been some children in the reviewed follow-up studies who self-identified as “trans” or “transgender” (in childhood) but I suspect that they constituted a (small) minority. And, of course, there are, no doubt, many children nowadays who would meet the DSM-5 diagnostic criteria for gender dysphoria and who self-identify as “trans” or whose parents label them as “trans” or whose clinician labels them as “trans.” But a transgender identity is not isomorphic with a mental health diagnosis of gender dysphoria or even the alternative label of gender incongruence proposed for the forthcoming ICD-11 (Drescher, Cohen-Kettenis, & Reed, 2016). One could use alternative wording to summarize the data, at least as how Temple Newhook et al. see them: Of X children referred to specialized gender identity clinics at one of two academic health science centers, the majority of whom met DSM criteria for gender identity disorder of childhood or gender identity disorder, “[a]n oft-accepted interpretation of [the] findings is that approximately 80%” did not appear to have, at the time of follow-up, the developmentally equivalent adolescence or adulthood diagnosis.

Temple Newhook et al. could have done a bit of a better job in summarizing the Wallien and Cohen-Kettenis (2008) and Steensma, McGuire, Kreukels, Beekman, and Cohen-Kettenis (2013a) data by providing separate percentages by birth-assigned sex in Row 13 (“Reported desistance rate”) of Table 1. In Wallien and Cohen-Kettenis, the persistence rate for birth-assigned males was 20.3% and for birth-assigned females was 50.0%. In Steensma et al., the corresponding percentages were 29.1% and 50.0%, respectively. Singh (2012), who used a methodology very similar to Drummond, Bradley, Peterson-Badali, and Zucker (2008), found a persistence rate of 12.2%. Green’s (1987) study, which is arguably the most important of the “earlier” follow-up studies, found a persistence rate of 2.2% (see, for example, Zucker and Bradley [1995, pp. 283–287]). In the two Dutch studies, the

persistence rate for birth-assigned females was 2.46 and 1.71 times more likely than it was for birth-assigned males. In contrast, in the two Toronto studies, the persistence rate was similar for birth-assigned females and males. These variations deserve scrutiny and thought.

These follow-up studies were the ones used by the Gender Identity Disorders Subworkgroup to summarize the extant follow-up data in the DSM-5, where it was asserted that the “Rates of persistence of gender dysphoria from childhood into adolescence or adulthood vary. In natal males, persistence has ranged from 2.2% to 30.0%. In natal females, persistence has ranged from 12% to 50%” (American Psychiatric Association, 2013, p. 455). I would have preferred that Temple Newhook et al. relied on this statement rather than make vague reference to “...the media...[the] lay public, and in [unnamed] medical and scientific journals” or they could have referenced the summary article by Ristori and Steensma (2016).

I must confess having some trouble in understanding Row 7 (T1-desistant/N) in Temple Newhook et al.’s Table 1. For example, in Row 7 for Drummond et al. (2008), they give a figure of 59%. This appears to have been derived by counting the 22 participants seen for follow-up who were classified as desisters and dividing by 37 (the total number of eligible participants, of whom 12 were not part of the follow-up study because they could not be traced, their parents did not want them to take part in the study, etc.: $22/37 = 59\%$ instead of $22/25 = 88\%$ (as reported in Row 13).⁵ The 59% figure could be interpreted as implying that as many as 41% of the potential participants could have been persisters, which is an absurd inference with no empirical basis.

Methodological and interpretive issues

Sampling

Temple Newhook et al. (2018) expressed concern about the “broad inclusion criteria” in the follow-up studies and that they were not “a representative group of transgender children.” That is true. Until there is a formal epidemiological, population-based study of children who meet diagnostic criteria for gender dysphoria, it would be prudent to limit any claims about generalizability to clinic-referred samples seen during

the same period of time during which the extant follow-up studies were conducted (cf. Steensma, van der Ende, Verhulst, & Cohen-Kettenis, 2013b). One could say the very same thing about contemporary samples of trans children recruited via support groups, conferences, word of mouth, etc., as in the recent studies by Olson’s research group (e.g., Durwood, McLaughlin, & Olson, 2017; Olson, Durwood, DeMeules, & McLaughlin, 2016; see also Kovalanka, Weiner, Munroe, Goldberg, & Gardner, 2017; Meadow, 2011).

Temple Newhook et al. (2018) also asserted the following:

...this research was limited to children whose parents chose to bring them to a clinic for diagnosis and treatment and thus may have believed the child’s difference was a problem, and one that required psychological treatment. Children whose parents affirmed their gender (or who did not wish to...access clinical treatment for any reason) were likely not included in these studies.

Ok, well Temple Newhook et al. (2018) were not entirely sure about this (“may have believed...likely not included...”). In Toronto, there is no question that some parents/many parents were concerned about their child’s gender identity development. Others were not sure how they felt – they wanted professional advice. Others were not concerned at all but brought their child anyway (for different reasons). It would certainly be important in future studies to see how parental attitudes about their child’s gender-variant behavior/gender dysphoria are associated with long-term developmental outcomes. In Steensma et al. (2013a), Temple Newhook et al. are, in my view, wrong about their “affirmation” argument because some of the children in that study “socially transitioned” from one gender to another prior to puberty, which one can only assume occurred in the context of “supportive” parents. One definition of “supportive” in the *Oxford Dictionary of Current English* (Soares, 2001) is “encouraging.”

Changes in diagnostic criteria

A second issue noted by Temple Newhook et al. (2018) is that diagnostic criteria have changed over time, between the DSM-III in 1980 and the DSM-5 in 2013. That is true. Indeed, Temple Newhook et al. state that “...these studies included children who, by current DSM-5 standards, would not likely have been categorized as transgender (i.e., they would not meet

the criteria for gender dysphoria) and therefore...it is not surprising that they would not identify as transgender at follow-up.” There is a lot to unpack in this statement. First, even using the earlier criteria for the diagnosis of gender identity disorder of childhood/gender identity disorder, not all of the referred children were threshold for the diagnosis (see below). Second, it is an empirical question about what the degree of overlap would be if one used DSM-III, DSM-III-R, DSM-IV, and DSM-5 criteria to classify children referred for possible gender dysphoria. I doubt that anyone will ever do such a study, so it is not worth ruminating about it. It is my clinical opinion that the similarities across the various iterations of the DSM are far greater than the differences (Zucker, 2010) and, as part of the work done by the Subcommittee on Gender Identity Disorders for the DSM-IV, provided one example of this (Zucker et al., 1998).

One minor point about changes in the diagnostic criteria: Temple Newhook et al. stated that “Evidence of the actual distress of gender dysphoria...was dropped as a requirement for [Gender Identity Disorder] in the DSM-IV...” This is wrong. Criterion D reads as follows: “The disturbance causes clinically significant distress or impairment in social, occupational, or other important areas of functioning” (American Psychiatric Association, 1994, p. 538). Of course, how exactly one operationalizes distress is a complex matter, as I have discussed elsewhere (e.g., Zucker, 1992, 2005).

Predictors of developmental psychosexual trajectories

An important point discussed by Temple Newhook et al. (2018) is that not all children seen in the follow-up studies were threshold for the DSM diagnosis in childhood (see also Olson, 2016). Indeed, they quoted from Drummond et al. (2008) the following:

...40% of the girls were not judged to have met the complete DSM criteria for GID [Gender Identity Disorder] at the time of childhood assessment...it could be argued that if some of the girls were subthreshold for GID in childhood, then one might assume that they would not be at risk for GID in adolescence or adulthood. (p. 42)

Since I was a co-author of Drummond et al., how could I disagree with this statement?

Ten years later, here is how. On the one hand, the clinical reality is more complicated than this statement suggests. For example, there is ample contemporary

clinical experience that there are many adolescents who come in the front-door for evaluation who meet the diagnostic criteria for gender dysphoria, but, retrospectively, would not have met the complete DSM-5 criteria in childhood (either by their own self-report, by their parent’s perspective, or both). Some of these adolescents would not have met any of the criteria. Other adolescents (particularly birth-assigned females in my opinion) had some degree of gender-variant behavior during childhood (sometimes markedly so), but not at the level where they would have been threshold for the diagnosis because they did not express the desire to be of the other gender or some alternative gender different from their assigned gender. Thus, I would argue that degree of gender-variant behavior in childhood (with or without the presence of gender dysphoria per se) is a potential predictor of gender dysphoria in adolescence or adulthood that should not be too readily dismissed (see, e.g., Scholinski, 1997; <https://dylanscholinski.weebly.com/bioinfocv.html>).

On the other hand, what can we learn about the predictive value of the childhood diagnosis with regard to persistence? Table 1 shows the percentage of children classified as persisters or desisters from four follow-up studies as a function of whether or not they were threshold or subthreshold for the diagnosis in childhood. Of the 127 children who were subthreshold for the diagnosis, only 9 (7.1%) were classified as persisters. Of the 241 children who were threshold for the diagnosis, 79 (32.8%) were classified as persisters. This yields a sensitivity rate of 89.7%, which is pretty good, but a specificity rate of 42.1%, which is not so good. In absolute terms, that 7% of the subthreshold cases were classified as “persisters” at follow-up is low; nonetheless, a 7% “prevalence” rate would be substantially higher than the base rate in the general population, if, for example, one relies on some recent epidemiological studies of high-school students or

Table 1. Gender dysphoria (“persistence”) at follow-up as a function of diagnostic status in childhood.

		Persistence	
		Yes	No
DSM diagnosis in childhood	Yes	79 (32.8%)	162 (67.2%)
	No	9 (7.1%)	118 (92.9%)

Note. Data from Drummond et al. (2008), Singh (2012), Steensma et al. (2013a), and Wallien and Cohen-Kettenis (2008). DSM diagnosis in childhood: Yes = threshold for the diagnosis of gender identity disorder of childhood/gender identity disorder in DSM-III, DSM-III-R, or DSM-IV. No = subthreshold.

adults who self-identify as transgender (Clark et al., 2014; Eisenberg et al., 2017; Guss, Williams, Reisner, Austin, & Katz-Wise, 2017; Wilson, Choi, Herman, Becker, & Conron, 2017). The relatively low specificity rate challenges the “No True Scotsman” argument (https://en.wikipedia.org/wiki/No_true_Scotsman), namely that desisters were not truly gender-dysphoric to begin with, so there was nothing to desist from. Only persisters were truly gender-dysphoric in childhood. But unless one wants to completely dismiss the validity of the childhood criteria, one must contemplate the fact that 67% of children who met the criteria in childhood were classified as desisters at follow-up.

In this regard, I would like to make one additional point about the Steensma et al. (2013a) study, which Temple Newhook et al. did not appreciate. Because Steensma et al. conducted what might be characterized as a relatively short-term follow-up study, they had to select from their patient pool those who were, at the time of the initial assessment, relatively old (M age, 9.15 years). Indeed, when compared to the other three follow-up studies, their probands were, on average, the oldest at the time of the childhood assessment. In other words, patients seen in the clinic at relatively younger ages were not yet old enough to be eligible to participate in the follow-up study. It is conceivable that children seen at a relatively younger age might be more likely to desist than children seen at a relatively older age.

It should, of course, be recognized that the use of the DSM diagnosis as a categorical metric (unless one does a symptom count, which no one has done) has its limitations. Accordingly, both Steensma et al. (2013a) and Singh (2012) also used dimensional metrics as predictors of persistence vs. desistance, including a childhood measure of gender identity (Wallien et al., 2009; Zucker et al., 1993), consistent with Temple Newhook et al.’s incisive reference to Schreier and Ehrensaft (2016): “Want to know a child’s gender? Ask.” Indeed, I have been asking for a long time (per Wallien et al., 2009 and Zucker et al., 1993).

In Singh (2012; see also Zucker, 2017), three demographic variables (age at assessment, IQ, and parent’s social class background) and a composite measure of gender behavior from the childhood assessment were used in a multinomial logistic regression to predict persistence at the time of follow-up. In Table 2, the persisters (all of whom were classified as biphilic or androphilic in their sexual orientation) were compared to the desisters who were classified as (cisgender) biphilic or androphilic in

their sexual orientation. It can be seen that age at assessment (older) was weakly related to prediction of persistence ($p = .09$), but that social class (lower social class background) and degree of gender-variant behavior in childhood were both significant predictors of persistence ($p < .001$ and $.02$, respectively). In Steensma et al. (2013a), for birth-assigned males, age at the time of assessment (older), several measures of gender-variant behavior, and whether or not the child was classified as having either a partial or complete gender role social transition prior to puberty significantly predicted persistence. For birth-assigned females, two measures of gender-variant behavior, but not age at the time of assessment or classification as having either a partial or complete gender role social transition, predicted persistence.

These studies suggest that dimensional measurement of gender-variant behavior, including measures of the child’s self-reported gender identity/gender dysphoria, within a population of clinic-referred children, has the potential to predict persistence, albeit imperfectly. The data also have therapeutic implications, which I will discuss further below.

Age at the time of follow-up

I have no quarrel with Temple Newhook et al.’s (2018) cautionary remark about the relatively young age at the time of follow-up in the four core studies (in Drummond et al., 2008: M age, 23.24 years; range, 15–36; in Wallien and Cohen-Kettenis, 2008: M age, 18.9 years; range, 16–28; Singh, 2012: M age, 20.5 years; range, 13–39; Steensma et al., 2013a: M age, 16.14 years; range, 15–19). In Green’s (1987) earlier follow-up study, the mean age at follow-up was 19 years (range, 14–24). However, to my knowledge, no serious

Table 2. Demographic and gender behavior predictors: multinomial logistic regression for the bisexual/androphilic persisters.

Predictor	β	SE	Wald	p	Exp (B)
Age	.26	.16	2.90	.09	1.30
IQ	.02	.03	.58	ns	1.02
Social class	-.12	.03	13.28	<.001	.89
Gender z-score	1.32	.55	5.82	.02	3.74

Note. Reference group was the bisexual/androphilic desisters ($n = 16$). For the bisexual/androphilic persisters, $n = 66$. Data from Singh (2012; see also Zucker, 2017). Social Class was measured with the Hollingshead (1975) Four-Factor Index of Social Status (absolute range, 8–66). The Gender metric was a combination of several dimensional measures administered at the baseline assessment in childhood. Exp (B), sometimes written e^B , is the multiplicative change in the odds of membership in the persister group for a one-unit increase in the corresponding predictor; thus, $100 \times (e^B - 1)$ represents the percentage change in the odds for a one-unit increase in that predictor.

scholar has claimed that “by default” it can be assumed that the desisters have been “...‘correctly’ categorized as cisgender for their lifetime.” That is an empirical question. I certainly have seen some adolescent patients who, for example, “fluctuate” between self-identification as transgender vs. gay. For example, one young birth-assigned male with whom I worked self-identified as trans at the age of 13 but by age 15 self-identified as gay, stating “I was having a hard time accepting myself as a gay boy...I wanted to be normal” (i.e., a girl who was sexually attracted to boys); or some adolescents who initially self-identify as gay and later on shift to a transgender identity; or some adolescents who initially identify as cisgender heterosexual and then shift to cisgender gay. Temple Newhook et al., however, then go on to cite Reed, Rhodes, Schofield, and Wylie (2009): “Research has found that many trans-identified individuals come out or transition later in adulthood.”

On this point, I am not sure if Temple Newhook et al. are being intentionally deceptive or do not fully appreciate that we have known for a long time that, among birth-assigned males, there are two developmental pathways leading to gender dysphoria in adulthood: early-onset (the object of this exercise) and late-onset (e.g., Blanchard, 1989, 1991; Blanchard, Clemmensen, & Steiner, 1987; Lawrence, 2010, 2017).⁶ Using late-onset patients as an argument for the possibility that early-onset patients will only come out or transition in adulthood does not strike me as particularly compelling – it borders on pure sophistry, mixing apples with oranges. Nonetheless, I have no problem with the suggestion that longer term follow-up studies would be worthwhile.

Patients “lost” to follow-up

Temple Newhook et al. (2018) worry about the significance of patients lost to follow-up. Agreed. In Drummond et al. (2008; see also Drummond, 2006) and Singh (2012), an effort was made to evaluate the “internal validity” (Campbell & Stanley, 1969) of the samples by comparing those who participated in the follow-up study with those who did not on a number of demographic variables, a general measure of behavioral and emotional problems, and measures of gender-variant behavior at the time of the assessment in childhood. The data strongly suggested that there were minimal differences between those who

participated in the study and those who did not. This is important. For example, suppose that the patients not seen at the time of follow-up had significantly more gender-variant behavior or were disproportionately more likely to meet the full criteria for a DSM diagnosis in childhood. Then one would indeed be worried that the followed-up sample was not representative of the entire pool of patients; it would threaten the internal validity of the sample. But this was not the case.

Authentic identities

Temple Newhook et al. (2018) make the argument that “Assertion of a cisgender identity at any point in the life cycle is often assumed to be valid and invalidates any previous assertion of transgender identity...a transgender identity is only viewed as valid if it is static and unwavering throughout the life course...” This is an absurd claim – one that is not even referenced. Who exactly makes this assertion? Who exactly is doing the invalidating? I don’t think that any of the authors of the four core studies have ever made this type of assertion. Identity is a subjective construction (Zucker & VanderLaan, 2016). Of course, some “identities” are more stable and “authentic” than others (consider, for example, the chaotic subjectivities of some people with a diagnosis of borderline personality disorder [e.g., Jørgenson, 2006, 2010]). One birth-assigned male who I assessed at the age of 7 had transitioned socially around a year prior, in a sort of passive way. This child’s mother asked: “So, do you want to be a boy or a girl?” The child’s response was “What do you want me to be?” In my view, exploration of this child’s “true” or authentic self could be explored in a psychotherapeutic safe space.

Conflation of gender identity and sexual orientation

Temple Newhook et al. (2018) argued that the follow-up studies “conflate” gender identity and sexual orientation, citing Drescher and Pula (2014) as guilty of making this conflation. This is a ridiculous assertion. In the four core follow-up studies, gender identity/gender dysphoria and sexual orientation (in relation to natal sex) were assessed with distinct measures. I am really baffled why Temple Newhook et al. make this claim. Temple Newhook et al. also suggested that the follow-up studies were too binary in their evaluations. If one makes a DSM diagnostic judgment of

gender dysphoria (present vs. absent), then, yes, one has made a binary diagnostic decision (that is the way the DSM rocks and rolls for all diagnoses). However, for many years now, I have, with colleagues, used a dimensional measure of gender identity/gender dysphoria (Deogracias et al., 2007; Singh et al., 2010), which can, in principle, capture gendered shades of grey. However, in using a binary cut-off score of caseness (cf. Wing, Bebbington, & Robins, 1981), sensitivity and specificity were shown to be quite high when comparing adolescents and adults seen in specialized gender identity clinics vs. comparison groups (Deogracias et al., 2007; Singh et al., 2010; see also Singh, McMains, & Zucker, 2011). Given the apparent increase of patients who self-identify as “non-binary” (e.g., Beek, Kruekels, Cohen-Kettenis, & Steensma, 2015; Koehler, Eyssele, & Nieder, 2018; Richards et al., 2016), employing dimensional measures of gender identity/gender dysphoria should always accompany the use of a binary diagnostic system, such as the DSM-5.

Generalization to contemporary samples of transgender children

It is not clear to me if there will be any additional follow-up studies from other specialized gender identity clinics who assessed children during a similar period of time as the four core studies, where the year of initial assessment ranged from 1975 to 2008 (the Gender Identity Development Service in London or the patients seen in-person or via internet consultation at the Children’s National Medical Center in Washington, DC come to mind as having samples where such a follow-up study could be done).

The recent proliferation of gender identity clinics in North America, Europe, and elsewhere should, in theory, allow for new follow-up studies of contemporary samples (Hsieh & Leinger, 2014), along with samples obtained from outside clinical settings, as in the studies by Olson’s research group (op. cit.). With the emergence in the last 10–15 years of a pre-pubertal gender social transition as a type of psychosocial treatment – initiated by parents on their own (without formal clinical consultation) or with the support/advice of professional input (e.g., Ehrensaft, 2014; Vanderburgh, 2009; Wong & Drake, 2017) – it is not clear if the desistance rates reported in the four core studies will be “replicated” in contemporary samples. Indeed,

the data for birth-assigned males in Steensma et al. (2013a) already suggest this: of the 23 birth-assigned males classified as persisters, 10 (43%) had made a partial or complete social transition prior to puberty compared to only 2 (3.6%) of the 56 birth-assigned males classified as desisters. Thus, I would hypothesize that when more follow-up data of children who socially transition prior to puberty become available, the persistence rate will be extremely high. This is not a value judgment – it is simply an empirical prediction. Just like Temple Newhook et al. (2018) argue that some of the children in the four follow-up studies included those who may have received treatment “to lower the odds” of persistence, I would argue that parents who support, implement, or encourage a gender social transition (and clinicians who recommend one) are implementing a psychosocial treatment that will increase the odds of long-term persistence.

Meditations on ethics

Do contextual effects matter?

Temple Newhook et al. (2018) identified putative ethical concerns about the reviewed follow-up studies. They begin with the rhetorical statement that “research itself is an intervention.” I am not quite sure what Temple Newhook et al. mean. The *Oxford Dictionary of Current English* (Soares, 2001) defines intervention as an “action...to improve or control a situation.” In developmental clinical psychology and psychiatry, there are, of course, hundreds of research studies in which patients are assigned to a treatment group and some type of control group (e.g., a sham psychosocial treatment, placebo, wait-list, etc.), but the core follow-up studies were not part of any formal therapeutic (an “intervention”) protocol. Now, of course, it could be argued that participation in a research study per se might have an effect on an individual and that is why ethics protocols must weigh the benefits and risks of participation. Suppose one is conducting a survey on suicidality among adolescents from the general population. It is conceivable that answering questions about suicidal thoughts might cause distress, so it is common that the protocol would give the adolescent options as to whom they could talk to about such feelings.

Temple Newhook et al. (2018) then drill down and assert that “These studies took place in the context of gender clinics in which children were put through a

substantial degree of testing over periods of months or years.” I can’t speak for my Dutch colleagues, but, with regard to the Toronto follow-studies, on what basis do Temple Newhook et al. make this claim? Zucker, Wood, Singh, and Bradley (2012, Tables 1 and 2) provided a summary of their clinical assessment protocol, at least around the time that their article was published. It included a family assessment interview (3 hours), an individual interview with the child (1 hour), and psychological testing (4 hours). A feedback session, often with parents alone, was estimated at 1–2 hours. I view this protocol as comprehensive and thorough, yet Temple Newhook et al. write as if it was some kind of psychological waterboarding. The “testing” hardly took place over periods of months or years.⁷ Other than to imply some kind of clinical malevolence, I really don’t understand what Temple Newhook et al. are trying to say. As far as I know, there is no “gold standard” for what constitutes a clinically sound assessment for children referred for gender dysphoria. If Temple Newhook et al. want to bid for an assessment hegemony, they should propose one and open it up to debate.

With regard to the children who participated in the follow-up studies, I can say this. Some of these children and their families were seen for an assessment and never seen again until the family was contacted for the follow-up (in Drummond et al., 2008, the mean interval between the baseline assessment and follow-up was 14.34 years; in Singh [2012], the mean interval was 12.88 years). Other children (and their parents) were seen for a long time in therapy (sometimes in the clinic; sometimes with clinicians in the community) because they needed it. But, to be clear, there was a lot of variability in how much clinical contact there was with families between the time of the assessment in childhood and the follow-up.

Temple Newhook et al. (2018) stated that there was “...an absence of information about whether research participation was optional and if steps were taken to ensure that children could decline research consent while continuing to receive needed services.” In the Toronto follow-up studies, I will make it perfectly clear the answer to this query. Yes, participation in the follow-up studies was optional and, yes, patients could decline to participate. Of the 25 participants in Drummond et al. (2008) at the time of follow-up, none were in treatment in the clinic. The one exception was an adolescent who returned to the clinic for reasons

unrelated to gender identity and, at the time, was enrolled in the study. To be transparent, both studies were approved by the Institutional Review Boards at the Centre for Addiction and Mental Health and the University of Toronto. The consent form for the Drummond et al. (2008) study can be found in Drummond (2006, Appendix C); for the Singh (2012) study, Appendix F.

Do therapeutic models matter?

...we cannot rule out the possibility that early successful treatment of childhood GID [Gender Identity Disorder] will diminish the role of a continuation of GID into adulthood. If so, successful treatment would also reduce the need for the long and difficult process of sex reassignment which includes hormonal and surgical procedures with substantial medical risks and complications. (Meyer-Bahlburg, 2002, p. 362)

Relatively little dispute exists regarding the prevention of transsexualism, though evidence about the effectiveness of treatment in preventing adult transsexualism is also virtually nonexistent. (Cohen-Kettenis & Pfäfflin, 2003, p. 120)

Over the last decade, we have seen a sea change in approach to pediatric transgender care, with the gender affirmative model now widely adopted as a preferred practice... (Chen, Edwards-Leeper, Stancin, & Tishelman, 2018, p. 74)

Temple Newhook et al. (2018) critique the follow-up studies in relation to therapeutic models that have been described in some detail elsewhere (deVries & Cohen-Kettenis, 2012; Zucker et al., 2012). Therapeutic models, one would hope, are informed by a conceptual/theoretical formulation about gender identity development, which, in turn, might be applied to help children, one way or the other, reduce their gender dysphoria (see also Ehrensaft, 2012, 2014; Menvielle, 2012; Pyne, 2014; Turban & Ehrensaft, 2017). A very significant problem in the field is that there are no randomized control trials (RCT) with regard to treatment of children with gender dysphoria, as has been noted in several authoritative reviews (American Academy of Child and Adolescent Psychiatry, 2012; American Psychological Association, 2015; Byne et al., 2012; see also Dreger, 2009; Edwards-Leeper, Leibowitz, & Sangganjanavanich, 2016; Green, 2017). And there won’t be, because many, if not most, parents would refuse to have their children randomized into

different treatment arms (and, quite frankly, I don't blame them). A parent who would like their child's gender dysphoria reduced via psychotherapeutic methods would refuse to allow their child assigned to a gender social transition treatment approach. A parent who would like their child's gender dysphoria reduced via a gender social transition would refuse to have their child assigned to a psychotherapeutic approach. It is possible that some parents would agree to randomize their child to a "wait-and-see" (Menvielle, Tuerk, & Perrin, 2005) or "watchful waiting" (Zucker, 2008) approach as opposed to their preferred therapeutic approach, but, to my knowledge, no one has attempted such a partial RCT.

Now, of course, it would not come as a surprise if Temple Newhook et al. (2018) took umbrage at the mere idea of a treatment arm designed to reduce a child's gender dysphoria via psychotherapeutic methods. They might, for example, offer up the following from the seventh edition of the Standards of Care:

Treatment aimed at trying to change a person's gender identity...to become more congruent with sex assigned at birth has been attempted in the past without success (Gelder & Marks, 1969; Greenson, 1964)...Such treatment is no longer considered ethical." (Coleman et al., 2011, p. 175)

Yet, on the very same page of the Standards, one finds the following: "Psychotherapy should focus on reducing a child's...distress related to the gender dysphoria..." (p. 175) or "Mental health professionals... should give ample room for clients to explore different options for gender expression" (p. 175). The lack of internal consistency between the first statement and the second and third statements is rather astonishing. Moreover, the "without success" remark offers up two citations as authoritative proof. One was a reference to a 5-year-old boy seen by a psychoanalyst in Beverly Hills, California (see also Greenson, 1966) and other was a reference to the use of faradic aversion treatments on birth-assigned male adults said to be "transvestites with moderate transsexualism" (Gelder & Marks, 1969, p. 394). Personally, I prefer the following summary statements about therapeutics with regard to children with gender dysphoria:

Different clinical approaches have been advocated for childhood gender discordance....There have been no randomized controlled trials of any treatment....the proposed benefits of treatment to eliminate gender

discordance...must be carefully weighed against... possible deleterious effects. (American Academy of Child and Adolescent Psychiatry, 2012, pp. 968–969)

Very few studies have systematically researched any given mode of intervention with respect to an outcome variable in GID and no studies have systematically compared results of different interventions....In light of the limited empirical evidence and disagreements...among experts in the field...recommendations supported by the available literature are largely limited to the areas [reviewed] and would be in the form of general suggestions and cautions... (Byne et al., 2012, p. 772)

...because no approach to working with [transgender and gender nonconforming] children has been adequately, empirically validated, consensus does not exist regarding best practice with pre-pubertal children. Lack of consensus about the preferred approach to treatment may be due, in part, to divergent ideas regarding what constitutes optimal treatment outcomes... (American Psychological Association, 2015, p. 842)

In the Drummond et al. (2008) study, no effort was made to attempt a link between therapeutic intervention and outcome:

It [was] beyond the scope of this report to describe the types of therapies (as well as their frequency and duration) that the girls and/or their parents may have received between the assessment in childhood and the follow-up (e.g., by a therapist within the Gender Identity Service at the Centre for Addiction and Mental Health or in the community). From the participants' clinic files, 13 of the 25 girls had at least some contact with our clinic during the interval between assessment and follow-up (e.g., as therapy clients or for a reassessment). Of the 25 girls and/or their parents, 18 had been in some type of therapy or counseling during the interval between assessment and follow-up; of these, 5 were patients of staff within the Gender Identity Service, and the remainder were seen by a professional in the community. (p. 36)

The same could be said for Singh's (2012) sample and, to my knowledge, the Dutch group as well.

Temple Newhook et al. (2018) go on to state that "It is important to acknowledge that discouraging social transition [with reference to the Dutch team's putative therapeutic approach] is itself an intervention with the potential to impact research findings..." Fair enough. But Temple Newhook et al. (2018) curiously suppress the inverse: encouraging social transition is itself an intervention with the potential to impact findings. I find this omission astonishing.

Harm, harm, and more harm

Temple Newhook et al. (2018) argue that attempts to delay or defer a gender social transition may cause harm and that such harm has been underestimated. Much of what it is argued here is, shall we say, “anecdotal” with the use of brief clinical clips. Fine. One has to start somewhere with an argument. Deep into this section, Temple Newhook et al. cite the work of Olson and her research group (op. cit.), stating that:

Emergent research on the health and well-being of trans children who are affirmed in their gender identity... indicates mental health outcomes equivalent with cis-gender peers...this is in stark contrast to the high levels of psychological distress and behavioral problems documented among children who were discouraged from asserting their identities in childhood.

This is a gross oversimplification, an oversimplification that Temple Newhook et al. require in order to assimilate their interpretation of the data into their theoretical/ethical argumentation. Disclosure: I think that the work of Olson’s research group is excellent, including the studies that have assessed various parameters of gender development (e.g., Dunham & Olson, 2016; Fast & Olson, 2018; Olson & Enright, 2017; Olson & Gülgöz, 2017; Olson, Key, & Eaton, 2015). However, the Olson et al. (2016) study on mental health measurement has serious methodological flaws, which affect the interpretation of the data (cf. Turban, 2017). First, as noted earlier, the sample was not representative of socially transitioned children in general. Second, the mental health outcome data were assayed at some unspecified time interval after the social transition had occurred. Thus, although the children had, on average, scores in the nonclinical range, it is completely unclear if they would have had similar scores prior to the social transition. In other words, Olson et al. had “post-treatment” data, but no “pre-treatment” data.

The reference to the high levels of distress among children who were discouraged from “asserting their identities in childhood” is without any empirical documentation. For example, the reference to the study by Cohen-Kettenis, Owen, Kaijser, Bradley, and Zucker (2003) did not measure whether or not parents (or others) encouraged, discouraged, or were neutral with regard to the gender-variant behavior/gender dysphoria of their children. Cohen-Kettenis et al. (2003) examined other correlates of behavioral and emotional problems, but not the one that Temple Newhook et al. (2018)

assert. So, here, Temple Newhook et al. have defaulted to rhetoric and dogma. In my own research over the years in which I have measured behavioral and emotional problems among children referred for gender dysphoria, I have always noted that there is a great deal of variability in clinical range problems. Gender-referred children under the age of 6 years, for example, do not show, on average, a great deal of behavioral and emotional problems (see, e.g., Zucker & Bradley, 1995, pp. 79–103; Singh, Bradley, & Zucker, 2011; for review, see Zucker, Wood, & VanderLaan, 2014). The determinants of mental health issues in children with gender dysphoria are multifactorial and should not be reduced to the simple narrative of parental support.

In general, it is of course extremely important to have systematic longer-term follow-up data on children with gender dysphoria with regard to their general well-being and psychosocial adaptation, not just information about rates of persistence vs. desistance. So, on this point, I agree with Temple Newhook et al. (2018). Drummond, Bradley, Peterson-Badali, VanderLaan, and Zucker (2018) evaluated the presence of clinical range behavioral/emotional problems and psychiatric diagnoses in the Drummond et al. (2008) cohort. Using the Child Behavior Checklist or Adult Behavior Checklist as rated by their mothers, 39.1% had clinical range scores; on the Youth Self-Report or the Adult Self-Report, 33.3% had clinical range scores. On either the Diagnostic Interview for Children and Adolescents (DICA) or the Diagnostic Interview Schedule (DIS), the participants had, on average, 2.67 diagnoses (range, 0–10). On the one hand, 33% did not meet criteria for any diagnosis; on the other hand, 46% met criteria for three or more diagnoses. (Of the three participants classified as persisters at follow-up, they had 0, 3, and 5 DICA/DIS diagnoses, respectively.) From the childhood assessment, five variables were significantly associated with a composite Psychopathology Index (PI) at follow-up: a lower IQ, living in a non-two-parent or reconstituted family, a composite behavior problem index, and poor peer relations. At follow-up, degree of concurrent homoeroticism (in relation to birth sex) and a composite index of gender dysphoria were both associated with the composite PI. Drummond et al. (2018) summarized their data as follows:

... girls referred for gender dysphoria show, on average, a general psychiatric vulnerability as they grow up. It is,

however, important to keep in mind that there [was] variability in this vulnerability and that not all gender-dysphoric girls manifest clinical range psychopathology, both at the time of assessment in childhood and at the time of follow-up....our data suggest that it is important to consider...an integrative, holistic approach in the clinical care of these patients, which not only tracks their long-term psychosexual development, but also their mental health in general. (p. 182)

At the end of their long ethical discourse about harm, Temple Newhook et al. (2018) conclude that "...longitudinal studies about identity 'desistance' or 'persistence' are not the best tools for understanding the needs of gender-nonconforming children." Although I agree it should not be the only metric for understanding the needs of children with a diagnosis of gender dysphoria, the implicit message is something like this: Research on persistence and desistance should be suppressed: it should just disappear without a trace. This is empirical and intellectual "no platforming" at its worst. I find this ominous, but not surprising.

Notes

- 1 I wrote the first draft of this essay "masked" to the identity of the authors. In the interest of transparency, now that I know who the authors are, two points: The second author, Tosh, has been no fan of mine, as exemplified in the scholarly title of an essay penned for the *Psychology of Women Section Review* of The British Psychological Society entitled "'Zuck off! A commentary on the protest against Ken Zucker and his 'treatment' of childhood gender identity disorder" (Tosh, 2011). The fourth author, Pyne, has not exactly been a fan either. In March 2016, I filed a "statement of claim" (in plain English, a lawsuit) against Pyne and the *Toronto Star Newspapers* for a piece written by Pyne (2015). As noted in the *Toronto Star* on December 19, 2017, "This material was subject to legal complaint by Dr. Kenneth J. Zucker, which has been resolved" (<https://www.thestar.com/opinion/commentary/2015/12/17/discredited-treatment-of-trans-kids-at-camh-shouldnt-shock-us.html>).
- 2 Per Bouman et al. (2017), the term "birth assigned sex" was suggested as part of the language policy for the 2017 meeting of the European Professional Association for Transgender Health. It was recommended over the terms "natal male or natal female." Natal is defined as in "relation to the...time of one's birth" (Soares, 2001). In the *Diagnostic and Statistical Manual of Mental Disorders* (American Psychiatric Association, 2013), terms such as "natal girls" and "natal boys" were used. It seems to me that all of these options are reasonable.

- 3 If one googles "Devita Singh doctoral dissertation," the pdf of the dissertation is the first entry (see also <https://search.library.utoronto.ca/details?9017513>).
- 4 I use the diagnostic term "gender identity disorder," along with the diagnostic term "gender dysphoria" because the former was the diagnostic label at the time of the follow-up studies that are reviewed.
- 5 The ethics protocol was such that parents were contacted first to let them know about the study and if they were willing to let us talk to the potential participants. In part, this was because we had no way or knowing if all of the adolescents or adults would have even remembered having been seen in the clinic as a child.
- 6 Late-onset birth-assigned females with gender dysphoria have, in recent years, become a very salient part of the clinical landscape, particularly among adolescents (see, e.g., Littman, 2017). They are not, however, exactly parallel with some aspects of the gender developmental histories of late-onset birth-assigned males.
- 7 It is true, however, that, several decades ago, we did a study in which 44 children referred to the clinic and their siblings were seen for psychological testing at a one-year follow-up (median interval, 371 days) that evaluated the evidence for stability and change in gender-typed behavior (Zucker, Bradley, Doering, & Lozinski, 1985). So what? It is common in specialized clinical programs at academic health science centers to conduct such types of follow-ups. Over the subsequent years, some children might have been seen for follow-up assessment, including psychological testing, on an as needed basis for clinical reasons. At times, such a re-assessment may have been for reasons completely unrelated to the child's gender identity (e.g., a learning disability, a psychopharmacology consult, etc.). So what? This is nothing more than being clinically responsible for the well-being of one's clients.

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