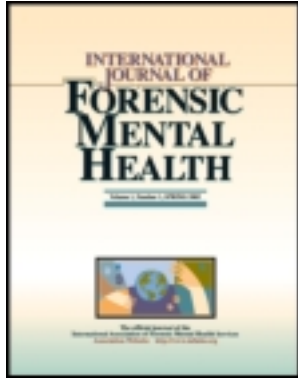


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COMMENTARY

The Errors of Karen Franklin's *Pretextuality*

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In her recent article, *Hebephilia: Quintessence of Diagnostic Pretextuality* (published in *Behavioral Sciences and the Law*, 2010), Karen Franklin expands on her previous argument that psychologists and psychiatrists should not diagnose as abnormal *hebephilia*, the sexual preference for early pubescent children. She supports her argument with a series of claims about the contents of the empirical literature and the scientists who produced it. The present document provides fact-checking of those claims, revealing that Franklin's conclusions are based largely on demonstrable falsehoods.

Keywords: pedophilia, hebephilia, DSM-5, sexually violent predator, ICD-10, sexual abuse, civil commitment

In *Hebephilia: Quintessence of Diagnostic Pretextuality*, Karen Franklin (2010a) expands on her previous argument that psychologists and psychiatrists should not diagnose as abnormal *hebephilia*, the sexual preference for early pubescent children, typically ages 11 to 14.¹ (To forestall a common error: *Puberty* should not be confused with *adolescence*.)² Very briefly, Franklin's essay outlines political and

financial aspects of sex offender management in the United States, asserts that the science (and the scientists) studying hebephilia are lacking, and concludes that the acceptance of hebephilia among professionals, professional organizations, and courts (including the U.S. Supreme Court) is due to the financial factors rather than any scientific merits. That is, Franklin's view is that a nearly nonexistent body of science is being used as a pretext to justify financial interests.

Franklin's argument is a synthesis. She supplies a series of claims, interpreting each one to show the reader how they are consistent with her main idea. The difficulty with discussing someone's interpretation of the facts is that, of course, facts are always open to interpretation, and the best I could hope for would be a "he says/she says" debate over how best to view the information. Luckily, we do not need to argue over the interpretations. Franklin very simply has the facts *themselves* in error, as simple fact-checking will demonstrate.

This article was accepted by Stephen Hart, the former Editor in Chief of the journal, as an "invited submission" and was not subject to the usual peer review process.

The subject of the Commentary, Dr. Karen Franklin, was given the opportunity to submit a rebuttal to this article but declined.

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¹It is the tradition for letters such as the present one to be submitted to the journal that published the initial article. Unfortunately, that journal—*Behavioral Sciences and the Law* (BS&L)—does not publish letters to the editor, and the BS&L Editor informed me that this letter could not be submitted to their anonymous peer review system either, because the specific points I raise necessarily reveal my identity. I therefore elected to submit these comments here.

²*Puberty* refers to the transitional period during which the several maturational systems variously activate, the first of which typically becomes apparent around age 11, and most of which have come online by age 14. *Adolescence* spans this period until the completion of maturation, usually in the late teens or early twenties. The first appearance of pubic hair occurs at a mean age of 11.0 years in females and 11.2 years in males (Roche, Wellens, Attie, & Siervogel, 1995); the first stage of breast development in females ("breast buds"), at a mean age of 11.2 (Roche et al., 1995); the initial growth of the penis and testes in males, at a mean age of 11.2 (Roche

et al., 1995); the first menses occur at a mean age of 12.9 years in Caucasian, American females (Herman-Giddens et al., 1997); and the pubertal growth spurt begins at a mean age of 10 in females and at a mean age of 12 in males (Grumbach & Styne, 1998, Fig. 31–11). This period lasts approximately until age 14: In females, the adult-pattern of pubic hair is typically achieved at ages 13.1 to 15.2, and of adult breasts, at 14.0 to 15.6 years (Grumbach & Styne, 1998, Table 31–2). In males, the adult-pattern of pubic hair is achieved at ages 14.3 to 16.1, and the genitalia attain adult size and shape at 14.3 to 16.3 years (Grumbach & Styne, 1998, Table 31–4). Approximately 0.3% of boys experience their first ejaculation before age 11, and 87.7%, by age 14 (Tomova, Lalabonova, Robeva, & Kumanov, 2011). Adult height is achieved at approximately age 15 in females and 17 in males.

The backbone of Franklin's *pretextuality* argument is that hebephilia is an obscure and nearly uninvestigated concept. (If hebephilia had a significant scientific following, then there could be a valid rather than a financially motivated reason to make hebephilia explicit in diagnostic systems.) Her evidence for the obscurity of hebephilia is her claim that it is absent from the professional venues where a legitimate idea would be expected to appear. For example, she tells the reader that "hebephilia is not included in any formal diagnostic system or authoritative text" (p. 752). Such claims are easily falsified, however: I have made available online a list of *one hundred* texts that do in fact include hebephilia, published from multiple fields, from multiple countries, and (despite Franklin's claim of recent emergence) from several decades (Cantor, 2011a). One cannot expect Franklin to have read every book in the field, of course, but the list does include the Kinsey Institute's *Sex Offenders: An Analysis of Types*, which Franklin called "a landmark study" (p. 755) and *Sexual Deviance*, which Franklin called a "widely consulted text" (Franklin, 2010b). It is worth pointing out, however, that there is no meaningfully objective basis for saying how many such texts there should be, nor even whether such a number is a useful indicator of legitimacy in the first place.

The falsity of Franklin's claim that hebephilia is not contained in any formal diagnostic system is demonstrated just as readily: The current version of the *International Classification of Diseases* (ICD-10) contains code F65.4, which defines *paedophilia* as "A sexual preference for children, boys or girls or both, usually of prepubertal or early pubertal age" (World Health Organization, 2007; emphasis added). That is, people with a sexual preference for early pubescent children do indeed receive a diagnosis in the ICD system. In Franklin's defense, one could claim that *the word* "hebephilia" does not appear in the ICD; however, *the people* with hebephilia would receive a diagnosis nonetheless.

A substantial justification for Franklin's portrayal of hebephilia as an obscure concept relies on her claim that hebephilia research is essentially idiosyncratic to my predecessors' and my research team at the Centre for Addiction and Mental Health (CAMH, formerly the Clarke Institute of Psychiatry), claiming that "Only one other group of contemporary researchers has targeted men with sexual offenses against pubescent minors for special attention" (p. 755). Yet, that claim too fails fact-checking: Research databases, including *scholar.google*, reveal the existence of multiple other contemporary researchers reporting data on samples of hebephiles (e.g., Beier et al., 2009; Brown, Gray, & Snowden, 2009; Desjardins & Granger, 2008; Studer, Aylwin, Clelland, Reddon, & Frenzel, 2002). There is no objective means by which one can claim how much research is enough research, how many researchers are enough researchers, nor how many replications are enough replications. To declare that none of this science and none of these scientists even exist, however, is a misrepresentation of fact. For reference, I have made available online a comprehensive list of 32 peer-reviewed,

empirical articles that include samples of hebephiles, whether published by my own team, the aforementioned teams, or other teams (Cantor, 2011b).

Franklin's remaining evidence for hebephilia's alleged obscurity represents more symbolism than meaningful indictment: Franklin insinuates that my colleagues and I have inappropriately popularized hebephilia because we were the first authors to use the word in an article title (i.e., Cantor et al., 2005), and she questions the concept of hebephilia because the word *hebephilia* does not appear in the Oxford English Dictionary (OED). The aforementioned research databases, however, reveal that the first use of "hebephilia" in an article title was actually by Greenberg, Bradford, and Curry (1993): "A comparison of sexual victimization in the childhoods of pedophiles and hebephiles." Finally, despite its standing, the OED has never been the standard for the acceptance of terms in highly specialized fields. Indeed, also absent from the OED is the word "pretextuality."

In sum, Franklin's claims that hebephilia is not widely recognized—or not widely investigated, or not widely cited—are simply a series of easily revealed falsehoods about the state of the peer-reviewed literature. Franklin's essay also passes on to readers some of the negative (but none of the positive) comments about my colleagues' and my research, comments published as letters to the editor in an issue of the *Archives of Sexual Behavior*.³ Blanchard (2009, 2010) addressed the critical elements in detail already; so, rather than repeat the same points here, I refer readers to those already-published responses.

Finally, Franklin provides in her essay a series of portraits of the researchers themselves, painting them primarily with vague insinuations, relying on what scholars of rhetoric call "the association fallacy." Franklin's criticisms of Glueck, who published over 50 years ago, did not contain the usual topics that a scholar might use when evaluating another's work: sample size, statistical methods, etc. Rather, Franklin's criticism of Bernard Glueck was that "his brother and sister-in-law, Sheldon and Eleanor Glueck, were

³Although Franklin uses those letters to cast doubt upon our research data, those writers actually extolled the scientific quality of my team's work: "Blanchard et al.'s findings are useful toward consideration of whether a pattern of erotic preference for pubescent and/or early post-pubescent humans is reliable, stable, and identifiable" (DeClue, 2009, p. 317). "I am not challenging their conclusion that sexual interests in pubescent and pre-pubescent minors are distinct entities (albeit with some overlap) or that the distinction may have utility for research purposes" (Moser, 2009, p. 323). "I find no problem with the plethysmography methodology employed" (Plaud, 2009, p. 326). "If I had been a peer-reviewer for . . . 'Pedophilia, Hebephilia, and the DSM-V,' I would have recommended publication with only minor revisions The bulk of this peer-reviewed article appears to be scientific and to contribute to the advancement of knowledge" (Tromovitch, 2009, p. 328). That is, the nature of the criticisms levied was not about the quality of my team's findings. The nature of the criticisms pertained to those writers' perceptions of the *implications* of our work (which is very much their right), but not of our work itself, as Franklin characterizes.

prominent eugenicists” (p. 756). Of course, one might point out that before World War II, “eugenics” meant something very different from the “racial purity” connotation it has now—but the entire argument is merely one of propaganda-style journalism, irrelevant to the actual phenomenon of hebephilia or to any of the evidence pertaining to it. Kurt Freund received similar treatment: Between 1972 and 1989, Freund published his eight articles that included samples of hebephilic men. Franklin cited none of those, but did deem it relevant to mention Freund’s *suicide*.⁴ I never met Freund myself, but the story surrounding his death is no secret: At the age of 80, he was diagnosed with terminal cancer (Wilson & Mathon, 2006). He continued to work as his health permitted during his physical deterioration—until the week before his death, in fact—and then, in what I am repeatedly told was characteristic of him, got his affairs in order and ended his own life when his condition became intolerable. If there were any relevance at all between Freund’s professional life and the manner of his physical death, Franklin didn’t provide it.

As Franklin’s primary scapegoats, my colleagues and I at CAMH receive the lion’s share of her innuendo. As already noted, Franklin’s central thesis is that the professional recognition of hebephilia is driven by financial interests, where “psychologists have flocked to fill a lucrative niche” (p. 753) providing government-retained assessment testimony in “sexually violent predator” (SVP) cases. Franklin singles out Blanchard, Zucker, and me as the central drivers of that recognition, but neglects to point out that our entire careers have been outside the American SVP system and, in fact, entirely outside the U.S. itself. Indeed, if there is a more effective means of avoiding the undue influence of money than is being ineligible to receive the money, Franklin never mentions what it might be. Nonetheless, to forestall birther-style rumors from emerging: Neither I, nor Blanchard, nor Freund, nor Zucker has ever been retained for SVP testimony, either by prosecution or by defense. Indeed, none of us has ever been paid for any testimony of any kind. I have no private practice, and my salary is at the Ontario Public Service Employees Union rate for psychologists at CAMH and is publicly documented (Ontario Ministry of Finance, 2011).

Next, Franklin insinuates we somehow use our editorial positions to favor the publication of our own ideas:

Zucker and study coauthors Blanchard and James Cantor serve together on the editorial board of the journal that published the study. Thus, the CAMH group is poised to exert tremendous influence over the revision process for the DSM-

5 sexual disorders and, by extension, the shape of forensic diagnosis of sex offenders for some time to come. (p. 765)

Although it should go without saying, I will for the record: Not only have I not exerted any undue influence over any article I have ever published or co-published, I am entirely *unable* to. This will already be clear to professionals who understand the operation of the masked review process, used by almost all academic journals, including the *Archives of Sexual Behavior* and all other journals in which I have published data about hebephilia. Nonetheless, for the benefit of readers who may not be as familiar with scholarly publishing: The very purpose of the masked peer review process is to make it impossible for authors to exert influence of exactly the type Franklin insinuates. Publishing in a journal for which one also sits on the editorial board is not only routine, it is what puts the *peer* in *peer review*. This is best illustrated by concrete example:

Franklin chose to publish her essay in *Behavioral Sciences and the Law* (BS&L), so to take that journal as the most obvious exemplar: The BS&L editorial board consists of 36 persons, and a standard literature search (which I have made available online; Cantor, 2011c) reveals that 16 of the board members have published one or more articles in that same journal within the five years prior to Franklin’s essay. All this is to say that publishing articles in journals for which one serves on the editorial board is routine, the handling of which is exactly what the masked review system was designed to do. If Franklin has any evidence whatsoever to justify her insinuation that Kenneth J. Zucker (editor of the *Archives*) has violated the integrity of the masked review process for my or for anyone else’s benefit, I invite her to present it and to report it to the appropriate regulatory body.

As the late Senator Daniel Patrick Moynihan put it: “Everyone is entitled to their own opinion, but not their own facts.” Nonetheless, that is the bulk of Franklin’s essay: A series of easily falsified mischaracterizations of the content and status of the science of hebephilia, a series of vague insinuations unrelated to the findings, and a misrepresentation of the operation of the masked peer review system, serving to evoke rhetorical sympathy from any readers unaware of how such systems operate.

Despite pointing out Franklin’s mischaracterizations of my and other scientists’ research, I should not be mistaken to be supporting any given sex offender management policy. Many jurisdictions have implemented multiple, poorly conceived, and emotionally driven policies based on no science at all, and worse, in direct opposition to the available science. For the interested reader, I recommend Tabachnick and Klein’s (2011) *A Reasoned Approach: Reshaping Sex Offender Policy to Prevent Child Sexual Abuse*, published by the Association for the Treatment of Sexual Abusers, and which provides summaries of the relevant research and recommendations for integrating the science into sound public policy.

⁴Franklin does, however, cite some of Freund’s pre-1972 working definitions of hebephilia. Franklin never cites the definition Freund came to use when primary data became available (in 1972) and which he then retained throughout his career. She did, however, emphasize various historical terms that eventually led to “hebephilia.” Thus, Franklin’s essay engages in what scholars of rhetoric call “the etymological fallacy,” that is, incorrectly asserting that early uses of terms are more valid than are contemporary ones.

Decisions about what should and should not be included in the DSM is part science and part value judgment. Regarding the science, I am personally of the opinion that the existing peer-reviewed data pertaining to hebephilia support the existence of hebephilia as a taxonomically meaningful category on par with the evidence that supports other categories already listed in major diagnostic systems. Regarding the value judgment, the many and opposing values cannot be decided by science. Franklin is entirely free to her view, as are the many other stakeholders—victim-advocates, an anxious but often misinformed public, policy-makers who are often pressured to make snap and sometimes ill-conceived decisions, professional providers of defense testimony (a group Franklin does *not* mention among those with a potential financial interest), advocates for the fair treatment of offenders, and even alternative sexuality advocates who philosophically reject the idea that any sexual interest (including hebephilia and pedophilia) should ever be deemed a mental illness. These and many other groups have the freedom to consider their social and political values in deciding how to apply scientific knowledge in the public policy arena. But to employ propaganda-style journalism where objective scholarship should be would be the quintessence of pretextuality.

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