

From a purely clinical perspective, the findings of Herbel and Stelmach are hard to apply to a pregnant woman. Their cases involved 22 incarcerated men, no women, and, in over a third, weapons. It is a retrospective review, implicitly open-labeled and unblinded. Perhaps pertinent to a woman who is expecting in less than two months, 10 of their 17 responders “did not show significant improvement until. . .at least three months of continuous treatment” (Ref. 1, p 55).

I support Tamburello’s assertion that further investigation into delusional disorder is of interest to forensic psychiatry. Readers will have to decide for themselves how effectively and how quickly to expect medication to subdue circumscribed delusions in the absence of hallucinations and disorganization.

### References

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Seth Powsner, MD  
Professor of Psychiatry and Emergency Medicine  
Yale University/Yale-New Haven Hospital  
New Haven, CT

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### Editor:

In an article published in the December 2011 issue,<sup>1</sup> John M. Fabian, PsyD, JD, reviewed scholarly, clinical, and legal questions concerning hebephilia, with particular reference to sexually violent predator civil commitment proceedings. The term hebephile refers to individuals (usually men) who are most sexually attracted to pubescent children rather than to persons older or younger. This label stands in contrast to the term pedophile, which refers to men who are most sexually attracted to prepubescent children, and to the term teleiophile, which refers to men most attracted to persons between the ages of physical maturity and physical decline. I use hebephile to refer to men with an erotic preference for children who are generally 11 through 14 years of age.

Among the many and varied questions considered by Fabian is “whether attraction to postpubescent adolescents is, in actuality, a sexual deviation at all,

especially given that from biological and evolutionary perspectives, such attraction patterns may be considered adaptive and normal” (Ref. 1, p 500). This question, as stated, contains several elements. I need to unpack them before I can explain a specific point on which Fabian misrepresented my views, thus necessitating this letter of correction.

It is true that normal men (i.e., teleiophiles) respond with some degree of penile tumescence, at least in the laboratory, to depictions of nude pubescent and even prepubescent children of their preferred sex. This finding was made in the Kurt Freund Laboratory,<sup>2</sup> and it has been confirmed in the same laboratory.<sup>3</sup> There is a difference, however, between the finding that teleiophiles respond at some detectable level to depictions of pubescents and the finding that other men (hebephiles) respond more strongly to depictions of pubescents than to those of prepubescents or adults. The former observation does not make the latter normal.

It certainly does not make the latter finding adaptive. That was the whole point of the study that I published on this topic a few years ago.<sup>4</sup> Unfortunately, Fabian accidentally reversed my conclusions from that study, thus seeming to place me in the camp of those who object to the classification of hebephilia as paraphilic on Darwinian grounds. The foregoing quote from Fabian’s article is followed by this sentence:

Along these lines, Blanchard suggests that when considering evolutionary adaptedness, men with erotic preference for pubescent females have greater reproductive success, either because they acquire female mating partners who are near their onset of fertility which prevents them from being impregnated by other men, or because they have more years in which to impregnate their female mates [Ref. 1, p 500].

That is the precise opposite of what I concluded from that study, in which I compared the mean number of biological children reported by 818 heterosexual teleiophiles, 622 heterosexual hebephiles, and 129 heterosexual pedophiles. The results showed that the teleiophiles had significantly more children than did the hebephiles, and the hebephiles had significantly more children than did the pedophiles. Here is my actual conclusion, which is the last paragraph of my two-page article:

I am not concluding from these results that hebephilia should be included in the DSM on the grounds of reduced reproductive fitness. That reasoning would imply that homosexual teleiophilia should be reinstated in the DSM, which is not my view at all. My conclusion, rather, is that contemporary heterosexual hebephiles are significantly less

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fertile than are heterosexual teleiophiles. Thus, there is no empirical basis for the hypothesis that hebephilia was associated with increased reproductive success in the environment of evolutionary adaptedness. That speculative adaptationist argument against the inclusion of hebephilia in the DSM cannot be sustained [Ref. 4, p 818].

I would like to point out in closing that this factual error does not affect the rest of Fabian's interesting article, which addresses various aspects of hebephilia, the law, and psychiatry.

### References

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Ray Blanchard, PhD  
Department of Psychiatry

University of Toronto  
Toronto, Ontario, Canada

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### Reply

Editor:

I thank Dr. Blanchard for drawing my attention to the error that he has described quite effectively and accurately. Unfortunately, my error was due to misreading his interpretations of another scholar. There was no intention to misclassify him in the wrong camp of scholars who are currently debating these questions in the field.

John Matthew Fabian, PsyD, JD, ABPP  
Board Certified Forensic & Clinical Psychologist  
Fellowship Trained Clinical Neuropsychologist  
Cleveland, OH

Disclosures of financial or other potential conflicts of interest: None.