infants suffered brain damage [5]. In the case series by Gesundheit et al [7], 4 infants developed recurrent genital HSV lesions and 1 infant was diagnosed with central nervous system disease with neurological sequelae. Neonatal HSV, even when treated with early initiation of antiviral medications, may be fatal and result in seizures and devastating neurological complications. We therefore disagree with the suggestion of Leas and Umscheid [2] that prospective observational trials should be conducted to address the risk of DOS. This practice poses an avoidable and unnecessary risk of infection to the infant and is contradictory to the American Academy of Pediatrics’ recommendation that circumcision should not include DOS [10]. Thus any such trial, even if observational, would be unethical.

We also strongly disagree with the assertion of Leas and Umscheid [2] and of Berman et al [1] that only DNA evidence can prove a causal relationship between DOS and neonatal HSV infection. DNA evidence is not necessary to document the risk of DOS. Direct orogenital suction poses an incontrovertible risk of transmission of HSV and other infections, and the practice violates the basic standards of infection control. The epidemiological data linking the practice of DOS to neonatal HSV infection are overwhelming. For all of these reasons, DNA evidence is not necessary to prove that DOS poses an unacceptable risk to infants. In many ultraorthodox Jewish communities, the practice of DOS has been modified through use of a glass pipette to reduce the risk of infection transmission [7], although there is no evidence that this practice reduces risk. As pediatricians, we advocate that DOS should never be performed.

Rebecca Pellett Madan,1 Betsy C. Herold,1 Adam J. Ratner,2 Lisa Saiman,2 Anne A. Gershon,2 and Lawrence R. Stanberry2
1Department of Pediatrics, Division of Infectious Diseases, Albert Einstein College of Medicine, Bronx, New York
2Department of Pediatrics, Division of Infectious Diseases, Columbia University Medical Center, New York

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Corresponding Author: Rebecca Pellett Madan, MD, MS, Department of Pediatrics, Albert Einstein College of Medicine, 1300 Morris Park Avenue, Bronx, NY 10467. E-mail: rebecca.madan@einstein.yu.edu.

Response to Recent Review of Literature on Transmission of Neonatal Herpes Through Ritual Circumcision With Oral Suction

To the Editor—A recent review [1] suggests a relationship between neonatal herpes and Jewish ritual circumcision. Its lead author Mr. Leas—from UPenn Medicine’s Center for Evidence-Based Practice—was the lead analyst in an Evidence Review on that subject. It echoes that review’s conclusion by summarizing as follows: “The evidence base is limited by the small number of infections and incomplete case data,” adding the deficiencies of the only statistical study ever attempted, and “only genetic testing of HSV isolates can confirm transmission.”

Supporting their claim, the authors mention case studies “consistent with transmission . . . from mohel to infant, including the location of lesions, timing of symptoms, and disease typology.”

However, the UPenn review considered both location and timing of lesions insignificant: “Diapers are changed very frequently in infants, . . . increases [ing] potential exposure to infection in the genital areas.” Additionally, it is well documented that neonatal herpes
often presents in traumatized areas (eg, circumcision sites).

Regarding timing of lesions, these authors state: “The incubation period is also consistent with transmission in the hospital prior to discharge.” Furthermore, if Metzizah B’Peh (MBP) babies were infected on their circumcision day, the incubation periods would, in 25 of the 30 globally reported cases, be outside the reported ranges of 10–12 days (for disseminated and skin, eyes and mouth cases) and 16–19 days for the remaining central nervous system infections [2].

Additionally, the authors opine that the uniformity of serotype 1 herpes simplex virus (HSV) strains among MBP cases suggests an oral transmission. However, since sexually transmitted infections are extremely rare among ultraorthodox Jews, genital HSV (generally caused by type 2), would be exceptional in this population. If the source of neonatal infection were, for example, family contacts and caretakers with oral HSV (which is well documented in the literature), we would expect HSV1 infections to predominate within this population. We reviewed the specific circumstances of many of the referenced cases, and indeed, the only boy with MBP who died since New York City (NYC) began its investigation in 2006, spent, at the age of 4 days, hours in intimate contact, even sharing a pacifier, with his sibling, who had active HSV infection on his lips. Gutierrez et al. write [3]: “Relatives and hospital personnel with oral labial herpes may be a reservoir of virus for infection of the newborn,” referencing DNA matching as part of the evidence.

The authors suggest that case ascertainment in the NYC study may have been incomplete, resulting in an underestimate of the risk. Given the high level of attention through public health alerts and the media to the few MBP cases of neonatal herpes and the absence of any attention to all others, health care providers are more likely to suspect herpes in ultraorthodox Jewish boys and miss it in others. The past 8 years had 7 cases with 1 death among MBP infants, versus 90 cases with 17 deaths in non-MBP herpes.

We agree that “future research using cohort or case–control designs that fully capture all of the relevant data are needed to . . . examine the association.” Until then, one can only conjecture an association. (The only additional “evidence” since the UPenn review is one small Israeli study [4], with the same limitations as previous studies.)

No DNA testing has been done, no valid statistical analysis has shown this association [5], and circumstantial evidence is extremely weak.

Daniel Berman, 1 Brenda Breuer, 2 and Awi Fedegruen 3

1 Infectious Disease Specialist, Albert Einstein Hospital and Montefiore Medical Center
2 Institute for Innovation in Palliative Care, Metropolitan Jewish Health Systems
3 Charles E. Exley Professor of Management, Graduate School of Business, Columbia University, New York, New York

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Corresponding Author: Daniel S. Berman, MD, Infectious Disease Department, Montefiore Medical Center, 1825 Eastchester Road, Bronx, New York, NY 10461. E-mail: dberman614@gmail.com.
Journal of the Pediatric Infectious Diseases Society, Vol. 4, No. 3, pp. 284–5, 2015 © The Author 2015. Published by Oxford University Press on behalf of the Pediatric Infectious Diseases Society. All rights reserved. For Permissions, please e-mail: journals.permissions@oup.com. DOI:10.1093/jpids/piu111 Electronically published January 28, 2015