Practice."[1] Dr. Dancer is regarded the world over for her expertise, research, and advocacy related to improving patient safety through mitigating transmission of healthcare-associated pathogens from near-patient surfaces to susceptible hosts. We welcome the opportunity to respond to several points she raised in her letter.[2]

The methodological considerations she posed included the potential that the low heterotrophic bioburden (HBB) we found could have been a reflection of habitual exposure of environmental surfaces to disinfectants; differences in sensitivity between dip slides and swab cultures; potential shortcomings in the manner in which dip slides were used; and possible improved sensitivity of the dip slide system with 48 hours incubation vs 24 hours. All have validity and are worth considering in future studies. Given the essential identical thoroughness of cleaning and large number of data points in both arms of our study, we believe that the magnitude of the analysis and the manner in which the dip slide system was used led to a symmetrical distribution of any confounding variables that might have adversely affected the sensitivity of our quantitative findings. Indeed, the magnitude of the difference in potency between the 2 disinfectants (ie, the novel disinfectant was 1.93 times more potent than the quaternary ammonium disinfectant) and the high level of the relative difference ($P < .0001$) between the disinfectants clearly support the sensitivity of the dip slide system as it was used. Because the kinds of comparative studies for which this new paradigm may be used to compare the effectiveness of interventions may have substantially less differences between the 2 interventions, maximizing the sensitivity of the sampling system employed will be an important consideration in future studies.

While limitations in the length of our report precluded a more in-depth discussion related to hygienic standards, it is important to note that the study was not designed to directly analyze this issue. Our findings, by chance, provided further observations regarding the challenges of using HBB independently as a cleanliness standard, and we addressed the issue in the discussion section of our report.

As has been noted in the past[3] and as recently as this year,[4] many published reports have observed, as we did, that the generally low HBB on healthcare surfaces appears to limit the potential for assessing the effectiveness of surface cleaning practice unless it is performed on a comparative basis, as we did. We support Dr. Dancer's hope that "future work will demonstrate which density adequately reflects risk in a range of healthcare environments."[2,5] In addition, the concern that ongoing use of disinfectants over time can decrease residual HBB has recently been raised.[6] Further work in this area, particularly with the new disinfectants that do not damage patient area surfaces,[7] needs to be conducted.

ACKNOWLEDGMENTS

Financial support: No financial support was provided relevant to this article.

Potential conflicts of interest: The author reports having served as a consultant to Steris and Ecolab and has licensed patents to and receives royalties from Ecolab not related to this correspondence.

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Infect Control Hosp Epidemiol 2015;36(7):850–851

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Letter to the Editor Regarding "Impact of Vaginal-Rectal Ultrasound Examinations with Covered and Low-Level Disinfected Transducers on Infectious Transmissions in France" by Leroy et al.

To the Editor—A simulation study on the impact of vaginal-rectal ultrasound examinations on infectious risks in France was published recently by Leroy and colleagues.[1] Although statistical methods with Monte Carlo simulations could be contributive, we would like to raise some points which might limit the interpretation of their results.

The uncertainty of several parameters was possibly very wide, and simulation did not take such variability into account.

https://doi.org/10.1017/ice.2015.87 Published online by Cambridge University Press

The probability that a pathogen lingered on the probe after cleaning and disinfection was derived from data on bacterial agents in 2 single-center studies,[2,3] whereas most infections simulated by Leroy et al were viral. The probability of probe contamination from an infected patient was extracted from observational data on sexual intercourse. However, the probability of transmission differed according to type of sexual intercourse, inoculum or viral load.[4] Sexual exposure was most probably very dissimilar from endocavitary ultrasound exposure. With hepatitis C virus, the rate of transmission differed strongly between infection observed among drug users[5] and patients after nosocomial exposure, such as hemodialysis.[6] Similarly, with human immunodeficiency virus, the probability of infection after accidental blood[7] and male-to-female sexual exposures[4] is distinct with 0.003 and 0.0019 probability densities, respectively. Sensitivity analyses should have been conducted to properly interpret the results.

In a hypothetical cohort of 4 million exposed patients in France,[1] the authors ascertained that a mean (SD) of 40 (20) would be infected by human immunodeficiency virus and 151 (63) by hepatitis C virus annually. Recently, our group studied a French prospective, observational, hospital-based cohort of 16,474 individuals[8] and found that the incidence of human immunodeficiency virus seroconversion was 0 (n = 0) per 10,000 patient-years in patients with endocavitary probe exposure within 12 months before testing and 6.7 (n = 13) in nonexposed patients (log-rank test: $P = .64$). The incidence of hepatitis C virus seroconversion was 16.1 (n = 1) per 10,000 patient-years in patients exposed to endocavitary probes and 23.4 in nonexposed patients (log-rank test: $P = .69$).

In a letter published elsewhere,[9] our group underlined that statistical analysis of a previous meta-analysis by Leroy,[10] based on 2 published studies, would be questionable owing to lack of weighting according to study size. However, similar data were analyzed, again with a dearth of details regarding the calculation of pooled prevalence.[7] We agree with Leroy et al[1] that the issue of probe contamination is important and could be a public health concern, particularly with human papillomavirus infection related to endocavitary ultrasound exposure. Additional sensitivity analysis would have improved the accuracy of estimations in the present study.[1] Appropriate prospective investigations are needed with a view to proposing the best preventive measures for patient safety regarding these exposures.

ACKNOWLEDGMENTS

Financial support. None reported.

Potential conflicts of interest. All authors report no conflicts of interest relevant to this article.

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Infect Control Hosp Epidemiol 2015;36(7):851–852

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Reply to Bénet et al

To the Editor—We thank Bénet et al.[1] for their letter discussing the difficulties in evaluating the infectious risk linked to

https://doi.org/10.1017/ice.2015.87 Published online by Cambridge University Press