The dangerous implication of this study is that such attempts to control outbreaks of MRSA may be futile. Perhaps such is the case, but before we concede that we are impotent to contain a nationwide MRSA epidemic, we must scrutinize the methodology in studies like that of Bacon et al. We feel the following points are worth considering:

- 1. Rectal, vaginal, or other-site carriage may explain apparent "persistence" or relapse of nasal carriage with MRSA. Personnel identified as nasal carriers should have other sites cultured to document the possibility of reimplantation into the nose of the MRSA strain.
- 2. MRSA may colonize the nose in variable numbers.<sup>2</sup> Furthermore, MRSA usually shares the niche of the nasal mucosa with methicillin-susceptible *S aureus*. Screening for nasal MRSA may best be accomplished by plating nasal swabs to media such at Staphylococcal 110 agar (Difco, Detroit, MI) with and without 12.5 µg/mL methicillin. In this fashion, the ratio of MRSA to other staphylococci can be determined.
- 3. Phage typing may not be an adequate method to separate strains. More sensitive methods such as determining the plasmid content<sup>3</sup> or analyzing labeled proteins<sup>4</sup> should be used.
- 4. Patient carriers should be identified before discharged and isolated upon readmission. The effect of control measures may take more than several months to become apparent. It is not clear what constitutes a successful control program.<sup>5</sup>

Hospital infection control teams around our country are probably at a loss to decide the best approach to control MRSA. The study of Bacon et al does *not* aid our strategy since the

lack of rigorous methods leaves many questions unanswered. For instance, at their Michigan hospital, do they currently attempt to decolonize MRSA nasal carriage in medical and nursing personnel? At our institutions, we have good evidence of interhospital transmission by medical personnel. Thus, until definitive studies show that carriage of MRSA among personnel is not related to the transmission, we contend that hospital personnel with patient contact who are carriers should decolonized.

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# Validity of Water Temperature Study Questioned

## To the Editor:

Smith et al<sup>1</sup> reported on the effect of water temperature on bacterial killing in laundry in the May 1987 issue of Infection Control. They used an impression plate method to determine bacterial flora on fabric. They did not discuss their choice of this method.

The efficiency of an impression plate method for this purpose is in fact low and the variation range high. In a comparative study of three methods to assess bacterial counts on fabrics, Hoborn and Nyström<sup>2</sup> found that the geometric average of contact plate counts in three sets of experiments were ten- to a thousand-fold lower than those obtained with two homogenization methods. Thus, low temperature washing processes for hospital laundry should be validated with more efficient methods than impression plates, eg, a homogenization method.

Using textile pieces artificially contaminated with enterococci (Streptococcus faecalis NCTC 10927) and a homogenization method, Nyström<sup>3</sup> demonstrated an inactivation factor around 10<sup>4</sup> for a wash process at 50°C, whether the process was with water only or with the addition of a detergent, and around 106 when an alkalization to a pH above 11 and a subsequent neutralization with acid was added to the process. The effect of bleach was not assessed in the study because bleach was not always used in the laundry where the study was performed.

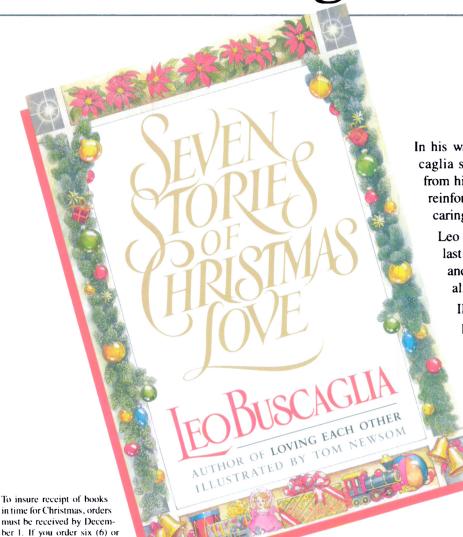
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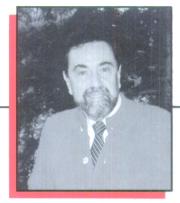
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