of-date and based on fairly superficial summaries in the sources reviewed. Large volumes of toxicity data are on file at FDA.

The effect of substantivity after washing with both these ingredients seems to confirm the reports already in the literature. These authors have chosen only certain elements out of the original Glove Juice Protocol and based their conclusions on miscalculation. I certainly think some changes in the Glove Juice Protocol are needed, but haphazard ones do a disservice to the products and to good science.

REFERENCES

1. Soulsby ME, Barnett JB, Maddox S: Brief report: The antiseptic efficacy of chloroxylenol-containing vs. chlorhexidine gluconate-containing surgical scrub preparations. *Infect Control* 1986; 7(4): 223-226.
2. *Federal Register*, September 13, 1974; January 6, 1978.
3. Rotter ML: Povidone-iodine and chlorhexidine gluconate-containing detergents for disinfection of hands. *J Hosp Infect* 1981; 2:273-280.

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To the Editor:

I have recently read the article by Soulsby et al,[1] wherein they compare a chloroxylenol-containing surgical scrub to Hibiclens. Since I am a supporter of the use of chloroxylenol in the proper circumstances, I find such work distressing. Allow me to address some of the points that I feel are incorrect.

The first point is the spelling of the material known chemically as 3,5 dimethyl, p-chloro-xylenol. This is also known as *chloroxylenol*, not *chlorxylenol*.

Another point is that while chloroxylenol is indeed a phenolic, chlorhexidine gluconate is a salt of a biguanide cation. They are *not* in the same chemical family.

There is no indication of the amount of either preparation that was employed in the test scrubs. It is well known that sponge material is capable of binding ingredients that are placed in contact with the sponge. This can include the chlorhexidine.

		Log Reduction	
	Hours	**Anti Sept**	**Hibiclens**
Day 1	0	.7478	.6429
	3	.4436	.4789
	6	.0995	.1793
Day 2	0	.8920	.7353
	3	.4967	.2607
	6	.1062 (incr.)	.0424
Day 3	0	1.0953	1.0676
	3	1.4405?	.6664
	6	.1957	.2953

The most distressing issue is their results. An 82% reduction is not a 1.9 log reduction; after all, a 90% reduction is only a 1.0 log reduction (eg, 100 − 10 = 90, or log 100 − log 10 = 1). The only way that they can obtain their data in Table 2 is to take the log of 82, which indeed is 1.91. However, 82% is not 82 but 0.82, a difference of a factor of 100.

Using their data in Table 1 to construct the proper table leads to the values shown above.

After having spent the past few years dealing with the activity of various antimicrobial preparations, I would consider both of these products to be inadequate for use as a surgical scrub, or the test is suspect. The data supplied by Dexide, Inc. on their chloroxylenol preparation shows it to be substantially more efficacious than either product showed in this test. Also, there are a number of independent studies on Hibiclens that would make this study suspect.

I would hope that the authors would submit a detailed (including raw data) correction so that this study can be properly evaluated.

REFERENCES

1. Soulsby ME, Barnett JB, Maddox S: Brief report: The antiseptic efficacy of chloroxylenol-containing vs. chlorhexidine gluconate-containing surgical scrub preparations. *Infect Control* 1986; 7(4):223-226.

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To the Editor:

I was disappointed to read the report by Soulsby et al published in the April issue.[1] I was somewhat confused in my attempt to decipher the data presented in this report and question some of the conclusions based on these data. Of principal confusion were the data transformation steps performed to obtain the "log reduction" and "percent reduction" values given in Table 2 as derived from the actual log bacterial count data in Table 1.

The high initial dilution of the hand samples (ie, 1:10,000) as stated in the Methods section of the paper dictates a minimum log recovery of 4.00 per hand. From this fact and the baseline values given in Table 1, one can calculate that log reduction values of greater than 1.6 and 1.7 for Anti Sept and Hibiclens, respectively, are impossible. Yet, log reduction values of 1.9 are reported in Table 2.

Also, the authors apparently derived the percent reduction values in Table 2 by taking the antilog of the corresponding log reduction values. This is not correct. Actual numbers for this parameter should be close to 99% for all of the reductions reported. Contrary to the authors' statement in the Results section, there is no significant difference between any of these reduction values.

Certain statements in the report raise several other questions that should have been corrected or clarified prior to publication. These relate primarily to test methodology and data analysis which leave the reader wondering how specific conclusions were drawn. For example, 1 mL from a 50 mL sample into 299 mL does not