Letters To The Editor

Nurses Suggest Further Question for Study of Sensations After Breast Cancer Surgery

We read with interest the article "Eighteen Sensations After Breast Cancer Surgery: A Comparison of Sentinel Lymph Node Biopsy and Axillary Lymph Node Dissection" in the May issue of *Oncology Nursing Forum* (*ONF,* Vol. 29, pp. 651–659) by Roberta H. Baron, RN, MSN, AOCN®, and colleagues. Oncology nurses at a nationally known cancer center are conducting an important study of patients' symptom experiences after undergoing this relatively new surgical procedure. We appreciate the authors bringing this valuable research forward.

Because of our interest in this area of research (National Institutes of Health-funded research: Prospective Nursing Study of Breast Cancer Lymphedema NINR# 1RO1 NR05342-01), we have some thoughts that may add to the potential knowledge development in this field.

Sensations after breast cancer surgery have not been well researched. Now, with the relatively new surgical procedure known as sentinel lymph node biopsy (SLNB), there is additional reason to assess sensations and, at the same time, compare and contrast this procedure with the more traditional axillary lymph node dissection (ALND). We believe it is important to identify a possible misconception that ALND "*was* the standard surgical procedure" whereas "SLNB *has become* a standard of care for this patient population" (Baron et al., 2002, p. 652). SLNB obviously is the standard of care for patients with early-stage breast cancer at Memorial Sloan-Kettering Cancer Center (MSKCC), a federally designated comprehensive cancer center in New York, NY. Clinical trials are ongoing to assess the survival impact of SLNB as compared to ALND. Although SLNB recently was added to the 2002 edition of National Comprehensive Cancer Network (NCCN) practice guidelines, it was with the requisite that experienced clinicians perform the procedure (Susman, 2002). Robert W. Carlson, MD, professor of medicine at Stanford University Medical Center in California and chairman of the new NCCN breast guidelines, acknowledged that adding SLNB was ahead of scientific confirmation that it provides a survival advantage over ALND. In the future, SLNB likely will become the new standard of care, but readers must keep in mind that SLNB is not being performed currently in many areas of the United States.

Regarding the purpose and objectives of Baron and colleagues' study, we would like to submit an additional research question for consideration by the researchers. Certainly, identifying sensations that can be discussed with patients preoperatively has significance in providing accurate patient expectations postoperatively. But what are the potential causative factors for persistent sensations postsurgery? A plethora of possible factors exists (e.g., traumatic nonhealing injury to the tissues, lymphatic obstruction, early lymphedema, chronic nerve injury).

Perhaps an additional research aim for this sizable sample would be to identify whether a correlation exists between persistent postoperative sensations and changes in arm circumference (or volume) over time. This study reportedly will collect and analyze data at 12 and 24 months. With a modest increase in resources, the study could measure arm circumferences at these two data points because the sensations described in the article frequently are the same sensations associated with lymphedema (Armer & Whitman, in press) commonly diagnosed when a 2-cm (or 200-cc) difference is detected between the affected and unaffected limbs (Gerber et al., 1992). Arm measurements comparing changes in affected and unaffected limbs likely would provide extremely helpful information. We understand that, because of limited time and distance, these women do not always return to MSKCC for follow-up, thus the probable justification for the telephone follow-up interviews. If that is the case, then a patient's primary care physician or nurse could perform the circumferential measurements as per protocol developed by the research team and report the results by telephone or fax to Baron and colleagues. An alternative might be to add circumferential measurements for only those participants returning to MSKCC for follow-up.

Finally, the researchers measured sensations using the Breast Sensation Assessment Scale (BSAS). At this point in the instrument's development, is the focus on the four subscales preferable to simply evaluating the set of individual sensations experienced? We understand why numbness might be a better fit in the subscale paresthesias, as reported in the pilot study (Baron et al., 2000), but we would like to better understand how to justify assigning it to that subscale when high correlation exists with the discomfort category. In summary, in order to consider using this tool, we would like to better understand the "clinically rational relationship" of the subscales (Baron et al., 2000, p. 219) and how the development of the subscales contributes to the overall findings of the study.

We commend the authors for bringing the symptom experience after breast cancer treatment to heightened awareness among health professionals. Through collaboration between research teams and expert clinicians, we can increase our understanding of important clinical issues and, ultimately, improve the quality of life of survivors of breast cancer.

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The Author Responds

The authors wish to thank the writers for their careful critique of our breast sensations study. We agree that SLNB is not *the* standard

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of care nationwide; we intended only to point out that it has become *a* standard of care at many institutions. Moreover, that is why we believe it is important to compare the morbidity of SLNB with that of ALND.

In their letter to the editor, Wendy J. Evans, RN, MS(N), AOCN®, and Jane M. Armer, PhD, RN, suggest that an additional research aim could be to correlate sensations with the presence of lymphedema. Such research is ongoing at MSKCC. Baseline and 12-month measurements of arm circumference were reported in a recent manuscript from MSKCC (Temple et al., 2002). At 12 months postsurgery, no correlation was found between sensations and lymphedema in the study population.

Regarding the use of subscales versus individual sensations, we believe that knowledge about the individual sensations is clinically more informative to both patients and clinicians in understanding postoperative morbidity. The subscales were created as a statistical tool, both to validate the instrument (Baron et al., 2000, 2002) and summarize outcome data for statistical comparison (Temple et al., 2002).

We appreciate the writers' interest in and comments on our study. Appropriate patient education and support require that clinical staff understand related quality-of-life issues for women with breast cancer. Through further research, including collaborations within the broader research community, such as those mentioned by Evans and Armer, we will continue to enhance our understanding of the needs and experiences of our patients.

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Baron, R.H., Fey, J.V., Raboy, S., Thaler, H.T., Borgen, P.I., Temple, L.K.F., et al. (2002). Eighteen sensations after breast cancer surgery: A comparison of sentinel lymph node biopsy and axillary lymph node dissection. *Oncology Nursing Forum, 29,* 651–659.

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Article About Cancer Screening Has Several Limitations

This letter is in response to the review article "Population-Based Cancer Screening" by Victoria L. Champion, DNS, RN, FAAN, Susan M. Rawl, PhD, RN, and Usha Menon, RN, PhD, published in the June issue of *ONF* (Vol. 29, pp. 853–861). The article certainly contained an abundance of information, including a brief screening primer, summaries of cancer statistics, and current adherence to guidelines and related issues for breast, cervical, ovarian, prostate, colorectal, skin, and lung cancers. Although we agree with the authors' conclusions regarding the need for research that addresses preventive behaviors and screening compliance, we would appreciate the opportunity to discuss a number of important issues.

The first relates to definitions of particular biases frequently encountered in epidemiologic research. In their discussion of two common but important problems that can be avoided by randomizing screened versus nonscreened groups, the authors muddled their description of the two biases common to epidemiologic studies and, in this context, cancer screening. The first, which the authors called lead-time difference, is more commonly known as lead time bias. The authors correctly defined this bias as "the time by which screening advances diagnosis of disease" (Champion, Rawl, & Menon, 2002, p. 854). However, the example provided does not describe lead time bias. If an individual's cancer diagnosis is pushed back one year because of screening, that individual automatically will survive one year longer than another individual who has an identical malignancy but was not screened. Survival time begins at the point at which a diagnosis is made, and, by definition, cases found through screening are detected (and diagnosed) earlier than their nonscreened counterparts. For a screening program to be considered beneficial (i.e., the use of screening reduces mortality), it must advance the point in the natural history of the disease at which effective treatment is offered, and this treatment must be a significant contributor to the increase seen in survival (Rothman & Greenland, 1998). The second bias, known as length time bias, refers to the fact that screening programs are more likely to detect slow-growing versus aggressive tumors; the latter are more likely to progress quickly, cause symptoms, and lead an individual to seek diagnosis and treatment between screens (i.e., aggressive tumors are more likely to be "interval" cancer) (Miller, 1985). A third problem that deserves mention is known as selection bias and refers to the fact that individuals who choose to participate in screening programs have a different probability of developing disease (including cancer) than the general population (Miller). Each of these biases must be understood and considered when evaluating the benefit of any screening program. *Modern Epidemiology* by Rothman and Greenland offered an excellent, in-depth discussion of important issues related to screening.

Second, we were surprised to read that 76% of African Americans have had a mammogram within the past two years. After inspection of the original source (which was incorrectly referenced; the *CDC Surveillance Summary* of interest appears in Volume 49, SS-2), the reason for this seemingly high rate became apparent. Table 19 in the *CDC Surveillance Summary* presents the percentage of women ages 50 and older who reported having had a mammogram in the past two years, by race and state (U.S. Centers for Disease Control and Prevention, 2000). This table summarizes results from the Behavioral Risk Factor Surveillance System (BRFSS), an annual telephone survey that uses random digit dialing. Traditionally, underserved populations, who are less likely to have access to health care and screening services, are the same populations without telephone service and, therefore, are not adequately represented in this type of survey. Furthermore, the two-year mammography compliance rates by race cited by the authors (white, 74%; African American, 76%; Hispanic, 64%) are median rates, calculated from widely varying state-specific rates (white, 59%–90%; African American, 44%–86%; Hispanic, 60%–80%). The median values were not labeled as such or discussed in a manner allowing readers to properly interpret their meaning. After a closer look at the original source, we found

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that the median value for African Americans was calculated using data from only 20 states; the median value for Hispanics was calculated using data from only 4 states. If the goal is to educate healthcare professionals regarding the proportion of women adhering to breast cancer screening guidelines (which, in our opinion, still holds much room for improvement), we hope readers understand that the two-year mammography rates are lower (white, 68%; African American, 66%; Hispanic, 61%) (Smith et al., 2002) and the guideline compliance rate (i.e., the rate at which age-eligible women receive an annual mammogram plus a clinical breast examination) is lower still.

Finally, we would like to refute the statement, "Little is known about the etiology of colorectal cancer" (Champion et al., 2002, p. 856). A pathophysiologic model has existed for years. The Vogelstein model gives a detailed description of the genetic events associated with various steps of the progression of this cancer (Vogelstein et al., 1988), and, as mentioned in the review article, a multitude of epidemiologic research studies have demonstrated the effects of numerous risk factors on the development of colorectal cancer.

We applaud the attempt of Champion and colleagues (2002) to bring together such a huge body of literature on cancer screening. Thank you for the opportunity to further discuss these issues.

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Champion, V.L., Rawl, S.M., & Menon, U. (2002). Population-based cancer screening. *Oncology Nursing Forum, 29,* 853–861.

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The Author Responds

Thank you for allowing me to respond to the letter written by Teresa D. Hill, PhD, and Hans J. Berkel, MD, PhD. I applaud their careful reading of this article, and although I disagree with several conclusions, I believe that this type of discussion is essential to developing good science. I will outline the issues addressed in this letter and my response.

1. The authors describe lead-time difference, which normally is described as lead time bias. The example provided does not describe lead time bias. If an individual's cancer diagnosis is pushed back one year because of screening, that individual automatically will survive one year longer. Survival begins at the point at which a diagnosis is made and does not address mortality benefit.

   Response: Hill and Berkel are correct in that lead time bias is the term most commonly used, not lead-time difference. This was a typographical error that I did not catch. The example of lead time bias, although not incorrect, was not as clearly developed as it could have been. We argued that without randomized trials, cancer might be detected earlier without an effect on outcomes. This is a true statement and indicative of lead time bias. We agree that the discussion of rapidly progressing cancer more logically belongs with a discussion of length time bias, which was not directly addressed. We thank Hill and Berkel for providing readers with a more detailed discussion of both lead time and length time biases.

2. The authors did not describe length time bias and selection bias. Each must be understood.

   Response: Although I agree that understanding all types of screening biases is important, this article was not intended to review all issues. As Hill and Berkel illustrated in their discussion of lead time and length time biases in their letter, these are complex concepts that require substantial detail to fully explain. The article would have been lengthened significantly with the inclusion of detailed discussions of lead time bias, length time bias, selection bias, and overdiagnosis bias. In addition, Black, Haggstrom, and Welch (2002) recently described two additional forms of bias that screening trials may be subject to: sticky diagnosis bias and slippery linkage bias. I strongly suggest that readers who are interested in learning more about the biases that potentially affect outcomes of screening trials consult the resources suggested by Hill and Berkel.

3. Hill and Berkel were surprised to read that 76% of African Americans have had a mammogram within the past two years. The original source was incorrectly referenced. The authors did not address problems with this source such as random digit dialing, median values, and data collection for African Americans in 20 states.

   Response: I am not clear as to why the authors indicated that referencing was incorrect. I reviewed the source and found it to correctly report what we had addressed and referenced. Hill and Berkel are correct that the reference uses BRFSS data. *Morbidity and Mortality Weekly Report* is widely cited for summary statistics using BRFSS data. Although I do not dispute the concerns raised about the limitations of this widely cited national data, this article was not intended to critique BRFSS. Every data set has limitations, and I believe that a detailed critique of BRFSS data collection methods would have been inappropriate content for this article.

4. We would like to refute the statement, "Little is know about the etiology of colorectal cancer."

   Response: This statement is referenced from Winawer and colleagues (1997). Winawer is a widely recognized international expert. Although the adenoma-carcinoma sequence is a well-known model of colorectal carcinogenesis, scientists do not know what initiates this sequence. A recent article by Anderson and colleagues (2002) indicated that current evidence "suggests that colorectal cancer results from the accumulation of diverse structural and functional genomic aberrations" (p.1127). These genetic alterations usually are random, and although epidemiologic evidence that dietary (fat, vegetables, folate) and lifestyle factors contribute to risk exists, the etiology of colorectal cancer is not yet known.

In conclusion, Hill and Berkel obviously are very knowledgeable about the fundamentals of screening. The intent of this article was not to provide an in-depth review of the issues related to screening, but rather to provide a general overview of the state of cancer screening. I would, however, like to thank them for the obvious time and thought required to develop their response. If more scientists were this concerned, our products would be much better. Keep up the good work.

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