Letters

Re: Invited Discussion of Silicone Gel Breast Implant Failure: Evaluation of Properties of Shells and Gels for Explanted Prostheses and Meta-analysis of Literature Rupture Data

We appreciate the opportunity to now comment on the invited discussion that was published contiguously with our paper in the September 2002 issue of this journal. We first read this invited discussion when our paper was published. It was authored by Brandon HJ, Young VL, Wolf CL, Jenna KL, Watson ME, and McLaughlin JK and was stated to have been prepared by the Center for Implant Retrieval and Analysis at Washington University, St. Louis, MO. It would seem appropriate to know the extent of any financial or other assistance they have received from Dow Corning or other commercial entities. They list only the National Endowment for Plastic Surgery.

We feel that only a rather brief response to the Brandon et al discussion of our paper is needed as follows:

It is first important to reemphasize two key points concerning our paper, which they appear to have missed or misunderstood:

1. Our meta-analysis should not be regarded as biased or based on "selected" data because it encompasses results from 42 separate clinical reports involving almost 10,000 implants. It is therefore by far the largest, most diverse, and most comprehensive meta-analysis concerning silicone gel implant failure based on clinical explant data that has been reported to date.
2. The Brandon et al discussion does not give sufficient emphasis to the significance of the recent *noninvasive* MRI evaluation of implant rupture status by Brown et al[1,2] (from the FDA and NIH), which is mentioned in our paper (pp. 240–241). The results of that research, which analyzed the status of implants by noninvasive MRI, are in complete agreement with our explant meta-analysis results for time-dependent rupture and frequency of additional surgeries.

Brandon et al stated that they were "puzzled" that we "neglected to reference almost all" their published data. If they will look closely, they will find that we actually did reference 3 of 8 of their reports, and we even used one in our meta-analysis. We would hope to see analytical data for many more implants from their group in the future, if they have access to such data. Actually, we find many of their papers lack details for such data as numbers tested, numbers ruptured, rupture versus time, and adequate details for mechanical testing (ie, ASTM or ISO methods), crosshead speeds, and standard deviations. The 42 studies we used in our meta-analysis all clearly provided the essential data required for the analysis of variance statistics and exponential regression failure analysis presented in our paper.

Brandon et al are argumentative about some details concerning the 1971 Dow Corning data reported by Manikian,[3] which describe the weakening of shells by swelling with silicone fluid (our reference 15). However, it is a fact that the tensile and tear strengths of shells were shown to significantly decrease due to shell swelling after gel-filled implants were tested at times of 1 month and at 4 to 5 years after manufacture. Similar results have also been obtained by other manufacturers. Furthermore, although the 1971 data should have resulted in routine quality control testing of shells from gel-filled implants, it is uncertain whether this was done by the manufacturers. From a mechanistic standpoint, as discussed in our paper, it is sufficient to emphasize that there can be little doubt that this swelling causes shell weakening and is a major factor in the time-dependent, cyclic stress-induced rupture of gel implants regardless of the extent of any possible additional in vivo chemical degradation of the silicone elastomer shells.

Brandon et al expressed concern about the possible "selection" of implants for which we conducted comprehensive physical property tests. As noted in our paper, we tested all explants received from various surgeons that could be tested. The exceptions were those implants (23 of 74) that were too fragile to be handled for sample preparation. The implants therefore "selected" themselves for testing. If the Brandon et al group have access to many more explants and implants in their inventory, why have they reported data for so few? Are there selection factors in their own studies? And if they have in fact had access to a greater number of explants, what have they found in terms of rupture versus time, change in mechanical properties, etc.?

We have received favorable informal personal peer comments on this paper. This is comforting because our goal has been to provide more scientific information to help improve patient counseling and preaugmentation informed consent. It is our view that plastic surgeons and their patients need more specific and more quantitative information than has usually been provided in product inserts to determine what their reasonable expectations should be concerning probable complications, their prevalence and severity, the probable life of the implant, and the likelihood and frequency of additional surgeries. Breast implant surgery should not be construed as a trivial matter for the patient and, like hip replacements or small-diameter vascular grafts (where the limited life and risk-benefit issues for these implants are usually well reviewed in informed consent), the risk-benefit issues for breast implants need to be as clearly understood by patients as possible before surgery.

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References

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Reply

We were pleased to have the opportunity to respond (in the form of an Invited Discussion[1]) to the 2002 article by Marotta and colleagues (comments on March 23, 2003 Letter to the Editor by Marotta JS, Goldberg EP, Habal MB, Amery DP, Martin PJ, Urbaniak DJ, Widenhouse CW)[2] and also appreciate this chance to address their comments to us. Their letter does nothing to change our evaluation of their original article and the data selectivity, data omissions, and data misrepresentations it contains. We must first state that we received no funding from Dow Corning or any other commercial entity to support the preparation of our Invited Discussion or this letter. The Center for Implant Retrieval and Analysis at Washington University is supported by the National Endowment for Plastic Surgery and the Aesthetic Surgery Education and Research Foundation. In the past, we have received unrestricted gifts from Dow Corning to support our basic breast implant research, and this support has been denoted in our publications where applicable.

Our response to the letter by Marotta et al. follows the order of their comments:

All the explant studies underlying the meta-analysis of Marotta and colleagues are biased by the self-selection of patients. Because patients who undergo explantation select themselves for surgery, they cannot be considered a random or representative sample of the population of all women with breast implants. Any investigation of explanted devices (many of which are removed because they failed) is inherently confounded by this same bias. The study of Brown et al,[3] which used MRI to assess implant status, used a less biased design to determine whether a breast implant has ruptured. Yet even this cross-sectional MRI study suffers from selection bias in that women volunteered themselves to participate and approximately 70% of their implants were Surgitek devices, which have been found more likely to fail than implants made by other manufacturers.[4] Another weakness with the Brown et al[3] study is that 92% of the implants imaged with MRI could be classified as "second-generation" devices, which are less likely to be intact than first- or third-generation implants.[5,6] Consequently, even this MRI study is limited because data are weighted toward a single manufacturer and implant generation and, therefore, cannot be considered an unbiased or representative sample of breast implants.

Our problem with the way in which the University of Florida researchers refer to our work remains their selectivity, omission, and misinterpretation of data. We indeed looked very closely at the references cited by Marotta et al in their paper. They referred to none of our peer-reviewed publications, at least nine of which were available by the end of 2001 (when the original article was accepted). They did cite meeting presentations, two in the text and two (not one as stated in the preceding letter) in Figure 12. The Wolf reference (number 10) in Figure 12 is illustrative. They use that presentation as the basis for plotting a data point of 80% implant failure at 16 years when, in reality, that particular investigation examined gel viscosity, had nothing to do implant failure, and involved only five explants, four of which happened to be ruptured. Five intact explants could just as easily have been studied, but implant integrity was not our purpose. The use of these five explants as a data point for predicting failure versus implant duration is absurd. For anyone confused about our implant testing protocols, a recently published explanatory article describes how we analyze silicone gel and saline-filled breast implants.[7]

There is no need to repeat what was said in our earlier Discussion regarding implant shell weakening as a result of swelling with silicone fluid from the gel. Although shell swelling does reduce an implant's overall mechanical properties, we have seen no evidence that swelling is the major factor in the time-dependent cyclic stress-induced rupture of gel implants, as Marotta and colleagues assert. In fact, our studies have found that the effect of cyclic sorption stresses does not lead to an appreciable degradation of the basic shell structure. In addition, we have determined that various types of Dow Corning silicone gel explants have remained intact despite swelling of approximately 20% to 40% for implantation times ranging from 13 to 32 years.[8]

Our approach in conducting breast implant research is quite different from that of Marotta and colleagues. We find no value in plotting numbers of explants that have failed according to implantation duration. Our work has proven to us that erroneous conclusions can be drawn when explants are not compared with proper controls and when differences between manufacturers, implant models, and implant/patient history are ignored. We have focused our work on specific questions, such as testing explants and controls to determine what happens to the material properties when implant shells are exposed to a physiologic environment. These studies have covered the entire range of implantation times that are available to date and include the oldest known silicone gel explants from first, second, and third generations (including two Cronin seamed explants removed intact after 32 years). We have tested the largest known inventory of explants with lot-matched controls as well as the oldest

saline explants (removed intact after 22 and 23 years) that have been analyzed to date. These investigations have shown that variations in the original shell properties must be considered when analyzing explants implanted between the middle 1960s and early 1990s. We have also analyzed explants to demonstrate how surgical instrument damage during implantation and explantation surgery can cause or look like a rupture. We have studied the effect that implantation surgery itself (ie, placing an implant in the pocket) has on the strength properties of silicone gel breast implant controls.

We could plot the status of every explant in our inventory on a graph according to integrity status and time in vivo, but the result would not help determine the failure rate or identify when and why breast implants are likely to fail. Moreover, the explants in our inventory are a biased sample and certainly not representative of all explants, let alone all still-implanted implants. We therefore will not use rupture data from our inventory to predict the failure characteristics of the general population of silicone gel breast implants.

We can agree with the stated goal of Marotta et al when they say they want to "provide more scientific information to help improve patient counseling and preaugmentation-informed consent." We disagree, however, that their meta-analysis is based on sound scientific principles; it is fraught with bias and misrepresentation (eg, the Wolf example discussed earlier). Furthermore, how does the meta-analysis improve patient counseling today? Women who have received more recently designed silicone gel implants—and those who choose augmentation in the future—want to know how long their implants can be expected to last. The approach taken by Marotta and colleagues does not help answer that question.

We agree with the general observation that breast implants seem more prone to fail over time. However, time alone does not seem the most likely reason for failure. After many years of testing explanted devices and appropriate controls, we still cannot fully explain the mechanisms of failure, although we think that several mechanisms are probably involved, including in vivo processes such as abrasion, inappropriate handling of an implant prior to its placement, implantation and/or explantation surgery, and breast or implant trauma. It appears that the thinner elastomer shells characteristic of second-generation implants are more likely to fail than thicker-shell devices. The evolution of implant designs over time illustrates the danger of graphing failure versus time. Because all the data points plotted by Marotta and colleagues cannot be considered equal, we still contend their meta-analysis tells us very little about breast implant failure and creates more confusion than clarity.

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Re: Nodal Metastatic Melanoma in the Neck of a 4-Year-Old Girl After Diagnosis of Spitz Nevus of the Cheek

The report by Reynolds and colleagues (*Ann Plast Surg*. 2003;50:555–557) of nodal metastatic melanoma arising after the diagnosis of a Spitz nevus serves as an important reminder of the difficulties in histopathological diagnosis of melanocytic lesions. We think that proper nosology is of paramount importance in the medical literature. For this reason, we take exception with one phrase in the authors' otherwise well-written report. The last paragraph of the discussion starts with "Clearly, most Spitz nevi are entirely benign. . .." We maintain that *all* Spitz nevi are benign.

If a melanocytic proliferation diagnosed as Spitz nevus metastasizes, it was diagnosed incorrectly. If the pathologist does not think a lesion is benign, the term "nevus" should not be affixed to it; rather, it should be diagnosed as a "melanocytic proliferation, see note" with an attached statement about one's uncertainty of its biology, or it should be referred for a second opinion to someone who might have more experience in diagnosing definitively such a lesion, or, in some cases, both.

We thus agree with the authors that melanocytic lesions for which the diagnosis is uncertain should be labeled as such and that there is no shame in being uncertain about the biology of such a lesion.

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Reply

I agree with Moody and colleagues that the phrasing of the sentence is misleading and I would like to change it accordingly. The original text reads: Clearly, the majority of Spitz nevi are entirely benign, and it would be wrong to suggest otherwise to an already anxious parent. However, it is those lesions that are difficult to categorize, the so-called MUMP lesions, which would warrant closer observation.

The text should be revised to read: Clearly, all Spitz nevi are benign. However, it is those lesions that are difficult to categorize the so-called MUMP lesions that would warrant closer observation.

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