Silicone Gel Breast Implants

To the Editor:

The report of statistically increased risk of fibromyalgia (FM) in women whose silicone gel breast implants had ruptured received media attention despite its obvious serious flaws. First, of women who chose to have implants for cosmetic reasons, the majority (only a minority received them after mastectomy) clearly differ in personality traits and characteristics and self-image from other women. Second, the authors of this study did not ascertain which women had commenced legal actions, and it is well known that the trial lawyers had coached the women’s answers and their favorite physicians used questionnaires...the authors acknowledged that the bias due to the questionnaires was likely to have influenced the results...Third, FM may have started out as a valid catch-all phrase to help in communication among clinicians, but it no longer has any validity except to designate women...and, in my opinion, are the first to start an epidemiologic analysis involving breast implant rupture. While Brown, et al did not present health outcome results for these 3 categories, they provided enough data to do this analysis. When I did, the data showed that fibromyalgia (FM) was indeed not associated with ruptured implants, neither for intracapsular nor extracapsular rupture.

From the authors’ Table 4, I calculated the number of women in 3 rupture status categories: those with intact implants, those with apparently intracapsular rupture, and those with extracapsular rupture (Table 1). Following Brown, et al, Table 1 also displays the number of women by the 2 implant status comparisons the authors reported: (1) any ruptured implant (either intracapsular or extracapsular) versus intact implants, and (2) extracapsular rupture versus intracapsular rupture and intact. The odds ratio (OR), while not given in Table 4 by the investigators, between implant status and health outcome is shown for the 3 conditions the authors found as potentially associated with extracapsular silicone: PM, Raymond’s phenomenon, and “other connective tissue disease.”

The OR between FM and extracapsular rupture (compared to intact devices) is 1.88 and not statistically significant. The OR between FM and intracapsular rupture (compared to intact devices) is 0.50 and also not statistically significant. The OR for any rupture versus intact devices is 0.87. It is obvious from Table 1 that the largest difference in FM risk is between extracapsular and intracapsular rupture. If a gradient in risk exists, these data seem to support a gradient for FM that is: intracapsular rupture < intact < extracapsular rupture. (While not statistically significant, these data surprisingly seem to point towards a FM risk for women with intracapsular rupture that is half that for women with intact implants, suggesting that intracapsular rupture may protect women against FM). Intuitively, if an association exists between implant status and PM, one would hypothesize based on clinical reasoning that the true gradient would be: intact < intracapsular rupture < extracapsular rupture. Brown, et al’s data plainly depict no consistent association between ruptured implants of any type and PM when compared to intact devices. The OR for the comparison the authors reported (i.e., extracapsular ruptured...
To continue please scroll to next page
Table I. Implant status by self-reported physician diagnosis of 3 health outcomes. Values are expressed as the number of women and odds ratios.

<table>
<thead>
<tr>
<th>Implant Status</th>
<th>Yes</th>
<th>FM No</th>
<th>OR (95% CI)</th>
<th>Raynaud’s</th>
<th>OR (95% CI)</th>
<th>Other CTD*</th>
<th>OR (95% CI)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Extracapsular</td>
<td>18</td>
<td>55</td>
<td>1.88 (0.83-4.29)</td>
<td>6</td>
<td>67</td>
<td>1.84 (0.45-7.94)</td>
<td>9</td>
</tr>
<tr>
<td>Intracapsular</td>
<td>13</td>
<td>150</td>
<td>0.50 (0.21-1.17)</td>
<td>3</td>
<td>160</td>
<td>0.39 (0.059-2.04)</td>
<td>6</td>
</tr>
<tr>
<td>Intact</td>
<td>16</td>
<td>92</td>
<td>1.13</td>
<td>5</td>
<td>103</td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td>Any rupture</td>
<td></td>
<td></td>
<td></td>
<td>9</td>
<td>227</td>
<td>0.82 (0.24-3.18)</td>
<td>15</td>
</tr>
<tr>
<td>Intact</td>
<td>16</td>
<td>92</td>
<td>1.13</td>
<td>5</td>
<td>103</td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td>Extracapsular and intact</td>
<td>18</td>
<td>55</td>
<td>2.73 (1.32-5.49)</td>
<td>6</td>
<td>67</td>
<td>2.94 (0.81-10.01)</td>
<td>9</td>
</tr>
<tr>
<td>Intra caps and intact</td>
<td>29</td>
<td>242</td>
<td></td>
<td>8</td>
<td>263</td>
<td></td>
<td>10</td>
</tr>
</tbody>
</table>

FM: fibromyalgia; CTD: connective tissue disease.
* Dermatomyositis, polymyositis, Hashimoto’s thyroiditis, mixed connective tissue disease, pulmonary fibrosis, eosinophilic fasciitis, polymyalgia.
1 Reference group in OR calculations.
2 95% CI for the OR are by exact methods. 9 Derived from Brown, et al Table 4: 236 (No. of women with ruptured implants) × 0.131 (proportion of women with ruptured implants who self-reported a physician diagnosis of FM) = 31 women with ruptured implants self-reporting physician diagnosed FM. 73 (No. of women with extracapsular silicone) × 0.247 (proportion of women with extracapsular silicone who self-reported a physician diagnosis of FM) = 18 women with extracapsular silicone who self-reported a physician diagnosis of FM. The number of women with apparently intracapsular rupture self-reporting a physician diagnosis of FM is 31 – 18 = 13. Other table sample sizes are similarly calculated. Values in bold face are statistically significant.

ture versus intact plus intracapsular rupture) is 2.73 and is statistically significant. However, this is an epidemiologically inappropriate comparison (i.e., by the combining of intact implants with intracapsular rupture) given the authors’ own data that show substantial differences in the OR for FM across the 3 implant status categories. By combining the intracapsular rupture and intact implant groups, Brown, et al decreased the FM frequency from 14.8% (in the intact-only group) to 10.7% (in the combined group). A consequence, the difference in FM frequency between the extracapsular rupture group (FM frequency = 24.7%) and the intact/intracapsular rupture group widened beyond that which would have been found if only the intact group was used for the comparison. Combining the implant status categories this way caused the OR to increase and produce a perception of FM risk that the data did not support.

For Raynaud’s phenomenon, a parallel analysis of Brown, et al’s data similarly shows (like FM) no evidence of an association for extracapsular rupture (Table 1). For the “other connective tissue disease” category, I found a borderline statistically significant association (OR = 3.67) for extracapsular ruptured devices compared to intact implants. According to the authors this category comprised 7 different self-reported physician-diagnosed conditions: dermatomyositis, polymyositis, Hashimoto’s thyroiditis, mixed connective tissue disease, pulmonary fibrosis, eosinophilic fasciitis, and polymyalgia. Data for 9 women with extracapsular rupture were spread out over these 7 self-reported diagnoses, resulting in no clear disease pattern. I find it difficult to interpret the findings for this non-specific category. I agree with the authors: “It is not possible to determine whether any of these disorders predominated. Our category of other CTD is artificial and therefore those results are difficult to interpret.”

In summary, for Brown, et al’s data, I argue that it is biologically and clinically more relevant and epidemiologically more appropriate to categorize (and analyze) implant status into 3 categories: intact, intracapsular rupture, and extracapsular rupture. When I analyzed Brown, et al’s data this way, it became clear that no association was evident between extracapsular rupture (in fact, for ruptured devices in general) and FM. If the authors believe that intact implants and implants with intracapsular rupture can be plausibly collapsed in the analysis, I respectfully ask them to explain their rationale, especially in light of their own data. Based on 2 of the authors’ written comments about the lack of information on breast implant rupture and disease, it seems sensible to me they would want to explore and provide information about the association of health conditions across both types of implant ruptures. If the data are analyzed as I maintain is more appropriate, then women with breast implants can be reassured that ruptured devices have not been found to be associated with FM in this study.

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Dr. Bowlin is an employee of Dow Corning Corporation.

REFERENCES

Drs. Brown and Pennello reply

To the Editor:

We appreciate the opportunity to further discuss our results. Dr. Ehrlich voices concern that women with silicone gel breast implants differ in personality traits and characteristics from other women. However, because our study only included women with silicone breast implants, that should not be an issue. Second, he believes that trial lawyers may have influenced women’s response to our questionnaire. If the women in this study misreported diagnoses of connective tissue disease or symptoms, for any reason, then that would tend to dilute any association of these outcomes with implant rupture because the women did not know their implant status prior to undergoing magnetic resonance examination. His third point is that fibromyalgia (FM) may be a catch-all and that women with this diagnosis are simply more apt than other women to report, presumably minor, com-

Correspondence

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Fibromyalgia: Can one distinguish it from simulation?

To the Editor:
The article by Khoostanteen, et al. would have major implications in “industrial” and insurance policy development. If there were not major methodology flaws. Premise assumptions and experimental design, however, preclude the article’s sweeping conclusion.

The question of whether fibromyalgia (FM) can be faked is quite pertinent. Can an individual study or be coached as to how to respond during examination? Perhaps that depends upon the examination form choice of control points.

What is the role of control points? Campbell, et al. found that control points were not tender in patients with FM. While Khoostanteen, et al. note that they have published their disagreement, perhaps an issue is specific to control points. One could question that the control points chosen in their study (forehead, mid-ulna, hypothenar, lateral gastrocnemius) were too physically distant from FM trigger/tender points to be clinically useful.

Yes, forehead tenderness might raise questions of second primary pain, and tenderness in the mid-ulna region might raise questions of general body tenderness, low pain threshold or interpreting touch sensations as pain. However, is that really the issue for assessing fakery or exaggeration?

I would suggest that a much better control point is one adjacent to a FM trigger point. Examining the trapezius ridge or rhomboid trigger points and adjacent scapula provides unequivocal evidence, in my experience. The points are too close for patients to discriminate. As for pain reporting on examining these points, I have not seen a single patient with FM who has tenderness also in the adjacent scapula. Those individuals with tenderness on scapula pressure also have a general body tenderness, something I classify (my own preconceived notion) separately from FM.

Thus, I would suggest that control points do matter — if appropriate ones are chosen, ones for which patient’s perception does not allow such 2 point discrimination.

The technique of pain assessment may also provide discrimination. It has been my approach to ask at the onset of examination for the patient to identify any pain produced during that examination. Thus, general or inappropriate tenderness can clearly be recognized and distinguished from FM. Delaying the question to when specific trigger points are examined may miss that important information.

I think the arbitrary selection of at least 4 trigger points provides a more uniform group of patients with FM. However, presence of trigger points could be interpreted as a patient with FM, who simply doesn’t have enough body regions affected to be entered into controlled studies. That, however, does not qualify them as controls. Would it not be more appropriate that controls be individuals without any trigger points. One certainly would not use as controls in a lupus study individuals who fulfill 3 criteria for lupus. Why then should we for FM?

One further critique must be considered. The control group was recruited from “posted notices in the hospital.” Relevance to the general population of controls derived from medical facility is not established.

Subtleties are often of great importance in rheumatologic diagnosis! The term “trigger points” has been used throughout this discussion. Some might distinguish trigger points and tender points, requiring a referral pattern of pain for use of the former term. It would be of great interest to examine patient pain responses, to assess whether the pain is felt at the site of pressure or whether it occurs in anticipated referral patterns. Clarification of specificity for FM of physical examination findings is indeed an important research subject.

Concern must be expressed that those with an agenda will attempt to turn the article by Khoostanteen, et al. into case law. However, the case has not been made. There is still no evidence that FM can be faked — as long as appropriate control points are assessed.

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REFERENCES


REFERENCES
1. Khostanteen I, Tunks ER, Goldsmith CH, Ennis J. Fibromyalgia:
   Can one distinguish it from simulation? An observer-blind
2. Campbell SM, Clark S, Tindall EA, Forehand ME, Bennett RM.
   Clinical characteristics of fibrositis. I. A blinded controlled study

Dr. Khostanteen replies

To the Editor:

Dr. Rothschild in his letter raises some very important issues. Control
points need more exact definition and the most appropriate would be those
which remain clinically nontender in the region of referred pain. A number
of studies had addressed this question; in our study the design was based on
previous work. Our experience was that control points become progressively
more tender in relationship to the tenderness at the tender points
comparing healthy controls to patients with fibromyalgia (FM) or myofascial
pain.

As per the concern regarding the technique of pain assessment, the
point is well taken. The volunteer group was given information regarding
technique of examination of tender points and were already examined by
the rheumatologist prior to enrollment into the study.

Another concern of Dr. Rothschild was choice of control group. The
inclusion criteria were: no history of chronic pain, which would by the
ACR criteria exclude any FM and clinical findings less than 4 points,
which was chosen arbitrarily from population studies.

As per the question of relevance of controls derived from medical facil-
ities, this population provided a convenient sample of healthy individuals.
From our study design the volunteers were randomized to the normal group
versus the simulators.

We agree that consistency in the use of terminology is important in
respect to tender point versus trigger point: descriptions. In our study we use
the term of tender points.

As stated in the Discussion this was not to be meant as a test for
malingering, but rather tested the sensitivity, specificity, and accuracy of
the ACR criteria to distinguish patients with chronic FM from motivated
simulators under the conditions of a clinical trial.

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Irena Khostanteen, MD, FRCP.

REFERENCES
1. Tunks E, Crook J, Norman G. Tender points in fibromyalgia. Pain
2. Wolfe F, Smythe HA, Yunus MB. The American College of
   Rheumatology 1990 criteria for the classification of fibromyalgia.
3. Wolfe F, Cathey MA. The epidemiology of tender points: a

Unusual Synovial Cyst of the Knee Treated with Fibrin
Sealant

To the Editor:

We read with great interest the report by Jerome and McKendry, describ-
ing a case of synovial cyst of the proximal tibiofibular (PTF) joint. As noted
by the authors, this condition is uncommon and no consensus exists on the
most appropriate management, although surgical excision is recommended.
We describe a case of synovial cyst arising from the PTF joint treated suc-
cessfully by a fibrin sealant injection.

Correspondence

Figure 1. Axial (a) and sagittal (b) T2 weighted MR images (2700/105) of
right knee. A fluid collection with high signal intensity is seen (arrows)
connecting with the proximal tibiofibular joint and extending cranioceau-
A 42-year-old man complained of a 2 month history of atrophic lateral right knee pain that worsened with ambulation. Medical and surgical history was unremarkable. Examination revealed a focal tenderness around the anterolateral aspect of his right knee, without limitation of range of motion. Distal pulses were palpable, the sensation to light touch was intact distally, and motor function of the leg was normal. Right knee radiographs were normal. Axial and sagittal magnetic resonance imaging (MRI) showed a well encapsulated fluid collection with low signal intensity on T1-weighted and homogeneous high signal intensity on T2 weighted images (Figure 1 A, B), arising from the PTF joint and measuring 40 x 20 x 20 mm.

On the basis of fluid consistency and the location of the lesion a diagnosis of synovial cyst of the PTF joint was made11,28. To avoid surgery, a nonoperative method was tried by injecting a fibrin sealant (Tissucol Kit, Immuno AG, Vienna, Austria) into the cyst with a fluoroscopic guide after needle aspiration of the contents. His pain resolved within a few days after the procedure. Followup MR study one month later (Figure 2) revealed that the cyst had decreased; the patient was asymptomatic and without relapse after 24 months.

Traditional nonoperative treatment of synovial cysts consists of mechanical rupture followed by applying pressure, aspirating the cyst followed by pressure, or aspirating with injection of a chemical irritant. Although this treatment often relieves symptoms, it is only temporarily effective because of cyst reinfation15. Fibrin sealant is a biodegradable surgical tissue adhesive used increasingly in many situations22 and it would be clinically useful in wall-to-wall adhesion cystic lesions. Recently Shigeno, et al assessed this substance in the management of cystic lesions of the soft tissue in which previous conservative treatment had failed, including a PTF cyst with multiple aspirations. In light of their successful results, in our patient we sought to inject fibrin sealant into the cyst to adhere to the cyst walls, as a more definitive therapy than aspiration alone. Given the complete resolution of one case after one injection, and the simplicity of the technique, we suggest this method before surgery of synovial cysts.

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REFERENCES

Drs. Jerome and McKendry reply

To the Editor:
The letter from Dr. Mifsut, et al on the treatment of proximal tibiofibular (PTF) joint cysts also highlights some diagnostic issues. It would appear that the PTF cyst in their patient was not superficial enough to be detected as a subcutaneous mass on physical examination, but rather was diagnosed by magnetic resonance imaging. How often is "knee" pain caused by an unsuspected, symptomatic PTF cyst? Could a PTF cyst of this size be reliably detected using less expensive and more readily available ultrasound imaging? The sustained improvement following intracyst fibrin sealant (Tissucol Kit) is impressive. We hope these authors, or others, will undertake a prospective trial comparing cyst aspiration to aspiration and injection of fibrin sealant to confirm that the excellent result in this patient is the rule rather than the exception.

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Assessment of Prothrombotic Risk in Patients with Behçet’s Disease Should Include Homocysteine Plasma Levels

To the Editor:
We read with great interest the paper by Toydemir, et al regarding the possible role of methylenetetrahydrofolate reductase (MTHFR) gene C677T, factor V (FV) gene G1691A (Leiden), and prothrombin gene G20210A polymorphisms in the genesis of thrombosis in patients with Behçet’s disease (BD). The authors found no correlation between thrombosis and MTHFR gene C677T mutation in patients with BD, confirming reports that did not show any association between cardiovascular thromboembolic events and MTHFR 677TT genotype9. However, it is of interest that while factor V gene G1691A (Leiden) and prothrombin gene G20210A mutations are recognized as independent thrombotic risk factors, MTHFR 677TT genotype should be considered only as an indirect predisposing factor for thrombosis. Indeed, several reports showed that MTHFR 677TT genotype in association with low plasma folate levels is the major determinant for mild or moderate hyperhomocysteinemia, which is a well known risk factor for both arterial and venous thrombosis10. Plasma elevations of total-Hcy (tHcy) are typically caused either by genetic defects in the enzymes involved in Hcy metabolism or, more frequently, by nutritional deficiencies.
in vitamin cofactors such as folic acid, vitamin B12 and vitamin B6. A screening for thrombophilic conditions in patients with BD should include not only MTHFR genotyping but also homocysteine folate, and vitamin B12 plasma levels to exactly define the prothrombotic risk.

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REFERENCES

Drs. Toydemir, et al reply

To the Editor:

Our primary focus was to evaluate the possible role of 3 polymorphic mutations, alone or in combinations, in thrombogenesis in patients with Behçet’s disease.

As we clearly pointed out in the Discussion, serum homocysteine levels may be affected by serum levels of folate, vitamins B6 and B12, cystathionine β-synthetase and methionine synthetase, as well as MTHFR C677T mutation, which we examined in this work. Our conclusion that MTHFR C677T mutation is not correlated with thrombogenesis does not preclude the association of these other thrombogenic factors, which, we concur, are worthy of study to expand on our findings.

We are pleased to see Danese and colleagues concur with our approach and we encourage others to extend our findings through similar studies.

PINAR BAYRAK TOYDEMIR, MD, PHD; ATIILA HALIL ELHAN, MD; AYSEL GÜRLER, MD; ALITAN TÜZÜNER, MD; İŞIK BÖKESOY, MD, PHD. Ankara University School of Medicine, Ankara, Turkey.

REFERENCE