Title
Clarifying the presence of posttraumatic stress symptoms following orthopaedic trauma [1] (multiple letters)

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Letters to The Editor

Clarifying the Presence of Posttraumatic Stress Symptoms Following Orthopaedic Trauma

To The Editor:

Posttraumatic stress disorder is a serious psychological condition that has received increasing attention over the past decade. Starr et al. should be commended for their attempt, in their article "Symptoms of Posttraumatic Stress Disorder After Orthopaedic Trauma" (2004;86:1115-21), to alert orthopaedic surgeons to the fact that psychological consequences of a severe orthopaedic injury are possible and important. Nonetheless, the percentage of respondents who "met the criteria" for posttraumatic stress disorder (as measured with the Revised Civilian Mississippi Scale for Posttraumatic Stress Disorder) was startlingly high. Because the presence of a serious psychiatric disorder in more than one-half of a traumatized sample is exceedingly rare, it led me to examine the methods and analytic strategy used in this report. There are a number of issues that deserve mention.

1. Posttraumatic stress disorder cannot be diagnosed until at least one month after the traumatic episode. The authors noted that some respondents had been injured as few as two days prior to assessment. Individuals seen less than four weeks after the trauma should have been excluded from the sample.

2. For a diagnosis of posttraumatic stress disorder to be made, the person's response to the event must involve intense fear, helplessness, or horror (Criterion A2 of the DSM [Diagnostic and Statistical Manual of Mental Disorders]-IV). These responses do not appear to have been assessed.

3. According to the DSM-IV, symptoms must be present for one month (Criterion E). The duration of symptoms does not appear to have been measured in the present investigation.

4. Criterion F—that the disturbance must cause clinically important distress or impairment in social, occupational, or other important areas of functioning—is considered by many to be the hallmark of the disorder. Again, it does not appear to have been assessed.

Thus, it is clear that, while the investigators measured symptoms that were consistent with criteria B, C, and D of the DSM-IV, the absence of a full assessment of posttraumatic stress disorder required the investigators to be extremely circumspect about their terminology. In fact, because all DSM-IV criteria were not assessed (e.g., degree of functional impairment and duration of symptoms), respondents should not have been assumed to have posttraumatic stress disorder.

Moreover, an important historical event occurred very close to the assessment of posttraumatic stress disorder among the respondents in this study. The September 11, 2001, terrorist attacks had a substantial impact on the psychological state of individuals across the country—not simply those who lived in a directly affected community[1]. Moreover, these attacks had a clear, demonstrable impact over the six months after the attacks, with substantial numbers of individuals from a nationally representative sample showing posttraumatic stress symptoms and elevated levels of distress[1]. The fact that the assessment of posttraumatic stress disorder was conducted within weeks after the attacks at one of the study sites and within months after the attacks at the second site may have inflated the results. In fact, many of the items on the Revised Civilian Mississippi Scale for Posttraumatic Stress Disorder assess trauma symptoms that are not specific to the orthopaedic trauma or injury (e.g., items 1, 4, 10, 11, 12, 13, 19, 21, 22, 24, 25, 26, 27, 28, 29, 30).

Finally, a more traditional way to analyze these data in order to examine demographic and injury-related predictors of the presence or absence of posttraumatic stress symptoms would have been to use logistic regression. The nontraditional analytic strategy employed in this study may have masked factors that, in combination, could have assisted the orthopaedic surgeon in identifying at-risk individuals who might benefit from psychological referral.

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A.J. Starr, W.H. Frawley, and C.M. Reinert reply:

Dr. Silver raises several good points. First, she points out that, according to the DSM-IV, posttraumatic stress disorder cannot be diagnosed until at least one month after the trauma, and she suggests that patients who were evaluated less than four weeks after the trauma should have been excluded from our sample. We considered excluding such patients but decided against it. In our sample, patients who were seen at a longer interval after the injury had more symptoms of posttraumatic stress disorder. Exclusion of patients seen less than four weeks after the trauma would have made the apparent prevalence of the illness even higher. If we excluded those assessed less than thirty days after the injury, the rate of posttraumatic stress disorder would have jumped to 55%. If a 50% rate of the illness seems startlingly high, 55% would be even worse.

Since our goal was to measure the prevalence of illness among orthopaedic trauma outpatients, we decided to include those assessed soon after injury. Patients who return to their orthopaedic surgeon's office two weeks after injury with symptoms of posttraumatic stress disorder may not meet rigid criteria for the illness, but the

symptoms are still present. Our goal was to record those symptoms and to bring them to the attention of other orthopaedic surgeons. For that reason we wanted to be as inclusive as possible.

Next, Dr. Silver raises concerns about our failure to assess other criteria listed in the DSM-IV and states: "In fact, because all DSM-IV criteria were not assessed (e.g., degree of functional impairment and the duration of symptoms), respondents should not have been assumed to have posttraumatic stress disorder." The question of impairment is very important. In fact, a search for causes of impairment after orthopaedic trauma was one thing that led us to do this study.

If one asks orthopaedic trauma patients, "Are you impaired?" many answer with a resounding "Yes!" We assumed, perhaps incorrectly, that the fact that the patients were seeking treatment at an orthopaedic clinic meant that their injury had caused a "clinically important impairment." It seems probable that some patients' impairment is due to their physical injury, but, for others, impairment may arise from psychological distress. In fact, it may be difficult to tell whether impaired function is caused by physical injury, by psychological distress, or by some combination of the two. Psychological distress is strongly associated with poor functional outcome scores among patients who have sustained high-energy lower-extremity trauma[2]. Could the same be true for less severely injured orthopaedic trauma patients? And how common are symptoms of posttraumatic stress disorder among orthopaedic trauma patients? As best as we can tell, nobody knows the answer to those questions.

The goal of our study was to try to estimate the prevalence of posttraumatic stress disorder in our patient population. In the future, we hope to find out if functional impairment can be reduced by treating psychological distress.

As we noted, "a diagnosis of posttraumatic stress disorder based on a questionnaire is not the same as a clinical diagnosis made by a mental health professional. A more rigorous diagnosis may reveal different results." Dr. Silver, a mental health professional, may be right when she says that we should have been more circumspect with our terminology. However, if we assume that our patients answered the questions honestly, it is hard to ignore their responses. Perhaps, in the interest of diagnostic rigor, it would be more accurate to say that the patients in our sample did not meet all of the criteria necessary to make the diagnosis of posttraumatic stress disorder, they just had lots of posttraumatic stress symptoms.

Given that we failed to adopt the one-month criterion for symptom duration and that we assumed that attendance at an orthopaedic trauma clinic constituted evidence of a clinically important impairment, Dr. Silver's criticism of our assignment of the diagnosis is probably deserved.

Dr. Silver also raises the question of the impact of the September 11 terrorist attacks on our patient sample and cites research carried out by her and her colleagues[1], a web-based survey of 933 people residing outside New York, NY. The sample of people assessed by Silver et al. included only one patient personally injured in the attacks. Thirty-eight percent of the respondents had no exposure to the attacks as they occurred, and another 60% reported watching them occur live on TV. Only 2% of the sample had direct firsthand exposure to the attacks. Surprisingly, at two months following the attacks, 17% of the respondents reported September 11-related posttraumatic stress symptoms; 5.8% did so at six months.

It may be that the September 11 attacks inflated the results of our study. Or it may be that direct personal injury, such as that sustained by our patients, is more likely to cause posttraumatic stress symptoms than indirect exposure to an event such as the September 11 attacks.

Dr. Silver also notes that "many of the items on the Revised Civilian Mississippi Scale for Posttraumatic Stress Disorder assess trauma symptoms that are not specific to the orthopaedic trauma or injury." Our patient sample was composed entirely of people who had sustained an orthopaedic injury and were seen for follow-up in an orthopaedic trauma clinic. The cover sheet for our questionnaire carried the title "Study of Stress after Orthopaedic Trauma" and stated, "You are being asked to complete this questionnaire because you have sustained an injury. Our goal with this study is to see how injury affects orthopaedic patients emotionally or psychologically." Questions 1, 4, 10, 11, 12, and 13 from the Revised Civilian Mississippi Scale for Posttraumatic Stress Disorder were altered by us to include references to "the injury," "my injury," or "since I was injured," instead of "the event," as originally written by Norris and Perilla, the questionnaire's developers[3]. Questions 19, 21, 22, 24, 25, 26, 27, 28, 29, and 30 were used verbatim from the questionnaire by Norris and Perilla. Our thought was that the cover sheet and the questions made it clear that the goal of the questionnaire was to assess how injury affected patients psychologically or emotionally. There is a possibility that symptoms arising from the September 11 attacks inflated our results. Since we did not address the attacks directly, we have no way to know if this is the case.

Finally, Dr. Silver suggests that a more traditional analytic strategy might have assisted us in identifying at-risk individuals who might benefit from psychological referral. Previously, we had performed a multiple independent variable logistic regression analysis with backward elimination, initially including those variables that were significant ($p < 0.05$) at a univariate level. The intent was to ascertain if combinations of significant variables were good predictors. When we used this technique, ISS (Injury Severity Score) remained in the model, whereas the summed Extremity Abbreviated Injury Score and elapsed time since the injury were dropped. Motivated by her suggestion, we increased the complexity of the model to include other variables and numerous first-order interactions. We found that age and ISS remained in an additive model with the predicted probability of posttraumatic stress disorder increasing with a higher ISS and a lower age. However, the area under the associated ROC (receiver-operating characteristic) curve was 0.57, a value in the same range as was reported for single variables in the paper. Thus, we think logistic regression adds little useful information to the analysis of the data.

As for identifying "at-risk" individuals, we think we have. Orthopaedic trauma patients are at risk for posttraumatic stress disorder, or at least for posttraumatic stress disorder symptoms. And, at least in our sample, patients who said that the emotional problems caused by their injury were more difficult than the physical problems were at increased risk. We think that this simple question may serve as a screening tool for identifying patients who may benefit from further screening or treatment.

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These letters originally appeared, in slightly different form, on jbjs.org. They are still available on the web site in conjunction with the article to which they refer.

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Comparison of Primary Total Hip Replacements Performed with a Standard Incision or a Mini-Incision

To The Editor:

The study "Comparison of Primary Total Hip Replacements Performed with a Standard Incision or a Mini-Incision" (2004;86:1353-8), by Woolson et al., is probably the quintessential example of comparing apples to oranges because the authors retrospectively compared their standard operation (with which it is assumed they were skilled) and a new operation (with which they were not skilled), without any scientific model, training, instrumentation, or guidance. It was also bad science because they performed a study operation on patients without institutional review board approval. This is the second study from Stanford University published in the last year in which the institutional review board was not involved in surgery that was "experimental."[1]

This manuscript is simply an arrogant statement by the authors who assumed that, with fellowship training and more than ten years of experience, they could perform a new operation as well as they perform the operations they have done for all those previous years.

There is not a single innovator of the small incision operations who has suggested that these operations are as easy as total hip arthroplasty with use of standard incisions or that they are not more stressful or do not require a learning curve with special instrumentation. At every meeting at which I have participated regarding this subject, it has always been emphasized that a surgeon should not go directly to an incision of ≤10 cm. The incision should gradually be decreased so that the surgeon becomes comfortable with the field of vision. These surgeons were less responsible to their patients than a low-volume surgeon who obtains training, has the proper instrumentation, and initially learns the operation with supervision.

The authors were also not well informed of the knowledge that the use of a mini-incision is more than just the incision and is a change in the process of total hip replacement. In combination with a shorter incision, there must be preoperative education, staff training, and coordination of the anesthesia and pain management for earlier discharge to be possible.

If the authors want to contribute to the orthopaedic community, they should design an appropriate scientific study model, such as the randomized study of Chimento and Sculco[2], and they should not subject patients to a new operation without obtaining the skill, understanding the principles of the new operation, and informing patients of their study. Otherwise, they simply contribute more "junk science." If the authors and the editors of *The Journal of Bone and Joint Surgery* wanted to publish information showing that mini-incision operations are not easy, are stressful, and require knowledge, skill, and training, they could have better done this with an editorial rather than publishing bad science that is apples versus oranges.

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S.T. Woolson, C.S. Mow, J.V. Lannin, and D.J. Schurman reply:

We would like to address some of the assumptions made by Dr. Dorr regarding our recent article. We emphasized that these results represented our learning curve with the procedure. All of the surgeons gradually reduced the size of their standard incisions prior to beginning this series of mini-incision operations, as suggested by Dr. Dorr. These mini-incision procedures were begun in 2001, the same year that Dr. Dorr began his experience[3] with the procedure using a 10 to 12-cm incision with standard hip instruments and retractors. The surgeon who performed procedures using incisions of <10 cm did use specialized retractors. Two of the three surgeons had training in the technique prior to using it, and one of them had attended Dr. Dorr's annual course and watched him perform live mini-incision hip replacement surgery for the last three years.

We did obtain institutional review board approval for this retrospective study. We are surprised that Dr. Dorr feels that institutional review board approval must be obtained in order to ethically perform a mini-incision hip replacement, since he and other proponents of the mini-incision technique have not mentioned this proviso in their publications. The American Academy of Orthopaedic Surgeons has supported courses, technique DVDs, and other educational resources regarding the mini-incision procedure and has published patient information about it on their web site but, to our knowledge, has not regarded it as an experimental operation requiring institutional review board approval. All of our patients were given explicit information regarding the risks and complications of total hip replacement and were told of the size and location of their incision.

Dr. Dorr wisely stated in his symposium talk at the Annual Meeting of the American Orthopaedic Association[3] that his claims of good pain relief and rapid functional recovery with the mini-incision technique could possibly be explained by anesthesia and pain management techniques rather than by the procedure itself. He also admitted that he had no data comparing the mini-incision with the standard technique. We feel that it should be the responsibility of the innovators of the technique to provide randomized, prospective studies of similar groups of patients, with respect to body mass index, age, and gender, managed with standard and mini-incision techniques with use of the same implants and postoperative rehabilitation protocols. Until peer-reviewed scientific evidence demonstrates significant short-term benefits from the procedure with equal