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Correspondence

John P. Lubicky and Henry R. Cowell
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TO THE EDITOR:

The article "Current Concepts Review. Prophylactic Use of Antibiotics for Procedures after Total Joint Replacement" (78-A: 1755-1770, Nov. 1996), by Deacon et al., appears to contain several factual errors.

First, on page 1756, in the section entitled "Dental Procedures," the authors state: "We know of only one randomized placebo-controlled trial confirming the benefits of such prophylaxis in the prevention of late prosthetic-valve endocarditis." The authors then go on to cite a study by Horstkotte et al.³. However, that study was not a randomized placebo-controlled trial. I have read and translated parts of that article from German and have also read an article in English by Horstkotte et al.² that appears to describe the same research. It is clear from these articles that this was a retrospective clinical study, which is similar in design to a retrospective cohort study. There is no mention of placebos, randomization, or prospective planning. Indeed, the last line of the English-language abstract in the German article and of the abstract in the English-language version states that "this retrospective study documents the benefit" of prophylaxis against infective endocarditis^{2,3}. In fact, I am not aware of any prospective, randomized, controlled trials that have been conducted to measure the efficacy of antibiotic prophylaxis in the prevention of endocarditis, and the performance of such a study is fraught with both logistical and ethical problems as endocarditis is rare and prophylaxis is now accepted therapy¹. However, several recent case-control studies have been performed that estimated the efficacy of prophylaxis among patients with native-valve endocarditis who had had recent invasive procedures. In those four studies, the relative risk (RR), which is approximated by the odds ratio, can be used in the formula $1 - RR$ to calculate the efficacy of prophylaxis. The odds ratios, and corresponding efficacy (with 95 per cent confidence intervals when given), were 0.09 or 91 per cent efficacy (7.0 to 100)⁴, 0.51 or 49 per cent efficacy (0 to 89)⁷, and 0.54 or 46 per cent efficacy (0 to 90)⁵; in a recent study, the odds ratio was more than one, indicating 0 per cent efficacy⁶.

Second, when Deacon et al. referred to the study by Horstkotte et al.³, they mistakenly cited the number of procedures as the number of patients. For example, Deacon et al. stated that "in 1986, Horstkotte et al. reported no cases of late prosthetic-valve endocarditis in 287 patients with prosthetic heart valves who had been given antibiotics." Actually, Horstkotte et al. reported no instances of late prosthetic-valve endocarditis in 229 patients who had had a total of 287 procedures. Deacon et al. also stated that "of the 390 patients [in the study by Horstkotte et al.] who did not receive antibiotics prophylactically, eight had late prosthetic-valve endocarditis." In fact, Horstkotte et al. reported late prosthetic-valve endocarditis in eight of 304 patients who had had a total of 390 procedures.

Although these latter errors are minor, the first one is important because the efficacy of antibiotic prophylaxis for patients who have a prosthesis in place is an important issue in both medical and dental

practice today and incorrect information could mislead readers and possibly slow down advances in this area.

Mark T. Drangsholt, D.D.S., M.P.H.: Department of Oral Medicine, School of Dentistry, University of Washington, Box 356230, Seattle, Washington 98195-6370

Dr. Deacon, Dr. Pagliaro, Dr. Zelicof, and Dr. Horowitz reply:

Dr. Drangsholt correctly points out our errors in the citing of the study by Horstkotte et al.³. Indeed, the study was neither prospective nor randomized and we reported the number of procedures rather than the number of patients. We agree that it is unlikely that a randomized, placebo-controlled study will be performed to examine the efficacy of antibiotic prophylaxis in patients who have a prosthetic heart valve. It is also unlikely that such a study will be performed to determine the effectiveness of prophylaxis in patients with prosthetic joints who have an invasive procedure.

Our intention in the section entitled "Dental Procedures" was to note that there are few data to support the use of prophylactic antibiotics in patients with a total joint replacement who are having an invasive procedure. Although not comprehensive, there are markedly more data to support the use of antibiotic prophylaxis for patients who have native heart-valve disease or a prosthetic heart valve. In our review, we wrote that the interest in providing antibiotic prophylaxis for patients with a total joint prosthesis who are having an invasive procedure is at least partly based on the belief that this situation parallels that of the use of prophylactic antibiotics for patients who have heart-valve disease. We reported the conflicting results in animal models of infection around heart valves and noted that failures in humans were well reported, but we omitted other relevant human studies (which Dr. Drangsholt supplies in his letter). The study by Horstkotte et al.³ was just one of several studies that were used to support the notion that antibiotic prophylaxis should be used for patients with prosthetic heart valves who are having an invasive procedure.

We are sorry that our miscitation of the data presented by Horstkotte et al.³ caused confusion. Regrettably, this was the only secondary reference in our review of the literature that we did not review firsthand. The error, however, does not change our argument or the results regarding the major theme of the review.

John M. Deacon, M.D.: Division of Emergency Medicine, University of California, Davis, Sacramento, California 95817

Andre J. Pagliaro, M.D.; Steven B. Zelicof, M.D., Ph.D.; Harold W. Horowitz, M.D.: Departments of Orthopaedic Surgery (A. J. P. and S. B. Z.) and Internal Medicine, Division of Infectious Diseases (H. W. H.), New York Medical College, Westchester County Medical Center, Room 209, Macy Pavilion, Valhalla, New York 10595

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TO THE EDITOR:

The article "Peroneal Nerve Entrapment" (80-A: 47-53, Jan. 1998), by Fabre et al., was very informative. However, I believe that a more sensitive evaluation of the peroneal nerve could have been performed by testing the distal motor and sensory latencies rather than the amplitude of the sensory potentials and nerve-conduction velocities. Amplitude testing is variable and depends on the thickness of the tissue and the accurate placement of the electrodes. Decreased nerve-conduction velocities are a less sensitive indicator of neural injury than distal latencies are.

Harvey R. Manes, M.D.: 256 North Wellwood Avenue, Lindenhurst, New York 11757

Dr. Fabre, Dr. Piton, Dr. André, Dr. Lasseur, and Dr. Durandeau reply:

We thank Dr. Manes for his interest in our article. In response to his question concerning our testing of the sensory potentials and nerve-conduction velocities rather than the distal motor and sensory latencies, we would like to point out that this series involved patients who were managed between five and sixteen years ago. Most of the electrophysiology reports in their records did not include data on the distal motor and sensory latencies. We chose to present only data that were available for all patients in the series for the sake of coherence.

Currently, tests of distal motor and sensory latencies are much more sensitive than tests of sensory potentials and nerve conduction velocities, especially when there is isolated sensory involvement.

Thierry Fabre, M.D.; C. Piton, M.D.; D. André, M.D.; E. Lasseur, M.D.; A. Durandeau, M.D.: Groupe Hospitalier Pellegrin, Place Amélie Raba Léon, 33076 Bordeaux CEDEX, France

TO THE EDITOR:

Prospective, randomized, double-blind studies can be false, erroneous, and misleading!

In "Editorial. The Prospective, Randomized, Double-Blind Clinical Trial in Orthopaedic Surgery" (79-A: 1191-1120, Aug. 1997), Clark eloquently discussed the virtues of such studies, leaving the reader with the impression that the ultimate study must have that format. Indeed, our medical students and residents have been convinced that these are the only studies of value.

In particular clinical settings, this approach can lead to the wrong conclusion, bias health-care decisions, and, ultimately, stifle progress. Consider, for instance, a study that purports to examine the effect of tenosynovectomy or neurolysis in patients managed with carpal tunnel release. A prospective, double-blind setup would randomize patients into two groups: one that would receive the so-called extra step of the procedure and one that would not. The inclusion of only one variable would seem to lead to the most reliable conclusion. Consider, however, that a patient may need tenosynovectomy or neurolysis, or both, depending on the severity of the disease and the operative findings. In this case, randomization would cancel out differences between the patients and the researcher would reach the unavoidable conclusion that these steps are not necessary.

We should be sure to teach our residents that prospective, randomized, double-blind studies can be good when done well but that there are contraindications to this approach. By no means should this approach be considered the prerequisite for a valid study or for publication.

Marwan A. Wehbé, M.D.: Pennsylvania Hand Center, 101 Bryn Mawr Avenue, Bryn Mawr, Pennsylvania 19010

Dr. Clark replies:

Perhaps a prospective, randomized, double-blind study can be false, erroneous, and misleading, as can virtually any study. The intent of my Editorial was to point out that it is possible to perform such prospective, randomized studies involving patients who are having an operative procedure. This does not mean that every study can or should be performed in this manner or that observational studies or retrospective reviews are of little or less value. Certainly, most reports that are published in *The Journal* are observational or retrospective and provide very valuable and useful information to the reader. The important point is that I believe that we can be doing more in the form of controlled studies involving patients who are having an operative procedure. However, any proposed study needs to be carefully designed. The example that Dr. Wehbé provides regarding tenosynovectomy or neurolysis does not appear to be well designed and would not be an appropriate study, let alone an appropriate prospective, double-blind study.

Prospective, randomized, double-blind studies need to be done well and should be done more often, but there are certainly many instances in which this type of study is not appropriate. The intent of my Editorial was by no means to imply that such a prospective, randomized, double-blind study was a prerequisite for consideration for publication. Prospective studies provide very valuable and useful information, and we need to be doing more of these in our field; however, such studies certainly should not be performed to the exclusion of retrospective and observational studies, which have provided the basic foundation on which much of our current knowledge rests.

Charles R. Clark, M.D.: Department of Orthopaedic Surgery, University of Iowa Hospitals and Clinics, Iowa City, Iowa 52242

TO THE EDITOR:

The investigation performed by Thordarson et al., entitled "The Effect of Fibular Malreduction on Contact Pressures in an Ankle Fracture Malunion Model" (79-A: 1809-1815, Dec. 1997), failed to determine the effect of an isolated fibular fracture on contact pressures in the ankle joint. The model selected by the authors demonstrated the effect of a combined osseous injury of the fibula and a substantial ligamentous injury of the ankle joint. I believe that this model probably resulted in a lateral talar shift that, in effect, may be a reproduction of the talar displacement described in the previous investigation by Ramsey and Hamilton² of the effects of lateral talar translation on contact pressures in the joint. Radiographic analysis of the specimen within the testing apparatus both before and after the loading is imperative to ascertain the alignment of the talus within the ankle mortise.

I have anxiously awaited an investigation of the effects of an isolated fibular fracture on the ankle joint. Unfortunately, the study by Thordarson et al. failed to achieve this goal, and therefore the conclusion that "displacement of the fibula in these injuries should not be accepted" is incorrect.

I hope that the authors repeat this investigation in a truly isolated fibular fracture model and add radiographic control to verify their testing design.

Paul E. Levin, M.D.: 625 Belle Terre Road, Suite 202, Port Jefferson, New York 11777

Dr. Thordarson, Dr. Motamed, Dr. Hedman, Dr. Ebramzadeh, and Dr. Bakshian reply:

We are somewhat confused by Dr. Levin's inquiry. He seems to be implying that fractures of the ankle occur as isolated osseous injuries. Beginning with the study by Lauge-Hansen¹, it has been a well known phenomenon that fractures about the ankle are associated with marked rotational stress and severe ligamentous disruption. If Dr. Levin is referring to the effect of an isolated fibular fracture, then he is not referring to an ankle fracture. Although we

agree with Dr. Levin that radiographs of the specimen within the testing apparatus would have provided useful information, such as radiographs were not available for our study. However, the jig that we employed in our study did effect displacement of the fibula, as we described. We did not report the position of the talus, just that of the fibula. If Dr. Levin is awaiting a study evaluating the effect of an isolated fibular fracture without ligamentous injury to the ankle joint, we do not suspect that this will be performed. We would not expect noticeable changes in contact pressures in the joint in the absence of ligamentous injury as there should not be noticeable displacement if the ligaments about the ankle are intact. We disagree that our conclusion is incorrect.

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Edward Ebramzadeh, Ph.D.: 2400 South Flower Street, Los Angeles, California 90007

Sam Bakshian, M.D.: 2921 South La Cienega Avenue, Suite A, Culver City, California 90232

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TO THE EDITOR:

I read with interest "Posterior Decompression and Stabilization for Spinal Metastases. Analysis of Sixty-seven Consecutive Patients" (79-A: 514-522, April 1997), by Bauer.

My colleagues and I reviewed a similar number of patients (seventy) who had an operation to treat spinal metastases². Partly because our study commenced earlier, a wider variety of treatment methods were used for our patients. Eighteen of our patients had posterior instrumentation with Hartshill or Luque rectangles and sublaminar wires.

It is always difficult to compare studies by different authors, particularly when the conventional method of presenting data has not been followed. In the report by Bauer, the data in Table II are not presented in a standard Frankel grid. Nevertheless, as far as can be determined, there does not seem to be a great difference between the two approaches. I do not mean to dismiss the value of posterior instrumentation with pedicle screws; indeed, some of our patients² were managed with this approach. However, I am concerned that the implied conclusion, that fixation with pedicle screws represents the only valid method of posterior instrumentation for vertebral metastases, is not supported by the literature.

S. J. Krikler, Ph.D., F.R.C.S.(Orth): Department of Orthopaedic Surgery, Coventry and Warwickshire Hospital, Coventry CV1 4FH, England

Dr. Bauer replies:

The study by Krikler et al. did not include seventy patients who had a metastasis as sixteen patients had a primary bone tumor². The immediate postoperative changes in neurological function in my patients were noted in the text, and I thought that a table would have been superfluous. Instead, I chose to present the data regarding neurological function during follow-up (Table II). The results regarding neurological function do appear to be similar in these two studies. It is not meaningful to compare survival because survival is mostly dependent on the site of the primary tumor and the extent of the metastatic disease¹. In my study, the most common primary lesion was carcinoma of the prostate. In the study by Krikler et al., the most common primary lesion was myeloma, which is associated with a more favorable prognosis.

I definitely agree with Dr. Krikler that pedicle screw fixation is not the only valid method of stabilization for patients who have a

metastatic lesion involving the spine. I did not intend to advocate this method of fixation, which is associated with important risks such as damage to dural structures and loosening of the implant. I prefer to use hooks in most patients, and I use pedicle screws only in the middle and caudad regions of the lumbar spine. In my practice, fewer than 20 per cent of patients who are managed operatively for cancer have a lesion in that area.

Henrik C. F. Bauer, M.D., Ph.D.: Oncology Service, Department of Orthopedics, Karolinska Hospital, S-171 76 Stockholm, Sweden

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TO THE EDITOR:

We read "Allograft Reconstruction of the Acetabulum after Resection of Stage-IIB Sarcoma. Intermediate-Term Results" (79-A: 1663-1674, Nov. 1997), by Bell et al., with great interest. On this side of the Atlantic, we are confronted with identical problems but have chosen to resolve them in a different way.

We agree with the authors' reluctance to perform hindquarter amputation, and our own experience details the considerable complications that can follow this procedure². We have chosen to use a custom-made endoprosthesis to reconstruct the periacetabular area of the pelvis, and we recently reported our results with thirty-five patients who had been managed with such a device after the excision of a primary malignant tumor¹. Our rates of complications were similar to those in the study by Bell et al.; specifically, we reported a 26 per cent rate of infection, a 23 per cent rate of local recurrence, and the subsequent high risk of metastatic disease and death. The overall mean functional score was 70 per cent for the thirteen patients in whom the prosthesis remained *in situ*. The most satisfying aspect of the procedure is the lack of failure due to late mechanical problems. Although loosening has occurred in some patients, the implants seem to have stabilized and revision has not yet been necessary.

We have considered all patients who have a periacetabular tumor to be suitable for reconstruction with an endoprosthesis and have largely avoided iliofemoral arthrodesis. The published results of iliofemoral arthrodesis have not been particularly satisfactory. In a study by O'Connor and Sim³, only three of nine patients had a good result; the remaining six had a fair or poor result.

Resection of the entire ilium, acetabulum, and hip joint remains a considerable challenge. We are currently addressing this challenge in selected patients with the use of the patient's own bone, which is irradiated and then reimplanted.

Bell et al. pointed out the difficulty in deciding the optimum level for resection of the bone. To help in this decision, we have frequently used so-called clearance biopsies, which are trephine biopsies performed across the plane of the pelvis at the planned level of resection at the time of the initial biopsy of the tumor. Clearance biopsies serve two purposes. First, they histologically confirm the absence of tumor at the planned level of transection. Second, they act as an identifiable landmark to define the level of transection both during the construction of the prosthesis and at the time of the definitive procedure. We recommend this simple technique to others.

Robert J. Grimer, F.R.C.S.; Simon R. Carter, F.R.C.S.; Roger M. Tillman, F.R.C.S.: Royal Orthopaedic Hospital Oncology Service, The Royal Orthopaedic Hospital NHS Trust, Woodlands, Northfield, Birmingham B31 2AP, England

Dr. Bell, Dr. Davis, Dr. Wunder, Dr. Buconjic,
Dr. McGovern, and Dr. Gross reply:

Abudu et al. presented their results with prosthetic reconstruction of the hemipelvis after resection of a periacetabular malignant osseous tumor¹. The outcome for their patients who had been man-

aged for this challenging clinical problem was similar to the outcome that we observed after allograft reconstruction of the acetabulum. However, critical reading of these two articles reveals that direct comparison is not feasible because of a so-called apples-and-oranges problem. The patient populations described in these two papers are different, and recognition of the differences provides an insight into the potential advantage of allograft reconstruction of the acetabulum after resection of a sarcoma of the pelvis.

Prosthetic reconstruction of the acetabulum, as described by Abudu et al.¹, is possible only if a stable, horizontal portion of the proximal aspect of the ilium is available for fixation of the implant with cement or through bone ingrowth. In contrast, fixation of allograft to the sacrum can be achieved even after resection of the entire ilium when the extent of the tumor as seen on magnetic resonance images dictates this proximal level of resection. Eight of the seventeen patients in our study had a resection at the level of the sacroiliac joint or through the sacrum. None of these patients would have been candidates for prosthetic reconstruction as described by our English colleagues.

The reconstructive protocol adopted by our group is based on the anatomical extent of the tumor within the pelvis and in the surrounding soft tissues. For lesions confined to the distal aspect of the ilium and acetabulum that do not involve the hip joint, we recommend the use of iliofemoral arthrodesis whenever possible. Although we have not yet published our results with this technique, the advantage of achieving solid fusion with autogenous bone, without the need for allograft or prosthetic materials, is appealing. For tumors in the distal aspect of the ilium that necessitate excision of the femoral head, we use either an allograft-implant composite, as described in our article, or occasionally a saddle endoprosthesis. The saddle endoprosthesis may be particularly useful for patients who are managed with excision of a large Ewing sarcoma of the pelvis followed by the use of postoperative radiation for the prevention of local recurrence, as we hesitate to use allograft bone in irradiated fields.

The tumors described here could also be treated with custom-made acetabular prostheses, as described by Abudu et al.¹. However, approximately 50 per cent of our patients who are managed for periacetabular sarcoma need an osteotomy of the posterior aspect of the pelvis at the sacroiliac joint or through the sacral ala. It is possible that Abudu et al. would manage these patients with hindquarter amputation, as they state in their article that preservation of the limb is contraindicated "if removal of the gluteal vessels and nerves is necessary to resect the tumour." In most patients who have extension of the periacetabular sarcoma into the posterior aspect of the pelvis, the gluteal vessels and nerves must be sacrificed in order for the tumor to be removed safely; such patients are not candidates for prosthetic reconstruction of the pelvis. In their article, Abudu et al. comment that, when a lesion necessitates an osteotomy of the proximal aspect of the pelvis through the sacroiliac joint, reconstruction can be performed with sterilized autogenous bone grafts. We look forward to the time when those authors describe their results with this technique. However, to date, their technique of reconstruction with a custom-made acetabular prosthesis would not be suitable for about 50 per cent of our patients who have pelvic sarcoma.

Robert S. Bell, M.D., F.R.C.S.(C); Aileen M. Davis, B.Sc., P.T., M.Sc., Ph.D.; Jay S. Wunder, M.D., F.R.C.S.(C); Tom Bucconjic, M.D.; Bruce McGoveran, M.D.; Allan E. Gross, M.D., F.R.C.S.(C); University Musculoskeletal Oncology Unit, Suite 476, Mount Sinai Hospital, 600 University Avenue, Toronto, Ontario M5G 1X5, Canada

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TO THE EDITOR:

We read with interest "Total Hip Arthroplasty with Cement in Patients Less Than Twenty Years Old. Long-Term Results" (78-A:995-1003, July 1996), by Torchia et al. Although we commend the authors on their attempt to establish the factors associated with a greater risk of failure in order to be able to accurately counsel prospective patients in the future, we believe that the study has several flaws. Therefore, it is difficult to support many of the conclusions reached.

The first factor is the inevitably small size of the series due to the rarity of total hip arthroplasty in patients less than twenty years old. This was compounded by the wide range of preoperative diagnoses, which created many smaller subgroups and led to the inclusion of seven hips that had a tumor, four of which were treated with proximal femoral resection. The success of any total hip replacement is highly dependent on the implant used¹, and all hip replacements, whether performed with cement or without it, cannot be regarded as being the same or be expected to produce equal results. The Charnley low-friction arthroplasty is widely regarded as the current so-called gold standard², and the Charnley implant, which was used in less than half of the hips in this series, was only one of eight different implants used. The number of each type of implant used in each diagnostic group was not stated, and the rate of failure was reported for the entire series but not for each diagnostic group or each type of implant. The influence of the design of the prosthesis was therefore ignored, and the inclusion of such a wide range of diagnoses, including tumor, precluded meaningful comparisons. Thus, it is highly improbable that valid conclusions can be drawn or significant findings can be claimed on the basis of these results.

For the purposes of the Discussion section, the authors chose to compare the results of this small heterogeneous series with those of a study that included 300 adults³ who had had a "similar" procedure at the same institution. These patients, in fact, had all been managed with a Charnley low-friction arthroplasty, but the levels of preoperative function and the diagnoses were not specified in the original article³. In light of the variety of prostheses as well as the large number of small diagnostic subgroups in the study by Torchia et al., it is hard to imagine that the historical control group (in which all patients had the same implant) bore any resemblance to the study group. Again, any comparison most likely would not be valid.

The authors suggested that the reason that the patients were uniformly satisfied with the results of the operation, despite a 50 per cent rate of failure at fifteen years, was that they had low expectations regarding the outcome. This is at variance with the findings of other authors, who have consistently stated that these young patients have extremely high expectations and are therefore willing to accept the risk of failure in order to achieve relief from pain and a degree of independence at a critical stage in their personal and professional lives. It is these high expectations, combined with an unwillingness to accept the lesser benefits offered by arthrodesis or osteotomy, that drive their continued demands for hip-replacement procedures². The authors' suggestion that the results of this study support the case for performing these alternative procedures, which carry substantial risks and may compromise the results of later arthroplasty, is difficult to justify. The results of hip replacements performed with cement in the general population of patients who have an arthroplasty are now extremely good and reliable when the implants that are used have been tried and tested^{6,9}, and the longevity of these implants is compatible with the life span of the typical patient. It is largely because of these young and demanding patients that continued research remains necessary in order to reduce the problems of wear and loosening.

The authors also suggested that patients who have juvenile rheumatoid arthritis have a low risk of failure of the prosthesis because of the restraints placed on them as a result of the generalized disease process. Although this perception was widely held in the past, many authors have reported poor results for such patients; in many cases, the poor results were thought to be due to poor bone quality resulting from a combination of osteoporosis, immobility, and the use of

medication (often including steroids)^{4,7,8}. It is also worth noting that the reported series included only sixteen patients who had juvenile rheumatoid arthritis; however, the level of function and the type of implant that was inserted were not specified.

In light of these flaws, the findings reported in this study have the potential to be misleading. Also of note is the fact that the relationship between acetabular wear and loosening (which the authors reported on page 1001, column 2) was demonstrated by Wroblewski⁹ in 1986. What is really needed is for investigators to study the long-term results associated with the use of one type of implant in a well defined, sizable series of patients. These results could then be used both as a baseline for the assessment of developments in the field of arthroplasty and as a way to counsel patients before an operation. The results of this paper do not appear to fulfill these criteria.

David H. Sochart, F.R.C.S.(Orth); Martyn L. Porter, F.R.C.S.(Orth):
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Appley Bridge, Wigan, Lancashire WN6 9EP, United Kingdom

Dr. Torchia, Dr. Klassen, and Dr. Bianco reply:

The first concern expressed by Mr. Sochart and Mr. Porter was related to the small number of patients in our study. Our rationale was clearly expressed in the introductory paragraph of our article. We stated: "The long-term results of standard total hip arthroplasty with cement in a large group of patients who were less than twenty years old and who primarily had non-inflammatory arthritis have not been reported previously." The report was intended to address this void in the literature. Considering that no similar data were available when we performed this study, sixty-three hips is an adequate number. We chose not to limit our study to the twenty-seven hips that had a Charnley implant because doing so would have precluded statistical analysis of the factors associated with both satisfactory and unsatisfactory outcomes.

The second concern was related to the effects of the prosthetic design and the preoperative diagnosis. Mr. Sochart and Mr. Porter state: "The influence of the design of the prosthesis was therefore ignored." In fact, the first paragraph of the Results section states that the type of prosthesis did not affect the probability of failure. The effect of the various diagnostic groups on the probability of failure was also reported in that paragraph.

The third concern was related to the comparison of our results with those reported by Kavanagh et al.³ in 1989. We are aware of the limitations of such a comparison and stated this in the Discussion section.

The fourth criticism was related to the reported high rate of patient satisfaction. Mr. Sochart and Mr. Porter suggest that this finding is not consistent with their experience. Differences in patient populations may explain this apparent discrepancy.

The fifth criticism focused on the results in the subgroup of patients who had juvenile rheumatoid arthritis. Again, differences in patient populations may explain this apparent discrepancy.

The final criticism concerned the inclusion of many implants in the study instead of only one. As stated previously, our purpose was not to report the results achieved with a specific implant. Rather, we chose to study the results of a more generic procedure (total hip arthroplasty performed with cement, regardless of the indication) in a specific population (patients who were less than twenty years old). We did this with full awareness of the current literature and of the inherent shortcomings of the study.

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TO THE EDITOR:

I am writing this letter in response to "Editorial. Wrong-Site Surgery" (80-A: 463, April 1998), by Cowell. As Cowell mentioned, the American Academy of Orthopaedic Surgeons has initiated a campaign against wrong-site surgery. Certainly, operating on the wrong side or wrong part is a potential problem that is bad for patient care and can lead to a lawsuit. A number of methods for preventing this problem have been developed, and identification of the proper site with some sort of a marker has been recommended. However, a number of objections have been made against such marking of the operative site, apparently because of the potential for infection. I suggest a much more simple, common-sense approach, which I have used for many years. My technique is to write "NO" on the side not to be operated on in large letters with a black marker. This lettering cannot possibly interfere with the operative site, and, because it is on the contralateral limb, it does not lead to any risk of infection. This technique serves the same purpose as writing on the side to be treated, without the need to actually put anything near the planned operative site.

I just offer this as another method of preventing wrong-site surgery.

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Dr. Cowell replies:

Dr. Lubicky correctly points out that there are a number of ways to avoid wrong-site surgery. As I noted in my Editorial, I, too, used a different method than that endorsed by the Academy.

The purpose of the Editorial was to alert the readers of *The Journal* to the problem and to provide the Academy's recommendations for avoiding wrong-site surgery. Each physician must choose what he or she believes, in his or her hands and in his or her judgment, to be the best means of avoiding wrong-site surgery. I thank Dr. Lubicky for bringing this simple alternative method to the attention of our readers.

Henry R. Cowell, M.D., Ph.D.: Editor, *The Journal of Bone and Joint Surgery*