
LETTERS TO THE EDITOR

Re: “Comparison of Synthetic Hydroxyapatite and Porous Polyethylene Implants in Eviscerated Rabbit Eyes”

To the Editor:

I am concerned by the implications of a recently published article by Schellini et al.¹ regarding synthetic hydroxyapatite (HA). I object to their statement: “The authors consider porous polyethylene a more suitable material than synthetic hydroxyapatite for use in anophthalmic cavity reconstruction.” They fail to make it clear that the synthetic HA implant used in their study was vastly different from other synthetic HA implants (e.g., FCI₃ synthetic HA, FCI, Issy Les Moulineaux, France) available worldwide (with the exception of the United States, due to patent restrictions).^{2–6} Such a statement has unnecessary potential negative implications for other synthetic HA implants.

In their introduction, the authors list implant materials used to replace volume in the anophthalmic socket, including porous polyethylene and synthetic HA. The synthetic HA implant they referenced is the FCI₃.³ They end their introduction with statements that imply they will be comparing the porous polyethylene and synthetic HA implants referenced in the preceding statement (the FCI₃). However, in the Methods section, the authors describe a synthetic HA sphere “fabricated by one of the authors and composed of calcium carbonate.” There is no discussion of the similarities or differences of this implant with the synthetic HA referenced in earlier statements (the FCI₃). The authors also fail to acknowledge that there are several different types of synthetic HA implants available,^{6,7} one of which was produced in their own country and has already been shown to offer little advantage to other currently available HA implants.⁶ (It is unfortunate that the authors did not reference my article⁶ in their study, as I believe the Brazilian HA more closely resembles their synthetic HA than any other synthetic HA I have seen.) There was little in the way of analysis of their synthetic HA implant other than that it had “no interlinked pores, only spaces between the calcium carbonate granules.” This is inadequate. Numerous published studies describe techniques to analyze im-

plants such as the one they studied.^{2,6–9} Were there impurities in their synthetic HA implants?

I am concerned because their results clearly show that their synthetic HA (1) caused an intense granulomatous reaction and (2) is associated with a loss of volume over time. The porous polyethylene used did not. These damning results for their synthetic HA implant justify their conclusion that porous polyethylene is a more suitable material than synthetic HA. However, without stating that the synthetic HA they used is different from the FCI₃ synthetic HA, their results and concluding statements are also damning for other synthetic HA implants.

Ophthalmic Plastic and Reconstructive Surgery has a total circulation of 1045, with 720 (68.9%) U.S. subscriptions (personal communication, G. J. Harris, *OPRS* Editor, June, 2003). For readers who may not be familiar with the FCI₃ or other synthetic HA implants available in other parts of the world, it is very easy, in my view, for them to assume that the synthetic HA implant studied by Schellini et al. is similar to other synthetic HA implants. Why would they think otherwise when the authors don't provide a comparison? I also suspect that the reviewers of this article let this omission go through.

The synthetic HA implant referred to in the introduction of the Schellini article is the FCI₃, which has been studied extensively in rabbits and in human beings.^{2–5} Its clinical appearance, physical characteristics, and chemical composition have been analyzed and compared with other porous orbital implants.^{2,4,6–9} I have found no evidence of the intense granulomatous inflammatory reaction or loss of cavity volume they describe when using the FCI₃ synthetic HA in a rabbit model or in over 150 patients post enucleation or evisceration.^{2,3} Granted, the authors placed their implants in rabbits after evisceration procedures, whereas my rabbit studies were on enucleated sockets with sclera-wrapped implants (using sclera from the enucleated rabbit eyes). However, I do not believe that this technique variation would explain the marked differences Schellini and coworkers report, compared with my findings on the FCI₃ and other synthetic HA implants.^{2,7}

I commend the authors for their research in comparing a synthetic HA implant with a porous polyethylene implant in 56 rabbits, which is a time-consuming process. However, failing to clearly state that their synthetic

HA implant is different from the synthetic HA they chose to reference (FCI₃ synthetic HA) is an unacceptable oversight, as there are potential negative implications for the FCI₃ implant and for other synthetic HA implants on the market. Who wants to use an implant that stimulates an intense granulomatous inflammatory response and loses volume with time? I believe that the *OPRS* readership and the manufacturer of the FCI₃ synthetic HA (FCI) require an apology.

David R. Jordan, M.D., F.A.C.S., F.R.C.S.(C)

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Response Re: “Comparison of Synthetic Hydroxyapatite and Porous Polyethylene Implants”

To the Editor:

I would like to thank Dr. Jordan for his careful review and comments about our paper.

I want to first call attention to Dr. Jordan’s comments on comparing our sphere implant to other implant material. In our research, we studied only porous sphere implants including synthetic hydroxyapatite (HA) and porous polyethylene. We selected comparable materials and did not compare these to other sphere implants such as nonporous implants or those with a different chemical composition.

The main purpose of our research was to compare the tissue reaction to the synthetic HA and to porous poly-

ethylene. Both of these materials were manufactured in Brazil, and their composition was presented in the Methods section.

Dr. Jordan mentioned a particular type of synthetic HA he has worked with, which is from Brazil,¹ and according to his thoughts it would be important to include comments about other implants such as the FCI3 in our study. However, the FCI3 implant is not widely available or used in Brazil or in the United States. Therefore, we did not discuss this implant or the Brazilian implant studied by Dr. Jordan in our report. Our intention was not to compare our implant material with others that we did not study.

Regarding the granulomatous reaction reported in our study, this reaction has been reported with other HA implants as well, and it is characterized by the presence of macrophages and giant cells. This cellular reaction has been consistently and previously described when using this material in cranial² and orbital reconstruction.^{3–9}

Giant cells and foreign body reaction are normal tissue reactions against any substance introduced in a receptor body. If the reaction is strong, the implant is not considered to have biocompatibility and stability. Our research showed very clearly that the reaction against the synthetic HA is stronger than against the porous polyethylene. Consistent with our results, Nunery et al.⁷ compared silicone and HA spheres and found more inflammation with HA. Goldberg et al.,¹⁰ comparing BioEye or Medpor implants in rabbit cavities, found greater inflammation with the BioEye implant. The porous polyethylene implant can also cause a granulomatous reaction.^{11,12} However, others¹³ did not observe this kind of reaction with the polyethylene material. One reason there may be a greater inflammatory reaction with the HA implant might be related to the irregularities or rugosities of the implant surface.^{6,14}

The loss of volume demonstrated with our synthetic HA implant in this study was similar to that of prior studies.^{15,16} The mechanism for the HA degradation is still unclear, but there are reasons to suggest that it depends on the particle desegregation in crystals with subsequent dissolution.¹⁷ The biodegradation is confirmed by the presence of the substance inside the macrophages.¹⁸ HA biodegradation and bioresorption might occur by a humoral or cellular mechanism¹⁹ and would be proportional to the tricalcic phosphate present.²⁰

Sires et al.⁸ noted, “This is critical for understanding the longevity of these spheres in patients,” and we agree with them. They reported a clinical trial of the long-term stability of porous HA implants in rabbit cavities.⁸

During the peer review of our study, the *OPRS* reviewers asked many questions about our sphere compo-

sition. Dr. Jordan believes that our comments might be against the FCI3 and he blames the reviewers of this article for neglecting to suggest that a comparison of materials be included. Although Dr. Jordan has extensive experience comparing HA and FCI3 implants, our report focused on HA and a newer, more frequently used, and more readily available material, porous polyethylene, in implants made in Brazil.

Silvana Artioli Schellini, M.D.

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Re: "Clinicopathologic Findings from Lacrimal Sac Biopsy Specimens Obtained During Dacryocystorhinostomy"

To the Editor:

I read with interest the article by Anderson, Wojno, and Grossniklaus (*Ophthalm Plast Reconstr Surg* 19:3:173-6, 2003) entitled "Clinicopathologic findings from lacrimal sac biopsy specimens obtained during dacryocystorhinostomy."

The authors looked at 377 DCR specimens from 316 patients, including lacrimal sac biopsy specimens; 31 distinct diagnoses were determined. Table 1 includes several diagnoses, which I must question. I initially thought I would see a clear listing of the pathologic diagnoses from 377 nasolacrimal sac wall biopsy specimens. After all, the title of the article is "Findings from lacrimal sac biopsy specimens," and the title of the table is "Lacrimal sac biopsy specimen diagnosis obtained during DCR." In the Methods section, the authors describe nicely how the lacrimal sac biopsy specimens were obtained, and they indicate that the pathologic diagnoses of the specimens were recorded. *Dorland's Illustrated Medical Dictionary*¹ defines "biopsy" as "the removal and examination, usually microscopic, of tissue from the living body, performed to establish the precise diagnosis." I therefore assumed the table would list precise microscopic diagnoses. Table 1 starts out listing lacrimal sac biopsy diagnoses, that is, nongranulomatous inflammation. However, toward the bottom of the list, I see "orbital fracture," "trauma," and "dacryolith" listed and have some concern. These entities are certainly not sac wall pathologic biopsy diagnoses. The list appears to include not only pathologic but clinical diagnoses, somewhat confusing the "biopsy" results.

After rereading the article several times, it appears as though the table is a listing of the "DCR specimen diagnosis," which would be more encompassing than a "lacrimal sac biopsy specimen diagnosis." Thus, some of the names in the table represent a pathologic diagnosis of the lacrimal sac wall (e.g., nongranulomatous inflammation), whereas others represent a diagnosis of the other contents submitted with the DCR specimen

(e.g., dacryolith, fragments of bone, and so forth). This is the only way I can appreciate why the authors would include dacryolith, trauma, or orbital fracture in their list of lacrimal sac biopsy specimen diagnoses. The “31 distinct diagnoses” could be more accurately described as “31 DCR specimen diagnoses” (which would include sac wall histopathologic biopsy diagnosis and other diagnoses derived from material in other parts of the DCR specimen submitted). I believe the readership would have been better served with a table listing only the nasolacrimal sac biopsy wall specimen diagnosis. This would also have been more in harmony with the text discussion, which emphasizes lacrimal sac biopsy results, that is, “at least 8 of 377 (2.1%) lacrimal sac biopsy specimens obtained during DCR demonstrate significant pathology that was unsuspected before surgery.”

David R. Jordan, M.D., F.A.C.S., F.R.C.S.(C)

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Re: “The Bioceramic Orbital Implant: Experience with 107 Implants”

To the Editor:

In their article “The bioceramic orbital implant: experience with 107 implants,”¹ David Jordan et al. stated that posterior sclerotomies were routinely performed in all their evisceration cases to allow placement of larger implants. Table 1 shows that the majority of implants in the evisceration group were either the 18-mm spheres or the newer 18-mm egg-shaped implants, which, according to the authors, are equivalent in volume to 19-mm spheres.

Performing evisceration with posterior sclerotomies would allow the placement of large 20- or 22-mm implants intraconally,^{2,3} but because of the large potential space created by this technique, I believe that placement of 18-mm implants would invariably lead to enophthalmos. In their landmark article, Masry and Holds² reported superior sulcus deficiencies with this technique even in some patients receiving 20-mm implants. Curiously, however, none of the patients in Jordan and coworkers' series had enophthalmos.

Another source of confusion in the article is the discrepancy between Table 1 and the text regarding

the number of patients receiving the implant. Although Table 1 lists a total of 75 patients receiving a secondary Bioceramic implant, the text only mentions 50. The table also mentions that 30 Bioceramic implants were used in evisceration patients, but the text counts 31 such patients. If the table were more accurate, then this would bring the total number of patients to 131 and not 107.

Hatem A. Tawfik, M.D.

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Response Re: “The Bioceramic Orbital Implant: Experience with 107 Implants”

To the Editor:

I am pleased to respond to Dr. Tawfik's letter as I believe that more informed dialogue will educate surgeons and ultimately improve patient care. Although I think that a closer reading of the article will clarify most of the issues raised, I am happy to provide additional information.

The first issue is with regard to the size of implants I used during evisceration surgery. Dr. Tawfik claims that I used mainly 18-mm spheres and 18-mm egg-shaped spheres, which are equivalent to 19-mm spheres in volume. He goes on to state, “Performing eviscerations with posterior sclerotomies would allow the placement of large 20- or 22-mm implants intraconally” (as suggested by Massry and Holds¹ and Kaltreider and Lucarelli²), and he believes “placement of 18-mm implants would invariably lead to enophthalmos.” Dr. Tawfik finds it curious that none of the patients in the series had enophthalmos.

I would like to remind Dr. Tawfik that the purpose of my report was “to assess the problems associated with the Bioceramic (aluminum oxide, Al₂O₃) orbital implant.” It was not about “an evisceration technique that combines scleral modification with optic nerve release for coverage of any sized implant” as presented by Massry and Holds¹ or about “selecting an

implant size for patients undergoing enucleation, evisceration and secondary implantations” as discussed by Kaltreider and Lucarelli.² I did not assess superior sulcus deformities or enophthalmos in my patient population because I was looking for problems associated with the aluminum oxide implant itself. The Methods section clearly outlines this. Nowhere in the report do I even mention the term enophthalmos or superior sulcus deformity, so I am curious where Dr. Tawfik got the notion that none of the patients had enophthalmos. This was never discussed.

On another note, and for accuracy, I used one 16-mm sphere, eleven 18-mm spheres, nine 18-mm egg-shaped spheres (equivalent to a 19-mm sphere), seven 20-mm spheres, and two 20-mm egg-shaped spheres (equivalent to a 21-mm sphere) during the eviscerations I performed. I believe this is quite a feat for the posterior sclerotomy technique I used, which is vastly different from the one Massry and Holds describe.¹ I referenced my posterior sclerotomy technique³ in the report, but Dr. Tawfik unfortunately must not have read it. The Holds and Massry technique¹ involves splitting the scleral shell in two completely separate halves. My technique³ involves (1) enlarging the anterior scleral opening by excising a triangle of sclera at the 3 o'clock and 9 o'clock positions and (2) posteriorly, disinserting the optic nerve followed by posterior radial sclerotomies. The scleral shell is never divided in two—only the posterior sclera is opened. Massry and Holds note that the anterior excision of sclera at the 3 o'clock and 9 o'clock positions does not change the scleral dimensions and only typically allows for a 16-mm implant.¹ How, then, did I get larger (18- to 20-mm) implants in the scleral shell? I would ask Dr. Tawfik to read the technique.³

Furthermore, in the Results section of my report, I stated, “secondary surgeries were listed separately because they may be required with any implant and were therefore not believed to be directly associated with the use of a Bioceramic implant.” One of the secondary surgeries was “additional volume augmentation” and was required in 9 of the 100 patients. Thus, some of my patients did have enophthalmos. Again, I remain curious how Dr. Tawfik came up with the notion that none of the patients had enophthalmos.

Last, Dr. Tawfik points out an error in Table 1 and the text. Table 1 states that 75 patients had secondary implant procedures and 30 had eviscerations procedures; whereas the text states that 50 had secondary implant surgeries and 31 had eviscerations. Dr. Tawfik has identified two typographical errors in the report, and I appreciate

him pointing them out. The text is correct, 50 patients had secondary surgeries and 31 had eviscerations.

David Jordan, M.D., F.R.C.S.(C)

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Re: “The Buried Vertical Mattress: A Simplified Technique for Eyelid Margin Repair”

To the Editor:

We recently read the article by Burroughs et al.¹ regarding the use of the buried vertical mattress suture for closure of eyelid defects. The authors comment on the use of the technique and its benefits of providing good wound edge eversion and avoiding corneal abrasions. However, they fail to credit the original report of the “buried” vertical mattress suture and refer to the technique as being “previously undescribed.” Although the technique may have not been used for closure of eyelid defects, it was first described by Zitelli and Moy² more than 10 years ago for closure of cutaneous wounds. Since then, there have been other reports regarding this suture technique.^{3,4} Burroughs and colleagues should, however, be commended for their application of the technique for closure of eyelid defects. Although we refer all our eyelid defect cases to a local oculoplastic group, we believe the buried vertical mattress is a superior suturing technique, and we use it on nearly all wounds elsewhere.

Ali Hendi, M.D.
Pittsburgh, PA

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Response Re: "The Buried Vertical Mattress: A Simplified Technique for Eyelid Margin Repair"

To the Editor:

We thank Dr. Hendi for his interest and are grateful for his concise list of references regarding the utility of the buried vertical mattress technique. We did not mean to imply that we were the first to use buried knots or vertical mattress sutures, but we remain unaware of any other description of an absorbable buried vertical mattress suture used in eyelid margin repair. We believed that this technique was worthy of publication, as most modern oculoplastics texts describe a complicated tech-

nique credited to Divine and Anderson in 1982 whereby a traditional vertical mattress suture is used to close the eyelid margin.¹ The tails of this knot are left long and captured within a series of interrupted sutures above the eyelid margin. This classic approach requires the use of multiple different types of suture and postoperative removal of the vertical mattress suture (before the other sutures dissolve or are removed as well). In essence, Dr. Hendi underscores two important points: first, that there are very few totally new ideas; and second, most of the best ideas in medicine come from a new application of an existing technology or technique.

John R. Burroughs, M.D.

Charles N. S. Soparkar, M.D., Ph.D.

James R. Patrinely, M.D.

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