

Perspective Analysis



Analysis of Reporting Bias in Vertebral Augmentation

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Osteoporotic vertebral compression fractures (OVCFs) are a significant cause of morbidity and mortality in the United States and worldwide, with estimates of 750,000 to 1.5 million occurring annually. As the elderly population continues to increase, the incidence of OVCFs will continue to rise, as will the morbidity and mortality associated with this condition. Vertebral augmentation (VA) was almost universally accepted as the appropriate treatment modality prior to 2 sham trials published in 2009 by the New England Journal of Medicine (NEJM). Subsequently, there is now significant controversy regarding the optimal treatment of OVCFs. Since 2009 there have been 6 prospective randomized controlled studies (PRCTs) and 2 meta-analyses on VA for the treatment of OVCFs. Five of the PRCTs and both of the meta-analyses have shown superior results with VA as compared with nonsurgical management (NSM).

However, a recent health technology assessment and review article continue to over-emphasize the 2 NEJM sham trials, despite the most current literature. These are examples of inconsistent or biased data reporting with overemphasis on certain trial types and exclusion of other types of data, resulting in the reporting of conclusions that are partially representative or not representative of the complete data. As clinical investigators, we have a responsibility to limit bias and ensure that the appropriate treatment modalities are made available to vulnerable populations.

The aim of this perspective analysis is to examine sources of bias in reporting and some of the publications that contain it, along with comparing the publications to the current body of published literature relevant to this topic.

Key words: Vertebral augmentation, vertebroplasty, kyphoplasty, bias, osteoporosis, compression fracture

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The issue of osteoporosis and osteoporotic fractures is substantial, with 750,000 occurring each year within the United States and increasing as members of the baby boomer generation began turning 65 in 2011 (1). The elderly population is estimated to be 70.2 million by 2030, twice as large as the population was in 2000 (2).

The increasing number of elderly patients will undoubtedly produce an increased number of osteoporotic vertebral compression fractures (OVCFs) and an increase in the morbidity and mortality that is associ-

ated with these fractures (3,4). The mortality of OVCFs can be substantial, with patients experiencing up to a 40% lower survival over time as compared to patients without OVCFs (5,6).

The importance of selecting the optimal treatment is emphasized by evidence that suggests the treatment of the underlying osteoporosis that predisposes patients to OVCFs can decrease mortality up to 11%, along with a Medicare claims-based analysis that showed that the median life expectancy can be increased from 2.2 to 7.3 years in patients treated with

vertebral augmentation (VA) versus those treated with nonsurgical management (NSM) (3,7).

Prior to the publication of 2 placebo-controlled randomized controlled trials (RCTs) in the *New England Journal of Medicine* (NEJM) in 2009, the literature on vertebroplasty (VP) and VA was almost universally supportive of these procedures, but the 2 trials published by Kallmes et al (8) and Buchbinder et al (9) found no beneficial effect of cement augmentation when VP was compared with paraspinal injection of local anesthetic (8,9). These studies were subsequently discredited due to fundamental flaws and downgraded to Level II evidence-based on the inclusion and exclusion criteria in both of the trials and the crossover rate in the Kallmes trial (10). Since 2009, there have been 6 prospective randomized controlled trials (PRCTs) on VA for the treatment of OVCF and 5 of them have shown superior results with VA as compared with NSM (11-19). The sixth study shows no significant benefit when compared with the control group (17,18). A meta-analysis published in 2012 (19) and one published in 2013 (10) conclude that osseous augmentation is a preferred treatment option for patients who have painful OVCFs. The most extensive meta-analysis published to date analyzed all Level I and Level II data and showed that VA was superior to NSM in the treatment of osteoporotic OVCFs in regard to reducing pain and subsequent fractures (20). This meta-analysis also recognized significant heterogeneity of effects, and the current evidence is delivering inconsistent messages.

Two articles published recently, including a review (21) and a meta-analysis (22), are examples of inconsistent or biased data reporting with overemphasis on certain trial types with complete exclusion of other types of data and reporting of conclusions that are partially representative or not representative of the data and results content of the manuscript. We will examine some examples of this reporting and some of the publications that contain it along with comparing the publications to the current body of published literature germane to this topic.

Inconsistent Data Reporting and Data Overemphasis

A health technology assessment article published in 2015 by the Cochrane Collaboration features a review authored by the 2 lead authors of the 2009 NEJM RCT articles along with 6 other authors (21). They conclude that based on moderate quality evidence they do not support using VP for treating OVCFs in routine clinical practice and they found no important clinical benefits

of VP compared with a sham procedure in regard to pain, function, and quality of life. They also conclude that the treatment results were no different in patients who had more than 6 weeks of pain versus those with less than 6 weeks. They go further to say that the open trials comparing VP with NSM are likely to have overestimated the benefit of VP and that correcting bias would have converted the results to those similar to the results found in the sham trials. They highlight serious adverse events that have been seen with VP and state that they cannot be certain if VP results in an increased risk of new OVCFs.

This review purports to review 11 RCTs and one "quasi-RCT" but classified all non-placebo trials as having a high risk of bias and based their conclusion that VP provides no benefits in pain, disability, and quality of life on the 2 sham trials rather than on the other 10 articles. They also base their conclusion that they are uncertain if VP increases the risk of new OVCFs on only one sham study and 2 other trials, thereby only including a small minority of the literature in their analysis.

This is indirect contradistinction to another meta-analysis of 29 studies authored by one of the authors of the Cochrane Review that finds a dramatic reduction of pain for both VP and kyphoplasty (KP), decreasing from 8.0 ± 1.5 in both groups to 2.9 ± 1.5 in the KP group and to 2.9 ± 1.7 in the VP group (22), a 5.1 point reduction in pain as compared with a 0.7 point decrease in pain in the 2 sham trial VP groups. The disability conclusions are equally disparate with an improvement of 4.8% in the disability measurement in the Cochrane Review as compared with a 36.3% improvement in the large meta-analysis.

The pain and function discrepancies between the Cochrane Review and the meta-analysis by Gu et al (22) are considerable and can be explained by the very small number of articles included in the Cochrane analysis and the prominent focus on the sham trials in lieu of the other literature. Two of the authors of the Cochrane Review were the lead authors of the sham trials, both of which reported pain relief scores far below other RCTs and meta-analyses including a 3.0 point reduction reported by Kallmes et al (8) and a 2.3 point reduction reported by Buchbinder et al (9). The meta-analysis by Papanastassiou et al (20) reported a pain decrease of 4.55 points for VP procedures, the Vertos II study by Klazen et al (16) reported a 5.7 point reduction, and the meta-analysis by Anderson et al (10) reported a highly statistically significant pain reduction. Prospective observational data also support dramatic pain reduction

with Hübschle et al (23) reporting a 4.0 point reduction in pain that remained up to one year in data from the Swiss Spine Registry.

In addition to pain, patient function and quality-of-life measurements are outliers in the Cochrane Review as compared with nearly all other high-quality literature including meta-analyses by Papanastassiou et al (20) and Anderson et al (10), registry data from the Swiss Spine Registry (23), and comprehensive health technology assessments by the National Institute of Health and Clinical Excellence (NICE) (24). This literature and the vast majority of the body of literature reports significant improvements in pain, function, and quality of life when comparing VP and VA with NSM. The meta-analysis by Gu et al (22) reported prominent improvements in pain and function that were not found in the Cochrane Review authored by a common author.

Source of Literature Discrepancy

The origin of much of the discrepancies found in the literature comes from the sham trials published in 2009. These trials were the basis and framework of the Cochrane Review analysis and the recommendations included within the manuscript. The same information or variations of it has appeared many times including a 2007 article describing the outline of the INvestigational Vertebroplasty Efficacy and Safety Trial (INVEST Trial) (25), a 2009 article concluding the patient demographics of patients enrolled in the INVEST trial were similar to those in a cohort of eligible but unenrolled patients (26), a 2010 article analyzing the blinding efficacy in the INVEST trial (27), a 2010 article stating an unblended injection of local anesthesia is ineffective for treating pain from OVCFs (28), and others published by the same author(s). All of these support the conclusions of the INVEST trial including a 2012 commentary where Kallmes et al (30) states that 2 other studies were consistent with the sham trials and reference Voormolen et al (14) and Rousing et al (17) both of which, however, report dramatic pain reduction with Voormolen concluding that pain relief and improvement of function after VP is immediate and significantly better with VP in the short term. Rousing reported a 6.0 point reduction in pain after both VP and NSM and concluded in a later article that VP is a good treatment for some patients with OVCFs.

Despite the apparent bias toward supporting the data from the sham trials, there are discrepancies in the conclusions of the reported data. In the Cochrane Review the RCTs were classified as being at a high risk

of bias due to lack of participant and study personnel blinding, however, one of the authors had previously reported in 2008 that the treatment of painful OVCFs in patients with dementia (and therefore a very low risk of bias and low susceptibility to placebo effect) demonstrates a high rate of pain relief and mobility and went on to conclude that the study offered additional evidence that VP has true benefit (29).

Possibly the most significant discrepancy in clinical practice versus data reporting is that despite the recommendation against VP in the Cochrane Review, the statement that VP is no better than sham in the INVEST trial and the commentary that there is no comparison between NSM and VP and that NSM is “the way to go (30),” in a 2011 publication, Luetmer and Kallmes (31) conclude that after the publication of the “INVEST and the Australian Trial” they “continue to offer the procedure to a high proportion of referred patients.”

The natural question is if VP were no better than NSM then why would the authors continue to offer it to most of their patients? Why do the sham trials show such poor improvements in pain function and quality of life measurements when virtually all other studies show exactly the opposite? How can we take this evidence and apply it to improve clinical treatment of patients with OVCFs?

Sham Treatment vs. Clinical Treatment Options

One of the conclusions in the Cochrane Review article is that there is no “role for vertebroplasty for treating osteoporotic vertebral fractures in routine clinical practice.” As stated previously, these conclusions are based primarily on the 2 sham trials. The issue with this recommendation is that it explicitly recommends against VP in routine clinical practice based on articles using a treatment that is never, or almost never, used in routine clinical practice. Sham treatment is not used to any significant degree in clinical practice and should not be the basis upon which clinical recommendations are made (32).

In a 2012 meta-analysis, Papanastassiou et al (20) found prominent reduction of pain in all of the Level I and Level II data except for the sham trials, which were the only 2 trials comparing VP and VA with treatments that are not offered in routine clinical practice. The remainder of the data found substantial improvements in pain, function, quality of life, and fewer additional OVCFs in those treated versus those undergoing NSM.

Prospectively collected observational data includ-

ing data from the Swiss Registry, the largest observational study for VA, concludes that VA is safe and effective in decreasing pain, improving quality of life, decreasing use of pain medications, and that these results show clinical improvement early that extends out to one year (23). Registry data from the Bench-Market Medical Registry shows excellent results in pain decrease and functional improvement with a mean pain reduction of 6.6 points in pain by 90 days and an improvement of 12.3 points on the Roland Morris Disability Questionnaire in the same time period (33). The registry data captures the data from the patients as treated in routine clinical practice and is highly reflective of the results commonly obtained from VP and VA.

The largest on-label, multicenter, prospective, observational study assessing KP in treating patients with symptomatic OVCFs has been completed and will be published after the one year data has been collected. The interim analysis of the 350 patients enrolled in the study suggests the final results will be similar to the other large RCTs published recently (12,34).

The results of the sham trials call into question some of the conclusions of the authors to extrapolate these results into treatment recommendations. If the sham studies have produced results that are very different than those obtained in typical clinical practice, large RCTs, and in VA registries, then are they really useful upon which to base treatment recommendations?

Implications of a Sham as a Nontreatment Arm

Placebo arms are typical for medication trials but are more controversial in surgical procedure and medical device assessments. The controversy stems from using a sham as a nontreatment arm and the implications that come from not treating patients.

It is certain that the risk of performing VA should be considered but it should also be compared with the risk of not doing the procedure as these patients are typically debilitated, have a relative rate of mortality of 8.6 times age-matched controls, and have 40% more mortality after 8 years (35,36). As mentioned previously, if the patient is treated with VA, the median life expectancy is increased between 2.2 and 7.3 years across all treated groups as compared with NSM (3). In the largest longitudinal, population-based comparison of mortality risk between surgical and NSM groups containing 858,978 patients with OVCFs, including 119,253 patients treated with KP and 63,693 patients treated with VP, the 4-year follow-up showed that the VA treat-

ment group was 37% less likely to die than the NSM group and that the adjusted life expectancy was 85% greater for the VA group. A retrospective review of VA for OVCFs by Gerling et al (35), where treatment with VA was compared with NSM in a hospital setting, found a significant survival advantage ($P < 0.001$) for patients treated with VA over those patients treated with NSM, regardless of comorbidities, age, or the number of fractures diagnosed at the start-date.

The inclusion of a sham as a treatment arm must take into consideration the well-known increased morbidity and mortality of those patients with OVCFs, as well as the information that has been published since the sham trials that VA can not only decrease mortality but that it can be life prolonging in the Medicare population suffering from OVCFs. Sham treatments have not been compared with NSM nor is it likely to be, given that sham treatments are not clinically accepted as a viable treatment option for patients with painful OVCFs.

A full discussion of the morbidity and mortality associated with OVCFs is beyond the scope of this article, but there are manuscripts by Chen et al (37) and Zampini et al (38) that support the reduced mortality of patients treated with VP and VA and data from Babayev et al (4) that details the morbidity associated with osteoporotic insufficiency fractures. The prominent morbidity and mortality is important information that must be considered when deciding to include a nontreatment placebo arm in a study to evaluate a medical device or procedure.

In the authors' conclusions of the Cochrane Review article they state that "numerous serious adverse events have been observed during vertebroplasty" and say that they cannot be certain whether VP "results in a clinically important risk of new symptomatic vertebral fractures (30)." It is interesting that the authors include Papanastassiou as a reference article but don't mention one of the key conclusions in this article, that patients treated with VP and KP had a rate of additional vertebral fractures of 11% as compared with those patients treated with NSM who had a rate of additional fractures of 22%. They also do not mention a recent meta-analysis by Song et al (36) that had rigorous inclusion criteria and reviewed VA versus NSM to evaluate the rate of additional fractures. The review included an updated review and systematic meta-analysis of RCTs and prospective non-RCTs (NRCTs) and showed no statistically significant difference in additional OVCFs among patients treated with VA compared with patients treated with NSM. Song et al (36) also stated

that VA was therefore preferred over NSM as the former can provide immediate pain relief and functional improvement.

Accordingly, if it appears that the best quality data indicates that there is either no difference in additional OVCFs after VP or that the rate of additional fractures is even less in those patients having undergone VP or VA than those having NSM, then the authors caution that numerous serious adverse events have been observed during VP should be considered more strongly and compared with the adverse event of not undergoing VP.

The most exhaustive report of VA was published by the NICE Committee in 2013 when they concluded a 2-year extensive review of VP and KP for the treatment of OVCFs. After reviewing thousands of pages of clinical studies, public commentary and forums, and scientific testimonies they concluded that the incidence of serious complication for patients undergoing VA is rare (24).

Compared to rare complications with VA, Chen et al (37) reported a 20% higher adjusted risk of death in those patients undergoing NSM than those undergoing VA. In a large study of 6,459 patients with osteoporosis, there was a 1.5 times increased mortality risk in patients with OVCFs compared to those without vertebral deformities, and women with 3 or more vertebral fractures were 4 times more likely to die than women without deformities (39).

Prior to basing conclusions on sham treatment and especially before including a sham treatment arm, the repercussions of not treating the patients must take into consideration including the well-established morbidity and preventable mortality in patients with OVCFs. The mortality information that has been published since 2009 indicates that VA may be life-saving and life-prolonging and should be taken into serious consideration before establishing a blinded non-treatment arm.

Although the placebo comparison originated with medication trials, the recent use of this in treating patients with osteoporosis with increased risk of fracture has been called unethical by Stein and Ray (40) in a 2010 NEJM editorial because of the effectiveness of the osteoporosis medications and the potential for an increase in preventable fractures in patients receiving placebo. They go on to say that osteoporosis medication trials using placebo cannot be justified by regulatory preferences for placebo-controlled studies (40). So if the treatment of osteoporosis can decrease

the patients mortality by 11% (7) and VA can decrease mortality by 24% (3), why would a placebo be acceptable in a VA study that has a much higher mortality rate and not acceptable in an osteoporotic medication trial?

Implications of Applying the Conclusion Recommendations to Clinical Practice

In a meta-analysis published by Gu et al (22), the authors conclude that there is “no significant difference” between “vertebroplasty and kyphoplasty in short- and long-term pain and disability outcomes.” While this is true, the authors emphasize the lack of statistical difference in pain and disability, but did not mention the lower rate of additional fractures, less cement leakage, and greater kyphosis reduction with KP that was reported in the results section. These elements are very important, as cement leakage is the leading cause of severe complications in VA, each additional fracture puts the patient at a higher risk for morbidity and mortality, and a recent study of KP showed that patients with better kyphotic restoration had significantly higher clinical benefits in terms of pain relief, better function, and quality of life improvement (41). In the largest VP vs KP comparison trial performed to date, the Augmentation and Restoration of Vertebral Body Compression Fractures (KAVIAR) trial, the rate of additional fractures was considered to be important enough to be the primary endpoint studied with differences in pain, function, and quality of life being categorized as secondary endpoints (42).

One of the same authors in the Gu et al meta-analysis (22) that emphasized the lack of significant difference between VP and KP for pain and function has also published an article that claims no difference in the efficacy of VP and KP (43). Similar authors have also published numerous articles centered firmly around the sham trials claiming that VP is no better than sham surgery and should not be used in routine clinical practice (8,9,21,25-28,30,31,44), with publication patterns to indicate that the natural extrapolation that VA or KP does not work and should not be used in routine clinical practice—all of this despite an enormous amount of data strongly supporting the use of VA for the treatment of OVCFs (20), no data stating that NSM is better than either VP or KP for the treatment of OVCFs, and no data supporting thoracolumbar bracing over VA for OVCFs. The process of gathering and publishing certain evidence in support of certain authors’ previously held beliefs is a known form of bias called content-based

Table 1. *Author contradictions.*

| Topic | Author – Time-Point 1 | Author – Time-Point 2 |
|--|--|--|
| VP for treatment of painful OVCFs | There is “no role for vertebroplasty for treating osteoporotic vertebral fractures in routine clinical practice (21).” | Author concluding that after analyzing the first 1,000 consecutively treated VP cases “practitioners can quote a high success rate and low complication rate for vertebroplasty when making treatment recommendations for painful spinal compression fractures (43).” |
| Study bias affect on treatment outcome | Open trials comparing VP with NSM are likely to have overestimated the benefit of VP and that correcting bias would have converted the results to those similar to the results found in the sham trials (21). | We used the Cochrane risk of bias tool for randomized trials and the Newcastle Ottawa Scale for non-randomized trials. The baseline pain in both of the groups was 8.0 ± 1.5 and decreased to 2.9 ± 1.5 in the KP group in the immediate postoperative phase and to 2.9 ± 1.7 in the VP group ($P = 0.39$) (22). |
| Affect of blinding on outcome assessment | RCTs classified as being at high risk of bias due to lack of participant and study personnel blinding (21). | Treatment of painful OVCFs in patients with dementia (and therefore very low risk of bias and low susceptibility to placebo effect) demonstrates a high rate of pain relief and mobility (29). |
| Offering VP to patients | VP is no better than sham in the INVEST trial (8); there is no comparison between NSM and VP and that NSM is the way to go (30). | After “INVEST and the Australian Trial,” they “continue to offer the procedure to a high proportion of referred patients.” |
| Pain reduction with VP | No important clinical benefits of VP compared with a sham procedure in regard to pain. Treatment results were no different in patients who had more than 6 weeks of pain versus those with less than 6 weeks of pain (21). | We conducted a meta-analysis that showed that VA is associated with significant reduction in back pain (52). |
| Disability Reduction with VP | There is no significant reduction in disability with an improvement of 4.8% (RMDQ) (21). | There is a large reduction in disability after VP with an improvement of 36.3% (ODI) (22). |
| Complications associated with VP | Numerous serious adverse events have been observed following vertebroplasty (21). | Based upon sham trials, there were no significant between-group differences in the number of serious other adverse events (VP = 3/106, placebo = 3/103; RR 1.01 [0.21–4.85] (21). |

bias and the overemphasis of the sham trials far out of proportion to their level of importance can be construed as conformation bias (45) (Table 1).

The authors of the Cochrane Review article state that there is no “role for vertebroplasty for treating osteoporotic vertebral fractures in routine clinical practice” based largely on sham treatment that is rarely, if ever, used in routine clinical practice. It is unusual enough to base a recommendation on something that is not done in clinical practice, however one author has also published a manuscript that has findings that are nearly the opposite—concluding after analyzing the first 1,000 consecutively treated VP cases that “practitioners can quote a high success rate and low complication rate for vertebroplasty when making treatment recommendations for painful spinal compression fractures (43).” This type of trial is similar to other large on-label, prospective studies that are similar to the Food and Drug Administration’s (FDA’s) Phase 4 trials that are used as post-market studies to provide information about the

procedure’s risks, benefits, and best use. These trials have been strongly supportive of VA and include trials with large patient numbers (42,46-48). The tendency to dwell on the sham studies and the seemingly one-sided presentation of the articles are similar to the situation surrounding their original publication when the NEJM did not solicit commentary from other practitioners’ analysis of the body of existing literature and did not include a balanced dissemination of information along with the publication of the sham trials (49).

Conclusions differing from data results in the same paper have also been seen. In an article on mortality associated with VA by McCullough et al (50), the authors, including 2 of the authors of one of the sham trials, concluded that due to selection bias “spinal augmentation did not improve mortality or major medical outcomes (50).” This conclusion was made in spite of their own information displayed in Table 2 stating that patients undergoing augmentation had significantly less mortality at 30 days and one year using multi-

Analysis of Reporting Bias in Vertebral Augmentation

Table 2. *VA controversies.*

| Topic | Author Group One | Author Group 2 | Comment |
|---|---|---|--|
| Morbidity and Mortality from osteoporosis and/or osteoporotic OVCFs | Osteoporosis medication trials using placebo cannot be justified by regulatory preferences for placebo-controlled studies, given the decrease in mortality of 11% in patients treated for their osteoporosis (40). | We need more sham trials (8). | If VA can decrease mortality by 24% (51), why would a placebo be acceptable in a VA study that has a much higher mortality rate and not acceptable in an osteoporotic medication trial? |
| Pain reduction | VP offers no significant pain reduction compared to sham (paraspinal injection of anesthetic) (8). | VP offers significant pain reduction in all clinical trials and with consecutive on-label use (20,23). | VP offers significant reduction of pain in all high-quality studies except for 2 studies comparing it to a procedure that has been shown to reduce fracture pain (53,54). |
| Patient function and quality of life | VP offers no significant improvement in patient function and quality of life. | Large meta-analyses, registry data, and comprehensive health technology assessments show significant reductions in patient function and quality of life (10,20,23,24,54,55). | Same group of authors stating there is no patient function or quality of life benefit, whereas the overwhelming body of literature states otherwise (10,20,23,24,54,55). |
| Adjacent level or additional level fractures | Numerous serious adverse events have been observed during VP, and it cannot be certain whether VP results in a clinically important risk of new symptomatic vertebral fractures (55). | Meta-analyses of RCTs and NRCTs and prospective studies showed no statistically significant difference in additional OVCFs among patients treated with VP compared with patients treated with NSM (10,20,36,54-56). | Best quality data indicates that there is either no difference in additional OVCFs after VP or that the rate of additional fractures is even less in those patients having undergone VP or VA than those having undergone NSM (10,20,38,54-56). |
| Use of VP | VP should not be used in clinical practice (21). | Meta-analysis data provides strong evidence in favor of VP in the treatment of OVCFs (10). | Sham treatment is not used to any significant degree in clinical practice and should not be the basis upon which clinical treatment recommendations are made. The repercussions of not treating the patients with VA must take into consideration including the significantly increased mortality seen in patients with OVCFs (51). |
| Comparisons of VP with BKP | Similar authors have published numerous articles based on the same 2 sham trials claiming that VP is no better than sham surgery and should not be used in routine clinical practice(8,9,21,25-28,30,31) and have attempted to extrapolate equivalence of VP to BKP (22). | There is an enormous amount of data strongly supporting the use of VA for the treatment of OVCFs (10,20,23,24,54) and that BKP is better than VP in terms of pain, quality of life, and mortality (20,51); there is no data stating that NSM is better than either VP or KP for the treatment of OVCFs and there is no data supporting thoracolumbar bracing over VA for OVCFs. | The literature can be and has been selectively parsed to support certain opinions but the existing body of literature data demonstrates strong support for VA. The extrapolation of recommending against a procedure based on a sham treatment that has been previously used for treatment effect and is rarely, if ever, applied in routine clinical practice is ill-advised especially when the associate increase in morbidity and mortality are considered. Reporting conclusions that are not representative of the data contained within the manuscript puts the authors at risk of bias and should be avoided. Extrapolation of conclusions based on an informal amalgam of different data sets and article types should be eschewed completely. |

variate analysis and at 30 days using propensity score matching analysis. The only group that did not demonstrate a statistically significant mortality reduction

was the propensity score-adjusted group at one year but the p-value was very nearly significant at 0.18. Their conclusion was that no mortality improvement

was made despite 3 of 4 analyses showing mortality improvement.

CONCLUSION

With a dramatically increasing number of patients predisposed to OVCFs and with an increasing need for effective treatment of this painful condition, it is important that a safe and effective treatment is utilized to treat these debilitated patients and to try to forestall the morbidity and mortality that are associated with these fractures (51).

The widespread acceptance of VA prior to the publication of the sham trials has been supported by 6 PRCTs on VA and 2 meta-analyses published in 2012 and 2013 supporting osseous augmentation as the preferred treatment option for patients who have painful OVCFs.

We focused our attention on 2 recently published articles that have examples of inconsistent data reporting by the authors of those articles, in which they overemphasize particular categories of data while excluding other important data types. The literature can be selectively parsed to support certain opinions and

discrepancy of the reported data, but the existing body of literature data should be examined carefully.

The extrapolation of recommending against a procedure that was previously demonstrated to be effective based on a sham treatment that is rarely, if ever, applied in routine clinical practice is ill-advised, especially when the associated increase in morbidity and mortality are considered if the recommended treatment is not applied. Reporting conclusions that are not representative of the data contained within the manuscript or the spirit of the data puts the authors at risk of bias accusations and should be avoided, and extrapolation of conclusions based on an informal amalgam of different data sets and article types should be eschewed completely.

Conflict of Interest

Dr. Beall is a consultant for Medtronic, Dfine, Osseon, Lilly, Smith & Nephew, Vertiflex, Synthes, Alphatech Spine, Benvenue, Convatec, Integral Spine Solutions, Medical Metrics, Zyga, Liventa, Vexim, and Mesoblast.

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