

CASE STUDY

# A Case Study of Misrepresentation of the Scientific Literature: Recent Reviews of Chiropractic

JOSEPH MORLEY, D.C., Ph.D.,<sup>1</sup> ANTHONY L. ROSNER, Ph.D.,<sup>2</sup>  
and DANIEL REDWOOD, D.C.<sup>3</sup>

## ABSTRACT

Accurate use of published data and references is a cornerstone of the peer-review process. Statements, inferences, and conclusions based upon these references should logically ensue from the data they contain. When journal articles and textbook chapters summarizing the safety and efficacy of particular therapies or interventions use references inaccurately or with apparent intent to mislead, the integrity of scientific reporting is fundamentally compromised.

Ernst et al.'s publication on chiropractic include repeated misuse of references, misleading statements, highly selective use of certain published papers, failure to refer to relevant literature, inaccurate reporting of the contents of published work, and errors in citation. Meticulous analysis of some influential negative reviews has been carried out to determine the objectivity of the data reported. The misrepresentation that became evident deserves full debate and raises serious questions about the integrity of the peer-review process and the nature of academic misconduct.

## INTRODUCTION

One of the major concerns of any profession is the reporting of negative findings in relation to outcomes and practices of that profession. Professional, academic, and clinical integrity require that such data be carefully analyzed and incorporated into ongoing audit and review of methods and outcome so as to refine and improve ongoing activity and to eradicate what is obsolete. Some recent influential publications (Ernst, 1998; Ernst and Assendelft, 1998; Ernst, 1999), which advise that chiropractic may be ineffective and potentially

harmful have caused concern. We have analyzed their content meticulously because their findings do not accord with our experience or that of the profession as a whole and because they have the potential to influence interprofessional relations and public policy adversely. We believe that what has emerged is cause for serious concern and debate. Ernst himself coauthored a study addressing the problem of bias within the context of complementary and alternative medicine generally (Resch et al., 2000). We report here repeated misuse and highly selective use of references, misleading statements, with inaccuracies in content and er-

<sup>1</sup>43 Elmside, Guildford GU2 7SJ, UK.

<sup>2</sup>Director of Research and Education, Foundation for Chiropractic Education and Research, Suite 315, 1330 Beacon Street, Brookline MA 02446-3202.

<sup>3</sup>1645 Laskin Road, Suite 103, Virginia Beach, VA 23451.

rors in citations by that same author in his evaluation of the chiropractic literature.

The case presented in this paper is based on the following publications:

- (1) "Chiropractors' Use of X-Rays," a commentary published in the *British Journal of Radiology* (Ernst, 1998).
- (2) "Chiropractic for Low Back Pain: We Don't Know Whether It Does More Good Than Harm," an editorial in the *British Medical Journal* (Ernst and Assendelft, 1998).
- (3) "Adverse Effects of Spinal Manipulation," a chapter in the textbook *Essentials of Complementary and Alternative Medicine* (Ernst, 1999).

Our documentation begins with the commentary in the *British Journal of Radiology*, which was the first item to be published. In the two subsequent publications, Ernst et al. repeat many questionable claims made in the *British Journal of Radiology* article. In our analysis of the *BJR* article, we note those instances that we believe represent serious problems with one asterisk [\*] and use two asterisks [\*\*] to indicate

what we believe are very serious misrepresentations of the literature. See Table 1 for brief examples.

## PUBLICATIONS AND PROBLEMS

### I. Ernst E. Chiropractors' use of X-rays. *Br J Radiol* 1998;71(843):249-251

In this *British Journal of Radiology* commentary, which concludes by suggesting that there is "an overuse of spinal radiography in the chiropractic profession" that "constitutes a safety problem," 36 references are cited. The nature and number of errors, and the way in which they are drawn from the papers cited, are such that it is simplest to report on each reference individually and in the order in which it is cited in the original paper, rather than according to the seriousness of the error.

#### Reference #5 (Nilsson, 1995)\*

##### Problems identified:

- (1) *Using a reference that fails to support the claim it is used to support.*

The first paragraph in the *British Journal of Ra-*

TABLE 1. EXAMPLES OF ERNST'S MISREPRESENTATION OF THE SCIENTIFIC LITERATURE ON CHIROPRACTIC

Reference	Specifics of misrepresentation
Penfil and Brown, 1968	480% exaggeration of the dose of gonadal radiation administered to chiropractic patients; failure to differentiate testicular and ovarian targets; failure to report routine use of essential shielding devices by chiropractors; repeating an erroneous quotation from a secondary source (Burns and Mireau, 1997) while claiming to be quoting from Penfil and Brown as a primary source
Carey et al., 1995	Altering critical text in Carey quote to state erroneously that chiropractors were the group using X-rays most frequently, while omitting data from Carey showing that orthopedists used X-rays more than chiropractors
Crelin, 1973	Use of a single, controversial quote from Crelin as sole supporting evidence to conclude that there is no basis to the chiropractic concept of subluxation, while omitting via use of ellipsis (...) a key qualifying phrase from Crelin's sentence that substantially diminishes its current relevance
Burns and Mireau, 1997	Quote from Burns and Mireau that chiropractors "tend to over-utilize x-rays, especially full spine studies," while omitting the sentence that immediately follows: "Fortunately this practice is in decline as the profession moves toward adherence to more standardized indications for radiography."
Frymoyer, ed., 1991	Use of Frymoyer reference to support conclusion that, "experts on low back pain uniformly agree that plain radiographs are not usually useful in this condition," while omitting the following exceptions to this statement noted in Frymoyer: patients with persistent radicular symptoms; back pain beyond 6 weeks; or spondylolisthesis
Michel et al, 1990	Inaccurate citation of criteria for evaluation of osteoporosis, leading to implication that chiropractors indiscriminately treat patients with this condition without justification or appropriate selection of techniques

*diology* paper by Ernst states: "Many other conditions are also treated by chiropractors, and for those there is little or no evidence for efficacy." However, Nilsson's reference used to buttress this claim concerns a randomized controlled trial of the effect of spinal manipulation in the treatment of cervicogenic headache. Contrary to Ernst's use of this reference, Nilsson did *not* state that there was little or no evidence for efficacy; rather the paper states: "The results suggest a possible effect of manipulation on cervicogenic headache, but because of methodological problems, such an effect could not be unequivocally demonstrated." Furthermore, a subsequent, larger trial by Nilsson demonstrated clinically and statistically significant effects of spinal manipulation upon cervicogenic headache (Nilsson et al., 1997). Nilsson recognized and rectified a classic Type II error (i.e., wrongly accepting the null hypothesis and therefore incorrectly concluding that there is no effect).

Ernst uses the Nilsson study, which looks at chiropractic treatment of headache, as the sole support for his contention that chiropractic lacks evidence of efficacy for the conditions he listed in his Reference #4 (Anonymous, 1996), which included back pain, anemias, infertility, and heart arrhythmias. Reference #4 is a brief, anonymous report of a single advertisement from a chiropractic clinic and is not representative of typical chiropractic practice. References #4 and #5 as cited by Ernst bear no relation to each other; their use is therefore neither logical nor appropriate.

#### **Reference #6 (Abbot et al., 1996)\***

##### **Problems identified:**

- (1) *Ignoring relevant references.*
- (2) *Inaccurate claim.*
- (3) *Claim unsupported by evidence.*

Ernst comments here on adverse events associated with manipulation. This is a very important issue but Ernst fails to reference the extensive literature discussing adverse events published in the chiropractic literature, claiming instead that "the safety of chiropractic is an important *albeit neglected* issue [*italics ours*]." Ernst references only two papers, both of which he himself was involved in writing (Abbot et al., 1996; Ernst, 1994).

Safety issues are not neglected but are taken very seriously by the chiropractic profession. Students are taught about this key topic in chiropractic colleges. Professional practice guidelines have been developed in the United States and Canada, which refer to extensive literature reviews on the subject of vertebrobasilar artery injury (Haldeman et al., 1993; Henderson, 1994).

One report by Terrett and Kleynhans (1992) discusses the potential cerebrovascular complications of manipulation and lists strategies for preventing complications associated with cervical manipulation. Based upon information available at the time their article was written they suggest that it is essential to:

- Understand the causes of reported complications from manipulation.
- Understand the contraindications to manipulation.
- Perform a diagnostic assessment of patients before manipulation.
- Avoid certain therapies in patients thought to be at risk.
- Avoid those techniques that appear to carry the greatest risk.

These authors go on to include a table listing reports of vertebrobasilar accidents following manipulations from around the world, covering a period of 55 years. The authors also list the type of provider (i.e., chiropractor, medical doctor, osteopath, naturopath, or physiotherapist) involved in each vertebrobasilar accident.

It is estimated that chiropractors perform 94% of spinal manipulations in the United States (Shekelle et al., 1992). According to Terrett and Kleynhans' data, of 29 deaths from spinal manipulation reported in the literature over a 55-year period, chiropractors were associated with 14, or 48%. The remainder were attributed to the following categories: medical doctors (8); osteopaths (2); naturopaths (2); wife (1); and unknown (2).

Although it is not possible to state precisely what percentage of manipulations medical doctors perform, it is unlikely to be more than about 3%, because chiropractors account for 94% of manipulations and physiotherapists and osteopaths also practice these methods.

Yet, 28% of the deaths were associated with manipulative procedures performed by medical doctors. It would appear therefore that patients who are undergoing manipulation are at greater risk of a cerebrovascular accident at the hands of a medical doctor.

It is surprising that Ernst chose not to reference this data, failing to draw to public attention the fact that the training of the practitioner (i.e., chiropractic versus medical) appears to be a risk factor for adverse events in spinal manipulation.

This is explored further by Terrett (1995) who shows that a number of complications and even death are wrongly attributed to a chiropractic neck manipulation but are more often caused by manipulation administered by a nonchiropractor. He concludes: "The words chiropractic and chiropractor have been incorrectly used in numerous publications dealing with SMT [spinal manipulative therapy] injury by medical authors, respected medical journals and medical organizations. In many cases, this is not accidental: The authors had access to original reports that identified the practitioner involved as a nonchiropractor. The true incidence of such reporting cannot be determined. Such reporting adversely affects the reader's opinion of chiropractic and chiropractors."

None of these relevant references are cited in the *British Journal of Radiology* review in relation to the safety of cervical manipulation, giving rise to an erroneous impression of risk.

A study by Dabbs and Laretti (1995), also not cited in the *British Journal of Radiology* paper, concluded that cervical manipulation for neck pain gives rise to fewer adverse reactions than nonsteroidal anti-inflammatory drugs (NSAIDs), by as much as a factor of several hundred times. Furthermore, NSAIDs have not been shown to be more effective than cervical manipulation for neck pain, a point elaborated upon in Sections II and III below. Moreover, there is an important qualification not cited by Ernst, in a companion volume to his reference #25 (Burns and Mireau, 1997), which states: "Exact incidences of CVAs following manipulation is unknown, estimates [are] between 1–3 per million. This indicates that carefully performed cervical manipulation is probably safer than complications from medical treatment for mechanical neck pain." (Gatterman, 1998).

Finally, Ernst writes that indirect risks associated with chiropractic are related to the "philosophy" and clinical practice of a given "school." Having used quotation marks, he does not define either, citing no reference to support this statement with obviously serious implications.

#### Reference #14 (Carey et al., 1995)\*\*

##### Problems identified:

- (1) *Altering critical text in quotation.*
- (2) *Inaccurate claim.*

The *British Journal of Radiology* article cites a "recent clinical trial testing the effectiveness of various approaches to treat acute low back pain," referring to a study by Carey et al. (Carey et al., 1995). In their abstract, Carey et al. state that a *prospective observational study* was done, not a clinical trial. Ernst then continues to quote the Carey et al. study: "Plain spine radiographs were used most frequently by the chiropractors (67% of all patients)." Carey et al.'s actual quotation is as follows: "Plain spine radiographs were used more frequently by the chiropractors and orthopedists (in 67 to 72 percent of the patients) than by the other groups of providers." As a table in Carey et al.'s article confirms, it was the *orthopedists* rather than the chiropractors who actually used radiographs the most. It seems unlikely that this misquote was accidental because the data refuting Ernst's contention is in direct proximity to the portion he misquotes.

#### Reference #16 (Michel et al., 1990)\*\*

##### Problems identified:

- (1) *Failure to understand the data contained in a reference, leading to a false conclusion.*
- (2) *Claim unsupported by evidence.*

Ernst states: "Osteoporosis is a relative contraindication for several chiropractic techniques, yet, plain radiographs of the spine do not allow osteoporosis to be diagnosed unless bone density has decreased by at least 40%," citing Michel et al.'s study (Michel et al., 1990) to back up his own statement. However, Michel et al. do not actually state this but refer to previous research stating that a decrease in bone density is detectable on lateral lumbar X-rays only after the bone loss is 40%.

A precise reading of Michel et al. is essential to appreciate the erroneous nature of Ernst's

claim. Michel et al. write: "Surprisingly, we found a higher correlation between radiographic criteria and bone mineral content for subjects above 110 mg/cm<sup>3</sup> [above this level, bone density is considered to be normal] than for those with a lower bone density. This suggests that osteopenia may be identified earlier than generally supposed (via lumbar radiographs). . . . Our results should not be interpreted to promote lumbar radiographs as a reliable tool for precise assessment of osteopenia. Nevertheless, in the hands of physicians familiar with lumbar radiographs, they may facilitate decisions on further evaluation. . . . *The traditional postulated requirement for bone loss to reach 40% to be detectable on roentgenograms has to be reconsidered. Our study shows that in the range of bone mineral content over 110 mg/cm<sup>3</sup> radiographic criteria allow even better differentiation of the degree of relative osteopenia than below 110 mg/cm* [italics ours]."

Ernst's misuse of Michel et al.'s key conclusions leaves the reader with an inaccurate impression. Elsewhere, Ernst goes on to write that several chiropractic techniques are relatively contraindicated in osteoporosis, yet, he does not reveal which techniques these are or why they are contraindicated nor does he provide any references to support his statements. Furthermore, he fails to note that chiropractic training places strong emphasis upon choosing techniques appropriate for the individual patient. Chiropractic guidelines emphasize that more forceful techniques should be avoided in cases where substantial osteoporosis is present (Haldeman et al., 1992).

#### Reference #17 (Grieve, 1994)

##### Problem identified:

(1) *Incorrect professional identification.*

Ernst writes: "Yet a chiropractic textbook states that 'bone disease is not immediately revealed by X-rays.'" The text to which he refers is not a chiropractic textbook but one on physiotherapy by Grieve (1994) in which none of the authors of any chapters are chiropractors.

#### Reference #20 (Crelin, 1973)\*\*

##### Problems identified:

(1) *Using a single, discredited reference as sole support for a broad, overarching conclusion.*

(2) *Omitting critical text from quotation.*

Ernst states that "mainstream scientists have demonstrated experimentally" that subluxation of the vertebra as defined by chiropractic does not occur. The sole evidence provided for this claim is a paper published in 1973 by Crelin, which Ernst quotes as follows: "Subluxation of the vertebra as defined by chiropractic . . . does not occur."

The Crelin study was convincingly discredited in court during the landmark *Wilk v. AMA* antitrust case. This study was expressly named among the extensive chiropractic-related literature which the American Academy of Physical Medicine and Rehabilitation (1981) found had "serious questions . . . concerning the impartiality of, the objectivity of, the conclusions, or the direct or indirect and sometimes undisclosed involvement of the AMA."

While a legal decision does not constitute scientific validity, Crelin's study has also been challenged on methodologic grounds (Giles, 1994), among them his questionable use of a drill press as a model for forces experienced during a subluxation. Crelin employed a commercial drill press to exert pressure upon vertebral columns excised from cadavers in order to produce a displacement, which he interpreted as representative of the subluxation in chiropractic theory. However, chiropractic theory and practice define subluxation in a larger clinical context; i.e., as a motion segment in which alignment, movement integrity, and/or physiologic function are altered, although contact between the joint surfaces remains intact (Gatterman, 1994; Gatterman, 1995). This dichotomy of models suggests that the narrow definition and working model provided by Crelin fail to represent subluxation adequately as it is envisioned by chiropractic theory and practice.

Ernst also chose to omit an essential portion of a quotation taken from Crelin's article which runs as follows: "subluxation of a vertebra as defined by chiropractic—the exertion of pressure on a spinal nerve which by interfering with the planned expression of Innate Intelligence produces pathology [italics ours]—does not occur." Inclusion of the entire quote from Crelin's article would have highlighted its weakness; omitting the italicized portion of the quotation lends credence to the idea that a basic tenet of chiropractic is scientifically discredited.

This quotation from a single, highly controversial article published nearly three decades ago, with a key section omitted, is the sole support for the contention that subluxation has been proven not to occur. The substantial body of literature supporting subluxation and spinal manipulation models is ignored (Bronfort, 1997; Foreman and Croft, 1995; Giles and Singer, 1997; Kirkaldy-Willis, 1983; Leach, 1993). The chiropractic understanding of the anatomic, physiologic, biochemical, kinesio-logic, and inflammatory aspects of subluxation has evolved substantially over the past 30 years (Cleveland, 1997; Rosner, 1997), a fact that might be expected to have been included for a balanced review. Moreover, the other "scientists" to whom Ernst refers as having proven that subluxation does not exist are not listed nor any supporting work cited.

#### Reference #21 (Consumer Reports, 1994)

##### Problems identified:

- (1) *Inaccurate use of direct quotation.*
- (2) *Incomplete identification of the quoted individual.*
- (3) *Misinterpretation of a key concept (failure to distinguish structure and function).*

Ernst attributes the following quotation to Haldeman: "Minor misalignments of vertebrae are normal and not necessarily a sign of trouble." He includes Haldeman's statement in quotation marks and lists *Consumer Reports* as the source. This is an American consumer magazine, not a scientific journal, and it is not peer-reviewed.

In fact, Haldeman did not write the article for *Consumer Reports* and there is no direct quote from Haldeman in the *Consumer Reports* article. It is the author of the *Consumer Reports* who states that Haldeman said this. There are no references cited in this article so it is impossible to know whether the author is describing Haldeman's views accurately. Ernst has instead paraphrased what Haldeman is alleged to have said or written, making it out to be a direct quote.

Furthermore, this alleged quotation from Haldeman is placed directly after the Crelin reference. This juxtaposition of references gives the reader the impression that the neurologist

Haldeman is endorsing the quote from Crelin to the effect that "subluxation of the vertebrae as defined by chiropractors . . . does not occur." By doing this, Ernst misses the opportunity to help the reader understand that Crelin's attempted refutation of the subluxation concept is based entirely on a structural approach. Spinal misalignments are structural, while subluxations include both structural and functional components. This was recognized as long ago as 1975 when the term "subluxation" was defined at the National Institutes for Neurological Disease and Stroke workshop on spinal manipulative therapy as follows: "the alteration of the normal dynamics, anatomical, or physiological relationships of contiguous articular structures." (Goldstein, 1975).

The *British Journal of Radiology* commentary is incomplete in identifying Haldeman as a neurologist. He is an M.D. specializing in neurology and holds a Ph.D. in neurophysiology as well as being a qualified chiropractor. Haldeman is an authority on chiropractic and has written extensively on the subject, is a frequent speaker at chiropractic seminars, and is the editor of a widely used textbook on chiropractic (Haldeman, 1992). To misquote him, identify him incompletely, and reduce his experience and expertise to a single phrase from a secondary, unverifiable source is, at best, misleading.

#### Reference #24 (Frymoyer, 1991)\*\*

##### Problems identified:

- (1) *Failure to note exceptions cited by authors of referenced article.*
- (2) *Incomplete citation.*

Ernst writes that, "experts on low back pain uniformly agree that plain radiographs are not usually useful in this condition," and cites the book by Frymoyer (1991) to support this. Again he quotes selectively, omitting the exceptions to this statement noted in the text. These are:

- Patients with persistent radicular symptoms.
- Back pain beyond 6 weeks.
- Spondylolisthesis.

Moreover, Ernst does not identify either the authors of the chapter or the pages in this 2400-

page, double-volume text where the relevant reference could be found (El-Khoury and Brander, 1991).

#### Reference #25 (Burns and Mireau, 1997)\*\*

##### Problem identified:

(1) *Omission of key point in reference that substantially modifies the part quoted.*

Ernst quotes from Burns and Mireau that chiropractic practitioners "tend to over-utilize X-rays, especially full spine studies." However, the very next sentence, which changes the whole meaning of the observation, is not given. We quote it here: "Fortunately this practice is in decline as the profession moves toward adherence to more standardized indications for radiography." Again, the specific page numbers for this reference are omitted, making it difficult to access the part of the chapter from which the selective quote is taken, thus leaving the reader unaware that the referenced source contains material that substantially modifies the impression created by Ernst's selective quote.

In fact, the conclusions arrived at by Burns and Mireau are as follows:

"Based on approximately 30 studies using controls and randomization, the following conclusions can be made:

- a. Spinal manipulation is the most studied form of treatment to date for low back pain.
- b. There is more evidence to support the use of manipulative therapy for back pain than for any other treatment.
- c. The effects of manipulation are time dependent (as with other treatments).
- d. Manipulation is most effective for uncomplicated mechanical low back pain of short duration and is less effective in chronic cases.
- e. There is no evidence that manipulation prevents low back pain or any other disorder."

#### Reference #26 (Deyo, 1996)

##### Problems identified:

- (1) *Selective presentation of data.*
- (2) *Citation error.*

This reference from Deyo discusses the clinical guidelines issued by the Royal College of Gen-

eral Practitioners, London, United Kingdom, regarding back-pain management. One of the recommendations is that radiography, imaging, and specialist referral are unnecessary in managing acute simple low backache, which is characterized by the following:

- Age 20–55.
- No radiation below the knee.
- Mechanical pain.
- Patient is well

However, Ernst omits the report's recommendation that "spinal manipulation may be considered for relief of symptoms within 6 weeks of onset," [for treating uncomplicated low-back pain]. He also omits the portion of Deyo's article, which states: "Modern medical care has not prevented a steady rise in back related disability in most developed countries, and some fear that medicine may have contributed to this rise." Pages numbers are listed incorrectly as 1333–1334; the correct pages are 1343–1344.

#### Reference #29 (Penfil and Brown, 1968)\*\*

##### Problems identified:

- (1) *Repeating an erroneous quotation from a secondary source while claiming to be quoting a primary source.*
- (2) *Incorrect mathematical calculation leading to overstatement of risk.*
- (3) *Failure to report essential information.*

*Ernst's misuse of the Penfil and Brown reference is arguably the most serious of those we have evaluated.*

Ernst begins by using this 30-year old article as a reference to back up his statement that "the gonadal radiation from one unshielded lumbar series has been estimated to be equivalent to the gonadal radiation from one chest radiograph per day for 6 years." This statistic is contradicted by the data he uses as a reference. Penfil and Brown's table of estimated average gonad dose per selected examination shows that a chest radiograph results in an average testicular dose of 5 millirads. A lumbar spine series results in an average testicular dose of 2268 millirads. This is equivalent to one chest radiograph per day for 454 days (2268/5, 1 year

and 3 months), not the 6 years cited by Ernst—a 480% overestimate.

It is surprising that this error escaped detection during peer-review, especially because the implications for public health and safety are so great. The dose to the unshielded testicles from a lumbar series is approximately 8 times greater than the dose to the ovaries, as Penfil and Brown demonstrate clearly. This difference is ignored by Ernst who uses the generic term “gonadal radiation.” Furthermore, Ernst fails to mention other aspects of Penfil and Brown’s study. Chest radiographs result in a significantly lower gonadal dose because the primary beam is collimated above the gonads. Penfil and Brown also state that the genetically significant dose can be reduced by almost 70% in males by simply excluding the testes from the primary beam. Ernst fails to mention that *this precaution is part of the protocol that is routinely taught to chiropractors in radiography*. In fact, chiropractors have been at the forefront in developing devices, such as The Wedge (Summit Industries, Chicago, IL), designed to reduce exposure to the gonads and other parts of the anatomy (Merkin and Sportelli, 1982). Chiropractors, like all responsible health professionals, pay great attention to minimizing exposure to radiation.

Despite using and quoting Penfil and Brown’s article as a reference, there is evidence that Ernst may not have obtained this information from Penfil and Brown. The statement he uses is to be found not in Penfil and Brown but in Ernst’s reference #25 (Burns and Mireau, 1997) as follows: “The gonadal radiation from one unshielded lumbar series has been estimated to be equivalent to the gonadal radiation from one chest radiograph per day for 6 years.” The error-laden statement is nowhere to be found in the paper by Penfil and Brown.

#### Reference #31 (Baer, 1996)\*

##### Problem identified:

(1) *Using a reference unrelated to the claim it is used to support.*

Ernst cites Baer to support his statement, “The validity of chiropractors’ X-ray diagnoses is not well established. Small vertebral displacements or malalignments have no proven clinical relevance.” The Baer article deals with

practice building and contains no statement that could be construed to support this contention.

#### Reference #35 (Dupuis et al., 1991)

##### Problem identified:

(1) *Citation error.*

In the penultimate paragraph of Ernst’s *British Journal of Radiology* commentary, he states that plain radiographs yield little relevant biomechanical information and cites the following reference: Dupuis PR, Yong-Hing K, Cassidy JD, Kirkaldy-Willis WH. Radiologic diagnosis of degenerative lumbar spine instability. *Spine* 1991;16:943–950. No such article exists in the journal cited. However, the journal and exact page numbers were previously cited by Ernst as reference #32, attributed to Dvorak, Panjabi, Novotny et al. (Dvorak et al., 1991).

#### II. Ernst E, Assendelft WJ. Chiropractic for low back pain: We don’t know whether it does more good than harm. *BMJ* 1998;317(7152):160

This editorial in the *British Medical Journal* speculates that chiropractic may be hazardous to health. From the vantage points of strength of evidence, safety, and cost effectiveness, the authors conclude that there may be more negative than positive evidence, leading them to suggest that chiropractic may do more harm than good.

Chiropractic has gained much of its recognition through the management of common low-back pain. At odds with Ernst and Assendelft’s conclusion that, “it is uncertain whether chiropractic does more good than harm” are the conclusions of multidisciplinary panels from at least two countries (Bigos et al., 1994; Rosen, 1994), following systematic reviews of the evidence in the peer-reviewed literature, that manipulation is one of the two best documented and efficacious approaches to the treatment of acute low back pain—the other being the use of analgesics and NSAIDs.

It appears that Ernst and Assendelft have selectively used references and omitted critical information. A detailed rebuttal to this editorial has been published elsewhere (Rosner, 1999). There are at least five misrepresented areas in the editorial:



- (1) *The strengths of chiropractic and medical evidence.*
- (2) *The relationship between chiropractic and manipulation.*
- (3) *Direct risk to the patient.*
- (4) *The use of radiographs.*
- (5) *Cost effectiveness.*

### 1. Strengths of chiropractic and medical evidence

Having pared the literature on spinal manipulation containing more than 40 controlled trials to just 8 studies, which are restricted to chiropractors (a highly unusual tactic to be discussed in item #2 below). Ernst and Assendelft discard the results mainly due to small sample sizes, incomplete tracking of outcome measures, failure to include blind or naive patients, and loss to follow-up. An accompanying study coauthored by one of the editorial authors (Assendelft) states that the majority of trials bore *positive* results and that "there certainly are indications that manipulation may be effective in some *subgroups* of patients." (Koes et al., 1996). A second and subtler problem is that too little is presently understood to be able to effectively discriminate between experimental and control groups in randomized trials involving chiropractic (Kokjohn et al., 1992; Balon et al., 1998).

Because it is impossible to mask or identify precisely what the chiropractor is actually doing, it is illogical for Ernst and Assendelft to fault the lack of patient blinding as a weakness implicit in the practice of chiropractic itself. Ernst calls for a solid evidence base as in orthodox medical practice. However, it has been estimated, for example, that only 15% of *medical* procedures have been found to be sufficiently supported by published literature and established medical guidelines are often based on little evidence and are subject to change (Smith, 1991). A good example is afforded by changes, which have occurred in recommendations for the treatment of otitis media in the past 6 years. During the previous few decades, the initial recommended intervention in the United States for the treatment of otitis media was to utilize tympanostomy with the optional use of antibiotics (Stool et al., 1994), despite the fact that, in the United Kingdom, Scandinavia, and the Netherlands, no deleterious conse-

quences ensue from pursuit of an expectant policy and reluctance to use antibiotics (Cantekin et al., 1991).

In the past decade, reports have cast doubt on the effectiveness of medical treatments for otitis media (Kleinman et al., 1994; Bright et al., 1993). Regarding tympanostomy, one study revealed that one quarter of the tubal insertions were judged to be inappropriate and another third equivocal (Kleinman et al., 1994). Another study pointed out that complications such as infection, healing problems, or pain occurred in 27% of tubal insertions, with 30% of the current tubes functioning as replacements for previous ones (Bright et al., 1993).

Treatment with antibiotics is also associated with risks and complications. The chance of incurring asthma, for example, has been shown to increase fourfold if antibiotics have been used in the first year of life, the risk having been shown to be dose-dependent (Wickens et al., 1999). Because the effectiveness of antibiotic treatment has been found to be scanty or equivocal in most recent studies (Cantekin et al., 1991; Del Mar et al., 1997; Fromm et al., 1997), the International Primary Care Network has recently concluded that "clinicians should immediately reconsider the routine use of antimicrobials for children with otitis media and consider treating symptoms with analgesics and observation for lack of improvement." (Fromm et al., 1997).

This recommendation has been endorsed in a commentary published in *Pediatrics* (Paradise, 1995) and in a study sponsored by the federal Agency for Health Research and Quality, which pointed out that antibiotic resistance in The Netherlands (where a waiting period is observed before antibiotics are given for otitis media) is 1%, whereas in the United States (where antibiotics are routinely given immediately), it is 25% (Agency for Healthcare Research and Quality, 2000).

Conventional medical guidelines can also rest upon insecure foundations of evidence. To suggest that chiropractic is based on an inferior system of guideline development may indicate a double standard. We believe that Ernst's arguments on the perceived paucity of research validating chiropractic may contain an implicit assumption that medical documentation and

medical guidelines are superior. This appears to be incorrect.

Attention must therefore be drawn to the strongly positive assessments of the evidence in support of spinal manipulation from multidisciplinary panels in two countries, the United States and United Kingdom (Bigos et al., 1994; Rosen, 1994), based upon rigorous criteria not cited in Ernst and Assendelft's editorial (Ernst and Assendelft, 1998). Secondly, in terms of patient satisfaction, patients who are under the care of chiropractors compared to conventional medical providers have routinely expressed two to three times the levels of satisfaction with their treatment. (Cherkin and MacCornack, 1989; Coulehan, 1985; Carey, 1995).

## 2. The relationship between chiropractic and manipulation

To support their assertions, Ernst and Assendelft use a tactic that runs contrary to the prevailing trend in systematic reviews, meta-analyses, and commentaries on spinal manipulation. Despite the substantial overlap of chiropractic, osteopathic, and other manipulative techniques and despite the broad diversity of manipulative techniques within each of the professions that utilize manipulation, Ernst and Assendelft have divided the literature on spinal manipulation into trials involving chiropractors and those involving everyone else.

The incontestable fact remains that, from the 25 randomized controlled trials reviewed in a meta-analysis by Shekelle, there is a statistically significant *positive* outcome for groups who underwent spinal manipulation in the resolution of acute low-back pain. Furthermore, in this study, no less than 94% of the manipulations were delivered by chiropractors (Shekelle, 1992). With more than 100 different chiropractic techniques having been described (Bergmann, 1993), the distinction between spinal manipulation and chiropractic adjustment is defined incompletely at present. Thus, to differentiate clinical trials involving spinal manipulation and chiropractic adjustment and to suggest that the latter is ineffective, is misleading. Further research is needed to differentiate each type of manipulation in terms of both efficacy and effectiveness.

## 3. Direct risk to the patient

Selective use of available data is apparent in Ernst and Assendelft's discussion on risks to the patient. In light of the following two critical points of view, the ostensibly discouraging risk-benefit ratio of spinal manipulation changes significantly.

(1) More precise estimates of serious complications from cervical manipulations have more recently been estimated to be 6 per 10 million manipulations, with fatal occurrences estimated at the rate of 3 per 10 million manipulations (Hurwitz et al., 1996). Rates of injuries and deaths following cervical manipulations are significantly less than the rates of injuries or deaths from the use of NSAIDs to manage similar conditions (Dabbs and Lauretti, 1995; Gabriel et al., 1991). If the use of medications as a whole is considered, the number of deaths directly attributed to medication has been shown to range in the United States from 79,000 (Johnson and Bootman, 1995) to 106,000 (Lazarou et al., 1998) annually, making it between the fourth and sixth leading cause of death (Lazarou et al., 1998). Chiropractic is clearly far below the risk of death from such activities as power boating, pregnancy, taking contraceptive pills, or automobile driving (Dinman, 1980).

(2) Risks of surgery to the spine have been ignored. The selective information presented in the editorial fails to account for the fact that death rates for lumbar-spine operations have been reported to be 300 times higher than the rate for CVAs following spinal manipulation (Boullet, 1990; Deyo et al., 1992); and for cervical surgeries, recent death rates have been estimated to be 700-fold greater than for CVA from spinal manipulation (Klougart et al., 1996a; Klougart et al., 1996b).

## 4. The use of radiographs

The "potential overuse of radiographs" alluded to in Ernst and Assendelft's editorial is based solely upon the *British Journal of Radiology* commentary by Ernst (1998), the problems of which have been addressed above. More robust and definitive information on the use of X-rays by chiropractors has been provided by a set of guidelines (Haldeman et al., 1993) in

place for the profession for more than 7 years and verified with appropriate red flags by multidisciplinary agencies from both the United States and British governments (Bigos et al., 1994; Rosen, 1994). Assendelft and Ernst have also overlooked the fact that the U.S. Medicare program until very recently *mandated* for nearly 30 years that chiropractors must order X-rays to receive reimbursement for their services. Allopathic doctors face no such requirement. After years of legislative efforts, the chiropractic community has finally succeeded in reversing this requirement.

### 5. Cost effectiveness

At odds with the conclusions drawn by Ernst and Assendelft is the bulk of studies from both worker's compensation data and insurance records which have shown chiropractic to be advantageous in the treatment of musculoskeletal disorders (Ebrall, 1992; Jarvis et al., 1991; Manga et al., 1993; Stano and Smith, 1996). The two studies that generated the editorial's assertions came to the opposite conclusion, suggesting that chiropractic services are more, rather than less, expensive than medical treatments for back conditions (Shekelle, 1995). However, these two papers have weaknesses that have been addressed in detail elsewhere (Rosner, 1995). Briefly:

- (1) The severity of illness is ignored.
- (2) The degree of recovery does not receive adequate attention.
- (3) Matching of services with provider type may be irregular.
- (4) Compliance has been disregarded.
- (5) Types of medications and their side-effects are not specified.
- (6) Medical expenses are capped by managed care while those of chiropractors are allowed to seek free-market levels.
- (7) Episodes are poorly defined or contained.
- (8) And, as is unfortunately true with many cost-effectiveness studies, indirect costs (such as time lost from work) have been ignored.

The cost advantages for chiropractic for matched conditions appear to be so significant that a leading Canadian economist has concluded that doubling the utilization of chiropractic services from 10% to 20% could realize

savings as much as \$770 million annually in direct costs and \$3.8 billion in indirect costs (Manga, 1998). Furthermore, no cost studies to date have factored in iatrogenic or legal burdens.

### III. Ernst E. Adverse effects of spinal manipulation. In: Jonas WB, Levin JS, eds. *Essentials of Complementary and Alternative Medicine*. Baltimore: Lippincott, Williams and Wilkins, 1999:176-179

This textbook chapter by Ernst (1999) repeats many of the points discussed above (Ernst, 1998; Ernst and Assendelft, 1998). Ernst asserts that chiropractors' "high velocity thrusts seem to be particularly burdened by complications." As primary support for this contention, Ernst cites "recent narrative and systematic reviews" that "located 295 case reports, including 165 vertebrobasilar accidents (25 of which were fatal). . . . Of the documented 295 adverse events, 135 had occurred at the hands of chiropractors."

As we have noted earlier, however, in reporting adverse consequences of any health intervention, context is essential. In presenting his case about the dangers of chiropractic, Ernst neglects two key facts:

- (1) These 295 adverse events were spread out over a period of 6 decades.
- (2) Because 94% of spinal manipulation is delivered by chiropractors (Shekelle, 1992), the fact that fewer than half of these adverse events were the result of chiropractic treatment would indicate that chiropractic adjustments, rather than being "particularly burdened by complications," are *safer* than spinal manipulations delivered by other practitioners.

In previous writings, Ernst failed to mention an important study (Dabbs and Lauretti, 1995) comparing the relative safety of manipulation to the use of NSAIDs. In this chapter, that article is addressed—but Ernst faults the study by claiming that its analysis is flawed, in large part because it compares a single spinal manipulative treatment to prolonged drug therapy. However, Dabbs and Lauretti did, in fact, compare a course of spinal manipulative treatments to a course of NSAID therapy for similar arthritic conditions.

## COMMENTS AND CONCLUSION

In a recent editorial, Vickers discussed Ernst's contribution to the literature and pointed out that the quality and quantity of research are what matter most (Vickers, 1999). It appears that, on the subject of chiropractic, the quality of Ernst's output must be questioned. The misuse of the scientific literature is a serious matter and raises fundamental questions about the peer review process itself. We believe it should be regarded as equivalent to serious professional misconduct. If the reviewers assigned by a scholarly journal to evaluate a paper are not intimately familiar with the subjects under discussion there will be times when even the most reputable journals will publish articles that fall far short of the high standards to which they aspire.

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Address reprint requests to:  
*Joseph Morley, D.C., Ph.D.*  
 43 Elmside  
 Guildford GU2 7SJ  
 United Kingdom

*E-mail: joemorley2@thefreeinternet.co.uk*

*Anthony L. Rosner, Ph.D.*  
 Director of Research and Education  
 Foundation for Chiropractic Education  
 and Research  
 1330 Beacon Street, Suite 315  
 Brookline, MA 02446-3202

*E-mail: rosnerfcer@aol.com*

*Daniel Redwood, D.C.*  
 1645 Laskin Road, Suite 103  
 Virginia Beach, VA 23451

*E-mail: danredwood@aol.com*