Dynamic Chiropractic

SOFT TISSUE / TRIGGER POINTS

Rebuttal on Thermography

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Dr. Croft's article "Thermography in Soft Tissue Trauma" in the 5-7-93 issue questioned the role of thermography, sparking many letters to the editor. Dr. David BenEliyahu, who writes a column for "DC" on thermography, rebutted Dr. Croft's article in the 7-2-93 issue.

Because thermography's usefulness is hotly debated, Dr. Croft asked "DC" to print his response to Dr. BenEliyahu's comments, noting that, "Such discourse is the foundation of intellectual and scientific growth."

In his rebuttal, Dr. BenEliyahu complains that my "review" of thermography was one-sided and limited to merely 21 references. I must point out to Dr. BenEliyahu that I specifically stated in that article that it was not meant as a review at all. My purpose was, in fact, merely to ask a question concerning the role of thermography in soft tissue trauma. I ask these questions not out of any ulterior motive but because I believe they are valid.

In a letter to the editor, one doctor suggested that I needed to learn about the subluxation syndrome, and in another, a doctor obtusely accused me of being "medically oriented," a phrase chiropractors use to insult a colleague who doesn't happen to subscribe to their particular ideology. And while some may misinterpret my comment on thermography as tantamount to chiropractic apostasy, I certainly hope I will be labeled as one "scientifically oriented."

Unfortunately, notwithstanding the chorus of praise heaped upon thermography by its ardent devotees, it seems quite clear that a good deal of the more rigorously conducted trials have not been terribly favorable to thermography. And the fact that some of the articles supporting thermography have been published in peer reviewed journals is no guarantee, contrary to Dr. BenEliyahu's suggestion that they are without flaws. In fact a large percentage of articles in such journals are seriously flawed.[1]

Dr. BenEliyahu rejects the Hoffman et al.,[2] meta-analysis rather out of hand, which I think is a bit too convenient, particularly since he feels my comments were one-sided. Meta-analyses do have their practical limitations but they are relied upon increasingly more these days and are considered valid.[3-5] Hoffman et al., found most of the thermography articles published in indexed literature to be seriously flawed.

As to the remark that thermography is used without bias in Europe and Asia, I can only suggest that Dr. BenEliyahu may have limited access to world literature. I did not mention Dr. BenEliyahu's research because I was not interested in getting into a personal battle with him. I do, however, appreciate any and all efforts in research, and I commend him for this. He should not consider that I do not read his work, and since he broached the subject, I will point out some of the weaknesses of the work he referenced.[6-9]

Reference 6: In this case report Dr. BenEliyahu provides an example of how infrared thermography (IRT) proved invaluable for treating a man with disc herniations and chronic back and leg pain; a man previously seen by an orthopedic surgeon who recommended surgery, a neurologist who prescribed medication, and a chiropractor who recommended chiropractic treatment. In addition to clinical signs of nerve root tension, trigger points were noted. An IRT examination revealed evidence of SI joint dysfunction and trigger points. This, according to the author, allowed for appropriate care and precluded the need for surgery. But there are several questions one might ask of the author. For one, how did his treatment differ from that recommended by the other chiropractor? Perhaps that doctor would have had similar success without the need for IRT. Secondly, he gives no information about his follow-up of this patient. I believe he had arrived at the diagnosis of myofascitis after his clinical exam. Did Dr. BenEliyahu also examine this man's SI joint? If so, how did the results correlate with IRT findings? Did the IRT cinch the diagnosis or was it merely an expensive shortcut?

Reference 7: In this paper, the authors propose to measure "the most common levels of disc protrusion seen in clinical practice," and to associate them with IRT findings -- a lofty and ambitious goal for a sample of only 66 cases, some of which had only disc bulge. There are many problems with this paper. Although this takes the form of a clinical trial, there was no control group. The authors consider both herniation and bulge to be encompassed by the term "protrusion" and only for one of four groups of patients do they mention the relative proportions of each. Also, the method of selection is not described. Disc bulges/herniations were neither quantified nor correlated with symptoms, leaving the reader to wonder about the validity of the conclusions about IRT. Correlation between symptoms, CT/MRI and IRT results is not provided. And since the majority of thermographic patterns covered three root levels, one is left to wonder how meaningful the results are. Perhaps the authors should have attempted to predict patients' complaints or CT/MRI findings based on IRT. That would seem a better measure of its real worth. No statistical method was reported; therefore we are left to wonder about the significance of the results. The authors cite Jinkins et al.[10] as providing support for their theories about IRT, yet these authors have, if anything, provided evidence of a condemning nature by showing how zones of head overlap necessarily below L2 in cases of autonomic referred pain. Dr. BenEliyahu did not mention that Jinkins et al., never discussed IRT. In fact, they considered mechanisms of pain, not thermoregulation, which involved not only the autonomic system but central mechanisms as well -- mechanisms which probably would not affect the autonomic nervous system. In fact, their theories are based loosely on the work of Kellgren[11] which also does not provide any direct support for thermography.

Finally, Dr. BenEliyahu misquotes Uematsu et al.,[12] as stating that IRT showed 94.7 percent positive predictive value and a specificity of 87.5. However, it is well to point out that the calculation of positive predictive value is made by dividing the number of true positives (89) by this number added to the number of false positives (5). This calculation is highly influenced by the incidence of disease in the population which, in this study is artificially quite large (72 percent) allowing for the equivalent of a statistical hat trick. Intuitive readers will see that when the real incidence of a condition in a population is low (as is disc herniation) the number of false positives will be higher and the positive predictive value will be correspondingly lower.

Reference 8: In this study, Dr. BenEliyahu evaluated 30 patients with patellofemoral disease (PFD) and

40 controls using IRT and reported a sensitivity of 97 percent and a specificity of 90 percent. These figures should be interpreted with caution. The study design was so narrow as to nearly guarantee this type of result. It is quite likely and predictable that 40 normal volunteers would have mostly 36/40 normal IRT exams. That provides us with the 90 percent specificity figure. In reality, however, there are many conditions of the knee, many of which are difficult to diagnose clinically, which would likely present as hot or cold IRT scans as Dr. BenEliyahu found with PFD. If a mixed group of say 100 patients with various traumatic, rheumatic or orthopedic disorders were included in the study, I would imagine that many would provide for abnormal IRT studies. It is doubtful that a skilled thermographer could differentiate between these disorders based solely upon IRT results. In this setting, thermography would probably be sensitive but not very specific for PTF. The fact that abnormal scans were seen as both hypothermic (86 percent) and hyperthermic (14 percent) leads one to wonder whether surface heat patterns are constantly changing. Do humans with injuries or disorders of the extremities vary with regard to skin temperature from day to day as Bennet and Ochoa[13] observed in rats? It seems likely. At least there is no convincing evidence to simply dismiss this possibility out of hand as Dr. BenEliyahu has done. And yes, these authors did suggest that thermography would be a useful tool to understand neuropathic pain. However, their statement was in the context of its usefulness in the experimental animal model, not clinical thermography for humans. Finally, Dr. BenEliyahu concludes that IRT is useful in the diagnosis and management of PFD, yet his results don't support that. His gold standard was a clinical exam and radiographs. His study doesn't provide any evidence that IRT will improve his diagnostic yield nor does he explain its role in management.

Reference 9: This study appears to be seriously flawed in two respects. First, the authors propose to study the overall sensitivity of IRT to detect neurological involvement using as their "gold standard," MRI, an anatomical test which cannot provide any information about neurophysiology. Thus, the authors cannot expect to achieve their stated goal. The EMG, NCV and SSEP would have been more appropriate "gold standards." Secondly, in their group of 62 patients, only 38 had abnormal findings on MRI, and only these were subjected to IRT; all patients should have been subjected to IRT. Since 24 were excluded, the calculations of predictive value, sensitivity, and specificity are invalid.

Regarding comments about SSEP, there is generally much less controversy with this test today than there was five to seven years ago owing to continued improvements in hardware and methods of interpretation -- a trend which thermography does not share. I would be quite happy to share the recent work in SSEP with Dr. BenEliyahu.

The Weisel et al.,[14] report on the incidence of positive CT scan findings in asymptomatic persons is frequently misquoted. Dr. BenEliyahu quotes the study as finding 39-45 percent false positive findings on CT/MRI. (The actual figures were 35.4-50 percent). More to the point, the authors did not evaluate MRI at all. Secondly, it should be understood that this work was done in 1982 and 1983 at a time when CT was quite new. With the advent of MRI, improvements in CT hardware and software, and a decade of clinical experience with these technologies, this false positive rate has been reduced significantly, a trend which is not enjoyed by thermography. In a recent edition of the same journal, for example, Parkkola et al.,[15] finds an incidence of disc bulging in only 11 of 180 disc spaces (6 percent) in healthy volunteers evaluated by MRI.

A widely accepted belief that myofascial trigger points can be seen with thermography is based on the common finding of trigger points on patients (most people have them) and hot spots on the thermogram. A recent small study (n=11) supports this.[16] Yet, in a very carefully executed larger trial, (n=365), Swerdlow and Dieter[17] came to the conclusion that trigger points and such hot spots were not associated. So the debate rages on.

Dr. BenEliyahu illustrates one of the great difficulties in understanding and interpreting the divergent pool of literature which seems to either support or refute thermography as a clinical tool. He cites a study by Uematsu et al.,12 in which the authors compared CT myelography with thermography and reported 94.7 percent sensitivity and 87.5 percent specificity. This is a study frequently quoted by advocates of thermography because most other such trials have failed to yield such impressive results.

Mahoney et al.[18] reported a sensitivity as low as 35-48 percent and a specificity of only 20-44 percent.

Harper et al.,[19] reported a sensitivity ranging from 78-94 percent and a specificity of only 20-44 percent. Since, in the Uematsu et al., study, the authors used primarily failed-back patients who had two or more surgeries, one should be careful in extrapolating these results to patients with less severe back problems. They also pointed out that thermography was not root-specific.

Dr. BenEliyahu did not mention another study of Uematsu et al.,[20] in which the authors found that 46 percent of patients with chronic low back pain have normal thermograms. Although an older report, it raises serious questions about the utility of thermography in instances of chronic low back pain not related to disc herniation.

Finally, Dr. BenEliyahu asks why a doctor would expose a patient with RSDS to the ionizing radiation of repeat radiographic examinations. The answer is quite simple: because only this type of serial examination can document and quantify the progressive osteoporosis seen with this condition, and because it is the standard of care in any community for chiropractors and medical doctors alike. Patients in all stages of RSDS are in significant, often excruciating pain. The disease is characterized by pain, edema, decreased motor function, limited ROM, atrophy, spasm, skin changes, nail changes, osteoporosis, joint pain and swelling. It is still difficult for me to understand why, in most cases, the thermogram is necessary to make this rather obvious diagnosis.

Finally, and for the record, I have not argued that thermography, as a procedure, is invalid. Nor have I suggested that many lesions will not be seen as abnormal thermograms. I encourage thermographers, such as Dr. BenEliyahu, to continue they're research, and I welcome their comments regarding the questions that I did raise.

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Editor's Note:

For more on personal injury, consult Dr. Croft's video, "Advances in Personal Injury Practice," #V-435, on the Preferred Reading and Viewing List, pages xx.