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Pseudoscientific Standards and Scientific Double Talk vs. Established Medical Model and Clinical Reality

The "expert panel" at the recent NIH/NIDR Technology Assessment Conference (May 1996) on Management of Temporomandibular Disorders concluded that there is no data to support *many* commonly held beliefs in TMD, nor is there data to support the superiority of any method of management as being *better than a placebo*. It decried the lack of *validated* diagnostic methods with respect to sensitivity and specificity, for identification of TMD and noted the obvious need for *basic* research (in contrast to clinical research) relative to TMD. It stated its belief that occlusal adjustments are invasive and that superiority of such treatment over other non-invasive therapies has not been demonstrated in *randomized controlled* prospective trials.

Dr. Barry Cooper presented results of a clinical study utilizing objective electrokinetic and electromyographic measurement on over 1,180 patients before and after treatment, demonstrating not only marked improvement in symptoms using occlusal therapy, but *improvement in status of physiological parameters* after treatment. Dr. Penti Kirveskari presented results of several controlled studies whose university approved protocols demonstrated a strong *association* between occlusal interferences and TMD. Dr. Glenn Clark then presented results of a *subjective* and *interpretive* literature search not based on original research, yet concluded that the studies of Drs. Cooper and Kirveskari were not scientific enough to merit his approval.

Dr. Gerald Murphy's presentation elaborated upon some of the numerous physical therapy modalities which he utilized to help his patients get better. Dr. Jocelyn Feine then presented the results of another subjective literature search from which she concluded that efficacy of such devices has not been scientifically documented.

In his presentation, Dr. Joseph Marbach contended that dentistry must regard the TMJ as a *joint like any other joint* and that all clinical study should be evidence-based. Clearly the TMJ has similarities to other joints but I cannot think of any other joint in which the functional range is dictated by the position of teeth. Moreover, Dr. Marbach's conception of "evidence" does not coincide with mine. "Evidence" lacking objective measurement defines inference for me.

Dr. Linda LeResche presented a paper in which she unequivocally asserted that pain should be regarded as the gold standard for TMD study. Such a contention would seem to be ludicrous since it contradicts the mega criteria of refutability and testability, which are sine qua non for scientific study. Pain, of course, is both irrefutable and untestable.

Clinical judgement is experience understood. I utilize non-invasive objective electrokinetic, electromyographic and electrosonographic measurements to

document the biological status of my patients before and after treatment. I understand what I did or did not do for and to my patients based on universally accepted physiologic standards. To my knowledge, there is no argument in the literature that healthy oral musculature of both right and left sides should function in balance in centric occlusion; that healthy musculature and joints function smoothly and not dyskinetically; that muscles should be relaxed in rest position; and that healthy joints should function silently. Repeatedly, however, researchers at this meeting said such methodology was unscientific.

The basic science researchers in TMD have structured a framework which conflicts with my model of reality. I feel my eighteen years of experience in diagnosis and treatment of TMD's and that of many colleagues is adequate to have reasonable expectations about clinical results. Why then do we continue to reach conflicting conclusions?

Why does a dichotomy exist between clinicians and researchers? I understand that in different cultures, often the same words mean very different things. Perhaps this is the case here. Maybe we need to agree on basic definitions before we speak to each other. On the other hand, maybe this is not just a language problem but also a perversion of clinical science methodology by employment of social science measurement techniques.

For years now, Dworkin, McNeill, Clark, LeResche and Von Korff, all of whom spoke at this NIDR/NIH meeting, have advocated the *biopsychosocial* model of TMD as the operative paradigm.

Dworkin, in a 1993 NIDR Conference, defined *disease* as an "objective biologic event involving disruption of specific body structures or organ systems caused by pathologic anatomic or physiologic changes" and he defined *illness* as "a subjective experience or self-attribution that a disease is present, yielding physical discomfort, emotional stress, behavioral limitations and psychosocial disruption." He claims "progressive pathophysiologic changes cannot be reliably diagnosed" in TMD's and concludes that, *"TMD is more usefully characterized as an illness."* His "scientific" diagnostic criteria thus becomes "You've got it if you say you've got it." *Pain is the "gold standard.*

Within the psychosocial model TMD is a *self-limiting* condition, with *no evidence of progressive deterioration of physical structures or physiologic functions*,[7] contrary to the experience of all clinicians. This is an example of psychosocial scientists using *extrinsic* evidence to do epidemiologic studies for measurement of TMD phenomena. Thus, can they *measure* TMD prognosis, treatment and disability by studying self reports of pain, anxiety, hostility and depression and *not doing physical examinations or physiologic treatments!* This is my *worst* nightmare of "HMO TMD practice" and I see it as their reality of good science.

As a fee for service clinician, I am very uncomfortable by the implications of this

model. My dictionary defines disease as a response to injury, sickness or illness; a *failure of the adaptive mechanism* of an organism to counteract adequately the stimuli or stresses to which it is subjected, resulting in a *disturbance in function or structure of any part*.

Finding the stimuli causing the disease as well as the stressors causing the *inappropriate clinical response*, in my opinion, requires a thorough history and clinical examination. Diagnosis involves analysis of the scientific evidence of what is wrong with the patient and why, and applying a tentative name to the disease. As a clinician, I direct treatment of disease toward ideal physiological parameters based on objective measurement.

Clinicians dealing with TMD's as diseases of maladaption do not *cure* the patient as in infectious diseases. The medical model utilized is that of Hans Selye. Based on the scientific data and evidence gathered in our thorough clinical work-ups and subsequent working diagnoses, clinicians treat such diseases of maladaptation by strengthening the body's own defense mechanisms and helping to decrease physiological and psychological stress to create an environment in which the body can get rid of the pain, heal itself or get better. *Illness* merely refers to symptomatology and *does not define TMD* for me.

It was clear that the NIDR/NIH panel felt that TMD referred to a collection of medical and dental conditions affecting the temporomandibular joint and/or the muscles of mastication.[1] While acknowledging the role of the muscles of mastication however, they ignored the role of *teeth*, *nerves, vasculature, ligaments* and *discs* in contributing to TMD's. In fact the panel purposefully omitted *occlusion* as a factor, and rationalized this exclusion by pointing out the lack of double blind controlled studies to substantiate any role of occlusion in temporomandibular disorders.

Paradigms are *never* proven right. They are only proven wrong. Einstein once said of his theory of relativity that it made him feel good that it was validated in hundred of studies, but if only one proved it wrong, it was wrong. The clinical experience of an overwhelming number of dentists indicate a relationship between malocclusion and certain TMD's. Dr. Kirveskari and Dr. Cooper corroborated, with excellent scientific data, a *positive correlation* between malocclusion and TMD. The absence of a double blind controlled study does not prove either clinical experience or these fine papers to be wrong. No scientific evidence was presented at this meeting or has *ever* been presented which demonstrates that occlusal therapy is not a valid treatment for certain temporomandibular disorders. Assault by credentialed experts does not constitute proof.

I have addressed the inappropriateness of epidemiologic study as a methodology for TMD in detail in a series of papers previously published in *Cranio*[,,] and elsewhere.[,,] This issue has also been raised by Sackett and Campbell when they

question whether stress disorders are appropriate for epidemiological study. Because of the insidious onset and high percentage of signs and symptoms of TMD in the general population it is very difficult to determine who has the disease, and when and who does not. Since there are not two clearly defined states "*diseased*" and "*disease-free*," classical epidemiological study is inappropriate. Thus the issues of sensitivity and specificity become perversions and likewise, decision matrix analysis becomes an illogical concern. We are being forced to deal with *academic double-talk* instead of clinical reality.