VOODOO Biomechanics

Evidence-Informed Medical Practice

"There are times, where I'm sure if you put me in front of a panel of my colleagues and said right Helen, justify why you did that, I'd really struggle with that and partly I think it comes down to that kind of gut feeling where you go, okay there's nothing specific but maybe it's on the edge of specific; maybe it's on the edge of where I would be comfortable treating and erring on potentially the side of caution."

Helen Banwell, Podiatrist: PodChatLive, Ep. 52, Nov 2018.
On using orthotic devices in children with "flat feet".

". . .I'm looking at these feet and saying to myself-- and I can't justify why-- I'm saying to myself, if this was one of my boys, I'm giving them a device. . . If this is the way I would treat my own child, is it not the way I should treat someone else's child?"

Ian Griffiths, Podiatrist: PodChatLive, Ep. 52, Nov 2018
On treadmill gait analysis of a child with asymptomatic pronated feet.

The above quotations from podiatrists well-versed in the published research of their specialties-- Podopaediatrics and Sports Podiatry-- highlight the difficulty establishing a therapeutic intervention when an out-of-date understanding of evidence-based medicine (EBM)-- what many now refer to as evidence-informed medical practice (EIMP)— may be in play. Many clinicians, including those engaged in research, believe treatments should be based solely on the published research, which is a misunderstanding of the modern definition of EBM or EIMP.

Before the 1960s, the clinical practice of medicine was considered an art. The phrase "the art of medicine" was often used both in the medical profession and more widely among the general population. The use of the scientific method, as in biomedical research and statistical analysis in epidemiology, was rare to non-existent in the world of medicine, with expert opinion, experience, and authoritarian judgment forming the foundation for therapeutic decision making. Although historical precedence, along with indoctrinated political mistrust of research and statistical analysis posed barriers to incorporating the scientific method and statistical analytic tools

into medicine, several events in different parts of the world during the 1960s paved the way for the introduction of EBM.

Trained at Harvard Medical School, Stanford Medical Center, and Johns Hopkins University, Suzanne and Robert Fletcher were early 1960s pioneers in the EBM movement. They recognized a deficit in medicine, i.e., biomedical science often had no translational application to clinical medicine. In 1969, the Fletchers enrolled in the Clinical Scholars Program funded by the Carnegie Foundation (later named Robert Wood Johnson Clinical Scholars Program) and received training in public health and clinical care. Graduates of the program were challenged with the task of straddling the political extremes of public health and medicine. Fortunately, they found opportunity at McGill University, where they taught epidemiology at the medical school. In 1982 they published *Clinical Epidemiology: The Essentials*, a textbook that described the scientific basis for clinical care,

In a series of articles published in the Canadian Medical Association Journal in 1981, a group of clinical epidemiologists led by Dr. David Sackett, first defined EBM as using the best research evidence to resolve clinical problems to the exclusion of the training and experience of the clinician[1]. Although this initial definition of EBM has evolved to include not only the clinician's experience and expertise, but also the patient's values and considerations, Sackett's 1981 definition of EBM is often espoused by clinicians who have not been exposed to the current definition of EIMP, and who fail to appreciate that the absence of evidence is not evidence of absence. A little more on this in due course, after we have looked at how, through years of work by other researchers and authors, Sackett's definition of EBM has been superseded.

In the abstract of their 2017 paper Critical Appraisal of Clinical Research[3], Al-Jundi and Sakka define the process of evidence-based practice as:

". . .the integration of individual clinical expertise with the best available external clinical evidence from systematic research, and patient's values and expectations into the decision-making process for patient care. It is a fundamental skill to be able to identify and appraise the best available evidence in order to integrate it with your own clinical experience and patients' values"[3].

Before we race to adopt this enhanced definition of EBP, we should consider what may seem pedantic by some, or important by others: Al-Jundi seems to confuse, or perhaps simply conflates "clinical expertise" with "clinical experience", which in the most current definition of EIMP are not considered to be the same thing. To explain terminological inexactitude, it is necessary to juxtapose Al-Jundi's definitions with that of Haynes et al, which appeared in the article Physicians' and patients' choice in evidence-based practice published in the British Medical Journal in 2002.

Hayne's offers a further updated model for EBP. As seen in figure 1, Haynes defines *Clinical expertise* as what the clinician brings to the medical decision-making process, which is the combination of clinical experience (annotated in fig. 1 as Clinical state), research evidence, and an appreciation of the patients' preferences and actions.

Later versions of the EBM decision making process state that research evidence alone is not an adequate guide to action. Rather, clinicians should apply their training and experience to assess the patients' state and circumstances, incorporate the best research evidence available, and consider the patient's preferences, values, and actions before recommending a therapeutic intervention[2] (Fig. 1)

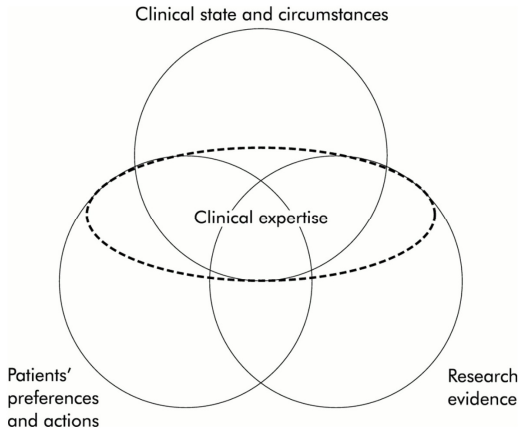

Fig. 1

Taken from Haynes, et.al., Clinical expertise in the era of evidence-based medicine and patient choice, BMJ Evidence-Based Medicine, March 2002

Let us ignore—as perhaps many clinicians do-- the patient's preferences in the decision-making process, EIMP should be considered a synthesis of clinical experience and the conscientious, explicit, and judicious use of current best research evidence in making decisions about the care of individual patients. What I see with some of my post-graduate podiatrist and physiotherapist colleagues, even some of those with PhDs, is the misinterpretation of the modern concept of EIMP. Many seem to confuse clinical experience with clinical expertise. Many appear to believe that an individual clinician's training and experience is null and void in the decision-making process when it comes to, let say, a particular therapeutic musculoskeletal intervention; that if a particular intervention is not supported by at least one RCT, with adequate power, that demonstrates a positive signal with a *p*-value of 0.05 (or less) the intervention should be dismissed out-of-hand.

AS previously mentioned, what some clinical "researchers" within the Professions Supplementary to Medicine seemingly fail to understand is that no evidence of effect is not the same as evidence of no effect. Many confuse the state of having no studies showing positive effect as the same as having studies showing a null effect. Some suggest that certain clinical interventions should not be employed if there is no published research to show effectiveness. To the contrary, under the caveat that clinicians must remain open to new knowledge, as proposed by the most current definition of EIMP, clinicians have every right to base therapeutic interventions on their individual experience of positive clinical outcomes in the absence of research demonstrating a positive effect; until the evidence of no effect emerges. I agree with podiatrist Craig Payne who puts it thus:

*"Obviously, I'm a huge fan of evidence, but the bulk of what we do is not and will never be evidence-based in that there's never going to be enough randomized control trials on everything we do; to support everything we do, so then we have to start using things like theoretical coherence, biological plausibility, and most importantly with the available evidence. . ."*PodChatLive Ep. 1, Dec 2017.

Clearly, from this statement, Craig places a great deal of confidence in RCTs. However, even a brief investigation into the "gold standard" of clinical research (superseded by systematic reviews and meta-analyses in the opinion of many researchers) reveals several shortcomings in the RCT protocol that should produce caution in interpreting the published research, along with more than a modicum of humility in the author(s), rather than the arrogance not uncommonly displayed. Let's look at some the well-aired concerns about RCTs:

Firstly, classical statistics-- used to analyse research data-- does not provide a language of causality. In *The Book of Why*, Joshua Pearl describes the early history of how research scientists – in particular Galton and Pearson-- failed to provide a theory of causation during the evolution of statistical analysis. Pearson's position was that because causation could not be determined by statistical analysis of the data, the concept of causation was scientifically invalid! Students of statistics chant ad nauseum "correlation is not causation". In fact, if pressed, their professors would be unable to give them a definition of, or a formula for, causation. Students are told they cannot say *X* is the cause of *Y,* only that *X* and *Y* are associated. Why? Because mathematical tools to answer causal questions were never developed as part of classical statistics. Statistics focuses on how to summarize data, not on how to interpret it. "Data is dumb", says Joshua Pearl without reservation; it can tell you that a group of patients who received an intervention healed faster that those who did not receive it, but it cannot tell you why.

It is beyond the remit of this article to discuss the mathematics of causal relationships (Bayesian Inference) that began in 1912 when geneticist Sewell Wright at Harvard University studied the markings on the backs of Guinea pigs' coats and used *path diagrammes* transposed into algebraic equations to demonstrate that 42% of a given coat pattern was caused by heredity, while 58% was due to developmental factors. Suffice it to say that there is now a statistical language that merges apriori knowledge, i.e., what we know (or think we know) with questions to which we wish to know the answer, such that causal relationships may be established with mathematical confidence.

In his provocatively titled paper *Why Most Published Research Findings are False*, published in <u>*PLOS Medicine*</u> in August 2005 (listen to Podcast 2), John P. A. Ioannidis-- a Greek American physician, scientist, writer, and Stanford University professor who studies scientific research itself-- a process known as meta-research, explains in detail the reasons why most current published research findings in most scientific fields are almost certainly false. Ioannidis explains that the probability that a research claim is true depends upon study power, bias, the number of other studies on the same question, and most importantly the ratio of true to no relationships among the relationships probed in each scientific field. In his articles he states:

"In this framework, a research finding is less likely to be true when the studies conducted in a field are smaller; when effect sizes are smaller; when there is a greater number and lesser preselection of tested

relationships; where there is greater flexibility in designs, definitions, outcomes, and analytical modes; when there is greater financial and other interests and prejudice, and when more teams are involved in a scientific field in chase of statistical significance". He continues: *"Simulations show that for most study designs and settings, it is more likely for a research claim to be false than true. Moreover, for many current scientific fields, claimed research findings may often be simply accurate measures of the prevailing bias"*.

Ioannidis decries what he calls the "ill-founded strategy" of claiming conclusive research findings solely based on a single study that claims statistical significance-- typically, with a *p*-value less than 0.05. Research, he believes, is not most appropriately represented and summarized by *p* values, but unfortunately there is a widespread notion that medical research articles should be interpreted based solely on this one metric.

In *Blueprint: How DNA Makes Us What We Are* [October 2018], psychologist, researcher and behavioral geneticist Robert Plomin, explains the troubling issue of what he calls "chasing *p*-values" as follows:

"Reaching a p-value of 5% means that if you did the same study 100 times you would find a similar result 95 times. A p-value of 5% does not mean that a finding is true, it means that 5 times out of 100 times you would not find the same result." He continues: "The situation edges closer to cheating when scientists chase *p* values, e.g., they might look at their data in different ways, such as using different types of analyses, and choose to write about the results that reached the *p*-value of 5%. But chasing *p* values in this way chases the validity of statistical tests right out the window." He concludes: "Very often statistically significant findings are not significant in any real-world sense because their effect size is negligible. Statistical significance depends on sample size and effect size. A tiny effect size will be statistically significant if the sample size is large enough, so when you hear about a scientific finding, always ask about the size of the effect. It's not enough to know that the finding is statistically significant."

Replication is also a serious issue in research as the positive results claimed in most research papers have never been replicated. The whole purpose of the scientific method is to allow other groups of researchers to reproduce the study and confirm the findings of the first group. It should be appreciated that good-quality RCTs are expensive, labour-intensive, and usually take a long time to reach a conclusion. Along with time delays in

publishing the findings, and the time it takes for the establishments to modify clinical guidelines that eventually filter down to clinicians on the front line of patient management, a study may be outdated by the introduction of new technologies, social or paradigm shifts, or the publication of other research before it makes the pages of a peer-reviewed journal. Despite these issues, experts in other medical fields are calling a crisis in science and considering the reasons why the results of many studies, including classic studies that form the backbone of textbooks in the fields, do not replicate. The journal Science reported that more than half of 100 studies in top journals fail to replicate. Outright fraud is considered uncommon, but the competition for publishing novel results in the best journals increases the risk for "massaged" results, i.e., when scientists select results that tell the best story and sweep inconsistencies under the carpet.

As the physicist Richard Feynman said: "*The first principle is that you must not fool yourself and you're the easiest person to fool"*.

References:

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3. Al-Jundi A, Salah Sakka S: Critical Appraisal of Clinical Research. J. Clin Diagn. Res. 2017 May; 11(5): JE01–JE05.