Essay Review

Lost in translation

**Up from Clinical Epidemiology and EBM.**

**Epidemiological Research: Terms and Concepts.**

In *The Book of Heroic Failures*, Stephen Pile tells the brilliant but tragic story of Pedro Carolino who produced a Portuguese–English phrasebook, published in 1883. He did not speak any English, and did not have an existing Portuguese–English dictionary, but had a Portuguese–French one and a French–English one. The resulting book made a major contribution to the development of the English language. Readers were educated to use poetic phrases such as ‘dress your hairs’ and ‘exculpate me by your brother’s’. Tired old proverbs such as ‘a rolling stone gathers no moss’ were transformed into the much more technically correct phrase of ‘the stone that roll not heap up not foam’; sterile phrases such as ‘to kill a mockingbird’ were transformed into the poetry of ‘to craunch a marmoset’.

But enough of this. This review is intended to hail a much more striking and successful translation: the International Epidemiological Association (IEA) Dictionary has been translated into Miettineses! Miettinen himself has done the work, once again apparently using several intermediate languages. He has also helpfully provided an asterisk alongside terms that appear in both dictionaries, so that we can compare the two. Who knew that the tired old definition of a cross-sectional study in the IEA Dictionary:

> A study that examines the relationship between diseases (or other health related characteristics) and other variables of interest as they exist in a defined population at one particular time. (p. 57)

could be transformed into the much more informative:

> Concerning the occurrence relation defining the object of an etiogenetic study, the quality that the temporal referent of the (potentially) causal determinant (of the rate of the outcome’s occurrence) is not the actual range of etiogenetically relevant time (retrospective, with T₀ the time of outcome), nor even a segment of this; that it is, instead, reduced (by object design) to a mere point in time and, specifically, to the point coincident with the outcome (its occurrence/non-occurrence). (p. 87)

or that the definition of a *P*-value:

> The probability that a test statistic would be as extreme as observed or more extreme if the null hypothesis were true. (p. 199)

could be transformed into:

> A statistic so derived (from a sample) that its (sampling) distribution conditional on the parameter value being tested is uniform on the 0-to-1 range, so that Pr(P < α) = α for any α in this range; and so derived that, in addition, the distribution on the ‘alternative’ hypothesis is shifted to the left in this same range, so that Pr(P < α) > α. (p. 61)
But I should begin at the beginning, with Miettinen’s book on clinical epidemiology and evidence-based medicine (EBM), which has been published together with Miettinen’s dictionary. Miettinen’s book represents a sustained polemic against clinical epidemiology and EBM, and his Canadian colleagues who have developed it. We learn (pp. 137–38) that the seminal event in the genesis of ‘clinical epidemiology’ and EBM was when ‘it dawned on [David Sackett] that epidemiology could be made as relevant to clinical medicine as his research into the tubular transport of amino acids’ and that this implied that ‘Sackett and his clinician colleagues themselves could effect this envisioned development – despite their having no record at all in the advancement of the theory of epidemiology, or biostatistics... it actually was a vision that amounted to replacing clinical professionalism by conceited dilletantism in the practice of pseudo-scientific medicine’. Leaving aside the obvious personal venom in these remarks, and the mystery of why they would be published in an academic text, it would be churlish of me to point out to Miettinen that, even if the above statement is true, it would not be the first time that someone had made a major contribution at one stage of their career, and a lesser contribution at others (who can forget the year when the Bee Gees went disco?).

I started doing epidemiology in 1981, and did the Amherst Summer Course (later the Tufts summer course), organized by Epidemiology Resources Inc. (ERI) in 1982. At that time, papers such as Miettinen’s seminal 1976 paper on case–control studies were still fresh, and were a real eye-opener and an epistemological and theoretical advance, compared with the ‘traditional’ approach involving the rare disease assumption. Miettinen clearly made many important contributions to epidemiology at that time, although many of them were redevelopments or reinventions of earlier ideas, once again without reference to the original authors. In my opinion, this all started to go downhill with the publication of Theoretical Epidemiology in 1985, which contained many new and interesting insights, but also many old concepts reinvented and made more complicated under new names, and with little reference to the original literature.

So what are we to make of this new pair of books? And the (to my mind) somewhat generous reviews that have accompanied these, and other recent publications of Miettinen? Is no one prepared to ask whether the Emperor is wearing any clothes? When you strip away all of the convoluted language, old wine in new bottles and references to Kant, Kahlil Gibrah’s The Prophet and Robert Pirsig’s Zen and the Art of Motorcycle Maintenance, the basic argument of Miettinen’s polemic against clinical epidemiology and EBM is as follows.

Miettinen argues that Sackett et al. have made a fundamental error in believing that evidence from clinical trials and clinical epidemiology studies can be applied to clinical decision-making, i.e. they are wrong to believe that studies of groups can help to inform decisions about individuals, or at least not in the way that EBM attempts to do. ‘The founding doctrine of the EBM cult is this: clinicians at large can and should acquire competence in the assessment of research evidence on topics in their respective disciplines... having gained the requisite competence they can and should do this assessment quite comprehensively and continually and... they should practice according to their own opinions on these topics.’ (p. 17) This approach is clearly not without its problems, since it requires clinicians to have the time and intellectual capacity to keep abreast of the literature and to make decisions accordingly. However, these problems pale into insignificance when compared with Miettinen’s alternative approach, which is ‘a practitioner of clinical medicine holds a professional position of public trust. (S)he therefore is obliged to measure up to what is expected ... practice in deference to the leaders – top experts – of the discipline ... Practice by the presumption of intrinsic superiority of one’s personal opinions over those of experts on matters scientific ... is antithetical to professionalism and betrayal of public trust.’ (p. 39) So Miettinen’s solution is to organize expert panels to discuss and
decide on the diagnosis, etiognosis (don’t try and understand the term, it’s Miettinese) and prognosis of hypothetical patients, this knowledge then being made available to clinicians in general: ‘the knowledge-base of clinical medicine would get to be codified in, and available for ad hoc retrievals from, practice-guiding expert systems’. (p. 131) ‘For a leader in a discipline of clinical medicine to bring about major improvements, (s)he is to satisfy two pre-requisites beyond being genuine and well-qualified as a leader: like all contributors to the ascent of man, (s)he is to have “an immense integrity, and at least a little genius.”’ (p. 4)

Leaving aside this somewhat disturbing cult of the leader, how is the information from expert panels to be summarized and presented? Miettinen argues that such information cannot be summarized or presented using methods such as Bayes Theorem (‘development of the scientific knowledge-base (acausal) of diagnosis has been held back by commitment to the theoretical framework of Bayes theorem’ (p. 31)), or the Cox proportional hazards model (‘development . . . of the knowledge base of prognosis has been retarded by commitment to the theoretical framework of Cox regression’ (p. 31)). So what new methods does Miettinen propose to replace these ‘traditional’ methods—logistic regression and Poisson regression! These are presented without references and without acknowledgement that Poisson regression and Cox regression are directly linked, and will yield the same answers when applied appropriately to the same data set.13

I managed to make it through the clinical epidemiology book, but about halfway through the dictionary I stopped reading because I had begun to lose the will to live. So maybe it all came together in the end. Who knows? In a response to a previous paper from Miettinen I commented on ‘his tortuous terminology which often involves inventing new complicated terms for old simple concepts’,11 an observation that Sander Greenland had made previously,14 in his review of Miettinen’s previous book.7 Others have commented on his apparent unwillingness to cite any previous authors,15 or even to concede that many of his concepts are simply new names for concepts that have been discovered and described many times before. If anything, these problems may have got worse since 1985; these new books contribute nothing in terms of synthesizing, or even referencing, recent concepts or methods; there is nothing on causal diagrams, mediation analysis, multilevel analysis, marginal structural models, shrinkage and structural nested models. What we get instead is Miettinen’s personal world, with few citations of other authors.

Despite these omissions, I have great sympathy for Miettinen’s ‘project’ to make epidemiology much more theoretically rigorous; in particular, I agree with him that terminology is important and that a dictionary should ideally specify the way that theoretical terms ‘should’ be used, in addition to summarizing how they are currently used. I also agree with some (but by no means all) of his specific criticisms of the IEA Dictionary (e.g. I agree with him that a population need not be defined in terms of ‘a given country or area’).

But taken as a whole, what do these books contribute? Not much, in my opinion. What is useful in them is not new (and has been described more clearly by other authors); what is new is not useful.

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References


NEIL PEARCE
Faculty of Epidemiology and Population Health, London School of Hygiene and Tropical Medicine, Keppel Street, London WC1E 7HT, UK
E-mail: neil.pearce@lshtm.ac.uk