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Conventional Wisdom and/or Evidence -Based Psychotherapy

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Optimal health is much influenced by views and values related to health and illness. These are in a process of more or less continous change.In psychotherapy the dramatic decentering of society´s concern from symptom relief or the development of the personality to cost-effectivenss has just begun to impact our discussion.

Health care costs - or more precisely the growing gap between the therapeutically possible and the economically affordable - are promoting a critical evaluation of health provison. There is broad agreement that limited and expensive therapeutic ressources have to be used in the most efficient way. However, who sets up the standards ? and by what criteria ?

The conclusions Klaus Grawe and his collaborators (1994) draw from their meta-analytic findings were based on more than 800 controlled studies; the issue for the conventional German clinician was: would he or she change his or her daily practice just because meta-analytic research has shown this or that kind of treatment has not demonstrated its efficacy. How much has the field of treatment research achieved to be so recommendative and re-educative. As a non-believing psychoanalyst I am faced with the situation that most of available research is not on the treatment I still do offer to some patients- not to all, not to many, but to some precious individuals.

Most of us have to ask themselves: do we have to question ourselves on how do we make the decision to go for which kind of treatment. And who of you ever has offered a patient to first telling him the real truth about the latest meta-analytic findings as part of the contractual sessions.

To illustrate this issue I will detail just one example: The Grawe et al. 1994 review judged a widespread method of relaxation - called 'autogenic training' invented by I H Schultz (1932) - about the same time when Jacobson invented his method - as poorly validated. The problem of such statememnts is the difficult to calculate the half life time (Halbwert-Zeit) of the research findings (Kächele 1996). At the time when the Grawe book appeared, a major German controlled study - comparing cognitive-behavioral interventions with a control condition, namely the autogenic training demonstrated that the so called control conditions was as powerful as the highly praised cognitive-behavioral intervention for the treatment of itching (Ehlers & Gieler 1994).
Meanwhile a recent meta-analytic review has identifed 64 controlled studies, 50 of them demonstrating positive effects, and 14 zero or negative effects of AT on psychological disturbances (Stetter 1998).

So what should the clinician do - wait and see when the research community reaches agreement ? but which research community, who defines that. A similar in scope review as the Grawe et al. one by Roth and Fonagy (1996) What works for whom ? was much more considerate and balanced, maybe due to the British nature of their authors. Was Grawe then too teutonic ?

Is there a way to bridge from clinical, traditional wisdom to more systematic , yet indiviually applicable knowledge ?

The recent rise of Evidence-Based Medicine (EBM) has been one of the more remarkable phenomena of the British health scence during in 1990s. Recently it also has come up in the German speaking world. Initially EBM grew as a bottom-up approach to continuing medical education under the name of Clinical Epidemiology. CE was based upon emphasizing the potential of epidemiological information for guiding clinical practice. CE was widely regarded as a refreshing approach that blew away cobwebs and let in some light. CE mobilized the enthusiasm of people to come to grip with interpreting clinical data for themselves for use in their own clinical practice.

Let us first listen to the prophet of EBM, David David Sackett´s introductionary statement:

"The practice of EBM is a process of life-long, self-directed learning in which caring for our own patients creates the need for clinically important information about diagnosis, prognosis. therapy and other clinical and health care issues., and in which we:

1. convert these information needs into answerable questions;
2. track down, with maximum efficiency, the best evidence with which to answer them (whether from the clinical examination, the diagnostic laboratory, from research evidence, or other sources);
3. critically appraise that evidence for its validity (closeness to the truth) and useful ness (clinical applicability);
4. apply the results of this appraisal in our clinical practice; and
5. evaluate our performance"

(Sackett et al. 1997, p.2)

The programme of EBM is a procedural conception, not a fact-based concept. Simply to proclaim that we have to practice evidence-based psychotherapy - meaning that we have to believe in the findings of RCT represents a misunderstanding.

Therefore Sackett recently had to declare "What EBM is and what it is not" (Sackett 1996).

It is not 'cook-book' medicine. Because it requires a bottom-up approach that integrates the best external evidence with individual clinical expertise and patient choice, it cannot result in slavish cook-book approaches to individual care. External clinical evidence can inform, but never can replace, individual clinical expertise and it is this expertise that decides whether the external evidence applies to the individual patient at all and, if so, how it should be integrated into a clinical decision. Similar, any external guideline must be integrated with individual clinical expertise in deciding whether and how it matches the patient´s clinical state, predicament[1], and preferences.

Let me clarified some of these issues.

[1]complex, difficult situation

First misunderstandings occur with the use of the notion of evidence. For example in German the expression "something is evident" means more self-evident, you do not need proof. The word "evidence" in English means proof, meaning that there are data available that are able to support a clinical statement.
EBM represent the continous effort to state which data support which conclusion - this is all, basically. Quality of these data and quality of the derived conclusions may be variable.
A total misunderstanding - which politically sometimes is heavily used (Charlton & Miles 1998) - would be to name EBM a representative for that kind iof medicine that only proven statement should be used in daily clinical practice. Even if one is committed to this new look in medicine, it would be foolish to deny that 50% of medical practices are based on soft evidence only.
David Sackett, the initiator of EBM, has investigated the decisions on a general medical ward. There he demonstrated that about 80 % of these decisions were based on hard scientific facts or on solid clinical experience. There is agreement that such a figure is unrepresentative for the whole field sof medicine; depending on the kind of the field and the status of its scientific development such figures are much lower. (are there figures available ? for psychiatry ?).
More important als the quality of the evidence is the question, whether a decision has been based on data at all and whether the necessary knowledge is available and whether the readiness for self-criticism is present.
One that has learned and has trained to critically evaluate once´s therapeutic actions is likely to become a enthusiastic follower of EBM.
EBM is less likely to be practiced when the health care provider does not wish to care for transparancy or when ethical aspects are not highly recognized.

The heart of EBM is the explicit and conscientious and most fitting application of the best available external evidence on a medical decision (Sackett). Mind you. the core of this statement resides with the expression. <best available>. This statement is likely to produce debate, as many of our collegues are convinced, to work along this principle since long. The huge variety of opinion among us speaks for itself. In psychotherapy we are still a long way from this debate as the degree of agreement about the value of research findings to the clinicians is still small. maybe therefore it is important to get used to this perspective early.

What is meant by best available evidence. A simple reasoning could be that in the english language use evidence means proof and proofs in medicine are only created by randomized studies providing the only control in the sense of experimental studies. This reading is a misapprehension. Our reading is different: evidence is not proof; evidence in EBM means the ethical obligation to support your clinical opinion by external facts thus providing evidence - the more reliable the better.
There are any number of issues in medicine that cannot be answered by a randomized controlled trial. For example:

the quality of a new diagnostic tool requires the parallel application of two tools, an old one and an new one.
In order to evaluate the validity of a prognostic factors this factor should be described in a well specified sample at a early as possible time of the illness and the course of the illlness should be observed as long as possible
Comparing a surgical with a medical intervention randomization hardly is an appropriate tool, as any practioner knows that as well the complicance of the patients as the adherence of the doctors with the randomization gets smaller, the larger the risks of the treatments will be. In such case patients and doctors - consciously or unconsciously - performs a classical economical analysis, comparing costs of treatment and consequences of alternative actions this deciding on the basis of his or her psersonal experiences and available informations. Recently these new findings limitating the performance of RCT have been summarized in a symposium (Abel & Koch 1998).

It is one of the important tasks of our field to estimate the role of diverse research methodologies. We do not have to share the prevailing notion of natural science medicine that only by a well done (lege artis) performed randomization, with the highest degree of internal validity, should be looked upon as representing "the highest level of evidence". As there are conditions in which randomization is not feasible one has to accept that this "highest level of evidence" cannot be achieved.
Reeturning to the example of a surgical with a medical intervention where randomization hardly is an appropriate tool, any practioner in our field knows that as well the complicance of the patients as the adherence of the doctors with the randomization gets smaller, the larger the inconevenience of the treatments will be.

In a recent German study on the treatment of anxiety disorders randomization totaly failed as the two treatments- inpatient three month versus outpatient one year were preferred by totally diverse populations. Middle aged male patients out of job went to the inpatient trreatment , mothers with children preferred the outpatient arm (Krauthauser & Bassler (1998)
.
So even if one has achieved the goal to successfully implement such a RCT one has to keep in mind that this "highest level of evidence" is only valid in respect with this one study; it is unclear, or less clear how far the results also have external validity. Therefore the critical evaluation of studies has become the focus of the educative enterprise labelled EBM. How to do this in a sophisticated way is at the heart of the various teaching devices. Among the the so called journal club is very important providing an exercise in reading scietific reports. The access to medical data bases is absolute necassary.

Important lessons for teaching:
We should make absolutely clear that good teaching has to cover three levels: first we have to inform the students about the rihth attitude (arrogant and aggressive colleagues need a special treatment before proceeding tio the next level). Second, the students have to aquire some skills (eg number needed to treat) einfach zu Hause bei mir anrufern, wenn die Formel verloren gegangen ist...and finally the students have to get some knowledge. Most teacher will not follow this sequence and focus mainly on the teaching of knowledge. We are convinced that the atttitude (to listen to the other colleagues, to reflect on that what others say, to be open to other minded people - these attitudes may be much more important than knowledge which will expire within a few years.-- the attitude has a much longer half life - decide by yourself what you want to do ..
In general the crucial issue is to understand what Ken Howard formulated very succinctly - as always -

that methodology cannot be evaluated independent of the research question.

There are three questions regarding the results of a treatment

1. Can this treatment be shown to work? -- efficacy
2. Does this treatment work in practice? -- effectiveness
3. Is this treatment working for this patient? -- efficiency

The first two questions are treatment focused (e.G., main effects) while the latter is patient focused. There is no necessary logical connection among the answers to these questions (a treatment could be shown to not be efficacious, yet it could be effective; a treatment could be ineffective and efficient for a particular case).
We have to keep in mind that the research methodologies for these questions are distinct:

1. Efficacy -- Experiment
2. Effectiveness -- Quasi-experiment
3. Efficiency -- Systematic naturalistic observation

Let me conclude by again mentioning the German Study on the inpatient treatment of Eating Disorders that I have presented in Buenos Aires two years ago.
These study was undertaken because the majority of these patients are treated within hospital settings for which hardly anyone would conceive of a randomized allocation of patients.
The scientific motivation for this enterprise derived from the accumulating awareness that sample sizes in traditional studies are much too small to investigate the multi-variate influences in a clinical field. Meanwhile the study has reached the follow-up time for the big majority of patients.
As you will see that leading hypothesis for this study - does duration of treatment influence the follow-up outcome - is very likely to produce a negative answer.
Given a huge variability forthe amount of treatment, with a median of around 11 weeks - we cannot predict the length of stay from any patients variables. Solely the type of hospital shows a strong predictor effect. Based on the findings of Kopta et al (1994) on patterns of symptomatic recovery Kordy studied the amount of therapeutic progress after the first four weeks: his finding is that there is a good likelihood to identify those who win and those who will not gain from the therapeutic enterprise across differential hospitals (Kordy 1998).

Coda

From my point of view psychotherapy is in a similar position to other fields of medicine (e.g.cancer research) where protocol guided research has become the standard to collect fairly homogeneous samples in the field. The humanistic motivation of the study resides in a growing awareness that formal research into the natural worlds of psychotherapies is a timely thing.

However for therapists viewing themselves as being engaged in a deeply humanistic enterprise, this constituted a major challenge to their professional identity. To strive for the very best for one's patient and also to understand the macro-economical conditions of psychotherapy really is a major emotional and intellectual strain. The trend of psychotherapy research thus becomes truly interdisciplinary, a sharing of responsibility between clinicians and researchers. Then, however, modern therapy research may have "enormous practical implications because it will lead to more focused therapeutic strategies and provide sharper answers to the question of what psychotherapy can do for particular patients, at what cost, and over what periods of time." (Strupp & Howard 1992).

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