Some issues of current psychoanalytic politics towards CBT raised by a survey on Empirically Supported Psychotherapies by Drew Westen[1]

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language was conceived in sin
and science is its redemption (Quine)

I would like to introduce a recent survey of so-called 'scientific evaluations' of the effectiveness/efficacy of psychotherapies, with their ambition of establishing a list of 'evidence based' treatments for particular 'disorders'. It was published in 2004, in the "Psychological Bulletin" – which means that it is to be considered as very serious and influential from the university point of view. Everybody should read this brilliant survey – you can obtain a digital copy from me.

What is the point of Drew Westen, the main author of this survey – and what is the problem with that point?

First of all Westen reminds us that we should always keep in mind that the background of this mania for evaluation of psychotherapies is an imaginary rivalry of university psychology with medicine. In order to establish itself as a science psychology has to prove at all costs that treatments based on it are as effective as biologybased treatments. Unfortunately medical discourse will only understand this proof if it is formulated in the language of pharmacology, being the experimental language of Randomised Controlled Trials. RCT as it is called, means that a new medication is tried by distributing carefully selected identical subjects at random among two conditions: in the experimental condition the medication is administered, but not in the control group that will function as a reference. In other words RCT is the classic experiment.

Now, Westen readily acknowledges that such experiments can prove that Cognitivist Behavioural Therapy can be as effective as medication. But at the same time he argues that this is only possible to the extent that CBT perfectly agrees with the drastic restrictions RCT-evaluation imposes – and which in the case of psychotherapies boil down to a 'purification' of both patients and treatments alike. Here I cannot go more deeply into this purification which reduces patients to one simple disorder, in terms of DSM, and treatments to one active ingredient, like in medication trials.

In the concrete 'pure patients' means that experimental samples only consist of patients with one and the same Axis I disorder –as you may know this is the main, behavioural axis of classification in the DSM-IV – with Mood Disorders like Depression, Anxiety Disorders like Panic and Eating Disorders like Bulimia. Patients with comorbidity, or more than one Axis I disorder, mostly are excluded from RCT, idem for patients with substance abuse or Depressive patients with suicidal ideation. Finally patients with an underlying Axis II disorder are also excluded – you may know Axis II is a kind of garbage can for

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'psychoanalytically inspired' diagnoses, with the Borderline Personality Disorder at the centre. So in RCT patients should not be borderline – or hysteric, in our language.

The problem is that this RCT requirement of patient purity may result in the exclusion of more than two thirds of real life patients. Which of course causes tremendous problems of generalisation to real life. As a consequence for most patients no psychologically based treatments are evaluated – and as 'unvalidated' or 'unevaluated' readily comes to mean 'invalidated', in reality the bulk of the patients are left at the hands of pharmacological treatments or at the mercy of all kinds of counsellors without formation in university psychology.

Besides 'pure patients' RCT also requires 'pure treatments'.
First of all this means that in order to be capable of RCT-evaluation psychotherapies should be short enough, as short as the pharmacological treatments they are to be compared with.
Secondly, and once again following the model of medication trials, it is assumed that 'the active ingredients' of any treatment can be identified and isolated, so that a particular dose of one 'active ingredient' can be administered for a particular lapse of time. This standardisation of what is to happen in each session of the experiment is called 'manualisation'. Among other things this implies that each treatment is asked to focus on its 'core business': psychoanalysis for instance may not pay attention to the symptom and CBT may not pay attention to history; it should be clear that this is only a handicap, and a serious one, for psychoanalysis with its basic rule of free association and evenly suspended attention … Westen is indeed right in arguing that RCT manualisation of treatment excludes both patient and therapist as active ingredients of the treatment.

Westen's point is that most psychotherapies cannot be evaluated the RCT way, simply because most psychotherapies and the patients who feel attracted are far too complex. For most psychotherapies RCT evaluation would require samples that are far too large and experimental design that are far too complex, leading to findings that it is far too difficult to interpret. So, Westen proves once and for all that most psychotherapies cannot be evaluated in the RCT way.

Of course this is music to our psychoanalytic ears. However, we should not crow too soon: although Westen is a psychoanalyst, and even a distinguished member of the IPA think-tank on science, he nowhere explicitly mentions this allegiance. Moreover he even takes no notice at all of the evaluation of psychoanalysis and the exceptional problems it raises.

No doubt this strategy is motivated by the best intentions, being the survival of psychoanalysis. But on the other hand we also have to ask ourselves whether this covert strategy finally does not risk betraying the very essence of psychoanalysis as "a therapy unlike all others". I think there are indeed at least two delicate political issues raised by Westen's doings – implying two crucial choices for the future of psychoanalysis.

First, as Oscar Wilde said, "one cannot be careful enough in the choice of his closest enemies" – so we have to ask ourselves who's actually our real enemy: behaviourism or medical discourse?

As already mentioned, although being a psychoanalyst, Westen readily acknowledges experimental proof that, under particular circumstances and for particular disorders, CBT can be as effective as medication. Now, it seems that, in exchange for this concession, Westen would expect CBT to concede that it cannot be proved experimentally that psychoanalysis is not effective –contrary to what CBT invariably states experimentally 'unvalidated' does not automatically mean 'invalidated'. Treatments that fit RCT requirements, like CBT, are likely to obtain the best results on such trials. Treatments that do not fit these requirements, like psychoanalysis, simply cannot be evaluated and invalidated by such trials. What I

suggest is that with this survey Westen enters into a kind of secret negotiations with the enemy. "We say that you are as effective as medication, and you don't say that we are not effective at all. And thus we can form a kind of monstrous psychological alliance against medical discourse."

Indeed, one of the main problems with psychoanalysis, from the point of view of university psychology, is that until now we have been siding too much with medical discourse, in particular psychiatry. Lacan for instance always considered 'applied psychoanalysis' to be a form of medical therapy, never mentioning psychotherapy in this respect.

So the strategic question is whether, in the present constellation and for the time being, psychoanalysis should not show some solidarity with our Big Brother, with our strong CBT friends – just in order to establish first our unity and identity as 'psychological science' in opposition to our common enemy, the medical discourse?

My idea is that RCT validated CBT never will be able to secure any identity at all outside medical discourse. This has to do with a nasty totalitarian trait of CBT, that finally will turn against CBT, making it more dependent than ever on medicine. Westen himself draws attention to the vicious way CBT transforms the experimental necessity of pure treatments for pure patients into a clinical necessity. This means that real life therapists also should limit themselves to 'treatment packages', applying specific manuals to specific behaviour disorders – but, as no one can be expected to be 'competent enough' to apply all manuals to all disorders, this will inevitably lead to specialisation. And at that point it is beyond doubt that university psychologists will be reduced once again to what they fear most, to para-medics or pure executors of medical prescriptions …

The second fundamental choice forced upon us by Westen's doings is between cognitivism or psychoanalysis itself as the foundation of the formation of the psychoanalyst. Indeed, everything seems to indicate that Westen is trying to seduce cognitivism to change partner, to break up its current hugely successful liaison with behaviourism and to come back home to psychoanalysis, where as a matter of fact it was bred – you should know that its inventors, Beck and Ellis, are of good psychoanalytic birth. Westen stresses for instance that unlike pure behaviourism Beck's cognitive therapy would be very difficult to evaluate experimentally – just like psychoanalysis, which however he does not mention … This secret gliding towards Cognitive Psychoanalysis is already professed much more clearly in recent publications by French IPA-president Widlöcher.

Anyhow, Westen's paper does have the merit of making shine through the fundamental problem with cognitive psychoanalysis – namely that it questions the analysis of analyst as the only and ultimate basis of psychoanalytic formation. If we follow Westen, and the IPA, the interpretation and the act will no longer be a matter of the desire of the analyst, resulting from his own analysis, but a matter of his competence to make empirically well informed decisions … from the desire of the analyst to the analyst as a competent decision maker.

RCT RESTRICTIONS WITH REGARD TO PATIENTS

simplification, purification of pts
pts can be treated as if they only would have a single, pure disorder = mono-symptomatic disorder

- reason: pts with more than one symptom disturb RCT-purity:
  - need for much larger samples of pts
  - RCT-design becomes too complicated
  - results are much more difficult to interpret (need for secondary correlational analyses)
- definition: the 'single disorder' in the DSM-IV
  - only one Axis I disorder (= disorder in terms of behaviour)
    typically: mood disorders (depression), anxiety disorders (panic, Generalised Anxiety Disorder), eating disorders (bulimia)
    - no comorbidity or association with other Axis I disorders
      the problem of comorbidity is solved by
      - recognising that there might be several problems or disorders, but these can be treated sequentially
      - assuming that poly-symptomatic conditions as seen in the community have no emergent properties – so it is suggested that these do not call for a common aetiology, implying that there's no need for a adapted treatment either
    - no substance abuse (= paradigmatic Axis I disorder)
    - no suicidal ideation (especially in the case of depression)
  - no Axis II disorders (= disorder in terms of personality = psychodynamic diagnosis)
    - no Borderline Personality Disorder (together with the Histrionic and the Narcissistic Personality this BPD makes up hysteria)
    - Obsessive-Compulsive Personality Disorder is already much more susceptible of entering the RCT-design, because it can be reduced to discrete forms of behavior as OCD
- result: up to 2/3 of pts are excluded from RCT
  mostly researchers do not mention how many pts have been excluded, why and at what stage of RCT
  - often researchers ask clinicians to supply pts with a particular disorder – in real life it is pts who ask for treatment (demand!)
  - often diagnosis is made by the referring clinicians
    - is their 'diagnosis' to be trusted?
    - unknown percentage of exclusions of patients by clinicians who know the study's exclusion criteria
- problems associated with exclusion of realistic pts in RCT
  - transfer to real life clinic becomes questionable
    - treatments tested on mono-symptomatic lab pts do not necessarily work for the poly-symptomatic patient in the community
    - in real life comorbidity and underlying personality disorder (borderline!) lead to much longer and less effective treatments, even in the case of CBT – *my question: should we promote the notion of borderline as a stumbleblock for RCT-evaluation of our efficacy?*
    - often enough in real life the primary symptom does not remain the main focus of treatment
  - for some disorders psy-treatments remain unvalidated, with the danger this implies that they are considered invalidated – *my question: will this mean that these disorders are left to the mercy of medical/pharmaceutical treatments?*
    - borderline (cfr spectacular 'bad' hysteria Zetzel)
    - suicide

- substance abuse – RCT-evaluation of specific treatments are also require exclusion of important percentages of real life pts
- many real life pts do not have a disorder at all according tot DSM-criteria, because their problems do not fit or cross thresholds for any existing category – the result is that research on problems that once dominated psychotherapy is virtually eliminated (unobtrusive 'good' hysteria) – *my question: will these pts be left at the mercy of 'counsellors' (clinician without a basic university formation in psychology – cfr proposition of law in Belgium)?*

RCT RESTRICTIONS WITH REGARD TO TREATMENTS

simplification, purification of treatments

- reason: in order for treatments to be subjected tot RCT-comparison with medication
- simplification on three dimensions: length, aim, efficacious elements
- length of treatment: should be short
  - for three reasons:
    - it should be of the same duration as medical treatments, in order to be comparable
    - the longer the treatment takes the more, unforeseen efficacious elements come into play and the more problems with RCT (need of larger samples, complicated design, need of secondary correlational analyses for the interpretation of results)
  - problem with transfer to the community
    - naturalistic studies consistently find a dose-response relationship, such that longer treatments, particularly those of 1 ot 2 years and beyond, are more effective – longer treatments are more effective in the long term (follow up)
    - this is especially the case with
      - comorbidity and BPD (character problems)
      - CBT that takes much longer than planned – once RCT-evaluation of CBT had established that results frequently did not last, this led to 'more of the same', even suggesting lifelong CBT for depression (in analogy with lifelong medication
- aim of treatment: should be identifiable on beforehand
- active or efficacious ingredients/techniques of treatments:
  - principle:
    - the active ingredients of a treatment can be identified and spelled out in manualised form (= treatment package) – what is to happen in each session can eventually be standardised
    - same technique should work for the same Axis I disorder, regardless of its etiology
  - model: medication trials, with standardisation of the specific ingredients of the medication/technique, the dose of the technique and the timing of that dose
  - impoverishment of treatments, in order to fit RCT-requirements (experimental purity):
    - treatments must be simplified and reduced to their principal active ingredient
      - e.g. psychodynamic treatments (IPT, psychoanalysis) must not focus on the symptom – CBT must not focus on childhood
      - *my remark: this is only a problem for psychoanalysis that does not focus at all*
    - the common active factor gets lost = transference
  - double exclusion:
    - exclusion of the 'activity' of pt (*free association and transference*) – clinician (or rather the treatment-manual) sets the agenda for each session
    - exclusion of the 'activity' of the clinician (*desire of the analyst and interpretation*) – because it is

too difficult to evaluate via RCT
    - clinicians are reduced to para-professionals who apply specific manuals (treatment packages) to specific behavioural disorders (Axis I) – if transported to real life this will be unworkable, because clinicians should be able to apply another manual for each disorder, so inevitably some kind of specialisation will force itself upon them
      my remark: via RCT evaluation psychotherapy based upon university psychologists try to secure a place of its own, as the equal of medical treatment – ironically, by degrading themselves to executors of a manual, they run the risk of being reduced to para-medics who are specialised in carrying out a particular form of treatment prescribed by some medical decision maker
    - clinicians in the control-condition often show less expertise and commitment than experts in the experimental condition, who are motivated to prove the efficacy of 'their' treatment – this is especially a problem where the same clinician functions in the two conditions
- non-bona fide treatments in the control condition (intent to fail) that may not contain the active ingredient of the treatment in the experimental condition (in order to guarantee experimental purity)
- problems with transfer of lab tested treatments to the community
    - studies of treatments show that:
        - with comorbidity, borderline or emotionally disturbed pts every clinician spontaneously uses more psychodynamic techniques – with emotionally constricted pts everyone spontaneously uses more CBT techniques
        - the more CBT techniques the more symptomatic improvement – the more psychodynamic techniques the better the global outcome
    - what is evaluated in the lab is only one coincidental combination of a few active, identifiable ingredients – it is always possible that another combination of non-identifiable ingredients is much more efficacious in real life
- shift in means and ends: from RCT-necessity to clinical necessity
    - RCT first establishes the effectiveness (as effective as medication) of pure treatments (short, only one or a few active ingredients) and then jumps to the prescription of this pure treatments in real life – the experimentally tested treatment package has to be implemented as such in real life
    - problems with this prescription in real life:
        - unvalidated treatments (long-term, many active ingredients, aim unclear) are considered to be invalidated – it is suggested that treatments as practised in real life would be less effective
    - ironical reversal: if a particular experimental limitation of a particular treatment leads to positive results in the labo, then it should also be observed in real life
        - e.g. IPT may not focus on the symptom
        - real life studies show that impure treatments (that borrow techniques of other forms of treatment) are more successful than pure treatments
        - ethical question: some active elements can be unethical not everything tested in a lab can be transferred to real life – e.g. electroshock
        - medical/pharmaceutical model: researchers develop new medications which pharmaceutical companies then market to physicians who are perceived as consumers

RESULTS: BUT WHAT IS A POSITIVE RESULT?

RCT proves that some treatments (especially CBT) have positive results – but what is a positive result?

what kind of results are to be interpreted as positive?

- basic assumption: malleability of psychological disorders –disordered 'behaviour' can readily be changed
- how to measure the effect of an experimental treatment:
  - effect size estimates – compared with:
    - pre-treatment/placebo measures
      problem with spontaneous improvement, "time heals"
    - other treatments
      mostly only one dimension is evaluated – e.g. is a treatment that has an enormous effect on 20% of pts superior to one that has a smaller but clinically meaningful effect on 90% of pts?
    - treatment in community
      less expert and motivated clinicians than the experts in the experimental conditions who are highly motivated to prove the effect of 'their' treatment
  - by means of outcome questionnaires and symptom diaries = *the language of the Other instead of the speech of the pt*
    - outcome questionnaires = signifiers of the Other
    - symptom diaries = seemingly the words of the pt, however:
      - CBT pts are already familiarised with symptom diaries as part of treatment itself – moreover CBT proceeds also through psycho-education, learning pts how to 'communicate' about their disorders in unambiguous terms – this could imply that complaints are less frequently noted when they do not meet the exact requirements of the language of the Other
      - clinicians sometimes collect outcome diaries themselves – this may influence pts
- interpretation of results:
  - no consensus on the definition of clinically significant improvement and recovery
  - statistical significance is a joint function of effect size and sample size, so that variable sample size can produce substantial fluctuations in significance values for treatments with equal effects
  - there's no consensus about the relationship between statistically significant difference and clinically significant difference
- positive bias towards effectiveness:
  - meta-analytic research only takes into account published studies, that are mostly positive – studies that did not produce positive results often don't get published at all
  - investigator allegiance is the best predictor of which treatment will be most successful, higher effect sizes for investigator's preferred treatment – *my remark: Luborsky's Dodoverdikt at the end of the caucus-race: "everybody has won and all must have prizes!"*
- results for five specific disorders (depression, panic, GAD, OCD, bulimia)
  - pts never end up completely symptom-free
  - only minor differences in outcome across types of treatment
  - nevertheless a tendency to CBT superiority in RCT-evaluation
    - superiority CBT for panic
    - *my question: should we, out of tactical considerations, in the first time preserve the unity of psychology in front of the medical discourse and sustain CBT in its endeavour to prove itself as the equal of medication – on the condition that it acknowledges that RCT does not invalidate psychoanalysis but only lets it unvalidated?*
  - transference:
    - all treatments obtain symptom relief after 5 to 6 sessions, before the active ingredients have been administered = *transference neurosis*

    - immediate syptom relief after the initial call, before even meeting the clinician = *signifiant of transference*
    - nevertheless this initial transference effects only consolidate themselves when treatments are continued (relapse when treatment discontinued)
  - follow up of effects:
    - some pts continue to improve after the end of treatment
    - how to interpret the seeking of additional treatment after the end? do they return to the same treatment or not? maybe they return just because they found the first experience with that treatment rather positive …
    - treatments differing in initial response may yield highly similar efficacy estimates at 2 years – treatments similar in initial response may have very different outcomes at 5 years
- eulogy of impure treatment:
  - results are better if the clinician does not remain faithful to his preferred treatment
    - psychodynamic clinicians using CBT interventions do better than their pure colleagues
    - CBT clinicians using use psychodynamic interventions also do better than their pure colleagues
  - *my remark: this comes down to a plea for the effectiveness of a common, non specific, 'impure' active element, being the transference – transference only has its chance when the clinician drops his standards (but not his principles)*

conclusion – problems with the 'strategy' of Westen & co

- to save psychoanalysis …
  - Westen never mentions RCT studies of psychoanalysis, but only RCT studies of CBT en IPT (interpersonal psychotherapy) proving that these are as effective as medication
  - Westen only proves that psychoanalysis cannot be validated the RCT way, and consequently it cannot be invalidated
- … by recuperating cognitivism
  - nowadays cognitivism has been incorporated by behaviourism – so Westen tries to place cognitivism at the side of psychoanalysis
  - Beck was/is a psychoanalyst: innovations should come from clinical practice – e.g. cognitive therapy emerged from the practice of a psychoanalyst, Beck (and from converging observations of another psychoanalyst, Ellis)
    - clinic: clinical data convinced him that the psychoanalytic methods of the time were too inefficient
    - research: clinical empiricism (= trial and error and careful assessment of its results) led to new techniques

    integration of clinic and research = true scientific spirit = changed his theories and techniques when the available data (clinical and empirical) suggested the importance of doing so = clinical practice as a natural laboratory
  - just like psychoanalysis Beck's cognitivist therapies would be extremely difficult to evaluate by RCT (too many active ingredients, too long, too much pt activity)
  - cfr current IPA politics – Widlöcher
- Westen's alternative:
  - competences (selfpsychology!)
    - clinical competence to read people
    - scientific competence to read applied and basic research = competence to inform themselves
  - decision maker
    - the clinician does not simply applicate/implement/administrate treatment packages to specific disorders – but he should be competent to make a combination of different empirically validated techniques which is adapted to a particular case
    - *my remark: this competent and informed decision maker is the contrary of the desire of the analyst whose capacity of technical invention is related to the end his own analysis (the pass)*
  - how to create such competent and informed clinicians?

    my question: does Westen question the didactic analysis –cfr *Grawe: the indoctrination (identification) with two types of interpretation (transference interpretation and ambiguous interpretation) is responsible for the fact that analysts do not take into account the scientific findings of university psychology with regard to treatment effectiveness*
- my conclusion: we have to be on our guard against our university friends = s'en passer à condition de s'en servir, to do without the university on the condition of making use of it – we will have to develop our own alternative methodology and procedures to prove our effectiveness, bases on our own assumptions, with our own findings and ways of reporting them