Hans Eysenck's Contribution to Clinical Psychology

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Hans Eysenck outlined elegantly in the early 1950s the attributes of an applied therapy: one based on consistent properly formulated theory leading to testable deduction, and concerned with habits existing in the present. This mission statement rapidly brought him into conflict with Freudian therapy (e.g., 1952 paper on the ineffectiveness of psychotherapy). Subsequently, Eysenck directed research into the role of conditioning, extinction and incubation in fear management at the Maudsley Hospital. He promoted the study of individual differences in psychophysiological functioning to account for variations in clinical response to medication and behavior therapy and advocated a dimensional rather than categorical approach to abnormal behavior. He was an early critic of the use of psychiatric diagnosis ("committee decisions") and illformed cognitive concepts ("speculation") amongst clinicians: criticisms echoed strongly in the current literature.

In a previous series of essays in honour of Hans Eysenck (Lynn, 1991), Professor Stanley Rachman has described from personal experience the role of Hans Eysenck and the psychology department of the Maudsley Hospital (of which both he and Hans Eysenck were members) in the development of the clinical psychology profession and the early battles for the recognition of behavior therapy. Here, I will not repeat the comprehensive account given by Prof. Rachman. Instead, I would like to discuss more generally the lasting legacy of Hans Eysenck's contribution to clinical practice in the cognitive-behavioral therapies.

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Four main points characterize the behavioral approach in clinical psychology. These are: its scientific basis as a body of theoretically organized knowledge; its quantitative and in particular dimensional approach to psychopathology; its concern to account for and exploit individual differences in treatment response; its acausal (bi-directional) psychophysiological account of abnormal mind-body interactions. It is not difficult to see Eysenck's legacy in evidence here.

Scientific Basis of Clinical Psychology

The first and indeed most important aspect of the success of behavior therapy is that it is explicitly a clinical science, and adopts what has become known as the scientist-practitioner model. Or perhaps it would be more correct to speak of a scientist-practitioner ideal. In practice, this means basing treatment interventions on soundly validated principles derived from experimental observation, rather than, say, personal dogma. Equally, of course, it implies a practitioner who approaches treatment as a scientist formulating hypotheses, and taking measurements of outcome rather than relying on intuition. This is why, I say, the model is an ideal since of course clinicians, as Eysenck himself lamented, can fall short on all these points (Eysenck, 1984).

Another aspect of the scientist practitioner model is the presence of a theory of process; an idea of what is happening and what is being affected during treatment. In the early days of behavior therapy, learning research and conditioning theory lent a valid theoretical framework to explore and interpret the maladaptive associations which behaviorally and physically characterized abnormal psychology. As Eysenck has argued in several of his writings, this concern to apply theory distinguished it from dynamic approaches, and at the same time advocate a new clinical paradigm (Eysenck, 1994). According to the new paradigm, symptoms are considered evidence of faulty learning not as evidence of repression; neurotic disorders are concerned with habits existing at present, their historical development is largely irrelevant. Cures are achieved by establishing more adaptive responses not by handling unconscious dynamics, or through establishing personal "transference" relations.

A theory of process ensured the early and continued survival of behavior therapy, since the therapist knew what s/he was doing and why s/he was doing it. Let us give as an example of this success the therapy of exposure. Experimental evidence had established that people habituate to stimuli over time; early behavioral experiments had established that conditioned fear responses could be deconditioned simply through exposure to the feared object in the absence of the feared aversive consequences. Knowledge of these processes allowed prediction of
the parameters for successful exposure of the client to anxiogenic stimuli. It is also crucial that the intensity of CS exposure be determined to avoid incubation effects. The person must be exposed long enough for the anxiety to arrive and then dissipate, and without any avoidance taking place. These principles have formed the basis for the most successful behavior therapy to date, which is still the treatment of choice for anxiety conditions. Furthermore, proving the validity of habituation to the client, through their experience of the habituation curve, lends credibility and confidence to person about their ability to unlearn old and learn new habits. As Eysenck (personal communication) once said "Why argue over something that can be proved?"

**Learning Theory and Behavior Therapy**

Eysenck was always keen that therapy should stay close to its learning theory base and that new developments in learning be incorporated into therapy (Eysenck, 1985). He gave convincing accounts of how learning could elegantly explain not only therapy but non-specific effects, and subtle effects like placebo and anticipatory response (Eysenck, 1980a; Eysenck, 1982). Using his high grade common sense, he pointed out the number of non-specific factors (such as talking to others, being in a more optimistic mood) which could vicariously but positively influence extinction of the CR. He also noted the importance of pavlovian B rather than A conditioning for explaining failure to extinguish some anxiety driven behaviors. In pavlovian B conditioning, the CR and UCR are very similar and reinforce each other since the CS is a motivational state and so may even in the absence of UCS act as its own stimulus to elicit a pattern of reactions. One of Eysenck's most important theoretical contributions was his outline of a conditioning model of placebo effects and his subsequent insistence on criteria for the controlled testing and evaluation of clinical versus placebo effects (Eysenck, 1987). He noted that a whole class of influences need to be considered to constitute a credible placebo condition. The person must have faith in the treatment, a convincing rationale, perceive the therapist and the place as credible; be appropriately engaged in the therapy; be optimistic and confident. He suggested that few placebo conditions were so stringent but even so the effect size of psychotherapy compared to placebo was negligible. So Hans Eysenck never saw any reason to revise his 1952 view of the effectiveness of psychotherapy as "not proven" even 40 years later (Eysenck, 1952, 1992).
Cognitive Therapy

Eysenck suspected, quite rightly, that cognitive therapists would turn away too prematurely from theories of conditioning, despite the fact that recent learning research has emphasized S-S rather than S-R approaches and explicitly addressed cognition learning (Eysenck & Martin, 1987). Unfortunately, current cognitive therapy has tended largely to experiment with an atheoretical "use anything that works" approach, heedless of its theoretical coherence. The problem with simply using a technique because it seems to work, is that one does not really know what is actually working.

Until recently cognitive therapy has lacked a coherent theory and even a definition. As Eysenck foresaw, it has paid for this oversight with a loss in credibility. Many of its assumptions, such as thoughts cause behavior, have not been supported and it is not clear what components in cognitive therapy are necessary. Furthermore, although once fashionable, cognitive therapy now finds itself being critiqued by more fashionable post-modern approaches, exactly because of its assumptions about the primordial role of cognitive representations. Indeed, arguably post-modern approaches with their emphasis on "deconstructing" assumptions, bypassing mental representations, and looking directly at "practices" in the everyday world share much in common with behaviorism. Behavior therapy requires less assumptions than cognitive therapy, in particular, concerning the causality between cognition and emotion. As Eysenck pointed out, cognitions were never excluded from behavior therapy but simply counted as behavior. It would be difficult to accuse someone whose early published work was in the area of aesthetic preference of not being aware of subjective thought processes. Hence when others talked of cognitive behavior therapy, he considered the epithet cognitive irrelevant. Hans won partially since the official title of the world organization is now the Association of Behavioral and Cognitive Therapies.

Eysenckian Triad of Clinical Research

It is essential, as Hans realized, to continually go back and forth between theory, experimentation and practice. To begin with description, quantify the description, then look experimentally at what factors are influential in manipulating the phenomenon. Translate this manipulation back into the clinical reality, and then if necessary, relate clinical findings back to give the experimental paradigm more clinical ecology. A proper model must have a causal and a descriptive element and be subject to clear cut predictions and the testing of such predictions (Eysenck, 1986).
Figure 1 illustrates Eysenck's three-pronged (clinical, psychometric, experimental) approach to clinical research. He suggested that an experimental approach must be combined with a psychometric one to obtain answers that go beyond the sterility often associated with a purely correlational approach. As you can see in the diagram, all three arms of the triad are guided by underlying theory, but the problem remains how to "operationalize" clinical insights into psychometric terms or experimental into clinical terms.

One possibility is to operationalize variables along quantifiable dimensions which group together disparate characteristics as different expressions of a single continuum. Eysenck's first book was of course the "dimensions of personality" where he established the pattern of taking disparate performance and using factor analysis (FA) to look for communalities that might parsimoniously explain disparate performance in terms of more general personal approaches to life. The use of factor analysis to group traits and syndromes to see if they really do represent qualitatively distinct clusters or a quantitative dimension from normal to abnormal, is in stark contrast to the current diagnostic approach where clinician consensus seems to replace the role of detached enquiry. As Eysenck (1983) himself concluded, the DSM nosology is based on foundations that are insecure, lacking in scientific support, and contrary to facts, and that the use of DSM criteria may be justified only in terms of social need or pressures (Eysenck, Wakefield & Friedman, 1983).

He would be heartened to know that his dimensional approach continues to challenge such committee decisions. Increasingly, psychological studies are showing that so-called abnormal phenomenon contrary to received psychiatric wisdom are indeed dimensional and prevalent, in some form, amongst the normal population.
And I speak here not only of anxiety, but of pathology such as obsessional ruminations, hallucinations, delusions, depersonalization.

The importance of a dimensional approach is firstly that it forces the clinician to specify criteria for deciding that a phenomenon is both necessary and sufficient to characterize a disorder and is present in what degree. Secondly, a dimensional approach leads readily to understanding the processes involved in producing, say anxiety, since the problem is viewed as a more extreme form of a "normal" phenomenon. This view not only "normalizes" the phenomenon for the clinician and the patient, but it encourages the researcher to consider "analogue" experimental studies which test hypotheses about events or contexts which might "abnormalize" the normal experience.

Although Eysenck was clear that psychology should be a quantitative and as such not concerned with idiographic methods (Eysenck, 1954), he was aware of the need for accurate clinical description and the need to match qualitative criterion to quantitative dimensions. He developed his own statistical method for doing this (criterion analysis). In this method (Eysenck, 1950), a quantitative psychometric dimension is considered along side a criterion for clinical classification. Eysenck's suggestion for deriving a unique invariant and psychologically meaningful solution was to rotate the first FA centroid factor into a position of maximum correlation with the criterion column, so permitting systematic clinical insights to match up with quantitative approaches to process. The principle remains an important way of systematizing clinical findings, although newer methods of correspondence analysis and Bayesian approaches may be more advantageous.

**Individual Differences and Psychological Interventions**

The third point I noted as defining current behavioral practice is the individual difference paradigm. This paradigm takes several forms. But its theme is that people respond differently to the same treatments, or similarly to different treatments or dosages, or paradoxically to the same treatment at different times. This sounds a mundane clinical observation, but it is surprising how much it is overlooked, or treated as an unsystematic embarrassment. Eysenck showed us that not only should we expect such individual differences, we should rejoice in them since our theory and predictions become stronger as a consequence. The implications of the paradigm are that we should devise manipulations and interventions to take account principally of interaction effects. The model for understanding these individual differences is an interactive one involving state-situation, on the one hand and trait and genetic vulnerability on the other hand. The beauty of the model is that it includes biology and psychology, not in a reductionist sense but rather in a mediationist sense.
In the individual difference model (Figure 2), there is an initial predisposition which is a combination of general biological limits and specific genetic factors. Degree of conditionability then interacts with other types of learning (e.g. received information) to determine the extent and the way a person reacts to a given situation. This reaction determines the appropriate intervention (cognitive or somatic approaches). In the long term, both psychological and physical well-being may be effected by individual differences in outcome to treatment.

**Figure 2:** Implication of individual difference paradigm for psychological intervention

It is particularly important to state that clinically speaking the individual difference model makes no assumptions about causality. Biology does not cause a specific psychological reaction, but rather a specific set of dispositions, stresses and situations give rise to reactions. The model leaves open in any particular instance whether behavior may give rise to physiological changes or vice versa.

The individual difference model was always a psychophysiological model and one of the first unifying constructs to operationalize individual differences in the clinical setting was the arousal construct. This arousal construct was derived from early physiological research (by Malmo and Duffy) describing the inverted U-shape arousal curve on the basis of muscle activity and other responses to stress and arousal, Eysenck developed the hypothesis that each person may have an optimum
level of arousal and that people may be positioned differently on the arousal curve and require different levels of stimulation to influence their degree of arousal. As is typical with Eysenck the model, as he construed it, was simple and elegant and powerful. It gave a framework not only for the exploration and prediction of differential effects in behavioral management, e.g. on the differential benefits of discovery learning and receptive learning for extraverted and introverted children (Eysenck, 1996), but also on the observed paradoxical nature of drug effects. I would like to make reference here to the work of Willhelm Janke (Janke, 1983) on individual differences in response to tranquillizers and stimulants and also note the continued absence of the systematic consideration of individual differences in main stream psychopharmacology practice.

Eysenck suggested that anxiolytics were more likely to affect degree of neuroticism whilst stimulants and depressants would affect degree of extroversion. But the effect of the drug and its dose would depend on the client's personality. Stimulants, for example, stimulate extraverts but show a paradoxical effect on introverts. Eysenck also listed a whole range of subtle factors that would affect drug response, such as: present state, physical health, time of day, previous experience, mode of administration, and social environment (Eysenck, 1983).

The dimension of neuroticism (N), for example, is clearly a predictor not only of drug effects, but of successful withdrawal from anxiolytic dependence. In a poster presented concurrently at this congress, we show that N is a powerful predictor of ability to successfully discontinue benzodiazepines (BÖlanger et al, 1998). This finding ties in with Eysenck' previous studies of personality and successful smoking cessation (Eysenck, 1980b).

Although the original Eysenckian inverted U-shaped arousal model was heavily criticized for its naïveté, it has spawned tremendous research into areas as diverse as psychotherapy, stress and cancer. I counted over 700 articles in a recent search between 1980-1997. Hans Eysenck himself together with Grossarth-Maticke (Eysenck & Grossarth-Maticke, 1991) recently developed "creative novation" behavior therapy, and demonstrated the negative effect of psycho-analysis and the positive effect of a stress-management-oriented "creative novation" behavior therapy on cancer and coronary heart disease outcome. This latter finding needs further replication but indicates the far reaching implications of Eysenck's vision of the role of individual differences in determining psychological and physical health (see Figure 2).

Other examples of how Eysenck's individual difference paradigm has spawned productive clinical theory can be found in: Robert Thayer's work on mood regulation; William Revelle's work on diurnal variation, individual difference, and coffee drinking; Marvin Zuckermann's development of sensation seeking and our own work on understanding the paradoxical effect of smoking behavior.

I could perhaps now illustrate elements of all the points I have previously listed under the Eysenckian approach to clinical research (Figure 1) with some work I completed, under Eysenck's supervision, on smoking behavior. We began
from some previous observations on the situational profile of smokers based on Chris Frith's earlier work. We then systematized these situational factors into a four-factor questionnaire which divided smoking cues into high and low emotional, high and low attentional. We validated the distinction experimentally by looking at individual differences in physiological effects and the smoker's regulation of smoking under attentional and emotional conditions. We found that introverts and extraverts not only inhaled different amounts of nicotine but experienced different physiological and mood and behavioral effects as a function of smoking situation. Introverted smokers tended to inhale little and use the cigarette more for sensorimotor stimulation and smoked to aid concentration and skilled performance. Extraverted smokers inhaled deeply and tended to be more motorically and autonomically activated by smoking. We then looked at the clinical implications of these findings for behavior therapy and designed a smoking treatment program based on: our predictions of individual differences in state-situation interaction; and the use of appropriately tailored behavioral substitutions to help smokers perform tasks without smoking (O'Connor, 1989).

<table>
<thead>
<tr>
<th>Personality</th>
<th>Preferred smoking situation</th>
<th>Experimental effects</th>
<th>Physiological factors</th>
<th>Smoking habit</th>
<th>Smoking intervention</th>
</tr>
</thead>
<tbody>
<tr>
<td>Introverts</td>
<td>High attentional stress</td>
<td>Smoking improves skilled performance, selective attention, sensory bias, inhibition</td>
<td>Smoking increases central orienting, but has little autonomic effects</td>
<td>Low reported inhalation, more sensorimotor activity during smoking</td>
<td>Tailored situational reduction and behavioral substitution program</td>
</tr>
<tr>
<td>Extraverts</td>
<td>Low activity (boredom)</td>
<td>Smoking improves motor preparation, activation, tapping rate, motivation</td>
<td>Smoking increases central motor readiness potential and increases autonomic arousal</td>
<td>High reported inhalation and deeper puff profile</td>
<td></td>
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*Figure 3: Individual differences and smoking*
Conclusion

Finally, still on the subject of drug effects, psychologists are facing the decision now of whether to pursue prescription privileges. In the 1950s psychologists were faced with a similar query about the extension of the profession; namely whether to become clinical practitioners or not. The forces ranged for and against were similar. As, is well known, Eysenck did not initially support the idea of psychologists becoming psychotherapists, but this was because he did not want them to become pale imitations of psychiatrists who were doing, at that time, mainly useless psychotherapy. He supported psychologists as therapists when he felt confident that psychologists could bring a different and more effective paradigm to treatment. I'm sure Eysenck would hold the same view about the prescription debate, and I too would hope, in the same spirit, that psychologists would bring a new paradigm to prescription behavior. Such a paradigm would examine individual differences in drug response; systematize and dimensionalize the psycho-bio-social variables accounting for individual differences in reaction to specific dosages; and bring an experimental based theory to understanding the psychological and physiological processes involved in drug reactions and placebo effects. Of course, it is psychopharmacology as well as psychology which would benefit from this individual differences approach.

References


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Doprinos Hansa Ajzenka
kliničkoj psihologiji

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Hans Ajzenk je u 1950-im godinama elegantno skićirao svojstva primenjene terapije: ona je zasnovana na konzistentnoj, ispravno formulisanoj teoriji iz koje sledi proverljive dedukcije, i bavi se navikama koje postoje u sadašnjosti. Ovako definisana misija dovela ga je ubrzo u sukob sa frojdovskom terapijom (na primer, članak o neefikasnosti psihoterapije iz 1952). Kasnije, Ajzenk je usmerio istraživanja na ulogu
uslovljavanja, otklanjanja i razvoja u terapiji straha u bolnici Modsli. On je pokrenuo izučavanje individualnih razlika u psihofiziološkom funkcionisanju da bi objasnio varijacije kliničkog odgovora na lekove i bihejvioralnu terapiju. Zalagao se za dimenzionalni pre nego za kategorijalni pristup abnormnom ponašanju. Bio je rani kritičar korišćenja psihiatrijske dijagnoze ("committee decisions") i loše formiranih kognitivnih pojmova ("speculation") medu kliničarima. Te kritike su snažno odjeknule u savremenoj literaturi.

Вклад Ханса Айзенка в исследование клинической психологии

КИЕРОН Р. О КОНОР

Ханс Айзенк в 1950-ые годы вскрывает характеристики прикладной терапии. Она обосновывается на консистентной правильно сформулированной теории, на основании которой выводятся подлежащие проверке дедукции; исследует существующие в действительности навыки. Такой подход вскоре вызывает столкновение с фрейдовской терапией (на пример, статья о неэффективности психотерапии из 1952 года). Впоследствии Айзенк в больнице Модсли исследует роль условленности, отстранения и развития в терапии страха. Он инспирировал исследования индивидуальных различий в психофизиологическом функционировании, в целях объяснения вариаций клинического ответа на лекарства и бихевиоральной терапии. Айзенк признавал количественный, прежде чем категориальный подход к ненормальному поведению. Среди клиницистов он был первым критиком применения психиатрической диагностики (committee decisions) и плохо сформулированных когнитивных понятий (speculation). Эти критики нашли отклик в современной литературе.