

The Good, the Bad, and the Ugly: A 50-Year Perspective on the Outcome Problem

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In the middle of the 20th century, Hans Eysenck reviewed studies of psychotherapy, which consisted primarily of psychoanalytic, psychodynamic, and eclectic treatments, and concluded that psychotherapy (as opposed to behavior therapy) was not effective and was possibly harmful. In the inaugural article in *Psychotherapy*, Hans Strupp challenged Eysenck's conclusions and discussed how psychotherapy research should progress. Eysenck criticized Strupp's conjectures and Strupp responded. In this article, I discuss progress in psychotherapy research by examining "the good, the bad, and the ugly" aspects of the Eysenck and Strupp interchange. Essentially, Eysenck and Strupp motivated researchers to pursue, with increased sophistication, process and outcome research, but each was defending a theoretical position (behavior therapy and psychodynamic therapy, respectively). Despite the progress, the conjecture at issue continues to be debated today.

Keywords: outcome, effectiveness, relative effectiveness, process

In the 1950s and 1960s, Hans Eysenck, one of psychology's most famous personages and preeminent scholar, claimed that the research evidence was insufficient to claim that psychotherapy was effective. He reviewed the extant literature on psychodynamic therapy and eclectic therapy, which involved uncontrolled studies, and reported the proportion of patients who were (a) cured or much improved, (b) improved, (c) slightly improved, or (d) "not improved, died, or left treatment" (Eysenck, 1952, p. 321). In general, based on 19 studies, according to Eysenck, 44% and 64% of patients receiving psychoanalytic treatment and eclectic treatment, respectively, were cured, much improved, or improved. This is descriptively interesting, but when compared with "patients treated custodially or by general practitioners" (p. 322), who improved at the rate of 72%, Eysenck concluded that "there thus appears to be an inverse correlation between recovery and psychotherapy; the more psychotherapy, the smaller the recovery rate . . . [and the data] fail to prove that psychotherapy, Freudian or otherwise, facilitates the recovery of neurotic patients" (p. 322). Indeed, this was a severe indictment of psychotherapy (read psychoanalysis, psychodynamic therapy, and eclectic therapy) as a means to improve people's lives. Not unsurprisingly, Eysenck's claims did not go unchallenged.

The lead article in the initial issue of *Psychotherapy: Theory, Research and Practice* by Hans H. Strupp, entitled "The Outcome Problem in Psychotherapy Revisited" (Strupp, 1963), was one of the earliest critiques of Eysenck's claims. Strupp indicated, "A

brief review of Eysenck's (1952) widely quoted survey, which capitalized upon and added considerably to the existing confusion may be instructive" (p. 2). And Strupp *was* instructive, making several claims about Eysenck's studies, psychotherapy, and research in psychotherapy more generally. Eysenck, never one to ignore a challenge, responded, not meekly: "In reply, I would like to suggest that Strupp's review is, in a lawyer's phrase, irrelevant, incompetent and immaterial" (Eysenck, 1964, p. 97). Strupp, of course, offered a rejoinder, in which he concluded, "The controversy about the value of psychotherapy has been with us for some time, and it is not likely to be resolved by argument or counter-argument" (Strupp, 1964, p. 101). In 2013, the controversy has been with us for 50 years: What evidence has been produced in that time span? What is known? And what can be learned from revisiting the Eysenck/Strupp/Eysenck/Strupp exchange?

In this comment, I will classify the answers to these questions into, to use the spaghetti western taxonomy, the good, the bad, and the ugly. Of course, judging the past through the lens of the present creates the impression, often false, that we have progressed and the claims we make are good, probably not bad, and never ugly.

The Good

Eysenck's claims were not simply an academic exercise—the claim that psychotherapy was not effective was disseminated in the popular press. Keeping in mind that in this period, psychotherapy meant primarily *psychoanalysis* (and less so psychodynamic and "eclectic"), was practiced typically by physicians, and the public often thought of psychotherapy as a medical practice. The New York Times, in 1962, published an extensive article on Eysenck's work, entitled "Analysis of Psychoanalysis" (Hunt & Corman, 1962). The authors stated that "even the eminent psychoanalyst Lawrence S. Kubler commented a few years ago that 'the lack of critical and objective evaluation of psychotherapy is . . . an indication of where the greatest deficiencies of psychiatry as a medical and therapeutic science are to be found'" (p. 31), which is a

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troublesome observation, coming from the psychodynamic camp itself. The article went on to discuss issues with psychotherapy research and to suggest that controlled research was needed. Looking back, it is easy to criticize Eysenck's conclusion on the basis that they were derived from questionable comparisons, yet it must be kept in mind that the randomized control group design and the necessary statistical procedures for analyzing such data were only emerging in the middle of the 20th century and were not required for the approval of drugs by the US Food and Drug Administration until 1980 (Danziger, 1990; Gehan & Lemak, 1994; Shapiro & Shapiro, 1997; Wampold, 2001a). The "good" in Eysenck's work and Strupp's (1963) response to it lays not in what they claimed was "truth," but in what they spawned as a result, as discussed in the next sections.

Proliferation of Outcome Research

Eysenck put front and center the question of whether psychotherapy was effective. With the emerging technology involved with clinical trials for the most part in place by the 1960s, researchers began to conduct research adequate to address the efficacy issue (Goldfried & Wolfe, 1998). Essentially, the question is, do psychotherapeutic treatments produce benefits in excess of what would occur without treatment? In research designed to answer this question, patients could be randomly assigned to psychotherapy (of a certain type) and to a no-treatment condition (say, a wait-list control group). By the middle of the 1970s, there were hundreds of studies that addressed this question, yet disagreements about the answer persisted (see Bergin, 1971; Luborsky, Singer, & Luborsky, 1975; Meltzoff & Kornreich, 1970; Rachman, 1971).

The singular event in this controversy was the Smith and Glass (1977) meta-analysis, published in the *American Psychologist*. Although meta-analysis is now an accepted standard for synthesizing primary studies (Hunt, 1997; Mann, 1994), this meta-analysis of psychotherapy outcome studies was one of the first applications of the procedure, and Smith and Glass found strong evidence that psychotherapy, of all types, was remarkably effective (see also Smith, Glass, & Miller, 1980). The results of this meta-analysis did not go unchallenged (see, e.g., Eysenck, 1978, 1984; Rachman & Wilson, 1980; Wilson & Rachman, 1983), yet when several critics reanalyzed or conducted their own meta-analyses to rule out threats, Smith and Glass' results were corroborated (see particularly Andrews & Harvey, 1981; Landman & Dawes, 1982). It is now well accepted that psychotherapy is effective (Lambert & Ogles, 2004; Wampold, 2001b, 2007). Indeed, about 80% of those receiving psychotherapy will have mental health status superior to those receiving no treatment (Wampold, 2001b). In addition, psychotherapy appears to be as effective as medication for many mental disorders, is longer lasting than medication, and less resistant to additional courses than medication (Wampold, 2007). Moreover, meta-analyses fail to find consistent differences among different treatments, in general and for specific disorders (Benish, Imel, & Wampold, 2008; Imel, Wampold, Miller, & Fleming, 2008; Leichsenring & Leibling, 2003; Miller, Wampold, & Varhelyi, 2008; Spielmans, Pasek, & McFall, 2007; Wampold et al., 1997).

Evidence from recent naturalistic studies involving very large samples also fails to support Eysenck's claims. First, the effec-

tiveness of psychotherapy in clinical settings, including psychodynamic, humanistic, and whatever else therapists use in practice, has been well established (Minami et al., 2008, 2009; Stiles, Barkham, Mellor-Clark, & Connell, 2008; Stiles, Barkham, Twigg, Mellor-Clark, & Cooper, 2006); indeed, clinicians in practice meet or exceed benchmarks achieved in clinical trials of evidence-based treatments (Minami et al., 2008). Eysenck's claims that "there thus appears to be an inverse correlation between recovery and psychotherapy" (Eysenck, p. 322) is not consistent with the evidence that although there is variability in the length of treatment, patients undergoing relatively lengthy treatment generally make steady progress and terminate when they approach the normal range (Baldwin, Berkeljon, Atkins, Olsen, & Nielsen, 2009).

The fact that psychotherapy is an accepted practice in the medical systems of most western countries is due in large part to the proliferation of clinical trials demonstrating the efficacy of this healing practice. Although it appears that Eysenck was not correct in his conclusion that psychotherapy was ineffective, and even perhaps harmful, the controversy ignited by Eysenck's claims motivated researchers to conduct trials necessary to test this conjecture—and for that, the field should be grateful. Arguably, these trials would have been conducted without the controversy, but there is nothing as motivating to a scientist as a good controversy.

Improving the Quality of Psychotherapy Research

Strupp, in his attempt to rebut—or at least criticize—Eysenck, expressed many insights into ways in which psychotherapy research could be improved, some of which the field has embraced, and several others that remain unsettled, but important.

Process, outcome, and theory. Strupp (1963) was critical of the ambiguity of the independent variable in outcome research: "What is 'treatment'? It seems to me that we shall not be satisfied with studies of therapeutic outcomes until we succeed in becoming more explicit about the independent variable" (p. 2). Essentially, Strupp was suggesting that a full description be made about what constitutes any particular type of "psychotherapy." It took 16 years for Aaron Beck and colleagues to produce a *manual* of psychotherapy—specifically, a manual of how to deliver cognitive-behavior treatment (CBT) for depression (Beck, Rush, Shaw, & Emery, 1979). A treatment manual contains "a definitive description of the principles and techniques of [the] psychotherapy, . . . [and] a clear statement of the operation the therapist is supposed to perform" (Kiesler, 1994, p. 295). Presumably, the treatment manual "standardized" the independent variable, and thus it was no longer a "fuzzy X" (Cook & Campbell, 1979). The proliferation of treatment manuals was described as a "small revolution" by Luborsky and DeRubeis (1984), and Kiesler noted that "the treatment manual requirement chiseled permanently into the edifice of psychotherapy research the basic canon of standardization" (1994, p. 145).

As much as the manual has been (appropriately) lauded as a revolutionary force in psychotherapy research, it does not guarantee that the treatment is standardized. Standardization requires therapist adherence to the treatment manual (Waltz, Addis, Kerner, & Jacobson, 1993), and clinical trials routinely need to demonstrate adequate adherence to be published. However, one troublesome aspect of this process—a key aspect to say the least—is that adherence to the treatment manual does not appear to

be related to outcome (Wampold, 2001b; Webb, DeRubeis, & Barber, 2010). That is, cases in which adherence to the manual was relatively high do not produce better outcomes, suggesting that what is standardized may not be the critical therapeutic ingredient (Wampold, 1997, 2007). Moreover, it also appears that the patient as well as the therapist contributes to adherence (Dennhag, Connolly Gibbons, Barber, Gallop, & Crits-Christoph, 2012; Imel, Baer, Martino, Ball, & Carroll, 2011), perhaps by the fact that compliant patients allow the therapist to use the manual as intended, whereas therapists of more difficult patients have difficulty using the manual. Or alternatively, the therapist may choose to deviate from the protocol with more difficult patients to respond to the patient's needs. In any event, it appears that adherence results from the interpersonal process involving mutual influence of patient and therapist, rather than something that is characteristic of the therapist.

But Strupp (1963) had in mind more than simply specifying the treatment:

It is this realization, I believe, which in recent years has caused investigators in the area of psychotherapy to lose interest in "simplistic" (Luborsky's term) outcome studies of the kind we have been discussing and turned them to sustained research on the psychotherapeutic process itself. Nevertheless, it seems to me, we shall again and again return, armed with more specific data, to the problem of outcome, no matter how arbitrary an end point it may represent (p. 5) . . . This discussion and the following paragraphs underscore the interdependence of "process" and "outcome" research and the importance of predictions at the beginning and throughout therapy (p. 8).

Packed in this statement are two critical ideas. First, it is not sufficient to look simply at the outcome of psychotherapy—we have to understand how psychotherapy leads to change. Apparently, despite decades of process research, we are left with the same issue, as noted by Alan Kazdin, eminent psychotherapy researcher:

Meta-analyses and narrative reviews of well-controlled studies have indicated that many forms of psychotherapy for children, adolescents, and adults lead to therapeutic change (e.g., Kazdin & Weisz, 2003; Lambert, 2004; Nathan & Gorman, 2007) . . . Arguably the most pressing question is how therapy leads to change. Currently, we do not know the reasons, although many ideas have been proposed . . . and fresh approaches are needed in conceptualization and research design. (Kazdin, 2009, p. 418)

The second point made by Strupp (1963) was that outcome is preeminent. Unless, process research—or any type of psychotherapy research—is intimately tied to outcome, in the long run it will not improve the quality of mental health services, which surely ought to be overarching goal. In his comment, Strupp also alluded to the problem that the status of a patient at the end of therapy is arbitrary, as the real focus should be placed on post treatment functioning—that is, functioning going forward. The interplay between process and outcome during the course of the therapy has of course been of interest to researchers throughout the history of psychotherapy research. Of interest here is Greenberg's discussion of small changes during the course of therapy (little os), leading to grander and more global change (big Os) (Greenberg, 1986). The focus on the interplay of process and outcome unfolding over time

is just beginning to take advantage of sophisticated longitudinal models to examine the relations between processes and outcomes (e.g., Falkenström, Granström, & Holmqvist, 2012; Hoffart, Borge, Sexton, Clark, & Wampold, 2012).

Interestingly, both Strupp and Kazdin focused on a critical element: Theory. According to Strupp (1963), a treatment's "claim to existence, survival, and development rests on the establishment of a large number of empirical, highly predictable relationships among key variables which are based on a coherent theory of demonstrable utility, that is, a theory which accounts for highly predictable *and* measurable therapeutic gains" (p. 5). And according to Kazdin (2009), the answer to how psychotherapy creates change "may involve basic psychological processes (e.g., memory, learning, information processing) or a broader theory (e.g., motivation). What is needed further is greater specificity in conceptualizing not only the critical construct but also how that operates to produce symptom change" (p. 423). Simply said, understanding change in psychotherapy is desperately seeking theory, and, somewhat controversially said, *not* psychotherapy theory. Yes, we have theories, indeed many theories, of how psychotherapy works: cognitive, behavioral, psychodynamic, humanistic, experiential, interpersonal, and many variations of each, with more developed each day—new waves following dissipating old waves. But do they explain how people change, in therapy and outside of therapy? Kazdin seems to be pointing to theory involving "basic psychological processes," but I would like to see us expand beyond psychological processes to include evolutionary theory, sociology, anthropology, cultural psychiatry, behavioral economics, as well as psychology (primarily, in my mind, social psychology). I have proposed one such model (see Imel & Wampold, 2008; Wampold, 2001b, 2007, *in press*; Wampold & Budge, 2012; Wampold, Imel, Bhati, & Johnson Jennings, 2006), which surely is not the "true" one, in the Popperian sense.

What is the outcome? And what are we treating? Strupp (1963) addressed both the issue of the nature of the disorder being treated and how the benefits of any treatment are gauged:

What is meant by "outcome"? In Eysenck's review and in many of the studies on which it is based, the term is used in extremely loose fashion. Eysenck himself treats neurosis in analogy to a form of physical illness, which allegedly one may contract at one time or another during one's lifetime, which seems to run an almost self-limiting course, and from which the patient somehow recovers through therapy or spontaneously. Anyone having the slightest familiarity with psychopathology and psychodynamics knows how erroneous and misleading such a conception is . . . It must be conceded that irrespective of our conception of neurosis or mental disorder, there is such a thing as outcome from therapy. But what kind of criterion is it? (p. 5).

To understand the exchange between Strupp (1963) and Eysenck (1964), one must keep in mind the time period. The condition about which Strupp and Eysenck wrote was neurosis, an ill-defined term that referred to a nonpsychotic "nervous" disorder. The *Diagnostic and Statistical Manual (DSM)* in use at the time was the *DSM I*, published in 1952 and largely based on psychodynamic theory (Grob, 1991). The outcome, as mentioned previously, was improvement: cured, improved, slightly improved, or not improved, usually based on the judgment of the clinician.

The issue of the disorder being treated raises many issues that are at the heart of the “outcome” issue about which Strupp and Eysenck were so impolitely discussing. For most of the history of psychotherapy, the focus was on the therapeutic approach, which, with some adjustments, would apply to all disorders, or at least to most disorders. Psychoanalysis was used to treat psychosis as well as neurosis, and Carl Rogers was known for working with clients regardless of the disorder. At the origins of behavior therapy, a medical model of disease was rejected and along with it the notion of diagnoses (Fishman & Franks, 1992). It was the Empirically Supported Treatment (EST) movement that spawned an emphasis on treatments for disorders. The task force that started the movement argued, “If clinical psychology is to survive in this heyday of biological psychiatry, APA must act to emphasize the strength of what we have to offer—a variety of psychotherapies of proven efficacy” (Task Force on Promotion and Dissemination of Psychological Procedures, 1995, p. 3). Having chosen to compete in the medical context, it was now necessary to adopt the official nosology of psychiatry. Indeed, the same task force impugned the results of the meta-analyses of Smith and Glass (Smith & Glass, 1977; Smith et al., 1980) because of the disorder issue: “Although some of these studies [namely, the studies in the Smith and Glass meta-analysis] did focus on clinical problems, the review predated the 1980 arrival of *DSM-III*, which represented a major advance in the reliable categorization of clinical disorders” (p. 1). The term EST has been transformed to evidence-based treatments (EBTs) or research-supported psychological treatments; nevertheless, the focus is on treatment for *particular* disorders, as is clear from the Society of Clinical Psychology’s Web site on research-supported psychological treatments (<http://www.div12.org/Psychological-Treatments/index.html>).

The focus on treatments for disorders raises some important issues, many of which Strupp (1963) foresaw. The most apparent issue is that our research efforts are tied to a nosology whose validity is suspect (see, e.g., Follette & Houts, 1996; Wakefield, 1992, 1999; Widiger & Trull, 2007). A question is raised, as alluded to in the EST task force report, as to whether, when the nosology changes, research on particular disorders is to be disregarded because the disorder is no longer recognized (or perhaps even if the criteria for the disorder changes). What will happen to all the informative research on the treatment of personality disorders when DSM V radically changes these diagnoses, as it will surely do? Are dialectic behavior therapy, mentalization-based treatment, schema-focused therapy, and transference-focused therapy no longer research-supported psychological treatments because the borderline personality disorder will be reconceptualized? Is the debate between Strupp and Eysenck moot because neurosis is not listed in the current DSM?

More distressing to me is that the focus on disorders suggests that seeking therapy for distress, or god-forbid, personal growth, is not a legitimate reason to utilize psychotherapy, if the client does not meet diagnostic criteria for a disorder, even though the criteria change over various versions of the DSM, and the validity of the DSM is suspect. As most clinicians know, we cannot be reimbursed for working with a couple with marital distress, say, caused by infidelity (unless of course, one of the partners meets DSM diagnosis covered by the insurer and the insurer is billed for individual therapy). It is worth repeating Strupp’s (1963) contention: “Eysenck himself treats neurosis in analogy to a form of

physical illness, which allegedly one may contract at one time or another during one’s lifetime, which seems to run an almost self-limiting course, and from which the patient somehow recovers through therapy or spontaneously” (p. 5).

Strupp raised a related point: “For the moment, it must be conceded that irrespective of our conception of neurosis or mental disorder, there is such a thing as outcome from therapy. But, what kind of criterion is it?” (p. 5).

The question seems to have been answered, although unsatisfactorily, as the corollary to the focus on treatments for particular disorders is that of paramount importance, as the objective of psychotherapy, is the reduction of disorder-specific symptoms. We see increasingly a focus on “targeted” symptoms as the test of the effectiveness of a treatment, forgetting that other domains, such as quality of life, role functioning, and well-being, are often what bring patients to therapy. Consider for example, McDonagh et al. (2005), who compared CBT, present-centered therapy, and a wait-list control group and found that although the two treatments were superior to no treatment in terms of targeted measures, “neither treatment was superior to [wait-list] in reducing symptoms of depression, dissociation, and anger or hostility, nor in improving quality of life” (p. 520). Perhaps, treatments focused on the symptoms of particular disorders are particularly effective at what they are intended to do—reduce symptomatology. This is a point not lost on advocates of psychodynamic therapy:

The goals of psychodynamic therapy include, but extend beyond, alleviation of acute symptoms. Psychological health is not merely the absence of symptoms; it is the positive presence of inner capacities and resources that allow people to live life with a greater sense of freedom and possibility (Shedler, 2010, p. 105).

As attractive as that appears, as we shall see, such efforts can be “ugly,” as discussed later.

From my perspective, Eysenck and Strupp struck close to the core of issues about psychotherapy and psychotherapy research. Disputes about the nature of phenomena are critical to scientific progress, as long as the participants are searching for truth by performing severe tests that can falsify conjectures (Miller, 1994; Popper, 1963). Unfortunately, Eysenck and Strupp were not disinterested scientific observers, which leads to the “bad.”

The Bad

Strupp and Eysenck, under the (transparent) guise of scientific discourse, were promoting competing “brands” of psychotherapy, psychoanalytic and behavioral. That is, in today’s parlance, they had an agenda.

As we have seen, Strupp (1963) criticized Eysenck’s claims about the efficacy of psychotherapy (Eysenck, 1952, 1961, 1966) and suggested many insightful ways that research might proceed to illuminate how psychotherapy leads to change. He freely gave some observations and solutions:

It appears that in the furor for “easy” quantifications, we have largely lost sight of the rich potentialities for research in the transference situation, which unquestionably represents the greatest single methodological discovery for interpersonal research in the 20th century (pp. 1) . . . These recommendations, which need to be spelled out in much greater detail before they can be translated into research operations, are in keeping with my conviction that the transference situ-

ation, as defined by Freud, is the richest source for observing and studying interpersonal data, and that it has a unique validity of its own. Nowhere else is it possible to study interpersonal processes as systematically, intensively, deeply, and with as much control over extraneous influences (pp. 13).

It is often forgotten that Eysenck's claims were not simply that psychotherapy was not effective, but that an alternative method—behavior therapy—was. His conclusion (1961) was that

Neurotic patients treated by means of psychotherapeutic procedures based on learning theory improve significantly more quickly than do patients treated by means of psychoanalytic or eclectic psychotherapy, or not treated by psychotherapy at all . . . It would appear advisable, therefore, to discard the psychoanalytic model, which both on the theoretical and practical plain fails to be useful in mediating verifiable predictions, and to adopt, provisionally, at least, the learning theory model, which, to date, appears to be much more promising theoretically and also with regard to application (pp. 720–721).

It is not the conjectures made by Eysenck and Strupp that are objectionable. On the contrary, science should consider all conjectures, no matter how outlandish they may be, as long as they can be put to the severest test (Miller, 1994; Popper, 1963). What is objectionable is that they put the “opponent” to a purported severe test, without doing anything near to that for their own positions. To put it simply, Strupp criticized Eysenck because he believed in the psychoanalytic method. On the other hand, Eysenck threw down the gauntlet to psychoanalytic theory, while at the same time relying on fragile evidence that would not have survived a fraction of the scrutiny he applied to psychotherapy (see Glass & Kliegl, 1983; Wampold, 2001b). This approach to science is “bad”; rather we should place our own theories to the severest test and be as much or more critical of our own evidence than of others.

The Ugly

Perhaps there are readers who have the same anxiety as I have—to have work evaluated, not by what is known and what is considered rigorous currently, but by what will be known and what will be considered to be appropriate in the future. In every publication, researchers make claims. Sometimes these claims stand the test of time; sometimes not. And of course, it is relatively easy to judge researchers from the past by today's standards. Nevertheless, this is the process of science—some claims whither under the intense scrutiny that science provides (and by the way, this is better, I should think than claims ignored and left to die in a solitary status). And thus, there are a few aspects of Eysenck's and Strupp's claims that, in retrospect, would be classified, at least by some, as “ugly.”

Defining Positive Functioning

First, alluded to earlier, there is a debate about what constitutes the proper outcome of psychotherapy. On the one hand, there is symptom reduction, which, quite rightly, has its limitations. On the other hand, there is an effort to create well-being, happiness, self-actualization, marital satisfaction, and improved role function (i.e., positive aspects of life). If we are to use the latter in psychotherapy research, we must define what is positive life functioning. Defining what is positive is fraught with issues.

Strupp (1963) described what the outcomes of psychotherapy should be, patterned after Knight (1941), on whom he stated he could not improve. The criteria fell into three classes. First, there was the “disappearance of presenting symptoms” (p. 9), which is not controversial and has been discussed above. Second is the “real improvement in mental functioning” (p. 9), which consists of the following:

- a. The acquisition of insight, intellectual and emotional, into the childhood sources of conflict, the part played by precipitating and other reality factors, and the methods of defense against anxiety that have produced the type of personality and the specific character of the morbid process;
- b. Development of tolerance, without anxiety, of the instinctual drives;
- c. Development of ability to accept one's self objectively, with a good appraisal of elements of strength and weakness;
- d. Attainment of relative freedom from enervating tensions and talent-crippling inhibitions;
- e. Release of the aggressive energies needed for self-preservation, achievement, competition, and protection of one's rights (pp. 9–10).

In this second class, it is easy to see that the criteria are saturated with psychodynamic constructs, although there are aspects of other treatments, not at the time developed (e.g., acceptance and commitment therapy), and a focus on anxiety reduction. This raises the issue of whether there are therapy approach-specific outcomes that are not germane to other treatments. Although on that basis, or on other bases, one might quibble with these criteria, they are not “ugly.”

The criteria in the third, and final, class involved “improved reality adjustment” (p. 10). The first three criteria were as follows:

- a. More consistent and loyal interpersonal relationships with well-chosen objects;
- b. Free functioning of abilities in productive work;
- c. Improved sublimation in recreation and avocations (p. 10).

Again, the criteria are reasonable given a psychodynamic perspective. It is the final criterion that fails miserably and demonstrates the perils of stipulating what is normal: “Full heterosexual functioning with potency and pleasure” (p. 10). Of course, current mores applied retroactively produce harsh criticism. However, for me, this criterion, to repeat myself, while so painfully wrong, is ugly because it sets out to define what is healthy. And for this reason, I am somewhat uncomfortable with (a) establishing “normal ranges” of any instrument (i.e., defining what is normal for an individual, a problematic endeavor since the British empiricists initiated this idea), (b) positive psychology, as an area of inquiry, (c) stipulating symptom removal as the primary goal of therapy unless this is the goal of the patient (without coercion of the therapist), and (d) charismatic healers, life coaches, politicians, or any other attempts to define what is desirable for others in life. On the other hand, I am uncomfortable restricting psychotherapy to the single purpose of remediating distress, rather than for changes that improve the quality of life.

This discussion raises the question about how we should define outcome? Well, fortunately instruments used to assess psychotherapy all measure three or four latent constructs: distress (i.e., symptoms), well-being, destructive habits (e.g., substance abuse), externalizing behavior (e.g., antisocial behavior), and social and work role functioning, so the particular choice of instruments is not as controversial as it might appear. Nevertheless, broadband assessment is preferred.

And now I turn to Eysenck's ugly.

Fierce Resistance to Evidence

Eysenck famously wrote in his 1990 autobiography *Rebel with a Cause*, "I always felt that a scientist owes the world only one thing, and that is the truth as he sees it. If the truth contradicts deeply held beliefs, that is too bad. Tact and diplomacy are fine in international relations, in politics, perhaps even in business; in science only one thing matters, and that is the facts" (Eysenck, 1990, p. 229). It is a shame that this statement was so disingenuous, as Eysenck never could adjust to evidence that many psychotherapies are effective, even those with psychodynamic bases, in clinical trials and in practice (Benish et al., 2008; Imel et al., 2008; Leichsenring & Leibing, 2003; Leichsenring, Rabung, & Leibing, 2004; Luborsky et al., 1975; Shedler, 2010; Smith & Glass, 1977; Smith et al., 1980; Stiles et al., 2006, 2008; Wampold et al., 1997; Westen, 1998). Indeed, a detailed analysis of Eysenck's own data revealed that psychotherapy was remarkably effective (McNeilly & Howard, 1991).

When Strupp's inaugural article was published in 1963, as we have seen, the debate could be parsed as one between promoters of psychoanalytic psychotherapy and promoters of behavior therapy, with each finding whatever evidence they could to support their cause, to use Eysenck's term. As I argued earlier, Eysenck changed the debate toward one with evidence as the primary warrant, yet he had a penchant for vociferous attacks on anyone who raised issues with his evidence, calling Strupp's comments, as I indicated earlier "irrelevant, incompetent and immaterial" (1964, p. 97). As discussed earlier, Smith, Glass, and colleagues (Smith & Glass, 1977; Smith et al., 1980) took up Eysenck's challenge to use evidence, applied meta-analysis as a means of synthesizing this evidence, and came to a very different conclusion than Eysenck. Not unexpectedly, Eysenck fired back:

A mass of reports—good, bad, and indifferent—are fed into the computer in the hope that people will cease caring about the quality of the material on which the conclusions are based. If their abandonment of scholarship were to be taken, seriously, a daunting, but improbable, likelihood, it would mark the beginning of a passage into the dark age of scientific psychology. (Eysenck, 1978, p. 517)

Eysenck, referring to the Smith and Glass (1977) article, expressed his aspirations: "This article, it is to be hoped, is the final death rattle [of the notion that] one can distill scientific knowledge from a compilation of studies" (1978, p. 517). The noise was not a death rattle at all, but on the contrary the heralding of a new age. Glass and colleagues (Glass & Kliegl, 1983; Glass & Smith, 1978) addressed the issues raised by Eysenck and even apologized, albeit in a sardonic manner, for having made life difficult for those who adhered to a position, despite evidence, and attacked method because they disliked the evidence it produced, in an article enti-

tled, "An apology for research integration in the study of psychotherapy" (Glass & Kliegl, 1983). Anyone interested in "the outcome problem in psychotherapy" should read the meta-analytic work of Glass and colleagues (viz., Glass & Kliegl, 1983; Smith & Glass, 1977; Smith et al., 1980).

As mentioned earlier, the meta-analytic results of the Smith and Glass meta-analyses have been put to the severest test and survived. Indeed, the initial publication in 1977 (viz., Smith and Glass) has been cited approximately 1000 times, according to the Web of Knowledge. Moreover, meta-analyses is the method of choice to synthesize research in the most respected scientific disciplines. Despite the acceptance of the meta-analytic method, the results of the numerous reanalyses and additional meta-analyses, which corroborated the Smith and Glass findings, and his proclamation in his autobiography, Eysenck continued to dispute the evidence and the method (see Eysenck, 1984, 1995) until his death in 1997. The truth did contradict Eysenck's deeply held belief that behavior therapy was superior to other therapies and that other therapies were no more effective than "spontaneous remission," yet he held fast to his position nevertheless.

Progress?

At higher order level, what have we learned from the Eysenck and Strupp interchange? And have we progressed? Eysenck most surely motivated the field to consider evidence and examine the results of research to make claims about psychotherapy. And Strupp most surely pointed the way for the study of process. Process and outcome research in psychotherapy has proliferated in the 50 years since this debate, and surely that is a progress, in my view of the world. Without this research, psychotherapy may have faded away in the competitive world of health care systems. Psychotherapy is more widely available to those suffering from mental distress than any other time, and that is, to a large extent, due to the research we have produced.

Evidence prevails—well, let's not believe we have come so far since the rather foul exchange between Eysenck and his colleagues. The primary issue that Eysenck wanted the field to accept was that behavioral treatments are scientific and effective, and all other treatments belong to the scrap heap of pseudoscience. Despite the evidence, there are those who argue that using anything other than CBT is unscientific and unethical—a sort of prescientific medical practice (see, e.g., Baker, McFall, & Shoham, 2008). Whenever evidence is produced that many treatments are effective, and as effective as CBT, that evidence is attacked, in much the same way as Eysenck attacked anyone who should suggest otherwise. See, for example, exchanges about the efficacy of CBT, humanistic, and dynamic therapy in practice (Clark, Fairburn, & Wessely, 2008; Stiles, 2008; Stiles et al., 2006, 2008), the efficacy of psychodynamic therapy (Anestis, Anestis, & Lilienfeld, 2011; Beck & Bhar, 2009; Bhar & Beck, 2009; Bhar et al., 2010; Coyne, Bhar, Pignotti, Tovote, & Beck, 2011; Leichsenring & Leibing, 2003; Leichsenring & Rabung, 2011; Leichsenring et al., 2004; McKay, 2011; Shedler, 2010, 2011; Thombs, Jewett, & Bassel, 2011), and the lack of lack of evidence of treatment differences for posttraumatic stress disorder (PTSD; Benish et al., 2008; Ehlers et al., 2010; Wampold et al., 2010). Debate is an intrinsic element of scientific progress, as it clarifies inconsistencies, reveals flaws, and motivates the production of evidence. But progress results *only* if

evidence trumps arguments in the end. In some very disconcerting ways, the three contentious interchanges (viz., practice outcomes, psychodynamic efficacy, and PTSD treatments) are reminiscent of the Eysenck era debates, and not simply in their tone. The *conjectures*, at their hard core, have not changed—there are treatments based on scientific psychology that are superior to others. Eysenck made the claim and the same claim, dressed a bit differently, is still being made (e.g., Baker et al., 2008). The issue, in the words of Stephen Jay Gould, is whether “‘scientific truth’ . . . represents a social construction invented by scientists (whether consciously or not) as a device to justify their hegemony over the study of nature” (Gould, 2000, p. 253) or whether what is known is informed by the evidence? I have an opinion, but what is needed is evidence, not opinion, so I shall refrain and simply state: Evidence is dead; long live evidence.

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