

## **The Two Cultures of Chemical Dependence Treatment: Time for Reconciliation?**

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The field of alcoholism and other chemical dependencies is characterized by a split between two cultures. On the one hand, there is the extant treatment and recovery culture—a so-called “traditionalist” culture that has grown largely out of the observations and experiences by persons in the so-called self-help and recovery movement in America. This culture is based largely on 12 Step ideas formulated in the 1930s by Bill Wilson and Dr. Bob, two pioneers seeking a way out of the maze of active alcoholism. In addition to 12 Step programs such as AA and NA, the culture is also evident in so-called Minnesota Model Treatment and the many variations on this model. It is important to point out that while many treatment programs share basic elements in this approach, it would be a mistake to fail to notice the many differences among programs that utilize 12 Step ideas and procedures. These differences may include differences in the use of psychoactive medications, therapeutic techniques, involvement of family members, treatment of co-occurring disorders, confrontation and so on and so forth. These differences are often overlooked or deliberately ignored by persons unsympathetic to 12 Step recovery approaches.

The other culture, a so-called “revisionist culture,” has grown out of academic and research contexts. This culture claims to be an “evidence-based culture.” For reasons that are not entirely clear, this culture, populated largely by a relatively small number of academics and their students in behavioral and cognitive-behavioral psychology, developed largely in opposition to 12 Step program principles, procedures and observations as well as traditional treatment models. It is interesting to note that members of this culture have been quick to champion any information contrary to traditional thought regardless of its actual scientific merit. It is important to point out from the outset that this split between the two cultures has had important implications for the increasingly important issue of evidence-based treatment. It is also important to point out that this culture could have developed in cooperation with rather than in opposition to the extant traditional treatment and 12 Step models of change and recovery.

In my discussion, I do wish to emphasize from the outset that I am addressing a subculture within the broader research community in alcohol and other chemical dependencies. My comments are not meant to diminish the important role that research has played and will continue to play in the field. Furthermore, my comments should not be taken as a generalized criticism of behavioral science in general, of particular theoretical orientations or of behavioral science researchers in general. It should be obvious that many behavioral scientists have made important contributions to our understanding of alcohol and other chemical dependencies. And it should be equally

obvious that the field needs more research if we are to achieve understanding of these enormously complicated and difficult problems.

In effect, as we approach the topic of scientific research on treatment, it will become apparent that we are necessarily involved in a much broader conversation, one that involves elements of the sociology of knowledge and even the politics of knowledge. When we talk about evidence-based treatment, a number of important questions come to mind.

1. What constitutes scientific evidence?
2. Whose evidence are we talking about?
3. How is evidence generated?
4. How do we come to agree or disagree that a particular piece of information constitutes evidence?
5. What scientific, social and political processes enter into consensus as to what constitutes evidence?
6. What are the implications of evidence-based treatment for the future of traditional treatment models in the chemical dependence field?
7. How did this split between the two cultures come about?
8. How can the split between the two cultures be closed and what benefits might accrue if this were to happen?

In our efforts to gain an understanding of the extent of this conflict between the two cultures of clinical practice and behavioral science research, it is helpful to view it in historical perspective. Perhaps the most significant event in the development of the two cultures was the publication of a study by the British researcher, D. L. Davies (1962). Davies claimed to have found a number of previously diagnosed alcoholics who were able to moderate their drinking. As Dr. Alan Marlatt (1983), the well known alcoholism researcher and revisionist thinker from the University of Washington, put it in an article in the *American Psychologist*, the official journal of the American Psychological Association:

**“Over two decades ago, Davies sent shock waves through the alcoholism field by publishing the results of a long-term follow-up of patients treated for alcoholism at the Maudsley Hospital in London. In his report, Davies challenged the traditional emphasis on total abstinence as the only viable “cure” for alcoholism by showing that of 93 male alcoholics who were followed up for a period of from 7 to 11 years following treatment, seven reported a pattern of normal drinking.” (Marlatt, 1983, p. 1097).**

Unfortunately for Marlatt, two years after he published this article in the *American Psychologist*, another British researcher Griffith Edwards (1985) published a further follow-up of the patients described by D.L. Davies. As Edwards pointed out, of the

seven men Davies categorized as normal drinkers, five were discovered to have resumed destructive drinking. Three of these five destructive drinkers were drinking non-normally even during the original follow-up by Davies who was unaware of this. One of the seven men eventually experienced Wernicke-Korsakoff syndrome, one was hospitalized with peptic ulcer and another experienced liver enlargement as a result of heavy drinking. Three of the seven men also used psychoactive drugs in addition to drinking heavily. Of the seven men only two of the entire sample of 93 men appeared to be drinking non-destructively in Edwards' further follow-up. However, one of these men was never more than slightly dependent on alcohol to begin with. In effect, only one man out of a non-randomly selected sample of 93 men appeared to be drinking non-destructively in Edwards' further follow-up. While Marlatt can be forgiven for publishing his comments two years before Edwards published his disconfirmation of Davies' claims, I find it significant that Marlatt has not, to my knowledge, published a retraction, clarification or apology to members of the American Psychological Association for so misinforming them.

The Davies study set the stage for the terms of the debate that followed on the heels of its publication, i.e., science has shown that "common sense" and the received wisdom among clinicians in the field and recovering persons in the community are wrong and any disagreement with scientific findings is based upon emotion, ideology and refusal to accept the truth. This theme has been sounded repeatedly throughout subsequent controversies (e.g. Searles, 1993; Wallace, 1993; Peele, S., 1998; Peele, S., 1990; Wallace, 1989; Wallace; 1990).

It is difficult to maintain the position that the argument is between scientists in possession of the truth and clinicians in the grip of ideology when the history of these controversies is examined in detail. As I hope to make clear, problems of scholarship, misrepresentation and ideology are not one sided in these debates. For example, revisionist psychologists opposed to traditional thought have been so eager to believe Davies' findings that some have continued to cite them without mentioning Edwards' disconfirmation. For example, Maltzman (2008) has reported this passage from a recent abnormal psychology textbook:

**"For decades, the popular wisdom among treatment professionals, especially those committed to the AA approach, was that the only reasonable goal for alcoholics was abstinence, because alcoholics could not control their drinking. Like many strongly held beliefs, this one seemed to be impervious to the influence of data. For example, Davies (1962) found that 7 out of 93 alcoholics in a long-term follow-up study showed a pattern of normal drinking. Granted, that is only 8% of his sample, but it is more than enough to refute the general proposition that no alcoholic is capable of controlled drinking (Raulin, 2003, p. 464).**

Unfortunately, as Maltzman (2008) has pointed out, Raulin is not the only author of a psychology textbook who has chosen to use the controlled drinking issue as the vehicle for lecturing the treatment community about their "popular wisdom" that seems to be "impervious to the influence of data." It is ironic that Raulin criticizes the treatment

community for their failure to appreciate scientific data while using a discredited study as a basis for criticism.

We'll have more to say about the issue of controlled drinking shortly since it is perhaps the primary wedge issue between the two cultures. Let's, for the moment, examine further evidence for the development of a culture in opposition to so-called traditional thought in alcoholism and other chemical dependence.

Rohan (1982) and Pattison and Kaufman (1982) as well as Pattison, Sobell and Sobell (1978) go to the heart of the matter and criticize the very notion of alcoholism. These revisionist authors voiced their criticism of "traditionalist's thinking" by rejecting the notion that there is a unitary phenomenon that can be identified as alcoholism.. Furthermore, they expressed their disbelief that alcoholics are essentially different from non-alcoholics. They do not agree that alcoholics have lost control over their drinking and expressed their opinion that alcoholism is a temporary, reversible 'syndrome' rather than a permanent, irreversible disease." (Wallace, 1989, p.21).

First, it is odd that these authors had chosen to attack traditional thinking by rejecting the concept of alcoholism as a unitary phenomenon that can be identified as alcoholism. Traditional thought about the need for addressing both similarities and differences among treatment populations had been apparent for a number of years. For example, the great alcoholism researcher, E. M. Jellinek (1960), recognized the diversity among persons with drinking problems and discussed the need for a typology in his categories of alpha, beta, epsilon, gamma and delta alcoholism. Furthermore, research had shown that alcoholism was more reliably diagnosed by psychiatrists than any other psychiatric diagnosis (Heltzer, et al, (1981). In commenting on the development of DSM-III, Robins(1982, p. 53) observed that:

**“It is remarkable, given these logical difficulties, the assembling of symptoms from grossly different conceptual realms, and the reported unreliability of alcoholics as historians, that the diagnosis of alcoholism by symptom self-report is repeatedly found to be one of the most valid and reliable of the psychiatric diagnoses.” The question that remains unanswered by critics such as Rohan, Pattison, Kaufman, M. Sobell and L. Sobell is how could alcoholism be one of the most reliable, if not the most reliable, of the psychiatric diagnoses if there were no important similarities among persons in this population of persons with drinking problems?”**

A further attack on traditional concepts was mounted by the revisionist Herbert Fingarette (1988). In a polemic against the disease concept, Fingarette went after Jellinek's attempt to demonstrate a uniform pattern of the progression of alcoholism. According to Fingarette, Jellinek's formulation of the progression of alcoholism was scientifically inadequate because it was based on just 98 returned questionnaires from a sample of 1,600 questionnaires distributed to AA members. What Fingarette either failed to mention or failed to appreciate was that Jellinek (1946) had first studied a small sample of AA members but Jellinek recognized the limitations of this initial, exploratory study. In a subsequent study, Jellinek (1952) obtained results on a sample of more than 2000

AA members. As Maltzman (2000) pointed out, this error did not stop with Fingarette, but was repeated in various writings by a number of revisionists (Lilenfeld, 1995; Mendelson and Mello, 1985). Maltzman (2008) reports how an abnormal psychology textbook by Barlow and Durand (2005) not only perpetuates this inappropriate criticism but teaches it to undergraduate majors in psychology as fact.

**“In 1945, the newly formed self-help organization , Alcoholics Anonymous (AA), sent out some 1600 surveys to its members asking them to describe symptoms related to drinking, such as feelings of guilt or remorse, and rationalizations about their actions and to note when these reactions first occurred. Only 98 of the almost 1600 surveys were returned, however. As you know such a small response could seriously affect data interpretation. Obviously, a group of 98 may be very different from the group as a whole, so they may not represent the typical person with alcohol problems” (Maltzman, 2008; Barlow and Durand (2005).**

Barlow and Durand point out the inadequacies of conclusions based on a sample of only 98 responders to a questionnaire from an initial sample of 1,600, but, like Fingarette, they fail to acknowledge that Jellinek’s (1952) conclusions were also based on data generated from a new sample of more than 2000 persons.

It is curious how errors like this manage get into the literature and once in, spread. Perhaps the more interesting question is why do they persist and stubbornly resist the light of truth? One begins to suspect that elements of the “sociology of knowledge” are at play in matters such as this rather than the transmission of reliable scientific information.

While errors concerning the nature of alcoholism and the progression of the disease are interesting, the social transmission of misinformation is most noticeable in revisionists’ views on abstinence.

The following quote from Dr. Alan Marlatt taken from his article in the American Psychologist reveals the rather astonishing misconceptions of some revisionists with regard to traditionalist thought concerning the reasons for emphasis on abstinence:

**“To some observers, the diagnosis of alcoholism carries the moral stigma of a new scarlet letter. Such critics argue that the contemporary disease model of alcoholism is little more than the old ‘moral model’ (drinking as a sinful behavior) dressed up in sheep’s clothing (or at least in a white coat). Despite the fact that the basic tenants of the disease model have yet to be verified scientifically (e.g., the physiological basis of the disease and its primary symptom, loss of control), and even though there is a lack of empirical support for the effectiveness of any particular form of alcoholism treatment (including inpatient programs geared toward abstinence), advocates of the disease model continue to insist that alcoholism is a unitary disorder, a progressive disease that can be temporarily arrested by total abstention. From this viewpoint, alcohol for the abstinent alcoholic symbolizes the forbidden fruit (a fermented apple?), and a lapse from abstinence is tantamount to a fall from grace in the eyes of God. Clearly, one bite of the forbidden fruit is**

**sufficient to be expelled from paradise. Anyone who suggests controlled drinking is branded as an agent of the devil, tempting the naïve alcoholic back into the sin of drinking. If drinking is a sin, the only solution is salvation, a surrendering of personal control to a higher power.” (Marlatt, 1983, p.1107).**

Dr. Stanton Peele echoes Dr. Marlatt’s sentiments and shows his lack of understanding of Alcoholics Anonymous in the following quote:

**“AA preaches a doctrine of total redemption, teetotaling forever. And many a former alcoholic believes that a single drink will send him on the short, slippery slope to alcoholic hell. It’s true that for some alcoholics who have been uncontrolled drinkers for many years and whose health has deteriorated, the option of moderation is no longer workable. However, the resolution never to have a drink again is not always a cure-all. The vast majority of alcoholics who try to abstain eventually return to the bottle or to another addiction” (Peele, 1985, p. 39).**

It is more than interesting to observe that both Marlatt and Peele misconstrue the nature of AA spirituality and confuse it with religiosity. Peele could not have attended many AA meetings to conclude that AA “preaches” teetotaling forever. AA members do not preach to each other about never drinking again. They do suggest that members take recovery a day at a time. “Can you stay sober today?” is a question one frequently hears as an old timer in the AA program tries to help a newcomer. Nobody is asked to give up drinking in AA for the rest of their lives. As for Marlatt’s misconceptions, AA members do not stay sober because drinking is a sin but because drinking has led eventually to uncontrolled drinking and severe negative consequences concerning health, relationships, mortality, jobs, marriages and families, legal matters and so forth.

Another revisionist thinker, William R. Miller, one of the authors of Motivational Interviewing, is persuaded that the often described mechanisms of denial, rationalization and minimization are simply wrong. According to Miller, there is no scientific evidence that alcoholics and other addicts use such primitive defenses:

**“Central to most U.S. addiction treatment programs from the 1960s until the 1990s was the notion of confrontation. It was assumed, implicitly or explicitly, that people with these disorders were somehow uniquely incapable of comprehending or accepting the nature of their condition, that they were literally unable to see reality.....(Miller, 2001, p x). It was a house of cards. There is not and never has been scientific evidence to support the belief that people with substance use disorders show abnormally high levels of primitive defense mechanisms such as denial. Studies instead reveal a heterogeneity of personality that parallels the general population. Prevention and treatment approaches based on education and confrontation have an abysmal track record when it comes to behavior change. Randomized clinical trials show little benefit from increasing the length or intensity of such treatment” (Miller (2001 p. xii).**

As Maltzman (2008) has pointed out, despite Miller's authoritarian certitude, Miller's scholarship is questionable. Miller is either unaware of, or reluctant to report research (Bromet and Moos (1977); Smart (1978); Finney, et al., 1981); Welte, 1981); McLellan, et al., 1993; Simpson, 1999); Simpson, et al.; 1999; Cavanaugh, 1989) that pointed clearly to the significance of length of treatment in traditional programs. Also, Miller, according to Maltzman, appears to be unaware of research on validated standardized tests for the assessment of denial (Newsome & Ditzler, 1993), the relationship of denial to neuropsychological damage to the prefrontal cortex from alcoholic drinking (Tarter et al., 1984) and the relationship of denial of symptoms by alcoholics to impairment of executive functions such as problem solving and planning (Rinn et al. 2002).

Perhaps the greatest wedge issue that separates the two cultures is the issue of controlled drinking. The early, controversial study by two revisionist psychologists, Linda and Mark Sobell (1973) on hospitalized alcohol dependent subjects at Patton State Hospital in California set the stage for continuation of this wedge issue. The Sobells set up a bar in the hospital. Using simple reward and punishment techniques (rewards for sipping rather than gulping drinks, ordering mixed rather than straight drinks and so forth as well as punishments—electric shock- for gulping rather than sipping drinks, ordering straight rather than mixed drinks) they claimed to have succeeded in producing controlled drinking that persisted in a three year follow-up by Caddy, Addington and Perkins (1976).

According to Caddy, et al, 50% of the patients treated with controlled drinking methods three years before functioned well during the third year of follow-up. These patients were subsequently followed up by Pendery Maltzman and West (1982). While 50% of the Sobell patients were said to be "functioning well" during the 3<sup>rd</sup> year of follow-up, Pendery, Maltzman and West reached a very different conclusion. Here are brief descriptions of the **six highest functioning "controlled drinkers"** identified by Caddy, et al (1976) during the 3<sup>rd</sup> year follow-up of the Sobell patients as reported by Pendery, et al. (1982) in their further examination of these patients.

**#1. Patient reports that he drank heavily during 3<sup>rd</sup> year follow-up. He had an alcohol-related felony in year 2. In year 3, police were called because he created a disturbance and threatened violence while drunk. He was too drunk to attend his brother's funeral in April of year 3. In year 4, he was arrested for drunk driving and re-hospitalized.**

**#2. Patient reports that his 3<sup>rd</sup> year included some of his worst drinking experiences. In August of the 3<sup>rd</sup> year, after drinking more than a fifth of liquor a day and having shakes and other withdrawal symptoms, he went to an alcoholism service for help. An M.D. told him that he had cirrhosis of the liver.**

**#3. Patient and collateral stated that year 3 was his worst year. He spent time in jail, in a state hospital and in a VA hospital because of actions while intoxicated. At the end of year 3, he stated he had a DWI as well as arrests for other reasons.**

**#4. Patient reported he was abstinent throughout year 3 as a result of additional incarcerations in hospitals, jail and road camp in the previous 2 years. He also spent year 2 at an AA oriented recovery home to which he attributed his abstinence.**

**#5. Patient and multiple collaterals reported that he was drinking as much as a fifth of liquor a day and some beer when he was not working. He had a blood alcohol level of 0.34 on an admission to a hospital during his 3<sup>rd</sup> year after treatment.**

**#6. Patient and collateral did report that patient felt that he successfully controlled his drinking during the 3<sup>rd</sup> year but “it would not be entirely accurate to say I never drank excessively.”**

It is difficult to understand why Caddy, Addington and Perkins described these six patients who had been treated previously by Sobell and Sobell as high functioning. “successful controlled drinkers” during the 3<sup>rd</sup> year of their follow-up. At least five of these six “highest functioning” controlled drinking subjects could hardly be described as “successful.” The reports of these patients with regard to their drinking and behavior during 3<sup>rd</sup> year after treatment clearly pointed to dramatic failures of the controlled drinking treatment procedures used by Sobell and Sobell.

How to explain these discrepancies? Maltzman has made it clear that he believed (and still believes) that the Sobells as well as Caddy and colleagues committed scientific fraud and he presented scientific justification for his belief. This set off a fire storm of controversy with revisionists rushing to the defense of the Sobells. For a complete discussion of Maltzman’s position on this issue, including his refutations of the arguments advanced by the defenders of the Sobells, see Maltzman’s recent texts (Maltzman, 1999; Maltzman, 2008). Given the rather dramatic failure of independent investigators to confirm the results obtained by Sobell and Sobell in their 1973 experiment, it is difficult to understand why other psychologists would chose to defend the position that controlled drinking is a viable goal for hospitalized alcoholics. For example, Davison et al. (2004), however, have chosen to do so in their abnormal psychology text book chose to do so:

**“The term controlled drinking was introduced into the domain of alcohol treatment by the Sobells (Sobell & Sobell, 1993). It refers to a pattern of alcohol consumption that is moderate, avoiding the extremes of total abstinence and inebriation. Findings of one well-known treatment program suggested that at least some alcohol abusers can learn to control their drinking and improve other aspects of their lives as well (Sobell and Sobell, 1976). (Davison, et al., 2004. p.393).”**

This remark is misleading for a number of reasons. First, the Sobell and Sobell research was not a “treatment program.” It was an experiment. Furthermore, the research did not involve “alcohol abusers.” It involved many hospitalized severely dependent alcoholics, virtually none of whom succeeded at moderating their drinking and its consequences. Finally, at best, only 1 of 20 patients given controlled drinking training in the Sobell research appeared to have been helped at all by the controlled drinking training offered in this research. This patient was considered by Pendery, Maltzman and West to be best

described as an alpha or psychologically dependent drinker who would not meet today's criteria for a diagnosis of alcohol dependence.

The textbook by Davison et al. (2004) is not the only text to misinform undergraduates about the feasibility of controlled drinking for hospitalized alcohol dependent patients. Maltzman (2008) reports on the "misguided discourse" provided by Nolen-Hoeksema (2004) in her abnormal psychology text:

**"The notion that some alcoholics can learn to engage in controlled, social drinking directly clashes with the idea that alcoholism is a biological disease and that, if an alcohol takes even one sip of alcohol, he or she will lose all control and plunge back into full alcoholism. In 1973, Mark and Linda Sobell published one of the first studies showing that a cognitive-behaviorally oriented controlled drinking program can work for alcoholics perhaps even better than a traditional abstinence program. They found that the alcoholics who had had their controlled drinking intervention were significantly less likely than the alcoholics in the abstinence program to relapse into severe drinking, and they were significantly more likely to be functioning well over the two years following treatment "(Nolen-Hoeksema, 2004, p.632).**

As I have pointed out in my discussion of the "six highest functioning" controlled drinking subjects in the Sobell experiment, five of these six patients experienced multiple relapses and devastating consequences of continued drinking in the third year of follow-up.. It is difficult to comprehend how or why Nolen-Hoeksema would perpetuate this falsehood and even more difficult to understand why it would be presented as fact to undergraduates, many of whom may either be suffering from beginning problems with alcohol themselves or come from families suffering from the disease. (As an aside, it is curious why Nolen-Hoeksema would describe the Sobell research as a cognitive-behavioral program since the Sobell research clearly did not involve such treatment).

Quickly following the Sobell research was the controversial so-called Rand Report that claimed to find that 22% of patients who had been treated in 8 NIAAA-funded treatment centers were engaging in controlled drinking. An additional 45% were reported to be abstinent. Moreover, in one of the treatment centers studied, of those patients who were drinking at follow-up, 70% were reported to be drinking normally! In other words, 3 of 4 patients in this particular treatment center were reported to be drinking normally. This remarkably deviant finding alone should have cast doubt on the reliability of the data analyzed by the Rand authors. While controversy stormed over the 22% normal drinkers, it seemed to me that the numbers were really nothing more than an effort by the then director of NIAAA to encourage data analyses that would show that the money spent on treatment was well worth it. A 67% positive outcome rate (45% normal drinking in the 18 month study plus a 22% normal drinking rate) added up to an impressive outcome statistic. These numbers were of course highly inflated by such things as an enormous loss of subjects and invalid quantity and frequency of drinking measures (Wallace, 1978). In a subsequent study, the 4 year study by the same Rand authors yielded sustained long term non-problematic drinking estimates corrected for the error in measurement of quantity and frequency mentioned, yielded an estimate of non-problem drinking of 3-4%

and an estimate of abstinence of sustained abstinence of 7% (Wallace, 1979). Dr. Alan Marlatt reported these long term results very differently:

**“Approximately 18% of the patients were reported to be drinking without problems or symptoms of dependence and less than 10% were able to maintain total abstinence during the four-year period following initial treatment (Marlatt, 1983, p 1101).**

Marlatt’s comparisons that supposedly showed the superiority of non-problem drinking over abstinence were based on vastly differing outcome windows. Marlatt chose to report non-problem drinking rates that were based on a time interval that varied from 1-6 months while the sustained abstinence rate of 10% was based on the entire 4 year outcome interval. Also, Marlatt failed to take into account the Rand author’s admission that there was a 25% underreporting of consumption rate in their subjects. Hence, applying this correction to the sustained non-problem drinking rate yields an estimate of only 3-4%. As with evaluation of all research, evaluation of “evidence- based treatment” studies requires attention to detail, skepticism and criticism. In research evaluation, the devil is always in the details. Such details do not seem to bother writers of psychology textbooks. Maltzman (1999) showed the extent to which text books in psychology can misreport research on alcoholism treatment in his discussion of the introductory text book by McConnell that was published in 1989. McConnell, after reporting that the Rand Report had shown that 22% of the subjects in the Rand/NIAAA study had controlled their drinking, went on to talk about the second study by the Rand authors as follows:

**“If so many patients achieve ‘controlled drinking’ without special training, what would the success rate be if you attempted to teach them cognitive self-control and social skills? Again, NIAAA turned to the Rand Corporation. The second Rand report involved a four-year follow-up of patients trained either from the disease model or the learned behavior perspective. The data showed that the controlled drinking approach achieved significantly better results than did abstinence training (Rand, 1980). (McConnell, 1989, p. 258).**

As Maltzman (1999, p.297) pointed out, **“The above is pure invention. An experiment was not designed and conducted by investigators at the Rand Corporation.”**

In actuality, there was no further experiment designed and conducted by the Rand Corporation. Patients were not taught cognitive self-control and social skills by the Rand investigators in an effort to teach them how to control drink. No comparisons were made between what McConnell referred to as the “disease model” or the “learned behavior perspective.” Rand, in its further study, had simply selected a cohort from the larger sample and continued to follow them for a longer period. As pointed out earlier, there were no fair comparisons that indicated a significantly greater percentage of controlled drinkers over abstinent subjects. One shakes one head in bewilderment as to how such enormous distortions of reality get formulated and transmitted to naïve students and other readers of psychology text books.

In 1986, William Miller and his co-author of *Motivational Interviewing*, Reid Hester, turned their attentions to inpatient programs. In an article in the *American Psychologist*, titled, "Inpatient Alcoholism Treatment. Who Benefits?" In this article, Miller and Hester argue that there are no differences between inpatient and outpatient care except for the fact that inpatient care is far more expensive. Their argument concludes as follows:

**“Given that the only clear significant overall difference between residential and nonresidential alcoholism programs is in the cost of the treatment, it would see prudent for public and private third-party payers to enact policy that deemphasizes the hospitalization model of care where it is nonessential and encourages the use of less expensive but equally effective alternatives. A ‘3 R’s model of treatment (remove from society, repair the problem and replace in society) is outmoded and inadequate as a means for addressing alcohol problems.” (Miller and Hester, 1986, p.803).**

In support of their position, Miller and Hester cite a number of studies purportedly comparing inpatient and outpatient. Unfortunately, the majority of these studies involved programs with very high relapse rates whether people were treated as inpatients or outpatients. For example, Mosher, et. al (1975) found relapse rates of 82% for outpatients and 77% for inpatients. Wilson, et. al (1978) reported relapse rates of 74% for both outpatients and inpatients. Pittman and Tate (1972) observed relapse rates of 71% for outpatients and 78% for inpatients. Given such high relapse rates, it is probably more accurate to state that the particular outpatient and inpatient programs cited by Miller and Hester were equally ineffective and not equally effective. Moreover, since it is a well known fact that patients receiving inpatient treatment are more impaired on several dimensions than patients receiving outpatient (for example, severity of psychiatric problems), comparisons between the two treatment contexts can be highly misleading. In the absence of careful matching or strictly random assignment (neither of which characterized the literature cited by Miller and Hester), the failure to find differences is meaningless. All that can be said in this case is that inpatient worked this well with a difficult population and outpatient worked that well with a less difficult patient population. As Harrison et al., (1991) conclude:

**“Studies that compare intake characteristics of inpatients and outpatients consistently report among inpatients a higher prevalence of factors generally associated with a poorer prognosis. In spite of initial differences showing inpatients to be sicker than outpatients, nonrandomized studies consistently show no differences in outcome at follow-up...But similar recovery rates for inpatients and outpatients do not prove that the treatments they receive are equally effective. Similar outcomes for the two groups may mean merely that less impaired drug and alcohol abusers respond to outpatient treatment about as well as more impaired drug and alcohol abusers respond to inpatient treatment. No conclusions about the relative efficacy of the treatments can be drawn from such studies, unless patient subgroups are matched at intake characteristics and analyses conducted for interactions between patient variables and treatment type. None of the studies reviewed reported such analyses (p1168).**

Among the studies cited by Miller and Hester in support of their contention that inpatient and outpatient are equally effective but different in cost was the often cited British study by Edwards and Orford in which brief advice was found to be just as effective as intensive treatments (It should be noted that this study was not a comparison of inpatient and outpatient but, for the most part, a comparison of brief advice to outpatient treatment. The problem with the Edwards and Orford study was simply that using abstinence as the outcome statistic, the majority of men given advice or treatment drank within a few weeks of the initial consultation. By the first year, all but 8 of 95 men had drunk. By the second year followup, only 2 men remained whose wives reported no drinking since the initial consultation but these 2 men returned to drinking shortly thereafter. It is curious that Miller and Hester did not report these results but merely cited the study as showing that brief advice was just as effective as intensive treatment. As Wallace (1989) pointed out:

**“Rather than showing that a single, brief outpatient session of advice was just as good as intensive long-term inpatient treatment, this “classic” study showed that in the early 1970s, the British were giving very poor advice and very poor treatment. Since both advice and treatment were ineffective, it certainly is puzzling why this study, from which absolutely nothing can be learned about how to conduct either inpatient or outpatient treatment” (Wallace, 1989, p.254)....**was described by Miller and Hester (1986) as “one of the most widely cited studies in the alcoholism treatment field” p.796).

In any event, it is a more than a bit disconcerting to note Miller and Hester calling for private third party insurance companies to enact policies de-emphasizing inpatient care. Furthermore, their conclusion that **“A 3 R’s model of treatment (remove from society, repair the problem, and replace in society is outmoded and inadequate as a means for addressing alcohol problems”** seems poorly informed (Miller and Hester, 1986, p.803) since it misses the point of a comprehensive continuum of care involving a variety of contexts of care of varying intensity and duration that are needed to treat chemically dependent people with behavioral, social and psychiatric problems that vary in terms of severity.

It is also curious to note that at the time Miller and Hester were decrying the **unnecessary popularity** of inpatient treatment for alcoholism, the actual data presented a very different picture. At that time, data from NIAA indicated that 82% of alcoholics were already in outpatient, 10% in residential treatment and only 8 percent in inpatient treatment. It was therefore very misleading of Miller and Hester to describe such treatment as the “popular context for treating adolescent and adult alcohol abuse (Miller and Hester, 1986, p 794).

It is “evidence-based treatment” such as that promulgated by Miller and Hester that we should all regard with fear and suspicion, especially when it hands over the health and well being of our patients and our treatment programs to third party payers.

Finally, Miller and Hester, violate a fundamental rule of hypothesis testing: The null or no difference hypothesis cannot be proved. Put simply, the hypothesis of no difference can be contradicted by finding a significant difference between treatments. However, a failure of no difference cannot be proven because if no differences are found there may be many reasons for this other than proof that differences do not exist. For example, a difference may emerge if different dependent measures were to be employed, different intensities of treatments, different durations of treatments, etc.

Now we turn to further presentations of evidence-based treatment by Miller and Hester. In a discussion of “What works? A methodological analysis of the alcohol treatment outcome literature,” ( Hester and Miller, 1995) Miller et al observe:

**“In an earlier review, Miller and Hester (1986) observed that although the scientific literature points to a list of treatment approaches with reasonable evidence of positive evidence, this list overlaps little, if at all, with those components commonly employed in the US alcoholism treatment programs. The negative correlation between scientific evidence and application in standard practice remains striking and could hardly be larger if one intentionally constructed treatment programs from those approaches with the least evidence of efficacy.”**

Miller and Hester point to their summary of some 211 “controlled trials” as evidence for their position about the inadequacies of US alcoholism treatment approaches. Why did Miller and colleagues select these particular criteria leading to inclusion of these particular studies and what evidence did they leave out by doing so that contradicts their position? Is it true that there is no evidence supporting traditional treatment approaches? The answer is clearly no. Are Miller and Hester’s studies which are included in their group of 211 controlled trial immune to serious methodological criticism? The answer is once again clearly no.

Miller and his colleagues’ research involving his Behavioral Self Control Treatment methods have been sharply criticized for multiple methodological shortcomings by Maltzman (1999, 182-199). As Maltzman has pointed out, Miller’s studies include such problems as extremely small samples, use of non-representative samples, lack of statistical power, inappropriate attempts to prove the null hypothesis, improper instructions, confounded variables and questionable outcome measurement procedures. The list reads like a list of things to avoid as taught in a first course on research methodology.

While Miller points to a very large body of literature bearing upon the effectiveness of his motivational interviewing and motivational enhancement procedures, inconsistent findings, confusion of efficacy studies with effectiveness studies and negative findings from well-controlled studies raise serious questions as to the usefulness of these procedures in the real world of drug and alcohol dependence, across sites and especially, across types of chemical dependency (e.g. drug versus alcohol dependence). (e.g., Ball et.al, 2007; Budney, et.al, 2000; Donovan, et al, 2001; Miller, et al, 2003; Booth, et al, 1998; Project Match, 1997; Dench and Bennet, 2000).

## **Studies of Effectiveness of Traditional Treatment Programs.**

An excellent study of the effectiveness of traditional treatment programs conducted in the Federal Republic of Germany (Feuerlein & Kufner, 1989; Kufner & Feuerlein, 1989). The study involved 21 different inpatient treatment facilities and data were reported on 1410 consecutively admitted patients. Approximately 90% were diagnosed as alcoholics and 5% were considered to be problem drinkers. An additional 5% were unclassified. 89% were physically dependent and 79% reported loss of control drinking. A primary diagnosis involving alcohol was required of all patients participating in the study although many patients also abused other drugs as well. Self-report was utilized as the primary outcome measure but a variety of reliability and validity checks including reports from collaterals, records of sick leave and other hospitalizations as reported by insurance companies, a smaller sample of control interviews, and cross checking through multiple interviews over time. Both longer term (6 month) and shorter term (6-8 week) planned treatment durations were studied.

**At eighteen month follow-up, 84% of the original 1410 patients were interviewed.** 3% of the patients had died during the follow-up interval. With regard to alcohol consumption, the following data were obtained:

**53% were abstinent for the entire 18 month follow-up interval.**

**8.5% had improved.**

**At 48-month follow-up, 81% of the original sample were interviewed and the following data were obtained:**

**46% were abstinent for the entire 4 year period since discharge.**

**77% of the patients who were abstinent in the first 6 months were continuously abstinent for a full 48 months.**

**Utilizing a six month window prior to the 48 month interview, the following data were obtained:**

**66% were abstinent**

**4% were improved.**

**It is of great interest to note that only two patients in an original sample of 1410 diagnosed alcoholics were able to continue moderate drinking in the long term. In contrast, 77% of those abstinent in the first six months maintained their abstinence for the entire 48 months. These results are in accord with previous research (e.g., Pettinati, et al, 1982) which showed that the only outcome category to show stability**

**over time after treatment ends is abstinence as opposed to moderate or controlled drinking.**

**It interesting to note that longer term treatment (6 months) proved superior to shorter term treatment (6-8 weeks). Longer term treatment facilities showed a 60.4% overall abstinence rate while shorter term facilities showed a 49.5% abstinence rate. Once again, these data contradict Miller's assertion that duration of treatment is not important (Miller, 2001).**

While the Feuerlein and Kufner could be considered a "traditional" treatment program, it was not a Hazelden type program since AA 12 Steps were not central to the treatment program proper. However, patients were encouraged to attend 12 Step group meetings in aftercare and many did so with good effect. Modalities employed were similar to Hazelden or "Minnesota Model" inpatient programs (group therapy, individual therapy, work therapy, occupational therapy, alcohol information, life plans and values, sport and physiotherapy). Behavioral or cognitive behavioral therapies were not provided and neither were relapse prevention or social skills training.

When the treatment goals of the multi-site programs are examined, they do share some similarities or compatibility with 12 Step ideas. The German treatment centers' "steps" and a comparison to the appropriate AA steps and/or procedures are as follows (German steps in bold):

**To recognize that things must change.** (The admission of the problem in AA's First Step).

**To accept the need to be helped.** (Turning to a power outside of self in AA's Second Step).

**To accept the offered help.** (Turning one's own will over to the power outside of self in AA's Third Step).

**To accept the status of an alcoholic.** (The practice in most AA meetings when speaking at a meeting to introduce oneself as "I'm Joe, an alcoholic," "I'm name is Mary and I'm an alcoholic," etc.

**To accept the goal of lifelong abstinence.** (AA stresses abstinence but does not require a "pledge" of lifelong abstinence. "A day at a time" is the AA advice).

**To accept the goals of a general change in behavior.** (There are many references to necessary change in behaviors of all kinds in AA meetings, e.g., changing friendship patterns from drinking to sober friends; changing recreational activities; having the "courage to change the things I can;" "getting the serenity to accept the things I cannot change," etc.

It is curious why a study as methodologically sound, inventive and thorough as the Kufner and Feurerlein multi-site outcome study did not make the cut in Miller et al's compilation of 211 controlled studies mentioned earlier. Was it ignored because it presented striking results in favor of traditional treatment? In a thoughtful paper on the problem of "researcher allegiance" in meta-analyses of cognitive behavioral therapy in the mental health field, Butler et al. (2006) point out that the tendency for authors of meta-analyses to prefer one therapy over other therapies is a very significant factor determining the findings from a particular meta-analysis. Butler, et al. report that in one of the first meta analyses of cognitive behavioral therapy for depression conducted by Dobson (1989), researcher allegiance accounted for half the difference between cognitive behavioral therapy and other treatments. Later meta analyses of CBT and depression did not show this large researcher allegiance effect suggesting that researcher allegiance is an important influence when new treatments are first tested but this effect diminishes over time. While Miller's compilation of some putative "211 controlled trials" mentioned above was not a formal meta analysis, it was subject to the same researcher allegiance influences.

There are more studies in support of traditional treatment that do not appear in Miller's list of evidence-based treatments.

**Cross, et al. (1990) report on a ten-year follow-up of 200 randomly selected patients from a single private inpatient facility in Georgia. Treatment duration was 4-6 weeks. Treatment consisted of detoxification followed by daily meetings on the 12 Steps of AA, counseling, and didactic sessions. In the evenings there were AA speaker meetings. Family members were involved in treatment and stayed at the facility the last week of treatment with the patient. Aftercare was emphasized and all patients were given an AA contact in the local community.**

**Outcomes showed that 60% were in remission for at least 3 years at followup and 84% were in stable psychosocial condition.**

**While Cross, et al. (1990) was not a "Randomized Controlled Trial," it was a reasonably well designed longitudinal study with a follow-up interval rarely seen in alcoholism treatment outcome research. It should not be summarily dismissed as methodologically inferior.**

**Yet another study in favor of traditional treatment but ignored by Miller is the VA Comparative Outcome Study by Moos, Finney, Ouimette, and Suchinsky (1999). This was a major treatment outcome study on approximately 3000 patients in VA hospitals.**

**The study was one of very few that involved a direct comparison of traditional 12 Step treatment to cognitive behavioral therapy.**

**The results showed superior findings for the traditional 12 Step model program over CBT. At 1 year, 45% of 12 Step program patients were abstinent as compared to 36% for CBT.**

**Once again, an important, well-designed study that yields results supporting traditional alcoholism treatment, (Moos et al., 1999) is not included in Miller's list of evidence-based treatments.**

**A study of the traditional Hazelden program was conducted by Stinchfield and Owen (1998).** A sample of 1083 patients was obtained from among 1128 patients admitted to Hazelden during the study's index period. 94% were diagnosed as suffering from alcohol abuse or dependence and 58% also met diagnostic criteria for marijuana abuse or dependence. Average length of stay was 28 days. Patients and family were encouraged to attend AA after discharge. Separate units for men and women were operated. Group therapy was the largest component of treatment, 80-90%. Individual therapy was used for issues not appropriate for individual sessions. Lectures, group discussions, homework assignments and attendance at self-help groups were integral. A family program was also provided.

**At 12 month follow-up, complete abstinence from alcohol and all other drugs was reported by 53% of the participants.**

**There was good agreement between collateral and patient reports.**

Unfortunately, the results are based upon a follow-up rate of 71% of the sample. The researchers recognized that follow-up bias was present since they achieved an abstinence rate of 75% in patients who returned their questionnaires while patients requiring a follow-up telephone interview reported a 45% abstinence rate. Still, a lower bound of approximately 45% to 50% abstinence at 12 month follow-up in the Hazelden study compares favorably to the results obtained on the similar 12 Step program studied by Moos, et al. (1999) reported above and by an earlier study of Hazelden's outcomes by Laudergeran (1982).

It is difficult to understand why Miller and Hester and other revisionists have persistently ignored the wealth of data in the Comprehensive Assessment and Treatment Outcome Research registry. (CATOR). Beginning in Minnesota, the CATOR treatment outcome registry came in time to involve intake data on 50,000 adults from 80 programs in 29 states and 6,000 adolescents from 28 programs in 15 states. Both inpatient and outpatient treatment programs were involved in contributing data to the registry. Drawing on this wealth of data, Harrison et al. (1991) reported on 1,900 inpatient completers and 911 outpatient completers. The results were complicated by biases related to sample attrition in both the inpatient and outpatient samples. However, Harrison et al. (1991) constructed a composite variable of 29 risk factors for each patient that was based on social stability, socioeconomic factors, antisocial behaviors and severity of dependence. A linear relationship was found between abstinence and the number of risk factors. Patients with no or only a few risk factors were found to be sober while only a third of patients with 15 or more risk factors were abstinent. Employing this creative approach involving the

number of risk factors to the common problem of sample attrition in treatment follow-up studies, Harrison et al. (1991) were able to provide some correction for sample bias due to sample attrition that yielded the following estimated results:

60% abstinence for the entire inpatient completer sample.

74% abstinence for the entire outpatient completer sample.

Harrison et al.'s method of utilizing risk factors to estimate abstinence rates in both contacted and non-contacted patients at follow-up is of definite interest but probably overestimates abstinence in the total samples in this application. But even if the method overestimates abstinence in non-contacted patients by approximately 20%, the results suggest an abstinence rate of approximately 50% for these samples of inpatients and outpatients. Once again, it is difficult to see why available data on traditional treatment programs available in the Hazelden studies and the CATOR registry are not even mentioned in revisionist arguments against such treatment. Whatever the value of this one study by Harrison et al., it is remarkable that this huge data base of intake and outcome characteristics has been virtually ignored by behavioral science researchers.

**Several neglected studies by Smith (1985, 1986) conducted in Australia are reported by Maltzman (2008).** These studies involved women in one study and men in a second study.

**The first study (Smith, 1985) compared 43 patients from a 12 step-oriented residential program to a matched non-treated group of 35 women who were detoxified at the same facility but did not enter treatment after detoxification. Both groups of women were matched initially on a large number of variables.**

**Follow-up interviews were achieved on 90% of the treatment group at 15 months after treatment. 73% of the women from the non-treated group were interviewed with 17% refusing to be interviewed.**

**Of the treated women, 79% reported abstinence throughout the follow-up interval while only 3% of the non-treated women reported abstinence. Treatment group women had no drunk days on the job during the past 30 days while the comparison group reported 3 drunk days.**

**Blood alcohol levels were significantly different between the groups at the time of the follow-up interview. Treated women had greater numbers of weeks of employment and significantly fewer mental and physical problems than untreated women.**

**Similar findings for men were reported by Smith (1986) for 274 men involved in a study of residential treatment effectiveness. Once again, half of the total sample received residential treatment while half received only detoxification. The treatment and comparison groups were matched on a number of variables.**

**At 15 month follow-up, the treatment group showed a 62% abstinence rate and the comparison group had only a 5% abstinence rate.**

**With regard to employment, treatment group men reported 51 weeks in the follow-up period compared to 33 weeks in the comparison group. Treatment group men spent considerably less money on alcohol and reported fewer drunk days on the job than comparison group men.** Once again, as Maltzman points out, the neglect of these studies by Miller and other revisionists is inexcusable. Granted that Smith's methodology did not involve the "gold standard" of a well-conducted randomized controlled trial, the findings remain interesting and suggestive of practical ways to provide alcoholism treatment for some treatment populations that are cost-effective.

Similar encouraging results are reported for the communal housing approach, Oxford House, that was developed by a community based organization working with professionals from DePaul University. Once again, Maltzman (2008) has provided both a description of this approach along with a Randomized Clinical Trial of effectiveness conducted by Jason et al. (2006).

Finally, the recent 27 million dollar study called Project MATCH that attempted to find patient-treatment matches to increase effectiveness provided little in the way of actual matches.

**However, Project MATCH did report abstinence data comparing something called Twelve Step Facilitation (TSF) treatment as well as for CBT and MET.**

**The findings should be sobering to revisionists. The results at 1 year were TSF – 24% abstinent ; CBT 15% abstinent; Motivational Enhancement Therapy, 14% abstinent.**

It is interesting to note that TSF is an invention that does not exist in programmatic form in the community. It is not Minnesota Model Treatment or any other form of 12 Step oriented treatment. It comprises teaching of the first three steps of AA in individual sessions by a trained therapist using a manual. This approach leaves out the social learning processes inherent in group therapy, AA meetings, social interactions in general including social support, social reinforcement and role models that characterize the AA program and 12 Step oriented programs. Even so, a bastardized and incomplete version of Minnesota Model 12 Step oriented treatment produced better results than either CBT or MET. These differences were maintained at 3 year follow-up as well.

**At three years, abstinence rates for months 37-39 were as follows: TSF (36%); CBT (24%); MET, 27%.**

**Once again, even a watered down version of traditional 12 Step treatment proved to be superior to both cognitive behavioral therapy and Miller's motivational enhancement therapy.**

**McLellan et al., (1993) studied treatment outcomes for 198 alcohol and/or cocaine dependent men referred from employee assistance programs. The men were treated in two inpatient and two outpatient programs that emphasized the 12 Steps of AA/NA.**

**At 6 month follow-up, a remarkable 94% follow-up rate was achieved and urine, breath and self-report measures were taken. An overall rate of 59% abstinence from alcohol was achieved and 84% were abstinent from drugs at 6 month follow-up.**

**There were substantial differences among treatment programs. The two inpatient programs had abstinence rates of 78% and 63% while the two outpatient programs had abstinence rates of 51% and 45%.**

It is interesting to note that while these patients were not assigned randomly, there were no major differences among patients assigned to the four treatment programs on a variety of measures including the Addiction Severity Index. Hence, these findings have relevance to the issue of inpatient versus outpatient treatment.

**It is interesting to note that McLellan et al's results for an employed treatment population at 6 month follow-up were similar to results at 6 month follow-up obtained by Wallace, et al.(1988) in a study of socially stable patients treated in a private facility.**

**Wallace, et al. found that 66% of alcohol only dependent patients and 52% of patients dependent on both alcohol and other drugs were abstinent throughout an entire six month follow-up period (patient self-report with collateral verification).**

**The follow-up rate was 94% of the initial sample.**

**These results for socially stable persons are in agreement with results obtained by Slaymaker and Owen (2006) on employed men and women.**

**Slaymaker and Owen point out that 77% of the estimated 20 million substance-abusing or substance dependent people in the United States are gainfully employed and do not fit the stereotype of the "Skid Road" alcoholic.**

**Continuous abstinence was achieved by 65% at six month follow-up and 51% at 12 month follow-up and 65% were retained by their original employer.**

While follow-up rates were 82% and 75% at 6 and 12 months respectively, followed and non-followed patients did not differ on baseline Addiction Severity Index scores.

These more positive findings for socially stable persons reported by Wallace and Slaymaker are in agreement with Baekeland's observation made some years ago (1977) as to the importance of social stability as a moderating variable that must be taken into account in interpreting the treatment outcome literature. In his review of the English language literature of the time, Baekeland observed recovery rates for socially stable persons that ranged from 32% to 68% while recovery rates for socially unstable persons ranged downward from 18%. Since the bulk of persons seeking chemical dependence treatment services are socially stable, critics of treatment effectiveness must not generalize results from treatment of socially unstable persons to the treatment of socially stable persons.

### Summary and Conclusions

A more inclusive and less biased review of the literature bearing upon so-called traditional concepts and treatment approaches indicates that so-called traditional abstinence based treatment is considerably more effective than we have been led to believe by a small but highly influential group of behavioral and cognitive-behavioral academicians. At the same time, closer examination of the evidence these critics point to in support of their concepts and approaches raises questions as to whether or not such concepts and approaches provide better understanding of chemically dependent people and the most effective ways to treat them. At the least, this review indicates that the field must not yield to propaganda all dressed up as science and "throw out the baby with the bath water." Approaches such as Miller's Behavioral Self Control treatment and Motivational Enhancement Therapy clearly cannot be regarded as "stand alone" treatments of alcoholism and drug dependence. While numerous studies purporting to provide the evidence base for these approaches can be cited, inconsistent findings and results from more rigorous studies fail to provide consistent support for "real world" applications across types of addiction and treatment facilities.

While it is clear that traditional, multidimensional inpatient and outpatient treatment programs do not result in recovery for all patients, it is also equally clear that a substantial number of persons treated in traditional programs do achieve stable abstinence and sustained recovery. This is especially true when treatment is of sufficient duration and intensity and provided in both inpatient and outpatient contexts. Appropriate inpatient care followed by outpatient continuing care as well as ongoing therapeutic contact and monitoring over a lengthy period of time is of greatest importance.

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**HIGHLY RECOMMENDED READING:** The following two recent books by Dr. Irving Maltzman, Professor Emeritus at the University of California, Los Angeles are highly recommended for those who are interested in a more searching and detailed examination of the issues and controversies involved in evidence-based treatment in alcoholism and other chemical dependencies.

1. Maltzman, Irving. (2008). *Alcoholism: Its treatments and mistreatments*. New Jersey. World Scientific Publishing Co. 27 Warren Street, Suite 401-402. Hackensack, NY 07601.
2. Maltzman, Irving. (2000). *Alcoholism: A review of its characteristics, etiology, treatments and controversies*. Boston: Kluwer Academic Publishers. 101 Philip Drive. Assinippi Park. Norwell, MA 02061,