The Current State of Affairs in Mental Health: Problems in the Profession and Where We Need To Be

Gregory K. Moffatt, Ph.D.
Atlanta Christian College

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ABSTRACT

The mental health profession has come a long way since the early days of Freud, Watson, and Adler. Yet many problems still remain in the profession that limit efficiency in the delivery of services and possibly even cause damage for the client in the process of attempting to provide help. Three major areas are explored in this article that need attention. The first problem to be addressed is the diagnostic process and the DSM IV-TR including a discussion of the circular logic in the diagnostic process, labeling symptoms rather than causes, subjective language, the use of medication and the medical model, and the poor application of the DSM with children and adolescents. The second problem addressed is billing and finances and the final area involves problems with research and theory. This article provides evidence of these problems as well as ways in which they can be addressed to push the field of mental health to a high level of professional functioning.

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Introduction

The field of psychology is very young in comparison to other sciences and errors, misinterpretations, and even tragic mistakes have been made by theorists and practitioners in past generations. It is easy to pick on Freud and to note his theoretical failings, for example, when we have the benefit of over a century of progress in the field at our disposal. However, the state of the profession currently is laden with errors, misinterpretations, and theoretical failings. It is likely that fifty or a hundred years from now, future researchers will look back at the turn of the 21st Century and point out glaring weaknesses in our practice of the profession just as we do with theory from 100 years ago. I have come to believe that a revolution is necessary in the field of psychotherapy. Herein, I identify some these problems. Included are problems with diagnostics and the DSM IV-TR, problems regarding finances, billing, and managed care, and problems with theory and research. In the summary, I suggest ways to address these weaknesses.

Diagnostics and the DSM IV-TR

The diagnostic process is laden with problems. Among the most significant problems are the circular logic in the diagnostic process, labeling symptoms rather than causes, subjective language, the use of medication and the medical model, and the poor application of the DSM with children and adolescents.

Circular logic. Some diagnoses in the DSM IV-TR appear to be based on circular logic. For example, learning disabilities are identified by symptoms that are then used to validate the disorder. The learning disabilities identified in the current DSM (reading disorder, mathematics disorder, disorder of written expression, and learning disorder not otherwise specified) are based on extremely vague criteria. In

essence, if a child has difficulty in one of these areas and is functioning below his or her expected level, the diagnosis can be applied. Treatment involves providing the child with more time for the subject, less academic pressure, and perhaps adjusted achievement goals until the child has overcome or learned to compensate for the disability. "Overcoming" the disability then is used as evidence for the existence of the disorder.

Using the same logic, I have a basketball disability. I hail from Indiana. I've been around basketball all my life and I am reasonably athletic. One would suppose I would function at a reasonable level in basketball. However, I'm a terrible basketball player and I function well below an expected level of proficiency. If I am less challenged on the court, have fewer defenders, more time to shoot, and a lower rim, I do much better. Based on our approach to learning disabilities, this argument validates the existence of my basketball disability. This is a ridiculous approach to athletics, but it appears to be a perfectly reasonable argument in both academics and mental health diagnostics.

Labeling symptoms rather than causes. The diagnostic process has improved from the original DSM to the current edition of the DSM IV-TR, but even though we have added diagnoses (a 300% increase in labels from the first DSM to the current edition), one has to wonder if we simply have more labels for symptoms rather than actually furthering the processes toward identification of root problems. This problem has existed in the medical profession for centuries. For example, Kihlstrom (2002) notes that at the turn of the last century, numerous types of fever, running virtually from A (blackwater fever) to Z (yellow fever)" existed (p. 292). Before the various types of "fever" were identified, many different fevers were treated in the same way. Physicians assumed they were treating an illness ("fever") when they were only treating its symptom.

It is very possible that we are making this same mistake. Could it be that many of our diagnoses are actually symptoms – fevers? Perhaps this explains why we find so many differing treatments for allegedly identical disorders - the disorders aren't identical. Kihlstrom (2002) summarizes this point when he argues that we must move beyond symptomology and pursue underlying causation saying, "This 'fall-back' has dominated our thinking for more than a century, and it is time to press forward, with all deliberate speed."

Problems with diagnoses may lead to even a more problematic situation if our labeling of symptoms leads the client to avoid responsibility. If I know I have a basketball disability, I may be less likely to challenge myself, try harder, or find ways to overcome my problem. Instead, I could easily invest my energies trying to change the rules of the game so I can succeed. Even worse, I might not try at all. Houts (2002) makes this point when he argues that labeling a person with a mental disorder makes the "behaviors less a matter of shameful personal responsibility or failing in character and more of a medical condition for which the individual should assume less personal responsibility," thereby removing personal blame and responsibility entitling the person to "the sick role" (p. 48).

Problems of addressing symptoms rather than causes may account for exceptionally high recidivism rates. The client's willingness to work and the desire to improve certainly are important factors, but I often hear experts talk matter-of-factly about recidivism rates of 40%, 50%, 60% and even higher. This "failure" rate in therapy would be unacceptable in nearly any other field. We cannot be complacent with such rates and these rates may be reflective of our treatment of symptoms, rather than causes.

Pharmaceutical treatment of mental disorders. Even though it has come a long way, the medical profession has not grown beyond the treatment of symptoms, especially in the area of mental health. For example, folklore taught generations of people that putting butter on burns helped soothe the pain and it aided in healing. It was believed that butter contained some medicinal property. However, what became clear over time was that in an era where access to ice was limited, butter was an item that was nearly always kept cold. It was the temperature of the butter, not the butter itself, that soothed the pain of minor burns and

butter has no value in healing burns. This tangent into folklore illustrates what may be happening in our treatment of psychiatric disorders. SSRI's, antidepressants, mood stabilizers, and many of the drugs that are commonly prescribed to "treat mental disorders" may simply be treating symptoms, perhaps for reasons far from what theory tells us, while never coming close to addressing the actual cause. The physiology of some disorders is relatively clear, but some of the most common disorders we see in our clinical practice (e.g. attention deficit/hyperactivity disorder, depression, post-traumatic stress), very little is clear about physiological causes. Medications, even though they can be very helpful, may only be treating symptoms.

Even when our diagnoses are correct, when drugs are prescribed more often than not, the prescribing physician is not the one who functions as the child's therapist on a regular basis. Checkups address symptoms and at the very best their diagnoses are best guesses based on brief interactions. Overworked pediatricians and psychiatrists see as many clients as possible, leaving the psychotherapy to us. Few therapists work closely with the physicians who prescribe medications for their clients.

Subjective language. Even though diagnostics using the current DSM involve many objective criteria, the DSM is laden with subjective language. Doucette (2002) notes that in the DSM-IV, "symptoms and behaviors are often characterized in terms of subjective frequencies ['often loses temper,' 'often angry or resentful' and so forth]" (p. 209). "Often" according to whom and in what context?

Not only is the language of the DSM subjective, but there is room for subjectivity even in assigning a diagnosis. The system allows us to diagnose clients with a given disorder even when they do not meet the criteria for that disorder. We can fall back on our largely subjective, yet professional opinion that subjects still suffer from the disorder even though they failed to meet the objective criteria for that diagnosis by using the "not otherwise specified" (NOS) classification. Even though this system is based on presenting symptoms, professional judgment, and logic, it is too vague to win my confidence.

Developmental limitations of the DSM. Perhaps the most obvious problem with diagnostics is that the primary means of diagnosis that we use, the DSM IV-TR, gives relatively little attention to children. Doucette (2002) argues that attempting to use a single standard for diagnosing children, adolescents, and adults is problematic. Even though each of these may have the same diagnosis, "they are likely to need different treatment interventions given their respective developmental status" (p. 205). Even the DSM IV-TR acknowledges that disorders "first diagnosed in infancy, childhood, or adolescence is for convenience only and is not meant to suggest that there is any clear distinction between childhood and adult disorders' " (2000, p. 39). Doucette concludes her argument by stating simply that, "the structural taxonomy of a system such as the DSM-IV is inadequate in its representation of child and adolescent mental health disorders" (2002, p. 216).

A system of diagnostics and classification is necessary. Flanagan and Blashfield (2002) correctly note that "a classification system contains the nouns from which a science develops its language to understand the events within its realm" (p. 121). But the current system is inadequate for diagnosing children. A classification system designed to address the many developmental issues involved in treatment and diagnosis of children and adolescents is imperative.

Houts (2002) even goes so far as to suppose that the diagnostic process is, among other things, a tool for generating income for the American Psychiatric Association through sales of DSM's. While I doubt this was a purpose for the development of the DSM, it still needs serious attention.

The DSM is a valuable tool and I appreciate the many hours invested in the development of the DSM. It is not haphazard and the professionals who produce the manual are serious scholars. I have practiced under four DSM's and subsequent manuals are better than their predecessors. Yet we have a long way to go. Malik and Beutler conclude that, "although even supporters of DSM-IV see it as an imperfect work in

progress, the general consensus appears to be that we do not yet know enough about diagnosis to propose viable alternatives" (p. 9). This process is necessary so that professionals can talk to each other, but refining the process of classification and diagnosis is a journey. We shouldn't suppose that we have arrived.
Finances/billing and managed care

Finances. As a graduate student I was taught that clients would be more engaged in therapy if they paid for their therapy, even if they only paid a small amount – a philosophy that made sense at the time. "You value what you invest in," I was told. However, experience has shown me that this was a myth. It is ironic that during the managed care explosion of the past fifteen years, many of us in the profession argued that insurance companies should cover the costs of therapy for our clients. Yet if it was true that clients invests more in therapy if they are paying their for services, we actually were arguing for something that was to the disadvantage both of our clients and ourselves. If clients had full coverage from an insurance carrier, theoretically they would invest less in therapy and providing mental health services would be more difficult for therapists. I never heard anyone argue that clients shouldn't use insurance payments that fully cover their costs. This all seems a little too contradictory for comfort.

Without question, some paying clients work harder than pro bono clients, but the reverse is also true. Money isn't the issue as much as one's commitment to change. A search of the literature over the past ten years has shown absolutely no direct evidence to support the idea that there is any connection between fees and a client's investment in therapy. Only one article directly addressed the effect of fees on delivery of therapeutic services and this article was limited to graduate students in training programs charging for services (Aubry and Hunsley, 2000). Contrary to what I was taught as a graduate student, it can be argued that fee-based services actually reduce the likelihood of participation in therapy because clients are more likely to drop out of therapy due to lack of funds.

I am not suggesting that therapists don't make concessions to provide more affordable services. Most mental health professionals work on a sliding scale and many therapists do pro bono work with some regularity. Some research has shown that many (maybe even most) health care professionals discount their fees. For example, one study of 970 American Psychiatric Association members found that 35% of them discounted their fees and that the average discount was 25% (Scheffler, Garrett, Aarin, and Pincus, 2000). However, therapy, especially cash-only systems, is still beyond the financial means of many of our clients. If there is a connection between fees and the level of commitment to therapy, let us have research to validate it. Otherwise, the myth of the connection between fees and commitment to therapy should be permanently and overtly disbanded.

I am not suggesting that therapists should provide services free to all clients. We do not take a vow of poverty when we receive our licenses to practice. However, we are in the helping profession and the profession status quo has too long subsisted on a fallacious notion that we have to charge for services. It may also be that we could do more to make our services affordable than we already do.

Managed care. Our fees are not the only problem related to finances. An equally pressing problem is managed healthcare. The first time an insurance company sent paperwork to me so that I might be included on their HMO/PPO board, I thought a mistake had been made. The document was nearly one hundred pages long. It would have taken me most of a day to complete the paperwork. Even after one is approved by an insurance carrier, the on-going paperwork for third party reimbursements can easily involve as many hours as the therapist actually sees the client. Because of this problem, many therapists have chosen to operate as a cash-only business. This reduces paperwork, but obviously can create problems for clients who cannot afford to pay the therapist's fees out of pocket.

A second problem with managed care involves the requirement to provide a DSM diagnosis. Often this is not a problem, but in some cases, the client doesn't need a DSM diagnosis. Consider my client "Eric." I

assisted his parents as they amicably worked through their divorce, custody, and court proceedings. Eric did not need a diagnosis and he didn't have a mental illness. He was not having problems at home or at school. I merely assisted him through the adjustment. Of course, the DSM IV (the current manual at the time) allowed for a diagnosis that was somewhat descriptive of my work with him, but the diagnosis* was so general I felt uncomfortable giving him that label. However, in order for his parents to avoid having to pay out of pocket for therapy, I had to submit a diagnosis. Who knows where this information goes and how it might negatively affect Eric in the future. This is yet another way money can interrupt therapy.

Even though there are many problems with managed care, not all of the changes that managed care has brought about have been bad. Managed care forced the profession to be more efficient in therapy and we could at least speculate that some less than ethical therapists have found it much harder to take advantage of their clients by unnecessarily prolonging therapy. However, these advantages have come with a huge price tag.

*In the case of this client, a diagnosis of V62.81 (Relational Problem NOS) was used. It was also possible to use the diagnosis of V71.09 (No Diagnosis or Condition on Axis I and/or Axis II), but the insurance company rejected this "diagnosis" when it was first submitted. They required a diagnosis other than "no diagnosis."

Research, Theory, and Practice

Research and theory. Research on therapeutic outcomes is not only discouraging, but it is almost non-existent. Very few theoretical approaches have been researched for outcomes. A few theories, cognitive-behavioral and behavioral theories especially, have been heavily researched and demonstrate reasonable outcomes (King and Ollendick, 1997; Howlin and Rutter, 1987; Fairburn, Jones, Peveler, Carr, Solomon, O'Connor, Burton, & Hope, 1991; Sanders, Shepherd, Cleghorn, and Woolford, 1994), but in almost all other cases, the research that exists demonstrates only very weak relationships between therapy and recovery. In other words, little empirical evidence exists that demonstrates that what we do makes any difference whatsoever.

Instead of empirical research, the literature is laden with "how to" articles that are based on the writer's personal approach. Seminars and workshops promote the presenter's position, but other than one's own anecdotal evidence, there is little to demonstrate one therapy as better than another. In fact, during a week of workshops and continuing education at the Association for Play Therapy Annual Convention, an attendee can go from one workshop directly into another addressing the same population (i.e. trauma therapy) and learn theoretical approaches that seemingly contradict one another. At the very least, these approaches are starkly different (i.e. filial play therapy, ecosystemic therapy, sand play). Do all roads lead to the same end?

How can we rely on theories when they are untested and they seemingly contradict one-another? Advocates of theory A allege that theory B cannot work and they provide theoretical reasons for their belief. Yet advocates for theory B make the same claims about theory A and provide their own logic for their beliefs. This leaves one wondering whether all theories are equally good or equally bad. Should our personal selection of a single theoretical approach or the development of our own eclectic approach be reduced to whatever therapy you like? Surely something more empirical should drive the profession.

In their comprehensive review of research in the field, Fonagy (2002) and his co-authors cite the many problems with almost any outcomes research including grouping by diagnosis, randomized controlled trials and meta-analytic reviews. Likewise, Houts (2002) addresses inherent problems in mental health research. Comparing a "diagnostic category with a 'normal' control group," he argues, virtually guarantees that one

will find "some difference between the two groups if for no other reason than the fact that individuals who seek services are likely to have multiple problems that distinguish them from those who do not seek services. The difference between the two such groups may be due to a statistical artifact or sampling bias known as Berkson's bias or Berkson's fallacy" (p. 49).

Many research studies on therapeutic outcomes are based on very small samples (e.g. $N<25$) and homogeneous populations making generalization, and more importantly cause-effect relationships, very difficult to demonstrate. Most researchers use college students because most of the people doing research are college professors. Even when a different population would be ideal, pragmatics often make it difficult or impossible to include subjects from other demographic groups. At its worst, some of this research is less than credible. Even though the peer review process weeds out many studies that are poorly constructed and executed, some journals either are poorly edited or they simply ignore bad methodology. Limited sample sizes, failure to randomize, and other red flags appear with disquieting frequency. Human issues regarding mental health, adjustment, development, family dynamics, and so forth are far too complicated to rely on small samples.

Many of the studies that exist are too simplistic and cannot be generalized well. The "publish or perish" system in academia tempts researchers to produce less than stellar research simply to hold their jobs or to gain tenure. This leads to the production of a body of literature that has far less value in the advancement in the field than could otherwise be produced if one were not under pressure to produce articles in number, rather than quality and contribution to the field. Where they would like to invest more time, money, and energy in meaningful, well-constructed, research, such projects simply may be unrealistic. Where it might take two years to produce a quality piece of research, one can produce three or four articles of lesser quality in that same time. Unfortunately, some of these lesser quality articles are not of lesser value – they are of no value. It is easier, cheaper, and pragmatically more reasonable for most researchers to use an available population and address easy topics than to produce the kind of research that would actually provide useful information to the field.

Practice. In their exceptional text reviewing outcomes of psychiatric treatment and evidence-based medicine, Fonagy (2002) and colleagues note that "there is a clear danger that "many treatments in common use are, in all probability, not efficacious" and that it is "an illusion that clinical experience [alone] can tell us what is effective" (p. 3). Without empirical data, not only could our methods be inefficacious, but they may also be detrimental. Without data, how can we know they are not?

Much of the research that exists on therapeutic outcomes either demonstrates no evidence for the effectiveness of the models studied on the populations and disorders to which they are often applied or it is laden with language that leaves one wondering what to think about research itself. For example, therapies "appear to be" effective or "seem to demonstrate some effectiveness." According to Fonagy (2002), very few therapies have actually been shown to be effective. Exceptions are cognitive behavioral therapies, problem-solving skills training, parent training, behavior therapy, and operant conditioning, and yet the phrase "appears to be effective," remains an ever-present specter in each discussion. (Fonagy, et. al, 2002). Lest those who regularly use these methods get too self-assured, Fonagy (2002) also notes that some of these treatments are only effective with certain disorders and can, in fact, worsen a person's situation if misapplied.

Clearly, there is something subjective in our practice. We know when our clients have been made progress and we can see improvement in their lives despite the lack of concrete, empirical data. We all believe in the approaches we use or we wouldn't use them. If we waited for empirical data for every theory, we could never use the very tools we know work. Fonagy (2002) acknowledges that, "many important outcomes can only be indirectly or inadequately measured" (p. 2). Later Fonagy and colleagues concede that, "It seems that questions such as 'What works for whom?' are inherently impossible to answer in the context of a

review of outcome investigations. The number of disorders is too large, the number of psychosocial interventions too great in number and too heterogeneous in implementation, the number of contextual (moderating) factors to be taken into account too many and too difficult to specify for the field to yield definitive answers to an inquiry as crassly empirical as the 'Who is likely to benefit?' question" (p. 33). However, we cannot rely exclusively on our subjective observations and beliefs. This is a dangerous approach. In the early days of medicine for example, physicians "observed" that their patients improved after blood-lettings.

In summary, even though there are some very serious researchers investigating these questions currently and their research is valuable, much more needs to be done. Fonagy and colleagues conclude that, "there is clearly a need to conduct further research into the origins and treatments of a wide range of child and adolescent mental health problems" (2002, p. 391). This seems clearly to be the case.

Summary

Regardless of how this article appears, this is not a fatalistic review. We all love children and we provide them with a healing process that they probably would not have if they did not come to us for help. All is not lost and we can have faith in the profession. I do not doubt the sincerity of those who participate in the construction of the DSM and I do not doubt the professionalism of most of the people in the profession. Instead, I wish to call the profession to move itself to a higher level of professionalism. It would be irresponsible and pointless to complain, but not to provide solutions. These are my suggestions for upgrading our profession:

First, graduate programs should provide students with options for meeting client needs other than tradition therapeutic models. Home visits, for example, could help clients who have financial or transportation difficulties. Therapists should seek ways to provide therapy that accommodates the varied needs of our clients by doing pro bono work as much as possible and perhaps expanding our sliding scales.

Second, research on what works is imperative. Researchers need to continue to seek grants that will fund longitudinal, in-depth research with large samples. Change is needed in academic environments where much of the research is done so that more difficult research can be accomplished. In the coming decade, the profession needs far more empirical evidence regard therapeutic approaches, what works, and what does not. We cannot continue to be satisfied with recidivism rates as high as they are.

Finally, more research is needed in the diagnostic process. Perhaps an alternative for to the DSM is needed for children – either a DSM children's version or a new manual altogether. Several theorists have proposed alternatives to the DSM and these alternatives should be examined, tested for validity, and utilized if they work (Schneider, Buchheim, Cierpka, Dahlbender, Freyberger, Grande, Hoffmann, Heuft, Janssen, Küchenhoff, Muhs, Rudolf, Rüger, and Schüssler, 2002).

Most of us are doing the best we can, but we can easily focus on our own worlds, our own clients, and our own approaches to the exclusion of others. A wake-up call can challenge us to reach for a higher plane of professional functioning. We are helping thousands of hurting children and families every day. If we could adequately address the issues noted in this article, we not only would be more efficient and perhaps even more effective, but we could have greater confidence that the work we do is the best it could possibly be.

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