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2000

Social Work Practice in the Real World: An Argument for Evidence Tested Practice

Tomi Gomory



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A note for the reader:

This is an unpublished manuscript written in 2000 to be published in a book, but withdrawn after one of the editor's had an intellectual and personal meltdown at what that editor perceived to be too strong a critique of his position. Authorities hate to be critiqued, but scientific advance can only take place by rigorous criticism and positing of alternate options for consideration in order to determine which may be the better alternatives as the paper argues. This paper was written at the beginning of the Evidence Based Practice (EBP) Brouhaha, which is now strangling innovation in Social Work. It is dated in its references but not in its argument. EBP continues to be a deeply contested issue with none of the original problems identified in this essay resolved.

Enjoy!

Tomi Gomory, Ph.D.
College of Social Work
The Florida State University
tgomory@fsu.edu

¹ Originally presented as a paper for Evidence-Based Practice Conference, George Warren Brown School of Social Work, Washington University, St. Louis MO. May 5, 2000.

Abstract

This chapter explores the relevance of practice guidelines for the advancement of clinical social work by attempting to explicate the current epistemology of empirical social work practice, Justificationism, and contrasting it with an alternate epistemology, Fallibilism (Karl Popper's Critical Rationalism). The chapter asserts the superiority of fallibilism for the advancement of knowledge and recommends its implementation. It is further argued that whether or not clinical practice guidelines are essential to practice depends on whether guidelines can be more explanatory (helpful) than some other alternative such as Fallibilistic Critical Thinking (Fa.C.T.) when critically assessed against it. Examples and arguments are offered for the reader's evaluation suggesting that practice guidelines may be of some heuristic value (when based on well-tested evidence), but are not superior to the more fundamental cognitive activity of fallibilistic critical thinking, and therefore may be redundant.

INTRODUCTION

The subject of considerable development work in allied professions, practice guidelines are increasingly discussed in social work. ... We view practice guidelines as a set of ... organized knowledge statements that are designed to enable practitioners to find, select, and use the interventions that are most effective and appropriate. ... [T]hey help practitioners discharge their functions. The content and structure of guidelines ... should be responsive to and facilitate the performance of practice tasks, yet recognize the uncertainty and tentativeness inherent in science-based professional knowledge (Proctor & Rosen, 2000, p. 1).

This quote from the editors of the current volume describes one of the latest efforts to extricate social work from the empirical limbo that the profession finds itself presently, in its quest for intellectual relevance and effective professional knowledge (Gambrill, 1999; Gomory, 2000). Most social work professionals engaged in this resuscitation agree that science is the key. What is unclear, is how science can best be used to advance our knowledge aspirations. As Kirk (1999) noted the profession has tried through a variety of ways to “bring science to practice” for over twenty-five years (p. 303). All of these prior efforts have been unsuccessful. He lays out the limitations and difficulties of the “latest” of these attempts, the development and implementation of practice guidelines and finds that

Practice guidelines are only as good as the knowledge base. The knowledge base for social work practice is thin ... Although there may be sufficient depth to a research literature in a few specific problem areas, I doubt that it would be possible to develop many guidelines grounded in an extensive social work literature (p. 309).

Serious attempts at evaluating the effectiveness of this new approach are scarce and at best marginally positive (Howard & Jenson, 2000). The editors of the current volume never-the-less seem to think that “while you may acknowledge that practice guidelines are not universally favored in social work we see the [present] book as an opportunity to move forward and contribute to eventual guideline development rather than engage in deliberation about the merits of guidelines for social workers” (personal communication). This a priori confidence about the utility of and their advocacy for forging ahead with the development of practice guidelines absent some well tested corroborating evidence seems itself to be problematic and antithetical to their expressed interest of, “promot[ing] the use of scientifically based knowledge to guide health and human services intervention”.(Proctor & Rosen, 2000, p. 1). Their commitment “inductively” relies on the use of guidelines in some of the other helping professions such as physical medicine and psychiatry, “The subject of

considerable developmental work in allied professions, practice guidelines are increasingly discussed in social work” (p. 1). Their observation of medicine, psychiatry and psychology’s use of practice guidelines inferentially “supports” or “justifies” the assumed preferential utility of guidelines in social work practice. The professional authority of medicine and psychiatry (following that profession’s successful political effort to legitimate itself as a viable medical subspecialty) have framed, organized, and validated social work clinical practice from the profession’s inception as it tried to gain acceptance and professional respectability (Leiby, 1979; Siporin, 1992, pp.87-89, Abbot, 1988, p. 302; Wong and Gomory, 2000). If however, the primary aim of the present effort is “to enable practitioners to find, select, and use the interventions that are most effective and appropriate” (Proctor & Rosen, p. 1) then the move to anoint one approach by proclamation over others may be premature and not necessarily in the best interests of either the profession, or clients.

Two alternative epistemological positions will be described. One built on inductively accumulated confirmatory evidentiary support characterized by frequent premature foreclosure of critical analysis, resulting inevitably in “well-justified” or “credible” explanations or interventions (Justificationism); the other, using rigorous critical debate and empirical tests, attempts to falsify competing conjectured explanations, resulting occasionally in tentative truths about the world (Fallibilism). The argument advanced is that fallibilism provides the methodology for social work that offers the best chance of scientific knowledge development in the field if the aim is to “help practitioners discharge their responsibility for effective intervention” (Proctor & Rosen, 2000, p. 1). The chapter will consider what is needed methodologically in order to find and determine “best practice” for “effective intervention”. This may or may not turn out to be the use of practice guidelines. There is little well tested evidence that the costs of implementing guidelines are outweighed by their benefits (Howard & Jenson, 2000), or that there is any real commitment in the field to take empirical effectiveness or efficacy seriously,

As is by now common knowledge, social work research capable of guiding interventions is scant, and the extent of research utilization in practice correspondingly meager (Proctor & Rosen, 2000, p. 24).

The real priority and the first step, before practice guidelines or other “centrally planned” professional regulatory “heuristic” devices are implemented, might be the provision of fundamental tools for problem solving (a method of critical thinking) that would promote well-reasoned autonomous decisions among both students and clinicians in general.

In what follows the two extant alternate epistemologies, fallibilism (critical rationalism) and justificationism², a heterogeneous set of philosophic notions, will be described. Justificationism in its many varieties is the current reigning intellectual paradigm (Gomory, 1997; Miller, 1994; Gellner, 1992; 1985b; Bartley, 1990; 1984; Munz, 1985; Agassi 1975; 1963; Popper, 1965)

THE EPISTEMIC ALTERNATIVES INTRODUCED

Karl Popper, fallibilism's best-known intellectual proponent originally christened fallibilism, Critical Rationalism (Miller, 1994; Bartley, 1984). It is often viewed with hostility by professional philosophers and other intellectual authorities in the social sciences including social work (see for example Tyson, 1995), but held in high esteem by most physical scientists. For example, the late Nobel Prize winning biologist Peter Medawar in dedicating a volume to Popper says, "The philosophic colour of the book ... reveals everywhere the teaching of Sir Karl Popper" (Medawar & Medawar, 1978, foreword).

Critical Rationalism argues that the appropriate use of evidence is for rigorously testing ideas (a logical and empirical possibility), and not for supporting them (a logical and empirical impossibility). Such a fallibilistic approach may be deemed to controvert the commonsense view of "knowledge building" which is relied on in developing practice guidelines, "Each practice guideline for intervention must be based upon a body of evidence supporting its effectiveness" (Proctor & Rosen, 2000 p. 17). Evidence Based Practice (EBP) as currently implemented could also be another such faulty commonsense view,

Attempts to classify medical decisions as justified [supported by] or unjustified by scientific evidence have no foundation in logic and that the term 'evidence based medicine' is logically indistinguishable from the term 'medicine'. The use of the term 'evidence-based medicine' calls for a new type of authoritarianism (Shahar, 1997, p. 109).

The approach of searching for support or justification, while sounding and appearing to have face validity, is a mistaken epistemic effort and is potentially dangerous.

Before we can begin to discuss the utility of EBP (describe by Gambrill in the present volume) or practice guidelines, our apparently unquestioned belief that clinical practice based on some appropriate degree of support is possible and that we all know what it is, needs to be scrutinized. This should entail a close review of our fundamental

² Justificationism, despite the apparent differences claimed to exist among positivism, relativism, hermeneutics, post-positivism, post-

assumptions, both the personal and institutional (Agassi, 1975, chapter 14), under which we are proposing to employ EBP/practice guidelines. In contrast with the justificatory perspective and the “inductive” method required for it, the present author recommends a deductive critical thinking approach based on Critical Rationalism, Fallibilistic Critical Thinking (Fa.C.T.)(Gomory & Gambrell, 2000), due to its hypothesized superiority in truth telling. If this argument is telling, then the fact of the matter appears to be that Evidence Based Practice has explanatory value only if interpreted conceptually as Evidence Tested Practice, using the Popperian method of eliminatory trial and error testing by falsification leading to more comprehensive explanations (Bartley, 1987; Popper, 1965; 1979), and practice guidelines, outside of some general heuristic value, as a result appear to be redundant. Recent social work and psychiatric literature³ is referenced for the purpose of illustrating the debilitating effect of justificatory views on the excavation of knowledge in social work and elsewhere.

A FUNDAMENTAL MISUNDERSTANDING IN KNOWLEDGE DEVELOPMENT AND ITS CONSEQUENCES

The generic problem solving approach currently promoted by most educators, parents, and institutional intellectual advocates as good critical thinking can be characterized as the enthusiastic exhortation to observe commonsensically obvious discreet phenomena (objective sense data), and prove, validate, support, confirm, or simply justify general arguments, a priori beliefs, or empirical claims based on their cumulation leading ultimately to the truth⁴. Typical examples of this attitude toward how problems may be solved in social work can be found both in the profession’s early and late development. The scientific effort that Kirk (1999) describes as the focus of interest for social work scholars during the last 25 years can actually be traced back to the turn of the 20th century and Mary Richmond’s efforts’ at Social Diagnosis (1917/1934), “The use of science as a guiding light for practice was articulated by Mary Richmond in Social Diagnosis” (Reid, 1994, p. 166). Richmond also set the methodological agenda of “inductive” clinical practice that is still prevalent today among “The Empirical Practice Movement,”

modernism or one of social work’s own, the heuristic paradigm (Tyson, 1995), can methodologically encompass them all.

³ This literature is used as illustrative of professional practice because social work has long historical, professional, and political-economic connections to institutional psychiatry (Wong and Gomory, 2000). Even the effort to look at guidelines is modeled at least in part on psychiatric guidelines as previously noted. Also, the majority of NASW members work in mental health with the result that many social work interventions must take place in these settings Gibelman & Schervish, 1995, 1237).

⁴ An important alternate educational approach based on Critical Rationalism is to be found in Perkinson (1984), that is incorporated into Fa.C.T. (Gomory & Gambrell, 2000).

The processes which lead up to social diagnosis and thence to the shaping of a plan of social treatment may be divided into the collection of evidence and the drawing of inference therefrom. The collection of evidence comes ... first ... [then] the further stage ... of comparing the evidence gathered from these various sources (inference) and of interpreting its meaning (diagnosis) (Richmond, 1917/1934, p. 38).

Thyer & Wodarski's (1998), recent two-volume, Handbook of empirical social work practice continues this empirical and positivist⁵ effort. They say the following about scientific practice and its capabilities,

One of the best tools to promote the values of the profession is empirical social work practice. Telling the truth is one of those values ... and discovering the truth is something that empirical research is very good at (p. 18).

They also quote Leon Williams approvingly when he asserts that,

Social work must assume, for the sake of epistemology, that the field can attain certain, if not valid, knowledge about the human condition. To settle for something equal to or less than probable knowledge is to settle for knowledge dictated by dogma and naïve belief, and that appears untenable in an applied discipline (p. 8).

And finally,

Clients should be offered ... interventions with some significant degree of empirical support (p. 16).

This view is so ingrained in our notions of knowledge development that we simply assume that such support or certainty (at least in theory) can be had and the pro forma result can be the "certain" truth and nothing but. The belief that there are prima facie better justified and less well justified or more probable and less probable "facts" in our world is widely held. Most of the pedagogical energy in our educational institutions is focused on "transmitting" (see Perkinson, 1984, pp. 9-20) the well-founded facts to our students while simultaneously requiring them to find, collect, and regurgitate the supportive data for their test answers and assignments by citing the authorities who hold the most "credible" views the field by consensus recognizes as such. This accumulation of citations is assumed to represent the facts as we know them. Perhaps unstated, but assumed is, that if you can establish (i.e. by counting) that there are more research articles (evidencing some type of systematic testing) favoring one side over the other, the larger accumulation is in some sense authoritative, its position more correct,

⁵ Science need not be and often is not a positivistic or empirical (sense or observation based) effort. The epitome of important non-positivistic science development is Einsteinian physics resulting from mind experiments not laboratory research. This approach is associated with Rationalism not Positivism.

One project ... examined 42 recent meta-analyses which covered thousands of interventions for a wide range of problems dealt with by social workers and other human service professionals. Thirty-one, or three-fourths, of these ... reported *in their conclusions* the existence of differential effects between competitive interventions (Reid, 1998, p. 6).

Along with these apparent glad tidings no mention is made of any of the well-known problems of using meta-analysis as a valid tool for reviewing intervention research. There are at least 5 reasons why meta-analysis is “uncritical empiricism” and not of much help (see Oakes, 1986, pp.157-163). Some statisticians feel very strongly about its inadequacies, “I devote a section to a new *bete noire*, meta-analysis. It should be stifled at birth” (p. viii). Putting these very important issues aside, does the literal “weight of evidence” by the consensus authority of experts serve as convincing proof of any notion? No! Far from providing proof, this approach may be in complete error.

The issue hinges on understanding the two radically different methodologies for knowledge development that are postulated in our world.

Justificationism

This approach, in the form of the “new science”, originated in the latter part of the 16th century (Flew, 1984) with Sir Francis Bacon’s ideas of Nature as an open book. It argues that the human mind is a blank slate, a *tabula rasa* on which the senses paint the facts of our world. As long as we don’t bias these pictures with our prejudices we will see the world as it really is. Error free knowledge therefore, is passively received knowledge. We introduce error, by our “speculations” based on our pre-judgements. Nature herself is obvious all we have to do is look directly and see the truth (Popper, 1983, pp.341-342). The “fact” that we can see reality in situ leads naturally to the conclusion that many similar “true” observations or associations of what is, will lead by the method of induction to universal statements or laws (see Popper, 1965). That for example, our intersubjectively well-validated everyday experience of the sun appearing on the eastern horizon and ascending to an apex by about noon, and then descending toward the western horizon concluding in nightfall, should provide the definitive data to assert the universal law that the sun rises and sets. This apparent truism is so well inculcated that the very popular musical, “Fiddler on the Roof”, has memorialized it in a song entitled “Sunrise, Sunset”. This view is also known as the “commonsense theory of knowledge”, what you see is what you get (Popper, 1983, chapter 2). It fits well into the routine expectations of people. Most of us are comforted by the sense of safety offered by experience confirming well-accepted “true” beliefs. Such beliefs provide structure in our lives by offering foundational

explanations for existence based on repetition implying permanence. A desire for stability and security in a communal refuge is one of the defining characteristics of human behavior. Another contrasting trait, linked with Fallibilism, is a striving toward autonomy, entailing risk, uncertainty, deviance from the norm, and potential alienation⁶.

Efforts at justification involve the search for authoritative knowledge. The logical conclusion of such a search must be well-justified knowledge, the best of which must be absolute knowledge, certainty. Generally we have relied on such authorities as, deity(s), or our intellect (rationalism or idealism) or our senses (positivism, operationalism, empiricism) as the ultimate arbiters of such knowledge. The contemporary presentation or rather oversimplification of our epistemological/philosophical tradition as evolving from religious authority based dogma to the rational methods of science and as such representing a “rebellion against authority” is a myth (Bartley, 1982, p. 149). We have instead replaced religious dogma with scientific dogma, irrational authority by rational authority (see for example the development of Positivism in Hayek, 1979 or generally Polanyi, 1964a; 1964b). One example of such contemporary dogma based on the consensual agreement of the appropriate scientific authorities despite the empirical counter evidence, is illustrated by the research on Programs of Assertive Community Treatment (ACT), a well accepted intervention for the severely mentally ill (Gomory, 1999). Wingerson and Ries’ (1999) statement is typical of ACT experts,

Despite acknowledged methodological difficulties, the *general consensus* of research to date indicates the ACT model to be effective in reducing hospital recidivism and improving client outcome. In light of these *positive research findings*, Assertive Community Treatment Programs are being widely disseminated and implemented ... throughout the United States (p. 14, emphasis added).

The inductive method that is supposed to validate the accumulation of discrete facts by summing or consensual agreement does not exist. Induction fails on both empirical and logical grounds (Gomory, 1997; Miller, 1994; Bartley, 1984; Popper, 1983; 1965).

The Empirical Grounds

⁶ A condition that according to Marx is a problem to be solved by communism’s ability to abolish human self-alienation. In contradistinction, Hayek and Popper view alienation as important and helpful in advancing the growth of knowledge by allowing our subjective productions (thoughts, theories, ideas) to be alienated or objectified by making them public (i.e. by writing them down) so that both the author and other interested parties can analyze and critique the material “objectively” (see Bartley, 1990, chiefly pp. 64-71).

As biological organisms we have evolved to be the organisms we are through the trial and error process of blind variation (trial) and selective retention (error elimination) (Radnitzky & Bartley, 1987). The individuals in a given environment who have more of those characteristics which will insure their physical survival will more likely live to the stage of reproduction. As a consequence, such individuals will produce more descendents who are more or better fit to the then prevailing environment than individuals lacking those essential physical characteristics, insuring over time the presence of those characteristics in most of the population. These evolutionary principles extend to our cognitive equipment. These faculties have also developed in particular ways as a response to the molar environment we inhabit. We have the sensory organs that we need to survive in such an environment. We have eyes because there is light and a need of vision for navigation. We have tactile faculties because the material world appears as solid and substantial at our biological level (although we know that indeed much of this is empty space held together by powerful forces sub-atomically). Our cognitive structures selectively categorize or construct those aspects of the world that impinge on our survival and ignore others. Our vision discriminates among “solid objects” so that we can avoid getting hurt by them but cannot see ultra violet or infra red light. We hear a limited range of sounds. We hear dogs barking for example, but cannot hear supersonic sounds which dogs can. We are only aware of what is “out there” selectively and through cognitive filters. The human world is literally a subjective construction, a limited version biologically of the “real”. This fact has led many to proclaim the absence of objective truth and knowledge. To move to fideism and to relativism, and to argue for a postmodern world of interpretive meaning. To assert that there are many ways of knowing and they’re all of a piece. Be that as it may, the possibility of unfiltered (objective) observation, one of induction’s fundamental assumptions has been falsified by what we know about our biological selves, and no major disagreements exists regarding this finding among contemporary scientists or philosophers (Munz, 1999; 1993; 1985).

The Logical Grounds

The second major assumption, that multiple and cumulative positive findings of “fact”, or numerous replicated confirmatory observations should entitle us to assert the finding of more “credible” or justified proof of a claim or explanation, fails through logical analysis.

Let’s look at the hypothetical example alluded to by Dr. Gambrell in her chapter of 3,000 uncontested, consecutive observations by a justificationary ornithologist, of white swans. Following the inductive method would lead

to the perhaps obvious universal statement; “all swans are white”. This empirical statement is highly testable. One single observation of a non-white authenticated swan would refute that statement. That is, it would refute it logically. The practical testing of this universal statement would probably require many such falsifications to control for potential error and a variety of experimental biases. The empirical fact as we know today is that in the antipodes, specifically New Zealand, there are black swans with red beaks. This finding falsifies what was an apparently well-supported, highly probable statement. The only difficulty with this well-justified statement was that it was false. This holds for all well confirmed claims. It is impossible to know based on what we know today, whether sometime in the future we will not find the counterexample that will falsify any of our current well tested notions. Historically, precisely this occurred in the growth of knowledge in physics as we moved from Aristotle, to Newton, then Einstein, and now to Quantum Mechanics for more broadly coherent explanations (Agassi, 1963).

The occurrence previously described, of the sun rising and setting, visible to all sighted individuals resulting possibly in unanimous observational consent (full consensus) never the less is wrong. We know today that it is the planet earth, which revolves on its axis and rotates around the sun, combining with our particular cognitive equipment’s ocular interpretation that provides the optical illusion of the sun’s rising and setting and not any direct physical activity of the sun. Our senses are highly fallible and prone to error. No observational guarantees can be had. Induction cannot tell us anything about either the reliability (the consistency or dependability) or the truth-value of any universal statement. A falsification, and thus the eradication of any reliability, probability, or validity claims, of a well-replicated theory may occur hundreds of years in the future, following thousands of confirmatory replications. Replications are not controlling, because they can’t foresee the development of new falsifying critical tests or alternate explanations that are unimagined and unimaginable at any present time (Miller, 1994; Bartley, 1990). The classic example is the replacement of Newton’s theories by Einstein some 230 years later.

The final logical problem of justificatory thinking is that of infinite regress. As we look to justify a claim (the methodological mandate of inductive science) we must rely on some authority to answer such questions as—On what do you base your argument? How do you know that to be correct? Or how do you justify your beliefs? These questions all require authoritarian answers. They must ultimately, or rather penultimately, also turnout to be arbitrary answers. Thyer and Wodarski (1998) for example rely on individual social work experts like Mary Richmond (p. 7) and Frederic Reamer

(p.3) to justify their assertions. When we examine these referenced authorities we find that they are not final, but rather “penultimate” authorities because they in turn use prior authorities to support their particular claims, who in turn rely on prior authorities, who in turn ... ad infinitum. If we were to trace the source of each claim as retrospectively cited by each authority that resulted in the claims of Thyer and Wodarski, we would find an endless or infinite regress operating. The problem of infinite regress forces us somewhere along our impossible search for the original or final authority to cease and desist, and through a leap of faith assert the facts through dogmatic commitment not certified truth (Bartley, 1982, p. 139).

We cite authorities like Mary Richmond for our foundational claims not realizing that all such authorities have to rely on other authorities and thus are incapable logically or empirically of ever providing us with the one thing we crave, certitude. It simply doesn't matter where we stop. Each such halt is arbitrary. That is, each such choice of authority is arbitrary and if continued each leads to another justificatory effort ad absurdum.

Due to the limitations of our cognitive equipment and the logical inadequacy of any finite set of observations to prove universal statements, we are unable to reason from the known to the unknown. No “credible” or well-justified knowledge can be had by human beings. As Campbell and Stanley (1963) following the argument of Karl Popper's 1934 refutation of induction (1959) note in their classic research design primer, Experimental and quasi-experimental design for research,

A caveat is in order. This caveat introduces some painful problems in the science of induction. The problems are painful because of a recurrent reluctance to accept Hume's truism that *induction or generalization is never fully justified logically*. ... Generalization always turns out to involve extrapolation into a realm not represented in one's sample. ... Thus, if one has an internally valid Design 4, one has demonstrated the effect only for those specific conditions, which the experimental and control group have in common.... *Logically* we cannot generalize beyond these limits; i.e., we cannot generalize at all (p. 17).

Our existential inability to justify our knowledge claims sometimes provokes fear and trembling. A dread and anxiety of the unknown. The tranquilizing solution of some contemporary philosophers to this most unsettling of human predicaments has been to validate the relative merit of all putative knowledge seeking efforts (see Tyson, 1995 for example, or Wittgenstein's, local speech communities language games explanation in Munz, 1985; 1993 and Bartley, 1985) thus reaffirming the “obvious”. All epistemological efforts are deemed of equal value. These intellectual authorities declaim that no methods exist to distinguish what is better more truthful knowledge from what is worse or false (Gergen,

1985). We are all as knowledge developers declared to be equidistant from the eyes of God, autonomous social constructors of our various worlds (Munz, 1985). Relativism appears to trump objective truth.

Fallibilism

This may leave us as one prominent social worker has suggested in the following state,

This editor takes the position that there are many truths and there are many ways of knowing ... Some will find the validation of their findings through statistical analysis and probability tests. Others will find it through the intensity and authenticity of “being there” ... or through public and shared consensus in what has been called [in social work] “practice wisdom” (Hartman, 1990, pp. 3-4).

And as the inventor of the Heuristic Paradigm suggests,

All arguments about whether it is better to study individuals, families, groups, political entities or public policies are in principle undecidable, because at bottom, they are conflicts about competing values. That is, since we can't know with certainty either the truth or the best way to approximate it, decisions about how to proceed are subjective or value judgements. If we realize the futility of pursuing the answer, then we become grateful for any explanation or intervention that promises to help with the question (Pieper, 1989).

Or, as an alternative, seeing that value beggars can't be empirical choosers, we may turn justificatory philosophy on its head and,

Reject the demand for rational proofs of our rational standards. ... *Nothing gets justified; everything gets criticized.* Instead of positing infallible intellectual authorities to justify and guarantee positions, one may build a philosophical program for counteracting intellectual error. ... [S]ince there are no guarantees or criteria of truth, no ways of definitely deciding... For such a philosopher, a different question would become important: *How can our intellectual life and institutions be arranged so as to expose our beliefs, conjectures, policies, positions, sources of ideas, traditions, and the like-whether or not they are justifiable-to maximum criticism in order to counteract and eliminate as much intellectual error as possible?* In effect, we shall attempt to learn from our mistakes, to adapt to the unforeseen and unanticipated (Bartley, 1984, pp. 112-113).

This apparently simple solution to the problem of justificatory failure has profound effects for epistemology. Since there is a logical asymmetry between verification and falsification because it is logically impossible to use finite statements to prove or justify universals, but it is logically possible to falsify a universal statement by just one valid counterexample, the new critical attitude requires that we place all aspects of our knowledge including our underlying beliefs at risk. Everything is fair game for potential falsification through rigorous testing, nothing must be shielded, and nothing is sacred or authoritative. The testing itself is never conclusive; all falsifying tests are open to further criticism. Proofs of falsehoods are as scarce as proofs of truths. But, as Popper notes many of our important conjectural truths have

withstood painstaking, repeated testing so that this problem is not as great as it might appear. Falsifications of such well-tested conjectures in the future would simply mean that we have advanced knowledge, learning something new about the world from the experience (i. e. Einstein's world is a better explanation than Aristotle's). All human knowledge is conjectural, tentative, and never authoritative. In a problem saturated world we can offer guesses about potential ways to reduce the problems, and by devising critical tests evaluate their truth content. Those explanations or theories, which are unable to withstand the critical assault, are rejected and deemed falsified. This trial and error approach over time leaves fewer and fewer well-tested conjectures remaining in the game. Those remaining, are ones which explain everything that the rejected ones did, plus something more. More comprehensive theories replace less comprehensive ones. Science thus grows through elimination and not accumulation of notions. By carving away at the granite block of ignorance (the infinite set of as yet untested theories) with the chisel of criticism, what emerges gradually and approximately, is the figure of objective knowledge. Although we can never confirm or attain absolute knowledge we can gradually eliminate more and more false knowledge and thereby get closer and closer to our regulative principle of the objective truth; perhaps even reaching it, although we would never know if we had (Popper, 1983, p. 26).

The beauty of this methodological approach is that one need not believe in one's own ideas, nor do ideas have to be "credible". As we have seen, even very credible universal theories have been falsified, and the most incredible ideas from one historical period, light is both a wave and a particle, become the "real" explanations of a later period. Neither commitment nor consensus is necessary. In fact uncritical commitment (often the result of early parenting or authoritarian institutional pressures) can be detrimental to the search for truth because it enhances our tendency for confirmatory bias (Klayman, 1995). All that is required, is that one have an interesting problem and some guesses⁷ about what to do, together with the willingness to subject those ideas to the strictest tests by attempted falsifications. This entails our detailing before the fact, what empirical event following our test would clearly falsify our theory. This is essential in order to avoid post facto efforts to save, that is justify, such favored theories by ad hoc explanations after apparently successful efforts at falsification (Popper, 1968, p. 42). Such ad hoc justificatory explanations explain why most contemporary research rarely admits to the empirical falsifications found in the research literature. But, instead, explains them away as

anomalies resulting from the circumstances of the particular research project or the complex nature of the populations or the interventions (Gomory, 1998; 1999). The inevitable result is that such theories live on well passed their empirically worthwhile lives because the political economy of the domain of research brooks no competition with the institutionalized theoretical framework (see for example Gomory, 1999; 1998; Kutchins & Kirk, 1997; Dawes, 1994; Kirk and Kutchins, 1992; Boyle, 1990; Szasz, 1987; Gellner, 1985a, on Mental Health, Lewontin, Rose, Kamin, 1984; Kitcher, 1982; 1985, on biology, and even more controversially, Lang, 1998; Mullis, 1998; Duesberg, 1996, on AIDS research).

BRIEF CASE EXAMPLE: The Handbook of Empirical Social Work and the reality of Schizophrenia

The Handbook of Empirical Social Work (1998) exemplifies the difficulties of relying or basing our claims on justified knowledge rather than on knowledge which has survived severe attempts at falsification. For example in their introductory chapter the editors make the following statement,

Few doubt that ... [t]he phenomena labeled *bipolar disorder* are real (regardless of the current fad in diagnostic criteria) and exert their deleterious influences on the lives of clients and their families. Of course there are gray areas – the validity of repressed memory syndrome, multiple personality disorder, and late luteal phase dysphoric disorder are a few examples. But schizophrenia was similarly gray 100 yrs ago (p. 4).

The quote appears to suggest that such entities as bipolar disorder and schizophrenia are real mental disorders as conceptualized by DSM-IV (p. ix). This is of some moment because all mental health researchers agree that the phenomena “are real” (i.e. that there are unpleasant, bizarre, frightening, violent or unwanted behaviors), the disagreement that exists is about how to conceptualize and explain such problematic behaviors. The claim that mental illness is a myth is not about the non-existence of problematic behaviors but of their categorization as medical disease (Szasz, 1987). How we answer this empirical question determines our societal and professional responses (treatments and interventions).

The editors of the handbook are ambivalent about this and suggest that the phenomena be labeled “behavioral, affective, and intellectual disorders to avoid an unwarranted etiological inference” (p. x). However, they do not explain how that renaming changes the admittedly vague etiological inference of the term disorder contained in their and DSM-IV’s definition or what empirical research “discovered” these alternate conceptual entities. Despite the proviso, they entitle their first volume, *Mental Disorders* and use the DSM-IV categories as the basis for their “credible reviews of [the]

⁷The use of the word guesses is not meant to trivialize what is being done, but to point out explicitly what laws, theories and hypotheses, no matter how much authority they carry, really are, “we must regard *all laws or theories as hypothetical or conjectural*;

contemporary empirical literature pertaining to” these entities (p. 18). The experts selected by Thyer and Wodarski to write these reviews are even more explicit in their agreement with justificationary psychiatric explanatory theories. They claim for example that one successful form of individual therapy for schizophrenia based on evidence is John Rosen’s direct analysis (Thyer & Wodarski, p. 254). The reviewers appear to be unaware of the fact that the only thing direct about this approach was the physical assault which Rosen committed on his clients and called treatment at least as adjudicated by our court system (Szasz, 1976, pp. 118-120).

It should be pointed out that these theories are a priori hypotheses and assert that mental disorders are medical disorders, most likely brain disorders, based on belief not rigorous tests. Such claim making has been going on at least since Emile Kraepelin invented dementia praecox, in 1896 (1919/1971), a time when there was no scientific technology to test such claims (Gomory, 1998). Silvano Arieti, one of the acknowledged leaders of psychiatric science in the 20th century described Kraepelin’s belief driven research agenda thusly,

Kraepelin’s aim was to define disease entities based on definite organic pathology, and he always felt that dementia praecox was ultimately based on a faulty metabolism, although he could never prove it (Arieti, 1959, p 456).

These claims continue today when testing is more feasible. Dr. Steve Hyman, the current Director of the National Institute of Mental Health a 102 years after Kraepelin puts it this way,

Schizophrenia is not a cultural artifact but a brain disease in which vulnerability is caused by genes; something happens during brain development that converts this genetic vulnerability into disease. Exactly what happens is the subject of neuroscientific research (Hyman, 1998).

The authors of the chapter on schizophrenia in the Handbook of Empirical Social Work Practice, say that schizophrenia, “is considered along with ... Bipolar Disorder and unipolar depression to be one of the major mental disorders” (Thyer & Wodarski, 1998, p. 245). More particularly these experts claim that for schizophrenia, “the biological basis [of] this brain disorder has been established” (p. 264). The apparent well-justified claims are the following a) we have established that schizophrenia is an illness b) we know it to be a brain disorder. Although these expert reviewers offer no references in their chapter for “supporting” these claims, they do reflect the current institutional psychiatric beliefs about the existence of this alleged entity and its site in the human organism, as publicized through the National Institute of Mental Health’s Decade of the Brain program launched in 1990.

that is, as guesses” (Popper, 1983, p. 90).

From the perspective of a Critical Rationalist in order to consider these claims about schizophrenia to be well tested, at a minimum we would want to see the research that has identified a reliable and valid clinical entity called schizophrenia, or found some neurological or physiological marker of the disease, or has isolated a coherent, non-random empirical entity (syndrome) that could be called schizophrenia, or found an etiology of the disease, or shows some neuropathological change attributable to this putative brain disorder. To evaluate the question of the reliability and validity of the DSM categories we have the work of two social work scholars, Stuart Kirk and Herb Kutchins, who have for several decades published on these issues. Their many publications, including two excellent scholarly books, undermine any claim to reliability that the DSM has, either in its most recent or previous versions (Kutchins & Kirk, 1997; Kirk & Kutchins, 1992). Without reliable categories one can't even begin to discuss issues of validity., but as they note,

There is, however, one ironic advantage of problems of reliability: they make it possible to forget about the messy problems of validity. Preoccupation with the consistency of clinicians' judgements ... has the attraction of avoiding the issue of the ... definition and meaning of disorder (Kirk & Kutchins, 1992, p. 31).

One wonders why our profession is so reluctant to utilize the work of its own rigorous scholars. Instead of, for example, providing a serious examination and evaluation of the available critical and supportive psychiatric literature on mental disorders, most schools of social work offer students psychopathology courses that appear to be no more than "responsive readings" of the psychiatric bible⁸.

With respect to the other issues listed above, Dr. Nancy Andreasen, the psychiatrist who was the chair of the Schizophrenia work group for DSM IV, and is perhaps the leading researcher of this 'disorder', can serve as our guide. Her recent writings have the following to say about this illness.

On the value of the DSM Schizophrenia definition

After receiving the 1999 Adolph Meyer Award of the American Psychiatric Association, Andreasen in her lecture following, said this about the definition that she helped construct for DSM-IV, "The DSM definition may have distracted us from the real illness by over emphasizing symptoms and even the wrong ones" (Andreasen, 1999a).

⁸ In contrast, the March 2000 issue of the British Medical Journal the letters to the editor section contains the kind of exemplary fallibilistic discussion about the problematic nature of the concept of schizophrenia among mainstream professionals that is so rare here in the United States (pp. 800-801).

On the status of biological markers for mental illnesses

There are at present no known biological diagnostic markers for any mental illnesses (Andreasen, 1997, p. 1586).

On what the most important problem is in contemporary schizophrenia research

At present the most important problem in schizophrenia research is not finding the gene or localizing it in the brain and understanding its neural circuits. Our most important problem is identifying the correct target at which to aim our powerful new scientific weapons. Our most pressing problem is at the clinical level: defining what schizophrenia is. (Andreasen, 1999b, p. 781).

What we now know about the alleged brain changes attributable to schizophrenia

It appears that psychotropic medication may be the real culprit, as the critics have charged for years, rather than the alleged disease for the brain volume and dopamine receptor density differences found in schizophrenic patients,

For many years, it has been assumed that medications affect brain chemistry and physiology but not structure. Recent reports suggest that neuroleptic medication [for schizophrenics] changes basal ganglia volume. ... **Results:** During the 2-year period, mean basal ganglia volume of patients receiving predominantly typical neuroleptics increased, while the opposite was observed for patients receiving mostly atypical neuroleptics. (Corson, Nopoulos, Miller, Pharm, Arndt, & Andreasen, 1999, p.1200).

And,

The greater number of dopamine D2 receptors in the basal ganglia of patients with schizophrenia has been a replicable finding in postmortem neurochemical studies of schizophrenia. Such an increase in receptor density is considered by many to be a consequence of neuroleptic treatment and has been shown to occur in animals as well as humans following chronic exposure to typical neuroleptic medication (p. 1203).

Despite these apparently unambiguous findings Andreasen continues to lead the justificatory charge in psychiatric efforts to validate these “diseases” because she is a believer⁹,

The long-term goal is to achieve a “scientific psychopathology”: to identify the neural mechanisms of normal cognitive processes and to understand how they are injured in mental illnesses. The work of neuroscientists studying two common mental illnesses—schizophrenia and depression—illustrates the consensus that is developing (Andreasen, 1997, p. 1586).

⁹ It also doesn't hurt that such commitment is amply rewarded by the pharmaceutical industry whose multi-billion dollar business in psychotropic drugs provides generous grants for research and consulting to those researchers who do the “basic” biological research (Wong & Gomory, 2000).

It should be noted by all believers in the inductive methodology “supporting” the consensus that is developing about the reality of schizophrenia, that the latest version of the DSM, DSM-IV-TR (2000) is forced, based in part on the deductive research of Andreasen and her colleagues cited above, to admit openly for the first time that brain size difference (larger of smaller Basal Ganglia volume) earlier claimed to be the result of having a schizophrenic brain is actually the iatrogenic result of the treatment,

A ... finding that has been consistently replicated is that of increased [or decreased] basal ganglia size, but there is increasing evidence that th[ese] may be ... epiphenomen[a] of treatment with typical [and atypical]neuroleptic medication(p. 305).

These results falsify some of the oldest “well-supported” schizophrenic brain claims (although ad hoc explanations for why this is not so are sure to follow from the justificatory psychiatric experts).

FOR SOCIAL WORK KNOWLEDGE TO ADVANCE: PROVIDE FALLIBILISTIC CRITICAL THINKING FOR AUTOMOUS PROBLEM SOLVING NOT PRACTICE GUIDELINES PER SE

In order to determine what is better and what is worse for effecting good clinical practice, a method for problem solving is necessary. This fundamental cognitive activity is prior to “rules of the game” like practice guidelines or Evidence Tested nee Evidence Based Practice (EBP). EBP and practice guidelines can only operate effectively if the conceptual entities defining and describing the problem situation are valid (i.e. being sure that mental illnesses are brain disorders). If they are not, we may be incurring enormous societal costs with no direct client benefits. There are for example, clinical practice guidelines for the use of eye movement desensitization and reprocessing therapy (EMDR) from the International Society for Traumatic Stress Studies (Chemtob et al., 2000), approved by the National Center for PTSD, Department of Veterans Affairs as an effective treatment for post traumatic stress disorder (PTSD), even though rigorous critical revues of this method have falsified both it efficacy and the theory undergirding it (Lohr et al., 1998). Practice guidelines and EBP at best are methods for determining the traffic rules of good practice and are arbitrary (you could drive on either the left or the right side of the road as long as everyone agreed, and maintain safe traffic). Critical thinking skills must already be present in the professional’s armamentarium in order to do EBP or develop practice guidelines. As Gray (1997) one of the leading proponent of EBP states,

The skills required of a practicing evidence based decision maker are: an ability to define criteria such as effectiveness, safety, and acceptability... find articles on the effectiveness, safety, and acceptability of a new test or treatment; ... assess the quality of evidence (p. 2).

Since justificationism as a knowledge building system uses “inductive” methodology to seek out observations that provide “credible” supporting evidence for previously held uncritical belief, relying on “consensus” expert authority for choosing what’s better or worse, critical thinking should be based on Fallibilism. It provides an alternative for resolving what is unresolvable under justificationism; the demand for proof of what is more credible or more likely true. Fallibilism asserts that we cannot find such proof and it is not needed in the first place. What we do know is very little, and that little bit is only conjectural and is determined through a competitive eliminatory trial and error falsificatory process. Following this display of honesty and modesty we should proceed with getting very busy at “guessing and testing” explanations and interventions to help our clients reduce in some measure their daily misery.

Fallibilistic Critical Thinking

Fallibilistic critical thinking (Fa.C.T.) for social work begins by identifying and specifying in some detail the real world problems (P1) of our clients (i.e. being very unhappy, poor, unskilled/uneducated/unsocialized etc.,). This should include information about the environment; broadly defined as the economic, physical and psychological incentives and constraints which impact the actors, together with background information such as client learning history and development. The next step is hypothesizing a possible tentative solution (TS) or effective intervention for the problem which can be tested empirically. Developing a critical test for the TS designated as error elimination (EE) follows this. The test will vary depending on the nature of what is being tested (i.e. rigorous intellectual debate in politics, randomized control trials in medicine), but should be the most rigorous permitted by the problem situation. The last step is the actual testing through attempted falsification of the favored hypothesis. If the test is passed, we can continue using the idea, theory, intervention, or policy as tentatively true and available for application to potential new problems, always remaining vigilant for potential negative feedback about our TS by actively seeking feedback. If the TS fails the test (in practice several¹⁰) we abandon it and hypothesize new alternatives (TS), eliminating in so doing false knowledge (the previous TS now falsified) and discovering tentative true knowledge (the yet unfalsified TS) leading in either case to a new set of problems (P2)(Gomory, 2000). This Popperian fallibilistic critical thinking can be symbolized thus: P1→TS→EE→P2 (Magee, 1999, p. 151), indicating that it is a recursive process. Solutions to or mitigation of some

problems (P1) simply lead to other problems (P2) which inevitably arise as unintended consequences of human activity¹¹ (i.e. societally well intentioned welfare support to needy families leading to societally undesired welfare dependency, or affirmative action policies implemented to correct discrimination leading to “reverse” discrimination). The Fa.C.T. formula after attending to the unintended problem looks like this $P2 \rightarrow TS \rightarrow EE \rightarrow P3$. Regardless of the outcome, the result is always a new problem requiring our attention. As the title of one of Karl Popper’s books indicates, All life is problem solving (1999).

Kirk (1999) and a host of other social work authors suggest that most clinicians are not interested in becoming “avid readers of research ... [p]ractitioners ha[ve] neither the time, skills, nor incentives” (p. 303). Perhaps that is because most social work research was and is of very poor quality, and may not be worth the effort. It might also be the case that the profession, especially the schools of social work, has failed to see the value of teaching good argumentation, sometimes called the Socratic method, using well tested evidence to eliminate worse ideas from those which are better (more comprehensive and more effective) and therefore more explanatory.

Fa.C.T. Evaluates Guideline Utility

The employment of Fa.C.T. to determine the value of guidelines for clinical practice leads to the following analysis. Professional social work is expected “to enhance human well-being” (NASW, 1997) and help people with a variety of problems in living, “for the most part, social work intervention assumes that a client has a problematic condition that is to be altered, prevented or ameliorated” (Kirk, 2000, p. 16). A part of the necessary problem solving process involves understanding how clients define (socially construct) their reality to make sense of a very confusing “out there”. Without clarity about the client problem (P1) first, it is hard to see how a “taxonomy of the targets toward which interventions are directed” (Proctor & Rosen, 2000, p 2), the tentative solution (TS) and error elimination (EE) of Fa.C.T., would be developed autonomously, or be of much utility, as Kirk’s analysis of Proctor and Rosen’s example of domestic violence (2000, p. 6) indicates,

¹⁰ Logically, one falsification of a hypothesis suffices, but in the real world “replications” are necessary, not for purposes of generalization (an impossible task since induction fails), but in order to make sure that experimental confounds are not providing false information about test results.

¹¹ Popper suggests that it is such unintended consequences that should hold most of our attention when it comes to social policy (Magee, 1985, p.77).

The outcome selected by the [clinician] is not the only or obvious target when a woman has been abused. ... A shelter placement may be the most appropriate intervention in this case, but how could a practitioner possibly determine what is most appropriate without considerable understanding of the nature, dynamics and history of this woman's experiences in her marital relationship (2000, p. 14).

Proctor & Rosen (2000) focus on the growth of intervention knowledge and the development of guidelines by emphasizing the development of a taxonomy of targets of intervention (p. 2). This approach is questionable. First, their insistence that "Guideline statements must ... direct practitioners to select the best interventive program, from an array of alternatives" (p. 13) requires a justificatory, inductive "weighing of evidence", an empirical impossibility. Only those programs, which are ineffective or less effective when subjected to falsifying tests, may potentially be identified and eliminated as argued earlier. Second, targets of interventions identified as "helpful" in whatever sense for groups or populations in the past, are not necessarily in any meaningful sense going to be helpful to a particular client and his problem in the future. Past cumulated experience (data) of whatever quantity or quality cannot unproblematically apply to future unknown persons, their problems, or circumstances. Generalization from the known to the unknown are logically incoherent as explained earlier. Suicidal risk factors identified through statistical analyses of high-risk groups (past information) are incapable of predicting which individual among this group will commit suicide or when (future information) (Gomory, 1997a), and hence offer no helpful information about potential effective treatment (i.e. inpatient commitment does not prevent suicide, see Gomory, 1997a). Past experience is analyzed through statistical methods which by definition look at group means (abstractions) and have nothing to say about individual behavior, the focus of clinical attention (not in small measure due to our almost complete scientific ignorance about the cognitive process of volitional behavior, Chomsky, 1975, p. 138; McGinn, 1993). For a taxonomy of targets or clinical practice guidelines to be more useful than Fa.C.T. plus some relevant data bases for reviewing the critical experimental research identified by Fa.C.T., for example by the guideline or taxonomy providing the correct approach to help the client, they would have to be infinitely comprehensive and variable while being reliable and valid for all situations. If they cannot maintain these characteristics consistently (i.e. they are only sometimes useful and sometimes not) there appears to be little reason to have them in the first place. Proctor and Rosen provide us with the answer,

Practice guidelines are not immutable and infallible knowledge statements. On the contrary, such statements should make clear that in practically every situation, practitioners will encounter gaps in knowledge (2000, p. 16).

And

Because of the tentativeness and uncertainty that characterizes scientific knowledge, inclusion of interventions in guidelines should always be conditional, subject to further evidence and re-evaluation, and to replacement as a result of newly developed and more appropriate alternatives (2000, p. 12).

If we have to critically evaluate guidelines anyway and cannot unproblematically “rely” on them we have gained no advantage over simply employing Fa.C.T. alone. As Proctor & Rosen admit,

For practice guidelines to function well as aids to selection and use of interventions, they must take account of the variables in the practice situation that impinge on and qualify the effectiveness of interventions (p. 14).

It’s hard to see how guidelines, which tend to be voluminous documents containing pages of instructions and “a treatise on the subject, including such topics as the definition, natural history, ... epidemiology ... treatment principles ... and other factors” (Kirk, 1999, p. 305), are going to allow for such flexibility and subtlety.

The cumbersome nature of guidelines is complicated further by the method of selecting the best interventions and reports about them to be included in guidelines. The recent Cochrane Collaboration which is “an international effort to review every randomized clinical trial in medicine ever conducted” (Howard & Jenson, 2000, abstract) in order to “best” inform expert opinion is illustrative of the problem. The Collaboration formed a group to evaluate the randomized controlled research on Assertive Community Treatment (ACT), hypothesized to be the most effective community treatment for the severely mentally ill. The group found that,

ACT is a clinically effective approach to manage the care of severely mentally ill people in the community. ... Policy makers, clinicians, and consumers should support the setting up ACT teams (Marshall & Lockwood, 1998, p. 2)

These findings dramatically differ from Gomory’s research findings that,

A reanalysis of the controlled experimental research finds no empirical support for any of ... [the] claims. Instead there is evidence that the program is both coercive and potentially harmful. The current promotion of PACT appears to be based more on professional enthusiasm for the medical model than upon any benefit to the client (1999, p. 147).

The Cochrane Collaboration experts may or may not necessarily be making the latest or the greatest information available in a critical fashion. They claim that, although possessing some shortcomings, PACT is an effective program. Gomory disagrees. A careful scholar/clinician interested in accurate information would have to be informed about both

reviews to critically evaluate the issue, and may also have to go back to the original research reports of each randomized controlled trial to be sure that the reviews themselves were not in error as to fact or analysis. But, the reality of the problem situation is that the Cochrane Collaboration has been “authorized” as a source of information for Evidence Based Practice (Adams, 1994, p. 185-188) and Gomory has not. So a person would have to proactively search for Gomory’s review, but may not, because they may be content with the advertised authority’s report and as a consequence may, if Gomory were to have the better analysis, be “relying” on mistaken claims and perhaps harm rather than help clients. What this means is that the information clinicians and others are receiving from various established “evidence based” data sources may or may not be the best available and clinicians would need to have the critical thinking skills to determine on their own if that is so or not. The effort of developing practice guidelines versus training social workers to be autonomous decision-makers may promote expert authority, but not necessarily good scientific practice. Guidelines further must assume that the problem conditions or outcome domains for which the guidelines are provided are “real” or as argued earlier, valid. For example the Practice Guideline for the Treatment of Patients with Major Depressive Disorder (Revision) (2000) must assume that the condition “major depressive disorder” is a real medical condition. But this is precisely what is being contested by those who are critical of the mainstream view of biological psychiatry (Breggin, 1991; 1997; Cohen, 1989; 1994; 1997; Szasz, 1987). Guidelines assume justified knowledge about the state of affairs of a problem where very little may as yet be known. In addition, taxonomies by their very nature as categories tend to restrict and reduce information (Kirk, 2000). If the taxonomic categories are in error (i.e. schizophrenia is not a brain disease but simply some random grouping of dimensional behavioral deviance, or very inept strategic responses to specifiable environmental constraints and incentives for example) the treatments employed (i.e. psychotropic medication, involuntary hospitalization) might cause great harm.

Finally Proctor and Rosen’s recommendation of researching among other things, the “means of enhancing provider adherence and compliance to guidelines” (2000, p. 23) raises the unpleasant issue of coercion. It would seem that the insistence on clinician compliance can only be broached if we are able to assert that we have the correct set of guidelines or taxonomies to help clients, an issue hardly as yet decided. Promotion of adherence and compliance is simply coercion based on authority otherwise, and not in any way defensible for restricting the clinician’s independent search for interventions.

CONCLUSION

It is clear that no method presently, be it practice guidelines or EBP, is able to determine what are the best treatments or interventions for various problems, though one method (Fa.C.T.) may be superior over the others in eliminating those that are ineffective or harmful. And while guidelines may have some general heuristic value, there appears to be no intrinsic advantage to them over the application of Fa.C.T. So, although there is every reason to critically explore all potentially helpful ways of increasing the impact of science on social work clinical practice, and the present volume is making a serious effort in that direction, the profession might need to take a closer look at the more fundamental issue of the validity of the present epistemology of practice, by testing it against the alternate explicated here, fallibilistic critical thinking.

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