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## Tautology and Coercion in Assertive Community Treatment (ACT): The "Treatment Effect" of Assertive Community Treatment Deconstructed

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Assertive Community Treatment Deconstructed.

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## Abstract

Assertive Community Treatment (ACT) has been identified as one of only six evidence-based practices for the severely mentally ill by federal, private foundation, and professional mental health experts. This article reviews the research of the inventors of ACT (the Madison Wisconsin ACT group) because their model is the criterion for all ACT replications. The focus is on the well known, but mysterious “disappearance” of ACT effect when ACT “interventions” cease. The analysis concludes provocatively that there is no ACT clinical effect in the first place. What actually is measured by these researchers and claimed incorrectly as “clinical” treatment effect is the artifactual residue of a combination of tautological administrative rules and coercive bureaucratic tools (e.g. “financial payee” mechanisms).

Were consensus a correct basis for inference, then a once flat earth must have become spherical ... Consensus itself requires no further justification, and may be based on shared beliefs that are irrational.

Kenneth J. Rothman, Department of Epidemiology, Harvard University

### **Introduction**

Assertive community treatment (ACT or PACT) has been identified as one of only six evidence-based practices (EBPs) for the severely mentally ill (SMI) recommended for national implementation by the Evidence-Based Practices Project sponsored by the Robert Wood Johnson Foundation (Mueser, Torrey, Lynde, Singer, & Drake, 2003). Professor of Social Work Mary Ann Test a clinical psychologist, psychiatrist Arnold J. Marx and psychiatrist Leonard I. Stein developed ACT over 30 years ago in Madison Wisconsin at Mendota State Hospital. (Stein & Test, 1985, pp. 9-10). It was considered to be an immediate treatment success, being labeled a *Gold Award: community treatment program* (Test & Stein, 1976) following its first randomized controlled study (Marx, Test, & Stein, 1973), and has come to be “widely recognized as an evidence-based practice for adults with severe mental illness ... with a research base includ[ing] 25 well-controlled studies” (Bond, Drake, Mueser, & Latimer, 2001, p. 155).

Along with psychiatry, social work the “helping” profession with the largest number of professionals in mental health practice also promotes ACT enthusiastically. In a recent text, *Social Work Practice in Mental health* (2002), Sands and Angell (2002) call ACT an “exemplar program” which “demonstrate[s] how effective mental health teams work”, (p. 272). ACT is now a federally recognized Medicaid-reimbursable treatment program as well (Bond et al., 2001, p, 147). Its success according to the consensus of its academic admirers is based on:

Reviews of the research [which] consistently conclude that compared with other treatments under controlled conditions ... [ACT] results in greater reduction in psychiatric hospitalization and higher levels of housing stability. The effects of

assertive community treatment on quality of life, symptoms and social functioning are [however] similar to ... other treatments. (Phillips, Burns, Edgar, Mueser et al., 2001, p. 771)

For example one of the latest reviews states:

In agreement with most other reviews we conclude that ACT substantially reduces psychiatric hospital use, increases housing stability and moderately improves symptoms and subjective quality of life, but has little impact on social functioning. (Bond, Drake, Mueser, & Latimer, 2001, p. 149)

The empirical consensus on ACT research thus attributes only two outcomes firmly to ACT when compared to alternate treatments: reduced hospitalization and greater community housing tenure. Other essential outcomes desired in the treatment of SMIs are no better achieved in ACT than in such far less “aggressive” community treatments (for definition of aggressive community treatment see Dennis & Monahan, 1996) as “brokered case management or clinical case management” ([Phillips et al., 2001, p. 771](#)). It should be noted however, that a start-up manual commissioned by the National Alliance of the Mentally Ill (NAMI) and co-authored by two members of the original Madison ACT team, social worker Deborah Allness and psychiatrist William Knoedler (1998), goes beyond these generally recognized but limited findings. Citing an “unpublished” but widely referenced paper “Long term care of schizophrenia: Seven year results” presented at the 1994 Annual Meeting of the American Psychiatric Association on the Madison ACT team’s long-term study (Test, Knoedler, Allness, Kameshima, et al., 1994) it asserts in addition that:

The PACT intervention group demonstrated relative to the control group  
Less time in a combination of hospitals, skilled nursing facilities, penal  
settings and conditions of homelessness (Allness & Knoedler, 1998, p. 5)

Political supporters, like NAMI use this particular paper’s findings to forcefully promote and lobby state and federal legislators on behalf of ACT. Its web site declares that:

Thirty years of research already demonstrates that ACT is more effective than office-based traditional care for people with the most disabling psychiatric illnesses. ACT reduces the most devastating outcomes of severe psychiatric disorders, *including hospitalization, homelessness, and criminal incarceration.*

Frustrated by the nation's continued failure to provide the effective treatment that researchers know works, NAMI decided to "take the bull by the horns" and promote the ACT model throughout the country. . . . NAMI commissioned two developers of the ACT model, Deborah Allness, M.S.S.W., and William Knodler, M.D., to put their knowledge into writing. They wrote *The PACT Model of Community-Based Treatment for Persons with Severe and Persistent Mental Illnesses: A Manual for PACT Start-up.*

([http://www.nami.org/Content/ContentGroups/Programs/PACT1/PACT\\_Across\\_America\\_An\\_Advocacy\\_Strategy.htm](http://www.nami.org/Content/ContentGroups/Programs/PACT1/PACT_Across_America_An_Advocacy_Strategy.htm), emphasis added)

Despite this early "success" at least one widely noted problem has been identified:

Following discharge from [ACT] . . . most of the gains made by the [ACT] patients were lost, and by the end of [the] . . . follow-up period their community tenure and functioning were quite similar to that of the control group" (Test, Knodler, Allness, Senn Burke et al., 1991, p. 240).

The ACT developers note that:

It is not clear why, across a range of empirically supported biological and psychosocial interventions [in current popular academic psychosocial parlance EBPs], the positive effects of treatment end when the intervention ends, but *a likely hypothesis* is that the underlying psychobiological vulnerabilities and /or deficits of *schizophrenia* persist for many patients (p. 240, emphases added).

This statement points out that the Madison ACT team is only hypothesizing, that is *guessing*, that the apparent treatment effect disappears and does so because of a set of underlying vulnerabilities particular to schizophrenia. But this hypothesis as a valid explanation is made problematic by the ACT inventors' own research finding that the ACT "effect" was lost for *all* program participants, even those who had diagnoses other than schizophrenia, "the patients had a wide range of diagnoses, and approximately 50% were schizophrenic" (Stein & Test, 1980, p. 393).

Despite the apparent failure of the schizophrenia vulnerability "hypothesis" to explain the general loss of ACT treatment effect in the experimental group this hypothesis has apotheosized into the well-accepted "fact" that the "chronic nature" of the illness prevents cures and requires ongoing treatment. Other academics had found similar problems in their own work with the severely mentally ill providing further support for this explanation. For example, Gerard Hogarty is quoted in Test, Knodler, Allness, Kameshima et al. (1994) as saying "When the intervention ends, so does the effect" (p. 5). The Madison team used this "fact" to gain funding from the Wisconsin Department of Health, the University of Wisconsin and from the National Institute of Mental Health (Test et al., 1985, p. 17) to:

Research ... the effects of ongoing long-term community care and treatment ...  
[R]esearch in this area could make a major contribution to knowledge about the course of severe mental illnesses and the factors that effect them. For instance, it could answer the question; can the functional impairment that develops and persists in schizophrenia be lessened by a longitudinal clinical strategy? (p. 19)

This study begun in January 1978 was designed originally as a "ten-year treatment and follow-along study" (p. 19), but may have been far longer. Elsewhere, the principle investigator claims, "we are ... implementing a 12-year prospective controlled study of ... long-term community treatment" (Test, 1992, p. 159). The published results of this long-term study corroborate only the oft-reported earlier claims that ACT reduces hospital stays and promotes

“sustained community tenure” better than control or alternate treatment (Marx, Test, & Stein, 1973). The first published report of these findings, based on data from the first six months of the study, were made in 1985 ([Test et al., 1985](#)), and were confirmed, using 2-years of data on their full sample, six years later ([Test et al., 1991](#)). No other data on outcome findings for the study have been published since, despite the fact that:

Both the founders of the model ([Marx et al., 1973](#); [Stein & Test, 1980](#)) and its current implementers in Wisconsin ([Test et al., 1985](#)) target goals for the model as going far beyond the reduction of time in hospitals. ... Additionally, improvements in patients’ psychosocial functioning and quality of life are sought. (Test, 1992, p. 164)

The researchers make clear that they “are well aware of the problems when hospitalization is used as a measure of outcome, and ... look forward to the examination of ... findings on other measures of psychosocial functions” ([Test et al., 1985, p. 25](#)). As they explain, “a change in the site of treatment says nothing about whether the patient’s clinical status or functioning has improved. Some would argue that only the place of a person’s suffering has changed” ([Test & Stein, 1978, p. 360](#)).

The Madison ACT group identifies the acquisition of coping skills as one of the necessary psychosocial goals of ACT. They identify this as a key treatment goal in their very first research article:

Treatment concentrated primarily on patients’ acquiring coping skills necessary to live in the community and enjoy a reasonable quality of life- the acquisition these skills would take place in the community. (Marx, Test & Stein, 1973, p. 506)

Sixteen years after declaring their intention to provide additional information on ACT effectiveness imminently, [Test and Stein \(2001\)](#), responding to my critique in *Psychiatric Services* ([Gomory, 2001](#)) on their limited published psychosocial outcome data admit that:



Dr. Gomory ... points to the shortage of evidence about the effects of assertive community treatment beyond the well-documented reductions in hospitalization, and he accurately notes that we have not yet published data on psychosocial outcomes from a long-term study of assertive community treatment ... [D]ata analyses and writing are now our primary focus, and readers can be assured that all dimensions of the findings will be published as soon as this work is completed. ([Test & Stein, 2001, p. 1396](#))

Three years after this second reassurance and twenty-six years after the beginning of the long-term study not a single publication providing further data on this study's psychosocial outcomes has appeared.

The present article argues that instead of ACT clinical treatment effect disappearing when ACT interventions cease, there is no ACT clinical treatment effect to begin with. This is so, if by clinical treatment effect we mean that some specified clinical/biological components of ACT motivated or caused internalized/volitional change, or the "acquisition of coping skills" by ACT patients, which lead to patients' improved functioning that results in reduced hospital stays and greater "independent" community tenure.

The specific claim is as follows: *no* superior clinical efficacy or treatment effect can be attributed to ACT clinical methodology when compared to no treatment or standard treatment. Any statistically significant impact putatively favoring ACT is either one or a combination of the following:

- a tautological outcome based on administrative/bureaucratic rules differentially applied to ACT and control groups, or
- are misattributions of aggressive/coercive ACT worker activity as client outcome, or
- are based on manipulation of data to indicate clinical and statistical significance for variables that are not supported by the data (by for example creating

variables that subsume previously independent outcome variables, some of which are statistically significant but are tautological, and some that are not statistically significant but empirically important).

### **Methodological Note**

The present article offers a critique specifically of the Madison ACT team's controlled studies and does not undertake a comprehensive review of the many ACT replication studies extant. The rationale for such a review of studies implemented mostly in the 1970s and 1980s is that all the contemporary ACT replications uniformly accept and rely on the claims made by the Madison group regarding the validity of the original Madison ACT research findings and the methodology employed. In fact, the latter generation of ACT proponents have developed fidelity scales to check how closely replications resemble the Madison model (Teague, Bond, & Drake, 1998) because "the more closely case management programs follow ACT principles, the better the outcomes" (Bond et al., 2001, p. 149). The largest randomized and controlled replication ever done on ACT, a Department of Veterans Affairs multisite demonstration project with over 800 clients is typical:

The VA ... program was designed through a comprehensive literature review supplemented by consultation from national experts in the Wisconsin Program of Assertive Community Treatment (PACT) (Stein & Test, 1980). ... Consultation, including several site visits and attendance at program-wide orientation and training meetings, was obtained from an expert in the PACT approach. Efforts were made to improve program implementation at sites where the model did not appear to be fully implemented. (Rosenheck & Neale, 1998, p. 193)

Another group of long-time ACT researchers, Bond et al. (2001), describe ACT's working assumptions and its impact on SMI treatment research thusly:

They ... hypothesized that, to avoid ... high failure rate [due to relapse after intensive hospital supports were removed], community programs needed to

replicate the array of medical, residential rehabilitation and other services provided by the hospital. That is, community programs needed to create a “hospital without walls.” ... Stein and Test’s initial study involved deflecting patients presenting for hospitalization at a state hospital. One group received PACT services, whereas the comparison group received the standard community services. *Results clearly demonstrated the advantages of the PACT program across a range of clinical and social outcomes.* ... The study by Stein and Test has been hugely influential in mental health services research; it is probably the single most cited study in the literature on psychosocial treatment of mental illness in the twentieth century. (p. 146, emphasis added)

Because of this broad professional acceptance and consequent reliance on the ACT originators’ research by the newer replication studies, if some or all of these well-accepted results turn out to be in error then the replication studies basing their work on these erroneous findings may also turn out to be flawed.

I have purposely stayed away from doing a “systematic empirical review” or a meta-analysis, the standard fare of current scientific research attempting to prove the evidence-based efficacy of psychiatric treatment, and rely instead on inferential reasoning of another sort. It is the “traditional methods of reflection, tracing of connections, [and] reaching tentative conclusions” (Bauer, 2000 p. 20).

A formulaic reliance on statistical significance testing and arbitrarily aggregated statistical findings can be harmful for good scientific work (McCloskey, 1985; Oakes, 1986;) and may obscure or mask deeper methodological problems such as the erroneous construction of the variables used for a study’s statistical analysis or the ignoring of the historical development of ideas, concepts, or theories fundamental to a research project (for example, see Gomory, 2002 for the history of the idea of ACT and its relation to coercion). As the distinguished economist Peter Bauer has suggested in another context:

The acceptance of quantitative methods as the most respectable [scientific] procedure has permitted the burgeoning of incompetent and inappropriate econometric studies, including those based on flawed data. Conversely, studies based on direct observation or detailed examination of slices of history are apt to be dismissed as anecdotal, unscholarly or unscientific... In short, preoccupation with mathematical and quantitative methods has brought with it regrettable atrophy of close observation and simple reflection. ... This type of reasoning ... has retreated not because it has been proved less informative ... [but] because it has been castigated as ... less rigorous than its more modish successors, largely because it less resembles the procedures of the natural sciences, especially physics. (Bauer, p. 20)

### **Assumed Treatment Effects**

#### **Reduced Psychiatric Hospitalization**

##### *Claimed ACT effect*

“The most consistent effects of the ACT ... models were in the areas of reduced time spent in hospitals” (Mueser, Bond, Drake, & Resnick, 1998, p. 47).

##### *Actual causal explanation*

There is *no* “clinical ACT effect” helping patients acquire proper skills or behaviors that will reduce the likelihood of ACT patients being hospitalized. What reduces hospitalization in ACT is a simple administrative rule prohibiting hospitalization of ACT patients regardless of their symptomatology (with extremely rare exceptions), while control treatment routinely hospitalizes its patients with similar symptomatology. All of the early research publications by the Madison ACT team are explicit about this simple administrative maneuver. Their first ACT publication, *Extrahospital management of severe mental illness* (Marx, Test, & Stein, 1973) lists second among its program guidelines the “[v]irtual abstention from rehospitalizing any patients being managed in the community” (p. 506). A follow-up publication, *Alternative to the hospital:*

*A controlled study* (Stein, Test, & Marx, 1975) states the administrative rule thusly; “In-hospital treatment, even with extremely symptomatic patients, is virtually eliminated” (p. 519). In their second study using a 14-month follow-up evaluation they describe the administrative process for the ACT patients this way:

Patients coming to the hospital for admission are immediately interviewed by a member of the community staff, and are then taken from the admission office to the community to begin their treatment program. Every effort is made to avoid hospitalization. ... In the rare instances when hospitalization is used, it is of short duration so that “community treatment” can begin with minimal delay. (Stein & Test, 1978, pp. 46)

It is clear from the prior quotes that the move to the community, even by highly symptomatic people is an administrative decision *preceding* the “treatment program” and not the result of ACT clinical work. The initial rapid move to the community and the subsequent refusal to (re) hospitalize symptomatic individuals in the ACT experimental group and instead treating them in the community regardless of “symptom” severity, by definition (tautologically) reduces the hospital stays of such individuals when compared to control treatment patients who, due to an absence of such a rule are routinely hospitalized. Borland, McRae, and Lycan (1989) explain how what are usually thought of as in-patient treatment activities are carried out in the community with highly symptomatic ACT patients:

Case managers provided services assertively; for example, they gave hospital emergency rooms and the state hospital a list of project patients and asked staff to call anytime a project patient presented to the facility in crisis. Case managers actively sought to prevent patient’s ... readmission to the hospital. ... They provided in-home medications when indicated. When crises occurred, case managers used program resources to provide an alternative to hospitalization. (p. 371)

Test and Stein (1978) fully recognize that it is the rule and not any treatment that is responsible for reduced hospital stays:

Community treatment results in less time spent in the hospital. This finding is certainly not surprising since experimental patients were usually not admitted to hospitals *initially* and there were subsequent concentrated efforts to keep them out. (p. 353-354, emphasis added)

Scott and Dixon in their 1995 review of ACT see this obvious tautological mechanism as well:

The effectiveness of ACT models in reducing rehospitalization may be a function of their capacity to control hospital admissions, length of stay and discharge. (p. 659)

We know this to be the case because where the ACT team does not have control over ACT patients' hospitalization, ACT fails to reduce either hospital stays or their length compared to control treatment. A British replication of ACT (Marks, Connolly, Muijen, Audini et al., 1994), called a "daily living program" (DLP) for example, found that "removing DLP responsibility for discharge from any in-patient phase of care prolonged admission. ... Removing responsibility trebled the length of admissions" (p. 185). Another recent study (Minghella, Gauntlett, & Ford, 2002), where "the team had little influence over admission and discharge and no medical input" (p. 27) found "significant increases ... in ... clients' use of in-patient psychiatric hospital beds during the year after acceptance by the [ACT] teams, compared to the year before" (p. 34). I hypothesize that *any* community treatment rigorously enforcing an *a priori* "no-hospitalization rule" would be just as successful in reducing hospitalization as ACT. And further, that reduced hospitalization is not influenced by any putative clinical ACT effect (none has been identified by ACT researchers). No clinical ACT effect for reducing hospitalization exists.

### **Improved Community Tenure**

*Claimed ACT effect*

“Assertive community treatment results in ... a higher level of [community] housing stability”  
(Phillips et al., 2001, p. 771).

*Actual causal explanation*

The “higher-level of housing stability” is the direct result of the increased community presence of ACT patients due to fewer hospital stays resulting from the previously identified administrative rule not to hospitalize ACT patients regardless of symptomatology along with the routine use of the “financial payee” mechanism by this group. Here again, Stein, Test, and Marx (1975) freely admit this:

These results may be viewed as tautological: i.e., reduction of time in the hospital  
“creates” increased time in productive community living. (p. 521)

This arbitrary rule against hospitalization combined with the aggressive efforts of ACT workers to keep the ACT patients in their community residences versus the routinely hospitalized control treatment patients’ corresponding necessarily reduced community presence due to their frequent hospitalizations, is the entire ACT tautological package masquerading as “clinical program effect”.

It is *the worker* who finds the community housing and determines its degree of autonomy or normality (another measured variable touted to be an outcome of ACT treatment) not the patient. Professor Test explains that the ACT staff “work with patients to find ‘normal’ kinds of housing such as apartments; few of our patients live in supervised settings” (Test, 1992, p. 157). More explicitly, the ACT workers avoid using any “supervised settings” if at all possible. This explains why ACT clients have more frequent autonomous settings than control patients, who have no such worker efforts undertaken on their behalf:

In the area of *housing*, we help clients find rooms and apartments in the  
community rather than using ...specialized residential settings. (Test et al., 1994,  
p. 4)

Furthermore, ACT makes sure that the patient stays in the housing no matter what, often via “financial payee” mechanisms. These mechanisms entail the appointment of staff to manage and disburse the various disability payments received by the patients regardless of patients’ preferences.

Since ACT research uses the length of community stay as a critical outcome measure (it is by far the most frequent finding after reduced hospitalization), rather than whether or not the patient is primarily responsible for and actively participates in efforts to obtain and retain the housing, it is easy enough to mistake community length of stay as an outcome of patient activity. Psychiatrist Ronald Diamond, a close associate of the Madison ACT team, describes the use of “financial payees” in the mental health system in Wisconsin this way:

A significant number of the clients in the community support programs in Dane County, Wisconsin ... have been assigned a financial payee who controls their SSI or Social Security Disability Insurance (SSDI) income. ... This kind of coercion can be extremely effective in helping to stabilize someone in the community. The use of a payee can ensure that rent and utilities are paid.

(Diamond, 1996, pp. 56-57)

Without the “financial payee” mechanism (a general bureaucratic procedure not specific to ACT) most ACT clients would lose their community housing, since this mechanism specifically removes the responsibility for actually paying rent or utilities from the patient and transfers it to the “payee.” Is this what we mean by enhancing patient choice and effective treatment?

The quality of that community presence is also not related to any ACT specific clinical interventions. It is rather due to the misattribution of ACT worker activities for “volitional” patient activities. This misattribution explains in part the “mysterious” finding that as soon as the worker activity (ACT clinical effect) is stopped so does the patient activity: there aren’t any autonomous patient activities in the first place. Increased “independent” patient behavior apparently is mostly the work actually done by the ACT workers:



Stein and Test (1978) worked side by side with patients in the activities of daily living, employment, and socialization areas and achieved favorable results in these areas as long as the treatment lasted. (Test & Stein, 1978, p. 14)

The workers “mak[e] frequent visits to patients’ residences, assisting them in performing daily living skills such as food preparation, personal hygiene, and health care” (p. 157). This includes providing:

Therapeutic input ... consist[ing] of motivation, supporting, and often “being by patients’ sides” day and evening. ... [P]atients are aided in ... the development of effective social skills by staff “prodding” ... their involvement in ... social activities. This frequently includes staff members accompanying patients to such functions. (Stein & Test, 1978, p. 47)

This worker engagement can be quite coercive and apparently intended as such by the ACT inventors. Here is their description of what “assertive” means for this treatment approach:

The program was “assertive”; if a patient did not show up for work a staff member immediately went to the patient’s home to help with any problem that was interfering. ... Medication was routinely used for schizophrenic and manic depressive patients. (Stein & Test, 1980, pp. 393-394)

And another:

When a patient did not show up for work one day, the psychiatrist accompanied other staff members to the patient’s home and got him out of bed and off to his job. (Stein & Test, 1976, p. 268)

And finally:

An extremely directive and assertive approach by treatment staff is necessary, ... a staff person attempting to assist an ambivalent patient to a sheltered workshop in the morning is likely receive a verbal and behavioral “no” ... with the ... question, “Would you like to go to work this morning?” If, however, the staff

member approaches ... with a firm, “It’s time for you to go to work; I’ll wait here while you get dressed,” the likelihood of compliance increases. (Test & Stein, 1976, p. 77)

ACT substitutes the authority and actions of the worker for the decision making of the vulnerable patient. An ACT home visit may be as invasive of personal privacy as rousting a patient out of his bed in his apartment and forcing him to go off to “work” if the staff deems it to be therapeutic despite the explicitly stated desire of the patient to the contrary.

Based on this self-description by the inventors of ACT of how ACT “motivates” patients it becomes difficult to claim that ACT is training patients to internalize the value of work activities. No internalization does take place since patients do not continue the coerced activities once the coercive ACT treatment is removed. The patients’ behaviors are responses to intrusive and aggressive statements or threats to withhold disability payments and other benefits by ACT psychiatric authorities if the patients refuse to comply. The true explanation for the alleged disappearance of ACT treatment effects is the removal of coercion, not the alleged chronic nature of the “illness.”

The ACT approach assumes that the paternalism of ACT workers and experts is best for the patients. As Diamond reports:

Paternalism has been part of assertive community treatment from the very beginning. ... [I]t was designed to do for the client what the client could not do for himself or herself. Staff were assumed to know what the client “needed.” Even the goal of getting clients paid employment was a staff-driven value that was at times *at odds with the client’s own preferences*. Current assertive treatment programs continue to be influenced by traditions. ... Paternalism continues to be reinforced by mandates from the community to “control” the behavior of otherwise disruptive clients. (Diamond, 1996, p. 53, emphasis added)

Regardless of what one might think about the ethics of these coercive “treatment” approaches none of these techniques as evidenced by the published research results of the Madison ACT model lead to improvement in patients’ psychosocial functioning. The latest research on ACT coercion (euphemistically called “therapeutic limit-setting activity” by ACT experts) shows that patients subjected to them do worse than those who are not (Rosenheck & Niele, 2004). Coercion may change briefly, observed behavior, but appears not to change the hearts and minds of those coerced after the aggressive ACT activities cease (see Dennis & Monahan, 1996, for detailed discussion on aggressive community treatment).

### **Reduced Homelessness and Penal Stays**

#### *Claimed ACT effect*

“ACT reduces the most devastating outcomes of severe psychiatric disorders, *including hospitalization, homelessness, and criminal incarceration*” (NAMI, 2004). This quote on the NAMI web page gives the impression that ACT independently affects all three important patient outcomes.

#### *Actual causal explanation*

Several ACT outcome variables, some statistically significant (hospitalization) and others non-significant (homelessness and penal stays), appear to be inappropriately combined in the long-term Madison ACT longitudinal study giving erroneous impressions of ACT effectiveness in domains where none were actually achieved. The researchers state in part that:

Analysis of data from the first seven years of this investigation has been completed. To date the ACT intervention group demonstrated relative to the control group, ... [l]ess time in a *combination* of hospitals, skilled nursing facilities, penal settings, and conditions of homelessness. (Allness & Knoedler, 1998, p. 5, emphasis added)

Specifically, they assert that the patients in the experimental group did significantly better in a “combined” category labeled “all poor settings” (time spent in hospitals/skilled nursing

homes + penal settings + homelessness) than the control group over the whole experimental period (Test et al., 1994). This variable appears to suggest that the experimental program not only reduces time spent hospitalized and in skilled nursing homes, (these settings are considered to be the same by the researchers, Test, Knoedler, Allness, Burke, Brown, & Wallisch, 1991, p. 243), but that it *also* significantly reduces, independent of reduced hospitalization, the amount of time spent in homelessness, and independent of either of the other components, time spent in penal settings. These are exactly the outcomes we would want such programs to impact and this appears to be the way NAMI has interpreted these results.

The Madison ACT inventors report this combined variable for the first time in 1994, some 16 years after the inception of the long-term study in 1978. They introduced it in an “unpublished” paper presented at the 1994 annual meeting of the American psychiatric Association (APA) (Test et al., 1994). Their previous published articles on this long-term study analyzed each of these variables separately, as was also done in their earlier studies (i.e. Stein, Test, & Marx, 1975). This *innovative* “summing” of the three independent variables yielded statistically significant measurements favoring the experimental group at certain, *but not all* measurement periods. What this combining of previously discretely measured variables conceals, is that the only statistically significant difference between the experimental and control groups among these components was on the component variable “time hospitalized/time in skilled nursing homes”. Neither the “homelessness” nor the “penal settings” component was statistically significant between experimental and control treatment for the first two years of the study when measured independently. Any claimed statistical significance for the combined variable had to have been driven by the statistically significant difference found for the “time in hospital/nursing home” component.

The only way one could have know this was by reviewing an earlier article, which gave the two year results of the long-term study, but did not mention any “combined” variable (Test, Knoedler, Allness, Senn Burke, Brown & Wallisch, 1991). Subjects in the experimental group

spent significantly ( $p = .001$ ) less mean time, from study entry through 24 months, in hospital/skilled nursing home settings than the controls (Test et al., 1991, p. 243). But when it came to the two-year findings on homelessness and penal settings, which constitute two-thirds of the combined variable “all poor settings”, the researchers state that,

[W]e also studied time spent in jail or other penal settings and in homelessness or homeless shelters. Throughout the first 2 years the time that patients in both groups spent in these settings was small and did *not differ significantly between the groups*. (Test et al., 1991, p. 244, emphasis added)

The paper introducing this new “combined” variable is cited in the publication, *The ACT Model: A Manual for ACT Start-Up* (Allness & Knoedler, 1998) and in many other publications supportive of ACT, to promote the claim that ACT is an effective long-term treatment for “all poor settings” (e.g. Mueser et al., 1998, Wasner, Pinkerton, Dincin, & Rychlik, 1999).

Beyond the just quoted statistically non-significant findings for homelessness and penal stays in the early two year data described in Test et al., 1991, neither Professor Test nor any other member of the Madison ACT team has ever published *any* data related to either this “combined” variable or on homelessness and penal stays as separate outcome variables. It is clear from my unpublished review of the 1994 paper, contrary to the Madison ACT team’s misleading claims, neither homeless conditions nor penal settings measured *independently* differ significantly between the experimental and control groups. This holds true not only for the first two years, but for the balance of the seven years as well. It is difficult to see why this combined variable was created so late in the analysis of the long-term study other than to suggest that there was a program effect in reducing homelessness and jail time, when none in fact exists.

An intriguing postscript surfaced just recently. Professor Test wrote a short article for the 2002 edition of the *Social workers’ desk reference* entitled “Guidelines for assertive community treatment teams.” This piece was published about a year after my criticisms appeared in

*Psychiatric Services*. In describing ACT effectiveness when compared to alternate treatments

Professor Test states that ACT obtains:

Marked reduction in days spent in inpatient settings, *with no greater time spent homeless or in jails/prisons*. (Test, 2002, p. 513, emphasis added)

Professor Test is unmistakably, if somewhat confusingly indicating that ACT *does not* reduce homelessness and jail time any more than control treatments while reiterating the long standing finding of reduced hospital stays. Her statement concerning homelessness and penal stays is unequivocally clear while the earlier statement by her colleagues, the co-principle investigators of the long-term study of which she is the principle investigator appears to be murky by design. To repeat:

To date the ACT intervention group demonstrated relative to the control group, ... [l]ess time in a *combination* of hospitals, skilled nursing facilities, penal settings, and conditions of homelessness. (Allness & Knoedler, 1998, p. 5, emphasis added)

This 2002 admission by Professor Test agrees with my analysis that when compared to alternate treatment only inpatient stays are reduced at a statistically significantly greater rate by ACT, while homelessness and penal stays are not. It also raises some serious questions about the Madison ACT's earlier "published data" (actually only published assertions, no data was ever provided) and the rationale behind the methodology of combining these variables for their promotional publications.

A concrete example of how the studied obtuseness of "[l]ess time in a *combination* of hospitals, skilled nursing facilities, penal settings, and conditions of homelessness" can lead to the convenient factual misrepresentation that ACT is effective on each of the individual variables separately rather than when summed together can be found in a 1997 edited volume of a very popular publication series on "New Directions for Mental health Services" by Jossey-Bass titled, *The successful diffusion of innovative*

*program approaches*. Citing the aforementioned 1994 APA paper the author, one of the original authors of the 1994 paper Deborah Allness, misstates the ACT research findings to say, “relative to the control subjects, the PACT intervention group showed ... less time being homeless **or** in hospitals and penal settings” (1997, p. 22). This way of putting the findings does not mean the same as PACT being effective in a summative combination of these settings.

### **Conclusion**

This article closely reviewed all the major claims made by the supporters of ACT for justifying it as an evidence-based treatment model. After analyzing the publications of the ACT originators despite these assertions no clinical ACT effect was found for:

- Reduced hospitalization
- Longer and more autonomous community tenure
- Reduced Homelessness and
- Reduced Penal stays

Instead, this review finds that an administrative rule to keep ACT patients out of the hospital is routinely enforced and tautologically reduces these patients’ time spent in the hospital, in turn by definition resulting in more time spent in community settings when compared to control patients not subjected to such rule restrictions (prohibition from being in the hospital=being in the community).

Further, the autonomous quality of the community settings of ACT patients is similarly the result of an administrative rule. This one requires that all ACT patients be placed into independent community apartments and rooms rather than specialized community residential settings. This rule tautologically results in more independent settings for ACT patients when compared to control patients who are not subjected to the same rule.

Finally, the ACT originators oddly combine three previously independently measured variables, hospital stays, homelessness, and penal stays. These when summed together as “all poor settings” were found by the Madison ACT team to be statistically significantly impacted by ACT. The present review after separating the three variables comprising “all poor settings” and reviewing the relevant research finds no clinical ACT effect successfully targeting independently homelessness or penal stays. Both homelessness and penal stays are statistically non-significant when separated from the statistically highly significant tautological finding of reduced hospitalization. Professor Test in a very recent publication (Test, 2002), interestingly enough following my criticisms of ACT, (Gomory, 2001), fully agrees with this analysis. This leaves the question of why the ‘all poor settings’ variable was created in the first place?.

All of the present findings can be subjected to empirical testing. For example, to test my claim that reduced hospitalization is due to the rule that *all ACT patients are to be treated in the community regardless of symptomatology* and not to ACT clinical treatment, a future ACT randomized and controlled trial should simply reverse the administrative application of this rule. The “control” community treatment patients would be kept in the community and treated there regardless of their symptoms while the ACT patients would be routinely hospitalized if they become highly symptomatic in the community. The aim would be to see if the “control” treatment now becomes the more “effective” community treatment by having fewer patients hospitalized. If ACT despite the rule change continues to have fewer hospital admissions than the control treatment, then we can conclusively attribute this outcome to ACT clinical effect as is now asserted by ACT experts. If not, then I am correct and no ACT clinical effect exists for reduced hospitalization. All the other findings could be evaluated similarly.

Why despite these obvious difficulties ACT remains aggressively marketed may in part be explained by the failure of institutional and biopsychiatric treatment efforts in general (Breggin, 1997; Fisher & Greenberg, 1997; Valenstein, 1998). It is also consistent with current trends to resort to increasingly coercive approaches (Torrey & Kaplan, 1995).



The conventional wisdom, promoted by the National Institute of Mental Health (NIMH), that behavioral disorderliness (mental illness) is medical/brain disease organizes and restricts the vast majority of potential research into helping interventions for seriously troubled persons almost exclusively to the biomedical model. This is the only model accepted as “scientific” by institutional psychiatry and its powerful political lobbies (i.e. American Psychiatric Association; NAMI; the pharmaceutical industry), despite the lack of clear evidence for this approach to “mental disorderliness”.

The Surgeon General’s recent mental health report (1999) for example, confirms the conjectural nature of claims about the biological basis of “mental illness”, “there is no definitive lesion, laboratory test, or abnormality in brain tissue that can identify [mental] illness” (p. 44). And the March 22, 2004 Federal Drug Administration (FDA) Health Advisory asking the manufacturers of the antidepressants Prozac, Zoloft, Paxil, Luvox, Celexa, Lexapro, Wellbutrin, Effexor, Serzone, and Remeron, to “include ... a warning statement that recommends close observation of adult and pediatric patients treated with these agents for worsening depression or the emergence of suicidality” (FDA, 2004) should also give one pause. Depression and suicidality are the “medical” problems these drugs are supposed to biologically remedy not iatrogenically worsen.

The profession of social work, a major player in the mental health field also supports this medical perspective. A recent analysis of graduate programs of social work psychopathology classes identified a clear bias toward the medical model in these classes despite the availability of rigorous research, some done by social workers, highly critical of it (Lacasse & Gomory, 2003).

NIMH’s over one billion dollar annual budget sends a powerful signal. Researchers must attempt to find solutions that support and justify mental illness as medical/brain disorder if they expect to be funded. The ACT model fully embraces this paradigm. As one ACT originator, psychiatrist Leonard Stein along with his co-author, the previously referenced psychiatrist, Ronald J. Diamond, freely admit:

Congruent with our conceptual model, we tell our patients that indeed we believe they are [medically] ill, otherwise we would not be prescribing medication for them. (Stein & Diamond, 1985, p. 272)

ACT's early results appeared to demonstrate treatment success (Gomory, 1998). By accepting the tautological arguments of the Madison ACT group the ACT replicators assumed the empirical validity of that model uncritically and committed to duplicating the Madison group's administrative rules and structure in their "independent" replication studies. This, not unexpectedly, insured that similar findings would result since all the findings for ACT effectiveness depend on a more or less strict adherence to ACT administrative rules and the use of coercive bureaucratic mechanisms as described earlier. These replication findings conveniently lend circular "support" to the original tautologically derived ACT results.

Once research careers are established around specific, well-entrenched treatment paradigms, supported by politically savvy constituencies and are anointed as "evidence-based", the need for self-justification rarely allows self-critical admissions of error. Contradictory evidence is ignored or explained away.

Olfson (1990a), in a draft report for the National Institute of Mental Health on ACT, identified rule-regulated hospitalization; the very high use of antipsychotic medications and their well know adverse effects (i.e. tardive dyskinesia); and the routine use of none blinded raters for administering the ACT clinical measures in ACT "controlled experimental" studies as serious problems that should be addressed in future ACT research. The article resulting from Olfson's report, published in *Hospital and Community Psychiatry* (now *Psychiatric Services*), does not mention any of these specific concerns. Referring to them only generally with the comment that they are "methodological issues of interest mostly to researchers" (Olfson, 1990b, p. 640). Virtually all-subsequent ACT replication studies have ignored these important issues.

ACT—a long-term, expensive, self-admittedly paternalistic program—continues to be promoted as "evidence-based" despite results that negate claims of ACT clinical treatment effect.

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