Commentary: Treatments for Enuresis: Criteria, Mechanisms, and Health Care Policy

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Mellon and McGrath (this issue) have done an admirable job of applying the modified Chambless criteria (Task Force on Promotion and Dissemination of Psychological Procedures, 1995; Chambless et al., 1996), as suggested by Journal of Pediatric Psychology editors, to the outcome literature on psychological treatments for medically uncomplicated nocturnal enuresis in children. This brief commentary addresses three issues. First, what can we learn from the past? Specifically, how can we place psychological treatment for enuresis in historical context and also in the current context of a general movement within clinical psychology to respond to a managed care environment by identifying empirically supported psychological treatments? Second, and in light of that history of psychological treatments, why should we take seriously the conclusion that “psycho” therapies, as contrasted with behavior therapies, hold any promise for the treatment of children’s bedwetting? In other words, there is something very mistaken with a criterion that has resulted in a blessing even so mild as “promising” for such “psycho” therapies. What might be added to a criteria set to correct this type of mistake? Third, and finally, how has it happened that the most effective treatment for a problem that affects 10% of secondary school-age children has remained so underutilized for over half a century? Something is seriously broken with our health care system and our culture of caring for children who wet the bed. What kind of health care system routinely foregoes curing bedwetting and opts instead for mere palliative treatment that costs considerably more than a cure?

History and Current Context of Psychological Treatments for Enuresis

We are fortunate to have the case of treatments for enuresis to teach a historical lesson about the pitfalls of our current enthusiasm for empirically supported treatments. The treatment of enuresis has a long and very colorful history dating as far back as 1550 B.C. Glicklich (1951) recounted two instructive examples of “treatments.” In West Africa, children who wet the bed were “treated” by attaching a large frog to their waist, and this apparently frightened them into being dry. Among the Navaho tribe, one preferred treatment was a ritual that required enuretic children to stand naked over a burning bird’s nest, and this was believed to produce a cure of bedwetting because birds did not soil their nests. We may snicker at these practices of the past, but the laugh is really on us. These practices worked, and they worked on a variable interval schedule of reinforcement because they were occasionally followed by the spontaneous cessation of bedwetting, something that happens for about 16 out of every 100 children within a 12-month period. It is no wonder then that so many peculiar treatments have been tried, and so many odd practices have persisted. In some ways what we really need to know about treatments for bedwetting is which treatments definitely do not work.

Such a Popperian approach to the issue of identifying empirically supported treatments is counter-
intuitive for most of us who have viewed science as an inductive process. Ollendick (1999) recently expressed the logic of current thinking regarding the empirically supported treatment movement.

Surely “treatments that work” are desirable and their development and promulgation should be encouraged; after all, to argue the converse, that “treatments that do not work” should be developed and disseminated hardly seems tenable and makes little sense for a profession committed to the welfare of those whom we serve (Ollendick, 1999, p. 1).

Quite the contrary and from a methodological point of view, I believe that what we need most urgently is to identify treatments that do not work. In the case of much positive psychotherapy outcome research for various mental disorders, we already know that people get better with the passage of time, and we believe some inert treatments are effective. Whenever we study a problematic condition such as bedwetting with a natural course toward remission, we need to be particularly attentive to the frog effect and to the burning bird’s nest effect. Otherwise, our cumulative wisdom will include all manner of procedures that have “worked” because we may mistake applying the procedure for the natural developmental resolution of the problem.

The process of identifying treatments that definitely do not work could benefit us in two ways. The obvious benefit is to rule out ineffective procedures. Knowing what definitely does not work would clear the air and purchase a small degree of progress even if we could not as yet identify a procedure that did in fact work. One of the great difficulties with developmental problems such as bedwetting is that at any given time, many different things can appear to work. The scientific task is to weed out procedures that may appear to work from those procedures that can be trusted to work reliably, not only because they have passed muster in numerous randomized clinical trials but also because we have verified knowledge about the mechanism through which they produced positive outcomes.

Second, identification of treatments that do not work could provide much needed psychological placebo control groups. For quite some time, treatment outcome researchers have needed a nonpill placebo condition. If a psychological treatment is compared to a pill placebo, the comparison is never exactly parallel because the placebo condition consists not only of an inert treatment but also of the expectancy that one is receiving medication as opposed to a nonmedication treatment. A procedure believable as a psychological treatment but known to be ineffective would provide the much needed parallel placebo condition for various psychological treatments.

The history of the treatment of childhood enuresis has demonstrated that verbal psychotherapies have not produced outcomes that were much better than pill placebo controls (Houts, Berman, & Abramson, 1994). Some research in the 1960s included treatments such as supportive counseling and psychotherapy (De Leon & Mandell, 1966; Werry & Cohrssen, 1965). However, by the mid-1970s, most psychological research had moved on and was devoted either to developing alternative behavioral procedures based on operant conditioning (e.g., Azrin, Sneed, & Foxx, 1974) or to improving urine alarm treatments within Lovibond’s (1963) avoidance learning formulation (e.g., Young & Morgan, 1972). Mellon and McGrath have presented much of that history in their review, but they have not been nearly so shocked as I have been to find the return of verbal psychotherapy as “promising.” It is important to reiterate that this judgment was fairly enough required of Mellon and McGrath, because they duly applied the modified criteria for empirically supported treatments.

In the interest of returning psychotherapy for enuresis back to the realm of the repressed, I offer the following analysis of the verbal psychotherapy studies reviewed by Mellon and McGrath. This analysis also leads to discussion of the problems with criteria for declaring some intervention “promising.”

Repressing Psychotherapy for Enuresis

Mellon and McGrath have included four reports on the use of hypnosis to treat enuresis and one report that included a cognitive self-control treatment. In the analysis that follows, I have confined my remarks to the two controlled studies that used random assignment to conditions, because there is no point in considering less rigorous designs in the case of such alternative treatments for enuresis. The fact is that we have over 50 randomized trials showing the effectiveness of urine alarm treatment, so any new treatment has to meet a rather high standard.

Hypnosis apparently qualified as promising on the basis of one controlled study (Edwards & Van Der Spuy, 1985), which was also accompanied by
two case series and one quasi experiment. The controlled study provided a good illustration about how research reports can be misleading. Kenneth Spence was alleged to have said that he could advance psychology immediately if someone would just give him enough money to pay other people not to conduct and publish research. Edwards and Van Der Spuy (1985) compared hypnotic trance induction and suggestions with trance alone, suggestions alone, and no treatment. The statistical reporting in this research report was completely inadequate. The investigators did not assess pretreatment differences between the four groups, and they did not provide the data for a reviewer to do so. This was important because there appeared to be differences in the groups at baseline with respect to wetting frequency. The main analyses were conducted as repeated measures analyses of variance on Z scores of weekly wetting frequency during the 6-week treatment period, but it was impossible to reconstruct the actual data because no standard deviations (SDs) were ever reported. The authors stated that both suggestion conditions resulted in a reduction of wet nights that was greater than the no-treatment controls in a 6-week treatment period, but it is impossible to tell if the analyses were done correctly because the authors do not report degrees of freedom with their F tests. In the main analysis, no mention was ever made about the number of children who became dry during the treatment period for each of the four groups. What was later reported was that after a 6-month follow-up, only 19.4% of the children from the three hypnosis treatment groups had completely ceased bedwetting. That outcome speaks for itself. Hypnosis was not effective except when it was compared to outcome for no-treatment controls. The authors did not even make that comparison using their own no-treatment control data, but they relied instead on the no-treatment control results from another previously published study conducted by completely different investigators and published 20 years earlier. The results from this clinical trial were only marginally better than what could be expected from investigations of spontaneous cessation of bedwetting due to maturation and the passage of time. There is nothing whatsoever promising about hypnotic suggestion to treat bedwetting despite the fact that the literature contains one adequately designed, if poorly executed, study.

Similar problems occurred in the report by Ronen, Wozner, and Rahav (1992), where the authors reported outcomes for their new cognitive therapy for bedwetting. These investigators compared a urine alarm protocol without any relapse prevention component to their new cognitive behavioral intervention that consisted of some combination of self-monitoring, self-reinforcement, and learning self-guided talk. They also included a token economy intervention and a no-treatment control group in their study. The outcomes from this trial were also reported in two other publications (Ronen, Rahav, & Wozner, 1995; Ronen & Wozner, 1995), where the same mistakes from the original publication were repeated. The authors established a criteria of 3 consecutive weeks of dry nights for the designation of “dry,” but they reported statistical analyses that were inappropriate. They displayed a 4 Group (Urine Alarm, Token Economy, Cognitive Therapy, No Treatment Control) × 3 Outcome (Dry, Improved, Dropout) table of the percentage of children in each outcome category for each of the four groups as their Table 2. The text repeatedly referred to this table as Table 1, which in fact showed average wetting frequency for the four groups. The authors reported a chi-square statistical analysis with nine degrees of freedom on the data presented in their Table 2, and this resulted in a chi-square value with p ≤001. The appropriate degrees of freedom for that omnibus test should have been six, not nine. Nevertheless, that reported chi-square test showed that at least one of the several possible one degree of freedom tests within the 4 × 3 table was statistically significant. In fact, the chi-square test for the comparison of the 63.2% cure rate for the urine alarm with the 75% cure rate for the cognitive intervention was never reported, and when I calculated it, the chi-square value was less than one (.64) and associated with p = .42. Nevertheless, the authors stated in their abstract that “cognitive intervention was the most effective treatment method, as evidenced by the highest rate of success and the lowest rate of drop out or relapse” (Ronen, Wozner, & Rahav, 1992, p. 1). The one degree of freedom test for the relative difference in rate of dropout was never reported by the authors, and when I computed the correct test, there was no difference in dropout between the cognitive group and the urine alarm group. This erroneous result regarding rate of dropout was reported again by Ronen and Wozner (1995). As for the authors’ claims regarding differences in relapse rate, it was impossible to discern how they calculated a nine degree of freedom chi-square from a 4 Group × 2 Outcome (Remained Dry vs. Relapsed) table. When I calculated the appropriate test, the comparison of relapse rate...
between the cognitive intervention (15%) and the urine alarm treatment (60%) was statistically significant. At least one of the three conclusions stated in the abstract was actually supported by the data presented. The authors of this report did not address the question of how their results compared to so much previous research that has shown quite different outcomes. For example, if nothing is done to prevent relapse with the urine alarm, we have known for over 25 years that the relapse rate is likely to be 40% or greater. This was never addressed. Why did the authors use a urine alarm intervention without some relapse prevention method?

What was even more surprising was the finding that 15 out of 20 children who received the cognitive intervention in this study attained the dryness criterion. How could that have happened in light of the history of repeated failure of verbal psychotherapies for bedwetting? The authors did not comment on that issue. In the introduction to their article, they spoke about the role of conscious learning of daytime control of bladder function, but they never addressed the issue of how changing what children said to themselves during the day could produce a change in physical control over their bladder function during sleep. In a separate publication that appeared 3 years later, the authors noted that children in the cognitive intervention also practiced retention control training, and they practiced urine retention during the day (Ronen & Wozner, 1995). The children were also told that “bedwetting does not depend on bad luck or illness but, rather, is a function of motivation and willpower” (Ronen & Wozner, 1995, p. 10). Apparently, the children in the cognitive intervention group received rather extensive coaching and therapy sessions from the investigators, whereas the urine alarm and the token economy interventions were conducted by parents. All of the children in the no-treatment control group were lost to follow-up. In sum, there is considerable reason to doubt the care and methodological rigor with which this study was conducted and reported.

In light of the extensive history of failure of cognitive types of intervention for bedwetting, the outcome from Ronen, Wozner, and Rahav (1992) should be considered an anomaly. The fundamental idea that children willfully control their thought processes during sleep and the proposition that such cognitive control accounts for bedwetting are simply not plausible. If such propositions were true, then why do we have a history of 3,500 years of parents and professionals talking to children about stopping bedwetting with little or no results? The one treatment that we know to work reliably, the urine alarm, does not require cognitive control and most likely works via a process of active avoidance conditioning (Houts, 1991, 1995). The issue in this case is about burden of proof. When we have very strong evidence for a dominant effective treatment, as we do in the history of treatment for enuresis, and when we have such clear evidence for failure of cognitively mediated treatments, the burden of proof is on those who propose a new variation of the failed treatment and a mechanism that has been shown to fail in the past. Why did such a treatment appear to work in this particular trial? Where is the evidence that the comparison treatments were faithfully conducted?

Scientific reasoning occurs in a web of belief, and that reasoning is inherently conservative. We change our beliefs so as to produce minimal adjustment in the overall web of belief. In this sense, the criterion of declaring an intervention promising on the basis of at least one well-controlled study is misleading. One well-controlled study is not enough, as the case of cognitive treatments for bedwetting shows. The history of investigation in a domain needs to be considered, and the identification of treatments that do not work can be relevant in assessing claims for “new and improved” treatments. It is disturbing enough to think that talking therapy for enuresis may make a comeback, but it is even more disturbing to consider, as Mellon and McGrath have indicated, that the most effective treatment known for children’s bedwetting is rarely promulgated by our current health care system.

### Enuresis Treatment and the Health Care System

As Mellon and McGrath have noted in their review, the available evidence regarding what treatments children receive for bedwetting suggests that the front line service providers, pediatricians and family physicians, have traditionally favored medication treatments and have only rarely recommended urine alarm treatment. Although we do not have more recent direct surveys of medical practitioners, there is every reason to believe that the situation has worsened due to massive advertising by the pharmaceutical industry, to typical procedures of managed care organizations, and to the failure
insurance policies provide benefits according to what is determined to be medically necessary by a physician, and those insurance policies also typically have different reimbursement policies for providers of psychological services, who are most likely competent to deliver or supervise urine alarm treatments. A medical doctor may judge that urine alarm treatment is medically necessary, but the treatment may not be reimbursable because of limitations of the policy on nonmedical providers. Similar scenarios can and do occur in managed care organizations where enrollee children are restricted to certain provider lists, and many of those organizations relegate mental health services to master’s level providers without psychological training in how to use the urine alarm protocol. Managed care organizations commonly do not know about treatment outcome evidence in the case of bedwetting, despite the fact that they are interested in reducing costs and providing evidence-based treatments. As a result of these processes, children who are diagnosed with functional nocturnal enuresis are treated with what is reimbursable and what is familiar, namely, medications.

In addition to not having any easy channel of referral for urine alarm treatment, most pediatricians and family physicians are visited regularly by representatives of the various pharmaceutical companies that manufacture and advertise medication treatments for bedwetting. The culture of professional medicine and the culture of many parents support medication solutions to children’s problems. As someone who has followed these cultures surrounding the problem of enuresis for the past 20 years, I am not surprised that urine alarm treatment remains so underutilized. Perhaps it will take another 20 years for a treatment that was devised over 60 years ago finally to be provided on a routine basis to the almost 7 million children affected by enuresis in the United States.

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References


