

So Why Aren't Counselors Reporting $n = 1$ Research Designs?

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There is a growing urgency for counseling to be evidence based. The $n = 1$ research paradigm offers counselors in the field an opportunity to evaluate their clinical casework and report this in methodologically rigorous form to their peers. Data from the past 20 years of the *Journal of Counseling & Development* indicate that $n = 1$ studies constitute just over 1% of the total articles reporting research data, and possible reasons for this are discussed, with some suggestions made for overcoming this reluctance.

Several authors have urged readers of the *Journal of Counseling & Development* (JCD) to evaluate and report their applied clinical research using $n = 1$ research designs (e.g., Hiebert, 1984; Lundervold & Belwood, 2000; Miller, 1985; Sharpley, 1986). There have been similar calls for similar research in associated journals such as *Counselor Education and Supervision* (e.g., Fong & Malone, 1994; Tracey, 1983) and in other outlets for counselors and counseling researchers. Most recently, Lundervold and Belwood have argued that the use of $n = 1$ research designs is a "secret" (p. 92) and outlined a suggested training curriculum for counselor educators to follow in preparing their students to apply the well-established principles of $n = 1$ research to everyday clinical practice. There are several reasons why this call should be heeded by counselors, none so urgent as the need to adopt the "scientist-practitioner" approach to the evaluation of counseling as is done in associated areas such as clinical psychology.

The Randomized Controlled Clinical Trial and Counseling Practice

Referring to research bases for choosing a particular therapy approach with a particular client is often called "evidence-based" therapy or "empirically-supported therapy" (Kendall, 1998, p. 3). The most common research methodology used to justify choice of therapy is that of randomized controlled clinical trials (RCCTs), borrowed from medical research, and which aims to compare two or more medications for specific illnesses. Although this methodology has strong roots in medical science and has a proven track record in the investigation of medication and allied treatments, Persons and Silberschatz (1998) have commented that the application of this methodology to everyday presenting problems encountered by counselors in the field is ill-conceived and has produced little of lasting value to the counselor because of the lack of direct relevance between the RCCT and everyday counseling. In fact, Seligman (1995) suggested that the controlled clinical trial is the "wrong method" for empirically evaluating psychotherapy

because it omits "too many crucial elements of what is done in the field" (p. 966). Seligman argued that some of the critical differences between RCCTs and everyday counseling practice include (a) the fact that RCCTs offer a treatment for a fixed term, whereas real-life clients have access to their counselors for as long as they wish; (b) the freedom that counselors in the field have to vary their treatment regime if the client fails to progress, whereas RCCTs require therapists to maintain the treatment regime regardless of its ongoing effectiveness; (c) clients' ability to choose their counselor and change that choice if they wish (clients and therapists are allocated to each other in RCCTs); (d) the presence of multiple and complicated concerns by everyday clients (RCCT research clients are selected for a specific and clearly defined presenting problem); and (e) the complete focus in RCCTs upon symptomatology and indices of psychopathology, whereas everyday clients mostly are concerned with their overall functioning and general happiness.

Although they may be supported by the overall data from large-scale RCCTs, various medications do not have uniform effects on each individual patient who takes them. Similarly, Luborsky, McLellan, Diguier, Woody, and Seligman (1997) also showed that RCCT-recommended "standardized" psychotherapeutic treatments do not result in identical outcomes for all clients. Luborsky et al. examined the results of counseling offered by 22 therapists who treated seven different samples of patients, who were drug affected and depressed, with the same treatment manuals and hence the same therapy procedures, and they noted that "the range of percentages of improvement for the 22 therapists . . . was from slightly negative change to slightly more than 80% improvement" (p. 63). Most counselors would argue that they treat each client as an individual, and therefore might eschew evidence supporting a particular therapy if that evidence is based on data from a large RCCT, particularly in light of the questionable relevance of that methodology to everyday counseling.

Although it remains of value to evaluate therapies, there has been considerable evidence for several decades that most ther-

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apies are about equally effective (e.g., Smith & Glass, 1977), but the delivery of those therapies by particular counselors may differ. This move away from evaluating the therapy per se to the application of that therapy, by a particular counselor with a particular client, constitutes a major and worthy focus of counseling research; the change has led to a consideration of what Garfield (1998) referred to as “therapist variables,” in turn reflecting Truax and Carkhuff’s (1967) comment that research into differences between therapists was more valuable than research into comparisons of therapies themselves. Shapiro, Firth-Cozens, and Stiles (1989) commented that some therapists were more effective than others (even if they were using the same approach) and argued for an examination of the particular behaviors that successful therapists exhibited when counseling particular clients, a research question that is beyond the scope of RCCTs.

What Is $n = 1$ Research Methodology?

As indicated at the commencement of this article, counselors do have access to an alternative research methodology that can be of value to their evaluation of treatment options with everyday clients, that is, the $n = 1$ research paradigm. Barlow and Hersen (1984) wrote the major basic text on $n = 1$ research methodologies, but others have complemented it in both text and journal article format (e.g., Bloom, Fischer, & Orme, 1995; Cook & Campbell, 1979; Horner et al., 2005; Kazdin, 1984; Odom & Strain, 2002). There are also texts and articles that provide data analysis procedures, including Glass, Willson, and Gottman (1975); Gottman (1981); Crosbie and Sharpley (1989); and more recently Crosbie (1993). Although it has been described in much detail by these and other authors, and is therefore not the focus of this article, it is worthwhile to briefly outline what is meant by $n = 1$ research methodology within the field of counseling research and practice.

Essentially, an $n = 1$ research study is one in which the data from a single participant (rather than a group) are the focus of the research design. Data from that person/client/participant are gathered via application of a research design. When that research design is of the “Baseline-Followed-by-Intervention” or AB (i.e., where A = baseline and B = intervention) model, the data on whatever behavior is being focused on are collected in a series over time. Because that data series is interrupted by the change from baseline to intervention, this type of research is often referred to as an *interrupted time-series* (ITS) design. In this kind of experiment, each client/participant acts as his or her own control group, and data are analyzed by visual or statistical methods to make comparisons between data from baseline and intervention conditions. The basic $n = 1$ ITS research design therefore collects data from a person/client/participant during periods of “baseline” (when no intervention is applied) and then during an “intervention” and compares

these two data sets to determine if the intervention was responsible for behavioral change in the client/participant. The specification of what actual behavior is the focus of the data collection process is determined by the counselor/researcher on the basis of the client’s presenting problem or the topic of interest to the researcher. It should be noted that other statistical procedures, such as latent growth curve modeling, may be useful if comparisons are to be made between an individual’s data and those from a group. However, the focus of this article is not on such comparisons but on the individual’s responses to therapy per se as a means of adding to scientific knowledge bases and building evidence-based treatments in counseling.

As mentioned earlier, the basic research design used is AB, where A = baseline and B = intervention. However, because this simple design is open to various sources of experimental invalidity, such as history (i.e., any change in data from A to B might have been as a result of extraneous factors, such as holidays, crises, illness, etc.), this particular design is of restricted use to the clinician/researcher who wants to demonstrate that the intervention actually works (i.e., that the treatment has internal validity) and also is worth applying with similar presenting problems in other clients (i.e., is the effect of the intervention “generalizable” to other clients/participants?). Therefore, the potential internal validity and generalizability of the intervention can be strengthened if it can be “turned off” via a return to the baseline condition, thus giving an ABA design. If the effect noted during the intervention also turns off, the causal relationship between the intervention and the effect is more robustly established (i.e., validity is demonstrated). This methodology may be extended to an ABAB design, and the consequent effects of turning the intervention on and off may be determined and thus the relative effect of the intervention on the presenting problem behavior can be clearly measured. This ABAB design is called a “reversal” or “withdrawal” design. Data from such designs are not only more internally valid, they are more likely to be generalizable to other participants and settings.

Another way of determining the effectiveness of an intervention can be via collection of baseline data on the same presenting problem behavior from more than one client/participant, but over different time periods. In this design, Client 1 may have baseline data collected from say May 1st to May 20th, then receive the intervention. Client 2 may have baseline data collected from May 1st to June 1st, and then receive the intervention. By comparing the data collected on the presenting problem behavior during the baseline periods of both clients, it is possible to see whether the intervention per se resulted in changes in Client 1’s behavior or (if the same changes occurred in Client 2 during the extended baseline) whether the change in Client 1 was due to extraneous factors. This design is called a “multiple



baseline” design and may also be applied to different behaviors for the same client/participant. Other variants of the basic AB design offer procedures that limit the vulnerability of the research design to sources of experimental invalidity and may be sourced from the references listed earlier. However, whichever variant of the basic AB design is used, the variants are all ITS designs that aim to test the effects of the intervention (i.e., B) compared with nonintervention or baseline (i.e., A). More complete accounts of these research designs and their variants may be found in the references cited at the commencement of this section.

■ Do Counselors Use ITS Research Designs?

Bearing in mind this availability of *n* = 1 ITS methodology descriptions and accompanying data analysis packages, it is of interest to determine how much use is being made of these research methodologies to inform counselors in the field about their peers’ discoveries in terms of therapies and their application to everyday clients. Reporting these findings in journals such as *JCD* is a very relevant method by which interventions can be initially validated and then tested for validity and generalizability by other counselors, thus contributing to the evidence basis of the particular therapy used. To help determine if this is currently being done, each issue of *JCD* for the period 1982 (Volume 61 as *The Personnel*

and *Guidance Journal*) to 2002 (Volume 80) was perused and the articles were classified according to whether (a) they were commentary or descriptive articles or presented data on participants, (b) these data were gathered from group studies or with single participants, and (c) the single-participant reports were true *n* = 1 ITS research designs or just case studies with no real assessment of the effects of the treatment being reported. Table 1 shows the results of this perusal of 20 years of *JCD* articles.

Clearly, with only 1.02% of all articles that reported data from participants doing so via the application of *n* = 1 ITS designs, it appears that this research paradigm has not yet been enthusiastically adopted by the readership of *JCD* or its potential contributors. Although there may be a host of reasons for this lack of enthusiasm, ranging from ignorance of the procedures involved (as suggested by Lundervold & Belwood, 2000) to misunderstanding of the relevance and applicability of *n* = 1 ITS designs to common clinical casework, the issue of how to urge counselors to use and report this research methodology remains of central importance to any attempts to increase the scientist–practitioner reputation of counseling and the relative status of it as a therapeutic endeavor within the overall evidence-based mental health field. The remainder of this article addresses some of the possible reasons why counselors have yet to embrace the *n* = 1 methodology and suggests some avenues to remedy this.

TABLE 1

Published Articles and *n* = 1 Designs in the *Journal of Counseling & Development* From 1982 to 2002

Volume and Years	Number of Articles	Participants Included	Group Design and Analysis	Case Study Design ^a	<i>n</i> = 1 Design ^b	Method of Data Analysis in <i>n</i> = 1 Studies and Source
61 1982/1983	127	20	19	1	0	
62 1983/1984	146	32	32	0	0	
63 1984/1985	146	7	6	0	1	Visual (Harvill, 1984)
64 1985/1986	116	19	18	1	0	
65 1986/1987	141	24	20	3	1	Statistical (O’Farrell, Hill, & Patton, 1986)
66 1987/1988	113	15	15	0	0	
67 1988/1989	176	15	15	0	0	
68 1989/1990	135	22	20	2	0	
69 1990/1991	122	32	29	3	0	
70 1991/1992	133	23	23	0	0	
71 1992/1993	111	26	25	1	0	
72 1993/1994	121	26	26	0	0	
73 1994/1995	102	37	35	1	1	Visual (Martin-Causey & Hinkle, 1995)
74 1995/1996	102	17	17	0	0	
75 1996/1997	38	7	6	1	0	
76 1997/1998	55	4	3	1	0	
77 1998/1999	68	14	13	1	0	
78 1999/2000	55	14	13	0	1	Visual (Lundervold & Belwood, 2000)
79 2000/2001	56	18	17	1	0	
80 2001/2002	59	19	17	2	0	
Total	2,122	391	369	18	4	
<i>M</i> per volume	106.1	19.55	18.45	0.9	0.2	

^aDefined as reporting therapy and outcomes on a single client, but not using an appropriate *n* = 1 design (e.g., AB, ABA, ABAB) and presenting data according to changes during that design. ^bDefined as reporting therapy and outcomes on a single client via an *n* = 1 design plus data analysis via visual (i.e., graphs) or statistical procedures.

Possible Reasons Why Counselors Don't Report $n = 1$ ITS Designs

A Secret?

As mentioned earlier and suggested by Lundervold and Belwood (2000), counselors may not be aware of how to conduct $n = 1$ ITS research, thus arguing for counselor educators incorporating this into their training programs. A suggested content analysis has been presented by Lundervold and Belwood, and this is a good starting point for inclusion of the single-participant research paradigm within counselor training schemes. However, the content of that 7-point program is both uncomplicated and probably already present within many counselor training programs, at least enough to argue that more than 1% of research articles in *JCD* should be presenting $n = 1$ ITS research methodologies. It is thus difficult to accept that, as Lundervold and Belwood suggested, the only reason why counselors have not applied and reported $n = 1$ ITS research methodologies is because they have not been aware of it or it has been a secret. Almost all credible counselor training programs include some reference to research design, including $n = 1$ ITS procedures, and common texts in the field have devoted chapters to these topics (e.g., Bloom et al., 1995; Heppner, Kivlighan, & Wampold, 1992).

Misunderstandings About Procedures

Another more likely reason why counselors in the field do not report (or, at least, get published) their casework on single participants could be the lack of clarity about some of the basic procedures inherent in $n = 1$ ITS designs. There is ample evidence from texts (e.g., Heppner et al., 1992) and articles (e.g., Lundervold & Belwood, 2000) that even so-called authorities on this issue have uncertainties in their discussions of major aspects of the $n = 1$ ITS methodological procedures, and this may well be transmitted to the students who read these texts or attend classes in counselor education programs that purport to teach $n = 1$ ITS research methodology. Several of the major sources of error and consequent possible practitioner unease about applying $n = 1$ ITS designs are briefly discussed as follows.

Long baselines. Some early writers mistakenly claimed that very long data series were required for adequate baseline data collection. It is worthwhile noting that this issue has been put to rest some time ago by Gottman's (1981) exposition on this matter, in which he proved that baseline series of as few as 5 or 10 observations are suitable for reliable testing of intervention effects. This finding places $n = 1$ ITS research designs well within the reach of counselors who may choose to use the first interview (during which information is gathered and basic empathic counseling is offered to the client) to commence establishing the baseline. Then the client can be instructed to collect daily data on the behavioral manifestations of the presenting problem during the coming week before

the next appointment, thus providing at least 7 observations of baseline data. Although there is some evidence that even self-monitoring has an effect on behavior (Miltenberger, 2001), this conceptualization may be validly applied within the paradigm that classifies "true" intervention as something more than basic empathic counseling. An example of applying this process from my own experience includes asking the client to collect daily data on the various symptoms of his or her presenting problem (e.g., arguments with partner, incidents of anxiety, frequency of depressive episodes). In addition, clients can be asked to gather information about the antecedents and consequents of the presenting problem so that a functional analysis can be performed. These two steps allow the client's valued outcomes (Bitsika, 2003) from the presenting problem to become a target for behavioral change processes that are embedded within the counseling environment and therapeutic relationship. With a week between appointments, the client is able to collect at least 7 data observations, thus also allowing the application of interrupted time-series analysis (ITSA) statistical programs to any changes that may occur following the commencement of other interventions. As well as directly involving the client in data collection and thus contributing to the evaluation of treatment, this procedure may sometimes also start the therapeutic process via self-monitoring. This is an important point in supporting counselors' use of $n = 1$ ITS research methodologies because it lessens the ethical aversiveness associated with refraining from offering treatment to the client as soon as the first visit is undertaken. Counselors should, however, remain aware of the balance required between collecting "clean" baseline data that is entirely free from any intervention effects (including even having an initial session with the counselor) versus the need to respond therapeutically to the client's needs immediately. Thus, collection of baseline data in the manner described earlier is unlikely to always produce such clean data but must be seen as sometimes being a blend of baseline and intervention data. When collecting baseline data via client self-report, counselors always need to be aware of the potential bias in such data, whether it arises from the kind of $n = 1$ research designs described here or from in-session comments made by clients on any topic.

Data analysis procedures: Issues and packages. A second potential impediment to counselors applying $n = 1$ ITS research methodologies might be confusion about data analysis. During the 1970s and 1980s, there was considerable vigor in the exchange between those who argued for purely visual analysis of $n = 1$ ITS data (i.e., via graphs [e.g., Parsonson & Baer, 1978]) and those who suggested that statistical yardsticks were required (e.g., Sharpley, 1986). This issue resolves into two questions: First, should visual or statistical data analysis procedures be applied? Second, if statistical analysis is required, which statistic is most applicable? Although these two questions require deeper treatment than is afforded herein, a brief description of the issues involved may be of assistance



in convincing counselors in the field that they can apply $n = 1$ ITS research methodologies with their clients.

Graphs or Statistics?

In terms of the first question, either/or positions are not supportable. Instead of suggesting that either visual or statistical data analysis procedures are best, the researcher should ask which one is needed to test for the kind of effects that have been hypothesized. In other words, always using statistics to glean a significant outcome from otherwise relatively unimpressive clinical results is probably clinically unsupportable as well as being poor research practice. Similarly, consistently applying visual analysis to data that are not stable can be clinically unethical as well as experimentally invalid.

Perhaps the clearest instances in which visual analysis is justified is when a hypothesized target has been set and reached. The setting of a criterion to be reached in the client's data allows for this, and counselors may feel satisfied if that criterion has been reached. For example, if a client wishes to reduce smoking from say 40 cigarettes per day to 10 cigarettes per day, then this is a criterion that can be seen to have been met or not, and consequently leads to clear evaluations regarding the clinical success of the treatment given to this client. Another situation in which visual analysis is satisfactory is when an unwanted behavior reduces to 0 or a desired behavior reaches 100% of the possible level obtainable by the client.

As noted at length previously (e.g., Gottman, 1981; Sharpley, 1987), it has been conclusively proven that the likelihood of making a Type I error (i.e., concluding an effect was present when none was) when applying statistical analyses to $n = 1$ ITS data of only five baseline and five intervention observations is at acceptable levels. Because almost all counseling research that would be likely to use $n = 1$ ITS designs would be focused on testing the validity of a hypothesis rather than on rejecting the null hypothesis, it is Type I errors that are most important.

Which Statistic?

The application of statistical analysis where needed thus requires that an appropriate statistical procedure is used, and the presence of *serial correlation* or *autocorrelation* in much behavioral data raises significant difficulties for traditional statistics such as t tests and analysis of variance (ANOVA). Data that are collected in a series of observations on a client are usually related to each other. That is, a client's behavior at time n will, to some extent, predict behavior at time $n + 1$, $n + 2$, and so on. Thus, data on behavior at say the 10th day of treatment will be related to data on behavior during all of the previous 9 days. This relationship between data in a series is referred to as serial correlation or autocorrelation because it refers to the correlations between data points in the series. Traditional statistics such as t tests or ANOVAs are not designed to deal with autocorrelation and, if used, will lead

to erroneous results because those statistics assume that all observations are independent of each other (i.e., not correlated with each other). On the other hand, to assume that the data counselors collect on their clients' behavior over consecutive days of observations are not autocorrelated would be to assume that client learning or cognition were impossible.

However, even if the data collected by a counselor on a client's behavior were not statistically significantly autocorrelated, it may still not be wise to submit them to a t test or an ANOVA because even small amounts of autocorrelation can distort the results from those statistical tests (Sharpley & Alavosius, 1988). Hibbs (1974) provided a formula based on Anderson's (1928) calculations for calculating the amount of distortion that would occur in a typical t test or ANOVA if autocorrelated data were analyzed that way. Extrapolation of that formula indicates that an autocorrelation of say .6 would inflate the results of a t test or ANOVA by as much as 200%.

Clearly, a specific statistical procedure is required for analyzing data from typical observations taken on clients' behavior. That statistic is referred to as ITSA, and there have been several generations of this, starting with Bower, Padia, and Glass (1974), moving to Williams and Gottman (1982) and Crosbie and Sharpley (1989), and most recently taking form in Crosbie's (1993) interrupted time-series analysis of autocorrelated data (ITSACORR) program. The latter is relatively easy to use and operates on a personal computer with no need for training apart from that provided in the program and the accompanying article. Data are input via the keyboard, and results are presented on-screen with tests for overall effects and changes in level (i.e., comparing the first intervention observation with the first baseline observation) and slope (i.e., comparing the trend lines of the baseline and intervention data). A graph is also shown, with regression lines for the baseline and intervention data sets. (The ITSACORR program is available from John Crosbie at johncrosbie@acceleratedme.com.)

What Now?

Thus, some of the more likely potential impediments against counselors in the field performing $n = 1$ ITS research and analyzing the data they collect may be successfully addressed. After having collected and analyzed their data, counselors also would benefit from an avenue for reporting them to their peers. This avenue for reporting exists in "congress" formats, scientific meetings, and clinical case conferences and also via the Best Practices section of *JCD*, which encourages the submission of manuscripts that use "evidence-based data," including "data-based outcome studies" to address the issues of what works best for which problem, with whom, and in what situation. The fact that there have been 22 case studies published in *JCD* over the past 20 years indicates that the editorship is willing to print such articles, even if 18 of the 22 were not $n = 1$ ITS studies with a rigorous methodology

to support them (they were simple case reports). The benefit of having a venue for updating their own understanding and knowledge of treatments via a dedicated $n = 1$ ITS section has been granted in some allied journals, and it is clear that *JCD* has followed Barlow and Hersen's (1984) comment that

initial efforts in a series experimentally demonstrating success of a technique on a single case are publishable. Direct replications are publishable. Systematic replications are publishable each time the procedure is successful in a different setting or with a different behavior disorder or whatever. Finally, after a procedure has been proven effective, failures or exceptions to the success are publishable. (p. 370)

Thus, with a ready avenue for publication, counselors in the field should feel welcome to submit their $n = 1$ ITS reports to *JCD*. As pointed out by Barlow and Hersen (1984), there is considerable value to the practitioner, from initial reports, to alert peers of the treatment used and how successful it has or has not been with particular clients and specific problems. Similarly, the replication process as described by Barlow and Hersen also allows for an exchange of data and views arising from those data, whether the data and views support the original report or not. Such an activated forum of evidence-based data would not only inform counselors in the field, it would increase the scientific credibility of counseling vis-à-vis parallel mental health disciplines.

Generalizability of Data From $n = 1$ Research Designs

As noted earlier by Barlow and Hersen (1984), the reporting of data from $n = 1$ research designs in counseling settings can be of benefit to practitioners by building a scientific basis for reference. Although the most important aspect of any research design must be the reduction of Type I errors (i.e., those that result in a claim being made about a therapy when no firm basis exists for such a claim), it is worthwhile to quote Barlow and Hersen on the matter of generalizability of $n = 1$ ITS findings to the wider population. They commented that "guidelines for clinical replication are similar to those for direct replication" (p. 369) and that "one successful experiment and three successful replications are usually sufficient" (p. 346) to establish generalizability when direct replication procedures are used (i.e., when a particular treatment is replicated with similar client presenting problems). Although there are other methods for assessing the generalizability of findings from traditional groups research designs, in $n = 1$ ITS research, the existence of sound data from a series of replications can substantiate the findings from individual $n = 1$ ITS research reports so as to comprise evidence-based data for practitioners.

Examples of $n = 1$ ITS Research Methodology in Counseling Practice

Although counselors may feel enthusiastic about applying $n = 1$ ITS methodologies to their clients' behaviors, it may be of value to give some examples of how that might be done in the field. The following two briefly described case studies are from clinical experience that were amenable to $n = 1$ ITS designs and that enabled some initial conclusions about the effectiveness of the intervention to be drawn by comparing data from baseline and intervention phases of therapy.

Case 1: Marital Conflict and Awarding Spousal Pleases

In this case, H. and M. presented with a concern that they were moving toward marital separation because of frequent arguments and frustration with each other. From the initial interview, the major source of concern appeared to be that the nature of their interactions had become dominated by negativity and that this had set up an expectation within both partners that there would be no positive interchanges between them, hence leading them to fear that the marriage would dissolve. Although much more information was obtained during the initial interview, H. and M. were asked to keep a daily record of the number of negative and positive remarks they each (a) made to the other partner and (b) received from the other partner during the next week. During the second interview, a week later, these data were tabled and the counselor graphed them so that later comparisons could be made. There were significant discrepancies between the estimation of positive exchanges initiated by each partner, depending upon who made the evaluation, so that only agreed-upon positive exchanges were included in the graph (there were none).

Therapy included role-playing the giving of an agreed-upon positive interchange (i.e., one that was perceived as positive by the partner receiving it as well as by the partner giving it), plus an agreed-upon positive response to this interchange. Data were to be collected during the next week on the relative frequency of both positive and negative interchanges. Thus, the 1st week following the initial interview was the baseline (A), and the 2nd and following weeks were the intervention (B). When this couple attended for the third interview, the data from the 1st week of intervention were graphed by the counselor and showed that there had been a noticeable increase in reception of agreed-upon positive interchanges, from an average of 0 per day during baseline to an average of 2.3 per day during the 1st week of intervention. During later weeks, this average increased to 4 per day. Of major interest was the concurrent reaction of both partners that their marriage was no longer "doomed." In fact, after just 2 weeks of intervention, this couple stated that they believed that they now "had the secret to make the marriage work" and rated their marriage, respectively, as 8 and 9 on a scale where 1 = *failure* and

10 = *ideal*, compared with ratings of 2.5 and 3.0, respectively, collected during baseline.

Although this is an “ideal” case (not all couples are even willing to negotiate changes in their interaction behavior), it is actual. Follow-up 6 months after the final session indicated that both partners considered that they now could control the fate of their relationship and that both were positive about its long-term health.

Case 2: Anxious Thoughts and Biofeedback

B. was a young woman of 19 years, studying at college. She sought counseling because she felt “anxious most of the time” and reported that she often found herself clenching her hands so tightly that her fingernails would leave indentations in her palms, and these would be painful “most of the time.” She wanted to “feel more relaxed” and not “have so many fearful thoughts.” During baseline (following the first interview), B. collected daily records of the frequency, duration, and intensity of her anxious thoughts. Frequency was simply how often these anxious thoughts occurred each day; duration was a record of how long the thoughts lasted; and intensity was her own evaluation of how anxious she felt while experiencing the thoughts, with 1 indicating very low anxiety and 10 indicating intense anxiety.

After the 1st week’s baseline data had been graphed by the counselor, a biofeedback training technique based on heart rate and designed to increase respiratory sinus arrhythmia (RSA) via deep abdominal breathing was taught to B. This technique has been shown elsewhere to establish a cycle of RSA, which enables a balance between sympathetic and parasympathetic nervous system activity (e.g., Reyes del Paso, Godoy, & Vila, 1992). Sympathetic nervous system activity is responsible for the “fight or flight” stress response and is the psychophysiological pathway for development of symptoms associated with anxiety disorders, with indications that it may also contribute to depression (Sharpley, 2002). In the case of B., this treatment had an immediate effect on her self-perception of anxiety and on the data she presented during the coming weeks on frequency, duration, and intensity. By Week 3 of intervention, B. reported that the frequency of anxious thoughts had dropped from an average of 23 per day during baseline to just 2.4 per day. Similarly, duration reduced from a baseline average of 12 minutes to less than 2 minutes, and intensity reduced from 9/10 to 2/10 (where 10 = *extremely intense anxiety* and 1 = *not anxious much at all*).

It should be noted that the data collected in both of these cases were graphed by the counselor during the second interview, and thereafter during each interview. However, it was clear that both sets of clients also adopted the graphing process themselves as they became more confident in their ability to change their unwanted behavior. Because the desired changes were obvious, there was no need to apply ITS statistical analysis for clinical purposes, although these might be relevant were the cases to be reported in the scientific literature.

Conclusion

It may be that the scientific standing of counseling and counselors could be enhanced if the kinds of treatments and interventions that counselors apply with their clients were able to be buttressed via valid research using relevant research methodologies, such as *n* = 1 ITS procedures. This could only advance counseling as an evidence-based professional mental health discipline and help its adherents follow the guidelines of a system of case management that requires counselors to be accountable for their therapeutic endeavors (Giles, 1993). Pressure to not only show their clients that the services they offer are useful and valuable, but also to demonstrate to the funding agencies that counseling provides a cost-effective treatment regime that deserves to be considered as worthy as other disciplines that have previously embraced evidence-based treatment, can only increase over time. Research on single participants, including *n* = 1 ITS studies, may serve to buttress the profession of counseling against undeserved criticism.

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