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A RESEARCH-ORIENTED PRIVATE PSYCHOLOGICAL CLINIC: THE POTENTIALITY AND THE ACTUALITY

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AUTHOR NOTE

The author wishes to express his gratitude to the numerous clients whose contributions to this article must necessarily remain anonymous. Reprints may be obtained from Ralph Mason Dreger, Psychology Department, Louisiana State University, Baton Rouge, LA 70803.

ABSTRACT

The potentiality for research in a private practice psychological clinic is often unrecognized. Clinical psychologists may present good reasons for not doing research for which they are uniquely trained among mental health professionals. Yet it is possible to do research through the private clinic *if research procedures are built into its structure and functioning*. It has been the observation of others as well as that of the writer that by and large clients are willing to participate in research if they can be assured that no harm will come to them or theirs. Additionally, research procedures can be planned in such a fashion as to maximize both service and research potential. One generalization which arises out of limited but representative experience is that adequate research is directly proportional to the adequacy of the clinic's files. That the potentiality for research can be actualized has been demonstrated by the author's private practice clinic in which different types of research have been carried out.

A RESEARCH-ORIENTED PRIVATE PSYCHOLOGICAL CLINIC: THE POTENTIALITY AND THE ACTUALITY

It is well known that the modal publication rate of clinical psychologists is zero. However, that figure may be misleading. If the proportion of manuscripts accepted by the major psychological journals is 15 percent or near that, then, obviously there are 85 percent of reports on research which have been rejected.

Even though many of the latter may ultimately be accepted somewhere, the truth is that there must be much more research going on than the zero modal rate usually cited. The fact that the several psychological conventions abound in research reports would underscore the contention that more research is being done than is published.

Nevertheless, examination of the latest convention program of the Southeastern Psychological Association, which presumably is representative of regional associations, reveals that only .075 percent of the participants can be identified, by generous interpretation, as private practitioners, clinical, I/O, or others, although it is no doubt true that many academicians who present at conventions also moonlight in private practice.

Granting, then, that publication rate is a flawed index of research activity, the reality almost certainly is that most private practice clinicians are not doing what by scientific standards would be acceptable research. To be sure, private clinicians are constantly engaging in setting up hypotheses and attempting to confirm or disconfirm them, so to an extent they can be considered to be doing "research." But such activity does not constitute scientific research as such; and clinicians know that it is not, for across many years and in many places this author has heard them apologize for not doing research.

How is it that so many individuals, obviously very bright and knowledgeable, highly qualified to do the one thing for which they are unique among mental health practitioners, scientific clinical research, are not doing so? As well-trained in research methodology as any social scientists, having role models in professors who usually do both practice and research, having had extensive experience with clinical subjects by the time their formal training is completed, equipped in other words ideally to carry on research with clinical populations, they simply do not do so?

Conversation with clinicians from all parts of the country produce the major and indeed over-riding reason, "I just do not have time to do research." It is not that private clinicians act on the "eight-(or nine-)to-five" mentality, for obvious reasons. When they say, "I do not have time for research," it *may* mean that research is not very high on their priority list; but may *not*. On the assumption that there is a fairly substantial number who genuinely would like to make use of their research expertise, but feel bound by realistic temporal restraints, what "I don't have time" may mean is "I do not know how to fit research procedures into my busy private practice." It is to these individuals, a number of whom have expressed guilt feelings about not doing research, the following suggestions are addressed to assist them in doing through their private practices what they have been so highly trained to do.

THE RESEARCH POTENTIAL OF THE PRIVATE PRACTICE PSYCHOLOGICAL CLINIC

First in the list of assets for doing research in private practice is that clients on the whole are willing and often eager to engage in research as part of their over-all evaluation and intervention regimen. As long as they can be assured of confidentiality and that no harm will come to them or theirs, clients give quite

willing assent to participate. Nine years of private practice in which one of the requirements for acceptance of service was signed consent to take part in the clinic's research programs is rather strong evidence for this assertion. Discussions with others who do engage in research through their private practices reinforces the same. Clinicians who truly do wish to engage in research, but fear that their clients might not be willing to do so, can fairly safely be assured that their fears are not based on empirical evidence. An ethical issue which may confront the clinician at the start is dealt with in a footnote because discussion here would sidetrack from the main theme.¹

Second, the majority of data-gathering can be meshed into regular clinic routines. And in fact, clinic routines can be devised so as to maximize the research potential of the clinic. Paradoxical as it may seem, maximizing that potential yields a high probability that the service potential of the clinic will also be maximized. However, there is a difficulty in this procedure which some who would like to do research but are not willing to pay the price will find a stumbling block. That is, a re-orientation in thinking about the aims of the clinic is required, not merely a vague idea that "We'd like to do some research." Some quite definite steps are then required to incorporate research procedures into the over-all clinic operation.

Initially, it must be decided that the records kept by the clinic on its clients are meant for both research and practice. Although the author has not examined the files of other private clinics than his own, from conversation with other clinicians the conclusion has had to be drawn that records of their clinics are most likely not suitable for research purposes. They seem not to be organized in a systematic way that they can be utilized for research. Some attempts are sometimes made to standardize social intakes; but often these are not quantifiable. Tests are given only "as needed," when in order to be useful for comparative and follow-up studies there has to be a standard core battery. Improvements have taken place in record-keeping for behavioral interventions; but for other types of interventions — and despite the emphasis on behavioral techniques, most clinicians seem to have more than one string to their bow — lack of uniformity would appear to be the rule rather than the exception.

A legitimate question arises at this point. "How much data should be gathered on clients, especially in the evaluation phases?" The response is, "As much as can feasibly be garnered. You never know when the information will be useful even beyond its initial purposes of research and service." This postulate is especially pertinent in the present litigious era. As an expert witness or as a defendant, the psychologist with a high amount and quality of information speaks with authority. One without such information has sometimes been made to look like a fool by a clever lawyer (Faust, 1985, 1989; Gudjonsson, 1985; Gutheil & Burszajn, (1986).

However, regarding the maximizing of research potential mentioned above, and exercising prolepsis in relation to the actuality section of this paper, the adoption of the research orientation in the author's private clinic resulted in providing superior service to clients. For one thing, the test batteries which were designed for the dual purposes of service and research, which together with social intakes and interviews constituted the formal evaluation of the client, proved to be *the only service* the clinic provided to *about one half* of its clientele, both individuals and families. With children, only one-third were continued in

treatment following the formal evaluation and interpretations. If evaluations were superficial, almost no service would have resulted or, worse, clients would have been inappropriately served. Agencies to whom referrals were made virtually all expressed considerable appreciation for the thoroughness of the reports they received. With adolescents and adults, the thorough analyses enabled the staff to determine rather precisely what disposition was required, based on more than peremptory judgment.² From the single standpoint of maximizing the research potential, it appears that for either retrospective or prospective research in a private clinic, as also in a public agency,³ the potentiality is directly proportional to the extent and adequacy of its files.

Third, in the present technological era the widespread presence of computer terminals or microcomputers in private clinics makes the possibility of doing research immensely greater than it was only a relatively few years ago. Anyone who follows *The APA Monitor* recognizes the burgeoning field of computerized testing, record-keeping, analysis, and report-writing available. Sophisticated statistical analyses are immediately available for both individual diagnosis and associated research purposes. Data bases are accumulating on floppies and hard disks, so that what at one time took hours and days to record — if done at all — now enters computer archives in minutes. With the possibility of developing data bases of accurate information so handily, an open invitation is extended to psychologists to do research.⁴

Fourth, if a private clinic is equipped with neuropsychological batteries, experimental manipulations can be carried out which are not harmful to the client but are highly useful in answering scientific questions. Some clinicians who possess more elaborate systems like the Reitan or Luria-Nebraska do think in research terms, for these lend themselves to setting up hypotheses and testing them in the course of neuropsychological examinations. Even as simple instruments as the Bender and Benton can be utilized in this way on a lesser scale.

Finally, the private psychological clinic has resources in the dual use of data gathered in its experimental testing of intervention procedures. A new technique is proposed in the literature, or a staff member returns from a workshop with a new idea, or someone in the clinic suggests a different way of treating a disorder. The new method has not been *proven* more effective than the established way, but it is worth trying. There are no known harmful effects, though to be sure one cannot prove the null hypothesis. If the new method had been proven differentially effective, it would be unethical to deny its use with clients. The fact is, however, that it is only, though seriously, *promising*. To apply it to a portion of the clients and not to others, is thus not unethical. Informed consent, of course, is required for any research done. But that requirement does not vitiate the random assignment of some clients to the traditional treatment and some to the new treatment.

The alternative open to animal experimenters, that of no treatment vs. treatment, is not open to the private practice clinician. And yet, a natural “no-treatment” group develops in any psychological clinical setting. There are those clients who for one reason or another do not continue beyond the initial formal evaluation phase of clinic contact. It may be that evaluation is all that is deemed necessary, either by the clinician, by the client, or both; or there could be other

causes for discontinuing service beyond the formal evaluation phase. With sufficiently adequate evaluations, the "remainers" and the "leavers" (with no pejorative implication to the term) can be equated. Then, over the treatment period(s) for the former, the two groups can be compared for changes on specific variables postulated in advance. It can be argued that the "remainers" are more seriously dysfunctional than the "leavers." Nevertheless, the "leavers" have *some* dysfunction, else they would not have sought help in the first place. Either the presumed general greater disturbance of the "remainers" can be taken into account in the analyses, or what is likely the more prevalent case, the two groups can be fairly well equated on the measures important to the investigation.

Another way of doing research in the intervention phase is by use of single case experimentation (e.g., Barlow & Hersen, 1984; Chassan, 1979). Measuring change is not as easy as it seems (Harris, 1967), so the clinician doing single case studies needs to be aware of the pitfalls and possibly seek the advice of statistical authorities before undertaking it.⁵ However, as behavioral studies have demonstrated, such designs can be fitted into regular clinic routines, and if done correctly can make a unique contribution to science. Computers which can rather effortlessly monitor progress are a boon to this type of research.

Taken all in all, the potentialities for research in a private practice psychological clinic are far greater than most practitioners realize: Clients are generally willing to participate if assured that they or theirs will not be harmed. Clinic routines can be utilized or devised to maximize research potential. Computer and/or neuropsychological equipment for data-gathering and analyses have vastly multiplied research possibilities. Some natural control groups develop in ordinary clinical operations making possible experimentation. And judicious use of single-case designs, with the client serving as her or his own control, may generate generalizable findings on important psychological principles (putting idiographic research to nomothetic uses).

THE RESEARCH ACTUALITY IN ONE PRIVATE PRACTICE PSYCHOLOGICAL CLINIC

Forced by a high and unexpected external demand to supplement his income as a professor of psychology, the author and two colleagues established and maintained for nine years a private all-purpose psychological clinic in a Southern City. This is a Standard Metropolitan Statistical Area, the home of two state universities; it is a highly industrialized region and a culture center supporting literature and the arts in a number of ways. As with most private practices, the clientele was predominantly white middle class, though a relatively small number of lower income whites and blacks took advantage of the clinic's services, since the fee schedule was adjusted to income level. The name of the practice revealed the twin purposes for which it was established: Psychological Research and Services (PRS).

The general structure of PRS was very similar in outline to the children's psychiatric clinics the author knew when he entered psychology; but the resemblance is quite misleading. In two very, very important ways PRS differed from its predecessors. The first was that the entire process from beginning to end was regarded as, and behaviors directed toward, the idea that *all* of the

functioning of the clinic was therapeutic (or non-therapeutic as the case might be). Even the Brief Inquiry Interview, which was designed to determine whether the clinic could be of service or not and for which a very modest fee was charged, was expected to be therapeutic in its way. Thus, it was not considered a failure in service when a client either was judged to be unsuitable for the clinic or himself or herself decided not to take advantage of further services. *Whatever* interaction the client and clinic had was regarded to have therapeutic implications. For the old-fashion psychiatric clinic this idea was not at all prominent if even thought of; the social workup, the psychological testing, and the psychiatric interview were three procedures capped by the pronouncement of a diagnosis by the psychiatrist; then, the interpretations were made, and treatment was instituted and only at that stage did "therapy" begin. Such was not the case with PRS, for the therapeutic aspects of all contacts from beginning to end were recognized.

A second major difference between the old-style psychiatric clinic and PRS was that research was built into the structure and functioning of the clinic. The application for service included a statement that part of the purpose of the clinic was to engage in research, that clients were expected to participate in research, and that by signing the application they agreed to do so. Assurance was given that no harm would come to them or the one(s) for whom they were responsible by taking part. In the course of the nine years of operation, not one person refused to sign the application form containing the research condition.

Across the years preceding the opening of PRS, the author had progressed from a "non-directivist," who followed Carl Rogers (1942) philosophy that one did not use psychological tests except in the later stages of therapy to answer specific problems, to the belief that a thorough psychological evaluation is required. For one thing, in dealing with individuals in therapy the author found he was sometimes taking many weeks and even months to find out facets of clients' personalities which could have been uncovered in a short time by use of psychological instruments. Second, just as Rogers found, for research to which the author was dedicated from the start, it was necessary to use such instruments.

Accordingly, for both service and research purposes, a set of test batteries was devised for seven overlapping age ranges from infancy to adulthood. The last battery included marital and vocational preference inventories. A distinctive feature of these batteries was that *a basic core of tests* was required at each age range, expressly for research though with service also in mind. A very large supplementary set of tests at each age level was provided which could be and were usually drawn upon as called for by specific client needs.

As explained above, the evaluations, including social intakes and interviews, were the only service provided by PRS to about one-half of its clients, both individuals and families.

Several different kinds of research were carried out during the years PRS was in operation, and other research has subsequently been done utilizing the data bases residing in the files. Examples of some of the research are given in abbreviated form below.

VOLUNTEERS, NON-VOLUNTEERS, AND NO-SHOWS

Dreger and Johnson (1974) made several specific predictions on the basis of the literature on volunteers, which claimed that volunteers are less conventional,

more sociable, better-adjusted, and more self-accepting than non-volunteers. These predictions were operationalized using the 16PF and the MMPI, which were part of the adult test battery or required of caretakers of children. Contrary to the results of some studies, principally with non-clinical subjects, clinical volunteers were not less conventional, but they were more sociable and well-adjusted than non-volunteers. And volunteers were also more self-accepting but only if their level of anxiety was low. An unexpected finding, which would have to be verified by additional research, was that no-shows proved higher in self-esteem than either volunteers or non-volunteers.

MARITAL ROLES AND DIMENSIONS OF PERSONALITY

Barton and Dreger (1986) endeavored to verify that previously-revealed relations between marital stability and normal temperament (personality) would likewise obtain between marriage roles and pathological dimensions of personality. In keeping with the previous design, the 16PF factors were selected to represent normal dimensions; and 25 MMPI scales, including four validity indicators, all 10 basic scales, the Welsh A and R scales, and nine additional chosen for relevance to the marital situation were utilized for abnormal variables. Assessment of perceived marital roles was made by means of the original 12 dimensions of the Marriage Role Questionnaire (MRQ) (Barton & Cattell, 1972). On the whole, the MMPI alone was a far superior predictor to MRQ dimensions than was the 16PF. However, in four cases the multiple *R* decreased from the MMPI alone to the MMPI and 16PF combined, in seven cases the combined *R* increased over that for the MMPI as sole predictor.

CBCP FACTOR II

This study has not yet been published, so it is given in greater detail than otherwise. Long-term research with the Children's Behavioral Classification Project (Dreger, 1981) led the author and his colleagues to place considerable confidence in the factor derived from the CBCP inventory known as "Factor II. Intellectual and Scholastic Retardation vs. Alert, Socialized Scholastic Achievement." For the present study, it was hypothesized that Factor II would be a valid indicator of intellectual level and school progress. To test this dual hypothesis correlations among five intellectual indicators were entered into a correlation analysis, the WPPSI or WISC-R, Goodenough-Harris Draw-a-Person Test, the Full-Range Picture Vocabulary Test, Factor B (assumed to assess "g") of the Early School or Children's Personality Questionnaire, and Factor II; in addition, all tests were correlated with the Wide Range Achievement Test Reading, Spelling, and Arithmetic scores. *N* was 210.

The correlational analysis results are shown in Table 1. Although rather weak, the hypothesized relations between Factor II and the standard intelligence tests is confirmed except for the relation to the Full-Range Picture Vocabulary Test, and even that is in the predicted direction. Quite interestingly, the second part of the hypothesis dealing with Factor II and the WRAT Reading, Spelling, and Arithmetic scores is confirmed also, just about as well as for the Wechsler tests and the FRPVT should those tests have been hypothesized to be related to WRAT scores.

Table 1

**INTERCORRELATIONS OF INTELLIGENCE INDICATORS
AND WRAT SCORES**

	II	WISC	GH	B	FRPVT	WRATR	WRATS	WRATA
II		-0.23	-0.18	-0.28	-0.10	-0.45	-0.44	-0.38
WISC			0.58	0.53	0.64	0.50	0.42	0.44
GH				0.38	0.35	0.33	0.26	0.24
B					0.38	0.34	0.37	0.34
FRPVT						0.47	0.37	0.39
WRATR							0.76	0.49
WRATS								0.61

Note: N = 210. II = CBCP Factor II, Intellectual and Scholastic Retardation; WISC = WISC-R of WPPSI; GH = Goodenough-Harris Drawing Test; B = Early School Personality Questionnaire, Factor B, "g" type intelligence; FRPVT = Full-Range Picture Vocabulary Test; WRATR, WRATS, WRATA = Wide Range Achievement Test Reading, Spelling, Arithmetic. All coefficients are significant beyond the .01 level, except II with GH ($p > .05$) and II with FRPVT ($p > .10$).

Table 2

**ROTATED FACTOR PATTERN OF INTELLIGENCE
AND ACHIEVEMENT INDICATORS**

Variable	Factor I "Intelligence"	Factor 2 "Achievement"
II	0.13	-0.89
WISC	1.09	-0.02
GH	0.87	-0.10
B	0.70	0.19
FRPVT	0.85	0.01
WRATR	0.15	0.84
WRATS	-0.06	1.05
WRATA	0.18	0.75

Note: For variable identification, see Table 1.

Inasmuch as the correlation matrix itself can give only a rough estimate of what the true relations are between or among the underlying constructs assessed by the tests, a principal factor analysis with Varimax and Promax rotations was carried out on the matrix. By both the Scree test and the K-G latent-root-of-one criterion, two factors only were retained; the first three eigenvalues were 5.554, 1.274, and 0.626. A bit of caution must be expressed here, for the correlation matrix is singular and two of the variables produced Heywood cases. The SAS program was instructed to substitute 1.0 for the communalities exceeding 1.0 and completed the analysis. Table 2 reveals the resulting rotated factor structure. Despite the fact that the variance accounted for is over 9.00, a manifest impossibility, the factors bear out the hypothesis. The two factors could be denominated "Intelligence" and "Scholastic Achievement" with Factor II's relation to each one quite clear.

With some reservations, it can be concluded that the dual hypothesis was supported by the analyses. The limitations of the WRAT should be recognized, however; about the 1965 edition Thorndike (1972) wrote that he hesitated to recommend it for anything other than a clinical or research setting with individual administration for a quick estimate of general ability level and educational background. Presumably, the present use qualifies for both. It should also be recognized that even at best, as with the Wechsler or the FRPVT, not more than a quarter of the variance in school achievement as assessed by the WRAT is accounted for by any of the intelligence indicators.

Other research can be cited, but these instances suffice to demonstrate that through its very service this one private psychological clinic was able to carry on research of a meaningful nature. With the modern trend to utilize existing data bases for research purposes, using computers to digest large masses of data, PRS is still producing research from its files. One special investigation is yet to be accomplished, a follow-up study of youngsters who scored high on Factor VII, "Anti-Social Aggressiveness," one of the few characteristics which in other studies are predictors of similar behavior in adulthood (Quay, 1986).

Did the research being done in this one private clinic affect its operating procedures as well as accomplish interesting scientific results? It can be said that in general there were no immediate changes in procedures, primarily because the total clinic operation had been carefully thought out well in advance. However, one very practical alteration came about because of the scientific orientation of PRS. It was in the beginning years that the "behavioral revolution" took place. The clinic was set up originally with traditional modes of intervention for children and adults predominating. But when the reports of behavioral methods became prominent, especially when those reports cited the unbelievable successes of 90 to 95%, the staff of PRS had to consider instituting such methods and techniques. Controlled observation of both traditional and behavioral methods with children was begun. The results confirmed what more sober estimates of success of behavioral methods had come to, that is, that the estimates for success were about two-thirds. It was found, however, that these methods could deal with some problems which could not be reached by the more traditional ones. Balancing that statement is another, that some, especially loss-related problems could not be reached by behavioral techniques.

REPORTING RESEARCH RESULTS FROM THE
PRIVATE PRACTICE CLINIC

Research can be done just to answer someone's questions. For the answers to be sound the clinician must exercise the same careful controls recommended in methodology texts. (Cf. Dreger, 1990, [still in press.]) The question may be about some new intervention strategy, or the introduction of a new assessment instrument. Scientific method need not give way to hunches and guesses which appear to be the usual decision factors, which is perhaps one reason for the exaggerated claims of success for the use of some new procedures. Careful measurement and keeping of ample records can enable the clinician to make sound judgments. Some of the studies of this nature may be worth publishing; but regardless, they are worth doing for their own sakes.

But after saying these things, if the clinician believes that her or his research is worth sharing, there are ways of disseminating the information which do not require formal publication. Private newsletters constitute one of the ways. A number of such newsletters cross the writer's desk. Although these publications do not in general enjoy high prestige, partly one would suppose because they are mimeographed or desk-top published sheets, some of the most respectable and useful research reports this author has read have been in some of these non-formal publications. Often when these are abbreviated reports, authors offer to send free of charge a full report.

Another well-known outlet, in which it is not so difficult to achieve acceptance as into a refereed journal, are local, state, regional, national, and international conventions. Even if one should be submitting a manuscript to a journal, a brief oral presentation or poster on the subject is permissible. In any case, even if one is only passing out tables or figures, it is incumbent on the presenter to have the source identified. Many times conventioners return from a convention with a sheaf of papers which seemed important at the time, only to find no title or author identification, so the inquirer cannot even go to the program to locate where to follow up on the information. This a failure in communication and is not good scientific practice.

Private distribution of manuscripts has been utilized for many years and has been one of the ways some scientists keep current with research in their fields. Along with newsletters and conventions, this form of disseminating information does enable one to be more fully abreast of a field than trusting entirely to journal articles, for the time lag from the initial research to its formal publication may be upwards of two or three years.

At long last, however, after due and sometimes painful recognition of the inadequacies of journals for conveying scientific information, it is still true that refereed journals constitute the basic depository of knowledge of a science. Private newsletters or privately-distributed manuscripts are not ordinarily filed in libraries. Papers at conventions are usually forgotten unless they are also published in journals (or in books which are devoted to, say, extensive research projects). By far the majority of studies reported in other ways are not incorporated in permanent form, and when they do get into books, they are not reported in such manner and extent that their methods can be examined. Only in journals (or books as above) can their methods be examined and the excellencies

or limitations of the research be fully recorded. Parallel to Churchill's famous dictum about democracy, journals are the worst of any reporting medium, except any other.

Therefore, the private clinician who has done a worthwhile piece of research should seek diligently to have it published in a reputable journal. Before doing so, however, an author must become familiar with the *Publication manual of the American Psychological Association* (3rd ed., 1983). Many non-APA journals like this one have adopted the APA Manual, so ordinarily the author is safe in using the APA format. At any rate, the submitter should consult the guidelines for authors in the front or rear cover of the journal. If the author does not have access to a particular journal, a covering letter can accompany the submission explaining the situation and suggesting that if the manuscript is otherwise acceptable it can be retyped in appropriate format.

To which journal would the practitioner submit his or her work? (Note the singular "journal" — editors are singularly averse to receiving manuscripts submitted to several journals at the same time.) There is a host of journals available, some more particularly suited to clinical studies than others, but any one of which could be a vehicle for a specific type of study, biological, neurological, abnormal, psychiatric, experimental, etc. The American Psychological Association lists *Respected resources for current research in psychology*, which consists of all APA periodicals, and provides a listing of other journals which will give discounted subscription rates to APA members, which is a source for prospective submitters. Besides the APA journals which are most likely known to readers, this journal (MECR), the *Journal of Clinical Psychology*, and the *Journal of Abnormal Child Psychology* are quite suitable; the so-called "Murchison journals" (like the *Journal of Psychology*) and the Ammons's group of journals published in Missoula, Montana (*Psychological Reports* in particular) are definitely worth considering.

Two points should be observed in submitting manuscripts. First, to be assured that one's report will be given due respect by the psychological public, submission should be to a refereed (sometimes called juried) journal. Even a fully adequate study has some flaws; at least in the hundreds of manuscripts this writer has examined for journals none has failed to have at least a few flaws. Critiques by thoughtful and knowledgeable reviewers can be of great help in remedying non-fatal weaknesses in design or writeup. (Some authorities suggest that an author have his or her manuscript critiqued by a colleague before submission, which may be a good idea depending on the caliber of the colleague and her or his willingness to devote considerable time to the task.)

Second, rejection of a manuscript need not be the end of the endeavor. To keep on keeping on is often necessary. A paper can be revised and submitted again, either to the same journal or to another one (preferably). A consideration in this regard which should not have to be a consideration but realistically is, according to recent discussion in the pages of scientific journals, is that some reviewers can be professionally biased with axes of their own to grind; this situation is not general, yet must be kept in mind. Other things being equal, patience, willingness to work over a manuscript again and again, may well pay off if the study has been done well and the writeup profits from reviewers' and editors' critiques.

SUMMARY AND CONCLUSIONS

The main thesis of this article is that there is great potential in a private clinic which wishes to do research. One clinic which combined research and service successfully is cited, though this is just one example of other such clinics, to demonstrate that the potential can become the actual. Some attention is given to what should be done with the results of research.

ADDENDUM

The reader may well ask, "Why did the author terminate his private practice? Did the clinic fail financially because of all this research business?" The answer is simply, "No, not at all." Because of the heavy demand for services of Psychological Research and Services, at one time during the nine years of its existence it became necessary for the author to decide whether to go full time and give up his teaching or to continue on a part-time basis. He was not willing to give up teaching, so continued part time, serving as many as he and his colleagues could handle. And when the final decision to close down the clinic came, the reasons were entirely extraneous to the clinic itself. As far as the success of the clinic itself was concerned, the part-time private practice could have continued indefinitely.

FOOTNOTES

¹Is it ethical to expect people to pay for service and at the same time engage in research which *may not directly* benefit them but which instead benefits science generally? Private practice of medicine has apparently solved this problem. The Mayo Clinic, Ochsner's Hospital and Clinic, Kaiser-Permanente, and other private medical facilities regularly engage in research with their paying patients. Not many private medical facilities could afford to accept *non-paying* patients on condition that they participate in research; nor could private psychological clinics afford to. Nothing in the actual operation of the private clinic described in the text ever caused doubt about the wisdom of combining service *and* research with its paying clients. In many cases, indeed, clients did receive *direct* benefit from the research requirement.

²The writer has a transcript of an adult psychiatric clinic staffing which would be almost ludicrous, if its implications were not so serious, in its arbitrary, non-data-based decision processes.

³From the writer's experience as head of a research team for the mental health facilities in Florida, he and the team found that the files of clinics they examined were abysmally unadaptable for research.

⁴A caution to be observed in respect to computer testing is that norms developed for standard (non-computer-assisted) testing are not necessarily applicable to the results obtained from computer testing. Mazzeo and Harvey (1988) review the literature comparing the methods and find a general lack of equivalence.

⁵The exaggerated success claims in the early days of behavioral interventions had to be revised downwards when the institution of better controls brought such estimates of success to a more credible 60 to 70% (Ross, 1978) One of the errors encountered in the first place was unfamiliarity with statistical problems in measuring change (Gurman & Kniskern, 1978).

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