Single Case Experimental Designs: An Essential Service in Communicatively Disabled Care

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ABSTRACT

A situation analysis of communication disabilities of and services to this population in the RSA reveals a lack of knowledge about the field and a paucity of research, probably due to therapists' extreme involvement in clinical practice.
In this article the advantages of single case experimentation are put forward and specific designs are discussed in an attempt to motivate and enable clinicians to be producers of research. It is pointed out that this type of research will not only add valuable scientific information to the field of speech pathology and audiology, but it will also increase accountability in clinical practice.

OPSOMMING

'n Situasie-ontleding van kommunikasie-afwykings van en dienste aan hierdie populasie in die RSA dui op 'n gebrek aan kennis op die gebied, sowel as 'n beperkte navorsingsuitset. Dit kan waarskynlik toegeskryf word aan die terapeute se ekstreme betrokkenheid by die kliniese praktyk.
In hierdie artikel word die voordele van enkelgeval eksperimentering uiteengesit. Spesifieke ontwerpe word voorgehou in 'n poging om terapeute te inspireer om navorsingsbeoefenaars te word. Hierdie tipe navorsing lewer nie slegs waardevolle inligting vir spraakheelkunde en oudiologie nie, maar help ook om die toerekenbaarheid van die kliniese praktyk te verhoog.

INTRODUCTION

The report of the inquiry into the circumstances of disabled people in the Republic of South Africa reveals inter alia that about 12,5% of the total population is disabled and it includes the recommendations that the quality of services should be improved and that research with a view to the improvement of services should be conducted (Department of National Health and Population Development, 1987).

Unfortunately, due to the paucity of services, trained personnel are swamped with medical and rehabilitative services - mostly diagnosing and treating patients on an individual basis. In a situation such as this, research is limited to a minimum, and research involving large groups of subjects, a near impossibility. A solution to this problem can, however, be found in the utilisation of the potential of clinicians, not only as consumers of research, but as producers of single case research. During their daily routine these professionals come into contact with potential subjects for research, but as has been pointed out, on an individual basis. These circumstances should be exploited for single case experimentation.

SINGLE CASE EXPERIMENTAL DESIGNS: ADVANTAGES AND LIMITATIONS

The results of single case study approaches have been used and quoted for many years - usually with questionable credibility due to subjectivity and lack of experimental control. McReynolds and Thompson (1986:195) suggest that "A better role for case studies is description and identification of potential variables to be evaluated in experimental studies". This is especially applicable to the RSA context. Because of the diversity of handicapped people, particularly from different language and cultural backgrounds, many unusual problems are encountered. The study of unique cases, will under these circumstances be of particular benefit to the science of communication pathology.

Single cases can be used in a number of different research designs. In the field of speech pathology and audiology single cases are often studied implementing a descriptive or even analytic survey design. These are, however, not experimental designs.

A single case design can also be an experimental design, the so-called cause and effect method, the pretest-posttest control group design, or the laboratory method. "In its simplest form, the experimental method attempts to control the entire research situation, except for certain input variables which then become suspect as the cause of whatever change has taken place within the investigative design" (Leedy, 1980:211). Such data obtained from these studies are functual, objective and reliable.

The question at this stage is: How can single case experimentation be utilised as a research input and as a service to the disabled? Theoretically these studies are valuable in measuring the effectiveness of treatment variables, of describing disorders, or identifiable components of disorders over long periods of time, of evaluating the effects of disorders on the resulting disability, and of ultimately describing subgroups of disorders. Single case experimentation can also be implemented in the identification of all the different variables related to or responsible for human development - be it normal or pathological.

One of the greatest practical advantages of single subject experimentation is the fact that the clinician can do research without sacrificing clinical intervention. In diagnosis the clinician identifies the behaviours to be modified (the dependent variables) and then selects specific procedures for treatment (the independent variables). This automatically forms the basis of a single case experimental design. The experiment can also be conducted during ordinary therapy sessions without the expense of sophisticated apparatus. This is often impossible when large groups of subjects are involved.

"Individuals differ in every physical and psychological attribute ever studied" (Lindgren, Byrne & Petrinovich, 1961:219). It is recognised that patients with a specific disorder are not necessarily homogeneous. They differ qualitatively, in degree of impairment, and with regard to responsiveness to a particular treatment variable. In group experimental research this variability is managed through statistical analysis, usually referring to averages which in the end conceals rather than clarifies individual variability.

Single subject experimentation must never be confused with the unscientific, unplanned observation of behaviour. The single subject experimental approach is a scientific method which requires meticulous planning in advance and strict control during the execution of the experimental phases.

SELECTION OF A SINGLE CASE EXPERIMENTAL DESIGN

"Research is not aimless, undirected activity ... (it) demands a definite aggressive plan" (Leedy, 1980:5). Planning is the most important prerequisite for successful research. The planning and execution of the research is done within the framework of the scientific method, which is a means whereby insight into undiscovered truth is sought (Leedy, 1980:82); a set of rules that can be used for describing events, explaining events, and predicting events (Silverman, 1977:29); and which involves certain decision taking steps (Mouton & Marais, 1985:16, 22).

Planning a single case study involves the following decision-taking steps:

- The selection of a research theme, or problem or question. The topic or theme should always be rephrased in terms of an answerable question. Not all questions are equally answerable, and "one cannot get a clear answer to a vague question" (Johnson, 1946:52). Silverman (1977:62) quotes the following example: "Is hypnosis effective in treating stuttering? (versus) Is the post-hypnotic suggestion, 'You will not stutter anymore', effective in reducing stuttering frequency?"

- The drafting of a research programme, including the conceptualization of theoretical issues and the operationalization of these concepts in terms of measurable parameters. In the humanities, measurement poses a bigger problem than in the physical sciences. The above example does, however, illustrate how behaviour can be measured - in this case by counting the frequency of the occurrence of stuttering. Without a clear operationalization of the central concepts included in the research question, it is impossible to select a design, or collect and interpret data.

- The selection of an appropriate design. There are many different kinds of single case experimental designs and the researcher must be sure that the appropriate design is selected for a specific study. This depends first of all on the research question and secondly on the nature of the data required. This will be dealt with in detail later on.

- The collection, organization and classification of the data. Data is collected within a closed system of controlled conditions - "an area sealed off by given parametric limitations" (Leedy, 1980:85). Factors which are critical to the research can be isolated and the nature of the variables can actually be determined by control. Without control the data will be worthless.

- The analysis and interpretation of the data. Accumulated data are only potentially meaningful. "The significance of the data depends upon the way in which the facts are regarded" (Leedy, 1980:6). In analyzing and interpreting the data, new insights are discovered and new meanings are revealed. This would then give rise to further unexplored questions for future research.

Only after this planning has been done systematically, can the researcher select the design.

In disabled care experimental designs are frequently used to explore the full range of intervention questions including the acquisition, generalization, and maintenance of behaviours for impaired individuals. A heuristic method of selecting an appropriate design to answer a given question, is, however, necessary. Kevin Kearns (1986:205) devised such a method, providing a variety of design options which depend on factors such as the nature of the target behaviours, the setting in which a study is conducted, the availability of additional subjects, and other practical exigencies. The sequential arrangement of steps in this design selection process is given in table I and this taxonomy could be viewed as a general, organizational tool that is intended to facilitate an understanding of factors to consider in the selection and use of single case designs.

Although the columns are presented in the order of evaluation strategy, clinical research questions, selected design options, and basic considerations, it is suggested that in using this method, the researcher deals with them in the order of clinical research question, evaluation strategy, basic considerations, and lastly selected design options.

The research question refers to the outcome of intervention and the way in which intervention strategies influence behaviour modification, e.g.: Is there a difference in behaviour with or without treatment? To what degree do separate components of the treatment contribute to behaviour modification?

After the research question has been asked, the researcher can state the nature of the strategy to be followed, e.g.: Can the behaviour during treatment be compared with the same type of behaviour without treatment?

At this stage the researcher should pay attention to some basic considerations, or factors which would influence the selection of the appropriate design, e.g.: If there is uncertainty about the reversibility of behaviour, an ABAB design should rather be replaced by a multiple baseline design. Only after all these factors have been dealt with, can the researcher select the design most appropriate for a particular study.

The following figures illustrate the use of different design options:

1. *Treatment – No Treatment Strategies.*

The initial phase is a period during which baseline measurements are taken. It is important that baseline measurements continue until a certain amount of stability in behaviour is reached. During the second phase the treatment is instated and a series of measurements are taken in order to indicate the modification of the behaviour. This is followed by a period of withdrawal of treatment, with the presumption that a relapse in behaviour will be demonstrated. Multiple measurements will indicate this change. Then treatment is again instated, etc.

Table 1. Evaluation strategies, research questions, design options, and considerations for single-subject experimental designs.

Evaluation strategy	Clinical research question	Selected design options	Basic considerations
Treatment – no treatment comparison	Does treatment, with all of its components, result in improved performance relative to no treatment?	Withdrawal and reversal designs ABAB BAB ABA	Is the therapeutic effect likely to reverse following the withdrawal of treatment?
		Multiple baseline designs (M,B) across behaviours across settings across subjects	Are functional independent behaviours or settings available?
		Multiple probe technique (variation of MB) (Horner & Baer, 1978)	Are functionally independent behaviours available? Are long or continuous baselines impractical?
Component assessment	Relative to a treatment package, to what degree do separate components of treatment contribute to improvement?	Interaction (Reduction) BC-B-BC-B BC-B-BC-A-BC-B-BC	Can the components be examined alone and in combination with the treatment package? Can replication be obtained across subjects?
	Does the addition of a component to a treatment package facilitate treatment effectiveness?	Interaction (Additive) B-BC-B-BC B-BC-B-A-B-BC-B	Can the components be examined alone and in combination with the treatment package? Can replication be obtained across subjects?
Treatment – treatment comparison	What is the relative effectivenss of two or more treatments?	Alternating treatments design	Can treatments be rapidly alternated for each subject?
		Replicated crossover design (Barlow, Hayes, & Nelson, 1984)	Are multiple subjects or target behaviours available? Can treatment be "crossed over"? Are nearly equal phase lengths possible?
Successive level analysis	Does treatment result in acquisition of successive steps in a chaining sequence?	Multiple probe technique	Are steps in the treatment sequence independent? Are earlier steps prerequisite to acquiring later steps?
	Does treatment effectively modify a single, gradually acquired behaviour?	Changing criterion design	Will changes in the dependent variable correspond to changes in the criterion level? Will the dependent variable stabilize at successively more stringent criterion levels?

(Kearns, 1986:205)

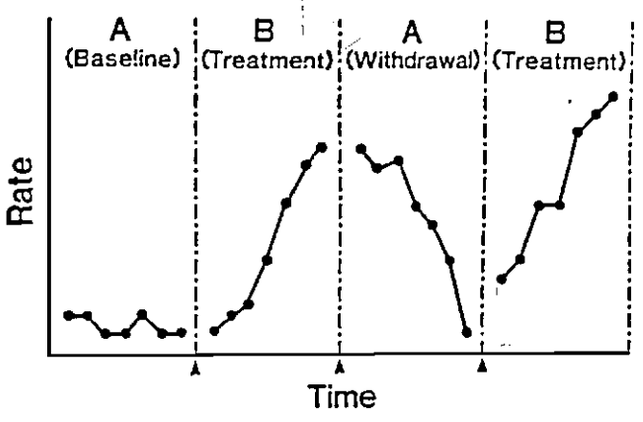

Figure 1: A basic ABAB withdrawal design with the alternation of no treatment and treatment phases.
(McReynolds & Thompson, 1986:199)

In this design multiple measurements are made on more than one similar, but independent behaviour, during the A phase. Treatment is then applied for only one behaviour, while the A phase continues for the second behaviour. Once a stable change is obtained in the first behaviour, the B phase is instated for the second behaviour.

This AB arrangement removes the necessity of withdrawal of treatment and can be used when withdrawal or reversing is impossible or even unethical.

2. *Component assessment strategies*

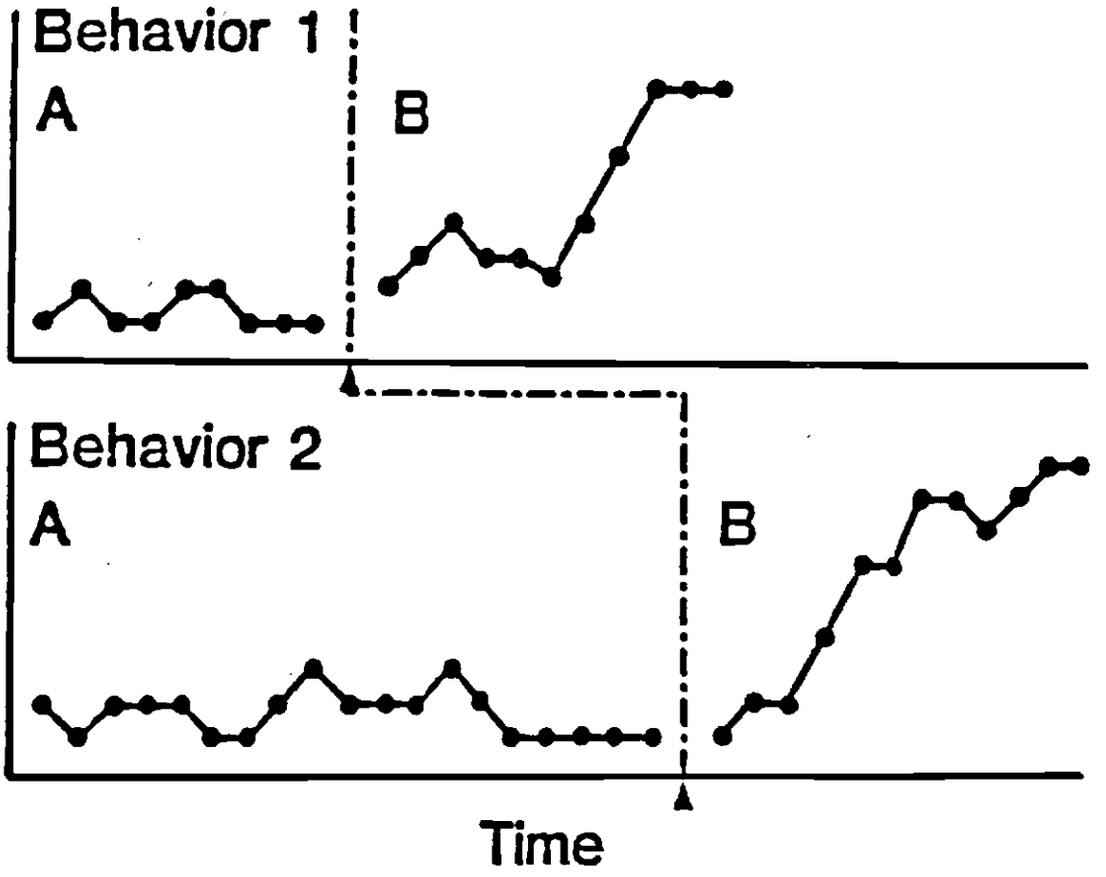

Figure 2: A multiple baseline across behaviours design with an extended baseline phase for behaviour 2.
(McReynolds & Thompson, 1986:199)

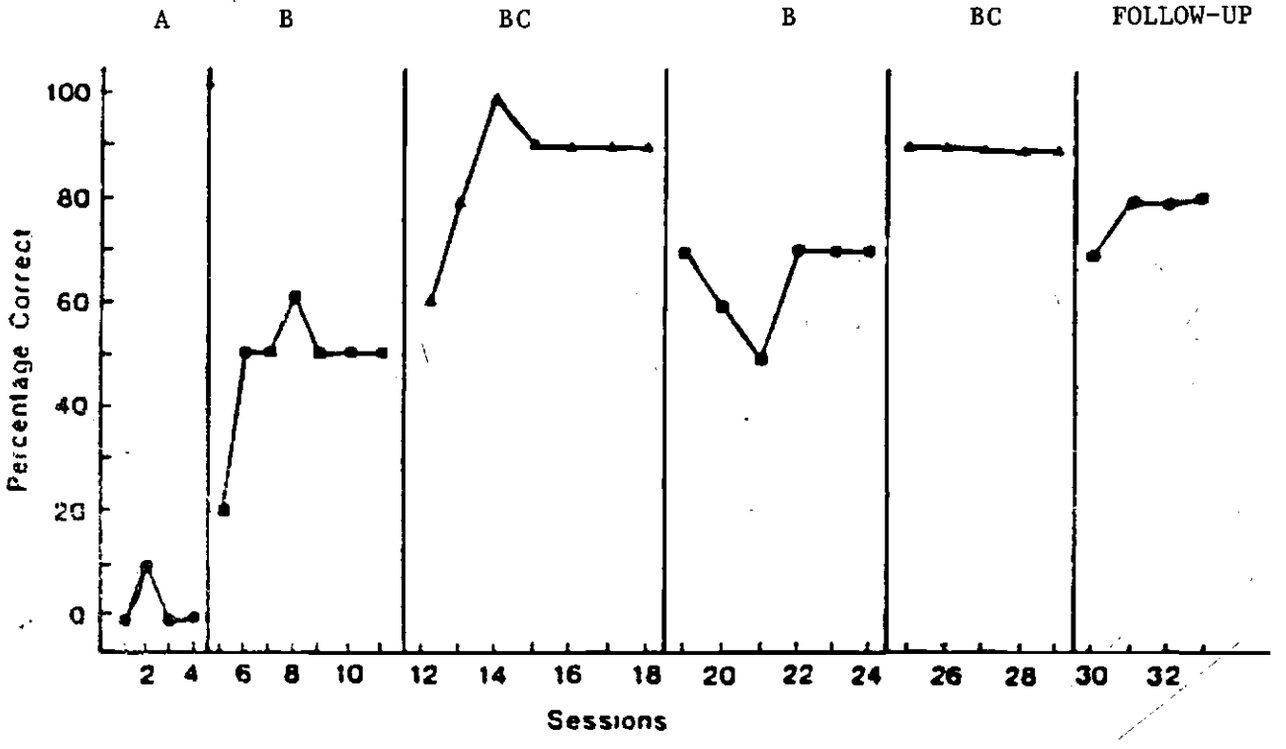

Figure 3: An interaction (additive) design including the phases A-B-BC-B-BC and a follow-up period.
(Kearns, 1986:210)

This design is selected when the researcher wants to establish whether the addition of another component to a specific treatment package would facilitate progress or enhance the effectiveness of the treatment. Once again baseline measurements are taken. During the first phase only one type of treatment is given, followed by a combination of that treatment and the additional treatment. The additional treatment is withdrawn, and again added. During these consecutive phases multiple measurements of the behaviour change should indicate when optimal change takes place.

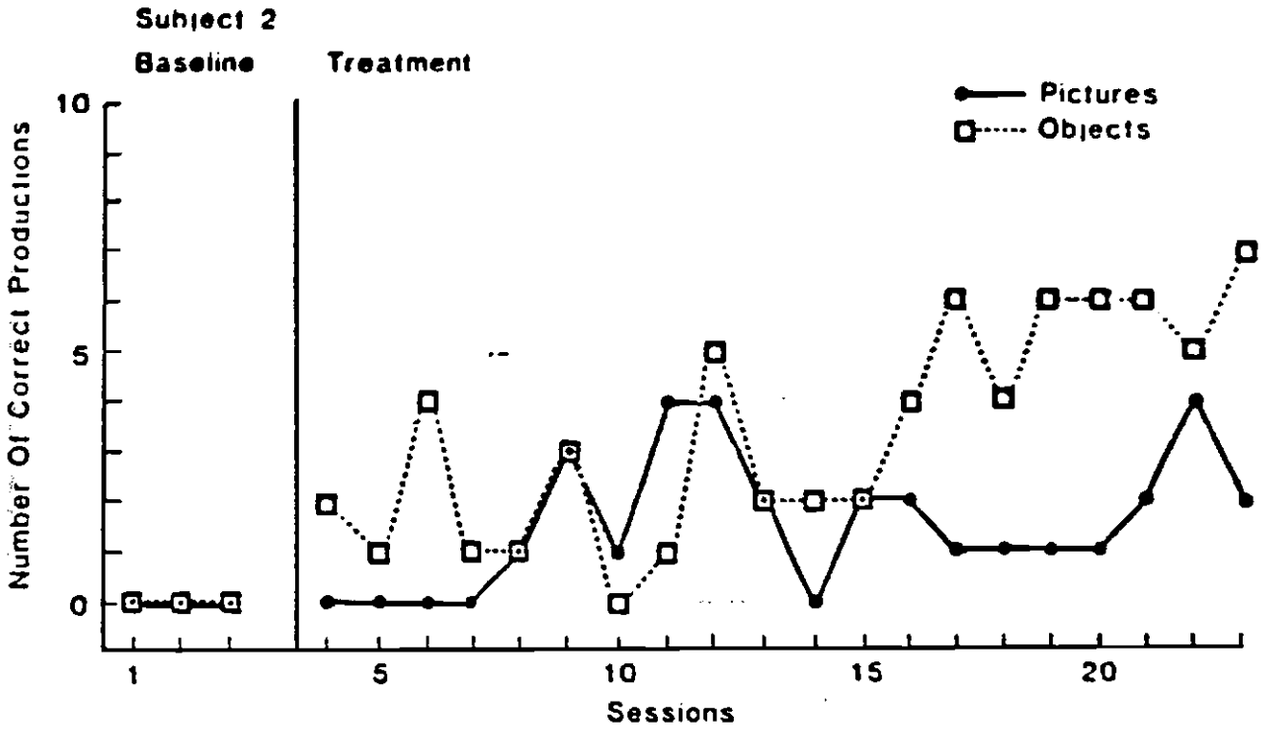

Figure 4: An alternating treatments design
(Kearns, 1986:211)

3. *Treatment – Treatment Comparison Strategies*

As the effectiveness of one type of treatment is compared to that of another, an initial baseline phase is not required. The incorporation of a baseline phase does, however, give additional information on a no-treatment-treatment comparison. In this design the treatments to be compared are administered and alternated rapidly, e.g. half of each session. Unfortunately this design does not permit unequivocal conclusions about the effectiveness of each method of treatment, but it does provide a means of evaluating the relative effectiveness of different treatment methods for specific patients.

4. *Successive Level Analysis*

This is a most successful method to employ in developmental studies. It answers the question about whether treatment would effectively modify a single, gradually acquired behaviour. After the baseline phase, a specific criterion is set according to which the success of the behaviour is measured. Once the patient succeeds, another criterion is again set. It is, however, necessary that the researcher is familiar with the successive steps in the acquisition of this specific behaviour pattern as each criterion would serve as a goal in the developmental pattern.

These are only a few examples of how single case experimental designs can be employed in disabled care research. Unfortunately, especially when dealing with people, Murphy's Law will come into operation. To counteract this contingency, the researcher can employ certain alternatives, which is a product of the flexibility inherent in the application of single subject experimental designs.

BUILDING IN INSURANCE THROUGH FLEXIBILITY

Every researcher should be creative in designing experiments. While it is true that specific designs are appropriate for ans-

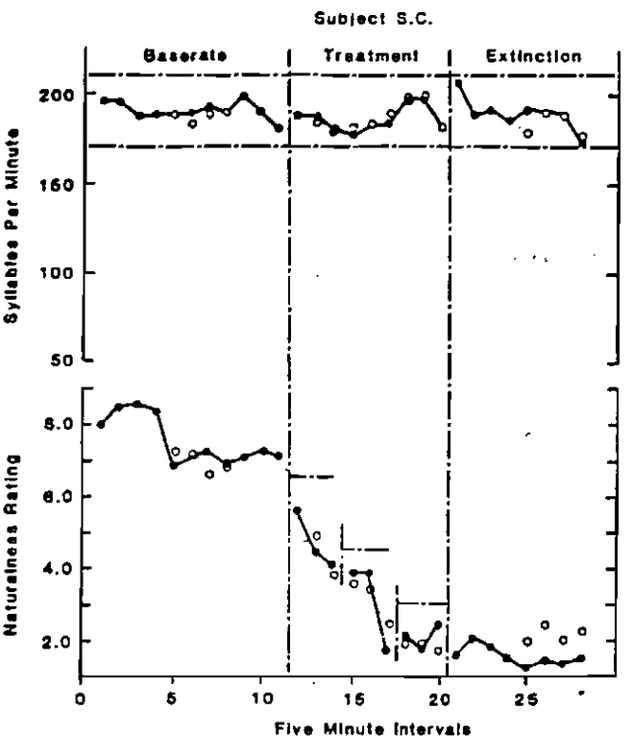

Figure 5: The changing criterion design
(Kearns, 1986:212)

wering specific questions and that research must be planned in such a way that control can be exercised throughout the project, there still is a certain amount of flexibility that can be utilized to provide for the unexpected - to provide additional sources of control.

Compared to research involving large groups of subjects (where similar control strategies must be maintained across the group), single subject designs lend themselves to flexible application (Connell & Thompson, 1986:214-215). Flexibility should never weaken the scientific nature of single subject research; it should be utilized as an insurance against lack of control while examining the relationship between independent and dependent variables.

> "Controls for an independent variable (treatment) applied in single-subject designs can be created to fit the particular characteristics of the treatment. The controls, if properly arranged, can be changed in response to changes in treatment once an experiment is underway." (Connell & Thompson, 1986:215.)

Two general forms of flexibility can be utilized in single subject research, viz. *a priori* and *ad hoc* flexibility.

A priori flexibility comes into operation during the selection-of-design stage, i.e., before the experiment is carried out. The maintenance of experimental control can be assured when more than one design is incorporated in a single study. "Even if all planned sources of control are preserved throughout the study, the extra control is not wasted because it is a form of replication that would increase confidence in the results." (Connell & Thompson, 1986:220.)

As ABA and multiple baseline designs are particularly compatible, the researcher can for instance decide on this combination when there is uncertae reversibility of behaviours. This combination will also reveal a possible dependency of behaviours which were previously regarded as independent of one another, as baselines might change even before treatment is introduced for a specific behaviour.

Connell & Thompson (1986:221) suggest the general principle that additional controls be built into all studies except those that examine highly predictable behaviours.

Ad hoc flexibility comes into operation while the experiment is being conducted. A study should be planned in such a way that the design can be modified during one of the experimental phases. These modifications are made in response to developments that arise during the course of the experiment in an attempt to maintain control.

> "The within-subject nature of experimental control allows for a certain amount of freedom in the selection of control strategies ... The controls, if properly arranged, can be changed in response to changes in treatment once an experiment is underway ... It is this freedom in how independent variables and control can be implemented and changed ... that distinguishes single-subject from group designs." (Connell & Thompson, 1986:215.)

This "response-guided" experimentation (Edgington, 1983: 64-65) comes into operation when unexpected patterns occur in the data. Without an extension of the original design, treatments cannot be studied in greater depth.

When investigating the effectiveness of a treatment package, the researcher might decide to select a multiple baseline across subjects design implementing a reversal design as control. During the course of treatment it becomes clear that the independent variable proves to be effective for two of the three subjects only. The experimenter can then systematically change the treatment implementing an interaction design while maintaining the reversal design control that was originally implemented. This modification will allow the experimenter to determine which modifications of procedures are effective for an individual subject. Connell & Thompson (1986:223) summarize the benefits of flexibility in single case research as follows:

> "By using flexible designs, it would be possible to obtain detailed information about factors in existing treatments that have a variable effect, a weak effect, or even no effect on learning. By contrasting variable and ineffective factors with consistent, effective factors across treatments, individuals and behaviours, it may be possible to identify common attributes of factors that are related to effectiveness within a particular disorder area and possibly across areas."

REVEALING NEW MEANING

As accumulated data are only potentially meaningful, the researcher needs to process the data in order to obtain greater insight into the nature and meaning of the data. One of the available tools for processing and interpretation is statistics.

In disabled care research wants to evaluate and draw conclusions about behaviour change, and experimental, as well as therapeutic criteria, and invoked to evaluate data (Risley, 1970:103-127). The experimental criterion refers to reliability. This criterion is met when the subject's behaviour changes reliably under specific experimental conditions. On the other hand, the therapeutic criterion is met when the level of behaviour change is such that the subject presents with adequate functioning in society, complying with the norm.

Evaluation and interpretation of data in terms of the experimental criterion include visual inspection and statistical analysis. Visual inspection may seem completely subjective, but special data requirements, in terms of specific criteria (e.g., trends in change during certain experimental phases) need to be met. Statistical methods again present the researcher with replicable computational methods and rules for making decisions about the reliability of a particular experimental criterion.

The applied or clinical significance, i.e., the therapeutic criterion, can be addressed by comparing the subject's post-experimental behaviour with the norm, or by having various raters evaluate the magnitude of the behavioural change. Interrater reliability then becomes an issue.

Although there are still sources of controversy, "statistical analyses in single case research may provide a valuable supplement rather than an alternative to visual inspection" (Kazdin, 1984:291) and a number of statistical tests can be applied to data obtained from a single case.

In a A-B-A-B design comparisons can be made of behaviour during the baseline and intervention phases (t-test), or analysis of variance (F test) can be made to compare the four phases.

If, however, the data are serially dependent, these tests may not be appropriate which would necessitate variations of these tests in order to reduce the effect of serial dependency.

Time series analyses can be used to compare behaviour change over time for a small group of subjects or even a single subject. In single case research the time series analysis is advantageous in that a t-test is provided and important information on the change in level and slope is given during the different phases.

Randomization tests are used when treatments are assigned randomly to different occasions and as such are useful for evaluating data obtained from alternative treatment designs. If the design complies with the criterion of randomization and rapid alteration of experimental conditions, these tests provide a useful set of statistical techniques for single case research.

The test of ranks (R) is used for evaluating data obtained in multiple baseline designs and requires that data be collected across several baselines, e.g., different behaviours, subjects, or settings - the minimum requirement for detecting a statistically significant effect at the .05 level of confidence being four baselines. In the case of slow and gradual, or even fluctuating performance, the intervention can still be evaluated on the basis of mean performance, which is an advantage of this statistical procedure.

A description of the rate of behaviour change over time is supplied by the split-middle technique. It reveals a linear trend in the data, characterizes present performance, and predicts future performance. "Rate of behaviour" (frequency/time) has been advocated as the most useful measure for this method (Kazdin, 1984:313). Problems do exist in drawing inferences when using this method, but as a descriptive tool the split-middle technique provides valuable information about level and slope changes, that is otherwise seldom reported.

On the issue of whether statistical tests should be used to draw inferences from single case research, Kazdin (1984:321) concludes that statistical analyses does not necessarily conflict with single case designs or their purposes; that when applied research attempts to develop a technology of behaviour change and to achieve clinically important effects, statistical analyses will have limited value, but that there are several uses of statistics that may contribute to the goals of applied research.

CONCLUSION

The World Health Organization and the report of the inquiry into disability in South Africa define rehabilitation as an effective, goal-oriented and time-limited process (Department of National Health and Population Development, 1987). With this implied emphasis on accountability in disabled care, personnel will find it increasingly advantageous to demonstrate scientifically the impacts of their clinical programmes on their patients/clients (Silverman, 1977:xiii). The subcommittee on speech impairment stresses the importance of a research methodology applicable to the wide variety of multilingual, multicultural disabilities in the Republic of South Africa.

A situation analysis of the state of disability care reveals inter alia that there are insufficient services and insufficient knowledge about the communicatively disabled in this complex society. While these facts stress the need for basic and applied research, the employment of typical subject designs (group designs) for the evaluation of applied behavioural interventions is rendered impossible - the primary task of speech-language-hearing therapists being the assessment and rehabilitation of the communicatively disordered, usually on an individual basis.

The value of research with large populations should not be underestimated, especially because of the advantage of generalization. But, as a solution to a variety of practical problems, viz. the one-to-one relationship in therapy, and individual differences, single case experimentation should be utilized more extensively.

Single case experimental designs are presented as a methodology particularly applicable to the needs of disabled care. It provides data concerning the "typical" behaviour of an individual subject under experimental conditions, allows for the evaluation of intervention strategies applied to a specific individual, and creates the possibility to generalize to the population from which the subject was selected, while not on a statistical, then on a logical basis (Silverman, 1977:67).

The research potential of health care personnel should be tapped by bringing the advantages of single case experimental research to their attention. Ultimately this will be to the benefit of the disabled.

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Hoofopskrifte moet, waar dit van toepassing is, in die volgende volgorde wees: METODE, RESULTATE, BESPREKING, GEVOLGTREKKING, ERKENNINGS en VERWYSINGS.

Tabelle en Figure moet op afsonderlike bladsye verskyn (een bladsy per tabel/illustrasie). Figure, grafieke en lyntekeninge moet oorspronklike weergawes wees en moet in swart ink op wit papier van 'n hoë gehalte gedoen word.

Letterwerk wat hierop verskyn moet eenvormig wees, professioneel gedoen word en daar moet in gedagte gehou word dat dit leesbaar moet wees na 'n 50%-verkleining in drukwerk. Letterwerk by die illustrasie moet onder geen omstandighede getik word nie. Verklarings of omskrywings moet nie in die illustrasie nie, maar daaronder verskyn. Die byskrifte van tabelle moet bo-aan verskyn en dié van figure onderaan. Tabelle en figure moet in die volgorde waarin hulle verskyn, genommer word (met Arabiese syfers). Die hoeveelheid materiaal in die vorm van tabelle en illustrasies wat toegelaat word, word deur die redakteur bepaal (gewoonlik nie meer as 6 nie).

Verwysings. Verwysings in die teks moet voorsien word van die skrywer se van en die datum, bv. Van Riper (1971). Waar daar meer as twee skrywers is, sal *et al.* na die eerste skrywer voldoende wees. Die name van alle skrywers moet in die Verwysingslys verskyn. Verwysings moet alfabeties in 3-spasiëring aan die einde van die artikel gerangskik word. Vir die aanvaarde afkortings van tydskrifte se titels, raadpleeg die vierde uitgawe (Oktober) van *DSH ABSTRACTS* of *The World List of Scientific Periodicals.* Die getal verwysings wat gebruik is, moet nie veel meer as 20 wees nie.

Let op die volgende voorbeelde:

Locke, J.L. Clinical Phonology: The Explanation and Treatment of Speech Sound Disorders. *J. Speech Hear. Disord.*, 48, 339-341, 1983.

Penrod, J.P. Speech Discrimination Testing. In J. Katz (Ed.) *Handbook of Clinical Audiology*, 3de ed., Baltimore: Williams & Wilkins, 1985.

Van Riper, C. *The Nature of Stuttering.* Englewood Cliffs, New Jersey: Prentice Hall, 1971.

Proewe. Galeiproewe sal waar moontlik aan die skrywer gestuur word. Die onkoste van veranderings, behalwe tipografiese foute, sal deur die skrywer self gedra moet word.

Herdrukke. 10 herdrukke sonder omslae sal gratis verskaf word.

Alle manuskripte en korrespondensie moet gerig word aan:

Die Redakteur,
Die Suid-Afrikaanse Tydskrif vir Kommunikasieafwykings.
Die Suid-Afrikaanse Vereniging vir Spraak- en Gehoorheelkunde,
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