



eCOMMONS

Loyola University Chicago
Loyola eCommons

Master's Theses

Theses and Dissertations

1997

Prevention of Industrial Accidents and Childhood Injuries: A Meta-Analysis

Julia Trillos
Loyola University Chicago

Follow this and additional works at: https://ecommons.luc.edu/luc_theses

 Part of the [Psychology Commons](#)

Recommended Citation

Trillos, Julia, "Prevention of Industrial Accidents and Childhood Injuries: A Meta-Analysis" (1997). *Master's Theses*. 4238.

https://ecommons.luc.edu/luc_theses/4238

This Thesis is brought to you for free and open access by the Theses and Dissertations at Loyola eCommons. It has been accepted for inclusion in Master's Theses by an authorized administrator of Loyola eCommons. For more information, please contact ecommons@luc.edu.



This work is licensed under a [Creative Commons Attribution-Noncommercial-No Derivative Works 3.0 License](#).
Copyright © 1997 Julia Trillos

LOYOLA UNIVERSITY CHICAGO

PREVENTION OF INDUSTRIAL ACCIDENTS AND CHILDHOOD
INJURIES: A META-ANALYSIS

A THESIS SUBMITTED TO
THE FACULTY OF THE GRADUATE SCHOOL
IN CANDIDACY FOR THE DEGREE OF
MASTER OF ARTS

DEPARTMENT OF PSYCHOLOGY

BY

JULIA TRILLOS

CHICAGO, ILLINOIS

JANUARY 1997

Copyright by Julia Trillos, 1997
All rights reserved.

TABLE OF CONTENTS

	Page
LIST OF TABLES	v
LIST OF FIGURES	vi
Chapter	
1. INTRODUCTION	1
Accident Prevention in Industrial Settings	4
Behavioral Program's Impact	6
Childhood Injuries	6
Meta-Analysis	10
Meta-Analysis of Single-Subject Research	11
2. METHOD	17
Sample of Studies	17
Variables Coded	18
Calculation of Effect Sizes	18
3. RESULTS	20
Study Characteristics	20
Interventions	20
Outcome Measures	25
Calculation of Effect Sizes	26
Comparing PNDs and Effect Sizes	27

Additional Analyses	27
4. DISCUSSION	39
Appendix	
A. COMPUTATION OF THE PND	43
B. CALCULATING EFFECT SIZES	45
C. CODING SCHEME FOR META-ANALYSIS OF ACCIDENT PREVENTION	54
REFERENCES	57
VITA	65

LIST OF TABLES

Table	Page
1. Summary of Study Characteristics	21
2. Reliability if Determined, Standardized Effect Size (d) and PND for Reviewed Studies	22
3. Relationship Between Effect Sizes Calculated by PND and Other Methods (Study Level)	29
4. Relationship Between Effect Sizes Calculated by PND and Other Methods (Intervention Level)	31
5. Outcomes Using Studies as Unit of Analysis	32
6. Outcomes Using Interventions as Unit of Analysis	33
7. Homogeneity Analyses	37

LIST OF FIGURES

Figure	Page
1. ES by PND and other methods (Study Level)	28
2. ES by PND and other methods (Int. Level)	30
3. Effect Size by Behavioral Technique	35
4. Effect Size by Combination of Behavioral Techniques	36

CHAPTER 1

INTRODUCTION

Industrial and personal accidents and their resulting injuries are a major public health concern. According to the National Safety Council (1985), every ten minutes two persons will be killed and 170 people will suffer disabling injury either on or off the job. Moreover, accidents cost billions of dollars annually for treatment of injuries, lost occupational time, insurance administration and other related costs (McAfee & Winn, 1989; Sulzer-Azaroff & De Santamaría, 1980).

Data showing the pervasiveness of Industrial accidents reveal that they are the fourth leading cause of death, after heart disease, cancer, and strokes (National Safety Council, 1979). In the case of children, unintentional injuries are the leading cause of death in childhood and adolescence (US Congress, Office of Technology Assessment, 1988). For this reasons, there has been a growing interest in injury epidemiology and prevention in these two fields.

Historically, accidents were attributed to fate or to some other uncontrollable source such as an act of God or a malevolent providence (Sulzer-Azaroff, 1978). Later, causation was seen as a function of personality characteristics such as accident proneness (Kerr, 1957) or deficiencies in environmental design. The more current view looks at

accidents as the result of an interaction between workers and their physical environment (Chhokar & Wallin, 1984; Hale & Hale, 1970). That is, the behavior (safe or unsafe) of the worker and the environmental conditions (hazardous or non-hazardous) as individual factors do not produce the accident. It is the combination of behavior and conditions that precipitates the accident. Consequently, researchers have attempted to identify environmental and human processes that affect safety.

However, traditional safety literature has tended to emphasize hazard and risk classification. For example, according to the Safety hierarchy presented by Barnett and Brickman (1986) the first priority in accident prevention is the elimination of danger, where danger is defined as a combination of hazard and risk. Hazard refers to an injury-producing agent, while risk is a measure of the frequency with which a hazard produces injury. Then, the second and third priorities in the Safety hierarchy have to do with safeguarding technology and placing warning signs respectively, whereas safety training techniques occupy the fourth place. In other words, safety practitioners' efforts are focused on controlling accidents through structural strategies, and although this approach has many advantages, these interventions have not been totally effective. For example, car passenger safety has been improved by adding seat belts to all new cars (i.e. by making structural changes); however, this strategy alone is not enough to enhance safety belt use among individuals. In other words, the presence of seat belts facilitates the use of a protective device to prevent injuries, but does not guarantee that the person is going to use it (Roberts, Fanurik & Layfield, 1987).

For these reasons, behavioral scientists have started to give attention to the circumstances that produce accidents, in the hope that behavioral based techniques (applied behavior analysis) could fill the gap (Reber & Wallin, 1983; Roberts, Fanurik & Layfield, 1987; Sulzer-Azaroff & DeSantamaría, 1980; Sulzer-Azaroff & Fellner, 1984).

The rationale behind the utilization of applied behavioral analysis arises from the premise that accidents and injuries are the result of an unsafe act performed by a person. Thus, behavioral interventions aim to pinpoint safe and unsafe behaviors, in order to change their rate of occurrence as a function of training, reinforcement, feedback, etc. (Haynes, Pine & Fitch, 1982; Karan & Kopelman, 1986; Komaki, Barwick & Scott, 1978; Sulzer-Azaroff, 1978; Sulzer-Azaroff & Fellner, 1984). The ultimate goal of the behavioral approaches is a reduction in injury rates, assuming that immediately observable behaviors are valid indicators of potential prevention and are significantly associated with longer term outcomes (Roberts, et.al., 1987). In other words, although injury rates or lost occupational time alone could serve as indicators of accident prevention, they do not represent a definite measure, given that not all accidents result on injuries. Therefore, the measurement of safe and unsafe behaviors provides a better estimate of the effectiveness of an accident prevention program. Programs to prevent accidents in industrial settings as well as to prevent childhood injuries have been consistently measuring safe and unsafe behaviors to assess their effectiveness. For this reason, this study attempts to integrate the findings in these two areas in order to calculate the overall magnitude of the effect for this approach.

Accident Prevention in Industrial Settings

According to Ellis (1975), safety experts can be divided between those who look at accident reduction as an engineering problem and those who see it as an issue of human motivation and education. The first school of thought was reflected by the Occupational Safety and Health Act of 1970, which put emphasis on eliminating engineering hazards through industrial safety inspection. In the five following years, safety experts were questioning the real benefits or effects of the law. The main concerns included: a) the safety standards being enforced by the law were not based upon scientific research in most cases; b) although there was a prevailing belief that safety inspections by government and insurance companies were effective in reducing accidents, the scientific evidence was contradictory or weak in the best case (Ellis, 1975).

A different approach looks at the correlation between psychological factors such as stress, age, experience level, personality characteristics, and accidents (Fellner & Sulzer-Azaroff, 1984). Some authors even looked at the relationship between absenteeism and accidents (Verhaegen, Vanhalst, Derijcke & VanHoecke, 1976).

Safety experts investigating human motivation use behavioral approaches where the antecedents and consequences of behavior are applied and analyzed. In general, the behavioral approach to accident prevention in industrial settings involves the following steps:

1) Identification of safety items or components, specifying safe and unsafe practices in order to identify targets for intervention. This is accomplished by examining accident reports, interviewing safety experts, supervisors and workers, referring to written regulations, observational data or insurance companies records (Komaki, Barwick & Scott, 1978; Komaki, Heinzman & Lawson, 1980; Reber & Wallin, 1983, 1984; Sulzer-Azaroff & Fellner, 1984). After this procedure, the pool of items must be refined to increase “the likelihood that improvement in those items will significantly impact upon safety in the plant” (Sulzer-Azaroff & Fellner, 1984, p.55).

In order to collect useful information on problematic practices and conditions, it is important to focus on the cause of the injury, corrective measures, the job operation being performed, and the appropriate procedure for performing the job.

2) Determination of which dependent variables to measure. In general, a list of safe and unsafe practices, conditions, and injury reports are taken as the dependent variables. According to Fellner and Sulzer-Azaroff (1984), practices refer to employees’ behaviors while operating a machine or performing a task. Conditions refer to the location of materials and equipment.

3) Development of a recording system. After an inspection of the plant, observing the practices and conditions, the behaviors to observe are set into a recording system where they can be checked as safe, unsafe, or not observed.

4) Design: usually, multiple baseline design across behaviors, setting or conditions is used.

5) Intervention: the behavioral approach in accident prevention typically uses a combination of feedback, reinforcement and training.

Behavioral Program's Impact

Although applied behavioral analysis has become extensively used in industrial settings, there are some concerns about its application. In their report of 24 studies using positive reinforcement and feedback, McAfee and Winn (1989) concluded that the selection of independent variables (cash, tokens, praise, etc.) and their magnitude was based primarily on practical considerations (e.g., ease of use, cost, what the company would support) rather than on "any conceptual models linking specific rewards of specific magnitudes to safety enhancement" (p.9). Moreover, programs using behavioral analysis show uneven effects across different types of outcome variables (McAfee & Winn, 1989).

The inconsistent results obtained across outcomes suggests the need to examine systematically program effects across different variables in order to summarize findings for the area as a whole and to identify, if possible, the conditions that give rise to differential findings. Such a task is suitable for meta-analysis as explained below.

Childhood Injuries

According to national statistics, unintentional injuries are the leading cause of death among children (Langley, Silva & Williams, 1987; Jones & McDonald, 1986; Rivara & Muller, 1987). For instance, injuries are responsible for half of the deaths of

children between 1 and 4 years old, three times more than the next cause, congenital anomalies. Moreover, injuries are responsible for more deaths than all the other causes combined, from this period through adolescence (Rivara & Muller, 1987).

Given the alarming statistics and the consequences for society in terms of loss of its youngest members, several attempts have been made to reduce childhood injuries.

In general, childhood injuries are considered the result of a combination of personal and environmental characteristics. However, there are several approaches to address childhood injuries. One of these approaches is the epidemiological model, where injuries are considered a disease that is the product of the interaction between a disease-producing agent and a susceptible host (Wilson & Baker, 1987). Within this framework, an agent is “an environmental entity whose action is necessary to produce the specific damage of interest and without which it cannot occur” (Haddon, 1980). Therefore, injuries are the diseases that result when the agents interact with the hosts (children). Among the agents, Wilson and Baker (1987) list different forms of energy such as mechanical, thermal, electrical, chemical and radiation. Then, in order to control injuries, the authors propose a structural approach whose function is to protect by design, that is, by concentrating in changes on the environment and the agent instead of behavioral changes (the Poison Prevention Packaging Act is an example). While some strategies can work without the child performing any action (passive, e.g., air bag), other strategies require modification of the child’s or caretaker’s behavior (active, e.g., safety belt use).

Strategies requiring frequent action are seen as less likely to prevent injury. In order for these active strategies to work, educational techniques have to be used (e.g. modeling, demonstrating, reinforcing, etc.) which becomes a behavioral approach.

A different method to approach childhood injuries is based on the American Academy of Pediatrics' (AAP) belief that health education through office-based counseling can contribute to injury prevention. Although a recent review of the literature (Bass et.al., 1993) examining the effects of primary care-based counseling in preventing childhood unintentional injuries shows positive results, the magnitude of the effects is not calculated.

The most extensively used approach to childhood injuries is the behavioral approach. Behaviorally based interventions emphasize changes in measurable behavior using reward and punishment, modeling, prompting and feedback, skills development and guided practice (Roberts, Fanurik & Layfield, 1987). These techniques have been widely used in training children on how to prevent accidents by poisoning, fire, and pedestrian injuries (Jones, Kazdin & Haney, 1981; Haney & Jones, 1982; Hillman, Jones & Farmer, 1986). However, professionals working in injury prevention are aware of individual differences among children for safe and unsafe behaviors. Several studies examining the history of children's injuries found that children's injuries were systematically related to children's behaviors (Matheny, 1987). The results showed that children with higher level of activity, emotional reactivity, inattention and distractibility were predisposed to have more injuries than contrasting children.

For these reasons, some researchers have been working at refining the psychological analysis of injury precursors and consequences. Peterson, Farmer and Mori (1987) propose three steps for the process analysis of injury risk: behavioral and environmental antecedents, the stages of response, and the consequences of the injury. That is, both victim's and caretaker's behaviors are assessed, starting with the behavioral sequence prior to the injury and proceeding until the risk of potential injury is removed. This model encompasses not only the physical and emotional state of the victim but also of the caretaker. Also, process analysis looks at injuries and near injuries, the latter being defined as situations in which serious injury is avoided by chance, not because of the child's deliberate intervention or response.

This analysis helps to identify more clearly what other authors call problematic behavior, which falls into two categories: excess behavior and deficient behavior. Excess behavior refers to instances in which unsafe behavior is so frequent that it produces too many opportunities for injury, whereas deficient behaviors increases chances of injury given that safe behaviors occurs too infrequently (Roberts, Fanurik & Layfield, 1987). Therefore, behavioral interventions target these two general classes of behavior, trying to decrease unsafe behaviors and increase safe behaviors. The interventions are generally focused on training children and caretakers, and assessing changes in safe or unsafe behaviors, in a way that a positive change is associated with longer term effects, i.e. a reduction in injury rates.

In other words, each intervention involves the observation of environmental hazards, unsafe behaviors and the safe behaviors that should be displayed to avoid injury. For example, in some studies about pedestrian injuries, the identification of children and situations with a high risk of injury is assessed using measures of exposure such as the relative risk ratio (RRR) (Stevenson, 1991). This ratio is calculated by taking the proportion of all injuries involving pedestrians in a subgroup (i), and dividing it by the proportion of all exposure accounted for by the subgroup (i). Then, an observation of the pedestrian's unsafe behaviors leads to the design of the training program. In other studies, a training program is designed and applied to different settings. Yeaton and Bailey (1983), implemented the program called PSIP (Pedestrian Safety Instructional Package) in different schools and then measured its effectiveness. Behavioral approaches have demonstrated that safety behaviors can be improved. However, many studies have included small numbers of subjects. As Roberts et.al. (1987) assert, there is a need to advance to larger scale programs and to consider generalization and maintenance. An analysis of the studies using behavioral techniques in childhood injury prevention could show the real magnitude of their effect, helping to clarify some of the main concerns about these kinds of interventions.

Meta-Analysis

Meta-analysis refers to “the statistical collection of analysis results from individual studies for the purpose of integrating the findings” (Glass, 1976, p.3). This statistical integration not only has the advantage of quantifying the magnitude of

treatment effectiveness, but also of combining the outcomes of different studies in order to find an average magnitude of effect (Light & Pillemer, 1984). Also, meta-analysis reveals the patterns of underlying relations between studies, making it possible to reach stronger conclusions given the amount of studies that can be included in the statistical review.

On the other hand, meta-analysis can avoid the potential problems of traditional literature reviews, namely: “1) selective inclusion of studies..., 2) differential subjective weighing of studies in the interpretation of a set of findings, 3) misleading interpretations of study findings, 4) failure to examine characteristics of the studies as potential explanations for disparate or consistent results across studies, and 5) failure to examine moderating variables in the relationship under examination” (Wolf, 1986, p.10).

In addition, meta-analysis is helpful in providing new directions for research by stressing gaps in the literature, and by finding trends that go unnoticed in individual studies (Light & Pillemer, 1984).

In the field of prevention of industrial accidents and childhood injuries, it is important to achieve the most precise interpretation of the interventions' outcomes, given that accidents are not only costly in terms of money, but also in terms of human lives and chronic physical disabilities. Moreover, a meta-analysis can be applied to assess the effectiveness of applied behavioral analysis even for interventions with small sample sizes.

Meta-Analysis of Single-Subject Research

Although the development of techniques of quantitative synthesis provided a powerful tool for research synthesis, researchers conducting meta-analysis usually do not include the results of single-subject research in their syntheses (Light & Pillemer, 1984). The main reason is the difficulty in obtaining a suitable effect size. In between-groups research, the metric used to summarize results quantitatively is based on standard statistical procedures (means, standard deviations, t or F values) reported in the studies. In contrast, intra-subject design research is difficult to summarize given the controversial methods of statistical analysis of time-series data (Center, Skiba & Casey, 1985-1986).

According to Scruggs, Mastropieri, Cook and Escobar (1986) more specific reasons to avoid the calculation of standardized mean difference effect sizes include: “a) limited within-phase observations and idiosyncratic variability of within-case data resulting in effect sizes of questionable meaningfulness; b) nonindependence of single case data which violates assumptions underlying parametric statistical analysis; c) the fact that baseline conditions often contain floor or ceiling levels of performance with no variability making computation of standard effect sizes impossible” (p.261).

These problems pose a special challenge for researchers working in areas using single-subject research (e.g., special education), given that synthesis efforts are nevertheless needed in order to enhance external validity and support conclusions across a large number of studies (Tawney & Gast, 1984).

Traditionally, the analysis of single-subject studies is performed using visual inspection of graphed data involving subjective evaluations of baseline trends,

overlapping data between phases, and changes in variability across phases. However, the reliability of visual analysis has been questioned, given the difficulty of highly trained analysts to achieve consensus (Center, Skiba & Casey, 1985-1986; Scruggs, Mastropieri & Casto, 1987).

Some authors have proposed the use of a one-way analysis of variance design based on repeated measures or in the comparison of aggregated baseline and treatment phases (Shine & Bower, 1971). However, one of the assumptions for ANOVA is violated in such analysis, that is, the error terms are not independent for all observations. Since successive observations of the same individual are assumed to be dependent, the residuals of these observations are also dependent (Gottman & Glass, 1978; Jones, Vaught & Weinrott, 1977).

The auto-regressive integrated moving averages model (ARIMA) has also been recommended for the statistical analysis of single-subject research, to avoid the problem of dependence among residuals (Jones, Vaught & Weinrott, 1977). Some of the problems with this model include: a) the process of model selection and testing is complex, since the number of iterations required may converge on no clear solution (Elashoff & Thoreson, 1978); and b) the analysis requires a minimum of 50-60 data points per phase, whereas most experiments using single-subject designs have many fewer observations (Center, Skiba & Casey, 1985-1986).

In the past few years, effect-size has been used to analyze single subject research. The most common formula to calculate effect size in group comparison is:

$$ES = \frac{\bar{X}_e - \bar{X}_c}{S}$$

where \bar{X}_e represents the mean of the experimental group, \bar{X}_c the mean of the control group, and S the pooled standard deviation of the sample. Although effect size is generally interpreted as the number of standard deviations the experimental group differs from the control group, this measure could be applied to single-subject research to generate effect size (Center, Skiba & Casey, 1985-86). Then, \bar{X}_e would be the mean of the subject's observations within the intervention phase, \bar{X}_c the mean of the observations in the baseline phase, and S the pooled within-phase standard deviation.

However, a clear interpretation of an effect size depends on the independence of error for the successive observations. Although this has been considered a problem with single subject interventions, research findings show that these concerns are unfounded. From his results in two studies, Huitema (1985) concludes that the residuals of general linear models fit to applied behavioral data are not autocorrelated. Therefore, conventional statistical methods could be employed to analyze this type of data.

According to Center, Skiba and Casey (1985-86) there is a more serious problem with the effect size because it measures the change in level, but does not take trend into account. For this reason, they propose a different way to calculate effect size using a step-wise regression approach, where it is possible to assess changes in level, changes in slope and the combined effects of level and slope changes.

However, this model has some limitations. First, it calculates three separate but interdependent effect sizes instead of one. Second, the interpretation of the effect size

measure is different for single-subject and between-group interventions. In single-subject interventions, the effect sizes obtained from the regression approach are interpreted as “indices of the amount of variance accounted for as a function of the treatment” (p.398), whereas a between-group effect size is generally interpreted “in terms of the number of standard deviations by which the experimental group outperformed the control group” (p.398). Therefore, it is unclear to what extent effect sizes from single-case and between-groups interventions are comparable. Finally, the number of data points in the baseline phase is in general too small, therefore, the regression model is applied with very few degrees of freedom (Center, Skiba & Casey, 1985-86).

Given the potential problems of data interdependence, sample size, normality and homogeneity in single-subject research, Scruggs, Mastropieri and Casto (1987) propose a nonparametric approach which evaluates the ordinal relationships in single-case data. According to these authors, “the most important evaluative criterion of an effective outcome is the proportion of overlapping data displayed between treatment and baseline” (p.27).

For this reason, to analyze single-subject research meta-analytically they recommend the use of an outcome metric based upon percentage of nonoverlapping data (PND) between treatment and baseline phases. The PND is computed by indicating the number of treatment data points that exceeds the highest baseline data point in an expected direction and dividing it by the total number of data points in the treatment phase (Scruggs, Mastropieri & Casto, 1987). As Scruggs, Mastropieri, Forness and

Kavale (1988) explain, “this procedure is analogous to the “D” statistic proposed by Kraemer and Andrews (1982) for computing effect sizes based upon the degree of overlap in treatment and control group distribution in group experimental research, when distributions are nonnormal and the value of all observations is known” (p.260) (For an example see Appendix A).

However, the authors consider that this procedure is not appropriate when there are orthogonal slope changes, inappropriate baseline trends and ceiling or floor effects. Orthogonal slope changes might be seen when there is an “extinction” effect present in the second baseline of a reversal design. In this case, it is recommended to compute the PND between the second phase of treatment and the first baseline. An inappropriate baseline trend that could generate overestimation of treatment effects when using PND can be seen when the baseline data shows trends in the expected direction of the outcome. Then, when inappropriate baseline trends are large, Scruggs, Mastropieri and Casto (1987) recommend to exclude those data from further analysis given that they compromise the interpretability of study outcomes. In the case of data presenting “floor” or “ceiling” effects, the following arbitrary rule was developed: “a measure of data overlap cannot be confidently calculated when the treatment data reflecting floor levels of performance are compromised by no more than three, nor less than 33 1/3% of zero baseline data points, and baseline and treatment levels of variability are markedly different” (p.29).

CHAPTER 2

METHOD

Sample of Studies

The literature search was done from 1970, when researchers practically abandoned accident proneness” theories in favor of a more integrated theoretical view (McAfee & Winn, 1989), defining accidents as caused by a combination of a person’s behavior and environment characteristics, through December 1994. To locate relevant studies, the first step was to conduct a computer-based information search through Psychological Abstracts (Psyc INFO). Then, a review of references from these studies provided new studies and the name of the journals most likely to cover material on accident prevention.. The following journals were manually searched study by study: Journal of Organizational Behavior Management; American Journal of Public Health; Journal of Safety Research; Journal of Occupational and Organizational Psychology; Personnel Psychology; Journal of Organizational Behavior; Journal of Occupational Psychology; Academy of Management Journal; Academy of Management Review; Journal of Social Issues; Journal of Applied Behavior Analysis; and Pediatrics. In order to control for publication bias, a computer search of Dissertation Abstracts International and ERIC/CAPS was also performed. In addition, the BBS directory of computer networks related to occupational safety and health was consulted to locate additional studies. From all these sources, 44 studies were located.

Variables Coded

Each study was coded for the following variables: 1) study characteristics (year of publication, source, type of setting, length of intervention, etc.); 2) design characteristics (type of design, sample size, threats to internal validity, target behaviors, target conditions, reliability); 3) comparison information (comparison number, type of comparison, type of control group, sample size of treatment group and control group for this comparison); 4) intervention characteristics (behavioral techniques used, combination of behavioral techniques, identity of treatment deliverer, number of observations in baseline and treatment phases); 5) outcome measures (type of outcome measure, specific outcome measure); 6) effect size information (see the coding scheme in Appendix C).

Calculation of effect sizes

From a careful review of the literature it appeared that interventions to prevent accidents typically follow a multiple baseline design to measure the effectiveness of the treatment applied, where a control group is not considered for a comparison. Then, the first approach used to calculate effect size was the PND (Percentage of Nonoverlapping Data) for those studies providing suitable graphics.

For studies reporting means and standard deviations the following formula was used:

$$ES = \frac{\bar{X}_e - \bar{X}_c}{S}$$

where \bar{X}_e is the mean of the observations within the intervention phase, \bar{X}_c the mean of the observations in the baseline phase, and S the pooled within-phase standard deviation.

The effect sizes of studies reporting t, F or p values was calculated using the formulae listed in Appendix B.

After the raw effect size per intervention was obtained, effect sizes were weighted in order to assure that greater weight was given to larger samples using the procedure proposed by Hedges and Olkin (1985). Then, interventions within the same study were combined to get an adjusted effect size per study using the following formula:

$$d = \frac{\sum_{i=1}^N d_i W_i}{\sum_{i=1}^N W_i}$$

Also, the number of outcome measures in each study was considered when calculating an overall effect size given that studies should not contribute differentially more data to the analysis. For this reason, two overall effect sizes were calculated, one using the study as the unit of analysis, and the other using outcome measure as the unit of analysis.

The next step in the meta-analytic procedure was to analyze whether or not effect sizes varied significantly from one study to another. That is, a test of homogeneity of ES was performed in order to find which characteristics of the studies could be used as predictors of the effect size.

Finally, an overall effect size was calculated for each one of the areas (industrial settings and childhood injuries) and then the effect sizes were combined in order to assess the effectiveness of the behavioral approach in general.

CHAPTER 3

RESULTS

Study Characteristics

Tables 1 and 2 present descriptive characteristics of the 27 studies. The studies included in the review were all published articles since searches in other publication outlets were fruitless. For instance, only one dissertation was located, but it was subsequently published as a journal article (Fellner & Sulzer-Azaroff, 1984). The initial number of studies found in the searches ($N = 44$) was ultimately reduced to 27 given that most of the studies failed to provide basic statistics and/or any graphical data precluding the calculation of any effect sizes.

Most of the studies ($N = 16$) were done in an industrial setting, and these studies had an average sample size of 191.73 ($SD = 211.67$), whereas those directed at childhood injury prevention had an average sample size of 56.81 ($SD = 82.84$). Although the length of intervention varied from study to study, industrial interventions were longer ($M = 77.31$ weeks including the particularly long Fox et.al. 1987 study, and $M = 37.66$ weeks without it) than the childhood injury programs ($M = 8.81$ weeks). The majority of the studies ($N = 22$) used a single case design, 19 of these implementing multiple baseline designs; four used a between-groups design and one used a one group pre/post-test design.

Interventions

In industrial settings, behavioral interventions in accident prevention use similar

TABLE 1 --SUMMARY OF STUDY CHARACTERISTICS

STUDY	LENGTH OF INTERVENTION (In weeks)	TOTAL SAMPLE SIZE	OUTCOME MEASURE	TYPE OF DESIGN
INDUSTRIAL SETTING				
Chhokar, J.S. & Wallin, J.A. (1984)	42	58	Safe behaviors	Single case
Chhokar, J.S. & Wallin, J.A. (1984)	40	58	Safe behaviors	Single case
Cohen, H.H. & Jensen, R.C. (1984)	12	96	Unsafe behaviors	Multiple baseline
Fellner, D.J., & Sulzer-Azaroff, B. (1986)	48	150	Safe beh./Safe cond.	Multiple baseline
Fellner, D.J. & Sulzer-Azaroff, B. (1984)	60	158	Safe beh./Safe cond.	Multiple baseline
Fox, D.K., Hopkins, B.L., & Anger, W.K. (1987)	672	850	Safe behaviors	Multiple baseline
Haynes, R.S., Pine, R.C., & Fitch, G.H. (1982)	18	425	Accident rates	Multiple baseline
Karan, B.S. & Kopelman, R.E. (1987)	43		Mean accidents	Between groups
Komaki, J., Barwick, K.D., & Scott, L.R. (1978)	25	38	Safe behaviors	Multiple baseline
Komaki, J., Heinzman, A.T., & Lawson, L. (1980)	45	55	Safe behaviors	Multiple baseline
Reber, R.A. & Wallin, J.A. (1984)	56	105	Safe behaviors	Multiple baseline
Rhoton, W.W. (1980)	64	225	Unsafe behaviors	Single case
Sulzer-Azaroff, B., et.al. (1990)	36	238	Safe beh./Safe cond.	Multiple baseline
Sulzer-Azaroff, B., & Santamaria, M.C. de (1980)	12	6	Unsafe conditions	Multiple baseline
Zohar, D. (1980)	36	250	Safe behaviors	Multiple baseline
Zohar, D., Cohen, A., & Azar, N. (1980)	28	164	Safe behaviors	Multiple baseline
CHILD SETTING				
Dershewitz, R.A. & Williamson, J.W. (1977)	16	205	Unsafe conditions	Between groups
Haney, J.J., & Jones, R. (1982)		4	Safe behaviors	Multiple baseline
Jones, R.T., Kazdin, A.E., & Haney, J.I. (1981)		5	Safe behaviors	Multiple baseline
Jones, R.T., Kazdin, A.E., & Haney, J.J. (1981)	7	4	Safe behaviors	Multiple baseline
Mori, L., & Peterson, L. (1986)	13	30	Safe behaviors	Between groups
Peterson, L. (1984)	8	6	Safe behaviors	Multiple baseline
Peterson, L., & Thiele, C. (1988)	4	14	Safe behaviors	Other
Rothengatter, T. (1984)	3	222	Safe behaviors	Between groups
Tertinger, D.A., Greene, B.F., & Lutzker, J.R. (1984)	16	6	Unsafe conditions	Multiple baseline
Yeaton, W.H., & Bailey, J.S. (1978)	3.5	24	Safe behaviors	Multiple baseline
Yeaton, W.H. & Bailey, J.S. (1983)		105	Safe behaviors	Multiple baseline

TABLE 2
RELIABILITY IF DETERMINED, STANDARDIZED EFFECT SIZE (d) AND PND FOR REVIEWED STUDIES

STUDY	RELIABILITY	d	PND
INDUSTRIAL SETTING			
Chhokar, J.S. & Wallin, J.A. (1984)	0.93		1.00
Chhokar, J.S. & Wallin, J.A. (1984)	0.93	0.37	0.70
Cohen, H.H. & Jensen, R.C. (1984)	0.90		0.81
Fellner, D.J., & Sulzer-Azaroff, B. (1986)	0.89	0.91	0.07
Fellner, D.J. & Sulzer-Azaroff, B. (1984)	0.86	1.66	0
Fox, D.K., Hopkins, B.L., & Anger, W.K. (1987)			0.96
Haynes, R.S., Pine, R.C., & Fitch, G.H. (1982)		1.85	0
Karan, B.S. & Kopelman, R.E. (1987)			0
Komaki, J., Barwick, K.D., & Scott, L.R. (1978)	0.99		0.47
Komaki, J., Heinzman, A.T., & Lawson, L. (1980)	0.95	1.14	0.63
Reber, R.A. & Wallin, J.A. (1984)	0.88	0.30	0.98
Rhoton, W.W. (1980)			0.92
Sulzer-Azaroff, B., et.al. (1990)			0.83
Sulzer-Azaroff, B., & Santamaria, M.C. de (1980)	0.94		0.68
Zohar, D. (1980)			0.94
Zohar, D., Cohen, A., & Azar, N. (1980)			0.44
CHILD SETTING			
Dershewitz, R.A. & Williamson, J.W. (1977)		0	
Haney, J.J., & Jones, R. (1982)	0.99		0.93
Jones, R.T., Kazdin, A.E., & Haney, J.I. (1981)	0.99	5.52	1.00
Jones, R.T., Kazdin, A.E., & Haney, J.J. (1981)			1.00
Mori, L., & Peterson, L. (1986)	0.90	1.51	
Peterson, L. (1984)	0.94		0.98
Peterson, L., & Thiele, C. (1988)	0.93	1.96	
Rothengatter, T. (1984)		0.93	
Tertinger, D.A., Greene, B.F., & Lutzker, J.R. (1984)			0.84
Yeaton, W.H., & Bailey, J.S. (1978)	0.93		0.69
Yeaton, W.H. & Bailey, J.S. (1983)	0.94		0.99

procedures. First, the characteristics of the accidents prior to the intervention are analyzed to pinpoint possible causes. The researchers look at archival data and/or interview the workers to determine which behaviors should be assessed. That is, for each type of work a set of safe/unsafe behaviors is defined and this specific set of behaviors is described, analyzed and discussed with and by the workers. Then, the specific behavioral intervention is implemented and observations are made periodically over time.

For instance, Komaki, Barwick and Scott (1978) conducted a study in a wholesale bakery where there had been a dramatic increase in the injury frequency rate. The first step was to look at previous safety efforts and the causes of previous injuries. It was determined that workers did not receive formal training, safety was rarely mentioned and no single person was responsible for safety. Furthermore, they found that few accidents occurred because of machine malfunctions. The observational code that was designed to measure safety level contained items taken from accident reports and information obtained from supervisors. The number and characteristics of the safety items were different for the two departments in the bakery, namely makeup and wrapping departments. Then, each item was clearly defined. For instance, instead of the vague phrase “liquid spill, the item was defined as “any liquid accumulation in excess of 20 cm in diameter and 1 mm deep”. Another example of a safety item was: “when picking up pans from the conveyor belt, no more than two pans are picked up prior to placing the pans on the pan rack”.

In contrast, interventions aimed at children's injury prevention use manuals where safe behaviors have been defined and described according to the situations where injury is to be prevented. That is, children learn how to respond to risk situations where the wrong responses could result in injury or even death (e.g., fire, poisons, cuts, reactions to strangers). Mori and Peterson (1986) provide an example of training criteria for hand cuts that includes the following steps: "1) Wrap a clean cloth around the cut; 2) Press firmly on the cut so that it closes and slows the bleeding; 3) Call the number Mom says to call; 4) Stay really quiet until someone comes; lie down with hand elevated" (p.108).

The main difference between behavioral interventions in industries and with children is that, in the first setting, the trained behaviors are practiced and measured in real day to day work situations, whereas children's behaviors are measured in hypothetical role playing situations. Therefore, actual reductions in injuries are only determined in industrial studies.

Among the 27 studies, 13 used only one behavioral technique, that is, only one phase of treatment. These treatments consisted of either training (8 studies), feedback (4 studies), and Goal setting (1 study). The other studies implemented either different behavioral techniques at different phases (3 studies) or a combination of behavioral techniques in the same treatment phase (11 studies). A study by Chhokar and Wallin (1984) can be used to illustrate the use of the behavioral techniques. For instance, training consisted of showing the workers 51 slides of unsafe and safe ways to perform several activities in the plant, featuring regular employees. Goal setting was done by

establishing a criterion for safe behaviors to be reached throughout the plant. In this case, it was determined to fix the goal on 95% safe behavior, based on baseline performance and the management's suggestions that this goal was difficult to achieve but attainable.

In some analyses, in order to evaluate the effectiveness of different behavioral techniques, each treatment phase was coded as a separate intervention within each study. Overall, 161 interventions were coded.

Outcome Measures

Outcomes consisted of safe and unsafe behaviors and conditions. An example of the use of unsafe conditions as an outcome measure is the study by Sulzer-Azaroff and Santamaria (1980) where the authors examined accident reports, interviewed safety experts, supervisors and workers and referred to written regulations in order to develop a checklist of possible hazardous conditions. This list included six major categories: (1) Obstructions of walking-working surfaces; (2) Exit, ladder, or sprinkler obstruction; (3) Hazardous materials; (4) Hazardous materials storage; (5) Hazardous machine guarding; and (6) Electrical hazards. As an example of safe and unsafe behaviors, Komaki, Barwick and Scott (1978) reviewed accident reports and interviewed supervisors to pinpoint safety items in order to construct an observational code. This code included clearly defined items specifying the safe behavioral performance for any given task and each observer checked each item as safe, unsafe, or not observed.

The studies coded in the present analysis showed that outcome measures included more behaviors (safe = 20 studies; unsafe = 2 studies) than conditions (safe = 3 studies; unsafe = 3 studies). A few studies used more traditional measures of safety rates (mean number of new accidents = 1 study; accident rates = 1 study). The study by Sulzer-Azaroff, Loafman, Merante and Hlavacek (1990) was the only one using both safe behaviors and conditions as outcome measures.

Calculation of Effect Sizes

The number of studies reporting data in both graphical form and summary statistics was very small (7 studies). For all those studies displaying graphical data, a mean PND was calculated across multiple intervention to obtain one overall PND per study. Similarly, in the case of studies providing suitable statistical information, one effect size was computed for each intervention and then they were averaged. In order to ensure that the average effect size was weighted by sample size, the procedure proposed by Hedges & Olkin (1985) was used. First, a weighting factor was estimated using the formula:

$$W_i = \frac{2(n_{i1} + n_{i2})n_{i1}n_{i2}}{2(n_{i1} + n_{i2}) + n_{i1}n_{i2}d_i}$$

where,

n_{i1} and n_{i2} = the sample size for each group on the comparison, and

d_i = the d-index of the comparison under consideration.

Then, in order to find the adjusted effect size per study, the following formula was used:

$$d. = \frac{\sum_{i=1}^N d_i W_i}{\sum_{i=1}^N W_i}$$

Comparing PNDs and Effect Sizes

Overall, it was possible to estimate both ESs and PNDs for only seven studies. In four additional studies only ESs were computed, while in 16 other studies only PNDs were calculated (see Table 2). Figure 1 and Table 3 show the relationship between ESs and PNDs for the seven studies providing the information necessary to obtain both estimates. Given the small amount of studies it was not reasonable to calculate a correlation to see if there was a relationship between the two types of estimates. Then, the same relationship between ESs and PNDs was explored at the intervention level, which results in 32 cases (see Figure 2 and Table 4). Statistical analyses revealed that there was a nonsignificant correlation of 0.15 between the two sets of data.

Additional Analyses

In additional analyses, effect sizes were computed across studies using different variables such as setting, behavioral techniques used and outcome measures. Table 5 shows the ESs and PNDs taking setting as a variable at the study level. These results show that accident prevention programs with children have greater mean effect sizes for both types of estimates (M ES = 1.98, M PND = .92) than studies in the industrial setting

FIGURE 1 ES BY PND AND OTHER METHODS (INT LEVEL)

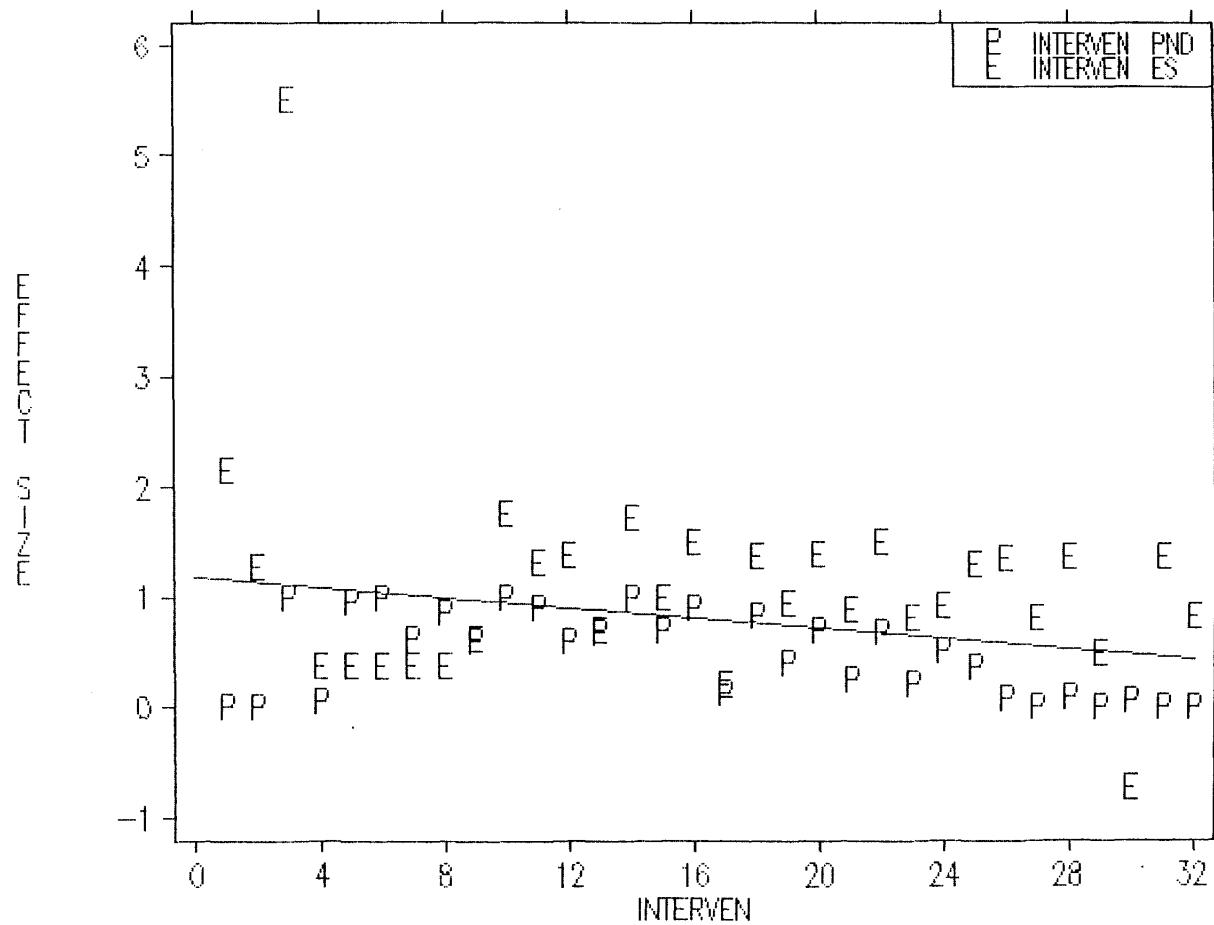


TABLE 3
RELATIONSHIP BETWEEN EFFECT SIZES CALCULATED BY
PND AND OTHER METHODS
(Study level)

STUDY	PND	ES Other
1	0.98	0.30
2	0	1.66
3	1.00	5.52
4	0.70	0.37
5	0.63	1.14
6	0.07	0.91
7	0	1.85

FIGURE 2 ES BY PND AND OTHER METHODS (STUDY LEVEL)

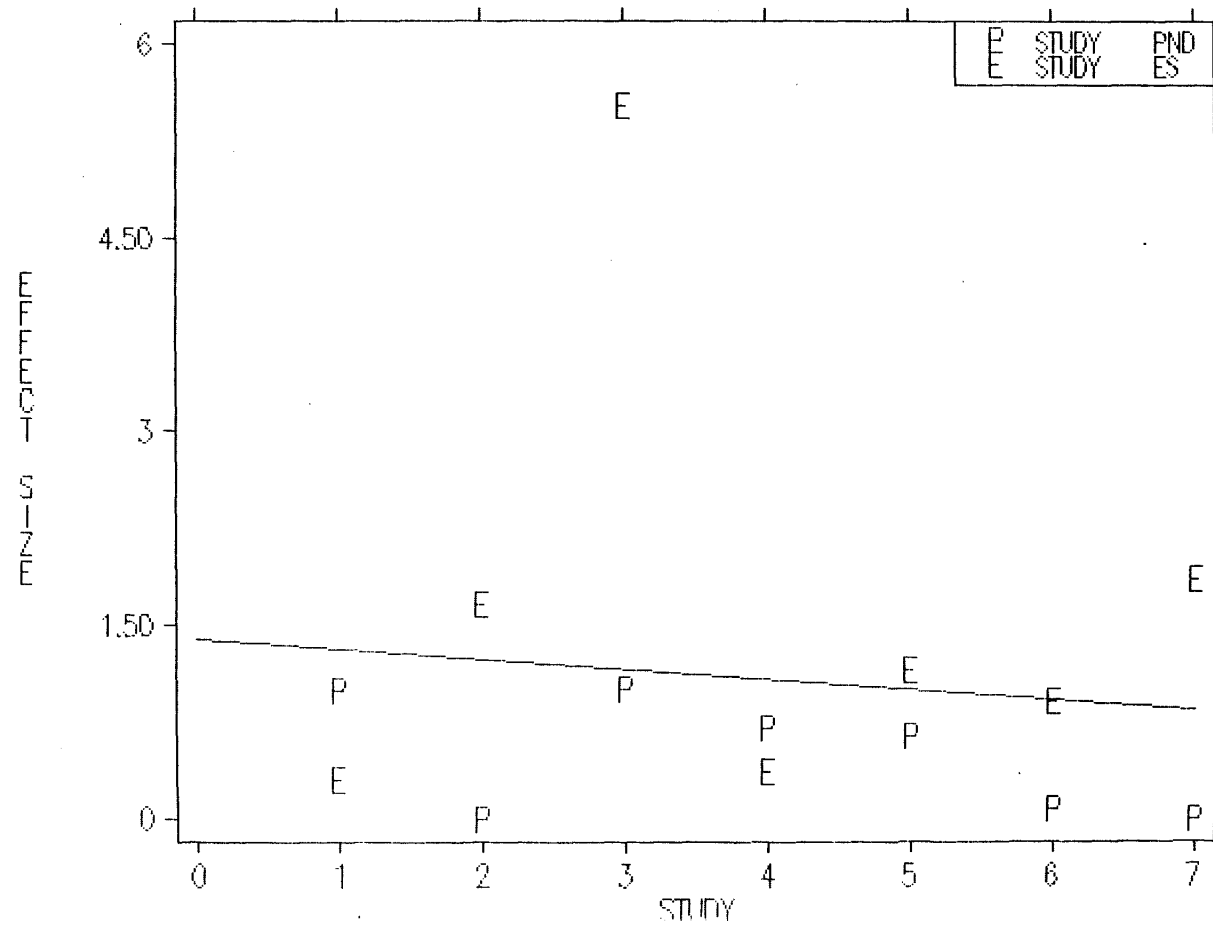


TABLE 4
RELATIONSHIP BETWEEN EFFECT SIZES CALCULATED
BY PND AND OTHER METHODS
(Intervention level)

INTERVENTION	PND	ES Other
1	0	2.16
2	0	1.28
3	1.00	5.52
4	0.06	0.37
5	0.96	0.37
6	1.00	0.37
7	0.62	0.37
8	0.87	0.37
9	0.60	0.61
10	1.00	1.76
11	0.90	1.31
12	0.60	1.39
13	0.67	0.69
14	1.00	1.72
15	0.70	0.99
16	0.90	1.50
17	0.13	0.21
18	0.83	1.36
19	0.40	0.93
20	0.70	1.38
21	0.25	0.89
22	0.67	1.49
23	0.20	0.80
24	0.50	0.92
25	0.35	1.29
26	0.07	1.35
27	0	0.81
28	0.10	1.37
29	0	0.49
30	0.05	-0.73
31	0	1.37
32	0	0.83

TABLE 5
OUTCOMES USING STUDIES AS UNIT OF ANALYSIS

VARIABLE	EFFECT SIZE		PND	
	Mean	N	Mean	N
<i>Setting</i>				
Industrial	1.04	6	0.59	16
Children	1.98	5	0.92	7

TABLE 6
OUTCOMES USING INTERVENTIONS AS UNIT OF ANALYSIS

VARIABLE	EFFECT SIZE		PND	
	Mean	N	Mean	N
<i>Behavioral Technique</i>				
Training	1.58	32	0.79	38
Feedback	1.72	2	0.57	15
Goal Setting	0.85	8	0.32	11
Combination of behavioral techniques	0.95	17	0.86	58
<i>Combination of behavioral techniques</i>				
Training/Feedback	1.44	8	0.83	30
Training/Goal Setting	0.37	3	0.56	3
Feedback/Goal Setting	0.3	1	-	-
Training/Goal Setting/Feedback	0.37	4	0.97	5
Training/Incentives	-	-	0.95	4
<i>Outcome Measure</i>				
Safe/Unsafe behaviors	1.21	49	0.81	89
Safe/Unsafe conditions	1.25	7	0.61	32

(\underline{M} ES = 1.04, \underline{M} PND = .59), but the differences are only significant for the PND estimates ($t = 2.22$, $df = 21$, $p < 0.037$). Table 6 shows the ESs calculated by PND and by any other method for behavioral techniques and outcome measures, using interventions as the unit of analysis. In the latter case, behavioral techniques were coded first into four main categories, i.e. training, feedback, goal setting, and combination of behavioral techniques (see Figure 3). Then, if the intervention was a combination, the specific techniques used were coded separately (see Figure 4). Finally, outcome measures were separated into two categories, safe/unsafe behaviors and safe/unsafe conditions.

After all effect sizes were computed, homogeneity analyses were carried out in order to determine if the variance of the effect sizes was significantly different from that expected by sampling error (Cooper, 1989). The first step of these analyses was to calculate the Q statistic to see if the d-indexes of all the studies were homogeneous, using the following formula by Hedges and Olkin (1985):

$$Q = \sum_{i=1}^n W_i d_i^2 - \frac{\left(\sum_{i=1}^n W_i d_i \right)^2}{\sum_{i=1}^n W_i}$$

When making comparisons across groups of studies, the researcher is looking for a nonsignificant Q within studies which suggests that the variability of ESs is due primarily to sampling error.

The results of this first analysis showed that the mean ES across all 10 studies was heterogeneous, that is, the difference between studies was not due to sampling error alone

FIGURE 3: EFFECT SIZE BY BEHAVIORAL TECHNIQUE

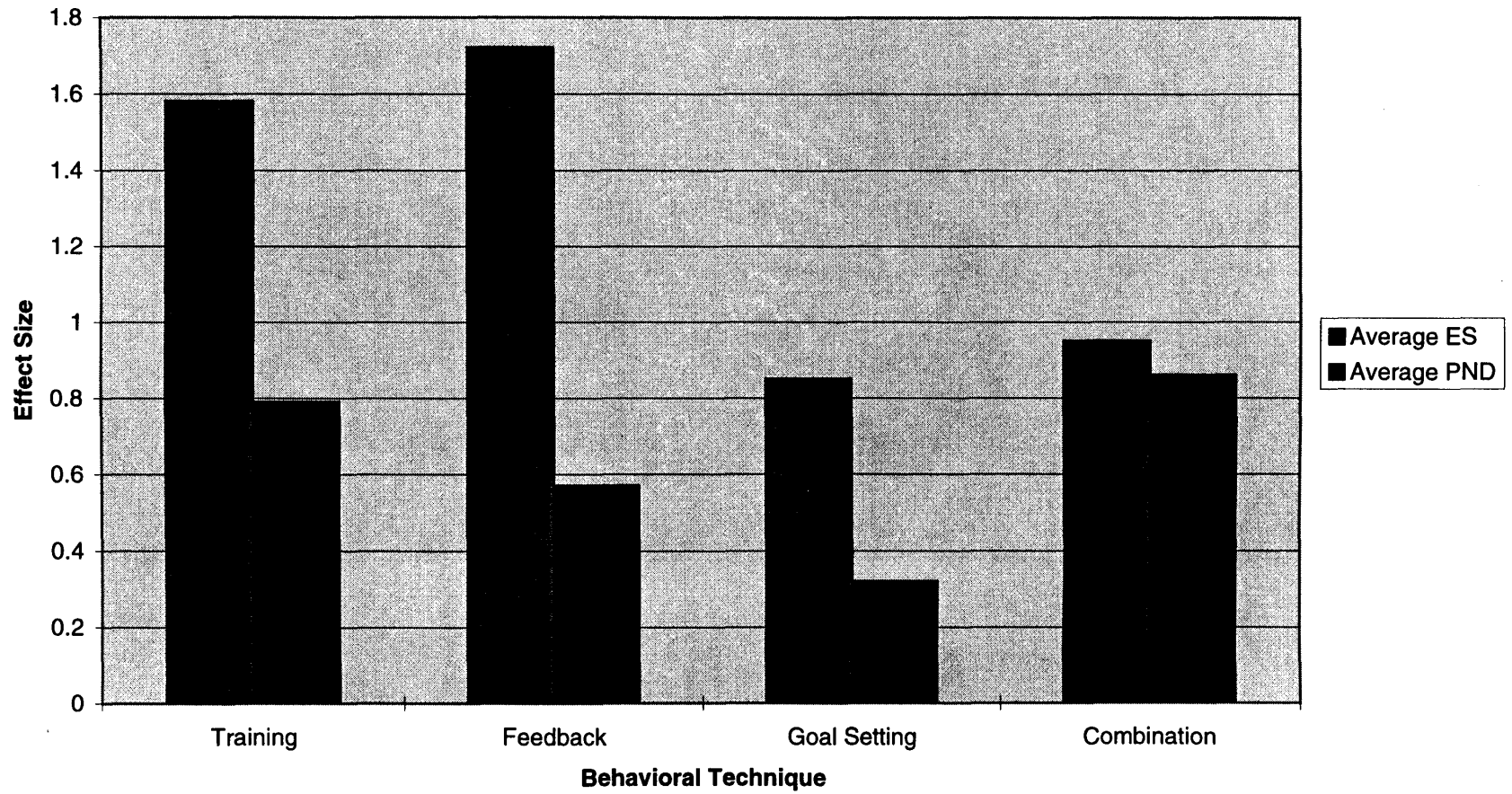


FIGURE 4: EFFECT SIZE BY COMBINATION OF BEHAVIORAL TECHNIQUE

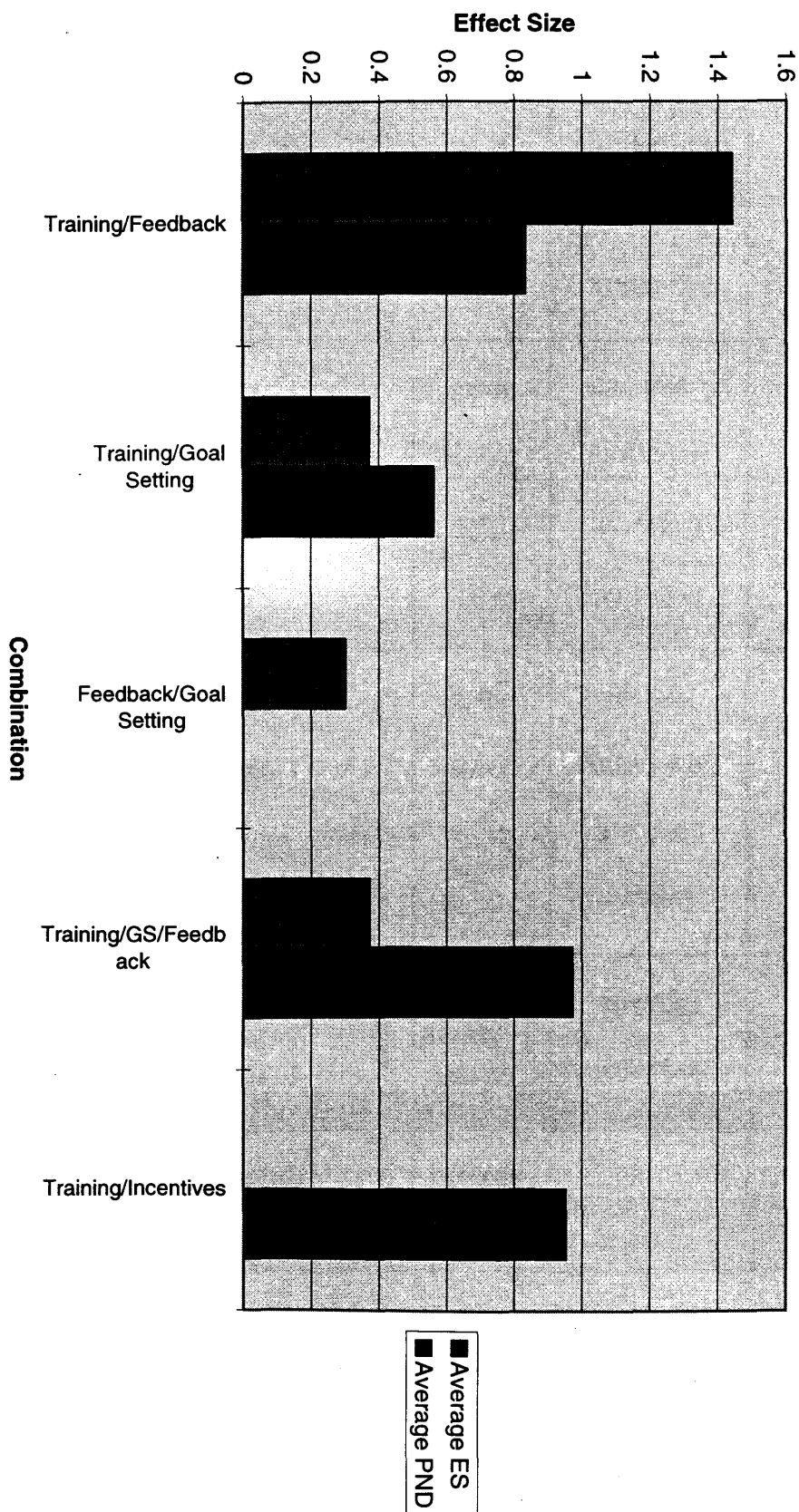


TABLE 7
HOMOGENEITY ANALYSES

		Q	Critical Value (N)
All studies		102.57	16.91(10)*
Variables			
Setting	Industrial	89.65	11.07(6)*
	Children	12.91	7.81(4)*
Behavioral Technique	Training	114.57	43.77(31)*
	Feedback	21.94	5.99(3)*
	Goal Setting	29.66	14.06(8)*
	Package	140.23	26.29(17)*
Training by setting	Industrial	9.00	15.50(9)
	Children	82.39	32.67(22)*
Outcomes	Behaviors	205.04	67.50(49)*
	Conditions	36.88	12.59(7)*
How effect size was calculated	Mean/S.D.	16.47	24.99(16)
	t score	108.75	19.67(12)*
	Estimate from p	0.36	15.50(9)
	F value	69.23	28.86(19)*

* = significant at 0.05 level

(see results on Table 7). Therefore, the next step was to test whether a methodological or conceptual distinction accounted for the variability of effects. Consequently, the studies were subdivided by setting, namely industrial and children, and a Q statistic was calculated for each subgroup of studies. Neither grouping factor produced a nonsignificant Q statistic. Several other grouping factors were tested at the intervention level, including behavioral technique used, outcome measures, how effect size was calculated, and training by setting (see the variables and results on Table 7). Unfortunately, even though there were a few nonsignificant findings, none of these partitions showed homogeneity within all groups and further subdivisions were not possible given the small number of studies.

CHAPTER 4

DISCUSSION

The main purpose of this review was to analyze in a systematic way the effectiveness of prevention programs implementing behavioral techniques to reduce industrial accidents and childhood injuries. This objective stemmed from the general concerns showing that although behavioral programs to reduce accidents seem to work, the findings in this area reflect arbitrary selection of independent variables, uneven effects of different outcome measures and the use of small sample sizes (McAfee & Winn, 1989; Roberts, Fanurik & Layfield, 1987).

The first obstacle confronted by the present meta-analysis was the small amount of studies in this area ($N = 44$), combined with the fact that 17 of the studies located failed to provide basic statistics amenable to meta-analysis. Furthermore, only 7 studies provided the information necessary to estimate both ES and PND; hence, any attempt to look into the degree of correspondence between these two approaches was thwarted.

However, the data show that effect sizes calculated by both PND and other methods show greater treatment effects for child studies than for the industrial setting (\underline{M} ES = 1.98 and 1.04, \underline{M} PND = .92 and 0.59, respectively). However, the differences between the two settings proved to be significant only for the PND estimates ($t = 2.22$, $df = 21$, $p < 0.037$). Furthermore, this finding is not conclusive given that the average sample

size for industrial studies was three times more than that of studies involving children. As Hedges (1981) demonstrated, the mean of effect sizes based on small samples is biased upward as an estimator of the population mean. In addition, it should be remembered that interventions in industrial settings measure the outcome in real day to day situations, which is not the case with children's interventions, where the behavioral sequences are staged.

In analyzing the overall effectiveness of behavioral programs in accident prevention, it should be noticed that the effect sizes obtained are far bigger than those reported by Lipsey and Wilson (1993), where the mean effect size for all 302 meta-analyses included is 0.50 ($SD = .29$). However, as Lipsey and Wilson (1993) state, there could be some artifacts making the studies look stronger than they are. The first artifacts that could inflate the effect size estimates is availability bias, given that the present meta-analysis only includes published studies. In their analysis, Lipsey and Wilson (1993) found that treatment effects reported in published studies are higher ($M = 0.53$) than those of unpublished studies ($M = 0.39$). Also, the studies included here used quasi-experimental designs, most of them multiple baseline designs, where the calculation of effect sizes could be analogous to that of a one-group pre-post design, showing an overestimation of treatment effects as Lipsey and Wilson (1993) found for the latter type of design.

Comparisons between different behavioral techniques in terms of their effectiveness seem to show that the effect size is higher when feedback is used in the intervention. However, this conclusion is only tentative since, as a general rule, interventions using feedback are part of a series of interventions where the first step is training. Therefore, we can see an improvement from training to feedback but is not clear if this improvement is a consequence of the impact of feedback or if it is due to the implementation of training followed by feedback. The same case could be made for the other techniques that are implemented after training and/or feedback. The need to include adequate control groups in these studies is evident.

In the present meta-analysis, some factors make it impossible to draw confident conclusions about the effectiveness of behavioral interventions in accident prevention. The relatively small number of studies and the characteristics of the data do not allow the researcher to carry out a complete analysis of the effect sizes, and even if the studies show positive effect sizes, we cannot identify which characteristics or components of the intervention influence outcomes.

In conclusion, although accident prevention interventions show an overall effectiveness, the implementation of stronger methodological designs is necessary to gather more information about how exactly these treatments work and how they can be improved. Such is the challenge for future researchers in this area.

APPENDIX A
COMPUTATION OF THE PND

APPENDIX A

Computation of the PND (example)

Source: Scruggs, T.E., Mastropieri, M.A., & Casto, G.
The quantitative synthesis of single-subject
research: Methodology and validation.
Remedial and Special Education, 8, p.28.

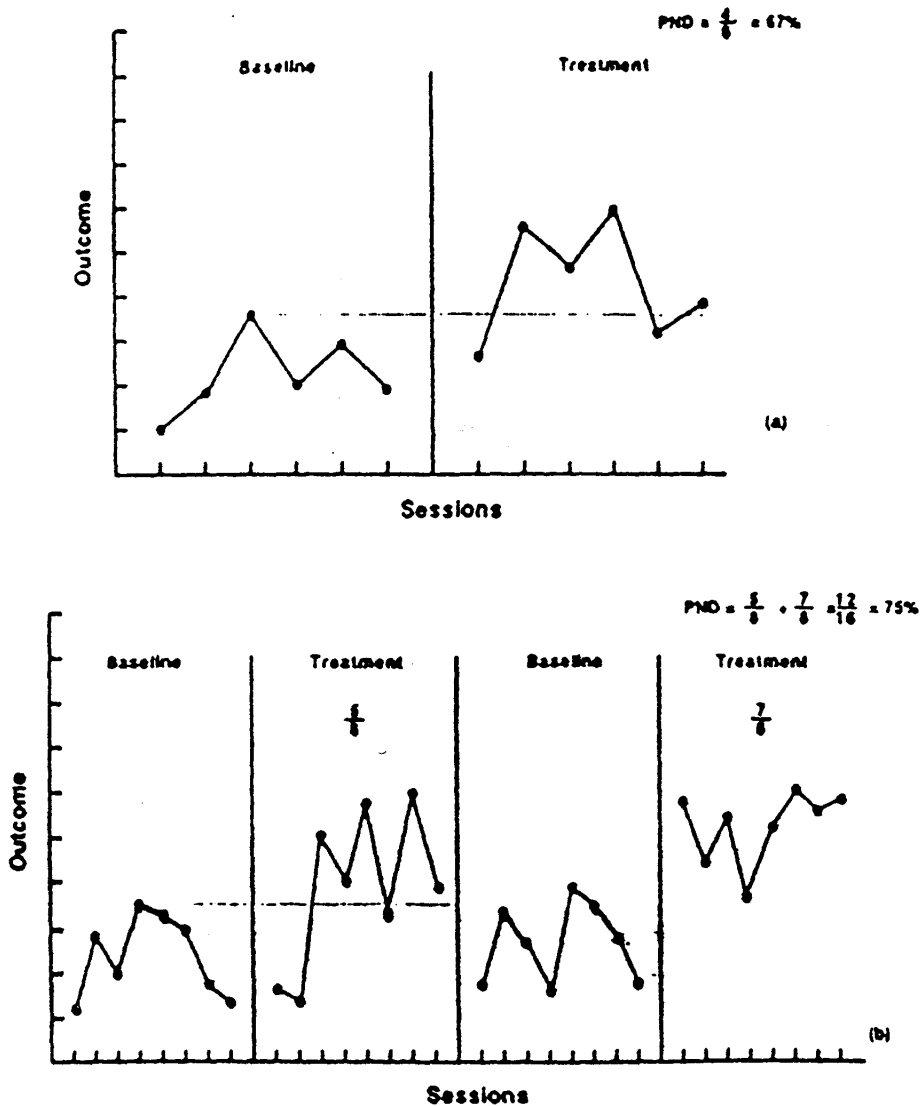


Figure 3. Computation of nonoverlapping data in an AB design (a) and an ABAB design (b).

APPENDIX B
CALCULATING EFFECT SIZES

APPENDIX B

From Durlak, J.A. (1993). Calculating Effect Sizes. Unpublished manuscript.

WE WANT TO CALCULATE AN EFFECT SIZE FOR EACH COMPARISON.

A comparison means between a treatment group vs a control group, we can also do comparisons between 2 kinds of treatments, i.e. behavioral vs nonbehavioral, etc., but we will be restricting our comparisons to treatment vs some type of control condition.

Comparisons are made on each outcome measure in each study!

An effect size is a quantitative way of showing how much more change occurred in one group vs another (over time when therapy or treatment is involved as in our case). We will ALWAYS put the treatment group first and then subtract the control group from this group.

Effect sizes are thus numbers. 90% of these numbers will fall somewhere between zero and let's say, .75. However, it is possible for effect sizes to be NEGATIVE IN SIGN, AND theoretically they may reach any value.

In our study, then, an effect size of +.50 would indicate that the treatment group changed positively and more than the controls. We will be obtaining multiple effect sizes for each study depending on several factors: (how many different types of treatment groups there are; what type of outcome measures are being used, etc.)

Different procedures are used to calculate effect sizes (ES), depending on what type of information is presented in the study.

TO CALCULATE AN EFFECT SIZE:

STEP ONE:

Determine if the ES is going to be positive or negative in sign. You have to be careful about this. Remember: we will ALWAYS be subtracting the control group number from the treatment group number. so if the control number is higher, the result will be a negative number. Here's the tricky part. For some outcome measures, higher numbers are bad (if the measure is of problems, or maladjustment, etc.) If higher numbers reflect more problems, then we expect the control group to have higher numbers (the treatment group should have fewer problems). So, if we subtract control from treatment, then a minus number is good, and we ignore the sign!!!! Sound confusing? Maybe all you should do for now is merely take note: did the treatment have positive effects? You can tell by: (a) what the authors say in their discussion or abstract, (b) by notes at bottom of tables where the outcome data are presented, and as a last resort, (c) by reading the METHOD section,

what it says about how a measure was scored. Somewhere it should tell you something like, ... "higher scores reflect more problems" (or less problems as the case may be).
CALCULATING EFFECT SIZES; SITUATION ONE: Means, standard deviations and the number of subjects in each group are presented. If situation ONE occurs in any study, **ALWAYS CALCULATE EFFECT SIZES AS DESCRIBED BELOW**

$$ES = \frac{M_t \text{ minus } M_c}{\text{pooled SD (standard deviation)}}.$$

M_t = mean of the treatment group at the end of treatment

M_c = mean of the control group at end of treatment

and pooled SD is calculated from this formula:

$$\text{pooled SD} = \sqrt{\frac{N_t \text{ minus } 1 \times S_{dt} + N_c \text{ minus } 1 \times S_{dc}}{N_t + N_c \text{ minus } 2}}$$

N_t = number of subjects in treatment group FOR THIS COMPARISON

N_c = number of subjects in control group FOR THIS COMPARISON

S_{dt} is the standard deviation for treatment group (you square this figure)

S_{dc} is the standard deviation for control group (you square this figure)

The figure, $\sqrt{\quad}$, means take the square root.

EXAMPLE: SEE ADDITIONAL PAGE

SITUATION TWO: IN THIS CASE, AUTHORS ONLY SAY SOMETHING TO THE EFFECT, THE RESULT OF AN ANALYSIS WAS NONSIGNIFICANT. They don't present M 's, SD 's or N 's.

This is easy!!!! The EFFECT SIZE IS SIMPLY ZERO.

SITUATION THREE: No M 's, SD 's or N 's given, but actual t test reported.

EFFECT SIZE = two times value of t divided by \sqrt{df} .

df = number of subjects in treatment group plus number in the control group FOR THIS COMPARISON minus two.

Remember, $\sqrt{\quad}$, is square root.

EXAMPLE. See additional page.

SITUATION FOUR: TWO GROUPS ARE COMPARED, AND A F TEST VALUE IS GIVEN.

$$t = \sqrt{F}.$$

Therefore, calculate the t value, given the F value, and then proceed according to instructions for situation THREE.

SITUATION FIVE: None of above situations hold, but authors say that t or F test analysis was significant at some p. level. p. .05, or p. .01 etc.

in this situation, we will work from tables to hunt backwards for what the actual t value was.

Look at the copy of Table A 4. The t values are listed in the table. we find t value by first:

(a) identifying appropriate column. This is the p. value reported by the authors. p. 01 for instance. then, we find the right row by noting how many df there are for the analysis. For a t test, df = Number of subjects in both groups minus 2. If there are ten subjects per group then df is 10 + 10 minus 2 = 18.

Find the t value that intersects 18 df with a p value of .01. in Table A 4. (answer is 2.878).

If df for a comparison is more than 120, just use the 120 row. If df is between some figure of df listed in the first row, say, df is 84, then use the row for the immediately higher df (i.e.90).

General note: in your calculations, bring figures out to 2 decimal places. t values of 2.878 = 2.88.

SITUATION 6.

Some studies use a repeated measures design and analysis. This means that pre and posttreatment scores for controls and experimental (treated children) are analyzed in a 2 by 2 ANOVA. when there are two groups in the study, one treated and one a control, **AND ONLY WHEN THERE ARE TWO SUCH GROUPS**, we calculate ES directly from the F values presented FOR THE INTERACTION F.

example... Report says the group by time interaction is significant. $F(1,23) = 24.49$, $p. .001$ (numbers in parentheses are the df. ES is determined by following equation:

$$ES = \frac{2 \text{ times } \sqrt{F}}{\sqrt{df(\text{error})}}$$

In the example, $\frac{2 \text{ times } \sqrt{24.49}}{\sqrt{23}}$

SITUATION 7.

Report presents 2-group interaction value for F , but df are not listed. You determine df as follows:

Number of subjects minus two.

Note that this is the same df as for a t-test. Logic thus says that calculating ES directly from a 2-group interaction F value should yield the same value as changing the F to a t .

You don't have to read through the following example, but it does show you how that works....

Suppose a study has ten treated children and ten no-treatment controls and investigators do a 2 by 2 repeated measures ANOVA to assess differential change over time for the two groups.

ANOVA Summary table:

Source:	df
Total	39 (Total data points minus one)
Between Ss	19 (number of Ss minus one)
Groups	1 (# of groups minus one)
Error	18 (df between Ss minus df groups)
Within Subjects	20 (df total minus df between)
Time	1 (# of trials (pre & post) minus one)
Time by Conds	1 (multiply Time by Conds dfs)
Error	18 (df within minus df time, minus df Time by Conditions)

The df for F test for a group by time interaction is thus:

df for groups (which is one)
& df for error Within Ss (which is 18).

SITUATION 8.

In a 2 group study, suppose from some dumb reason researcher reports only the F value for groups (sometimes called "treatment", or "conditions") and fails to give the F value for the group by time interaction.

We will use the F for groups. BUT RECORD YOU ARE USING THIS F IN THE MARGIN ON THE CODING SHEET.

Note that df for this F test & for group by time interaction are the same. So if df are missing, df for error term is total sample size minus two.

Unfortunately, because of the statistical properties of the repeated measures ANOVA the F test for groups is actually an underestimate of the true effect for treatment. (Trust me on this: I can give you the reference in Psychological Bulletin to read if you wish...)

SITUATION 9

Now we are talking about a design with more than 2 groups. More than one treatment and/or more than one control group. In such designs, data usually analyzed via F tests and differences groups at posttreatment/follow-up are determined by various “post-hoc” tests. (e.g. Duncan multiple range, Tukey, Newman-Keuls, multiple t tests, Dunnett’s test, etc.)

Our strategy here is to “convert” these post hocs into t tests and proceed in the usual fashion. How to do this?

Often different types of information are present in the study. Several steps are involved...

(1) We need to know the size of each group. **UNLESS THERE IS SOME REASON TO SUSPECT OTHERWISE, ASSUME EQUAL SIZES FOR EACH GROUP.**

If total sample size doesn’t divide evenly into equal sized groups, look for clues throughout article and **ASSUME ODD NUMBERED SUBJECTS ARE IN TREATMENT GROUPS, NOT CONTROL CONDITIONS.**

For example, suppose there are 51 Ss randomly divided into 3 treatment groups and one control condition. 4 into 51 equals 12.75. Obviously, groups are unequal. Allot Ss to groups acc. to order of treatment of groups mentioned. Assume Group one has 13, Group two has 13, group three has 13 and controls = 12. Reason we can do this is that unless Ns per group are grossly uneven post hoc tests will be reasonably accurate.

(2) Determine p level of the results of the post hoc tests. If no p level given, **ASSUME P LEVEL OF .05 (FOR ALL TESTS!).**

(3) Knowing the Ns per group and p level you can then convert results of post hocs into t tests.

EXAMPLE:

Supposed following a significant F test for groups in a design involving 3 treatment groups, researcher conducts some post hoc tests to determine which groups differ significantly from which other group (that’s the purpose of the post hocs). She or he might say “Duncan multiple range tests disclosed that groups 1 and 3 differed significantly from the controls, but group 2 did not”. (There are 12 subjects per group).

Since no p level was stated, assume one of .05.

Since there are 12 Ss per group, df for a t test would be 22. Locate value of significant t at 22 df . (It’s 2.074). Proceed according to usual steps to calculate ES. $ES = 2 \text{ times } 2.07 \text{ divided by square root of } 22 = +0.88$.

(4) Be careful to note ES for each treatment vs. control comparison!

e.g. ES for treatment one is .88; same for treatment 3, but ES for group two is (What?)

Zero. Why? Because post hoc showed results to be nonsignificant and under such limited data reporting conditions, we calculate the ES to be zero. (See situation two above).

(5) Be extra careful of post hoc comparisons which author fails to describe. For instance if in above example, he/she merely said “group one differed from the controls”,

then ASSUME GROUPS TWO AND THREE DID NOT, AND THEIR ES'S ARE ZERO.

SITUATION TEN.

In this case, the only outcome data available for the groups are expressed in frequencies or percentages. It doesn't matter how many treatment and control groups there are, we will proceed in such cases with a "PROBIT TRANSFORMATION" OR ANALYSIS.

The general case is described in Glass et.al., chapter five. A problem generally arises when extreme proportions or %'s are reported. In these instances, we must "adjust" the proportions using what is called a "Bayesian correction".

EXAMPLE: Data are as follows: A treatment group of enuretic children (n=10) and no-treatment controls (n=13). Doesn't matter if group are of unequal size. Author reports that all 10 of the treated children were "cured" (14 straight dry nights) whereas only one control spontaneously improved without treatment.

(1) first determine the number of each group giving rise to the proportion or percentage. (ten and one respectively).

(2) plug these numbers into the following equation:

(Obtained Number plus one) divided by (N per group plus two)

Experimentals = $(10 + 1)/(10 + 2) = (11/12)$

Controls = $(1 + 1)/(13 + 2) = 2/15$

Experimental "corrected" proportion thus = .916

Controls "corrected" proportion = .133

(3) convert the proportions into "z scores" Refer Table A. Refer to "area below" column and find appropriate match to .916 and .133.

Experimental = 1.355

Controls = -1.13

Be careful to retain the sign of the z score for each group.

ES is calculated via $z(\text{controls}) \text{ minus } z(\text{treatment})$

ES = $-1.13 - 1.355 = -2.485$

Now, in this case, the tricky part is remembering what the + or - z score means. In Table A the z scores we calculated, refer to the area below the normal curve, in effect the reverse meaning in relation to how usually determine ES (Mean t - Mean c / pooled SD).

In the case of z scores, then, MINUS Z SCORES ARE POSITIVE EFFECT SIZES AND VICE VERSA.

BE SURE TO RECORD EFFECT SIZE AS POSITIVE ON THE CODING SHEETS!!!!!!!!!!!!!!!!!!!!!!

If you get confused, simply refer back to the article. In our example, it's obvious more treated than control children were cured, so ES must be positive.

(Note, as a supplementary aide, there's a copy of Table 5.5 from Glass et.al. somewhere in this hand-out). This table provides a crude estimate of ES given the proportions or percentages for treated and control groups. You find the estimated ES by intersecting the respective proportions for E and C groups. In our example, these values are .916 and

.133 (you can round off and note that Table 5.5 yields an ES of +2.32 for Pe of .90 and Pc of .15).

Note. USE TABLE 5.5 ONLY AS A GUIDE TO SEE IF YOUR CALCULATION OF ES IS IN THE BALL PARK. Table 5.5 provides too much of a crude estimate of effect sizes that are affected by extreme proportions. That's why we are using the "Bayesian correction". For example, if we didn't "correct" the obtained proportions for extremes, note what happens. Actual obtained proportion for E group is 1.00 (all cured!), but Table 5.5 doesn't have a value for a proportion of 1.00. In our example, with obtained proportions so high, crude estimate of ES would be at least one-fifth too high. (estimated of ES of around 2.97 or so; quite a difference from our "corrected" ES of 2.48).

SITUATION ELEVEN:

In this design, there are more than 2 groups, and F was computed, and the F summary table and the Means per each group are provided somewhere in the article.

$$ES = \frac{M_t - M_c}{\sqrt{\frac{Msb}{F}}}, \text{ where}$$

M_t = mean of treatment group

M_c = mean of control group

Msb is the mean square value for "between groups" or "between conditions" in the ANOVA or F table, and F is the value for "between groups" or "between conditions"

$\sqrt{}$ = square root.

SITUATION TWELVE:

In this situation, there are differences at pre-treatment between groups. If this is so, then our usual method of calculating ES on post-treatment data only could provide a very biased estimate of the true impact of treatment. Here's an example:

	PreTreatment		Post treatment	
	Mean	Pooled SD's	Mean	Pooled SD's
E group	30		20	
Controls	18		18	
		4.0		2.0

Suppose the measure is one of hyperactivity, meaning higher scores are "bad" (i.e. reflect more hyperactivity).

If we were to calculate our usual ES on posttreatment data, it would be $M(e)$ minus $M(c)$ divided by pooled SD = 1.00. However, since higher scores are bad, the ES is minus (-1.00).

But look at the data: the treatment did greatly improve the scores of the E group although they still ended up “worse” than the controls.

In his article with Wortman, our own Fred Bryant developed a way to “correct” for pretreatment nonequivalence. It simply involves calculating TWO ES’s one for posttreatment, one for pretreatment and subtracting the latter from the former.

In our above example,

ES (posttreatment) minus ES (pretreatment) = corrected ES. or.....

20-18/2.0 minus 30-18/4.0 = -1.00 minus (-3.0), or....

-1.00 + 3.0 = +2.0 (watch the signs of the ES’s!!)

All of a sudden, the ES is positive! Yet, this more accurately reflects the large change in E group resulting from treatment.

We will affectionately refer to situation twelve as the “Fred Adjustment” in our future discussions.

Note. When should groups be considered “nonequivalent” at pre?

Certainly, when researcher indicates statistically significant differences based on some analysis. However, researcher won’t always explicitly say this. A good clue is use of ANCOVA to “adjust” for pretreatment differences (though, sometimes ANCOVA’s used to control for IQ, age, etc., or something other than the data on the outcome measure).

Some general tips: we want to use situation #12 especially in those situations like our example above: When the E group does improve, but they are still “worse” than controls at end of treatment and our ES would normally be minus in sign.

1. Nonequivalence more likely when assignment to conditions is not random. (Even with random assignment still possible for nonequivalence to occur.)

2. Sometimes, with many outcome measures, groups won’t be equivalent on all measures (we only “adjust” using #12 when needed; adjusting on only 1 of 5 measures is okay).

3. In general, three major factors would affect nonequivalence: magnitude of the mean differences between groups, N of each group, and the standard deviations per group. Unfortunately, these factors interact. As Ns per group increase, more likely for groups to differ. However, as SD’s per group increase, less likely for groups to differ.

If you are in doubt, calculate the posttreatment ES in usual fashion and alert me to the situation.

APPENDIX C

CODING SCHEME FOR META-ANALYSIS OF ACCIDENT PREVENTION

APPENDIX C

Coding scheme for meta-analysis of accident prevention

I. Study Characteristics

1. Study ID# (1-99)
2. Year of publication
3. Source (1-5)
 - 1= journal article
 - 2= dissertation
 - 3= other published document
 - 4= unpublished document
 - 5= other
4. Type of setting (1-2)
 - 1= industrial
 - 2= children
5. Length of the intervention (in weeks)
6. Total number of treatment groups
7. Total number of comparisons
8. Total number of outcome measures
9. Follow-up data available (Yes/No)

II. Design Characteristics

10. Type of design (1-4)
 - 1= single case
 - 2= multiple baseline
 - 3= between groups
 - 4= other
11. Total sample size
12. Threats to internal validity (1-7)
 - 1= history
 - 2= instrumentation
 - 3= selection
 - 4= sel/inst
 - 5= sel/hist
 - 6= other
 - 7= none

13. Target behaviors (1-4)

- 1= safe
- 2= unsafe
- 3= other
- 4= none

14. Target conditions (1-4)

- 1= safe
- 2= unsafe
- 3= other
- 4= none

15. Reliability

16. Reliability based on (1-2)

- 1= interjudge agreement
- 2= other

III. Comparison information

17. Comparison number

18. Type of comparison (1-4)

- 1= baseline vs. treatment
- 2= between treatments
- 3= treatment vs. control
- 4= combination

19. Type of control group (1-4)

- 1= none
- 2= no treatment
- 3= waiting list
- 4= other

20. Sample size of treatment group for this comparison.

21. Sample size of control group for this comparison.

IV. Intervention characteristics

22. Behavioral techniques used (1-8)

- | | |
|---------------|-----------------|
| 1= feedback | 5= goal setting |
| 2= modeling | 6= praise |
| 3= incentives | 7= package |
| 4= training | 8= none |

23. Combination of behavioral techniques (1-5)

- 1= training/feedback
- 2= training/goal setting
- 3= feedback/goal setting
- 4= training/goal setting/feedback
- 5= other

24. Identity or treatment deliverer (1-6)

1= supervisor

2= teacher

3= parents

4= experimenter

5= other

6= unknown

25. Number of observations in baseline

26. Number of observations in treatment.

V. Outcome measures

27. Type of outcome measure (1-8)

1= safe behaviors

2= unsafe behaviors

3= safe conditions

4= unsafe conditions

5= injury rates

6= lost time injuries or accidents

7= combination

8= other

28. Specific outcome measure

VI. Effect size information.

29. Effect size by PND

30. Effect size at post-treatment

31. Positive/negative effect size at post-treatment

32. Length of follow-up in weeks

33. Effect size at follow-up

34. Positive/negative effect size at follow-up

35. How effect size was calculated (1-7)

1= means/standard deviation

2= ANOVA summary table

3= t score

4= estimate from p

5= F value

6= correlations

7= mixed

REFERENCES

- Barnett, R., & Brickman, D. (1986). Safety hierarchy. Journal of Safety Research, 17, 49-55.
- Bass, J., Christoffel, K., Widome, M., Boyle, W., Scheidt, P., Stanwick, R., & Roberts, K. (1993). Childhood injury prevention counseling in primary care settings: A critical review of the literature. Pediatrics, 92, 544-550.
- Center, B.A., Skiba, R.J., & Casey, A. (1985-1986). A methodology for the quantitative synthesis of intra-subject design research. The Journal of Special Education, 19, 387-400.
- Chhokar, J., & Wallin, J. (1984). Improving safety through applied behavior analysis. Journal of Safety Research, 15, 141-151.
- Chhokar, J., & Wallin, J. (1984). A field study of the effect of feedback frequency on performance. Journal of Applied Psychology, 69, 524-530.
- Cohen, H.H., & Jensen, R.C. (1984). Measuring the effectiveness of an industrial lift truck safety training program. Journal of Safety Research, 15, 125-135.
- Cooper, H. (1989). Integrating research: A guide for literature reviews (2nd ed.). Newsbury Park: Sage Publications.
- Dershewitz, R.A., & Williamson, J.W. (1977). Preventions of childhood household

injuries: A controlled clinical trial. American Journal of Public Health, 67, 1148-1153.

Elashoff, J.D., & Thoreson, C.E. (1978). Choosing a statistical method for analysis of an intensive experiment. In T.R. Kratochwill (Ed.), Single subject research:

Strategies

for evaluating change (pp.287-312). New York: Academic Press.

Ellis, L. (1975). A review of research on efforts to promote occupational safety. Journal of Safety Research, 7, 180-189.

Fellner, D.J., & Sulzer-Azaroff, B. (1986). Occupational safety: Assessing the impact of adding assigned or participative goal-setting. Journal of Organizational Behavior Management, 7, 3-24.

Fellner, D.J., & Sulzer-Azaroff, B. (1984). Increasing industrial safety practices through posted feedback. Journal of Safety Research, 15, 7-21.

Fox, D.K., Hopkins, B.L., & Anger, W.K. (1987). The long-term effects of a token economy on safety performance in open-pit mining. Journal of Applied Behavior Analysis, 3, 215-224.

Glass, G.V. (1976). Primary, secondary and meta-analysis of research. Educational Researcher, 5, 3-8.

Gottman, J.M., & Glass, G.V. (1978). Analysis of interrupted time-series experiments. In T.R. Kratochwill (Ed.), Single subject research: Strategies for evaluating change (pp. 287-312). New York: Academic Press.

- Haddon, W. (1980). Advances in the epidemiology of injuries as a basis for public policy. Public Health Reports, 95, 411-421.
- Hale, A.R., & Hale, M. (1970). Accidents in perspective. Occupational Psychology, 44, 115-121.
- Haney, J., & Jones, R. (1982). Programming maintenance as a major component of a community-centered preventive effort: Escape from fire. Behavior Therapy, 13, 47-62.
- Haynes, R., Pine, R., & Fitch, H. (1982). Reducing accident rates with organizational behavior modification. Academy of Management Journal, 2, 407-416.
- Hedges, L.V., & Olkin, I. (1985). Statistical methods for meta-analysis. San Diego, CA: Academic Press.
- Hedges, L.V. (1981). Distribution theory for Glass's estimator of effect size and related estimators. Journal of Educational Statistics, 6, 107-128.
- Hillman, H., Jones, R., & Farmer, L. (1986). The acquisition and maintenance of fire emergency skills: Effects of rationale and behavioral practice. Journal of Pediatric Psychology, 11, 247-258.
- Huitema, B.E. (1985). Autocorrelation in applied behavior analysis: A myth. Behavioral Assessment, 7, 107-118.
- Jones, R.R., Vaught, R.S., & Weinrot, M. (1977). Time-series analysis in operant research. Journal of Applied Behavior Analysis, 10, 151-166.

- Jones, R., Kazdin, A., & Haney, J. (1981). Social validation and training of emergency safety skills for potential injury prevention and life saving. Journal of Applied Behavior Analysis, 14, 249-260.
- Jones, R., Kazdin, A., & Haney, j. (1981). A follow-up to training emergency skills. Behavior Therapy, 12, 716-722.
- Karan, B., & Kopelman, R. (1986). The effects of objective feedback on vehicular and industrial accidents: A field experiment using outcome feedback. Journal of Organizational Behavior Management, 8, 45-56.
- Kerr, W. (1957). Complementary theories of safety psychology. The Journal of Social Psychology, 45, 3-9.
- Komaki, J., Barwick, K., & Scott, L. (1978). A behavioral approach to occupational safety: Pinpointing safe performance in a food manufacturing plant. Journal of Applied Psychology, 4, 434-445.
- Komaki, J., Heinzman, A., & Lawson, L. (1980). Effect of training and feedback: Component analysis of a behavioral safety program. Journal of Applied Psychology, 65, 261-270.
- Langley, J., Silva, P., & Williams, S. (1987). Psychosocial factors in unintentional childhood injuries: Results from a longitudinal study. Journal of Safety Research, 18, 73-89.

- Light, R.J., & Pillemer, D.B. (1984). Summing up: The science of reviewing research. Cambridge, MA: Harvard University Press.
- Lipsey, M.W., & Wilson, D.B. (1993). The efficacy of psychological, educational, and behavioral treatment: Confirmation from meta-analysis. American Psychologist, 48, 1181-1209.
- Matheny, A. (1987). Psychological characteristics of childhood accidents. Journal of Social Issues, 43, 45-60.
- McAfee, B., & Winn, A. (1989). The use of incentives/feedback to enhance work place safety: A critique of the literature. Journal of Safety Research, 20, 7-19.
- Mori, L., & Peterson, L. (1986). Training preschoolers in home safety skills to prevent inadvertent injury. Journal of Clinical Child Psychology, 15, 106-114.
- National Safety Council. (1980). Accident Facts: 1979 edition. Chicago, IL: Author.
- National Safety Council (1986). Accident Facts: 1985 edition. Chicago, IL: Author.
- Peterson, L. (1984). Teaching home safety and survival skills to latch-key children: A comparison of two manuals and methods. Journal of Applied Behavior Analysis, 17, 279-293.
- Peterson, L., & Thiele, C. (1988). Home safety at school. Child & Family Behavior Therapy, 10, 1-8.
- Peterson, L., Farmer, J., & Mori, L. (1987). Process analysis of injury situations: A complement to epidemiological methods. Journal of Social Issues, 43, 33-44.

- Reber, R., & Wallin, J. (1984). The effects of training, goal setting, and knowledge of results on safe behavior: A component analysis. Academy of Management Journal, 27, 544-560.
- Rivara, F., & Mueller, B. (1987). The epidemiology and causes of childhood injuries. Journal of Social Issues, 43, 13-31.
- Roberts, M., Fanurik, D., & Layfield, D. (1987). Behavioral approaches to prevention of childhood injuries. Journal of Social Issues, 43, 105-118.
- Rothengatter, T. (1984). A behavioral approach to improving traffic behavior of young children. Ergonomics, 27, 147-160.
- Rhoton, W.W. (1980). A procedure to improve compliance with coal mine safety regulations. Journal of Organizational Behavior Management, 2, 243-249.
- Scruggs, T.E., Mastropieri, M., Cook, S.B., & Escobar, C. (1986). Early intervention for children with conduct disorders: A quantitative synthesis of single-subject research. Behavioral Disorders, 11, 260-271.
- Scruggs, T.E., Mastropieri, M.A., & Casto, G. (1987). The quantitative synthesis of single-subject research: Methodology and validation. Remedial and Special Education, 8, 24-33.
- Scruggs, T.E., Mastropieri, M.A., Forness, S.R., & Kavale, K.A. (1988). Early language intervention: A quantitative synthesis of single-subject research. The Journal of Special Education, 22, 259-283.

- Shine, L.C., & Bower, S.M. (1971). A one-way analysis of variance for single-subject designs. Educational and Psychological Measurement, 31, 105-113.
- Stevenson, M. (1991). Analytical approach to the investigation of childhood pedestrian injuries: A review of the literature. Journal of Safety Research, 22, 123-132.
- Sulzer-Azaroff, B. (1978). Behavioral ecology and accident prevention. Journal of Organizational Behavior Management, 2, 11-44.
- Sulzer-Azaroff, B., Loafman, B., Merante, R.J., & Hlavacek, A.C. (1990).
- Sulzer-Azaroff, B., & Fellner, D. (1984). Searching for performance targets in the behavioral analysis of occupational health and safety: An assessment strategy. Journal of Organizational Behavior Management, 6, 53-65.
- Sulzer-Azaroff, B., & De Santamaria, C. (1980). Industrial safety hazard reduction through performance feedback. Journal of Applied Behavior Analysis, 13, 287-295.
- Tawney, J.W., & Gast, D.L. (1984). Single subject research in special education. Columbus, OH: Merrill.
- Tertinger, D.A., Greene, B.F., & Lutzker, J.R. (1984). Home safety: Development and validation of one component of an ecobehavioral treatment program for abused and neglected children. Journal of Applied Behavior Analysis, 17, 159-174.
- US Congress, Office of Technology Assessment. (1988). Healthy children: Investing in the future. Washington, DC: US Government Printing Office.

- Verhaegen, P., Vanhalst, B., Derijcke, H., & Van Hoecke, M. (1976). The value of some psychological theories on industrial accidents. Journal of Occupational Accidents, 1, 39-45.
- Wilson, M., & Baker, S. (1987). Structural approach to injury control. Journal of Social Issues, 43, 73-86.
- Wolf, F.M. (1986). Meta-analysis: Quantitative methods for research synthesis. Beverly Hills, CA: Sage.
- Yeaton, W., & Bailey, J. (1983). Utilization analysis of a pedestrian safety program. Journal of Applied Behavior Analysis, 16, 203-216.
- Yeaton, W., & Bailey, J. (1978). Teaching pedestrian safety skills to young children: An analysis and one-year follow-up. Journal of Applied Behavior Analysis, 11, 315-329.
- Zohar, D. (1980). Promoting the use of personal protective equipment by behavior modification techniques. Journal of Safety Research, 12, 78-85.
- Zohar, D., Cohen, A., & Azar, N. (1980). Promoting increased use of ear protectors in noise through information feedback. Human Factors, 22, 69-79.

VITA

The author, Julia Trillos, the daughter of Pedro Trillos and Isabel (Gamboa) Trillos, was born May 20, 1965 in Medellín, Colombia.

Ms. Trillos completed her secondary education in 1982 at Nuestra Señora del Buen Consejo, Barranquilla, Colombia.

In January, 1984, Ms. Trillos transferred from the Computer Engineering Program to the Psychology Program at Universidad del Norte, Barranquilla, Colombia, where she received the degree of Bachelor of Arts in Psychology with Honors in March 1989.

In May, 1991, Ms. Trillos received a Fulbright/LASPAU scholarship to attend Graduate School. With this scholarship, Ms. Trillos enrolled in the Applied Social Psychology Program at Loyola University of Chicago, where she completed her Master of Arts degree in January, 1997. Ms. Trillos is completing her Ph.D. in Applied Social Psychology at the same school.

THESIS APPROVAL SHEET

The thesis submitted by Julia Trillos has been read and approved by the following committee:

Joseph A. Durlak, Ph.D., Director
Professor , Psychology
Loyola University of Chicago

Emil J. Posavac, Ph.D.
Professor, Psychology
Loyola University of Chicago

Fred B. Bryant, Ph.D.
Professor, Psychology
Loyola University of Chicago

The final copies have been examined by the director of the thesis and the signature which appears below verifies the fact that any necessary changes have been incorporated and that the thesis is now given final approval by the committee with reference to content and form.

The thesis is, therefore, accepted in partial fulfillment of the requirements for the degree of Master of Arts in Psychology.

November 26, 1996
Date

Joseph A Durlak
Director's signature