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RESEARCH STRATEGIES AND METHODOLOGIC STANDARDS IN STUDIES OF RISK FACTORS FOR CHILD ABUSE

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Abstract—A major focus of studies of child abuse has been the identification of children who are at high risk for abuse. Despite this emphasis, little has been written about the research methodology in such studies. This paper reviews the three major research strategies to investigate risk factors: (1) randomized controlled trials, (2) prospective or observational cohort studies, and (3) retrospective or case-control studies. In addition, eight methodological standards are presented that should help to minimize bias in studies of risk factors for child abuse. These standards are: (1) clear description of abuse, (2) choice of a specific control group, (3) equal demographic and clinical susceptibility, (4) clear definition of the risk factor or protective factor, (5) unbiased ascertainment of the risk factor, (6) clear temporal sequence between risk factor and abuse, (7) equal detection of child abuse, and (8) equal review of abused and nonabused subjects. Most studies of risk factors for child abuse have used either a case-control or, less frequently, an observational cohort design, both of which are nonexperimental research strategies. In such studies, the use of comparable control groups (standards 2 and 3) and equal detection of abuse in exposed and nonexposed children (standard 7) are of major importance in minimizing bias.

Key Words—Child abuse; Risk factors; Methodologic standards; Research methodology.

Résumé—Depuis 20 ans, on s'efforce de définir avec toujours plus de précision les facteurs de risque en maltraitance d'enfants. On n'a cependant pas été assez rigoureux dans la méthodologie de ces études. L'auteur décrit les stratégies généralement utilisées dans ce genre de recherche jusqu'à présent et suggère des améliorations méthodologiques afin d'obtenir une définition plus fiable des facteurs de risque. Les modèles de stratégie utilisés ont été les suivants jusqu'à présent: (1) On a essayé d'introduire au hasard un facteur protecteur ou supposé tel (par exemple le "rooming in") dans certaines familles et pas dans d'autres (cela suppose la participation volontaire des familles et aussi que la maltraitance n'est pas encore survenue). On attend et on observe si le facteur censément protecteur l'est vraiment, c'est-à-dire s'il diminue le risque de maltraitance; (2) On a fait des études prospectives de groupements bien définis de familles que l'on suppose être à risque, comparées à des familles que l'on pense ne pas être à risque; (3) Le plus souvent on s'est livré à des études rétrospectives—on regarde si dans le cas d'enfants maltraités (probants) on retrouve plus souvent un ou plusieurs facteurs de risque que chez les enfants non-maltraités (témoins). Les études sur les facteurs de risque devraient absolument être construites en suivant une méthodologie rigoureuse afin d'éviter que les résultats ne soient biaisés, ce qui n'a pas été le cas jusqu'à présent. Les sources de distorsion et de biais dans les études sur la causalité des phénomènes (en général) ont fait l'objet de recherches récentes qui ont conduit à des recommandations destinées à diminuer l'importance de ce problème. L'auteur reprend et adapte de Horwitz et Feinstein 8 principes qui sont censés diminuer la distorsion dans de telles études. Ces principes sont, en ce qui concerne la maltraitance d'enfants, les suivants: (1) description précise de ce qu'on appelle maltraitance d'enfant; (2) choix d'un groupe témoin spécifique, c'est-à-dire comparable au groupe probant; (3) susceptibilité clinique et démographique égale (égalisation des groupes comparés, si nécessaire, par appariement, stratification, ou analyse multivariée); (4) définition précise du facteur de risque ou du facteur de protection; (5) évaluation impartiale du facteur de risque (dans les études rétrospectives); (6) respect d'une séquence chronologique bien définie entre le facteur de risque et la maltraitance; (7) détection uniforme de la maltraitance ou de son absence; (8) évaluation équivalente des sujets victimes de sévices et des sujets témoins (c'est-à-dire n'y a-t-il pas eu de cas de sévices parmi les témoins?). La plupart des études de facteurs de risque ont été fondées sur l'étude de cohortes d'enfants qui avaient subi des sévices et de cohortes d'enfants qui n'en avaient pas subi et qu'on prenait pour témoins (modèle 3). Dans ce genre

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d'étude les observateurs ont recherché si le facteur de risque qui les intéressait était présent ou pas et lorsque ce facteur de risque était plus souvent trouvé chez les enfants probants que chez les enfants témoins, alors le facteur de risque a été considéré comme étant en rapport avec la survenue de la maltraitance. Ce genre d'étude, de même que le modèle No. 2, n'utilise pas une stratégie expérimentale. Ces modèles pourraient être améliorés par l'utilisation de témoins rigoureusement comparables aux probants en respectant les principes 2 et 3 cités ci-dessus (c'est-à-dire une détection de la maltraitance fondée sur une base identique dans les deux groupes), était aussi respecté.

DURING THE LAST TWO DECADES, investigators and clinicians concerned with the problem of child abuse have tried to detect children who are at high risk as potential victims, so that suitable interventions could be undertaken. The risk factors emphasized in the research have included attributes of the social environment, family, parents, child, and birth experience, as well as attitudes and behaviors of the parents toward the child. Despite major efforts to refine these factors, little attention has been given to the methodology of the research. If rigorous scientific principles are not used in the research design, false conclusions may be drawn about the importance of the risk factors.

The purpose of this review is to describe the major research strategies used to determine risk factors for child abuse and to consider the methodologic principles needed to improve the scientific quality of the work.

RESEARCH STRATEGIES

Three major research strategies have been used to investigate factors contributing to abuse: (1) randomized controlled trials, (2) prospective or observational cohort studies, and (3) retrospective or case-control studies (Figure 1). The discussion that follows contains a brief outline of each research strategy and an example from the literature on child abuse.

1. Randomized Controlled Trial

A randomized controlled trial is a prospective research strategy that uses an experimental paradigm. When abuse is the outcome event in this type of study, an investigator cannot assign risk factors—such as prematurity, social isolation, or abnormal approaches to child-rearing—to one group of families and not to the other. Instead, an investigator assigns a protective factor,

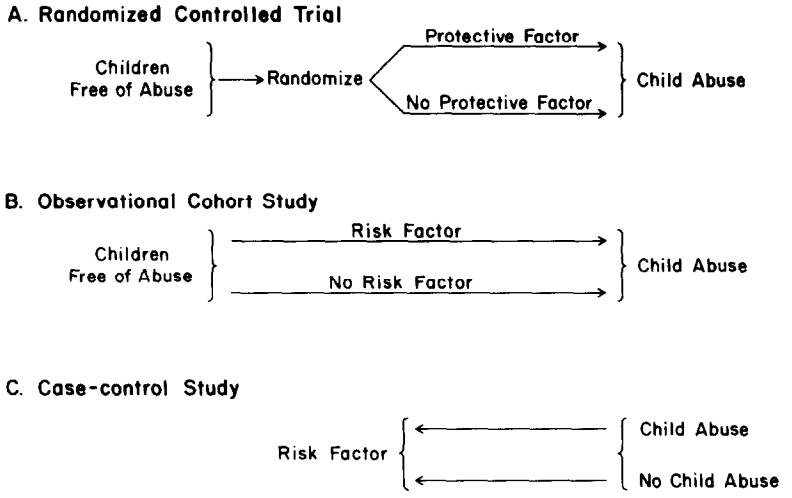

Figure 1. Three Research Strategies to Investigate Factors Contributing to Child Abuse

such as rooming-in or the assistance of a parent-aide. The absence of this protective factor can be considered a risk factor.

There are three major components of a randomized controlled trial. First, patients enrolled in the study are free of the outcome event, child abuse. Second, the investigator assigns the protective factor by a process of randomization to subjects who enter the trial. For patients enrolled, the randomization produces two groups: those exposed to the protective factor and those not exposed. Third, the two groups are observed over time to determine if child abuse occurs. If the incidence of abuse is significantly lower in the presence of the suspected protective factor, then this factor is regarded as protective against the subsequent occurrence of abuse.

O'Connor et al. [1] used a randomized controlled trial to determine whether rooming-in was a protective factor that prevented inadequate parenting during the first two years of the child's life. Consecutive mother-infant pairs who met the eligibility criteria of the study were assigned on a random basis either to post-partum contact that was limited to periods of feeding or to rooming-in. To determine whether any of the subjects were abused during the first two years of life, the investigators reviewed hospital records and monitored reports of maltreatment received by the state child abuse agency. In this study, rooming-in decreased the incidence of abuse.

2. *Observational Cohort Study*

The second strategy uses an observational cohort approach. The major difference between this cohort approach and the controlled clinical trial is that the factor under study is not randomly assigned by the investigator. Instead, the investigator conducts a survey by assembling two groups of children who are free of abuse: in one group, called the exposed group, the risk factor is present and in the other group, called the nonexposed group, the risk factor is absent. Both groups are then observed over time to determine the occurence of abuse. If the incidence of abuse is significantly higher in the presence of the risk factor, then this factor is suspected as causally related to child abuse.

An example of such a study was performed by Altemeier et al. [2] who conducted a structured prenatal interview on a cohort of 1,400 pregnant women. The interview data were used to categorize the women into high and low risk groups with respect to the likelihood of child abuse. For instance, a mother was considered high risk if her responses placed her above the 99th percentile in one of eight categories of data, such as her perceptions of the nurturing she received during childhood. Children born to the mothers in the high and low risk groups were observed during the first two years of life to determine whether abuse occurred. Five times as many children born to mothers in the high risk group were reported to state authorities compared to children born to low risk mothers.

Despite a direct approach, cohort studies of risk factors for child abuse are infrequently performed because of several reasons: (1) a large sample size is required because child abuse is an uncommon outcome event; (2) it is difficult to observe this large group of people over time; (3) uniform examinations are needed to obtain the outcome data for each subject; (4) the projects may be quite expensive; and (5) there may be ethical difficulties if high risk groups are identified and observed without intervention.

3. *Case-Control Study*

The third approach, the case-control study, is the one most commonly used to investigate risk factors for child abuse. In the case-control study, the investigative direction is backwards. Instead of observing children who are free of abuse to determine the occurrence of the outcome event, the direction of the study is reversed. Two groups of children are assembled according to the presence or absence of the outcome event, child abuse: abused children are called "cases" and nonabused children are called "control subjects." Children in each group are then examined to

determine if the risk factor under study has occurred previously. If the prevalence of the risk factor under study is significantly higher in cases than in control subjects, then the factor is usually regarded as causally related to the occurrence of abuse.

An example is an investigation performed by Cater and Easton [3]. Cases were abused children who were identified from hospital records. To provide a comparison group of nonabused children, the control subjects were siblings. The risk factor under study was early separation of parent and infant due to admission either to a special care nursery or to a hospital during the first year of life. By reviewing hospital records of the child's first year of life, the authors determined that case children were exposed more frequently to the risk factor than were control children.

Case-control studies of abuse are performed frequently because investigators usually find it convenient and feasible to identify and study a group of abused children and control subjects, and because this strategy is relatively inexpensive compared to a cohort approach. As noted below, however, rigorous scientific standards are needed to avoid the important methodologic pitfalls that can arise in the design of such studies.

STANDARDS TO MINIMIZE BIAS

The major goal of studies of risk factors is to determine whether the attribute under study is causally related to the outcome event. To make valid causal inferences about risk factors, such studies should be designed in a methodologically rigorous manner so as to minimize bias or distortion of the results. Several recent investigators have outlined major sources of bias in studies of causality and have recommended ways of reducing the problem [4–6]. This section contains an account of eight major research standards which are in part adapted from the work of Horwitz and Feinstein [5]. The eight standards, outlined in Table 1, should help to minimize bias in studies of child abuse.

1. Clear Description of Abuse

The first methodologic standard requires a clear description of what is meant by child abuse so that the results of the study are generalizable to similar patient groups. This standard is equally important in all three investigative strategies. Since investigators use different definitions of child abuse and different types of abuse may be associated with different risk factors, it is important to know how the investigator defines abuse and the type of abused children included in the target group. For instance, in a case-control study of family characteristics, Baldwin and Oliver [7] included as cases only children who were severely abused and whose injuries met one of six criteria: (1) death, (2) skull or facial bone fractures, (3) bleeding into or around the brain, (4) two or more instances of mutilation requiring medical attention, (5) three or more instances of fracture and/or severe bruising requiring medical attention, or (6) multiple fractures and/or severe internal injuries. In contrast, Egeland and Brunnquell [8] in a cohort study of 275 primiparous, poor women and their children, used a much broader definition of maltreatment; based on data collected from visits to the mothers' homes, the investigators defined inadequate care to include cases of abuse, abuse and neglect, failure-to-thrive, and severe neglect. Only five of the 26 inadequately cared for infants actually were reported to Protective Services.

2. Choice of a Specific Control Group

The second standard is concerned with the way the investigator chooses a comparison group. In a randomized controlled trial, patients randomized to the nonintervention group are the control subjects. In contrast, in a cohort study the investigator must choose a comparison group that is free of the risk factor. Two kinds of comparison groups can be chosen: *specific* or *non-specific*. A specific comparison group consists of children assembled as part of the same investigation,

Table 1. Summary of Methodologic Standards and Research Strategies

Methodologic Standard	Research Strategy		
	A Randomized Controlled Trial	B Observational Cohort Study	C Case-Control Study
1. Clear description of abuse	Necessary to generalize results of study	Same as Column A	Same as Column A
2. Choice of a specific control group	Achieved through process of randomization	Achieved if group not exposed to risk factor observed as part of same study	Achieved if group of nonabused subjects observed as part of same study
3. Equal demographic and clinical susceptibility	Checked by comparing groups after randomization	Achieved by using special procedures such as matching, stratification, or multivariate analysis	Same as Column B
4. Clear definition of the risk factor or protective factor	Necessary to randomize subjects into two groups	Necessary to divide subjects into exposed and nonexposed groups	Necessary to investigate presence or absence of risk factor
5. Unbiased ascertainment of the risk factor	Not applicable	Not applicable	Achieved by having research assistants blind to group status and by having equal recall in both cases and controls
6. Clear temporal sequence between risk factor and abuse	Achieved by identifying exposed and nonexposed subjects who are free of abuse and who then are observed to determine if abuse occurs	Same as Column A	Achieved by checking temporal relationship of risk factor and abuse
7. Equal detection of abuse	Achieved by: (1) equal examination of subjects by investigator; (2) restricting definition of abuse; or (3) choosing subjects from the same clinical setting	Same as Column A	Same as Column A
8. Equal review of abused and nonabused subjects	Not applicable	Not applicable	Achieved by reviewing subjects to determine if cases have been abused and controls have been free of abuse

who are similar to the exposed children in three characteristics: (1) the geographic location of the sample, (2) the secular time period of the study, and (3) the way the comparison group is observed over time. A non-specific comparison group, on the other hand, consists of children who are assembled as part of a different investigation or for a different purpose. A common source of non-specific comparison groups is local or national populations. Because such comparison groups are dissimilar to the exposed children in one (or more) of the three cited characteristics, the results may be biased. For instance, in a study of prematurity as a risk factor, a difference in the geographic location of the exposed and comparative children may be accompanied by a major difference in socioeconomic status of the two groups. This difference may in turn affect the rates of abuse in the two groups and lead to biased results. Similarly, if the two groups are studied in different decades or using different methods of observation, differences in the definition of abuse or differences in the clinicians' recognition of the problem may affect the rates of abuse.

In case-control studies, the investigator also must choose between a specific and non-specific comparison group; but in these studies, the case group consists of abused children and the comparison group of nonabused children. The presence of the risk factor is then determined in each group. As in observational cohort studies, non-specific comparison groups often are chosen from regional populations. Thus, if prematurity is suspected as the risk factor, the state population might serve as a non-specific comparison group, and the percentage of premature births in the state would then be compared with the rate of prematurity in the abused group.

Examples of specific and non-specific comparison groups. In both cohort and case-control studies, since non-specific comparison groups may lead to bias, this standard requires the use of a specific comparison group. A few examples can serve to illustrate this point more clearly.

In the cohort study previously mentioned, Altemeier et al. [2] prospectively followed 1,400 families from the time of the prenatal interview to 18 months after delivery. The risk factor was a high risk response to questions on a prenatal interview. Women with high risk responses were compared to a specific control group of low risk responders from the same population. In contrast, the cohort study by Hunter et al. [9] used a non-specific comparison group. In this study, the 3.9% incidence of maltreatment in 225 premature and sick newborns discharged from a regional newborn intensive care unit was compared to a statewide incidence of 0.5% for reported cases of abuse and neglect. Since the statewide population may differ substantially from families of children admitted to a newborn intensive care unit in demographic features related to the development of abuse, this non-specific comparative rate is difficult to interpret.

In case-control studies examining the relationship of prematurity to child abuse, several studies have used either specific or non-specific comparison groups [10]. For instance, Cater and Easton [3] chose a specific comparison group; the percentage of premature infants was compared in abused children and in their nonabused siblings who served as controls. This study showed no relationship of prematurity to abuse. In contrast, Klein and Stern [11] found that the rate of prematurity was much higher in abused children admitted to the Montreal Children's Hospital between 1963 and 1969 than the rate noted in the city of Montreal in 1967. However, the comparison with this non-specific, regional population may have led to an overestimation of prematurity as a risk factor.

3. Equal Demographic and Clinical Susceptibility

The use of a specific comparison group does not necessarily ensure that the cases and controls are comparable. Therefore, the third standard requires that where there is a specific comparison group, the investigator should use special procedures to adjust for differences in demographic or clinical susceptibility factors in the two groups. If adjustments are not made, then the findings may be due to important differences in the characteristics of the two groups rather than true

differences in the rates of abuse. Demographic variables might include socioeconomic status or ethnicity; clinical variables might include parity, maternal alcoholism, or whether or not the pregnancy was wanted.

In a randomized controlled trial, no special procedure is necessary since the randomization process itself is intended to equalize the susceptibility factors between the cases and control subjects; checks can be made to determine whether this equalization did in fact occur. In contrast, in a cohort or case-control study, special procedures such as matching, stratification, or multivariate analysis are necessary to attempt to equalize the susceptibility factors in the two groups.

Matching often occurs during the selection process of the comparison group. In a cohort study, matching occurs when each nonexposed subject is chosen so as to be identical to an exposed subject in certain characteristics such as ethnic group or socioeconomic status. In a case-control study, each control subject is matched to a case. Matching also may occur during the analysis phase of a study. Stratification and multivariate analysis are analytic techniques that attempt to equalize differences in susceptibility factors. For instance, stratification occurs when the rates in the study and comparison patients are calculated in certain subgroups such as unmarried women or children of low socioeconomic status.

The effects of matching cases and control subjects is indicated in the following two examples of case-control studies. In the first study, which investigated the parents' experiences during the pregnancy and perinatal period, Oates et al. [12] compared cases with control families that were matched for marital status, nationality, education, employment status, and socioeconomic status of the parents and for age, sex, and health of the child. Thus, differences in the prevalence of the risk factors in the cases and control subjects could not be attributed to differences in these matching variables. In contrast, Lynch and Roberts [13] failed to make adjustments for socioeconomic status in their case-control study of perinatal risk factors. For each of 50 abused children, the control subject was the next live child born in the same hospital. The authors showed that several factors, including mother's age less than 20 at the birth of her first child, evidence of emotional disturbance, and recorded concern over the mother's ability to care for the child, were more common in the abused group than in the control group. However, seven cases but 29 control subjects were in the upper social classes, so that differences in the prevalences of the risk factors in the two groups may have been due to differences in the social classes of the cases and control subjects rather than the presence or absence of abuse.

4. Clear Definition of the Risk Factor or Protective Factor

This standard requires a precise definition of the exposure factor so that the findings can be reproduced by other investigators. A precise definition is usually not a problem in a randomized controlled trial since the investigator assigns the protective factor to one group and not to the other. Similarly, in a cohort study, the risk factor is usually clearly defined because the study patients are divided into two groups according to the presence or absence of the risk factor under study. In contrast, in a case-control study, the risk factor is not used to divide the patients into two groups; instead, the investigator determines the previous presence or absence of the risk factor in abused and nonabused children. Since the exposure to the risk factor may have occurred many months or years in the past, a precise definition of the risk factor is crucial. For instance, in studies investigating young maternal age, it is necessary both to provide an age demarcation to define young age, e.g., age less than 20 years old at last birthday, and to provide a specific event that can be related to the age. Thus, the risk factor might be age less than 20 at the time of abuse, at the birth of the abused child, or at the birth of the mother's first child.

5. Unbiased Ascertainment of the Risk Factor

This standard is necessary only for case-control studies. The ascertainment of the risk factor can be affected by bias in the collector or the provider of data. To minimize the first type of bias,

the person collecting data about the risk factor should be unaware of the case or control status of the patient and should be unaware of the hypothesis under investigation. These conditions are most important when the data about risk factors are collected by interview or by other techniques that require clinical judgment. For instance, in a case-control study comparing aspects of aggression in the Thematic Apperception Test in abusive and non-abusive mothers, Evans [14] had psychologists blindly rate the protocols.

A bias in the respondent can occur when parents in the case group have a different incentive to recall particular details of the early experience compared to parents in the comparative group. Thus, abusive parents may selectively recall the negative experiences. For instance, in the case-control study by Oates et al. [12], the authors interviewed case and control mothers about their perinatal experiences and perceptions of the infant. Significantly more case mothers described their early perceptions of their babies as poor to very poor (on a five-point ordinal scale from ideal to very poor) compared to control mothers. However, because of the retrospective nature of these ratings, it is difficult to know the degree to which the ratings were affected by the mother's present view of her child. If these data were collected prospectively during infancy, they would be less likely to be influenced by the problems of distortion or incomplete memory. One approach to minimize differences in the memories of cases and control subjects is to design a retrospective interview so that mothers are asked to recall specific details rather than broad generalizations about the infant's behavior (or the event of interest). By recalling the details, the mother might be more likely to remember accurately the past events and her perceptions of them. Although this interview technique has been used in a case-control study of failure-to-thrive, the validity of this method has not been investigated [15].

6. *Clear Temporal Sequence Between Risk Factor and Abuse*

This standard, which is necessary to establish a causal relationship between the risk factor and the target event, requires that the risk factor clearly precedes the abuse. In randomized controlled trials and observational cohort studies, the risk factor is identified in subjects who are free of abuse and who are observed over time to determine if abuse occurs. In all such studies, the risk factor precedes the development of abuse.

In case-control studies, certain risk factors such as perinatal events or abuse to a previous child clearly precede the occurrence of abuse. However, because the direction of observation is reversed in case-control studies, many factors under investigation may not precede the occurrence of abuse. In these studies, there may be an association between the factor and abuse, but not a causal relationship. For instance, Burgess and Conger observed parent-child interactions in families identified as having abused or neglected their children and in control families [16]. Compared to controls, abusing and neglecting parents demonstrated fewer verbal and physical behaviors toward their children, and these behaviors were more negative in affective tone (e.g., a disapproving comment). The authors conclude that there are differences in parent-child interactions among the types of families studied. These differences, however, do not imply a causal relationship since there is no evidence (although there may be strong clinical suspicions) that the patterns of interaction preceded the identification of abuse or neglect. To make a causal statement about negative parent-child interactions and abuse or neglect, either of two research strategies are necessary: (1) an observational cohort study that investigates negative interactions as a risk factor for the later development of abuse or neglect, or (2) a case-control study that relies on data about parent-child interactions collected before the identification of the abnormal parenting practices.

7. *Equal Detection of Child Abuse*

The seventh standard is concerned with equal detection of the outcome event, child abuse. This standard is particularly complicated and difficult to maintain.

In a randomized controlled trial or cohort study, equal detection of abuse occurs if three conditions are met: (1) equal efforts are made to obtain follow-up on all subjects in the exposed and nonexposed groups; (2) all subjects in the two groups are examined in the same way to determine if abuse has occurred; and (3) the assessment of abuse is performed in a blind manner without knowledge of the previous exposure to the risk factor. For instance, when Altemeier et al. [2] examined the incidence of nonorganic failure-to-thrive in the high and low risk groups of 1,400 women, each condition was fulfilled: (1) the sample loss was similar in both high and low risk groups so that follow-up was equal; (2) growth data were obtained on all patients followed at the participating hospital; and (3) the investigators blindly reviewed growth data on all high risk infants and in a randomized sample of low risk infants.

Problems of equal examination. Of the three conditions necessary to achieve equal detection of child abuse, the second condition, equal examination, is the most difficult to fulfill because of the complicated nature of the outcome event. Equal examination is straightforward when the assessment involves weighing a child so that a decision can be made about the presence or absence of nonorganic failure-to-thrive. Equal examination is somewhat more complicated when the investigator assesses the caretaking environment and decides to categorize a child as abused or not. In such studies, equal examination is accomplished if the same research instrument is used to evaluate the exposed and nonexposed groups. Investigators, however, have seldom directly examined and categorized the patients under investigation; instead, investigators have relied on clinicians' assessments and reports of abuse in hospital records or local abuse registries. In these cases, it is very difficult to determine if equal examination of the children has occurred because the recognition of abuse often depends on the clinician's suspicions and the intensity of medical surveillance. Both of these variables may be affected by certain potential risk factors such as prematurity or young maternal age. For instance, if infants of teenaged mothers are followed intensively in a special clinic for young mothers, small bruises may be recognized and reported to the state abuse registry, whereas similar bruises in a child of an older mother may go unrecognized because of less intensive medical surveillance.

Alternative approaches to achieve equal examination. In the absence of direct examination of the subjects by the investigator, two alternative approaches might minimize the problem of unequal examination that occurs because of the reliance on clinicians' assessments of abuse. These approaches are: (1) restricting the definition of the outcome event and (2) choosing cases and controls from the same clinical setting. If the definition of the outcome event is restricted to *severe* cases of abuse, such as injuries requiring hospitalization for medical care, differences in the thoroughness or frequency of medical care are unlikely to affect the recognition of these obvious forms of abuse. Therefore, since all (or almost all) severe cases of abuse come to the attention of the clinicians, equal examination is achieved. In the second approach, if the rates of abuse are compared in children who are followed in the same clinical setting where the frequency of visits is similar for exposed and nonexposed children, equal examination may be approximated. However, the frequency of visits is only a rough indicator of clinical surveillance.

Equal detection in case-control studies. In case-control studies, the problem of equal detection of the outcome event also is important. In these studies, the standard is concerned with the circumstances leading to the detection of child abuse and the resulting assignment of the child to the case or control group. Since clinical suspicions and the intensity of medical surveillance may affect the identification of abuse, there is a potential for bias when there is an unequal chance for abuse to be identified in exposed and nonexposed children. The three approaches to minimize this

bias in case-control studies are identical to those described in prospective studies: (1) equal examination by the investigator, (2) restricting the definition of the target event, and (3) choosing cases and control subjects from the same clinical setting.

In the first approach, equal examination of exposed and nonexposed subjects by the investigator divides the subjects into abused children who became cases and nonabused children who became controls. An example of such a study was reported by Egeland and Brunnquell [8]. Based on a standardized approach to home visits by the investigative team, mothers were categorized according to their parenting skills: 26 parents who were providing inadequate care became cases of abuse and were compared with 25 "good mothers offering high quality care" who became controls. The prevalence of the risk factor was then ascertained in each group of mothers.

A study that used the second approach and restricted the case population to severely abused children was described by Baldwin and Oliver [7]. In this investigation, a case of abuse was included if it met at least one of six criteria for severity (e.g., an injury that resulted in a skull or facial bone fracture).

The third approach, choosing cases and controls who are followed in the same clinical setting, could not be found in any case-control studies of abuse.

8. *Equal Review of Abused and Nonabused Subjects*

In case-control studies, since the subjects are categorized according to the outcome event, equal review of the subjects is necessary to determine whether case children have been abused and control children have been free of abuse. The child's status is not in question when the study population is divided into cases and control subjects according to the same diagnostic evaluation (as occurred in the study by Egeland and Brunnquell [8]). However, this approach has seldom been used in studies of risk factors for child abuse; rather cases are usually chosen from an abuse registry after the abusive event and controls from a log of births, hospital admissions, or clinic visits. Therefore, to be certain of the child's status, a special evaluative procedure of the study population, such as an interview with the child's parent or a review of the medical records, is required. For instance, in one of the earliest retrospective studies of the developmental characteristics of abused children, Elmer and Gregg reviewed the histories of 33 supposedly abused children and found that four had been accidently injured rather than intentionally abused [17]. Some investigators have made efforts to be certain that the control group is free of abuse. For instance, in a study of perinatal risk factors, Lynch and Roberts made extensive inquiries to determine whether the 50 control children had been abused, neglected, or considered at risk [13]. This was done by sending questionnaires to family doctors and checking the social service, pediatric, and emergency departments of the local hospital.

DISCUSSION

Of the three research strategies outlined, the randomized controlled trial can fulfill most easily the standards described above. Despite the advantages of this experimental approach to the study of causality, few randomized controlled trials of factors protecting against the occurrence of abuse have been performed. Perhaps, as the focus in child abuse research shifts from studies of risk factors to studies of protective factors and preventive interventions, more randomized controlled trials will be used.

Because of the difficulties in conducting randomized controlled trials in studies of abuse, the nonexperimental research strategies discussed are especially relevant. In cohort studies, three of the standards are of major importance in minimizing bias. If the comparability of the exposed and nonexposed groups cannot be ensured by using a specific control group (standard 2) and by equalizing demographic and clinical susceptibilities (standard 3), then there is a major potential

for obtaining distorted results. In addition, the problem of detection bias (standard 7) may occur if the outcome event is not measured equally in both exposed and nonexposed groups.

In case-control studies, because the direction of observation is reversed, compliance with the eight standards usually is more difficult than in prospective studies. This research strategy, however, will continue to be popular because of its relatively low costs and ease of execution. Although a substantial bias may occur because of failure to comply with any one of the standards, perhaps the most critical standards are those concerned with the comparability of the control group (standards 2 and 3). In a methodologic review of case-control studies of child abuse investigating the risk factors of prematurity or young maternal age, most of the studies did not satisfy these two standards: only 2 of 10 studies of young maternal age and 3 of 18 of prematurity met both standards [10]. Studies of prematurity that used a specific control group compared to studies that used a non-specific control group, were significantly more likely to conclude that prematurity was not a risk factor for abuse. This difference indicates the importance of choosing comparable control groups in case-control studies of abuse.

Defining those factors that contribute to complicated human behaviors such as child abuse requires careful attention to research design. Regardless of which of the three research strategies is used to investigate these factors, the principles outlined in this review should be helpful in evaluating previous studies and in designing future, methodologically rigorous studies that will provide unbiased results and new insights into the understanding of abuse.

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