

IDENTIFYING PSYCHOSOCIAL DISORDERS IN
CHILDREN: ON INTEGRATING EPIDEMIOLOGICAL
AND ANTHROPOLOGICAL UNDERSTANDINGS

The development of a more satisfactory understanding of psychosocial disorders in children depends upon the successful combination of medical anthropological and epidemiological information. In this paper we describe one way this can be done. Our approach is to focus first on the level of the logic of inquiry, and only secondarily on the use of specific methods.

Questions of method become meaningful only after epidemiological and anthropological understandings have been used together to conceptualize a research project. Our approach therefore contrasts with some discussions in anthropology and in epidemiology which have been conducted as though problems of method can be considered alone. Some of these discussions have suggested that the methods of each discipline are complementary. Our view is that it is a serious mistake to focus discussions of the integration of anthropology and epidemiology primarily on the level of the methods or techniques of research. If the research question is set without the benefit of a conceptual integration of epidemiological and anthropological understandings, then a research program is unlikely to improve our understanding, no matter how sophisticated a combination of methods and techniques is used.

The success of researchers in integrating medical anthropology and epidemiology should not be evaluated by looking for the use of specific methods in particular ways. At times the conceptual integration may be successful yet the research question can be appropriately answered using methods from only one of the disciplines. In this paper we explore how better conceptual integration of these approaches, regardless of methods, can help to advance our understanding of psychosocial disorders in children. We seek to illustrate how taking up the concerns and asking the questions of one discipline can usefully inform work done using the methods of the other discipline.

We view this conceptual integration as important because often the focus on methods leads researchers to accept overly simple accounts of the phenomena they seek to understand. Too often the inadequacy of these accounts is discovered only after they have had harmful consequences (Rubinstein 1984; Simon 1983; Hall 1982). The principle reason for the failure of these understandings is that they have been developed using data from an artificially limited range of experience. Because of this observations are channeled toward particular normatively appropriate data, information anomalous to the model is discounted (Ward and Werner 1984; Argyris 1980; Rubinstein et al. 1984).

One way to avoid this is to insist that research keep theoretical models as

open as possible to new information. To do this we must recognize that researchers interested in mental health and illness have recourse to a number of different "levels" of structure in their explanations of the behaviors they observe (e.g., genetic, cognitive, psychological, social structural, experiential, and the like). Frequently a single level is associated with the theoretical work of a single discipline. Yet each of these levels is comprised of organized subsystems (Whitehead 1960). Focusing on one level to the exclusion of others always leads to the confounding of scientific models rather than to their improvement (Quine 1964; Newell 1973).

Each level affects and is affected by adjoining levels. Because of this it is important to follow a "rule of minimal inclusion: any explanation of behavior must take into account any and all levels of systemic organization efficiently present in the interaction between the system operating and the environment of that system" (Rubinstein et al. 1984: 93). Precisely which systemic levels need to be considered varies with the research question asked.

It is additionally important to understand that (*both* basic and applied) scientific research is a continuing process alternating between inductive and deductive work (cf., Rubinstein 1984). Any research finding is a product of this process and is in a fundamental sense an artifact abstracted from ongoing activity. These research products can help to provide categories, such as nosological systems, through which useful judgements about the world can be made. But the value of such categories depends upon their providing information that is useful for particular purposes. It is, therefore, also important to be conscious that, fundamentally, categories are reifications of processes and do not exist independently of the purposes for which they are developed.

The evaluation of nosological categories in psychosocial work is one area in which it is possible to sketch a strategy for bridging between epidemiological and anthropological levels of analysis. We argue that this bridging can be accomplished, for example, by asking an essentially anthropological question about the nature, meaning and significance of a particular nosological category and answering that question using whatever methods turn out to be appropriate (whether quantitative, qualitative, or a combination of the two).¹ In this approach methodological decisions appropriately follow from theoretical judgements. Methodological decisions in turn allow the selection of methods; questions of technique are secondary to questions of logic. In the following sections of this paper we use the example of "hyperactivity" to illustrate how this approach can be used to further the integration of epidemiology and anthropology and ultimately the understanding of psychosocial disorders of children.

CHILDREN AND THE "NEW MORBIDITY"

During the past twenty-five years people interested in children's health in the

United States have become increasingly concerned with questions of mental health and illness. This shift in focus has several interdependent causes. Among these are the dramatic changes in patterns of childhood morbidity and mortality that took place prior to this time. During the first half of this century morbidity and mortality among children were due primarily to infectious diseases and acute illness. But, by the middle of the century, the development of antibiotics and public health programs which improved environmental conditions had virtually eliminated these as serious threats to children's health (Perloff et al. 1984).

As a result of these developments, patterns of practice changed among pediatricians and others involved with promoting child health (Pawluch 1983). These practitioners began to spend more time on "well-child" care than they had previously, and this led to an increasing concern with the behavioral, psychosocial problems of children (Haggerty 1983, 1982). Well-child care in part has come to be viewed as a way to prevent psychosocial problems in children (e.g., Green 1985, 1983; Green et al. 1982). Yet, despite such professional judgements and the faith of many in the value of well-child care, evaluations of the efficacy of well-child visits for preventing psychosocial problems yield results that are equivocal at best. At worst, such research suggests that well-child care does not help prevent psychosocial difficulties among children (Feldman 1984; Gilbert et al. 1984; Rogers et al. 1974).

Equally important in focusing attention on children's mental health and illness has been the perception that the incidence and prevalence of psychosocial difficulties among children have been increasing. In 1975 Haggerty and his associates identified this apparent trend as the "New Morbidity". The new morbidity is comprised of psychosocial, behavioral and other problems which involve processes outside of the "strictly medical" (or perhaps, traditionally medical) purview (Haggerty et al. 1975: 94). It includes "Learning difficulties, and school problems, behavioral disturbances, allergies, speech difficulties, visual problems, and the problems of adolescents in coping and adjusting . . . [and] family social problems and the management and handling of everyday life stresses" (Haggerty et al. 1975: 316). They noted the growing importance of these problems of children, and for their caretakers.

Reports from clinicians and researchers appear to confirm this judgement. Starfield et al. (1980: 159), for example, report that between 5% and 15% of all children seen at seven primary care facilities during 1977 were "recognized as having behavioral, educational, or social problems." More recently, Starfield reported that in a seven year longitudinal study of 2,591 children (from primarily white middle-class, two-parent homes), 25.1% were diagnosed as having "psychosocial morbidity" and 17.3% as having "psychosomatic morbidity" (Starfield et al. 1984: 825).

Several studies of mental health problems and the use of medical services by children in Monroe County, New York underscore this sense of a rising

prevalence among children of psychosocial and behavioral problems. In a series of studies analyzing data from a cumulative psychiatric case register, Roghmann and his associates report that the treated prevalence of psychosocial problems among various categories of children ranges from 1% to 10%. They add that "as only about half of the children with such problems, as identified by pediatricians, are seen by psychiatric specialists, the total treated prevalence would be appreciably higher" (Roghmann et al. 1982). Their work thus suggests an identified prevalence of psychosocial problems among children of up to 20%, and leaves open the possibility that the true prevalence of such problems is in fact even higher.²

More detailed analyses of the cumulative psychiatric case register show increases in the treated incidence of situational disorders and behavioral disorders from 1960–1977. Roghmann et al. (1984: 789) suggest that this rise is due "partly to changes in diagnostic classification and partly due to changes in the availability of care". Although the observed rise in treated prevalence may be, in part, artifactual, they note that the "morbidity and mortality still encountered will increasingly be due to 'social causes' such as poverty or lack of care due to maldistribution of manpower and facilities" (Roghmann, et al. 1984: 372).

Despite such reports, it has turned out to be particularly difficult to identify cases of the new morbidity among children. This result is due to at least three factors. First, the new morbidity is comprised of a very heterogeneous set of difficulties. For instance, in a recent study Starfield, et al. (1980: 160), included;

psychoses, neuroses, personality disorders, behavioral and psychological problems, learning disabilities, educational problems, mental retardation, developmental problems, situational problems, social maladjustments, parent-child problems, syphills, gonorrhoea, sexual problems, pregnancy, family planning, enuresis, urinary frequency, feeding problems, adverse effects of medicinal, chemical and environmental agents, and complications of medical care.

Second, each of the specific psychosocial disorders that make up the "new morbidity" is comprised not of a single distinguishing behavior but rather of a heterogeneous and sometimes overlapping complex of behaviors. Third, it has been observed that the "pathological behaviors" associated with some presumed psychosocial disorders show great context sensitivity. Some researchers have come to believe that many of the children classed as having psychosocial disorders suffer not from some deficit but from the "medicalization of deviance" (Conrad and Schneider 1980).

In some settings certain children behave in ways that are called "pathological", but in other settings these same children act "normally". Viewing this inconsistency these researchers conclude that the children's "sickness" is an artifact of social processes — that it is a socially constructed category — rather than an objective fact. The socially constructed nature of sickness categories, they say, is further demonstrated by the flexibility of the category

boundaries. Frequently, they argue, the result of this flexibility has been that, as the authority of the medical profession has increased, behaviors or other characteristics previously thought of as odd, or curious, or handicapping have been redefined as sick; they have been medicalized. This medicalization, in turn, has the effect of narrowing the range of behaviors considered normal in a society and places many of these behaviors and attributes outside of the scope of things that lay-people can legitimately talk about (Conrad and Schneider 1980; Hufford 1982, 1983). In addition, the medicalization of deviance has been shown to lead to changes in the social roles (usually a narrowing) that are open to persons possessing the medicalized attribute (Penfold and Walker 1983; Scott 1969; Ablon 1984).

Kaplan (1983, see also Penfold and Walker 1983), for instance, shows how changes in psychiatric nosology have led to the linking of many feminine behaviors with specific psychiatric disorders (such as, "Hysterical Personality" or "Passive-Dependent Personality"). Conrad and Schneider (1980) show how this process has effected such diverse areas as alcoholism, opiate addiction, juvenile delinquency, child abuse, and homosexuality. Several researcher have argued that this same kind of social construction of sickness, and the increasing amount of behavior said to require medical rather than other kinds of attention, are illustrated by the case of "hyperactivity", which we discuss later in this paper.

Two research traditions have investigated the new morbidity. These two traditions can roughly be distinguished by their focus and research style. The first, grounded in epidemiological concerns, tends to seek patterns and to rely heavily on surveys and experimentation. The other, rather more anthropological in commitment, seeks to understand processes and favors the use of naturalistic observation and detailed behavioral description. Both traditions have produced interesting and important results. But, because they raise essentially different questions and use different logical rules of evidence and inference, each tradition has largely failed to take full account of the work of the other. Therefore, our understanding of the nature and significance of the new morbidity in children has not developed as fully as it could have.

COMBINING QUALITATIVE AND QUANTITATIVE APPROACHES

Coherently integrating qualitative and quantitative approaches is very difficult. Qualitative research, such a ethnographic descriptions of the context and meaning of particular behaviors, is often taken by survey researchers as interesting for its anecdotal value. But, because the results of this work are not generalizable to large populations and because the outcomes of ethnographic work may not be directly replicable, epidemiological researchers often value this anthropological research less than more traditional survey-style epidemiological research.³ In their view, qualitative work is at best useful for preliminary studies associated with the subsequent validation of a

survey instrument. For instance, a number of variables now generally considered important in psychosocial epidemiological studies — indices of social class, ethnicity, household patterning, social networks, and the like — were derived from anthropological understandings of particular communities (e.g., Hollingshead and Redlich 1958; Dunham 1959; Rapoport 1959; Srole et al. 1962; Leighton et al. 1966).

When viewed in this way, anthropological research is seen as valuable for two principle reasons. First, anthropological research can establish that some “things” previously unattended to should be taken into account. For instance, new “cultural” variables might be added to a survey questionnaire. Or, a traditional intervention (like psychotherapy) might be packaged differently for minority populations (e.g. Acosta et al. 1982). Second, the results of anthropological work undertaken early in a project can be built into sampling and questionnaire strategies. Such “preliminary” research may be seen as worth the investment of resources and time because it provides results that reused throughout the course of project can lead to improvements in research instruments.

Some qualitative researchers reject this conditional acceptance, claiming that it is mistaken for at least three reasons. First, they argue, it is wrong to assume that an anthropological understanding of a specific community can simply be transported to inform work in another, albeit demographically similar, community. They assert that survey instruments and questionnaire items can be developed which both assess social process in a community and reliably reflect the health care community’s professional judgement of the nature of illness and disease in that community. Yet, when used in another community these questionnaires and surveys may not adequately reflect community definitions of illness and disease. Rating instruments, questionnaire responses and the like, simply cannot be well understood without a thorough understanding of the specific context in which they are given (Adair and Deuschle 1970). Some epidemiological researchers have heeded this warning, but not always with results conforming to the spirit of the anthropological objection. For instance, cautions about the need to attend to social processes have often resulted in the definition of a new index (like, “ethnic group”) rather than in a concern with the local meanings (of ethnicity, for example) and their impact on the research.

Campbell (1978) described some aspects of this situation when he noted that although,

rules regarding the use of qualitative knowing are clearly present in the quantitative methods books, they are rarely exercised. The researcher is apt to feel that presenting such content undermines the appearances of scientific certainty, or that weaknesses on these points are evidence of his own incompetence. The field experimenter’s defensiveness vis-à-vis his laboratory colleagues leads to further minimizing of this content and still more so when he is earning his living as a contractor in programme evaluation (Campbell 1978: 194).

Second, anthropologically-oriented researchers argue that because within a

community the meanings and importance placed upon forms of behavior constantly undergo change, instruments appropriately used in a community at one time may at a later time be inappropriate. The anthropological understanding over time of even a single community requires ongoing qualitative research involvement (Ablon 1981, 1977; Foster et al. 1979). And, third, they argue that research structured around the use of preset questions excludes from consideration some types of important data (Argyris 1980; Rubinstein et al. 1984). The use of preset questions assumes a knowledge of the phenomenon being studied, often an inappropriate assumption. The structure hinders the revision of our knowledge in light of new information which might have been offered unsolicited.

For their part, anthropologically-oriented researchers have taken to heart some methodological lessons taught by epidemiological researchers, but not always in ways of which the latter would approve. Questions of sampling and the statistical treatment of data are now more widely used by anthropological researchers interested in health and behavior (Landy 1977; Brewer and Collins 1981; Schensul and Borrero 1982). Standardized interview schedules and survey questionnaires also sometimes comprise the major research focus in some areas of field work. However, the anthropological researcher's interest in these techniques often is not to make the research results generalizable but to deepen contextual understanding. For example, Estroff (1981) used the "Community Adaptation Schedule" with a convenience sample to gather texts that could then be analyzed in-depth, rather than only scored.

Despite the ambivalent attraction of anthropological researchers to epidemiological methods and of epidemiological researchers to anthropological information, attempts have been made at integrating the approaches. Yet, very little progress has been made. In part this is because attempts at integrating the two methods have most often really involved sanctioning under one administrative umbrella two parallel, but substantially separate, research programs, rather than the integration of them.

The results obtained by using either approach alone are incomplete and less robust than results obtained by successful integration of anthropological and epidemiological perspectives, employing a variety of methods and tools of inquiry (Campbell and Fiske 1959; Collins 1981; Wimsatt 1981). Even results of social research intended to provide information upon which interventions can be designed, if carried out within the canons of only one methodological approach, will be of limited use (Rubinstein 1984).

QUALITATIVE AND QUANTITATIVE UNDERSTANDINGS: "HYPERACTIVITY"

The effect of the schism between anthropological and epidemiological researchers is extraordinarily clear in the case of "hyperactivity". As one of the most frequency diagnosed psychiatric disorders in children during the

last two decades, "hyperactivity" serves as an exemplar of the new morbidity.⁴

Estimates of the prevalence of "hyperactivity" have varied widely (see, Table I). The generally accepted estimate is that "hyperactivity" affects between 3% and 5% of school-aged children (Barkley 1981). The prevalence of "hyperactivity" is thought to be higher in boys than in girls (around 6: 1), and in lower-income (or lower socio-economic) groups. Interestingly, however, based on analyses of teacher questionnaires or ratings, Trites (1979) classified as "hyperactive" 14.3% of a 14,083 sample of school children, and Lapouse and Monk (1958) estimated on the basis of teacher ratings that as many as 57% of boys and 42% of girls could be considered "hyperactive".

TABLE I
Some estimates of the prevalence of "hyperactivity" in children^a

Source	Prevalence estimate
Huessy (1967; 1974)	10–20%
Hussy and Gendron (1970)	10–20%
Miller et al. (1973)	9.3% boys; 1.5% girls
Cantwell (1975)	5–20%
Wender (1971)	5–20%
Stewart et al. (1966)	4%
Renshaw (1974)	3%
Lambert et al. (1978)	1.19%
Barkley (1981)	3–5%
Trites (1979)	14.3%
Sprague et al. (1974)	9% boys; 2% girls

^a After Bosco and Robin (1980).

Part of this great range in prevalence estimates is due to the fact that the behaviors thought characteristic of "hyperactive" children appear to be context sensitive. Thus, children may at one time display, and at other times not display, the diagnostically significant behaviors (Collins 1981; Douglas 1972; Sleator and Ullman 1981). In fact, Lambert et al. (1978) have shown that the diagnosis of "hyperactivity" varies depending upon whether the judgements of one or of a combination of parents', teachers', or physicians' determinations are accepted. They also argue that the diagnosis of "hyperactivity" varies as a result of the number of observers needed to agree on the diagnosis and the social position of these observers vis-à-vis the child.

Since the beginning of this century, the syndrome of behaviors that has been identified as hyperactivity have been classed under a series of changing diagnostic labels. Characteristically, children diagnosed as "hyperactive" are said to be more aggressive than non-hyperactive children, or to be "dis-

ruptive", because they often engage in "excessive vocal noises" that disrupt ongoing social activities. Risk-taking behavior of "hyperactive" children appears to exceed that of non-hyperactive children and is often associated with an increase in physical mishaps (like, scrapes and bruises, broken bones, and general accidents). Although generally not scoring significantly lower on intelligence tests (especially individually administered tests) than matched controls, "hyperactive" children are generally academic underachievers (Cantwell and Satterfield 1978; Douglas 1972), and their risk of school failure is 2—3 times greater than for non-hyperactive children.

In general, "hyperactive" children are thought to exhibit three types of problems which result in referral for treatment (Ross and Pelham 1981): (1) they tend to have problems in academic learning, resulting in school failure; (2) they seem to have trouble relating to their peers; and, (3) they have trouble complying with adult requests and commands (Safer and Allen 1976; Barkley and Cunningham 1979).

For some time researchers reasoned that since children who showed these troublesome behaviors acted very much like children with brain injuries, "hyperactive" children must also be neurologically impaired (Strauss and Lehtinen 1940). These children were therefore diagnosed as having Minimal Brain Damage (MBD). Much research effort went toward characterizing the differences between MBD and "normal" children and toward finding better ways to treat and manage troublesome MBD children. For the most part, this work proceeded using epidemiological research strategies such as various experimental designs and prevalence surveys among school-aged children. These efforts did not produce a convincing account of the etiology of MBD or effective treatments for these children.

In a recent review, Rutter (1978) noted that less than 5% of all children diagnosed as MBD show any evidence of structural brain damage, and that most children suffering brain damage did not develop the syndrome of behaviors used to define MBD children. Moreover, MBD children did not respond uniformly to pharmacologic treatment. This further indicated that it was unlikely that they suffered from some shared but unidentified organic disorder. MBD was replaced as a diagnostic category by the designation of "hyperactivity". This new diagnostic category was thought to have the advantage that it was descriptive of the "behavioral disorder" shown by this group of children.

When researchers working in qualitative traditions also became interested in the problem they noted that the group of children first diagnosed as MBD, and later as "hyperactive", was very heterogeneous. They relied mostly on naturalistic observation and pointed out that the behaviors of this group of children varied as to when, where and how troublesome they were. These researchers also noted that in everyday interaction many "hyperactive" children did not seem to be less bright than their peers. They therefore questioned whether the condition was really a disease entity. They offered a

number of alternative explanations — labeling and the social control of deviance, for instance — to account for the difficulties faced by these children. Each account argued that these children did not have a disorder and hypothesized that the source of problems lies in their social environments (Conrad 1975).

Although the conclusion that hyperactivity was not a “disease entity” was objectionable to epidemiological researchers, their own work *also* suggested that children classified as “hyperactive” were a very heterogeneous group.⁵ Continuing to rely heavily on the canons of epidemiological-experimental research, they too showed that although children diagnosed as “hyperactive” could be seen as different from other children on a number of measures of behavioral and cognitive activity, “hyperactive” children showed great variability in relation to all of these measures (e.g., Homatidis and Konstantreas 1981).

This turned out to be so, for example, for attention and impulse control (Douglas 1972; Brown and Quay 1977), for cognitive style (Sykes et al. 1973; Sprague et al. 1974), and for levels of motor activity (Whalen and Henker 1980). Variation in the characteristics of “hyperactive” children undoubtedly accounts in part for the wide range of estimates for the prevalence of “hyperactivity”. This variation was troublesome to both clinicians and to researchers. And, in 1972 the epidemiological research community’s attention was redirected when in an influential paper Douglas (1972: 260) argued that “hyperactivity is only one of a constellation of critical symptoms. [and that] it may be just as important to consider the quality of the hyperactive child’s behavior as its quantity”.

In addition to “over-activity”, Douglas identified inability to sustain attention or to control impulsivity as important qualities of “hyperactive” children’s behavior. It followed that disagreement about the etiology of these behaviors might be resolved (and appropriate interventions designed) if, rather than taking these children to form a single class whose behavior could be contrasted with other children’s behavior, research efforts were directed at understanding the difficulties faced by smaller, better diagnosed and more homogeneous groups of children.

There were two possible responses to Douglas’s suggestion. The first was to continue to view the global category of hyperactivity as consisting of a number of subtypes. Further research could then seek systematic contrasts between the global group of hyperactive children and other children, as well as to identify differences among subtypes of “hyperactive” children. This view depended upon interpretations of data that conformed to standards of experimental-epidemiological research.

An alternative response to Douglas’s argument was to conclude that it was a mistake to cling to the global category of hyperactivity. This view would require that the global category be partitioned into a number of smaller categories; the adequacy of each as a diagnosis would then be evaluated.

Thus, through work with smaller well-described groups of children, a number of etiologies and syndromes might be defined or a portion of the children previously diagnosed as "hyperactive" (sick) would now be understood to be "normal". This conclusion would be congenial with that reached by the qualitative research community, who had already challenged the usefulness of the global category of hyperactivity.

The eventual response of the psychiatric community was to redefine the American Psychiatric Association's Diagnostic and Statistical Manual of Mental Disorders, Second Edition (DSM-II) diagnostic category of hyperactivity (properly, "Hyperkinetic Reaction of Childhood") as the DSM-III category of Attention Deficit Disorder. Included *within* Attention Deficit Disorder were two subtypes — "with hyperactivity" (ADHD) and "without hyperactivity" (ADD). This response expanded the earlier experimental-epidemiologically derived model of what is happening with these children, and required no change in the governing values of this clinical or research community.

EVALUATING NOSOLOGY: ANTHROPOLOGY AND EPIDEMIOLOGY COMBINED

The establishment of the nosological category of Attention Deficit Disorder with two subtypes appears to us to have pushed the results of the earlier experimental-epidemiological work to its limit. However, efforts have continued towards evaluating the new nosological category. To the extent that this empirical research shows that the category Attention Deficit Disorder fails to deal adequately with the earlier noted anomalies, it suggests the need for new models of research and practice.

There have been several evaluations of the reconceptualization of hyperactivity as an Attention Deficit Disorder. In each case the investigators have worked from only one theoretical level of analysis; be it psychological, sociological or clinical (Douglas 1972; Eysenck et al. 1983; Conrad and Schneider 1980; Russell et al. 1983; cf., Collins 1981). As a result these investigations each encompass only a single systemic level. They have, in effects, ignored the rule of minimal inclusion which would require that an explanation of "hyperactive" behavior in individuals take into account the other systemic levels interacting with and in the environment of the children (Rubinstein, et al. 1984: 93). Thus, this empirical work has resulted only in general disagreement about the nature and reality of "hyperactivity" and Attention Deficit Disorder.

Even in research reports that consider information from different disciplines, we find that the theoretical levels of analysis are separated while methodological debates are joined (Whalen and Henker 1980). This joining of questions of method, while expanding the methodological options for studying behavior, cannot hope to achieve the objective of expanding our

fundamental understanding of behavior. Indeed, the latter is not possible unless our investigations encompass a broader range of systemic levels. As long as attention is focused on methodological issues, and the incorporation of conceptual materials from different levels is ignored, little progress toward better understanding this behavior should be expected (Rubinstein and Laughlin 1977; Argyris 1980).

A recent study undertaken to evaluate the diagnostic category of Attention Deficit Disorder (Rubinstein and Brown 1984) illustrates one way the bridging of levels might be achieved. In that study the investigators asked if the new nosological system presented in DSM-III served the joint diagnostic and research purposes for which it was developed. The strategy was to ask about the fundamental nature and role of the category of ADD/ADDH, much as anthropologists often ask about the nature and meaning of "folk" categories in everyday life. This essentially anthropological question might have been answered by detailed and intense observational study of children who had been diagnosed by clinicians and researchers as having ADD/ADDH. It turned out that the question could also be answered by using epidemiologic methods. The study serves to illustrate the point that the integration of different theoretical levels of analysis can provide results not obtained when efforts at integration are restricted to issues of method.

The heterogeneity of the group of children included in the category "Hyperkinetic Reaction of Childhood" provided an impetus for its redefinition as Attention Deficit Disorder with and without Hyperactivity. The psychological and epidemiological research in the area suggested that this difficulty could be dealt with if these children were sorted into two groups, one of which had more and the other of which had less severe problems. The qualitative research in contrast supposed that the heterogeneity of these children's behavior could be accounted for by seeing their difficulties as social, and therefore external to the children. The situation that results is a standoff: Qualitative researchers acknowledge the methodological rigor of psychological and epidemiological studies in the area but denigrate them for having reified social process; epidemiological and psychological researchers dismiss the qualitative results as unhelpful and hopelessly imprecise. The consequence for the research community in general is that both sets of heuristics and both sets of attendant biases are reinforced.

In order to avoid the methodological bickering that characterizes debates in this area, Rubinstein and Brown (1984) sought to evaluate the nosological category (in spirit a medical anthropological question) using a variant of a classic epidemiological research design, the retrospective case-control study.

Much epidemiological work takes place after some disease or disorder had affected a group. Because of this, epidemiologists interested in discovering the factors that increase an individual's risk of getting a disease or disorder must "work backward" from effect to cause. As epidemiology developed, a methodology for working with information about past expo-

tures was articulated and refined. The result is the “case-control”, or retrospective study, “probably the major contribution of epidemiologists and their predecessors to the general area of research methods” (Kleinbaum et al. 1982: 69). The *logic* of the case-control design requires that systematic, controlled comparisons be made between two groups, only one of which has the disorder or disease in question. Schlesselman (1982: 14) describes this research strategy:

The *case-control study*, also commonly called the *retrospective study*, follows a paradigm that proceeds from effect to cause. In a case-control study, individuals with a particular condition or disease (the *cases*) are selected for comparison with a series of individuals in whom the condition or disease is absent (the *controls*). Cases and controls are compared with respect to existing or past attributes or exposures thought to be relevant to the development of the condition or disease under study.

This kind of research is most straightforward when the conditions of interest are clearly bounded, identifiable entities. It is especially useful if these can be indexed by a specific test or syndrome. Even then retrospective data may be biased for a number of reasons, such as the refusal or the inability of study participants to respond to questions, lapses of memory, bias inadvertently introduced by the researcher, and the like. (See, Sackett 1979 for a review of these and other sources of bias.)

Assuming that cases are selected for clear reasons and that potential sources of bias in the retrospective data are guarded against, any case-control study is better or worse depending on the controls picked (Kleinbaum et al 1982: 67–70; Schlesselman 1982). In many studies controls have been selected from among healthy persons who otherwise are “matched” with the cases. But, this is a matter of tradition not of logic. Insisting that controls always be “normals” confuses issues of logic and design with questions of technique and strategy. Using normal controls can even result in a study’s being less useful than it might be otherwise. There can be good reasons for choosing controls who share with the cases some general diagnosis (like, cancer) when trying to figure out what makes the different sub-diagnoses (like, breast, throat, lung, etc.) different.

One area for the potential integration of anthropological and epidemiological understandings is in the specification of appropriate controls. Anthropology’s contribution can be made through better defining homogeneous groups through the proper identification of important demographic and other social characteristics. The Rubinstein and Brown study sought to assess to what degree children diagnosed as having Attention Deficit Disorder without Hyperactivity differ from children diagnosed as having Attention Deficit Disorder with Hyperactivity on a number of factors that previous research had shown to distinguish “normal” children from the general group of “hyperactive” children.

The specific objective of this research was to see whether a number of

standard clinical measures and diagnostic measures available in program records could be used to successfully predict the diagnosis of children seen at a therapeutic research program for ADD/ADDH children during a single year. Upon entering the program children were classified as either ADD or ADDH, using the DSM-III criteria. The classifications were made using the independent judgements of two clinical members of the program staff. Clinical ratings were made without knowledge of each child's individual history. As a result of the classification, an ADD sample was identified ($N = 23$), an ADDH sample was identified ($N = 23$), and three cases for which the classification could not be decided were deleted from further study. A satisfactory level of interrater agreement was obtained. The samples were equivalent on a number of possibly confounding characteristics including age, sex, birth history, and intelligence test scores. In addition, before being treated at the clinic each child was given a thorough physical examination. If that examination showed evidence of gross neurological, sensory, or motor impairment, obvious physical defects, or major disease the child was referred elsewhere for treatment and not entered in to the clinic's program. Therefore, all 46 of the children included in the study showed no organic basis for their diagnosis.⁶

Comparisons were made between the ADD and ADDH samples on number of variables that previous research had shown to distinguish normal children from "hyperactive" children. These factors are grouped in three sets of measures: attention and impulsivity measures; achievement tests; and rating scales of behavior. (These measures are described briefly in the Appendix to this Chapter, where the study means and standard deviations for each measure are also presented.)

Because the variable-to-subject ratio was too small to allow all of the variables to be used to estimate a discriminant function equation, a variable selection procedure was used. Three step-wise discriminant function procedures, using ADD/ADDH as the dependent variable, were carried out, one for each class of variables listed above (see, Table II). The objective was to select the measures that might be used in a fourth discriminant function analysis to best classify children as ADD or ADDH. Only one of the measures (Teacher Rating Scale of Impulsivity) had a level of significance sufficient for entry into the step-wise discriminant analysis.

Little success was achieved in sorting the children from each sample into ADD and ADDH categories on the basis of the discriminant function model containing only the TRSI (Wilk's lambda = 0.9153, $F(1, 38) = 3.517$, $p = 0.0684$). Using this model 54.5 percent of the children could be classified correctly as to diagnosis, only slightly more accurately than if the assignment had been made randomly (see, Table III).

Because hyperactivity is thought to increase the inability to control attention and impulsive behavior, ADDH children had been expected to score significantly lower on measures of achievement, attention, and impulse

