planning for the next generation of risk studies*

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Robert White's (1959) landmark article "Motivation Reconsidered: The Concept of Competence" focused attention on the possibility of conceptualizing social development in terms of the mastery of basic life tasks. This viewpoint, which had a major impact within psychology, helped renew interest in studying the acquisition of social skills during childhood. A similar emphasis had permeated much of the psychiatric literature during the 1950s which characterized the social development of the child in terms of adjustment, coping, and adaptation. During the following decade, attempts by social scientists to develop operational procedures for measuring adjustment or competence during childhood proliferated. Using the new found power of the computer, researchers applied factor analysis in an attempt to identify major dimensions of adaptation in both normal and clinic samples of children. Beginning in the late 1960s a separate group of investigators—a conglomeration of psychologists and psychiatrists interested in studying the development of severe psychopathology—joined forces in a series of longitudinal studies of clinic samples of children defined as having a greater than expected probability of developing severe psychopathology including schizophrenia. This body of research, referred to as "high risk" research (Garmezy and Streitman 1974), is in many ways heir to the earlier efforts to assess social competence in children with an additional emphasis upon developing assessment methods optimal for longitudinal research. The assessment of child competence, in this context, requires the development of age-appropriate measures of critical domains of child functioning so that course of development may be assessed continuously across the childhood years (Garmezy 1970, and Jones 1974 and 1978).

The present paper is based in part on a review of 70 longitudinal studies conducted over the past three decades dealing with the prediction of various facets of competence in children and adolescents. This review was undertaken in preparation for a meeting of the Consortium of Risk Researchers held in Rochester, N.Y., in April 1977 which addressed itself to the definition and assessment of child and adolescent competence. The review indicated that despite the excitement generated by early work in the field, the intervening years have witnessed a slowing of the development of instruments for assessing competence in childhood, particularly within the framework of longitudinal research. The present paper is an attempt to pinpoint reasons for the field's slow development and to suggest ways in which development might be facilitated.

The discussion to follow will be organized into three sections dealing with (1) methodological problems inherent in longitudinal and especially "risk" research in which the assessment of competence in children is central; (2) problems inherent in the institutional structure which underlies funding and support for such research; and finally (3) some suggestions for changes in research and funding establishments which might facilitate the methodological developments in child competence assessment which would be crucial in carrying out the next generation of "risk" studies. An outline of major problem areas in longitudinal risk research as discussed in the text is presented in table 1.

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1A complete listing of these studies is available from the first author upon request.
Table 1. Major problem areas in longitudinal risk research

I. Methodological
A. Clarifying objectives: Description vs. prediction
B. Choosing the sample
   • Specificity of selection criteria
   • Selection of controls
   • Size of sample
C. Choosing measures: A cost-benefit analysis
   • Intensive vs. extensive measurement
   • Objective vs. judgmental measurement
   • Static vs. process measurement
   • Measurement and theory
   • Sophistication in measurement selection
   • Old vs. new measures

II. Institutional
A. Funding problems
   • Instrument development
   • Application for funds
   • Getting grants vs. project continuity
B. Organizational problems
   • Breadth of leadership teams
   • Investigator turnover
C. Setting problems: Career advancement

Methodological Problems

It is difficult to imagine an area of research which poses more methodological dilemmas than a longitudinal investigation of the development of childhood competence. In reviewing 70 longitudinal and predictive studies of child adjustment and competence from 1950 to 1976, we found that most have been undertaken on a modest scale (for example, Baumrind 1975, Bell 1973, Birns and Golden 1972, Engelhardt 1970, Karlberg et al. 1968, and White et al. 1973). They appear as isolated efforts, brave attempts to answer extremely complex and difficult developmental questions with methods and resources poorly matched to the task. The studies we reviewed can, as a total group, be characterized by smallness of scale, diversity of outcome criteria (e.g., academic success, delinquency, mental retardation, and adjustment), diversity of assessment procedures, lack of replication, utilization of inappropriate statistics, and a frequent failure to employ a coherent theory of development. Yet many of these shortcomings are less a matter of ignorance than of eagerness to gather at least some preliminary data concerning the interrelations among basic developmental measures. The following sections deal with specific methodological problem areas within longitudinal competence research that we have either culled from our review of the literature or experienced directly in our ongoing research.

Clarifying Objectives

Before making specific design decisions about selection of samples, measures, and age of assessment, the investigator is faced with a more basic decision which is often not fully appreci-
ated. This decision concerns the clarification of the fundamental objective of the study. Is the primary objective the description of those aspects of the child's functioning which constitute some model of competence at one or more points in time, or is the objective the prediction of an outcome of interest? It is extremely expensive to do a good job of both at the same time since the achievement of these separate objectives often requires the use of largely nonoverlapping behavioral and contextual measures.

In attempting to describe current child functioning, one attempts to answer the question, "How is the child doing at the present time?" (Goldfried and D'Zurilla 1969). In contrast, when the main job of the study is prediction, one attempts to answer the question, "What is the most parsimonious set of predictors to a specified outcome criterion that accounts for as much of the variance as possible?" As every statistics student is taught, multiple predictors are more powerful the more highly correlated each predictor variable is to the outcome criterion and the less highly correlated each predictor variable is to every other (Overall 1975). Whereas in description one seeks a moderate to high degree of redundancy in measurement to aid efforts to interrelate findings, in prediction one seeks independence of measurement. Consequently using descriptive variables as predictors may represent an inefficient strategy in predicting long-term or intermediate outcomes. Clarifying one's objectives, then, has implications not only for the direction of the study but for the selection of measures as well.

Choosing the Sample

The definition of the sample—particularly when one is dealing with the type and degree of pathology in parents, the assessment of social disadvantage, or the quality of family life—represents the application of highly sophisticated technologies of measurement. Typically, samples are collected on the basis of a few relatively undifferentiated selection criteria plus on-site availability. The development or even utilization of highly sophisticated criteria for selecting subjects would require time and money that is totally unrealistic for most investigators. As a result, longitudinal studies of children typically employ sample selection and descriptive criteria that are global and vague so that the comparison of one sample to another is difficult if not impossible. Indeed, due to the uniqueness of populations in various hospitals and urban centers, samples tend to be far more difficult to replicate than measures.

The issue of adequate controls is equally complex, costly and vexing. Every time a control group is added to a research design, the cost of running subjects and analyzing data jumps dramatically. As a result, normal controls are rarely employed—to say nothing of adequate controls for type and duration of illness, type of treatment, race, socioeconomic status, and family intactness. The typical economical alternative to running various control groups is to control for the presence of such critical factors by partialing them out of the sample, i.e., selecting a small, relatively homogeneous sample of subjects that represents only a circumscribed segment of the general population. Yet in a highly circumscribed sample, variables that account for a small portion of variance in the general population when compared to variables that are controlled for, such as socioeconomic status, IQ, or family disorganization, may appear to have exaggerated importance and may assume a disproportionately large role in the formulation of causal hypotheses. But the selection of such a small homogeneous sample is often the only practical alternative to the confusion in data interpretation that would result from a small heterogeneous sample.

The issue of sample size is also particularly important in relation to the statistical analysis of findings. As this is only a first generation of risk studies, there is much less hypothesis testing than there is searching for promising procedures and findings that might serve as a point of departure for future research. As a means of making sense out of often complex data, multivariate statistics are frequently employed and their use is often inappropriate. Since multi-
variate statistics inflate the probability of significant findings and in many cases mandate findings, large samples are needed for appropriate usage, and replication is necessary for confirmation. Because of costs, the findings from the initial analyses are often of dubious validity due to the small \( N \), and few studies are able to gather a replication sample as a corrective to the inflation of probabilities inherent in the multivariate procedures.

Choosing Measures: A Cost-Benefit Analysis

In choosing measures, the investigator must at some point do a systematic analysis of the tasks that a child must perform in a typical day or at a given age period as a basis for measurement selection, whether the study is biased in the direction of prediction or description. This analysis in combination with developmental theory, clinical theory, and experience with children provides the basis for selecting a limited number of behavioral, emotional, or cognitive domains of functioning which will be explored in depth. Next, one chooses the life settings in which the critical areas of functioning would be most clearly manifest. Only then does one begin the process of choosing specific measures to coincide with each setting. The selection of measures, therefore, ultimately resolves into an issue of sampling, of getting the most representative picture of the child’s life with available funding. Hence, the selection of measures needs to be based on cost-benefit analysis.

Intensive vs. Extensive Measurement

If one concentrates on only a few prominent settings and proceeds prematurely to choose among various measures within those settings, one runs the risk of measurement overkill within a limited life space. The result is a series of measures which account for less of the critical variance, whether for description or prediction, than would have been accounted for by sampling less thoroughly in more settings. In this regard, it is particularly easy to assume that highly dissimilar assessment methods account for independent predictive variance. However, measures as disparate as questionnaires and complex analyses of communication patterns can be highly redundant. At the point of choosing, there is no substitute for extensive experience with each measure. Such experience is rare given the relative scarcity of such studies for it implies knowledge of the relationships among the selected measures before their use in the upcoming project.

Objective vs. Judgmental Measures

Another common fallacy in choosing measures is to assume that the most valid measure of something is a direct measure of the phenomenon in the natural setting in which it occurs. If, for example, peer affiliation is the construct of interest, one may feel that it is most “validly” observed directly in its natural ecological context such as the playground or backyard. But there is no simple or direct relationship between validity and type of measure or setting. Direct observation often changes the phenomenon being observed, particularly in home settings, and even when a satisfactory mode of observation can be arranged, one may be privy to only the most superficial or highly common events. Since naturalistic observations are typically time consuming and costly to collect, reduce, and analyze, it may be both cheaper and more practical to obtain the information indirectly from caretakers or from the individual himself. Again, a cost-benefit analysis needs to be seriously considered.

Static vs. Process Measures

Another critical cost-benefit issue is the investigator’s decision to rely on what we will refer to as static versus process measures. A static measure assesses the current state of functioning of an individual or the nature of a relationship or set of attributions along predetermined quantitative dimensions. Adaptive behavior inventories and IQ tests are perhaps the first things that come to mind, but static
measures can also assess emotions or one's perceptions of complex social interactions. Process measures, on the other hand, directly assess the sequence and implied nature of social transactions. Process measures usually examine behavioral interactions or communication patterns and include measures as different as sequential behavioral observations on the one hand and an analysis of control styles from a verbatim transcript of a verbal interaction on the other. While it is easy to overgeneralize in characterizing static versus process measures, from the vantage point of a cost-benefit analysis, static measures tend to be far cheaper than process measures due to scoring and data reduction costs. The investigator who opts for process measures should be able to justify the extra costs and demonstrate that the job could not be done adequately by a static procedure. Naturally, the standard rationale for employing a process measure is that the type of transactional analysis that it supplies could not be provided using any other means. However, the use of such measures may ultimately face the investigator with a multitude of difficulties which may make the procedure more trouble than it is worth. Typical examples include the need to create a schema to summarize multiple scoring categories, the use of complex factor or cluster analyses, and the problems involved in stating conditional probabilities of events which follow one another.

**Measurement and Theory**

A noticeable deficit in much longitudinal developmental research is the general lack of an explicitly stated developmental theoretical framework. This is particularly dangerous in predictive research since, due to the inflation of probabilities that occur from the collection of multiple predictors, it is almost impossible not to find some "significant" predictors of the target phenomena. This inflation of probabilities is further exacerbated by the frequent inappropriate use of multiple regression procedures as discussed above. Consequently, measures must be chosen to perform a specific job or else, as in many studies, one runs the risk of generating many findings that do not act in concert to clarify basic developmental issues.

**Sophistication in Measurement Selection**

In many longitudinal studies, investigators typically demonstrate expertise in the assessment of their area of specialization and naiveté in the selection of measures in other areas. While it is perhaps natural that researchers would spend most of their time and effort designing assessments in their primary area of interest, many of the studies reviewed could have been greatly strengthened by adequate consultation concerning the choice of measures during the design stage. This issue relates to the organization and structure of research teams which will be dealt with in a later section.

**Old vs. New Measures**

Finally, within risk research, emphasis has shifted from a primary focus upon ultimate outcomes such as schizophrenia in mid-adulthood to a focus upon intermediate outcomes during middle childhood and adolescence. The emphasis on intermediate outcome calls for the development of improved measures of social-emotional functioning during childhood and adolescence for use as descriptors, predictors, and outcome criteria. In assessing the development of competence, however defined, investigators are often faced with the decision of whether to take some measure "off the shelf" which is not quite appropriate to their needs or theoretical preoccupations, or, alternatively, to develop their own measure which is more precisely suited to the experimental task. Investigators tend to be reticent to engage in instrument construction since it is slow, costly, relatively dull, and produces few publications. Needless to say, the low rate of instrument development has impeded the systematic development of this field of research and has undoubtedly contributed to the relative inefficiency of predictive studies in general.
Institutional Issues

In this section we shall review the major institutional and logistical factors which affect the feasibility of carrying out large-scale longitudinal research projects. Although these factors heavily influence the planning and operation of risk research programs which focus on the development of severe psychopathology, many are chronic issues plaguing longitudinal research in general. The issues addressed here concern the organizational nature of research in this country: the rituals, expectations, roles, and practices of the large institutions in which we work. At the outset, we should acknowledge how easy it is to throw stones at corporate entities and power structures. Our aim in this section is not to castigate a system which has proven itself viable and productive in many spheres, but rather to point out ways in which longitudinal research in particular is hampered by a system that was designed to encourage and facilitate many other types of research programs. It is our view that the risk research paradigm is particularly vulnerable to discontinuities of financial and institutional support given the timespan of such studies, the expense of collecting a sample of cooperative patients, and the common need to develop basic measurement instruments. Combining Bell and Hertz's (1976) comments with those arising from our own review and experience, we have labeled three primary problem areas having to do with (1) funding, (2) research organization, and (3) research setting.

Funding Problems

Long-term research requires long-term planning, the implementation of which requires long-term and predictable funding. Given the present situation of reduced Federal dollars and exceedingly high competition for private funds, major problems arise in obtaining continuous funding at levels sufficient to carry out programmatic research. The dilemma for longitudinal researchers is particularly acute since any discontinuity of funding may force the abandonment of basic research objectives. What effect does this present funding situation have in research planning?

Instrument Development

Reduced funding spanning relatively short time periods should be expected to lead to (1) a greater emphasis on descriptive over predictive studies, (2) a de-emphasis on the development of adequate and appropriate instrumentation, (3) an emphasis on cross-sectional over longitudinal studies, and (4) fewer assessments spaced farther apart. While studies tailored to these restrictions can be meaningful in their own right, there is little question that without assurances of stable long-term financial support, investigators will be reticent to approach some of the important questions that risk methodology enables us to study.

A particular difficulty of short-term funding is the creation of an atmosphere in which efforts are geared to producing relatively quick results. This situation often relegates the development of appropriately normed and standardized assessment procedures to a secondary position. Such downgrading of priorities for measurement development will simply put off the evolution of an adequate methodology of social competence assessment. This must ultimately limit the range and power of findings from major high risk studies since most of the presently operating risk research programs utilize indices of child competence as primary dependent variables against which family, parental, physiological, and other variables are to be compared.

Application for Funds

The funding base for longitudinal studies has in recent years been unpredictable at best. Since longitudinal programs must compete for funds with nonlongitudinal programs, and since relatively short-term programs produce findings
and publications rapidly, longitudinal projects are faced with a disadvantage in terms of repeatedly demonstrating productivity. To justify their existence in this competitive framework, investigators in longitudinal research often invest heavily in data analysis for grant renewals and yearly reports even though the analyses are on partial samples and are of little practical or theoretical use. Such reports are extremely expensive from two vantage points. First, staff must be pulled from subject acquisition as well as data collection and reduction tasks for data analysis and writing. Second, the dollar cost for data analysis is not small by any means. While preliminary data analysis part way through a program of research may be very helpful in getting a sense of the data, in identifying directions for future planning, and in determining which measures seem to be more productive than others, the extensive analyses required by most review committees often greatly exceed the point of diminishing returns. Further, it is not uncommon for a large-scale project to derive funding from two or three simultaneous grants, often from both governmental agencies and private foundations. In such cases, grant deadlines from one source or another may come up as often as once a year. In such cases, it is not unusual for senior investigators to spend a majority of their time analyzing data and writing reports that are unpublishable and serve only to keep the project alive. The waste is increased further as granting intervals at NIMH are shortened, as many have been in the past decade, from 5 to 3 years.

**Getting Grants vs. Project Continuity**

An additional problem exacerbated by short-term funding and the need for repeated self-justification in the marketplace is the fact that funding intervals and cohort intervals are typically unrelated. Plans for seeing a group of subjects as they reach their next critical age of assessment must usually be made in advance of actual grant review and funding. Yet funding agencies do not typically take into account the necessity of tying together start-up time for data collection at new age periods with start-up time for funding. At times assessment has to stop so that renewal grants and progress reports can be written or often critical procedures must be scrapped at the last minute due to a short-term hiatus in funding.

**Organizational Problems**

**Breadth of Leadership Teams**

A major organizational problem involves the bringing together of senior investigators with diverse specialties. At present, most risk projects operate under the leadership of one or two senior faculty members who bring with them extensive experience and expertise in only a few component areas of risk methodology. However, high risk methodology is so multifaceted and complex that various investigators' strengths are needed in areas as diverse as experimental research with adult schizophrenics, diagnosis of adult disorders, child clinical assessment, developmental and longitudinal methodology, psychophysiology, or family interaction assessment. We have already discussed how consultation at the early stages of the research enterprise could improve the selection of appropriate measurement tools in many studies reviewed. Such diverse demands on investigative expertise strain the resources of single researchers or even of most research teams. As a result, families, children, and adults are often assessed on a more limited or biased range of measures than might be the case if greater input at the senior level had occurred. This dilemma highlights the need for the development of new forms of cooperation and sharing of expertise between research projects, a subject which will be dealt with in detail in a later section.

**Investigator "Burn Out"**

Since risk research requires expertise in a variety of areas, it can become an enormously demanding endeavor in terms of time commit-
ment and knowledge. Senior investigators can therefore be at risk themselves for what we might call the "burn out phenomenon." Having spent several years responding to subject problems, scheduling and procedural difficulties, funding demands and the like, researchers often begin to lose interest, feel overburdened, and express concerns about the project limiting their own professional development. It may be that an organizational structure which rests on the long-term commitment of a few senior researchers may not be the best model for future studies. Such long-term commitment may be too demanding, restrictive, and repetitive for senior investigators so that the ultimate success of the project could become seriously compromised.

Leadership Turnover

We have heard many esteemed colleagues joke that they might not be present when outcome data become available. Since most projects run under the energies and dynamic leadership of one or two senior investigators, one may legitimately ask what will happen to the project should a principal investigator pass away, become disabled, or leave the institution for another position. When leadership is concentrated, groups tend to become dependent, and a research program may become stalled or even disbanded when one or two senior people leave for any number of reasons.

Setting Problems

Most risk studies operate within academic settings of one kind or another: some through large medical colleges, others through colleges of arts and science. What impact does the press of academe have upon planning for the next generation of risk studies?

One problem which may discourage young faculty members from getting involved with risk research is that the publication of outcome data is years away. Recently appointed instructors and assistant professors with limited tenure are faced with demonstrating research productivity from the beginning or facing a refusal to renew their employment. Given reduced funding for faculty positions in general, the competition for existing slots often forces young faculty to focus on research areas which yield publishable data in the short run.

An equally disturbing problem for research already underway is the disruptive effect of losing co-investigators should they be refused advancement. When they leave, such investigators often take with them an area of expertise that does not overlap with those of other co-investigators and is therefore difficult if not impossible to replace.

Perhaps one of the most insidious problems deriving from this pressure for a high rate of publishing in academia, which is magnified for junior faculty, is an atmosphere which tends to discourage the innovative development of basic assessment instruments. The development of a measurement tool is tedious, time consuming, and often boring—an enterprise that is guaranteed to suppress one's rate of publication and reduce one's visibility.

Suggestions About the Next Generation of Risk Studies

In reviewing the methodological and systems problems in longitudinal risk research, it becomes clear that we are now faced with a series of rather urgent problems—problems that must be dealt with concertedly if we are to plan effectively for a future which will soon be upon us. From a historic perspective, unless some very fundamental issues of research planning and implementation are attended to, we may well replicate the methodological shortcomings of previous research efforts.

Yet the problems indicated by this review are not easily solved from a scientific or an institutional perspective, and we do not presume to possess "solutions." At best we would hope to raise salient points for future discussion. In the sections to follow, we would like to suggest
possible constructive courses of evolution for two relatively independent institutions concerned with research, the university on the one hand and governmental or private funding institutions on the other hand. Below we shall describe each of these proposals in some detail.

The Organization of Research

Several years ago The Grant Foundation provided funds for the creation of a consortium of risk researchers\(^2\) to meet once or twice yearly for what have been termed “mini-conferences.” Under the administrative direction of Norman Garmezy, Norman Watt, and a small executive council, several 1½-day conferences have been held each year dealing with such issues as diagnosis, psychiatric breakdown, ethical concerns, and family interaction. Indeed, the present review was undertaken in preparation for a mini-conference in Rochester, N.Y., in April 1977 which focused on the assessment of child competence. The aim of these conferences has been to provide a forum for increased communication and collaboration among research teams and to serve as the basis for the training of young professionals in the risk area. An outgrowth has been the increased sharing of methods and procedures and the development of more personal relationships among the researchers.

If we are to begin the process of systematically addressing many of the methodological and institutional issues detailed above, the high risk field should move in the direction of increased collaboration and possible increased centralization of efforts, of which the consortium may serve as a beginning. The many problems of conducting studies with dissimilar measures, small samples, demographic variability, and dissimilar diagnostic criteria, as briefly described above, underline the need for a more concentrated attack on these problems so that research decisions might be based less upon personal preference and subject availability and more upon systematic long-range problem solving.

A collaborative group could pool expertise in establishing a common direction, a setting of priorities, and a shared format for the next generation of studies. Planning groups composed of experts in specialty areas such as design, diagnosis, statistics, sample selection, and choice of measures in given areas could be established so that a maximal pooling of expertise could occur without requiring major commitments or geographical concentration on the part of investigators. For example, a research program could be designed by co-investigator/consultants who meet regularly while most of the daily management of the program could be delegated to senior administrators and technicians. Such a program might afford the researcher maximum career flexibility, freed from the grind of project management that produces “burn out.” In addition because of the backup support of the research team as a whole, the devastating effects caused by the departure of a senior investigator could be greatly reduced. In any case, the degree to which data would or would not be pooled, the degree of centralization, and the structure of the group itself could be decided upon at a later date, but the trend toward more collaborative efforts is desperately needed. What positive outcomes could be expected from such a collaborative development? While it is impossible to predict the direction of such a group with certainty, it conceivably could address some if not all of the following issues which have at times been discussed by the members of the consortium:

1. Agreement as to diagnostic criteria or a series of defined diagnostic dimensions to be used across studies would facilitate the pooling of patient groups where possible. Although it is certain that the dictated use of only one diagnostic schema would be exclusionary and constractive, the cost of using many different and often redundant criteria in various studies is much too high.

\(^2\) The consortium consists of the risk projects based at UCLA, University of Massachusetts, University of Minnesota, New York State Psychiatric Institute, University of Rochester, Washington University, State University of New York at Stony Brook, University of Vermont, and University of Waterloo.
2. The use of certain key measures across projects as a means of linking findings, as well as the development of new instrumentation as needed, would be a major goal. The sharing of instrument development requirements would certainly make such activities more palatable due to reduced cost in terms of time, funds, and interest.

3. Since presently operating risk projects appear to have sampled only a small range of potential risk populations, the research group could set priorities for developing projects to deal with, for example, social class 5 patients, patients with only one brief psychotic episode, or patients living in rural areas. Such efforts could reduce duplication and serve to round out data pools so that crucial comparison groups could be generated and pooled for use in multiple studies.

4. The confederation could make decisions about the selection of control groups to serve the needs of several studies simultaneously. For example, it may decide to employ certain psychiatric or normal controls to round out a general program of research rather than making such decisions in a piecemeal fashion as is presently being done.

5. Because of its diversification and involvement with several projects at different stages of operation, the confederation could become a major training center for those interested in psychopathology, child development, or longitudinal research. Some funds are already available to the consortium for this very purpose.

6. By virtue of its expertise and experience, the proposed research group could become an effective lobby for procuring sufficient funding from public and private institutions to provide stable, predictable, long-term research support. In addition, it could provide strong external support for young faculty members as they approach issues of tenure and promotion.

7. The group could become the center for developing techniques of subject acquisition and maintenance as well as providing a central location for tracking patients who move to different parts of the country.

There are some potential drawbacks to increased centralization, however, whether it is established as a confederation or a federation of research teams. These potential hazards should receive thorough consideration lest decisions for collaboration be made on romantic rather than practical grounds. Potential drawbacks include the fact that (1) centralization could lead to a cumbersome bureaucracy and a paper mill of organizational charts and memoranda, (2) uniform use of given scales and standardization of diagnostic criteria could restrict theoretical focus and reduce the opportunity for innovation, (3) centralization could greatly increase administrative costs including increased need for travel, and (4) allocation of funds could become highly politicized unless a spirit of cohesiveness and esprit de corps were maintained among individual researchers. The research effort could conceivably become dominated by a narrow group which would serve to restrict the breadth of input and constrain creative effort. The maintenance of a constructive, collaborative focus including freedom within the constraints of cooperation would undoubtedly require leadership which embodies quite sophisticated management skills.

Considering all of this, however, we believe the potential payoff may far exceed the drawbacks in terms of generating a viable solution to the large number of unsolved, repetitive problems facing the field. The existing consortium clearly demonstrates the practical utility of increased collaboration and may serve as a first step toward the development of a coordinated research effort on a broader scale.

**Proposals for Funding Mechanisms**

In this section we wish to explore a modification of the traditional grant review process so as to make it more responsive to the particular problems and dilemmas facing longitudinal research. These suggestions should be seen as guidelines to help funding institutions meet the changing needs of the research community.
First, it is apparent that the pressures and problems of longitudinal research greatly differ from those of standard laboratory and clinical undertakings. Time deadlines for followup, subject maintenance, continuity of measurement, and the host of problems described above create different and at times more complex dilemmas for longitudinal than for short-term research. With this in mind, it seems a handicap for longitudinal research to compete for the same funds under the same mechanisms as short-term research. Consequently, we would suggest that applications to fund longitudinal research be kept separate from other kinds of research support. If this procedure were followed, longitudinal projects could compete with each other on an equal footing without pressure to produce, at times, wasteful short-term data so as to stay competitive with projects which complete data collection in only a few months’ time.

It is suggested that a separate review process be carried out by a small group of experts experienced in the idiosyncracies of longitudinal method and design. These scientists could play a very helpful role in critiquing risk projects since many investigators are relative beginners in the longitudinal research area. The creation of such an expert group of reviewers might also serve to take both researchers and reviewers out of an adversary relationship and create an atmosphere of collaboration. This is not to say that a collaborative arrangement would serve to create a cozy, ingrown, self-perpetuating body. On the contrary, we suggest that the highest standards of critical review and cost-effectiveness planning be maintained by such a group. But at the same time, since both reviewers and applicants have faced the same difficult problems in longitudinal research, their common focus could provide a collaborative atmosphere which would be conducive to effective joint problem solving.

The creation of a separate review group for longitudinal research has a number of advantages to the existing process.

1. The time period of the grant could be jointly agreed upon based on the specifics of the research design rather than on more arbitrary general policies. For example, renewal intervals might coincide with cohort intervals. This would greatly aid project continuity and efficiency by eliminating temporary funding shortages which, if they were to occur near the beginning of the assessment of a given subject cohort, could force the premature abandonment of valuable assessment procedures.

2. The timing of progress reports and grant reviews could be set at the beginning to coincide with the completion of the assessment of various subject cohorts so that data analyses for such purposes could utilize full samples and thereby serve as “final” project analyses suitable for reporting into the public domain.

3. The review group could serve as a continuous monitoring body throughout the lifetime of the grant. Often the composition of the review committee which originally approves an application has largely changed by the time of renewal or extension. This may produce a continually changing review policy as well as criteria of judgment. We would suggest that the original review committee remain intact as much as possible for the duration of a project much as a dissertation committee does from initial proposal to the final presentation of results. This committee, along with providing continuity, would also be able to respond to unforeseen events arising during the course of the research which might lead to “mid-course” design corrections and to the reallocation of funds when the data and the circumstances warrant.

4. Because of its continuous association with the project and its ability to re-allocate funds when necessary, the review committee might possess a degree of clout which could facilitate institutional support in undertakings such as instrument development, tracking down subjects, and carrying out pilot work which is often relegated to a place of secondary importance by most current academic review committees.

In summary, we are proposing that longitudinal research be removed from competition with short-term projects; that a continuous re-
view process be implemented throughout the duration of the project which has the power to approve design changes; and that the committee be held responsible to maintain a cost-benefit perspective in decision making.

There are several drawbacks to this proposal which deserve comment. First, a middle management mentality could develop which creates a response to short-term and not long-term needs. For example, getting through the budget year without cost overruns can often assume greater importance than reaching the overall goals of the research several years hence. Second, the reviewing group could become heavily identified with the project and hence lose its critical perspective and objectivity. Third, it could misuse its power and become heavyhanded in dictating decisions of judgment which are traditionally the domain of the investigator. Yet these potential problems are held in common with all such review processes, and from our point of view, a cost-benefit decision highly favors such a system. Perhaps at least a pilot implementation would be useful and informative.

Conclusions

The issues to which we have addressed ourselves following a review of the literature on longitudinal research in child competence are concerned primarily with mechanisms which might effectively deal with chronic methodological and institutional problems that have hampered this area of research over the years. The fact that the same methodological problems have characterized the literature for the past three decades leads us to believe that unless we act concertedly, we are likely to continue producing research marred by the same limitations indefinitely.

Our comments regarding an alternative to the present funding and review process should be seen as an attempt to reduce waste and to provide a more coherent and pragmatic mechanism for review and funding which takes into account the special problems of longitudinal research design. Of course, any wholesale restructuring of review and funding mechanisms may create more problems than it solves. It is to be hoped that we, as scientists, would approach this task cautiously and develop procedures in a stepwise, empirical fashion. The present paper asserts, simply, that it is time to begin the task.

Summary

Seventy longitudinal studies of social competence in children conducted over the past 3 decades are critically reviewed in an effort to pinpoint current and potential problems in conducting longitudinal research with samples of children at elevated risk for developing severe forms of psychopathology. The review outlines chronic problems in accurately specifying objectives, choosing adequate and appropriate samples, and selecting maximally useful predictor and outcome measures in this research area. A variety of institutional and organizational problems that make risk research difficult to undertake and to carry to completion are also described. A federation of risk projects, which could seek unified funding and coordinate institutional support, is suggested. In addition, new grant review procedures that embody coordination of management efforts between funding and research agencies are recommended as one possible alternative to present practices.

References


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fifth world congress of psychiatric surgery

"Surgery for Psychiatric Illness," with emphasis on neurochemical aspects of stimulation and ablation, will be the topic of the Fifth World Congress of Psychiatric Surgery. The congress will be held on August 21-25, 1978, in Boston, Mass.

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